

Particle physics, from Rutherford to the LHC

Steven Weinberg

Our understanding of fundamental particles has developed in ways that were unimaginable 100 years ago, when the atomic nucleus was first glimpsed.

Steven Weinberg is a professor in the physics and astronomy departments of the University of Texas at Austin, where he heads the physics department's theory group. This article is a slightly edited version of a talk opening the session "100 Years of Sub-Atomic Physics" at the April 2011 meeting of the American Physical Society.

On 7 March 1911, Ernest Rutherford attended a meeting of the Manchester Literary and Philosophical Society, the society before which a century earlier John Dalton had reported the measurement of atomic weights. At the 1911 meeting Rutherford announced the discovery of the atomic nucleus. The American Physical Society has decided to mark the date as the beginning of a century of elementary-particle physics.

I think it's a wise choice. For one thing, the experiment on which Rutherford based his conclusion about the nucleus, carried out under Rutherford's direction by Hans Geiger and Ernest Marsden, was a paradigm for the scattering experiments that have been an occupation of particle physicists ever since. Only instead of a beam of protons or electrons from an accelerator, Geiger and Marsden used alpha particles from the radioactive decay of radium, incident on a gold foil target. And instead of wire chambers or spark chambers or bubble chambers to detect the scattered particles, they used a screen coated with zinc sulfide that would emit a flash of light when struck with an alpha particle.

Even more important, the observation of elastic scattering of alpha particles at large angles convinced Rutherford that most of the mass and positive charge of the atom were concentrated in a nucleus of small volume. Previously it had generally been thought that the atom was something like a pudding, in which electrons are stuck like raisins in a smooth background of positive charge. The discovery of the nucleus was the essential first step in a chain of developments, from Niels Bohr (who had been a visitor to Rutherford's laboratory at Manchester) to Louis de Broglie to Erwin Schrödinger and Werner Heisenberg, that led to modern quantum mechanics.



Ernest Rutherford lecturing at McGill University in Canada. This pastel, by R. G. Matthews, was drawn in 1907, four years before Rutherford announced the discovery of the atomic nucleus. (Courtesy of the AIP Emilio Segre Visual Archives, PHYSICS TODAY Collection.)



This aerial view of CERN shows the 27-km circumference of the Large Hadron Collider as well as the locations of several LHC experiments and the Super Proton Synchrotron (SPS) that will inject protons into the LHC. (Photograph courtesy of CERN.)

Theoretical and experimental barriers

After the initial successes of quantum mechanics, there remained two obvious frontiers for fundamental physics. One was the extension of quantum mechanics to relativistic phenomena. Paul Dirac's approach was to generalize the Schrödinger wave equation to a relativistic wave equation. That seemed to score a great triumph in predicting that elementary particles must have spin $\frac{1}{2}$ (in units of Planck's constant $\hbar$), but today we know the prediction was a failing rather than a success. There are particles with one unit of spin like the W and Z that seem every bit as elementary as the electron, and many of us think that an equally elementary particle with no spin will be discovered at the Large Hadron Collider (LHC). Also, it was awkward to generalize the Dirac equation to systems containing more than one electron. The future belonged instead to quantum field theory, developed in various collaborations including Max Born, Heisenberg, and Pascual Jordan in 1926, Heisenberg and Wolfgang Pauli in 1926, and Pauli and Victor Weisskopf in 1934. (Weisskopf once told me that Pauli aimed in their 1934 paper to show that Dirac was wrong about the necessity of spin $\frac{1}{2}$ by constructing a perfectly sensible theory of particles of spin zero.) Quantum field theory found its first application in Enrico Fermi's 1933 theory of beta decay, and it has been the mathematical framework for all the successes of elementary-particle theory ever since.

The other obvious frontier was the atomic nucleus. The great obstacle there was the Coulomb barrier, which had prevented the alpha particles from radium in Rutherford's laboratory from getting into the nucleus. It was the problem of Coulomb repulsion that led to the initial development of particle accelerators.

