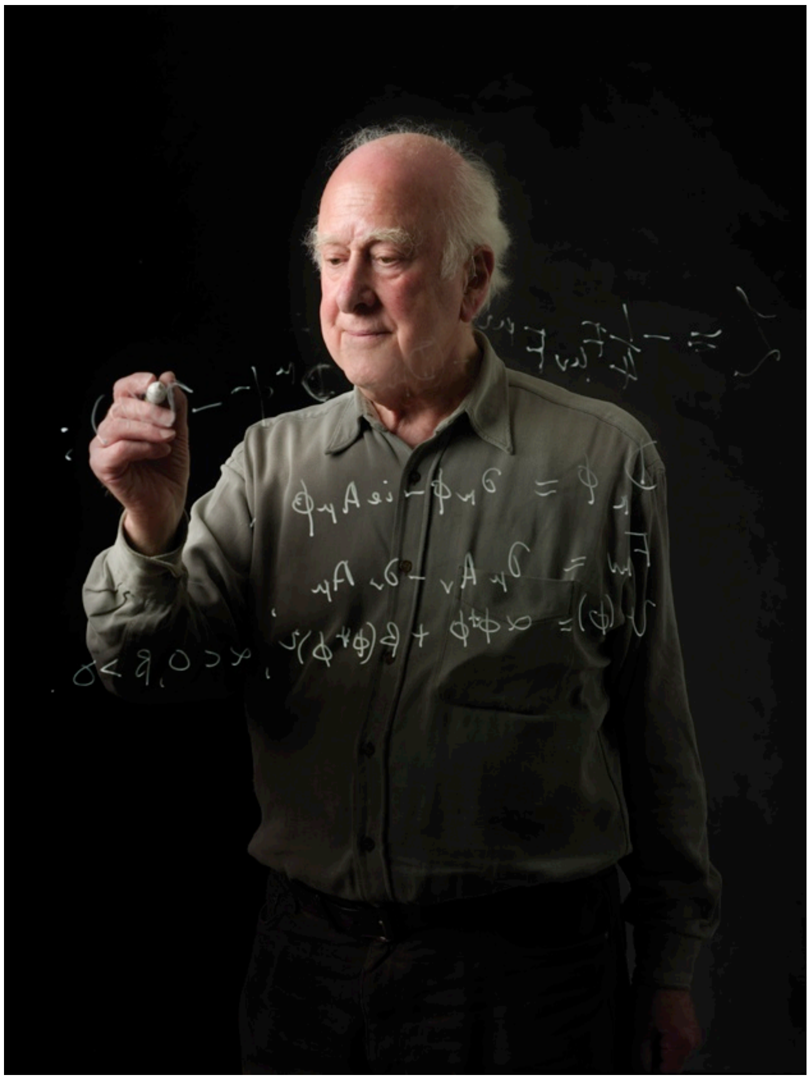
This is the account of a conversation I had with Peter Higgs on the 8th of December 2008 at Peter Osborne's home in Edinburgh.

Vittorio Del Duca



Peter Higgs during his visit at CERN, in April 2008. (© CERN, Geneva)

***Q**: Firstly, I would like to understand the physics context. When I studied gauge theories, spontaneous symmetry breaking, the work on the vacuum structure by Nambu, the BCS [Bardeen-Cooper-Schrieffer] superconductivity theory, the Ginzburg-Landau phenomelogical theory, and also Phil Anderson's hint that the BCS theory could have something to do with quantum field theory, I wondered how so many topics came together, which were at first sight unrelated. Now we have all these topics structured in textbooks, but how did you see it in the early 60's ? For you and for the others, who were studying these topics at the time, what was clear and what was shrouded in the mist ?*

A: Well, my involvement in this begins in 1960, which was the year that I was appointed to a lectureship in Edinburgh. I had previously been working in London and not really doing anything very worthwhile in terms of particle physics. Nambu's ideas were first published in a short paper in Physical Review Letters in 1960, but, in fact, the first thing I read was the longer paper with Jona-Lasinio.

This was a time when most people, certainly most of my colleagues in Edinburgh, were working on things based on dispersion relations, because a very widespread belief was that quantum field theory was not the right kind of theory to use, and it was certainly not working for hadronic physics, and some people believed that it was a sick kind of theory, particularly because Landau had shown years before that there was something wrong even in quantum electrodynamics when he looked at the structure of the propagator.

So, to work in quantum field theory at all was really a minority thing at the time. Most people in the European particle physics community were involved in dispersion relations, Regge poles, all those kind of things. In the United States there was a sort of divide between the East Coast and the West Coast. California was very heavily into new ideas such as the S-matrix theory of Geoffrey Chew. On the East side, however, some disciples of Julian Schwinger's still worked on field theory, and former students of Schwinger's included people like Glashow.

Just before I took up the lectureship in Edinburgh, I was told they were running a Summer School, the first Scottish Universities' Summer School of the series [on the subject of dispersion relations], and I should come along: I had been given a job to do on the committee, a job as a steward. The job, as it turned out, consisted of buying wine [laughter], because the grant which they had for travel had money to spare because one of the lecturers had obtained a National Science Foundation grant to cross the Atlantic. So there were a few hundred pounds that we could use to buy wine, and it was decided to have wine with dinner at the School. My job was to buy it, and to look after it.

This is where my friends Cabibbo, Glashow and Veltman come into the story, because the Summer School was outside Edinburgh, near Dalkeith, at Newbattle Abbey College. A kind of common room had been established in the crypt of the old abbey, dating from about 1200, on which the present house had been built, which was several hundreds years old. In the crypt, people had discussions and there was a group of four students who kept discussing physics all through the night. Their names were Cabibbo, Glashow, Veltman and Derek Robinson, an axiomatic field theorist. Well, I didn't get involved in those discussions, because I was looking after the wine and I had to get up in the morning [laughter].

