I WOULD LIKE to thank the organizers for the invitation to speak on this great occasion. To set the tone, I’d like to begin with a couple of quotes from our honored guest. The first comes from the 1973 Photon-Lepton Conference held in Bonn. Sid introduced his talk on Deep Inelastic Physics with

Ancient explorers in search of distant lands and treasures had no idea how great the challenge or how difficult the passage. They sailed into uncharted seas. Their only scale of distances came from previous journeys starting from ancient Phoenicia and . . . Crete, through the Mediterranean to the North Sea. . . We can look back today with ancient mariners and view how far we have progressed . . . in our explorations of the Lepton [Hadron] frontiers of Nature.

I have to confess: at the time, I had little idea what Sid was talking about. Now I understand that he was planting in the historical record a classical context for the talk I am about to give.

As all of you know, Sid was a much more practical man than this quote would suggest. His down-to-earth attitude toward particle theory rubbed off on most of his students. It is exemplified by the following quote from an afternoon discussion section at the 1969 Erice School. Sid was asked what he thought of the attempt to understand deep inelastic phenomena with the aid of the DGS representation. You don’t have to know anything about the DGS representation to appreciate his response:

I just want to make a remark about the use of the DGS representation. When I started this problem, I studied this representation in detail and found that it

1 Though hearing it in Sid’s Atlantic City accent helps.
was, to me, void of any physics. Therefore I put this DGS representation aside and I will not use it again, because with pure mathematical jiggling around you can’t solve physics. I can’t understand how to even approximately introduce physics into the DGS spectral function and so I give it up.

This practical, explicit, straightforward attitude toward theoretical physics permeated Sid’s teaching and was inherited by all the people who worked with him.

When Mike Peskin called up and asked me to talk about “Four Eras in Electron-Proton Scattering”, I was too ashamed to tell him I didn’t know what the four eras were. So, I decided I would have to make them up myself. I make no claims of historical accuracy, nor is this a review. Instead, borrowing the image from Feynman’s sum-over-histories, you should view this talk as one particular path in the path integral. I’m not sure my contribution to the sum-over-paths is very large. I know I will be idiosyncratic, I hope to be provocative. With this in mind, I have changed Mike’s suggested title slightly, to “One Theorist’s Perspective on Four Eras of Electron-Proton Scattering”.

Inspired by Mike’s suggested title and Sid’s first quote, I have chosen to organize my talk in a somewhat classical form.

In the beginning was an “Archaic Era” (1954–1966), when certain primitive cultures flourished, distinguished by an unusual rite known as “Electron Scattering”. One developed on the shores of San Francisco Bay. Remains of other contemporary and similar cultures can be found in Cambridge, Massachusetts and in Hamburg, Germany. During the late 1960s the primitive culture by the Bay experienced a “Classical Era” (1966–1972) when great cultural archetypes were crafted. There was a rich interaction between the “priestly” (read “theoretical”) and “artisan” (read “experimental”) classes. Like many great cultural awakenings, it was catalyzed by an invasion of barbarians, who challenged the established order and enriched the gene pool.

All of this climaxed in the emergence of QCD in 1970–1972. Then followed a “Hellenistic Era” (1972–1980) when electron scattering cultures spread throughout the world. Many migrated away from the center of high culture in California, and the practice of Deep Inelastic Scattering became ritualized. The local culture here at SLAC turned away from its classic roots toward different goals. One might call this a “Dark Age”, but given the accomplishments of the intervening years, I don’t think it’s appropriate. Finally, a Renaissance in deep inelastic physics arose in the 1980s and continues today. The classic rituals were rediscovered and reinterpreted as probes of the parts of QCD that we really don’t understand.

As a way of keeping time as I tell this story, I managed, with Harriet’s help, to find pictures of Sid from each of the four eras. So our clock begins with Figure I, a picture of Sid in Boston with his son, Daniel, before his migration to California.

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2I apologize to D (Deser), G (Gilbert), and S (Sudarshan).... It certainly wasn’t their fault, and they have made many great contributions to physics and biology.
The year was 1954, when the first elastic electron-proton scattering experiments were undertaken by Robert Hofstadter and collaborators at Stanford.

**The Archaic Era, 1954–1966**

Hofstadter’s experiments were incredibly important and influential. The form factors of the proton were measured for the first time, and showed the nucleon to be composite. Figure 2, from McAllister and Hofstadter’s first paper on the subject [4], shows that the experimental cross-section could not be fit if the proton was treated as a pointlike Dirac particle (the “Mott” cross section), nor with the addition of an anomalous magnetic moment alone. More structure was required.

Sid became very involved in this physics. Sid’s early papers, to which Mike has already referred, explored the physics behind the form factors of the proton, the neutron, and nuclei. In fact, in a very real sense, Sid wrote the book on the subject: Drell and Zachariasen’s *Electromagnetic Structure of Nucleons*, published in 1961, is the classic monograph on the subject [5].

