Weinberg and the Role and Meaning of Symmetries in Physics

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The topic that the organizers have suggested is quite broad as there were many facets to Steve Weinberg's contribution to understanding the role of symmetries in physics. I tried to make a list of highlights:

- Early interest in spontaneously broken global symmetry; proof of Goldstone's theorem, with Goldstone and Salam (1962)
- Low energy theorems for soft photons and gravitons; relation of gauge invariance and general covariance to non-decoupling at zero momentum (1964-5)
- Current algebra and effective field theory; spontaneously broken global symmetry in the hadronic world (1965-7)
- Spontaneously broken gauge symmetry and the standard model (1967 and after)
- Approximate global symmetry as an accidental consequence of gauge symmetry (late 1970's)
- Effective field theory as the central truth of modern physics (1980's and after).

My most vivid personal collections: learning about current algebra and effective field theory from Weinberg.

Coming to Harvard as a postdoc - fall of 1976.

Weekly family meetings

Weinberg's little lectures on current algebra – how I personally learned about soft pions and effective field theory in hadronic physics.

I spent part of the weekend rereading some of Steve's most classic papers on the role and meaning of symmetries.

A strange experience: so often one's reaction is "but everyone knows that." Then you pinch yourself and realize that this paper is why everyone knows that.

One option for this talk: organize it around highlights of some of the classic papers.

But many of them are very well-known, and some of the most important are figuring in other lectures this afternoon.

I decided it would be more fun to organize the talk around some of the lectures and articles in which Steve explained the evolution of his own thinking about physics. I picked three that are spread out in time:

Nobel Prize acceptance speech (1979)

"What is Quantum Field Theory and What Did We Think It Is?" (1997)

"Half A Century Of The Standard Model" (2018)

Part I: Nobel Prize Acceptance Speech (1979)

Main theme of this lecture: symmetries and renormalizability as the central organizing principles.

"To a remarkable degree, our present theories of elementary particle interactions can be understood deductively, as a consequence of symmetry principles and of a principle of renormalizability that is invoked to deal with the infinities."

After 1905 with Special Relativity as a precedent, "symmetry principles took on a character in physicists's minds of universal principles, expressions of the simplicity of nature at the deepest level." So becoming accustomed to imperfect and partial symmetries like isospin, starting in the 1930s, and later strangeness was "painfully difficult." Later the shocks of learning that P and then CP were not valid: nature was not as perfect as assumed.

Why was it so hard to accept the idea of symmetries that are only approximate or only apply to some interactions?

“If symmetry principles are an expression of the simplicity of nature at the deepest level, then how can there be such a thing as an approximate symmetry? Is nature only approximately simple?”

A turning point in Steve’s work: learning about the idea of spontaneously broken global symmetry from Goldstone and others.

After learning of the idea of spontaneous breaking of symmetry, “As theorists sometimes do, I fell in love with this idea. But as often happens with love affairs, at first I was rather confused about the implications.”

To Steve, the initial attraction of the idea of spontaneous symmetry breaking was the hope that “the approximate symmetries – parity, isospin, strangeness, the eightfold way – might really be exact *a priori* symmetry principles, and that the observed violations of these symmetries might somehow be brought about by spontaneous symmetry breaking.”

“It was therefore rather disturbing for me to hear of a result of Goldstone,” showing that spontaneously broken continuous symmetry implies the existence of a Goldstone boson.

Steve became extremely interested in this result, and one of his important early papers (1962), with Goldstone and Salam, consisted of three proofs of Goldstone’s theorem.

At the time Weinberg’s conclusion was that spontaneously broken symmetry is not a useful idea for fundamental physics because “it seemed obvious that there could not exist any new kind of elementary particle of this sort that would not already have been discovered.”

Peter Higgs and others showed by about 1964 that spontaneously broken continuous *gauge* symmetry does not lead to existence of a Goldstone particle. But "[particle] physicists who heard about this ... generally regarded it as a technicality. This may have been because of a new development" ... the Adler-Weisberger sum rule (1964) which turned Goldstone bosons from "unwanted intruders to ... welcome friends."