This is the first part of the story, this is how I didn't come to learn about Glashow's paper on the unification of the electromagnetic and weak interactions as soon as I should have done, because he'd already written it, it just hadn't been published until another year later. The gang of four never turned up at the first lecture in the morning, they were still sleeping. When I met Cabibbo the next time, he confessed he still didn't know anything about dispersion relations [laughter], he also confessed that the discussions had been lubricated by some of my wine [laughter], which had been smuggled out of my very insecure store, which didn't have a lock. (I mean, nobody had thought to have wine to look after anyway). Anyway, it had been hidden in a grandfather clock in the crypt of the abbey for the discussions later in the night. So this was the sort of informal side of the Summer School, where these people educated one another about problems in electromagnetic and weak interactions in 1960.

Newbattle Abbey College, near Dalkeith, Edinburgh. The 1960 Scottish Summer School was held here, in which Higgs took part before taking the lectureship at the University of Edinburgh.
(© Newbattle Abbey College)

So, now I've started my lectureship at Edinburgh, and during that first year I read the paper by Nambu and Jona-Lasinio, and the idea that symmetries in particle physics, like isospin, which phenomenologically are broken symmetries, might be exact, which is what Nambu was suggesting, was something which I thought was a very attractive idea, so I decided to work in this area and see what I could contribute. Quite soon after Nambu's papers in 1961-62, it was pointed out that when you spontaneously break a symmetry in a relativistic theory you get massless particles, Goldstone bosons. Goldstone pointed it out first, and then Goldstone, Salam and Weinberg wrote a proof of the theorem in Physical Review in 1962. That was rather disappointing to somebody like me, because it seemed to imply that Nambu's programme was not viable: if you have massless spin-zero particles, they are easy to produce, easy to detect, and even if there is some reason why the experimentalists have not yet detected them, they would upset what we understand about the energy generation in stars, they'd be very easily produced. So that seemed to rule out Nambu's programme as a way of doing particle physics at that time.

***Q**: OK, if a global symmetry is spontaneously broken, then Goldstone's theorem implies that there are massless particles, which are the Goldstone bosons. An example of that is Nambu's spontaneous breaking of chiral symmetry, and thus the identification of the pions as Goldstone bosons. However, Nambu's work came first, so how did it go precisely ?*

A: The way that I understand it is that Nambu had learnt about BCS theory from Schrieffer, who in the late 50's moved to a postdoctoral position in Chicago. Then Nambu, and I think independently, and elsewhere, Anderson re-formulated BCS theory which was formulated in a rather, well to me, complicated many-body quantum mechanical language as a piece of quantum field theory. When it was re-formulated in quantum field theory language, it became obvious that there was spontaneous breaking of the symmetry involved.

In a normal metallic conductor you have a partially filled energy band, and the electrical conductivity, or resistivity, arises because an electron in the partially filled band can scatter off phonons, and it can scatter into states which are now close in energy because the band is only partially filled. The basic point of Nambu's work was that if you look at the fermion spectrum for a superconductor, where you have the Bose condensate of pairs, a gap opens up in that partially filled energy band, and there's now an obstacle to the scattering of electrons, because there's a gap to overcome, so the electrons are now no longer the carriers of the electrical current. The carrier of the electrical current is the charge condensate. Then if you look at this gap it's analogous to the gap between the Dirac sea and the available one-particle states of the Dirac equation for a massive particle: if there were no gap it would be analogous to a massless Dirac fermion. So Nambu's thinking, as far as I understand it, was ``Well isn't this a way that we could generate mass for a Dirac fermion in relativistic physics ?'' in other words, take something which apparently has zero mass when you look at the Lagrangian, but spontaneously breaks the chiral symmetry, and the gap opens up, and that's the mass of a fermion. That was really the content of the Nambu, Jona-Lasinio paper, except the symmetry was dressed up to be chiral $SU(2)\otimes SU(2)$, so not only do you generate fermion mass, but you generate the different fermion masses for protons and neutrons.

So, in the Nambu and Jona-Lasinio paper, the elementary field is fermionic (because in those days quarks had not been invented, they were thought to be protons and neutrons), and the pion appears as a bound state. OK, so the fact that you do have something which looks like pions is a good aspect of the theory, but the fact that they are massless is a bad aspect of the theory. So, in that paper, they succeeded in generating fermion masses, but failed to generate mass for the pions. I don't know whether Nambu saw this, at that time, as something which would be a theorem. Goldstone, on the other side of the Atlantic, about the same time, looked at models in which you had elementary scalar fields, which were, we now say, the analogue of Ginzburg-Landau theory, and it was perfectly clear to him that there had to be massless spin-zero particles, because when the vacuum state is degenerate you've got excitations around the trough of the potential energy, and those are the Goldstone bosons. In a $SU(2)\otimes SU(2)$ model they would be candidates for pions, but they had to be massless. So this seemed to be a defect of the Nambu programme, that apparently you could not get away with having just spontaneous symmetry breaking, you would have to put in something extra which would generate a pion mass, if you wanted the phenomenology to be right.

Yoichiro Nambu (left) receives the Dirac Medal in 1987
from H. Schopper of the International Center for Theoretical Physics *(ICTP) in Trieste.*
In the centre is Abdus Salam, who founded the ICTP. (© ICTP Photo Archives.)

***Q:** But still, it took quite a leap in thinking to go to a local theory, I mean, to a gauge theory. How did it cross your mind, to go from a global theory to a gauge theory ?*

A: Goldstone had the idea of the theorem in 1961, and Goldstone was someone who didn't publish in a hurry. But Salam and Weinberg, who was visiting Imperial College that year, got together with Goldstone and convinced him that they should write a paper together and make it into a proper theorem, which they did. And that seemed, at first sight, to be the end of the story: spontaneous symmetry breaking in a relativistic theory means Goldstone bosons. But there were some doubts about this theorem, particularly, I think, on the other side of the Atlantic, from Abraham Klein and Ben Lee, who were at Pennsylvania at the time. They looked at the proof of the theorem, and said ``Isn't it conceivable that in this commutator in the spectral function which is discussed, could be some extra terms of the kind you have in the non-relativistic models, like BCS and so on ?" I think their paper was published in March 1964, and in June '64 there was a reply from Walter Gilbert who wrote a rebuttal of the Klein-Lee paper saying ``No, these extra terms violate Lorentz invariance, the Goldstone theorem is correct.". It's only because these other systems are not relativistic in dynamics that these other terms can occur and you can escape the Goldstone theorem.