Already at this time it was becoming clear that nucleons are very composite. Calculations in model field theories in which a pointlike nucleon interacts with pointlike pions could account for the nucleon’s magnetic moment and charge radius, which parameterize the form factor near zero momentum transfer, but they could not account for the observed rapid falloff of the form factor at momentum transfers of order 1
Figure 2: First data on the proton form factor from McAllister and Hofstadter, 1955.

GeV. The form factor fell like a dipole, $O(1/Q^4)$ rather than a monopole, as predicted by “vector dominance” [6]. Protons seemed to be very squishy indeed.

Sid pioneered the use of dispersion relations to characterize form factors. When dispersed in the current channel, the form factor is expressed in terms of mesons with $1^{--}$ quantum numbers, an approach that led to vector dominance. When dispersed in the nucleon leg, an approach pioneered by Sid and his wonderful student Heinz Pagels [7], the nucleon’s compositeness is related to the baryon resonances with the same quantum numbers as the nucleon. In either case, the compositeness of the nucleon was related to the proliferation of hadrons that was occurring in the late 1950s and early 1960s. Compositeness has thoroughly dominated the thinking about hadron structure ever since.

During the later Archaic period, physicists started to ask how to obtain more detailed information about the structure of nucleons. A few people started thinking about inelastic electron scattering at that time; Sid was one of the first. Mostly, however, the community focused on elastic scattering. In 1962 Carl Barber, then a collaborator of Hofstadter’s at Stanford, wrote a review on inelastic electron scattering from nuclei [8]. Apparently there was so little interest in the potential of inelastic scattering to elucidate the structure of the nucleon that Barber specifically excluded particle production, that is, pion production, from his 30-page review.

The proliferation of hadronic resonances and the apparent compositeness of the
Theorist’s Perspective on Four Eras of Electron-Proton Scattering

nucleon led to a picture of the strong interactions that is now very difficult to appreciate. The idea was called “nuclear democracy”, or the “bootstrap”. It was a deep and subtle picture, founded on dispersion theory, and like many stations along the way to QCD, it contains much that remains true. However, it gave little help to young students, like myself at the time, who sought to visualize the nucleon. In those days when one asked the question, “What is a proton?” one was told, “It’s a neutron plus a pion.” When one asked “What is a neutron?” one was told, “It’s a proton plus a pion.” And when one asked, “What’s a pion?” one was told, “It’s a proton plus an antineutron.” It is hard to appreciate, in retrospect, how seriously this was taken and how passionately it was studied. Inelastic electron scattering did not seem like a promising way to explore the bootstrap.

Of course, continuum inelastic electron scattering was first carried out in the famous MIT-SLAC program headed by Friedman, Kendall, and Taylor. Those early experiments are the topic of books, so I won’t really cover them in any detail [9]. Jerry Friedman tells me that the principal goals of the experiment were first elastic scattering, then electroproduction of resonances, and finally continuum electroproduction, for which they had few expectations. Sid, however, took inelastic electron scattering very seriously. In 1964 he and Dirk Walecka published an Annals of Physics article that, drawing upon earlier work by Bjorken, von Gehlen, Gourdin, and Hand [10], defined the inelastic kinematics and the famous structure function \( W_{\mu\nu} \) [11]. It became a bible for those of us who wanted to study the subject.

Now I have arrived at 1966, when things really began to change. An excellent perspective on the state of particle theory can be found by looking at the proceedings of the Berkeley Conference, which was the Rochester meeting of that year. In the introductory session, Murray Gell-Mann talked about quarks. His remarks give one a glimpse at the dominance of nuclear democracy and the dilemma facing anyone who thought seriously about quarks at that time. Quoting from Murray [12],

We consider three hypothetical and probably fictitious spin-1/2 quarks. . . .

Now what is going on? What are these quarks? It is possible that real quarks exist, but if so they have a high threshold for copious production, many \( |G|eV \); if this threshold comes from their rest mass, they must be very heavy and it is hard to see how deeply bound states of such heavy real quarks could look like \( q\bar{q} \), say, rather than a terrible mixture of \( qq\bar{q} \), \( q\bar{q}\bar{q} \), and so on. Even if there are light real quarks, and the threshold comes from a very high barrier, the idea that mesons and baryons are made primarily of quarks is difficult to believe, since we know that, in the sense of dispersion theory, they are mostly, if not entirely, made up out of one another [my italics].

So this is the bootstrap philosophy. Murray goes on,

The probability that a meson consists of a real quark pair rather than two mesons or a baryon and antibaryon must be quite small. Thus it seems to me that whether or not real quarks exist, the \( q \) and \( \bar{q} \) we have been talking about
are mathematical; in particular, I would guess that they are mathematical entities that arise when we construct representations of current algebra. . . . Their effective masses, to the extent that these have meaning, seem to be of the order of one-third the nucleon mass. One may think of mathematical quarks as the limit of real light quarks confined by a barrier, as the barrier goes to an infinitely high one. . . .

