Opportunities, Challenges, and Fantasies in Lattice QCD

Frank Wilczek\textsuperscript{a}∗

\textsuperscript{a}Center for Theoretical Physics
Massachusetts Institute of Technology
Cambridge, MA 02139-4307

Some important problems in quantitative QCD will certainly yield to hard work and adequate investment of resources, others appear difficult but may be accessible, and still others will require essentially new ideas. Here I identify several examples in each class.

There has been a notable renaissance of interest and progress in QCD over the last few years. This has come about for many reasons, including

\begin{itemize}
  \item a vigorous experimental program in heavy ion physics explicitly devoted to exhibiting the fundamental dynamics of quark and gluon degrees of freedom and to recreating conditions last seen in the Universe at $\sim 10^{-2}$ s following the Big Bang,
  \item a vigorous experimental program in heavy quark physics, including measurements of fundamental CP violation and weak mixing parameters, that requires accurate theoretical calculation of strong matrix elements to reach its full potential,
  \item creative developments in finite temperature theory, effective field theory, and numerical algorithms that dovetail beautifully with these experimental programs,
  \item realization that high density is a regime of QCD in which weak coupling but non-perturbative methods can be used to give tractable yet rigorous models of confinement and chiral symmetry breaking, with possible application to neutron star interiors (or quark stars!? or strangelets!?)
  \item development of lattice regularizations that incorporate chiral symmetry,
  \item incremental developments in algorithms and hardware that have now matured to the extent that lattice gauge theory has become a powerful tool capable of providing reliable quantitative information genuinely useful for guiding and interpreting experimental work, and
  \item increasing anticipation of new frontier hadronic accelerators, the upgraded Tevatron, and especially the Large Hadron Collider (LHC), where QCD will dominate both input and output. It will provide a challenging “background” against which to analyze and interpret any essentially new phenomena.
\end{itemize}

Lattice gauge theory has been an essential ingredient in this ferment. This subject now appears, I believe, more promising and more important than ever before. It is blessed with many attractive research programs on various time scales. We can classify these roughly into:

\begin{itemize}
  \item opportunities – significant problems that will almost certainly yield to hard work and adequate investment of resources in a reasonable, predefined period of time,
  \item challenges – more ambitious problems, which may or may not yield to incremental development of known techniques, and
  \item fantasies – grand problems that we can articulate, but presently lack usable tools to address.
\end{itemize}

∗The research is supported in part by funds provided by the U.S. Department of Energy (D.O.E.) under cooperative research agreements Nos. DE-FC02-94-ER40818 and DE-FG02-91-ER40676. MIT-CTP-3337.
I’ve had fun using this framework to think strategically about the future, in general. I’ll use it here to touch on three major branches of lattice gauge theory – few-body, many-body, and conceptual/algorithmic aspects – by providing a couple of examples in each category.

1. Fundamental Particle Physics

1.1. Opportunities

1.1.1. The Best Determination of $\alpha_s$

A major triumph of lattice QCD is already enshrined in the Particle Data Book, as shown in Figure 1. Nonperturbative calculations of heavy quark spectroscopy provide a precision determination of $\alpha_s$, competitive with the most accurate determinations from perturbative QCD. There is probably more room for improvement on the nonperturbative side, because on that side the relation of the calculations to observables are in principle more tightly controlled (no structure functions, jet parameterizations, ...), the experimental measurements are very precise, and one is accessing $\alpha_s$ (or $\alpha_p^0, p > 0$) effects directly, rather than digging them out as corrections. It is important to pursue this direction further, for several reasons.

Most obviously, a better determination of $\alpha_s$, by sharpening input to the perturbative framework, supports more precise predictions for high-energy experiments. Also, if we are to have confidence in using lattice QCD as a mathematical tool for determining weak matrix elements, we need to make sure it gives consistent, accurate results for a variety of quantities in spectroscopy and electromagnetic transitions, where the underlying physics is securely known. These quantities, of course, are just those that go into the lattice QCD determination of $\alpha_s$.

Precision determination of $\alpha_s$ will also add to the power of the unification of couplings calculation, which at present provides our best quantitative handle on fundamental physics beyond the standard model. The central value is on the low side for supersymmetric grand unified theories, but perhaps within the margin of plausible threshold corrections. If and when we start to pin down the low-energy supersymmetric spectrum, we will be able to leverage this precision to gain insight into how unification symmetry and supersymmetry are broken.

