The 7 Percent Solution

The problem was to find the right laws of beta decay. There appeared to be two particles, which were called a tau and a theta. They seemed to have almost exactly the same mass, but one disintegrated into two pions, and the other into three pions. Not only did they seem to have the same mass, but they also had the same lifetime, which is a funny coincidence. So everybody was concerned about this.

At a meeting I went to, it was reported that when these two particles were produced in a cyclotron at different angles and different energies, they were always produced in the same proportions - so many taus compared to so many thetas.

Now; one possibility, of course, was that it was the same particle, which sometimes decayed into two pions, and sometimes into three pions. But nobody would allow that, because there is a law called the parity rule, which is based on the assumption that all the laws of physics are mirror-image-symmetrical, and says that a thing that can go into two pions can't also go into three pions.

At that particular time I was not really quite up to things: I was always a little behind. Everybody seemed to be smart, and I didn't feel I was keeping up. Anyway, I was sharing a room with a guy named Martin Block, an experimenter. And one evening he said to me, "Why are you guys so insistent on this parity rule? Maybe the tau and theta are the same particle. What would be the consequences if the parity rule were wrong?"

I thought a minute and said, "It would mean that nature's laws are different for the right hand and the left hand, that there's a way to define the right hand by physical phenomena. I don't know that that's so terrible, though there must be some bad consequences of that, but I don't know. Why don't you ask the experts tomorrow?"

He said, "No, they won't listen to me. You ask."

So the next day, at the meeting, when we were discussing the tau-theta puzzle, Oppenheimer said, "We need to hear some new, wilder ideas about this problem."

So I got up and said, "I'm asking this question for Martin Block: What would be the consequences if the parity rule was wrong?"

Murray Gell-Mann often teased me about this, saying I didn't have the nerve to ask the question for myself. But that's not the reason. I thought it might very well be an important idea.

Lee, of Lee and Yang, answered something complicated, and as usual I didn't understand very well. At the end of the meeting, Block asked me what he said, and I said I didn't know, but as far as I could tell, it was still open - there was still a possibility. I didn't think it was likely, but I thought it was possible.

Norm Ramsey asked me if I thought he should do an experiment looking for parity law violation, and I replied, "The best way to explain it is, I'll bet you only fifty to one you don't find anything."

He said, "That's good enough for me." But he never did the experiment.

Anyway, the discovery of parity law violation was made, experimentally, by Wu, and this opened up a whole bunch of new possibilities for beta decay theory. It also unleashed a whole host of experiments immediately after that. Some showed electrons coming out of the nuclei spun to the left, and some to the right, and there were all kinds of experiments, all kinds of interesting discoveries about parity. But the data were so confusing that nobody could put things together.

At one point there was a meeting in Rochester - the yearly Rochester Conference. I was still always behind, and Lee was giving his paper on the violation of parity. He and Yang had come to the conclusion that parity was violated, and now he was giving the theory for it.

During the conference I was staying with my sister in Syracuse. I brought the paper home and said to her, "I can't understand these things that Lee and Yang are saying. It's all so complicated."

"No," she' said, "what you mean is not that you can't understand it, but that you didn't invent it. You didn't figure it out your own way, from hearing the clue. What you should do is imagine you're a student again, and take this paper upstairs, read every line of it, and check the equations. Then you'll understand it very easily."

I took her advice, and checked through the whole thing, and found it to be very obvious and simple. I had been afraid to read it, thinking it was too difficult.

It reminded me of something I had done a long time ago with left and right unsymmetrical equations. Now it became kind of clear, when I looked at Lee's formulas, that the solution to it all was much simpler: Everything comes out coupled to the left. For the electron and the muon, my predictions were the same as Lee's, except I changed some signs around. I didn't realize it at the time, but Lee had taken only the simplest example of muon coupling, and hadn't proved that all muons would be full to the right, whereas according to my theory, all muons would have to be full automatically. Therefore, I had, in fact, a prediction on top of what he had. I had different signs, but I didn't realize that I also had this quantity right.

I predicted a few things that nobody had experiments for yet, but when it came to the neutron and proton, I couldn't make it fit well with what was then known about neutron and proton coupling: it was kind of messy.

The next day, when I went back to the meeting, a very kind man named Ken Case, who was going to give a paper on something, gave me five minutes of his allotted time to present my idea. I said I was convinced that everything was coupled to the left, and that the signs for the electron and muon are reversed, but I was struggling with the neutron. Later the experimenters asked me some questions about my predictions, and then I went to Brazil for the summer.

