Intersections 2000: What's New in Hadron Physics *

James D. Bjorken

Stanford Linear Accelerator Center
Stanford University, Stanford, California 95409

Abstract. Hadron physics is that part of QCD dealing with hadron structure and vacuum structure, almost all of which is nonperturbative in nature. Some of the open problems in this field are outlined. We argue that hadron physics is a distinct subfield, no longer within particle physics, and not at all the same as classical nuclear physics. We believe that it needs to be better organized, and that a first step in doing so might be to establish hadron physics as a new division within the American Physical Society.

I. THE BIG PICTURE

The main portion of this talk deals with the subject of hadron physics: what it is, what some of its challenges are, and why I believe the hadron-physics community needs to identify itself more strongly and precisely in order to define and protect its long range experimental program. But before turning to that, it may be of use to put this subject in the context of the bigger picture of basic particle-physics goals. Another reason is that this conference has not been just about hadron physics. By my count about 60 percent of the parallel sessions dealt with hadron-physics issues, while only 45 percent of the plenary talks were on hadron physics, the remainder dealing with the bigger picture.

For most of the last twenty years, the Big Picture in particle physics has centered around the Big Three issues, namely Higgs, SUSY, and CP. I believe, as does most everyone, that the most important of these is the problem of mass and the nature of the Higgs sector, responsible for electroweak symmetry breaking. While this question seems timeless, having been around in an almost unchanged form for over two decades, our perspective of it has actually shifted somewhat. Thanks to the discovery at Fermilab of a very heavy top quark, and to the many beautiful precision electroweak measurements from CERN, SLAC, and elsewhere, the mass of the Higgs boson cannot be too large. This is encouragement that we will within

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a decade have a direct experimental handle on the question, from experiments at the Fermilab Tevatron and/or the CERN Large Hadron Collider.

How this turns out experimentally will have a profound influence on the future of the field. I like to contrast the options in terms of two extremes. One is the "desert" scenario, where the theory remains essentially what we now have all the way up to a very high mass scale, for example the Grand Unification scale of 10^{15} GeV or so. This can only occur if the Higgs boson, the only undiscovered particle that the desert scenario requires, has a mass of 160 ± 20 GeV. Other masses are ruled out by the requirements of vacuum stability and the absence of strong Higgs self-interactions (cf. Fig. 1). The other extreme is that of the supersymmetric extension of the standard model (SUSY, or MSSM), where each known particle has its superpartner, differing in spin by one-half unit, and with the superpartner masses typically less than a TeV. Also, the MSSM Higgs sector is larger, with at least one member expected to have mass less than about 130 GeV.

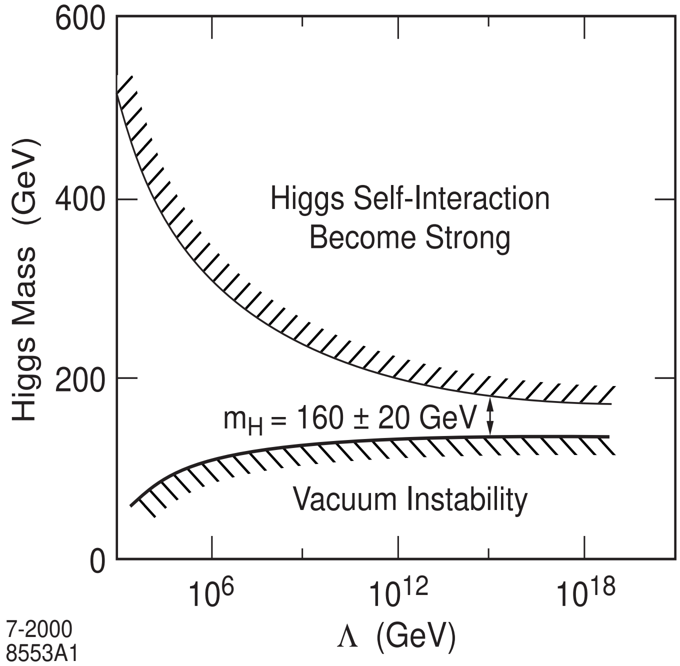

FIGURE 1. Values of m_{Higgs} and momentum scale for which the Standard Model exists, *i.e.* where electroweak perturbation theory converges. The upper region is forbidden because the self-interactions of the Higgs particle become strong. The lower region is forbidden because the vacuum itself becomes unstable.

If the SUSY scenario is correct, there will be full employment for experimentalists. They not only need to discover the superpartners, but also to determine the more than 100 extra fundamental parameters that characterize this extension of the standard model. On the other hand, if only the 160 GeV Higgs boson is seen, and nothing else new is found, this will be rather strong direct evidence for the desert scenario. In that extreme, there would be no reliable, landmark, higher-mass scales for new experimental facilities to aim for. It would become more difficult to justify multi-billion dollar future colliders were one to be unable to certify in advance new discoveries, in the way that has been done for previous facilities (W at the SPS, top at the TeVatron, Higgs at the SSC and LHC). For these reasons alone, I see the outcome of the Higgs search as a crucial turning point for the future of the field.

The desert scenario is rather unpopular because of the hierarchy problem, namely the problem of why the Higgs mass remains so low when the natural scale for it, via quadratically divergent radiative corrections, appears to be much larger. This has led to the demand that something be invented to cure the problem, the leading candidate being the MSSM. However, the cosmological constant suffers from a very similar situation, one for which a straightforward application of SUSY does not work. So it seems to me that a serious, viable approach to the Higgs hierarchy problem is to ignore it for the present, arguing that a much deeper source of the solution, at the level of what is required for the cosmological constant problem, is required. And once the hierarchy problem is ignored, the "desert" theory is really very consistent, with no trace of the quadratic divergence problem remaining, after renormalization, in the phenomenology.