Finally, I think it’s very legitimate to regard precise determination of $\alpha_s$ as an end in itself. Together with the electron mass, the ordinary fine structure constant $\alpha$, and the up and down quark masses, $\alpha_s$ is one of a handful of parameters that determines the structure of ordinary matter. It is a unique glory of physics that we demand precise, quantitative agreement between our calculations and reality, whenever comparison is possible. Enough said.

1.1.2. Exploring Canonical Structures

The fact that QCD is a tight, conceptually defined theory implies that the structures it produces are canonical.

Let me explain what I mean by this, by way of an analogy with black hole physics. A feature of black holes that makes them especially fascinating and attractive is that their theory is so clean. The key to this cleanliness is that, as Wheeler put it, “black holes have no hair”. Specifically, classi-
general relativity produces unique predictions for the properties of a black hole, given only its mass and angular momentum. This is quite different from stars or ordinary matter, whose structure depends on many parameters including elemental composition, temperature, and processing history. The canonical, nearly parameter-free structure of black holes means that it is worthwhile to study solutions of the equations that govern them minutely, because these solutions are sharply delimited.

In QCD the structure of all the hadrons is canonical, and the structure of protons and neutrons is especially so since they are stable particles, i.e., discrete eigenstates of the Hamiltonian. Nucleons are much balder than black holes, for they have no hair, not only classically but even in quantum theory. Furthermore, there is no freedom to specify their mass and angular momentum arbitrarily. Also protons and neutrons can be well modeled in QCD Lite™ (the idealization of QCD using only massless u and d quarks). In that approximation, they are entirely conceptually determined structures, allowing no continuous parameters whatsoever!

And they can be accessed experimentally. Indeed, the answer to many “practical” questions relies on understanding their structure. For example, structure functions are vital to the interpretation of accelerator experiments, since we use nucleons as projectiles. Notably, the primary mechanism for Higgs particle production at the LHC will be gluon fusion, \( gg \to h \) through a top-quark loop. To predict the rates, and to check for possible deviations from standard model expectations for the \( h \) coupling, we obviously need to know the gluon distribution in the proton accurately.

Another sort of canonical structure in QCD is the flux tube between heavy quark sources. There are very interesting questions concerning its spatial structure and modes of excitation, and specifically how accurately it can be modeled as a bag or an elementary string.

1.2. Challenges

1.2.1. Precision Values of the Quark Masses

\( m_u \) and \( m_d \) In contrast to the beautiful and inspiring situation regarding \( \alpha_s \), our present determinations of \( m_u \) and \( m_d \) are an embarrassment. They are uncertain at the factor-of-two level, at least. Despite their crucial importance for the structure of the world\(^2\), they are by far the worst measured fundamental parameters of physics. It is a major challenge for lattice gauge theory to improve this situation.

Again, a comparison may be in order. Quite properly, tremendous effort has been put into measuring neutrino masses. Yet the precise values of neutrino masses are no more fundamental than light quark masses, and they are much less important for the structure of the world. In any case, it is unlikely we will reach a profound understanding of one without the other, since in unified theories quarks and leptons come as a package.

Besides this general sort of motivation, there are several more specific reasons to be especially interested in the values of \( m_u \) and \( m_d \). We would like to be absolutely sure that \( m_u \neq 0 \), since this empowers the P- and T-violating \( \theta \) parameter, which provides the prime motivation for Peccei-Quinn symmetry and axions, with their profound physical and cosmological implications. The ratio \( m_u/m_d \) partially determines the axion coupling, and will play a vital role in pinning down the underlying microscopic parameters if and when axions are observed. The proton-neutron mass difference, whose value makes all the difference for nuclear stability and for stellar and cosmic nucleosynthesis – and thereby to the structure and composition of matter – of course depends quite directly upon \( m_u - m_d \). Finally, there is the prospect of making precise quantitative predictions for appropriate measurable isospin-violating processes. Especially promising in this regard are transitions between heavy quark-heavy antiquark states by soft \( \pi^0 \) emission. Conversely, these processes allow a different, and potentially cleaner, path to the determination of \( m_u - m_d/m_u + m_d \)

\(^2\)See, in this connection, the following discussion of quantitative anthropics in Section 1.3.2.
than the traditional ways involving electromagnetic mass differences or light hadron processes.

