"Swords into ploughshares": Breaking new ground with radar hardware and technique in physical research after World War II

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A survey is offered of applications to fundamental physical research, in the years immediately following World War II, of the instrumentalities developed for radar during that war. Attention is given to radar astronomy and radio astronomy, linear and cyclical accelerators, microwave spectroscopy, molecular beams, nuclear magnetic resonance, electron paramagnetic and ferromagnetic resonance, measurements of resistivity at high frequencies in metals and of second sound in helium II, and to the concepts of information and signal-to-noise ratio as basic to the design and analysis of experiments. In conjunction with this survey, consideration is given to the autonomy of physics as a knowledge-producing enterprise, framed as a question of continuity in research directions. As that question implies a baseline, the survey of postwar applications is preceded by a survey of those prewar directions of physical research requiring the highest available radio frequencies. Some 500 references are given.

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I. INTRODUCTION

"Physicists are sometimes guilty of being a little conceited when comparing engineering and physics," the editor of the *Journal of Applied Physics* observed early in 1939. "They feel that progress in engineering is limited by the work of the physicist and that they, therefore, are the important cogs in the wheel." (Hutchisson, 1939). This conceit found much reinforcement in the following years through the physicists' contributions to the revolutionary weaponry developed during the Second World War (Baxter, 1946; Suits, 1947; Kevles, 1978, pp. 302–323). But at that war's conclusion physicists felt not merely heightened self-esteem, but also some embarrassment by the public repute their war work had brought them. As I. I. Rabi (1945) observed, in trying to keep his fellow Americans from being misled by the physicists' wizardry at weaponry,

> The embarrassment of the physicists stems not only from the fact that they are unaccustomed to being courted with such ardor, but also from their realization, admitted readily by four out of every five, that in the past five years, apart from the development of certain techniques which may be useful in later research, the progress of the science of physics has been less than moderate. The same profound questions which furrowed the brows of physicists before the war and forced them to spend long days and nights in their laboratories are still with us.

The physicists' self-esteem demanded that they now, taking hold of those new techniques, pitch themselves back into fundamental research with even greater intensity than they had mustered for their military-technical work.

However proud American and British physicists were of their contributions to the advance of radar technology into the microwave range during World War II, and however attached they were to the view that new technologies are ever the result of fundamental physical research—and thus that technology owes far more to physics than physics to technology—the debt which they were then incurring to engineering, in the form of radar hardware and technique, as they broke new ground in their research activities following World War II, was simply too great to pass without remark. Thus, for example, when Charles Townes, as Sigma Xi lecturer for 1951, presented microwave spectroscopy on various U.S. college and university campuses, he concluded by pointing out that

> We have been discussing the many types of fundamental information that have been obtained as a result of the technological development of radar. The historical development in this case is just the opposite of what most "pure" scientists like to regard as typical. They would argue that in most cases pure science develops principles and ideas, which are then applied to technology by other scientists and engineers and incorporated in equipment and devices. But in the case of microwave spectroscopy,

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> the reverse has occurred. The equipment and devices were developed first, and pure science owes a considerable debt to technology (Townes, 1952, p. 287).

The need to make the point that Townes here makes—and it was repeated often enough to rank as a rhetorical trope[1]—surely sprang from an awareness, however tacit and ambivalent, that traditional distinctions between physics and technology had become blurred during the course of the war. Now more than ever before, physicists were lionized. But as Rabi recognized, it was now much less as magi of the mysterious cosmos and much more as wizards of wonder-weapon warfare (Rabi, 1945; Lang, 1948, p. 82; 1959, p. 216).

The institutional bridges and personal unions between "pure science" and advanced military technologies that had been created in and for the war continued in various forms during the postwar years, significantly altering the self-image, and ultimately the knowledge-producing activities of the physicists—first and foremost the American physicists.[2] Thus Townes (1952), following the passage quoted, immediately added: "There is, however, every reason to believe that microwave spectroscopy will in time be able to repay this debt" to technology. And indeed Townes himself, by the invention of the maser, three years later, contributed very largely to that repayment (Bromberg, 1991; Forman, 1992, 1995a).

It is not, however, the purpose of this paper to wrestle over whether these "radar swords" were being beaten into "scientific ploughshares,"[3] or, on the contrary, were merely being returned to the armorers to receive a finer temper and sharper edge. My aim here is more modest: to present a multiplicity of examples, drawn from diverse fields of physical research in the years immediately following the Second World War, in which radar hardware and technique played an important role—even an indispensable role—in achieving significant experimental advance. These examples bear out what has often been stated by the physicists involved: the artifacts of wartime radar technology, and the experience of creating and of using them, were indeed of great significance for the postwar pursuit of fundamental physical research—of significance even beyond the realm of experimental technique, affecting theorists and theorizing as well.

Striking testimony to this latter, indirect influence upon the direction and content of postwar conceptualizations of the physical world comes from an unlikely witness: Julian Schwinger, a theoretical physicist who is justly regarded as one of the most "unphysical" of the postwar generation in the distance of his work from experimental data and its domination by formal and aesthetic considerations. On receipt of a Nobel prize for his contribution to the extraordinarily successful quantum electrodynamics created in the years immediately following the war, Schwinger (1965, p. 154) confessed, in the impersonal third person, that at the MIT Radiation Laboratory

> He first approached electromagnetic radar problems as a nuclear physicist, but soon began to think of nuclear physics in the language of electrical engineering. That would eventually emerge as the effective range formulation of nuclear scattering. Then, being conscious of the large microwave powers available, Schwinger began to think about electron accelerators, which led to the question of radiation by electrons in magnetic fields. In studying the latter problem he was reminded, at the classical level, that the reaction of the electron's field alters the properties of the particle, including its mass. This would be significant in the intensive developments of quantum electrodynamics, which were soon to follow. . .

If this is true of Schwinger, what is to be said of all those other physicists, both theoretical and experimental, immersed in radar research projects and operational research efforts during the war?[4] In short, this paper joins a growing consensus that a closer look at how individual physicists were influenced professionally by their wartime radar work would provide an important new dimension to the expansion of physical research in the de-

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[1] The debt of "pure physics" to war-inspired technology had been noted by Walter Gordy (1948, p. 668) and still more emphatically by C. J. Gorter (1951) in the opening address at a conference on radio-frequency spectroscopy attended by Townes.

[2] Most of the recent historical scholarship on mid-twentieth-century physics and related sciences has engaged with such questions primarily. Thus: Baracca (1989, 1993), Bromberg (1991), Cassidy (1994), Dennis (1994b), DeVorkin (1989, 1992, 1995), Doel (1995), Eckert and Osietzki (1989), Eckert and Schubert (1986), Feffer (1992), Forman (1987, 1989, 1992, 1995a), Galison (1988), Galison and Hevly (1992), Galison, Hevly, and Lowen (1992), Gillmor (1986, 1989), Goldberg (1989), Heilbron (1989) Hevly (1987, 1995), Kevles (1990, 1992), Krige (1995), Leslie (1993), Needell (1987a, 1987b, 1989, 1993, 1995), Olwell (1991), Ordoñez and Sanchez Ron (1995), Pestre (1990, 1992, 1994), Pickering (1989a, 1989b, 1989c), Schweber (1988, 1989, 1992, 1994), Seidel (1983, 1987, 1989, 1994).

[3] The metaphor is taken from C. K. Coogan (1970): "Purcell *et al.*, having beaten their radar swords into scientific ploughshares. . . ."

[4] For Schwinger see Schweber (1994, pp. 275–374) and Oliner (1984, pp. 1027–1030). Similarly, Felix Bloch stated in his Nobel Prize lecture (1952, p. 209) on nuclear induction that "The acquaintance with radio techniques during the war suggested to me still another and much simpler way" to observe nuclear magnetic moments. Edward Purcell (1986) took as the theme of his Morris Loeb Lecture in physics the various and numerous elements of radar technology that came to be important as instruments or foci of postwar physics. The wartime radar work of Charles Townes, an experimentalist well schooled in theory, and its role in his postwar research, is explored by Nebeker (1993, p. 61–92).

cade following World War II.[5]

Though I here set aside the questions of motive and goal for the postwar expansion of physical research, the present inventory offers data pertinent to such questions, and more particularly to the issue of the autonomy of mid-20th century physics as a knowledge-producing enterprise—an issue standing at the focus of much of the historical literature cited in footnote 2, above. The survey here undertaken does bear upon this issue of autonomy, framed in the following terms: to what extent were the physical questions investigated by radar hardware and know-how in the immediate postwar period already present as problems in the science of physics before the war?; to what extent were physicists in the years immediately following the war returning to just those researches that they had been pursuing prior to the war, but now with technical means—specifically the technology for producing and "handling" microwaves—to carry out measurements that they had previously attempted or contemplated but found unapproachable?

To answer these questions requires another survey complementary to the intended inventory of *postwar* applications of radar hardware and technique: a survey of prewar physical research that employed the highest available radio frequencies. Such a baseline survey should, moreover, pay particular attention to the prewar physicists' perceptions of the limitations imposed by the available technology upon the physical problems accessible to investigation. Section II is, then, such a baseline survey. The incompleteness and bias of this baseline may be excused, at least in part, by the absence of like surveys in the physical and historical literature, and by the need to provide some background, however sketchy, for the postwar applications following upon it.

II. SOME PREWAR RESEARCH PROGRAMS LIMITED BY THE AVAILABLE HIGH-FREQUENCY TECHNOLOGY

Although the application of high-frequency radio and microwave tubes and techniques to physical research during the two decades before the Second World War remains largely unsurveyed, the development of those tubes and techniques themselves has drawn considerable historical attention, beginning with Henry Guerlac's (1947, Chap. 8) admirable official history of the MIT Radiation Laboratory (Dennis, 1994a), and continuing with numerous studies and reminiscences of the origin of the magnetron and the klystron and of high-frequency electronics generally.[6] What is not often recalled in this literature is that the interwar push to ever higher frequencies was to a considerable extent a reconquest of that very frequency domain—but now with a technology permitting its full and effective exploitation—in which Hertz (1893, 1994; Bryant, 1988) had first demonstrated the existence and properties of electromagnetic waves.[7] Where radio technologists soon abandoned that domain, backing way down to audio frequencies, in order to produce the intense, coherent waves on which stable, reliable communication systems could be built (Aitken, 1976, 1985; Brittain, 1992), physicists went forward in frequency to "close the gap" in the electromagnetic spectrum between Hertz's centimetric waves and those of a few tenths of a millimeter wavelength that the infrared spectroscopists had succeeded in isolating—to close it using Hertz's technique of spark-generated wave trains.

