

Properties and Phenomena: Basic Plasma Physics and Fusion Research in Postwar America

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I review the changing conceptions of basic physics that the U.S. plasma-physics community put forward in postwar America. I give special attention to the tense relationship between fusion research and the more general study of plasmas in astrophysics, space science, and industry. Although fusion research often led to results that were regarded as basic plasma physics, its dominating influence tended to weaken other plasma work, as becomes evident when I compare the public statements and professional fortunes of plasma scientists during the 1960s, when fusion research experienced a downturn, with those of the 1970s, when fusion research flourished. I also show that the plasma-physics community's conceptions of basic physics were not highly regarded or easily understood by science administrators and the general physics community. To make this point, I contrast two general ideas of basic physics: the Big Questions conception and the Properties and Phenomena conception.

Key words: Sanborn C. Brown; Francis F. Chen; Bruno Coppi; Harold Grad; Richard F. Post; Barrett H. Ripin; Marshall N. Rosenbluth; Norman Rostoker; Lyman Spitzer, Jr.; plasma physics; basic science; fusion research; plasma astrophysics; space science; Q machine; tokamak; inertial confinement.

Introduction

Physics traditionally has identified and prided itself as being the most basic of the sciences. In the United States, Henry A. Rowland expressed this attitude clearly in a talk he gave in 1883 at a meeting of the American Association for the Advancement of Science in Minneapolis, Minnesota, when he bemoaned the state of physics in America, suggesting that the American public did not respect science that was not aimed at applications. He used the term “pure science” to refer to the disinterested and virtuous love of knowledge for its own sake and argued that the best researchers who pursued it should receive public support. “We are tired of seeing our professors degrading their chairs by the pursuit of applied science instead of pure science ... [and] lingering by the wayside while the problem of the universe remains unsolved.”¹ Rowland's plea grew in strength during the twentieth century as evidenced in books, articles, funding requests, and editorials, including a recent one in *Physics in Perspective* where the editors stress that physics is the science that has “sought answers to the most basic ques-

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tions – the Big Questions.” In response to the challenge that the biological sciences are now posing in addressing the Big Questions of “the molecular dynamics of life,” physicists should not “justify their pursuit of basic knowledge by pointing to the inevitable spinoffs that have financial benefits for commerce,” but should reaffirm their commitment to “basic physics” as “the quest for the fundamental laws of Nature.”²

As physics spawned more and more subdisciplines beginning in the nineteenth century, most expressed their commitment to basic research, but apart from cosmology and particle physics, most also have had difficulty in meeting this lofty ideal. Plasma physics is an especially good case in point. The plasma-physics community faced two problems in affirming that theirs is a basic science: First, its cognitive goals often conflicted with its patron relationships and institutional circumstances; and second, the nature of the physical problems plasma physicists addressed led them to a conception of basic science that was not embraced or even respected by the physics community as a whole.

Military patronage has received extensive attention from historians, particularly the question of whether it has unduly constrained the development of physics. Stuart W. Leslie begins his study of physics at the Massachusetts Institute of Technology (MIT) and Stanford University by positing Senator J. William Fulbright’s notion of a “military-industrial-academic complex” and concludes that this nexus of interests had a strong and detrimental influence on university research: By the 1950s, MIT had become “a university polarized around the military,” and Stanford’s academic program was virtually directed by its military sponsors.³ Military interests, in other words, steered academic scientists away from other, potentially more fruitful areas of research. Paul Forman paints a similar picture in his study of post-World War II physics. He argues that many specialties in quantum electronics, solid-state physics, and laser research were profoundly affected by military funding and the requirements of secrecy. Not only was basic research forced to live “from the crumbs that fall from the table of a weapon development program,” in Lee DuBridge’s famous expression, scientists fell prey to the illusion that their work involved a kind of pure or disinterested science, even as it was severely constrained in its content and dissemination by its applied, military origins.⁴ These issues are also pertinent to the many plasma-physics specialties that were funded by military agencies and affected by security concerns. For example, magnetic-confinement fusion was classified secret from 1951 to 1958, partly owing to concerns that a successful fusion reactor could be used to produce bomb-grade fissionable material, and laser-plasma research was classified secret until 1972 because of its direct connections to military applications.

A balanced assessment of the effect of military patronage on physics, however, requires us to challenge the view that it deformed otherwise healthy research. Roger L. Geiger in his study of university research between 1945 and 1970 suggests that Leslie’s and Forman’s accounts exaggerate the negative effects of military patronage,⁵ oversimplifying the relationship between science and the military as one in which academic scientists were dragged away from basic science and toward applications. He suggests instead, especially for the early period of his study, that “the interests of patrons and those of scientists largely coincided.”⁶ Until the funding crisis of the late 1960s, the defense establishment was willing to support a significant amount of basic

research at universities, and academic scientists were often satisfied that their work under defense contracts entailed fundamental research.

Apart from military patronage, there are of course many other institutional contexts for scientific research, as this question necessarily extends to other ways in which other patrons might be seen either as limiting or as enabling fundamental research in other applied programs. In plasma physics, the most important applied program (which sometimes but by no means always had connections to military research) was the fusion-energy program. As Joan Lisa Bromberg has shown,⁷ the plasma-physics community was able to sell its efforts to the federal government and the general public as a way of attaining practical fusion power during two periods of time. During the first period, from 1951 to 1958, the plasma-physics community and its administrators hoped that an effort like that of the Manhattan Project would produce a sudden and decisive breakthrough in fusion research. During the second period, from roughly 1970 to 1982, the promise of new high-temperature machines (especially tokamak magnetic-confinement and laser inertial-confinement machines) led to higher and higher budgets for fusion research. I will argue that Geiger's picture fits part of the relationship between plasma physicists and their patrons. In particular, their research that was directed to the practical goal of attaining fusion power often defined the frontier of basic plasma physics.

Despite the close connection between much basic and applied research, we nevertheless should account for the specific ways in which applied research and its relationships to particular patrons modify or redirect the research program of a particular scientific community. The history of plasma physics offers us a unique opportunity for this investigation, because between the above two periods of greatest activity in fusion research is the decade of the 1960s, when the fusion program experienced numerous technical setbacks and sluggish funding. While this intervening period is of less interest in the history of fusion research, the abatement of fusion research in the 1960s throws into relief the plasma-physics community's conception of basic research and how it changed during the ensuing decade when plasma physicists reasserted their commitment to attaining the practical goal of fusion power – but at the same time did not abandon their commitment to basic plasma physics. The interaction of these two commitments, however, entailed difficulties; it generated disagreement within the plasma-physics community, and it was met with ambivalence by the physics community as a whole.

Beyond the question of how patron relationships affected the plasma-physics community's commitment to basic research, we must examine its definition of basic plasma physics, which was considerably different than the one that the discipline of physics as a whole accepted. This problem of definition arguably has affected other subdisciplines as well and has confounded the historiographical debate regarding applied and pure science. I therefore must invent some terminology to assist us in traversing this messy rhetorical terrain. I will introduce two broad conceptions of basic science, and argue that both have been important in the history of physics and its subdisciplines. The first is the one that is familiar in cosmology and particle physics whose respective goals are understanding the origin and development of the universe and searching for the ultimate constituents of matter and the forces between them. I will refer to this as the Big Questions conception of fundamental physics.

By contrast, in many subdisciplines basic physics is conceived in the sense of understanding the basic properties and phenomena in a particular realm or physical system. Spencer R. Weart's study of the development of the solid-state physics community offers one example. In the middle of the twentieth century, solid-state physics developed in a wide range of studies aimed at industrial and military applications, much of which produced results that the solid-state physics community deemed to be basic research. As different groups of solid-state physicists studied different physical behaviors of solids, they recognized the inherent diversity of their subdiscipline. They therefore came to see solid-state physics as fundamental from the general standpoint of using known laws of physics to encompass more and more phenomena. This cognitive diversity was reflected in their social structure: Just as solid-state physicists studied a diverse range of phenomena in solids, so the solid-state physics community formed a sort of amicable confederation among its component specialties.⁸

Similarly, plasma physicists have seen their work as the long-term and exhaustive characterization of the "fourth state of matter." Their research program has not sought to reduce all plasma phenomena to a single law or theory of physics, but rather to isolate the diverse behaviors of ionized gases experimentally and to then create a series of related models. I will refer to this conception of basic physics as the Properties and Phenomena conception. The U.S. plasma-physics community, from its origins in the 1950s, has embraced a Properties and Phenomena conception of basic plasma physics, which has involved the investigation of a host of basic plasma effects in many physical realms, including industrial applications, fusion research, space physics, and astrophysics, and often in making connections among them.

I will draw two general conclusions. First, in terms of prestige, the Properties and Phenomena conception of basic plasma physics ranked a clear and distant second to the Big Questions conception of cosmology and particle physics. Even during the 1960s, the Properties and Phenomena conception of basic plasma physics commanded a weak influence in the general physics community, compared to the Big Questions conception. Second, the diffuseness of the Properties and Phenomena conception of basic physics embraced by the plasma-physics community opened it up to being strongly reformed and redirected by institutional and financial circumstances. Its Properties and Phenomena conception was narrowed and constrained during the 1970s, after being subjected to the pressures imposed by the fusion effort, by its cognitive preoccupations, and by its management by the Atomic Energy Commission (AEC). My study will partly confirm Geiger's view that patron relationships directed at applied research do not necessarily clash with the conception of basic science by a subcommunity of physicists. I add, however, that this does not diminish the duty of historians to trace how institutional contexts change during the history of a scientific community and how they modify its conception of basic science.

The Making of Plasma Physics

During the 1950s, the plasma-physics community grew around the secret fusion programs in the United States, the Soviet Union, and the United Kingdom. The U.S. program comprised three main research groups, each of which featured different machines



Fig. 1. Astrophysicist Lyman Spitzer, Jr. (1914–1997) of Princeton University. *Credit:* Photograph by Ulli Steltzer; courtesy of the American Institute of Physics Emilio Segrè Visual Archives.

that sought to confine plasmas with magnetic fields: the program at Los Alamos National Laboratory (LANL), which centered on the pinch machine; Project Matterhorn at Princeton University, which was headed by astrophysicist Lyman Spitzer, Jr. (figure 1) and was devoted to his stellarator machine concept; and the project at Lawrence Livermore Laboratory, which developed the magnetic mirror.

When Lewis L. Strauss became Chairman of the AEC in July 1953 (he served for five years), he focused his attention on the AEC's relatively small program on controlled thermonuclear fusion research. He sought to expand the U.S. fusion effort with generous increases in funding and did all he could to keep the project classified secret. His drive to beat both the Soviet and British fusion efforts impelled him to "[leave] no stone unturned to accelerate the Sherwood project to the greatest extent that availability of funds permit."⁹ Many in Congress, industry, and the scientific community shared Strauss's enthusiasm for fusion research. Still, Lyman Spitzer recalled being surprised and a little worried when Strauss pulled him and others aside to ask questions such as "How can I help?" and "How much money can you use?"¹⁰

Strauss believed that fusion research should be kept classified secret for two reasons. First, it had potential military applications: A successful fusion reactor would produce a strong flux of neutrons that could be used to transmute uranium ore into plutonium fuel for the production of bombs. Second, and more importantly, Strauss hoped that the U.S. was ahead of its Cold War competitors (primarily the Soviet Union but also Britain) and wished to withhold scientific information from them. This caused a certain amount of tension in Congress, the private sector, and the physics community. For example, Clifford Furnas, Chancellor of the University of Buffalo, suggested in testimony before the U.S. Committee on Government Operation in 1957 that fusion research did not have direct military applications but should be considered as research on the “general laws of nature.” Furnas claimed that “basic scientific information should hardly ever be classified” and suggested that greater progress would be made if the fusion project were declassified.¹¹ Representative Clare Hoffman (Republican-Michigan), a supporter of Strauss’s policies, countered by noting the many military uses of nuclear research and suggested that the release of information concerning “basic science” might help an enemy of the United States during wartime. After Furnas insisted, “But it helps us more than it helps them,” Hoffman shot back: “You mean we know less than they do?”¹² Despite Strauss’s and Hoffman’s concerns, after two years of mounting pressure from many in Congress, industry, and the scientific community to declassify fusion research, the AEC decided to feature fusion research at the Second International Conference on the Peaceful Uses of Atomic Energy in September 1958.