If the mesons and baryons are made of mathematical quarks, then the quark model may perfectly well be compatible with the bootstrap hypothesis, that hadrons are made up out of one another.

Here, clearly, is a great physicist struggling with the apparent contradiction that dispersion theory tells us that hadrons are composed on one another, and spectroscopy had begun to suggest that hadrons are made up of quarks. It was a time of great dissonance.

Later in the Berkeley Conference, in his very last remarks as rapporteur of the electromagnetic interactions session, Sid said something remarkable, which pointed the way to the future. After delivering his talk, and after answering all questions from the audience, Sid asked himself a question,

\[\text{What would I like to see measured? Let me just say briefly that I'd much like to see inelastic electron or muon cross sections measured; they provide the inelastic nucleon form factors that are of great interest in their own right. Moreover they are also the necessary input that goes into the neutron-proton mass-difference calculations, if their isovector structure can be measured, or into the hyperfine-structure calculation, if their spin structure can be measured.}\]

And then,

\[\text{Also there are some sum rules, asymptotic statements derived by Bjorken and others, as to how these inelastic cross sections should behave in energy [my italics].}\]

This, of course, was a prescient remark, made very early in the history of the subject. I want to spend a couple of transparencies discussing it.

This thread goes back to the work of Ken Johnson, who in 1961 wrote a little-recognized paper on the relation between the amplitudes that are measured in current-hadron scattering and the time-ordered products ($T$-products) of local operators. Johnson pointed out that the relationship is not as direct as is taught in field theory courses. Symmetries can require and infinities can generate extra terms that must be removed from the product of local operators before they can be related to physical amplitudes. He called his new objects $T^*$-products. They differ from $T$-products by local terms that generate polynomials functions of the momenta in scattering amplitudes. It doesn’t sound like much. In 1966, Johnson and Low and Bjorken independently explored how to extract $T$-products from scattering amplitudes. They

\[\text{Bj reminds me that the conflict between dispersion theory and the quark model still has not been laid to rest. For some thoughts on the subject, see Refs. 13, 14.}\]
introduced a limit (the Bjorken-Johnson-Low limit), where the energy transfer ($q^0$) is taken to imaginary infinity to isolate operator products, and then explored the implications. They wrote two very different and both very influential papers. Johnson and Low explored what happened in perturbation theory. They discovered anomalies that were precursors of anomalous dimensions that now figure so centrally in deep inelastic phenomenology. Bjorken, the optimist, examined the implications of the BJL limiting procedure under the hypothesis that products of operators have free field singularities at zero separation. Bjorken’s paper, with the formidable title “Applications of the Chiral $U(6) \times U(6)$ Algebra of Current Densities”, founded the field of deep inelastic physics. He took the $q^0 \rightarrow i\infty$ limit, which we now understand to be the deep inelastic limit, and when he encountered products of local operators, he replaced them by their values in free field theory,

$$\left[ J^a_\mu(x), J^b_\nu(y) \right] \rightarrow \cdots \delta^3(x-y)\Theta_{\mu\nu}^{ab}(x).$$ (1)

At the time Bjorken was cautious, but the implication is that there are pointlike constituents inside hadrons, whether you say it explicitly or not. Bjorken’s assumption of free-field behavior at short distances was quite radical, and Johnson and Low had already shown that it is violated in perturbation theory. Now we know that it’s almost correct: the corrections are only logarithmic.

To capture the spirit of the times, I have reproduced a small section of Bjorken’s paper. Eq. (6.18) is the famous Bjorken Sum Rule that has played an important role in my life. As you can see, Bjorken didn’t think much of his sum rule at the time. Most likely he doubted that it could be tested experimentally. So he changed the sign of these two negative terms and concluded that inelastic electron-nucleon scattering cross sections had to be greater than something that falls like a power of four-momentum transfer. Implicit in Eq. (6.19) were the ideas that led to scaling, to quarks, and to the future. So with this equation, I think it is appropriate to mark the end of the Archaic Era in electron scattering.

Returning to our unconventional time-keeping system, Figure 4 clocks in with Sid together with Pief Panofsky at the 1968 Vienna Conference, when the first results of the MIT-SLAC inelastic electron scattering experiments were announced in a parallel session by Jerry Friedman.

The Classic Era, 1966–1972

The 1968 Vienna Conference marked a turning point in this subject: the first public presentation of deep inelastic scattering data and the first direct evidence for pointlike constituents inside the nucleon. The MIT-SLAC group plotted their data in many ways. The now famous plot, first suggested by Bjorken, and shown by Friedman in his parallel session talk [19], is reproduced in Figure 5 and displays the approximate
Robert L. Jaffe

Figure 3: From Bjorken's paper "Applications of the Chiral $U(6) \times U(6)$ Algebra..."

"Bjorken" scaling, which has been so important over the years. At the time, Friedman found Figure 6 more compelling. It displays the data as a function of four momentum transfer ($Q^2$) in bins of fixed final-state mass, $W$. Kinematic effects are removed by dividing the inelastic cross section by the cross section for scattering from a point charge. For elastic scattering this isolates a combination of squares and products of the elastic form factors, which is shown for comparison. Although the elastic cross section falls like a bomb compared to scattering from a point charge, the inelastic form factors stay large, indicating that there are pointlike objects inside the nucleon.

