The Problem of Asymptotic Freedom

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Abstract

There is a growing body of evidence that the running of $\alpha_s$ predicted by perturbation (PT) theory is not correctly describing the accelerator experiments at the highest energies. A natural explanation is provided by the authors’ 1992 proposal that in fact the true running predicted by the nonperturbatively defined lattice QCD is different, leading to an ultraviolet fixed point near $\alpha_s = .1$. It is explained how this can be understood from the fact that the conventional perturbative method is ambiguous and does not provide the correct asymptotic expansion. It is pointed out that there is a large amount of lattice data that are supporting this scenario rather than the conventional one.

1. Introduction

Asymptotic Freedom (AF) is reputed to be:

(1) a property of nature (the strong force)

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(2) a property of the theory called Quantum Chromodynamics (QCD), expressed by
the familiar formula for the running coupling constant

$$\alpha_s(\mu) = \frac{12\pi}{(33-2n_f) \ln \frac{\mu^2}{\Lambda^2}} \left[ 1 - \frac{6(153 - 19n_f)}{(33-2n_f)^2} \frac{\ln \ln \frac{\mu^2}{\Lambda^2}}{\ln \frac{\mu^2}{\Lambda^2}} \right] + ... \quad (1)$$

Both properties are a priori logically independent and require experimental and theoretical confirmation, respectively. This would also help answer the important question whether QCD indeed describes correctly the strong interaction.

For most particle physicists these appear to be questions that were settled long ago; we want to emphasize that both of these two properties mentioned above are far from being established beyond any reasonable doubt and we think there are good reasons to expect that AF in the strict sense neither holds in QCD nor in nature.

We will point out that Lattice QCD (LQCD), if one approaches its results with an unprejudiced mind, in fact suggests the existence of a nonzero fixed point for the strong coupling constant $\alpha_s$, in conflict with the property of AF derived from perturbative QCD (PQCD) more than 20 years ago [1, 2]. Assuming that QCD indeed describes the strong interaction correctly, this in turn predicts that in nature $\alpha_s$ will run to a fixed point $\alpha_{fp} > 0$, and that at high energies the running of $\alpha_s$ is slower than the PQCD prediction.

2. The Need for a Nonperturbative Definition of QCD

QCD is a field theory of quark and gluon fields; but nature does not know any particles identifiable with quarks and gluons, only hadrons, which are considered as permanently bound states of the former. To substantiate this claim, i.e. to show that quarks and gluons are permanently bound into states corresponding to the known hadrons, one obviously needs a nonperturbative definition of the theory. To this day the only serious and useful such definition is provided by LQCD [3]. In view of the undoubted successes of LQCD in describing the low energy phenomenology of hadrons we accept as a working hypothesis that QCD, defined nonperturbatively as the continuum limit of lattice QCD, in fact correctly describes the strong force.

AF, on the other hand, is a property derived exclusively in the framework of perturbative QCD (PQCD) [1, 2]. For many phenomenologists, QCD has become synonymous with PQCD. It has to be stressed that this standpoint is not satisfactory for two reasons:

1. PT is an algorithm that produces for a physical quantity, such as

$$R(Q) = \frac{\sigma(e^+e^- \rightarrow \text{hadrons})}{\sigma(e^+e^- \rightarrow \mu^+\mu^-)} \quad (2)$$
a formal power series in $\alpha_s(Q)$:

$$R(Q) \sim \sum_{k=0}^{\infty} c_k \alpha_s(Q)^k;$$

(3)

the radius of convergence of this formal series is generally expected to be zero. The meaning of eq.(2) is therefore a priori unclear. PQCD does not really predict definite numbers, but sequences of numbers, depending on where one decides to truncate the divergent series (2). Generally one stops at a low order because the computation of high orders is beyond human capabilities anyway, and hopes that the truncated series represents the ‘truth’ to a good approximation. That there is a problem becomes clear if one assumes that a fairy would give as all the PT coefficients. We could not extract in an unambiguous way a definite number as the ‘sum’ of this series!

Many people would object and say that we should try to use some prescription like Borel or Borel-Padé summation. But even if these procedures would in fact produce a number as the ‘sum’ of the series, in the absence of a nonperturbative definition this would be just an ad hoc procedure and one could not even meaningfully ask if the answer is ‘right’.

2. Of course it is generally said that PT is an asymptotic series. To give meaning to this statement, one has to accept a nonperturbative definition of QCD, such as the continuum limit of LQCD. Then one can ask whether PT is indeed asymptotic to this nonperturbatively defined theory, i.e. (for the example of $R(Q)$): Are there numbers (or maybe functions of $Q$) $d_k$, $k = 1, 2, 3, ...$ such that the following inequality holds for all $N$

$$|R(Q) - \sum_{k=0}^{N} \alpha_s(Q)^k c_k| < d_{N+1} \alpha(Q)^{N+1}?$$

(4)

3. Can Perturbation Theory be Trusted?

LQCD is defined by the (infinite volume limit) of the Gibbs measure

$$d\mu = \frac{1}{Z} \exp(-\beta S)$$

(5)

with $\beta = 6/g_o^2$.

