

# The History of the Guralnik, Hagen and Kibble development of the Theory of Spontaneous Symmetry Breaking and Gauge Particles

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## Abstract

I discuss historical material about the beginning of the ideas of spontaneous symmetry breaking and particularly the role of the Guralnik, Hagen Kibble paper in this development. I do so adding a touch of some more modern ideas about the extended solution-space of quantum field theory resulting from the intrinsic nonlinearity of non-trivial interactions.

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# 1 Introduction

This paper is an extended version of a colloquium that I gave at Washington University in St. Louis during the Fall semester of 2001. But for minor changes, it corresponds to my recent article in IJMPA [1]. It contains a good deal of historical material about the beginning of the development of the ideas of spontaneous symmetry breaking as well as a touch of some more modern ideas about the extended solution-space (also called vacuum manifold or moduli space) of quantum field theory due to the intrinsic nonlinearity of non-trivial interactions [2, 3]. While I have included considerable technical content, a reader interested only in the historical material should be able to follow the relevant content by just ignoring the equations. The paper is written from a very personal point of view. It puts particular focus on the work that I did with Richard Hagen and Tom Kibble (GHK) [4]. Our group and two others, Englert-Brout (EB) [5] and Higgs (H) [6, 7] worked on what is now known as the “Higgs” phenomenon. I discuss how I understand symmetry breaking (a viewpoint which has changed little in basics over the years) and cover the evolution of our 1964 (GHK) work, which was done in its entirety without any knowledge of others working on the same problem of symmetry breaking and gauge systems. Later in this document, I will make a few brief statements accentuating the differences between our work and that of EB and H.

An intense collaborative effort between Hagen, Kibble and me enabled us to produce a paper that to this day gives us great satisfaction. The physics I am about to describe was very exciting. Although we knew the work was basic, we had no appreciation as to how important the concepts involved would become. Despite the current significance of our work, to me the most important thing that has come from it is my enduring long friendship with Dick and Tom. We came together to do this work because of fortunate overlaps of interests and geography. Hagen had been my friend and physics collaborator since our undergraduate days at MIT. I went to Harvard for graduate school, while Hagen stayed at MIT. At that time, in practice, there was little difference in the training a particle theorist received at either of these universities. We could easily take courses at either place and we were constantly doing the short commute between the schools to “cherry pick” course offerings. Many of us from Harvard attended a particle phenomenology course taught by Bernard Feld at MIT. Also, in the mix, was the painful but very important year-long mathematical methods course taught at MIT by Feshbach using his book with Morse. Almost all of us, Harvard or MIT, attended Schwinger’s field theory courses at Harvard. In addition, I took the field theory course at MIT taught by Ken Johnson, who was Hagen’s thesis advisor. This course was beautifully done and more calculational in nature than Schwinger’s courses. I also sat in on Weisskopf’s nuclear physics course at MIT, which was really fun and, in fact, largely taught by Arthur Kerman. Of course, I took most of the “standard” graduate courses at Harvard. Roy Glauber and Wally Gilbert taught courses that were so well reasoned that I find my notes from their classes still useful.

Hagen and I wrote our first paper [8] together when we were graduate students, and we continued to talk about life and physics after he went to Rochester as a postdoctoral fellow, while I concluded my thesis on symmetry breaking and spin-one fields with primary emphasis on Lorentz symmetry breaking in four fermion vector-vector couplings [9, 10]. I was working under the direction of Walter Gilbert who, by this time, had largely switched over to biology (Nobel Prize in Chemistry, 1980). Early in 1964, I passed the thesis exam and with Susan,

my new wife, and a NSF postdoctoral fellowship went, in February, to Imperial College (IC) in London, where I immediately met Tom and soon began the discussions that eventually led to our three-way collaboration and the GHK paper.

While I recount the history of our work, I will do so embedding a fair amount of physics reflecting how we understood symmetry breaking. Even after all these years, I feel our understanding is still appropriate and indeed recently it helped form the basis of what I believe is a fundamental approach to understanding the full range of solutions (solution-space — this used to be called “vacuum manifold”; however, nowadays it is generally referred to as “moduli space”) of quantum field theories [2, 3]. Some constructs from these modern papers are used in the following discussion to help clarify my points.

## 2 What was front-line theoretical particle physics like in the early 1960s?

To set our work in perspective, it is helpful to review what high energy physics was like when this work was being done. Many “modern” tools such as the Feynman path integral and the now nearly forgotten Schwinger Action Principle were already available. However, the usual starting point for any theoretical discussion was through coupling constant perturbation theory.

The power of group theory beyond  $SU(2)$  was just beginning to be appreciated, and was realized through flavor  $SU(3)$  as introduced by Gell-Mann and Ne’eman. The  $\Omega^-$ , needed to fill in the baryon decuplet (10 particles) was found in 1963. The Gell-Mann–Zweig quark (ace) ideas had just been formulated, but were far from being completely accepted. There was no experimental evidence for quarks, and the ideas about color that allowed three quarks to make a nucleon only began to take form in 1964 [11]. Calculation methods were manual and limited. For the most part, coupling constant perturbation was the only tool available to try to get valid quantitative answers from quantum field theory. Because of the doubts that field theory could ever move beyond perturbation theory,  $S$ -Matrix theory was king, at least far west of the Mississippi River. Current algebra, a mixture of symmetry and some dynamics was beginning to take shape with work by Nambu and Lurie [12], but was still in the wings. Indeed, starting in the 1960s, several of the major contributions to what was to become a dramatic reinvigoration of quantum field theory came from the works of Nambu and collaborators.

The Nambu–Jona-Lasinio model [13, 14] described by the interaction:

$$g \left[ (\bar{\psi}\psi)^2 - (\bar{\psi}\gamma_5\psi)^2 \right]$$

played a key role in the development and the understanding of spontaneous symmetry breaking in quantum field theory. There is no bare mass term in this interaction and consequently the action has a conserved chiral current. This interaction, in itself, is disturbing (as were the four-fermion interactions used to describe weak processes) because perturbation theory in  $g$  produces a series of increasingly primitively divergent terms, making this expansion unrenormalizable. Nambu and Jona-Lasinio studied this model by imposing a constraint requiring that  $\langle\bar{\psi}\psi\rangle \neq 0$ , which induces a non-vanishing fermion mass, and thus seems in-

consistent with (or “breaks”) chiral symmetry. Based on this assumption, they formulated a new leading order approximation (not a coupling constant perturbation theory). This approximation is, in fact, consistent with the conservation of the chiral current, thereby assuring that, despite the induced mass, the approximation remains consistent with the basic requirements of the field equations for this action.

In addition, their results showed that there was a zero mass composite particle excited by  $(\bar{\psi}\gamma^5\psi)$ . This particle is now called a Goldstone boson or occasionally a Nambu-Goldstone boson. The name was acquired after developments in other papers [15, 16]. Stated somewhat more generally, Ref [16] proves that if the commutator of a conserved charge (resulting from a continuous current) with a local field operator has non-vanishing vacuum expectation value, then the local field operator must have a massless particle in its spectrum. This result is exact and not confined to a leading order approximation. This is the Goldstone theorem and it is always true, provided its basic assumptions, outlined carefully in what follows, are satisfied.

The Nambu–Jona-Lasinio approximation can be extended in a consistent way to a well defined expansion (all orders, but not in coupling  $g$ ), much as is done in Refs [9] and [10]. The resulting expansion can be renormalized, and the resulting theory looks like a sum of conventional theories involving pseudoscalar and scalar couplings to fermions. However, the underlying fermionic mass in this “phenomenological” theory is generated by symmetry breaking information carried in the vacuum state, and the pseudoscalar boson is constrained to be massless.

### 3 A simplified introduction to the solutions and phases of quantum field theory

Before presenting a solution to symmetry breaking quantized scalar electrodynamics, the core component of the unified electroweak theory, it is worth a short review of properties that are found in solutions to any quantum field theory. Considerable insight into the nature of spontaneous symmetry breaking in QFT is available through the examination of the solutions of comparatively simple differential equations. Some of the material in this section, is relatively new and was not available at the time of publication of the GHK paper [2, 3]. In fact, the development of these clarifying ideas was inspired by the GHK paper.

