Status of QCD*

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I have been asked to discuss the status of QCD. It seems to me that there are three main points to be made about the present status of QCD:

• QCD is right, and we can do many beautiful things with it.

• There are several important concrete problems that lie just beyond the edge of our current understanding.

• There are some foundational issues in QCD, and some recent developments, that may point toward entirely new directions.

These points will, I believe, emerge quite clearly from the following more detailed discussion. The discussion will be in three parts. I’ll first discuss elementary processes, then more complicated processes, and then finally foundational issues.

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1. Elementary Processes

1.1. Testing QCD, measuring $\alpha_s$

Clearly the best way to test QCD, which is formulated as a microscopic theory, is to test that the elementary processes it postulates in fact occur and are correctly described by the theory. Even verifying the first part of this – that the processes occur – is highly non-trivial from a logical point of view. Indeed the entities in terms of which QCD is formulated, quarks and gluons, cannot be studied at leisure in isolation. By now however all practicing physicists, if not all philosophers of physics [1], are comfortable with the very tangible reality of quarks and gluons. If theorists had not invented them beforehand, quarks and gluons would surely have been “discovered” as the only adequate description of hadronic jets in high-energy collisions.

Because of asymptotic freedom, perturbative QCD supplies quantitative predictions for a wide variety of experiments. I think it is fair to say that no major discrepancy between theory and experiment has emerged thus far. The question naturally arises, just how stringent are the tests? A popular quantitative measure of this is how tightly the strong coupling constant is constrained. This is useful also in checking the mutual consistency of the QCD fits made in (often very) different experiments, and in testing models of unification.

This subject, experimental determination of the strong coupling, has been thoroughly reviewed recently at the Dallas Conference by Bethke [2] and this morning by Marjorie Shapiro [3]. Therefore I will not attempt to do justice to the many experiments in detail, but rather confine myself to an impressionistic survey.

Because of the way that the strong coupling constant runs, that is decreasing as $Q^2$ increases, testing QCD by measuring the strong coupling constant as a function of $Q^2$ has a two-faced character. At large $Q^2$ the predictions are precise – the coupling gets small, and we can do accurate calculations. Also mass corrections, which are non-perturbative and generally difficult to estimate theoretically, are suppressed by powers of mass$^2$ over $Q^2$. On the other hand such large $Q^2$ measurements are limited in their power to resolve possible values of $\alpha_s$ quantitatively. At small $Q^2$ the theory is much harder to control and make precise, but if you are interested in quantitative results for $\alpha_s$ there is a large premium for working at small $Q^2$.

These features are clearly evident in the classic plot of coupling constants measured at different $Q^2$ [Figure 1]. You see that at low $Q^2$ there is a big spread in the couplings, so that is where you can determine what $\alpha_s$ or equivalently $\Lambda_{QCD}$ is. On the other hand for very large $Q^2$ we get – remarkably – a more or less unique
prediction for the value of the physical parameter $\alpha_s (Q^2)$. Almost any reasonable value of $\Lambda_{\text{QCD}}$, that is anything that is sensible to regard as an overall scale for strong interactions, will give you within about 10% the same results for $\alpha_s$ at large $Q^2$.

If your goal is simply to check that QCD is right, then you want a unique prediction and the high energy processes are particularly favorable. But if you want to be quantitative about $\alpha_s$, then the low energy determinations have a big advantage. This is the two-faced character I mentioned.

Figures 1, 2 and 3, taken from Bethke (with a small addition, to be discussed presently), summarize the quantitative results from a wide variety of experiments which measure $\alpha_s$. Figure 1 shows the range of theoretical expectations and the relevant experiments in each $Q^2$ range, Figure 2 summarizes their theoretical foundations and uncertainties, and Figure 3 exhibits the results in tabular form. In these Figures DIS means deep inelastic scattering, NLO means next leading order, NNLO means next to next to leading order, and MC means Monte Carlo. Finally GLS refers to Gross-Llewelyn-Smith. The GLS determination of $\alpha_s$ is based on the difference between the parton model prediction for their sum rule, which highlights the baryon number content of the nucleon, and the measured value. The overall consistency of the results, extracted as they are from widely different experiments covering a vast kinematic range, is certainly most impressive.

Most of what is in these Figures has been discussed by Bethke and by Shapiro, and also by Altarelli [4], with greater insight and authority than I can bring to the subject. Anyone really interested in the quality of the fits should turn to their reviews. I would, however, like to add a few supplementary comments.

First I would like to call attention to the final entries on the right of Figure 1 and the bottom of Figures 2 and 3. These refer to a determination of the strong coupling which is different in character from the more direct QCD measurements [5]. Its inputs are $\alpha_{\text{QED}}$, that is the fine structure constant, and the Weinberg angle. In the context of grand unification, the three gauge coupling constants of the standard model are not independent, so that – for a given model of unification – any two determine the third. These considerations give determinations of the strong coupling whose precision is comparable to that from the other, more conventional determinations. The accuracy of the determination is another matter. It depends on the specific model of unification, as we shall see.

The uncertainties in this determination of $\alpha_s$ are rather different from those in the other cases. Mundane problems like hadronization corrections, non-perturbative contributions, higher twists, small lattice sizes and the quenched approximation – none of these is important. The unification of couplings is the ultimate large $Q^2$ process, and sheds the dross of low-energy QCD. However there
remains a very important caveat to these determinations, namely that they are ambiguous and could be completely wrong. There is precious little independent evidence for the unification theories on which they are based. They can’t all be right, because you get slightly different determinations depending on what you assume about unification.

In fact the minimal grand unified model, based on SU(5), leads to a small but clear-cut conflict with the data. Given the enormity of the extrapolations involved it is most remarkable that the prediction works out even as well as it does. It might be wise to stop at this point, declaring victory for the principles of unification and of quantum field theory extended down to distances of $10^{15}$ GeV while acknowledging that there’s some fuzziness in the details. On the other hand, quite remarkably, the prediction following from SU(5) unification of the minimal supersymmetric extension of the standard model is in adequate agreement with the data [5]. It is certainly tempting to take this agreement as an indication that simple ideas about unification and low-energy supersymmetry have something to do with reality.

Another entry in the table of Figure 3 is particularly interesting and provocative, and suggests some later developments, so I want to go into a little more detail regarding it. It is the determination of $\alpha_s$ from QCD with corrections to the tau lepton lifetime [6]. Tau lepton decay of course is a very low energy process by the standards of LEP or most other QCD tests. So we can expect, in line with the previous discussion, that the prediction will perhaps be delicate but on the other hand it will have a favorable lever arm for determining $\alpha_s$.

