

## APPENDIX C. THE EVOLUTION OF LARGE-SCALE RESEARCH IN PHYSICS

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Even the most casual glance at the great accelerator complexes reveals how dramatically the scale of experimental physics has grown over the last half century. It is a growth that has, at the same time, presented unprecedented opportunities for research and provoked spirited debate within the community of physicists as to the nature and direction of the discipline. In these few pages I would like to organize that growth into three stages, and to provide a few details about the turning points in the creation of "big physics." Perhaps this historical background will shed some light on contemporary planning for the next generation of team research.

### Stage I: The Transformation of the Outer Laboratory

During the 1920s, American physics underwent a decisive shift. Drawing on the support of philanthropies such as the Carnegie Corporation and the Rockefeller Foundation, the physical sciences began to construct apparatus on a scale unimaginable just years before. Caltech, led by its scientist-entrepreneur Robert Millikan, guided the way toward cooperation between private and university interests. For example, the Southern California Edison Company provided a million-volt transformer for nuclear research. Similarly financed by private sources, Mount Wilson grew to become, by the early 1920s, the site of one of the great observatories of the world. Corporate funding brought Caltech into partnership with industry in the development of high voltage lines, wind tunnels, aqueduct projects -- pure and applied science grew to the grand scale on many fronts.<sup>1</sup>

The decision to improve science by innovations in instruments was natural for the United States. Even in the 19th century some of the greatest accomplishments of American physics had been in instrument design -- Albert Michelson's precision optical work brought the United States its first Nobel Prize, and Henry A. Rowland's gratings were widely recognized as the key to a new generation of accurate spectroscopy. In the 1930s, by combining skilled machine building with the scale and organization of industry,

American physicists created a niche for their skills at which they not only could compete, but excel.

Ernest Lawrence, at the University of California, quickly learned to build on the corporate largess that was boosting Caltech to national pre-eminence. Beginning with tiny cyclotron prototypes, Lawrence quickly advanced to one of one-billionth of an ampere and 1.22 MeV protons in 1932, to the prized 27-inch machine shortly thereafter; to the 37-inch cyclotron that produced 8 MeV deuterons in September 1937, and to the 60-inch cyclotron that came on the air in June 1939 yielding 16-MeV deuterons. Just a few months later, Lawrence began planning for a 100 MeV cyclotron; it would cost well over \$1 million and measure 184 inches in diameter. It was this device that served as the model for Lawrence's wartime project: the electromagnetic separation of U-235 from U-238 using electromagnets a hundred times bigger even than that used in the 184 inch.<sup>2</sup> Berkeley's startling pace of expansion created a reputation for the laboratory as a leader in the new machine physics, and a legacy that later projects would inherit.

The industrial model for laboratories learned during the 1930s set the stage for the wartime expansion of physics that restructured many aspects of the practice of physics. Not only were budgets hiked upwards, but the oddities of Berkeley laboratory work were expanded outward. Night shifts and check lists, supervisors and hierarchical organizations were built into the running of large-scale research at the Metallurgical Laboratory in Chicago, into the Oak Ridge isotope separation facility, and into many facilities at the Los Alamos Scientific Laboratory. The implications of these developments for postwar research were not lost on the participating scientists. Nor was the expansionary spirit confined to National Laboratories.

Consider the following situation at Harvard. At a meeting on 20 November 1944, the President and Fellows of Harvard College established a panel to direct the expansion of physics, chemical physics, and engineering. When the panel first met, Edwin Kemble voiced an increasingly common belief: "the war has given a great boost to physics. It has stressed the importance of physics to industry and national defense and has underscored the usefulness of men trained in pure physics when emergency requires that they turn to

applications." Above all, postwar research had to hire "top caliber" theorists and vastly increase the budgets for construction. As Kemble recognized, the new physics would call for a thoroughgoing collaboration between theorist and experimentalist, and profound connections between engineering and physics.<sup>3</sup>

Princeton physicists too began to plan for a new style of physics, even before the war came to a close. John Wheeler still had doubts about the immediate capabilities of accelerators as contrasted to the easy accessibility of cosmic rays. Nonetheless, he agreed that Luis Alvarez's latest plans to build a linear electron accelerator would eventually lead to a 5-GeV proton accelerator capable of fabricating pairs of mesons.