So this brings us to the Summer of '64, which is where I got involved, although I had been trying specific models which failed to evade the Goldstone theorem before.

Jeffrey Goldstone.

***Q**: What kind of models ?*

A: Well, it wasn't very systematic, it was just trying to look at specific things to understand the dynamics and the mathematics a little bit better, but it never got to a point where I would publish it. At this point I should bring Anderson into the story. Anderson published a paper in 1963 in Physical Review with the title ``Plasmons, gauge invariance and mass'' in which he observed that, as he put it, there was a zero-mass problem with spontaneous symmetry breaking, the Goldstone theorem, there was a zero-mass problem with Yang-Mills theory, and he speculated that these could cancel one another. So he was right, but nobody would really believe that paper, at least nobody who was in particle physics, because he hadn't given an example of it working relativistically, and he hadn't actually shown that there was any flaw in the theorem.

***Q**: He was a visionary, in a way ...*

A: Yes, he had the right sort of physical intuition. He described what happens in a superconductor. He said ``What happens in a superconductor is that there would be Goldstone excitations, but for the fact that there is a long-range electrostatic force, which, when you include it in a calculation, pushes up the energy of these excitations, and they become massive''. But that wasn't a convincing argument to a particle physicist, who believed the theorem which claimed that relativistic theory was different. He was still right, but he didn't convince anybody really, apart from himself. Why was he right ?

The crucial period, as far as I was concerned, was two weeks in July 1964. At the time, we didn't have an email subscription to Physical Review Letters, and it took a month to arrive. Gilbert's paper was, I think, published in the journal in the middle of June, and I saw it on July the 16th, before anybody else in Edinburgh because I was the member of staff who looked after the little departmental library, including the journals, and so ...

***Q**: You seem always to be in the right place, first in the wine cellar ... [laughter]*

A: So I think it was Thursday, July the 16th (1964). [laughter] I mean, I checked this once because one of my jobs as the guardian of the journals was to put a date on the cover as they came in, and to put them on the shelf for other people to read. And the cover of that issue of Physical Review Letters has been bound into the bound copy which went into the University Library, with my dating on it still. [laughter] So, that's the evidence.

Anyway, when I read this paper by Gilbert, it seemed really to tidy things up completely: you couldn't evade the Goldstone theorem, because you couldn't have these extra terms in the spectral function, of the kind which was in the Goldstone, Salam and Weinberg proof. And I got very upset about that. When people ask me how I reacted, I say I think I probably said ``Shit! That's all''. And my colleagues wondered what I was worried about. [laughter]

So, during the weekend, it gradually occurred to me that I knew an exception to this statement of Gilbert's. The reason that I knew an exception to it, was that I'd been reading papers by Julian Schwinger, published in 1962 in Physical Review, under the title ``Gauge invariance and mass''. What Schwinger had done was to examine what had become folklore in the particle physics community: that in quantum electrodynamics the photon is massless because gauge invariance prevents any mass generation. Schwinger had shown this isn't true, that you could perfectly well have a gauge field theory, like quantum electrodynamics, in which the spin-one particle is massive. He wrote down some spectral functions for, I think, a commutator of vector potentials in a sort of Maxwell-type theory, and, because he was Schwinger, he used the Coulomb gauge. He thought that the way that people, you know, mutilated the Lagrangian and used a Lorentz gauge condition wasn't the right way to do it. You should start from Coulomb gauge which has no gauge freedom left in it, and if you want to get to a Lorentz gauge, which looks formally relativistically invariant, you transform all the things in the theory to this new gauge. And this had been spelled out in the very first issue of the Journal of Mathematical Physics by Zumino in 1960, I think. And I had read that too.

The thought which came to me that weekend in July was that if Schwinger can, in Coulomb gauge, write down the spectral function for the commutator of two four-vector fields which don't look properly Lorentz covariant, then you can do it in other places too. But it's the gauge freedom which is crucial there, this is how it can happen. If you don't have the gauge freedom, which in quantum field theory is a kind of embarrassment, because you have to fix the gauge before you have well defined quantum operators ... I mean: in anything but a gauge theory, you'll end with standard manifestly covariant formalism. But a gauge theory doesn't have to look manifestly covariant. As long as the physics which comes out at the end is relativistically invariant, it's all right. So, that's when I wrote the first short paper, which suggests a way out of the Goldstone theorem, and the way out is to combine spontaneous symmetry breaking with a gauge theory. In other words you have to have a gauge field coupled to the charges associated with the symmetry which you want to break spontaneously, and that was the trick.

Julian Schwinger (right) in a conversation with Abdus Salam. (© AIP Emilio Segre Visual Archives)

So, that was how it happened that weekend. I mean, you could put it this way, I'd been taking a lot of interest in two aspects of symmetries which I'd previously not thought they were related and suddenly realised they should come together. And I'd learnt a great deal about gauge invariance whilst supervising the master's degree of my first graduate student, Jack Smith, who is now professor at Stony Brook. Between 1960 and '61, we were trying to understand more about gauge invariance in quantum electrodynamics, because I had a lot of puzzles about it. So I had a lot of background in how gauge invariance works in quantum electrodynamics. Also, in my years in London I'd got involved in seminars in the [Hermann] Bondi group at King's College on general relativity and that sort of thing, and I'd learned a lot about gauges and gauge fixing in classical gravity too. So, it was two things I knew something about which certainly seemed to help one another to solve this problem.

My first short paper, which said there's a way out of Goldstone theorem, arrived on the desk of the Editor of Physics Letters at CERN, I think nine days after I'd read the paper by Gilbert.