While the Higgs situation has not changed all that much in the last two decades, this does not mean that the Standard Model has remained unchanged. Thanks to the strong evidence that neutrino oscillations really exist, we now have a New Standard Model to replace the venerable Old Standard Model. Instead of the twenty or so parameters characterizing the Old Standard Model, we now have thirty or so parameters for the New Standard Model, the exact number depending a bit on what one wants to include in the count. For sure there are three neutrino masses and four CKM-like mixing angles to determine. In addition perhaps the masses of the three heavy Majorana particles of the seesaw mechanism should be included as parameters, as well as a couple of phase factors seen at best only in double-beta-decay processes and the like.

There is, in addition to the new parameter count, a definite shift at the GUT level from $SU(5)$-like thinking to $SO(10)$-like thinking. All this to me represents a significant advance, despite the presence of the extra parameters that require explanation. As evidenced in this meeting, there clearly will be increasing emphasis on the neutrino sector in the future: it carries with it more than 30 percent of the parameters of the New Standard Model, and these parameters will be at least as difficult to determine as accurately as the CKM parameters, at present the focus of the B-physics program. But twenty years ago, the determination of CKM phases seemed to be a remote experimental possibility. Hopefully future progress on the neutrino front will parallel what is happening now in the realm of B physics.

II. HADRON PHYSICS: WHAT IS IT?

The main thrust of this talk has to do with hadron physics. I define it as the physics of hadron structure and of (strong-interaction) vacuum structure. This puts it as a subfield of quantum chromodynamics (QCD), just as chemistry, condensed-matter physics, and atomic physics are subfields of quantum electrodynamics (QED).

In their infancy those three subfields were part of elementary particle physics, but now are not. This was also the case for nuclear physics. And I think the same has already happened to hadron physics. Most elementary particle physicists, including those in positions of influence, do not pay much attention to the issues in this field [1]. And in fact most of the experimental research in this field is done within the nuclear physics community, even though it is a stretch to identify hadron physics with nuclear physics. Because experimental (and of course theoretical) hadron physics research spans all energy scales, this creates social and organizational problems, a subject I will return to later [2].

To deal with these social issues I think that it is fundamental and important to define in detail what hadron physics encompasses, what its long range goals are, and what experimental and theoretical programs are necessary to attain those goals. I cannot by myself articulate this here in full. Hadron physics is very a big subject and I am sure to make errors of omission, and to bias the subject matter toward my own particular interests. In fact a better method might be simply to peruse the contents of the proceedings of this conference. In the next section, I will simply catalog some open problems I find interesting, as examples of the huge challenges that are present in this subject, challenges which exist at quite fundamental levels. And because the subject matter of hadron physics rarely allows reliable perturbation-theory calculations, real progress requires a data-driven approach, characterized by close interaction between theory and experiment. In the final section I will return to the social issues confronting hadron physics.

III. SOME OPEN PROBLEMS IN HADRON PHYSICS

QCD, the basic theory of the strong interactions, is at short distances a perturbation theory of the pointlike quark and gluon constituents of hadrons. At very large distances QCD is a theory of pions and nucleons (and their strange counterparts), and is characterized by spontaneous symmetry breaking of the approximate chiral symmetry of QCD. At intermediate distance scales, there is the rich arena of *e.g.* hadron resonances, Regge trajectories, soft diffraction, and hadronization of the partons, just to name a few of many topics.

But the physics at all distance scales is linked, and it is hard to find a situation, even within the relatively clean, perturbative regime, where the nonperturbative effects do not enter. Our first example is chosen to illustrate this phenomenon.

1. Perturbing the Chiral Vacuum

A classic way of trying to understand the properties of a macroscopic system is to perturb it with a small, localized impurity and study its response. In the case of the chiral vacuum of long-distance QCD, a nice way of doing this is by putting a small color dipole, such as heavy onium, into the vacuum and examining its response. This implies creation at large distances of a very weak pion cloud around the onium. The importance of this cloud can be assessed by putting in another small dipole, and determining the long range force between them, due to essentially two-pion meson exchange. This has been done elegantly and cleanly by Fujii and Kharzeev [3], who find that this force dominates at separations greater than about 0.6 fermi. To be sure the potential energy associated with this effect is quite small, under 1 MeV.

Nevertheless, this effect can be amplified by putting the two dipoles into motion. At the qualitative level, one can see that the original clouds will be compressed into pancakes when the onia become extreme-relativistic. And the original rest energy of a pion cloud, however small, can be turned into an arbitrarily large amount of energy-momentum of a pionic pancake if an arbitrarily large boost is applied. Now boost the two onia in opposite directions and put them into collision, again at a large impact parameter. When the momentum density in each of the pancakes exceeds, say 1 GeV/fermi^2 in the overlap region, there will be ample amounts of cms energy available for particle production, and the two onia should act like light hadrons as far as their collision properties are concerned: in the jargon of the trade they will "exchange a soft Pomeron".

Now the study of onium-onium collisions at extremely high energies is a favorite playground of perturbative-QCD theorists. This is the so called BFKL regime, where much effort has gone into summing up Feynman diagrams to obtain a candidate phenomenology of "hard Pomeron exchange" [4]. Nevertheless, the above argument implies that, for a fixed size of the dipoles, however small, if one goes to high enough energy the perturbation theory approach is destined to fail, and the soft physics is destined to re-emerge. The smaller the dipoles are, the higher will be the energy scale at which this phenomenon occurs.

