

My Life as a Boson

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The plan of this talk is that I will introduce the ideas of spontaneous symmetry breaking and discuss how these developed from condensed matter through the work of Yoichiro Nambu and Jeffrey Goldstone to the work of Robert Brout and Francois Englert and myself in 1964. That will be the main part, and other topics such as the application of these ideas to electroweak theory are much better known to this audience, so I shall skim through them.

My own story, in relation to spontaneous symmetry breaking, begins when I was appointed to a lectureship at Edinburgh University in October 1960. I was told when I accepted the job that I should also attend the first Scottish University Summer School in Physics, with the job of Steward, because there was some money to buy wine which was to be served with dinner. My task was to buy the wine and look after it, which I achieved rather badly, I think, thanks to the assistance of a gang of four students whose names became very well known. This gang of four comprised Nicola Cabbibo (who unfortunately passed away recently), Sheldon Glashow, Derek Robinson (who was a axiomatic field theorist at Oxford) and last but not least Tini Veltman. The gang of four stayed up half the night discussing theoretical physics, and mostly did not get up in time for the first lecture. It was not until Cabbibo told me about it some nineteen years later that I discovered they had been lubricating their discussions with some left-over wine from my cache. Well, not from my cache but from my store, they hid it in the grandfather clock in the crypt at Newbattle Abbey College.

So let me now come to the early history of spontaneous symmetry breaking. It is best known as a phenomenon in condensed-matter physics. The earliest example is perhaps the theory of ferromagnetism as formulated by Werner Heisenberg in 1928 [1]. The feature of spontaneous symmetry breaking is that you have some continuous symmetry that is broken by the ground state of the condensed-matter system. If it is of infinite volume, which of course a ferromagnet never is, then the ground state is degenerate. But if you were in an infinite ferromagnet then there would be a spontaneous magnetization, which could point in any direction, and you would think that the system was no longer rotationally invariant.

The examples that are most relevant to particle physics are superfluidity and superconductivity. Superfluidity results from the formation of a Bose condensate, which was described theoretically by Nikolay Bogoliubov in 1947 [2]. The symmetry that is broken there is not a space-time symmetry, but it is a symmetry associated with the phase transformations, namely the U(1) symmetry of the boson wave function. If you multiply the wave function by $e^{i\alpha}$, that is normally a symmetry. However, in a superfluid, when the condensate forms that symmetry is spontaneously broken.

Superconductivity is a little more complicated. It depends on the formation of an electrically-charged Bose condensate, and the first ideas on this were published by Vitaly Ginzburg and Lev Landau in 1950 [3], who said it could be explained if you have a Bose condensate of charged spinless particles. No such particles were known in metals, so it seemed a rather strange idea, but then seven years later John Bardeen, Leon Cooper and Robert Schrieffer (BCS) put the theory in its current form [4]. Cooper had shown that it was possible for electrons to pair through the electron-phonon interaction [5], and then you had effectively a boson. The pairs could then exhibit Bose condensation, and you had a Bose condensate of particles with charge 2.

One way these ideas fed into particle physics was via ferromagnetism, which I think influenced Heisenberg in the years leading up to 1957 when he formulated a rather short-lived non-linear spinor theory [6]. But the main route as far as we are concerned is from BCS into particle physics through a seminar that Schrieffer gave at the University of Chicago, in which Nambu learnt about BCS theory. This led to the work of Nambu and his collaborators in the following years. So, let us look at what Nambu did, because Nambu's is really the crucial breakthrough.

Nambu published in 1960 a Letter [8], and with Giovanni Jona-Lasinio the following year he published a full paper with the title 'Dynamical Model of Elementary Particles based on an Analogy with Superconductivity' [9]. The symmetry was a chiral symmetry acting on massless fermion fields, namely $SU(2) \times SU(2)$, that was spontaneously broken by the vacuum state. This generated mass for the fermions, not only mass but also mass differences. Nambu's fermions were proton-neutron pairs, because this was before the days of quarks. Now, what was going on here is illustrated in Fig. 1. In a normal metal there is a conduction band that is half-filled with electrons. These electrons can be scattered by phonons into neighbouring states in the empty part of the band, and it is the scattering by phonons that gives rise to normal metallic resistance. However, in a BCS superconductor the formation of the condensates splits the conduction band apart into two halves. The lower-lying electrons are confined in the lower half, where they have no neighbouring energy states to be scattered into, and so are lost as conductors of electricity. On the other hand the condensate itself is a very good conductor, in fact essentially a perfect conductor.