The leading American researchers, E. F. Nichols and his collaborator J. D. Tear (1923, 1925),[8] carried the canonical Hertzian electric dipole vibrator to diminutive dimensions and great technical refinement—as indeed magnetrons would be carried in the early 1950s in the endeavor to generate these same frequencies (Forman, 1995a). In Russia, however, Aleksandra Andreevna

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[5] More recently the Forum for the History of Physics has sponsored sessions at the American Physical Society meetings in Seattle, WA, 25 March 1993, and Washington, DC, 12 April 1993, titled, respectively, "Radar, World War II, and Postwar Physics," and "Applied Research, World War II, and Postwar Physics" (Bulletin **38**, 610–611 and 938). The former session comprised papers by Brebis Bleaney (1993), "Centimeter-wave spectroscopy of gases and electron paramagnetic resonance," R. V. Pound (1993a), "Microwave technology, nuclear magnetic resonance, and radio astronomy," Lillian Hoddeson (1993), "Crystal rectifiers and the invention of the transistor," and Woodruff T. Sullivan, III, "Wartime radar and postwar English and Australian dominance in radio astronomy"; the latter session included a presentation by William Higinbotham, "The postwar legacy of wartime instrumentation development in physics research at Brookhaven."

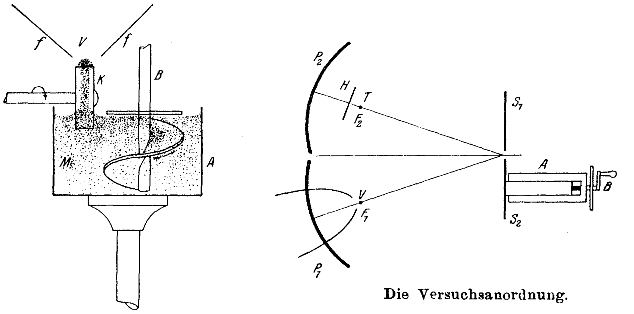
[6] The greatest part of this literature is on, or pointed towards, the history of radar, to which Louis Brown's bibliography (1994) is the best and most comprehensive guide—until his book appears. Access to the historical literature on high-frequency electronics generally is to be had through the bibliographies of Finn (1991) and Shiers (1972) and the recent paper by Swords (1994). In that literature the following works are particularly extensive or thorough: Bell Telephone Laboratories' (1975–1985) six-volume technical history of the Bell System; Tyne's (1977) history of the vacuum tube to 1930; Gebhard's (1979) history of naval radio-electronics at the U.S. Naval Research Laboratory; Hackman's (1984) history of sonar in the British navy; Snyder's (1986) history of radio-frequency work at the US National Bureau of Standards; and the collection of some thirty papers on the history of microwave technology edited by Saad (1984).

[7] Recently, Buchwald (1994) has published an admirably detailed account of the development of Hertz's thought and experiments.

[8] The latter paper bears on p. 17 the note: "This paper was being orally presented by Professor Ernst Fox Nichols at the meeting of the National Academy of Sciences on April 29, 1924, when his death occurred on the platform of the auditorium." It is preceded by an obituary by Nichols' cousin, the astronomer Philip Fox (1925).

Glagoleva-Arkadieva (1924a, 1924b; Vavilov and Joffé, 1947) carried into practice a simple but ingenious idea of her husband, V. K. Arkadiev (Dorfman, 1970), for meeting the ever-incompatible demands of high frequency and high radiated power (Fig. 1). The trough, in this drawing of her apparatus, contains machine oil in which metal filings, sieved for rough uniformity of size, are kept suspended by the stirrer *B*. The rotating wheel *K*, about a centimeter wide, dips into the suspension and retains on its rim a "tire" of this mixture, which is thus continuously transported between the two wires *F*. Across this gap the spark, generated by an inductor, passes. Oscillations are excited in the filings—in effect small dipoles—which then radiate at their fundamental and low harmonic frequencies. The measurement technique—a standard one, due to Boltzmann—was interferometric: the "mass vibrator" *V* was placed at the focus F_1 of one parabolic mirror P_1, while the detector *T* was placed at the focus of the second. (*H* is a removable screen.) Oscillations in detector output result from displacement, by means of a micrometer screw, of one half (S_2) of the plane mirror, relative to the other half. The results of this simple arrangement were impressive: many frequencies were measured in the range 0.1–50 mm. But as with these waves nothing more was to be done, the physicists' attention turned in the late 1920s and early 1930s to the field of vacuum tube electronics, which was then gradually working itself up toward the centimetric domain.

Two features of this latter development—one historical, one physical-technical—are of special pertinence to the following discussion of prewar physical research dependent upon such high-frequency techniques. First, historically, this development accelerated greatly towards the end of the interwar period. "Publications on magnetrons surged dramatically beginning in 1933," James Brittain (1985) has noted, "and remained at a high level until 1940, when publication of papers ceased because of wartime secrecy rules." Waveguides themselves were still quite novel in 1936/37: the first public announcements that such devices should and did work were made by George C. Southworth, of Bell Labs, and Wilmer L. Barrow, of MIT, at a May 1936 meeting of the Institute of Radio Engineers, though Southworth had been submitting patent applications since 1933.[9] The klystron was described in print only shortly before war broke out in Europe, while the cavity magnetron, invented shortly after, was one of Britain's most carefully protected technical secrets.



Der Massenstrahler.

Die Versuchsanordnung.

FIG. 1. Apparatus with which A. A. Glagoleva-Arkadieva generated and measured electromagnetic waves in the range 50 mm to 0.1 mm. Reproduced from Glagoleva-Arkadieva (1924b, p. 155).

Consequently there was very little time for the most advanced techniques to "trickle down" into the practice of physical research before such work itself began to be put aside in order that the physicists might devote themselves to war work. For the prewar practice of physics, the only high-frequency oscillators available off the shelf were those manufactured for short-wave radio communications, none of which went much above 100 megahertz. Vacuum tubes capable of generating oscillations in the range 100–1000 megahertz were available from RCA and Western Electric and from several European firms (Harvey, 1943), but the oscillators making use of them had to be constructed in-house, following circuit diagrams supplied by the manufacturer. And "for generating frequencies higher than 10^9 cycle/sec"—Gaylord Harnwell and Louis Ridenour (1940, p. 93) reported—"no tubes are now available commercially."[10]

Second—and here the historical melds with the physical-technical—at every stage of this development, the highest frequencies were attained only at the cost of diminished, indeed vanishing, output power. The advance of the technology was simultaneously an advance in the highest frequencies attainable and in the power attainable at lower frequencies. Consequently the history of radar in this prewar period is shaped above all by this circumstance—the trade-off between high frequency and high power (Guerlac, 1947, Chaps. 5 and 8; Brittain, 1985, p. 65). And considering that the strength of the radar signal returned to the sender diminished with the fourth power of the distance to the target, power inevitably took precedence. The necessity for this same compromise between high frequency and high power also played an important role in the application of high-frequency technology to physics, particularly in the development of particle accelerators for nuclear research.

A. Linear accelerators

In the late 1920s, as the nucleus was beginning to assume the role, taken over from the atom's extra nuclear

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[9] Guerlac's (1947, pp. 196–204, 237–238) discussion of the history of waveguides was already good; Packard (1984) improved on it substantially. Southworth's text (1950, pp. 8–12, 666) has some history, and the latter half of his volume of reminiscences (Southworth, 1962) is devoted to his work on waveguides.

[10] This report, as we see below in considering Reber's work, was overly discouraging—unless it was construed to refer to the order of magnitude.

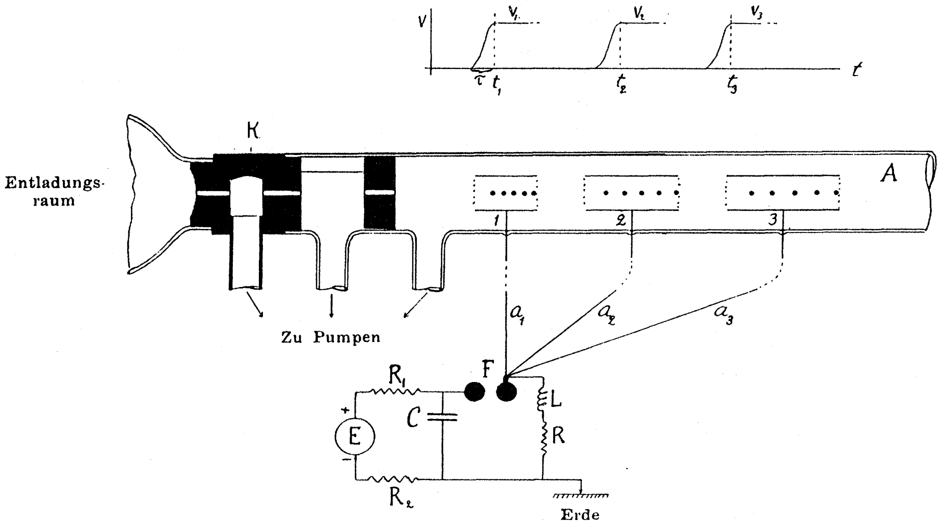

FIG. 2. Gustaf Ising's proposal, 1924, for a linear accelerator. Reproduced from Ising (1925, p. 2).

electronic structure, of principal research frontier, the view was also becoming widely accepted that effective investigation of atomic nuclei and their controlling laws would be possible only with "atom smashers," i.e., with yet to be invented machines generating beams of atomic particles more intense and more controllable—perhaps even more energetic—than could be obtained from even very large quantities of naturally occurring radioactive substances. Although it was only after 1930 that designing and building particle accelerators became a widely pursued "necessity" for nuclear physics, the first proposals for such devices, originating largely on the margins of the research community, date from the early twenties (Grinberg, 1975; Heilbron and Seidel, 1989, pp. 45–102).