The pion-cloud argument is not inconsistent with BFKL ideology, which also anticipates a similar phenomenon occurring, due to "diffusion of gluon ladder transverse momenta into the infrared". However, I am not sure that the energy scale where the transition occurs is the same in the two approaches. But the bottom line remains the same: no matter how hard one works for cleanliness in short-distance QCD, the soft physics usually finds its way into the picture.

2. Foundations of Perturbative QCD

Perturbative QCD (pQCD) is a highly sophisticated and well developed subject. However, at a fundamental level there are, I believe, some real problems. The basic issue is that, despite the fact that it is commonly done, it is not legal in pQCD to put the quarks and gluons on mass shell, *i.e.* to treat them as asymptotic states that propagate to infinity. They clearly do not—there is no S-matrix for quarks and gluons. It is rather ironic that in the old days before QCD one had an S matrix formalism without a field theory, while now we need a field theory formalism without an S-matrix.

To see the problem more concretely, it is only necessary to look at the classic process of electron-positron annihilation into hadrons, in lowest order. The associated vacuum polarization amplitude is a quark loop (Fig. 2) which at large spacelike Q^2 is a safe short-distance calculation. Its absorptive part at timelike Q^2 is essentially the cross section of interest. It is not completely ultraviolet safe, requiring an energy average to make it safe. But even with that done, our problem emerges when one wants to look at the angular distribution of the quark-antiquark dijets which build the total cross section. To get that differential cross section, one typically calculates the absorptive part as if the quarks could be put on mass shell—which we must admit is illegal.

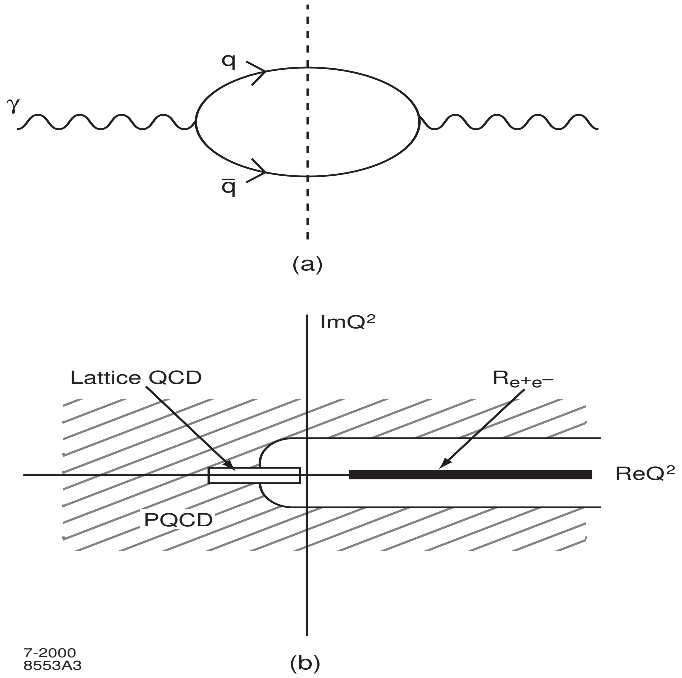

FIGURE 2. (a) The vacuum polarization amplitude whose absorptive part describes the quark-antiquark dijet final state in electron-positron annihilation. (b) The complex Q^2 plane appropriate to this process; only in the shaded region is perturbative QCD justifiable.

That this is not academic can be seen by imagining a QCD where only the bottom quark exists. With no light quarks available, the b and $\bar{b}$ quarks will (probably) be connected by an essentially unbreakable QCD string or flux tube. This leads (probably) to the conclusion that the final states will be a dense spectrum of excited onia, with no jets to be seen. The only way jets could occur is through glueball emission, and this appears to be a highly inefficient mechanism.

So the bottom line is that identification of final state jets with on-shell partons is a model assumption, which at present lacks a firm foundation. To be sure, it is an eminently reasonable assumption. But it would be better to have a sounder line of argument.

3. Parton Correlations and Multiplicities

How many quarks are there in a proton? "Three", says the spectroscopist. But the deep-inelastic community will (or should) answer "infinity". Both answers have their place, but connecting the two is still a problem. For example, there is a substantial sub-community of hadron physicists, in particular the practitioners of "exclusive QCD", who use a Fock-space description of the partons comprising a light-cone proton, with the "leading Fock-space component" having three and only three quarks in it. Now given that the average number of quarks is infinite (being essentially proportional to the integral over $\ell n\, x$ of $F_2(x)$), this would mean that the multiplicity distribution of partons is quite peculiar, with the "Fock-space" piece of it of finite mean multiplicity (with how much total weight, please?), while the rest is of infinite multiplicity.

I am simply baffled that this inconsistency of approach seems not to be recognized at all as a problem. When I mention it to others, the response seems to be that I am the one with the problem. Maybe this is so. But maybe there is a clue in the example of pion clouds around onium in item 1. A partonic description of the collision process described there (especially if one of the onia is replaced by a spacelike photon, which also makes a splendid small color dipole) leads essentially to a parton distribution as shown in Fig. 3. The region to the right is essentially perturbative and probably amenable to the Fock-space, perturbative methodology. But present in addition are all the nonperturbative partons comprising the pion cloud. For phenomenology which concentrates on the large-x valence system, the cloud partons are presumably inconsequential. However, as one goes from the case of small color dipoles to realistic light hadrons, the role of the cloud partons becomes much more important, and in the light-quark limit one must work hard to justify their neglect.

Even leaving this issue aside, the parton multiplicity distribution itself is poorly understood, to say the least. Is it Poissonian, or KNO? How might one distinguish one from the other? And the correlations of the partons in the transverse plane are largely unknown. For example, are most of the infinite sea of wee partons inside the three constituent quarks, are they mostly outside, or are they mostly uncorrelated?

