This talk covers a selection of topics that were discussed at this meeting in the non-heavy-ion sessions: (a) The high $Q^2$ events at HERA and their theoretical interpretation. (b) The issues of how well we know the predictions of QCD, with a particular emphasis on resummation. (c) Anomalies in jet production in association with $W$ bosons at the Tevatron and in dijet production in deep-inelastic scattering. (d) Diffractive hard scattering.

1 Introduction

There were many interesting talks at this meeting, and I have chosen to make a summary in the style of the headline news on television: I have picked a few issues to discuss that particularly appealed to me when I was preparing the talk. I apologize to the many speakers whose work I do not mention.

2 High $Q^2$ events at HERA

The most notorious results presented at this conference were probably the excess events observed by both H1 and ZEUS at high $Q^2$ and high $x$. Since no theory talks were presented that analyzed that data, I will give a brief summary of recent work on the subject. This will provide a convenient lead-in to the rest of my talk.

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aTo appear in Proceedings of Rencontre de Moriond, “QCD and High-Energy Hadronic Interaction”, March 22-29, 1997, Éditions Frontières.
2.1 Data

H1 \cite{4} reported 7 deep-inelastic events in a region where only about 1 is expected on the basis of standard model physics. The events appear to be ordinary DIS events, with an electron-jet topology, and the region in which they occur is where $M_{e^-\text{-jet}} = 200 \pm 25 \text{ GeV}$, $y > 0.04$, and $Q^2 > 15000 \text{ GeV}^2$.

ZEUS \cite{5} reported 2 events in $Q^2 > 35000 \text{ GeV}^2$ where $0.145 \pm 0.013$ are expected and 4 events in $x > 0.55$ and $y > 0.25$ where $0.91 \pm 0.08$ are expected.

There is a 1\% probability that the H1 events arise by chance, and a few percent probability for the ZEUS events. The backgrounds are small, and the uncertainties in the QCD calculation of the background are small, since normal DIS in this region is governed by valence quark distributions. Thus the result is a priori significant. One should remember the following:

- There were about 55 experimental talks presented at this meeting, and I estimate that each talk had between 1 and 40 distinct physics results. That is, many hundreds of results were presented, and one should expect that several results were presented that are significant at the 1\% level.

- The higher luminosity experiment, ZEUS, with 20 pb$^{-1}$ has a smaller signal than H1 (14 pb$^{-1}$).

- In assessing a small probability for any particular anomaly to occur, one should remember that there is a noticeable probability of a major bug in applications of theory and in the analysis of data. It is easy to recall examples from earlier experiments. One should also recall Steinberger’s talk here \cite{3}; he showed how a significant anomaly in $R_b$ disappeared after a close analysis of systematic errors.

- On the other hand, one should remember that the masses of the muon and pion are fairly close and that the masses of the tau lepton and the charm quark are fairly close. These coincidences between the masses of first and second generation hadrons and certain leptons caused confusion in the initial investigation of these particles. Perhaps a leptoquark of mass 200 GeV is there to create equal confusion in the investigation of the slightly less massive top quark?

Clearly, one cannot say that there really is new physics until the results have withstood the scrutiny of (a) a repetition of the measurements in a new run at HERA, (b) a search for corresponding physics (see Sect. 2.2) at other places (Tevatron and LEP), and (c) a careful check for errors in both theory and experiment. Too great a scepticism is also unwarranted. One must treat searches for new physics in the same way as a drug company’s search for compounds with interesting pharmaceutical properties. A first round of screening of a large collection of compounds will inevitably produce false positive results. A second round of tests is necessary to verify which are the real results.

The cautionary tale of the discovery of the $J/\psi$ must also be remembered. The $J/\psi$ was first seen \cite{1} as what we now know is a substantial shoulder above the Drell-Yan continuum. The shoulder was the narrow resonance smeared out by detector resolution. But one of the conclusions of the paper was “No resonances (i.e., 1$^-$ bumps) are observed, . . . ”. In fairness to the experimentalists we should remember that the theorists weren’t doing so well: the Drell-Yan curve was well above not only the continuum but also the smeared resonance! Despite this, we now know that the Drell-Yan model provides a correct first approximation to real QCD predictions. We clearly see how much theory has improved in the last 25 years.
2.2 Theory

A consistent picture of how to interpret the HERA excess appears in a large number of recent preprints. None of this work was reported at this meeting, although a number of relevant experimental results were reported.