During the period of secrecy from 1951 to 1958, each of the three main U.S. fusion research groups competed with each other in attempting to achieve the practical goal of producing fusion power. Nonetheless, fusion scientists at the same time identified experimentally and analyzed theoretically a host of heretofore unknown plasma behaviors. One of their most important findings was the presence of plasma instabilities, the reason why the new machines failed to work as they had hoped. They presented their studies of plasma instabilities, which became more and more important during the 1950s, at secret scientific meetings.

The earliest work on plasma instabilities approached them from a magnetohydrodynamic (MHD) perspective – seeing the plasma as a sort of fluid of charged particles – which the Swedish astrophysicist Hannes Alfvén, motivated by his study of sunspots, had pioneered during the 1940s. The Princeton group now demonstrated particular strength in MHD, with some of their theoretical work growing out of studies of particular machines. For example, one of their earliest MHD analyses developed out of a collaboration between Martin Kruskal of Princeton and James Tuck of LANL. LANL’s pinch machine created moving plasmas that were constrained or “pinched” by a self-generated magnetic field. Kruskal and Tuck subjected LANL’s pinch machine to an MHD analysis and confirmed earlier Princeton results that the machine’s plasmas were unstable. Tuck hoped that an externally applied longitudinal magnetic field might suppress the instabilities, but Kruskal recognized that, although such a field would eliminate high-frequency plasma vibrations, low-frequency disturbances would remain unstable, which was confirmed by experiments at LANL in 1954.¹³ Perhaps the most important early accomplishment of the Princeton theorists was Ira Bernstein, Edward Frieman, Martin Kruskal, and Russell Kulsrud’s general study of plasma stability, which



Fig. 2. Plasma theorist Marshall N. Rosenbluth (1927–2003) of General Atomics sometime in the late 1950s. *Credit:* General Dynamics; courtesy of the American Institute of Physics Emilio Segrè Visual Archives.

was applicable to many different fusion machines. They used a variational technique to calculate energy changes resulting from small deviations of a particular plasma from its equilibrium state. To solve the MHD equations for the detailed time evolution of a particular equilibrium situation often presented intractable problems. Bernstein and his colleagues' variational approach merely tested for energy changes, but did not seek information on their detailed time evolution. Three years after they first presented it at secret meetings in 1955, they published their "energy principle," as it became known, in the open literature.¹⁴

Marshall N. Rosenbluth (figure 2) perhaps most embodied the push for greater rigor in plasma theory. In 1956, after working at Los Alamos for six years, he joined the private company General Atomics, which had begun a successful fusion program and eventually secured AEC support. In 1955, before leaving Los Alamos, instead of adopting the MHD perspective Rosenbluth analyzed LANL's pinch machine using a variational analysis of the orbits of individual particles. He found a specific prescription for the stability of the pinch that involved the application of a longitudinal magnetic field

with a sharp cutoff alongside the plasma. Initial experiments at LANL in 1956 seemed to confirm that Rosenbluth's prescription improved stability.¹⁵ His analysis remained secret until it appeared in 1957 in a paper that he coauthored with Conrad Longmire of LANL.¹⁶ They stressed that MHD was an unreliable approximation compared to a particle-orbit analysis. On the one hand, the particles in a normal fluid experience numerous collisions. On the other hand, the particles in a high-temperature plasma had such large mean-free paths that they did not collide with each another. They reviewed the analysis of specific plasma instabilities from a MHD perspective and showed that consideration of particle orbits yielded a more general and reliable treatment of them.

Statistical-Mechanical Analysis

Rosenbluth's work was also crucial in moving beyond both the MHD and particle-orbit perspectives and developing the statistical-mechanical analysis of plasmas. Some of his earliest work on kinetic theory was done while he visited the University of California at Berkeley during the 1954–1955 academic year and developed a theoretical interpretation of the mirror machine. The mirror machine was based upon a principle similar to that involved in the partial confinement of charged particles in Earth's magnetic field. Because the mirror was an open linear system, it was important to know how much plasma was lost at its ends, even in the absence of instabilities. To answer this question, Rosenbluth, along with William MacDonald and David Judd of the Lawrence Berkeley Laboratory, considered the Coulomb collisions among plasma particles, which altered their orbits and often scattered them out of the machine. The Boltzmann equation, the fundamental transport equation that describes a plasma as a collection of particles, contains an all-important term describing their interactions that had to be specified. Instead of setting the collision term in the Boltzmann equation to zero (yielding the so-called Vlasov equation), Rosenbluth and his collaborators set the collision term equal to the Coulomb-interaction term (yielding the Fokker-Planck equation).¹⁷

During the late 1950s, plasma theorists continued to use statistical mechanics to understand the relationship between plasmas as a fluid of strongly-interacting particles and plasmas as a collection of weakly or noninteracting particles. Geoffrey Chew, Marvin Goldberger, and Francis Low (CGL) did important work here at LANL in 1956. In the MHD approach, plasma pressure was modeled as a simple scalar quantity, so it was isotropic. CGL started from the Boltzmann equation and modeled plasma pressure as a more general tensor quantity. Although a nonscalar plasma pressure often contradicted MHD, the Los Alamos theorists identified conditions in which the Boltzmann equation yielded the MHD equations.¹⁸ In 1958, Rosenbluth and Norman Rostoker at General Atomics produced an influential study that compared and contrasted the MHD and CGL analyses from a more general perspective. They started from the Boltzmann equation but found macroscopic equations that were more general than those of either MHD or CGL, from which they could derive both. In the years to come, plasma theorists, with Rosenbluth the most preeminent, came to see the Boltzmann equation as the conceptual core of plasma physics.

Plasma Physics, Space Physics, Astrophysics, and Gaseous Electronics

Not all members of the nascent plasma-physics community began working in fusion research; many came from and remained connected to other plasma specialties. Indeed, from its earliest years fusion research intersected – both in terms of research topics and personnel – with plasma research in space physics, astrophysics, and gaseous electronics. This set the stage for the broader conception of plasma physics that developed during the 1960s.

For example, after Lyman Spitzer pitched his idea for the stellarator to the AEC in early 1952, the Commissioners suggested that Princeton should strengthen its plasma-physics program by hiring an experimentalist.²⁰ Spitzer then approached James Van Allen of the University of Iowa, and in his successful AEC proposal that May, Spitzer promised Van Allen's involvement as well as that of one of his Iowa graduate students.²¹ Van Allen worked at Princeton for three years, from 1952 to 1955, where he developed an interest in incorporating some of the Princeton work into his nascent space-physics research program, writing in his research notebook in September 1953:

A line of thought with respect to Iowa's role in future of thermo-nuclear work: conduct of physical experiments--conductive and other properties of ionized plasmas--checking of proposed astrophysical energy sources by laboratory tests--or at least measurement of relevant cross sections--ionospheric type measurements--checking ionospheric theoretical expressions--using microwave techniques, etc.²²

Years later, Van Allen reflected that his work at Princeton “introduced me to plasma physics” and “was very helpful in my later work in radiation belt and magnetospheric physics.”²³

Astrophysics also benefited from an association with fusion research. Between the late 1940s and middle 1950s, Lyman Spitzer, Richard Härm, and Martin Schwartzchild at Princeton University had made significant contributions to the study of astrophysical plasmas, especially interstellar gases. Then, after the declassification of fusion research in 1958, Spitzer wrote a proposal arguing that “Princeton University establish a program of graduate and postgraduate education in the broad field of plasma physics.” He stressed the need for a new generation of researchers who had a balanced background in plasmas: “At the present time very few scientists receive broad training in plasma physics or hydromagnetics. In looking for scientists trained for work at Project Matterhorn we have found virtually no people with any general background in this area.”²⁴ The Princeton team received the university's approval; its plasma-physics graduate program began operation in January 1960, becoming one of the first to be established in the country.

The field of gaseous electronics also had strong intersections with fusion research, but since the gaseous-electronics community studied relatively low-temperature plasmas involving complex atomic processes, fusion scientists, who generally studied fully-ionized gases, did not take as strong an interest here as they did in space or astrophysical plasmas. Relations between the fusion and gaseous-electronics communities were significant, but also exhibited signs of strain. William Allis, a pioneer in the study of



Fig. 3. Gaseous-electronics scientist Sanborn C. Brown (1913-1981) of the Massachusetts Institute of Technology. *Credit:* American Institute of Physics Emilio Segrè Visual Archives, Physics Today collection.

plasma waves, restarted his program of gaseous electronics at MIT, which had been interrupted by the war, in the late 1940s.²⁵ Allis and Sanborn C. Brown (figure 3) now sought to make their research, particularly on the development of plasma diagnostics using microwaves, relevant to the fusion-research community. Their style of research, however, which stressed methodical characterization of well-behaved plasmas, differed from that of the fusion-research community. Thus, in a talk at a classified fusion meeting in 1957, Brown declared that “all our previous work on the fundamentals of gas discharge behavior has convinced us that fundamental measurements should always be made from steady-state plasmas where any transient conditions which may exist have died out before we try to make measurements.”²⁶ Brown stressed much the same thing four year later when he wrote that MIT planned to start projects “which would be of interest in the thermonuclear-fusion field,” but only those that remained true to their established research program: “we are attempting to go from what we know about gas discharge physics to the regions that we would like to understand, without taking jumps over what we do not understand....”²⁷

A number of companies with expertise in gaseous electronics became involved in the fusion-research program, but often with limited success. In February 1954, Lewi Tonks and Willem Westendorp of General Electric (GE) participated in a Princeton study group to design the Model D stellarator, which was to be a reactor prototype. Then, in the spring of 1955, GE started its own fusion program at its research laboratory in Schenectady, New York. At first, GE concentrated on approaches that used its own expertise, with Guy Suits, GE's Director of Research, proposing a fusion device that was similar in operation to a mercury-arc rectifier.²⁸ In June 1957, however, GE started new work on a mainline fusion machine called the "theta pinch."²⁹ GE's effort ran into technical and financial problems in the early 1960s and, after unsuccessfully seeking funds from the AEC, was canceled in 1967.

During the years of secrecy of the 1950s, fusion scientists reported many of their results at gaseous-electronics conferences, but shortly after the declassification of fusion research in 1958, the fusion-research and gaseous-electronics communities largely segregated, especially after the Division of Plasma Physics (DPP) of the American Physical Society (APS) was founded in 1959. As Allis noted, "We hoped they [the fusion scientists] would continue [to meet] with us but in fact their work is mostly too specialized and they soon established their own conference."³⁰ Indeed, the Conference on Plasma Physics and Controlled Nuclear Fusion Research, sponsored by the International Atomic Energy Agency (IAEA) in Vienna in 1961, catered to the fusion community by design: The IAEA spoke of the need for "an international conference entirely devoted to work in plasma physics directed specifically to fusion research." Topics concerning "fundamental processes in gas discharges" were "excluded" from the conference. Gaseous-electronics researchers were asked instead to submit their work to the Fifth International Conference on Ionization Phenomena in Gases, which would meet one week earlier.³¹ Even GE's Lewi Tonks, whose work with Irving Langmuir was considered to be one of the foundations of plasma physics, experienced difficulty participating in the IAEA conference. Tonks had a contract from Lawrence Livermore Laboratory to develop a theoretical interpretation of its Astron machine, and after he requested funds from the AEC to enable him to attend the IAEA meeting, Arthur E. Ruark supported his request, declaring that "any international conference on plasma physics and controlled thermonuclear research without Lewi Tonks present would be something like Hamlet without the ghost, and without Hamlet."³² Nevertheless, the American planning committee for the IAEA conference rejected Tonks's paper, and Livermore informed him that it could not spare funds for his trip to Vienna.