There are good reasons why such simple questions remain unanswered, and most of them have to do with the fact that it is very hard to get at them experimentally. Double parton—or multiple parton—collisions have the potential to provide information on the correlations. While some experimental work has been done already,

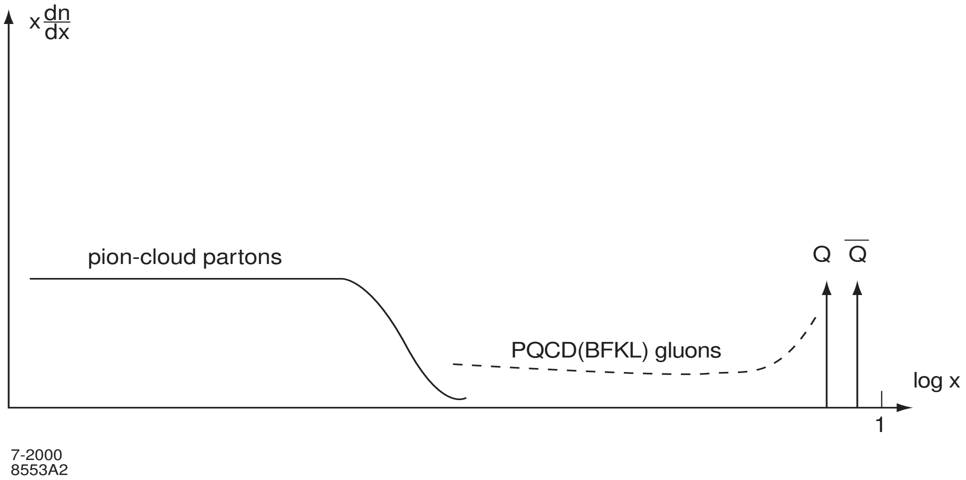

FIGURE 3. The parton distribution expected for a small, massive color dipole. The BFKL region where perturbative QCD may be applicable is of width $\sim \ell n\, Q^2 d^2$, where d is the (color) dipole moment of the onium. A similar picture should apply for the structure function of a virtual photon with squared momentum $P^2 \sim d^{-2}$.

this subject will become increasingly practical at the LHC [5], provided that at least one of the several thousands of experimentalists working there will care enough to make the measurements and to do the analysis. Nevertheless, even in the absence of data, it still might be interesting to have the various theoretical options compete with each other at the Monte-Carlo simulation level, in order to search for sensitive experimental indicators.

4. Spectroscopy of Light Quarks

Thus far, our examples have swung from the very large distance regime to the very short distance regime, with minimal emphasis on the intermediate distance scale. That scale is the richest phenomenologically, and is certainly the crux region to understand, in order to obtain a command of what QCD is really about. And at the heart of the subject is the hadron spectrum, in particular the spectrum of hadrons built from light quarks. There is a long and distinguished history of hadron spectroscopy. It deserves at least half, and probably more than half, of the credit for the establishment of the standard-model quark picture of hadron structure. For me, a high point of hadron spectroscopy occurred in the mid 1970s with the very sophisticated measurements and analyses of baryon resonance spectra. An enormous body of work could be summarized by the SU(6) classification "**56**, L even; **70**, L odd", very consistent with a quark-diquark picture of baryon structure [6]. I am not sure how well this picture has survived the subsequent 25 years. But whatever the present situation is for baryons, there has never been such an easy summary of the situation regarding the meson multiplets. For a given choice of J^{PC}, the lowest lying multiplet may be in good shape. But as soon as one looks at higher excitations, there are missing states and there are extra states, as we heard at this meeting from Jim Napolitano [7]. Without question, there is a great need—and opportunity—for a new round of experiments, especially utilizing hadron beams.

At present multiparticle spectrometers such as MPS and Omega are being phased out, and the only replacement for the future is a new facility in a photon beam at Jlab. From the technical point of view, it seems to me that it should be possible with state-of-the-art technology (as used by *e.g.* ATLAS, CMS, CDF, D0, *etc.*) to create an "electronic bubble chamber" for hadron-induced processes with acceptance, resolution, and particle identification capability at least as good as the old bubble chambers, and with rate capability better by a millionfold. And the analysis power for partial wave analyses or amplitude analyses likewise should exceed what was done then by a millionfold. So a next generation attack across the board would seem to me to be natural and to be potentially extremely productive. It should be the case that a command of hadron resonance spectra up to and beyond a mass scale of 2 GeV should yield a great deal of understanding of the systematics of hadron structure.

5. Non-Singlet Regge Trajectories

One of the most profound and fertile topics in pre-QCD strong interactions was that of the Regge theory of complex angular momentum and of Regge poles. It is a subfield with a distinguished heritage, leading, via duality and the Veneziano amplitude, to the creation of string theory and the modern superstring industry.

In the context of hadron physics, Regge theory remains of great significance. Most importantly it works. The experimental evidence for the existence of Regge trajectories is in some cases extremely quantitative, in particular for the trajectories containing vector mesons. This is especially impressive for the very pure power-law energy dependences of K regeneration amplitudes, recently reviewed for engineering reasons by the KTeV collaboration [8].

Specific properties of the Regge trajectories are strong indicators of the underlying dynamics. In particular linearly rising trajectories suggest strongly a QCD string type of dynamics. This again places emphasis on spectroscopic measurements at high spin and at high mass scales. In any case the Regge picture systematizes in an important way the properties of the resonant states.