In 1924 Gustaf Ising,[11] at the University of Stockholm, had a highly original idea quite outside his special field of research (the application of extremely sensitive galvanometers and electrometers to geophysical measurements). He surmised that large amounts of kinetic energy could be imparted to charged particles traveling down a long, straight metal tube in an evacuated chamber, by repeatedly applying a given accelerating voltage across a series of transverse gaps in that otherwise electrically shielded channel (Fig. 2). Ising (1925) proposed to use as accelerating potential the voltage "pulse" from a non-oscillatory spark discharge. By inserting a transmission line of appropriate length between the spark and each of the cylindrical electrodes ("drift tubes," as they would later come to be called), one could cause the elevated voltage to arrive at any given drift tube just after the ever speedier (positive) ions had passed into it—i.e., passed from the previous drift tube, at high positive potential, across the acceleration gap to this drift tube, then still at ground. (The form and arrival times of the voltage "pulse" at the successive drift tubes, 1,2,3, after traveling down delay lines, a_1, a_2, a_3, is indicated by the V-t graph in Fig. 2. Thus the 15 black dots within the accelerator represent not 15 different ions at a single instant, but the same group of five ions at three successive instants, t_1, t_2, t_3.)

Ising was well aware that the alternative to a non-oscillatory spark discharge as source of the accelerating voltage was a high-frequency oscillator. [In this case Fig. 2 would have shown alternate drift tubes connected to the opposite terminals of the oscillator output, and the ion bunches present only in every second drift tube.] But as Ising (1925, p. 3, note 2) then said of this alternative, "it would be difficult to provide the needed high intensity of these oscillations at the necessarily very small wavelength."[12] That the wavelength of any oscillator employed must necessarily be very small follows from the circumstance that the length of an accelerator of given energy—i.e., the lengths of the individual drift tubes in an accelerator with a given number of drift tubes (and hence acceleration gaps)—is proportional to the period, hence the wavelength, of the oscillation. (In Ising's chosen method, only the rise time of the voltage "pulse"

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[11] This Ising is not the (Ernst) Ising of the Ising model—for whom see S. G. Brush (1967). For this (Gustaf) Ising, see Beckman (1960) and Bond-Fahlberg (1993, p. 458).

[12] Ising followed the work on particle accelerators that began to appear in print three or four years after his pioneering proposal, and published, in Swedish, a lengthy and comprehensive review (Ising, 1933). He there stated (p. 173, note 1) that "a larger number of swarms of accelerated ions could be obtained per second if the traveling voltage pulse were replaced by periodic voltage variations, supplied, with suitable phase differences, to the accelerator cylinders. This possibility was considered also. But since the oscillations must necessarily be of a short wavelength (about ten meters), it then appeared rather hopeless to obtain an adequately large voltage amplitude." Mr. Henry Hanson, Smithsonian Institution volunteer, kindly provided this translation.

sets a minimum length for the drift tubes.) That the "intensity" of such continuous oscillations must be high follows not merely from the inverse relation between accelerating voltage and total length of an accelerator yielding a given energy, but, more significantly, from the circumstance that the string of drift tubes presents the high-frequency oscillator with RC losses that grow with the number (and the size) of the drift tubes.

Thus the number of acceleration gaps cannot be increased indefinitely—without multiplying the number of high-frequency "drivers"—and consequently it is also desirable to apply the highest feasible voltage to a smaller number of drift tubes. But an oscillator's output voltage V is itself a function of its power P: roughly, $V \sim (P \cdot Q)^{1/2}$, where the "quality factor" Q of the output circuit (the inverse of the energy loss in that circuit) is just what those additional and longer drift tubes diminish. This unfavorable situation is further aggravated by that steep decline in output power with frequency invariably characteristic of the upper frequency range of available oscillators. In the phrasing of scientists at the Research Laboratories of the Tokyo Electric Power Company, "Provided more powerful oscillator be used, shorter wave will become applicable and ions of greater energy may be obtained" (Tanaka and Nonaka, 1938, p. 36).

The scientific advantages of smashing atoms with electrons or protons, the simplest ions, rather than hitting complex nuclei with other complex nuclei, were generally apparent. But the resort to high-frequency oscillators to generate the accelerating voltage pushed physicists into using the most massive ions available: because the velocity of an ion of given energy is proportional to $1/\sqrt{m}$, the energy attainable in an accelerator of given length is proportional to $\sqrt{m}$. Otherwise put, increasing the mass of the accelerated ion was a convenient surrogate for increasing the frequency of the oscillator as a means for holding down the length of the accelerator. As Ernest Lawrence (Lawrence and Sloan, 1931, p. 65) would say in motivating the cyclotron by the limitations of the linear accelerator, "This arrangement is not applicable to speeding up electrons. . . . Even for protons the requisite length of a series of tubes becomes hardly practicable, but in this instance we have overcome the difficulty by causing, with the aid of a magnetic field, the protons to traverse circular paths back and forth between semicircular hollow plates upon which a high-frequency voltage is applied."[13]

Five years earlier than the experiments of Lawrence and his students, when the immediate task was not yet the attainment of the highest-energy ions, but rather merely to demonstrate the practicability of the principle of successive accelerations by a single high-voltage source, the use of a high-frequency oscillator obviated numerous experimental difficulties: timing a single high-voltage pulse so as to arrive at each acceleration gap in step with the ions was rather a trick; moreover, the intensity of the accelerated beam, which was "on" only for an instant following each spark discharge, would be painfully low. Thus Rolf Wideröe (1928, 1964, 1984), in undertaking to demonstrate Ising's principle in his doctoral research at the Technische Hochschule Aachen, 1925–1927, applied 20 kV twice, to a single cylindrical electrode, using a 2 MHz oscillator. Unsurprisingly, "Mainly the voltage was limited by the rated power of the [oscillator] tube."[14]

Upon publication, Wideröe's (1928) paper came into the hands of Ernest Lawrence, who, recently arrived at the University of California, Berkeley, was looking for a line of work. Enlisting his student David Sloan—who with his engineering background would prove a most inventive applier of high-frequency concepts to the acceleration of ions (Heilbron and Seidel, 1989)—Lawrence extended Wideröe's work from a mere proof of principle to a megavolt accelerator. With a maximum of 11 kV applied to eight drift tubes, Lawrence and Sloan (1931) obtained 90-kV mercury ions with an ease that clearly astonished them: the radial focusing of the ions by the electric field lines curving between the drift tubes was not anticipated, nor yet understood.[15] They then extended the accelerator to 21 tubes and got—or persuaded themselves that they had gotten—200-keV ions. Up to this point they had been relying on a mere 75-watt Radiotron UX-852—a 3-MHz tube that RCA had put out three years earlier for radio amateurs (Tyne, 1977, p. 329)—but, in typical Lawrence style, putting several times its rated voltage on the anode.

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[13] On Lawrence, linac, cyclotron, and the extent of indebtedness to Wideröe, see Heilbron and Seidel (1989, pp. 80–102).

[14] Wideröe (1928, p. 397; in reprinted version, p. 104) used a Telefunken RS-21-IV radio transmitter tube, developed for the German navy, whose power rating was probably a few hundred watts (Tyne, 1977, p. 441).

[15] In his admirably comprehensive survey of the first decade of accelerator concepts, Grinberg (1975, p. 816) quotes Ising (1933, p. 173): "Directly after publication of the principle, an acceleration tube was prepared for the purpose of carrying out a preliminary trial of this idea. Instead of canal rays, electrons from a hot filament were utilized. One of the students working with the author performed some experiments, but they were soon abandoned. . . . In addition, the author must acknowledge himself to be at fault in that at that time he was unnecessarily skeptical in evaluating the practical possibilities of the method, particularly in regard to the beam intensity. One reason is that it was possible to obtain only a relatively low repetition rate of spark discharges; a second, even more important reason is that it was expected that the swarms of ions would spread to the walls, as a result of which the number of particles would fall off roughly exponentially with the number of drift tubes." This translation was checked and corrected with the help of Mr. Henry Hansen.

With this success Sloan and Lawrence (1931) brought into play a 10-MHz Federal Telegraph water-cooled power oscillator, which they operated at just half its rated 20 kW. With 42 kV applied to 30 drift tubes, they produced 1.26-MeV mercury ions. A few years later, with six more drift tubes—but the resistance and capacitance greatly reduced by decreasing all diameters and increasing all separations—Sloan and Coates (1934) were able to get 80 kV out of the same oscillator, thus obtaining 2.85-MeV mercury ions.[16] But still, these remained essentially demonstrations, and little more was done in this direction in Lawrence's laboratory or elsewhere. Collisions of heavy ions with target nuclei were too complex to interpret, while the available high-frequency technology was not high enough to accelerate light, simple ions. Hartman and Smith (1939), reviewing the theory and experimental work on the "linear resonance accelerator"—they were themselves building one at Cornell—referenced only the work of Wideröe, of Lawrence and Sloan, of Sloan and Coates, and of Tanaka and Nonaka.[17]

Appraisals of the merits of a technique are always relative to its alternatives, and, as we saw, it was relative to Lawrence's other, concurrent scheme for producing "high-speed ions without the use of high voltages," the cyclotron, that the oscillator-driven linac soon ceased to seem promising. Still, the cyclotron was not without its own limitations. It was not suited to the acceleration of electrons, for these extremely light particles already show relativistic increases in their mass at energies less than a hundred thousand volts, and so fall quickly out of step with the high-frequency accelerating field. Neither was it a simple matter to extract from the cyclotron the ions it successfully accelerated, nor was it possible to form them into a narrow, well-collimated beam.

These latter considerations led Jesse Beams—a virtuoso experimentalist who had been Lawrence's collaborator at Yale before the fair-haired boy went off to El Dorado and he himself to the University of Virginia—to reinvent Ising's proposed method. Using a voltage pulse from a spark discharge and delay lines, Beams and collaborators succeeded in accelerating protons as well as electrons to over 1 MeV (Beams and Snoddy, 1933; Beams and Trotter, 1934; Snoddy *et al.*, 1937).[18] Their accelerator, as it stood in October 1936; appears in Fig. 3. The spark gap is seen on the far right, the ion source is in front of Beams, and the accelerator in its wooden cradle extends to his right in front of L. B. Snoddy and J. R. Dietrich, and beyond. (The rings around the first five drift tubes are probably not corona guards, but rather inductive loading to reduce the velocity of propagation of the voltage pulse.) Although no mention is made of these accomplishments in Beams' National Academy of Sciences biographical memoir (Gordy, 1983), they were an experimental *tour de force* that drew respectful attention at the time (Wells, 1938).

Meanwhile, back around San Francisco Bay, attention was focusing on improving high-frequency oscillators, in particular, on schemes for so increasing the electric fields generated in their output "circuits" that electrons could be accelerated to high energies in a single passage through them, thus obviating all the problems associated with multiple acceleration, whether in the cyclotron or the linac. At Berkeley, in Lawrence's Radiation Laboratory conventional concepts were pushed extremely hard. David Sloan developed a 75-kilowatt, 6-megahertz oscillator, at the end of whose "antenna," coiled in an evacuated metal box, the voltage swung from −800 kV to +800 kV (Heilbron and Seidel, 1989, pp. 116–121, 497–498).