The formation of the APS Division of Plasma Physics itself brought dissension, since other APS divisions had hoped to integrate plasma physicists into their ranks. APS Secretary Karl K. Darrow noted that both the Division of Electron Physics (which was loosely associated with the gaseous-electronics conferences) and the Division of Fluid Dynamics expressed "some feeling of grievance because of the formation of the Division of Plasma Physics." That came as a surprise to Darrow, because after conversations with a number of electron physicists he had concluded that "they would welcome a segregation of the plasma group." He also was perplexed that the Division of Fluid Dynamics would lay claim to the field of gaseous discharges, which was not mentioned in the division's By-laws.³³

During the years of secrecy in magnetic-confinement fusion research, significant new phenomena were observed and analyzed, prompted by the development of the new machines that were directed toward the goal of attaining fusion power. During this period other plasma-related communities developed alongside the fusion-research community and often interacted with it. In the coming decade of the 1960s, these other plasma specialties increased in influence and importance in defining the field of plasma physics, although, as we shall see, not without experiencing difficulties.

Properties and Phenomena: Basic Plasma Physics during the 1960s

After 1958 and into the 1960s fusion experiments continued to demonstrate disappointingly short confinement times, and to a significant degree the plasma-physics community turned away from the practical goal of attaining fusion power and toward the experimental identification and theoretical analysis of plasma waves and instabilities as the locus of their research. Plasma physicists thus deepened their commitment to a general study of the “fourth state of matter” and to what I have called a Properties and Phenomena conception of basic physics.

This change of emphasis is reflected in numerous review articles. Lawrence Livermore’s Richard F. Post, writing shortly after the declassification of fusion research in 1958, suggested that during the 1950s “the lure of the final objective was so great that some of the traditional scientific precautions were by-passed in hopes of leapfrogging into an early solution.” The difficulty of attaining fusion power led the community to “now see that a more fundamental attack on the problem ... would probably have put us farther along the way of understanding.”³⁴ In 1964 Post and Harold Furth, another Livermore physicist, wrote an internal AEC review that criticized the course of fusion research in the 1950s as having been dependent on “short-term technological pressure.” As a result, the plasma-physics community failed to allow sufficient time to interpret its experimental results theoretically so that “the importance of stability considerations was poorly understood.”³⁵ Post and Furth advocated that instead of the “technology-oriented research effort” of the 1950s, a more “physics-oriented research effort” should now be followed. That would not only pave the way to solving the fusion problem but also would have the benefit of producing “basic results of interest to the whole scientific community.” Characterizing plasma instabilities theoretically not only promised to solve the practical problem of attaining fusion power, which was “profoundly relevant to the welfare of mankind,” it also presented “the opportunity to solve a series of basic problems in the physics of matter.”³⁶

That fusion researchers should acknowledge basic plasma physics as a concept affected not just the fusion community, but also the broader discipline. A strengthened awareness began to emerge that plasma physics included a number of specialties in addition to fusion research – and that a loose sort of conceptual unity bound the plasma-physics community together. That is evident in the first overview of physics that was conducted by the National Research Council (NRC) of the National Academy of Sciences (NAS) and published in 1966. The Plasma Physics Panel stressed the scientific nature of plasma research, asserting that although plasma phenomena were analyzed with well-known classical equations (Maxwell’s equations, the hydrodynamic equation,

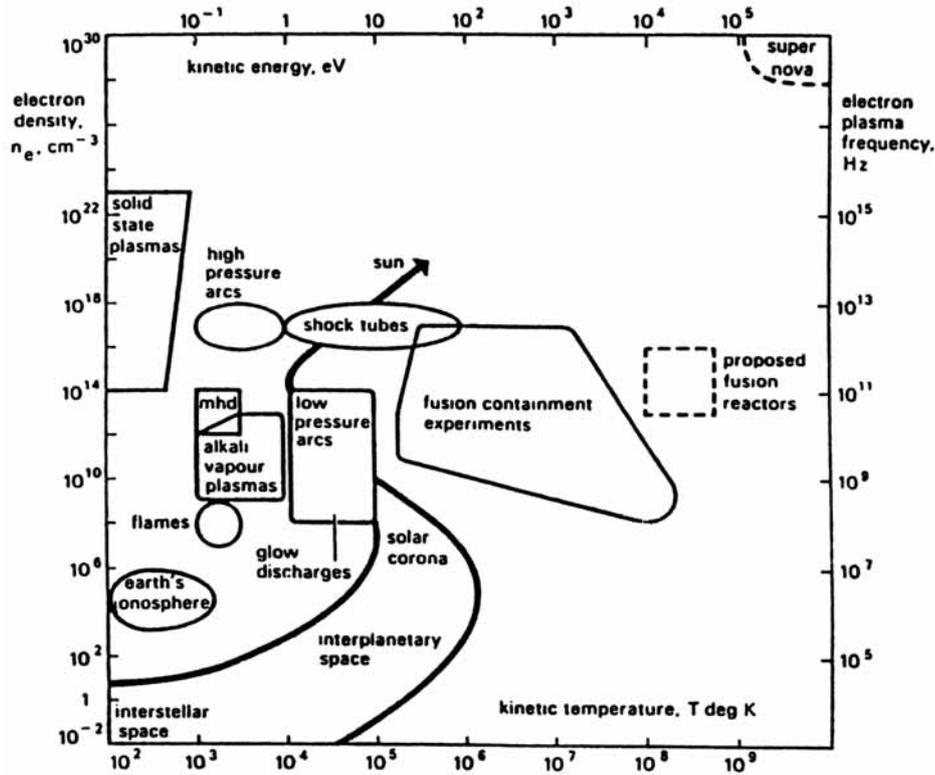


Fig. 4. A variant of the Kantrowitz-Petschek map, showing various plasma regimes, including those pertaining to space and astrophysical plasmas, gaseous electronics, and fusion research, against logarithmic scales of electron density and kinetic temperature. *Source:* Physics Survey Committee, *Physics in Perspective* (ref. 95), p. 219.

the Boltzmann equation), the new collective effects were so complex and poorly understood that “we feel it to be quite obvious that many plasma problems deserve to be considered as ‘pure’ physics.”³⁷ To provide an overview of the many areas of plasma research, the report opened with “the famous Kantrowitz-Petschek map” that Arthur Kantrowitz and Harry Petschek of the AVCO-Everett Research Laboratory had developed (figure 4). Their map depicted the different plasma regimes by plotting electron density against kinetic temperature;³⁸ variations of it became a staple of plasma-physics reviews and textbooks, giving a visual overview of all of the plasma-physics specialties. Although the Panel’s report stressed fusion research most, it also carefully discussed nearly every other plasma-related study, including space physics, astrophysics, gaseous electronics, plasma dynamics, and military applications.

The Panel’s treatment of plasma physics as a whole no doubt seemed especially attractive at a time when the results of fusion research were disappointing. At one of its meetings in August 1966, the AEC Standing Committee noted that the weak image

that controlled thermonuclear research (CTR) had in the general scientific community was connected to the lack of interspecialty work in plasma physics. As Lewis Branscomb commented, “controlled thermonuclear research can be considered only in a limited sense to be a research field. It is part of an overall field of plasma physics with contributions to and from other fields.” To improve the image of fusion research, he suggested that “the U.S. in-house laboratories should be given a broader charter and include a mix of astro-physics, etc.” Amasa Bishop, the AEC’s Assistant Director of CTR, suggested that plasma physics should be strengthened in universities by the AEC increasing its support for “basic plasma physics” and establishing “closer cooperation with astrophysics and space physics.” CTR also should make its presence known at APS meetings and at those of other scientific societies.³⁹

The way in which plasma specialties were joined together by a Properties and Phenomena conception of basic plasma physics was very different from the unification found in high-energy physics or cosmology. The grandest statement of the goal of theoretical physicists traditionally was to find “universal law embracing the whole of physical reality.”⁴⁰ Plasma physicists, however, never were committed to a reductionist program in the sense of finding Steven Weinberg’s “final laws of nature.”⁴¹ Instead, the plasma-physics community resembled the solid-state physics community, which in Spencer R. Weart’s analysis pursued a research program whose inherent diversity insured that it “could never be completed.”⁴²

A particularly strong unifying spirit in plasma physics can be found in the work of plasma theorists like Marshall Rosenbluth, who sought to clarify the limitations of MHD theory and to put plasma physics on a firmer conceptual foundation. Rosenbluth and Longmire, in their famous 1957 article on the stabilization of the pinch, ventured beyond their particle-orbit analysis to a statistical-mechanical approach and the Boltzmann equation “which, of course, knows all the answers.”⁴³ Four years later, at the first IAEA conference, Rosenbluth declared that “on the most fundamental level ... there appears to be almost universal acceptance of the Boltzmann equation as an adequate description of a plasma.”⁴⁴

Even for Rosenbluth, however, the goal of plasma theorists stopped well short of finding universal laws. As they labored to show how many different types of plasmas and plasma effects could be modeled and interrelated, they found that their interpretation required a diversity of plasma models, each of which embodied different physical assumptions and approximations, which to a considerable extent they acknowledged and embraced. New York University theorist Harold Grad (figure 5) typified this commitment to diversity. He opened his review of plasma physics in *Physics Today* in 1969 by noting that “the wealth of physical phenomena encountered in the plasma state exceeds the variety spanned by substances as diverse as air, water, peanut butter, and superfluid helium.” To Grad plasma physics shared with particle physics the need to “study unknown territory simply ‘because it is there,’” but because of the broad range of physical effects encompassed by plasma physics,

no evident single focus unites the subject other than our desire to discover what we can about ionized and conducting matter. Whether the conceptual unity hoped for in fundamental-particle physics will ever overtake plasma physics is doubtful.⁴⁵



Fig. 5. Plasma theorist Harold Grad (1923-1986) of the Courant Institute of Mathematical Sciences at New York University. *Credit:* American Institute of Physics Emilio Segrè Visual Archives, Physics Today collection.

Grad concluded that the goal of plasma physics as a “recognized academic discipline” was “not to find *one* theory of plasma behavior but to find very many theories of the behavior of many different plasmas.”⁴⁶

To many observers, both inside and outside of the plasma-physics community, one of the main problems confronting plasma physics in the 1960s was inadequate contact between theory and experiment. To Grad, this began as an empirical problem: To analyze a plasma effect theoretically, it first had to be isolated experimentally. But the complex geometries of many fusion experiments often involved plasma conditions that made it impossible to isolate single effects. Toroidal systems, such as Princeton’s stellarator, with their complex magnetic-field topologies, harbored many effects at once: “It appears more than likely that the reason we do not yet understand the limitations of toroidal confinement is that there are so many comparable competing effects, not that a single elusive effect remains to be discovered.”⁴⁷ Grad felt that by focusing on the complex fusion machines, both experimentalists and theorists had lost sight of their primary target, which was to isolate individual plasma effects.

