- *"What was front-line theoretical particle physics like in the early 60's?"*
  Many "modern" tools were available in some form.
- Schwinger Action Principle.
- Feynman Path Integral.
- Group Theory — flavor SU(3). 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.
- Calculation methods were primitive: Mostly coupling constant perturbation theory applied to 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 included a zero mass (now called Goldstone) boson.

- Little was understood beyond coupling constant perturbation theory about how to actually solve a QFT. Attempts had been made to re-sum these series but there was no realization that one should be looking for an entirely different solution.
- The 1952 work of Dyson showing that there is a singularity at zero coupling in QED was an early indicator that perturbation theory was not the whole story and indeed that the perturbation expansion is asymptotic.
- We can get insight to help understand the ideas of the solution space of QFT and also spontaneous symmetry breaking in by examining simple differential equations (zero dimensional QFT). Consider,

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

- *“How many solutions?”*
  Three! They can be found from an integral representation (zero dimensional Feynman path integral for quartic interacting scalar field theory):

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

  The J derivative of Z satisfies the original differential equation when the integral is evaluated over COMPLEX 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.
- 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 stationary phase points to discover asymptotic expressions for each of the three solutions. The path along the real axis corresponds to the stationary point at $\phi = 0$ and is the familiar solution found by perturbation expansion around $g = 0$.
- The perturbative solution vanishes at $J = 0$ is regular in g at $g = 0$.
- The other solutions break reflection symmetry and are singular at $g = 0$

- The Goldstone Theorem - 4 spacetime dimensions:
- 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.
- Goldstone's Theorem can be *shown* to be true using exact results of QFT without use of *any* perturbation techniques in am manifestly covariant theory.
- Generally, in an interacting theory, you can not break a symmetry in using a coupling constant based perturbative expansion.
- The symmetry breaking solutions show singularities as the coupling vanishes.
- *"What is Goldstone's Theorem good for?"*
- Only the photon is massless.

- 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

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

- The current is required to have non-vanishing vacuum expectation.
- The symmetry that is broken is Lorentz symmetry — relativistic invariance.

- I showed that BJ's basic conclusion that this theory is equivalent to QED is correct. Careful calculation shows that the Lorentz symmetry breaking is trivial and does not manifest itself in a physically observable way.
- This is a surprise if you only know *coupling constant perturbation theory*.
- This interaction is then hopelessly divergent
- The Bjorken model result can be formally regarded as a (messy) *re-summation* of unrenormalized perturbation theory (as can the Nambu, Jona Lasinio model).
- This is very misleading!
- In fact this model is a calculation in a different phase corresponding to symmetry breaking boundary conditions.

- This is the direct analogue to the multiple solutions of the ordinary differential equation previously discussed.
- 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.
- I removed the offending chapter in the final version.

- Somewhat before my thesis was finished I had discussed a related project with Gilbert. He made the observation that the action of a massless *scalar* particle (B) and a massless *vector* particle (A^λ) with the simple "interaction"

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

  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 told David Boulware about this and he spoke to Gilbert and they 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 4 dimensions *confirmed* this again.
- It is a *trivial* step from the BG model to the lowest approximation used in the GHK paper. They are essentially the same! With hindsight, all the ingredients for the GHK paper were available at Harvard in 1962!

- I went 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*.
- IC was probably the best High Energy Theory place in the world at that time and I met a fantastic bunch of physicists there. The ones I interacted with the most were Tom Kibble, Ray, Streater, John Charap, and to a lesser degree Paul Matthews and Abdus Salam.
- I also learned that while Harvard was relatively safe field theory 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.

- 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 careful construction of a free model *convinced* him that the axioms were too restrictive. He published a paper on this, which amusingly got a lot more attention than the paper I wrote giving the simple free example and a major error.

- This terribly wrong paper was driven by my obsessive need to prove the photon massless.
- Apparently Coleman's lesson had not sunk in!
- The understanding of the error in this paper (which, incidentally, 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.

- I begin by discussing the error in my earlier PRL paper. As I proceed, it will be clear why my explanation is the basis of the GHK paper.
- The proof in QED is straightforward: There is an asymmetric conserved tensor current,

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

- We use the gauge $\vec{\nabla} \cdot \vec{A} = 0$ (a very natural gauge in operator QED) so that we only deal with physical excitations.