Q*: So that was July 25th or 26th ?*

A: Yes, it was July 25th (1964), the second Saturday after that Thursday (July 16th), when I'd first read Gilbert's paper. I wrote it very rapidly, and there were signs of carelessness in the writing. But by then I realised that what I had to do was to look and see what actually happens in the simplest possible example, and the simplest possible example would be Abelian gauge theory describing quantum electrodynamics of a spinless charged particle with a term that breaks the symmetry spontaneously, which is what I did in the second very short paper I wrote in the following week. So this was what became known as the Higgs model.

Years later I learned that this was really just writing a relativistic version of Ginzburg-Landau theory, which I'd never in fact read in the original form back in 1950. I think that in 1950, when that was published, the cold war was at its worst, and it wasn't easy to read Soviet journals anyway.

***Q**: This Abelian gauge theory was the simplest model you could think of. How was this model received by the particle physics community ?*

A: The second paper was rejected! [laughter]. Yes, it also went off to the Editor of Physics Letters at CERN, who for some reasons which I don't understand accepted the first paper. It was a puzzle to me that he accepted a paper which was a piece of mathematics simply saying there's a way out of the Goldstone theorem, and this is it, but then rejected a paper in which I gave an example showing it happening. And the example was much more interesting to me, from the point of view of physics, because it was showing what otherwise would be the photon acquiring mass, and picking up the third spin state from the Goldstone field, which was the modern mechanism for generating the masses of the spin-one particles.

I got a polite rejection saying ``If you develop this work and write a longer paper, you might consider sending it off to Il Nuovo Cimento''.

***Q**: Oh, he didn't even say you can re-submit it here after revision. He said ``Send it somewhere else'' [laughter]*

A: Maybe I should be careful what I say at this point, because of your nationality, but it was only about at that time that I'd discovered that Il Nuovo Cimento didn't use referees at all. [laughter] In fact, I had an embarrassing experience at a later date. I had a slightly cranky student, who wrote a paper which was not actually wrong, well it wasn't very obviously wrong, but it was an interpretation of something which was extremely unlikely, and I said I didn't believe it, but ``OK, you see if you can get it published''. And he did. And that was when I think I discovered that it hadn't been refereed. And I think it was Stanley Deser of Brandeis, who I met shortly afterwards, who said ``You ruin your reputation if you let students publish rubbish like that''. [laughter]

***Q**: Then what happened ? I mean, when the Editor said ``Send it off to Il Nuovo Cimento'' meaning then you could get by without any peer review. What did you do ?*

A: Well, I was very indignant. [laughter] What I concluded was that I'd been too much in a hurry, I'd written a paper which was even shorter than the first one, which is probably no more than a page of A4, and it only said what was to me necessary, but it hadn't provided any argument which convinced people that this was exciting or interesting.

At that time the Department was in town, not out at King's Buildings [current location of the Physics Department in Edinburgh]. It was in a street called Roxburgh Street, near the old Natural Philosophy Building, near the Old College. It so happened that I shared an office at that time with Euan Squires, who was a Regge theorist. While I was in the midst of writing these papers Euan went off to spend the Summer at CERN. When he came back from CERN, he said ``They didn't understand what you were talking about. They didn't think it had anything to do with physics''. [laughter] He told me that what would normally happen was that the Editor at CERN would think of the most likely person in the CERN Theory Group to referee a paper, and he just passed it around. But at that time there weren't people [at CERN] doing that kind of thing at all.

Peter Higgs (left) in a conversation with Yoichiro Nambu (right) at the ICTP in Trieste. (© ICTP Photo Archives.)

***Q**: OK, but you sent it back. You just added one page ...*

A: I expanded it, but I didn't send it back. I decided they didn't understand what I was talking about. I would send it across the Atlantic, and the expanded version was published in Physical Review Letters. And the referee, when he accepted it, asked me to comment on the work of Brout and Englert. The day on which my paper arrived in the Physical Review Letters Editorial Office was the day on which they published the paper by Englert and Brout, on the mass generation of spin one, which was essentially the same thing, except they just did it starting from Feynman diagrams, with no Lagrangian or anything like that.

***Q**: So, you didn't know about their work, and they didn't know about yours.*

A: No, we didn't exchange preprints with Brussels, because we thought that Brussels was a place where they studied theory of phase transitions, and things of that sort. I mean, Brout is the author of ``Phase transitions'' [Benjamin Press, 1965]. We had no idea that there was any interest in particle physics there. And in fact I think that paper was the first time Brout had published anything on particle physics.

Q*: OK, pretty good start!*

A: Yes! Well ... skipping forward several years, twenty years later in fact, I met Nambu for the first time, and he told me he'd been the referee who accepted my paper. [laughter] I think he was probably a little upset he hadn't done it himself, but apparently one of his children had been very ill, and that had set him back in terms of programming research, otherwise I think he should have found the same thing very quickly. But Englert and Brout had also been inspired by Nambu's work.

Q*: So, they had thought along similar lines. Where do the other people come into play ? What's their contribution ?*

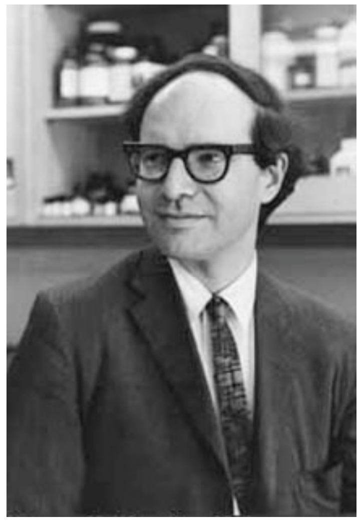
A: Guralnik, Hagen and Kibble. Well, they were working in this area too. And in fact, Guralnik came to Edinburgh to give a seminar at about that time. In their paper they did show how to evade the Goldstone theorem, but only in a dynamically rather trivial context. What they did was to take a model which consisted of a single scalar field coupled to a gauge vector field: it was just a quadratic Lagrangian, and showed that this was a theory of a massive spin-one particle. But dynamically this was just the same thing as what had been known many years ago to Stuckelberg, which is that if you write down the free field theory of spin one, which is the Proca theory, then you can take it apart into a theory which seems to have a gauge invariance, because you just decompose it. So it is equivalent to putting together spontaneous breaking with a rather trivial symmetry, which is a translation in the scalar-field variable, ϕ goes to ϕ + constant. What I found was something which had much more dynamics in it: it was a fully interacting field theory.