New physics below its threshold can be described by a contact interaction. The obvious contact interaction needed to get an excess of DIS events is a 4-fermi electron-quark vertex. To get the correct number of events, this must be stronger than the standard electro-weak interaction due to $Z$ and $\gamma$ exchange, in the relevant kinematic range. Such an interaction must give substantial effects at LEP. There are several possible Dirac-matrix structures, and current data rule out almost all relevant terms in a contact interaction. With more statistics, all the terms can be ruled out.

It is therefore more reasonable to postulate an interaction with an enhancement in the electron-quark channel. This can be most easily modeled as the production of a leptoquark, either of spin zero or spin one. Its mass should be about 200 GeV, according to the H1 data. The value of the electron-quark-leptoquark coupling $\lambda$ depends on which quark is involved and on the branching ratio to the observed channel. If one assumes a branching ratio of one and a coupling to the predominant valence quark $u$, then one gets the smallest coupling, substantially smaller than the electromagnetic coupling (0.02 to 0.05). This implies that the leptoquark is a narrow resonance.

Since a leptoquark must have normal QCD couplings to gluons, it can be pair-produced at the Tevatron. In the case of a scalar leptoquark, the cross section is completely predicted. In the case of a vector leptoquark there is an anomalous color magnetic moment coupling, and there is a range of predictions for the cross section, with the lower bound being higher than the cross section for a scalar leptoquark. At this meeting, Valls showed the bounds from the Tevatron: 175 GeV for a scalar leptoquark with a 100% branching ratio to an electron plus quark, and 298 GeV for a vector leptoquark. Because of the weak value of the coupling, the rate for single leptoquark production ($g + q \rightarrow e + \text{leptoquark}$) is too small to be measured.

So a leptoquark is not quite ruled out.

Clearly we should have better data within a year. The HERA experiments will be able to confirm the excess, and if the excess is real, then there are predictions for LEP and the Tevatron that it will be possible to test.

3 QCD

Overall, we see good agreement between experiment and the predictions of QCD. (In this context, this means perturbative predictions, since these are the only predictions we have for high-energy scattering.) A good example (out of many possibilities) is a plot shown by Ghez of event shapes measured by the OPAL collaboration in $e^+e^-$ annihilation at 161 GeV. Given the value of $\alpha_s$ (at some given scale in a given scheme), the predictions are absolute. Basically, the predictions are obtained using fixed-order NLO calculations. But at the more extreme values of the event-shape variables resummation is needed to get accurate predictions.

The result of much experience is that we trust QCD as a predictive theory of strong interactions. (However, we do not trust ourselves to get these predictions correct every time!)

A larger class of cross sections involve non-perturbative quantities: parton densities, fragmentation functions and their kin. The predictive power of QCD is, of course, that the parton densities, etc., can be measured in a limited set of processes and then used to predict many other processes. One example is the single-jet-inclusive cross section at the Tevatron. Nang’s talk showed data from CDF and D0. The D0 data is in agreement with the predictions of a range of jet $E_T$ from 50 to 500 GeV, while the cross section falls by close to 7 orders of magnitude. But it should be noted
that the errors get quite large at the higher values of $E_T$: up to 50%. As is well-known, the CDF data becomes somewhat high at large $E_T$. Despite this, the CDF and D0 data are in agreement. (This is no contradiction since equality within errors is not a transitive relation, i.e., if $A = B$ and $B = C$ within errors, it does not follow that $A = C$ within errors.)

If one wants to know where the excess events in D0 disappeared to, see Sect. 4.1.

3.1 Errors

This brings me to some important issues:

- How well do we know parton densities? I.e., what are the errors on the global fits?
- How accurately can we make QCD predictions? I.e., how accurate are the theoretical formulae?

It should be obvious, for example from many talks at this meeting, that QCD predictions are critical to new physics searches. Signals of new physics are generally a difference between data and a standard-model prediction with a large QCD components. Clearly we need to have a reliable quantitative estimates of the errors in our QCD predictions. This is quite non-trivial.

