A Physics History of My part in the Theory of Spontaneous Symmetry Breaking and Gauge particles

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What was frontline theoretical particle physics like in the early 60's?"

To set the following in perspective, it is helpful to review what high energy physics was like in these ancient times. While not as commonly used as now many " modern" tools like the Feynman path integral and the now nearly forgotten Schwinger Action Principle were available. The common starting point for any theoretical discussion was usually through perturbation theory or "S matrix theory".

The power of Group Theory beyond SU(2) for High energy theory was (then through flavor SU(3)) was just beginning to be appreciated. The Ω^- needed to fill in the baryon decuplet (10 particles) was found in 1963, The Gell-Mann Zweig quark (ace) ideas existed but were far from completely accepted. There was no experimental evidence and the color ideas that allowed three quarks to make a nucleon only began to take form in 1964 " O. W. Greenberg, Phys. Rev. Lett. 13, 598 (1964)" Calculation methods were primitive. For the most part, coupling constant perturbation was the only tool available to try to get quantitative answers from quantum field theory. S Matrix theory was king. Current algebra, a mixture of symmetry and some dynamics was in the wings (Nambu & Lurie 1961).

Nambu launched the study of spontaneous symmetry breaking of an internal group through his work on the BCS model (1960) and the Nambu, Jona-Lasinio model [] with interaction

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

This interaction in itself was disturbing (as was the four fermion interaction to describe weak processes) because perturbation theory in g produces a series of increasingly divergent terms that cannot be renormalized. Nambu-JL studied this model by imposing a constraint that seemed to be inconsistent with its symmetry and then formulated a new (not coupling constant perturbation theory) leading order approximation. Their results showed that there was a zero mass composite particle excited by $(\bar{\psi}\psi)^2$ This is now called a Goldstone boson.

We can get insight to help understand the ideas of spontaneous symmetry breaking in quantum field theory by starting with simple differential equations. Consider

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

- *"How many solutions?"*
  Three! They can be found from a integral representation (zero dimensional Feynman path integral):

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

  To satisfy the differential equation, this integral must be evaluated over paths which do not contribute at the end points. It is easy to show that there are 3 allowed independent paths in the complex plane.

- Use the definition

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

- It follows that y satisfies the original differential equation. Note: Associated with the three integration paths, the integral has 3 stationary points that correspond to the three solutions of the original differential equation.
- These are $(J = 0)$ located at

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

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

For the path along the real axis (corresponding to the stationary point at $\phi = 0$) it follows that:

- $y(J)$ is an odd function of J

  $$y(J) = a_1 J + a_3 J^3 + \cdots$$

  i.e. $y(0) = 0$: This is the familiar solution found by perturbation expansion around $g = 0$.
- For the other paths, y is a mix of odd and even powers of J. Note: only the solution that vanishes at $J = 0$ is regular in g at $g = 0$.
- The other solutions are of the "symmetry breaking type", because

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

  vanishes if the boundary conditions are chosen in a symmetric manner so as to respect the even nature of the exponent.
- Back to Quantum Field Theory:
  QFT is described by operator fields, which obey the rules of Quantum Mechanics, and a complete set of states.

- In particular, $\exists$ a state of *lowest* energy: the vacuum, $|0\rangle$;
- Simple Example: Free four dimensional Relativistic Scalar Field Theory,

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

$$\Rightarrow (-\partial^2 - m^2)\,\phi = 0 \ ,$$

- $|0\rangle$ does not depend on space-time parameters,

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

- The statement that (P^μ is the energy-momentum operator)

$$P^\mu \ |0\rangle = 0 \ ,$$

i.e., the vacuum has no *energy* or *momentum* leads to

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

i.e., uniformity under translation

$$\Rightarrow \ m^2\,n = 0$$

- This means that either

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

- This is a trivial example of Goldstone's (Nambu's) theorem (Nambu, Jona-Lasinio, Goldstone [1961]; Goldstone, Salam, Weinberg [1962]),
- Roughly: If a *charge* associated with a *conserved current* in a relativistic field theory does not *destroy* the vacuum ⇒ the theory has zero mass excitations.
- *"What is the current here?"*

$$J^\mu(x) = \partial^\mu \phi(x)$$
$$\partial_\mu J^\mu(x) = \partial^2 \phi(x) = -m^2 \phi$$
$$\therefore \text{ if } m^2 = 0 \Rightarrow \partial_\mu J^\mu(x) = 0 \; .$$

