FEYNMAN AND PARTONS

'I am more sure of the conclusions [of the parton model] than of any single argument which suggested them to me for they have an internal consistency which surprises me and exceeds the consistency of my deductive arguments which hinted at their existence.'

James D. Bjorken

For me, as for so many others, Richard Feynman is a special hero. He became so while I was learning quantum electrodynamics in graduate school at Stanford. The course happened to be organized historically, and for several months it was taught in the 1930s style out of Heitler's classic text, using old-fashioned perturbation theory and Dirac matrices α and β (but not γ). After this trial by fire came a seemingly endless, gloomy, turgid mass of field-quantization formalism. When Feynman diagrams arrived, it was the sun breaking through the clouds, complete with rainbow and pot of gold. Brilliant! Physical and profound! It was instant conversion to discipleship.

For many years thereafter my discipleship developed like most everyone else's, through the strong influence of his writings and the occasional rare treat of hearing him perform live. But it became my privilege that for a few years my research path ran in parallel with his. This convergence came about because of the remarkable and historic series of inelastic electron scattering experiments at the Stanford Linear Accelerator Center by a SLAC–MIT collaboration. These experiments played a crucial role in revealing the existence of point-like, quark constituents of the proton, while Feynman's insights and intuition provided much of the theoretical motive power for the interpretation of the experimental developments.

In the late 1960s, when the SLAC program was initiated, Feynman was working on descriptions of high-energy hadron–hadron collisions. He pictured the typical reaction as occurring by the exchange of constituents—Feynman called them partons—between the rapidly moving projectiles. The primary basis for his parton picture was empirical; significant evidence was the apparently exponentially bounded transverse-momentum distribution of produced or scattered secondary particles. This indicated a predominantly "soft" interaction; that is, the important dynamics occurred at an intrinsic distance scale on the order of the proton size. Exchange of constituents satisfied this "softness" criterion very well. Indeed there was no explicit interaction introduced at all, only the implicit one constraining the constituents to be within the proton.

Inclusive processes

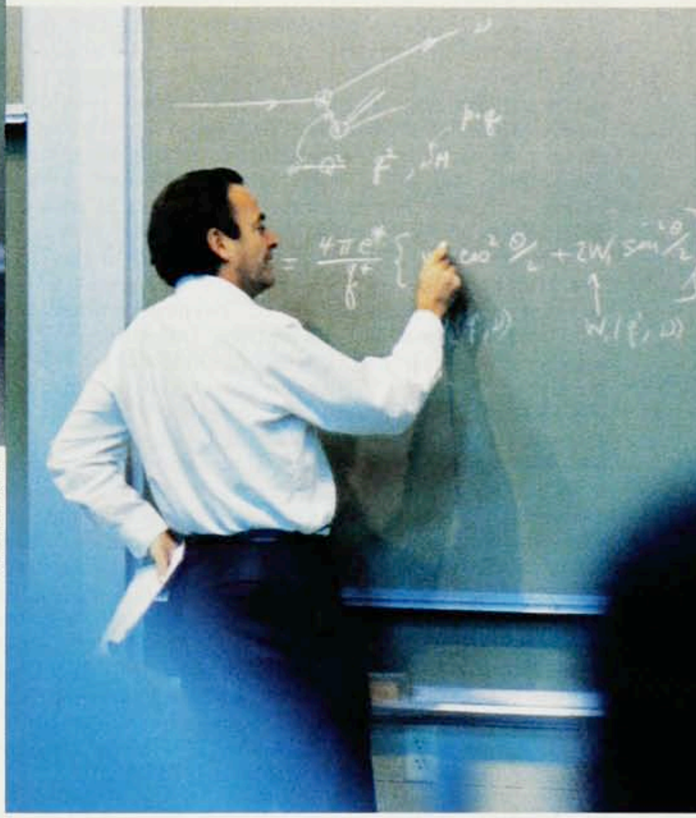
In those days, using local field theory to describe the strong force was no more fashionable than using it nowadays to describe quantum gravity. Rather, those were the glory days for Regge-pole theorists. It was believed that the processes important for detailed study were ones with no more than two particles in the final state. The high-energy limit of the cross section for such collisions is the natural domain of applicability of the Regge-pole theory, which need not be elaborated here in detail. Feynman's partons provided a novel way to interpret the Regge-pole picture. But more important was Feynman's introduction of a new language for describing inelastic collisions involving the production of many, not just two, particles.

Multiparticle collisions were in those days largely shunned by theorists, who preferred to study only processes in which all the final particles are observed and all the momenta determined. Feynman called such processes "exclusive," and he emphasized, by contrast, "inclusive" processes, in which one (or a few) particles in the final state are identified and their momenta specified, but all other possibilities are summed over. Such pro-

James D. Bjorken is a theoretical physicist at Fermilab and a professor of physics at the University of Chicago.