A QFT is described by assuming a form for an action. From the action, all solutions and their Greens functions and hence all properties of the QFT can be calculated. One of two equivalent methods, the Schwinger action principle or the Feynman path integral, is commonly used to formulate the equations used for explicit calculations. Of course, a major part of a physics problem is to guess a form for the action which is consistent with known physics and predicts correct new physics. The basic points that we want to emphasize here can be demonstrated by looking at the action for a single scalar field  $\phi$  interacting quartically with itself and linearly with an external source  $J(x)$  in 4 space-time dimensions. The (Euclidean) action is given by:

$$\int d^4x \left[ \phi(x) \frac{(-\square + m^2)}{2} \phi(x) + g \frac{\phi^4(x)}{4} - J(x)\phi(x) \right].$$

The (Euclidean) Schwinger Action principle:

$$\delta \langle t_1 | t_2 \rangle = \langle t_1 | \delta S | t_2 \rangle ,$$

results in the equation:

$$(-\square + m^2) \phi(x) + g \phi^3(x) = J(x)$$

Defining  $\mathcal{Z}$  as the matrix element of a state of lowest energy in the presence of the source at very large positive time measured against the “same state” at very large negative time and again using the Schwinger action principle leads to:

$$\left[ (-\square_x + m^2) \frac{\delta}{\delta J(x)} + g \left( \frac{\delta}{\delta J(x)} \right)^3 - J(x) \right] \mathcal{Z}[J] = 0 .$$

It is important to observe that this method removes all operators and produces number valued equations. Any Green’s function can be calculated by taking functional derivatives with respect to the source  $J(y)$  of the above equation.

A good way to make sense of this equation is examine it on a space time lattice with  $N$  space time points. This approach can be regarded as the original definition of a quantum field theory which is realized only in the limit of vanishing lattice spacing. On a hyper-cubic lattice:

$$\square \phi_n = \sum_k (\phi_{n+\hat{e}_k} + \phi_{n-\hat{e}_k} - 2\phi_n) , \quad (1)$$

where  $\hat{e}_\mu$  is a unit vector pointing along the  $k$ -direction and, for convenience, the lattice spacing is set to 1. Since functional derivatives become ordinary derivatives at a lattice point the equation for  $\mathcal{Z}$  on the lattice is:

$$\left[ - \sum_k (\phi_{n+\hat{e}_k} + \phi_{n-\hat{e}_k} - 2\phi_n) + m^2 \frac{d}{dJ_n} + g \left( \frac{d}{dJ_n} \right)^3 - J_n \right] \mathcal{Z}[J_1, J_2, \dots, J_p, \dots] = 0 . \quad (2)$$

The space-time derivatives have served to make this an equation involving three lattice points with the functional derivatives becoming normal derivatives acting on the variable at the central lattice point. This lattice equation makes it clear that  $\mathcal{Z}$  is described by  $N$  linear third order coupled differential equations in the source  $J(x)$ . Generally this can be expected to result in  $3N$  independent solutions. Many interesting things occur in the limit of taking an infinite number of lattice points while also moving the lattice spacing to zero to produce the physical continuum limit. The number of independent solutions is reduced leaving a continuum theory with phase boundaries and a generally a non-trivial solution space. This process is very complex and is carefully discussed in [2, 3]. The important thing to remember from this is that there are multiple solutions/phases to an interacting QFT. This is ultimately the consequence of the non-linear nature of the original interaction. In the early attempts to understand interacting QFT, the only tool available was coupling constant perturbation theory built around the free solution. The richness of the solution space of the

theory could not be observed and was not generally anticipated. Even now, the extent of the possible solution space of quantum field theory is not fully appreciated.

It is possible to make the same comments about the solution space of QFT starting with the Feynman path integral formulation. However, in most works the path integral is defined as integration over fields valued on the real axis. This does not yield the full range of solutions of canonical field theory that we discussed above. In fact, defined this way, the path integral only produces solutions that are regular as the coupling vanishes. Such solutions consequently approach those produced by coupling constant perturbation theory. The conventional Path Integral formulation is correct as far as it goes, but it excludes explicit access to much of the interesting content of canonical QFT, such as symmetry breaking. However, many of these deficiencies are corrected by extending the definition of the path integral into the complex plane by not restricting the path integrations to the real axis but instead allowing any range of integration through the complex plane for which the endpoints give zero contribution. This procedure reproduces the Schwinger action principle conclusions discussed above. Of course, the resulting complex contributions must be combined so that only real (Euclidean)  $\mathcal{Z}$  results. These comments will be expanded by studying the simplified lower dimensional version of the above equation in the next paragraphs.

Zero space-time dimension means that only one point exists and thus the lattice equation becomes:

$$g \frac{d^3\mathcal{Z}}{dJ^3} + m^2 \frac{d\mathcal{Z}}{dJ} = J\mathcal{Z} .$$

While losing any space-time structure and thus the possibility of understanding all the interesting structure that occurs in the continuum limit, the above still maintains the non-linear nature of quantum field theory and the associated multiple solutions. Calculating solutions is now straightforward. While the finite dimensional case potentially has an infinite number of solutions before accounting for the collapse of the solution set, the current equation, representing “zero dimensional QFT” only has three independent solutions. The solutions can be found easily by using series methods. Alternatively, solutions can be found by examining the integral representation:

$$\mathcal{Z} = \int e^{-g \frac{\phi^4}{4} - \frac{m^2}{2} \phi^2 + J\phi} \mathcal{D}\phi .$$

This clearly is the Feynman path integral of the action in zero dimensions, which (within a normalization constant) corresponds to the vacuum-to-vacuum matrix element of a zero dimensional quartic scalar field theory. The integrand is negative of the Euclidean action of this theory. In order to obtain the same information as is available through solution of the differential equation, this integral must be evaluated over all possible independent paths in the complex plane where the contributions at the end points of the paths vanish. Straightforward evaluation shows that there are three allowed independent paths in the complex plane corresponding to the three solutions to the differential equation. It is always possible to weight each of the three solutions with a complex number in order to produce three independent real solutions. This is exact correspondence to the comments above for the theory with space-time structure. The integrand has 3 stationary points which are ( $J = 0$ )

located at

$$\phi = 0, \phi = \pm \left\{ \sqrt{\frac{-m^2}{g}} \right\}.$$

It is easy to expand around these saddle points to discover asymptotic expansions for each of the three solutions.

The single field expectation value  $\phi(J)$  is defined as

$$\phi(J) = \frac{dZ}{dJ}.$$

From the integral representation it follows that:

$$\phi(0) = \int \phi e^{-g \frac{\phi^4}{4} - \frac{m^2}{2}\phi^2} \mathcal{D}\phi.$$

This integral has three values corresponding to the three independent integration paths. For the path along the real axis (corresponding to expanding around the stationary point at  $\phi = 0$ ) we find  $\phi(0) = 0$ . This associated asymptotic solution reduces to the usual perturbation expansion around  $g = 0$  for small  $g$ , and as a consequence the vacuum expectation of a single field (or any odd power of the field) vanishes for a vanishing source. In general, the symmetry under reflection of  $\phi$  of the integrand of the above integral representation might suggest we look only at integration paths that respect this symmetry, but if we did so we would leave out two of the solutions to the differential equation. For the two other paths, which include imaginary points,  $\phi(0)$  is not zero and the global reflection symmetry of the integrand is “spontaneously broken”. In theories with space-time structure, the situation is more interesting and complex because we have breaking associated with a conserved local current. However, the observation that a theory has multiple solutions as a consequence of its original non-linearity remains an essential fact, and it follows that most of these solutions will have lower symmetry (symmetry breaking) than the manifest symmetry of the original action. Direct calculation shows that these solutions diverge as  $g$  becomes small. They must diverge, else they could be described by coupling constant perturbation theory and produce the same result as that of the previously discussed path corresponding to the saddle point at the origin.