\( m_s \) The strange quark mass plays a singular role in QCD. For all the other quark masses, it makes good sense to expand either in \( m/\Lambda \) or \( \Lambda/m \), where \( \Lambda \) is the primary QCD scale, vaguely in the neighborhood of 300 MeV. For the strange quark neither expansion is clearly appropriate.

A fundamental question that I find quite interesting, and which is quite important for the interpretation of numerical work on light hadron spectroscopy, is how this spectroscopy depends on \( m_s \). Specifically, how does the ratio-sequence \( f_\pi : m_\rho : m_N : m_\Delta \) vary as \( m_s \) is taken from infinity down to zero? The (un?)reasonable success of the valence-quark/bag model, and of the quenched approximation, seem to suggest very mild dependence, but the instanton liquid model seems to suggest more dramatic effects.

There are several major issues in QCD, especially regarding its phase structure, that hang on the value of \( m_s \). I’ll mention them in due course.

1.2.2. Weak Matrix Elements that Add Value to Experiments

The precise determination of \( \epsilon'/\epsilon \) in \( K \) meson decays, and the cornucopia of results on \( B \) meson decays and CP violation emerging from the BABAR and BELLE collaborations, are beautiful and outstanding achievements in experimental physics. But to extract this work’s full potential we’ll need to produce theoretical calculations of hadronic matrix elements with comparable accuracy. Only after controlling this “QCD background” can we make inferences regarding the precise values of weak mixing angles and phases, and the possible influence of physics beyond the standard model.

Here there is a picture that is worth a thousand words. Figures 2a and 2b both depict allowed regions for a variety of experimental measurements whose results depend on the weak mixing parameters \( \rho, \eta \), in the Wolfenstein parametrization of the CKM matrix. There is a consistent determination, and no evidence for physics beyond the standard model, in the region of overlap. Figure 2a is the contemporary situation; Figure 2b assumes the same data, but with the theoretical precision improved by a few teraflop-years of computation. You can see the value added.

1.3. Fantasies

1.3.1. Pinning Down \( (g - 2)_\mu \)

Another recent heroic, beautiful, and outstanding achievement in experimental physics is the precision measurement of the anomalous magnetic moment of the muon, \( (g - 2)_\mu \), reported by the Brookhaven \( g - 2 \) collaboration. In this case too, the significance of this result would be greatly enhanced if we could do a better job on the QCD ingredients, here low-energy vacuum polarization and light-by-light scattering. Our inability to do these calculations brings home the
general point that the existing technology of lattice gauge theory is quite fragile, in the sense that it can be used to calculate a few things very well, but most others essentially not at all. So my fantasy here is of techniques that degrade more gracefully.

Actually, in the specific case of \((g - 2)_\mu\), numerical work might supply an important input even while falling well short of the “fantasy” of first-principles calculation. At present the major uncertainty in the standard model prediction arises from a discrepancy between the value of the hadronic vacuum polarization as extracted from \(e^+e^-\) annihilation or from \(\tau\) decay. Even a fairly crude numerical determination of the vacuum polarization at a single Euclidean momentum might tell us which to believe.

1.3.2. Quantitative Anthropics

At the frontiers of where physics has come under control, we can start to contemplate expansion into metaphysics. Specifically, knowing the continuous parameters (in mathematicalese, moduli) of our world-theory, we can ask – and hope to answer! – a precise scientific version of the great question: Could things have been different? It seems to me that this question has taken on a new urgency due to recent developments in physics, as I have discussed at length elsewhere. There are two important refinements of the question:

- **Anthropic Principle**: Are the values of some or all of the moduli determined by the requirement that there should be intelligent observers capable of detecting them? This could be true if the Universe were inhomogeneous on ultra-large scales, with variable values of the moduli. Then the principle would emerge as a sort of converse to natural selection, selecting out the environments to which intelligence might successfully adapt.

- **Misanthropic Principle**: Are the values of some or all of the moduli determined by historical accident? This could arise in the same circumstances. It is the analogue of genetic drift.

If we could make a strong case that small variations in \(m_u\) and \(m_d\), to be specific, would destroy the possibility of intelligent observers, then that would be **prima facie** evidence for the relevance of the anthropic principle, since these quantities appear to be very remotely conditioned by central principles of unified field theories, string theory, or anything else.