Progress in exploring those frontiers in the 1930s was

hampered by an odd unwillingness of theorists to suggest new particles. Here are three examples:

First, the continuous spectrum of electrons in beta decay, discovered by James Chadwick in 1914, was not what one would expect if the electron carried off all of the energy released in the nuclear transition. This was so puzzling that Bohr was led to suggest that energy might not be conserved in those decays. Pauli's proposal of the neutrino in 1930 met with widespread skepticism, which was not entirely gone until the neutrino was discovered a quarter century later.

Second, Dirac at first thought that the holes in the sea of negative-energy electrons in his theory must be protons, the only known positively charged particles, despite the fact that atomic electrons could fall into those holes, rendering all ordinary atoms unstable. He later changed his mind, but the 1932 discovery of the positron in cosmic rays by Carl Anderson and Patrick Blackett came as a surprise to most physicists, including Anderson and Blackett.

Third, in order to give atomic nuclei the right masses and charges, physicists at first assumed that nuclei are composed of protons and electrons, even though that would make the nitrogen-14 nucleus a fermion, whereas it was already known from molecular spectra that it is a boson. The idea of a neutron did not take hold until neutrons were discovered by Chadwick in 1932.

Today the previous reluctance to suggest new particles, even where there was a clear theoretical need for them, seems quite peculiar. A theorist today is hardly considered respectable if he or she has not introduced at least one new particle for which there is no experimental evidence. In 1935 it took considerable courage for Hideki Yukawa to propose, on the basis of the known range of nuclear forces, that there should exist a boson with a mass about one-tenth the mass of a proton.

Meanwhile, the similarity in mass between the neutron and proton suggested that there was some sort of symmetry between them. In 1936 the proton–proton nuclear force was measured by Merle Tuve and colleagues and found to be similar to the known neutron–proton force. Almost immediately Gregory Breit and Eugene Feenberg, and Benedict Cassen and Edward Condon, concluded that the symmetry relating neutrons and protons was the isospin (or isotopic spin) conservation group, known to mathematicians as $SU(2)$.

Particle physics began again after World War II. (At this point I am going to stop naming the physicists who carried on the work, because it would take too much time, and I fear that I might miss naming someone who is still aboveground.) In the late 1940s, the old problem of infinities in quantum electrodynamics was solved by renormalization theory. Yukawa’s meson, the pion, was discovered and distinguished from a particle of similar mass, the muon, which had been discovered in 1937. Particles with a new quantum number—strangeness—were discovered in 1947. All those new particles were found in cosmic rays, but in the 1950s accelerators began to displace cosmic rays as a tool for discovering new particles. Accelerators became larger and larger—they moved from the basements of university physics buildings to eventually become geographical features, visible from space.

Obstacles to a comprehensive field theory

The brilliant success of quantum electrodynamics naturally led to hopes for a comprehensive quantum field theory of all elementary particles and their interactions, but that program ran into serious obstacles. For one thing, such a quantum field theory would require a choice of elementary particles, those whose fields would appear in the Lagrangian of the theory. But with so many new particles being discovered, it was not possible to take seriously the selection of any small set of them as elementary. Also, it was easy to imagine any number of quantum field theories of strong interactions, but what could anyone do with them? The strong interactions were strong—much too strong to allow the use of perturbation theory. A school of theorists was even led to give up quantum field theory altogether, at least with regard to the strong interactions, and rely solely on the general properties of the S -matrix, the set of probability amplitudes for all scattering processes.

Another problem: What should we make of approximate symmetries like isospin conservation, or the spontaneously broken “chiral” $SU(2) \times SU(2)$ symmetry, which accounted for the properties of low-energy pions, or the even more approximate $SU(3)$ and $SU(3) \times SU(3)$ symmetries that connect larger families of particles? Even invariance under space and time reversal and charge conjugation (P , T , and C) turned out to be approximate. If symmetries are an expression of the simplicity of nature, are approximate symmetries an expression of the approximate simplicity of nature?