Despite the fact that we have examined a simple system with one degree of freedom, our observations carry over (with appropriate modifications) to higher dimensions. At most, one of these solutions has a finite value at zero coupling and that solution is approximated by an asymptotic expansion around  $g = 0$  corresponding to coupling constant perturbation theory. Should the perturbative (Taylor) expansion in  $g$  about  $g = 0$  not be asymptotic but convergent, then it is the only solution, which, in general, cannot be the case. This comment corresponds to the content of Dyson’s famous paper [17]. The other solutions are generally of the spontaneously broken type, meaning that they have non-vanishing expectation values associated with some Green’s functions that vanish in the perturbative calculation. These Green’s functions are singular for vanishing coupling. This is easily confirmed in the zero dimensional quartic example given above. Unlike the relatively trivial zero dimensional cases, the symmetry of the action that is “broken” by a choice of boundary conditions (i.e. vacuum state) is usually continuous. This is reflective of the more complex and rich set of solutions

that occurs in higher space time dimensions. An important feature of the complexity of the higher dimensional solutions follows from the fact that while in the zero dimensional case solutions are totally unconnected phases with no way to move from one phase to another, in higher dimensions it is possible to have phase transitions through changes of parameters so that different phases can be associated with different values of a parameter set labeling the solutions. This is discussed in Ref [3]. Finally it should be noted that there appear to be interesting cases in higher dimensions where even the perturbative solution does not exist. This appears to be the case in 4 space-time dimensions where it is believed that there are no non-trivial solutions that are regular as the coupling goes to zero and this even while the perturbation theory appears to exist, in the end it apparently just defines a non-interacting theory. Note that this is a much more complicated phenomena then the case of cubic interacting scalar field theory in any space-time dimension. Even in zero dimensions, this theory, while having solutions, does not have any which are regular as the coupling vanishes.

## 4 Examples of the Goldstone theorem

I now explicitly examine some four (1 time, 3 space) dimensional quantum field theories with spontaneous symmetry breaking. I begin with the simplest possible free model to demonstrate the ideas associated with Goldstone's theorem but will end up examining some very interesting non-perturbative solutions of the type discussed above.

Assume that at least one state of *lowest* energy, the vacuum,  $|0\rangle$ , exists, and that the free relativistic scalar field is described by the operator action:

$$\int d^4x \left[ \frac{(\partial_\mu \phi)(\partial^\mu \phi)}{2} - \frac{m^2 \phi^2}{2} \right].$$

From this, the free one-field Green's function equation is:

$$(-\partial^2 - m^2) \langle 0|\phi(x)|0\rangle = 0 .$$

The requirement that the vacuum energy-momentum  $P^\mu$  generates space-time translations, combined with the requirement that the vacuum is an eigenstate of this energy-momentum operator, leads to the useful condition that

$$\langle 0; n|\phi(x)|0; n\rangle = \langle 0; n|\phi(0)|0; n\rangle \equiv n .$$

The label  $n$  is introduced in the states to explicitly categorize the vacua by their expectation values. Because the fields are translated by exponentials of  $P^\mu$ , it follows that

$$m^2 n = 0 .$$

Thus, the one-field Green's function reduces to the requirement that

$$m^2 = 0 \text{ or}$$

$$n = 0$$

where  $n$  can be chosen to be an arbitrary real number. Each choice for  $n$  yields a new independent vacuum state and as a consequence there is an infinite number of totally disjoint (inequivalent) sets of states producing identical disjoint (free) theories [18]. This is an elementary example of Goldstone's theorem [13, 14, 15, 16]. As pointed out previously, this theorem says that if a charge associated with a conserved current in a relativistic field theory does not destroy the vacuum, the theory must have massless excitations.

The current in this example is

$$\begin{aligned} J^\mu(x) &= \partial^\mu \phi(x) \\ \partial_\mu J^\mu(x) &= \partial^2 \phi(x) = -m^2 \phi \\ \therefore \text{if } m^2 &= 0 \Rightarrow \partial_\mu J^\mu(x) = 0 \end{aligned}$$

and the charge is

$$Q \equiv \int d^3x (\partial^0 \phi(\vec{x}, t)) .$$

This charge (which is defined only when it appears in commutation relations as follows) does not destroy the vacuum. From the canonical commutation relations:

$$\begin{aligned} i [\partial^0 \phi(x), \phi(y)] \Big|_{x^0=y^0} &= \delta^{(3)}(x-y) \\ \Rightarrow i [Q, \phi(y)] &= 1 \\ \therefore i \langle 0; n | [Q, \phi(y)] | 0; n \rangle &= 1 . \end{aligned}$$

It should be noted that this is consistent with

$$\frac{dQ}{dt} = 0 ,$$

despite the fact that the vacuum  $|0; n\rangle$  is not an eigenstate of the charge  $Q$ . This example, with no interaction, is particularly simple. It does not show the complex phase structure that always occurs with interacting fields and is even demonstrated in the preceding zero dimensional discussion. However, it does illustrate the association of non-vanishing field expectations of translationally invariant symmetric theories with massless particles.

Massless particles, particularly now that we know neutrinos have a small mass, seem to be limited to photons. After the Goldstone theorem was discovered, it was natural to ask how symmetry breaking could be used to explain the photon. After Bjorken gave a talk (1963) at Harvard examining this possibility, my thesis advisor, Wally Gilbert, suggested that I look at Bjorken's proposed alternative four fermion model of quantum electrodynamics [19], which is a variant of the Nambu–Jona-Lasinio model with vector-vector interaction:

$$g (\bar{\psi} \gamma^\mu \psi) (\bar{\psi} \gamma_\mu \psi) .$$

The methodology of Bjorken's model seemed dangerous because the “breaking” requires that the conserved current that appears in the interaction has a non-vanishing vacuum expectation value. This picks a preferred direction in space-time, which seems to destroy Lorentz

symmetry and therefore could result in solutions that violate relativistic invariance. However, breaking in this manner will respect relativistic invariance if matrix elements are consistent with the operator commutation relations involving the six generators of the Lorentz group. I showed this restriction can be met for the above interaction by constructing an iterative solution scheme with non-vanishing expectation of the current, consistent with all conservation constraints including the commutation relations of the Lorentz group. This demonstrated that the details of Bjorken's work were not quite correct, but, with some modification, his conclusion that this theory is physically equivalent to QED is correct [9, 10]. The result is rather elegant in that the symmetry breaking parameter becomes "calculationally inert" in the sense that it is harmless, and is just passed through any calculation involving commutators. In the process of doing this, I set up what was probably the first well defined all-order relativistic expansion about a non-perturbative saddle point. This expansion, in composite vector propagator loops, is identical to the approach later named "the large- $N$  expansion". The resulting electrodynamic equivalent theory is a surprise, because from coupling constant perturbation theory this interaction is well known to lead to a hopelessly divergent non-renormalizable expansion in the coupling constant. The expansion I examined corresponds to an expansion about a different saddle point of the path integral than that for the coupling constant expansion, and leads to a solution in a symmetry breaking phase that shows the requisite small  $g$  singularities.

Said differently, the solutions of the Bjorken model, as well as the usual solutions of the Nambu–Jona-Lasinio model, correspond to alternative (i.e., non-coupling constant perturbation) solutions of a theory with a four fermion interaction. The new solutions can be shown to be equivalent to those of normal perturbative quantum electrodynamics. However, in this case, the original theory does not have gauge invariance and the propagators, as directly calculated, correspond to normal electrodynamics in the Lorentz gauge. This theory explicitly has a massless particle behaving as a photon because of the Goldstone theorem. It is perhaps useful to point out that we have not shown that vector-vector four fermion interactions are identical to normal QED, but have shown that these two different operator theories share one solution of their otherwise different solution set.