Q*: What happened then ? Except for the referee at Physics Letters, how was it received by the community as a whole ? I mean by people like Weinberg and Salam and so on, who had been working on related aspects.*

A: I don't know the complete answer to that. In October 1964 I was invited down to Imperial College [in London] to give a seminar. Salam wasn't present but I think Kibble was. The seminar was organised by Streater, an axiomatic [field] theorist, who was interested in proofs of the Goldstone theorem.

So, I didn't get a reaction from Salam, who was in Trieste, but I did get a very rapid reaction from Walter Gilbert, telling me I was wrong. [laughter] He raised some technical objections which I found difficult to answer immediately, because I'd done two things which didn't yet fit together properly. I mean the first paper had said essentially you can have these extra terms in the spectral function which spoil the proof of the Goldstone theorem, provided you have a gauge freedom which allows you to fix a gauge in an apparently non-covariant way. The other thing was to write down what was actually a classical field theory model. I hadn't done any quantising, except for saying that these waves correspond in quantum theory to a massive particle, just using the standard

Einstein-De Broglie correspondence. So I couldn't really answer the objections which Gilbert wrote down, until I'd done more work. I really had to work out the consequences of my model in quantum theory, and I didn't get around to do that until the following year, which is when I went to spend a sabbatical year in North Carolina.



Walter Gilbert.

At North Carolina in the Summer of 1965, the first thing I did, after I found somewhere to live for myself and family, was to work out the tree diagrams for that model and show that in fact, despite of the lack of relativistic invariance of the gauge I'd chosen, the non-covariant terms cancelled out between the diagrams, and left you with a perfectly relativistic theory in terms of the physics. The argument I used previously was simply to quote Zumino's papers, saying that if you want to check whether the theory is relativistic what you do is to check the commutator structure for the Poincare-group generators. Since Zumino himself had shown how that worked for a non-covariant gauge in quantum electrodynamics, I simply pointed out that his proof didn't depend on the symmetry not being spontaneously broken, it would work just as well if the symmetry was broken. So I was confident that it would come out by cancellation in the Feynman diagrams, which in the end I found that it did. Having worked out of the details of the model, I wrote up the long paper which was published in Physical Review in 1966.

The next stage in terms of reactions came in Spring 1966. The group at Chapel Hill in North Carolina was run by Bryce De Witt, and the interest there was mainly in relativity and gravitation. I think I'd been invited as a result of things I'd done in connection with the beginnings of quantum gravity, when I was in London in the late 50’s: De Witt was very disappointed to find out I was working on this other nonsense. [laughter] The circulation of the preprints there included Dyson, and that was an absolutely crucial step. I got a reaction from Dyson in the form of a very kind letter saying ``I've read your paper with interest, and it's helped me to understand a number of things which I'd not previously understood''. And that, written by Dyson, was quite a compliment. He invited me to talk at the Institute [for Advanced Studies] at Princeton, and that was on 15th March 1966.

I'd also been in contact previously with Stanley Deser at Brandeis through my gravity interests, and when he knew that I was going to be over on his side of the Atlantic, he said ``If you get some invitation to do a seminar tour, let me know and I'll fix up a talk for you in the Boston area''. And that was fixed up at Harvard for the day after the Princeton seminar, in fact! [laughter] So March the 15th and 16th in 1966 were two rather exhausting days in my life.

***Q**: It's like a pianist in a grand tour.*

A: Yes. The experience of Princeton was quite intimidating at first. I mean, I got on very well with Dyson, but the talk which I gave was scheduled second in a programme of talks in the afternoon, the first having been given by Dyson himself on the stability of the matter, a very high-powered mathematical presentation on the question of when does a quantum system have a proper ground state. [laughter] In the tea interval, I met Klaus Hepp, who was an axiomatic field theorist, as well as another former student from the 1960 Summer School. He told me that what I was going to talk about must be wrong [laughter] because the Goldstone theorem had just been proved in the language of C* algebras (which I hardly understood, I hardly understood what the word meant) by Kastler, Robinson (the fourth of the overnight party at the 1960 Summer School) [laughter], and Swieca. In spite of the warning that I was going to be talking nonsense, I survived the seminar, and I was told later that at least I'd convinced Arthur Wightman that what I had done was all right, that gauge theories were an exception to the axioms of this theorem.

***Q**: OK, but from the people like Veltman, or Weinberg, who were more involved with gauge theory, with particle theory, were there any reactions in those years ?*

A: Well, the next day it was Harvard. [laughter] The Harvard seminar was more of a conversation than a monologue. Weinberg, who was based at M.I.T., was not there. I think he was somewhere over in California at that time. Schwinger wasn't there either but he apologised afterwards, having been collared by a graduate student on his way. Halfway through, when I mentioned Gilbert's paper, the audience burst out laughing, and I discovered that this was because Gilbert had just walked in. [laughter]

At the end of the seminar, Shelly Glashow, who was there, said ``That's a nice model you've got, Peter''. But he didn't see that it had anything to do with him. [laughter] He didn't say that, but clearly he thought that it was just a curiosity. I think the problem was that this became so much of a dialogue between me and members of the audience, that I didn't get to the point to say what I'd done with this kind of model. It convinced people that I wasn't talking nonsense, but it didn't have much of a practical impact.