HENRY KENDALL

Richard Feynman and the author (left) before Feynman's first seminar on partons at SLAC in October 1968 (below).



HENRY KENDALL

cesses were largely unknown to theorists, although the experimental community knew measurements of inclusive distributions as "beam surveys": chores required when commissioning a new accelerator to ensure proper design and implementation of secondary beam lines and radiation shielding. Feynman suggested that the inclusive distributions were themselves worthy of theoretical attention and suggested a scaling behavior in the variable x_F, the ratio of longitudinal momentum of a secondary particle to the maximum value allowed by energy–momentum conservation. He also emphasized rapidity (essentially the logarithm of the particle momentum) as an especially useful variable and argued that the distribution of particles produced in high-energy collisions was essentially uniform in that variable.

But this initial motivation for Feynman's partons was soon replaced by a stronger one. It happened almost by chance. Feynman was visiting his sister in the San Francisco Bay area and happened to stop by SLAC for a short visit. He was shown the latest electron–proton scattering data, along with fits to a scaling law I had suggested to the experimentalists. I was out of town, and a puzzled Feynman did not get a clear picture from the experimentalists of where the scaling law originated: "something about current algebra, sum-rules, Regge-theory. . . ."

It took Feynman only an evening of calculation with his partons to interpret what was going on. He viewed the process in a reference frame in which the motion of the target proton was extremely relativistic. In that frame the proton was replaced, as in his previous calculations, by a "beam" of its constituents, or partons. He assumed the electron scattered elastically and incoherently from these partons, which he regarded as point-like quanta with no interactions among them. Feynman viewed the scaling function I had introduced as giving the probability of finding a parton of a given momentum in the *incident* proton beam, weighted by the square of the parton electric charge.

As I recall, I returned to SLAC just before Feynman was to leave and found much excitement within—and beyond—the theory group there. Feynman sought me out and bombarded me with queries. "*Of course* you must know this. . . . *Of course* you must know that. . . ," he kept saying. I knew about some of the things Feynman mentioned; others I didn't know. And there were things that I knew at the time but he did not. What I vividly remember was the language he used: It was not unfamiliar, but it was distinctly *different*. It was an easy, seductive language that everyone could understand. It took no time at all for the parton model bandwagon to get rolling.

Feynman's calculation of the electron–proton scattering cross section invited generalization to many electromagnetic and weak processes. Feynman continued to develop the ideas at Caltech; I worked with Emmanuel Paschos and others at SLAC; and a horde of others joined in. Ideas and methods were developed for determining the parton spin, charge and weak-interaction properties, and with time the natural identification of (charged) partons with quarks became established. Central to this course of events were the experiments, especially the elegant, sophisticated series of electron–proton scattering measurements by the SLAC–MIT collaboration,[1] as well as the data from neutrino experiments at CERN and Fermilab.

As the quark–parton model took hold, an immediate problem arose: Why (or, at the very least, *how*) no fractional charge was seen in the collision debris. These questions became a major topic for Feynman and for me. And it was in this "second generation" evolution of the parton model that my scientific life ran most in parallel with Feynman's. It became an ongoing challenge for me to figure out how he was thinking about a given problem or how he would think about that problem if he got around to it. On occasion this attitude helped in finding the solution. Only rarely did we directly communicate and compare notes—although I did sometimes get indirect information from others who had made the pilgrimage to Caltech.

In one instance I had a tangible measure of success in my attempt to follow Feynman's ways. In a review talk on partons and related issues, I cited "Feynman's notebooks" at a rate of about one citation per transparency, for I suspected that he had worked out all kinds of things but not published his results. The audience loved it. But not only did I not really know what Feynman knew and when he knew it, I did not even realize that he *kept* notebooks. (I am told that there exist very careful and complete logbooks, cross-referenced, of his day-by-day work). Some time later I had the opportunity to reminisce with him about that talk, and he confirmed that with one exception (I forget what it was), it was all there. That unpublished work included light-cone quantization (with some sophisticated applications to QED), independent work on operator-product expansions, and his "fluid analogy," which compared the properties of parton, as well as produced-hadron, distributions in relativistic phase space with those of ordinary fluids (having short-range correlations only) in configuration space. (The fluid-analogy ideas were revealed to the outside world by Kenneth Wilson).

Deductive vs inductive thinking

During this parallel interaction with Feynman, there occured a strong influence on my style of thinking in physics. The problems that the parton model raised were not to be solved using the methods one learns in Physics 101. Characteristically, Feynman addressed the fundamental issues raised by the parton model very directly right from the start. For example, he wrote in his first paper on partons[2]:

> These suggestions arose in theoretical studies from several directions and do not represent the result of consideration of any one model. They are an extraction of those features which relativity and quantum mechanics and some empirical facts imply almost independently of a model. I have difficulty in writing this note because it is not in the nature of a deductive paper, but is the result of an induction. I am more sure of the conclusions than of any single argument which suggested them to me for they have an internal consistency which surprises me and exceeds the consistency of my deductive arguments which hinted at their existence.

