arXiv:hep-th/9702027v1 4 Feb 1997

UTTG-05-97

What is Quantum Field Theory, and What Did We Think It Is?*

Steven Weinberg**

Physics Department, University of Texas at Austin
Austin, TX 78712

Quantum field theory was originally thought to be simply the quantum theory of fields. That is, when quantum mechanics was developed physicists already knew about various classical fields, notably the electromagnetic field, so what else would they do but quantize the electromagnetic field in the same way that they quantized the theory of single particles? In 1926, in one of the very first papers on quantum mechanics,[1] Born, Heisenberg and Jordan presented the quantum theory of the electromagnetic field. For simplicity they left out the polarization of the photon, and took spacetime to have one space and one time dimension, but that didn't affect the main results. (*Response to comment from audience*: Yes, they were really doing string theory, so in this sense string theory is earlier than quantum field theory.) Born et al. gave a formula for the electromagnetic field as a Fourier transform and used the canonical commutation relations to identify the coefficients in this Fourier transform as operators that destroy and create photons, so that when quantized this field theory became a theory of photons. Photons, of course, had been around (though not under that name) since Einstein's work on the photoelectric effect two decades earlier, but this paper showed that photons are an inevitable consequence of quantum mechanics as applied to electromagnetism.

The quantum theory of particles like electrons was being developed at the same time, and made relativistic by Dirac[2] in 1928–1930. For quite a long time many physicists thought that the world consisted of both fields

*Talk presented at the conference "Historical and Philosophical Reflections on the Foundations of Quantum Field Theory," at Boston University, March 1996. It will be published in the proceedings of this conference.

**Research supported in part by the Robert A. Welch Foundation and NSF Grant PHY 9511632. E-mail address: weinberg@physics.utexas.edu

and particles: the electron is a particle, described by a relativistically invariant version of the Schrödinger wave equation, and the electromagnetic field is a field, even though it also behaves like particles. Dirac I think never really changed his mind about this, and I believe that this was Feynman's understanding when he first developed the path integral and worked out his rules for calculating in quantum electrodynamics. When I first learned about the path-integral formalism, it was in terms of electron trajectories (as it is also presented in the book by Feynman and Hibbs[3]). I already thought that wasn't the best way to look at electrons, so this gave me an distaste for the path integral formalism, which although unreasonable lasted until I learned of 't Hooft's work[4] in 1971. I feel it's all right to mention autobiographical details like that as long as the story shows how the speaker was wrong.

In fact, it was quite soon after the Born–Heisenberg–Jordan paper of 1926 that the idea came along that in fact one could use quantum field theory for everything, not just for electromagnetism. This was the work of many theorists during the period 1928–1934, including Jordan, Wigner, Heisenberg, Pauli, Weisskopf, Furry, and Oppenheimer. Although this is often talked about as second quantization, I would like to urge that this description should be banned from physics, because a quantum field is not a quantized wave function. Certainly the Maxwell field is not the wave function of the photon, and for reasons that Dirac himself pointed out, the Klein–Gordon fields that we use for pions and Higgs bosons could not be the wave functions of the bosons. In its mature form, the idea of quantum field theory is that quantum fields are the basic ingredients of the universe, and particles are just bundles of energy and momentum of the fields. In a relativistic theory the wave function is a functional of these fields, not a function of particle coordinates. Quantum field theory hence led to a more unified view of nature than the old dualistic interpretation in terms of both fields and particles.

There is an irony in this. (I'll point out several ironies as I go along — this whole subject is filled with delicious ironies.) It is that although the battle is over, and the old dualism that treated photons in an entirely different way from electrons is I think safely dead and will never return, some calculations are actually easier in the old particle framework. When Euler, Heisenberg and Kockel[5] in the mid-thirties calculated the effective action (often called the Euler–Heisenberg action) of a constant external electromagnetic field, they calculated to all orders in the field, although their result is usually presented

only to fourth order. This calculation would probably have been impossible with the old fashioned perturbation theory techniques of the time, if they had not done it by first solving the Dirac equation in a constant external electromagnetic field and using those Dirac wave functions to figure out the effective action. These techniques of using particle trajectories rather than field histories in calculations have been revived in recent years. Under the stimulus of string theory, Bern and Kosower,[6] in particular, have developed a useful formalism for doing calculations by following particle world lines rather than by thinking of fields evolving in time. Although this approach was stimulated by string theory, it has been reformulated entirely within the scope of ordinary quantum field theory, and simply represents a more efficient way of doing certain calculations.

