

Some Sociological Consequences of High-Energy Physicists' Development of The Standard Model

By

Mark Bodnarczuk¹

The Breckenridge Institute®
PO Box 7950
Boulder, Colorado 80306-7950
1-800-303-2554
www.breckenridgeinstitute.com

¹ Mark Bodnarczuk, "Some Sociological Consequences of High-Energy Physicists' Development of the Standard Model," in Lillian Hoddeson et. al. eds, *The Rise of the Standard Model: Particle Physics in the 1960's and 1970's*, (New York: Cambridge University Press, 1997).

Some Sociological Consequences of High-Energy Physicists' Development of The Standard Model

By
Mark Bodnarczuk

In a scientific discipline that went from experiments with less electronics than a videocassette recorder to 10^5 channels, and from collaborations with 5-10 members to 300 during the years 1964-1979, the notion of what high-energy physics is, or what constitutes *being* a high-energy physicist, cannot be viewed simply as an immutable category that is "out there" - that remains fixed despite these and other developments. What high energy physics is as a discipline and what it means to be a high-energy physicist are renegotiated by participants relative to the experimental and theoretical practices of the field at any given time. In this article I will explore some of the sociological consequences of the decisions made by high-energy physicists as they constructed the edifice that has come to be known as the Standard Model.²

Many of these physicists' decisions about the Standard Model have already been carefully documented in Andrew Pickering's sociological history of the development of particle physics, as well as numerous chapters from this volume.³ I am thinking particularly of factors like the postulation of the notion of quarks and the development of the Eightfold Way and S-matrix bootstrap theory; scaling, hard scattering, and the 1967 MIT-SLAC experiment's evidence for point-like structure in hadrons; the quark-parton model that was supported by experimental evidence for J/ψ , bare charm, and upsilon particles; the development of gauge theory, the unified theory of electroweak interactions, with the experimental evidence of neutral currents; and finally the development of a theory of strong interactions--quantum chromodynamics.

Other than to underscore physicists' decisions to pursue higher and higher energies (as evidenced in the construction of a 200, then 400, GeV proton accelerator at Fermilab), I will not recount these details here. Rather, within the context of such decisions I will attempt to describe how the increases in scale, cost, and complexity mentioned earlier were *consequences of the choices* to go to higher and higher energies in response to the experimental evidence and theoretical constructs that emerged from 1964 to 1979.⁴ More particularly, one consequence witnessed at Fermilab was the development of an increasingly complex and bureaucratic organizational infrastructure that I will characterize below as a number of interrelated resource economies, each

² Currently, there are numerous approaches to the social study of science. For a traditional view of the sociology of science, see Robert Merton, *The Sociology of Science, Theoretical and Empirical Investigations* (Chicago: The University of Chicago Press, 1973). Some of the earliest work in the sociology of knowledge can be found in Karl Mannheim, *Ideology and Utopia; An Introduction to the Sociology of Knowledge* (New York: Harcourt Brace Jovanovich, 1985), and the early development of the "strong programme" of the sociology of scientific knowledge (SSK) is best represented in David Bloor, *Knowledge and Social Imagery*, 2nd ed. (Chicago: The University of Chicago Press, 1991). Some of the more moderate proponents of SSK include Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge: Harvard University Press, 1987) and Trevor J. Pinch, *Confronting Nature: The Sociology of Solar-Neutrino Detection* (Dordrecht: D. Reidel, 1986). Perhaps the most radical SSK position is in Steve Woolgar, *Science, the Very Idea* (New York: Tavistock Publications, 1988). More recently, Andrew Pickering has collected a number of essays that focus on the central role of practice in SSK, in Andrew Pickering, ed., *Science as Practice and Culture* (Chicago: The University of Chicago Press, 1992), and Stephen Cole has provided the first serious critique, by a traditional sociologist, of the SSK position, in Stephen Cole, *Making Science, Between Nature and Society* (Cambridge: Harvard University Press, 1992).

³ Andrew Pickering, *Constructing Quarks; A Sociological History of Particle Physics* (Chicago: The University of Chicago Press, 1984).

