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Physics in Perspective

The Plasma Archipelago: Plasma Physics in the 1960s

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With the foundation of the Division of Plasma Physics of the American Physical Society in April 1959, plasma physics was presented as the general study of ionized gases. This paper investigates the degree to which plasma physics, during its first decade, established a community of interrelated specialties, one that brought together work in gaseous electronics, astrophysics, controlled thermonuclear fusion, space science, and aerospace engineering. It finds that, in some regards, the plasma community was indeed greater than the sum of its parts and that its larger identity was sometimes glimpsed in inter-specialty work and studies of fundamental plasma behaviors. Nevertheless, the plasma specialties usually worked separately for two inter-related reasons: prejudices about what constituted "basic physics," both in the general physics community and within the plasma community itself; and a compartmentalized funding structure, in which each funding agency served different missions.

Key words: William Allis; Nicholas Christofilos; Harold Grad; Arthur Kantrowitz; Charles F. Kennel; Alan C. Kolb; Leonard B. Loeb; Richard F. Post; Norman Rostoker; James Van Allen; plasma physics; aerospace engineering; fusion research; gaseous electronics; ionospheric physics; space science; unity of physics; funding.

A Fundamental Disunity?

Writing in 1969, Harold Grad of New York University described the plasma physics community, then only ten years old: "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."[1] Many physicists of the time might have seen this as an embarrassing admission or perhaps even a shrug of defeat. Perhaps even today, nearly fifty years later, physicists have similar sentiments. Physicists and philosophers alike have long claimed that the primary aim of physics is to unify knowledge of the physical world. From this viewpoint, after the unifications accomplished by Isaac Newton and James Clerk Maxwell, the physics

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community and the general public have rightly been transfixed by the grand aim of explaining the four forces of nature in a single, unified description. And yet, this does not square well with the fact that most members of the community, working in subdisciplines such as atomic, molecular, solid state, fluid, and plasma physics, had no truck with the unification sought by particle physicists and cosmologists.

Debates about the unity of physics have been dominated by considerations of its conceptual foundations. This is evident in the first comprehensive review of physics by the National Research Council (NRC) in 1966, often referred to as the Pake Report after its chairman, George Pake. The Elementary Particle Physics Panel announced its theme with its first sentence: “The ultimate ambition of the physicist is to discover a few basic principles in terms of which all natural phenomena can be understood.” It asserted that the laws of atomic physics “now appear to be completely known” and therefore establish “the foundation for an understanding, at least in principle, of the laws of chemistry, solid-state physics, and to a certain extent, biology.” After this, the panel stressed particle physics as “pure science” and a “search for truth,” suggesting “it may be that before we can proceed to a more basic understanding we shall have to discover a new principle that will change our whole picture of nature.”[2] A quarter of a century later, Steven Weinberg offered much the same message in his famous retelling of Thomas Henry Huxley’s “On a Piece of Chalk,” according to which the whiteness of chalk is explained by the light it reflects, which, in turn, is explained by how photons interact with atoms, then by quantum mechanics and, finally, by elementary particles and forces. Against such reductionist dreams, physicists like Philip Anderson and biologists like Ernst Mayr put forward the idea of “emergence,” which holds that the phenomena of complex systems (like spin glasses or living organisms) are different in kind from the phenomena seen at the level of elementary components.[3] In response, Weinberg stressed that he was concerned not with method but with explanation, “not so much with what scientists do, since this inevitably reflects both human limitations and human interests, as with the logical order built into nature.”[4]

Preoccupation with the “big questions” of physics has served at least two functions.[5] First, it has buoyed the community at large with assurances that physics is the king of the sciences. Despite the fact that most physicists have no direct connection with high energy physics, they frequently take refuge in the sentiment that “it’s all physics in the end” in their discussions with other scientists from other disciplines. But once this reductionist freight train is loosed, it probably cannot be stopped until it reaches the conclusion that elementary particle physics “represents the frontier of our knowledge in a way that condensed matter physics does not.”[6] Many physicists, like Anderson, have felt that Weinberg’s “two cheers for reductionism” was at least one cheer too many.[7] Beyond the issue of emergence—a knotty conceptual problem that Weinberg surely did not dispel—there is the difficulty raised by a second function of his reductionist argument: his attempt to rally support for high energy physics against a background of limited funding. This directs our attention to the social organization and patron relationships of physics communities, an issue that Weinberg's abstract discussion virtually ignored.

Historical studies of physics communities must account for the development not only of their conceptual foundations but also their institutional circumstances. Spencer Weart's account of the solid state physics community shows how different specialties, studying different aspects of solids, joined together yet did not lose their individual autonomy and development. After tracing the early development of specialties such as crystallography and magnetic materials, Weart describes their combination during the 1950s. Of central importance to the formation of the solid-state community was the realization, on the part of its members, that the different specialties demonstrated some form of unity: "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."[8] Weart sees the social organization of solid-state physics as developing in rough consonance with the diversity of physical effects studied by the community. The solid-state specialties came together in "a sort of confederation in which each part kept its own individual identity."[9]

Peter Galison offers a somewhat similar view in his account of two strains of research in the particle physics community. He traces the development of cloud chambers and spark counters during the first half of the twentieth century to their hybridization during the 1980s in instruments capable of electronically produced images. Galison describes the relations between these two specialties in terms of a "disciplinary map"[10] that includes three elements—theory, experiment, and instrument building—which frequently overlap and interact with one another, but, at the same time, develop in relative autonomy.[11] He calls this a "polycultural history," one that strikes a balance between the local and interconnected aspects of physics: "Different traditions of theorizing, experimenting, instrument making, and engineering meet … but for all that, they do not lose their separate identities and practices."[12]

The plasma physics community of the 1960s makes a particularly interesting case study, both confirming and contradicting Weart's and Galison's accounts. As suggested by Harold Grad, plasma physics never sought anything like the particle physics community's conception of unity or fundamental physics because it studied a bewildering range of behaviors. Grad noted that "we can grasp the significance of the field of plasma physics only in the context of its enormous phenomenological variety," so that the goal of the young discipline was to "find many theories of the behavior of many different plasmas."[13] This ecumenical conception of plasma physics resonates with Weart's picture of a confederation of specialties, but, as we will see, the young discipline experienced greater division and disagreement than most plasma physicists expected.

Part of the reason for this is suggested by Joseph D. Martin's recent account of the formation of solid state physics. The great topical breadth of the nascent community led it to adopt a precarious professional identity, balanced between two main constituencies. One the one hand, were members of the community who sought to conform to the American Physical Society's established commitment to subject-based divisions and basic research. On the other hand were members who wished to accommodate the applied work of industrial physicists, which had increased greatly during and after the war. The resulting tension of professional identity between the two constituencies figured strongly in struggles concerning prestige and funding and, over the next three decades, influenced the development of condensed matter physics and materials science, which Martin considers to be "distinct historical entities."[14]

Similar disagreements regarding fundamental science and professional identity are found in the history of the early plasma physics community. Such issues were made all the more vexing by the fact that many of the new plasma phenomena could be understood in terms of classical mechanics (Newton's laws, Maxwell's equations, fluid dynamics, and statistical mechanics), making the field seem like one of the least promising twentieth-century-physics subdisciplines. The second NRC review of physics of 1972—known as the Bromley Report—well reflected the sentiments of the previous decade. The Physics Survey Committee conducted a "jury rating" of the physics subdisciplines in terms of two categories: "*Intrinsic merit* we define as criteria internal to science. *Extrinsic merit* relates to impact on technology and the resolution of human problems."[15] The results were summarized in a simple graphic that fit a line to the numerical jury ratings, the intrinsic responses grouped to the left and the extrinsic to the right (figure 1). Although certain program elements of plasma physics scored high on intrinsic categories such as "ripeness for exploration," they scored uniformly low on categories like "potential for discovery of fundamental laws." Overall, plasma physics was judged to be of primarily extrinsic interest, along with acoustics and optics. Condensed matter, nuclear, and atomic-molecular-electron physics were judged about evenly, whereas elementary particles and astrophysics-relativity were deemed of primarily intrinsic interest.[16]

It is sometimes suggested that the American scientific community experienced a "golden age" after World War II by virtue of its participation on advisory bodies like the NRC and therefore its influence on government funding. Though there is some truth to this, one must also remember that many scientists of the time were concerned that federal and military funding might endanger the autonomy of science and affect the structure of the scientific community, as scientists—particularly those doing applied research—came to rely on specific funding agencies.[17] These concerns were elevated by the fact that, though agencies like the Atomic Energy Commission (AEC) and the Office of Naval Research (ONR) had been formed quickly after the war, the National Science Foundation was long delayed, not only its foundation but also its command of significant resources.[18]

Within this milieu, the judgment by the general physics community that plasma physics was nowhere near the frontier of research was both a determinant to and a

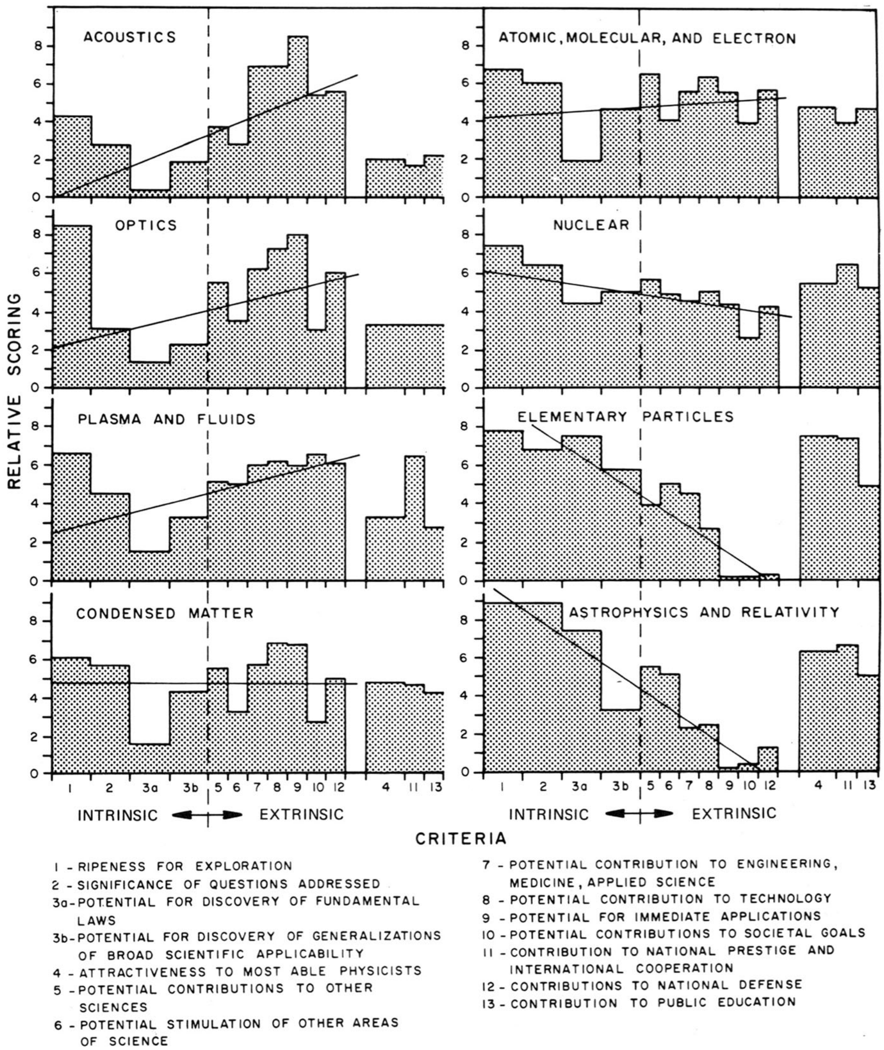


Fig. 1. Results of the Survey Committee jury ratings of the core subfields of physics. Source: Physics Survey Committee, *Physics in Perspective*, vol. 1 (Washington, DC: National Academy of Sciences, 1972), 404

