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Milestones in Physics (1)

Heretical Ideas that Provided the Cornerstone for the Standard Model of Particle Physics

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Milestones in Physics

The articles appearing in future under "Milestones in Physics" will give an insight into special events or situations that have been decisive for the evolution of Physics. Undoubtedly 1964 was such a year of pioneering work, when theoretical foundations for Particle Physics were established, which now after almost 5 decades were verified by an unprecedented master effort in experimental physics. One of the protagonists of those early years is Professor Gerald S. Guralnik, who will give in the following text a first-hand account to the emergence of the theoretical framework.

Heretical Ideas that Provided the Cornerstone for the Standard Model of Particle Physics

Gerald S. Guralnik

On July 4, 2012, I sat in the CERN auditorium next to long-time friend and collaborator Richard Hagen, listening with excitement to the presentations of data from the ATLAS and CMS collaborations. It soon became clear that these magnificent experiments had unquestionably found a particle that looked very much like the long-sought "Higgs" boson.

Figure 1: Gerald Guralnik (left) and Richard Hagen, and, between the two, Peter Higgs at CERN.

I find it amazing that my part in the prediction of this particle had its roots in my Ph.D. thesis of nearly 50 years ago (1964). I was a student at Harvard, enthralled with quantum field theory, and totally seduced by the marvelous lectures of Julian Schwinger. However, it seemed that quantum field theory (QFT) was near death almost everywhere outside of Harvard Yard. Its miraculous success in electrodynamics was eclipsed by its apparent failure to describe the interactions of the rapidly expanding catalog of known "elementary" particles. The only available calculational approach of the time, namely, coupling-constant perturbation theory, so successful for electromagnetism, obtained answers by expanding about the solutions of the free fields using a small numerical parameter that characterized the strength of the interaction Hamiltonian. This method could not be applied to examine the strong interaction, the force that binds protons and neutrons together, because, in this case, that parameter is too large. The weak interaction, responsible for radioactivity, then described by a small parameter that multiplied a term involving four fermion fields, was also incalculable because of uncontrollable divergences beyond the leading approximation. Nevertheless, although it took a while to realize it, QFT was already on the road to rebirth.

Following a seminar Bjorken gave at Harvard in 1963, Walter Gilbert, my thesis advisor, asked me to look at his work as well as at the earlier seminal papers by Nambu and Jona-Lasinio (1961). These papers re-examined the QFT calculations of four-fermion interactions. In particular, instead of starting with the free-field approximation used in coupling-constant perturbation theory, they introduced a coupling-constant-dependent leading approximation. These approximations, which were carefully engineered to be consistent with the original equations, can be superficially (and partially) understood as representing a limit of appropriately arranged sums of perturbative results involving all orders of the coupling. However, the end results are entirely different from those of perturbation theory, in that, as the coupling of the interaction goes to zero, these new solutions are singular and consequently do not approach the free-field limit. They describe a new "phase". Initially, these solutions were not universally accepted as being valid. However, since the original operator equations were not linear, the possibility of multiple solutions should not have been surprising. This skepticism was reinforced because the new solutions did not respect the symmetry of the original Lagrangian. The "symmetry breaking" happens in a very special way. While the equations of motion are obeyed, boundary conditions are imposed which are not consistent with the symmetry. It is assumed that the state of lowest energy (the vacuum state) of a QFT is not an eigenstate of the time-independent operator (which is basically a generalized charge) always associated with the symmetry through Noether's theorem.

Figure 2: The picture of three of us with a Sunbeam Alpine was taken in a parking lot at Harvard while we were graduate students (Hagen, standing at the right, was at MIT). The fellow making the hand gesture is Robert May who is now Lord May of Oxford (1962).

Generally, it is then possible to find a local operator (a combination of quantum fields at the same point in space-time) that has a constant expectation value in this vacuum. Such solutions, introduced by Nambu and Jona-Lasinio, are called spontaneous symmetry-broken solutions. They were something entirely new!

A massless scalar particle was always present in the approximations used in the original papers. These massless particles were also present in the famous result of Goldstone (1961) that found similar "spontaneously broken" solutions in a theory of scalar bosons with a quartic interaction. An exact proof that a massless particle is required under such circumstances was presented by Goldstone, Salam and Weinberg (1962). This result is now known as the Goldstone or Nambu-Goldstone theorem.