Grad's view was reflected in much of the plasma experimentation of the 1960s, when relatively simple machines were designed to demonstrate basic plasma effects. One early example was in fusion research using the mirror machine. During the late 1950s, mirror machines in the U.S., Soviet Union, and Britain all produced extremely low plasma-confinement times. One mystery of the mirror's problems was solved by a Russian team headed by Mikhail Ioffe at the Kurchatov Institute in Moscow. As Ioffe reported at the first IAEA meeting in 1961, he and his team had shown that the mirror machine was susceptible to the "flute instability," a lateral displacement of the plasma in the confining magnetic field, and had found a way of eliminating it. Based upon an MHD analysis, Ioffe suggested that a cusped-shaped geometry should be adopted, in which the magnetic field assumed a convex shape toward the plasma and increased in magnitude in moving away from the center of the machine. When this "minimum-B" shape was produced by adding conducting bars to the Russian machine, the flute instability was suppressed. Richard Post often acknowledged Ioffe's accomplishment in his numerous review articles, citing the experimental bridge that Ioffe had established to theory as "a classic experiment in plasma physics."⁴⁸

The most significant work that exemplified the Properties and Phenomena conception of basic plasma physics was not done in fusion research. One of its pioneers was Francis F. Chen at the Princeton Plasma Physics Laboratory. After working on the stellarator for a few years, Chen became convinced that its confinement problems were too difficult to isolate and analyze because of its complex geometry. He therefore designed a new plasma source featuring a simple, straight geometry. This machine, dubbed L-1 (the L standing for linear), was built during 1959 and featured a reflex-arc (or thermionic) type of discharge, in which the cathode was heated by electron bombardment. The plasma column itself was six feet long and four inches in diameter. After gaining experience in the construction and operation of this machine, Chen obtained funding from the laboratory to construct the L-2, which was twice as long as the L-1, and which began operation in 1960.⁴⁹ Years later, Chen commented on his transition from fusion research to basic plasma experiments, saying that, "I believe that Lyman Spitzer never forgave me for forsaking the stellarator but at least he listened to reason."⁵⁰

The Q machine (figure 6), another type of machine that was designed to produce simple plasmas, was perhaps the most significant example of the trend toward fundamental plasma experiments. As they reported in 1960, Nathan Rynn and Nicola D'Angelo at Princeton built the first one,⁵¹ which created fully-ionized, quiescent plasmas (hence the Q prefix) confined to a simple, linear geometry. These fully-ionized plasmas were created by bringing beams of alkali metals, such as cesium and potassium (which have low ionization potentials), into direct contact with hot tungsten plates. These plasmas were of no interest for fusion reactions (since the alkali ions were not fusion fuels), but were of great use for identifying and analyzing plasma effects that hitherto had been either mixed together or swamped by larger effects. Seemingly echoing Sanborn Brown, Rynn characterized research with Q machines by saying, "if you want to study water waves, drop a pebble into a quiet puddle – not into the surf at the seashore."⁵² Or as Francis Chen put it: "Usually fusion reactors have such complicated geometries that it would be almost impossible to make careful analysis of what is going on. This is the rationale for having small, simple experiments."⁵³

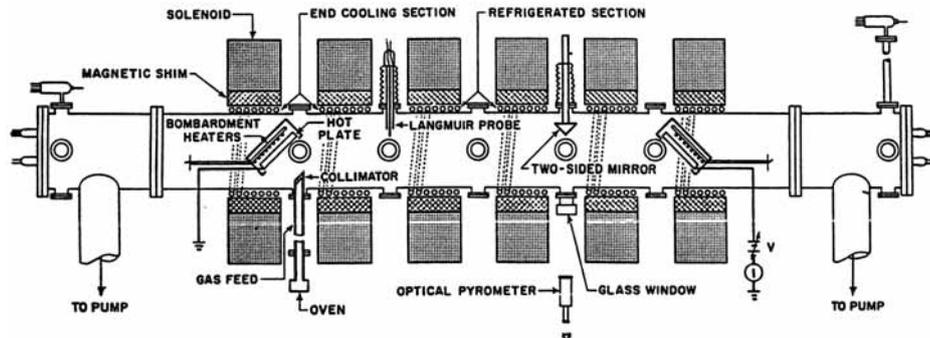


Fig. 6. Diagram of the Q machine, showing the solenoids that produce the axial magnetic-confinement field and the two hot tungsten plates mounted at 45 degrees for the production of alkali-metal plasmas. Source: N. Rynn, "Plasma Column End Effects," in Pardo and Robertson, *Plasma Instabilities and Anomalous Transport* (ref. 53), p. 107.

Chen and his team at Princeton accomplished one of the most important results on Q-machines, the suppression of "drift-wave instabilities." During 1965 Chen remodeled his reflex-arc machine as a Q-machine and soon found that by altering electric fields applied at the edge of a potassium plasma the L-2Q machine (as it was now designated) could be run in a mode where it was free of low-frequency disturbances. Chen suspected that these disturbances were caused by drift waves: Small differences in pressure at the edges of the plasma produced electric fields that caused the ions to drift across the magnetic-confinement field lines. Working with graduate student David Mosher, Chen added a magnetic field in the azimuthal direction – a so-called "magnetic shear" – by installing a water-cooled aluminum core in the L-2Q that was capable of carrying high currents. Mosher and Chen established that slight variations of temperature across the faces of the L-2Q's tungsten plates led to electric fields that were asymmetric with respect to the aluminum core. The magnetic shear served to twist the asymmetric electric fields into long spirals and thereby elongate the path over which the ions had to drift to leave the machine. From 1966 to 1969, Chen and Mosher showed that by varying the magnetic shear they could control the drift waves and achieve confinement times that were twice as long as the seemingly impenetrable boundary of so-called classical diffusion or Bohm diffusion.⁵⁴ Studies of "shear stabilization" such as Chen and Mosher's were regarded as fundamental contributions to basic plasma physics and years later even became important in fusion research.

Such nonfusion research had the advantage of being relatively inexpensive and thus could be carried out with moderate AEC support. One reason for the limited development of plasma-physics programs in universities had been the great expense of the high-temperature fusion experiments at the four major laboratories (Los Alamos, Livermore, Princeton, and a new program at Oak Ridge), which commanded the lion's share of AEC funds. An internal AEC review of fusion research in 1962 (the Abelson Committee) worried that the AEC had insufficient funds to support university programs: "Broadly speaking, existing University research on plasmas has been chosen

under a limiting condition, that such research must be done with relatively cold plasma.” The Committee recommended that the AEC request an additional budget item, of a half-million dollars, to “expand support of high-quality plasma research at universities” and hoped that this would eventually command as much as one-fourth of the AEC’s budget.⁵⁵

Although AEC support for university programs remained modest during the 1960s, basic plasma-physics research with relatively small machines using relatively low-temperature plasmas enjoyed a brief flowering at universities. For example, in 1966, the AEC supported a conference on plasma instabilities at the University of Miami that focused on small machines, including Q-machines, positive columns, and arc discharges. Of the thirty-nine participants, about half came from small plasma programs at schools such as the University of South Florida, the University of Maryland, Stevens Institute of Technology, Rensselaer Polytechnic Institute, and the University of Miami.⁵⁶

In addition to the rebirth of research with relatively low-temperature plasmas, basic plasma physics in the 1960s also included significant and influential efforts to address problems in space and astrophysical plasmas. One particularly good example here were those of Bruno Coppi, who before transferring to MIT in 1967 had published a number of theoretical papers on space and astrophysical problems, in addition to fusion research. One of his most significant contributions grew out of visits to the International Center of Theoretical Physics in Trieste, Italy. During one visit in the summer of 1965, Coppi, in collaboration with Guy Laval and René Pellat of Fontenay-Aux-Roses, enlarged on ideas that he had published earlier that year in *Nature* regarding a model of instabilities in Earth’s geomagnetic tail. After Eugene Parker’s hypothesis of a solar wind was confirmed in 1961, space theorists such as James Dungey and Ian Axford explored its implications for the macroscopic shape of the magnetosphere and found that, in the region behind Earth along its equatorial plane, the magnetic-field lines would be stretched out by the solar wind into a plasma sheet with a magnetic field of zero. They also suggested that this “neutral sheet” might demonstrate “magnetic-reconnection” effects in which the field lines repeatedly separate and reconnect in an altered field topology. Inspired in part by Norman Ness’s experimental confirmation of the neutral sheet in 1965, Coppi recognized that the geomagnetic neutral sheet behaved similarly to plasmas in pinch-fusion machines: The low-density geomagnetic tail, like the plasmas in these high-temperature fusion machines, would be fully ionized. Coppi, Laval, and Pellat therefore analyzed the neutral sheet using the collisionless Boltzmann equation (the Vlasov equation) to model collective plasma effects that were not based upon classical collisions. Their model predicted that magnetic reconnection did indeed occur in the geomagnetic tail, as the result of a class of plasma instabilities called tearing-mode instabilities.⁵⁷

Individual scientists also took the initiative to showcase space and astrophysical plasmas at APS meetings. In April 1965, Stanford astrophysicist Peter Sturrock wrote to Lyman Spitzer suggesting that they organize a special session on astrophysical plasmas at the 1965 annual meeting of the APS Division of Plasma Physics (DPP). Sturrock felt that such a session would attract the interest of the DPP community and encourage astronomers and astrophysicists to report their results at APS meetings in addition to those of the American Astronomical Society (AAS). Spitzer responded enthusiastically.

cally: “Here at Princeton I have been trying to bring plasma physicists and astrophysicists close together and I believe that both groups have much to gain from an interchange of ideas.” Further, Spitzer suggested that magnetospheric researchers also should be invited, since they “do not regard themselves as astronomers but rather more as geophysicists.”⁵⁸ The ensuing joint AAS-DPP session at the 1965 DPP meeting was the beginning of a relatively small but significant DPP interest in astrophysical and space plasmas: During the late 1960s and early 1970s, each DPP annual meeting featured one special session of about 14 papers on astrophysical and space-physics topics (accounting for about 4% of the papers presented).⁵⁹

Extreme Plasma Properties and Phenomena: The Rise of Fusion Research

During the late 1960s and early 1970s, a series of political and technical developments profoundly redefined the research program of the plasma-physics community and the funding commitments of its patrons. The political developments returned fusion research to the center of public attention. One was the breakdown of the American postwar consensus on science policy, as criticism of science and its relationship to the federal government arose and increased owing to the Vietnam War, the nuclear-arms race, and the antinuclear movement. Another was the energy crisis of the 1970s, which asserted the need for new energy sources. Low oil prices had played a significant role in global economic expansion, but these ended after the Organization of Petroleum Exporting Countries began increasing its prices in 1970. These developments, along with the burgeoning environmental movement, encouraged hope in both the plasma-physics community and the general public that peacetime fusion research would yield a relatively clean energy source and lessen the U.S. dependence on oil. The decade from 1972 to 1981 proved to be a kind of Golden Age of fusion. The federal government’s annual funding for magnetically confined fusion began at around \$30 million in 1972 and reached \$410 million in 1981. Similarly, inertial-confinement fusion was funded at \$20 million in 1972 and peaked at \$210 million in 1981.⁶⁰

Magnetic-Confinement and Inertial-Confinement Machines

Work on two new types of fusion machines capitalized on these changes in the political and cultural milieu. The first machine, the tokamak magnetic-confinement device, was proposed by Russian physicists Andrei Sakharov and Igor Tamm in 1950 (though it was given the name tokamak seven years later). Like Spitzer’s stellarator, Sakharov and Tamm’s tokamak was a closed toroidal device, but unlike the stellarator, the tokamak used a helically-shaped magnetic field produced by a combination of a strong longitudinal magnetic field and a relatively weak poloidal magnetic field.⁶¹ The international fusion-research community took little notice of the tokamak until the third IAEA conference in Novosibirsk in August 1968, when a Russian team from the Kurchatov Institute in Moscow reported excellent results. Lev Artsimovich concluded his report by quoting temperatures and plasma-confinement times that were ten times better than what the U.S. fusion community had found up to that time.⁶² A team of British

scientists checked the Russian results during the following year. Amasa Bishop predicted that:

if the British measurements are fully confirmed, the Soviet Tokamak results may be properly referred to as a “major breakthrough” in the CTR [controlled thermonuclear research] program. The door would then be open to the rapid development of advanced Tokamak systems and to demonstrating the scientific feasibility of controlled fusion on a much shorter time scale than hitherto envisioned.⁶³

Bishop certainly proved to be correct about rapid development. By 1971 large tokamak efforts were underway in the U.S. at Princeton, MIT, and Oak Ridge – and around the world. At the November 1970 DPP meeting, two out of forty-five sessions concerned the tokamak, accounting for five percent of the papers presented (24 out of 520).⁶⁴ Three years later, the DPP meeting devoted seven out of its fifty-four sessions exclusively to the tokamak, accounting for twelve percent of the papers presented (89 out of 715).⁶⁵

Inertial-confinement fusion (ICF) was the basis for the second new type of machine. During the late 1950s, it had become evident that high-power lasers, if they could be developed, might be trained on small targets of nuclear fuel, containing deuterium and tritium, to create miniature nuclear explosions in the laboratory. If a laser deposited its energy on the surface of a pellet containing nuclear fuel quickly enough, then its outer surface would ablate away with great energy. The ablation of its outer layers, like an accelerating rocket, pushed the pellet’s contents inward, thereby compressing and heating the nuclear fuel, and for a brief instant its inertia prevented the pellet from disassembling, during which researchers sought to ignite a thermonuclear reaction. Such experiments were of great interest not only to civilian but also to military fusion researchers, and shortly after Theodore Maiman demonstrated the laser in 1960, large ICF efforts were underway in the U.S., the Soviet Union, and France. Most of these governmental programs were highly classified.

