What Has Happened to QCD?

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1 Introduction

It needs hardly to be said to members of this workshop that experimental research on QCD, especially the nonperturbative part, is falling on increasingly hard times. We will not document this claim here, with one exception: the content of the recently released report of the HEPAP subpanel chaired by Fred Gilman. As detailed in Section 2, there is no priority given for QCD research: the subpanel seems to consider it not even worthy of comment. This does not appear to be a casual oversight. It seems to rather accurately reflect a majority view of the high-energy physics community, especially of its leadership.

We suggest that something must be done to reverse this deteriorating condition. We propose that is time to build a firewall to protect the broad spectrum of QCD experimentation from an increasingly focused high-energy physics community. What is suggested here is that the QCD experimental program in High Energy Physics (HEP) be moved into the Nuclear Physics (NP) program administratively. For this to work, additional support would have to be given to the NP program, such that the necessary utilization of HEP beamlines and/or collision regions could be appropriately supported. Thus HEP would evolve into a community dedicated to the study of processes initiated by elementary particles: leptons, quarks, gluons, and photons. NP would evolve into the community which examines processes involving in an essential way extended, structured objects: hadrons and nuclei.

In Section 3, the basic scientific argument for this reassignment is discussed. In Sections 4 to 6, we lay out a plan for proceeding.

2 The Gilman Report

The title of the Gilman report is "Planning for the Future of U.S. High-Energy Physics". In the charge made by DOE to the subpanel, the first bullet on future facilities begins: "What will be the compelling physics topics in the next decade...?" In the portions of the report most likely to be read, namely the Executive Summary and the final, seventh section of the report, entitled "Plan for the Future of U.S. High-Energy Physics", nowhere is QCD or the study of strong interactions mentioned, even in passing. This is the case, despite the fact that a variety of other subfields (muon, kaon, neutrino, non-accelerator) beyond the Big Three (Higgs, SUSY, CP) are discussed.

One might have expected some words in the third section of the report, entitled "The Physics Questions Before Us", seven pages long and divided into four subsections entitled "What do we know now?", "What questions remain open?", "What is likely to be explored?", and "What will remain unanswered after the LHC?" Only in the first subsection is QCD alluded to, in less than four lines of text. Three of them explain that the strong force is described by QCD, and the fourth states that "for example, experiments using protons and antiprotons at the Tevatron collider have verified the predictions of QCD."

If one looks deeper into the 44 pages of boiler-plate in the fourth section describing the activities of the individual laboratories, occasional sentences or even paragraphs on QCD activity can be discerned, adding up to no more than 2 pages. But two thirds of this prose is a description of what has been done, not what might be done in the future.

And that's all there is on QCD, folks, in the entire document. It sure sounds like a closed subject, doesn't it?

3 Is it time for a change?

As already mentioned in the introduction, we do not think that the absence of consideration of QCD by the Gilman panel is to be dismissed as a casual oversight, made in a situation where admittedly much weightier issues dominated the deliberations. Instead it seems to be consistent with the priority ranking that QCD research is given nowadays by many members of the HEP community. And in many ways it is not at all surprising.

The tradition of HEP, from its birth at the beginning of the century until now, has always been to push to ever smaller distance scales and ever higher energies, leaving the remainder behind. The "remainders" in the past include all of atomic, molecular, and optical physics, condensed matter physics, chemistry, and more recently nuclear physics. The essential problem addressed by QCD research nowadays, that of hadron structure and the effects of nontrivial hadron structure on properties of collision and decay processes, is now entering this "remainder" category. It is becoming a distinct field of its own. The label QCD is no longer enough to unite the physics of isolated partons to that of structured hadrons, any more than the label QED suffices to unite the physics of isolated photons and electrons to the physics of complex atoms and molecules.

In the past, the "remainder" disciplines could be trusted to evolve according to their intrinsic importance and intellectual interest, since the research, largely by small groups within individual universities, could naturally develop with a variety of means of support. Recent changes in HEP have made this natural evolution more difficult. The overwhelming transformation to large scale projects has brought reductions in direct university funding, replaced by increased project specific support. The HEP university groups may have less flexibility than they did in previous years. Also, for hadronic physics, the number of facilities for doing the research is extremely limited, and consists in large part of those same HEP facilities. So this creates special circumstances, and arguably implies that special means may be needed to deal with it.

4 A sketch of a proposed solution

As we have already indicated, we suggest that all QCD experimental programs within HEP be transferred to NP. The large general-purpose experiments such as CDF, D0, CLEO, BABAR, etc would remain under HEP and the support for QCD activities within those programs could be handled as a special case.