The charge is

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

- This charge (which is actually only defined when appearing in commutation relations as follows)does not *destroy* the vacuum: From the canonical commutation relations:

$$i \left[\partial^0 \phi(x), \phi(y) \right] \Big|_{x^0=y^0} = \delta^{(3)}(x-y)$$

$$\Rightarrow \; i\,[\,Q, \phi(y)\,] = 1$$
$$\therefore \quad i\,\langle 0\,|[\,Q, \phi(y)\,]|\,0\rangle = 1\ .$$

  Note that

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

- In general: Goldstone's Theorem can be *shown* to be true using exact results of QFT without use of *any* perturbation techniques.
- *"What is Goldstone's Theorem good for?"*
  Very few massless particles: photon is the most familiar one.

- This is where I come in: After Bjorken gave a talk (1962) at Harvard, my thesis advisor, Walter Gilbert (Nobel Laureate Chemistry 1980), suggested that I look at Bjorken's proposed model of E&M — a variant of the Nambu- Jona-Lasinio model with interaction

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

- This seemed to *smell* because the symmetry that is broken is Lorentz symmetry -relativistic invariance.
- Turns out that Bjorken did it *wrong*, but with some modification his conclusion that this theory is equivalent to QED is correct.
- This is a surprise if you only know *coupling constant perturbation theory*, because this interaction is well known to be hopelessly divergent using this approach.

- The Bjorken model result can be regarded as a (messy) *re-summation* of perturbation theory (as can the Nambu, Jona Lasinio model), but what you are really doing is calculating in a different phase that corresponds to an alternate solution to the functional differential equations of QFT, corresponding to symmetry breaking boundary conditions. This is directly related to the multiple solutions of the ordinary differential equation previously discussed.
- These boundary conditions are

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

  and, since

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

  where $\mathfrak{J}^{\mu\,\nu}$ generates LT's,
- This requires that

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

- $\therefore$ the symmetry of the vacuum is broken, and $j^{\mu}\,|0\rangle$ contains a zero mass (spin 1) particle — a photon.

- Despite the fact that Schwinger had argued by that time that there was no dynamical reason for the photon to have zero mass, I thought from the arguments made for the Bjorken model that I could construct a symmetry breaking argument that would require massless photons in conventional E&M. This argument was wrong and, fortunately, Coleman detected this in my (1963) 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.
- I was being particularly dense, because somewhat before my thesis was finished I was (weakly) involved in another project with Gilbert. He made the observation that the free action with a massless *scalar* particle (B) and a massless *vector* particle (A^λ) with the simple "interaction"

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

produces a free spin 1 field with mass g^2.

- This can be *anticipated* by *counting* degrees of freedom and noting that g carries the dimension of mass (this model has a conserved current and a trace of gauge invariance).
- I spoke to Dave Boulware about this and he spoke to Gilbert and wrote a paper on this (Boulware, Gilbert; 1962).
- Thus, at this time, the 2-dimensional Schwinger model (E&M in 2 dimensions) showed that gauge theories need not have zero mass and the BG model in 4d *confirmed* this again.
- Indeed, it is a *trivial* step from the BG model to the lowest approximation Higgs model. With any insight, all that was needed to describe the "Higgs phenomena" was available at Harvard in 1962.