At Stanford, W. W. Hansen (1936, 1938) introduced the unconventional concept of a cavity resonator—whose high Q he also demonstrated experimentally—intending to use it to build up intense electric fields with less powerful oscillators. Hansen labored to accelerate electrons with the field of such a cavity resonator (Bloch, 1952, p. 126) and dreamed of eventually producing 100-megavolt electrons by repeatedly recirculating them through this "rhumbatron" (Hansen, 1936; Heilbron and Seidel, 1989, p. 497; Norberg and Seidel, 1994). Such energies would become possible *without* recirculation, after the war, in consequence of the enormous increases in

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[16] Sloan and Lawrence (1931) had projected a 4.5-MeV linac (56 drift tubes times 80 kV) which was to be achieved by introducing power amplifiers between oscillator and accelerator—one amplifier to drive the first 36 drift tubes, another to drive the latter 20. In a curious anticipation of the proton linac built at Berkeley after the war by Alvarez and collaborators—originally projected to reach 1 GeV with twenty-five 40-foot sections—here too only the first, 36-drift-tube section was constructed. Both thus evaded the difficult problem of phasing successive sections.

[17] These do indeed appear to be the only detailed descriptions of such work, although brief reports were made of further efforts in Lawrence's lab with another apparatus (Thornton and Kinsey, 1934; Kinsey, 1936). It should, however, be mentioned, in order to avoid an incorrect impression, that the work of Tanaka and Nonaka did not represent the most important in the development of particle accelerators in prewar Japan. Indeed, as Hoddeson (1983, p. 4) points out, apart from Lawrence's Radiation Laboratory at Berkeley, the two most ambitious accelerator programs *anywhere* were in Japan, but elsewhere in Japan: at the Rikagaku Kenkyusho, Tokyo, and at Osaka University, in which programs cyclotrons and high-voltage dc machines (Van de Graaff and Cockcroft-Walton) were emphasized.

[18] Ising's original proposal was known only through Wideröe's (1928) reference to it. True, Ising (1933) reproduced and discussed it, but in a journal rarely seen outside Sweden. Consequently its real content remained unknown until E. O. Lawrence (1952) chose to feature our Fig. 2 in his Nobel prize lecture, delivered in Ising's Stockholm in December 1951—some twelve years after the award.

both frequency and power achieved through the wartime developments in radar—and also in consequence of a better idea than Hansen thus far had had about how to apply those increases. That idea—the traveling-wave linac (Fig. 4)—arose by a cross-fertilization between Stanford and Berkeley, a cross-fertilization that was itself a result of an early initiative by Ernest Lawrence in the interest of microwave radar.

American physicists had been concerned about the war in Europe from its outbreak in September 1939, but they became acutely concerned, and began looking for ways to contribute their talents to the advancement of military technologies, only when, within ten weeks in the spring of 1940, German armies seized Denmark, Norway, The Netherlands, Belgium, and most of France (Bryant, 1990). Lawrence was well aware of the efforts at Stanford to develop the klystron—recently invented there by Russell and Sigurd Varian (1939), on the basis of Hansen's concepts—into a practical and powerful microwave oscillator. He gave earnest of his concern by seconding to that effort David Sloan, his high-frequency expert (Heilbron and Seidel, 1989, p. 497). And a year and a half later, Sloan (1941), with a good grasp of all the resonant acceleration concepts that had been produced to that date, submitted a patent application for the acceleration of electrons by microwaves in a waveguide, discloaded in order to match the phase velocity of the microwaves—generated by a high-power klystron amplifier—to the velocity of the electrons.

B. Molecular-beam magnetic resonance spectroscopy

The power and fecundity of the molecular beam as an instrument of physical research had been demonstrated by Otto Stern at Hamburg University, 1923–1933. When I. I. Rabi came to Hamburg from Columbia University in the autumn of 1927 it was as a postdoctoral fellow in theoretical physics with Wolfgang Pauli. Gradually, however, Rabi was seduced by the molecular-beam

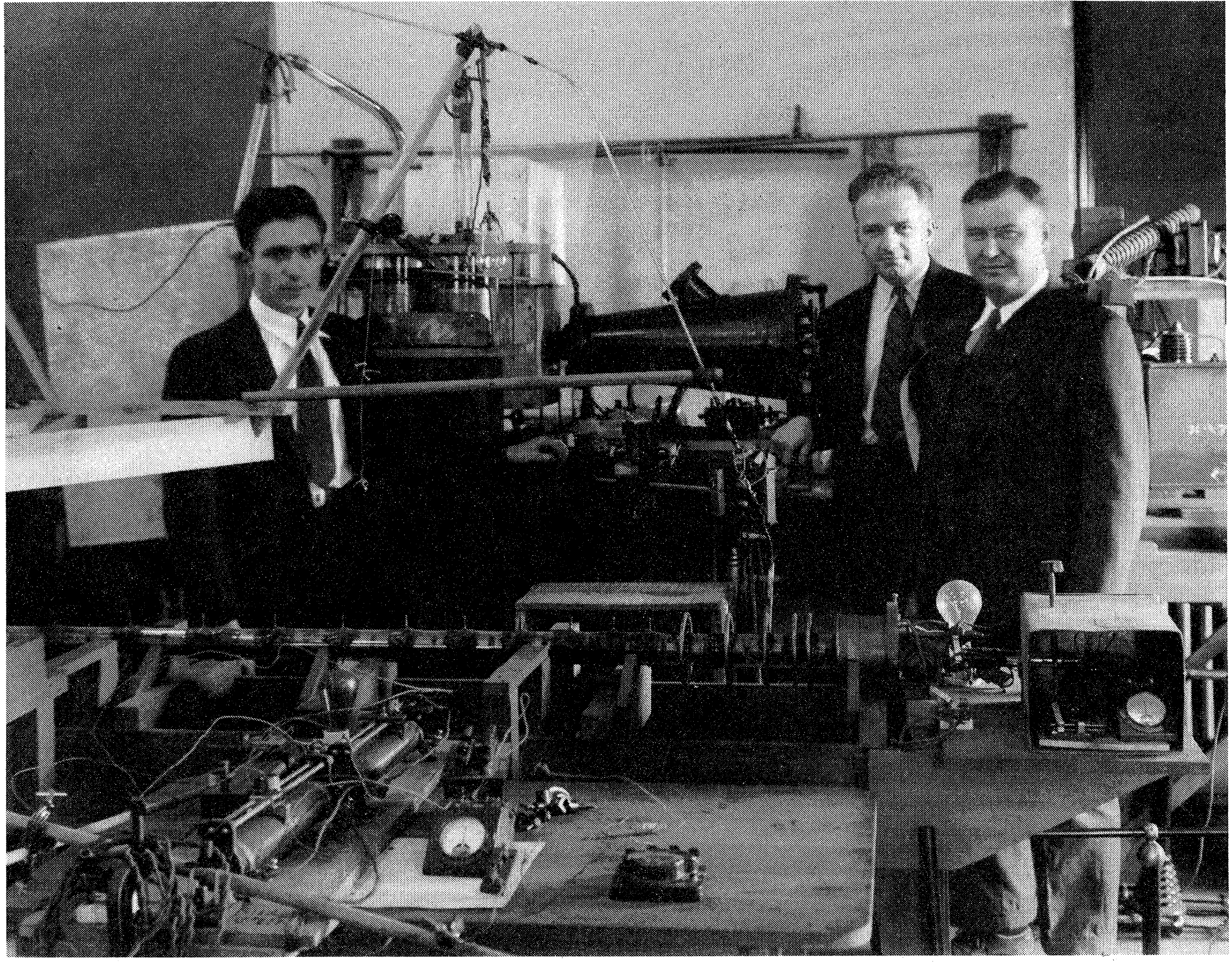
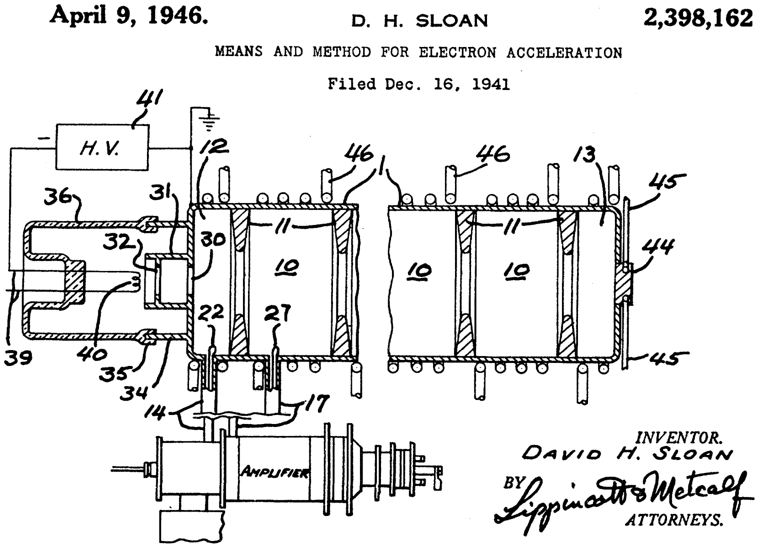


FIG. 3. Jesse Beams, right, and his collaborators at the University of Virginia stand behind their linear accelerator, October 30, 1936. (Science Service photo, Smithsonian Institution).



April 9, 1946. D. H. SLOAN **2,398,162**

MEANS AND METHOD FOR ELECTRON ACCELERATION

Filed Dec. 16, 1941

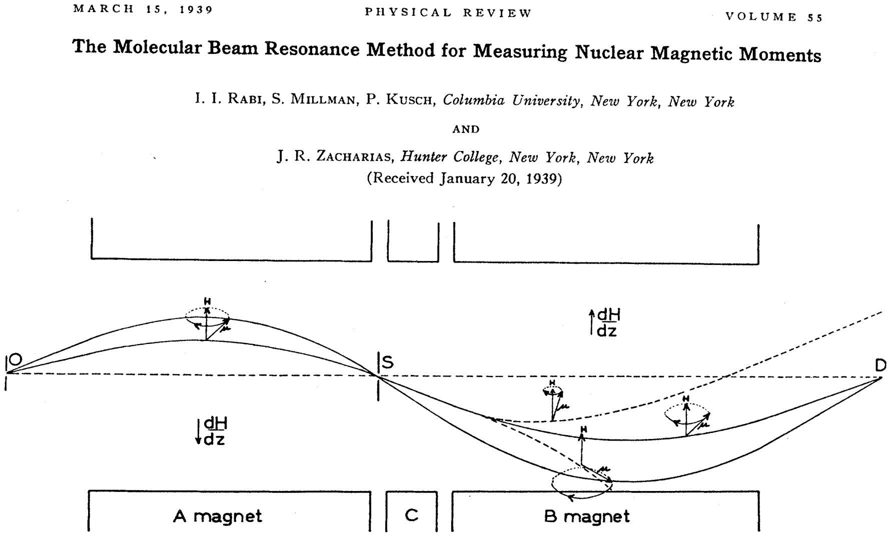
INVENTOR.
DAVID H. SLOAN
BY Lippincott & Metcalf
ATTORNEYS.