I think much more work could be done in this field. As best I can recall, it was only in the mid-seventies when the Regge behavior became well-established experimentally, more or less concurrent with the discovery of the ψ and the subsequent change in direction of the field as a whole away from that line of research. So a return visit and systematic study in hadron-hadron collisions, with modern detectors and analysis techniques, might be very productive.

One area of special importance is that of non-singlet Regge behavior in deep-inelastic processes. In some processes there is evidence for the expected Regge behavior in the scaling limit, *e.g.* the neutron-proton difference, in the F_3 for neutrino reactions, and in the integrand of the Adler neutrino sum rule. However, for the spin sum rule that bears my name, there seems to be evidence for slow convergence of the sum at small x, while Regge arguments would suggest that rapid convergence should be the case. Examination of the Regge limit of the GDH sum rule in photoproduction and its extension to low Q^2 electroproduction could be of considerable use. But aside from details, measurements and analyses at small x which incisively test for Regge asymptotics are rare in the contemporary deep inelastic scattering culture. Given the enormous amount of attention paid to deep inelastic scattering in general, I find this situation perplexing.

And just at the theoretical level, does QCD imply that Regge-pole contributions scale, or should they be of higher twist? Can the Regge residues be calculated from pQCD or something close to it? And while there is a great deal of attention (rightfully) paid by theorists to the vacuum Reggeon singularity (Pomeron), the structure of non-singlet Regge-pole trajectories should if anything be an easier problem. I believe they deserve a closer look by theorists.

6. Heavy Quark Spectroscopy

With the high statistics and superb quality of recent charm and bottom physics experiments, the opportunities for incisive spectroscopic studies have increased dramatically. Things have gotten to the point that the statistics of decays such as $D \to K\pi\pi$, or even $D \to 3\pi$ is so high that examination of the Dalitz plot yields useful information on spectroscopy of ordinary mesons made of light quarks [9].

But the heavy quark excitations are themselves very interesting. The onium systems are so clean that pQCD is the most appropriate starting point. And their final states are fertile territory for glueball searches. Especially interesting to me are the D and especially B mesons, where the machinery of heavy quark effective theory can be applied. What this boils down to is that everything having to do with the heavy quark is relatively trivial, computable within pQCD, leaving the nontrivial system something very close to a single constituent quark. In fact, a viable definition of a constituent quark is the B meson "without" the b quark. So the excitations of B's quite directly probe the properties of the single constituent quark: for example its couplings to pions and photons, its mass, its size, and (if the energy scale of the B^* excitations can be made large enough) any intrinsic excitations of its own.

While the electron-positron B factories are ill-suited for this kind of physics, the hadron-hadron colliders are very well-suited. And in addition to the intrinsic hadron-physics interest in the classification and study of B^* and other excitations of bottom hadrons, there is a good engineering reason to do so. B^*s can help distinguish a secondary B_d from a $B_{\bar{d}}$ in CP studies, freeing the experimentalist from having to find a second tagging B in the event.

7. Confinement, Instantons, and the Vacuum

From the point of view of theory, the belief that QCD is a viable theory at all distance scales, despite the intractability of perturbation theory in the infrared, rests on a hypothesis—that of confinement. There is pretty good evidence for this from lattice calculations, not to mention the experimental facts—which include the fact of our existence. Nevertheless, there is as yet no consensus amongst theorists as to the mechanism that is responsible for confinement. This is probably the leading outstanding problem in all of hadron physics.

The confinement problem is closely linked to the problem of vacuum structure. We have already alluded to the presence of chiral symmetry breaking, leading to a chiral condensate at large distance scales. In addition to that structure, there is the vacuum structure induced by the occurrence of gauge potentials with nontrivial topology, and the existence of instanton-induced transitions between vacua with differing gauge topologies. This creates both good news (existence of mass of the η' meson) and bad news (the possibility of CP violation in the strong interactions).

When the instantons were first discovered, their effects were in poor theoretical control. But at present the situation seems to be much better. The instanton size distribution seems to be sharply peaked about a value characterized by a momentum scale of 600 MeV. The density (in Euclidean space-time) is relatively low, so that the fraction of spacetime containing instanton fields is only a couple of percent or so. And there is a rather convincing line of argument that this instanton population distorts the Dirac sea of light quarks in just the right way to induce chiral symmetry breaking. It would be nice if the argument were to go further and account for confinement as well, but this seems to be much less likely.

It would also be nice to have a better handle experimentally on instanton-induced effects. Efforts have been made to search for signatures of instanton effects in multiparticle final states in collision processes, although this is very difficult and speculative territory [10]. I have my own favorite candidate for a "smoking-gun" instanton-induced effect, namely the leading decay modes of the 0^- η_c charmonium state. They are $\eta\pi\pi$, $\eta'\pi\pi$, and $\bar{K}K\pi$, each with about a 5% branching ratio, and each being a state naturally produced via the 't Hooft instanton-induced interaction:

$$\mathcal{L} \sim (\bar{c}c)(\bar{u}u)(\bar{d}d)(\bar{s}s) \ . \tag{1}$$

I think close theoretical and experimental attention to these modes might well be useful.

8. Equations of State and Quark-Gluon Plasma

Macroscopic properties of QCD are described by equations of state, which can be studied as a function of the parameters of the theory, including quark masses, number of colors, and of course temperature and chemical potentials. There is plenty of activity and theoretical progress, as described here by Krishna Rajagopal [11]. And of course the heavy ion program provides plenty of experimental impetus. The future looks bright indeed.