One of the key elements in the triumph of quantum field theory was the development of renormalization theory. I'm sure this has been discussed often here, and so I won't dwell on it. The version of renormalization theory that had been developed in the late 1940s remained somewhat in the shade for a long time for two reasons: (1) for the weak interactions it did not seem possible to develop a renormalizable theory, and (2) for the strong interactions it was easy to write down renormalizable theories, but since perturbation theory was inapplicable it did not seem that there was anything that could be done with these theories. Finally all these problems were resolved through the development of the standard model, which was triumphantly verified by experiments during the mid-1970s, and today the weak, electromagnetic and strong interactions are happily all described by a renormalizable quantum field theory. If you had asked me in the mid-1970s about the shape of future fundamental physical theories, I would have guessed that they would take the form of better, more all-embracing, less arbitrary, renormalizable quantum field theories. I gave a talk at the Harvard Science Center at around this time, called "The Renaissance of Quantum Field Theory," which shows you the mood I was in.

There were two things that especially attracted me to the ideas of renormalization and quantum field theory. One of them was that the requirement that a physical theory be renormalizable is a precise and rational criterion of simplicity. In a sense, this requirement had been used long before the advent of renormalization theory. When Dirac wrote down the Dirac equation in 1928 he could have added an extra 'Pauli' term[7] which would have given the electron an arbitrary anomalous magnetic moment. Dirac could (and

perhaps did) say 'I won't add this term because it's ugly and complicated and there's no need for it.' I think that in physics this approach generally makes good strategies but bad rationales. It's often a good strategy to study simple theories before you study complicated theories because it's easier to see how they work, but the purpose of physics is to find out why nature is the way it is, and simplicity by itself is I think never the answer. But renormalizability was a condition of simplicity which was being imposed for what seemed after Dyson's 1949 papers[8] like a rational reason, and it explained not only why the electron has the magnetic moment it has, but also (together with gauge symmetries) all the detailed features of the standard model of weak, electromagnetic, and strong, interactions, aside from some numerical parameters.

The other thing I liked about quantum field theory during this period of tremendous optimism was that it offered a clear answer to the ancient question of what we mean by an elementary particle: it is simply a particle whose field appears in the Lagrangian. It doesn't matter if it's stable, unstable, heavy, light — if its field appears in the Lagrangian then it's elementary, otherwise it's composite.[***]

Now my point of view has changed. It has changed partly because of my experience in teaching quantum field theory. When you teach any branch of physics you must motivate the formalism — it isn't any good just to present the formalism and say that it agrees with experiment — you have to explain to the students why this the way the world is. After all, this is our aim in physics, not just to describe nature, but to explain nature. In the course of teaching quantum field theory, I developed a rationale for it, which very briefly is that it is the only way of satisfying the principles of Lorentz invariance plus quantum mechanics plus one other principle.

Let me run through this argument very rapidly. The first point is to start with Wigner's definition of physical multi-particle states as representations of the inhomogeneous Lorentz group.[9] You then define annihilation and creation operators $a(\vec{p}, \sigma, n)$ and $a^\dagger(\vec{p}, \sigma, n)$ that act on these states (where $\vec{p}$ is the three-momentum, σ is the spin z-component, and n is a species label). There's no physics in introducing such operators, for it is easy to see that

[***]We should not really give quantum field theory too much credit for clarifying the distinction between elementary and composite particles, because some quantum field theories exhibit the phenomenon of bosonization: At least in two dimensions there are theories of elementary scalars that are equivalent to theories with elementary fermions.

any operator whatever can be expressed as a functional of them. The existence of a Hamiltonian follows from time-translation invariance, and much of physics is described by the S-matrix, which is given by the well known Feynman–Dyson series of integrals over time of time-ordered products of the interaction Hamiltonian $H_I(t)$ in the interaction picture;

$$\begin{aligned} S &= \sum_{n=0}^{\infty} \frac{(-i)^n}{n!} \int_{-\infty}^{\infty} dt_1 \int_{-\infty}^{\infty} dt_2 \cdots \int_{-\infty}^{\infty} dt_n \\ &\quad \times T\{H_I(t_1) H_I(t_2) \cdots H_I(t_n)\} \ . \end{aligned} \tag{1}$$