FIG. 4. Concept of a traveling-wave linear accelerator of electrons, as patented by David Sloan. Reproduced from Sloan (1941).

methods and by the easy, English-spoken-here atmosphere of Stern's laboratory. Rabi returned to Columbia in the fall of 1929 as a junior faculty member and, soon after, began to build up a laboratory and to attract a group of enthusiastic younger collaborators (Rigden, 1987; Ramsey, 1988, 1993). Their objective was the application of molecular-beam methods to the determination of spins and magnetic moments of nuclei.

The exceptional pertinence of Rabi and Company's enterprise to nuclear physics at this juncture can be better appreciated by recalling that accelerators were then only just beginning to chart nuclear energy levels, and that, prior to the acceptance of the existence of nuclear electric quadrupole moments, it was the general opinion that the four quantities, nuclear charge, mass, spin, and magnetic dipole moment, "together are necessary for a complete description of nuclear fields; whether or not they are sufficient has been a matter for frequent speculation" (Feather, 1936b, p. 78). Either way, the central importance of the spin and magnetic-moment data Rabi's group worked to provide is clear. And their central role in providing it: by the latter 1930s Rabi and Company were responsible for about a quarter of all experimental papers on "Hyperfine structure and nuclear moments" abstracted in *Physics Abstracts.* The other three quarters were based on optical spectroscopic techniques, the "traditional" and widely practiced method.

Through the 1930s, as the proliferation of cyclotrons was obliging most experimental nuclear physicists to educate themselves in radio engineering (Alvarez, 1987, p. 40; Heilbron and Seidel, 1989, p. 324 *et passim*)—while the development of particle counting techniques compelled an acquaintance with electronics generally (Hendry, 1984)—Rabi's molecular-beam group, although accounted nuclear physicists, made no significant use of either of those technologies. This circumstance was immediately altered when, in the autumn of 1937, Rabi *et al.* (1938, 1939) took the decisive step in the develop-



MARCH 15, 1939 PHYSICAL REVIEW VOLUME 55

The Molecular Beam Resonance Method for Measuring Nuclear Magnetic Moments

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AND

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(Received January 20, 1939)

FIG. 5. Schematic illustration of the molecular-beam magnetic resonance method. Reproduced from Rabi *et al.* (1939, p. 527).

ment of the molecular-beam technique, namely that to radio-frequency resonance spectroscopy (Rigden, 1985). Then they "acquired a copy of the *Radio Amateur's Handbook* and went to work" (Goldstein, 1992, p. 34).

The principle of the "molecular-beam resonance method," of which a detailed description appeared only in the spring of 1939, is given schematically in Fig. 5, taken from that publication.[19]

Apart from the restrictions imposed on the processes of beam generation and detection by the physical properties of the atoms and molecules employed (Ramsey, 1956, pp. 361–393), the range of application of this technique depended only upon the range of the radio spectrum for which stable, precisely tunable oscillators were available.[20] Indeed, Harnwell and Ridenour (1940, p. 91) single out "the molecular-beam technic of Rabi and his colleagues" as a direction of physical experimentation demanding the highest attainable frequencies. Subsequently Kusch (1955, p. 301), on receiving a Nobel prize for his postwar molecular-beam magnetic resonance (MBMR) researches, would recall "the prewar difficulty of generating rf power at high frequencies."

Among the data most eagerly sought through the MBMR technique were the hyperfine energies (or structure, or slitting) of the several isotopes of the various elements. These energy states arise through the interaction of the magnetic moment of the nucleus with the magnetic field produced by the swarm of extranuclear electrons; they correspond to frequencies in the range 150 to 15 000 megahertz (Hamilton, 1941, p. 334). Consequently, more often than not, they lie above 700 megahertz, i.e., above the highest frequency "conveniently obtainable in our laboratory" (Millman and Kusch, 1940, p. 438).

To a considerable extent this inaccessibility of the pertinent frequency, $\Delta\nu$, to direct measurement was obviated by clever exploitation of the Zeeman effect, the further splitting of the hyperfine structure by an applied magnetic field. Thus Rabi and his collaborators Polykarp Kusch and Sydney Millman obtained the hyperfine splitting, $\Delta\nu$, of the alkali atoms from the asymptotic value of that fur-

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[19] The molecules—either diatomic or monatomic (i.e., atoms)—stream from the oven O into the evacuated apparatus through a series of slits, and must traverse a gauntlet of three magnets to reach the detector D. The distribution of molecular velocities along the axis OD is essentially Maxwellian (with the mean velocity determined by the temperature of the oven), while the transverse velocities of the molecules have been reduced by the slits to a narrow range around zero. The fields of the first and third magnets, although they are in the same direction (indicated by the four vertical arrows H) and roughly equal in strength, are strongly inhomogeneous, with their gradients oppositely directed.

A small but fortunate fraction of these molecules—two such are indicated by the solid curves—have an appropriate combination of longitudinal and transverse velocity, and—most important—of net transverse component of magnetic dipole moment, such that they are "focused" by the inhomogeneous field of magnet A through slit S.

Beyond S is the C magnet, which has a uniform field and hence exerts a rotational torque, but no net transverse deflecting force upon the constituent magnetic dipoles of the molecules. Assuming that the molecules have gotten through the C magnet without any alteration of the component of their magnetic dipole moments in the direction of the magnetic field, the last magnet in this series, the B magnet, will return the molecule to the axis—sooner or later, depending on the strength of its (reversed) gradient. The important consequence, however, is that, as Kellogg, Rabi, and Zacharias (1936) had earlier discovered, the point of intersection with the axis, D—for here the detector is placed—is the same for all molecules, regardless of their velocity components. (The algebra is given by Ramsey, 1956, pp. 90–92.)

In the A magnet we see one of these molecules that will be "focused" through S and onto the detector; it is represented by its magnetic dipole moment μ, precessing at a fixed (quantized) angle around the direction of the magnetic field H. In the B magnet, this same molecule is shown as it has emerged from the C magnet—shown in three possible magnetic states: on the solid curve, unchanged from its original state; and on two dashed curves, trajectories resulting, respectively, from a larger component of its magnetic moment in the direction of the magnetic field (hence "defocused" toward the axis of the apparatus), and from a small, negative moment (hence repelled from the axis).

Such transitions between magnetic states are induced in the uniform field of the C magnet by a weak, radio-frequency magnetic field perpendicular both to the beam axis and to the C field. The probability of such a transition increases enormously when there exists a resonance between the rf field and the precession of one of the constituent magnetic dipoles of the molecule in the constant field of the C magnet, i.e., when, in quantum theoretical terms, the frequency ν of the rf field, multiplied by Planck's constant h, is equal to the difference in energy between the initial and the final orientations of the molecule in the C field: $h\nu=\Delta E=\Delta(\mu\cdot H_c)$. The fact of such resonance occurring is indicated by a drop in the number of molecules arriving at the detector D, and this drop, together with a knowledge of the rf frequency at which it occurred, constituted a precise measurement, hitherto unparalleled, of that energy difference and thus of the magnetic moment giving rise to it.

For clarity, Rabi *et al.* have here greatly exaggerated the distances between the poles of the three magnets. In the apparatus of these experiments the A and B magnets were about 50 cm long, while the opposing pole faces were separated by only 1 mm. (Each pole face was half of a circular cylinder, one concave and the other convex, having radii of 1.5 mm and 1.25 mm, respectively.) The slits were usually oriented vertically, the magnetic fields horizontally, in order to avoid the (velocity-dependent) gravitational deflection of the ribbonlike beam.

[20] Further problems arise when the dimensions of the magnetic resonance region in the apparatus are no longer much less than a wavelength of the radio waves employed, but such circumstances would present themselves only after World War II, in consequence of operation in frequency realms and at levels of precision inconceivable before the war (Ramsey, 1956, pp. 124, 139–140).

ther splitting, $\Delta\nu/(2i+1)$, where i is the number of quanta of angular momentum possessed by the nucleus of that particular element and isotope. "This happy circumstance," Kusch, Millman, and Rabi (1940, p. 768) wrote,

> is very important for these experiments because. . . . One can thus avoid some of the technical difficulties involved in the use of extremely high frequencies.

Still, one hyperfine splitting—the most fundamental, that of hydrogen, at about 1400 megahertz—escaped all the tricks of the MBMR method. From 1938 to 1941 many and unprecendentedly precise measurements were carried out by Rabi and Company on the hydrogen molecule and the hydrogen atom, using the MBMR method—measurements that revealed, *inter alia,* the electric quadrupole moment of the deuteron (Kellogg *et al.*, 1939), with its unsettling implication that the force between proton and neutron is not central.[21] But all that effort and technique notwithstanding, their only measurement of the hydrogen hyperfine splitting remained that which Kellogg, Rabi, and Zacharias (1936) had earlier achieved, with a mere 5% precision, by their outdated "nonresonant" methods (Rigden, 1983). Here then was a field of fundamental physical research whose practitioners were painfully aware of the limitations imposed by the available high-frequency technology.