I believe that the heavy-ion programs, especially from RHIC and the LHC, will have important spinoffs into high energy physics in at least two respects. One is that if the goal of observation of quark-gluon plasma is achieved quantitatively, measurement of the critical temperature should provide a quite good, competitive value of Λ_{QCD}. More generally, the methods by which ion-ion collisions are studied, with the emphasis on space-time evolution and hydrodynamic flow, will be of great use in dealing with generic hadron-hadron collisions at Tevatron and especially LHC energies, where the number of parton-parton interactions per collision rivals the number of nucleon-nucleon interactions per Au-Au collision.

9. High Parton Densities at Very High Energies

The energetic proton carries with it a very large number of partons, in particular wee gluons, when it has momentum of a TeV and above. This was anticipated by pQCD theorists, and has been well established by the measurements at HERA. As we mentioned above, this has great implications for central, generic collisions at LHC energies, where the phenomenology of typical collisions is expected to differ sharply from that at low energies, essentially because opaque discs of dense gluons are coming into collision. Even at the partonic level there will be strong absorptive effects, copious minijet production, and possibly collective flow. There deserves to be at the LHC (as well as at the TeVatron) serious attention paid to the commonplace collisions as well as the high priority rare ones [12]. There is a frontier of new physics to be explored.

I cannot resist mentioning here a vaguely related, speculative application of high gluon-density physics for RHIC. Consider a RHIC Au-Au collision, not in their laboratory, cms frame, but in a frame where one of the Au nuclei is at rest. What that Au nucleus sees is a very energetic ion bearing down on it, carrying momentum of about 20 TeV per nucleon. There clearly is an enormous wee-gluon density that the rest-frame ion sees. With probability unity, each parton in each rest-frame nucleon gets hit by a gluon, and is "Compton-scattered" into a relativistic final state, with large longitudinal laboratory momentum.

What this essentially means is that everything that was in the rest-frame nuclear matter gets swept up by the projectile and carried away with it at the speed of light—the nuclear matter is stuck to the pancake. So in the volume originally occupied by the resting ion there is "nothing" left.

While this "nothing" is probably not vacuum, it cannot be highly excited either, at least that part of the "nothing" which will radiate secondaries more or less isotropically. This follows simply from conservation of $E - p_z$. In the initial state the value of $E - p_z$ is essentially A GeV, and this is spread over a large volume. So the density of $E - p_z$ in the final state must be low, no more than 140 MeV/fermi^3. This would seem to imply that at most only soft pions can be isotropically emitted from the "nothing". Perhaps these pions could be coherently emitted, a la Bose condensation or via a disoriented-chiral-condensate (DCC) mechanism. In the RHIC laboratory frame, this would imply a secondary "beam" of these pions emerging in the forward direction, with the same velocity as the incident ion beam. In other words, given 100 GeV/nucleon incident momentum, this cluster of pions would emerge with 14 GeV/pion, and with a transverse-momentum spread perhaps as low as 100 MeV. Such pions would be difficult to detect with the existing detectors, although it is not hard to envisage detectors which could do the job [13].

10. The Approach to Scaling

A very large amount of activity now exists in electron-nucleon and electron-ion scattering at intermediate energies, especially at Jlab, where a variety of very beautiful measurements are emerging. Here the basic challenge, as I see it, is to map out in detail the transition from manifestly long-distance descriptions (*e.g.* elastic e-p scattering at relatively small Q^2) to manifestly short distance descriptions, as used in the scaling region in deep-inelastic scattering. There are a variety of kinematic regions to experimentally explore, the two most important ones being high Q^2 at small inelasticity ("exclusive QCD") and low Q^2 at large inelasticity ("soft Pomeron physics"). Both are merged at moderate values of Q^2 and inelasticity with the physics of resonances and non-singlet Regge dynamics.

Among the theoretical challenges is the search for the best descriptive tools. Help comes from duality concepts, both of the Bloom-Gilman type as well as the Regge-resonance type, not to mention parton-hadron duality in its more general context. Sum rules are a powerful tool. While much more can be done with such tools, I think the time has come to search further for specific dynamical descriptions good enough to incorporate the sum rules (in particular a fully relativistic description) and other general features. An excellent example of what I have in mind is the chiral quark model of hadrons, as laid out by D. Diakonov, M. Polyakov, and their collaborators [14]. They start at the fundamental level of short distance QCD, integrate out instanton effects, and find a viable description of hadrons in the chiral sector which still is consistent with relativity and deep inelastic phenomenology. While the assumptions made are perhaps not under complete theoretical control, the credibility level is still very high, and capable in the future of going higher.

11. Diffraction

High energy hadrons, as extended objects, are nearly black discs. Their elastic scattering amplitudes provide clear evidence that this is the case. However, the shadows that hadrons cast in their high energy interactions are much more interesting and subtle than the shadow physics of elastic processes. In particular, inelastic diffraction is endemic, not only in hadron-hadron collisions, but also in electron-hadron collisions, where arguably 20 to 30 percent of all deep inelastic collisions lead to a diffractive final state (defined as a large final-state "rapidity gap", not exponentially suppressed, within which no secondary hadrons are to be found). In addition, diffractive final states are seen in a significant fraction of hard-collision events, namely events containing high-transverse-momentum jets, Ws, Zs, and/or leptons in the final state.

The favored descriptive tool for diffractive processes is that of the t-channel exchange of a Reggeon, the so-called Pomeron. Ingelman and Schlein, in a seminal work which created and thus far has defined the field of hard diffraction, suggested that this Pomeron should have a partonic description, like ordinary hadrons [15]. This concept has greatly helped to drive the field in a very productive manner, especially with regard to creation of a vital and exciting experimental program. Nevertheless, the foundations of the idea are speculative. And the field at present is confused. The data refuse to be easily integrated into the formalism. This was evidenced at this meeting in the excellent talk by Hatakeyama [16]. I personally think that it is time to retreat from the language of Pomeron structure functions, and to search, at the experimental level, for more general and reliable descriptive tools to organize and systematize the phenomenology.