consequence of its patron relationships. Related biases, existing between the individual plasma specialties, affected the plasma community's own disposition of resources. As a result, plasma physics of the 1960s demonstrated strong tensions between unity and disunity and between open-ended development and compartmentalization. Although the community put forward a loose conception of unity, one that sometimes enabled connections between areas of research, its specialties usually worked independently, beholden to different funding agencies with different institutional imperatives. And although it is possible to argue that the institutions of postwar American science fostered diversity, it is just as easy to argue that they enforced isolation. On the one hand, the plasma specialties represented a multitude of theories and machines, corresponding to different regimes of plasma density and temperature. On the other hand, their separate missions constituted a sort of prison, one that forced its scientists to work on separate islands, sowing discord and making communication difficult and frustrating.

Plasma Spring

Plasma physics became an official subdiscipline in April 1959 with the foundation of the Division of Plasma Physics (DPP) of the American Physical Society (APS). In order to discuss the relationships between the plasma specialties of the 1960s, I begin by sketching four plasma specialties that existed before the advent of the discipline. The largest, and arguably the oldest, developed from the study of relatively low-temperature ionized gases. After the famous experiments leading up to J. J. Thomson's identification of the electron, schools devoted to the conduction of electricity through gases were founded at Oxford University, headed by J. S. E. Townsend, and at the University of California, headed by Leonard B. Loeb, who eventually published the monolithic textbook *Fundamental Processes of Electrical Discharges in Gases*.[19] Because such work was of great industrial interest, research and development efforts were supported at Phillips Laboratory, headed by Frans Penning, and at General Electric, headed by Irving Langmuir. The specialty went by a variety of names—gas discharge physics, electrical discharge in gases, and gaseous electronics—but it was Langmuir who, inspired by the glow of ionized gases, borrowed the word "plasma" from medical science. In 1929, he and Lewi Tonks identified a first type of plasma wave resulting from the electrostatic vibration of the electrons (not the ions), which they called "plasma-electron oscillations."[20] At the close of World War II, one of the most important academic programs was founded at the Massachusetts Institute of Technology (MIT) by William Allis and Sanborn Brown. Although Allis and Brown embraced the term "plasma physics," they interpreted it as a synonym for "gaseous electronics."[21] Many members of the gaseous electronics community belonged to the APS and joined the Division of Electron and Ion Optics when it was formed in 1943 (today, the Division of Atomic, Molecular, and Optical Physics). The community soon broke away from the Physical Electronics Conference organized by the division to found the annual Gaseous Electronics Conference in 1948.[22] An international meeting, Ionization Phenomena in Gases, soon followed in 1953, organized by Oxford's Alfred von Engel.

Aspects of geophysics also nurtured plasma research. Ionospheric physics was well established by 1959, having developed in tandem with commercial and military radio communications. Many of the earliest radio researchers were members of the Institute of Radio Engineers, formed in 1912 (now part of the IEEE), and published in journals associated with that organization, the International Union of Radio Science, and the American Geophysical Union (AGU). By the mid-1920s, Edward Appleton and M. A. F. Barnett at Oxford University and Merle Tuve and Gregory Breit at the Naval Research Laboratory successfully reflected radio signals off the ionosphere. Appleton soon built on the work of William Eccles, Joseph Larmor, and Wilhelm Altar to develop the "magneto-ionic theory" describing the propagation of electromagnetic waves in an ionized medium.[23] In 1935, making use of magneto-ionic theory, Thomas Eckersley showed that the strange descending tones reported on military radios during the World War I were low-frequency waves propagating in Earth's upper atmosphere. Such "whistler waves," as they became known, were vibrations of the electrons of the plasma due to electromagnetic waves launched by lightning strikes near the Earth's poles. In addition to being a second fundamental plasma wave, whistlers were significant because they followed propagation paths reaching altitudes well above the ionosphere, a region later studied by the space science community.[24]

Whereas the communities of gaseous electronics and ionospheric research were relatively well-defined, other plasma specialties were little more than special topics pursued within larger disciplines by relatively few researchers. Auroral phenomena constituted one such topic in geophysics. Work was begun in earnest around the turn of the century by the Norwegian scientist Kristian Birkeland, who hypothesized that the aurora resulted from charged particles that were ejected by the sun and then guided in Earth's magnetic field. Birkeland carried out three polar expeditions and a series of terrella laboratory experiments while his colleague Carl Stoermer supervised the laborious calculations of single-particle trajectories in the geomagnetic field. Efforts to determine the chemical composition of the aurorae were pursued via spectroscopy.[25] During the 1930s, Sydney Chapman and Hannes Alfvén each developed theories to explain how streams of charged particles emitted by the sun deformed Earth's magnetic field and induced geomagnetic storms.[26] Attempts to explain how the sun emitted corpuscular radiation in the first place were made by both Chapman and Ludwig Biermann but the phenomenon was not fully understood until the work of Eugene Parker and later satellite measurements.

Astrophysics and astronomy also featured a number of topics concerning plasmas. In 1942, Hannes Alfvén made an especially important contribution after hypothesizing that the magnetic fields associated with sunspots were due to currents in the solar plasma. By combining Maxwell's equations and the hydrodynamic equation, Alfvén showed that a plasma, considered as a continuous conducting fluid, could produce low-frequency "electromagnetic-hydrodynamic waves." He visualized this as the ions of a plasma glued to the magnetic field lines and vibrating in response to a propagating electromagnetic wave. This third general type of plasma disturbance became known as an "Alfvén wave," the centerpiece of a new field dubbed magnetohydrodynamics (MHD).[27] Alfvén's colleague Stig Lundquist demonstrated the existence of MHD waves in laboratory experiments as Alfvén's influential book *Cosmical Electrodynamics* went to press in 1950.[28] Other astrophysicists contributed to other plasma-related topics. Donald Menzel at Harvard University investigated the spectra of the solar atmosphere and gaseous nebulae. Lyman Spitzer of Princeton University produced trailblazing papers on the electrical conductivity of interstellar gas. Because MHD was of no use to such work, Spitzer turned to statistical mechanics and the Boltzmann equation to view plasma interactions as "the cumulative effect of many distant encounters," a topic stressed in the final chapter of his elegant and widely read book of 1956, *Physics of Fully Ionized Gases*.[29] The primary allegiance of the solar scientists and plasma astrophysicists was to the American Astronomical Society (AAS), though some were also members of the AGU.[30] Interest in astrophysical topics related to high-temperature gases was strong enough to motivate a series of topical meetings on cosmical aerodynamics, which concerned "borderline problems between astrophysics and gas dynamics." The first meeting took place in Paris, France, in 1949. Thirty-four of the participants were astronomers or astrophysicists and eighteen were aerodynamics experts or physicists.[31]

As subsequent sections will discuss, three new fields involving the study of plasmas developed during the postwar years and reconfigured the established plasma specialties. Space science arose from aspects of many disciplines, including physics, geophysics, and astrophysics. Although many plasma scientists in this community began their careers in the APS, most eventually shifted their alliance to the AGU. Aerospace engineering arose from the combination of aeronautics, used for airplane technology, with astronautics, used for rockets. Though dominated by engineering, aerospace also offered a career path for many physicists.

The third new field, controlled thermonuclear research (CTR), was the most centrally concerned with the study of plasmas and particularly important for the formation of the new community. The story of fusion research has been well told by a number of authors and a brief review will suffice here.[32] The first fusion programs were closely connected with work on nuclear weapons and were founded as secret projects by the Atomic Energy Commission (AEC) during the early 1950s. Each of the major labs, at Los Alamos National Laboratory (LANL), Princeton University, and the University of California Lawrence Radiation Laboratory, Livermore (hereafter, Lawrence Livermore National Laboratory, LLNL), pursed a different machine concept: the pinch, the stellarator, and the magnetic mirror, respectively. For our purposes, it is important to note that the fusion community was assembled through piecemeal choices made in secret by the AEC and that many of these personnel came from fields not directly associated with the established plasma specialties. The American CTR program remained classified secret for four years, with the hope that the American scientists might beat their Soviet and British counterparts with a quick success. Partial declassification in 1956 allowed discussion of general scientific principles but not details of the fusion machines. With the realization that the three major powers had run aground on similar shoals, the full declassification of fusion occurred in late August 1958, just in time for the second International Conference on Peaceful Uses of Atomic Energy in Geneva, Switzerland. Eight months later, the Division of Plasma Physics (DPP) was incorporated in the APS.

One indication that the new plasma physics community might establish connections between its specialties was the sharing of meetings before the first DPP meeting in December 1959. Because the Soviet fusion community was especially eager for declassification, in 1956 they presented results of their pinch-machine experiments in three papers at a symposium of the International Astronomical Union's titled "Electromagnetic Phenomena in Cosmical Physics."[33] The Americans soon followed. In June 1957, the Third International Conference on Ionization Phenomena in Gases was "overrun by the new breed of researchers working on the control of thermonuclear reactions," with about one third of the conference papers devoted to fusion.[34] The Eleventh Annual Gaseous Electronics Conference (GEC) in October 1958 had to cope with 441 registrants, up from 297 the previous year. David J. Rose, the secretary of the GEC, noted that "this circumstance arose from the interest in the recently declassified fusion physics and technology."[35] Because the study of plasmas overlapped with fluid dynamics, the Division of Fluid Dynamics asked the plasma theorist Marshall Rosenbluth to give an invited talk on plasma instabilities and to chair a session on fusion research at its annual meeting of November 1958.[36] The general physics community also took interest. In 1959, both the APS annual meeting (in January) and the spring meeting (in April) added special sessions on plasma physics and fusion research.