The quantity governing inclusive $\tau$ decay, the square of the amplitude summed over intermediate states, is the vacuum matrix element of a current product. The product could be the vector current times the vector current, or axial current times axial, or axial times vector, with different isospins and in principle different strangeness – all these can be separated out by projecting on the quantum numbers of the final state. Suppressing all indices, the relevant object for theoretical analysis is the vacuum matrix element of the current product

$$J(Q)J(-Q) \approx \mathcal{C}_1(Q^2)I(0) + \mathcal{C}_{\bar{\psi}\psi}(Q^2)m\bar{\psi}\psi(0) + \mathcal{C}_{GG}(Q^2)\text{tr}G_{\mu\nu}G_{\mu\nu}(0) + \ldots .$$

The current product in field theory is evaluated, as above, using the operative product expansion. If we had a complete set of operators on the right hand side we’d expect to get a complete representation of this current product. The successive operators have higher and higher mass dimension and therefore their coefficients, since the overall dimension has to be the same, have larger and larger powers of inverse $Q^2$. So, given that their matrix elements are characterized by some
typical strong interaction scale we’ll be getting higher and higher powers of the strong interactions scale over $Q^2$. Keeping the first few terms should be a good approximation even at 1.8 GeV. It is very helpful that the mass dimensions of the gauge invariant operators start at 4.

The Wilson coefficients, the operator product coefficients $C$ above, obey renormalization group equations. They can be calculated in perturbation theory in the effective coupling at large $Q^2$, of course. However, at $Q^2$ of approximately $m^2$ we cannot simply ignore plausible non-perturbative corrections and still guarantee worthwhile accuracy. A term of the form $\Lambda_{\text{QCD}}^2/Q^2$ would show up, through the mechanism of dimensional transmutation, as a contribution proportional to $\exp(-c/\alpha_s)$ in this coefficient, where $c$ is a calculable numerical constant. It is an important question whether there is such a contribution, because if there were, and they were not under tight control, it is formally of such a magnitude as to ruin the useful precision of the predictions. Such a correction would be bigger than the ones coming from higher operators because these operators have dimension 4, so their coefficients have $Q^2$ over $\Lambda^2$ squared, which is a priori smaller.

Mueller [7] has given an important, although not entirely rigorous, argument that no $\Lambda^2/Q^2$ term can appear. The argument is a little technical, so I won’t be able to do it full justice here but I will attempt to convey the main idea. The argument is based on the idea that at each successive power of $1/Q^2$ one can make the perturbation series in QCD, which is a badly divergent series in general, at least almost convergent, that is Borel summable, by removing a finite number of obstructions. Furthermore the obstructions are captured and parameterized by the low dimension operators mentioned before. Once these obstructions are removed, the remaining (processed) perturbation expression converges on the correct result for the full theory. Neither in the obstructions nor in the residual perturbative expression do the potentially dangerous terms occur – which means that they don’t occur at all.

Maybe I should draw a picture of this [Figure 4]. One has the current product, and one is doing an analysis of its behavior when large virtual momentum is flowing through the current lines. The principle of the operator product expansion is to exhibit the powers of $Q^2$ by breaking the propagators in the graph into hard and soft parts. Any soft part costs you a power of one over $Q^2$ so you want the minimal number. If you just take out a couple of lines you have one of those low dimension operators, so those are interpreted as the operators, with the understanding that now all internal propagators in the graph are hard. This really provides the rigorous definition of these operators, which is non-trivial to do and inevitably introduces scale dependence due to ultraviolet divergences in products of the basic field operators at the same point.
The Wilson coefficients in the operator product are computed from these graphs with hard internal propagators. They obey renormalization group equations, which tell you how they change if the nominal scale dividing “soft” from “hard” is re-defined. These equations lead by the usual arguments to the appearance of the running coupling constant in the graphical evaluation of the Wilson coefficient. Formally, then, because of asymptotic freedom, one can evaluate this coefficient perturbatively. In this evaluation, to all orders of perturbation theory the only scale dependence comes from the dependence of the running coupling on $Q^2$, and it is logarithmic in $Q^2$. You have to show, to complete the argument that eliminated the possibility of power law corrections in perturbation theory, that they don’t arise in the true answer. This is not obvious, because the perturbation theory does not converge. Thus it is overly optimistic to suppose that it converges on the true answer.

There has been a lot of theoretical work, starting with Dyson, on the question why perturbation theory diverges, and how in favorable cases one can salvage valid results from it nonetheless. Although a technical account would be out of place here, let me give you a little taste of how it goes [8]. One starts with a formal perturbation series

$$ f(\alpha) \sim \sum_n c_n (\alpha)^n $$

that does not converge, because the $c_n$ do not fall off rapidly enough with $n$. It may happen however that the modified series

$$ g(\alpha) = \sum_n \frac{c_n}{n!} (\alpha)^n $$

does converge and defines a legitimate function $g(z)$. If so, then

$$ \tilde{f}(\alpha) \equiv \int_0^\infty ds \ s^{-1} e^{-s/\alpha} g(s) , $$

if it exists, defines a legitimate function whose formal power series expansion agrees with that of $f$. The underlying point is that a normal power series is limited in its radius of convergence by the nearest singularity, whereas a more flexible representation may converge in a different shaped region. In quantum field theory there is generally a singularity at arbitrarily small negative $\alpha$; but in favorable cases the Borel transform exists and its inversion uniquely defines a function analytic in a wedge excluding the negative real axis. This function then gives strict meaning to the divergent perturbation series. One can show that when it works this procedure defines amplitudes which satisfy the axioms of quantum field theory such as
causality and unitarity. The usual demonstrations that these properties hold order by order in perturbation theory can be adapted to the re-processed version, which is more complicated but has the virtue of actually defining an answer. In fact we can agree that it gives the answer, since after all the whole point of quantum field theory is to give non-trivial realizations of the axioms, and that is what we have found.

QCD is not quite so favorable as this ideal, which occurs only for massive super-renormalizable theories in low dimensions. There are several known obstructions to Borel summability in QCD, which go by frightening names: ultraviolet and infrared renormalons, instantons, and threshold-induced oscillations. What Mueller did was to analyze these known sources of possible dangerous terms. He argued that the infrared renormalons are essentially just the higher-order terms in the operator product expansion, the ultraviolet renormalons generate singularities in $g(\alpha)$ away from the real axis whose influence on the truncated form of $g(\alpha)$ one actually computes can be minimized by judicious mappings in the $\alpha$ plane, that the threshold-induced oscillations are negligible quantitatively, and that the instanton contribution is both small and in principle calculable.