In particular, it should not be taken as obvious that the t-channel exchange picture is the most appropriate language. It may be that it is better to emphasize more the s-channel, shadowy origins of the diffractive phenomena, perhaps in the style of Good and Walker's original description of diffraction dissociation [17], and/or of absorption models. And no matter what, one must acknowledge that the heart of the subject resides, from a diagrammatic point of view, in loop diagrams, not tree diagrams, and that quantum effects, as opposed to quasiclassical partonic visualizations, are essential no matter what descriptive viewpoint is adopted.

My favorite example for appreciating the subtlety of the phenomenon is that of high mass, soft inelastic diffraction. A typical final-state pseudorapidity distribution of secondary particles is illustrated in Fig. 4. First imagine that the reference frame is chosen such that zero rapidity is at position A. Then what one would see at early times is an ordinary multiparticle final state developing, with soft particles being produced at large angles at early times, and more energetic particles being produced at small angles at later times (the "inside-outside" cascade). But at some "macroscopic" late time (proportional to the energy of the fastest right-moving diffractively produced secondary), which can in practice be tens or hundreds of fermis, the leading right-moving emitter suddenly stops emitting: a quantum decision has been made that the right mover (neither a nucleon or "not a nucleon", but a quantum superposition of each possibility) projects to the right-moving nucleon state.

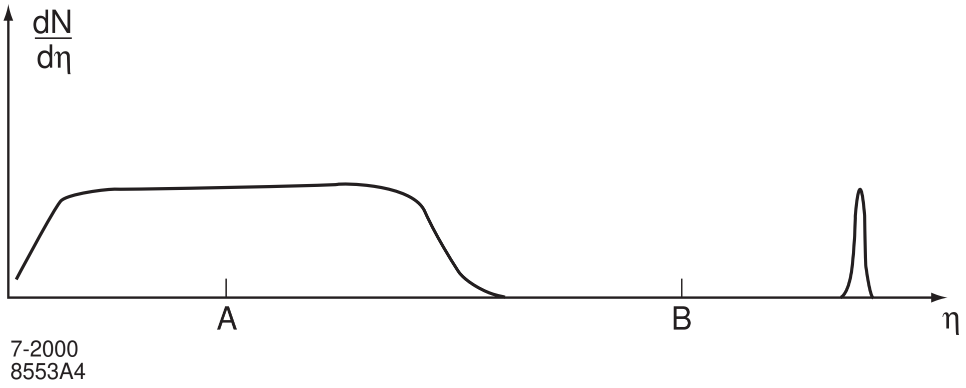

FIGURE 4. Typical pseudorapidity distribution of secondary hadrons in massive soft inelastic diffraction.

Now view the same process in a different reference frame, where zero pseudorapidity occurs at location B. In such a frame at early times there is *no* particle emission, as if the process were at most elastic scattering. But then, again at a "macroscopically" late time (proportional to the energy of the least energetic left-moving diffractive secondary—actually the same particle as before), the left mover (which has to be neither "not a nucleon" nor a nucleon, but a quantum superposition of each possibility) makes a quantum decision *not* to be the nucleon and to emit particles.

These two viewpoints represent the *same* physics, and a good picture of diffraction should be able to explain how and why they are the same. I find this a very interesting challenge, one which the present diagrammatic/partonic approach does not begin to touch. To do a really good job on diffractive physics may well be the last great frontier in hadron physics to be solved.

12. Hadronization Dynamics

A primary task of the hadron-physics subfield of multiparticle production is to understand and describe the transition from the multipartonic evolution at very short distance scales to the multihadronic final states observed experimentally. The prototypical reaction for doing this is electron-positron annihilation into hadrons. In that case, there is a rather satisfactory level of understanding, thanks both to the intrinsic cleanliness of the basic process, and to the large data set of complete events over a large energy scale.

Even so, it is interesting that two apparently competitive viewpoints, that of the nonperturbative QCD string, *a la* Lund, and that of the QCD partonic cascade, peacefully coexist. Quite sophisticated phenomena, such as the "string effect" in three-jet final states, can be described in either picture with comparable success.

Which is right? Conventional wisdom seems to be that most of the space-time evolution is in fact perturbative, with a rather quick transition to the final configuration of emergent hadrons. (This is basically the "preconfinement" picture). However, were the lightest quarks to have mass of a GeV or so, then QCD strings would not easily break, and the spacetime region of final-state evolution would be enlarged, with an interior boundary between perturbative evolution and stringy evolution (Fig. 5). In the heavy quark limit the future light cone gets filled with string. It might be of interest to study the hadronization phenomenology as function of quark masses in order to sort out perturbative from stringy effects.