This should all be familiar. The other principle that has to be added is the cluster decomposition principle, which requires that distant experiments give uncorrelated results.[10] In order to have cluster decomposition, the Hamiltonian is written not just as any functional of creation and annihilation operators, but as a power series in these operators with coefficients that (aside from a *single* momentum-conservation delta function) are sufficiently smooth functions of the momenta carried by the operators. This condition is satisfied for an interaction Hamiltonian of the form

$$H_I(t) = \int d^3x \ \mathcal{H}(\vec{x}, t) \tag{2}$$

where $\mathcal{H}(x)$ is a power series (usually a polynomial) with terms that are local in annihilation fields, which are Fourier transforms of the annihilation operators:

$$\psi_\ell^{(+)}(x) = \int d^3p \ \sum_{\sigma,n} e^{ip\cdot x} u_\ell(\vec{p}, \sigma, n) \, a(\vec{p}, \sigma, n) \tag{3}$$

together of course with their adjoints, the creation fields.

So far this all applies to nonrelativistic as well as relativistic theories.[†] Now if you also want Lorentz invariance, then you have to face the fact that the time-ordering in the Feynman–Dyson series (1) for the S-matrix doesn't look very Lorentz invariant. The obvious way to make the S-matrix Lorentz invariant is to take the interaction Hamiltonian density $\mathcal{H}(x)$ to be a scalar,

[†]By the way, the reason that quantum field theory is useful even in nonrelativistic statistical mechanics, where there is often a selection rule that makes the actual creation or annihilation of particles impossible, is that in statistical mechanics you have to impose a cluster decomposition principle, and quantum field theory is the natural way to do so.

and also to require that these Hamiltonian densities commute at spacelike separations

$$[\mathcal{H}(x), \mathcal{H}(y)] = 0 \quad \text{for spacelike} \quad x - y \ , \tag{4}$$

in order to exploit the fact that time ordering *is* Lorentz invariant when the separation between spacetime points is timelike. In order to satisfy the requirement that the Hamiltonian density commute with itself at spacelike separations, it is constructed out of fields which satisfy the same requirement. These are given by sums of fields that annihilate particles plus fields that create the corresponding antiparticles

$$\begin{aligned} \psi_\ell(x) \quad = \quad & \sum_{\sigma,n} \int d^3p \left[e^{ip\cdot x}\, u_\ell(\vec{p},\sigma,n)\, a(\vec{p},\sigma,n) \right. \\ & \left. + e^{-ip\cdot x}\, v_\ell(\vec{p},\sigma,n)\, a^\dagger(\vec{p},\sigma,\bar{n}) \right] , \end{aligned} \tag{5}$$

where $\bar{n}$ denotes the antiparticle of the particle of species n. For a field ψ_ℓ that transforms according to an irreducible representation of the homogeneous Lorentz group, the form of the coefficients u_ℓ and v_ℓ is completely determined (up to a single over-all constant factor) by the Lorentz transformation properties of the fields and one-particle states, and by the condition that the fields commute at spacelike separations. Thus the whole formalism of fields, particles, and antiparticles seems to be an inevitable consequence of Lorentz invariance, quantum mechanics, and cluster decomposition, without any ancillary assumptions about locality or causality.

This discussion has been extremely sketchy, and is subject to all sorts of qualifications. One of them is that for massless particles, the range of possible theories is slightly larger than I have indicated here. For example, in quantum electrodynamics, in a physical gauge like Coulomb gauge, the Hamiltonian is not of the form (2) — there is an additional term, the Coulomb potential, which is bilocal and serves to cancel a non-covariant term in the propagator. But relativistically invariant quantum theories always (with some qualifications I'll come to later) do turn out to be quantum field theories, more or less as I have described them here.

One can go further, and ask why we should formulate our quantum field theories in terms of Lagrangians. Well, of course creation and annihilation operators by themselves yield pairs of canonically conjugate variables; from the as and $a^\dagger$s, it is easy to construct qs and ps. The time-dependence of

these operators is dictated in terms of the Hamiltonian, the generator of time translations, so the Hamiltonian formalism is trivially always with us. But why the Lagrangian formalism? Why do we enumerate possible theories by giving their Lagrangians rather than by writing down Hamiltonians? I think the reason for this is that it is only in the Lagrangian formalism (or more generally the action formalism) that symmetries imply the existence of Lie algebras of suitable quantum operators, and you need these Lie algebras to make sensible quantum theories. In particular, the S-matrix will be Lorentz invariant if there is a set of 10 sufficiently smooth operators satisfying the commutation relations of the inhomogeneous Lorentz group. It's not trivial to write down a Hamiltonian that will give you a Lorentz invariant S-matrix — it's not so easy to think of the Coulomb potential just on the basis of Lorentz invariance — but if you start with a Lorentz invariant Lagrangian density then because of Noether's theorem the Lorentz invariance of the S-matrix is automatic.