Bjorken's work is particularly interesting because his model identifies the massless Nambu-Goldstone boson with the physical photon. The current-current four-fermion interaction appeared to reproduce – at least in the first broken approximation – the results of usual quantized electromagnetism. This was troublesome, as the broken symmetry is that of Lorentz invariance, and it seemed unlikely that there would be no physical trace of this undesirable result in the full solution. Despite appearances to the contrary, I was able to show that the broken-symmetry solution respected Lorentz invariance for all physical observables. I also formulated a consistent iteration scheme to calculate beyond the first-order approximation. The results are made finite using standard renormalization methods, and replicate usual electrodynamics. Similar techniques applied to the Nambu-Jona-Lasinio model show that this symmetry-breaking solution is also renormalizable to all orders. This was more than enough for a good thesis. However, I was not satisfied! Schwinger had argued that the usual electromagnetic QFT Lagrangian generated a massless photon because of the smallness of the electromagnetic coupling. I thought that this was wrong, and that I could use what I had already learned to prove that this theory, in fact, requires massless photons without regard to the strength of the coupling. I managed to construct a "proof", and added another chapter to my Ph.D. thesis with the details. During my final oral examination, Sydney Coleman became (correctly) skeptical of this result, and I was required to remove the chapter from my dissertation, but luckily I was allowed to pass the examination!

I moved to Imperial College in early 1964 as a National Science Foundation Postdoctoral Fellow. My first choice had been to go to CERN, but fortunately my application was rejected. Imperial College was a lively and exciting place, driven by the imagination and wisdom of Abdus Salam and Paul Matthews, and further enriched by a constant stream of visitors. I met many superb younger physicists there, including Tom Kibble.

Figure 3: A. Salam and T. Kibble at Imperial College (late 1960's)

I remained obsessed with the idea that there was a dynamical reason requiring the photon of the quantized Maxwell equations to be massless. After a lot of mathematical experimentation, I found a set of new, rather strange, (seemingly) time-independent operators, and showed that the vacuum was not an eigenstate of these generalized "charges". Applying the Nambu-Goldstone theorem for the commutators of the "charges" with the vector potential, I was then able to show that the photon must be massless in any phase! I quickly wrote a paper (in April) that included this result and some other historically important observations. This paper turned out to be the first of the five papers published in 1964 that were associated with the Higgs phenomenon and the Higgs particle. Almost immediately after this paper was submitted, I realized that I had made a very subtle error. What I did is correct in manifestly covariant gauges, but the massless particles, required by the Nambu-Goldstone theorem, are purely gauge (not physical) objects. In the Coulomb gauge, which only involves physical quantities, normally irrelevant spatial integrals do not fall off quickly enough to be discarded, and consequently these strange generalized charges can leak out of any volume. They must therefore depend on time! The basic assumption needed to prove the Nambu-Goldstone theorem was not met! Further analysis showed that it is impossible to use the theorem to prove the existence of physical massless particles in any "phase" of any gauge theory! This is an exact result. My initial belief was completely wrong! I expected to be able to revise this paper in proof, but because of a series of unlikely events, I never saw the paper again and it was published by Physical Review Letters after its receipt on June 1, 1964. Ironically, and showing the seductiveness of an elegant error, Peter Higgs published a now famous paper in *Physics Letters* (received July 27, 1964), where he makes the same subtle mistake, and reaches a different but equally unjustifiable conclusion - but one that is now more to everyone's liking.

Figure 4: The author enjoys his Leica III f (ca. 1970)

After being wrong twice, I knew that I needed help, and enlisted the aid of Hagen and Kibble. By the end of April, we had all of the essential results (including the now famous boson) of our "GHK paper" that was received by *Physical Review Letters* on October 12, 1964. The only thing missing was the final consistency check given in the last equation in our paper. This paper presents the above general result, namely that the Nambu-Goldstone theorem cannot be applied to physical particles described by gauge theories. It also introduces spontaneously-broken scalar electrodynamics as a new specific model of this phenomenon, and examines the leading-order solution. As expected, it has no resemblance to solutions in coupling-constant perturbation theory. The massless photon, after absorbing one degree of freedom of the charged scalar boson, is replaced by a massive particle of unit spin. This leaves a neutral scalar boson, now known as the "Higgs" boson.