Fortunately, this subject has an empirical side. We can search for variation in the physical “constants” as a function of location or of time. For theorists, there is the challenge to quantify what we learn about possible changes in the fundamental constants from cosmic nucleosynthesis and from measurements of hyperfine splittings at different times, for example. One of the most powerful constraints on variation of constants comes from the Oklo natural reactor, which would have left a different residue had an accidental near-degeneracy in the levels of \(\text{Sm}^{149} + n\) and \(\text{Sm}^{150}\) not been present two billion years ago! Clearly some of the requisite calculations enter further into the realm of fantasy than others. But it is a valid and I think profound general observation that the freedom to do numerical experiments with unrealistic values of moduli could help bring this particular branch of metaphysics into the realm of hard science.

2. Many-Particle Physics

2.1. Opportunities

2.1.1. The \(T \neq 0\) Equation of State

A major achievement of lattice gauge theories in recent years has been to map out the energy and pressure, as a function of temperature, for various idealizations of QCD. It is quite striking how the number of effective degrees of freedom ascends to something near the free quark-gluon value, at a remarkably low temperature by the standards of typical hadron masses. This phenomenon is one of the main inspirations for experimental programs to study quark-gluon plasma at RHIC and eventually ALICE.

A fully realistic simulation, with good implementation of chiral symmetry and accurate values of the quark masses, is called for. The value of the strange quark mass is especially important. It
is likely that for sufficiently heavy strange quarks there is a first-order phase transition, whereas below some threshold value there is only a crossover. A very specific, concrete, and achievable goal is to locate the critical strange quark mass, and to verify (or not) existing indications that the physical strange quark mass is subcritical. It is also important to quantify the differential strange quark contribution to pressure and density. This would be an interesting measure of how nearly free the quarks in the plasma are, and could provide a baseline for interpreting enhanced strangeness multiplicities observed in heavy ion collisions.

2.1.2. Exploring Small $\mu/T$

Finite chemical potential has long been terra incognita for numerical simulation of QCD, because configuration by configuration the usual path integral for the partition function contains a complex phase, and there are big cancellations. These cancellations are less severe for small $\mu/T$ and not too large volumes, and are absent for imaginary $\mu$. Exploration of these regimes has begun, but much more remains to be done, especially in working towards realistic quark masses and reliable error estimates. There is a potential connection to heavy ion experiments, where the effective chemical potential varies with rapidity.

2.2. Challenges

2.2.1. Locate the True Critical Point

There are good reasons to think that at zero temperature, as one varies the chemical potential, there is a first-order quantum phase transition between nuclear and quark matter. There may in fact be several transitions of various kinds, including meson condensation and alternative pairings in color superconductivity. I expect that most of these are essentially low-temperature phenomena, leaving only a single first-order transition above $\sim 50$ MeV. In any case, for purposes of discussion let me assume this. Within this context one discovers a somewhat unconventional but I think sharp and insightful perspective on the definition of “quark-gluon plasma”. At sufficiently high (but not too high) chemical potential, as one increases the temperature, or at sufficiently low (but not too low) temperature, as one increases the chemical potential, there is a sharp phase transition. We are justified in calling this a hadron-to-quark transition, since the respective phases on either side evolve without further discontinuities into matter accurately described as nearly free hadrons or nearly free quarks.

As the temperature rises the distinction between hadronic and quark matter becomes less significant; the discontinuities associated with the first-order transition grow smaller and eventually vanish altogether. The precise point in the temperature-chemical potential plane where the distinction vanishes is called the tricritical point. It is associated with a second-order phase transition, and critical fluctuations. There is some chance that the effects of such fluctuations could lead to observable consequences in heavy ion collisions.

The challenge for lattice gauge theory is very clear and concrete: locate this point! It is a notable landmark in the QCD phase diagram, and knowing its location would be very helpful for organizing the experimental exploration of that diagram.

There is probably a close relationship between this physical tricritical point and a more theoretical tricritical point implicit in our earlier discussions. That one arises in considering behavior in the temperature-$m_s$ (strange quark mass) plane. For very small $m_s$ one expects a first-order transition, which weakens as $m_s$ increases, and vanishes altogether at some critical $m^{\text{crit}}_s$. A very interesting possibility is that the theoretical tricritical point evolves into the physical one, moving off into nonzero values of $\mu$ above $m^{\text{crit}}_s$. If so, it ought to be possible to locate the physical tricritical point for values of $m_s$ just above $m^{\text{crit}}_s$ by exploiting small $\mu/T$ techniques.