- I went off to Imperial College at the beginning of 1964 with a new NSF postdoctoral fellowship and the certainty that something interesting happened with gauge theories and symmetry breaking.
- At IC, which in retrospect was probably the best High Energy Theory place in the world at that time, I met a fantastic bunch of physicists. The ones I interacted with the most were Tom Kibble, Ray, Streater, John Charap, Paul Matthews and Abdus Salam. I also learned that while Harvard was relatively safe ground, protected by Schwinger's large (but indifferent) umbrella, the idea that there was even such a thing as symmetry breaking in field theory was not universally accepted - even at IC where Salam (with Goldstone and Weinberg) had already published a nice paper on these ideas. For example, Ray Streater (an axiomatic or constructive field theorist) stated that his community did not believe that symmetry breaking was possible.
- A lot of arguing and my careful construction of the free model (earlier in this talk) eventually *convinced* him that the axioms were wrong. He published a paper on this, which amusingly got a lot more attention than the earlier paper I wrote giving the simple free example.

- Meanwhile, I, over frequent lunch talks with Kibble (vile, hard boiled eggs in crumb wraps, unspeakable other options, and desert — and almost everything else — covered with a yellow custard sauce), discussed the apparent failure of Goldstone's Theorem in solid state physics. It did not bother me much, because these models were non-relativistic.
- Still obsessed, by the need to prove the photon massless, I wrote - in March - a paper that was terribly wrong. (Apparently Coleman's lesson had not sunk in). Because of delays in typing and one of many postal strikes in Britain, this paper did not get to PRL until June 1st. This was long after I knew it was wrong but I thought it would not be printed until proofs were approved. I was traveling and a very nice John Charap saw it in the mail, proof read it, and sent it back to PRL.
- The understanding of the error in this paper (which, incidently, was also caught by Dave Boulware) was the final key to understanding, within the context of the Goldstone theorem and without resorting to perturbation theory, why symmetry breaking in a gauge theory, does not require massless particles. The conditions of the Goldstone theorem are easily violated, or equivalently, are only applicable to non-physical excitations.

- The proof in QED is straightforward: There is an asymmetric conserved tensor current

$$\begin{aligned} J^{\mu\,\nu} &= F^{\mu\,\nu} - x^{\nu}\,J^{\mu} \\ \partial_\mu\,J^{\mu\,\nu} &= 0 \\ \Rightarrow\;\; Q^{\nu} &= \int d^3x\left[F^{0\,\nu} - x^{\nu}\,J^{0}\right] \\ \text{and (?)}\;\; \frac{d}{dt}Q^{\nu} &= 0\,. \end{aligned}$$

- We use the gauge $\vec{\nabla}\cdot\vec{A} = 0$ (one of the few gauges allowed in operator QED) so that we only deal with physical excitations.
- By the commutation relations it is easily seen that this requires

$$\left\langle 0\left|\left[\,Q^{k}, A^{l}(\vec{x}, t)\,\right]\right|0\right\rangle = (\text{non-zero constant})\,.$$

- However, direct calculations using spectral representations show that this expression is time-dependent for $e \neq 0$!
- *"What went wrong?"*

  The radiation gauge is not explicitly Lorentz invariant, and we can not use *causality* arguments to prove that the *commutator* above is *confined* to a local region of spacetime.
- This means that, even though $\partial_0 J^0 + \partial_k J^{0\,k} = 0$, we can not neglect surface integrals of $J^{0\,k}$. It means: Charge leaks out of any volume!

- This leads us, at once, to consider the proof of Goldstone's theorem.
- *"What have we learned?"*

  Goldstone's theorem is true for a manifestly covariant theory, i.e., a theory where $\partial_\mu J^\mu = 0$ and *surface terms* vanish *fast enough* so that

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

- That is to say
$$Q = \int d^3x \, J^0$$
has a zero mass particle in its spectrum.
- This includes electromagnetism with the weird charge above if you re-gauge to a manifestly covariant gauge
- However, in this case, you can demonstrate exactly that the zero mass particles are gauge excitations.
- Note that these are very general statements: Goldstone's theorem need not require physical zero mass states in any gauge theory (and it does not).
- This is because these theories are made to be relativistic by introducing extra gauge degrees of freedom.
- Indeed, the Goldstone bosons are always non-physical.

