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A WORD OF CAUTION*†

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I should be wearing a black cloak to show you that I am the devil's advocate, invited here with the task of convincing you that the candidate is unworthy of beatification. The candidate is perturbative QCD and it has not, I believe, performed the miracle of "testing QCD" which is claimed for it.

The problem is that all these so-called tests of QCD depend on perturbative QCD plus certain assumptions about the magnitude of possible non-perturbative corrections. The fact that the "tests" are passed tells us that these corrections must indeed be small but, were they to fail, I, for one, would not discard QCD. I would simply say that we are observing the non-perturbative corrections to our perturbative calculations. A test which cannot be failed should not be called a test – we are not "testing QCD" by these experiments, we are testing *perturbative* QCD plus hadronization models.

This does not mean that the experiments and the predictions for them are not interesting – on the contrary, I am very impressed by the continuing success of this combination of theory and phenomenology – I believe it does indeed provide strong circumstantial evidence in favor of QCD as a candidate theory. However I think we should be more precise in the statement of the aims and limitations of such a study.

In order to explain my viewpoint a little I will present to you an example of an imaginary world where the results of the perturbative calculations would not be so readily applicable,[1] and then suggest what lessons can be learned about the real world from this imaginary one. My imaginary world is one in which the *lightest* quark is heavy – where by heavy I mean $m_q \gg \Lambda$ or $\alpha(m_q) \ll 1$. [In this

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imaginary world the lightest hadrons would be glueballs and their masses would be of order Λ, while the usual quark- containing mesons and baryons would all have masses of order m_q.] By making the lightest quark heavy I have removed the possibility of color shielding by "soft-hadronization" via the production of soft quark-antiquark pairs – either from soft gluons if you like perturbative modelling or from string-breaking if you prefer the non-perturbative words. In my world the string cannot break easily – the energy density in the string is so low compared to the quark mass that the production of quark pairs is very suppressed.

Let us consider e^+e^- annihilation in this world – say at q^2 of order 10-100 m_q^2. (I am not sure what happens at q^2 exponentially large, possibly the perturbative analyses works as usual in that case.) The calculation of $R(e^+e^- \rightarrow \text{hadrons})/(e^+e^- \rightarrow \mu^+\mu^-)$ gives, as usual,

$$R = e_q^2\Big[1 + O\Big(\alpha_s(Q^2)\Big)\Big] .$$

Now I ask what channel provides the 1 in this expression – what is the dominant final state?

In addition to perturbative QCD, I assume that color is confined. Thus when I produce the initial quark-antiquark pair moving off in opposite direction, I must either turn them around and get them back together or produce at least one additional pair which must be roughly anti-aligned with the first (in other words, I require color-shielding in some finite angle cone). In perturbative QCD the first alternative, which would produce a final state of one onium plus many glueballs, is suppressed by powers of α_s. However, so also is the second, because I have made the lightest quark heavy. It costs me at least a factor of $\alpha(m_q)/m_q$ to produce this additional aligned pair and in my world this is small, by definition. Non perturbatively I would expect the first process to dominate – a string would form and, since I have arranged the world so it only has a very small probability of breaking, it will eventually stop the quarks and bring them back together (in the so-called yo-yo mode). As they pass, glueballs will be radiated due to the rapidly changing color fields and the quark motion would thus be damped until it settles into an onium state.

The point of my example is that this contradiction within the usual perturbative discussion (between the 1 in R and $\alpha(Q)$ or $\alpha(m_q)$ suppression of individual color-shielded channels) does not vanish as a power of Q^2, it is controlled instead by the ratio m_q/Λ. In the real world m_q/Λ is quite small, so that the probability of a string breaking almost as soon as it forms is quite high. Hence effects due to unbroken strings are presumably small in the real world. However I argue they are not "higher twist" effects; they are not suppressed by powers of Q^2. Hence they provide an incalculable correction to any perturbative calculation. If the string does not break the usual rule that hadronization does not alter the parton momentum flow is not true – but how big such contributions are in real experiments I cannot even estimate. The success of the whole industry of perturbative QCD says that they are indeed small.

With all this said, what should we do? Should we discard this whole avenue of experiment? I certainly would not advocate that – as I said already, even without providing a yes/no test of the theory of QCD, the experiments serve a useful purpose. They do indeed reinforce the view that we have the right picture of the world in this theory. I would argue rather that the work should continue. The emphasis should always be on the comparison of processes in as model independent a fashion as possible – the more we can factor out the hadronization model the clearer one view of the underlying processes at the parton level will become.