⁴ Pickering claims the relationship between experimental and theoretical research traditions is symbiotic in that each generation of practice within one tradition provides a context within which the succeeding generation of practice in the other finds its justification and subject matter. Peter Galison claims that the truism that "experiment is inextricable from theory" or that "experiment and theory are symbiotic" is useless because, while vague allusions to Gestalt psychology may have been an effective tactic against dogmatic positivism, experimentalists' real concern is not with global changes of worldview. For Galison, the salient issue is where theory exerts its influence in the experimental process and how experimentalists use theory as part of their craft. My point is that once physicists decide to study certain physical phenomena and theoretical constructs at higher and higher energies, such a decision has physical consequences (larger accelerators given the technologies during the 1964 to 1979 era, and larger more heavily instrumented fiducial volumes in apparatus to detect myriad particle interactions) and sociological consequences of the types that constitute the remainder of this chapter. See Pickering, *Constructing Quarks*, pp. 10-11, and Peter Galison, *How Experiments End* (Chicago: The University of Chicago Press, 1987), p. 245, also Chapter 18, this volume.

having its own commodity.⁵ Another consequence of larger more complex detectors was the need for larger more complex social structures for the collaborations that designed, fabricated, installed, and operated them, as well as an increased scale and complexity for the computing power needed to collect data samples and bring the results to final publication.

After 1972 Fermilab operated the highest-energy particle accelerator in the world, and consequently competition for use of the wide variety of particle beams it produced was intense. In order to gain access to one of these particle beams, experimentalists had to navigate a number of interrelated resource economics that were embedded within an institutional structure headed by a single scientist, the director, who had ultimate authority in all matters scientific and otherwise.⁶ Experimentalists had to learn to trade with and for these commodities in order to participate in the production of knowledge in high-energy physics. Physicists negotiated with these commodities and often fought over them.⁷

One economy at Fermilab was proton economics, based on protons as the commodity. The overall magnitude of the economy was limited by such factors as accelerator flux, efficiencies in primary beam transport, cross sections for secondary beam production, secondary beam transport efficiencies, and expected reaction rates in experimental targets. For example, given the cross section for neutrino scattering (10^{-36} cm²) and the pion cross section (10^{-26} cm²), the decision to approve experiments using incident beams of neutrinos was already a major decision that affected proton economics.⁸ A neutrino beam was more costly than a pion beam in terms of the number of protons needed to produce it; this was further complicated if the cross section for event production in the proposed experimental target was low and the experiment required a large number of particle events to be competitive with previously accumulated world samples. Given the intense competition for protons, beam management issues became very complex, especially in the kind of user-based environment that typified Robert Wilson's philosophy at Fermilab.⁹

A second economy was based on experimental real estate; here the commodity was possession of an experimental hall at the end of a beam spigot to house the collaboration's apparatus. As detectors became larger and more complex, the lead time needed to assemble and operate apparatus also increased. Consequently, physicists who *were* given a piece of experimental real estate and some beam time tended to move into an experimental hall with the explicit goal of performing that experiment, and the implicit goal of not moving out. Gaining access to an experimental hall, especially when the incumbent collaboration was desperately attempting to hold its place in line, made possession of this commodity one of the most important items to be obtained in a user-based environment.

Another economy, which I call "physicist economics," is based on the commodity of physics expertise. Although the scale and complexity of the experiments during the 1964 to 1979 period continued to increase at an unprecedented rate, the number of high-energy physicists who could commit themselves to perform experiments was constrained by the total number available at that time and the rate at which new Ph.D. graduate students were being produced. Consequently, the enormous increases in the scale and complexity of experiments made physics expertise an increasingly valuable commodity.¹⁰ Larger, more complex and increasingly modularized detectors

⁵ I developed the resource economy model from a case study of several Fermilab experiments. See Mark Bodnarczuk, "The Social Structure of Experimental Strings at Fermilab: A Physics and Detector Driven Model," Report No. Fermilab-PUB-91/63, Batavia, March 1990, pp. 2-7. This model is not unlike Bruno Latour's more generic model of cycles of credit that involve conversions of different types of capital (recognition, grant money, equipment, data, arguments, articles, etc.) into the "credibility" that scientists need to make moves within a scientific field. Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton: Princeton University Press, 1986), pp. 187-233.

⁶ Maurice Goldhaber, in an unpublished article entitled "The Beginning of Program Committees," remarks on the early formation of program committees appointed by laboratory directors for the purpose of obtaining independent assessments of the laboratory's research program.

⁷ Using numerous case studies, David Hull claims that not only are infighting, mutual exploitation, and even personal vendettas typical behavior for many scientists, but that this sort of behavior actually facilitates scientific development. David Hull, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science* (Chicago: The University of Chicago Press, 1988), p. 26.

⁸ The pion cross section is roughly constant for energies above 2 GeV at about 40 millibarns. The neutrino cross section is not constant, but is linearly proportional to the energy. For Fermilab, a reasonable neutrino energy to use was 100 GeV, which would give a neutrino cross section of about 0.7 picobarns.