Even in these earliest results it was possible to see three essential features of deep inelastic scattering. First was that the cross sections remained large at large momentum transfer, to which I have already alluded; second was that the structure functions scale; and third was that the structure functions are numerically small. The last suggested that the pointlike constituents of the nucleon have fractional rather
than integer charges. It also played a role in understanding the momentum content of the proton, as Chris Llewellyn Smith will relate in his talk [20].

You have all heard and read about this period at SLAC. Rather than attempt another general overview, I would like to describe what it looked like to me as a young theory graduate student newly arrived at SLAC in 1969. Historians of science have pointed to the emergence of QCD as an example of a Kuhnian revolution in science [21]. Although I am skeptical about much that is written about modern particle physics by historians of science [22], I believe they are correct in this interpretation of the developments that led to QCD. It is a fine example of a time when physicists were enthusiastically using ideas amalgamated from old and new models. They knew the ideas were contradictory, but they used them anyway and got the right answers.

To set the tone, consider Gell-Mann, who five years before had said that quarks were mathematical and hadrons were made up out of each other. At the 1971 Coral Gables Conference Gell-Mann said, “Nature reads books on free field theory” [23]. So what did he mean by that? At that time, he and Harald Fritzsch had invented an ingenious generalization of Bjorken’s free-field commutator algebra, where com-
mutators of operators at light-like separation were given by free field theory,

\[ [J^a_\mu(x), J^b_\nu(y)] \bigg|_{x^+ = y^+} \Rightarrow \text{Free field theory}. \]  \hspace{1cm} (2)

The Fritzsch–Gell-Mann algebra was accompanied by the ad hoc rule that the matrix elements of the operators that appear on the right hand side of these commutators should be taken parameters, and certainly not from free field theory.\footnote{Unless, of course, they are determined by symmetries of the theory.} This was a conservative approach, which could not be applied to processes other than deep inelastic scattering (and $e^+e^-$ annihilation).

An alternative picture was the parton model of Feynman, Bjorken, and Paschos\footnote{At least those with great physical intuition, like Bjorken and Feynman, got the right answer.} [24, 25]. Their’s was a promiscuous approach – in which one didn’t know when to stop. Basically, one used free field theory in momentum space to calculate cross sections, and used it wherever one wanted. The rules were quite vague; theorists found themselves doing calculations that they could hardly believe they should be doing: calculating strong interactions with free field theory – and getting the right answer.

Sid responded characteristically to this remarkable situation. Together with Tung Mow Yan and Don Levy, he undertook to construct a model field theory with a real Hamiltonian, that incorporated some minimal dynamical assumptions necessary to obtain Bjorken scaling\footnote{26}. They used infinite momentum frame methods, which soon...
afterward were canonized as light-cone field theory. They introduced a transverse momentum cutoff – the essential dynamical ingredient that guaranteed scaling. We now recognize this as a crude but effective implementation of asymptotic freedom. Armed with this model they could study other deep inelastic processes and learn where parton model results could be justified and where they could not. Some of the processes they studied had been studied before, but others, notably $pp \rightarrow \mu^+\mu^-X$ were new. Now famous, the “Drell-Yan” process is a mainstay of collider physics, and a subject of Tung Mow Yan’s talk later this morning [27].

There was another way of approaching deep inelastic physics which was influential in the early 1970s, but has faded from memory somewhat more than the parton model or light-cone algebra. It spread through the SLAC theory group after it arrived with the barbarian invasion I referred to earlier. The approach is called “canonical field theory” and the barbarians brought it from England [28]. Figure 7 shows you one of the English invaders. This particular barbarian, whom you may recognize, is John Ellis. David Broadhurst, Frank Close, John Ellis, Chris Llewellyn Smith, and Geoff West arrived at SLAC in 1969 through 1971; Tony Hey, who was at Caltech, was
a frequent visitor. Figuratively at least, they mixed their intellectual genes with the local population of theorists, who included Bj, Stan Brodsky, Sid of course, Fred Gilman, and Haim Harari, and the students, who included Mike Creutz, Inge Karliner, Joe Kiskis, John Kogut, Joel Primack, Dave Soper, and me.

Figure 7: John Ellis sometime after his arrival at SLAC.

Canonical field theory was quantum field theory with a real Hamiltonian but without the infinities associated with radiative corrections. We calculated deep inelastic processes either in coordinate or momentum space, whichever was more convenient; we checked that our results were preserved in models with nontrivial interactions; but we ignored the perturbative anomalies associated with renormalization (just as Bjorken had done in his 1966 paper). Those model field theories included quarks, color, vector gluons, soft chiral symmetry breaking – all ingredients that we now associate with QCD. Canonical field theory amounted to QCD with zero anomalous dimensions, and the results, many of which Chris Llewellyn Smith will mention, are standard results that we now attribute to QCD.