The idea of the pion as a Goldstone boson of a spontaneously broken approximate chiral symmetry goes back to earlier work of Nambu and others. But it was the Adler-Weisberg sum rule that got Steve really excited about current algebra. I often heard him say that this sum rule got quantum field theory back on track after years in the doldrums. The original formulation was different, but in Steve's interpretation, the Adler-Weisberger sum rule was a current algebra formula for low energy $\pi N \to \pi N$ scattering, combined with a previously known dispersion relation.

Steve spent much of the years 1965-7 working on current algebra and reformulating it in terms of effective field theory. He introduced effective field theory as a tool that makes it straightforward to calculate the consequences of spontaneously broken symmetry (exact or approximate). This was enormously influential at multiple levels. It made current algebra transparent, and it was a prototype for thinking about other aspects of physics.

The other strain of thought that led to the standard model was renormalizability. A lot of physicists had viewed renormalization of divergences as sweeping problems under a rug. Weinberg's view was different. "I learned about renormalization theory as a graduate student, mostly by reading Dyson's papers. From the beginning, it seemed to me to be a wonderful thing that very few quantum field theories are renormalizable. Limitations of this sort are, after all, what we most want, not mathematical methods which can make sense of an infinite number of physically irrelevant theories."

"I thought that renormalizability might be the key criterion, which also in a more general context would impose a precise kind of simplicity on our theories and help us pick out the one true physical theory out of the infinite variety of conceivable quantum field theories."

Soon after getting the Ph.D. (1957) Weinberg proved a relatively difficult theorem ("Weinberg's theorem") that in a sense completed the proof of renormalization theory.

By mid-1967, Weinberg was trying to make a gauge theory of chiral symmetry with the ρ and A_1 as gauge bosons. The pion was also supposed to be a Goldstone boson of the same symmetry. It won't come as a surprise in hindsight that the pieces didn't fit together.

"At some point in the fall of 1967, I think while driving to my office at MIT, it occurred to me that I had been applying the right ideas to the wrong problem. It is not the ρ meson that is massless; it is the photon. And its partner is not the A_1, but the massive intermediate bosons, which since the time of Yukawa had been suspected to be the mediators of the weak interactions."

Once the ideas were in place, it was not difficult to make a model. Soon what we now call the Weinberg-Salam model of electroweak interactions was born. But Weinberg did not make much progress in proving renormalizability – partly because of trying to use unitary gauge, and partly because of lack of familiarity with path integrals. Weinberg says that he was initially skeptical of the work of 't Hooft and Veltman because of unfamiliarity with path integrals. (In one of the later lectures, he explains that in the early part of his career, he viewed path integrals as a throwback to an earlier epoch in which physicists distinguished particles from fields.) It was the work of Ben Lee (deriving 't Hooft's Feynman rules from canonical quantization in an appropriate situation) that resolved Weinberg's skepticism.

Assessing the electroweak theory in 1979, Weinberg said that it was “all very nice” that the original Weinberg-Salam model has passed experimental tests. “But I must say that I would not have been too disturbed if it had turned out the the correct theory was based on some other spontaneously broken gauge group, with very different neutral currents.... The important thing to me was the existence of an exact spontaneously broken gauge symmetry....”

He then went on to talk about the strong interactions and QCD. "Experiments have increasingly confirmed QCD as the correct theory of strong interactions." He highlights one consequence of QCD, "its impact on our understanding of symmetry principles.... The constraints of gauge invariance and renormalizability proved enormously powerful. These constraints force the Lagrangian to be so simple that the strong interactions in QCD must conserve" various global symmetries (isospin, baryon number, charge conjugation, etc.).

Famously, this thinking does not work for CP, which led to Steve's proposal of the axion to fix that issue.

Concluding the lecture:

"I suppose that I tend to be optimistic about the future of physics. And nothing makes me more optimistic than the discovery of broken symmetries."

Part II: "What is Quantum Field Theory and What Did We Think It Is?" (1997)

Here is Weinberg describing his views early in his career:

"There were two things that especially attracted me to the ideas of renormalizability and field theory. One of them was that the requirement that a physical theory be renormalizable is a precise and practical criterion of simplicity. . . . The other thing I liked about quantum field theory was that it gives a clear answer to the ancient question of what it means for a particle to be elementary: it is just a particle whose field appears in the Lagrangian. . . ."