Accepting now LQCD as our nonperturbative definition, we ask about the trustworthiness of standard PT. On the lattice PT is really a saddle point expansion around an ordered state (in gauge theories a state with vanishing gauge field strength). While in a fixed lattice volume, by making the (bare) coupling small, one can make the deviations from such an ordered state small, provided one fixes the gauge completely, in an infinite system this is not the case. There are old arguments and rigorous
results \cite{4} that show that in the (complete) axial gauge there are still arbitrary large fluctuations no matter how small the coupling. PT – which always has to be done in an arbitrarily large lattice volume if we want to approach the continuum, even if we are in a finite physical volume – is thus an expansion in a quantity that is not small. (In the Landau gauge, the problem appears in a different guise: the integration is cut off arbitrarily close to the saddle point due to the presence of the Gribov horizon \cite{4}).So why could one expect it to give the right answer, even if in certain quantities the volume divergences seem to cancel term by term? 

The presence of these large fluctuations can be understood from the existence of excitations of arbitrarily low energy (action) that lead the system arbitrarily far from any assumed ordered state at arbitrarily low cost in energy. These low-lying excitations were dubbed ‘superinstantons’ by us \cite{7} in order to emphasize that at weak coupling they will be present much more frequently than instantons, which have a finite excitation energy over the classical vacuum. The true QCD vacuum really has to be imagined as a condensate of superinstantons.

That something is indeed dubious with the usual procedure of performing a saddle point expansion with an infrared (IR) cutoff (such as a finite volume) and then removing the cutoff term by term, can be seen explicitly if we study the dependence of PT on boundary conditions (b.c.). It is a general fact that precisely due to the large fluctuations that put PT into question, the true, nonperturbatively defined infinite volume limit becomes insensitive to b.c.

On the other hand, we have shown in \cite{4} that the PT coefficients in fact do have different infinite volume limits for different b.c.! This affects even the Callan-Symanzik function $\beta_{CS}$. So PT gives ambiguous results, and certainly has to fail for some b.c. Nobody knows for which b.c. it might be right. Most likely it does not give the right answer for any simple b.c.; to get the right answer, in addition to the trivial saddle point, all the low lying superinstantons (almost degenerate with the trivial saddle point) have to be taken into account to get the true asymptotic expansion. How this is to be done concretely, remains an unsolved problem.

4. Numerical and Analytic Evidence

For LQCD the perturbatively computed Callan-Symanzik function $\beta_{CS}$ has the form

\[ \beta_{CS} = b_0 g_0^3 + b_1 g_0^5 + \ldots \] \hspace{1cm} (6)

It leads to the prediction that any dynamically generated mass will be proportional asymptotically for $\beta \to \infty$ to

\[ \Lambda_L(\beta) = \exp(-\frac{\beta}{12 b_0})^{\frac{1}{2}} \] \hspace{1cm} (7)
Numerical studies have found consistently that this is violated as one enters the scaling region, where the correlation length begins to grow and continuum behavior should begin to be seen. This is the so-called dip in $\Delta \beta$, the change in $\beta$ corresponding to a doubling of the correlation length. This ‘dip’ occurs likewise in the $2D$ toy analogues of QCD such as the $O(N)$ nonlinear $\sigma$ models. The deviation is always such that the correlation length increases faster with $\beta$ that eq.(5) predicts, just as if the theory was really approaching a critical point.

There have been some proposals to argue that phenomenon away. First of all people have tried to compute masses for $\beta$ values so large that it is impossible to work on ‘thermodynamic’ lattices (much larger than the correlation length). In essence these proposals are using small lattices and try to extrapolate what they find there to thermodynamic ones (this is the common feature of the so-called Monte Carlo Renormalization Group (MCRG) and Finite Size Scaling (FSS)). What has been found using these ideas is that apparently the ‘dip’ disappears at larger $\beta$ values and the PT $\beta$ function really seems to describe what is happening. But this is merely a reflection of the fact that the small lattices are actually very much ordered and don’t admit the large fluctuations that are necessary to describe the continuum behavior (see [8] for a detailed discussion of an example). It has also been noted long ago by Gutbrod [9] that the ‘dip’ deepens, as data from larger lattice are introduced. In our opinion there is not a dip, but a zero of $\Delta \beta$ at some $\beta$.

Another popular fix is to replace $\beta$ in eq.(4) by an ‘effective’ quantity that asymptotically becomes equal to $\beta$ (such as in the so-called energy scheme). This procedure appears to be completely ad hoc; it is not surprising that by a suitable transformation $\beta \rightarrow \beta_{eff}$ one can achieve perfect asymptotic scaling over any finite range.