To put some of the above into more explicit form, the solutions of the Bjorken model under discussion are associated with symmetry breaking boundary conditions that follow from the requirement that

$$\begin{aligned}\langle j^\mu \rangle &= n^\mu \\ j^\mu &= \bar{\psi} \gamma^\mu \psi.\end{aligned}$$

Clearly, since

$$[\mathfrak{J}^{\mu\nu}, j^\lambda] \propto [g^{\mu\lambda} j^\nu - g^{\nu\lambda} j^\mu]$$

where  $\mathfrak{J}^{\mu\nu}$  generates Lorentz transformations. This requires that

$$\mathfrak{J}^{\mu\nu} |0\rangle \neq 0.$$

The symmetry of the vacuum is therefore broken, and  $j^\mu |0\rangle$  contains a zero mass (spin-one) particle — a photon. It is important to note that we have constrained the solutions by requiring translational invariance imposed by constant expectation values independent of space-time.

## 5 Gauge particles and zero mass

Despite the fact that Schwinger had convincingly argued by that time that there was no dynamical reason for the photon to have zero mass [20], from the arguments I gave about the Bjorken model, I thought that I could construct a symmetry breaking argument that would require massless photons in conventional QED. This argument was wrong and, fortunately, Coleman detected this in my (early 1964) thesis presentation. Needless to say, this did not make me happy but I should have known better. We did not socialize for some time, but he was right and the chapter was removed. Even after removing the offending chapter from my thesis, I was still sure I had missed something. Indeed, it turned out that this chapter had in it the seeds that led to the GHK work.

Another set of information generated at Harvard also turned out to be directly relevant, although that was not appreciated until after we understood the basics of symmetry breaking and gauge fields. Well before my thesis was finished, I spoke with Gilbert about another project. He was very interested in how a massive field theory of spin-one made the transition to electromagnetism as its mass vanishes and had done some interesting calculations illustrating the mechanism involved. I told Dave Boulware about this and he, in turn, discussed this work with Gilbert, and they wrote a nice paper containing the original calculations and other related calculations [21]. They observed that, given a massless *scalar* particle ( $B$ ) and a massless *vector* particle ( $A^\lambda$ ) with the simple “interaction”:

$$g A^\lambda (\partial_\lambda B - g A_\lambda)$$

there results a free spin-one field with mass  $g^2$ . That this simple quadratic action describes a massive vector and nothing more can be anticipated by counting the degrees of freedom and observing that  $g$  carries the dimensions of mass (the Boulware–Gilbert model (BG) has a conserved current and a trace of gauge invariance). We realized, during the course of writing the GHK paper, that the BG model is essentially identical to the leading approximation we had developed for broken scalar electrodynamics.

In summary, by 1962 it was understood that the striking four dimensional calculations of quantum electrodynamics have a zero-mass photon because of the structure imposed by perturbative iteration of the free zero mass electrodynamic theory. That a massless photon is not mandated in general, was demonstrated by two counter examples. The two dimensional Schwinger model [22] (QED in two dimensions) demonstrated explicitly that gauge theories need not have zero mass, a result which was confirmed by the BG model in four dimensions. Of course, while interesting, both of these models are somewhat trivial and not of direct physical importance. It turns out that the reason finite mass is not a feature of just these models but is also realizable in physical, non-perturbative gauge theories can be understood in a general way within the framework presented in GHK. Looking back, all that was needed to describe the “Brout, Englert, Guralnik, Hagen, Kibble, Higgs” phenomenon” was available at Harvard in 1962.

I was very fortunate to have received an NSF postdoctoral fellowship, and like many of the other recipients at that time, wanted to use this as an opportunity to go to Europe. Not only was I very interested in gaining a new perspective on daily life and physics, but also was attracted by the fact that the high value of the dollar would make the stipend go very far compared to what it could do in the U.S. This fellowship could be used at any

institution that would welcome its holder. My first choice was CERN which seemed like a very interesting and exciting place. Fortunately, my request to visit with this fellowship was turned down by Van Hove, in what seemed a very rude manner. After some serious thought and discussions, I decided that the best place for me to go was Imperial College (IC). I was aware of the beautiful work done on renormalization by Paul Matthews and Abdus Salam and particularly on symmetry breaking by Salam (with Goldstone and Weinberg)[16]. Furthermore, this choice would involve no major language difficulties. Lowell Brown, a Schwinger student who was just returning from his NSF fellowship, partially spent at IC, thought this was a good choice and gave me valuable wisdom on the desirability of avoiding freezing of outdoor plumbing pipes and the necessity of keeping Alfa Romeos idling at all times during cold stretches. I had just ordered an Alfa for Italian delivery, as he had previously done. At the time, these were relatively cheap and fun sports cars, even though their lack of reliability was beyond anything most of us have ever experienced.

Fortunately, Paul Matthews sent me a friendly letter of invitation to IC in response to my request for a two year visit. Initially, I did not fully appreciate how very appropriate it was to choose IC. This choice turned out to be a major life altering decision. Indeed, my understanding of the history of IC, which would have served to further validate my decision, remained incomplete until recently, when Tom Kibble, after reading a draft of this note, gave me a detailed summary of the intellectual development at IC before my arrival which, when combined with our work, led to the unified electroweak theory. It is very interesting and, because of this, I include most of his note to me at this point. Some of his comments are a bit premature for my developing story, but any confusion should become resolved as the reader continues.

Tom wrote: “There had already been a great deal of discussion at IC of the possibility of symmetry breaking in gauge theories. Salam was convinced, from a very early stage, that the ultimate theory would prove to be a gauge theory. As I’m sure you know, his student Ronald Shaw developed the same model as Yang and Mills independently at the same time (1954), though it was never published except as a Cambridge University PhD thesis. And of course Walter Gilbert was his student too. Initially, the emphasis was on a theory of strong interactions, but that gradually changed and already in 1958 Salam and Ward published their first attempt at a unified gauge theory of weak interactions. There were several later versions, including the one you mention in 1964. It ran up against two major obstacles, of course — parity violation and the need to give the weak bosons a large mass — both obviously demanding some kind of symmetry breaking. Weinberg and Salam discussed this problem at great length when Steve was here on sabbatical, with each other and with me and others. There was of course a lot of discussion of approximate symmetries, in the context of strong interactions. But certainly for the weak interaction case, the nicest possible explanation was obviously spontaneous symmetry breaking. So it seemed clear that the big stumbling block was the Goldstone theorem, which is why they were so interested in studying its proof, to see if they could find any loopholes (they didn’t of course), leading ultimately to the 1962 paper with Goldstone.

So I would say that as a group we were very much primed to see this as a key problem. That was certainly why I was so interested when you first started discussing your ideas on the subject.”

When I started my NSF postdoctoral fellowship at Imperial College at the beginning

of 1964, I was certain that something interesting would happen with gauge theories and symmetry breaking. At IC, which, in retrospect, was arguably the best high energy theory place in the world at that time, I met a fantastic bunch of physicists. The ones I interacted with most were Tom Kibble, Ray Streater, John Charap, Paul Matthews, and Abdus Salam.

Even though I did not know the detailed history recounted by Kibble above, I expected no questions to the basic assumption of the possibility of spontaneous breaking of symmetry. The Goldstone-Salam-Weinberg paper [16] seemed to ensure that. Indeed, I naively could not even conceive that anyone could justifiably question that this could happen. I was soon to learn that, while Harvard was relatively safe ground protected by Schwinger's large (but indifferent) umbrella, the understanding of field theory in most of our community was much different and for the most part probably far less sophisticated. The idea that there was even such a thing as symmetry breaking in field theory was not universally accepted, even at IC. Ray Streater (who was, in the language of those times, an axiomatic or constructive field theorist) told me that his peers did not believe that symmetry breaking was possible. Streater and his community were certainly very sophisticated, but perhaps too much so. A lot of arguing, and my construction of the free model of symmetry breaking (examined earlier in this paper), convinced him that the axioms that led to disbelief in symmetry breaking were wrong. Well after these discussions, he published a very nice paper on this matter [23] which was largely responsible for convincing his more rigorous group of theorists that symmetry breaking was possible.