C. Microwave absorption spectroscopy of molecules

Whereas Rabi and Company's need for precise control and measurement of their radio frequencies restricted them to the technology codified in the *Radio Amateur's Handbook,* a student of Neil H. Williams at the University of Michigan had earlier pushed the technology of radio-wave generation to frequencies some 50 times higher than Rabi's laboratory employed, in order to reach up from below to the smallest energy differences to be found in molecules. These energy differences, due in most cases to differences in rotation (angular momentum) of the molecule considered, had long been recognized as fine structure of optical and infrared spectra—just as the still smaller energy differences arising from nuclear magnetism had been recognized, and very roughly measured, as hyperfine structure in optical spectra. This frustratingly imprecise technique—pulling the desired number out of the last significant digit of a (*per se,* extremely precise) spectroscopic measurement—was, in the case of nuclear magnetism, made obsolescent by the introduction just before war's outbreak of Rabi's magnetic resonance method of *direct* measurement of that energy, bringing to bear upon such energy differences all five or six digits of radio-frequency measurements. Similarly, the first tentatives toward a *direct* measurement of the small energy differences in molecules were made still earlier, in Ann Arbor, with its long and uniquely strong tradition in infrared molecular spectroscopy.[22]

Infrared spectroscopy had been brought to Ann Arbor in 1911 from Friedrich Paschen's Tübingen institute by Harrison M. Randall (1954), who after taking command of Michigan's Physics Department in 1918 gradually added to its faculty a series of homegrown infrared spectroscopists: Walter F. Colby, W. W. Sleator, Ernest F. Barker, F. A. Firestone, Charles F. Meyer (Johns Hopkins Ph.D.), and, as a most important complement, molecular theorist David M. Dennison (Crane, 1980; Rigden, 1990). As they said of themselves (Meyer *et al.*, 1944, pp. 23–24), "In all, about one hundred and twenty papers dealing with spectroscopy of the infrared have been published, and this laboratory is generally recognized the world over as the principal center for such work."

Specifically, the idea of using microwaves to investigate the energy levels responsible for the fine structure of infrared spectra arose out of Ernest Barker's work in the late 1920s on the spectrum of ammonia. NH_3 was a favorite of the infrared molecular spectroscopists (Schaefer and Matossi, 1930, pp. 248–258) because easily observed—ammonia's infrared absorption is very strong—and because easily understood—the molecule is a pyramid with the three hydrogen atoms as base and the nitrogen atom as apex, having the relatively simple "symmetric top" rotational spectrum. Barker (1929) had found a puzzling doubling of the lines in one of ammonia's absorption bands, which Michigan's molecular theorist David Dennison then seized upon as evidence of a theoretically anticipated (Hund, 1927), but theretofore undemonstrated, "inversion" of such a pyramidal molecule. This quantum-mechanical transition between the states defined by symmetric and antisymmetric wave functions translates into classical mechanical terms as the snapping back and forth of the triangle formed by the three hydrogen atoms, from one side of the nitrogen atom to the other, rather like an umbrella turning inside out and then righting itself again.[23]

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[21] The first firm evidence that nuclei could possess electric quadrupole moments—dipole moments were excluded by the absence of negative charges in the nucleus—came from the optical spectroscopic observations of hyperfine structure by Schüler and Schmidt (1935). An indication of the physicists' resistance to such a departure of the nucleus from spherical symmetry is given by Feather (1936, p. 106–109). For the electric quadrupole moment of the deuteron's forcing an abandonment of the assumption of central forces between nucleons, see Peierls (1979, p. 195).

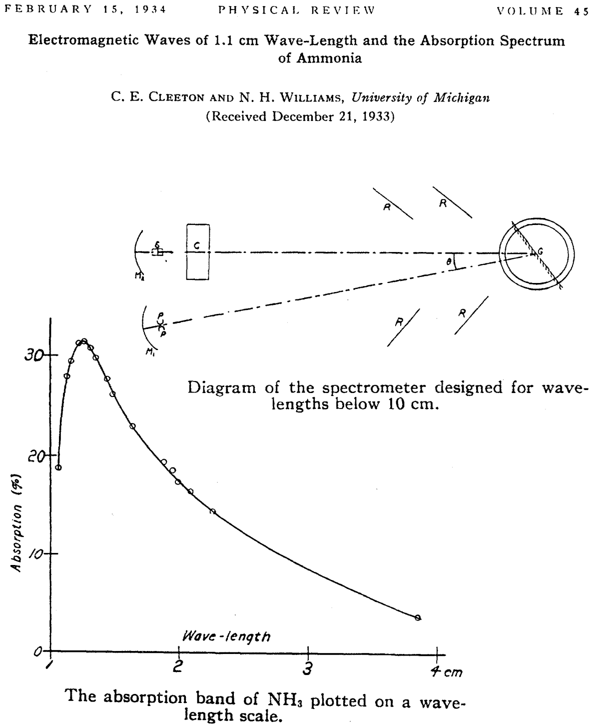
[22] An overview of the history of infrared spectroscopy is provided by a triplet of papers in the July 1977 issue of the Journal of the Optical Society of America: Genzel and Sakai (1977); Ginsburg (1977); Palik (1977).

[23] Barker (1929, p. 691) credits Dennison with suggesting this explanation of the doubling. When, however, Dennison (1931, p. 338) came to publish it, he was emphatic that such a quantum-mechanical doubling had been anticipated in 1927 "in a very important paper by Hund."

Further, Dennison and Hardy (1932, pp. 942–944) pointed to another band—i.e., another mode of vibration of the ammonia molecule—in which the barrier to inversion is lower, and in which, consequently, this doubling should also be present but so narrow as to be still unresolved by spectroscopists. They estimated the frequency separation ($\Delta E/h$) of the symmetric and antisymmetric states of the molecule as 24 GHz—what would eventually become the most famous frequency in microwave spectroscopy, for the transition in question, in the $J=3, K=3$ rotational state of the molecule, is uniquely strong (Gordy *et al.*, 1953, p. 128). In arriving at this estimate of 24 GHz they had averaged values for molecular parameters differing by as much as 50%, and so judged that "It does not appear probable that the accuracy with which we have determined this constant can be greater than 10 to 20 percent." Ironically, they had actually hit it to within 1%.

Randall, the dean of Michigan's infrared spectroscopists, brought into play his newly built, higher-resolution spectroscope, resolved the lines, and in the summer of 1933 reported the separation as 20 GHz, again with a precision of 10 to 20 percent (Wright and Randall, 1933). Meanwhile, however, Dennison, together with his more mathematical colleague George Uhlenbeck, whom he had interested in more exact calculations of the dimensions of the molecule, came to the idea that ammonia should strongly absorb electromagnetic waves of this frequency.[24] These 24 GHz, a frequency difference a little less than what the infrared spectroscopists had previously been capable of resolving, were a little more than any frequency previously produced by an electronic oscillator (i.e., any but Hertzian oscillators, generating short, damped wave trains unsuited to spectroscopy). The challenge of this intriguing prediction was accepted by Neil Williams, and he set graduate student Claud Cleeton to the task in the summer of 1932 (Cleeton and Williams, 1933, 1934; Cleeton, 1934, 1935). Within a year, results, improving substantially upon Randall's concurrent infrared spectroscopic measurements, were attained (Fig. 6), using the apparatus pictured in Fig. 7.

Particularly to be noted in Fig. 7 is the wall-mounted rack containing dozens of split-anode magnetron tubes, hand blown and hand built by Cleeton. The "good" ones generated considerably higher-frequency coherent oscillations than had previously been recorded. In the foreground is the echelon reflection grating, constructed of aluminum slats in a wooden frame, to achieve the intended measurement of wavelength. The length of waves diffracted (i.e., reflected under conditions of constructive interference) into the detector is determined by the "setback" between adjacent slats, which is varied by rotating the slats—all mounted by vertical axles in the wooden frame and linked mechanically. The frame is then rotated about its vertical axis until the perpendicular to the plane of the center slat bisects the angle between the magnetron and the detector.



FEBRUARY 15, 1934 PHYSICAL REVIEW VOLUME 45

Electromagnetic Waves of 1.1 cm Wave-Length and the Absorption Spectrum of Ammonia

C. E. CLEETON AND N. H. WILLIAMS, *University of Michigan*
(Received December 21, 1933)

Diagram of the spectrometer designed for wavelengths below 10 cm.

The absorption band of NH_3 plotted on a wavelength scale.

FIG. 6. Claud Cleeton's measurement of the ammonia inversion transition at 24 GHz = 1.25 cm. Collage of title and figures 1 and 3 from Cleeton and Williams (1934).

For our purposes, however, it is still more significant that this initial success at Ann Arbor, which in hindsight might be regarded as opening the field of microwave spectroscopy, was not followed up vigorously there and not followed up at all elsewhere—not until, nearly ten years later, the last phases of wartime radar development demanded analogous measurements. True, Williams (1937), with theoretical guidance from Dennison, continued work in this direction: he constructed a waveguide absorption cell tens of meters long and supervised one or two further dissertations. Still, the intensity of activity in this direction at Ann Arbor was not high. Cornelis Gorter (1967), who had suggested in 1932 in his Leiden doctoral dissertation that the 2*S*-2*P* structure of hydrogen should be investigated with radio waves (see the discussion of the Lamb shift, below), arrived in Ann Arbor in hope of doing microwave work with Williams about the time that Williams was preparing to depart for the American Physical Society meeting in Madison, June 22–23, 1937, to present the above referenced paper (Williams, 1937). Instead, however, of returning to Ann Arbor for further experimental work, Williams retreated to his vacation home for the entire summer, leaving no one but

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[24] Meyer *et al.* (1944, p. 27) speak of "The prediction in 1932 by Dennison and Uhlenbeck that microwave radiation would be absorbed by NH_3." Their joint publication (Dennison, and Uhlenbeck, 1932) contains, however, no such prediction. I have, nonetheless, assumed the correctness of the local lore.