For example, predictions for cross sections at the Tevatron and at HERA rely heavily on the values of parton densities. These are obtained from global fits that involve 30 to 40 parameters in the theory and over a thousand data points from many different experiments. The difficulty of obtaining reliable errors can be illustrated by observing that adjusting a correctly defined $\chi^2$ to within 1 unit of its minimum is the correct way of obtaining the best fit, whereas the actual value of $\chi^2$ will be about 1000. Thus $\chi^2$ must be calculated to better than 0.1% accuracy.

In the case of purely statistical errors, we understand $\chi^2$ and have no problem understanding its calculation. But in many cases systematic errors dominate. Almost by definition, we do not understand the statistics of systematic errors, so any attempt to treat them suffers from some imprecision. But we must try to do our best.

Perhaps the most important feature of systematic errors in a measurement of a differential cross section is that they are normally highly correlated between different data points. Consider a measurement of a structure function $F_2(x)$ with, say, 12 data points when the dominant systematic error is a calibration error with a definite shape in $x$. A 1σ change in the data due to the calibration error is not very significant, and if $\chi^2$ is correctly calculated it should only change by about 1 unit due to the change in the data. But if we were to follow the common procedure of adding systematic and statistical errors in quadrature, then a deviation between theory and experiment of such a form would change the calculated $\chi^2$ by 12 units — enormously significant, apparently.

Unfortunately in global analyses, incorrect calculations of the effect of systematic errors on $\chi^2$ are typically used. This is not through incompetence, since the necessary information is not always available. This situation has to be changed. Without a correct treatment of correlated systematic errors, the effects of certain deviations between theory and experiment are enormously overstated. If one consequently adjusts the threshold in $\chi^2$ which one considers to give a significant deviation between theory and experiment, there is a danger of ignoring many genuinely significant deviations.

Although error analysis appears to be a recondite subject, it is vital to the progress of QCD and to the search for new physics. So it was very gratifying to hear Yu’s talk [9] about an analysis of neutrino scattering data from CCFR and other experiments with a correct treatment of correlated errors. Another improvement in the accuracy of this work was the use by Kataev and collaborators [10] of NNLO calculations. A combination of the methods in these two talks will result in very reliable determinations of $\Lambda_{\overline{MS}}$ and of the non-singlet quark densities.

I should add that good analyses should attempt to treat errors on the theory due to higher order corrections, etc., in a similar fashion. The errors are highly correlated.
3.2 Resummation

Unfortunately, the theoretical errors are often bigger than the experimental errors. This is very embarrassing to theorists, of course, and work is urgently needed to remedy the situation. Brute force higher order calculations are only part of the answer, because higher order corrections with the standard techniques perturbative QCD are often excessively large. One can see the symptoms of this in many of the talks at this meeting.

One reason for the large corrections is that the cancellation of soft and collinear parton emission is not particularly good. For example, one might meet a LO and an NLO term like

\[ f(x) + \frac{\alpha_s}{2\pi} \int_x^1 d\xi f(\xi) \left( \frac{1}{\xi - x} \right) . \] (1)

There is a divergence in the integral over the region \( \xi > x \), and the plus-distribution cancels this by what is in effect a delta-function with an infinite coefficient localized at \( \xi = x \). If the parton density \( f(x) \) is rapidly varying, this cancellation is numerically poor and results in a large numerical coefficient for the \( \alpha_s \) term.

There are many variations on this theme. The problem is not just that one gets a large NLO correction, but that a typical source of the large correction will also induce large corrections in even higher order terms. The only remedy for this situation is to reorganize the theory, for example by a resummation over all orders of the large terms. Only by such means does one get accurate predictions.

Indeed, if an appropriate resummation is not done, one can easily end up with unphysical cross sections: for example an isolated direct photon cross section that is larger than the inclusive cross section. There were a number of talks in this area, and it is evident that resummation is quite essential in many processes. Experimental evidence was quite nicely shown in a plot of event shapes at SLD. Without resummation, theory (at \( O(\alpha_s^2) \)) fails to describe, for example, the thrust distribution below a thrust of about 0.1.