The shared interests of the plasma specialties were also reflected by special symposia and seminars, which sought to lay out the foundations of the new discipline. In June 1958, the International Symposium on Plasma Dynamics, held in Woods Hole, MA, featured sessions concerning many areas of specialization, including fusion, space, astrophysics, and beams. Although the conference was limited to about fifty scientists, they were selected "to represent all major aspects of this growing branch of physics."[37] The International School of Physics offered a series of three plasma seminars in 1958 and 1959, under the auspices of the Italian Society of Physics. Introducing the third seminar, Hannes Alfvén stressed that the rapid increase in interest in plasma physics arose from aspects of astrophysics, fusion, and space research.[38]

A flood of books called plasma physics to the attention of the scientific community. Earlier texts by Loeb, Alfvén, and Spitzer bore the imprint of their authors' specific interests and communities. The same was true of lecture notes from established graduate courses that were published quickly. Two examples are Sanborn Brown's course at MIT and Subrahmanyan Chandrasekhar's course at the University of Chicago. Though both books used "plasma physics" in their titles, Brown's centered on the atomic processes studied by gaseous electronics whereas Chandrasekhar's focused on fully ionized plasmas of interest to many astrophysicists and fusion researchers.[39] On the other hand, Donald Menzel's work of the 1930s on gaseous nebulae and stellar atmospheres entailed use of atomic physics and spectroscopic analysis, which were potentially of interest to many researchers. When he republished his papers in an edited collection, *Selected Papers on Physical Processes in Ionized Plasmas*, Menzel wrote in the introduction that "since modern terminology refers to such ionized media as plasmas, I have altered the title to indicate the general applicability of the developed theory to problems other than those of gaseous nebulae."[40]

More important, volumes soon appeared offering thorough surveys of plasma physics as a whole, intended as textbooks for graduate study. David J. Rose and Melville Clark of MIT's new program in Nuclear Engineering published a textbook in 1961 "suitable for graduate study in either the first or intermediate years." The first twelve chapters of the textbook covered a broad range of topics, and provided "a course in plasma physics, hydrodynamics, and elementary gaseous electronics" and the final four chapters introduced the reader to CTR. James E. Drummond of the Plasma Physics Laboratory of Boeing Scientific Research Laboratories took a different tack by commissioning articles from representatives of different areas of plasma study and assembling them into a coherent volume. In the book's preface, Drummond singled out seven chapters "which I believe would provide a suitable graduate course" and seven additional chapters "for the mature research worker as well as the student."[41] As Drummond's institutional affiliation would suggest, his volume stressed material of use to aerospace scientists and featured authors from that field.

Textbooks by theorists soon provided a much-needed taxonomy of plasma waves and instabilities. William Allis, Solomon Buchsbaum, and Abraham Bers of MIT felt that the development of plasma physics resulted in confusing, sometimes contradictory jargon and analyses that involved a wide range of different physical assumptions. In their own textbook, they saw their task as summarizing what was known and to adopt a consistent terminology: "In this monograph, we try to bring order into this diversity."[42] To this end, the book features what later became known as the "CMA diagram," a means of visualizing the interrelation of a large number of waves in a graphical display. The diagram used two variables: a horizontal axis increasing with plasma density and a vertical axis increasing with the strength of an externally applied magnetic field. Although the diagram was limited to "cold" plasmas (meaning that the thermal motions of the plasma ions could be neglected), it displayed regions for many of the then-known plasma waves. A second influential textbook on plasma waves was written by theorist Thomas Stix, based on a course he taught as part of Princeton University's new graduate program in plasma physics. Stix began with a detailed discussion of the CMA diagram and cold plasmas but then moved on to extensive discussions of hot plasmas and their attendant instabilities.[43]

In the years surrounding declassification, there was considerable excitement, not only about fusion research but also about plasma physics as a general study, as was evident in meetings, special symposia, textbooks, and graduate programs. The DPP seemed to demonstrate an ecumenical conception of plasma physics, stating in its bylaws that "the object of the Division shall be the advancement and diffusion of knowledge regarding the physics of highly ionized gases of natural or laboratory origin—including their creation, containment, heating, and acceleration; their radiations, oscillations, and stability; their transport, collective and wave properties."[44] However, as the new discipline continued to develop during the 1960s, differences between the specialties, pertaining to everything from research style to sources of funding, made it clear that holding the community together would be difficult.

The Fuddy-Duddies of Low Temperature

Although members of the gaseous electronics community continued to interact with fusion physicists throughout the 1960s, these connections weakened after the initial burst of enthusiasm. This can be seen, first, in meeting attendance. After the founding of the DPP, few members of the fusion community attended the Gaseous Electronics Conference (GEC). Allis commented, "We hoped they would continue with us but in fact their work is mostly too specialized."[45] Meanwhile, relatively few members of the gaseous electronics community attended DPP meetings. Although the DPP always featured one or two sessions on low-temperature plasmas, this represented a small portion of the scientists who attended the GEC. The gaseous electronics researchers had already established loose relations with the Division of Electron Physics (DEP, renamed from the Division of Electron and Ion Optics in 1947). But despite the fact that Ladislaus Marton judged (in 1964) that the GEC "amounts almost to a subdivision of our Division,"[46] it was not formally incorporated with the DEP any more than it was with the DPP.

In addition to their tenuous connection to the DPP, the gaseous electronics community appears to have felt marginalized by physics in general. At the fourteenth Gaseous Electronics Conference (GEC), held in Schenectady, NY, in 1961, the GEC Committee on Publications, chaired by Lewi Tonks, expressed concern that the publication of their work was "somewhat hampered in this country."[47] In addition to the fact that the *Physical Review* had recently altered its editorial policy by accepting fewer gaseous electronics papers, other journals were perceived as subjecting the community's papers to a slow review process. Tonks sent out a questionnaire to participants of the previous two GEC meetings and received mixed results, with eleven out of seventy-one replies complaining of publishing problems. Leonard Loeb objected to the fact that the *Physical Review* rejected one of his papers because "it was more qualitative and not quantitative, in that it did not have any mathematical theory" and suggested that he submit the paper to the *Journal of Applied Physics*.[48] Other respondents complained that reviewers were often late and raised objections that seemed due to a lack of understanding. Tonks concluded that, although the survey results did not demonstrate a strong discrimination against gaseous electronics, journal editors should be advised to exercise better judgment regarding the competence of their referees and ensure that decisions were made promptly.[49]

The DPP community had substantial reservations about the gaseous electronics community. Because fusion experiments necessitated high temperatures, the new discipline saw the frontier of its knowledge as being concerned with the behavior of "highly ionized gases," a phrase stressed in the first bylaws of the division. In the preface of his influential book of 1956, *Physics of Fully Ionized Gases*, Lyman Spitzer acknowledged that gaseous electronics and astrophysics had encouraged interest in plasmas but quickly added that, in analyzing a fully ionized gas, "most of the phenomena important in normal gaseous electronic disappear; electron attachment, dissociative recombination, excitation and de-excitation of atoms and molecules, electrical breakdown, etc."[50] Because fusion scientists were privy to the machines capable of generating these high temperatures, they dominated the research program of the discipline. Also writing in 1956, Richard F. Post of LLNL expressed the hope that "a new and fertile field of experimental and theoretical physics is arising. From a thorough understanding of the physics of ultra-high temperature plasmas and their interaction with electromagnetic fields, one can hope not only for the achievement of controlled fusion power, but also, as a result of this increased knowledge of nature, there will no doubt arise new and important applications to other fields of science and technology."[51] A particularly strident opinion appeared in the introduction of a 1961 plasma physics textbook written by J. G. Linhart of CERN. After sketching the early history of the field, Linhart reflected that fusion research "was a new lease of life for plasma physics which was becoming rather unfashionable and … regarded by most other physicists as a rather charming subject, full of small, colorful experiments, where there was little left to discover and whose only real justification was the amusement of those who bothered to waste their time on it."[52]

The gaseous electronics researchers returned the favor. Because they maintained a strong interest in atomic effects in relatively low-temperature systems, they did not show much interest in the new plasma work. Leonard Loeb, a Navy reservist, had been called to active duty during World War II at the Naval Proving grounds at Dahlgren (figure 2). At the war's close, he applied for and received a substantial grant from the Office of Naval Research (ONR) to support his gaseous-electronics program. The contract commenced in 1947 and was renewed for fifteen years, allowing Loeb to produce twenty-two doctorate graduates and remain active after his retirement in 1958. In his autobiography, Loeb enthused, "All I can say is Thank God for the Office of Naval Research and its enlightened policy of support of basic research!"[53] To Loeb, gaseous electronics had been revitalized by the war effort, through the development of new instruments, including microwave devices, pulsed power sources, and improved vacuum systems.[54] Fusion research, on the other hand, held out little promise to him. He

Fig. 2. Leonard Loeb in naval uniform around the close of World War II. Credit: American Institute of Physics Emilio Segrè Visual Archives

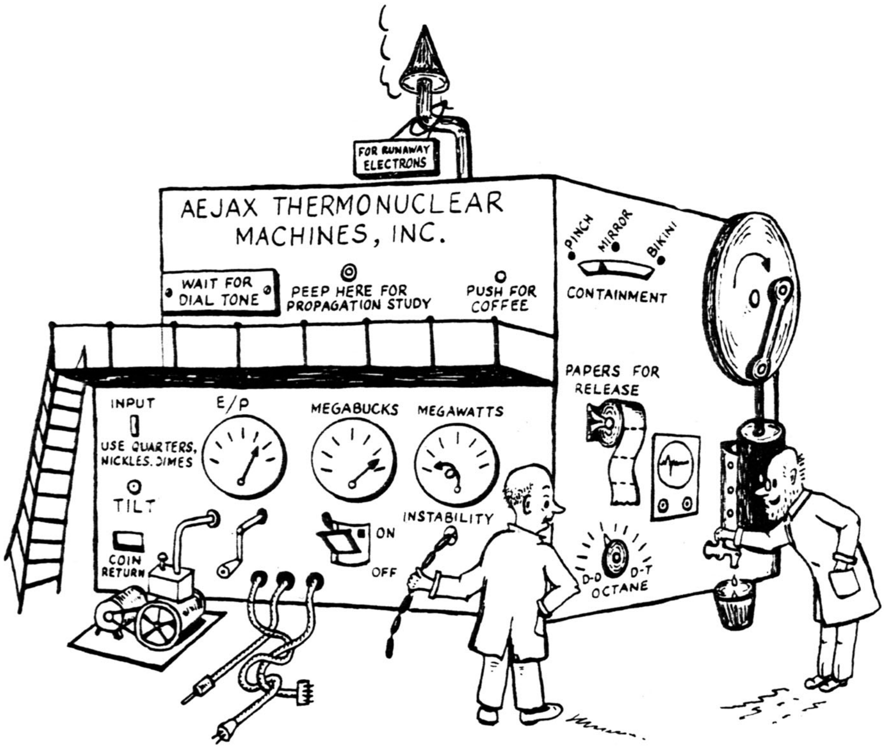
found fusion scientists to be "young nuclear physicists ill trained in general physics." In his view, these "conceited little know it alls" refused to take the trouble to learn basic atomic physics. While teaching classes in 1960, he found that students drawn to fusion research were an "unappreciative audience."[55] In a letter to Lewi Tonks, Loeb explained his continued interest in gaseous electronics by citing the very reasons that the fusion community lacked interest: a concern for relatively low temperatures and industrial applications. "The physics of the very highly ionized gases quite frankly is something which I have been keeping away from because there are already too many in this field. I am still interested in basic atomic processes because most of the time we deal with these and our problems of every day industrial application are not all related to the extreme conditions."[56]