2.2.2. Measure Critical Behavior

The renormalization group analysis of QCD Lite – that is, $SU(3)$ color gauge theory with two massless quarks – suggests the existence of a second-order phase transition at finite temperature, associated with the restoration of chiral symmetry. Universality arguments suggest that it should exhibit the critical exponents of a 4-component magnet, governed by the $O(4)$ linear
There are many precise predictions for the singular behavior of effective meson masses, specific heat, magnitude of the condensate, etc., based on this picture. One can go on to predict analytically the equation of state near the critical point, also allowing for small but nonzero – conceivably, even realistic – quark masses.

It would be a landmark achievement to check some of these predictions, say specifically to measure a critical exponent that can be clearly distinguished from mean field theory. This would provide welcome confirmation of some of our deepest prejudices about the nature of chiral symmetry and its restoration; conversely, any deviation from the predictions would force us to rethink some fundamentals.

A few years ago some doubts were raised about the applicability of universality in this context. They have been convincingly addressed, but live on in modified form as valid questions about the scope of universality that deserve to be addressed quantitatively. While now no one doubts that there is universal behavior near the critical point, it remains to quantify how wide the critical region is, and how important are the critical fluctuations against the background of conventional thermodynamic behavior. These are abstract but precise forms of the very basic question, What is the hadronic fluid like near the phase transition? Is it dominated by color gauge fields (glue degrees of freedom), so that the collective behavior of the critical \(\pi\) and \(\sigma\) fields are a minor side-show? Or is it best pictured as a strongly interacting meson gas? In the former case, the critical singularities will be blips on a smooth background; in the latter case, they will be fractionally large or even dominant. The location of the critical point \(T_c\), well below glueball masses, or even the scale set by the QCD string tension) and that it is well below the \(T_c\) for deconfinement in the pure glue theory, suggest to me that the latter result is more likely, but I’m not at all certain.

To do justice to these problems, one must both respect the chiral symmetry and work in large spatial volumes, so as to allow the appropriate modes to exist and have their proper scope. This will be very costly to do directly, so use of a realspace (infrared!) renormalization group or some other novel technique might be necessary.

After this is done, it will still remain to address the related but perhaps still more challenging challenge of measuring the tricritical behavior. They are expected to be governed by mean field theory, up to calculable logarithmic corrections. In addition there are crossover exponents and a critical equation of state awaiting measurement.

2.3. Fantasies

2.3.1. Reinvent Nuclear Physics

The historical origin of strong-interaction physics, and its main appearance in the natural world, is of course the physics of atomic nuclei. Although with modern QCD we have achieved an extraordinarily beautiful and “complete” theory of the strong interaction, this fundamental progress has not greatly advanced our understanding of nuclei. There is an obvious, good reason for this. The energy scales of interest in nuclear physics are of order a few MeV, while the basic QCD scale is a hundred times this. So essentially nuclear phenomena are governed by the residua of complicated cancellations among much larger basic QCD forces. It is probably unrealistic, and unfruitful, to contemplate a brute force assault from first principles, since small errors in large cancelling quantities will dominate the computed answers.

But it would be dereliction of duty, and a lost opportunity, to abandon the field entirely. A major goal of lattice gauge theory should be to compute the parameters of effective field theories, formulated in terms of pion and nucleon degrees of freedom, from first principles. (Then the effective theories could be turned over to many-body theorists.) Specifically, we should verify that the theory generates such key features as the hard core and the spin-orbit force, which are central to qualitative aspects of nuclear physics. And we should get a convincing qualitative explanation of these effects, backed up by exploration of QCD variants (see below).

It would also be amusing, and instructive, to explore the limits of “nuclear” physics as we know it. Why are there nuclei at all? In other words, Why does the nuclear force saturate? Or in mod-
ern terms, Why do protons and neutrons in nuclei retain their individual identity, rather than agglomerating into a single shared bag? Does this occur for two-color QCD, or for supersymmetric QCD? – maybe not, since in both these cases there are bosonic baryons. Does it occur in a world without light quarks? In another direction: How far is our world from a (literally) strange world, in which the ground state for baryonic matter has nonzero strangeness? How much lighter would the strange quark have to be?

2.3.2. Reinvent Extreme Astrophysics

There is a chance to break new ground, and to advance our understanding of some of the most violent and crucial events in cosmic evolution, by getting a better handle on the behavior of hadronic matter in extreme conditions. Contemporary astrophysical modeling of supernova explosions and of the structure and evolution of compact stars is largely based on poorly controlled extrapolations of models abstracted from nuclear and resonance physics well beyond their region of validity, into regimes where the particles are strongly interacting or even overlapping.