***Q**: When was your idea perceived as a groundbreaking idea within the particle physics community ? When did you become famous ?*

A: I became famous in 1972, after the conference at Fermilab. One of my colleagues, Ken Peach, came back from that conference, and said ``Peter, you're famous!''. [laughter]

But there were previous consequences of the work. I don't know whether Salam read the paper, but he certainly got to know what I'd done through the other members of the group at Imperial College, Tom Kibble for example. Weinberg got to know about it, because there was his paper in 1967, which was essentially the electroweak theory, which used it. I got the impression later, that having missed my seminar, he learnt something about it from Zumino, who had been at the Harvard seminar. That was the first time I met Zumino, and he certainly knew a lot about it and understood it.

There's also the question mark about how much Weinberg picked up, when he passed through Imperial College, I think, at the end of August 1967. Later I met Weinberg at Brookhaven very briefly when I was on my way to a conference in Rochester and I called in to visit some friends in Brookhaven. We met in someone's office at Brookhaven, and I got involved in a discussion with Weinberg and others about the mass spectrum of hadrons.

Weinberg and Glashow (sitting, from left).
(© Will Owens, courtesy AIP Emilio Segre Visual Archives, Physics Today Collection)

Now I should point out that up to the time when Weinberg published his paper, people like myself and Brout and Englert had stupidly tried to make it work for hadrons, and it didn't work. That was, I think, because broken hadron symmetries were well known. (Broken symmetries in the lepton sector had only been played with by people like Glashow and they weren't very well known). Weinberg had been playing with spectral function sum rules, and not managing to understand the hadron spectrum in this language. I said I tried to use models of the type that I formulated for hadrons, and still it didn't work either. Basically, the problem was that we wanted the pion to get mass, but what happened if you introduced a kind of gauge field model was that you generated the vector meson with mass, but you lost the pion completely. So it really wasn't a context in which spontaneous symmetry breaking was going to be successful. And it was shortly afterwards that Weinberg saw the light! [laughter] He realised that he'd been working on the wrong system. In his Nobel lecture, he said that he'd been applying the right ideas to the wrong system, and that leptons were the things to go for.

***Q**: What happened then between 1967, when Weinberg wrote his paper, and 1972, when you became famous ?*

A: Well, nothing very much, because if you look at how Weinberg followed up his paper, the answer is he didn't. He didn't know how he was going to deal with renormalisation. Neither did I. Years later I was reproached by David Wallace [former student of Higgs] ``Why didn't you put me on that kind of theory ?'' I said, quite honestly, I didn't dare to put a graduate student on that. I hadn't got sufficient grasp of the programme myself to carry it through: it was Veltman's programme which brought this, and then of course at the last minute the bit that Veltman really wanted to do was already solved by 't Hooft. From the time that 't Hooft published his paper gauge theories had a new-found popularity.

***Q**: That was 1971.*

A: Yes, I heard about it from John C. Taylor, who was at the Amsterdam conference. He wrote me a letter saying ``I think you should be interested in the work of 't Hooft. It seems to connect with some ideas of yours.'' And then at the Fermilab conference in 1972, Ben Lee was the rapporteur on weak interaction theory, and essentially what happened was that he attached my name to everything involving spontaneous symmetry breaking. And people like Englert and Brout only got mentioned in a footnote saying that other people had worked in this area too. [laughter]

***Q**: So, they must haven't been very happy.*

A: No, in 1981 I visited in Aachen to give a lecture, and I met Lalit Sehgal. He'd just given a seminar in Brussels, and he told me he suddenly became embarrassed, because he'd been referring to the Higgs mechanism and he saw someone sitting in the front row looking displeased. So he tried to recover from this by saying ``Of course I realise that this mechanism was discovered by a number of people, but in accordance with custom I attach to it the shortest name of those involved.'' And a voice from the front row said ``My name has five letters too.'' [laughter]

***Q**: That was 1981. And then it became a long wait. Nobody knew what mass the Higgs boson would have. It wasn't clear when and where it would be discovered.*

A: Yes, I think for me the crucial paper in getting experimentalists to take an interest was a 1976 paper by Ellis, Gaillard and Nanopoulos ``A phenomenological profile of the

Higgs boson''. They said ``We can't really tell you anything definite about it, but don't miss it. Look out for it.''

Martinus Veltman (third from left) at a conference in honour of his 60th birthday, at Ann Arbor, Michigan, on 16 May 1991, with Nicola Cabibbo (left). Cabibbo and Veltman were two of the participants at the 1960 Scottish Summer School. (© AIP Emilio Segre Visual Archives)

Q*: Yes, that was, I guess, Higgs-boson production from gluon-gluon fusion.*

A: Anyway, that was written in 1976 when the SPS [Super Proton Synchroton: proton antiproton collider. It started operation at CERN in 1976] was being built. So the experimentalists were alerted to something they ought to scan the data for.

Q*: You have been at CERN last Spring, haven't you ?*

A: Yes, at the beginning of April [2008].

Q*: What's your reaction to the building of a machine that costs several billions of whatever currency you want to use, euros, dollars, or pounds, to look for something that popped out of your head ?*

A: Well, I'm happy with this rather tentative start of Ellis and others, but the Higgs boson was seized on as the selling point of the latest machines [LEP, LHC]. I think it was not very good tactics perhaps, because of what happened with LEP [Large Electron Positron collider. It started operation at CERN in 1989]. LEP didn't quite get there, and an article appeared in the Times, by somebody who should have known better, saying that's so many billions of pounds down the drain (into a Black Hole). [laughter]

I think it was unwise to over-emphasize that particular thing, when in fact as physicists they knew that there was so much more that the machine was going to do. I'd apply the same criticism to the propaganda associated with the LHC. If they don't find it, who's going to provide the money for anything else ?