The power of the parton model came not from a linear, deductive logical line such as one finds in an ordinary computer, but rather from a multidimensional logical network more typical of the human brain. And this situation applied not only to the creative process, where it is not uncommon, but also to the end product. It was the inner consistency of a broad variety of lines of attack that was impressive. One may legitimately question this house-of-cards approach to science: One good argument is better, after all, than 52 mutually supporting inferior ones.

I came to realize, in fact, that in my work leading to the ideas of scaling in inelastic electron scattering this inductive approach had also predominated. But I was a young postdoc at that time, and I had little confidence in it: to me a claimed result required a clean line of logic (even were it to be constructed *ex post facto*) in order to meet the standards of the trade. And such logical lines were hard to find.

The situation was clearly present for Feynman as well. His original journal article[2] on parton-model ideology nowhere mentions the word parton or proton constituent. The parton was introduced only in a less formal conference talk given at about the same time.[3] And even years later, in his book[4] *Photon–Hadron Interactions*, the ambivalence still appears. The concluding pages of that book contain the following phrases:

> We have built a very tall house of cards making so many weakly-based conjectures one upon the other and a great deal may be wrong. . . .
>
> Finally it should be noted that even if our house of cards survives and proves to be right we have not thereby proved the existence of partons. . . .
>
> From this point of view the partons would appear as an unnecessary scaffolding that was used in building our house of cards.
>
> On the other hand, the partons would have been a useful psychological guide as to what relations to expect—and if they continued to serve this way to produce other valid expectations they would of course begin to become "real," possibly as real as any other theoretical structure invented to describe nature.

Of these phrases, the last one has turned out to be the most prophetic.

It is hard to document here the reasons for trusting the parton ideology. Many of the results seemed to be based on broad principles of a mostly kinematical nature. For example, a main feature of the parton picture is the remarkably nonrelativistic character of the extreme relativistic limit. Not only do the internal motions of constituents of a high-momentum hadron slow down because of relativistic time dilation, but the transverse dynamics really does look nonrelativistic. This can already be glimpsed from the energy–momentum relation for a free particle moving rapidly in the z direction,

$$E^2 = c^2(p_z^2 + p_x^2 + p_y^2) + m^2c^4$$

rewritten as the Hamiltonian for the transverse dynamics

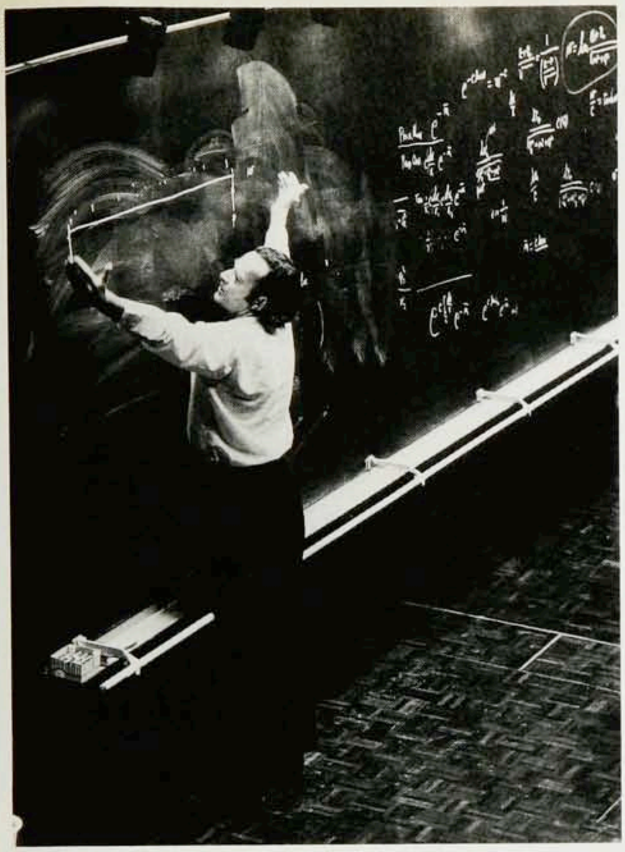
$$\mathsf{H} = E - p_z c = \frac{p_x^2 + p_y^2}{2\eta} + \frac{m^2c^4}{2\eta}$$

