
GUEST EDITORIAL

James Bjorken (Bj), who played a central role in the development of the quark-parton model at SLAC from 1963–1979, spent ten years at Fermilab in 1979–1989, including a period as Associate Director of Physics. He is presently co-spokesperson of the Fermilab test/experiment T864 in the Tevatron collider which he describes as being “closer to Roentgen in style than to CDF or to DØ.” Since returning to SLAC in 1989, he has been active in theoretical and experimental activities.

THE FEATURE ARTICLE in the summer issue of the *Beam Line* was the serendipitous discovery of X rays 100 years ago by Wilhelm Roentgen. Upon observing fluorescence occurring some distance away from a Crookes tube he was operating, he set aside his duties as university rector and teacher and retreated into his laboratory to work alone, sharing nothing with colleagues. He reemerged six weeks later with his discoveries ready for publication and for the subsequent press conferences.

In this issue is featured the particle-physics discovery of the year, that of the top quark. It is at an opposite extreme, a programmed discovery under way for most of a decade at Fermilab, featuring two competing experimental groups, the CDF and DØ collaborations, each consisting of 400 to 500 physicists, plus countless engineers, technicians, and support personnel, especially the many members of the Fermilab Accelerator Division who provide the essential ingredient of the colliding beams. This article is written by the spokespersons of the two competing collaborations and tells much of how scientific discovery is done in such an extreme social environment. Each collaboration is a community in itself, with three branches of governance, robust bureaucracy and organization charts, and a considerable level of specialization.

It happens that over the last year or two I have spent a lot of time at Fermilab, much of it precisely halfway between these behemoth detectors. So I watched with interest and bemusement the excitement of the chase, along with the frustrations, the impatience, the fatigue, the

anxieties, and some of the inevitable politics. I witnessed the awkward first stage of CDF evidence, when four short draft papers were by compromise merged into a very long paper. I even promised CDF collaborators to read the thing and did, carefully, beginning to end, and found it a classic—a masterful exhibit of how science results should (but seldom do) get reported. Throughout I maintained a somewhat dour posture toward those CDF/DØ crowds, baiting them regarding their obsessive pursuit of top to the apparent neglect of all else (not quite true of course), including that hidden piece of New Physics Beyond the Standard Model lurking undetected in their data. But when the top quark results eventually bubbled up to the surface, I could not do anything but be bouyed by them as well, joyful in the occasion and deeply respectful of the magnitude of the accomplishment.

The history of physics is full of near-simultaneous discoveries by separate individuals or groups, and with that often has come acrimony and controversy, from Newton and Leibnitz to Richter and Ting, and down to the present time. There has been competition between CDF and DØ as well. In fact, it was built in from the beginning by then-director Leon Lederman, who visited CERN's big collaborations, UA1 and UA2, while they were discovering intermediate bosons W and Z and searching for the top quark. At CERN, it was vital to have two collaborations as checks and balances, and Lederman upon his return strongly encouraged the creation of the present DØ collaboration, something which was not in the works prior to that. And the ensuing CDF/DØ competition has served for constructive

purposes; I have never seen this competitiveness to be corrosive. The evidence is in these pages for the reader to see, in the very fact of co-authorship and in the nature of the interactions between the collaborations as described in the article. This piece of competition has been a class act.

Not only has this been true between the collaborations, but it seems also to have been the case within them. This is no mean feat, since harmony within a big group of strong individualistic physicists of great talent and often even greater ego is not easy to maintain. I can do no better than quote here what is found near the end of the article, and I do this without regrets for creating some redundancy:

In the end, the chief necessity for the convergence on the top discovery was the willingness of a collaboration to abide by a majority view. Securing this willingness requires extensive attention to the process—of being sure that all shades of opinion, reservations, and alternate viewpoints are fully heard and understood. It is more important perhaps that each point of view is carefully listened to than that it be heeded. A fine line in resolving these viewpoints must be drawn between autocracy and grass-roots democracy. The process must have the confidence of the collaboration, or its general effectiveness can diminish rapidly.

These are not mere words, but an account of successful actions. In this increasingly fractious world of ours, it should be read and taken to heart by all those who despair of progress being made through reasoning and consensus.

James Bjorken