In March 1946, the first objective of the Joint Services Electronics Program (JSEP) was to ensure that MIT's Radiation Laboratory, its wartime funding having run out, would not scatter its resources. Parts of the Rad Lab were supported by JSEP funding but renamed the Research Laboratory of Electronics (RLE). William Allis and Sanford Brown founded the plasma program, receiving $60,000 a year from the RLE. Although much of the research at the RLE was related to military applications, most of it was publishable as basic research.[57]

Unlike most gaseous electronics programs, the RLE developed strong ties with the AEC and sought to make aspects of its work relevant to the fusion community. Allis and Brown even gave talks at four of the secret AEC fusion meetings between 1952 and 1958. Nevertheless, their participation was hesitant. In addition to the fact that they objected to the circumstances of secrecy, they did not want to disturb unduly their established research on relatively low-temperature, well-behaved plasmas. In a talk given at the 1957 meeting, Brown outlined ten projects pursued by RLE that could make a contribution to the fusion effort “by extending our studies to high density, high percent ionization discharges in magnetic fields in hydrogen.”[58] By 1959, RLE’s microwave gaseous discharges group enjoyed enough AEC funding to support six faculty members and thirteen graduate students. The group pursued theoretical studies of plasma waves and an experimental program on guided plasma waves and microwave diagnostics.[59] Although the group intended eventually to study plasmas “of interest in the thermonuclear field,” as of 1961, Brown commented “we are very far from that at the moment.”[60]

During the 1960s, the RLE continued to expand, gaining further support from JSEP, AEC, NSF, and the US Air Force. By 1966, its plasma and fusion effort comprised six programs within a large Plasma Dynamics division, supporting thirty research staff and sixty-one students. The Plasmas and Controlled Nuclear Fusion program did not include any complete fusion machines but rather experiments on the heating of plasmas by beams and the general study of plasma instabilities. One part of its efforts was a collaboration with MIT’s Department of Nuclear Engineering (founded in 1958). This was headed by David Rose, once a student of Allis and Brown, and concentrated on feasibility studies of hypothetical fusion systems from an engineering perspective.[61]

Despite benefiting from the AEC fusion program, most RLE researchers continued to look askance at the relatively large, complex fusion machines. Reflecting on the development of plasma physics, Allis noted that government money flowed into new areas of research during periods he characterized as the “steep front, when the need is obvious.” However, the plasma physics community’s arrival at the steep front of government-funded fusion research was made possible by the earlier and more meticulous “long approach” of the gaseous electronics researchers; “the long approach has to be done by the universities on their own funds, because the men who do this are ‘the fuddy-duddies who don’t see what is important.’ Yet this is when the people are trained who will spend the big money later.” Allis’s article was accompanied by a playful drawing of a fusion machine by David Rose (figure 3). The controls on the large machine wryly point out that it soaks up a great deal of “megabucks” but generates precious little “megawatts.” Meanwhile, a toilet paper dispenser is labeled “papers for release.”[62]



"Well, at least we got the water back."

Fig. 3. Drawing of a fusion machine by David J. Rose of MIT's Nuclear Engineering Department. Source: William P. Allis, "Plasma Research: A Case History," *Technology Review* **63** (November, 1960), 27, Reprinted with permission from *Technology Review*

Plasma with a Vengeance

Space science and aerospace engineering affected the plasma physics community in ways that were nearly as significant as fusion research. Because these fields were largely supported by the military and often combined personnel at private companies with those at university campuses, many scholars have raised questions about their benefit to postwar science. Although the present study does not go so far as to suggest that patron relationships and institutional missions virtually compromised the autonomy of American science—as is suggested by the studies of David DeVorkin and Stuart Leslie[63]—it pursues the narrower objective of showing that such imperatives guided and constrained the development of the plasma community, especially concerning the relations between its specialties.

DeVorkin's account of the history of the space science community between 1946 and 1954 shows some rough parallels with the fusion community during its years of secrecy. Both were made possible with new technology associated with military systems: thermonuclear weapons in the case of fusion and the V-2 rocket in the case of space science. Both relied on funding agencies connected with the war effort and its aftermath such as the AEC and ONR, as well as government installations such as Los Alamos National Laboratory and the White Sands Missile Range. Finally, both communities were formed by piecemeal selection of a new set of workers, with training and interests that were not closely connected to plasma physics or upper-atmosphere research.[64]

During the 1950s and 1960s, fusion researchers and space scientists usually went their separate ways, responding to the goals of different patrons. However, significant inter-specialty work sometimes took place and was seen by plasma scientists as exemplifying the larger objectives of their community. One good example begins with the magnetic mirror fusion program at LLNL. Richard Post's early career was characteristic of many of the first wave of fusion researchers in having a limited background in plasma work. After taking his doctorate at Stanford University in accelerator physics, he worked briefly on the synchrotron program at the Berkeley Radiation Laboratory. In early 1952, Post redirected his career after attending a seminar given by Herbert York of the Berkeley Physics Department. As the first director of LLNL, York sought to identify a magnetic-confinement fusion concept that was different from the pinch at Los Alamos and the stellarator at Princeton. He became interested in straight-geometry machines, which had not originally seemed promising, since they were subject to fuel loss out of the open ends. York's interest in straight machines strengthened after discussions at the Berkeley seminar. After he reviewed the problem of containment in a linear machine, "One of the attendees, whose name is Kenneth MacKenzie … said something like 'Well, you know, the particles in cosmic rays are reflected by the field at the Earth's poles.'" This led York to conclude that "the way to do this in our geometry was to add coils at the end which would strengthen the field."[65] Shortly after the seminar, Richard Post accepted a position as head of an experimental program in CTR at LLNL. After evaluating various ideas for plasma confinement, he opted for the "magnetic mirror" approach, using strengthened fields at the ends. In August, 1953, he submitted a successful grant proposal to the AEC for three years of experiments.

Almost immediately, LLNL's mirror program highlighted the differences of research style that developed even within the fusion community. By late 1954, most of the AEC fusion programs had become deeply concerned with the numerous plasma instabilities that made it difficult to contain the hot plasmas. The LLNL team believed that its mirror machines would not be susceptible to relatively low-frequency instabilities affecting the plasma as a whole and understood from the MHD fluid perspective (although these caused them trouble in later years). Instead, they concentrated on "microinstabilities," relatively highfrequency and fine-grain disturbances, which were best investigated with the formalism of kinetic theory and the Boltzmann equation. In this effort, they enlisted the services of theorist Marshall Rosenbluth, then working at General Atomic.[66]

The Livermore effort was initiated fifty years after Carl Stoermer's calculations of charged particles in Earth's magnetic field and five years before James Van Allen's discovery of Earth's radiation belts. Van Allen was typical of the new workers in space science in much the same way that Richard Post was in fusion. During his graduate career at the University of Iowa, Van Allen concentrated on solid state and nuclear physics. After his war work put him in contact with rocket research at the Applied Physics Laboratory (APL) of Johns Hopkins University, he shifted his career toward the study of cosmic rays and the upper atmosphere, using rocket-borne instrumentation. After finishing five years at APL, he returned to the University of Iowa to set up his own research program. There, he received a grant from ONR, which he maintained for thirty eight years.[67]

On leave for the 1953–1954 school year, Van Allen took the opportunity to explore fusion research. Although the AEC expressed strong interest in Lyman Spitzer's proposal of the stellarator in 1951, it was concerned that the Princeton team lacked strong experimentalists. Spitzer appealed to Van Allen, suggesting that there was "no question" that the AEC would make an sizable award to Princeton "if you indicate that you will be available to supervise the experimental work, which will be the core of our program."[68] Van Allen took the position. After realizing that the practical goal of CTR would require much more time than the program had estimated, he returned to Iowa in August 1954 to continue his upper-atmosphere work and to help plan for the International Geophysical Year (IGY). Before returning, he recommended Melvin Gottlieb, a young Iowa physicist, as his replacement. Gottlieb, who had worked with Van Allen on cosmic ray research, originally came to Princeton on a one-year contract but stayed and, by 1961, was named the head of the Princeton effort (after Spitzer stepped down).

In his work for the IGY, Van Allen helmed a team that discovered the famed belts of radiation trapped in the Earth's magnetosphere. In this, his stay at Princeton proved to be serendipitous. As has been recounted in many places, the Explorer I and III satellite data was difficult to interpret. After initially suspecting that the satellite's detectors had failed during certain portions of the flight, the Iowa team concluded the detectors had saturated, indicating the presence of high-energy radiation. In his memoirs, Van Allen attributed this realization, in part, to his experience with fusion: "By virtue of my familiarity with an early paper of Stoermer and with magnetic field confinement of charged particles in the laboratory during my 1953–1954 work building and operating an early version of a stellarator at Princeton, I further concluded that the causative particles were present in trapped orbits in the geomagnetic field, moving in spiral paths back and forth between the northern and southern hemispheres and drifting slowly around the Earth."[69] During 1958 and early 1959, data from further US satellites—Explorer IV and the Pioneer series—led the Iowa team to conclude that there was a second belt of trapped radiation beyond the first.

Military interest in the study of nuclear explosions in space encouraged further connections between fusion research and investigations of the Van Allen belts. The first such experiment was proposed in October 1957 by one of Post's colleagues at LLNL, Nicholas Christofilos, who was working on a variation of the mirror machine that he dubbed the "Astron." In Christofilos's approach, high-energy electrons were injected into a linear containment chamber, where they set up a rotating electron layer to help confine the fusion fuel (the positive ions). The electron accelerator was about forty feet long and the containment tank ninety feet long (figure 4), making the Astron the largest and most expensive AEC fusion experiment of the 1960s.