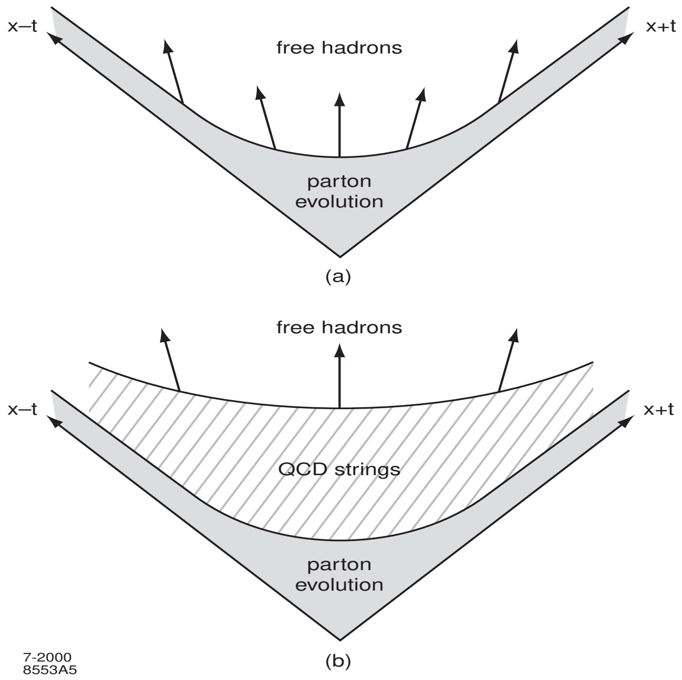

FIGURE 5. (a) Final-state evolution in dijet production in e^+e^- annihilation. (b) The same, in the case of the lightest quarks having masses ~ 1 GeV.

Hadronization phenomenology in hadron-hadron collisions is different and more difficult. The whole subject is in a much more primitive state, especially at collider energies. Not only is the theory much harder, but also there is a paucity of data. Much more experimental attention needs to be paid to the generic collision phenomenology. I think it is a necessary condition for significant progress to be made [18].

IV. THE FUTURE OF HADRON PHYSICS

In the introductory section, we argued that hadron physics has social problems that require it to be better defined and organized than at present. The main reason that drives this notion beyond the merely academic and that requires, in my opinion, some action is that this would facilitate a more rational pattern of funding and support, and that it would facilitate better access to the high-energy physics laboratories, built and managed for purposes other than exploring the details of hadron structure. In the previous cases of chemistry, atomic, and nuclear physics, those who chose to specialize in those fields instead of moving on to the higher energies and shorter distances of particle physics could rather easily do so. Experimental facilities fit, until quite recently, on university campuses. And the funding structures, including peer review systems, adiabatically evolved to adapt to changes of scientific emphases. But hadron physics now presents itself as a crossover field. It is beyond nuclear physics, although heavily populated by nuclear physicists. And much of it is within the energy scale of high energy physics, despite the fact that not many high energy physicists are practitioners.

Because of this, access of experimental hadron physicists to high energy laboratories is made especially difficult. If, in an austere fiscal situation, a hadron physics initiative of the highest quality is put in competition with, say, a quality next-generation neutrino experiment, there will likely be very little support within the high-energy community for the hadron-physics initiative. Indeed, were I myself wearing my high-energy physics hat, I would have a hard time too. Under these circumstances, it seems to me that the best way for hadron physics initiatives to be viable at high energy labs is that there be independent funding available, and that there be agreements with the high energy laboratories for a certain amount of access to collision regions, beam lines, luminosity, running time, infrastructure, *etc.* in return for appropriate contributions to the laboratory budgets. Hadron physics review structures at the program-committee level would be essentially independent of those of particle physics, although at higher policy levels there would necessarily be mixing of the communities.

Examples of this kind of setup exist. At SLAC the NPAS program allowed use of the linac for fixed target experiments at moderate energies of interest to the nuclear community. Despite its modest size, I have been told it was difficult to establish. The best example seems to me to be CERN, which for a long time has had nuclear physics as part of its program, a feature which is now expressed in the heavy ion initiatives at the SPS and at the LHC. The presence of the nuclear physics component at CERN has been important not only in providing a broader scientific base, with all the opportunities for cross-fertilization and diversification that that implies, but has also been useful in broadening its political base.

In the United States, Fermilab is a good example of a high energy laboratory where hadron-physics could be pursued much more aggressively than at present. One of the main-injector beam lines could well support that dream next-generation multiparticle spectrometer for spectroscopy, Regge dynamics, and correlation studies mentioned in item 4 of the previous section. Hyperon physics and charm physics are other possible options. Antiproton sources have been productive venues for charmonium and other studies, both at Fermilab and CERN, and there is more to be done. Indeed a workshop is scheduled for investigating such future options at Fermilab [19]. Finally, the C0 collision region of the TeVatron collider, the presumed home of the future BTeV B-physics initiative, is also a very attractive venue for studying hadron physics. The topics include charm physics, low p_T, diffraction, leading particle studies, and the study of collision dynamics, especially were full-acceptance detection of complete events available. And the future facilities under discussion, in particular muon colliders and/or neutrino sources based on muon-collider technology, are rich sources of hadron-physics spinoffs. While other spinoffs, such as K-decay physics, muon physics and deep inelastic neutrino reactions, have been discussed in this context, very little attention has been paid to the hadron-physics opportunities [20].

I think that a necessary condition for the situation to change is that the hadron physics community organizes itself better. It must not only identify itself and exhibit some political strength, but it must also define better what hadron physics comprises, what its fundamental scientific goals are, what the experimental programs are that deserve the greatest attention, and what the basic challenges to theory are. Since organizational changes within funding agencies and advisory structures are likely to be slow, it seems to me that the best opportunity for getting things going might be within the professional societies. In particular there perhaps should be a Division of Hadron Physics within the American Physical Society. It might provide the venue and organizational structures for achieving the above goals, and provide a basis for going further if, as I suspect is the case, it is deemed necessary to do so.

But most fundamental of all is that there exists a vital community of experimental and theoretical physicists just doing hadron physics, no matter what the obstacles. This meeting has been a splendid example that there are at present plenty of people doing just that. We should do everything we can to not only keep this field healthy, but to strengthen it. The scientific challenges will take quite some time to overcome, and in the meantime we must make every effort to acquire the means to overcome them.

V. ACKNOWLEDGMENTS

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