The moving of QCD work from HEP to NP should take place if and only if there is available additional support within NP to accommodate the QCD programs now existing in HEP and their likely extensions in the future, without damage to the ongoing programs in NP. This support should take into account the operating costs of the NP programs at the HEP laboratories.

Physics programs which fall into this category include (1) fixed-target experiments at the AGS, (2) the use of RHIC for experimentation with pp collisions in addition to heavy-ion work, future QCD experiments at Fermilab in (3) the 120 GeV main-injector external beam lines, in (4) the antiproton source, or in (5) the C0 collision region, (6) future fixed-target electron-scattering experiments at SLAC, and conceivably (7) future QCD work at the LHC in ALICE or FELIX, if the politics were to allow it.

There are basic reasons why such a transition is a very natural one. Light-quark spectroscopy is a discipline already shared with the nuclear physics community, e.g. via CEBAF. Everything to do with lepton-nucleon scattering is likewise a shared enterprise, even at quite high energies. This is especially the case with respect to polarization phenomena (e.g. HERMES at DESY), so that a pp spin program at RHIC fits in very naturally as well. Charm spectroscopy, a program option for both C0 and the antiproton accumulator, has kinship with light-quark spectroscopy as well as with B physics, and thus has crossover status. Finally, the challenge of understanding very high energy hadron-hadron collisions, where even at the parton level there are hundreds of active partons in each beam, is not dissimilar to the understanding of high energy Pb-Pb collisions. The heavy-ion and HEP communities indeed find a great deal of common ground.

What about QCD theorists? Many of them are intellectually attached as much to the big institutional experiments as to the ones which would move to NP. If there were to be a stronger experimental presence of QCD in NP, this might lead to a stronger flow of grant applications into NP from the QCD theory community. But in any case natural evolution may be adequate to deal with any such transition, with no particular policy pronouncement necessary, at least in the short term.

5 The problem of "small" experiments in HEP

The QCD program defined above is only one part of the "small-experiment" portion of HEP. The remaining parts are also heavily stressed by the competition of the very big experiments and programs. We believe that there needs to be a "firewall" built to protect them from extinction in times of fiscal stress. We would argue that a fixed percentage investment in the small-experiment component should be maintained no matter what. The reasons are familiar: the maintenance of diversity in the program, the preservation of an environment where individual initiative is most strongly encouraged, and the usefulness of the small-experiment environment in training of students.

Moving QCD programs into NP from HEP does create a firewall, while at the same time exposing these programs to competition from other NP experiments—we believe fair competition. Should the non-QCD small experiments follow suit? Inviting that aggrieved community on board could also be justified, although there is less of an intrinsic case that can be made.

Perhaps the other small-experiment subcultures may best be served by remaining freelance, seeking support on either side. They are for the most part explicitly recognized in the Gilman report as being part of the HEP program. On the other hand, their scientific goals are occasionally co-opted by physicists within NP. We would hope that they see the moving of QCD into NP as advantageous to them in giving them extra options for survival.

6 How to get started?

The most important first step is to define in a comprehensive way the community of interest. This means identifying and contacting every Principal Investigator who might be affected, along with those in the younger generation (e.g. OJI level) who could be considered as future leaders in this field. It is those who would put their careers on the line who should carry the cause forward. Then via polls and/or meetings, or perhaps via a Web listserver, a consensus on what, if anything, to do would have to be reached. Vital is determining the risk factor involved in moving over to NP from HEP. Also vital will be keeping a focus on the big picture and avoiding internal dissension.

Certainly the nature and scope of the proposed transition needs to be defined, especially with respect to the magnitude of increased funding. The political realities need to be explored and assessed. The probable posture of the leadership in HEP and NP, as well as that of the funding agencies, needs to be determined. And the scientific case for support of such a change must be developed.

Opinions of the idea and general advice need to be solicited as soon as possible from wise men and women and elder statespersons in both HEP and NP. Potentially sympathetic members within HEPAP and NSAC should be identified. Appropriate individuals in DOE/NSF need to be consulted. And so on. And one should take into account that there is not only a changing of the guard at BNL, but also replacements quite soon for David Hendrie in NP and John Peoples at Fermilab. Their successors, as well as the relevant nominators, should be sensitized to this problem. This argues for a minimum of delay in getting going.

If the above stages go well, a more public campaign could then be mounted. Important at that level would be laying out a long-range view of the program of experimental QCD—dreaming all the dreams and setting them down, together with the more immediate and parochial wish lists. The dreams would include, among many other things, a fully state-of-the-art spectrometer in a Main Injector beamline for light-quark spectroscopy. It would describe all the QCD that would be in principle accessible in a future muon collider. It would demonstrate that the study of the strong force has not ended, as the Gilman report suggests, but that it has only begun.