When I came back to the United States, I wanted to know what the situation was with beta decay. I went to Professor Wu's laboratory at Columbia, and she wasn't there, spinning to the left in the beta decay, came out on the right in some cases. Nothing fit anything. When I got back to Caltech, I asked some of the experimenters what the situation was with beta decay. I remember three guys, Hans Jensen, Aaldert Wapstra, and Felix Boehm, sitting me down on a little stool, and starting to tell me all these facts: experimental results from other parts of the country, and their own experimental results. Since I knew those guys, and how careful they were, I paid more attention to their results than to the others. Their results, alone, were not so inconsistent; it was all the others plus theirs.

Finally they get all this stuff into me, and they say, "The situation is so mixed up that even some of the things they've established for years are being questioned - such as the beta decay of the neutron is S and T. It's so messed up. Murray says it might even be V and A."

I jump up from the stool and say, "Then I understand EVVVVVERYTHING!"

They thought I was joking. But the thing that I had trouble with at the Rochester meeting - the neutron and proton disintegration: everything fit but that, and if it was V and A instead of S and T, that would fit too. Therefore I had the whole theory!

That night I calculated all kinds of things with this theory. The first thing I calculated was the rate of disintegration of the muon and the neutron. They should be connected together, if this theory was right, by a certain relationship, and it was right to 9 percent. That's pretty close, 9 percent. It should have been more perfect than that, but it was close enough.

I went on and checked some other things, which fit, and new things fit, new things fit, and I was very excited. It was the first time, and the only time, in my career that I knew a law of nature that nobody else knew. (Of course it wasn't true, but finding out later that at least Murray Gell-Mann - and also Sudarshan and Marshak - had worked out the same theory didn't spoil my fun.)

The other things I had done before were to take somebody else's theory and improve the method of calculating, or take an equation, such as the Schrödinger Equation, to explain a phenomenon, such as helium. We know the equation, and we know the phenomenon, but how does it work?

I thought about Dirac, who had his equation for a while - a new equation which told how an electron behaved - and I had this new equation for beta decay, which wasn't as vital as the Dirac Equation, but it was good. It's the only time I ever discovered a new law.

I called up my sister in New York to thank her for getting me to sit down and work through that paper by Lee and Yang at the Rochester Conference. After feeling uncomfortable and behind, now I was in; I had made a discovery, just from what she suggested. I was able to enter physics again, so to speak, and I wanted to thank her for that. I told her that everything fit, except for the 9 percent.

I was very excited, and kept on calculating, and things that fit kept on tumbling out: they fit automatically, without a strain. I had begun to forget about the 9 percent by now, because everything else was coming out right.

I worked very hard into the night, sitting at a small table in the kitchen next to a window. It was getting later and later - about 2:00 or 3:00 A.M. I'm working hard, getting all these calculations packed solid with things that fit, and I'm thinking, and concentrating, and it's dark, and it's quiet... when suddenly there's a TAC-TAC-TAC-TAC - loud, on the window. I look, and there's this white face, right at the window, only inches away, and I scream with shock and surprise!

It was a lady I knew who was angry at me because I had come back from vacation and didn't immediately call her up to tell her I was back. I let her in, and tried to explain that I was just now very busy, that I had just discovered something, and it was very important. I said, "Please go out and let me finish it."

She said, "No, I don't want to bother you. I'll just sit here in the living room."

I said, "Well, all right, but it's very difficult." She didn't exactly sit in the living room. The best way to say it is she sort of squatted in a corner, holding her hands together, not wanting to "bother" me. Of course her purpose was to bother the hell out of me! And she succeeded – I couldn't ignore her. I got very angry and upset, and I couldn't stand it. I had to do this calculating; I was making a big discovery and was terribly excited, and somehow, it was more important to me than this lady - at least at that moment. I don't remember how I finally got her out of there, but it was very difficult.

After working some more, it got to be very late at night, and I was hungry. I walked up the main street to a little restaurant five or ten blocks away, as I had often done before, late at night.

On early occasions I was often stopped by the police, because I would be walking along, thinking, and then I'd stop - sometimes an idea comes that's difficult enough that you can't keep walking; you have to make sure of something. So I'd stop, and sometimes I'd hold my hands out in the air, saying to myself, "The distance between these is that way, and then this would turn over this way..."

I'd be moving my hands, standing in the street, when the police would come: "What is your name? Where do you live? What are you doing?"

"Oh! I was thinking. I'm sorry; I live here, and go often to the restaurant..." After a bit they knew who it was, and they didn't stop me any more.

So I went to the restaurant, and while I'm eating I'm so excited that I tell a lady that I just made a discovery. She starts in: She's the wife of a fireman, or forester, or something. She's very lonely - all this stuff that I'm not interested in. So that happens.