Finally, what is the motivation for the special gauge invariant Lagrangians that we use in the standard model and general relativity? One possible answer is that quantum theories of mass zero, spin one particles violate Lorentz invariance unless the fields are coupled in a gauge invariant way, while quantum theories of mass zero, spin two particles violate Lorentz invariance unless the fields are coupled in a way that satisfies the equivalence principle.

This has been an outline of the way I've been teaching quantum field theory these many years. Recently I've put this all together into a book,[11] now being sold for a negligible price. The bottom line is that quantum mechanics plus Lorentz invariance plus cluster decomposition implies quantum field theory. But there are caveats that have to be attached to this, and I can see David Gross in the front row anxious to take me by the throat over various gaps in what I have said, so I had better list these caveats quickly to save myself.

First of all, the argument I have presented is obviously based on perturbation theory. Second, even in perturbation theory, I haven't stated a clear theorem, much less proved one. As I mentioned there are complications when you have things like mass zero, spin one particles for example; in this case you don't really have a fully Lorentz invariant Hamiltonian density, or even one that is completely local. Because of these complications, I don't know how even to state a general theorem, let alone prove it, even in perturbation theory. But I don't think that these are insuperable obstacles.

A much more serious objection to this not-yet-formulated theorem is that there's already a counter example to it: string theory. When you first learn string theory it seems in an almost miraculous way to give Lorentz invariant, unitary S-matrix elements without being a field theory in the sense that I've been using it. (Of course it is a field theory in a different sense — it's a two dimensional conformally invariant field theory, but not a quantum field theory in four spacetime dimensions.) So before even being formulated precisely, this theorem suffers from at least one counter example.

Another fundamental problem is that the S-matrix isn't everything. Spacetime could be radically curved, not just have little ripples on it. Also, at finite temperature there's no S-matrix because particles cannot get out to infinite distances from a collision without bumping into things. Also, it seems quite possible that at very short distances the description of events in four-dimensional flat spacetime becomes inappropriate.

Now, all of these caveats really work only against the idea that the final theory of nature is a quantum field theory. They leave open the view, which is in fact the point of view of my book, that although you can not argue that relativity plus quantum mechanics plus cluster decomposition necessarily leads only to quantum field theory, it is very likely that any quantum theory that at sufficiently low energy and large distances looks Lorentz invariant and satisfies the cluster decomposition principle will also at sufficiently low energy *look* like a quantum field theory. Picking up a phrase from Arthur Wightman, I'll call this a folk theorem. At any rate, this folk theorem is satisfied by string theory, and we don't know of any counterexamples.

This leads us to the idea of effective field theories. When you use quantum field theory to study low-energy phenomena, then according to the folk theorem you're not really making any assumption that could be wrong, unless of course Lorentz invariance or quantum mechanics or cluster decomposition is wrong, provided you don't say specifically what the Lagrangian is. As long as you let it be the most general possible Lagrangian consistent with the symmetries of the theory, you're simply writing down the most general theory you could possibly write down. This point of view has been used in the last fifteen years or so to justify the use of effective field theories, not just in the tree approximation where they had been used for some time earlier, but also including loop diagrams. Effective field theory was first used in this way to calculate processes involving soft π mesons,[12] that is, π mesons with energy less than about $2\pi F_\pi \approx 1200$ MeV. The use of effective quantum

field theories has been extended more recently to nuclear physics,[13] where although nucleons are not soft they never get far from their mass shell, and for that reason can be also treated by similar methods as the soft pions. Nuclear physicists have adopted this point of view, and I gather that they are happy about using this new language because it allows one to show in a fairly convincing way that what they've been doing all along (using two-body potentials only, including one-pion exchange and a hard core) is the correct first step in a consistent approximation scheme. The effective field theory approach has been applied more recently to superconductivity. Shankar, I believe, in a contribution to this conference is talking about this. The present educated view of the standard model, and of general relativity,[14] is again that these are the leading terms in effective field theories.

