My Life as a Boson: The Story of "The Higgs"

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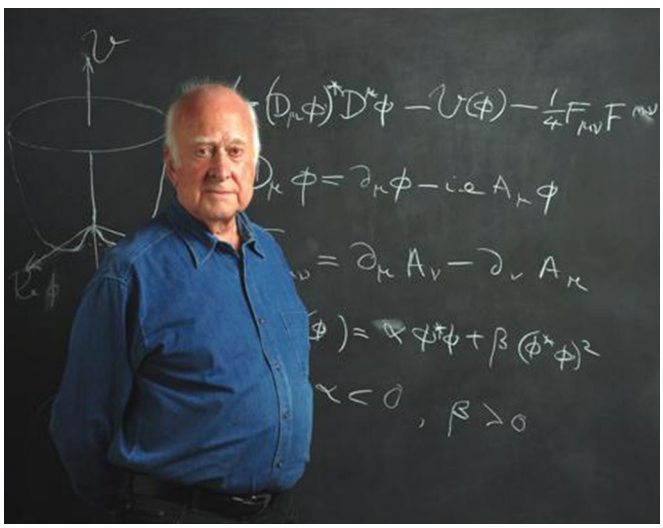
The story begins in 1960, when Nambu, inspired by the BCS theory of superconductivity, formulated chirally invariant relativistic models of interacting massless fermions in which spontaneous symmetry breaking generates fermionic masses (the analogue of the BCS gap). Around the same time Jeffrey Goldstone discussed spontaneous symmetry breaking in models containing elementary scalar fields (as in Ginzburg-Landau theory). I became interested in the problem of how to avoid a feature of both kinds of model, which seemed to preclude their relevance to the real world, namely the existence in the spectrum of massless spin-zero bosons (Goldstone bosons). By 1962 this feature of relativistic field theories had become the subject of the Goldstone theorem.

In 1963 Philip Anderson pointed out that in a superconductor the electromagnetic interaction of the Goldstone mode turns it into a "plasmon". He conjectured that in relativistic models "the Goldstone zero-mass difficulty is not a serious one, because one can probably cancel it off against an equal Yang-Mills zero-mass problem." However, since he did not discuss how the theorem could fail or give an explicit counter example, his contribution had little impact on particle theorists. If was not until July 1964 that, following a disagreement in the pages of Physics Review Letters between, on the one hand, Abraham Klein and Ben Lee and, on the other, Walter Gilbert about the technical details of the Goldstone, Salam and Weinberg proof of the theorem, it suddenly occurred to me that the ingredient that is crucial for evading the theorem is local gauge invariance. This is because gauge freedom complicates the implementation of Lorentz invariance.



My letter on how to evade the Goldstone theorem was accepted by Physics Letters. But when, a week later, I sent them a second letter outlining the simplest relativistic model in which spontaneous symmetry breaking generates a vector boson mass (the Higgs model), it was rejected. It was at this point that "Higgs bosons" made their first theoretical appearance; my revised version of the second letter (which I sent for publication to Physics Review Letters) drew attention to "incomplete multiplets of scalar and vector bosons" as a characteristic feature of non-Abelian generalizations of the model. This version was accepted, and the referee (Nambu, as I learnt when I met him in 1984) brought to my attention the related work of Brout and Englert.

The controversy over the Goldstone theorem did not end with the publication of my two letters. Gilbert raised technical objections, which I was unable to answer until,

at Chapel Hill in 1965, I had studied my Abelian model in more detail. The resulting preprint led to an invitation from Dyson to give a talk at the Institute, Princeton in March 1966; there I confronted an audience containing axiomatic field theorists whose belief in the Goldstone theorem was based on the vigorous algebraic proof by Kastler, Robinson, and Swieca. The next day Stanley Deser had arranged for me to talk at Harvard, where an equally skeptical audience awaited and; Sidney Coleman told me (in 1989) that they "had been looking forward to tearing apart this idiot who thought he could get around the Goldstone theorem".

My Princeton and Harvard seminars succeeded in convincing people that I was not a crackpot, but they clearly failed to persuade them that the combination of gauge theories and spontaneous symmetry breaking might be useful. At Harvard, Shelly Glashow complimented me after the seminar on "a nice model", but he did not see that this might be the cure for the difficulties of his 1961 electroweak model. That was left to Weinberg and Salam the following year. Meanwhile, Brout, Euglert and I tried fruitlessly to find an application in hadronic flavour symmetry breaking.

It was in 1972, following the Veltman-'t Hooft proof of the renormalizability of gauge theories, that my life as a boson really began. At the international HEP conference at Fermilab that summer Ben Lee, reporting on the gauge theory bandwagon which had began to toll, attached my name to everything involving spontaneous symmetry breaking, including the "Higgs meson".

By 1976, when LEP was being planned, this had been introduced to experimentalists in "a phenomenological profile of the Higgs boson" by John Ellis, Mary K. Gaillard and Dimitri Nanopoulos. Apologizing for the vagueness of this profile, they concluded "we do not wish 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" .

Fifteen years later, with much of the Standard Model verified experimentally, John Gunion, Howard Haber, Gordon Kane, and Sally Dawson in "The Higgs Hunter's Guide" felt able to be more assertive: "The success of the Standard Model has been astonishing. The central problem today in particle physics is to understand the Higgs sector".

From 1989 onward measurements at LEP defined the parameters of the Standard Model with ever increasing precision. Once the top quark had been discovered at Fermilab, the fit between experiment and theory at one-loop level depended only (logarithmically) on the Higgs mass(es). The predicted mass range indicated that a Higgs boson might be within the reach of LEP's last run. As is well known, some promising events were seen at around 115 GeV in the last week before LEP closed in the Fall of 2000. Now it is for Fermilab to continue the search for "Physics' most-wanted particle."

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