A few summary remarks about this very creative period: First, I’d like to emphasize that the Bjorken-Johnson-Low method, not the operator product expansion,
dominated thinking about short-distance physics at SLAC during this period. Wilson’s OPE did wonderful things for theoretical physics, but it was this other, equivalent stream of development that first led to scaling. Second, the paper that did bring Wilson’s methods (through the Callan Symanzik equations) to bear on deep inelastic scattering was a wonderful work by Norman Christ, Brosl Hasslacher, and Al Mueller that people were reading in 1971 [29].

Thus, the classical era in electron-proton scattering had drawn to a close. The major theoretical results now associated with QCD were in place in 1971 and 1972. In the Kuhnian tradition, this occurred before QCD had been properly formulated and well before the discovery that QCD is asymptotically free.

So, onward to the Helenistic Period, when deep inelastic scattering spread to the far corners of the earth. Returning to the Drell clock, there seem to be many pictures from this period, perhaps because photography had become more common. I chose one in Figure 8 because it is particularly meaningful to me – I took it. That’s Sid

![Figure 8: Sid at the grave of Anton Chekhov in 1974.](image)
on the right! And on the left, more or less the same size and shape, is the tomb of
Anton Pavlovich Chekhov. Taken in the Novodevichy Monastery, Moscow, in 1974,
it shows Sid in one of SLAC’s more distant colonies.

The Hellenistic Era, 1972–1980

After the formulation of QCD came a period of great activity and discovery. In the
early 1970s there was a great migration from SLAC to the provinces: Bjorn Wiik to
Germany, Chris Llewellyn-Smith and John Ellis to CERN, Frank Close, Roger Cash-
more, and others to England, many to Israel. I myself migrated to the intellectual
backwater of Cambridge, Massachusetts. Deep inelastic scattering experiments were
carried out with electron, muon and neutrino beams at DESY, CERN, and Fermilab,
as well as at SLAC.

So much happened in the 1970s as the pieces of the Standard Model were put in
place that it is impossible to do more than scratch the surface. First I will mention
a few of the developments that impressed me most, then point out some signs of
things to come. My first example is the decisive test of the Standard Model through
parity violation in deep inelastic scattering of polarized electrons from unpolarized
deuterium carried out at SLAC in the mid 1970s. This tour-de-force achieved by
Charlie Prescott and collaborators confirmed the predicted $Z^0/\gamma$ interference and
determined $\sin^2 \theta_W$ to be near $1/4$ \[30\]. A summary of their data is shown in Figure 9.

By this time the QCD-parton picture of deep inelastic scattering was so reliable that
it formed the foundation for experimental analysis.

Deep inelastic neutrino scattering experiments at CERN and Fermilab were rais-
ing puzzles and insights of their own. Production of opposite-sign dimuons ($\nu p \rightarrow
\mu^+ \mu^- X$), originally an anomaly, was understood as production and decay of charmed
quarks, and finally turned into a measure of the strange quark distribution in the
nucleon through the process shown in Figure \[10\]. So dimuon production went from
an anomaly to an experimentalist’s tool in short order.

I can’t survey this period without an homage to the logarithms.\[6\] Of course, the
great prediction of QCD was not scaling, but scaling violation. The theory predicted
weak – logarithmic – dependence of the deep inelastic structure functions on squared
four-momentum transfer. In those days, every deep inelastic scattering collaboration
was displaying its data versus $Q^2$ at fixed $x_{Bj}$. Data from that period from three of
the preeminent experiments (EMC, CDHS, and BCDMS) are shown in Figure \[11\].
The slopes of the fits to the data are predicted by QCD, and the agreement between
theory and experiment left little doubt that QCD is the correct theory of hadrons.
By 1980 QCD was established beyond a reasonable doubt; factorization theorems

\[6\] Not to be confused with the “Logarythms”, one of MIT’s better known choral groups.
had quantified the parton model and "canonical" field theory; and we had learned what the transverse momentum cutoff in Drell, Levy, Yan really meant.

Now I would like to shift the focus away from the dominant ideas of the Hellenistic Era and onto three developments that were perhaps not so important at the time, but which played an important role in the future.

The first was that scaling in electron scattering set in at very low \( Q^2 \). This was first pointed out and analyzed by Elliott Bloom and Fred Gilman [32]. In retrospect, it was quite remarkable that scaling could have been discovered in the original MIT-SLAC experiments which were dominated by \( Q^2 \approx 1 \text{ GeV}^2 \), when scaling is supposed to be an asymptotic phenomenon. A couple of simple examples illustrate the point: First consider the asymptotic equipartition between quark and gluon "momentum" –

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**Figure 9:** Early data confirming the standard model prediction for \( \bar{e}d \rightarrow e'X \).