Building on this idea, Christofilos speculated that a nuclear bomb could be used to inject high-energy charged particles into Earth's magnetic field, creating an artificial belt of radiation. Satellites could then be launched to study the degree to which the bomb plasma had been trapped. The US military was interested in such a test, since it was concerned that nuclear explosions, and consequent trapping of radiation, might interrupt radio communications. In addition, the military hoped detecting trapped radiation would be a means of uncovering Soviet nuclear tests.[70]

Van Allen joined the effort after receiving informal word that the Advanced Research Projects Agency would organize the high-altitude tests, which now went by the code name "Project Argus." Argus was a large program and included personnel at the University of Iowa, LLNL, Lockheed Corporation, the Stanford Research Institute, and the University of Maryland. The tests were conducted between August 27 and September 6, 1958, just five months after the discovery of the first radiation belts and at the same time as the declassification of fusion research. Three small fission bombs (with yields of about 1.5 kilotons) were exploded near Earth's south pole, about 300 km above the surface, followed by measurements by the Explorer IV satellite (launched on August 24). During the fall of 1958, Van Allen's group at Iowa analyzed the satellite data and found that the three atomic bursts had created three well-defined radiation belts. To present and interpret the results, a ten-day workshop was convened at LLNL in February 1959.[71]

Years earlier, Hannes Alfvén had found that the magnetic moment of a charged particle spiraling in a slowly varying field remained constant. In analyzing the Argus results, Livermore scientists T. G. Northrop and Edward Teller generalized Alfvén's analysis into a theory of "adiabatic invariants." They found that the trapped bomb products remained stable not only according to the "first adiabatic invariant of Alfvén" (the magnetic moment), but also maintained a stable longitudinal drift across the magnetic field lines, which could be explained by the existence of a second and third adiabatic invariant.[72] Northrop and Teller's analysis benefited the Livermore fusion program in a number of ways. By 1962, all three adiabatic invariants were confirmed in the LLNL mirror machines and, in designing future experiments, LLNL restricted its attention to machine

Fig. 4. A bird's-eye view of Nicholas Christofilos's Astron, around the time of its completion in 1963 at LLNL. The forty-foot electron accelerator sits horizontally at the bottom of the photo and the ninety-foot cylindrical tank on the left side is where the E layer was to be formed. Credit: Lawrence Berkeley National Laboratory, courtesy of Emilio Segrè Visual Archives, American Institute of Physics

dimensions that insured adiabatic invariance.[73] Richard Post cited the Argus results to support his claim that the magnetic mirror was a promising approach to fusion. In 1970, he wrote that "any doubt as to the potency of the mirror invariants should be dispelled by the observation that over a decade following the audacious Argus experiment that seeded the Van Allen belts with energetic particles some of these particles are still in evidence."[74]

Despite such collaborations between space scientists and plasma physicists, the communities stayed largely separate, as was made clear by the foundation, in 1962, of a professional organization for space science. Reflecting years later, Van Allen judged that space science was a "loosely defined mixture" of aspects of many disciplines.[75] It is not necessary to trace all of the migrations and dual associations of the early space scientists, since much of it does not involve the study of plasmas. What is most important for us is that the physicists who entered upper-atmosphere research during the 1940s and 1950s came to identify themselves primarily with geophysics and not with physics. Van Allen used his considerable influence to bring the nascent community into the AGU. Part of the reason for his shift of allegiance had to do with the APS's reception of his research during the 1950s. "In about 1956 (as I recall) I submitted a paper with L. J. Cahill on the measurement of the Earth's magnetic field at high altitude to *Phys. Rev*. This paper was rejected by the editor as an unsuitable subject for that journal … I then switched my allegiance to the *Journal of Geophysical Research*."[76] During 1959, the latter journal improved its operations by subsuming the older *Transactions of the American Geophysical Union* and by gaining a grant from the NSF. As Vice President of the AGU, Lloyd Berkner assured Van Allen: "I hope that you and your associates will submit a substantial fraction of your manuscripts to this periodical. I am sure that such work would enjoy a most expeditions processing."[77] Beyond Van Allen's experience, there was a general perception among physicists-turned-geophysicists that their work was not welcomed by the APS.[78] During 1960 and 1961, Van Allen worked with Homer Newell to incorporate the space physics community within the AGU. After two special committees of the AGU determined that a new section was desirable, the Planetary Sciences section of the AGU was created in April, 1962.[79]

Ionospheric researchers of the early 1950s had not been strongly attracted to the developing space community, partly because they preferred to use radio signals to probe the upper atmosphere. Their lack of enthusiasm for rockets (shared by the astrophysics community) was due to the fact that the earliest flights destroyed the on-board equipment and yielded data of limited value. By the mid-1950s, however, they recognized that rockets and satellites would become an important part of their research program.[80] Along with space scientists, they experienced a chilly reception from the APS. Therefore, during the 1960s, a significant portion of the ionospheric community became associated with geophysics; about a third of the community's publications appeared in the *Journal of Geophysical Research* and the *Journal of Atmospheric and Terrestrial Physics*.[81]

Space scientists (and ionospheric scientists) made little use of the APS. Although the DPP usually devoted two or three sessions to gaseous electronics, it did not immediately set aside special sessions for space plasmas. The DPP finally acted in April 1968, after it was informed of an application by space physicists to found a "Division of Cosmic Radiation" of the APS. In response, "It was suggested that earnest personal contacts be initiated by committee members with these individuals, in an attempt to convince them that the Division of Plasma Physics meetings represent a proper forum for their research and could well represent their interests to the Society." That fall, the DPP featured a special session on "Space Plasmas," a practice that continued in later years.[82]

Space-plasma physicists often reported their military-related work at AGU meetings but special meetings and symposia also became available. One series was organized by Billy McCormac, who, after gaining his PhD in nuclear physics in 1957, became a Lieutenant Colonel working in the Defense Atomic Support Agency (DASA). The earliest symposia, concerned with nuclear testing and verification, required a balance between military and scientific interests. When Van Allen was invited to participate in a 1963 symposium, McCormac advised him that a number of military analysts and planners would be present: "Some of these military representatives will be from quite important offices. Therefore, it is important that each speaker briefly emphasize in the introduction to his paper the military significance of what he is about to talk about and in the summary at the end of his paper briefly emphasize the military significance of what he has just talked about."[83] McCormac soon organized symposia that addressed the general interests of space-plasma physicists, including the trapping of charged particles, the large-scale structure of the magnetosphere, and the bow shock caused by the solar wind. After the first of these in August 1965, the series continued annually for thirteen years, sponsored by DASA, the Army Research Office, and the ONR. Martin Walt, of the Lockheed's Missile Systems Division, recalled them as "a legendary series of international space physics symposia."[84]

Shocks and Pinches

The development of aerospace engineering further encouraged the study of high-temperature gases. As with space science, though aerospace applications often served to segregate the plasma specialties, they sometimes enabled cross-fertilization and showed the wider aims of plasma physics. Aerospace engineering was greatly encouraged by the development of intercontinental ballistic missiles (ICBMs), which opened up new markets to the American aircraft industry. Aerodynamics engineers had long used wind tunnels to design aircraft, but ICBM technology required companies to expand their expertise to include the highly ionized gases faced by missiles as they reentered Earth's atmosphere. Responding to this, Lockheed Missiles and Space Company formed a partnership with the Stanford University Aeronautics Department in 1957. Nicholas Hoff, then head of Aeronautical Engineering at Brooklyn Polytechnic Institute, was attracted to Lockheed with a dual position: a professorship at Stanford and a consultancy with the company. Reaching the other way, from industry to academia, Daniel Bershader, Lockheed's head of high-temperature gas dynamics, took a joint appointment as an associate professor at the university.[85]

One of the most important pieces of equipment for the testing of materials and for graduate education, was a "hotshot wind tunnel" or shock tube. By using two chambers, one at high pressure and the other at low (containing the sample to be tested), an arc discharge could be used to heat the high-pressure gas further and break a membrane separating the chambers. Such machines were able to deliver high-temperature, hypersonic bursts of gas that, however briefly, simulated the effects faced by a space vehicle reentering Earth's atmosphere. Walter Vincenti and Ronald Smelt, working as a team designed and completed (in 1960) two such wind tunnels for their respective institutions, Stanford and Lockheed.[86]

After being awarded large contracts from the Navy and Air Force (for the Polaris missile system and the Advanced Reconnaissance System, respectively), Lockheed's research, development, and manufacturing effort grew by leaps and bounds. To help foster a university-friendly atmosphere, the company sponsored symposia on magnetohydrodynamics, held annually for seven years starting in 1956 and featuring talks by researchers from a wide array of plasma specialties. In the introduction to the symposium of 1958, Francis Clauser, of the Aeronautics Department of Johns Hopkins University, suggested that such meetings helped to mitigate the isolation brought by scientific specialization:

> As the various fields of science become more advanced, we tend to lose contact with our neighbors across the walls of specialized terminology and differences in point of view grow up between our different disciplines. Fortunately, an occasional event occurs which reverses this process and removes portions of these walls that separate us. At present, the great upsurge of interest in magnetohydrodynamics is serving to accomplish this by bringing together scientists from the fields of astrophysics, geophysics, gaseous discharges, electron tube and electron beam research, statistical mechanics, thermonuclear fusion, aerodynamics, and fluid mechanics. Each is being forced to learn the concepts of the others and is finding it a very stimulating experience.[87]

Arthur Kantrowitz (figure 5) was another important shock-tube researcher. In 1939, while working on low-temperature wind tunnel testing at NACA's Langley Memorial Aeronautical Laboratory, Kantrowitz and his boss Eastman Jacobs conducted one of the earliest fusion experiments. After reading Hans Bethe's work on stellar fusion, Kantrowitz and Jacobs sought to attain fusion reactions using ionized hydrogen as fuel, confined magnetically in a toroidal chamber about two feet in diameter and heated with a radio-frequency generator. After a few months of work, they found no evidence of fusion reactions and the director of Langley withdrew support.[88] Despite this disappointment, Kantrowitz maintained his interest in fusion and hot ionized gases. After he moved to Cornell University in 1946, as a professor of aeronautical engineering and engineering physics, he built a program of aerodynamic testing, aided by a team of about six graduate students and supported by ONR. Kantrowitz made improvements to the hotshot shock tubes but also employed electromagnetic shock tubes, which used capacitor discharges to produce still higher temperatures and velocities.[89] He used the new equipment to make another attempt at thermonuclear fusion reactions but was

Fig. 5. Arthur Kantrowitz and Alan Kolb in discussion at AIP's Northwest Science Writers Association Plasma Physics Seminar of May 1960. Credit: American Institute of Physics, Emilio Segrè Visual Archives

again disappointed, finding it fell well short of the temperatures required for fusion (reaching thousands of degrees rather than millions).[90]

Fusion was a passion of Kantrowitz's, but only a sidelight in his productive career of using shock tubes and other MHD devices to address applications in aerospace and the military. In 1955, he was approached by the aviation company Avco Manufacturing Corporation, which was seeking to expand its capabilities in space technology in response to the concern that the United States had fallen behind the Soviet Union. Kantrowitz took a two-year leave from Cornell to found the Avco-Everett Research Laboratory (AERL) in Everett, MA, which quickly gained a contract from the Air Force for the study of ICBM reentry. One objective was to develop new bomb-casing materials capable of withstanding such conditions. Another was to investigate the plasma sheath that formed around reentering space vehicles and interfered with tracking and communications. Throughout the 1960s, AERL used various shock tubes to produce short-lived bursts of plasma but also developed machines known as "plasma arc jet wind tunnels" that were capable of operating as long as a minute.[91] Although Kantrowitz maintained his interest in fusion during the late 1950s (without the benefit of AEC funding), the plasma instabilities found at higher temperatures convinced him to abandon the quest in 1963.[92] Instead, AERL developed machines using dynamic flows of plasma to generate electrical power. These "MHD generators" relied on electromagnetic induction and not fusion.[93]

Most members of the aerospace community belonged to the Institute of the Aerospace Sciences, which merged in 1963 with the American Rocket Society to form the Institute of Aeronautics and Astronautics (AIAA).[94] Many belonged to multiple organizations. Kantrowitz, for example, was a member of the AIAA, the AAS, and the DPP of the APS. Throughout the 1960s, aerospace engineers from Lockheed, Boeing, Convair, and many others were a significant presence at DPP and GEC meetings, accounting for about ten percent of the presentations.