So now I have fleshed out my earlier description of Mueller’s argument a bit. The key underlying assumption is that the known obstacles to Borel summability are the only ones. In principle, one can test this circle of ideas by calculating the operator product coefficients directly in the full theory (i.e. numerically, using lattice gauge theory techniques). If they were to fail, it would signify that there is an important gap in our understanding of quantum field theory.

On the experimental side, the Aleph group has tested the framework leading to this operator product expansion by comparing the resulting specific predictions for decay into semi-inclusive final states with specific quantum numbers, including the $Q^2$ dependence (which you can look at by looking at final states of different mass) [9]. They got a good fit with no one over $Q^2$ term and with matrix elements of the lowest dimension relevant operators $m\bar{\psi}\psi$, $trG_{\mu\nu}G_{\mu\nu}$ fitted to other experiments. These quantities also appear in other similar applications, where observed hadron parameters are correlated using the so-called QCD or ITEP sum rules, which arise by saturated various operator products. By taking suitable moments one can define quantities that are insensitive to the higher dimension operators, and for these the predictions of perturbative QCD are especially stringent.

I went into some detail into the analysis of tau decay because I think it’s not only important in itself but quite fundamental, and it connects with many other issues. In particular, this kind of argument could potentially provide a rigorous foundation for the QCD or ITEP sum rules which are the basis of a very successful phenomenology. Also similar analyses could be attempted in relatively low $Q^2$
e^+e^- annihilation and possibly in deep inelastic scattering.

As far as the specific issue of determining \( \alpha_s \) is concerned, however, all this will probably soon be moot. The character of the enterprise of determining \( \alpha_s \) and testing QCD quantitatively is, as we speak, undergoing a decisive transformation. It is becoming obvious that before very long direct simulation of heavy quark systems using the full non-perturbative force of QCD, simply by discretizing the theory and doing the integrals numerically (lattice QCD), will provide the best determinations. In this application, of course, one works at reasonably small \( Q^2 \) and includes the whole theory. Also there are directly measurable effects such as hyperfine splittings which are directly proportional to a power of \( \alpha_s \), so that instead of looking for a small correction to parton behavior one is getting the strong coupling parameter out front. When we consider these favorable theoretical features together with the precision possible in the corresponding measurements, it becomes evident that this method is the future of \( \alpha_s \) determination. Indeed, as we heard from Paul Mackenzie, it may be that the future is now [10,11].

So much for determinations of \( \alpha_s \). I’d like to emphasize something that may get lost in the enthusiasm for presenting numbers for \( \alpha_s(M_W) \), and competing to see who has the most exact. That is, that numerical accuracy in determining \( \alpha_s \) is not a completely adequate figure of merit for experimental tests of QCD. Let us suppose, what I think is now eminently reasonable to suppose, that QCD correctly describes many processes in nature – that the quarks, gluons, and Yang-Mills dynamics are here to stay. Then in “testing” QCD perhaps the point of greatest interest is not to continue to verify yet again that these degrees of freedom exist and their mutual interactions are correctly described, but to test interesting hypotheses for new physics. Let me give an example. Yesterday when I was talking with one of my colleagues in preparing this talk, we both had a laugh over how silly it was that people were testing the flavor independence of QCD, checking that their determinations of \( \alpha_s \) in different flavor channels were consistent. Then I thought a little more about it, as undoubtedly the experimentalists had beforehand, and realized that it’s not silly at all. For instance the existence of a Higgs particle would be very interesting. The coupling of a Higgs particle to quarks is typically to the quark mass, and diagonal in flavor. It would therefore show up mostly as a quantitative correction to heavy quark processes otherwise dominated by QCD. If the Higgs particle had vacuum quantum numbers and complicated decay channels events involving its exchange (or production) might not be trivial to distinguish from normal QCD events. However presumably one way it would show up would be as a correction to flavor independence of the nominal \( \alpha_s \).

The moral of this little tale is that qualitative tests of salient features of QCD are neither silly nor obsolete – and besides they’re a lot of fun. So I’d like to spend
a few minutes touring a few of these.

Figure 5 [12] is historic because it shows the verification very first type of quantitative prediction to be extracted from QCD [13]. Little did we dream that it would ever be measured so accurately, but here it is, the $Q^2$ dependence of the non-singlet $F_3$ structure function. This is a particularly clean application of QCD because it comes from the operator product expansion, the corrections are under good control, and the comparatively poorly known gluon distributions do not enter. You see that the structure function does just what it's supposed to do, dropping steeply with $Q^2$ at large $x$ and less steeply at small $x$.

A pretty test, especially satisfying because it is intuitively clear that you are seeing the running of the coupling constant in a single process, is exhibited in Figure 6 [14]. It is the three-jet production rate as a function of energy in $e^+e^-$ annihilation (at a fixed value of the Durham cutoff [2]). This is, of course, a direct measure of the probability of radiating a gluon and thus of its effective coupling. Plotted as a function of energy presented versus an inverse logarithm the rate should lie to first approximation on a straight line, and you see that it works very nicely over a wide range of energies, with the inverse coupling itself changing by about 50%. This was all done originally for $e^+e^-$ annihilation; at the Conference we have a report from Fermilab experiment 665 where a similar phenomenon is now observed in deep inelastic scattering.

Direct evidence for the Rutherford cross-section in jet physics, showing that indeed the dominant hard process in the strong interaction is the exchange of elementary spin one boson, is shown in Figures 7 and 8 [15]. It is worthwhile to emphasize that at very high energy, I think at upgraded Fermilab and certainly at LHC, in the bulk of hard events one will be seeing two gluons scattering into two gluons. Thus the triple gluon vertex characteristic of the nonabelian gauge interaction will be directly responsible for most of what goes on there.

So much for showing that QCD is right. I think there is no reasonable doubt anymore that it is right, in the strongest sense that a scientific theory can be right. That is, QCD will be used for the foreseeable future, in essentially its present form, as the primary description of a wide body of phenomena.

1.2. New methods in QCD perturbation theory

In searching for new physics at high energies, possibly involving new kinds of heavy particles that decay into several light particles and several jets, you can have severe QCD backgrounds. It is therefore quite important to be able to compute complicated processes in QCD. Such computations can get out of hand quite rapidly due to the proliferation of Feynman graphs, each of which corresponds to an amplitude with many terms due to the complicated vertices.
Fortunately, lately remarkable improvements in the technique of calculating complicated processes at high energy in perturbation theory have been discovered. Several insights, including important ones from string theory, have been combined into an approach powerful enough to address processes that would have seemed hopelessly out of reach even a year ago [16]. Perhaps the most stunning example appears in a recent Physical Review Letter by Bern, Dixon and Kosower [17], where they calculated the rate for two gluons goes to three gluons to one loop order in QCD. (Although it is somewhat off the immediate point, I would like to mention that Bern and Dunbar [18] have also computed graviton-graviton processes scatterings in a compact form. If you’ve ever attempted to calculate even very simple graviton processes in perturbation theory, you’ll be suitably impressed.)