⁹ In Chapter 20 of this volume, Catherine Westfall notes that Edwin Goldwasser (who later became Wilson's Deputy Director) was one of the first to address user discontent in the United States.

¹⁰ For example, a recent study of the research program for the 1990s performed by the High Energy Physics Advisory Panel included a detailed demographic study of "manpower considerations" during the time period under study. See the HEPAP Subpanel, "The U.S. High Energy Physics Research Program for the 1990s," Report No. DOE/ER-0453P, Washington, D.C., April, 1990, pp. 68ff.

required larger, more complex and increasingly modularized social structures with the appropriate *number* of physicists and the *distribution* of expertise needed to design, fabricate, install, and operate the apparatus and to develop the computing systems and software programs used to reconstruct and analyze the particle events that were recorded. By the late 1970s, collaborations were characterized by an unprecedented division of labor so that no single member of the collaboration had a detailed knowledge of the entire detector. As pointed out by Galison, this kind of modularization provided each institution with a visible manifestation of its contribution to the experiment.¹¹ Not only was the modularization of detectors an important aspect of carving out a piece of physics to work on, it was also an important political issue back at the home university. Proposals were increasingly judged on the "physicist design" of the experiment and how well it mapped to the experimental design, with laboratory directors and their advisory committees focusing more and more on whether the collaboration had enough physicist power to make good on its experimental claims.

But the consequences of physicists' choices (increased scale and complexity of detectors, accelerators, and the associated social structures) are most easily seen in a fourth resource economy, computing economics, based on the commodity of on-line and off-line computing power. One example was the attempt to do high-statistics charm production experiments at Fermilab in the late 1970s.¹² On the one hand, the advantages of on-line data reduction using sophisticated trigger processors had to be balanced against the risk of coming up empty handed due to incorrect trigger assumptions and the problems of obtaining the required off-line computing power. On the other hand, the more secure approach to on-line data acquisition (the write-it-all-to-tape approach) had to be balanced against the problem of obtaining immense off-line computing resources, which was difficult given Wilson's belief that the bulk of computing for experiments should be provided by the collaboration's home institutions.¹³ There was an abrupt explosion in the number of channels of electronics in detectors after 1980. In terms of the magnitude of computing and number of channels, the period during the development of the Standard Model was the calm before the storm -- before the explosion in scale, cost, and complexity of hadron collider detectors (such as CDF) that were conceived after 1977.¹⁴

A final resource economy was physics economics; the commodity of published physics results was traded back to the laboratory director as a return on investment and was the key to obtaining additional resources to perform follow-up experiments. Within physics economics, the laboratory director's ability to approve or disapprove an experiment was a powerful management tool for leveraging wayward experimenters who failed to make good on their promises, especially when they wanted to move on to the greener pastures of follow-up experiments without first publishing their results.

The study of various Fermilab experiments mentioned earlier also shows that within this socioeconomic-scientific infrastructure of the laboratory, experiments were performed in a series of follow-up experiments in which an experiment was performed, then transformed into a second, then a third, or a fourth experiment. I call these series of experiments "experimental strings".¹⁵ Key to describing these transformations is the ability to characterize the continuities between individual experiments in such strings. Evidence that emerged from the previously mentioned study suggests that these experimental strings exhibit well-defined continuities in the physics goals, the detector configuration design, and in the core group of collaborators that participated in 9 or more experiments over a 20-year period spanning three laboratory directors.¹⁶ These continuities transcend a single

¹¹ Peter Galison referred to the visibility that modularization provided participants in his talk at this Symposium (unpublished).

¹² See Bodnarczuk, "The Social Structure of Experimental Strings," for a detailed case study of Fermilab experiments E-516, E-691, E-769, and E-791 that performed high-statistics photoproduction and hadroproduction of charmed particles.

¹³ Mark Bodnarczuk interview with Robert Wilson, 24 September 1992.

¹⁴ The UA1 detector at CERN had about 50,000 channels, the CDF and DO detectors at Fermilab, and the SLD detector at SLAC each had over 100,000 channels, the ALEPH detector at LEP had about 700,000 channels, and the proposed SDC and GEM detectors at the SSC might have had as many as 50,000,000 channels, depending on the available technology.