**Figure 10:** Process by which opposite-sign dimuon production becomes a measure of the strange quark distribution in the nucleon.
Figure 11: Logarithmic scaling violation in deep inelastic scattering.
actually $P^+$ – in any hadron. A famous prediction of QCD is that momentum should be shared roughly equally between quarks and gluons at asymptotically large $\log Q^2$. This prediction presumes that terms suppressed only by logs of $Q^2$ are ignorable, so all moments (in $x_{\text{Bj}}$) of the structure functions except the lowest are negligible – the structure functions are $\delta$-functions at $x_{\text{Bj}} = 0$. One would therefore expect to see equipartition only at the highest $Q^2$ available, perhaps at HERA in the 1990s. Instead, it works pretty well at 1 GeV, excellently at 10 GeV. Why is asymptopia so close? A second impressive example comes from the Gross-Llewellyn Smith Sum Rule named in part after the next speaker [33]. Figure 12 is taken from the 1982 Paris Conference presentation, after an analysis by Bill Scott [34]. At large $Q^2$ the integral of the chiral-odd neutrino structure function, $F_3$ should asymptote to three. As the figure shows, it equals to three at $Q^2$’s well below 1 GeV, where much of the contribution comes from the elastic peak. The corrections to this sum rule can be calculated up to an overall constant that measures a correlation between quarks and gluons in the proton. Apparently that constant is negligibly small.

These examples give us a modern restatement of the old puzzle that first surfaced with the measurement of the elastic form factor: Why is the proton so soft? The proton is very composite even though it is made of pointlike objects. The distributions of the nucleon’s constituents are only very weakly correlated. Once confinement is taken care of (by constructing color singlets) we see little evidence for non-perturbative interactions between quarks. The proton looks very much like a bag of confined but

\footnote{The prediction is exact, $3n_f/16$, where $n_f$ is the number of “active” quark flavors – four in the early days, now five.}
otherwise free quarks, an idea that Sid and I pursued intensely on during this period [35, 36].

Another hint at the future came with the first precision measurements of deep inelastic scattering off nuclei [37]. The European Muon Collaboration at CERN scattered muons from iron and deuterium and compared the structure functions. They didn’t expect to see much – principally the effects of Fermi motion. In Figure 13 Fermi motion is the dashed curve. Instead, they found a strong $x_{Bj}$ dependence

Figure 13: First results on the ratio of nuclear to nucleon structure functions.

with the opposite slope from Fermi motion. The data were first shown at the 1982 Paris Conference, where they attracted very little attention. The elaboration of this subject properly belongs in the next section of my talk, where I will return to it.

Also at this time, SLAC ran a small program in deep inelastic scattering of polarized electrons from polarized protons. Polarized beam was required for the parity violation program, while the polarized target technology was new. The object was to measure the helicity distribution of quarks in the polarized target. Vernon Hughes and his group from Yale joined Dave Coward and others at SLAC in this enterprise. Data from their two experiments, E80 and E130, are shown in Fig. 14 [38]. This data attracted rather little interest, although its importance as a probe of the spin content of the proton was “well known among a small group of people”.

After E130, the SLAC program turned in other directions. Polarized scattering at SLAC was focused on parity violation; future development focused on $e^+e^-$ collider physics. Deep inelastic scattering was no longer SLAC’s highest priority. Hughes’s request for extensions of the polarized target program were turned down by the SLAC PAC. So, perhaps this is a good point to mark the end of the robust expansion of
deep inelastic scattering physics – the end of the Hellenistic Era. QCD was supreme; deep inelastic scattering was routine; and Vernon Hughes was a prophet cast out in the desert.

Well, next is the Renaissance. It seems appropriate to clock in with Sid enjoying a conversation with his granddaughter Cornelia. The date is approximately 1988, just when new energy was appearing in the electron-nucleon scattering community.

**Renaissance, 1983–Today**

Interest in deep inelastic scattering and related phenomena (including even form factors) has been reborn in recent years because they provide a unique probe of the mysteries of confinement.

People no longer seriously doubt the basic validity of QCD, although a few rogues remain unconvinced. Considerable interest in QCD remains because we must understand the quark and gluon content of beams, targets and final state fragments in order to identify novel phenomena beyond the Standard Model. This effort is very important in the search for physics beyond the Standard Model, but as a sort of “QCD engineering”, it is perhaps not as intellectually exciting as the early days of deep inelastic scattering.

Although QCD is very familiar, and models abound, confinement is still not understood. QCD is very well understood at short distances, where perturbative methods apply. However, at long distances, where hadrons form, our theoretical tools are few.
This seems like an inadequate state of affairs for such a beautiful problem. After all, the Lagrangian of QCD is extraordinarily simple,

$$\mathcal{L} = -\frac{i}{4} \text{Tr} F^2 + \bar{q}(i \not{D} - m)q .$$

I have suppressed a few sums over degrees of freedom, but this Lagrangian can be written on a postage stamp, not to mention a t-shirt.