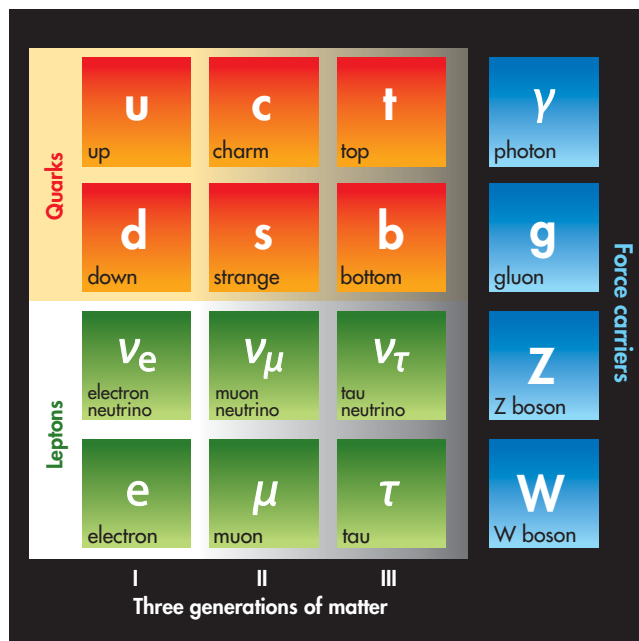
For the weak interactions we had a quantum field theory in good agreement with experiment—Fermi’s 1933 theory of beta decay, with vector currents supplemented with axial vector currents. But when that theory was carried beyond the lowest order of perturbation theory, it gave infinities that apparently could not be removed by renormalization.

The standard model

All of those obstacles were overcome through the development in the 1960s and 1970s of a quantum field theory of elementary particles: the standard model. It is based on exact symmetries that generalize the gauge invariance of electrodynamics. Some of those gauge symmetries are spontaneously broken, some not. The LHC will undoubtedly reveal



An early image of the pion, captured in a photographic emulsion. Created in a cosmic-ray collision, the pion enters from the bottom right, comes to rest, and decays into a muon and a neutral (hence invisible) neutrino. The muon proceeds from bottom to top and decays into an electron and a neutrino. Before the development of high-energy particle accelerators, cosmic rays were the most fertile source of new particles. (Image from Cecil Powell’s Nobel Prize lecture, http://nobelprize.org/nobel_prizes/physics/laureates/1950/powell-lecture.pdf.)



The elementary particles of the standard model. Shown here are the three generations of quarks and leptons and the bosons that carry the strong, electromagnetic, and weak forces.

Effective field theories

It is now generally understood that any theory that is consistent with quantum mechanics and special relativity (together with a technical requirement that distant experiments have uncorrelated results) will look at sufficiently low energies like a quantum field theory. The fields in such effective theories correspond to particles, whether elementary or not, with masses small enough to be produced at the energies in question. Because effective field theories are not fundamental theories, there is no reason to expect them to be particularly simple. Rather, all of the infinite variety of possible terms in the Lagrangian of the effective theory that are consistent with assumed symmetries will be present in the theory, each term with its own independent coefficient.

It might seem that such a theory, with an infinite number of free parameters, would not have much predictive power. The utility of effective theories arises from the circumstance that anything that can make an interaction more complicated, such as adding factors of fields or spacetime derivatives to the interaction, will increase its dimensionality (in units of mass, with $\hbar$ and c taken as unity). In a renormalizable theory, all terms in the Lagrangian must have dimensionality of four or less; this gives rise to the condition of simplicity referred to in the text. But in an effective field theory, all but a finite number of terms in the Lagrangian density will have dimensionality greater than four. The coefficients of those complicated terms must then have denominators proportional to powers of some mass, because the Lagrangian density itself must have dimensionality equal to four. If the effective field theory arises from “integrating out” high-energy degrees of freedom in an underlying fundamental theory (or at least a more fundamental theory), then the mass that characterizes the magnitude of the higher dimensional interactions will be of the order of the mass scale of the fundamental theory. As long as the effective field theory is used only to explore energies much less than that mass scale, the effective field theory provides a perturbative expansion, not in powers of coupling constants, but rather in powers of energy divided by the characteristic mass scale of the underlying fundamental theory.