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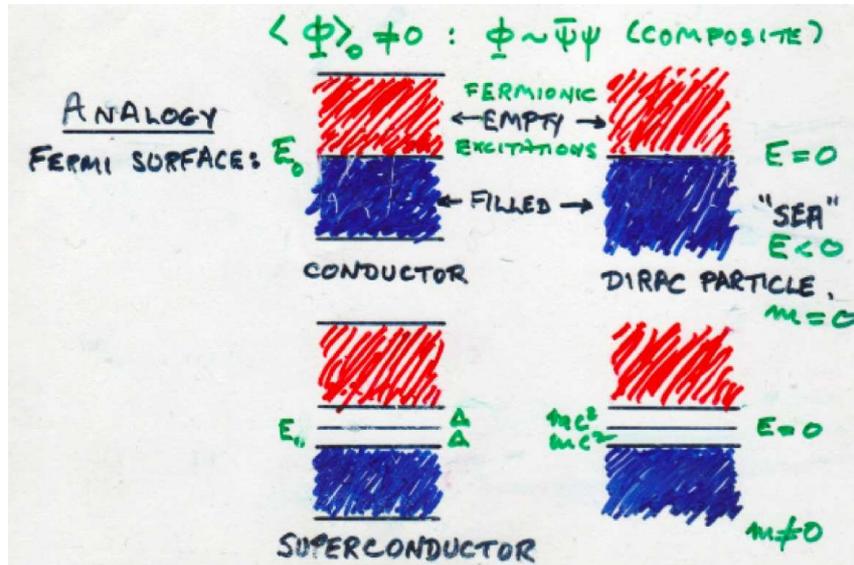


Figure 1: *Illustration of Nambu's model of elementary particles based on the BCS theory of superconductivity.*

The analogy that I think Nambu noticed was with the Dirac theory of a spin-half fermion. If you have a massless Dirac particle there are excited states of positive energy and there's the sea, and there is no gap between them, but if you have a massive particle then those are split apart. There is an energy difference between the sea and the states of the fermions, so there is a BCS-like gap of $2mc^2$. This is basically the way in which Nambu's BCS model involving spontaneous symmetry breaking generates fermion masses. It was Nambu who did that first of all.

In 1960 Jeffrey Goldstone, who had read the Nambu paper in Physical Review Letters [8], published a paper on field theories with superconductor solutions, in which he introduced elementary scalar fields [10]. Conceptually, this was a lot more transparent than BCS-type theories. He introduced the so-called wine bottle or Mexican hat potential, and it is then very easy to see how spontaneous symmetry breaking occurs, because the lowest-energy state of the system is down in the trough of the wine bottle, not at the symmetry point. This Goldstone-type model is similar to the Ginzburg-Landau theory in the history of superconductivity.

Brout recalls that in 1960 at a Cornell seminar Victor Weisskopf remarked that 'Particle physicists are so desperate these days that they have to borrow from the new things coming up in many-body theory like BCS. Perhaps something will come of it.' So there

was a lot of scepticism about whether this weird kind of field theory was going to work.

Well, for a time it did not work because of something that became known as the Goldstone theorem. Nambu, Jona-Lasinio and their successors found that they always had some massless scalar particles in the theory. In broken chiral $SU(2) \times SU(2)$ they had a massless triplet which were formed as bound states. These states looked like pions, since the elementary particles were the proton and neutron, and that was fine, except there was no mass for them and no way of generating mass without spoiling the model. Goldstone's models made this phenomenon intuitively obvious, because the Goldstone bosons are the excitations around the trough of the wine-bottle potential.

The theorem was formally proved by Goldstone, Abdus Salam and Steven Weinberg in 1962 [11], and says, roughly speaking, that if there is a continuous symmetry with the Lagrangian invariant then either the vacuum state is also invariant, or there must exist spinless particles of zero mass. Now that really spoils the programme, because such particles would easily have been discovered experimentally and, besides, they would upset the energy balance in stars, since stars would be able to radiate easily Goldstone bosons rather than photons.

The question arose in the following couple of years: can one evade the Goldstone theorem? A crucial paper that did not receive the attention it probably deserved was by Philip Anderson [7], the condensed-matter theorist. He pointed out that in a superconductor the Goldstone mode becomes a massive plasmon mode, due to its electromagnetic interaction. This mode is just the longitudinal partner of the transversely-polarized electromagnetic modes, which are also massive, and this is associated with the Meissner-Ochsenfeld effect.

Anderson continued with this suggestion, which in the context of the paper I would describe as speculation: 'The Goldstone zero-mass difficulty is not a serious one, because we can probably cancel it off against an equal Yang-Mills zero-mass problem.' But why is that a speculation? He never discussed the theorem, he did not say what was wrong with it, and he did not discuss explicitly any relativistic model.