Meanwhile, Wheeler envisioned the impressive Flying Fortresses as a fine and appropriate tool for hoisting cosmic-ray apparatus, and even these massive tools soon gave way in his plans to the cosmic-ray opportunities afforded by the experimental resources of captured V-2 rockets and their German designers. Such exploitation of war-related materiel was a fundamental feature of the wartime expansion: surplus capacitors served in Luis Alvarez's Berkeley proton linac, and around the country vast quantities of microwave equipment and instrumentation were literally hauled from wartime efforts to peacetime physics.<sup>4</sup>

At other universities the organization of wartime research had its consequences. Government contracts had replaced prewar philanthropic and corporate bequests. In reports as early as August 1942, Stanford planners began to consider the effects of substantial contracts: they would grant the university Federal war priority, keep faculty on campus, create interdisciplinary research, and engage a new cadre of talented students who would stay on after the war. Above all, government-sponsored research would rocket Stanford to a position comparable to Harvard, Chicago, Caltech, the University of California, and Columbia by bringing in "substantial additional income."<sup>5</sup>

But beyond the direct transfer of equipment, and a restructuring of funding from the private to the public sphere, the war brought a new model for research, now organized upon complex managerial lines. There was widespread admiration for the accomplishments of the wartime collaboration between

experimentalists, theorists, and engineers working under a goal-directed and centralized organization. So it was that just a few weeks after D-Day, Henry Smyth sketched at Princeton a proposal for a new kind of physics laboratory, one that could duplicate the scientific/engineering successes already in hand from the institutions that had produced radar, the bomb, proximity fuses and rockets.

Smyth christened his July 1944, "A Proposal for a Cooperative Laboratory of Experimental Science," and the document reflected on the vast changes facing physics. "The war," he wrote, "has now reached the stage where it is desirable to make plans for the postwar period and the period of transition. The complete disruption of the normal activities of the universities and, in particular, of the scientific groups in the universities leaves the whole condition of science in this country highly fluid." Smyth's remark strikes at a central, and often ignored point: the radical reformulation of physics during the war was due not only to the increased funding but to the dislocation of physicists, laboratories, funding mechanisms, and even teaching practices. Departmental boundaries within universities came to a new configuration, connections between universities and research groups resettled in new, often productive networks. Physicists learned the skills of radio engineering and microwave technology; metallurgists and nuclear physicists combined forces; theoretical nuclear physicists learned to work with new methods of calculation. While "normal procedures" were suspended, deeper and faster mutations were imposed on the infrastructure of physics than would ever have been possible in peacetime.<sup>6</sup>

As Smyth noted, the direction of those mutations began to shape the very definition of a physicist and physical research:

Forty years ago the physicist working on a research problem usually was largely self-sufficient. He had available a certain number of relatively cheap instruments and materials which he was able to assemble himself into an apparatus which he could operate alone. He then accumulated data and interpreted and published them by himself. Most of the special apparatus that he needed he himself constructed with his own hands. He was at once machinist, glassblower, electrician, theoretical physicist, and author. He instructed his students in the various techniques of mind and hand that were

required, suggested a problem, and then let the student work in the same fashion under his general supervision.<sup>7</sup>

Of course, the 1930s had already witnessed an increase in complexity. Laboratories had begun adding specialized technicians on whom even graduate students had come to depend. Bigger devices -- especially Lawrence's cyclotrons -- dwarfed the table-top apparatus of earlier times.

Smyth voiced a concern that would be heard over and over again: there was danger in the increased size, a danger that only a few elite institutions would be left in command of research, weakening the "background of strength in science which [had] grown up so successfully in this country in the past twenty years." Only cooperative research could rescue that strength. As the war dragged on, Smyth, seeing more of wartime massive research, added that such big projects should not "oppress the individual scientists [who] must be the servants, not the masters of the research man." The cooperative project that Smyth had in mind did come to pass, first in the form of the Brookhaven reactor project, and later in the accelerator laboratory that became an exemplar for cooperative laboratories around the world.<sup>8</sup>

The massive war projects -- radar and the atomic bomb -- made a deep impression even outside the United States. Indeed, when Kowarski wrote his influential "Note on the creation of a western European atomic research center," -- the center that later became CERN, he explicitly cited Europe's need to keep up with the new industrial scale physics of America -- in pure physics as well as applications.<sup>9</sup>