My discussions with Streater reinforced my belief in the power of the Goldstone theorem and that (combined with my obsessive belief that the photon was massless for reasons more basic than the smallness of the coupling constant) led me to write in April of 1964 a paper that was published in Physical Review Letters (PRL) [18]. This paper, the precursor to GHK, contains the simple free scalar example and a related argument for quantum electrodynamics. The electrodynamic construction has very useful content in relativistic gauges. However, this paper has a subtle error for interacting electromagnetism in the radiation gauge that led to our full understanding of symmetry breaking with gauge fields.

I must make it clear that neither Tom nor Dick were in any way responsible for the error because I was not wise enough to have discussed the paper with them until after I submitted it. I had, unintentionally, effectively compartmentalized my thesis work and discussions with Streater from my more or less ongoing conversations with Hagen and my new direct conversations with Tom. During frequent lunches, consisting of vile hard boiled eggs in crumb wraps, unspeakable other options, and dessert and almost everything else covered with a yellow custard sauce, we discussed physics in general and, in particular, the apparent failure of Goldstone's theorem in solid state physics. Although I was woefully ignorant of anything about this, it did not initially bother me much, because these models were non-relativistic, but Tom was sure it was important and succeeded in convincing me that this was the case. He was correct, but trying to understand in detail how this actually worked, after we had solved the relativistic case, held up the publication of the GHK paper by several months.

This compartmentalization fortunately ended within days after the paper [18] was sent to PRL. Refining my handling of gauges, quickly led to a complete understanding of the amazing structure of symmetry broken scalar quantum electrodynamics. Because of delays caused by the many postal strikes in Britain at the time and the peculiarities of IC's mail,

this paper did not get to PRL until June 1st. This was long after we knew it was wonderfully wrong, but I thought it would not be printed until after I received the proofs or, more likely, the unfavorable referee reports. I intended to modify it at that stage. I was traveling when the proofs arrived at Imperial College, and a very accommodating John Charap saw the returned paper in the mail, proofread it, and sent it back to PRL. I remain embarrassed that the paper was published, but yet there is much that is correct about it and the proper finishing of the analysis in it gives an elegant overview of symmetry breaking and the solution set of gauge theories. To my knowledge, only a handful of researchers, including Streater and a famous independent reviewer who thought the paper was of no value on general grounds, has ever referenced this work. Amusingly, Streater's elegant paper got a lot more attention.

## 6 General proof that the Goldstone theorem for gauge theories does not require physical massless particles

In this section, I will quantify much of the foregoing discussion. The realization that my paper [18] had an error (which was also caught by Dave Boulware) was in a rather amazing way the final key to our understanding that the assumptions of the Goldstone theorem are not necessarily valid in theories not showing manifest Lorentz invariance. It follows, as an exact statement, that symmetry breaking in a gauge theory, does not require physical massless particles. This is because these theories have valid representations in gauges such as the radiation gauge that are not manifestly covariant. In the following, I repeat the arguments of my PRL paper [18], but now correctly analyzed, to show what happens to the massless constraint of the Goldstone theorem in gauge theories. Here, as in GHK, I avoid similar but more complex arguments of QCD and confine the discussion to QED. For QCD, see [24] and for a comprehensive overall review see [25]. In QED, there is an asymmetric conserved tensor current

$$J^{\mu\nu} = F^{\mu\nu} - x^\nu J^\mu$$

which is easily shown to satisfy

$$\partial_\mu J^{\mu\nu} = 0.$$

Proceeding in the “usual” manner, it can be argued that the four charges given by

$$Q^\nu = \int d^3x [F^{0\nu} - x^\nu J^0]$$

are time independent, and that

$$\frac{dQ^\nu}{dt} = 0$$

is an immediate consequence of current conservation. However, the existence of a conserved charge depends on the assumption that the surface integral of the spatial current vanishes over a closed surface as that surface tends to infinity. In quantum field theory, this possibility is tested by evaluating matrix elements of the current commuted with other operators. It is

easily seen that, in a manifestly covariant theory, causality always ensures that the surface integrals over spatial currents vanish. Without manifest covariance, there is no such guarantee. While physical results evaluated in any gauge in QED must be fully consistent with special relativity, gauge dependent quantities have no such restriction. If a gauge that is not manifestly covariant is chosen for the vector potential, matrix elements involving  $A^\mu$  will reflect this lack of covariance. The radiation gauge  $\vec{\nabla} \cdot \vec{A} = 0$  is obviously not manifestly covariant, and yet it has many advantages. In particular, canonical quantization in the radiation gauge is straightforward and non-physical degrees of freedom are not required. Using standard radiation gauge commutation relations, the asymmetric current defined above satisfies:

$$\langle 0 | [Q^k, A^l(\vec{x}, t)] | 0 \rangle \neq 0.$$

If  $Q^k$  were time independent, we could conclude that the right hand side is simply a constant and that, by the Goldstone theorem,  $A^k$  excites a zero mass particle, namely the photon. In the case of no current (equivalent to  $e = 0$ ), this is true and there it is a trivial Goldstone theorem for free electromagnetism consistent with the photon being massless. However, direct calculation using spectral representations shows that this expression is time dependent for  $e \neq 0$ ! What went wrong? Exactly what was discussed above! To emphasize this very important point, we repeat that the radiation gauge is not explicitly Lorentz invariant, and we therefore cannot use causality to prove that the above commutator or any commutator involving gauge dependent quantities vanishes outside a finite region of space-time. This means that, even though  $\partial_0 J^{00} + \partial_k J^{0k} = 0$ , we cannot neglect surface integrals of  $J^{0k}$ . In other words, charge leaks out of any volume!

This leads us to consider the proof of Goldstone's theorem with consideration for currents other than the special one introduced above. What we have learned is applicable to any current, and it follows that Goldstone's theorem is true for a manifestly covariant theory, namely a theory where  $\partial_\mu J^\mu = 0$  and surface terms vanish sufficiently rapidly, so that

$$\langle 0 | \left[ \int d^3x (\partial_\mu J^\mu), (\text{local operator}) \right] | 0 \rangle = \frac{d}{dt} \langle 0 | \left[ \int d^3x J^0, (\text{local operator}) \right] | 0 \rangle.$$

Under these circumstances,

$$Q = \int d^3x J^0$$

has a massless particle in its spectrum. All the original proofs assumed manifest covariance, so there was never the possibility that the commutators involving an infinitely distant space-like surface could contribute. If the theory is not manifestly covariant, there is no guarantee that the “charge” is effectively time independent in all commutators. In particular, Goldstone's theorem need not and does not require physical massless states in any gauge theory. This is because these theories are made to be manifestly relativistic through the introduction of extra (i.e., gauge) degrees of freedom. Indeed, the Goldstone bosons are always non-physical. Consistent with this observation, when the Green's function  $\langle 0 | [Q^k, A^l(\vec{x}, t)] | 0 \rangle$  is re-gauged to the manifestly covariant Lorentz gauge, it becomes a non-zero constant, and the conditions of the Goldstone theorem are met. The resulting massless excitations are pure gauge.

Before moving on, it is important to make some additional observations. In the trivial case when  $j^\mu = 0$  in the above example, the Goldstone theorem is valid and the free electromagnetic field does indeed have zero mass associated with a legitimate Goldstone theorem. The free case is, however, fundamentally different from the interacting case, for which there is no Goldstone theorem (even though perturbation theory does yield a massless photon). Thus, despite the absence of a Goldstone theorem for physical particles, the photon maintains its masslessness, as argued by Schwinger, because of the smallness of the renormalized coupling constant. The perturbation solution is just one of a set of solutions possible for small coupling as a consequence of the non-linearity of the equations of motion. This is a manifestation of Dyson's argument [17], which shows that the perturbative solution is asymptotic when evaluated around vanishing coupling constant. (It is interesting to note that the exact solutions for zero dimensional models, such as the quartic one discussed above, confirm the asymptotic behavior deduced by Dyson.) If the Taylor expansion in  $e$  existed, the solution would be unique. The other, totally independent, solutions are associated with symmetry breaking, and must be singular in the  $e \rightarrow 0$  limit. Even though there is no Goldstone theorem for the electromagnetic interaction, the perturbative solution has a massless photon. It is necessary to verify that other solutions are not massless and actually are associated with massive vector particles. The GHK paper provides this verification by looking at the simple example of broken scalar electrodynamics.