The story of ICF secret research and its sudden declassification in 1972 has close parallels to magnetic-confinement research before 1958. Laser-fusion research, like magnetic-confinement fusion research, yielded insights into basic physical behaviors. In the case of ICF, these concerned plasma-laser interactions. The primary governmental motivations that kept money flowing, however, were the possibilities of achieving controlled thermonuclear power and military applications. During the late 1960s, the ICF community held its own secret scientific conferences, which required its participants to obtain security clearance. Regarding a topical conference on laser plasmas sponsored by the AEC in February 1969, Amasa Bishop wrote to U.S. Army Lieutenant Colonel Jack Rosen that “while convened primarily for CTR purposes, this group also will keep in mind some non-CTR potential applications of which you are well aware.”⁶⁶

During the 1960s American universities and industries took an interest in developing their own ICF programs, but the need for security was often seen as a hindrance. The AEC’s most significant university laser-fusion program was an unclassified one at the University of Rochester. When the AEC sought to reassert restrictions on the dissemination of information on laser fusion in 1970, Herman Postma of Oak Ridge, who

had close ties to the Rochester program, complained that classification would hinder university research and that “continued research by outstanding people in universities should remain active in order to maintain vigor and expertise within government laboratories.” Postma also warned that secrecy would hurt the public perception of fusion research, noting the danger of “today’s charged campuses,” and that “the AEC might avoid unfortunate publicity [as had occurred] in having ‘secret’ status applied to its CTR work.”⁶⁷

The American ICF program was declassified with a short paper that John Nuckolls, Lowell Wood, Albert Thiessen, and George Zimmerman of Lawrence Livermore Laboratory published in the September 15, 1972, issue of *Nature*.⁶⁸ Subsequently, many laser-plasma physicists joined the APS Division of Plasma Physics and flooded into its open scientific meetings. Between 1965 and 1971, the DPP annual meetings included one or two sessions on laser plasmas, usually featuring papers by members of relatively small, unclassified projects. The few papers presented by laser-plasma physicists at national laboratories did not report technical details but general physical effects and plasma theory. Just one year after declassification, at the DPP annual meeting in November 1973, nine out of fifty-four sessions were devoted directly to laser-plasma physics, accounting for sixteen percent of the papers presented (116 out of 715).⁶⁹

In response to the substantial political and technical sea changes between 1968 and 1972, many in the plasma-physics community changed or modified the direction of their research. Relatively low-temperature plasma programs at smaller universities tended to suffer. For example, Harry S. Robertson, one of the organizers of the 1966 symposium on plasma instabilities at the University of Miami, began new research on theoretical statistical mechanics and abandoned plasma physics altogether.

The work was progressing nicely, at which time almost all funding for university plasma research was cut to zero by the AEC.... So, without funding for what was a relatively large project, for a university, I was forced to abandon this research.⁷⁰

Interspecialty work also diminished. When Bruno Coppi (figure 7) moved to MIT in 1967, he intended to work in plasma astrophysics,⁷¹ but soon decided to submit a proposal to the AEC for his own design of a tokamak that he called the Alcator. Still, he cleverly avoided dropping his astrophysical interests altogether by building them into his proposal, arguing that the Alcator, which featured high magnetic fields, would enable him to pursue both fusion research and astrophysical studies such as that of synchrotron radiation.⁷² Nevertheless, his astrophysical research decreased. Between 1964 and 1973 he published an average of nearly two astrophysics-related papers per year (accounting for nearly 20% of his output), but between 1974 and 1980, when he worked intensely on fusion research, he published on average less than one astrophysics-related paper per year.⁷³

Many fusion researchers came to feel that aspects of basic plasma physics they had embraced a decade earlier now should be rejected. They were encouraged to some extent by administrators like AEC’s Robert L. Hirsch, who declared that fusion research should become more practical: “Don’t play around with idealized systems any longer than you absolutely have to. Get to work on the real problems as fast as you

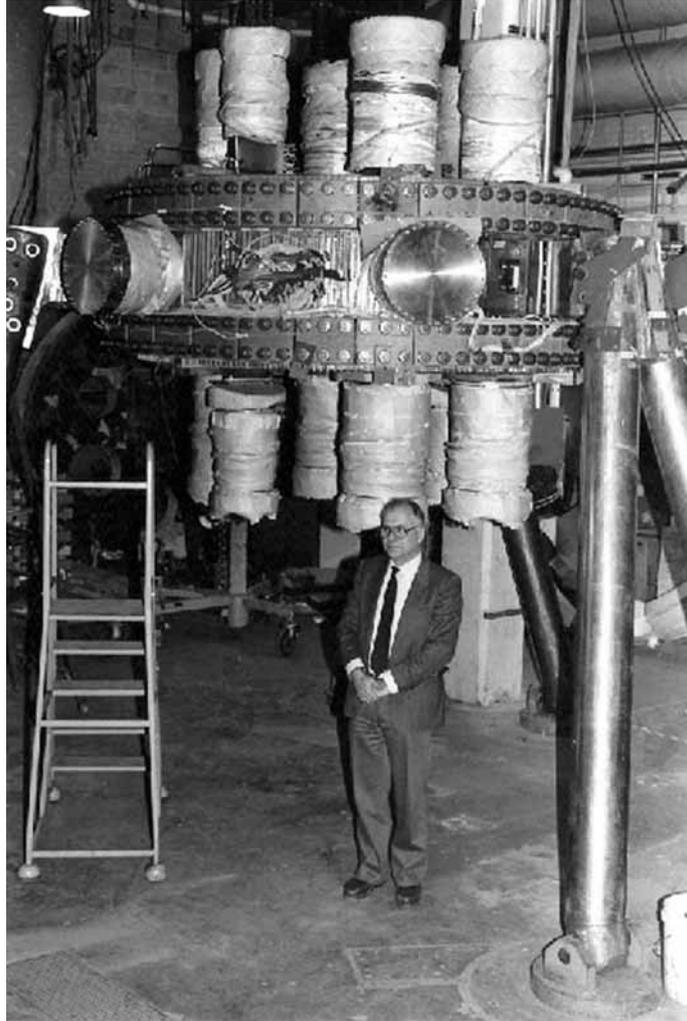


Fig. 7. Bruno Coppi (b. 1935) with the Alcator C tokamak at the Massachusetts Institute of Technology around 1980. *Credit:* Courtesy of Bruno Coppi.

can.”⁷⁴ Princeton physicist Harold Furth agreed with Hirsch, now claiming that the paper he and Richard Post had published in 1964, in which they had argued for fundamental plasma research, was wrongheaded: “The Russians never learned the basics. No one yet understands how tokamaks work. This proved no impediment to them getting on with the job.”⁷⁵

It would not be correct to conclude, however, that the fusion community abandoned basic plasma physics. Instead, many plasma physicists redirected what I have called the Properties and Phenomena conception of basic plasma physics back to the

idea that high-temperature research defined its frontier. Thus, Richard Post, in a 1970 review article, repeated his earlier sentiment that fusion machines, with their high-temperature plasmas, uncovered the most extreme plasma behaviors, which in turn motivated further developments in plasma theory.⁷⁶ In general, the forefront of fusion research shifted back to the high-temperature and relatively high-pressure section of the Kantrowitz-Petschek map, usually labeled something like “proposed fusion reactors,” which required ever larger machines to investigate ever more extreme plasma conditions. Inertial-confinement fusion embodied a strong element of this shift, as shown in an AEC review of the laser-fusion program in April 1974. In discussing programmatic priorities, the review stressed the importance of developing higher-power lasers and pursuing careful empirical studies, to be followed by theoretical modeling of the coupling between intense electromagnetic fields and highly unstable, expanding plasmas.

The largest element of the laser-fusion program is development of high power lasers for light-matter interaction study.... Basic light-plasma interaction measurements are needed to allow advances in theory and the design of high energy experiments which will demonstrate the feasibility of laser-initiated thermonuclear burn.⁷⁷

In sum, the plasma-physics community did not abandon the idea of investigating fundamental plasma behavior, but it did shift its attention to the extreme plasma conditions that were produced by the new fusion machines. This was a sort of Extreme Plasma Properties and Phenomena conception of basic plasma physics, which fusion researchers adopted as a way to have their cake and eat it too.

Computer Simulations

Interest in extreme plasmas was reinforced by the growing availability of computers during the late 1960s and early 1970s, making it possible for theorists to grapple with greater levels of complexity. Fusion researchers had always been interested in computer simulation, with specific machines and physical problems providing special encouragement. One such catalyst was the study of nonlinear plasma behavior: Oscar Buneman made one of its first computer simulations at the University of Cambridge in the late 1950s in a study of the interpenetration of two ion beams. His results provided the first examples of a class of instabilities later known as “two-stream instabilities”; his paper of 1959 on the subject led many to acknowledge him as a “founding father of the particle simulation of plasmas.”⁷⁸ John Dawson at Princeton University became interested in nonlinear problems partly through Buneman’s work; in 1960 he began a long and rewarding engagement with the computer simulation of two-stream instabilities and ion waves. He made these calculations with a computational method that became known as the “particle in cell” (PIC) approach, in which the continuum of space is approximated by a grid of discrete points.⁷⁹ His earliest codes were one-dimensional, but later algorithms extended them to two and three dimensions. One of his graduate students, Bruce Langdon, later improved the resolution of PIC simulations: During the early 1970s, after moving to Livermore, he and Buneman collaborated on the use of

high-order derivatives to calculate the value of model functions between the PIC grid points.⁸⁰

In 1966 the organizers of one of the earliest seminars devoted to nonlinear plasma theory noted that “the arrival of high-speed computers has led to a remarkable expansion in this field.”⁸¹ Two years later (and a month after the DPP held its first session devoted to computer simulations) the AEC Standing Committee, in an executive session, discussed the increasing importance of computer simulations to the fusion program and the problem of rising computing costs. The laser-plasma physicist Keith Brueckner spoke of “a change of approach, i.e., from linear theory to simulation,” and asserted that “simulation is a vital aspect” of the fusion program.⁸² By 1970 the organizers of the fourth annual Conference on Numerical Simulation of Plasmas could claim that the field had reached “a highly sophisticated degree of maturity and won the acceptance of the general Plasma Physics Community.”⁸³

The ICF community made especially extensive use of computer simulation, since it routinely modeled laser-plasma conditions that were far from equilibrium or linear behavior. During the early 1970s, Livermore’s George Zimmerman wrote the LASNEX computer program, which was based upon earlier efforts at Livermore on the computer simulation of nuclear weapons (once again, highlighting the connections between military applications and plasma research).⁸⁴ In 1974 Livermore was chosen as the home of the CTR Computing Center, serving the entire U.S. fusion program. The center was opened in March, with the AEC’s Division of CTR taking responsibility for doling out time to its various laboratories.⁸⁵

Messianic Fusion

The rise of the Extreme Plasma Properties and Phenomena conception of basic plasma physics and the computing revolution was accompanied by what we might call “messianic fusion.” Reflecting on the early fusion program in his memoirs, Lewis Strauss enthused that: “Out of our laboratories may come a discovery as important as the Promethean taming of fire.”⁸⁶ Richard Post used Strauss’s image in a number of talks in the late 1970s and early 1980s. To Post, the story of Prometheus was symbolic of “mankind’s quest for sources of energy, a quest that goes back as far in time as we humans have been on this planet.” Fusion research was just the most recent example of this quest, which held out hope for “an energy source that has fuel reserves sufficient to take care of human needs for all time.”⁸⁷ Such enthusiasm also was evident in Congress. Representative Mike McCormack (Democrat-Washington), a member of the Joint Commission on Atomic Energy (JCAE) since 1972, proposed the Magnetic Fusion Engineering Act of 1980, which included significant increases in funding for fusion research. The bill asserted that “the energy crisis can only be solved by firm and decisive action by the Federal Government” and made sweeping speculations about the future, predicting that fusion research, if successful, would “initiate a new era of energy abundance for all mankind forever” and “ultimately reduce the pressures for international strife by providing access to energy abundance for all nations.”⁸⁸