Steve changed his viewpoint largely in the course of teaching about QFT and writing his books. He came to view QFT as the inevitable low energy outcome of relativity + quantum mechanics + cluster decomposition. Why are there Lagrangians? Because otherwise there is no link between symmetries and conservation laws, and Lorentz invariance is difficult to achieve.

"Although you cannot argue that relativity plus quantum mechanics plus cluster decomposition leads only to quantum field theory, it is very likely that any theory that at sufficiently long distances looks Lorentz invariant and satisfies the cluster decomposition principle will also at sufficiently low energy *look* like a quantum field theory."

I think most physicists would find this convincing, although a quibble is that the discussion does not apply readily to theories that are not infrared-free and so cannot be described in the language of particles. (However, to be best of our knowledge, such theories are still QFT's at long distances.)

What is the reason for the gauge invariance of the Standard Model and General Relativity? "One possible answer is that it is the only way for massless particles of spin 1 or 2 to have nontrivial couplings at low energies."

(Another quibble: to me, this is a true and important fact and one of the many things that Steve pioneered - back in the early 1960s - with his work on soft theorems, which was the state of the art until the rather recent work of Strominger and others. But I do not really find it convincing as an *explanation*: it shifts the question to "why are there massless particles of spins 1 and 2 that do not decouple at low energies?")

In effect, in 1997, relative to his view in 1979, Weinberg drastically demoted the concept of renormalizability. He no longer saw it as a fundamental principle but rather as a powerful tool in looking for a useful description of (most of) the experiments we can actually do because we are (mostly) limited to comparatively low energies. In part, a Wilsonian view.

All of our theories of today, Weinberg emphasized, are effective field theories, low energy approximations to something potentially much different.

"The present educated view of the standard model and of general relativity is that these are the leading terms in effective field theories.... I don't see why anyone today would take Einstei's theory of general relativity seriously as the foundation for a quantum theory of gravitation, if by Einstein's theory is meant the theory with a Lagrangian given just by the term $\frac{1}{16\pi G}\int d^4x\sqrt{g}R$."

"Likewise, since now we know that without additional fields there is no way that the renormalizable terms in the standard model could violate baryon conservation or lepton conservation, we now understand in a rational way why baryon number and lepton number are as well conserved as they are, without having to assume that they are exactly conserved. Unless someone has some *a priori* reason for exact baryon and lepton conservation that I haven't heard, I would bet very strong odds that baryon number and lepton number conservation are in fact violated by suppressed non-renormalizable corrections to the standard model."

“Another answer” to the question of what is QFT: QFT is “S-matrix theory made practical.”

While specific QFT’s like the standard model make detailed predictions, the general structure of QFT has no content beyond the general principles that went into S-matrix theory ... though Weinberg remarks that he considers the emphasis on analyticity in S-matrix theory to have been misguided, since he views it as a consequence of the more basic principles (relativity, quantum mechanics, cluster decomposition).

Weinberg actually proved a theorem with Joaquim Gomis, proving that order by order in perturbation theory, an arbitrary theory can be renormalized provided one is willing to include all possible local operators as adjustable counterterms. I think one could describe this theorem as saying that including nonrenormalizable operators does not lead to any anomalies beyond the known ones that occur for minimally coupled fermions.

Weinberg responds to those who find it discouraging to question the fundamental significance of our deepest theories of today:

“In regarding the Standard Model and General Relativity as effective field theories, we are simply balancing our checkbook and realizing that we perhaps didn’t know as much as we thought we did, but this is the way the world is, and now we are going to take the next step and find an ultraviolet fixed point or (much more likely) find entirely new physics.”

Part III: "Half A Century Of The Standard Model" (2018)

Here is Steve's assessment of where physics was when he started. Feynman, Schwinger, Dyson, and Tomonaga in the late 1940s had figured out how to calculate in QED in a Lorentz invariant way and to deal with the infinities. But "more than that, the theorists of the 1940's had discovered a rationale for the simplest version of quantum electrodynamics. The symmetries of electrodynamics, Lorentz and gauge invariance, by themselves would not take you very far. For instance, you could add terms to the Lagrangian that would make the magnetic moment of the electron anything you like. But then renormalization would not work. For the theory to be renormalizable, the Lagrangian had to be very simple, and it was in just that simple theory that you could calculate specific results and get stunning agreement with observation."