- There is no reason for the photon to be massless in normal QED, but the smallness of the coupling constant and hence the applicability of perturbation theory.
- We can see an approximate example of the failure of Goldstone's theorem by looking at the action

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

- This is a fancy way of writing scalar electrodynamics.
- Now, if we replace $i\, e_0\, \mathbf{q}\, \phi$ by the constant matrix $\mathbf{n} = (n_1, 0)$, this action becomes the Boulware-Gilbert action with an extra free massless field ϕ_2.
- Consequently, the action in this approximation describes a 3-degree of freedom spin 1 particle and a 1-degree of freedom spin 0 particle.

- Goldstone's theorem does not apply, even though the current

$$J^{\mu} = i\, e_0\, \phi^{\mu}\, \mathbf{q}\, \phi = \phi^{\mu} \cdot \mathbf{n}$$

is conserved.

- It is easy to show that

$$\left\langle 0 \left| \int d^3x \left[J^0(\vec{x}, t_1), \phi(\vec{y}, t_2) \right] \right| 0 \right\rangle = n_1\, e^{-i\, n_1\, (x^0 - t^0)} \\ \neq n_1$$

so that the charge varies in time and the vacuum does not have a definite charge.

- However, and this is the point: this is a perturbative argument! In fact, we have not even defined the perturbation method beyond the leading term! NOTE: we have extended the definition of perturbation theory to include any set of nested approximations to an exact solution. We have not confined the definition to an expansion in an obvious parameter. Indeed, the solutions that arise here show (in this leading order) singularities as the electromagnetic coupling vanishes.
- We have no proof that these results reflect a correct solution of the theory.
- The early history of QFT is filled with failures because Lorentz symmetry was not treated properly. This result could represent as similar failure because

we have violated the symmetry of the dynamics without justifying that this is possible in an exact answer. The only thing that gives this credence as a possible general solution is the earlier exact analysis.

- Historically, this entire set of results existed in the spring of 1963.
- The only parts of the argument that were missing were the additional consistency proof by the straightforward evaluation of an integral that the charge of the vacuum does not vanish and (I thought as a result of my many discussions with Kibble) the important detailed calculation confirming that the above arguments also explain the failure of the theorem in the Condensed Matter cases.
- 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.
- I had, unwisely, decided not to publish until after I spoken to many people freely about my result. I did not think anyone else would ever be interested in the problem.
- We did not solve the condensed matter problem, although Lange worked it out later and published in PRL in January of 1965.

- 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 pleasant resort on Lake Como.
- I wanted to talk to him to show him how it all worked and how close we had been years before. He had published a paper in PRL earlier in the year, which was correct but that, again, narrowly missed the point. However, the arguments were exact and his example included the case actually relevant to the Goldstone bosons of gauge theories being gauge excitations.
- When I returned to IC in the fall, I decided it was time to publish.
- My old friend Richard Hagen had arrived for a visit and we cleaned up the arguments and he did the charge oscillation calculation. This calculation confirms the general argument and thus convinced me that our conclusions were iron-clad. After he finished this and confirmed all the other results, I was confident that this time there were no errors. I had worked with Hagen solving problems as an undergraduate and as a graduate student and had even written my first paper with him. I had learned that he was impressively accurate.
- We wrote up the paper and showed it to Kibble, who had offered continuous encouragement and often provided essential insight and wisdom. I certainly would have quit after my second stupid attempt to prove the photon massless but for Kibble.

- After we showed him the paper and asked for any additional input, he told us that some papers by Brout and Englert and Higgs had just showed up.
- Hagen and I quickly glanced at these and thought that, while they addressed the point, they were not a serious challenge to our work. I felt that Brout and Englert's work was the result of an uncontrolled approximation and that Higg's certainly did not offer the integrated insight that we had developed.
- These were observations made in haste and with the exuberance of youth. In retrospect, while I still feel that our work is more complete and tightly argued, these other papers certainly pointed to the correct conclusion and that the details of how to do this have become all but irrelevant in a physics community that now can accept the wonderful range of solutions to QFT that this work opened. These ideas are now part of our common lore.
- In any event, I, without hesitation, did the completely honest thing and added to the text in a couple of places so as to refer to these papers. Not a single thought or calculation was removed or added nor was any change but to the referencing made in our paper as the result of Kibble pointing out the existence of these new works. In retrospect, I wish we had added the true statement - "after this work was finished, it was brought to our attention that related work by – etc"