The essential point in using an effective field theory is you're not allowed to make any assumption of simplicity about the Lagrangian. Certainly you're not allowed to assume renormalizability. Such assumptions might be appropriate if you were dealing with a fundamental theory, but not for an effective field theory, where you must include all possible terms that are consistent with the symmetry. The thing that makes this procedure useful is that although the more complicated terms are not excluded because they're non-renormalizable, their effect is suppressed by factors of the ratio of the energy to some fundamental energy scale of the theory. Of course, as you go to higher and higher energies, you have more and more of these suppressed terms that you have to worry about.

On this basis, I don't see any reason why anyone today would take Einstein's general theory of relativity seriously as the foundation of a quantum theory of gravitation, if by Einstein's theory is meant the theory with a Lagrangian density given by just the term $\sqrt{g}R/16\pi G$. It seems to me there's no reason in the world to suppose that the Lagrangian does not contain all the higher terms with more factors of the curvature and/or more derivatives, all of which are suppressed by inverse powers of the Planck mass, and of course don't show up at any energy far below the Planck mass, much less in astronomy or particle physics. Why would anyone suppose that these higher terms are absent?

Likewise, since now we know that without new fields there's 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 of which 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.

These effective field theories are non-renormalizable in the old Dyson power-counting sense. That is, although to achieve a given accuracy at any given energy, you need only take account of a finite number of terms in the action, as you increase the accuracy or the energy you need to include more and more terms, and so you have to know more and more. On the other hand, effective field theories still must be renormalizable theories in what I call the modern sense: the symmetries that govern the action also have to govern the infinities, for otherwise there will be infinities that can't be eliminated by absorbing them into counter terms to the parameters in the action. This requirement is automatically satisfied for unbroken global symmetries, such as Lorentz invariance and isotopic spin invariance and so on. Where it's not trivial is for gauge symmetries. We generally deal with gauge theories by choosing a gauge before quantizing the theory, which of course breaks the gauge invariance, so it's not obvious how gauge invariance constrains the infinities. (There is a symmetry called BRST invariance[15] that survives gauge fixing, but that's non-linearly realized, and non-linearly realized symmetries of the action are not symmetries of the Feynman amplitudes.) This raises a question, whether gauge theories that are not renormalizable in the power counting sense are renormalizable in the modern sense. The theorem that says that infinities are governed by the same gauge symmetries as the terms in the Lagrangian was originally proved back in the old days by 't Hooft and Veltman[16] and Lee and Zinn-Justin[17] only for theories that are renormalizable in the old power-counting sense, but this theorem has only recently been extended to theories of the Yang–Mills[18] or Einstein type with arbitrary numbers of complicated interactions that are not renormalizable in the power-counting sense.[‡] You'll be reassured to know that these theories are renormalizable in the modern sense, but there's no proof that this will

[††]The extra fields required by low-energy supersymmetry may invalidate this argument.

[‡]I refer here to work of myself and Joaquim Gomis,[19] relying on recent theorems about the cohomology of the Batalin–Vilkovisky operator by Barnich, Brandt, and Henneaux.[20] Earlier work along these lines but with different motivation was done by Voronov, Tyutin, and Lavrov;[21] Anselmi;[22] and Harada, Kugo, and Yamawaki.[23]

be true of all quantum field theories with local symmetries.

I promised you a few ironies today. The second one takes me back to the early 1960s when S-matrix theory was very popular at Berkeley and elsewhere. The hope of S-matrix theory was that, by using the principles of unitarity, analyticity, Lorentz invariance and other symmetries, it would be possible to calculate the S-matrix, and you would never have to think about a quantum field. In a way, this hope reflected a kind of positivistic puritanism: we can't measure the field of a pion or a nucleon, so we shouldn't talk about it, while we do measure S-matrix elements, so this is what we should stick to as ingredients of our theories. But more important than any philosophical hang-ups was the fact that quantum field theory didn't seem to be going anywhere in accounting for the strong and weak interactions.

One problem with the S-matrix program was in formulating what is meant by the analyticity of the S-matrix. What precisely are the analytic properties of a multi-particle S-matrix element? I don't think anyone ever knew. I certainly didn't know, so even though I was at Berkeley I never got too enthusiastic about the details of this program, although I thought it was a lovely idea in principle. Eventually the S-matrix program had to retreat, as described by Kaiser in a contribution to this conference, to a sort of mix of field theory and S-matrix theory. Feynman rules were used to find the singularities in the S-matrix, and then they were thrown away, and the analytic structure of the S-matrix with these singularities, together with unitarity and Lorentz invariance, was used to do calculations.