Other researchers used shock tubes more centrally for fusion research. One was Alan C. Kolb of the Naval Research Laboratory (NRL) in Washington, DC (figure 5). In 1955, NRL gained funding to set up a new AEC fusion program. Kolb joined the program and, two years later, became head of NRL's Plasma Physics Branch. The NRL team used T-shaped shock tubes, in which a sudden arc-discharge across two arms of the tube heated a plasma in the third arm. Kolb improved the system by positioning one of the leads producing the initial discharge near the wall of the tube, such that the resulting magnetic force increased the shock to the third arm. At the same time, a set of coils in the third arm created a simple mirror configuration to better contain the plasma.[95]

Pinch machines were another type of pulsed device. The magnetic pinch effect, the tendency for a propagating burst of plasma to constrict as a result of its own magnetic field, was first described by Willard Bennett of Ohio University in 1934. One of the earliest applications of Bennett's idea to fusion was made by a team at Los Alamos National Laboratory (LANL) led by James Tuck. The first LANL pinch, the "Perhapsatron," was a toroidal machine that used a bank of capacitors to produce currents on the order of hundreds of thousands of amperes. Construction began in September 1953, but the machine was soon abandoned when it was found that its plasmas were broken up too quickly by instabilities.[96] Later experiments used linear plasmas and capacitor banks capable of faster discharges, with the objective of forcing the plasma to constrict so quickly that the instabilities did not have time to grow.[97] These experiments were also unsuccessful.

The earliest pinch machines became known as Z-pinches, since they directed a current along its central axis. In 1957, LANL explored a new pinch approach in which a pulsed current was delivered around a cylindrical plasma chamber. When a capacitor discharge sent a current around the cylinder, an axial field was induced in the plasma and started an imploding shock wave.[98] Because the direction of the current is conventionally specified with the variable "theta," this family of machine became known as a "theta pinch." Over the next eight years, LANL built four such machines in a series it designated as "Scylla." Partly as a result of the speed of compression in the Scylla machines, it was difficult to identify the neutrons produced as being of thermonuclear origin.[99] However, it was determined that Scylla achieved temperatures on the order of one hundred million degrees, which was deemed promising enough to attempt a reactor capable to delivering thermonuclear power. In 1965, plans were made for an enormous toroidal theta-pinch named “Scyllac.”

Theta pinches proved useful in many laboratories for the purpose of creating high-temperature, high-density bursts of plasmas, not just for fusion research but also for space science, astrophysics, and military applications. Alan Kolb’s team at NRL started an important program about the same time as Los Alamos. The NRL machine also featured a band of metal surrounding the cylindrical plasma chamber. The discharge of a capacitor bank initiated compression of the deuterium fuel, while mirror coils at either end of the tube limited losses. By the time of the Second United Nations Conference on the Peaceful Uses of Atomic Energy in 1958, Kolb’s team was able to present initial results of current and density measurements, as well as neutron production. Despite continued success, and after ten years of support, the AEC canceled the NRL fusion program and concentrated its investment on Los Alamos’s Scyllac. In 1965 Kolb founded Maxwell Laboratories, a defense contractor, in San Diego, CA. He returned to NRL for five years, pursuing other projects, including the use of shock tubes and pinches for military applications, especially the simulation of high-altitude nuclear explosions.[100] In 1970, he returned to Maxwell for a twenty six-year career.

The plasmas produced by shock tubes and pinch machines were of interest not only to fusion and military researchers but also space physicists. A good example can be found in the early career of Charles F. Kennel. After gaining an undergraduate degree in astronomy at Harvard University, Kennel received a PhD from Princeton University’s Department of Astrophysical Sciences in 1964. During his time in graduate school and through 1967, he worked at AERL. There, in 1966, he made an important contribution, working with Harry Petschek, addressing anomalous losses of particles from the Van Allen belts. Satellite measurements showed higher rates of loss than was predicted by theoretical analyses based on simple particle collisions. Kennel and Petschek turned to the kinetic theory to explain the losses, adopting a model was closely related to the one developed by Rosenbluth and Post for their analysis of a microinstability in LLNL’s mirror machines. Kennel and Petschek’s analysis of “pitch-angle scattering”—interactions between plasma waves and particles, which change the pitch angle of the particles—gave predictions in reasonable agreement with the satellite data.[101]

During 1967 and 1968, Kennel expanded on this work by making important contributions to the analysis of “collisionless shock waves.” Occurring only in highly ionized plasmas, these shocks are based, not on particle-particle Coulomb interactions, but on wave-particle interactions. The possibility of collisionless shocks had first been pointed out in a theoretical analysis published in 1950 by Frederic de Hoffmann and Edward Teller. After elaborations by many other theorists, laboratory experiments were conducted during the late 1950s and early 1960s, including one at AERL. A breakthrough came in 1962 and 1963 when satellite measurements found evidence of collisionless shock waves in the form of a sharp plasma boundary on the sunward side of the Earth.[102] Kennel produced pioneering analyses of collisionless shocks, working first with Roald Sagdeev and then with Petschek.[103] According to Kennel, the "golden age of collisionless shock research" extended from 1964 to 1974. Part of this period's success, he said, resulted from the "marvelous collaboration" between researchers working on plasmas in the laboratory and in space. The theoretical analysis of collisionless shocks (which usually required numerical simulation) could be compared to the experimental results from pinch and shock-tube experiments at facilities such as LANL, AERL, and NRL. This overlap between space science, aerospace, and fusion research ended in 1974. This happened, first, "because the financial support for laboratory experiments disappeared when interest in magnetic pinch fusion waned." Second, the space physicists developed their own laboratory programs and also benefited from *in situ* measurements of the solar wind made by spacecraft.[104]

Quiescence and Confusion

The 1960s were unique for being a time during which fusion scientists, after the rush of the 1950s and before to the rush of the 1970s, were attracted to general, fundamental plasma studies. In light of the disappointing results from the fusion experiments, many researchers eschewed the complex geometries of the toroidal fusion machines and sought instead to work on smaller machines with simpler geometries. The AEC funded a number of modest experimental programs at the national laboratories and at universities. Harry S. Robertson of the University of Miami started one such program to isolate basic plasma waves using a simple linear machine. In 1966, Robertson organized a small conference at his home institution, inviting about twenty fellow researchers who worked on a family of plasma losses termed "anomalous transport." In the foreword to the volume published from the conference, AEC staff member William C. Gough made a clear statement of the AEC's interest in such research:

> The U.S. Atomic Energy Commission recognizes that a solid understanding of plasmas will be required before controlled thermonuclear power can be achieved. Therefore, the U.S. controlled fusion program emphasizes not only a detailed understanding of the physics of high temperature plasmas and the means for confining and heating them, but studies of a basic nature in the broader aspects of the science and technology of plasmas. These latter studies, particularly at universities, are also important for training the next generation of investigators who will be needed to contribute new ideas to controlled thermonuclear research.[105]

The Q-machine represents the paradigm case of AEC support of fundamental plasma experiments. At the Princeton Plasma Physics Laboratory, Nathan Rynn and Nick D'Angelo developed the Q-machine (the "Q" standing for "quiescent") in 1960, using a linear geometry and alkali metals to create fully ionized plasmas at relatively low temperatures. Although such machines were not of direct interest to fusion research, since they did not use thermonuclear fuels, they allowed experimentalists to isolate plasma effects that had been swamped by larger effects in the fusion machines. One of the most important effects isolated by the Princeton group was a new class of plasma wave. Plasma scientists had already identified the electrostatic oscillation of electrons ("plasma oscillations") and the vibration of electrons and ions due to the propagation of electromagnetic waves ("whistler waves" and "Alfvén waves"). D'Angelo, with Alfred Wong and Robert Motley, isolated examples of a fourth general class known as "ion-acoustic waves," the electrostatic oscillation of the plasma ions.[106] Also at Princeton, Francis Chen used the Q-machine to track down the "resistive drift instability," a microinstability plaguing the fusion experiments. Using a theoretical analysis based on a two-fluid model (in which electrons and ions are treated with separate equations), Chen identified a means of stabilizing such turbulence by adding a "magnetic shear" circling the plasma and thereby twisting the field lines. In 1966, working with graduate student David Mosher, Chen isolated drift instabilities in a Q-machine and demonstrated that the disturbances were reduced significantly by magnetic shear.[107]

The Q-machines were used to identify a wide range of plasma effects, at facilities in the United States, England, the Soviet Union, and West Germany.[108] Earl Tanner, historian of the Princeton program, noted that their contribution "was far out of proportion to the modest commitment of resources that they represented."[109] Speaking in 1996, plasma theorist Norman Rostoker recounted the importance of the basic plasma program and how it was swept aside by renewed enthusiasm for the goal of achieving practical fusion energy:

> When I was working at General Atomic in the late 1950's, most sensible people in the field thought that we should stop building gadgets and things that looked like they'd become reactors and just make measurements on good experiments so that we knew that we didn't have foundations of sand. And, as a matter of fact the whole program did pause to do that. At Princeton, they invented the Q-machine and that was actually the longest-lived basic research program in this field. And at General Atomic, we invented something called the T-machine which was along the same lines. It made a well-defined plasma on which people studied waves and things like that for years. And there was a period where some basic research was done. But it didn't last very long. The pressures to get on with it were great.[110]

Indeed, at the end of the 1960s, the AEC withdrew support from many university plasma programs. Harry Robertson closed his plasma laboratory at the University of Miami and migrated to a different field of physics (theoretical statistical mechanics). Reflecting on the late 1960s, Robertson recalled: "The four AEC laboratories were money-hungry and arrogant. Each claimed it was wasteful to spend money anywhere else, since each was on the brink of solving the

Fig. 6. Norman Rostoker, during the time he worked at General Atomic (1956–1967). Credit: American Institute of Physics, Emilio Segrè Visual Archives

thermonuclear problem, if only the funds were available." Regarding the fusion researchers, Robertson felt that "the pressure on them was high because of their promises, and those of us in the universities could not get them to act on our conviction that no significant progress would be made by throwing money at the problem, without first understanding the underlying physics. This is one of the reasons I left plasma research."[111]

During the same time that the fusion community conducted basic experiments using fully ionized alkali-metal plasmas, it continued the pursuit of thermonuclear reactions using fusion fuels in a wide array of machine designs. This diversity introduced separations between the fusion researchers themselves. Norman Rostoker (figure 6) offers a good introduction to such lines of fracture. As was typical of many fusion researchers, Rostoker felt that "all the other problems in plasma physics are relatively unimportant compared to fusion."[112] It would be a mistake, however, to conclude from this that the fusion scientists presented a unified front. This is exemplified by Rostoker's abiding interest in the alternative fusion concept of “field-reversed configuration.” In 1967, after working eleven years at General Atomic, Rostoker moved to Cornell University, where he founded a new research effort that combined ideas from two of his close colleagues and friends: Nicholas Christofilos of LLNL and Alan Kolb of NRL. In his Astron mirror machine, Christofilos sought to create a cylindrical layer of electrons, leading to the generation of a magnetic field that doubled back on itself, thus attaining “field reversal.” Despite sixteen years of labor and a large budget, Christofilos was unable to reach the goal. Rostoker suggested that high-power beams be injected into the Astron using the beam technology that Kolb had developed at NRL, but Christofilos rejected the idea on the grounds that such beams might damage the machine. Working at Cornell, Rostoker took it upon himself to use Kolb’s beam technology on a variant of Christofilos’s approach in order to establish a field-reversed configuration. And indeed, by 1971, the Cornell team was able to demonstrate that this concept had promise, by creating reasonably stable field reversal in their machine.[113] Nevertheless, this did not help the Astron program, which was canceled by the AEC in 1972, shortly after Christofilos’s death.