## 7 Explicit symmetry-breaking solution of scalar QED in leading order, showing formation of a massive vector meson

We now construct the classical example of the failure of Goldstone's theorem by considering the action

$$\begin{aligned} L = & -\frac{1}{2} F^{\mu\nu} (\partial_\mu A_\nu - \partial_\nu A_\mu) + \frac{1}{4} F^{\mu\nu} F_{\mu\nu} + \phi^\mu \partial_\mu \phi + \\ & + \frac{1}{2} \phi^\mu \phi_\mu + i e_0 \phi^\mu \mathbf{q} \cdot \phi A_\mu \\ \mathbf{q} &= \sigma_2 \\ \phi &= (\phi_1, \phi_2) \\ \phi^\mu &= (\phi_1^\mu, \phi_2^\mu) . \end{aligned}$$

This is a useful way of writing scalar electrodynamics in terms of real fields. Observe that we have not added any explicit self interactions of the scalar boson fields or an explicit scalar bare mass. This is also the case in the GHK paper. This was done intentionally so as to put the emphasis on what happens to the vector particle. To leading order in the symmetry breaking approximation we study, additional scalar terms (such as a scalar mass term or quartic scalar interaction) added to this action have no effect on the vector field. In GHK, we wrote down the field equations that follow from the above action. As we were well aware, the cleanest way to proceed is to add sources to the action, then to write the Green's functions that follow from the field equations, and then define an iterative

approximation scheme. This is how I handled such problems in my thesis and associated papers, and indeed in notes building up to the GHK paper. (Note, that the expansion generated in this case is not equivalent to the large- $N$  expansion.) However, since we chose the leading order approximation to be a linearization of the operator equations, it was, in this case, reasonably simple to construct an internally consistent approximation without invoking the entire Green's function methodology. We felt this was the clearest and most efficient approach for a space-limited journal such as PRL.

In the next paragraphs, we outline the elements of the GHK calculation performed in the radiation gauge. Our scalar electrodynamics model has two physical field degrees of freedom associated with the electromagnetic field and two physical field degrees of freedom associated with the spinless boson field along with their corresponding conjugate momentum fields. We solve this theory in the symmetry breaking phase by imposing the symmetry breaking condition

$$ie_0 \mathbf{q} \langle 0 | \phi | 0 \rangle = \eta = \begin{pmatrix} \eta_1 \\ \eta_2 \end{pmatrix} .$$

The leading approximation is obtained by replacing  $ie_0 \phi^\mu \mathbf{q} \phi A_\mu$  in the Lagrangian by  $\phi^\mu \eta A_\mu$ . (The result is essentially the Boulware-Gilbert action [21] with an extra scalar field)

This “reduced Lagrangian” results in the linearized field equations:

$$\begin{aligned} F^{\mu\nu} &= \partial^\mu A^\nu - \partial^\nu A^\mu ; \\ \partial_\nu F^{\mu\nu} &= \phi^\mu \eta ; \\ \phi^\mu &= -\partial^\mu \phi - \eta A^\mu ; \\ \partial_\mu \phi^\mu &= 0 . \end{aligned}$$

These equations are soluble, since they are (rotated) free field equations. The diagonalized equations for the physical degrees of freedom are:

$$\begin{aligned} (-\partial^2 + \eta_1^2) \phi_1 &= 0 ; \\ -\partial^2 \phi_2 &= 0 ; \\ (-\partial^2 + \eta_1^2) A_k^T &= 0 . \end{aligned}$$

For convenience, we have made the assumption that  $\eta_1$  carries the full value of the vacuum expectation of the scalar field (proportional to the expectation value of  $\phi_2$ ). The superscript  $T$  denotes the transverse part. The two components of  $A_k^T$  and the one component of  $\phi_1$  form the three physical components of a massive spin-one field while  $\phi_2$  is a spin-zero field. As previously mentioned, the Goldstone theorem is not valid, so there is no resulting massless particle. If the Goldstone theorem were valid,  $\phi_1$  would be massless. It is very important to realize that it is an artifact of the lowest order approximation for the above action that  $\phi_2$  is massless. The excitation spectrum of this field is not constrained by any theorem. I emphasize that the Goldstone theorem naively (and incorrectly) appeared to constrain  $\phi_1$  to have a massless excitation but neither it or any other condition imposes a direct constraint

on the spectrum of  $\phi_2$ .  $\phi_2$  acquires mass if higher order corrections are calculated for the simple interaction above or in leading order if scalar self-interactions of  $\phi$  are added to our simple action. Of course, the conclusions about the invalidity of the Goldstone theorem are unchanged. An interesting example of this results from the addition of an explicit quadratic scalar bare mass term and a scalar quartic interaction term. In this case, in lowest order, the electromagnetic field has no effect on  $\phi_2$  and the equations satisfied by  $\phi_2$  and hence its leading order mass are identical to those determined by Goldstone in his original (purely scalar field) symmetry breaking paper [15]. The boson excited by  $\phi_2$  has come to be called the Higgs boson. The equations and hence the results that lead to the massive spin one particle in our original example are unchanged.

At this stage, it might be thought that we have written down an interesting, but possibly totally uncontrolled, approximation. There is no *a priori* reason to believe that this is even a meaningful approximation. The main result, that the massless spin-one field and the scalar field unite to form a spin-one massive excitation, could be negated by the next iteration of this approximation. However, this approximation meets an absolutely essential criterion that makes this unlikely. While the symmetry breaking removes full gauge invariance, current-conservation, which is the fundamental condition, is still respected. This is clear from the above linearized equations of motion. Further, we can directly demonstrate that the mechanism, described earlier in this note for the failure of the Goldstone theorem, applies in this approximation. As shown by the above equations, the linearized conserved operator current is  $J^\mu = \phi^\mu \eta$ . Using this, direct calculation leads to the result

$$\langle 0 | \int d^3x [ J^0(\vec{x}, t_1), \phi_2(\vec{y}, t_2) ] | 0 \rangle = \eta_1 e^{-i\eta_1(x^0 - t^0)} \\ \neq \eta_1$$

so that the charge varies in time and is therefore not conserved, in accord with our claim. This is a direct demonstration of the failure of the Goldstone theorem. Observe that the zero-time value of this commutator yields  $\eta_1$ , which is proportional to the vacuum expectation value of  $\phi_2$ , consistent with the equal time commutation relations. The internal consistency and the consistency with exact results gives this approximation credence as a leading order of an actual solution. As mentioned previously, it is, in fact, not hard to make this the leading order of a well defined approximation scheme.

Historically, this entire set of results existed in the spring of 1964. The only part of the published GHK argument that was missing was the detailed calculation of the time variation of the charge as shown in the above equation. It seemed clear that our arguments, as presented above, would explain why the Goldstone theorem could not be expected to be valid in condensed-matter, but we lacked confirmation through detailed calculation. Because of my early discussions with Kibble had often brought up this question, I felt that we had to do a condensed-matter calculation before publication.

I spent the summer of 1964 traveling through Europe (on \$5.00 a day) and stopped to visit my advisor Walter Gilbert in Italy, where he was giving biology lectures at a meeting at Lake Como. I explained to him how it all worked and how close we had been a year before. He had published a clever paper in PRL [27] earlier that year that still missed the point, but nevertheless came close to describing aspects of a mechanism to avoid massless particles. The work that I showed Gilbert was complete in every major way and identical to

that in the GHK paper, but for the beautiful consistency check of the charge oscillation in the leading order approximation that Hagen did in late summer.