Unfortunately to use these assumptions it was necessary to make uncontrolled approximations, such as the strip approximation, whose mention will bring tears to the eyes of those of us who are old enough to remember it. By the mid-1960's it was clear that S-matrix theory had failed in dealing with the one problem it had tried hardest to solve, that of pion–pion scattering. The strip approximation rested on the assumption that double dispersion relations are dominated by regions of the Mandelstam diagram near the fringes of the physical region, which would only make sense if π–π scattering is strong at low energy, and these calculations predicted that π–π scattering is indeed strong at low energy, which was at least consistent, but it was then discovered that π–π scattering is *not* strong at low energy. Current algebra came along at just that time, and was used to predict not only that low energy π-π scattering is not strong, but also successfully predicted the values of the π–π scattering lengths.[24] From a practical point of view, this was the greatest

defeat of S-matrix theory. The irony here is that the S-matrix philosophy is not that far from the modern philosophy of effective field theories, that what you should do is just write down the most general S-matrix that satisfies basic principles. But the practical way to implement S-matrix theory is to use an effective quantum field theory — instead of deriving analyticity properties from Feynman diagrams, we use the Feynman diagrams themselves. So here's another answer to the question of what quantum field theory is: it is S-matrix theory, made practical.

By the way, I think that the emphasis in S-matrix theory on analyticity as a fundamental principle was misguided, not only because no one could ever state the detailed analyticity properties of general S-matrix elements, but also because Lorentz invariance requires causality (because as I argued earlier otherwise you're not going to get a Lorentz invariant S-matrix), and in quantum field theory causality allows you to derive analyticity properties. So I would include Lorentz invariance, quantum mechanics and cluster decomposition as fundamental principles, but not analyticity.

As I have said, quantum field theories provide an expansion in powers of the energy of a process divided by some characteristic energy; for soft pions this characteristic energy is about a GeV; for superconductivity it's the Debye frequency or temperature; for the standard model it's 10^{15} to 10^{16} GeV; and for gravitation it's about 10^{18} GeV. Any effective field theory loses its predictive power when the energy of the processes in question approaches the characteristic energy. So what happen to the effective field theories of electroweak, strong, and gravitational interactions at energies of order 10^{15}–10^{18} GeV? I know of only two plausible alternatives.

One possibility is that the theory remains a quantum field theory, but one in which the finite or infinite number of renormalized couplings do not run off to infinity with increasing energy, but hit a fixed point of the renormalization group equations. One way that can happen is provided by asymptotic freedom in a renormalizable theory,[25] where the fixed point is at zero coupling, but it's possible to have more general fixed points with infinite numbers of non-zero nonrenormalizable couplings. Now, we don't know how to calculate these non-zero fixed points very well, but one thing we know with fair certainty is that the trajectories that run into a fixed point in the ultraviolet limit form a finite dimensional subspace of the infinite dimensional space of all coupling constants. (If anyone wants to know how we know that, I'll explain this later.) That means that the condition, that the trajectories hit a

fixed point, is just as restrictive in a nice way as renormalizability used to be: It reduces the number of free coupling parameters to a finite number. We don't yet know how to do calculations for fixed points that are not near zero coupling. Some time ago I proposed[26] that these calculations could be done in the theory of gravitation by working in $2 + \epsilon$ dimensions and expanding in powers of $\epsilon = 2$, in analogy with the way that Wilson and Fisher[27] had calculated critical exponents by working in $4 - \epsilon$ dimensions and expanding in powers of $\epsilon = 1$, but this program doesn't seem to be working very well.

The other possibility, which I have to admit is *a priori* more likely, is that at very high energy we will run into really new physics, not describable in terms of a quantum field theory. I think that by far the most likely possibility is that this will be something like a string theory.