New Knowledge from Fusion Machines

Despite the renewed strengthening of fusion research and the weakening of low-temperature work, a number of plasma physicists continued to pursue a relatively broad conception of basic plasma physics. The work on the fusion machines usually fit the category of Extreme Plasma research, but there were notable exceptions. One pertained to a class of plasma instabilities in the mirror machine called “microinstabilities.” During late 1964, low-frequency MHD-type instabilities were largely suppressed in Livermore’s 2X mirror machine through the use of Ioffe’s minimum-B configuration, but radio-frequency turbulence was then detected. Richard Post searched for new ways that plasma might be lost through the velocity “loss-cone” at each end of the machine. He and Marshall Rosenbluth analyzed microinstabilities based upon the physical mechanism underlying so-called “Landau damping,” the interaction of plasma waves and individual plasma particles. In 1966 they wrote a paper in which they identified one of the most important of the “loss-cone modes,” the Drift Cyclotron Loss Cone (DCLC) mode, in which plasma particles are scattered from a plasma wave into the loss cone, when the wave oscillates close to the cyclotron frequency of the plasma ions.⁸⁹ Post later proposed a solution to the DCLC instability that involved the passage of a warm plasma (at a lower temperature than the fusion plasma) through the machine. Remarkably, it took nearly eight years to isolate the DCLC instability. During 1975 a later variant of Livermore’s mirror machine, the 2XIIB, gave poor confinement results, with telltale radio-frequency plasma oscillations observed near the ion-cyclotron frequency. Suspecting the presence of Post and Rosenbluth’s DCLC, the Livermore team tried “warm plasma stabilization,” which improved the machine’s confinement time by a factor of ten. This success motivated Livermore to mount a large effort on the theoretical analysis of the DCLC and its suppression.⁹⁰

The mirror machine in a sense was seen as being intermediate between the complex tokamaks and the relatively well-behaved Q machines. It demonstrated the significance of fusion research in producing new plasma conditions and met the preference of some scientists for relatively simple machines that were relatively susceptible to analysis. Post stressed in his numerous review articles that the theoretical prediction of the DCLC microinstability and its experimental confirmation eight years later was an example of the improving relationship between plasma theory and experiment, and that the mirror machine was an exemplar of such improvement. Years later, in his history of plasma physics, Post went further and criticized the reliance of the plasma-physics community on the tokamak. Compared to the mirror machine, Post felt that the tokamak had proved “to be intractable to detailed theoretical analysis.”⁹¹

Other physicists who had worked in basic plasma physics found new avenues for their research using the new machines. One example was Barrett H. Ripin, who did his doctoral research at the University in Maryland in the late 1960s, focusing on basic plasma effects, particularly nonlinear plasma echoes. Then, after a brief stint at the Princeton Plasma Physics Laboratory, he accepted an adjunct assistant professorship at the University of California at Los Angeles (UCLA) where he continued to work on basic plasma effects, studying the excitation of drift-cyclotron wave instabilities when beams of hydrogen ions pass through quiescent background plasmas. Years later, he

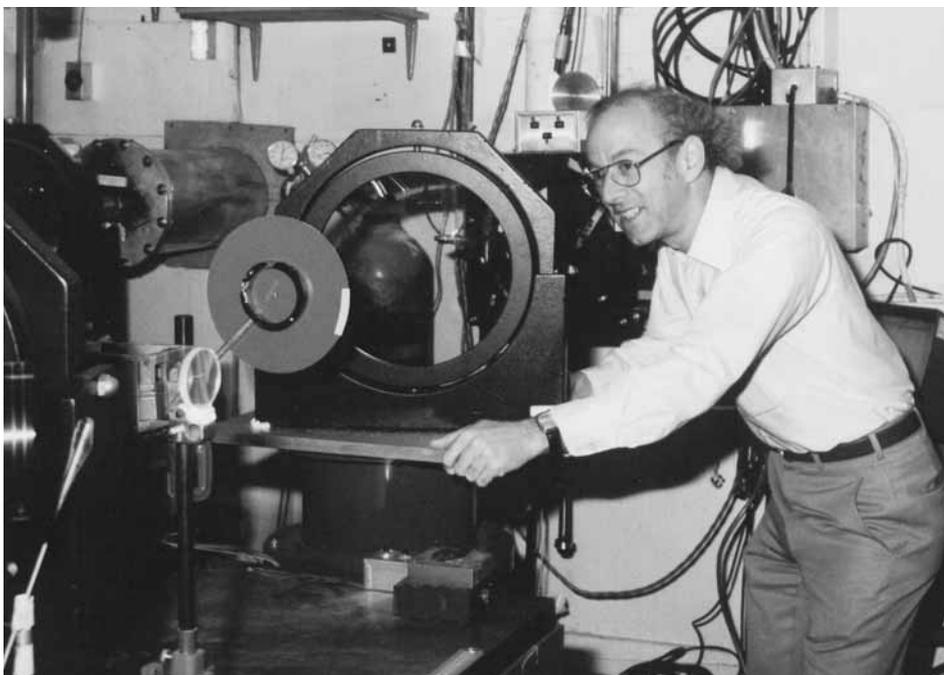


Fig. 8. Barrett H. Ripin (b. 1942) at the Naval Research Laboratory around 1980, shown measuring the focal length of the Pharos laser. *Credit:* Courtesy of Barrett H. Ripin.

recalled that while the national laboratories concentrated on fusion work, the universities were the only place to do basic plasma research. Perhaps echoing DuBridg, Ripin also recalled that university research was funded by the “droppings off the table” of the federal budget.⁹²

Ripin (figure 8) then moved to the Naval Research Laboratory (NRL) in Washington, D.C., in 1973, joining a group working on ICF, where he had access to machines that were not available at universities, such as NRL’s Pharos neodymium-glass laser. Ripin’s research on laser-produced plasmas over the next decade uncovered a number of fundamental plasma phenomena, including two of particular importance to ICF: an instability that caused backscatter of laser light, thus depleting its energy, and the ablative acceleration of thin foils of relatively simple geometries (instead of the spherical pellets used by the ICF community), studying their efficiency and stability. Ripin’s NRL group won funding from the Defense Nuclear Agency in the early 1980s to study aspects of high-altitude nuclear effects in laboratory experiments. Even though these were originally motivated by military testing, Ripin’s group “stumbled over lots of interesting things.” For example, they found that the same rules to scale a one-centimeter laboratory experiment to a 1000-kilometer nuclear explosion could be used to scale to astrophysical plasmas such as solar plasmas and supernovae.⁹³ Not surprisingly, Ripin became convinced that NRL’s applied-fusion program did not impede his

commitment to basic plasma physics, but supported it by opening up previously inaccessible plasma-parameter regimes.

The Weakness of Basic Plasma Physics and the Failure of Fusion

Despite the successes of fusion research, new laboratory measurements, and further space and astrophysical-plasma work, it seems that the general scientific community never took the idea of “basic plasma physics,” in either its 1960s or 1970s variants, seriously. This increased the vulnerability of the U.S. plasma-physics community to pressures on fusion funding and, later, to the new political and economic changes of the early 1980s.

Marshall Rosenbluth, while completing the plasma-physics committee’s first NRC report in 1964, wrote to Arthur E. Ruark, head of the AEC’s Controlled Thermonuclear Research (CTR) Division, noting the committee’s concern that plasma physics had “not yet attained respectability in the eyes of those working outside the field,” and therefore that “the primary objective of this report was to stress the interesting scientific questions in the field.”⁹⁴ This situation had not improved by the time of the second NRC report in 1972. Not surprisingly, the Plasma and Fluid Physics Committee now primarily stressed fusion research, and treated relatively briefly lower-temperature plasmas and space and astrophysical plasmas.

The balance of the 1972 NRC report is highly informative about how the general physics community judged plasma physics relative to other subfields of physics. The Physics Survey Committee conducted a “jury rating” of the subfields in terms of both intrinsic merit (“man’s understanding of his world or universe”) and extrinsic merit (“the technological opportunities arising from science”).⁹⁵ The Committee’s characterization of intrinsic merit was close to what I have called the Big Questions conception of basic physics: One group of subfields, elementary-particle physics, astrophysics, and relativity, scored highest in intrinsic merit and low in extrinsic merit. A second group, condensed-matter, nuclear, and atomic-molecular-electron physics, scored about equally in intrinsic and extrinsic merit. A third group, plasma physics, acoustics, and optics, was judged to be primarily of extrinsic merit. Although certain plasma-physics programs scored fairly high in the intrinsic-merit category of “ripeness for exploration,” they scored quite low on those for “potential for discovery of fundamental laws” and “significance of questions addressed.” Especially clear was the contrast between plasma physics and elementary-particle physics. Plasma physics scored as high as any subfield for its “contribution to national defense,” while elementary-particle physics barely registered here. Plasma physics, however, scored among the lowest subfields for its “potential for discovery of fundamental laws,” while elementary-particle physics was rated the highest of all subfields.⁹⁶

Similarly, it seems that neither the U.S. Congress nor the AEC administration of the 1970s understood or appreciated the plasma-physics community’s conception of basic physics. Instead, they waited for positive results in fusion research, hoping to improve America’s advantage in scientific prestige and military prowess during the Cold War. We can observe this by comparing the reactions of the Congress and AEC administration to developments in the magnetic-fusion program before and after the appearance

of the tokamak. When Amasa Bishop became Assistant Director of the AEC's CTR Division in 1966, the fusion program had shown little in the way of success. He therefore faced an uphill battle in convincing the Joint Commission on Atomic Energy (JCAE) to maintain or increase the budget for fusion research. At one of its meetings in early 1966, Senator John Pastore (Democrat-Rhode Island) complained to Bishop that the results in fusion research had not been impressive during the early 1960s and that this "has been more or less a let down to some, including myself." Chairman Chet Holifield (Democrat-California) asked: "Do you believe that the progress made, and will you tell us what progress has been made, justifies the continuation of this program?" Bishop then launched into an arcane discussion of the program's efforts to reduce plasma impurities, raise temperatures, and increase confinement times in the fusion machines. After Bishop's presentation of what he must have felt was significant progress, Representative Holifield asked, "what actual tangible progress have you made on those three points?" When Bishop attempted to answer, Pastore stopped him, saying "you lost me a half hour ago, Doctor."⁹⁷

Congress, however, had no difficulty in hearing the message that the Russian program in fusion research might be ahead of the U.S. program. The JCAE took immediate note of the Russian tokamak results, in part because these breakthroughs appeared in trade magazines and the popular press. At the JCAE hearings for fiscal year 1970, Representative Craig Hosmer (Republican-California) focused on a statement that Lev Artsimovich had made as reported in *Nucleonics Week*: "In the logarithmic scale we have traversed one-half of the road to the thermonuclear El Dorado of abundant energy."⁹⁸ Bishop explained that Artsimovich's remark meant that the Russian tokamaks had attained improvements in confinement time that were a factor of 80 longer than the "Bohm time" (the disappointingly short confinement time that was a consequence of various forms of anomalous plasma diffusion). Bishop also noted that there were relatively modest improvements in confinement time in the U.S. program, which confirmed that the Bohm time apparently was not the presumed upper limit, and that the Soviets were much closer than the Americans in achieving the practical goal of fusion power. That led Representative Hosmer to remark on "the sorry state of the escalation of our budget."⁹⁹