So now we come to 1964. How to evade the Goldstone theorem? Technically, the proof by Goldstone, Salam and Weinberg [11] works as follows. It is based on the spectral representation of the commutator of a generator of a symmetry with a field component in a model where you have a number of scalar fields forming a multiplet under the group. The result of the differential form of a conservation law and Lorentz invariance is that you are driven to the conclusion that there is a zero-mass pole in the spectrum of particles.

However, in March 1964 in Physical Review Letters, Abraham Klein and Ben Lee pointed out that in a superconductor the spectral representation has a more general form than that forced on you by relativistic invariance [12]. This is because a superconductor or any condensed-matter system has a preferred frame of reference, and there is a preferred

unit time-like vector that specifies the rest-frame of the ionic background. This allows you extra terms that enable you to avoid the Goldstone theorem. Klein and Lee speculated that perhaps this could actually happen in a truly relativistic case.

Three months later, Walter Gilbert, who was in transition from being a particle theorist to being a molecular biologist, wrote a response [13] to this suggestion saying ‘No, you cannot do that in a relativistic theory. You cannot have a preferred unit time-like vector like that.’ This is where I came in, because the next month was when I responded to Gilbert’s paper by saying ‘Yes, you can have such a thing’, but only in a gauge theory with a gauge field coupled to the current [15]. So here we come to the crucial stage in 1964, and I have written a timetable of it as it happened in my experience.

The mid-June issue of Physical Review Letters arrived in Edinburgh on Thursday, the 16th of July, 1964. One of my jobs at the time was to receive from the central library current periodicals, look through them quickly, put a date on them, and put them on the shelves. So I opened up that Physical Review Letters and saw Gilbert’s paper [13]. I think my reaction was to say ‘shit’ because he seemed to have closed the door on the Nambu programme. However, over the weekend it gradually dawned on me that I already knew a quantum field theory with the property that Gilbert said was impossible, and that quantum field theory was quantum electrodynamics in the form favoured by Julian Schwinger.

I had read papers by Julian Schwinger in 1962 on the subject of gauge invariance and mass [14]. Schwinger was a devotee of the Coulomb gauge in electrodynamics, which is not relativistically invariant. It is tied to a particular frame of reference, and my Coulomb gauge is not the same as the Coulomb gauge of someone travelling at some speed relative to me. So over the weekend, that side of my memory infiltrated the problems that I was thinking about on the other side of my memory. I saw that what was likely was that you had to have a gauge theory, because in a gauge theory if you use a Coulomb gauge a Lorentz transformation is not just linear on the fields, there is an accompanying gauge transformation that takes you from one person’s Coulomb gauge to another person’s Coulomb gauge.

By Friday the 24th of July I had sent off a short note [15] to Physics Letters at CERN, where the editor was, and it was accepted. By the time I had written that, I knew what I had to do. I had to take the simplest model of spontaneous symmetry breaking involving scalar fields with the simplest $U(1)$ symmetry, and couple it to a Maxwell field. In other words, Goldstone meets Maxwell.

By the end of the next week, Friday the 31st of July, the second short Letter [16] was sent off to Physics Letters at CERN. It was rejected, and I was rather shocked. I did not see why they would accept a paper that said this is a possible way to evade the Goldstone theorem, and then reject a paper that showed how you actually do it. So, I thought (and

this was verified by a colleague of mine, Euan Squires, who came back from time at CERN shortly afterwards) it is no good revising this and sending it to Physics Letters at CERN, the people at CERN do not understand this sort of thing.

So during August I revised the paper by adding paragraphs to the rather stark presentation of what is now known as the Higgs model, which had been in the first version, and this was essentially to try to make the point that this sort of theory had experimental consequences. The reference to the so-called Higgs boson would never have appeared in print if Physics Letters had accepted the original version. One of the additions was the sentence ‘it is worth noting that an essential feature of this type of theory is the prediction of incomplete multiplets of scalar and vector bosons.’

My revised paper was received by Physical Review Letters at the end of August, and accepted [16]. The referee who, I discovered later, was Nambu, drew my attention to a paper by Englert and Brout that they had just published in Physical Review Letters [17], which had been received on the 22nd of June, before I even started my piece of work, and published on the 31st of August. I had never thought of Brout as being a particle theorist until then, because I associated him with phase transitions. That resulted in my adding a footnote to the paper to the effect that essentially the same thing had already been done by Englert and Brout. The main difference between us was that Englert and Brout started from Feynman diagrams and had trouble checking the gauge invariance of their theory, because gauge invariance gets distributed over Feynman diagrams, whereas I started from traditional classical Lagrangian field theory and, as long as I was careful in turning it into a quantum theory, the gauge invariance should be OK. I believe that Brout and Englert never mentioned the so-called Higgs boson because they thought it was quite obvious that if you formulated this kind of theory there would be a scalar mode that was massive. This corresponds to the field varying up and down the sides of the trough, rather than around the trough.