The techniques used to do these calculations are of great interest in themselves. They are techniques that arise naturally in string theory, although some of them could have been, and to some extent were, developed independently in other contexts [19,20].

I’ll say a bit more about this subject toward the end.

1.3. The boundaries of perturbative QCD

Lately there have been very pretty and promising developments pushing the edge of perturbation theory, pushing into domains where you can just barely justify the use of perturbation theory, or where it’s starting to break down.

A well known phenomenon that follows from the evolution of the structure functions is that density of wee gluons, as measured in the gluon structure function, piles up at small x as Q^2 grows. This arises because if you look at higher Q^2 it has an admixture of two gluons with smaller Q^2 and smaller x, because the Hamiltonian is not diagonal in the states of definite gluon number, but connects these configurations. If in the evolution the number of gluons piles high enough eventually you have to take into account not only that fragmentation process but also the inverse process: two of the existing gluons already in the nucleon, combine into one gluon. This is a qualitatively distinct and quite interesting process from the fragmentation, because it reflects the properties of the nucleon – the nucleon can no longer be regarded as just a container for a dilute gas of quarks and glue, even at large Q^2.

This effect is estimated to be quantitatively small until you get to very small x, but it may turn out to become very important as you get to exceedingly low x as is being probed at HERA. Presumably balancing the fragmentation against the recombination leads to a saturated gluon density which is proportional to one over Q^2 times one over some radius^2 over which the gluons are spread. (This
calculation should really be done in the infinite momentum frame, so there are only 2 transverse directions.) The fragmentation and recombination are of the same order, so it’s a non-perturbative problem to find the balance. Fortunately however there is still a small expansion parameter one over $Q^2R^2$, and progress can be made [21]. I’m giving you a cartoon introduction to the theory, which is actually very technical and difficult to understand in its present state.

Another way of thinking about the main phenomena is to think of it as shadowing, that is as the effect that gluons as they are attempting propagate through the nucleus can be absorbed by other gluons. But here, unlike in any other situation in physics I can think of, we have a case of significant shadowing by simple fundamental objects, namely the gluons, and at small coupling. Thus it is an interesting challenge actually to calculate it quantitatively. This is I think a very promising area at the edge of perturbation theory, where fundamental ideas about diffractive scattering can be tested in a precise way.

Another related possibility I find very intriguing is suggested in a recent paper by Balitsky and Braun [22]. Their idea is that in this small $x$ region where you have lots of soft gluons they can gang up, so to speak, and produce instantons. Balitsky and Braun estimate in fact that the instanton contribution to the $F_2$ structure function is 2 to 5% at $Q^2=400$ GeV$^2$. That’s quantitatively small as a fraction of the cross-section, but the events involved would be quite striking, they would have a high multiplicity proportional to $\frac{2\pi}{\alpha_s}$ in one rapidity unit with large transverse momentum. That’s what you would find accompanying the current jet. This suggestion of Balitsky and Braun definitely needs more work to make it quantitative and precise but I think it is extremely promising. It is closely related to problems people discuss in the electroweak interactions, where it has even been suggested that the electroweak interaction becomes strong due to contributions from instantons at high energies. I think for the weak interaction this is a very dubious proposition, but in the Balitsky-Braun process you have a version of the same ideas which might be sensible and accessible to experiment. Certainly we can look forward to learning interesting things about quantum field theory by being forced to address, under the prod of ongoing experiments, challenging problems at the frontier of perturbation theory.

Finally, on a different edge of perturbation theory, I’d like to show you one little tip of a very large iceberg [23], a simple representative coherence effect, just to show the inadequacy of overly naive partonology. (See Figure 9). This is work by L3 collaboration; I think it’s quite pretty. The question is: do jets fragment independently? They do according to a parton model; that’s the core assumption of the model. What’s measured to test this is what happens if you have a three jet event by an appropriate criterion which cuts on the invariant mass in the jets,
and then make the resolution finer, so you’re demanding that the jets have smaller invariant mass. What is the probability that these jets, when looked at finer, fragment?

Now if the jets fragmented independently you would get a very large probability from the three jet events because the gluon has a large color charge and just adds to the other two fragmentation probabilities for just the quark and antiquark. Thus you’d have a large contribution which simply adds to the two-jet contribution. But in QCD these things are not independent, there is interference between the different processes because they are described by quantum-mechanical amplitudes, not probabilities. The accurate calculation shows that the fragmentation probability, which according to the incoherent model goes up, instead should actually, according to QCD, go down. The accurate calculation agrees very nicely with the data.

Thus we have both quantitative and qualitative tests of the elementary processes of QCD, and also “elementary” processes that are at the hairy edge of existing quantitative methods.

2. More Complicated Processes

2.1. QCD as a Service Subject

Given that QCD is right, it ought to become a service subject for many other parts of physics and perhaps ultimately for natural philosophy.

Here is a little table [Figure 10] summarizing some of the services we should expect QCD to provide for fields of physics. In every case there is much room for improvement, and these fields of “applied QCD” present many intellectual challenges.

- For high-energy physics: It may seem peculiar to speak of the application of QCD to high-energy physics, since QCD is usually considered a part of high-energy physics. However “today’s sensation is tomorrow’s calibration”, and if one defines high energy physics by its frontier, by the search for new fundamental laws, then one may properly speak of QCD as a service subject for high-energy physics.

As I mentioned before, experimentalists doing experiments at the highest energies rely heavily on QCD to estimate their backgrounds. Their very specific and concrete needs are stretching the limits of perturbative QCD technology, as I mentioned, both for calculating complicated processes at the level of quarks and gluons and for turning these calculations into quantitative predictions for practical observables (hadronization and jet algorithms).
There is also another field of application of QCD to high energy physics with quite a different flavor [24]. One often attempts to extract information about weak interaction parameters (e.g. Kobayashi-Maskawa angles and CP violation parameters) from measurements of processes involving hadrons. In the quantitative interpretation of such measurements, uncertainty in estimates of hadronic matrix elements of known operators is frequently the limiting factor. It is quite embarrassing that we still don’t have a really solid understanding of the origin of the $\Delta I = 1/2$ rule. On a brighter note, in the past few years simple but profound insights regarding the universality of heavy quark couplings have blossomed into a richly detailed and useful phenomenology [25].