However, doing the resummation correctly is somewhat tricky. For example, while Berger \[11\] and Fontannaz \[12\] agreed on the need for resummation in the cross section for the production of isolated direct photons in hadron-hadron collisions, they disagreed on the details, including such a fundamental question as to whether the cross section is finite before resummation. The kinematics of the soft gluons that cause the problem is tricky enough that it takes a substantial time for outsiders (like myself) to understand the issues well enough to gain a reliable opinion on the rights and the wrongs of the issue.

But getting these details sorted out will be essential to the progress of QCD and of its applications. Resummation in all its varieties is essential to high-precision phenomenology.

4 Anomalies in jet cross sections

There were two anomalous results presented at this meeting that seemed to represent more significant problems than the widely publicized large \( Q^2 \) events at HERA and the high \( E_T \) jets at CDF.

4.1 \( W + \) jets at D0

The first \[13\] was in the rate of \( W + \) jet events in the D0 experiment. Between 20 and 60 GeV, the ratio \( \sigma(W + 1 \ \text{jet})/\sigma(W + 0 \ \text{jets}) \) is consistently 50% and more above theory, well outside the errors on both theory and experiment. This is in a domain where one generally considers QCD and the
standard model to be valid. In particular, inclusive $W$ cross sections and inclusive jet cross sections are in agreement with QCD predictions.

For the same cross section, data was presented \[13\] from the CDF experiment, and the graphs show good agreement between theory and experiment. However, this good agreement only concerns the shape of the cross section, as a function of jet $E_T$. The theory curve was normalized to the data.

4.2 Dijets in DIS

A second deviation between theory and experiment was in dijet rates in DIS \[14\]. H1 has found that the rate of dijets in DIS is consistently well above NLO QCD predictions for a whole kinematic range ($5 < Q^2 < 100 \text{GeV}^2$ and $10^{-4} < x < 10^{-2}$). Earlier measurements \[15\] of multi-jet rates were in agreement with QCD models. This gives a “problem for [the] measurement of $\alpha_s$ or [the] extraction of [the] gluon density from jets”. Interestingly, the color dipole model (ARIADNE) does describe the data perfectly well. Since the values of $Q^2$ and $x$ are rather small, it is likely that the problem is that the use of conventional fixed order calculations that is wrong. (Note in particular that $Q^2/s$ in this data sample goes down to $5 \times 10^{-5}$.) It is the higher $Q^2$ data that should be used for measuring $\alpha_s$ with the aid of fixed order QCD calculations.

This point is further strengthened when one observes that the new data involves a minimum $p_t$ for the jets of 5 GeV, which is rather larger than the scale $Q$ set by the virtual photon. A treatment in analogy with photoproduction is perhaps appropriate, and on the basis of the $x_\gamma$ distribution, the speaker suggested that there is evidence for a resolved component in virtual photons. (I would add that since the photons have a $Q^2$ that is in the perturbative region, this resolved component should be amenable to a perturbative analysis.)

Clearly more theoretical work is needed here, and it is not obvious that we have a contradiction with QCD. Certainly it is an area ripe for the application of suitable resummation techniques.

5 Diffraction

Over the past few years we have seen a renaissance of interest in diffractive scattering. It is now respectable to discuss the pomeron. It is not as if the physics represented by the pomeron ever disappeared; it gives, in fact, the bulk of hadronic cross sections. An important but extremely difficult problem is to understand pomeron physics, diffraction in particular, within QCD. What is possible for the first time in the current generation of experiments is to study hard scattering in association with diffraction.

Diffractive processes can be defined as those with non-exponentially suppressed rapidity gaps. There are several distinct classes of diffractive processes that are being investigated: (a) those with rapidity gaps between a beam hadron and the rest of the final state, (b) those with rapidity gaps between jets, and (c) highly exclusive final states in photo- and electro-production. Because of limited time I only discussed the first of these topics.

We saw the latest data from CDF, D0, H1 and ZEUS \[16–18\]. This may be regarded as probing the partonic structure of the pomeron, with the Ingelman-Schlein model representing a benchmark model.