***Q**: If I can get a bit more personal, this circus which started around your name, did it affect your personal life at all ? Has it had an impact on you as a person ?*

A: Well, I suppose yes. I think there were mainly two aspects of this, that I'd say something about. On a scientific side, it gave me such an exaggerated reputation, that it almost stifled my subsequent research. There was a gap, I might say something about in a moment, during which I wasn't doing very much anyway. When I became more active again, I got interested in supersymmetry, but by that time I was really too old to do anything new, in the sense that there was a lot more background of mathematics involved in supersymmetry and I simply couldn't absorb it as quickly as the people who were just getting their PhD's and writing all the papers. So in the end I gave up, in the early 80's.

***Q**: You didn't have enough piece of mind to concentrate.*

A: No, it was as if I had become too ambitious. If I'd settled for more straightforward things to work on, I might have produced more, but having had one success, I had to go for the latest promising development, which was supersymmetry, supergravity, and so on, and I was no longer capable of learning that fast.

The other thing that I wanted to say, was that it probably did affect me in other ways: I think it certainly contributed to the break-up of my marriage, because I think my wife did not at the time quite appreciate what I succeeded in doing and how important it was to me. When I gave more priority to things which involved my career, like conferences and so on, rather than the interests of the family, the marriage went downhill, with the result that there was a period in which I didn't do very much at all, about the time of the 't Hooft work.

***Q**: So, you must have had mixed feelings about your fame: it was a bless and a curse.*

A: Yes, it obviously had a tremendous impact on my self-confidence: I was now a theoretical physicist. Maybe too much impact ... [laughter] Previously, I thought of myself as an outsider, because my thesis work wasn't on particle physics at all.

***Q**: I'd like to get to that ... but first let me ask you about Lederman's practical joke, [laughter] that when he wrote a book on the Higgs boson he wanted to call it ``The goddam particle'' ...*

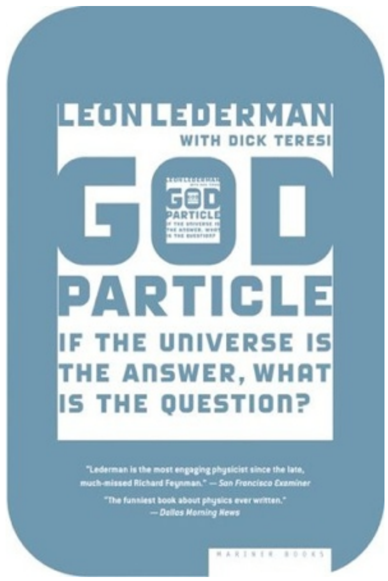
A: ... and the Editor wouldn't let him ...

***Q**: ... for obvious reasons, right ? [laughter] So he called it ``The God particle''. Of course, given that it was just a joke, we shouldn't attach too much importance to it, right ? But does it bother you in a way ?*

A: Yes, I'm not a theist myself, but I thought that the title might offend some people, unnecessarily. I once spent a night in a little Bed & Breakfast in the far North of Scotland, before getting on a ferry boat to the Orkney Islands, and they had quite a collection of books in that place, and what did I see there but ``The God particle''! [laughter]

***Q**: There you go ...*

A: No escape.



The cover of the last edition of God Particle. If the Universe is the answer, what is the question? *by Leon Lederman with Dick Teresi. The book was first published in 1989.* (© Mariner Books)

***Q**: But that's only because it was called ``The God particle'' that you found it there. Were it called `The goddam particle'' you would have never found it on those shelves.*

A: I don't know. The people who ran [the Bed & Breakfast] were an interesting couple. Most of their books were to do with travel and climbing mountains. They had both been in the Army, and had been involved in expeditions, in training in all sorts of places, involving the highest mountain in this or that place, and that was their main background. But they clearly had rather wide interests, and that book was just part of it.

***Q**: I'd like to ask a little bit about your schooldays, because it might inspire high-school kids, people who might do physics in the future. Were you at high school in the 50's, in the late 40's ?*

A: In the 40's, yes. Most of my high-school time was in Bristol, during the war. I was at a school which had been originally called the Merchant Venturer's Technical College but had become Cotham Grammar School, a normal state high school. That was perhaps when I began to get interested in being a theoretical physicist, and eventually a particle physicist. One of the first memories I have was of standing at the back of the school assembly hall, and looking at the array of names on the board at the back of the platform. It listed former students who gained some sort of distinction and there was one name there which kept occurring many times, and that was Paul Dirac. [laughter] Obviously, one effect was to make me wonder what he'd done, and of course it was a number of years before I found out.

***Q**: Did you actually meet any particle physics during your high-school years ?*

A: I was 16 at the time when the bombs were dropped on Hiroshima and Nagasaki in 1945, and the professors of Physics at Bristol University decided that it was their duty to inform the public about the physics background to these bombs, and they arranged a series of public lectures. The two professors were Cecil Powell, who discovered the pion in 1947 [Nobel Prize in 1950], and Neville Mott [Nobel Prize in 1977]. So I went to the lectures. As a consequence of the success of the lectures (there was a good audience for them), Powell decided that he should give another set of lectures on his own research, to see whether there was any public interest in those. So I learnt what was the state of particle physics on the experimental side by going to Powell's lectures, just a couple of years before the pion was discovered. So, that gave me a little bit of background.