In other words, it is sophisticated guesswork. We have good asymptotic theories based on QCD for the highest temperatures and densities, but there is much uncertainty about where and how asymptopia is approached. We may look forward to a wealth of relevant new data from x-ray, neutrino, and gravitational wave detectors, in addition to dramatic enhancements of the traditional optical and radio windows. Can we do justice to this information? Specifically, can we devise concrete signatures for quark matter and color superconductivity in “neutron star” interiors, or for phase transitions during or in the immediate aftermath of supernova explosions or “neutron star” mergers?

3. Conceptual and Algorithmic Issues

3.1. Opportunities

3.1.1. See the Effects of Chiral Fermions

Thanks to some very ingenious recent work we now know how to implement chiral symmetry in a precise way in lattice QCD, suitable for numerical integration. As a result many fundamental questions become accessible, including

- quantitative spectroscopy of the pseudoscalar mesons. Since the masses of the octet are most sensitive to the values of small light quark masses, the meson masses can be used in principle to determine the light quark masses, which then become inputs to calculations of flavor SU(3) breaking and isospin violation, including “electromagnetic” mass differences;

- questions of phase structure. As has already appeared in our earlier discussions, chiral symmetry restoration is a major feature distinguishing different phases, and conditions the properties of collective modes at the phase transitions, it is important in these contexts to implement it accurately;

- study of axial baryon number violation. Axial baryon number (more precisely, axial $N_u + N_d$) is broken intrinsically in the quantum theory by an anomaly, and to a small extent by quark masses, and spontaneously by $\bar{q}q$ condensation. Simulations with large artifactual quark masses mangle this quasi-symmetry, just as they mangle chiral $SU(2) \times SU(2)$. For all the associated questions of $\etaibir$ phenomenology, squeezing out of instantons and approximate $U(1) \times SU(2)$ restoration at high temperature, and axion cosmology, accurate implementation of classical $U(1) \times SU(2)$ is essential;

- tests of the instanton liquid model. This model predicts significant dependence of certain hadronic quantities on light quark masses.

3.1.2. Explore QCD Variants

Besides enabling the noble bread-and-butter work of computing properties of real-world QCD, lattice gauge theory offers us the opportunity to explore variants, for example with different quark mass spectra, different representations, or different gauge groups. I have already mentioned many possibilities for the creative use of such flexibility,
in quantitative anthropics, in testing our understanding of the phase structure, and in testing instanton liquid ideas. This list is far from exhaustive. As a general remark, we can test proposed qualitative explanations of phenomena in QCD by seeing whether they predict the correct direction of change in the phenomena as the underlying parameters change. For example, a question I find fascinating is simply: Why does the naive quark model work so well? In thinking about this question, it would be very useful to identify QCD-variants where the naive model begins to fail! (Perhaps simply QCD with many light quark flavors?)

Let me just mention one further potential application, to clear up a long-standing debate whose origins are in the prehistory of QCD but that remains contentious even now. This is the question of glueballs. When a particle is observed experimentally, it does not come labeled “quark-antiquark” or “glueball”, nor is there any strict objective criterion to distinguish them. The whole distinction is tied up with the naive quark model, which has no firm basis in quantum field theory, and ignores the inevitability of mixing. Nevertheless there is a simple objective question correlated with this classification: If we vary quark masses, how does the mass of the particle respond?

3.2. Challenges

3.2.1. Reduce Lattice Degrees of Freedom

Full-scale QCD simulations tend to be unwieldy, inflexible, and opaque. One would like to have procedures that fall somewhere between full-scale simulation of the microscopic theory and rough semiphenomenological modeling.

There are several suggestions regarding what are the important degrees of freedom that could be used as the basis for a reduced description of QCD that keeps contact with the microscopics, including central vortices, abelian projections, instantons, and strings. The challenge is to promote such proposals into reliable quantitative tools, or at least to identify some limit in which they become good, systematic approximations.

Alternatively, more directly numerically based approaches might rise to the challenge. The original Wilsonian program of the renormalization group, to integrate up to a coarse lattice, and then solve the coarse lattice theory by other means (presumably, some form of strong coupling expansion) might be revisited in light of the vast increase in knowledge and computer power over the last 25 years.