One interesting property is that diffractive hard scattering is much rarer in Chicago than in Hamburg: the fraction of hard scattering that is diffractive is typically under 1% in hadron-hadron collisions at the Tevatron, whereas the fraction is closer to 10% at HERA. Thus it has taken rather longer for CDF and D0 to establish their signals for diffractive hard scattering.

As Kaidalov \[19\] emphasized, an exchanged pomeron is not to be regarded as a real particle. Its parton densities do not obey a momentum sum rule. Moreover, there are absorptive corrections in
diffractive hadron-hadron scattering, but not in diffractive DIS. This implies that a simple application of the methods perturbative QCD will not predict the Tevatron cross sections given the ones at HERA, and that the cross sections should be rather lower than predicted. This presumably explains the lower diffractive fractions at the Tevatron.

Quantitative phenomenology is progressing very nicely at HERA. Given the absence of the absorptive corrections, one can treat the results as directly measuring the parton densities of the pomeron. (Though whether this is legitimate is another question.) H1 showed for the first time data on diffractive dijet production in DIS. The measured cross sections can only be fit if there is a large gluon content in the pomeron, since the quarks are constrained by diffractive DIS. This very prettily confirms the large gluon content deduced by ZEUS from their diffractive photoproduction data (and also confirms that the theoretical picture is consistent). It is also consistent with the results obtained by H1 by a quantitative analysis of the scaling violations in their data on $F_2^{\text{diff}}$. We must regard the pomeron as a glue-ball state to a first approximation.

With these analyses it is clear that the Tevatron data are well below the expectations if hard-scattering factorization holds. Goulianos [16] showed a plot of momentum fractions in the pomeron. At both the Tevatron and at HERA we have measurements of two or more processes, so that both the quark and the gluon content of the pomeron are constrained. The Tevatron cross sections are a factor of several below those that one would expect on the basis of the HERA. This is very direct evidence for absorptive corrections, and therefore diffractive hard scattering is proving to be an effective tool as a microscopic probe of the structure of soft hadronic cross sections.

We can expect more incisive results in the future, for example, from an analysis of the data [18] from ZEUS where the diffracted proton is explicitly measured (and corresponding future data from the other experiments).

6 Conclusions

Cliché 1 There are a few anomalies among the generally good agreement between theory and experiment. The notable ones are: (a) the high $Q^2$/high $x$ events at HERA, (b) an excess of $W +$ jets events in D0, (c) an excess of dijet events in DIS in H1. The last two are in an area in which we expect QCD to be valid. The first is in an extreme domain of $Q$, so is a potential signal of new physics; but the new physics should be visible in other experiments at LEP and/or the Tevatron in the near future.

Cliché 2 We have seen a number of significant effects disappear or not find themselves confirmed by other experiments: the $R_b$ anomaly at LEP, the four-jet events at ALEPH, the high $E_T$ jets at CDF. Given the success of the standard model, one must put the balance of the probability on other anomalies as being due to a fluctuation or an error of some kind.

Cliché 3 QCD is a highly non-trivial theory, as are the experiments needed for its investigation. A high level of quantitative understanding of QCD is needed in current and future searches for new physics. There are many prospects for gaining understanding of QCD in innovative ways: e.g., the various programs on resummation, and the active work on diffractive physics. Data that is improved in accuracy and in the range of phenomena measured is pushing the theorists to match the experiments.
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1. H1 talk at this meeting.
2. ZEUS talk at this meeting.
3. J. Steinberger, this meeting.
4. J.H. Christenson et al., Phys. Rev. D8, 2016 (1973).
5. Relevant preprints can be found among hep-ph/9703nnn.
6. J.A. Valls, this meeting.
7. P. Ghez, this meeting.
8. F. Nang, this meeting.
9. J. Yu, this meeting.
10. A. Kataev, this meeting.
11. E.L. Berger, this meeting.
12. M. Fontannaz, this meeting.
13. J. Dittmann, this meeting.
14. J. Spiekermann, this meeting.
15. H1 Collaboration (I. Abt et al.), Z. Phys. C61, 59 (1994).
16. K. Goulianos, this meeting.
17. E. Shabalina, this meeting.
18. J. Grosse-Knetter, this meeting.
19. A. Kaidalov, this meeting.