We had no idea or suspicion that anyone else was working along related lines and felt absolutely no sense of urgency to publish our results even though I had been freely discussing them with anyone who would listen. From the reactions I got, I thought it unlikely that anyone else would ever be truly interested in our approach, let alone believe the results.

After I returned in late August, I made several visits to Oxford to talk to my old Harvard friend Bob Lange, to see if we could figure out how this worked in condensed matter. We did not succeed at that time but Lange figured it out later and published in PRL [28] in January 1965.

After giving up on the condensed matter problem, I conceded that it was time to publish. Hagen had arrived at IC for a visit thus making collaboration straightforward. We cleaned up the arguments and he did the charge oscillation calculation in the approximate model. This calculation confirmed the general argument and convinced us that our conclusions were ironclad. After he finished this and confirmed all the other results, I was confident that, this time, there were no blunders. I had learned over the years, that Dick was impressively accurate and rarely if ever missed an error.

Dick and I wrote up the paper and we gave it to Kibble for final scrutiny. As we were writing, he provided the essential insight and wisdom that insured that we had a coherent and logical argument. I certainly would have quit after my second stupid attempt to prove the photon massless but for Tom's depth of understanding of what was going on. Tom approved the paper.

## 8 Our initial reactions to the EB and H papers

Shortly thereafter, as we were literally placing the manuscript in the envelope to be sent to PRL, Kibble came into the office bearing two papers by Higgs and the one by Englert and Brout. These had just arrived in the then very slow and unreliable (because of strikes and the peculiarities of Imperial College) mail. We were very surprised and even amazed. We had no idea that there was any competing interest in the problem, particularly outside of the United States. Hagen and I quickly glanced at these papers and thought that, while they aimed at the same point, they did not form a serious challenge to our work.

Higg's Physics Letters paper [6] indicates that there might be an escape from the masslessness condition imposed by the Goldstone theorem, if calculations are done in the radiation gauge. However, we felt that an explicit quantum field theoretic example is needed to show that this had content. This follows from the result outlined above where I show the "failure" of a Goldstone theorem in pure unbroken radiation gauge electromagnetism. This happens through essentially the same mechanism as outlined by Higgs in his paper. Nevertheless, perturbative QED is characterized by a physical zero mass photon. In the end, to obtain the results necessary for the unified electroweak theory, it is necessary to show that there is no physical zero mass excitation. Higgs attempts to fill in this deficiency in his PRL paper [7], but does not revisit the radiation gauge and does not completely calculate the spectrum in this paper as discussed in more detail below.

We felt that Englert and Brout's work [5] was clearly related to GHK, but presented a

less than completely defined approximation. This paper (as well as the Higgs PRL paper discussed below) did not appear to fully recognize how very important it is to keep track of degrees of freedom. As a result, these authors did not provide, nor correctly comment on, the entire mass spectrum of their models. In particular, EB “assumed” that the Goldstone theorem is correct, which is true in their case, since they calculate in a covariant gauge. However, they demoted the corresponding massless excitation to a “pole at  $q = 0$  which is not isolated.” The correct leading order approximation reveals a distinct zero mass pole. This pole is the one required by the Goldstone theorem. From the detailed calculation is easily seen that is purely gauge in nature. That is to say, this pole does not contribute to any physically measurable quantity. EB does not do the calculation or make this observation. While EB does start with a two component scalar field, no comment is made on the spectrum associated with the component with non-vanishing expectation value. This component provides the “Higgs Boson.” To sum up, we felt that EB had found the dimensional parameter created by symmetry breaking and used it to make a massive boson. This, in itself, is only part of the problem. They did not provide a convincing argument to justify the correctness or consistency of their approximate and partial lowest order symmetry breaking solution.

Higgs’ more complete PRL paper [7] examines an approximation to broken scalar electrodynamics but does not pick a gauge. As I have pointed out, a consistent calculation in a covariant gauge must have a massless Goldstone boson and to establish if this excitation is a gauge particle or an actual particle takes analysis. In the radiation gauge, there is no Goldstone theorem, but the explicit absence of a physical massless modes must be confirmed by direct calculation. Higgs missed this entirely. He proceeded by writing down an approximation to the exact arbitrary gauge equations of motion. He then observes that these approximate equations of motion describe a massive vector particle and a scalar particle with mass determined in this approximation by the form of the initial scalar interaction. No statement is ever made about gauge. While the physical content of this coincides with the end result of GHK, much is taken for granted in reaching this conclusion, and it is not justified or developed in detailed solutions using covariant gauges. The whole analysis is classical. As it has been stated, the Goldstone theorem requires quantum mechanics and hence quantum mechanics is essential to describe accurately and fully the phenomena that occur. While Higgs deferred to Englert and Brout for quantum mechanics, as mentioned, we felt that this work was less than complete. The quantum structure and symmetry structure are determined by the exact equations. If the leading approximation is not consistent with these conditions then some additional “higher order” approximation must correct these inconsistencies. This means that any result from the so called leading approximation is likely to be negated by corrections. That is to say the approximation, if not completely consistent with the general requirements of the exact equations, can not be expected to predict actual physical results. Higgs did not subject his approximation to this test in that he failed to observe that his approximation is gauge sensitive. In this approximation a careful calculation in radiation gauge shows no massless particle while the calculation in a covariant gauge does have a massless particle to satisfy the Goldstone theorem but this particle is purely gauge in nature.

Neither EB or Higgs fully analyze the consistency of their approximations nor recognize that massless Goldstone Goldstone particles survive in covariant gauges but are unphysical gauge particles. This argument is a center piece of the GHK calculation. I quote my collab-

orator C.R. Hagen, “In a sense EB and H solved half of the problem — namely massifying the gauge particle. GHK solved an entire problem — massifying and also showing how the deadening hand of the Goldstone theorem is avoided.”

In summary, we felt that while these papers aimed in the correct direction, they did not form the basis for serious calculation. Because of the many discussions we had with those outside our collaboration, we knew that our work was going to be very controversial (i.e., generally regarded as just plain wrong), and the EB and H approaches even more so. Once there is a dimensional parameter from the symmetry breaking, it is easy to put in a mass almost anywhere you want it. It is quite another thing to show that you have a self consistent theory that could be the basis for an extension beyond the initial approximation. Although these were observations made in haste and with the arrogance and exuberance of youth, I still feel, particularly with the understanding and concerns of that time, that they were essentially correct.

At the same time, Kibble brought our attention to a paper by P.W. Anderson [26]. This paper points out that the theory of plasma oscillations is related to Schwinger’s analysis of the possibility of having relativistic gauge invariant theories without massless vector particles. It suggests the possibility that the Goldstone theorem could be negated through this mechanism and goes on to discuss “degenerate vacuum types of theories” as a way to give gauge fields mass and the necessity of demonstrating that the “necessary conservation laws can be maintained.” In general these comments are correct. However, as they stand, they are entirely without the analysis and verification needed to give them any credibility. These statements certainly did not show the calculational path to realize our theory and hence the unified electroweak theory. It certainly did not even suggest the existence of the boson now being searched for at Fermi lab and LHC. The actual verification that the same mechanism actually worked in non-relativistic condensed-matter theories as in relativistic QFT had to wait for the work of Lange [28], which was based on GHK. We did not change our paper to reference the Anderson work.

In any event, after seeing the competing EBH analyses, we unhesitatingly thought that we should do the completely honest thing and reference them, as they were clearly relevant with examples, even if not convincing to us. Our paper was finished and typed in final form when we saw these other works and made this decision. We only altered the manuscript by adding in several places references to these just-revealed papers. Not a single thought or calculation was removed or added, nor was any change made but to the referencing in our manuscript as the result of Kibble’s having pointed out the existence of these new papers. In retrospect, I wish we had added the true statement — “after this paper was completed, related work by EB and H was brought to our attention.”