But the transformation of the laboratory, begun with the philanthropically-supported big apparatus of the 1920s and 1930s and accelerated in the 1940s, left the immediate surroundings of the experimentalist unchanged. Thus even as the accelerators grew in size and organizational complexity, physicists were still importing the same detectors they had used previously. The antiproton, to take an important example, was discovered at Berkeley in an emulsion stack borrowed from the much smaller scale cosmic-ray tradition flourishing in England. Indeed, physicists lifted the assorted cloud chambers, scintillators, and nuclear emulsions, from cosmic-ray experiments, and placed them in the beam of the new accelerators. One experimentalist

went so far as to call the betatron beam a source of "artificial cosmic rays." For this reason, I find it useful to refer to the building of cyclotrons and cooperative laboratories up to the mid-1950s as a transformation that overhauled the outer laboratory.

Thus the outer environment of the accelerator complex shifted first, while the immediate work space -- what an evolutionary biologist would call the microenvironment -- of the experimentalist remained essentially unchanged from the earlier radiochemical and cosmic-ray methods.

### Stage II: The Transformation of the Inner Laboratory

But once the outer laboratory had undergone the shift to industrial scale and organization, it was only a matter of time before the sanctum of the inner laboratory also yielded to the new form of research. The first element of the inner laboratory to make the transition was the bubble chamber.<sup>10</sup>

As in almost every other detector innovation in the 1950s, the bubble chamber had its roots in cosmic-ray physics. Glaser had his training at Caltech -- where a long tradition of precision, individually-based research had begun with Millikan and continued through the efforts of his student, Carl Anderson. Years before, Millikan had won his Nobel Prize (the second for an American) for his work on the photoelectric effect, and Anderson secured his trip to Sweden through his stunning cloud chamber studies leading to the discovery of the positron. Brought up in this line of relatively small-scale and precisely instrumented work, Donald Glaser had learned physics from Anderson, refining the use of cloud chambers for the momentum determination of high-energy sea level muons. Ph.D. in hand, Glaser set off to Michigan specifically to avoid being inducted at other universities into what Glaser dubbed the "factory environment" of the big machines.<sup>11</sup>

It was a time when the cosmic-ray physicists and their cloud chambers were under siege. The diffuse gas offered relatively few targets in which an incoming particle could interact. This meant that most particles simply passed through the chamber. Film emulsions, which were denser, suffered from their inability to resolve short time intervals, and the enormous

difficulty of extracting information from the cumbersome stacks of emulsion material. Working by himself, Glaser was determined to find a new principle of particle detection that would allow him to "save cosmic-ray physics." With only tiny grants of several hundred dollars, Glaser tried electrical, chemical, and thermodynamic instabilities -- all to no avail. From one grant agency, Glaser garnered the funds to assemble a continuously operable cloud chamber, but despite his efforts that particular device never amounted to more than a demonstration machine.

Finally, in late 1952, Glaser succeeded in contriving a workable bubble chamber in the form of a tiny glass bulb filled with diethyl ether. He struggled valiantly to design a triggering system to make the device function as a cosmic-ray detector. But, like Frankenstein, the invention took on a life of its own, resolutely resisting all Glaser's attempts to make it triggerable after an event. At last the young physicist brought the chamber to an American Physical Society meeting where two attentive listeners -- Darragh Nagle and Luis Alvarez -- took notice, caring not a bit that the device was no good for cosmic rays.

From his first encounter with Glaser, Alvarez began to visualize a massive hydrogen chamber built to an engineering style typical of the biggest scientific/engineering collaborations. Unlike Glaser, since the beginning of the war, Alvarez had learned not only to live with the new style of the transformed outer laboratory but to revel in it. From MIT's Rad Lab he had gone to Los Alamos, after Los Alamos to the construction of a proton linear accelerator, and then on to the supervision of Lawrence's ambitious program to produce vast quantities of neutrons, code-named the Materials Testing Accelerator. Enlisting the help and equipment of the AEC and the National Bureau of Standard's cryogenic laboratories (first designed to produce and study hydrogen isotopes for the thermonuclear), Alvarez mobilized the Berkeley laboratory to do to bubble chambers what Lawrence had done to cyclotrons. As the chambers grew in size, the spectrum of resonances burst open and particle physics as a distinct discipline came into its own.

Now the inner laboratory underwent a transformation akin to that of the outer laboratory in the 1930s. Safety procedures, forced on the team by the dangers of hydrogen, standardized routines in the experimental area itself.