These applications reflect the maturity of QCD. It is taking its place beside its venerable sister, QED, as a model of respectability and service. The applications mentioned in the previous two paragraphs are rather precisely parallel to the inclusion of QED radiative corrections in high-energy processes and the use of QED to calculate atomic properties for atomic parity-violation experiments, respectively.

- **For nuclear physics:** In principle, of course, QCD provides the logical foundation for nuclear physics in the same sense the QED provides the logical foundation for chemistry. In fact I think this analogy is quite appropriate, because in both cases there is a wide gap between principle and practice, and for similar reasons. Chemistry will presumably always use its own semi-phenomenological language and methods (valence bonds, molecular orbitals, ...), only loosely connected to fundamental concepts of QED, because the questions of most interest to chemistry involve delicate balances of effects at energy scales of small fractions of an electron volt, whereas the simple fundamental interactions are characterized by scales of several electron volts. Similarly nuclear physics will use its own semi-phenomenological language and methods (shell and liquid drop models, pairing correlations, ... ) only loosely connected with fundamental concepts of QCD. This is because the energy scales of interest to nuclear physics are an Mev or less, whereas the natural scale for the fundamental interactions of QCD are at least a hundred times larger. It is way too much to expect .1% calculational control of QCD in the foreseeable future as a practical matter. On the other hand it does not seem unreasonable to hope that major qualitative aspects of nuclear phenomenology involving large energy scales, such as the existence of the hard core, the saturation of nuclear forces, and the rough magnitude and distance dependence of the tensor interaction, from fundamental principles.

Perhaps more glamorous is the prospect of exploring nuclear physics in extreme or unusual circumstances, where the fundamental interactions are less thoroughly balanced and masked. Such possibilities arise for example when one considers baryonic matter with high strangeness content, at high density and/or temperature,
or when equilibrium is violently disturbed as in relativistic heavy ion collisions. I shall say more about these various possibilities below.

- For astrophysics: Extreme nuclear densities are reached in supernova collapse and in neutron star cores. There are some very interesting QCD-based speculations for what might happen under these conditions, as I shall discuss momentarily.

- For cosmology: Big bang cosmology forces you to consider the behavior of QCD at extreme temperatures, since it leads one to expect that a QCD plasma of quarks, antiquarks and gluons filled the Universe and dominated its matter content for at temperatures above \( \sim 100 \text{ MeV} \). These occurred during the first 1/100 of a second or so after the Big Bang. That might seem like a short interval of time, but of course it is larger by many orders of magnitude than the mean free time between collisions at that temperature, so that from the point of view of the quarks and gluons things were developing at a very leisurely pace and (in the absence of hysteresis, i.e. supercooling) thermal equilibrium should be accurately maintained. So for the foundations of Big Bang cosmology it is important to understand the nature of near-equilibrium QCD at high temperature and essentially zero baryon number – its equation of state, possible phase transitions, etc. (Except in very heterodox models of baryogenesis, the asymmetry between quarks and antiquarks was a few parts in a trillion at these early times, so the approximation of zero baryon number is appropriate.) We do have some pretty good theoretical ideas and numerical simulation results on these questions, as I'll discuss below.

- For numerical experiments: Finally, there is the charming domain numerical experiments. Here you have great flexibility: you can simulate different numbers of massless quarks, you can use SU(2) or E\(_8\) as your strong interaction gauge group, and so forth. Of course I'm too young to remember it, but I've read that in the 60's there was a graffito that appeared in Berkeley and became popular, which read \textit{Reality is a crutch}. This is a profound truth [26]. In any case, a deep and intellectually satisfying understanding of QCD must include ability to anticipate how its predictions change as you change its parameters; something one can only explore “experimentally” by numerical simulation. Numerical experiments also allow you to look at the underlying dynamics in simple and directs ways that are not at all practical in real experiments: for example, one may look at the statistics of fluctuations in the gluon field, look for instantons, (perhaps) study the fully chiral symmetric theory with either two or three massless quarks ... .

- For natural philosophy: With the success of QCD the “radically conservative” principle that the behavior of the physical world is captured in universal mathematical laws is once more triumphant. QCD gives no encouragement to doubts about the foundations of quantum theory. It is firmly based on received principles of quantum field theory. Indeed, it allows tests of these principles, including
the effects of virtual particles and the detailed workings of renormalization theory, far more extensive and in many ways more stringent than was possible before. I think there are interesting reasons not to be entirely satisfied with this outcome, however, as I'll mention at the end.

2.2. Hot QCD [27]

At temperatures well above the QCD scale, QCD is expected to be qualitatively different from what it is at $T = 0$. Confinement should no longer be taking place, the chiral condensate should melt or ionize. It is very interesting, of course, to ask, are there sharp phase transitions associated with these phenomena and if there are, what is their nature?

The Wilsonian renormalization group technology based on the concepts of scale invariance and universality gives an entry into this question, because it tells us that to get some insight into these questions you don’t have to analyze QCD itself but just a theory with the same symmetries and very long wavelength modes, something in the same universality class. Furthermore if we are interested in the properties nearest second order phase transition we should look for a scale invariant theory in the same universality class. Such theories are very hard to come by. If we find one then there is a good chance that we have actually found a model that describes important aspects of the behavior of QCD near the phase transition precisely, because all theories in the same universality class exhibit the same behavior. I want to emphasize that we are speaking of precise predictions even though we are dealing with a strongly interacting theory under conditions that sound horribly complicated.

Working out this program produces candidate second order phase transitions for the confinement/deconfinement phase transition in pure glue SU(2), which in the universality class of the Ising model, but not pure glue SU(3). For chiral symmetry restoration we have a candidate second order phase transition for two massless quarks, which is in the universality class of a 4-component Heisenberg magnet, but no candidate for three or more massless quarks. Thus you arrive at seemingly bizarre, highly nonintuitive predictions that whether one has a second order phase transition or a first order one, that is whether the free energy is continuous (but not analytic) or discontinuous at the transition depends on the exact number of colors or the number of quarks.

Remarkably, the evidence from lattice simulations is fully consistent with these ideas so far [28]. That is for SU(2) you have a second order phase transition with at least the rough character of the phase transition of the Ising model, for color theories with several massless quarks you have what appears to be a second order phase transition for two massless quarks but a first order transition for three or
more. (Actually the simulations are not yet good enough to convince a reasonable skeptic; but it is fair to say that they are consistent with prior theoretical expectations, and do show striking changes of behavior as the number of species is varied.)