So that is the basic story, and I will simply skim through the next few years to remind you of subsequent history. I did not finish writing all this up properly, in other words a full Physical Review paper [18], until September 1965, while I was at the University of North Carolina at Chapel Hill. That paper was distributed as a preprint through their mailing list, and one of the people who got a copy was Freeman Dyson. In January 1966 I got a very nice letter from Dyson, saying my paper had helped him to understand some things that he had always been very puzzled about, and would I come and give a talk about it at Princeton, which I did on March 15th. I had also, before going across the States for my sabbatical year, talked to Stanley Deser about my plans, and he had said that, if I had any plans to travel up the East Coast giving talks, to let him know and he would arrange for me to talk at Harvard too. So I talked at Harvard the next day, March 16th.

What were the reactions there? Before the Princeton seminar I was told by Klaus Hepp, an axiomatic field theorist who had been at the first Scottish summer school, that what I was going to say must be wrong, because Goldstone's theorem had been proved rigorously using C^* algebras. I did not understand C^* algebras, but I did not think it was possible that they could prove that without hiding something in their axioms. At Harvard the atmosphere was very different, it was more of a dialogue with the audience than a seminar. I was told afterwards when I met Sidney Coleman again they had been looking forward to tearing apart this idiot who thought he could get round the Goldstone theorem, and they were going to have fun. But I had fun too.

The only thing that did not really go right was that I did not make any impact in terms of the possible applications. All of us, Brout, Englert and myself, had been going in the wrong direction, looking at hadron symmetries. After the Harvard seminar Shelley Glashow came up and said 'that is a nice model, Peter', but he did not see that it had anything to do with his work in 1960/61 [19]. Since I was Steward at the Scottish universities summer school, and could not be invited to the late-night discussions between Cabibbo, Glashow, Robinson and Veltman, I missed hearing about it in 1960, or I might have thought of applying my ideas about spontaneous symmetry breaking to the electroweak interactions. So it was left to Weinberg [20] and Salam [21] in 1967, as is well known, to bolt my and Brout and Englert's type of model onto Glashow's $SU(2) \times U(1)$ model of leptons, and the rest of the story is well-known to you.

The breakthrough finally in terms of having a useful theory came with the work of Veltman and Gerard 't Hooft in 1970, when they proved the renormalizability of pure Yang-Mills theory [22], and in 1971 when 't Hooft extended this proof to Yang-Mills theories with masses generated by spontaneous symmetry breaking in a scalar field system [23]. In 1972 Ben Lee, who had learnt about it first at a party in the University of Rochester at which we were both holding a glass of wine and a plate of sandwiches, then plastered my name over everything connected with spontaneous symmetry breaking, and other people were relegated to a footnote.

So let me come finally to 1975, which was when the hunt for the Higgs boson began, and in particular to the last sentence of the paper published in 1976 by John Ellis, Mary K. Gaillard and Dimitri Nanopoulos [24]: 'We should perhaps finish with an apology and a caution. We apologize to experimentalists for not having any idea what is the mass of the Higgs boson, unlike the case with charm, and for not being sure of its couplings to other particles, except that they are probably all very small. For these reasons, we do not want to encourage big experimental searches for the Higgs boson, but we do feel that people performing experiments vulnerable to the Higgs boson should know how it may turn up.' It is quite a long statement, but that was the hint that experimentalists should not neglect

to look for the so-called Higgs boson. Fourteen years later the situation was quite different when the Higgs-Hunters Guide was published by Sally Dawson, Jack Gunion, Howie Haber and Gordy Kane [25], and they wrote that: ‘The success of the Standard Model has been astonishing. The central problem in particle physics today is to understand the Higgs sector.’

Between 1989 and 1999, precision measurements at LEP of the Z boson, etc., gave bounds on the mass of the Higgs boson from the occurrence of logarithmic dependences in one-loop radiative corrections [26]. These were strengthened by the discovery of the top quark at the Fermilab Tevatron in 1994/5 [27], indicating that its mass was less than about 200 GeV. LEPs last run in 1999/2000 excluded a Higgs boson with mass up to 114 GeV. There were indications of a Higgs boson at about 115 GeV, but later the backgrounds were found to be worse than had been realized [28]. In 2001 the improved Tevatron started up, and has excluded a range between 158 and 175 GeV [29]. Now we are waiting for the LHC to complete the story.

Thank you.

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