Engineers now had to work with physicists not only to provide the beam, but to design, build, and operate the detectors. Computers came to play an essential role in analyzing the millions of photographs that came pouring out of the production line of bubble chamber studies. In addition to structural engineers and accelerator engineers, computer specialists were called to assist as were picture scanners and cryogenic experts. Suddenly, the very practice of experimenting was altered -- the Alvarez group began to ship film to laboratories across the world. Groups would come to the laboratory and specify particular sets of experiments to do, which meant that other people went down to the accelerator. A later Director General of CERN fretted that the growth of large-scale bubble chambers and data-analysis equipment was driving more and more physicists and engineers into administration. Others worried about the ensuing "feeling of remoteness from the experiment" or argued that students might not learn what it meant to get their hands dirty in real experimentation and be cut off from "the realities of the experiment."<sup>12</sup>

For years Alvarez had argued to the community that they could not shy away from the methods of the new scale of physics if they wanted the goods -- and the goods were indeed being produced. New mesons, details of the weak interactions, strange particles, and much of the empirical basis for the (excuse the anachronism) flavor SU(3). Through the productive years of the 1950s and early 1960s, the big bubble chambers at Berkeley, Brookhaven, CERN, and elsewhere simultaneously contributed to the centralization and decentralization of physics. In an obvious sense resources were collected into fewer locations. In a less evident way the new facilities allowed groups from universities without accelerators to do particle physics. For example, one outside group from Johns Hopkins, led by Aihud Pevsner, used film from the Berkeley chamber and discovered the 550 MeV eta; another, led by Bogdan Maglic, found the isospin-zero omega meson. Some emulsion groups, exploiting the similarities in emulsion and bubble chamber track reduction procedures, requested and received film from the chamber, as did a group from Gottstein in Germany.

But in the end some physicists, including Alvarez himself, began to despair at an industrialized experimental nuclear physics that had become, in his

words, "just a little dull:"

. . . so much work can be done by technicians. . . . You have technicians who run alpha particle spectrometers and beta ray spectrometers and gamma ray coincidence circuits. And the people working in the field are doing very much what our graduate students are doing: they are putting things into computers and analyzing the print-out, and they are pretty disconnected from the experimental side of it, in the same way that we are. I can't complain because our people don't go down and look at the bubble chamber very often or at the Bevatron. They ask the bubble chamber operators to expose a certain number of millions of frames of film, and then they ask somebody else to measure them, and then run them through computer programs, and then they start with computer program output and process the data.

Alvarez certainly was not the only one to become disenchanted with the magic of the new scale of experimentation. At Stanford in the mid-1950s, some physicists looked fearfully across the Bay. "The pendulum has swung," one wrote in a memo to colleagues, "through the roof." The sky is now the limit. Research money is to be had for the asking and sometimes agents come around seeking to give it away . . . In our own department the mails are heavy with outgoing manuscripts and incoming honors, all based upon the really splendid research . . . The disquieting thing about it is that this development has infected us with berkeleyitis." Another Stanford scientist worriedly concluded that "Megabuck research projects should be confined to Government laboratories, industrial laboratories, and university departments which have megabuck teaching budgets." Otherwise there can be a "breakdown of the academic atmosphere."<sup>13</sup> Even at Berkeley itself others were trying to devise new methods of inquiry precisely to avoid this sort of work. Wenzel, for example recalled how "I'd always been interested in electronics and after a while I got tired of the business of scanning and reconstructing the film . . . I started to think; what were other ways to record the information?" By 1964, Wenzel's discontent was shared by an ever widening circle of displaced counter physicists. In response, like-minded counter physicists convoked a March meeting at CERN on "Film-less Spark Chamber Techniques and Associated

Computer Use." At the conference physicist after physicist proposed new methods that might allow them to escape the tyranny of large centralized bubble chamber facilities or film-producing spark chambers. As one put it,

with computers on-line the physicist will receive certain answers during the running of the experiment in the experimental halls [which] might give him back the pleasure of being an experimenter and not only an operator, who is able to act and to put new questions on the grounds of this information during the running of the experiment.<sup>14</sup>

Propheying the eventual triumph of the imageless devices, he concluded, "The high cost of the new techniques might damp the speed of the development you have initiated, but not withhold it." If some physicists, driven by discomfort with the larger-scale detectors and their mode of operation, were led to new discoveries so too were those more at ease with the new physics.<sup>15</sup>