We were naive enough to feel that these other articles offered no threat to our insights or to the crediting of our contribution. Nearly 45 years later, it is clear that we were very wrong. An unbiased reading of all the papers should make it clear that GHK is the result of an entirely independent train of thought. Over the decades, the awareness of the need to address what in 1964 were many worrisome points has vanished because of the acceptance of the end results and the general increase in our theoretical understanding. This, in turn, has affected the appreciation of the extremely significant differences in “correctness” and “completeness” of our work relative to the others. While we were too innocent in our slowness to publish and in the way the referencing was included, we never thought that this would in any way affect

our claim, since so many knew of the evolution of our work and my open discussion of it for nearly six months before we submitted it for publication. We were sure that a clear claim had been staked for us, even without the first-publication place holder. Over the short term this was correct, but, after decades, colleagues died and many seemed to forget. A major loss in this regard and many many other ways was Paul Matthews, who was aware of the detailed evolution of our work. Initially, there seemed to be no problem getting recognition for what we did on a more than equal basis to the EB and H papers. This seemed to change around 1999, when our work began to be omitted from the references contained in important talks and papers, even by authors who had previously referenced us.

## 9 Reactions and thoughts after the release of the GHK paper

What followed after the paper was sent out is quite interesting. While I spoke about this work informally to many people and in many places before the GHK paper was released, I also gave several seminars after its release. My presentations were greeted with fairly uniform disbelief. I was told in no uncertain terms that I did not understand electromagnetism or quantum field theory. In a community conditioned by coupling constant perturbation theory, it seemed that our work was nonsense. It is probably interesting to note that all of my talks were in Britain or Europe. After I moved to Rochester in the fall of 1965, I was never again asked to speak about GHK, not even at Brown, until the colloquium that I gave in 2001 at Washington University, on which this paper is based. Similarly, Hagen has never given a talk on our paper other than at Rochester, while I believe that Kibble spoke at several institutions.

Two particularly interesting talks were ones I gave at Edinburgh and at a conference outside of Munich. The work on symmetry breaking done by Higgs caught the eye of N. Kemmer, who was the professor of theoretical physics at Edinburgh. He wondered what his colleague was up to, and called Paul Matthews (who was Kemmer's student) at Imperial College. Paul, who was always very kind to me, told Kemmer that he should invite me to speak at Edinburgh, and see if that helped him make head or tails about what was going on. My wife and I visited Edinburgh on November 23, 1964. I gave a seminar and had a delightful time talking with colleagues and particularly Peter Higgs. In the evening, we had a pleasant dinner with him and his wife. I found Peter to be a very warm and friendly person. I recall thinking that his understanding of the topic of symmetry breaking was less extensive than ours, and I offered him my version of how everything fit together. He published much of that discussion (with acknowledgment) in his 1966 Phys.Rev. article [29].

In the summer of 1965, I gave a talk at a small conference outside of Munich, that was sponsored by Heisenberg [30]. He and the many other senior physicists at the conference thought these ideas were junk, and let me know with much enthusiasm that they felt that way. This evaluation, was made very clear to me by Heisenberg, who arguably had discovered spontaneous symmetry breaking in the first place. This contributed considerably to my fear that I could not survive in physics. Ken Wilson also spoke at this conference on his ideas of doing calculations on space time lattices. He also got beaten up rather badly. Hagen spoke

(twice) at the same conference but on different topics.

One redeeming aspect of this conference was that I got a demonstration ride in Julian Schwinger's factory-fresh Iso Rivolto (a beautiful quick machine that was powered by a Corvette engine). Julian remembered from my Harvard days that I loved cars and would be very interested in the wonderful machine on which he had spent a noticeable part of his Nobel prize money. The ride was made all the more interesting by Mrs. Teller attaching herself to us as we walked to the car. She sat in the front seat, thereby placing me in the "imagination seats" in the back. While I struggled to breathe, she told poor Julian that "in the US such expensive cars have automatic transmissions". This was uttered while he was in the midst of a stunning display of clutch work. Schwinger was kind enough not to say a word about my talk, even after the ride was over and Mrs. Teller had left.

As formidable as I found Mrs. Teller, her famous husband impressed and scared me even more. When I was sitting alone at a table in the hotel where the conference was held, Teller sat down and asked me to explain  $SU(6)$  classification schemes, which were popular at that time. Fortunately, although it was not my thing, I had a decent knowledge of the literature. Teller grilled me without mercy for what seemed like hours. When he was finally satisfied, he left. Despite my efforts to make myself invisible, he caught me again the next day and showed me a large number of calculations that he had performed. They were extensive in their coverage of the subject and, where I knew the results, absolutely correct.

My experiences in general at this conference, the first one at which I spoke, left me feeling depressed and more than a bit beat up and worried about my survival as a physicist. Fortunately for me, Hagen helped me get a postdoctoral job at Rochester, where Robert Marshak was the dominant force, as well as the head of the high energy theory group. Marshak, who was a commanding and wonderful presence, had a conversation with me after I had been at Rochester for about a year. Much of what I was doing involved symmetry breaking and was done with Dick. He told me I had to work on something else if I wanted to stay in physics. The job market was very tight (not a new thing!). I obeyed. I am still sure he was correct.

Years later, at the 2nd Shelter Island conference, he publicly apologized to me for stopping my work on symmetry breaking and "probably stopping him from getting a Nobel prize." There were many important people present, and I remain impressed by his decency and courage as well as his excessive faith in my abilities.

*"What about the unified theory? How did we miss it?"* Timidity, slowness and bad luck. After the GHK paper was published, John Charap and I, while sitting in his Ford Anglia in a rainstorm, had a discussion about the possibility of describing weak interactions unified with E&M through this mechanism. We thought it was possible, but the idea drifted away, largely through lack of action on my part. I dismissed the possibility of working on it because of other interests and because I was mostly receiving a less than warm response on GHK. Once again, I foolishly did not report my conversation with Charap to Dick or Tom. I did not focus seriously on this idea again until I went to Rochester. I started thinking about this discussion while at Rochester because of Marshak's intense interest in the weak interactions. This was clearly an interesting possibility. But because Hagen was away at the time, I kept my thoughts to myself and let them slip entirely after my "survival" discussion with Marshak.

Another bit of bad luck came about earlier at Imperial College in my interactions with

John Ward. Around the same time that we were working on symmetry breaking with gauge fields, Salam and Ward were working on a precursor to the Weinberg-Salam model. They were rather secretive about this, but one day a case of champagne appeared at the Imperial College physics department. I was told this was in anticipation of the prize they were going to get for their current work. Shortly after this, Ward and I went to a pub together for lunch. I started to tell him about our work on symmetry breaking but did not get far before he stopped me. He proceeded to give me a lecture on how I should not be so free with my ideas because they would be stolen and often published before I had a chance to finish working on them. Needless to say, I did not ask him about his work with Salam! If he had only listened, the two of us had enough information to have had a good chance to solve the unification problem on the spot. Of course, I could have read their papers after they came out, but I did not do this. Ironically, later Tom explained the details of our work to Salam who used this information to complete his unification model. This was told to me by Tom and is verified in the body of Salam's Nobel acceptance speech. To sum it up, I was simply not paying attention to all the signals coming to me. In hindsight, they were clear.

While I was at Rochester, I got several calls from my Harvard classmate and fellow Gilbert student Marty Halpern, who was and still is at Berkeley. He asked me many questions about our paper and told me that he would be passing on the contents of our conversation "to Steve." I would like to think that this helped Weinberg put it all together for his brilliant paper [31], but I have no idea if any of the conversations were actually passed on. I had already stopped thinking about symmetry breaking because of Marshak's warning.

In retrospect, my work with symmetry breaking was really fun and exciting. As mentioned at the beginning, I made new friendships, particularly with Tom and his, very sadly deceased, wonderful wife Anne. Dick and Tom (and others, particularly at IC) taught me much about how to think and how to be a practicing physicist. I made many errors of judgment and certainly errors in physics. Facing up to the possibility of errors and particularly career-damaging ones was hard for me, and surely made me be more conservative than I should have been. At this stage, the ideas seem very simple and natural. At the time they were not.

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