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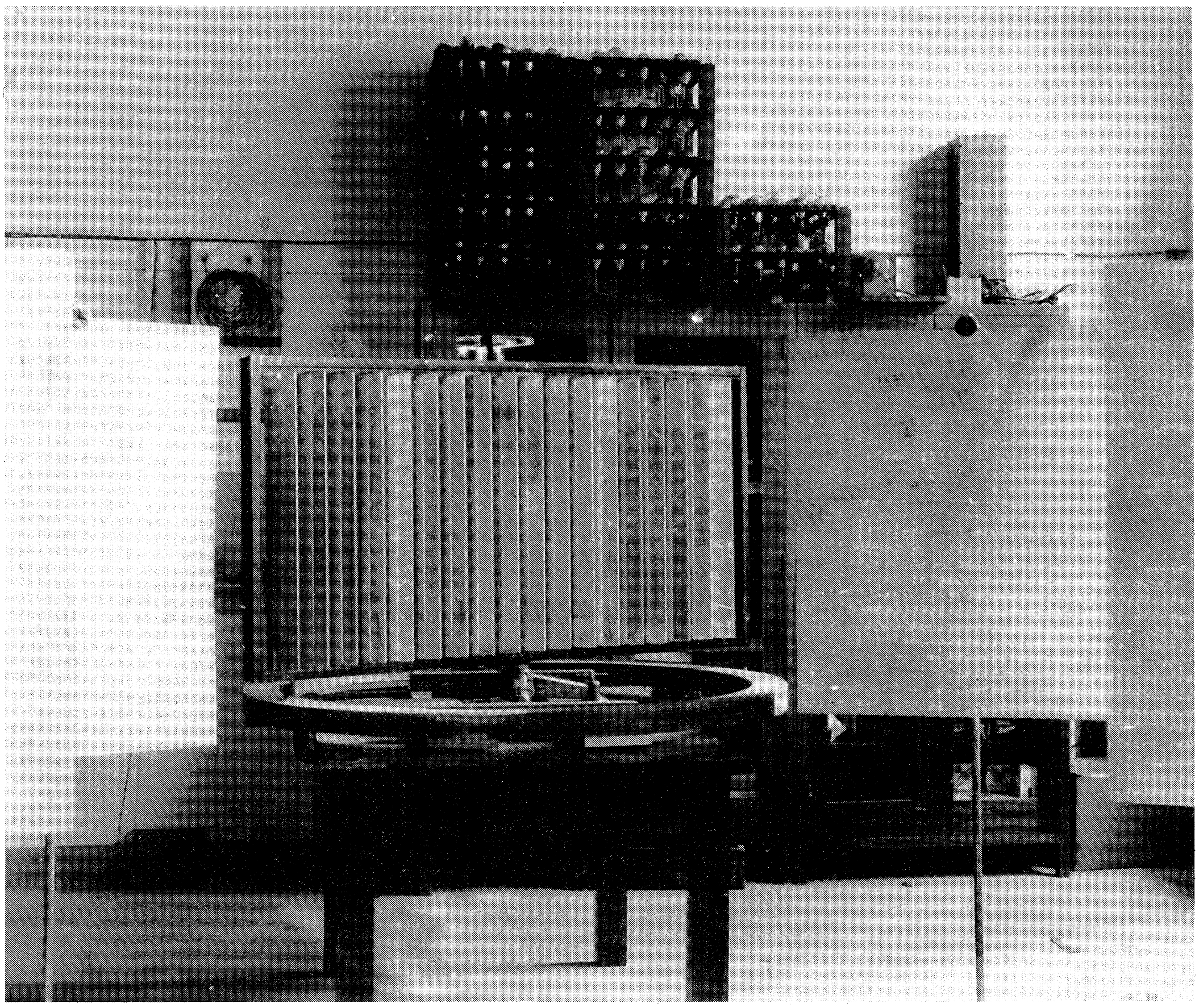

FIG. 7. Cleeton's diffraction grating and magnetron tubes. Photograph reproduced from Cleeton (1934, p. 29).

Gorter in his laboratory.[25]

Thus while in principle this direction was in the line of forward advance already well established in the physics discipline—*Physics Abstracts* lists some fifty papers published each year in infrared molecular spectroscopy—there occurred in this field only rarely what did exist regularly in the realm of nuclear magnetism and was reflected in Rabi's work with molecular beams: a clear sense of the urgent pertinence of particular numbers, so that an elaborate experiment to a limited end could seem

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[25] Number 6 of the 16 "Stellingen" appended to Gorter's doctoral dissertation (1932)—which theses were to be defended at his public oral examination—asserts that the small energy differences between the optical energy levels are in the range of short radio waves, and that it should be possible to observe in this way the fine structure of the $2S$ and $2P$ levels of hydrogen. Although this thesis could have been wholly original with Gorter, it seems likely also to have arisen out of the continuing contacts between the Dutch physicists and Samuel Goudsmit and George Uhlenbeck, who had emigrated from Holland four years earlier to join Randall's Ann Arbor physics department.

worthwhile. Thus without a powerful, flexible, off-the-shelf microwave technology, which could be used to turn out masses of precise data—and that technology is what World War II wrought—molecular microwave spectroscopy could not become an active field of physical research.

D. Radio astronomy

Because astronomical observation at radio frequencies was to be among the most prominent postwar applications of radar hardware and know-how, the prewar status of radio astronomy, and especially the limitations imposed by available high-frequency techniques, are not to be omitted from any survey, however partial. The case is the more interesting as the neglect of this research direction prior to World War II was by no means due to technical limitations (Sullivan, 1982, 1984a).

The initial observations of extraterrestrial radio emissions were made in 1930–1933 at 20 megahertz by Karl Jansky at Bell Telephone Laboratories, while investigating radio static (Southworth, 1956; Friis, 1965; Sullivan, 1984b). Jansky identified the Milky Way—i.e., our galaxy, at large—as the source of this cosmic noise and, over a period of several years, published several papers in the *Proceedings* of the Institute of Radio Engineers describing his apparatus, observations, and conclusions. Though scientifically sound and exciting much popular interest, Jansky's work was followed up experimentally only by one brief, isolated investigation—by a professor of electrical engineering at Caltech and his graduate student—and theoretically likewise by only one brief, isolated investigation—by a professor and graduate student in astronomy at Harvard (Sullivan, 1984b, pp. 28–34). The sole individual to mount a sustained pursuit of this phenomenon was Grote Reber, a young electrical engineer and radio amateur, who, excited by Jansky's discovery, made "cosmic static" his passionate hobby (Reber, 1958).[26] (The term "radio astronomy" was of postwar—and not even immediate postwar—coinage.) Only gradually, during the war years, did Reber's far more precise and extensive data begin to impress professional astronomers (Reber and Greenstein, 1947; Greenstein, 1983; Kellermann and Sheets, 1983).

Reber's homemade radio telescope (Fig. 8), so often pictured in this—its final—form, ca. 1939, carries at its focus a large cylindrical cavity designed to resonate at 160 megahertz (187 cm wavelength), a frequency comfortably within the range of well developed, although not yet off-the-shelf, technology. Reber's quest had begun, however, at much higher frequencies. Reasoning from the Planck distribution of radiation from a hot body, Reber had expected that the intensity of cosmic radio emissions would rise steeply with their frequency. He had therefore first prepared receiving equipment to match the highest-frequency oscillator tube available, an RCA type 103A end-plate magnetron capable of about 3000 megahertz (10 cm wavelength). When, however, Reber pointed his receiver skyward, he found nothing at these extremely high and awkwardly manageable frequencies. He then backed down to 900 megahertz, still a troublesomely high-frequency domain, and again found nothing. Finally, Reber retreated to 160 megahertz, for which a well-developed technical base was then available—not off the shelf, but in the published literature (Reber, 1958, pp. 44–49). Thus the failure of radio astronomy to "catch on" among professional scientists and "take off" as a research field was certainly not due to limitations of the high-frequency technology available before the war.

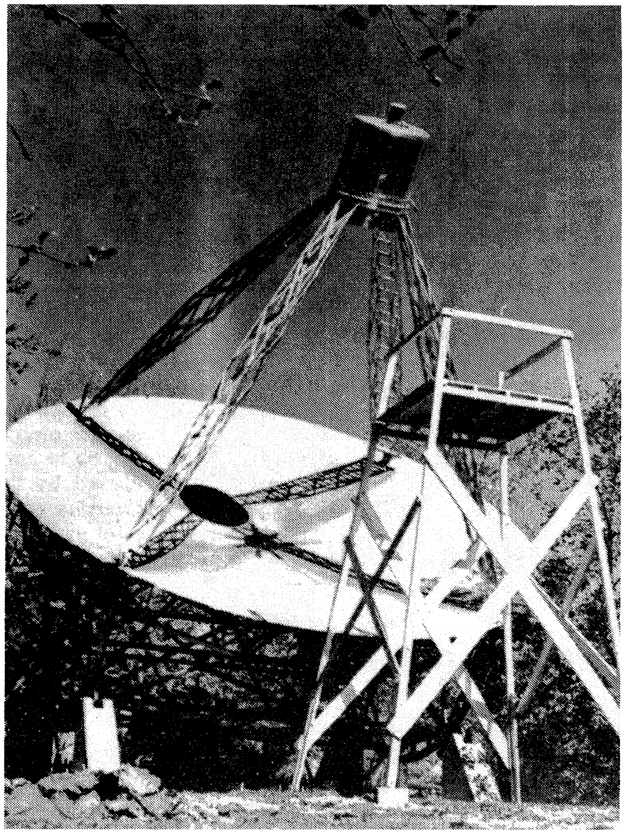
FIG. 8. Grote Reber's radio telescope at his home in Wheaton, Illinois, circa 1939. Reproduced courtesy of G. Reber.

III. POSTWAR APPLICATIONS OF RADAR HARDWARE AND TECHNIQUE TO FUNDAMENTAL PHYSICAL RESEARCH

Logically, two sorts of "applications" of radar to fundamental physical research following the Second World War may be distinguished: on the one hand, those in which the hardware and technique of radar, or that developed for radar, were directly employed to yield new knowledge; on the other hand, those in which there

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[26] It should be said, however, that the interest in extraterrestrial radio emissions never died out entirely at Bell Telephone Laboratories, as indicated by the experimental work of Friis and Feldman and of Mueller (Reber and Greenstein, 1947; Southworth, 1956), and of Southworth (1945), the theoretical work of Townes (1947), and the revival of cosmic noise observations by Penzias and Wilson (Wilson, 1990) in the early 1960s.

was no direct employment of devices whose existence was owing to World War II radar, but in which the experiences—better, the familiarities—that physicists acquired through their involvement with radar research, engineering, and operations during the war resulted in new ways of thinking about nature, scientific investigation, and their postwar careers. Naturally, the physicists themselves, in their historical practice, abided by no such logical distinction and interlaced their doing and their thinking, investigative as well as organizational, inextricably.

American theoretical physicists had long been regarded as commonly maintaining much closer contacts and cooperation with experimentalists than did their European counterparts. So, for instance, in the summer of 1936 Hans Bethe, who had emigrated from Germany and had some experience also of academic physics in Britain before landing a permanent post at Cornell University, wrote from Ithaca to his Munich mentor, Arnold Sommerfeld: "What is characteristic of physics in America is team work. Working together in the large institutes [i.e., physics departments] in each of which really every one of the subfields that make up physics is pursued, and where the experimenter continually discusses his problems with the theorist. . . ."[27] And of this circumstance we have seen, above, an outstanding example in the collaboration between experimentalists and theorists on problems of molecular spectroscopy in the Physics Department at the University of Michigan from the late 1920s to the late 1930s.