- At the same time, Kibble brought our attention to a paper by P.W. Anderson. I did not think Anderson really has the full understanding either, but to this day I can not follow his paper unless I add a lot of my own thoughts.
- What followed after is quite interesting: While I talked about this work informally in many places, before the actual paper was released, I also gave many 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 physics world conditioned by coupling constant perturbation theory, it seemed that I was speaking nonsense.
- This symmetry breaking work done by Higgs caught the eye of N. Kemmer at Edinburgh. He wondered what his colleague was up to and called Paul Matthews 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 over what was going on. I went there with my wife and had a delightful time talking to people and particularly talking to Peter Higgs and dining with him and his wife. I found

him to be a very nice person. I felt his understanding was less than complete on what was going on with symmetry breaking and I explained in detail how it worked. He published much of this explanation (with acknowledgment) in his Phys.Rev.145:1156-1163,1966 paper.

- In the summer of 1965 I gave a talk at a small conference outside of Munich, sponsored by Heisenberg.
- He and the other famous people at the conference thought these ideas were junk and let me know clearly that they felt that way. This evaluation, coming from Heisenberg who really had discovered spontaneous symmetry breaking in the first place contributed considerably to my fear that I could not survive in physics.
- Ken Wilson also spoke on this conference on his ideas of doing calculations on latticised space time. He also got beat up rather badly.
- One redeeming aspect of this conference was that I got a demonstration ride in Julian Schwinger's factory fresh Iso Rivolto (Corvette powered). 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 Noble prize money. The ride was made all the more interesting by Mrs Teller attaching herself, occupying the front seat, and telling poor Julian that "in the US such expensive cars have automatic transmissions". This was done while he was doing a stunning display of clutch work.
- Schwinger was kind enough to not say a word about my talk.

- My experiences in general left me feeling a bit beat up and worried about survival as a physicist at the end of my NSF fellowship. Fortunately for me, my old friend Hagen helped me get a job at Rochester, then run by Marshak.
- Marshak, who was a commanding and wonderful presence (I thought), listened to what I was doing — which was, in retrospect, leading to what become the unified model of weak and electromagnetic interactions — and told me I had to work on something else if I wanted to stay in physics (the job market was very tight: this is not a new thing!).
- Since he was an expert on weak interactions and the job market, I obeyed. I am still sure he was correct.
- Years later, he publicly apologized to me for stopping my work on symmetry breaking and "probably stopping him from getting a Noble prize" at the 3rd Shelter Island conference. There were many important people present and I remain impressed by his decency and courage as well in his excessive faith in my ability.

- *"What about the unified theory? How did we miss it?"*
  Stupidity, slowness and bad luck.
  Shortly after our paper was sent out, John Charap and I were sitting in his Ford Anglia discussing the possibility of describing weak interactions unified with E&M through this mechanism. We thought it was fairly clear on how to do it, but we ended up dismissing the possibility of working on it because I had received such a hostile response. I did not seriously think about it again until I went to Rochester.
- Another bit of bad luck came about because of my interaction 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 champaign appeared at the Imperial College physics department. I was told this was in anticipation for 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 free with

my unpublished 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 only he had 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.

- While I was at Rochester I got several calls from my Harvard classmate Marty Halperin who was already at Berkeley. He asked me many questions about our paper and told me he was 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, 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.
- The status now:
  Interest is strong again because the "Higgs" boson was possibly observed last year at an energy of 115 Gev/c^2 at LEP.

- *“What is the ’Higgs’ ?”*
  It should really be called the Goldstone. It is the particle that remained at zero mass in our toy model. Putting in an interaction of the form $-g\,\phi^2\,\phi^2$ leaves all else unchanged but gives a mass to the preciously massless spectator particle.
- In retrospect, my work with symmetry breaking was really fun and exciting. I made many errors of judgment and was certainly often accused of errors in physics. Facing up to that possibility was hard and 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.

Citations

Y. Nambu and G. Jona-Lasinio, Phys. Rev. **122**, 345 (1961).

fill in the rest