We need theoretical tools to understand confinement. To develop them we need probes of hadrons that give precise, interpretable information. In the old days Sid used to say (perhaps this is even a direct quote), “Leptons make great probes of hadrons because leptons are pointlike.” The Renaissance that deep inelastic physics has enjoyed over the past 15 years came from the realization that we now have a new version of that old slogan: “Quarks and gluons make great probes of hadrons in hard processes, because at large momentum transfer, quarks and gluons are pointlike.”

We can use perturbative QCD, which we understand at short distances, to probe the aspects of confinement at long distances that so far defy understanding. This helps organize the most interesting developments in electron scattering and related subjects in this fourth period. Let me close my talk by giving some examples.

The first concerns how quarks are distributed in nuclei. Someone once said, anonymously, that “Looking for quarks in the nucleus is like looking for the Mafia in Sicily. Everyone knows they’re there, but it’s hard to find the evidence.” The first
sign that the quark distribution in a nucleus differs significantly from the distribution in isolated nucleons came from the European Muon Collaboration data shown in Figure 13.

As you know from freshman world history, during the Renaissance, people became interested in archaeology. They went out and dug around to see what happened before them. In a marvelous example of particle physics archaeology, Ari Bodek of the University of Rochester and his collaborators did exactly this in order to check the EMC results. He exhumed the old SLAC deep inelastic scattering data. The targets were hydrogen and deuterium, which, of necessity had been contained in metallic vessels. In order to subtract the background scattering on the vessels, “target empty” data had been taken off the empty vessels which were aluminum or iron. So, without applying to SLAC’s program advisory committee, Bodek was able to get SLAC data on deep inelastic electron scattering off nuclei. The data proved so interesting that he and his collaborators were then able to launch a new program at SLAC to measure electron scattering from a variety of nuclei. The data obtained this way are shown in Figure 16. It may be difficult to extract the trends from these separate curves, so instead consider Figure 17, the global fit to nuclear data recently done by Gueorgui Smirnov, which shows clearly the power of deep inelastic scattering. The ratio of structure functions of nuclei to deuterium is shown as a function of $x_{Bj}$ and $A$, atomic mass, from $A = 20$ to $A = 200$. There are four

![Figure 16: Electron scattering from a variety of nuclei: SLAC E139.](image)
distinct domains. At very low $x_{\text{Bj}}$ there is shadowing — quarks do not see the whole nucleus. Beyond the range of $x_{\text{Bj}}$ shown, the cross sections shoot up, because of

Fermi motion: the nuclear structure functions don’t have to vanish at $x_{\text{Bj}} = 1$ but the proton and neutron structure functions do, so the ratio must go to infinity. The intermediate region $0.3 < x_{\text{Bj}} < 0.7$ held the greatest surprise. In this region there is a systematic suppression of the nuclear structure function relative to the free nucleon. The final domain, where the ratio exceeds one at moderately low $x_{\text{Bj}}$, is required by sum rules to compensate the depletion at larger $x_{\text{Bj}}$.

The depletion at large $x_{\text{Bj}}$ and the corresponding enhancement below show that quarks are systematically shifted from large $x_{\text{Bj}}$ to small $x_{\text{Bj}}$ by the presence of the nuclear medium. As first pointed out by Boris Ioffe in the late 1960s [42], $x_{\text{Bj}}$ is conjugate, in the uncertainty principle sense, to a correlation length along the light-cone. Thus a shift to lower $x_{\text{Bj}}$ means that the quark “mean free path” along the

Figure 17: A fit to the world data on the ratio of nuclear to nucleon structure functions.
light-cone has increased in nuclei. They propagate further along the light-cone between the absorption and re-emission of the deeply virtual photon exchanged in deep inelastic scattering. One can say, quite independent of the specific dynamical model, that quarks are less severely confined in nuclei. Nearly as many mechanisms have been proposed as papers written on this subject. Perhaps the microscopic origin lies in an effective medium dependent change in $\Lambda_{QCD}$, or perhaps in quark exchange between nucleons. Other papers study meson exchange. All these are only models, but the basic fact remains: quarks are shifted to lower $x_{\text{Bj}}$ because they are partially deconfined in the nuclear medium.

Another beautiful and exciting example of the use of deep inelastic phenomena to explore confinement concerns the distribution of angular momentum within the nucleon. First studied by Vernon Hughes and collaborators during the Hellenistic Era, the Renaissance began with measurements of deep inelastic scattering of polarized muons from polarized nucleons by the European Muon Collaboration at CERN \[43\]. Not surprisingly, Hughes played a major role, first as collaborator and then as co-spokesman for experiments that mapped out the quark spin distributions in the proton and neutron over the period 1986–1996. The EMC discovered, and later experiments at CERN, SLAC, and HERA confirmed, that only about 30% of the spin of the proton is carried by the spin of the quarks. This was quite a surprise, since expectations based principally on the hypothesis that polarized $s$-quarks are not important, suggested something closer to 60–65%. This puzzle, known affectionately as the violation Ellis-***** Sum Rule, will be explored in some depth by Chris Llewellyn Smith in the next talk \[44\]. Just to give you an idea of how far this field has progressed, Figure 18 shows the spin asymmetry measured at HERA (by the Hermes Collaboration) together with data from SMC and SLAC \[45\].