To this end I would recommend that it is valuable to have *a few* different hadronization models so we can test the sensitivity to the variations of the models (maybe the two now popular versions and perhaps one more of the "inside-outside" cascade type are enough). However these models should be standardized and used in the same way for a variety of different processes. Right now, as I understand it, we have variations of the models that are different from one e^+e^- experiment to the next, so we poor theorists cannot even tell if two measurement of the same process get the same result, let alone make comparisons of jets in different processes. Listening to the talks here, I get the impression that there is indeed a great deal of similarity of the jets in various processes – but I would very much like to see that qualitative statement made more quantitative.

I would also like to make a brief comment on the hadronization of gluon jets. It seems to me there are three competing processes here – a hard gluon can be color shielded either by picking up a soft quark and an antiquark from the vacuum, making a meson, or by picking up a soft gluon to form a glueball (which subsequently decays into an isospin 0 collection of mesons) or it can decay in $O(\alpha_s)$ into a hard quark-antiquark pair which then hadronize as quark jets. None of these mechanisms is accurately modelled by splitting the gluon momentum evenly between a quark and an antiquark as I believe is done in some versions of hadronization models. The method of the Lund model corresponds to the first mechanism, but I know of no attempts to include the second, while the third is clearly a higher order α_s process. However, because different D-functions will enter in the three cases, it may be competitive with the first two. The glueball mechanism would dominate in my heavy quark world, in the real world it may only be a small contribution, but its effects should be considered.

I would also like to second the remarks of Gunther Wolf[2] – who told you how, by looking at a variety of distributions, one can optimize the various parameters of the model fairly independently. Clearly it is important to do this – when results are given only for a fit to energy-energy correlations for example, they do not allow me to see whether this optimization has been achieved. Thus my recommendation is to choose a few models and then to compare the optimized model parameters from various processes. This is not testing QCD – anymore than solid-state physicists are testing QED when they ask how well their models perform. However it is good physics. Assume QCD, and then ask how well the perturbative calculation plus hadronization model performs as a description of the data. I believe that by continually changing the modelling detail by detail one does not learn much – provided the various distributions can be fit reasonably well the model is good enough and can be used to see whether we have roughly the same kinds of jets in various processes, or whether we can observe significant differences between say quark jets and gluon jets.

I want to make another comment about models – and that is that there is always a trade-off in modelling between assumptions and parameters. We can remove parameters by making assumptions – sometimes this is a good thing to do, but one should always be a little suspicious of models with too few parameters. As an example I suggest you look at the papers of Field and Wolfram[3] who use a model based on high order perturbative QCD calculations to give many partons and then add phase space algorithm for hadronization of color singlet clusters. They then find that to fit data they need $\Lambda_{\overline{MS}} = 1.4$ GeV and a low mass cutoff on their clusters of 1.8 GeV. This is an inconsistent use of perturbation theory – I would say that lattice calculations of the Wilson loop teach us that perturbation theory cannot be used below about $Q^2 \simeq 10\ \Lambda^2$. For lower Q^2 the coupling grows faster than the perturbative calculation would indicate, or one would say the effective Λ gets larger if one tries to fit with the perturbation formula in this range. The perturbative formulae provide a few parameter models, but only by an unwarranted application at too low Q^2. The Λ extracted from this model cannot be compared with the Λ relevant for the large-Q^2 evolution of the coupling constant. Even the more refined – and very clever – perturbative calculations such as those of Mueller[4] or of Marchesini and Webber[5] suffer from the same problem that one is using the perturbative QCD formalism all the way down to the infrared region where it cannot necessarily be trusted.

I am not sure that there is any clear answer in the trade off between parameters and assumptions – a common path in modelling is to make as many assumptions as possible to start with, and to trade them in for more parameters when the measurements force it on you. Perhaps it is more reasonable to begin with several parameters in the model than to make many ad-hoc assumptions.

Let me end by summarizing what I have said. I think we should discard the phrase "testing QCD" with respect to all these experiments and talk instead of studying the phenomenology of perturbative QCD. If the data lead to discrepancies with perturbative predictions plus hadronization models, we may learn something about the possible non-perturbative effects, but we will almost certainly not be led to discard QCD. Meanwhile, when the model works, we are continually reinforcing the view that our assumptions about hadronization as a soft process work well, and that the underlying hard parton process do indeed follow the behavior calculated from perturbative QCD.

REFERENCES

1. S. Gupta and H. R. Quinn, Phys. Rev. D25, 838 (1982).
2. Gunther Wolf, proceedings of this conference.
3. R. D. Field and S. Wolfram, Nucl. Phys. B213, 65 (1983).
4. A. H. Mueller, Nucl. Phys. B213, 85 (1983) and Columbia University preprint CU-TP 263, May 1983.
5. G. Marchesini and B. R. Webber, CERN preprint TH-3525, February 1983.