¹⁵ See Bodnarczuk, "The Social Structure of Experimental Strings," pp. 14--20; Joel Genuth, "Historical Analysis of Selected Experiments at US Sites," *AIP Study of Multi-Institutional Collaborations: Phase I. High Energy Physics*, Report 4 (New York: American Institute of Physics, 1992); Frederik Nebeker, "Experimental Style in High-Energy Physics: The Discovery of the Upsilon Particle" (unpublished), January 1993.

¹⁶ The major fixed-target experimental strings at Fermilab were the E-82, 226, 383, 425, 486, 584, 617, 731, 773 string, the E-531, 653 string, the E-8, 440, 495, 555, 620, 619, 756, 800 string, the E-21A, 262, 320, 356, 616, 770 string, the E-594, 733 string, the E-98, 365, 665 string, the E-1A, 310 string, the E-95, 537, 705, 771 string, the E-70, 288, 494, 605, 608, 772, 789 string, the E-87, 358, 400, 401, 402, 687 string, and the E-516, 691, 769, 791 string. By way of comparison with those counter experiments, the major continuity between the experiments performed with the 15-foot bubble chamber at Fermilab (experiments E-28A, 31A, 45A, 53A, 155, 172, 180, 202, 234, 341, 380, 388, 390, 545, 564, and 632) seems to be the chamber itself. In a less well-defined way, there were some continuities in the target substances with which the chamber was filled. But the social structures of these collaborations were different from the fixed-target counter experiments. Bubble chamber spokespersons

experiment and provide a method for understanding more complex social structures and research programs that exist for more than 15 years. Each experimental configuration in a string displays a more complex iteration of the original apparatus that leaves the fundamental design of the modularized detector subsystems largely intact. In other words, experimental strings are like mini-institutions within the organizational infrastructure of the laboratory. People outside the laboratory really do not know about them because they do not have formal names.

I believe the experimental string is the preferred and more interesting unit of study for sociological and historical analysis, because the numbers that laboratories such as Fermilab assign to experiments are not at all indicative of what actually constitutes "an experiment." Actually, the experimental numbers assigned by laboratory management are more indicative of such factors as the laboratory's accounting practices, the bureaucratic steps involved in the approval process as defined by a particular director, funding scenarios both inside and outside the laboratory, contrasts between the in-house/facility approach to doing experiments (where strings were largely determined by the laboratory management), and the user-based/non-facility approach (where institutions came together and formed strings more voluntarily). But these numbers do not define what an experiment is.

While it has been common practice for philosophers, historians, and sociologists of science to "extract" an experimental "case study" from the organizational infrastructure of the laboratory in which it was performed and attempt to study it as a stand-alone unit, the fact is that experiments like those performed at Fermilab did not exist independent of the organizational infrastructure of the laboratory in which they were embedded. Experiments and collaborations were not closed systems, cohesive entities, or "objects" that had unambiguous boundaries and could be divorced from the dynamics of laboratory life.

Of course, experimentalists did attempt to draw a firm line of demarcation around the "collaboration"- or "experiment" and its activities for the sake of defining which names appear on scientific publications, but laboratory personnel often play crucial roles in experiments, and whether or not their names appear on the published paper is a socially negotiated matter that is decided by the people involved.¹⁷ Attempts to "map" the names on various experimental proposals (or the resultant publications) to the collaboration members who actually performed the day-to-day tasks associated with the experiment show that the names on proposals or papers are often not indicative of those who actually performed the work of the experiment. Names of individuals who did not play any substantive role in a particular experiment are included on a proposal or the physics publications because, in some cases, those individuals may have had major responsibility for constructing a portion of the detector in an earlier experiment in the string. In other cases they may have committed a fraction of their overall professional time at the proposal stage but never came through on their commitments because of the heavy load of administrative duties at their home institution, the host laboratory, or their commitments to other experiments that they perceived were producing more important physics results.¹⁸ This is probably related to the problems associated with "physicist economics," and is a fruitful issue for future sociological research.

Physicists' choices to go to higher and higher energies in response to the experimental evidence and theoretical constructs that emerged during the 1964 to 1979 period, and the effect of these choices on increasing scale, cost, and complexity, reveal interesting contrasts between the American and European (CERN) styles of doing physics. During this period many American physicists preferred the more informal, non-bureaucratic,

seemed to draw upon the expertise of the international community of bubble chamber physicists each time they formed an experimental group, and consequently the collaborations did not exhibit the same type of well-defined core-group structure found in large, complex fixed-target counter experiments. My preliminary studies show that the relatively noncomplex social structure of these collaborations resulted from the existence of a Fermilab-based Bubble Chamber Department devoted solely to the operation and maintenance of the complex systems of the chamber, independent of the collaborations that used it. This type of heterogeneous Fermilab /collaboration social structure with a dedicated support group is not evidenced in even the largest fixed-target counter experiments, but it is interesting to note that a similar phenomenon (dedicated support departments) does appear with the advent of the mammoth collider detectors such as CDF and DO. For historical details see Mark Bodnarczuk, ed., *Reflections on the Fifteen Foot Bubble Chamber at Fermilab* (Batavia: Fermilab, 1989).