Spark chambers, greatly expanded, figured in one of the most remarkable physics experiments of the post-war epoch. In November of 1959, students and faculty gathered around T. D. Lee at Columbia to debate how to test weak-interaction theory at high energies. It occurred to Melvin Schwartz the exploration might be done with neutrinos -- a thought that occurred almost simultaneously to Bruno Pontecorvo, by then in the Soviet Union. Unfortunately, Schwartz soon realized that tanks of scintillator, banks of Gieger-Muller counters or stacks of neon-filled tubes all had poor spatial resolution. And bubble chambers, big as they were, were not massive enough to use as an adequate neutrino target. During the summer of 1960, Irwin Pless from MIT reported to Schwartz and to Leon Lederman about Jim Cronin's desktop-sized spark chamber. In pursuit of a possible answer to their dreams, Schwartz and Lederman raced to Princeton to look at it.<sup>16</sup>

With money from the United States Navy, the Atomic Energy Commission, Columbia, and Brookhaven, the hunt for a second species of neutrino was set in motion. The Navy supplied surplus cruiser deck plates, weighing between two and three thousand tons gratis. The successful hunt for a second neutrino using the enormously scaled-up spark chamber provided a model to the physics community; for many years this "Schwartz-Steinberger-Lederman" experiment served as the

prototype for neutrino physics. By a transformation in scale, the neutrino -- until then difficult enough even to detect -- had become a viable instrument for probing elementary physical interactions.<sup>17</sup>

Thus the second transformation of experimental work, like the first, had some unpredicted effects. The new technologies and application of engineering vastly expanded the kinds of experiments that could be imagined and executed. The spark chamber, originally designed as a cosmic-ray device had become the workhorse of a new field of experimental particle physics -- neutrino interactions. In Berkeley the bubble chamber built to study weak interactions had become the centerpiece of a profound new investigation into the strong resonances. Some physicists adapted easily to the new style of work, finding excitement in the industrial scale efforts; for others the change in work organization drove them to desperation -- some left physics, others struggled to save their subdiscipline with innovations. Glaser tried to rescue cosmic-ray physics from the onslaught of accelerator-based research by inventing the bubble chamber. The electronic ("logic-circuit") specialists sought to rescue the new style of work in the inner laboratory by inventing new electronic detectors: current dividers, vidicon systems, magnetostrictive devices and so on. Ironically, these very inventions themselves became the basis for ever larger devices.

### Stage III: The Transformation of Data Analysis

But even the twin transformations of the inner and outer laboratories do not capture the full impact of the scale change of experimental high energy physics. A third stage of evolution came, in the 1970s, as the data-analysis process itself began to devolve into specialized tasks. Thus in experiments such as the famous "Gargamelle" (heavy liquid bubble chamber) discovery of weak neutral currents, the unraveling of the signal was a composite process. One subgroup argued that the signal could not be due to neutrons from the walls, another that it could not be due to neutral kaons, and in this way a final argument was assembled of the form: the muonless events must be neutral currents because all other reasonable explanations A, B, C, D, . . . are ruled out by the following arguments.<sup>18</sup>

The new prominence of data reduction gave rise to new opportunities. In the earlier periods there was a division of labor in the building of the cyclotron; later a division of labor in detector construction. Now, the division of labor in data reduction meant that individuals, or small groups were positioned to make their own speculative cuts on the data, or pursue classes of anomalous events. Surprisingly, some of the largest groups, when they foster a degree of partial autonomy among their participants, can offer greater opportunity for individual exploration than earlier groups of twenty or thirty participants. In addition, the proliferation of subgroups, each checking the hardness of a new effect, provided additional checks against ephemeral, spurious results.<sup>19</sup>

As in the earlier turning points, physicists have once again raised questions about the implications of the new specialization. They have pondered: What does it mean that experimentalists have become Monte Carlo specialists or experts on this or that background signal? What are the implications of 200 (in the planned SSC, 400) - member groups for the apprenticeship system that originally functioned as a broadly-based training ground for experimental physics? How will this ever-narrowing funnel of expertise affect the possibilities for future, radical departures from current practice?

Though it may at first sound paradoxical, the very size of the new generation of experiments may offer the opportunity to resolve some of these questions. For, in a fascinating way, the new scale of experimentation has begun to exhibit many features previously located in the community as a whole. Large groups have their own internal conferences, internal model-building, internal theoretical assumptions, internal arguments about physics, and internal publications. Haeckel famously introduced to biology the dictum that "ontogeny recapitulates phylogeny." It may be that we are now witnessing the recapitulation of the scientific community within the structure of single experiments.<sup>20</sup> With the advent of the SSC, that process will be taken one step further.