If there is a second order phase transition, that is if we do have a scale invariant theory and a continuous transition near the critical point then one can make obtain precise predictions for QCD simply by going to the library and looking up calculations done by our brethren in condensed matter. It is quite arduous to obtain predictions for the scale invariant theory, even for the Heisenberg magnet model (that is, for the symmetric n-component massless \((\phi^2)^2\) theory), because it occurs at a large value of the coupling constant. In a remarkable tour de force Baker, Nickel and Meiron calculated 353 graphs for the wave function renormalization and 789 graphs for the vertex renormalization in the Heisenberg magnet model (that is, the symmetric n-component \(\phi^4\) theory) – see Figures 11 and 12 – estimated the higher orders using semiclassical techniques, and used Borel summation techniques on a suitably transformed coupling constant. At the end of the day you obtain, from a theory whose coupling constant is of order 1 in sensible units, two significant figures in predictions for the critical exponents characterizing the behavior near the phase transition. So, for instance, the specific heat is supposed to go like this:

\[
C(T) \sim A_\pm |t|^{-\alpha} + \text{less singular}
\]

for \(t \equiv |T - T_c|\) with coefficient \(A_+\) above and \(A_-\) below \(T_c\). Here \(\alpha = -0.19 \pm .06\) and \(A_+/A_- = 1.9 \pm 0.2\); thus it has a cusp with a specific shape above a continuous background. This is one among a host of predictions. There is, for example, a precise prediction for the functional form of the condensate \(\langle \bar{q}q \rangle\) as a function of temperature and intrinsic quark mass near the transition temperature [Figure 13]. This is the sort of thing that numerical simulations will presumably be able to do well in the long run. Present results [Figure 14], while encouraging qualitatively, are far too crude for detailed comparison [29]. Maybe by the year 2000 we’ll have it; the situation doesn’t look much worse than what we had in the early days of testing for anomalous dimensions in deep inelastic scattering.

There is a lot more to say about these phase transitions, but perhaps these highlights give you a flavor of what’s possible. I think it is quite a remarkable thing that you can get precise predictions for the behavior of QCD, in these specific circumstances, at long distances and high temperatures.
2.3. Equation of state; QCD astrophysics

Now I would like to discuss a specific problem in astrophysics where better understanding of QCD would be very valuable. Kaplan and Nelson [30] proposed, and Politzer and Wise [31] have analyzed more quantitatively, the possibility that in dense nuclear matter $K^-$ mesons condense. Formally, this means that the expectation value $\langle \bar{u}\gamma_5 s \rangle \neq 0$. Its existence would mean that portions of “neutron” stars with such high densities would in fact contain protons together with a background Bose condensate of $K^-$ mesons. The physical mechanism underlying the possibility of $K^-$ condensation is simply that there is a strong attractive interaction between these mesons and nucleons, which lowers their effective mass\(^2\) perhaps through zero in a dense medium. These authors concluded that the possibility of such condensation is not precluded for baryon number densities roughly three times the usual density of nuclear matter or more.

In a very intriguing paper Bethe and Brown [32] propose that this effect softens the nuclear equation of state, and makes cold neutron stars with masses greater than 1.5 solar masses unstable to collapse. If true, this implies that most progenitor stars with masses greater than about 18 solar masses wind up as black holes after a supernova explosion and stars with mass greater than 30 solar masses go directly into a black hole. This potentially solves a mystery in the skies, the mystery of where is the star in Cassiopeia A? There was a supernova explosion there in historical times, somewhere in the period 1659-1675, as one infers from the size and motion of the remnant ejecta. Cassiopeia A it is one of the strongest radio sources in the sky today, but there is no pulsar or thermal x-ray source in the neighborhood, as one might expect to see for a remnant neutron star. This is actually an instance of a larger puzzle: after an exhaustive study of radio, optical, x-ray and $\gamma$-ray surveys Helfand and Becker [33] came to the startling conclusion that nearly half the supernovae in the galaxy leave no observable remnant.

Bethe and Brown suggest that in most of these cases there is no neutron star because the progenitor collapsed into a black hole following the supernova event. They point out that there is a developing crisis with supernova 1987a, which hasn’t yet revealed any sign of a neutron star in the middle, and they suggest that in that case too there is a black hole. If they’re correct there shouldn’t be any neutron stars heavier than about 1.5 solar masses.

So there is a test of the theory of K-condensation, or at least this consequence of K-condensation, there shouldn’t be any neutron stars with masses above about 1.5 solar masses. Presently only the Vela pulsar challenges this bound; it is reported to be 1.7 solar masses but there are large uncertainties in the measurement.

In any case I think its a fascinating possibility and a great challenge that con-
siderations from QCD potentially play a crucial role in astrophysics. If we could do better calculations, and reliably decide under what circumstances K condensation and whether it or any other effect drastically softens the nuclear equation of state at high density we would be performing a real service for astrophysics and nuclear physics.

2.4. Strange matters

A fascinating possibility, suggested by Witten and others [34] in the early 80’s and analyzed in some depth by Jaffe and others [35], is the possibility that there are compact stable or metastable states of finite baryon number carrying large strangeness, and with much higher density than ordinary nuclear matter. It is an outstanding challenge in QCD to decide whether such states in fact exist, and to calculate their properties. The simplest candidate is the so-called H particle, which has the quantum numbers of the (uuddss) 6-quark configuration. Bag model estimates are formally consistent with the possibility that this would actually be stable; within the uncertainties it might also be below the Λ Λ threshold and thus unstable only to weak decays, or above this threshold and therefore presumably highly unstable. There is an active effort to find this particle. It clearly would be highly desirable to have a reliable (accurate, though not necessarily extremely precise) theoretical estimate for its mass; to my mind it is something of a scandal that lattice gauge theory has not produced one for us.

Less radical but perhaps more sure-fire, and still extremely interesting, are loosely bound nuclear molecules, some of which are predicted to be stable against strong decay, but of course not against weak [36]. The spectroscopy of such states might provide a testing ground for low-energy QCD which is complementary to, and perhaps fresher than, conventional nuclear physics.

2.5. Heavy ion collisions [37]

Parton cascade models, which although crude can hardly be wrong qualitatively at high energies, suggest that at proper time 1.5 fermi over c following the collision for gold on gold at 100 GeV per nucleon, that is under the conditions attainable at RHIC, gluons attain a effective temperature of 325 MeV. At LHC the attainable temperature is estimated to be at least twice as large. The quarks are found to be in kinetic equilibrium, that is their kinetic energies are characterized by a Boltzmann distribution with the same kind of temperature, but do not reach chemical equilibrium. There aren’t quite enough quarks and anti-quarks for the given temperature.
In any case, the estimated characteristic temperatures are well above the nominal QCD phase transition temperature, which is probably not far from 150 MeV according to lattice simulations. So the coming heavy ion collisions will certainly take us into qualitatively new regimes, where the fundamental constituents of hadronic matter and their symmetries come into their own.