Rostoker’s reliance on beam physics contrasted with most of the fusion community in departing from the “adiabatic approximation,” in which the particle orbits, spiraling in the confining magnetic field, are much smaller than the dimensions of the machine (as was the case in LLNL’s mirror machines). In Rostoker’s approach, the injected particles would have orbital radii much larger than the machine dimensions. Commenting in 1996, he noted:

> There’s a group of people called accelerator physicists and there’s a group of people called plasma physicists. And they each have their own magic and their own inventions. And they kind of like to stick to them. They’re not very different from people in business. The people in accelerator physics, they don’t know so much about this adiabatic approximation and they think it’s very limited. If you look at a book on accelerators, you’ll never find one with a toroidal magnetic field. There’s no accelerator ever built—there’s lots of them in toroidal shape but never with a toroidal magnetic field. What on earth could you want that for? You look at a book on plasma physics today, or fusion, you won’t find one without a large toroidal magnetic field.[114]

During the 1960s, similar separations were evident throughout fusion research. Different laboratories worked on different machine concepts, each of which entailed different experimental difficulties and theoretical approaches. Because the experiments were still relatively small during the 1960s and because the results did not point toward any particular concept, the AEC funded a wide range of initiatives. This often elicited complaints from Congress, since it appeared that a great deal of money was being spent on a program that lacked a focus.

But to some extent, this diversity of approaches was celebrated as indicating the health of the overall enterprise. In 1970, Richard Post published a long review article that provided a reasonably complete taxonomy of magnetic confinement machines: open-ended or closed, symmetric or non-symmetric, and featuring currents inside the plasma or not. Rather than bemoan this state of affairs, Post stressed that the long list of machine types indicated scientific vitality of the field and improved chances for reaching the goal of fusion power. He presented fusion research as a sort of evolutionary development, affirming that "there is no such thing as *the way* to controlled fusion power; there are instead *approaches*—several of them—and new ones have tended to be proposed with every advance in understanding (or every roadblock that seemed to frustrate a given line of attack)."[115] In speculating on the future, Post listed three technical requirements that any successful magnetic-fusion concept must satisfy: a field topology that is able to contain the plasma pressure; insuring that individual particle orbits are adiabatically invariant and will not encounter the machine walls; and the identification and suppression of plasma instabilities. In order to determine which machines satisfy these conditions, Post suggested that "there must be carried out a Darwinian survival of the fittest." After giving an exhaustive list of instabilities, from both the MHD and statistical-mechanical perspective, Post asserted that the objective of fusion research was to study "the origin and anatomy of these beasts" and ultimately control them.[116] Post's biological metaphors might have been interpreted as reflecting a mood of gloom in the fusion community. But his article gave upbeat conclusions, stressing that the community benefited from healthy competition. Referring to his three technical requirements, Post claimed that "there are systems, perhaps several, that give high promise of coming through, if not unscathed, at least unconquered." With the improved understanding of magnetic confinement and plasma instabilities, "the question is no longer: Can fusion be achieved? The questions are instead: How soon can it be accomplished—and by which approach?"[117]

At New York University, the applied mathematician Harold Grad also celebrated the diversity of fusion research, but went on to note the dangers this posed to the community. Grad observed that the growing pains of the program were compounded by the fact that the fusion scientists had communicated poorly. The members of the different laboratories were preoccupied with different experimental problems and theoretical interpretations. As an example, he noted that the Princeton stellarator group was largely concerned with MHD instabilities and so concentrated on fluid models, whereas the Livermore mirror group had greater concern with effects that were analyzed with kinetic theory. Another contrast was that, on the one hand, the stellarator and the mirror shared the fact that they were "low-beta machines" (low plasma pressure) and, in such cases, theorists began by inquiring about the orbits of individual particles. On the other hand, high-beta machines, such as the Los Alamos theta pinches, usually were checked for equilibrium and stability before particle orbits were considered. As a result of such differences, the different fusion groups often talked past each other: "even though the scientists attended the same meetings, there was essentially no communication."[118]

Grad went on to stress that plasma physics should not be a loose confederation of sub-specialties that worked in isolation from one another. He applauded a decision of 1967 by the AEC's assistant director of CTR, Amasa Bishop, to form ad-hoc panels that combined scientists from different parts of the fusion program. In this way, scientists working on one type of machine would be reminded of the theoretical and experimental issues faced by other groups.[119] At a 1970 meeting of the AEC's Standing Committee on fusion research, Grad developed this point by contrasting two general perspectives on fusion research, which he called "the vertical approach," and "the horizontal approach." If a particular type of fusion machine promised quick success, then Grad agreed that the community could concentrate on developing a particular theoretical model, or small group of models. This was the "vertical approach." However, if the development of fusion proved to be a long-term project, then the community should continue to investigate many plasma machines and commit itself to a complete study of plasma waves and instabilities, as well as a rigorous mathematical development of plasma physics. In Grad's view, such a "horizontal approach" was called for by the situation faced by the community of 1970. In this case, it was not possible to bring the different plasma behaviors under a single theory. Instead, Grad wanted to map and compare the different areas of plasmas and develop theories that were appropriate to each.[120]

History was to intervene to dispel not only Post's optimistic visions of fusion diversity but also Grad's cautious calls for greater coordination. At the IAEA's Third International Conference on Plasma Physics and Controlled Nuclear Fusion Research in August 1968, Soviet scientists announced new results from the tokamak, a toroidal machine that was similar to Princeton's stellarator but featured a somewhat different magnetic field configuration. The tokamak results were significantly, even breathtakingly better than anything accomplished by the international CTR effort up to that point. A year of uncertainly followed, during which time the international community digested the news. A British team brought their own diagnostic equipment to Moscow to check the temperatures attained in the Soviet machines. In August 1969, 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 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."[121] And indeed, shortly after Bishop's letter, the British team announced success. Within the next year, tokamak programs were started at Oak Ridge National Laboratory, Princeton, and MIT. Over the next sixteen years, the bestiary of fusion machines was virtually cleared out. Already in 1970, the AEC canceled the mirror program at Oak Ridge and the stellarator program at Princeton. In 1977, the Department of Energy also canceled LANL's Scyllac program and, nine years later, the titanic LLNL mirror program. After these extinctions due to lost funding, the tokamak was virtually the only class of machine remaining. Nevertheless, the bestiary of plasma instabilities was as diverse as ever and caused the tokamaks continued difficulty as they became ever larger and more expensive.[122]

Really Existing Plasma

In 1969, Harold Grad believed that plasma physics was "developing into a recognized academic discipline"[123] Considering the wide-ranging plasma work that had been done immediately before the foundation of the DPP and during its first decade, it might have been easy to agree with him. Such a sentiment might have been reinforced by reading the NRC's Pake Report of 1966. The Plasma Physics Panel reviewed virtually every plasma-related study, including fusion research, space physics, astrophysics, gaseous electronics, and plasma dynamics, thus suggesting that the plasma community had brought together all of the plasma specialties under one roof.

Such gestures toward plasma physics as a general study or discipline held some truth, as the examples of the present review have sought to demonstrate. At the same time, some of the tensions that we have noted are also evident in the Pake Report. The Plasma Physics Panel's judgment of the different plasma specialties varied considerably, especially in terms of what it deemed fundamental plasma physics. In a letter to Arthur Ruark (Chief of the AEC's CTR Branch), the chairman of the Panel, Marshall Rosenbluth, noted that because plasma physics had "not yet attained respectability in the eyes of those working outside the field … the primary objective of this report was to stress the interesting scientific questions in the field." Rosenbluth went on to warn Ruark that this effort created tension within the committee: "I was also a little perturbed to notice a sharp difference of opinion within the Panel between the fusion and non-fusion members. I am sure that you will have no difficulty in noticing various evidences of this dichotomy in the report."[124]

Indeed, it is not difficult to find lines of division. The eight-person panel included representatives from a number of plasma specialties but was dominated by those with fusion experience. Three members—Richard Post, Marshall Rosenbluth, and William Drummond—had devoted most of the careers to fusion. Two others—Melvin Gottlieb and David Rose—had significant experience in other plasma fields but had since moved on to fusion. Gottlieb was then the head of the Princeton fusion effort but had worked in space physics with Van Allen. David Rose worked on fusion feasibility studies in MIT's Department of Nuclear Engineering but had extensive knowledge of gaseous electronics, having been a student of Allis and Brown. Three other members were from specialties other than fusion: Arthur Kantrowitz and Eli Reshotko were physicists working in aerospace engineering and Peter Sturrock of Stanford University was a mathematical physicist who had recently turned his attention to space and astrophysical plasmas. In light of the strong representation of fusion on the panel, it is not surprising that its report tended to see work on fully ionized gases and their associated instabilities as being on the research frontier. Although "the basic set of plasma equations is well known," the panel asserted that "the richness of dynamical phenomena which may emerge from these well-known equations is so great and the phenomena so incompletely understood that we feel it to be quite obvious that many plasma problems deserve to be considered as 'pure' physics."[125]

The panel gave its highest evaluations to fusion research and work on astrophysical and geophysical plasmas, which shared an interest in fully ionized gases. It argued that fusion research had been "the greatest impetus to plasma physics in the last decade" and that it was "inherently a strongly physics-oriented program." To insure that "important new physics results" could be attained in fusion, federal funding had to be increased, since it was "inadequate to meet the competition" from other national efforts.[126] Even though the growth of plasma physics had been partially motivated by applied goals, astrophysical and geophysical research showed that the new discipline "would nevertheless be an important branch of physics even if there were no such applications in sight," since "such study is essential to a quantitative and even a qualitative understanding of a wide range of important natural phenomena" (such as solar flares, the solar wind, the magnetosphere, and geomagnetic storms). The panel concluded that the importance of such work indicated that "a much greater research effort is warranted in coming years."[127]