- By the commutation relations it is easily seen that this requires

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

- However, direct calculation 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 cannot use *causality* arguments to prove that the *commutator* above is *confined* to a local 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}. It follows that our weird 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

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

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

- That is to say
$$Q = \int d^3x J^0$$
has a zero mass particle in its spectrum.
- This includes electromagnetism with the special charge introduced 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

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

$$
\begin{aligned}
\mathbf{q} &= \sigma_2 \\
\phi &= (\phi_1, \phi_2) \\
\phi_\mu &= (\phi_1^\mu, \phi_2^\mu)
\end{aligned}
$$

- This is the Lagrangian for scalar electrodynamics. It is a very non-trivial interacting theory characterized by a conserved current. It is renormalizable in the coupling constant expansion with an induced ϕ^4 interaction. No other non-trivial ϕ^n interaction can be added to it and keep it renormalizable.
- We want to look for solutions other than the coupling constant expansion. At the time, it was very natural for us to put in a source for ϕ and order an iterative expansion by the number of derivatives with respect to the source. This generates a loop expansion. A variant of this method was used in my thesis to study the Bjorken model.

- The leading approximation is obtained by replacing $i\, e_0\, \phi^\mu\, \mathbf{q}\, \phi\, A_\mu$ in the Lagrangian by $\phi^\mu\, \eta\, A_\mu$. (The result is essentially the Boulware-Gilbert action 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 η_1 carries the full value of the vacuum expectation of the scalar field (proportional to the expectation value of ϕ_2). The superscript ^{T} denotes the transverse part. The two components of A_k^T and the one component of ϕ_1 form the three physical components of a massive spin-one field while ϕ_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, ϕ_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 ϕ_2 is massless. The excitation spectrum of this field is not constrained by any theorem.

- 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.
- We can directly demonstrate that the mechanism, described earlier in this note for the failure of the Goldstone theorem, applies in this approximation.

- The internal consistency and the consistency with exact results gives this approximation credence as a leading order of an actual solution. It is, in fact, not hard to make this the leading order of a well defined approximation scheme.
- This solution of the action 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

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

is conserved.

- The reception of the GHK work is quite interesting: While I talked about the work informally in several places and particularly Oxford before the actual paper was released, I also gave several seminars after its release.
- I was invited to give a talk at Edinburgh almost immediately and met Peter Higgs who I found to be a pleasant and friendly person.
- My presentations were greeted with fairly uniform disbelief.

- In the summer of 1965 I gave a talk on GHK at a small conference outside of Munich, sponsored by Heisenberg.
- Heisenberg and other famous people at the conference thought these ideas were junk and made it clear that they felt that way.
- Hagen also attended, but he talked on other topics.
- Schwinger did not say a word about my talk.
- BUT - One redeeming aspect of this conference was that I got a demonstration ride in Julian Schwinger's factory fresh Iso Rivolta (Corvette powered).

- Fortunately, Dick had helped me get a job at Rochester. Rochester's high energy theory group was, as was often the case then, under the control of one senior physicist, Bob Marshak.
- Marshak, was a commanding and wonderful presence. He and George Sudarshan were the originators of the $V - A$ theory of weak interactions which was another crucial cog in the development of the unified theory of Weak Forces and Electromagnetism.
- After a few months at Rochester, Marshak called me into his office and told me that working on spontaneous symmetry breaking problems was not wise. He told me that I should work on something else if I wanted to stay in physics
- The job market was very tight: this is not a new thing!
- I obeyed. I am still sure he was correct.

- Years later, Marshak publicly apologized to me for stopping my work on symmetry breaking at the second 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.
- Several months after this discussion, I was invited to give a talk at Brown University. I spoke on the electromagnetic mass splitting of pions.
- Ironically, this talk was based on work done based on current algebra calculations by Steve Weinberg and it led to Brown offering me a job and an early tenure promise.
- This is the first time that I have given a talk on this work at Brown and only the third time that I talked on this in the United States.

- *“What about the unified theory? How did we miss it?”*
- 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, in part, 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 could be stolen and 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 Halpern 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.