Unfortunately these regimes are only reached fleetingly in the initial fireball, and what one actually gets to observe is what emerges from it after expansion and cooling, a final state containing many hundreds or even thousands of particles. Under these conditions there is almost limitless potential for uninspiring measurements that have no crisp interpretation. However, there are also a few important qualitative effects so distinctive that their signature might be discernible even in this noisy environment.

First, there is vastly greater entropy in a quark-gluon plasma than in a pion plasma at the same temperature, because there are 8 colored gluons that come with two helicities, plus two or three relevant flavors of colored quarks and antiquarks with two helicities, as opposed to a measly 3 scalar pions. Making some bold but not unreasonable approximations to set up the hydrodynamics of the expansion, one can work out the observable consequences of radically altered equation of state at high temperature (if the quark-gluon plasma forms). Crudely speaking, it is that there should be a higher phase-space density in particles boiling off at the early stages, which are generally those with the highest transverse momentum [38]. It would be quite gratifying, though hardly shocking, to see that this dramatic increase in entropy actually occurs.

A second thing that might happen – this is much less certain – is that the chiral condensate melts [39]. The temperature is certainly high enough, and the only question is whether this degree of freedom equilibrates in time. If the chiral condensate does melt, then as it cools it will re-solidify. It is as if you heated a magnet past its Curie point and then cooled it back down; it will spontaneously magnetize again – but perhaps with the spins all aligned in a different direction! Now in QCD it is not quite true that all directions for the chiral condensate are equivalent; that would be true if the light quark masses were rigorously zero, but they are not. Nevertheless in the initial stages the difference in energy density between alignments in different directions is much less than the ambient energy densities, or the energies which correlate the relative alignment at different points in space. So there will be quasi-stable configurations where the chiral condensate is assigned in the same wrong direction over a large region of space. Putting off for a moment the question whether it is plausible that such regions actually form, let us consider how they would evolve once formed. They will relax coherently toward the correct direction, emitting coherent waves of pions as they do so – we have a
sort of pion laser. The radiation will consist of many pions with a fixed ratio of charged to neutral, and low relative moment. One can work out the distribution of the neutral to charged ratio from simple geometry; it is

$$\text{Prob}(R \leq \lambda) = \frac{1}{2} \lambda^{-\frac{1}{2}},$$

where

$$R \equiv \frac{N_{\pi^0}}{N_{\pi^0} + N_{\pi^+} + N_{\pi^-}}.$$

This is of course markedly different from the Gaussian centered at 1/3 one would expect for uncorrelated emission, and could be very distinctive even for rather modest numbers of pions in an emission region (defined by small relative momenta).

Do such regions form? Near equilibrium, we are really just asking whether the correlation length becomes large near the phase transition. That’s the signature for large correlated regions. It sounds promising at first hearing that there is likely a second-order chiral transition for massless quarks, since that is precisely the situation where you expect long correlation lengths. Unfortunately the quark masses are not zero, and for this application they are too large. One measure of this is that the effective pion mass at the nominal transition temperature is comparable to the temperature itself (rather than zero as it would be for massless quarks), so that a correlation volume will only contain enough energy to create a small number of pions.

An interesting physical mechanism for producing large correlated regions, which might or might not be a good idealization of what happens in a heavy ion collision, is amplification after a quench. A quench is what happens if you suddenly change the temperature of a system, removing the energy in highly excited modes but not in low-lying modes which do not have time to equilibrate. The prototype quench is to plunge a hot iron bar suddenly into ice water. The free energy functional changes suddenly from being the free energy functional appropriate to high temperature to the one appropriate to zero temperature or very low temperature, which (if we have cooled through a phase transition temperature) may lead to a change in symmetry. In terms of pion dynamics specifically we have the following. The frequency of oscillation of the pion field is given by a dispersion relation

$$\omega^2 = k^2 - \mu^2 + 2\lambda v^2$$

where \(v\) is the expectation value of the magnitude of the chiral condensate, \(\lambda\) is the coupling and \(-\mu^2\) the (negative) bare mass\(^2\). In the ground state the two last terms on the right cancel, and we have massless Nambu-Goldstone bosons. (I
am ignoring the quark masses at this point; they do not alter the essence of the matter.) However if we start with $v^2 = 0$, as at the beginning of a quench, then the effective mass $\tilde{m}^2$ is negative, and the frequencies are imaginary indicating the possibility of exponential amplification. The amplification factor is larger, the smaller is $k^2$ – thus long-wavelength modes are the most enhanced, as we desired to show.

Finally one expects copious production of strange quarks and antiquarks at high temperatures, so that conditions are favorable for production of the strange matters discussed above, and it will be important to search for them in the debris of heavy-ion collisions.

3. Foundational Issues

Now I’d like to discuss some outstanding foundational questions. The purpose of this part is more to raise questions than to provide answers.

3.1. Does perturbation theory suffice?

We have already touched on to some extent on the question of how far can one push perturbation theory. We argued, following Mueller and others, that it is possible to push it quite far as an asymptotic expansion for large momenta. What is the general situation?

It is very common for people to talk loosely as if there were one thing called perturbation theory and another thing called non-perturbative effects, as if non-perturbative effects were a world apart. However it is also conventional wisdom – although perhaps unconscious conventional wisdom – to think that perturbation theory in fact determines the theory completely. Indeed it is a very simple matter to read off the Lagrangian from low-order graphs, and the Lagrangian is normally supposed to determine the theory completely, even non-perturbatively.

This “determination in principle” is of course a very different matter from claiming that the perturbation theory converges, or providing a constructive procedure to go from it to the correct answer. I am not aware that anyone has even made a really plausible conjecture for how to do it, in the spirit of Borel resummation. (Borel resummation itself will not work, for reasons mentioned before.)

If one could find it, such a procedure would be informative in many ways. For one thing, it would allow one at least to truly define chiral gauge theories, and to regulate QCD in a manner that is manifestly chirally symmetric. We certainly know how to construct the perturbation series for such theories, which I remind you include such interesting examples as the Weinberg-Salam model and essentially
all realistic unification schemes. It is less clear that we know how to regulate them non-perturbatively (but see below). It could also be instructive for string theory, where one has very little besides the perturbation expansion to go on, and the same problems arise in what appears at least superficially to be a more severe form.