Before I leave the renormalization group, I did want to say another word about it because there's going to be an interesting discussion on this subject here tomorrow morning, and for reasons I've already explained I can't be here. I've read a lot of argument about the Wilson approach[28] vs. the Gell-Mann–Low approach,[29] which seems to me to call for reconciliation. There have been two fundamental insights in the development of the renormalization group. One, due to Gell-Mann and Low, is that logarithms of energy that violate naive scaling and invalidate perturbation theory arise because of the way that renormalized coupling constants are defined, and that these logarithms can be avoided by renormalizing at a sliding energy scale. The second fundamental insight, due to Wilson, is that it's very important in dealing with phenomena at a certain energy scale to integrate out the physics at much higher energy scales. It seems to me these are the same insight, because when you adopt the Gell-Mann–Low approach and define a renormalized coupling at a sliding scale and use renormalization theory to eliminate the infinities rather than an explicit cutoff, you are in effect integrating out the higher energy degrees of freedom — the integrals converge because after renormalization the integrand begins to fall off rapidly at the energy scale at which the coupling constant is defined. (This is true whether or not the theory is renormalizable in the power-counting sense.) So in other words instead of a sharp cutoff *a la* Wilson, you have a soft cutoff, but it's a cutoff nonetheless and it serves the same purpose of integrating out the short distance degrees of freedom. There are practical differences between the Gell-Mann–Low and Wilson approaches, and there are some problems for which one is better and other problems for which the other is better. In

statistical mechanics it isn't important to maintain Lorentz invariance, so you might as well have a cutoff. In quantum field theories, Lorentz invariance is necessary, so it's nice to renormalize *a la* Gell-Mann–Low. On the other hand, in supersymmetry theories there are some non-renormalization theorems that are simpler if you use a Wilsonian cutoff than a Gell-Mann–Low cutoff.[30] These are all practical differences, which we have to take into account, but I don't find any fundamental philosophical difference between these two approaches.

On the plane coming here I read a comment by Michael Redhead, in a paper submitted to this conference: 'To subscribe to the new effective field theory programme is to give up on this endeavor' [the endeavor of finding really fundamental laws of nature], 'and retreat to a position that is somehow less intellectually exciting.' It seems to me that this is analogous to saying that to balance your checkbook is to give up dreams of wealth and have a life that is intrinsically less exciting. In a sense that's true, but nevertheless it's still something that you had better do every once in a while. I think that in regarding the standard model and general relativity as effective field theories we're 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're going to go on the next step and try to find an ultraviolet fixed point, or (much more likely) find entirely new physics. I have said that I thought that this new physics takes the form of string theory, but of course, we don't know if that's the final answer. Nielsen and Oleson[31] showed long ago that relativistic quantum field theories can have string-like solutions. It's conceivable, although I admit not entirely likely, that something like modern string theory arises from a quantum field theory. And that would be the final irony.

References

1. M. Born, W. Heisenberg, and P. Jordan, *Z. f. Phys.* **35**, 557 (1926).

2. P.A.M. Dirac, *Proc. Roy. Soc.* **A117**, 610 (1928); *ibid.*, **A118**, 351 (1928); *ibid.*, **A126**, 360 (1930).

3. R. P. Feynman and A. R. Hibbs, *Quantum Mechanics and Path Integrals* (McGraw-Hill, New York, 1965).

4. G. 't Hooft, *Nucl. Phys.* **B35**, 167 (1971).

5. H. Euler and B. Kockel, *Naturwiss.* **23**, 246 (1935); W. Heisenberg and H. Euler, *Z. f. Phys.* **98**, 714 (1936).

6. Z. Bern and D. A. Kosower, in *International Symposium on Particles, Strings, and Cosmology*, eds. P. Nath and S. Reucroft (World Scientific, Singapore, 1992): 794; *Phys. Rev. Lett.* **66**, 669 (1991).

7. W. Pauli, *Z. f. Phys.* **37**, 263 (1926); **43**, 601 (1927).

8. F.J. Dyson, *Phys. Rev.* **75**, 486, 1736 (1949).

9. E. P. Wigner, *Ann. Math.* **40**, 149 (1939).

10. The cluster decomposition principle seems to have been first stated explicitly in quantum field theory by E. H. Wichmann and J. H. Crichton, *Phys. Rev.* **132**, 2788 (1963).

11. S. Weinberg, *The Quantum Theory of Fields — Volume I: Foundations* (Cambridge University Press, Cambridge, 1995)

12. S. Weinberg, *Phys. Rev. Lett.* **18**, 188 (1967); *Phys. Rev.* **166**, 1568 (1968); *Physica* **96A**, 327 (1979).

13. S. Weinberg, *Phys. Lett.* **B251**, 288 (1990); *Nucl. Phys.* **B363**, 3 (1991); *Phys. Lett.* **B295**, 114 (1992). C. Ordóñez and U. van Kolck, *Phys. Lett.* **B291**, 459 (1992); C. Ordóñez, L. Ray, and U. van Kolck, *Phys. Rev. Lett.* **72**, 1982 (1994); U. van Kolck, *Phys. Rev.*, **C49**, 2932 (1994); U. van Kolck, J. Friar, and T. Goldman, to appear in *Phys. Lett. B*. This approach to nuclear forces is summarized in C.