The study of spin-dependent deep inelastic phenomena has moved beyond the initial excitement engendered by the quark spin measurement. It has blossomed in the past decade and is one of the richest areas in QCD research at this time \[46\]. Experiments underway at CERN and at HERA hope to determine the gluon spin distribution in the nucleon. There is a new program at the Brookhaven Relativistic Heavy Ion Collider (RHIC) aimed at unravelling the quark and gluon spin structure of the nucleon \[17\]. One of the prettiest ideas is to use $\vec{p}p \rightarrow W^\pm X$, known as polarized-Drell Yan, to measure the spin distribution of $u$ and $d$ quarks in the nucleon separately \[48\]. The simple dynamics of the Drell Yan process and the parity violation in $W^\pm$ production make this possible. Finally, a whole new understanding of the deep inelastic physics of transverse spin has emerged from intense study of polarization phenomena. We now understand, for example, that a complete understanding of the quark spin in the nucleon in the deep inelastic limit requires measurement of a third quark polarization distribution, known as the “transversity” distribution \[49\]. It describes the distribution of transversely polarized quarks in a nucleon polarized transverse to its direction of motion. Plans are underfoot to measure transversity at...
HERMES, at HERA, and at RHIC, where one possibility is $\vec{p}_\perp \vec{p}_\perp \rightarrow \mu \bar{\mu} X$, known appropriately as transverse-Drell, transverse-Yan.

My final example harkens back to the earliest days of this culture, back to the 1950s. It turns out that there are still very interesting things to be learned from elastic electron-nucleon scattering. The electromagnetic current, which has been so well studied in elastic scattering, is a pure SU(3)$_f$ octet. By exploiting flavor symmetries one can separate two flavor structures: the isovector, $u - d$, and the hypercharge, $u + d - 2s$. Because it has no singlet component, no information on $u + d + s$ can be obtained, so there is no way to separate the $u$, $d$, and $s$ contributions to any matrix element. The $Z^0$ offers a new flavor coupling to the nucleon proportional to weak isospin, which samples $u - d - s$ in the light quark sector \[50\]. The $c$, $b$, and even $t$ quarks also contribute, but their effects can be handled in the heavy quark approximation by the methods of perturbative QCD. Since the matrix elements of $u - d$ are already known, the $Z^0$ effectively probes strange quark matrix elements in the nucleon. The hitch is that $Z^0$-nucleon couplings can only be measured in elastic neutrino scattering or in parity violating elastic electron nucleon scattering – both extremely difficult experiments. For example, the parity violating asymme-

![Figure 18](image_url)  

Figure 18: The spin asymmetry measured at HERA (by the Hermes Collaboration) together with data from SMC and SLAC.
tries in elastic $eN$ scattering are of order $10^{-7}$. The rewards are great, however, since one can measure the strangeness analogues of the axial charge ($\bar{s}\gamma_5 s$), magnetic moment ($\frac{1}{2}s\dagger r \times \bar{a}s$), and “charge” radius ($s\dagger r^2 s$). The first would provide an independent confirmation of the quark spin fraction measurements without the difficulties of extrapolation to $x_{Bj} = 0$ and use of exact $SU(3)_f$ symmetry. The latter two are otherwise unknown measures of strangeness in the nucleon. Major experiments, which are the direct descendents of the program begun nearly half a century ago at Stanford by Robert Hofstadter, are underway to measure these quantities at low-energy electron machines. The first accurate measurements of the strangeness magnetic moment have been announced by the SAMPLE collaboration working at Bates near Boston [51]. Further experiments are being mounted at Jefferson Lab in Virginia.

I have reached the end of my survey. In retrospect, I think Sid is a very lucky man: to have watched not only the acceptance but also the apotheosis of his ideas. Electron scattering has gone from a relative backwater to the principal paradigm for understanding in particle physics. All those battles for funding that Sid helped to wage now appear vindicated. As for pedagogy, Mike Peskin has already spoken of Sid’s wonderful book with Bj. I would only add that when a new book on quantum field theory appears – by Weinberg, or Brown, or Peskin and Schroeder – the question everyone asks is “Will it be the next Bjorken and Drell?”

Personally, I would like to add that I can hardly imagine physics at SLAC without the memory of Sid’s gruff voice echoing down the corridors of the third floor to herd students toward the Green Room for the Friday lunchtime student seminar. Sid, as many of you know, was the only person with a PhD allowed in the room, because he promised to ask all the stupid questions that students were too shy to ask. And he did. Thank you, Sid for all that, and thank you all.

Acknowledgments

I would like to thank Jim Bjorken, Jerry Friedman, Henry Kendall, and Chris Llewellyn Smith for help with the background of this talk. Thanks also to Mike Peskin and the conference organizers for the invitation to speak and for their support, and especially to Harriet Drell for providing wonderful pictures of Sid.
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