where

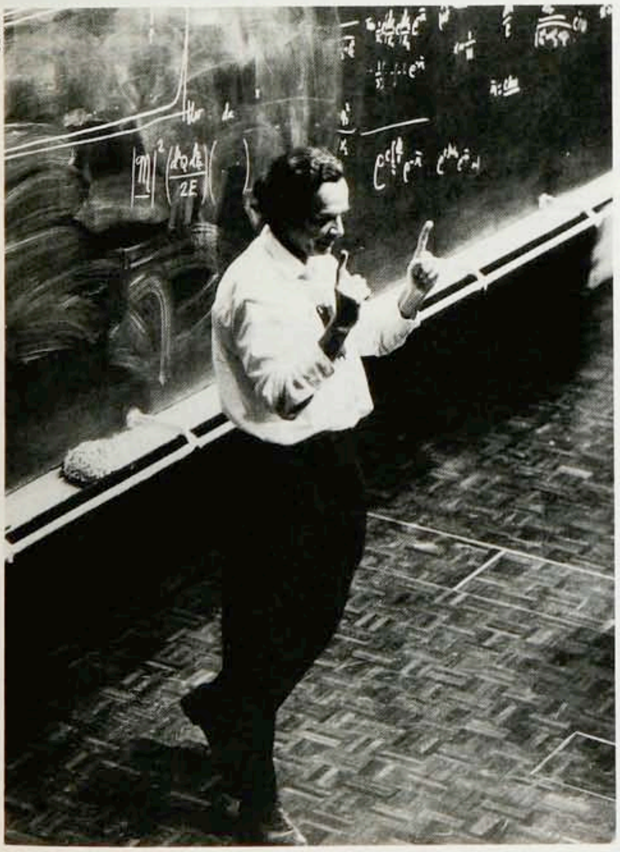
$$\eta = \frac{E + p_z c}{2c^2}$$

represents *inertia*, in proportion (as $E, p_z \to \infty$) to the total momentum of the particle. This analogy invited a

CERN



CERN



Richard Feynman discussing the parton model at CERN, January 1970. (Photographs courtesy Michael Riordan.)

qualitative, intuitive view of the problem, abstracted from nonrelativistic quantum theory.

But this nonrelativistic intuition about dynamics in the extreme-relativistic limit was only one line of attack. Another was the consistency, sometimes hard won, of the proposed answers when the dynamics of the processes were studied in a variety of reference frames. Yet another was the smooth matching of the predictions for, say, a given inclusive process with the expectations for the set of exclusive processes comprising that inclusive process. The buzzword for this criterion is duality.

The net result of such second-generation attempts to understand the final states in these hard-collision parton processes turned out to be remarkably unremarkable: These processes should look essentially the *same* as ordinary collisions at the same available center-of-mass energy. This was *ab initio* not obvious to the theoretical community. Because of the unspectacular nature of this result, the data supporting it created little stir in the experimental community. But for me, and I suspect for Feynman also, the experimental results were deeply satisfying.

It is worth emphasizing again that throughout this development of the parton model the essential input assumption about the dynamics was that strong interactions were "soft," that is, characterized by a force whose range was about the same as the proton size. As it turns out, this assumption is not quite right. The currently accepted theory of the strong force, quantum chromodynamics, contains, in addition to the strong, soft interaction, a not-so-strong hard interaction that becomes significant at much shorter distance scales. The latter is analogous to the inverse-square electromagnetic force but with a fine-structure constant of about $^1/_7$. Long before QCD emerged on the scene, the possibility of such a hard strong interaction was entertained. Feynman was always careful to set this hypothesis separate from those of the basic parton model. As the evidence for QCD grew, Feynman (with Richard Field) worked out the modifications to the "naive" parton model phenomenology implied by QCD, and grappled with the fundamental properties of QCD that might explain confinement. By now the basic parton model concepts have been deeply integrated into the formalism of QCD, to the extent that most theorists take the parton picture to be a self-evident consequence of QCD. I suspect there is more to the story than that, yet to be uncovered. But it consists of questions of rigor and of detail; the parton approach will not become obsolete.

With the emergence of QCD, my interests drifted apart from Feynman's. Even during the period when we had common interests, I had relatively little personal contact with Feynman. Our relationship was warm, but it was not closely personal. It is not that I don't feel close to Feynman. Something of him is very much in me and always will be. And I will always treasure that.

References

1. For a general review, see J. I. Friedman, H. W. Kendall, Ann. Rev. Nucl. Sci. **22**, 203 (1972).
2. R. P. Feynman, Phys. Rev. Lett. **23**, 1415 (1969).
3. R. P. Feynman, in *Proc. III Int. Conf. on High-Energy Collisions*, organized by C. N. Yang *et al.*, Gordon and Breach, New York (1969).
4. R. P. Feynman, *Photon–Hadron Interactions*, Benjamin, Reading, Mass. (1972). ■