That is what attracted Steve to QFT. In the 1950's and much of the 1960's, it was hard to make progress with QFT, so some physicists – not Steve – explored S-matrix theory – trying to understand the S-matrix by general principles, without field theory. "This aim was in a sense achieved much later in effective field theories, but it could never be implemented in the way that was tried in the 1950's. Complex analysis with many complex variables is just too hard."

The importance of symmetries was clear, but there was a puzzle: Why are there approximate symmetries? (isospin, strangeness, P, CP) “If symmetry principles are fundamental truths about nature, how could they be approximate or apply to some interactions and not others, and if they are not fundamental truths, what are they?”

He then went on, as in his Nobel Prize acceptance speech, to tell the story of spontaneously broken symmetry, Goldstone's theorem, the Higgs mechanism, the Weinberg-Salam model, and 't Hooft and Veltman. I won't repeat that part of the story.

“So now we have the standard model. Its success is also the success of quantum field theory.

“Or is it? Since the 1970s we have understood that within broad limits, any relativistic quantum theory will look like a quantum field theory, what is called an effective field theory, at energies E less than some fundamental scale M.

“As I like to put it, nonrenormalizable theories are just as renormalizable as renormalizable theories.”

He means by this, in the spirit of his theorem with Gomis that I mentioned before, that an arbitrary theory (free of the usual anomalies) can be renormalized order by order in perturbation theory, provided that one is willing to allow all possible counterterms. To me, this raises a question about which, as far as I know, Weinberg never directly expressed an opinion. What he is saying is that order by order in perturbation theory, one can construct a family of unitary, physically sensible S-matrices with the coefficients of all possible (gauge-invariant, Lorentz-invariant) local operators as free parameters. But do unitary S-matrices with those parameters actually exist or is this only a perturbative statement?

I actually wonder if Weinberg considered that question irrelevant, because of his view that QFT was fundamentally effective field theory. But I am not sure. He was also at times in his career very interested in ultraviolet safety - the existence of a UV fixed point, possibly including gravity - which is a framework that would at least (he used to say) only allow a finite number of free parameters, corresponding to relevant or marginal perturbations. He had a consistent interest in that idea, which might entail a "fundamental QFT" that "really exists." But as far as know he generally viewed it as a sort of dark horse - not the most likely future prospect. He used to say – including in the lectures I am describing – that the likeliest future prospect would involve a completely different kind of physics, possibly string theory, But he always used to try to be open to all possibilities, including the ones that looked less likely.

But regardless of this, on the question of whether the family of unitary, relativistic S-matrices depending on infinitely many parameters really exists, or only makes sense in perturbation theory, I am not aware that Steve ever expressed an opinion. For what it is worth, I think it is an important question, and I am not sure of the answer, but I tend to think that the answer is "no." If the answer is "yes," I think the generic theory must have a UV limit of a type that is unfamiliar to us – not an ordinary local QFT fixed point. (I think this for the same reason that Steve said that asymptotic safety would leave only finitely many parameters.)

Anyway Steve in 2018 considered any theory to be as good as a renormalizable one. What is special about renormalizable theories, he explains, is that any theory looks renormalizable (possibly free) at low energies. Unfortunately, for the most part only relatively low energies are accessible to us. "With hindsight, this is why the search for renormalizable theories turned out to be such a good idea."

As in the previous two lectures that I summarized, Weinberg notes that global symmetries can be low energy accidents, resulting from the structure of gauge couplings, and as such are likely to be violated by corrections to the renormalizable standard model. By 2018, Weinberg could point to neutrino masses as a confirmation of this picture.

I think I will leave the last word to Steve:

"The present generation of young physicists may envy those of us who had the excitement and delight of developing the standard model. This might be a mistake, just it it turned out that my generation would have been mistaken to have envied the earlier heroes of quantum electrodynamics. Our newly minted experimentalists and theorists now have a chance to participate in taking the next big step beyond the standard model. They may even be able to see their way clear to the very high energy scale where a final theory will be revealed."