## Conclusion

The history of experimental physics since the 1920s is an astonishing one and, while its results stand as a monument to the most abstract of the sciences, its methods and orientation have been inseparably wed to the events of the latter

twentieth century. The new scale of physics has its material roots in a wide variety of soils: early resources and models of grand enterprise came from the industrial magnates of the Western United States, government financing and scientific/military filiations were permanently established in the radar and atomic weapons systems of World War II; new techniques of data acquisition and processing issued from electronics and computer revolutions of the 60s, 70s and 80s; the era of huge bubble chambers began in the epoch of massive AEC funding and the cryogenic spin-offs of the Cold War.

The assimilation of the methods and tenor of industrial, government, and military execution of large-scale projects has enabled physics to pursue goals that would have astonished the previous generation. Each step toward a new scale has come at a price and generated heat -- and often light. In each generation some physicists have seized the new technology and work organization; using them, they have opened new fields of high-energy physics. Other physicists have not taken lightly to having their autonomy hemmed in, and at each turning point have searched for ways to preserve a wider scope of experimental activity. Many of the issues raised remain open, and as the physics community contemplates the next era, of LEP and the Superconducting Supercollider, it would do well to bring them to the fore. What will it take to keep the pleasure in being an experimenter and not just an operator? And how will the next generation of physicists learn what that term means?

At the same time, the very large scale offers the possibility of solutions to some of these problems in ways that will need to be addressed. The advent of a division of labor in data analysis vastly increases the number of people who can participate in the give and take of model-building, cut-selecting, and the exploration of anomalies. And, as experimental particle physics has begun to grow, the experiment has, by necessity, expanded to take over many of the tasks previously occupied by the physics community writ large. The task of the next generation of experimentation (at LEP, at the SSC, or at yet other facilities) is to make the new scientific community -- the big experiment -- in the image of the community we want. It must be a community that allows individuals to retain significant control over their work, while providing a larger framework of mutual assistance in a cooperative environment.

## Notes

1. On the history of Caltech, see R. Kargon, The Rise of Robert Millikan (Ithaca: Cornell, 1982) and on Berkeley see J. Heilbron, R. Seidel, and B. Wheaton, Lawrence and His Laboratory: Nuclear Science at Berkeley: 1931-1961 (Berkeley: OHST, 1981).
2. Heilbron, Seidel, and Wheaton, *ibid.*
3. All citations to material on the transformation of physics at Harvard and Princeton during and after World War II are from P. Galison, "Physics Between War and Peace, " in Mendelsohn and Row, eds., Science and the Military (Boston: Reidel, forthcoming 1988).
4. *Ibid.*
5. *Ibid.*
6. *Ibid.*
7. *Ibid.*
8. On Brookhaven see Needell, Allan, "Nuclear Reactors and the Founding of Brookhaven National Laboratory," Historical Studies in the Physical Sciences 51 (1983): 93-122.
9. See Hermann, A.; Krige, J.; Merists, U.; and Pestre, D., History of CERN Volume 1 (Amsterdam: North Holland, 1988).
10. The following discussion and quotations on the history of the bubble chamber are from P. Galison, "Bubble Chambers and the Experimental Workplace," in Achinstein and Hannaway eds., Experiment, Observation, and Hypothesis in Modern Physical Science (Cambridge: MIT-Bradford, 1985).
11. *Ibid.*
12. *Ibid.*
13. Kirkpatrick to Physics Staff (Stanford), 30 April 1956. Stanford Archives.

14. On the development of electronic detectors, see P. Galison, "Spark, Bubble, and the Postwar Laboratory," in Brown, L.: Dresden, M.; and Hoddeson, L., Elementary Particle Physics in the 1950s (Cambridge: Cambridge Univ. Press, forthcoming 1988).
15. Ibid.
16. Ibid.
17. Ibid.
18. For a detailed history of the neutral-current discovery and general discussion about the effects of expansion on the conduct of particle physics experiments, see P. Galison, How Experiments End (Chicago: Chicago Univ. Press, 1987).
19. Ibid.
20. I have argued this point at much greater length in How Experiments End (see above.)