It took us so long to publish our work, because none of the many physicists we approached believed our very strange results. My painful history of errors combined with the fact that we ended up with the exact opposite result to that which I had originally expected, was adequate justification for caution. Indeed, even after publication, Heisenberg made it clear to me at his conference at Feldafing (1965) that he thought our work was wrong. The many seminars I gave on our ideas were generally met with (mostly polite) skepticism.

Just as our paper was about to be put into the mail, we were surprised by the arrival of two preprints containing related work by Englert and Brout (EB) and also Higgs (H). While these addressed the same problem, we felt that they missed many crucial points. Neither paper raised the fundamental point of the relevance of the Nambu-Goldstone theorem to only gauge modes, and certainly not to approximation-independent results. Both the EB and H papers examine leading-order broken scalar electrodynamics in manifestly covariant form. Such solutions must have massless Nambu-Goldstone particles, despite that our work showed that these cannot correspond to physical solutions. EB did not do a complete study, and entirely missed the "Higgs Boson", and made only a weak remark about the Nambu-Goldstone massless particle. Higgs only considered a classical solution, and ignored the massless Nambu-Goldstone boson contained in his equations. This massless boson is absolutely required by quantum mechanics, and by overlooking this Higgs neglected to address the formidable problem presented by the Nambu-Goldstone theorem.

Stimulated by the recent experimental developments, discussions about these old papers have resumed. It has been observed that, in the GHK paper, the mass of the "Higgs" boson is zero, while it is not zero in Higgs' paper. In fact, the mass given in the H paper can take on any value (including zero), as it depends on undetermined parameters. Moreover, the mass in the H and GHK papers has nothing to do with the physical mass of the "Higgs" boson. This is because only the leading-order approximation is considered, and higher-order iterations produce divergences which, even after renormalization, leave the physical mass an undetermined parameter that can only be set by experiment. The theory puts absolutely no constraint on this mass! This is why there was no direct guidance of where experimenters should look for the particle. The GHK and the H papers give different leading-order masses because the first-order approximations are different due to the implementation of different - but equivalent - renormalization philosophies. As the order of iteration increases, the renormalized Greens functions of the two different approaches converge.

There are two additional papers that complete the five 1964 series of papers discussed above. The paper that I gave at the conference where I had the discussion with Heisenberg mentioned above was published in the *Proceedings of seminar of unified theories of elementary particles*, July 1965. This paper contains a significant extension in detail of the GHK paper. It has recently been republished in *Mod. Phys. Lett.* A26 (2011) 1381-1392 and posted as an eprint in *arXiv*:1107.4592. Peter Higgs in 1965 submitted a paper to *Physical Review* that has much in common with my paper mentioned above and indeed, he thanks me for conversations. He adds one element by displaying calculations of tree graphs which contribute to the next order of correction to the 1964 PRL model.

It is worth noting that the discussion of the above mechanisms can be generalized to the full standard model, since the non-abelian analysis is not different in any fundamental way from the abelian case. Along this line another relevant paper by Tom Kibble was published in 1967 in *Physical Review* specifically applying the arguments of the 1964 GHK paper to non-abelian gauge theories.

Only after the work by Weinberg and Salam proposed a unified electroweak theory, did any of these papers receive serious attention. If the particle described on July 4th turns out to be the "Higgs Boson", as seems very likely, the major missing part of the standard model will be in place, and our work will be verified to be far more than the questionable mathematical exercise that it was initially thought to be.

For readers interested in more of the sociology behind this work as well as the detailed mathematics and extensive explicit referencing, I suggest reading my historical review:

"The History of the Guralnik, Hagen and Kibble development of the Theory of Spontaneous Symmetry Breaking and Gauge Particles." e-Print: *arXiv*:0907.3466 [physics.hist-ph], *Int.J.Mod.Phys.* 24:2601-2627, 2009.

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