Now actually it is not quite true that the perturbation theory defines QCD completely. QCD has – it is commonly believed – one additional parameter, the famous angle $\theta$, whose effects do not show up in any order of perturbation theory. Nevertheless I believe there is a visible signal for the existence of such a parameter in perturbation theory. In analyzing the high order behaviors of the coefficients in perturbative expansions, one finds that classical solutions of the Euclidean field equations play a crucial role. They induce specific forms of the growth of high-order coefficients in the perturbative expansion of all amplitudes, which can be inferred from the classical solution. Conversely, by comparing the high-order coefficients in the perturbative expansion of various processes one can reconstruct the classical solution. In this way QCD instantons, to be specific, are visible upon close scrutiny of perturbation theory. Now whatever resummation procedure one uses to take care of these contributions, it must contain an ambiguity corresponding to the possibility of introducing different values for the $\theta$ parameter. Based on the standard formal analysis of the continuum theory [40], I suspect that one will find that the resummed series will not define amplitudes which obey the cluster decomposition axiom unless one adds together weighted sums of graphs for perturbatively distinct processes in defining the amplitudes, just as one must pass in the formal continuum analysis from $n$-vacua to $\theta$-vacua.

This is an example of how supposedly non-perturbative effects such as axial baryon number violation in QCD with massless quarks – or just plain baryon number violation in SU(2)×U(1) – can and should be visible, in principle, in perturbation theory. It also brings home, I hope, that the problem of behavior of high orders and resummation in perturbation theory is definitely not an insipid technicality; it encodes deep yet tangible physical phenomena. It natural to ask: are there specific perturbative signatures of confinement? of chiral symmetry breaking?

### 3.2. Chiral symmetry on the lattice?

There have been promising suggestions recently for how to put fermions on a lattice, while respecting chiral symmetry [41]. Can these methods be made practical, and allow one to accomplish what one normally hopes to accomplish using lattice techniques? Specifically: can one compute a strong-coupling spectrum that does a reasonable job on the pseudoscalar mesons – both the octet and the $\eta'$? Can one use the scheme together with importance sampling, for practical numerical work?
3.3. What are the string rules telling us?

I mentioned before that there are new methods for doing QCD perturbation theory, which seem to be remarkably efficient for some purposes, whose formulation has been largely guided by string theory techniques. The central result of the analysis is a set of rules, different from the Feynman rules, for graphs. It must be emphasized that at the end of the day there is no reference to strings; the rules are rules for calculating quantum field theory processes.

To my knowledge there is no straightforward derivation of the rules that gives complete insight into their origin. (For some partial successes, see [20, 42].) It is quite suggestive that rules can be formulated in a way that involves following color flows and spin flows through the diagrams [20]. So we are led to ask, concretely: is there a first-quantized action for particles, including internal degrees of freedom, that naturally leads to the string rules [43]? One could conceivably hope to abstract from the relationship between the properly formulated particle theory and its field theory, insight into the poorly understood relationship between strings and their field theory.

3.4. How does God do it?

Finally I would like to mention a question that in various forms has bothered a few physicists [44], and bothers me too. It is that in every approach I know to defining QCD, one must appeal to completed infinities. Specifically, in a lattice approach one must say that the real amplitude is computed by calculating on a lattice with a given spacing and then taking the limit as the spacing goes to zero; in the (incompletely formulated) approach of resumming perturbation series one will almost certainly have to compute every term in an infinite series, perform analytic continuations, ... . These tasks are extremely computationally intensive: in fact, they call for an infinite amount of computation. Yet somehow the good Lord manages to get the results quite effortlessly in small amounts of time. Does this indicate that the mathematical notion of computability is fundamentally different from the physical one? Can one imagine physical systems with more computational power than Turing machines? Are non-trivial quantum field theories in a finite volume examples of such systems? Or does a careful consideration of limitations on the actual observables show that most of this apparent computational power is never really called forth?

One aspect of these questions, I suspect the deepest, is whether the existence of an infinite number of degrees of freedom in any finite volume is physically acceptable. Closely related problems arise in a slightly more conventional physical context, in the physics of black holes. When one takes the ground state (or any
reasonable excited state) of any reasonable quantum field theory, and traces over
the degrees of freedom inside a specified spatial region, the entropy of the resulting
density matrix is infinite [45, 46]. In the presence of a black hole one is forced to
take just such a trace, over the inaccessible degrees of freedom inside the horizon.
(In fact the difference between flat space and a black hole in this regard is probably
very slight, because the crucial correlations are those between nearby points just
inside and just outside the horizon, and for a large black hole the curvature there
is small.) This is an infinite correction to the Bekenstein-Hawking entropy, which
presumably is physically undesirable [45]. Since the source of this infinity is an
ultraviolet catastrophe, it might be relieved in string theory; but the calculations
necessary to reach a definite conclusion in this matter seem quite challenging [47,
48].

So perhaps there are signs that the deep principles of quantum field theory
that QCD embodies so perfectly are not the last word about ultimate reality.

There is much more to say about each of the topics I have mentioned, and there
are many important, vital topics in QCD that I have not been able to mention at
all, for which I apologize. My summary of the status of QCD was given at the
beginning, and I hope that now you find it more plausible.

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FIGURE CAPTIONS:

Figure 1: Determinations of the effective coupling $\alpha_s(Q^2)$, compared with QCD predictions (updated from Bethke, Ref. [2]. I thank Professor Bethke for supplying this Figure, and also the following two.)

Figure 2: Summary of theoretical foundations and uncertainties for the determinations in Figure 1.

Figure 3: Tabular form of Figure 1.

Figure 4: Cartoons referred to in the analysis of current products.

Figure 5: The variation of the structure function $F_3(x, Q^2)$ with $Q^2$. QCD predicts an $x$-dependent logarithmic decrease, reflecting the softening of the effective quark distribution by gluon radiation.

Figure 6: Relative percentage of 3-jet as opposed to 2-jet events in $e^+e^-$ annihilation. This reflects directly the strength of the effective coupling, since the third jet arises from this coupling.

Figure 7: The angular distribution of jet in pp collisions, if they are supposed to arise from exchange of quarta with different spins, or from QCD.

Figure 8: The actual data, showing a good fit to QCD.

Figure 9: Data regarding a coherence effect in jet physics, explained in the text.

Figure 10: QCD as a service subject: what it can offer other fields, and conversely what questions in QCD are stimulated by other fields.

Figures 11, 12: Some of the graphs calculated by Baker, Meiron, and Nickel to obtain accurate critical exponents.

Figure 13: An example of the information that can be extracted from analysis of critical behavior in 2-light flavor QCD.

Figure 14: Numerical data, of the kind ultimately will be confronted with Figure 13.
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