The presence of interactions of dimensionality greater than four means that effective field theories cannot be renormalizable in the same sense as quantum electrodynamics. That is, beyond the lowest order of perturbation theory, one encounters diver-

gent integrals that cannot be canceled by the redefinition, or renormalization, of a finite number of parameters in the theory. But those infinities can be canceled by a redefinition of the infinite number of parameters in the theory. Moreover, to each order in perturbation theory one encounters only a finite number of free parameters, and only a finite number of divergent integrals, whose infinities can always be canceled by renormalization of those free parameters.

Effective field theories in particle physics were first used in this way in the study of low-energy pions, where the underlying mass scale is about a GeV. The effective theory of low-energy pions has also been extended to processes involving fixed numbers of nucleons. (It does not matter that the nucleon mass is not small compared to a GeV, as long as one does not consider processes in which nucleons are created or destroyed.) In the effective field theory of pions and nucleons, the chiral symmetry mentioned in the text does not allow any interactions that are conventionally renormalizable (that is, with coupling constants of dimensionality of four or less).

Similarly, in the quantum theory of gravitation, coordinate-choice invariance does not allow any gravitational interactions that are conventionally renormalizable. Quantum gravity, too, has been treated as an effective field theory. The problem with quantum gravity is not its infinities but the fact that (as in all effective theories) it loses all predictive power at sufficiently high energies—in this case, at the Planck scale of about 10^{19} GeV, or perhaps a little lower.

The old Fermi theory of beta decay could have been treated as part of an effective field theory, with the four-fermion interaction just the first term in an expansion in powers of the energy divided by a mass scale of the order of 100 GeV, roughly the mass of the W and Z bosons. In the next order in the expansion we would encounter divergent integrals, which could be made finite by the renormalization of a few new four-fermion interactions, including some with extra factors of momentum. As it turned out, the theory underlying the Fermi theory was discovered before it was understood how to use the Fermi theory as part of an effective field theory. The underlying theory here is, of course, the standard electroweak theory, which allows the use of perturbation theory at energies far above 100 GeV, possibly all the way up to 10^{15} GeV.

to us the mechanism that breaks the gauge symmetry governing the weak and electromagnetic interactions. There is a clear choice of elementary particles whose fields appear in the standard model—quarks, leptons, and gauge bosons. It is still hard to calculate a good deal about the hadronic particles built from quarks, which feel the strong interactions, but the weakening of strong interactions at high energy allows enough things to be calculated so that we know the theory is right, and, further, the strengthening of strong interactions at low energy presumably explains why isolated quarks cannot be observed.

A simplicity is imposed on the standard model by the condition of renormalizability—the Lagrangian can include only terms with a limited number of fundamental fields, on which there act a limited number of spacetime derivatives. That condition is required in order that all the infinities encountered in perturbation theory may be absorbed in a redefinition of a finite number of constants in the Lagrangian.

That simplicity provides a natural explanation of the mysterious approximate symmetries of the strong interac-

tions, such as isospin conservation. The strong-interaction part of the theory cannot be complicated enough to violate those symmetries, aside from small effects due to the lightest quark masses. Likewise, the theory of strong and electromagnetic interactions cannot be complicated enough to violate the conservation of strangeness and other flavors or (aside from some subtle quantum effects) P , T , and C .

Not the last word

It is clearly necessary to go beyond the standard model. There is a mysterious spectrum of quark and lepton masses and mixing angles that we have been staring at for decades, as if they were symbols in an unknown language, without our being able to interpret them. Also, something beyond the standard model is needed to account for cosmological dark matter.