¹⁷ Melvin Schwartz shows how tenuous these socially negotiated walls are for today's large collaborations when he advocates divorcing some of the detector builders from the collaboration, then subdividing the remaining members of these megacollaborations into distinct (smaller) collaborations that would develop their own research programs and compete for time using the detector. In a sense, Schwartz is advocating a return to a social structure that is not unlike that displayed in large bubble chambers as I described in the previous note on the 15-foot bubble chamber at Fermilab. Also, see Faye Flam, "Big Physics Provokes Backlash," *Science* 30 (11 September 1992), p. 1470.

¹⁸ Some collaborations (such as CDF) required members of the collaboration to run a certain number of data-taking shifts in order to have their name on publications, but many collaborations had no such policies.

quick-and-dirty, frugal style of doing physics."¹⁹ But the European style was typified by what American physicists considered to be an overly formal, inflexible, bureaucratic, overengineered, "gold-plated" approach to doing physics. Even after the mammoth collider detectors began to be conceived in the late 1970s, both American and European physicists were relatively unreflective about the role that social factors were beginning to play in their work. And consequently the sociological challenges that were intrinsic to collider detector environments with 10⁵ or more channels received little or no systematic study by practicing physicists. The sociology of large collaborations just was not viewed as a part of doing high-energy physics and as with the policy of physics journals, the social and human factors were simply *left out*.²⁰

But despite this lack of conscious self-reflection on both sides of the Atlantic, the values embodied in European culture more naturally gave rise to a style of physics that was more formal in terms of well-defined roles, responsibilities, and authorities for physicists and engineers, and was more focused on producing robust engineering and physics designs that were less flexible in terms of programmatic changes. As it turned out, these were the very practices, values, and beliefs that became crucial to mounting mammoth collider-detector experiments. Conversely, the less formal approach to doing physics put American physicists at a disadvantage in terms of confronting the kinds of organizational and management problems that emerged from this enormous growth in scale, cost, and complexity. While the American style of doing physics may have been an advantage with the scale, cost, and complexity typified by the detectors in most of the 1964 to 1979 period, it became a crucial disadvantage for experiments conceived in the late 1970s, and was absolutely terminal for the proposed SDC and GEM detectors.²¹ Also, the European style allowed a more natural transition from the smaller experimental scale that typified the 1964 to 1979 period to the detectors of the present day. In the modern detector environment, not only can social factors no longer be left out of any salient definition of what high energy physics is, but they become one of the most crucial aspects of doing high-energy physics -- they could even become *the* limiting factor of the future of the field.

¹⁹ In Chapter 20, Westfall refers to a similar type of nonbureaucratic, quick-and-dirty, frugal style at SLAC. Also see Catherine Westfall and Lillian Hoddeson, "Frugality and the Building of Fermilab, 1960-1972," to be published in *Technology and Culture*.

²⁰ Kevles attributes the scientist's tendency to leave social factors out of their accounts of science to being accustomed to the literary convention of journal editors and the fact that many scientists consider themselves to be incompetent to write about anything except science itself Daniel Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Alfred Knopf, 1978), p. x. Pickering claims that references to "judgments" or "agency" on the part of scientists are left out of scientists' accounts so that scientists are portrayed as passive observers of nature, with experiments appearing to be the supreme arbiters of competing theories. Pickering, *Constructing Quarks*, pp. 5-18. Latour claims that there are definable processes that operate to remove all aspects of the social and historical context in order that scientific "facts" do not appear to be socially constructed. See Latour and Woolgar, *Laboratory Life: The Construction of Scientific Facts*, pp. 176-183; and Latour, *Science in Action*, pp. 22-29.

²¹ It is interesting to note that in an address in honor of the 75th Anniversary of the Max Planck Institute for Physics in Munich, Germany, James D. Bjorken devoted a major portion of his visionary article to the problems associated with the sociology of large collaborations and the possibility that these social factors might have an effect on the physics itself. See James D. Bjorken, "Particle Physics - Where Do We Go from Here?" *SLAC Beam Line*, Vol. 22, Winter 1992, p. 10.