3.2.2. Empower Reduced Continuum Theories

Extremely useful and important phenomenologies have been constructed using reductions of QCD. They are based in one way or another on integrating out hard degrees of freedom, using asymptotic freedom. The primeval example is the analysis of deep inelastic scattering using the operator product expansion, which reduces what would otherwise be an entirely hopeless problem to the evaluation of a discrete series of low-energy operator matrix elements, or alternatively the structure functions of which they are the moments. Other applications, similar in spirit, bring in Isgur-Wise functions and fragmentation functions. Many of these are poorly determined, yet they are important for the interpretation of frontier experiments. It would be a major service, as well as an intellectual achievement, to compute more of them from first principles.

3.3. Fantasies

3.3.1. Expand the Frontiers of Effective Computation

Throughout most of this talk I have been discussing how numerical work can address quantitatively demanding and/or sophisticated questions about a particular strongly interacting quantum field theory, QCD, or slight variants of it. Of course, at present this is the only precisely defined, strongly coupled quantum field theory with proven relevance to the description of Nature. But a lot of brainpower has been expended on special limits of gauge theories, more or less distant cousins of realistic QCD, for which simplifications occur and some nonperturbative results can be obtained analytically. Notable among these are large $N$ limits and theories with various degrees of supersymmetry. These and many other theories have been discussed in the context
of attempts at unification, as possible catalysts of electroweak, unified, or super symmetry breaking. It would be very desirable to have even relatively crude results about general classes of gauge theories. Ironically, the limits that provide analytical simplifications seem to be especially difficult to simulate numerically using currently known techniques. So an attractive fantasy is to imagine numerical techniques capable of coping with

- large $N$ gauge theories,
- chiral gauge theories, and
- supersymmetric gauge theories.

Recently there has been remarkable progress in formulating chiral fermion theories (gauging these symmetries is not straightforward) and supersymmetric lattice gauge theories. The constructions are very technical and delicate, and not as general as one might hope for. Furthermore, potential Monte Carlo simulations suffer from the fermion sign problem. So the fantasy here is to develop any usable algorithm. For large $N$ the standard is higher. In principle, we know how to do the calculations. But one would like to have algorithms that somehow simplify, rather than becoming increasingly unwieldy, as $N$ increases!

Perhaps needless to say, even bigger game would come into sight if one could develop usable nonperturbative methods for dealing with fermions in general, finite density, or real-time dynamics.

### 3.3.2. Define the Limits of Effective Computation

I find it disturbing that it takes vast computer resources, and careful limiting procedures, to simulate the mass and properties of a proton with decent accuracy. Nature, of course, gets such results fast and effortlessly. But how, if not through some kind of computation, or a process we can mimic by computation? We are accustomed to the idea that simulation of complex systems, or systems with many elementary parts, may be slow. But here we are dealing with the simplest of objects in an ideally simple physical theory. Of course the underlying phenomenon is that in quantum field theory many interacting degrees of freedom are in play, even in the simplest of physical circumstances.

Does this suggest that there are much more powerful forms of computation that we might aspire to tap into? Does it connect to the emerging theory of quantum computers? These musings suggest some concrete challenges: Could a quantum computer calculate QCD processes efficiently? Could it defeat the sign problem, that plagues all existing algorithms with dynamical fermions? Could it do real-time dynamics, which is beyond the reach of existing, essentially Euclidean, methods? Or, if all that fails, does it suggest some limitation to the universality of computation? There is, after all, no guarantee that models abstracted from our (real or imagined) implementations of how to compute things capture the process by which Nature decides how to operate. Maybe she just “does it”, without computing anything.

A different sort of limitation to effective computation has become a major theme of recent investigations in classical dynamics. It is the ubiquity of extreme sensitivity to initial conditions, in problems ranging from the double pendulum to the long-term behavior of the Solar System. Are there hadronic properties that depend sensitively on the fundamental parameters we use to compute them in QCD (specifically, quark masses), or on the small “nonrenormalizable” corrections due to other interactions, or discretization artifacts? This is plausible, since in an effective description we will have many channels with the same quantum numbers, or in the language of dynamical systems overlapping resonances, contributing to high orders in perturbation theory. So even if the parameters of the effective theory depend smoothly on the fundamental inputs, physical quantities such as exclusive scattering amplitudes at intermediate energies may not. This would provide a rational explanation for the terrible difficulties we’ve had in devising reasonable algorithms for computing such quantities. But our understanding of questions like this is not even in its infancy.