The next morning when I got to work I went to Wapstra, Boehm, and Jensen, and told them, "I've got it all worked out. Everything fits."

Christy, who was there, too, said, "What beta-decay constant did you use?"

"The one from So-and-So's book."

"But that's been found out to be wrong. Recent measurements have shown it's off by 7 percent."

Then I remember the 9 percent. It was like a prediction for me: I went home and got this theory that says the neutron decay should be off by 9 percent, and they tell me the next morning that, as a matter of fact, it's 7 percent changed. But is it changed from 9 to 16, which is bad, or from 9 to 2, which is good?

Just then my sister calls from New York: "How about the 9 percent - what's happened?"

"I've just discovered that there's new data: 7 percent..."

"Which way?"

"I'm trying to find out. I'll call you back."

I was so excited that I couldn't think. It's like when you're rushing for an airplane, and you don't know whether you're late or not, and you just can't make it, when somebody says, "It's daylight saving time!" Yes, but which way? You can't think in the excitement.

So Christy went into one room, and I went into another room, each of us to be quiet, so we could think it through: This moves this way, and that moves that way - it wasn't very difficult, really; it's just exciting.

Christy came out, and I came out, and we both agreed: It's 2 percent, which is well within experimental error. After all, if they just changed the constant by 7 percent, the 2 percent could have been an error. I called my sister back: "Two percent." The theory was right.

(Actually, it was wrong: it was off, really, by 1 percent, for a reason we hadn't appreciated, which was only understood later by Nicola Cabibbo. So that 2 percent was not all experimental.)

Murray Gell-Mann compared and combined our ideas and wrote a paper on the theory. The theory was rather neat; it was relatively simple, and it fit a lot of stuff. But as I told you, there was an awful lot of chaotic data. And in some cases, we even went so far as to state that the experiments were in error.

A good example of this was an experiment by Valentine Telegdi, in which he measured the number of electrons that go out in each direction when a neutron disintegrates. Our theory had predicted that the number should be the same in all directions, whereas Telegdi found that 11 percent more came out in one direction than the others. Telegdi was an excellent experimenter, and very careful. And once, when he was giving a talk somewhere, he referred to our theory and said, "The trouble with theorists is, they never pay attention to the experiments!"

Telegdi also sent us a letter, which wasn't exactly scathing, but nevertheless showed he was convinced that our theory was wrong. At the end he wrote, "The F-G (Feynman-Gell-Mann) theory of beta decay is no F-G."

Murray says, "What should we do about this? You know, Telegdi's pretty good."

I say, "We just wait."

Two days later there's another letter from Telegdi. He's a complete convert. He found out from our theory that he had disregarded the possibility that the proton recoiling from the neutron is not the same in all directions. He had assumed it was the same. By putting in corrections that our theory predicted instead of the ones he had been using, the results straightened out and were in complete agreement.

I knew that Telegdi was excellent, and it would be hard to go upstream against him. But I was convinced by that time that something must be wrong with his experiment, and that he would find it - he's much better at finding it than we would be. That's why I said we shouldn't try to figure it out but just wait.

I went to Professor Bacher and told him about our success, and he said, "Yes, you come out and say that the neutron-proton coupling is V instead of T. Everybody used to think it was T. Where is the fundamental experiment that says it's T? Why don't you look at the early experiments and find out what was wrong with them?"

I went out and found the original article on the experiment that said the neutron-proton coupling is T, and I was shocked by something. I remembered reading that article once before (back in the days when I read every article in the Physical Review - it was small enough). And I remembered, when I saw this article again, looking at that curve and thinking, "That doesn't prove anything!"

You see, it depended on one or two points at the very edge of the range of the data, and there's a principle that a point on the edge of the range of the data - the last point - isn't very good, because if it was, they'd have another point further along. And I had realized that the whole idea that neutron-proton coupling is T was based on the last point, which wasn't very good, and therefore it's not proved. I remember noticing that!

And when I became interested in beta decay, directly, I read all these reports by the "beta-decay experts," which said it's T. I never looked at the original data; I only read those reports, like a dope. Had I been a good physicist, when I thought of the original idea back at the Rochester Conference I would have immediately looked up "how strong do we know it's T?" - that would have been the sensible thing to do. I would have recognized right away that I had already noticed it wasn't satisfactorily proved.

Since then I never pay any attention to anything by "experts." I calculate everything myself. When people said the quark theory was pretty good, I got two Ph.D.s, Finn Ravndal and Mark Kislinger, to go through the whole works with me, just so I could check that the thing was really giving results that fit fairly well, and that it was a significantly good theory. I'll never make that mistake again, reading the experts' opinions. Of course, you only live one life, and you make all your mistakes, and learn what not to do, and that's the end of you.