A typical example of the ‘dip’ is provided by Fig.1 produced with data of the Bielefeld group [10], actually not referring to a mass, but the deconfinement temperature:

It should be noted that the three points are obtained by some extrapolation from a lattice that is too small, so it is not certain what the true infinite volume answer is; we expect the points eventually to move down as computing power and lattice sizes increase. In fact earlier studies, using smaller lattices [11, 12], claimed that asymptotic scaling sets in around $\beta = 6.0$, which is obviously not borne out by the Bielefeld results.

Bali et al. [13] gave another example; they find (for the $SU(2)$ pure gauge model at $\beta = 2.5$) a value for $\beta_{CS}$ that is only 63% of the PT prediction.

Now to the analytic evidence: as mentioned, LQCD has a little brother in the $2D$ $O(N)$ models for $N > 2$. These models also share the property of perturbative asymptotic freedom and large fluctuations that make PT both suspect and ambiguous. For those models we developed an argument based on percolation theory leading to
Figure 1: The ‘dip’ as obtained from the deconfinement temperature; the solid line is the 2 loop PT prediction and the data are taken from 10].

the conclusion that there is a critical point (fixed point) at nonzero coupling, in drastic conflict with the predictions of PT. This chain of arguments, while not completely rigorous, has remained without serious challenge over the more than four years since it was presented publicly [14, 15]. (For LQCD this kind of argument cannot yet be made, just like there is not yet a good cluster algorithm for gauge models; but we think, like the rest of the community, that the two types of models are really behaving the same way).

So we think there is good reason to doubt the conventional wisdom and accept the existence of a phase transition of LQCD at nonzero coupling.

5. Consequences

Finally let me say something about the consequences of the proposed fixed point in the running of $\alpha_s$. These were first pointed out by us in [16]. It is quite clear that such a fixed point means that eventually the running of $\alpha_s$ has to be slower than
what PT predicts; this may also be expressed by saying that $\Lambda_{QCD}$ is not a constant but an increasing function of the momentum $Q$.

At which energy scale are deviations expected to be seen? Is it something like the Planck or GUT scale, or is an accessible energy? The answer to this questions in some sense decides if we are talking about physics or metaphysics.

To find an answer, one first has to estimate the critical lattice coupling. From an analysis of the numerical results it seems plausible to assume $\beta_{crit} \approx 7 - 10$. This has to be converted into a mass scale (i.e. the size of the UV cutoff), and for lack of anything better we use PT for the moment to estimate it. This leads, maybe surprisingly, to a not so huge scale of 1 to 3 TeV. Since of course the existence of the assumed critical point means that PT really has to fail drastically at this value of $\beta$, we expect drastic deviations from perturbative running at the scale of 1 to 3 TeV. But of course these deviations will set in gradually, and therefore we predicted that deviations will be seen at 1 TeV or less.

A naive ansatz for the variation of $\Lambda$ with $Q$ is to replace $\Lambda^2$ by $\Lambda^2 + Q^2$. This ansatz can be shown to correspond to mean field critical behavior; mean field behavior is expected to hold in 4D LQCD at least in one instance, namely in the PCAC relation. With this simple minded ansatz, the true running of $\alpha_s$ might look as in Fig.2:

Soon after we made our prediction, the LEP results seemed indeed to indicate such a deviation: instead of $\alpha_s(m_Z) = .113$ as predicted by Altarelli [17] on the basis of lower energy results, the central LEP value is now .123. Of course there is no consensus on the significance of the deviation, which is (depending on whom you ask) at the 2 or 3$\sigma$ level. But this has not deterred people to interprete it as a sign of the existence of light gluinos [18, 19] or more generally as signifying the presence of new physics [20]. The obvious and much more economical explanation that the actual running is different from what PT says, seems to be impalatable to people.

(It should be noted, however, that at least one team has picked up on the idea of a fixed point in $\alpha_s$ [21]. The authors checked whether the deep inelastic scattering (DIS) data are compatible with a fixed point and found that they are. Since we know of no reaction of the community to this analysis, we don’t know if there are any valid objections to it).

A similar (social) phenomenon could be observed again this year with the publicity surrounding the CDF results on enhanced jet production at very large $p_T$. Dramatic claims of new physics were made, but the obvious possibility of a slowly running $\alpha_s$ remained unexplored.

All these questions are too important to remain uninvestigated. Experimental physicists should be convinced that it is worthwhile to provide better results on the running of $\alpha_s$ (supposedly CLEO could provide a very good value at a ‘low’ energy).
Lattice theorists should at least recognize that there is an important and challenging issue to be resolved, instead of simply taking for granted what most people believe. The data provide strong hints that something is going on, and it is not helpful to argue these hints away by changing the rules of the game, introducing ad hoc fixes like the ‘energy scheme’ or to produce a host of Monte Carlo data at small lattices that could just as well be be produced without much effort by using lattice PT, and which therefore do not make as any smarter as to the nonperturbative behavior of LQCD.

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