***Q**: Then, when you went to College ...*

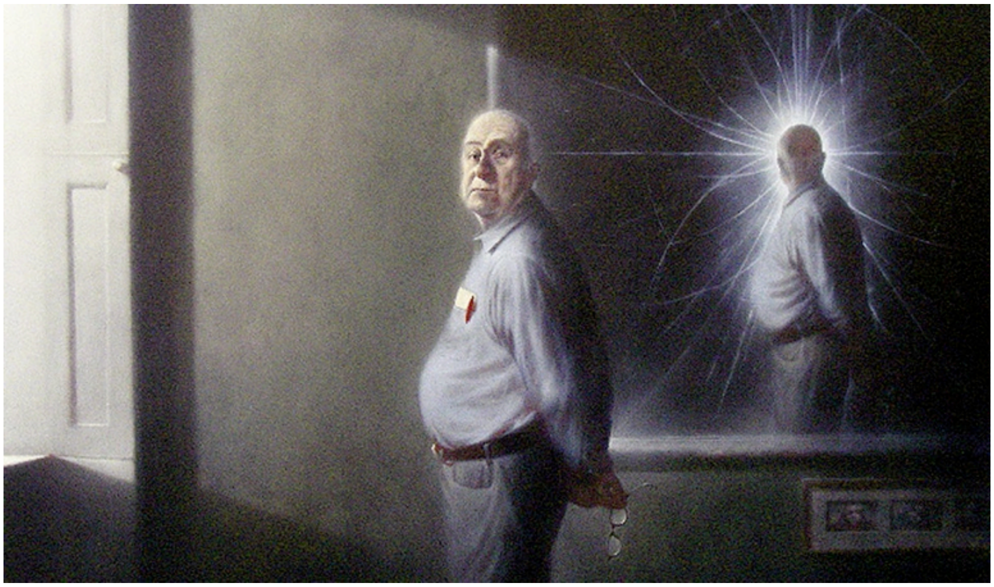
A: King's College, in London

***Q**: ... did you do Physics as a major ?*

A: Yes, and I happened to be in the first cohort of students who had the possibility of a theoretical physics option in the final year. So I chose that. Wisely, because I was hopeless as an experimentalist. [laughter]

***Q**: Many of us have gone down that path ... What brought you to a PhD in physics ?*

A: By the time I was at King's College as a student, I was already interested in research in a Natural [scientific] field if I got a good enough degree. In my final year, which was 1949-50, I was among the students who were expected to get a good degree, and we were invited to chat with a Head of Department about possible research. The Theoretical Physics professor was Charles Coulson, whose work was really in theoretical chemistry. When he asked me whether I was interested in research I said ``Yes, what I really want to do is to work in particle physics''. And the only person I knew that I might have done work with was Dirac, of course, at Cambridge. He told me two things. One was that nobody managed to work with Dirac as a graduate student, and the person at Cambridge who supervised all the students was [Nicholas] Kemmer. The other thing that he said was ``The theory of particle physics is in a terrible mess, and it's very risky for a PhD. You may end up doing nothing worthwhile.'' So I compromised and did my PhD essentially in theoretical chemistry, although, because my degree was physics, it was called molecular physics. Then in 1953, when I was getting close to write the thesis, I was proposed for a rather prestigious studentship [which would also fund post-doctoral research]. The full title, I think, is ``Royal Commission for the Exhibition of 1851 Senior Studentship'' [laughter]



Peter Higgs in a 2008 painting by Ken Currie,
at the School of Informatics, University of Edinburgh. (© Ken Currie, 2008)

***Q**: Does the studentship still exist ?*

A: It does, I think. The Royal Commission for the Exhibition of 1851 is a body which was set up after the great exhibition of 1851 [in Hyde Park, London]. The exhibition was a great success, and I think it made quite a lot of money, some of which went into building museums in South Kensington, and some into funding a rather prestigious studentship. Again, Dirac had been one of the earlier holders of it. Curiously enough, I was not proposed from the theoretical side. It was John Randall, the experimental professor, who put my name forward. The experimental side at Kings was developing the beginnings of molecular biology: there was Maurice Wilkins [Nobel Prize for Medicine in 1962, with Watson and Crick, for the determination of the structure of DNA] and Rosalind Franklin studying DNA, and that kind of thing. I think what helped Randall taking an interest and proposing me for this studentship was the fact I'd written a paper or two on the dynamics of helical molecules. [laughter]

Years later, I apologised to Randall because I said I thought he must have been disappointed in what I in fact did, which was to hold [the studentship] in my final year of PhD in London and then ask the committee which awarded it ``Would it be OK if I transferred it to Edinburgh ?'' That enabled me to make the transition to Edinburgh, where meanwhile Kemmer had moved to from Cambridge. Kemmer was a lecturer in Cambridge from about the end of the war to 1953, and then he was the successor of Max Born in the Tait Chair of mathematical physics at Edinburgh. A year later, it was very convenient that I was able to move to Edinburgh, which was a place I decided I'd like to live, and change my field of research.

Q*: I thought you would apologise for not having discovered the helical structure of DNA, but you could say ``Well, after all, I found the Higgs mechanism".*

A: Well, I think the interest in the helical molecule arose from the fact that there was the helical structure for proteins proposed by [Linus] Pauling, and there were certainly two of us, who wrote papers on various aspects oh helical symmetry in relation to molecules.

Walter Gilbert, whose paper about the Goldstone theorem I managed to evade, later got the Nobel Prize for his work in molecular biology. By the time I met him in the Boston area, he'd moved from theoretical physics, where he'd work alongside with Jeffrey Goldstone in Cambridge, and he moved over to molecular biology. He got the Nobel Prize for Chemistry in 1982, I think. And it was, actually, for an experimental technique - a long way from where he'd been previously as a theorist. It's a technique in which you discriminate between molecules of various lengths by letting them diffuse through blotting paper, or something of that sort. And that was apparently very important in analysing various things of interest in molecular biology.

Peter Higgs during his conversation with Vittorio Del Duca at Peter Osborne's home in Edinburgh, on 8 December 2008. (© Vittorio Del Duca)

Q*: So, in the mid 50's you got to Edinburgh. Were you alone there in doing particle physics theory at that time ?*

A: This was the end of 1954. (My PhD was written in the Summer of 1954). Well, it was a small group. In the first few years, I was really still learning, because I'd attended some lectures on quantum field theory in London by someone who turned out to be using Dyson's 1951 lecture notes from Cornell. [laughter] Kemmer was a person who was always claiming he didn't understand what was going on in the subject any more, despite the fact he had successfully supervised a very large number of students at Cambridge, many of whom had gone on to be professors in the field. When I arrived, I went up and met him, and he agreed to have me join his group. He said ``I don't understand what's going on any more: dispersion relations, S-matrix theory stuff. But I can tell you what you ought to read about'' and he gave me a vast reading list, and let me get on with it and find my own way after that.