The panel gave comparatively mild evaluations to plasma research associated with lower-temperature plasmas or established applications. This was especially noticeable regarding gaseous electronics. The panel judged that, as "the oldest branch of plasma physics," gaseous electronics was not at the center of the discipline's current advances. "Such familiar things as light sources and vacuum-arc processing constitute a very large application that will presumably benefit in a gradual and unspectacular way from plasma advances." Compared to the dynamical behavior of high-temperature plasmas, the atomic effects investigated by gaseous electronics research were "somewhat incidental." The panel went on to say that "gas discharges constituted an important sub-field of physics from 1900 to 1930, but by 1940 scientific interest in the field had waned considerably." Because of this, and because "most of such activity is under industrial support," the panel did not recommend an increase of funding.[128] The specialty of plasma dynamics was also given relatively low priority. Because applications such as direct conversion appeared to be approaching feasibility and because it made use of relatively straightforward MHD theory, the panel judged it to be an "important field of technology in which it will perhaps not be necessary to increase the level of physics input." Similarly, the panel judged that very little of ion rocketry or plasma propulsion entailed basic physics and that whatever fundamental questions did arise were too difficult, since the flows were not tractable to theory.[129]

Despite the panel's weak evaluation of certain areas of plasma physics, it also warned that the different plasma specialties relied on different sources of funding and were therefore segregated from one another. “Since the practical objectives of the various agencies may be quite different, there has tended to be too little coordination.” It added “that there has been too great a fragmentation within plasma physics with insufficient contact among those interested in different plasma applications.”[130] As a step toward coordinating and interconnecting plasma research, the panel suggested that laboratories be encouraged to work on more than one research area. It also made a plea for “increased coordination and consultation between the supporting agencies” to insure the support of “plasma-physics studies of general interest which do not correspond precisely to the missions of a particular agency.” In particular, the panel recommended an increased role for the National Science Foundation.[131]

This plea for better coordination between the plasma specialties was no doubt encouraged by Plasma Physics Panel member Peter Sturrock. For 1965–1966, Sturrock served as chairman of the DPP and seems to have had a special concern about the fragmentation of the plasma community. Under his stewardship, the DPP revised its bylaws to include a new and more ecumenical statement of purpose: “The object of the Division shall be the advancement and diffusion of knowledge regarding assemblages of charged particles of natural or laboratory origin—including their radiations, oscillations, and stability; their transport, collective and wave properties and their interaction with matter and with radiation.” Of particular importance is the removal of the term “highly ionized gases.” Sturrock also made repeated efforts to establish a general prize for plasma physics. After he brought up the issue at a DPP meeting of November 10, 1965, the minutes note, “This subject has come up before and some members are not enthusiastic,” with James Drummond questioning whether such a prize would encourage progress in plasma research. Five months later, after “Sturrock again brought up the question,” the committee agreed at least to investigate “how other Division who have prizes feel about them.” The James Clerk Maxwell Prize was not established until 1975, when Alan Kolb (of Maxwell Laboratories) agreed to “contribute $3,500 plus $500 expenses annually for the next five years.”[132]

Sturrock also sought to make astrophysicists and space physicists feel welcome in the plasma community. In 1965, he and Lyman Spitzer organized a special session on astrophysical plasmas at the 1965 meeting of the DPP.[133] The joint session with the AAS at the DPP meeting of November 1965 was a small affair made up of five invited papers on astrophysical topics but marked the DPP’s more formal (though still limited) engagement with astrophysical plasmas. During the summer of 1966, Sturrock organized a three-week summer school on “plasma astrophysics” in Varenna, Italy, warning that “the application of plasma physics to astrophysics has fallen into arrears” and announcing that the school’s primary objective was to foster dialogue between the two communities. At the same time, he expressed disappointment that there was not enough time available to include the participation of space physicists. “Reluctantly, I had to decide that it would be necessary to eliminate the extensive and fascinating body of material concerned with solar-terrestrial relations.... This material is reviewed regularly in a series of international conferences on 'space science' which—rightly or wrongly—is coming to be regarded as an autonomous scientific discipline."[134] As noted above, the DPP's engagement with space research took longer to materialize and its first session on "Space Plasmas" was held at its 1968 meeting.

Applied scientists also sought to build bridges with the larger community. Plasma Physics Panel-member Arthur Kantrowitz had a desire, shared with many other scientist-entrepreneurs of the period, to bridge the gap between research seen as pure and as applied. In a contribution to the Committee on Science and Astronautics of the US House of Representatives, Kantrowitz cautioned that at the universities there was "a vast difference between the stature of basic science and the stature of applied science" and that this discouraged the latter.[135] As a remedy, Kantrowitz suggested that federal agencies act to ensure that centers of applied science (including government laboratories and industry) be integrated more closely with the American educational system. To balance the stress on basic science at the universities with a concern for applications at centers such as AERL, Kantrowitz recommended "legislation enabling these agencies to fund educational efforts in applied science to be conducted outside the universities with university cooperation."[136]

Despite such efforts at greater coordination between the plasma specialties, lines of funding for plasma physics did not change significantly in the years following the Pake Report. Although the NSF made piecemeal interventions into general plasma physics, it remained without a funding category until 1970, when it lumped plasma physics with atomic and molecular physics. By 1973, the NSF devoted $34.9 million to the Physics Section, only $1.1 million of which went to plasma physics projects, a figure that was dwarfed by the total federal outlay for fusion research of about $80 million.[137] Furthermore, it is clear that the NSF support for plasma physics was motivated by renewed excitement in fusion research. The NSF's annual report of 1971 prominently featured a plasma program at the University of Texas at Austin. The program, originally funded by a consortium of Texas power companies, had been started seven years earlier by Plasma Physics Panel–member William Drummond. The project funded by the NSF was headed by Kenneth W. Gentle, who had arrived at the University of Texas just five years earlier. Although the NSF introduced the program by stressing its interest in basic waves and instabilities, it noted that studies of how plasmas dissipate energy "may be crucial to controlling plasmas for thermonuclear fusion generation of electrical power."[138] Indeed, within two years, Gentle's laboratory boasted a major AEC tokamak program.

At the height of the rollback of fusion funding during the 1980s, NSF Director Eric Bloch removed plasma physics as a funding category. A decade later, Barrett Ripin of AIP (after a long career at NRL), fought successfully to get a funding category for plasma physics reinstated at NSF. Reflecting then on the state of the discipline, he noted that "there is no funding agency that feels that the scientific integrity of plasmas is theirs to nurture."[139] Ripin's comment accurately characterizes the previous four decades. While it would be an exaggeration to claim that the funding patterns of plasma physics entirely explain its poor coordination, cognitively and institutionally, it would also be dangerous to underestimate the effect of patron relationships. Establishing and maintaining funding depended on at least two things: the institutional imperatives of the different funding agencies and the conceptions of fundamental physics held by segments of the physics community. Both issues affected the deliberations of funding agencies and advisory committees to limit the collaborative research that was called for by so many observers.

Plasma physics had not one but two "fundamental problems." First, the general physics community did not consider plasma physics as being anywhere near the research frontier. Its elaborations of classical theory, however sophisticated, made plasma physics seem to have barely left the nineteenth century. Second, the plasma physics community itself, each part focusing on different regimes of temperature and density, disagreed about what constituted basic research. The stress on high-temperature ionized gases by the fusion community relegated low-temperature plasma research to the second tier of an already embattled discipline.

Fusion funding had a decisive effect on the young discipline. During the 1960s, the AEC supported a small but significant number of basic plasma experiments that were not directly associated with the fusion effort but concentrated on the identification of basic waves and instabilities. At the close of the decade, most of these initiatives were swept away, as thermonuclear fusion again appeared to be an attainable goal, a goal soon made all the more imperative by the looming energy crisis of the 1970s. In the wake of the Soviet tokamak results of 1969, the fusion program was pursued with renewed vigor, no longer by Cold War technocrats, but by managers hoping to demonstrate that science could contribute to the public good. In 1972, the Plasma Physics Panel of NRC's Bromley Report doubled down on the Pake Report's stress of highly ionized plasmas as the community's research frontier. At the same time, the Panel stressed the social relevance of controlled fusion, predicting that "the scientific feasibility of fusion power will be demonstrated around the end of this decade" and "could see its first economic application in the last years of the present millennium."[140]

As the fusion community continued to rely primarily on AEC funding, the other plasma specialties went their own way. Plasma physicists working in astronomy and astrophysics, having the strongest ties to "pure physics," benefited from an Astronomy Section at the NSF. Those working in space science also drew on NSF funds (as part of the Astronomy Section) but continued to rely heavily on military sponsors and NASA. Aerospace engineers also remained strongly tied to military and industrial sponsors, as did gaseous electronics. Although some applications were shared between different areas of plasma study (such as MHD power generation and waves in the upper atmosphere, pursued by both gaseous electronics and space science), the different applications and funding sources of the specialties usually acted to increase their fragmentation. After reviewing the agencies supporting plasma research, the Plasma Physics Panel of the Pake Report concluded that "support is based almost entirely upon project goals, and very little money is available for general studies in plasma physics that would be of use to a wide spectrum of applications."[141]

None of this is to say that the plasma community of the 1960s lacked success. By the end of the decade, plasma physics, seen as a general study of the fourth state of matter, could boast of identifying a wide array of fundamental plasma effects, from the partially ionized plasmas of gaseous electronics to the highly ionized plasmas of fusion research and Q-machines. Plasma scientists had also made numerous connections between phenomena in space and astrophysical plasmas and laboratory experiments conducted in fusion and aerospace research. But plasma physics, seen as the discipline dominated by fusion research, was encumbered with an uncertain past and a future of ever-increasing promises, budgets, and disappointments. In the face of this, the AEC was either unable or unwilling to pause long enough to support the larger community. No other funding agency picked up the slack.

It might be tempting to celebrate the diversity of the plasma specialties as somehow similar to the varied human cultures studied by anthropologists or the geographically isolated animal species studied by Charles Darwin. At the close of his gargantuan study of particle physics, Peter Galison enthuses that "it is the *disorder* of the scientific community—the laminated, finite, partially independent strata supporting one another; it is the *dis*unification of science—the intercalation of *different* patterns of argument—that is responsible for its strength and coherence."[142] It is clear from Galison's choice of words that he intends to make a conclusion not just for high-energy physics, and not just for physics, but for science generally.

Such a vision of diversity, or cheerful disunity, does not apply to plasma physics. The funding sources relied on by the community served not to enable communication between the specialties, but to drive them apart and to encourage a raft of biases and disagreements, both with the general physics community and within the plasma community itself. To celebrate the diversity of plasma communities during the 1960s would be to turn a blind eye to the circumstances that enforced their isolation. The American scientific institutions of the postwar years effectively fragmented this area of physical research as a result of narrow conceptions of fundamental physics, abiding institutional imperatives, and a lack of any funding agency that considered the health of the discipline as a whole. In the face of this, plasma physics as a general study was little more than a phantom that haunted the pages of review articles and NRC reports. Not all of these affirmations of a larger plasma community can be explained away as mere rhetorical flourishes intended to strengthen the legitimacy of a weakened discipline. They were also a sort of regulative ideal, a resource that sometimes encouraged and enabled scientists to conduct studies of basic plasma behaviors and to make connections between areas of specialization. These relatively few efforts, especially notable in the 1960s, serve as a reminder that diversity is not the same thing as compartmentalization and that scientific pluralism cannot be built on a foundation of island monocultures.

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