Robert L. Hirsch's stewardship of the AEC during the early 1970s stood in contrast to Bishop's both in terms of his style of management and of his presentation of the status of fusion research. Rather than speak to members of Congress about difficult details of fusion research that they could not understand, Hirsch concentrated on tangible progress, simply pointing out that the numbers were improving. At the JCAE hearings for fiscal year 1971, Hosmer, referring to the discussion of the previous year, asked Hirsch to speculate on "how far along we are on the road to the El Dorado of limitless, almost free electric energy?" Hirsch then defended Bishop's earlier predictions and the shifts that had occurred in the AEC program as a result of the tokamak breakthroughs. Whereas Bishop had suggested that the scientific feasibility of fusion power would be demonstrated by 1978, Hirsch suggested that it "could be much sooner."¹⁰⁰

Science journalist Robin Herman concluded, after interviewing a number of fusion scientists, that during the 1970s Hirsch had taught them that "the people who held the

pursestrings in Washington were now in charge.”¹⁰¹ While many fusion scientists were concerned that Hirsch was pushing the fusion program too hard (much as Strauss had in the 1950s), they came to accept the challenges he laid out for them. Their willingness to define and judge plasma physics in terms of fusion research, however, proved to be a Faustian bargain. Hirsch’s successors as directors of the AEC CTR research program (Erwin Kintner and John Clarke) continued to stress technical progress and program milestones throughout the late 1970s and early 1980s. Years later, however, plasma physicist Norman Rostoker felt that this approach made it difficult for the fusion-research community to alter its program. Once committed to the tokamak, it was difficult to turn back:

You don’t get large sums of money without, not only making large promises, but also conveying a sense in the whole community that you are on the right track. You can’t tell them “I guess it wasn’t quite the right track; we should take a different direction.” The chances that the money will disappear – when it’s handled the way it is by Congress – are quite good. So changing course is terribly risky.¹⁰²

To avoid the perception that they wished to change course, Rostoker felt that fusion scientists had to “sing in harmony,”¹⁰³ both with each other and with the U.S. Department of Energy, to maintain their budget. For magnetic-confinement research this led to a weeding out of machine concepts in favor of the tokamak. Princeton’s stellarator was the first fusion machine that was eliminated, in 1969, when the laboratory turned to a tokamak. Los Alamos’s Scyllac was the next major machine that was eliminated, in 1976, in the face of rising costs and technical problems.

Financial pressure became even more serious during the 1980s, when funding for magnetic-confinement fusion research began to decline. Enthusiasm for nuclear power had dwindled, as had concern about rising oil costs. This precarious situation was made worse in 1983, when Lawrence Lidsky of MIT’s Plasma Fusion Center sounded an exceedingly sour note in the fusion choir by publishing an article in the MIT *Technology Review* that criticized the U.S. fusion program in general and the tokamak in particular. He began by acknowledging the scientific and technical strides in the U.S. fusion program, but then went on to claim that any fusion reactor stemming from it would be too complex to have any hope of economic viability: “The costly fusion reactor is in danger of joining the ranks of other technical ‘triumphs’ such as the zeppelin, the supersonic transport, and the fission breeder reactor that turned out to be unwanted and unused.”¹⁰⁴ In 1986, faced with declining budgets, the U.S. Department of Energy cancelled Livermore’s enormous project, the MFTF-B mirror machine. Only inertial-confinement fusion research, by virtue of its direct military applications, was able to escape most of the economic contractions of the 1980s. In sum, the plasma-physics community, after nurturing a conception of basic plasma physics in the 1960s that was difficult to communicate to the general physics community and to its representatives in Congress, embraced fusion research during the 1970s – and lost big.

Basic Plasma Physics: An Unresolved Tension

Bruno Coppi, speaking at the Erice conference on alternative fusion concepts in 1981,

regretted that the fusion community had not argued its case for support more like the high-energy physics community had:

I think that the usefulness of doing the fusion work lies in learning what we don't know. Now the trouble is that somehow the fusion community has deceived the governments, in letting the governments expect practical results in a short time. We should have gone about our lack of funding as the "elementary particle" or "astrophysics" people do. In fact, they have proved that it is possible to get money and make a decent living without deceiving.¹⁰⁵

My study suggests that Coppi's preference, or hope, was rarely a practical option for the plasma-physics community. The conception of basic physics developed by plasma physicists never attained the cultural cachet of that touted by high-energy physicists and cosmologists – the Big Questions conception – with policymakers, AEC administrators, the general physics community, and the general public. Moreover, their Properties and Phenomena conception was periodically redirected to the related Extreme Plasma conception by dint of the strength and size of the fusion program.

The weakness of a Properties and Phenomena conception of basic physics is evident even in popularizations written by scientists themselves. Steven Weinberg in his book, *Dreams of a Final Theory*, intervened publicly in support of the Superconducting Supercollider by claiming that high-energy physics seeks "the *principles* that govern everything" and that these principles necessarily will be elegant: "It is when we study truly fundamental problems that we expect to find beautiful answers."¹⁰⁶ By contrast, plasma physicists have never been able to claim a reductionist or elegant plasma theory as their goal. Kenneth Fowler in his book, *The Fusion Quest*, which he wrote to drum up support for the International Tokamak Experimental Reactor (ITER), expressed great hope and enthusiasm for the goal of attaining practical fusion power, but he could not impress the reader as had Weinberg. At the beginning of one chapter – a bewildering discussion of a reef of plasma instabilities and the statistical-mechanical analysis of plasmas – Fowler could only muster: "I hope that the reader who bears with me will find some reward in a glimpse of the pleasure, as well as the complexity, that can be found in the world of plasma physics."¹⁰⁷ Instead of attracting public attention with grand epistemological goals, Fowler relied on the idea of messianic fusion, beginning his book with the Prometheus myth so beloved of Lewis Strauss and Fowler's colleague at Livermore, Richard Post.

Fowler, in his understandable enthusiasm for large fusion experiments, leaned toward the Extreme Plasma Properties and Phenomena conception, which has persisted throughout the history of plasma physics. A relatively recent example can be found in debates concerning the ITER. Marshall Rosenbluth, in the June 1996 issue of *Physics Today*, warned that not building the ITER would cause the U.S. program to fall behind the efforts of the rest of the world, both in the quest for fusion power and in plasma physics generally. Only the ITER would make possible the study of plasmas under more extreme conditions. Without direct experiments, the plasma-physics community would be left guessing: "the nonlinear physics and novel engineering issues of fusion are so complex that only a real experiment at the approximate parameters required for ignition will ever resolve them quantitatively."¹⁰⁸ Taking the other side,

Andrew Sessler of Lawrence Berkeley National Laboratory and Thomas Stix of Princeton University worried about the unresolved physics questions in the design of the ITER and warned of “the negative impact on future research that would redound from a mechanical or physics failure in this single device.” They concluded that “the step is too large and the overall concept, for all its attractiveness, is both premature and overambitious with respect to current knowledge.”¹⁰⁹ Technical doubts and lack of political traction led the U.S. to cancel its involvement with the ITER in 1999, although the Bush Administration renewed it in 2003.

While the Extreme Plasma conception of basic plasma physics has persisted, so has the discipline’s awareness that it has sometimes crowded out the more general Properties and Phenomena conception, which first became visible during the 1960s and addressed both low and high-temperature work at the national laboratories and universities. Before, during, and after the fusion buildup in the 1970s, there were persistent calls for experimental and theoretical studies of basic plasma effects and for strengthening university plasma programs. In 1971 an AEC policy analysis considered two routes for expanding the national fusion effort, both of which (“a significantly expanded program” or “an all-out effort”) included a recommendation for “an expansion of basic plasma physics research in the universities.”¹¹⁰ Nevertheless, small university programs dwindled during the 1970s because of the overwhelming amount of fusion funding going to the big laboratories. In 1986 the Panel on the Physics of Plasmas and Fluids – part of the NRC’s third physics review – noted that “direct support for basic laboratory plasma-physics research has practically vanished” and that “only a handful of universities receive support for basic research in plasma physics.” The panel pointed out that, although the expectation was that the large fusion programs of the 1970s would support basic plasma physics, “in fact, the support has almost disappeared.”¹¹¹ The panel recommended “a renewed commitment by the federal government to basic research. ... An adequate level of basic research, free from short-term, application-oriented goals, should be established in order to provide the foundations for future scientific advances and new technologies.”¹¹²

The continuity of the Properties and Phenomena conception of basic plasma physics became especially clear when it was reasserted during the 1990s after the weakening of fusion research. In the fourth NRC report of 1995, the Plasma Science Committee sounded a particularly strong alarm about the continued weakness of basic plasma experiments, noting that this was “the area of most rapid decline in the last 20 years.” The committee warned that “small-scale research provides much of the fundamental base for plasma science” and that “the future health of plasma science as a discipline hinges on the revitalization of basic plasma science.”¹¹³ The committee also stressed the need for an agency to coordinate funding for plasma physics: The Department of Energy concerned itself primarily with fusion research and the National Science Foundation shied away from general plasma physics.

In addition to stressing the importance of basic plasma physics, the fourth NRC report treated all of the regions of the Kantrowitz-Petschek map evenly. Perhaps more than any of the three preceding reports, the fourth reasserted the importance of low-temperature industrial plasmas. The NRC’s Plasma Science Committee regretted that “because basic research in this area has been neglected for many years, there is a severe

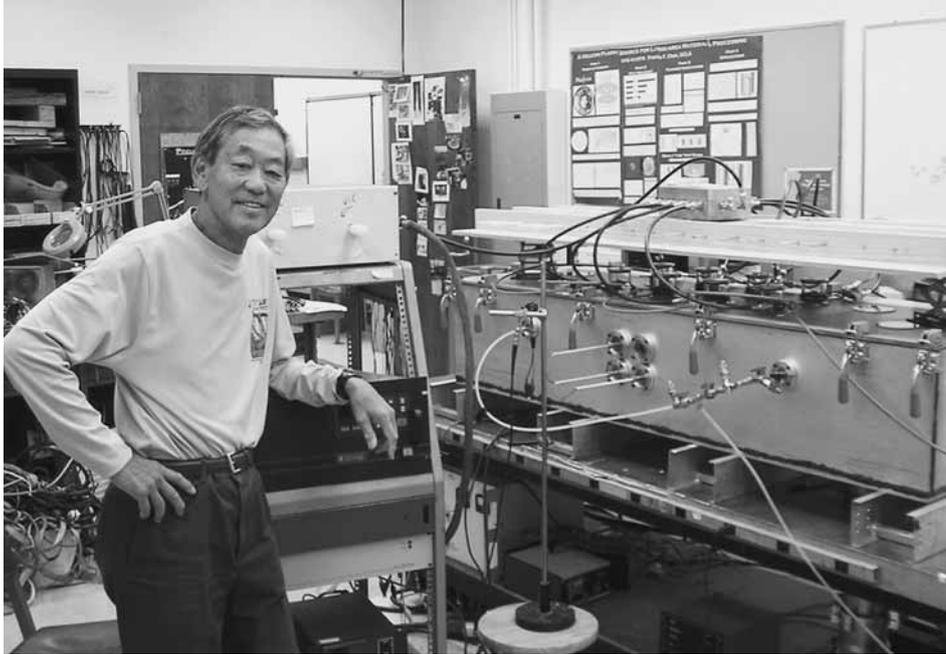


Fig. 9. Francis F. Chen (b. 1929) in a recent photo at UCLA with a large-area helicon source. *Credit:* Courtesy of Francis F. Chen.

lack of quantitative and experimental understanding of a wide range of phenomena that occur in low-temperature collision-dominated plasmas.”¹¹⁴ After reviewing a number of applied areas, such as lighting, isotope separation, electric propulsion, and materials processing, the committee warned that “shrinking budgets” had reduced the amount of work in these areas and that, to rectify the problem, an agency such as the Advanced Research Projects Agency or the National Institute of Standards and Technology should take responsibility for the coordination of low-temperature plasma research.¹¹⁵