It is now widely understood that the standard model is just an effective field theory (see the box above), the low-energy limit of some more fundamental theory involving a scale of mass much larger than the masses with which we are familiar. That means we should expect the standard model to

be supplemented with interactions that are not renormalizable in the usual sense—in fact, with all interactions allowed by symmetry principles—but suppressed by denominators proportional to powers of the large new mass. Infinities are still absorbed in a redefinition of the constants of the theory, but the number of constants that need to be redefined is no longer finite.

In recent years we have found evidence that there is a new mass scale somewhere in the neighborhood of 10^{16} GeV. The renormalizable interactions of the standard model automatically conserve baryon and lepton number, but there is no reason to suppose that those are absolute conservation laws. In fact, the discovery of tiny neutrino masses indicates that the standard model must be supplemented with nonrenormalizable interactions that do not conserve lepton number and that are suppressed by a denominator on the order of 10^{16} GeV. I fully expect that sometime in this century we will find similarly suppressed baryon nonconserving processes, so that proton decay will become a major concern of particle physicists.

Of course, long before the discovery of neutrino masses, we knew of something else beyond the standard model that suggests new physics at masses a little above 10^{16} GeV: the existence of gravitation. And there is also the fact that the one strong and two electroweak coupling parameters of the standard model, which depend only logarithmically on energy, seem to converge to a common value at an energy of the order of 10^{15} GeV to 10^{16} GeV.

There are lots of good ideas on how to go beyond the standard model, including supersymmetry and what used to be called string theory, but no experimental data yet to confirm any of them. Even if governments are generous to particle physics to a degree beyond our wildest dreams, we may never be able to build accelerators that can reach energies such as 10^{15} GeV to 10^{16} GeV. Some day we may be able to detect high-frequency gravitational waves emitted during the era of inflation in the very early universe, that can tell us about physical processes at very high energy. In the meanwhile, we can hope that the LHC and its successors will provide the clues we so desperately need in order to go beyond the successes of the past 100 years.

What is all this worth? Do we really need to know why there are three generations of quarks and leptons, or whether nature respects supersymmetry, or what dark matter is? Yes, I think so, because answering this sort of question is the next step in a program of learning how all regularities in nature (everything that is not a historical accident) follow from a few simple laws.

The program first began to seem possible with the advent of quantum mechanics, in the years after Rutherford's discovery of the nucleus. Before then, chemistry had been regarded as a separate science based on principles independent of the principles of physics—so much so that at the turn of the century scientists could speak of physics being complete, though nothing had been done to derive the principles of chemistry from those of physics. Physicists didn't worry about that, because explaining chemistry didn't seem to them to be their job. But in 1929, after quantum mechanics was developed, Dirac announced that "the underlying physical laws necessary for the mathematical theory of a larger part of physics and the whole of chemistry are thus completely known."

The reductionist program—tracing all scientific principles to a few simple physical laws—is not the only important kind of science, or even the only important kind of physics, but it has a special importance of its own that will continue to motivate particle physicists in the century to come. ■



STPIX455
Maglev Turbopump



STP1303
Maglev Turbopump



STPIX2206C
Maglev Turbopump

VACUUM & ABATEMENT

INNOVATIVE TECHNOLOGY

GLOBAL STRENGTH

LOCAL SUPPORT

Think Turbopump, Think Edwards...

Edwards STP magnetically levitated turbopumps are at the forefront of vacuum for R&D institutes and high energy physics.

The use of magnetic bearings ensures ultra low noise and vibration, with a consistent reliable performance, making the STP turbopump the ideal choice for critical and demanding applications.

- Excellent pumping performance with speeds from 300 to 4500l/s
- Compact in design with fully integrated controller options
- Maintenance free for most applications
- Affordable - excellent value for money

For exceptional performance, reliability and maintenance free pumping, you can rely on STP maglev turbopumps to deliver every time.

Think Turbopump, Think Edwards

www.edwardsvacuum.com/STP
turbo@edwardsvacuum.com

EDWARDS

WWW.EDWARDSVACUUM.COM

© Edwards Limited 2011. All Rights Reserved