Ordóñez, L. Ray, and U. van Kolck, Texas preprint UTTG-15-95, nucl-th/9511380, submitted to *Phys. Rev. C*; J. Friar, *Few-Body Systems Suppl.* **99**, 1 (1996). For application of these techniques to related nuclear processes, see T.-S. Park, D.-P. Min, and M. Rho, *Phys. Rep.* **233**, 341 (1993); Seoul preprint SNUTP 95-043, nucl-th/9505017; S.R. Beane, C.Y. Lee, and U. van Kolck, *Phys. Rev.*, **C52**, 2915 (1995); T. Cohen, J. Friar, G. Miller, and U. van Kolck, Washington preprint DOE/ER/40427-26-N95, nucl-th/9512036.

14. J. F. Donoghue, *Phys. Rev.* **D 50**, 3874 (1994).

15. C. Becchi, A. Rouet, and R. Stora, *Comm. Math. Phys.* **42**, 127 (1975); in *Renormalization Theory*, eds. G. Velo and A. S. Wightman (Reidel, Dordrecht, 1976); *Ann. Phys.* **98**, 287 (1976); I. V. Tyutin, Lebedev Institute preprint N39 (1975).

16. G. 't Hooft and M. Veltman, *Nucl. Phys.* **B50**, 318 (1972).

17. B. W. Lee and J. Zinn-Justin, *Phys. Rev.* **D5**, 3121, 3137 (1972); *Phys. Rev.* **D7**, 1049 (1972).

18. C. N. Yang and R. L. Mills, *Phys. Rev.* **96**, 191 (1954).

19. J. Gomis and S. Weinberg, Nuclear Physics B **469**, 475–487 (1996).

20. G. Barnich and M. Henneaux, *Phys. Rev. Lett.* **72**, 1588 (1994); G. Barnich, F. Brandt, and M. Henneaux, *Phys. Rev.* **51**, R143 (1995); *Commun. Math. Phys.* **174**, 57, 93 (1995); *Nucl. Phys.* **B455**, 357 (1995).

21. B. L. Voronov and I. V. Tyutin, *Theor. Math. Phys.* **50**, 218 (1982); **52**, 628 (1982); B. L. Voronov, P. M. Lavrov, and I. V. Tyutin, *Sov. J. Nucl. Phys.* **36**, 292 (1982); P. M. Lavrov and I. V. Tyutin *Sov. J. Nucl. Phys.* **41**, 1049 (1985).

22. D. Anselmi, *Class. and Quant. Grav.* **11**, 2181 (1994); **12**, 319 (1995).

23. M. Harada, T. Kugo, and K. Yamawaki, *Prog. Theor. Phys.* **91**, 801 (1994).

24. S. Weinberg, *Phys. Rev. Lett.* **16**, 879 (1966).

25. D. J. Gross and F. Wilczek, *Phys. Rev. Lett.*, **30**, 1343 (1973); H. D. Politzer, *Phys. Rev. Lett.*, **30**, 1346 (1973).

26. S. Weinberg, in *General Relativity*, eds. S. W. Hawking and W. Israel, eds. (Cambridge University Press, Cambridge, 1979): p. 790.

27. K. G. Wilson and M. E. Fisher, *Phys. Rev. Lett.* **28**, 240 (1972); K. G. Wilson, *Phys. Rev. Lett.* **28**, 548 (1972).

28. K. G. Wilson, *Phys. Rev.* **B4**, 3174, 3184 (1971); *Rev. Mod. Phys.* **47**, 773 (1975).

29. M. Gell-Mann and F. E. Low, *Phys. Rev.* **95**, 1300 (1954).

30. V. Novikov, M. A. Shifman, A. I. Vainshtein, and V. I. Zakharov, *Nucl. Phys.* **B229**, 381 (1983); M. A. Shifman and A. I. Vainshtein, *Nucl. Phys.* **B277**, 456 (1986); and references quoted therein. See also M. A. Shifman and A. I. Vainshtein, *Nucl. Phys.* **B359**, 571 (1991).

31. H. Nielsen and P. Oleson, *Nucl. Phys.* **B61**, 45 (1973).