The committee no doubt was influenced by its knowledge that a number of fusion scientists had migrated into industrial plasma research during the 1980s. One particularly dramatic example was Francis Chen (figure 9), who had moved from working on laser-plasma interactions to plasma processing of materials. In 1985, shortly after the second edition of his classic plasma-physics textbook was published, Chen had a career-changing encounter with Rod Boswell, who was working on small plasma sources at the Australian National University in Canberra. Boswell’s sources produced “helicon waves” (right-hand polarized electromagnetic waves) which he used to etch surfaces of silicon wafers, an important application for the growing semiconductor industry. Chen returned to UCLA excited by what he had seen in Canberra. He and Boswell wrote: “Around 1989, Chen decided to leave the comfort of a strong and well-funded group in laser plasma interactions, which he had founded, in order to explore

the possibilities of helicon discharges.”¹¹⁶ Chen’s initial attempts to interest the fusion community in his new work met with “apathy,”¹¹⁷ but he continued to plead for developing greater interest in low-temperature plasmas. In 1995 he presented his case particularly forcefully in a review of industrial applications in the influential journal, *Physics of Plasmas*. He began by noting that “the science of high-temperature and collisionless plasmas has grown explosively, fueled by the challenging problems in magnetic fusion, inertial fusion, and space plasma physics,” but then pointed out that researchers in these “classical areas of plasma physics” have not shown much interest in low-temperature plasmas: “Gas discharges are viewed by them as being an empirical discipline, devoid of elegance and beset with unnecessary complications.” To combat their negative attitude, Chen reviewed a wide range of industrial plasma research “to show that intellectually challenging problems can be found in low-temperature plasma physics.”¹¹⁸

Also in 1995, Barrett Ripin organized a special forum on “Mid-Career Change” at the annual meeting of the APS Division of Plasma Physics. Ripin believed that, historically, the DPP had “only paid lip service” to low-temperature plasmas,¹¹⁹ and he hoped that his special forum would help to stem the loss of low-temperature researchers to other professional organizations such as the American Vacuum Society or the Materials Research Society, a goal that he felt had created a “big resonance” in the plasma-physics community.¹²⁰ Francis Chen received the DPP’s James Clerk Maxwell Prize at this same meeting and took that opportunity to call attention to some of the changes occurring in plasma physics. Regarding semiconductor etching, Chen declared that, “I’ve been able to find very good physics inside of this topic, which has generally been considered ‘dirty,’” adding that “you can apply all the fancy stuff we learned in fusion and space physics.”¹²¹

The fourth NRC report not only tried to give even treatment to all of the regions of the Kantrowitz-Petschek map, it also displayed their interconnections. Thus, it identified four scientific issues (wave-particle interactions, turbulence and nonlinear dynamics, plasma boundaries, and magnetic reconnection) that overlapped three different areas of plasma research, fusion, industrial plasmas, and space and astrophysical plasmas. “The coherence of plasma science as a discipline is apparent when one considers some of the challenging intellectual problems ... that span applications in many of the topical areas.”¹²²

A significant number of researchers, in fact, continued to study space and astrophysical plasmas and to link their results to those of laboratory experiments. The magnetic-confinement fusion researcher Bruno Coppi rejuvenated his activity in astrophysical problems during the 1990s. He participated in the APS Plasma Astrophysics Working Group along with a number of astrophysicists and plasma physicists, helping to organize joint symposia at the APS DPP meetings. This led them to push for the creation of an APS Topical Group in Plasma Astrophysics. In their initial appeal, they objected to the separation between plasma physics and astrophysics: “Despite identification of problems of mutual interest, the plasma physics and astrophysics communities have remained, for the most part, quite distinct, with different societies and memberships, conferences, and archival journals,” noting that a primary reason to create the topical group was “to build a stronger bridge between the two communi-

ties.”¹²³ The APS approved the formation of the Topical Group in Plasma Astrophysics in 1997.

Laser researchers also nurtured an interest in space and astrophysical problems. By the late 1980s Barrett Ripin wondered that if scientists accepted scaling arguments from the laboratory to space, then they might accept ones from the laboratory to cosmic dimensions. Ripin, who now was head of the Space Plasma Branch that he had founded in the NRL's Plasma Physics Division, broadened its program to include research in plasma astrophysics. One of its most significant contributions was to analyze and scale shock waves caused by supernova explosions. Astrophysicists at this time sought to understand the shape of the Crab Nebula when theory suggested that this supernova remnant had begun as a “cue-ball smooth” explosion. Ripin and his team found, however, that if certain high-charge nuclei were included in its ambient plasma, then a pulsed-laser shock wave developed structured turbulence.¹²⁴ Although their comparison of their NRL experimental results to theoretical models seemed promising, Ripin experienced difficulty in getting astronomers and astrophysicists to listen to his arguments. They “snickered” when he tried to convince them that an experiment “the size of a golf ball” could tell them something about a supernova.¹²⁵

With the end of the Cold War, the NRL experienced a budget crunch, and its Space Physics Branch was eventually shut down and divided among other branches of NRL's Plasma Physics Division. Research in laser-plasma astrophysics survived at other facilities, however. The National Ignition Facility (NIF) at Lawrence Livermore Laboratory, for example, cited the potential for research in plasma astrophysics as one of the scientific rationales for its construction, research that included studies of nuclear-reaction rates, the equation of state, and the radiative opacity of stellar material.¹²⁶ In its discussion of the proposed NIF, the 1995 NRC plasma report used the work of Ripin's team to exemplify the sort of “multidisciplinary phenomena” that the NIF program would be capable of investigating.¹²⁷

From the 1950s through the 1990s, the Properties and Phenomena conception of basic plasma physics permeated the history of plasma physics – a conception that encompassed both low and high-temperature plasmas, and that served as a basis for research support both at large fusion laboratories and at smaller universities. This conception, however, underwent significant changes with the waxing and waning of fusion research, the largest benefactor of funding in plasma physics. At the height of the fusion push of the 1970s, with the AEC (or Department of Energy) and Congress encouraged by certain political and technical developments, the plasma-physics community shifted its overall conception of basic plasma physics toward extreme plasma conditions at the high end of the Kantrowitz-Petschek map. While this research yielded many significant results, it also crowded out research at lower plasma temperatures and inhibited efforts to unite the various plasma specialties. Many members of the plasma-physics community not only noted these problems but repeatedly called for their correction. Nonetheless, a significant fraction of the community found it necessary or desirable to play the high card of fusion research.

To some degree, the redefinition of basic plasma physics over time must be seen as part of the political maneuvering of the community to maintain or increase its funding. When fusion did well, plasma physicists stressed – to the AEC and to Congress – the

final goal of practical fusion power and turned to an Extreme Plasma Properties and Phenomena conception of basic plasma physics. Conversely, when fusion did poorly, plasma physicists stressed the more general Properties and Phenomena conception of basic plasma physics for all plasma regimes and highlighted the value of cross-specialty work. They used the rubric of “basic plasma physics” as a cushion against the losses of fusion funding and to look for alternative sources of funding, both during the 1960s (after the fusion push of the 1950s) and during the 1980s (after the fusion push of the 1970s).

This apparent symmetry in the interplay of funding for plasma physics and politics must be qualified, however. Perhaps most obviously, the practical political results were very different. The plasma-physics community received extremely generous funding both times when it played (or were told to play) the fusion card, but when the community sought to present plasma physics to the AEC, the NSF, or to Congress as a basic science devoted to the general investigation of plasmas, it met with indifference and modest funding.

More importantly, we must pay close attention to the cognitive content of these two visions of basic plasma physics. Saying that a scientific community uses the concept of basic science as a political tool and that the cognitive vision it adopts is informed by institutional circumstances is not the same as saying that these institutional circumstances determine the scientific content of the community’s research – however much “postmodern” sociology of science has sought to argue for such a causal connection.¹²⁸ In understanding and evaluating the different visions of basic plasma physics that the plasma-physics community adopted at different times in its history, we should not assume an indifferent or reductionist stance regarding their cognitive contents.

Spencer R. Weart’s study of the solid-state physics community offers a clue as to how to compare the two visions of basic plasma physics. Weart argued that the solid-state physics community formed from a number of different specialties that studied different aspects of solids, and that these specialties retained their autonomy to a considerable degree after the discipline was founded. The “separate communities did not combine within an overarching field. When we speak of the emergence of solid-state physics ... we ... mean a grand rearrangement of an entire array of specialties, old and new, into a novel constellation.”¹²⁹ This social rearrangement, however, came after the solid-state physics community realized that the different specialties overlapped cognitively. “Solid-state physics could become a social community only after its cognitive parts had drawn together in the minds of some physicists. The social institutions would follow hard on the heels of this new way of thinking and would bring it to the attention of the rest of the physics community.”¹³⁰

Plasma physics exhibits notable similarities and differences. Like the solid-state physics community, the plasma-physics community framed a vision of plasma physics that encompassed and connected together the different plasma specialties. Unlike the solid-state physics community, however, this was never fully reflected either in the social interactions or in the patron relationships of the plasma-physics community. An article commemorating the space-physics pioneer Kirsten Birkeland highlighted this, lamenting that:

There seems to have been a decline of consciousness about plasma science as an independent, but unifying, scientific discipline. There has been a tendency towards fragmentation of plasma science into specialties like fusion plasmas, space plasmas, weakly ionized laboratory plasmas and industrial plasma processing, and many plasma physicists find it easier to market themselves to the funding agencies as fusion scientists, space physicists, and so on The main lesson we can learn from Birke-land and the other great founders of plasma science is to regain faith in the unity and basic nature of our science, and to strive to tear down the walls that are erected between specialized areas.¹³¹

That the concept of basic plasma physics changed significantly to match changing institutional circumstances suggests that Roger L. Geiger's perspective overly stresses the conjunction between the cognitive goals of a scientific community and the interests of its patrons. To claim that the plasma-physics community fits Geiger's mold would require an evaluation of this conjunction in light of a changing concept of basic plasma physics. However, if historians allow themselves to cherry-pick the definition of basic science that they use, then they run the risk not only of ignoring disagreements between segments of a scientific community but also of pursuing an analysis that *always* will show agreement between scientists and their patrons.

I have argued that over the course of a half-century the plasma-physics community consistently sought to use the Properties and Phenomena conception of basic plasma physics to embrace the concerns of the entire community, while only a portion of it forged and used the Extreme Plasma conception. The main difference between Geiger's findings and mine is due to the remarkably large effect that fusion funding had on the research program of the plasma-physics community. Geiger celebrated the "pluralistic and mature system of university research" to which military research contributed,¹³² but this does not describe the patron relationships of the plasma-physics community. On the contrary, one huge patron, the AEC (later DOE), which was concerned with a single specialty, fusion research, and which was beholden to Congress, exerted a dominant influence, one that was scarcely concerned with the other areas of plasma physics, and even less with university research.

In addition, I have argued that "basic physics" means more than a Big Questions conception and, in the subdiscipline of plasma physics, means what I have called a Properties and Phenomena conception. This conception, however, has proved to be difficult to communicate to government representatives, administrators, and the general public. One reason perhaps is that it does not overlap with the grand, quasi-religious themes of a Big Questions conception ("What is the ultimate theory of nature?" "How did the universe begin and what will happen to it?"). The physics community as a whole has tended to defer to the Big Questions conception of the high-energy physics and cosmology communities and has done a poor job in communicating the nature of other areas of physics, in which the majority of physicists work.

Plasma physics has had these difficulties in spades. The plasma-physics community was crippled by the need to communicate complex plasma models that seemingly were built upon old-fashioned, classical laws. Further, the concept of basic plasma physics became a moving target that changed according to the fortunes of fusion research.

Fusion researchers more than once turned from the Properties and Phenomena conception of basic physics and moved toward the Extreme Plasma conception and to the notion of fusion power as the savior of civilization. In facing the weaknesses of the Properties and Phenomena conception, messianic fusion might be regarded as the plasma-physics community's substitute for a Big Questions conception of basic physics.

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I have used the following abbreviations:

AIP: Niels Bohr Library at the American Institute of Physics, College Park, Maryland

AR47/AIP: Joan Lisa Bromberg Papers, 1922–1982 (unprocessed)

HO/DOE: Historian's Office of the U.S. Department of Energy, Germantown, Maryland

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CROSS-WORLD

The world is a cross-word
immersed and immense
with letters that fit
in each spot.
And the tiniest, teeniest
details make sense
but the entire pattern
does not.

Piet Hein

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