But the enlistment of theorists in the practical tasks of radar development and operations pushed American theorists, and European as well, much farther in this direction. Some went so far as to become supervisors of experimental work or even part-time experimentalists themselves, as did Willis Lamb and Norman Kroll for the K-band magnetron development at the Columbia Radiation Laboratory, and as did Frederick Seitz at the University of Pennsylvania, Henry Torrey at the MIT Radiation Laboratory, and Heinrich Welker in wartime and postwar Germany—each of these latter three engaged in the development of semiconductor diodes.[28] Such involvements could hardly have remained without effect upon postwar research styles and orientations. With these overseers of solid-state research in mind, Hoddeson (1994) found that, "One result was that during the war scientists tended to shift their interest from theories of general, abstract, perfect objects to theories of particular, real, imperfect materials."

This is by no means the full extent of the indirect effects of participation in wartime radar research upon postwar physics. Schweber (1988, 1989, 1992), Hoddeson (1992), and other historians of twentieth-century physics—see the literature referenced in note 2, above—have gone farther still in claims for the influence of the style of wartime research—coordinated, collaborative, fast-paced, amply supported—upon the character, the directions, and the results of postwar research. But in the present survey of research techniques in the narrower sense, such considerations have no comfortable place and are here left aside.

While we shall be focusing on scientific achievement that grew specifically out of the technology of radar, we should not forget that wartime research also produced devices and materials of a more generic nature, having applicability to any and all work at high frequencies—or, indeed, to electronic work, generally. Of this nature are the polyethylene-insulated low-loss coaxial cables by means of which the transfer of microwave oscillations from one place to another, from one piece of equipment to another, became a simple, convenient, penalty-free procedure. This advance and its significance was heavily stressed, not merely in many institutional advertisements by DuPont and other manufacturers in the early postwar issues of *Electronics,* but also by radar pioneer Robert Watson Watt, who in August, 1946, declared that

> The availability of polythene [British for polyethylene] transformed the design, production, installation and maintenance problems of airborne radar from the almost insoluble to the comfortably manageable. Polythene combined four most desirable properties in a manner then unique. It had a high dielectric strength; it had a very low loss factor even at centrimetric wavelengths; it could fairly be described as moisture-repellent; and it could be moulded in such a way that it supported aerial rods directly on watertight vibration-proof joints backed up by a surface on which moisture films did not remain conductive. And it permitted the construction of flexible very high frequency cables very convenient to use.[29]

Another convenience of great consequence was the silicon crystal diode rectifier, which made possible sensitive, reproducible measurements at microwave frequencies—and which gave so great a fillip to the, ultimately revolutionary, development of semiconductor electronics.[30]

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[27] Quoted by Eckert (1993, 1995). See also Schweber (1986b, pp. 71–75). On Sommerfeld's longstanding concern to foster more and easier contacts with experimentalists in Germany, see Forman (1970). The dynamics of this cooperation between producers and construers of experimental data in the context of American astrophysics forms a principal theme of the biography of Henry Norris Russell now being prepared by D. H. DeVorkin.

[28] For which see, respectively, Schweber (1994, pp. 141–144, 212–219), Hoddeson *et al.* (1992, pp. 457, 464–466), Torrey and Whitmer (1948, pp. 8–9), Pound (1993a, 1993b), and Eckert (1993, pp. 215–220, 231).

[29] E.g., Electronics, Jan. 1946, p. 208; Mar. 1946, p. 209; Apr. 1946, p. 285. Further on cables and polyethylene, see Bryant (1984, pp. 973–974). I have taken the quotation of Watson Watt from Swallow (1957, p. 7); I have not succeeded in identifying its original source. Further on the history of polyethylene: Raff and Allison (1956, pp. 6–16) and Allen (1968, pp. 7–52). E. M. Purcell (1992) drew my attention to this important technical development.

[30] On the wartime development: Henricksen (1987), Torrey and Whitmer (1948, pp. 5–7), Scaff and Ohl (1949). On the postwar consequences: Hoddeson (1993, 1994), Pound (1993a).

Similarly, the cathode-ray oscilloscope of enormously increased flexibility and frequency range made it far easier to monitor one's experiment and to discover just what was going on at any point in the steadily lengthening chain of electronic equipment.[31] Indeed, we may go further and add the general, generic development of electronics, to which the Manhattan Project contributed much (Seidel, 1987b). By the end of World War II, electronics was established as fundamental to modern technology in all its branches. As the historian of Columbia University's School of Engineering recorded,

> One of the few major changes that the war produced was the inclusion of a course in electronics as a required subject for all engineering students in response to the ever widening applications of this new technique (Finch, 1954, p. 119).

These developments, are beyond this scope of this survey and will not be discussed further here. However, a case could be made that precisely because of their generic character and universal employment, they are of greater significance to postwar research—fundamental and otherwise—than the magnetrons and klystrons that figure prominently in the following sections.

Hardware for generation and manipulation of microwaves became available to physicists in various ways, in all of which they were the beneficiaries of the military services, directly or indirectly. (The largest of these benefactions—from the US Army Signal Corps to Ernest Lawrence on behalf of Luis Alvarez—is described below under linear accelerators.) University laboratories in which radar research had been done—such as MIT, Harvard, and Columbia in the US, and Oxford's Clarendon Laboratory in the UK—held onto large stocks of equipment. That was true of the rump of the disbanded MIT Radiation Laboratory—reconstituted as the MIT Research Laboratory of Electronics—even after very considerable quantities of equipment had been transferred to a score of other universities through a giveaway program organized by the U.S. Navy's Office of Research and Inventions (1945).[32] Where initially covetous physicists approached the military, soon the military agencies themselves began to solicit widely for donees: David Lazarus (1981, p. 61) recalled that at the University of Chicago "we'd get fliers in the mail" offering surplus radars *gratis.*

It was not merely the domestic American research enterprise that benefited from the "dumping" of radar and microwave equipment by the U.S. military. E. G. Bowen (1983) recalled "two huge warehouses full of these good things near Botany Bay, which we were to draw on for many years to come"—equipment that the U.S. Navy had first dumped from its own warehouses into the ocean, and that was then salvaged by Australians. More typically, however, laboratories or individuals, through personal contacts and initiative, obtained from some military agency or installation gifts of specific desired equipment. Bernard Lovell (1983, pp. 99–100), particularly, has emphasized that the success of the two principal British radio astronomy centers, his own at Manchester University and Martin Ryle's at Cambridge, was based on their ability "to borrow or beg ex-military radar equipment." This point has been underscored by Martin Harwit (1981, pp. 48–50, 234–240) and amplified by him with systematic evidence of the continuing dependence of astronomy upon military technology in the decades following World War II.

For physicists of the vanquished nations, the superfluity of U.S. military radar gear was of prime importance for their entry into microwave work—although the means of acquiring such surplus items were necessarily rather less direct. Thus in Italy microwave physics got an early start at the Scuola Normale Superiore, Pisa, because, as A. Gozzini (1988, p. 68) recalled, "a depot of stores had been left by the American troops in the nearby camp of Tombolo. That material was subsequently marketed in the Arar stores, but at that time was sold clandestinely. For just a few lire it was possible to buy marvelous objects, such as klystrons, magnetrons and sophisticated apparata" that remained significant components of Gozzini's research equipment as late as 1955 (Galison, 1989, pp. 228, 230, 249).[33] Similarly, at the University of Tokyo, Koichi Shimoda, the leading figure in microwave spectroscopy in postwar Japan, and his first student, Tetsuji Nishikawa, constructed their ap-

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[31] On the impetus radar provided to oscilloscope development: Bell Telephone Laboratories (1949, pp. 903–905), Atwood and Owen (1944), Soller *et al.* (1948, pp. 9–10, 252–253, 260–261, 264–265), and Lee (1986, pp. 15–16, 30–31, 46–47, 53).

[32] On MIT: Slater (1975, pp. 216–225) and Leslie (1993, pp. 22–28). On Columbia: Forman (1992, 1995a). On Harvard: Galison (1988), to which attention is drawn by Pestre (1992, p. 68), in drawing the contrast to conditions at French universities. However, Pestre (1994, pp. 410–412) emphasizes the extensive "bricolage" in France, too, using surplus German and American equipment. For Stanford, see Galison, Hevly, and Lowen (1992, pp. 58–59). On the Clarendon, see the discussion of ferromagnetic resonance, below.

[33] Cf. Eduardo Amaldi (1979, p. 59), who, while not referring specifically to radar hardware, is quite explicit that "The continuation and development of our work was made possible by using materials acquired from the camps of ARAR (Azienda Rilievo e Alienazione Residuati) where the residual war materials had been collected. A few of our young people had specialized in this type of recovery operation which sometime was rather adventurous. Once S. Sciuti and F. Lepri, another time Pancini and Quercia, brought to the Institute from the ARAR camp near Capua a truck of electrical (oscilloscopes, amplifiers) and optical (cameras, theodolites, etc.) materials. Considerable amounts of equipment were brought in a number of expeditions, from a camp near Rome by Pancini and Quercia, from another near Bari by Pancini, Quercia and Rispoli and from the camp of Tombolo, near Pisa, by Cacciapuoti, Lepri, and Pancini, who arrived at the Institute with a six truck caravan." (I am indebted to Gianni Batimelli for this quotation.)

paratus from

> klystrons, silicon detectors and parts of wave guides purchased from the second-hand stores in Akihabara (an area of Tokyo similar to the Canal Street section in lower New York City), which were then selling used radar equipment sold by the American army (Hoddeson, 1983, p. 5).

The comparison with New York is fully appropriate, for equipment purchased at giveaway prices from war-surplus dealers also became important for U.S. researchers within a year or so of war's end. Again, Lazarus (1981, p. 16) recalled that what he did not obtain *gratis* directly from the military he picked up for a song at war-surplus stores in Chicago. Finally, the Office of Naval Research (what the Office of Research and Inventions became in 1946) provided the keystone to his apparatus, namely, the money to buy a DuMont oscilloscope with microsecond resolution. Even a researcher at such well-stocked radar laboratories as Bell Labs and Columbia University could recall, as does Charles Townes (1992), the utility of Manhattan's exceptionally "rich" second-hand electronic equipment stores for his postwar research work. By and large, however, while in Europe and Australia "war surplus" hardware continued to play a role of some significance for ten years after the war, in the United States such resources appear to have been important for a much shorter period, although the shops of New York's "radio row" continued to play some role.

A. Radar astronomy

The fullest—and most direct—application of radar hardware and technique to fundamental research would be, of course, turning such radio detection and ranging apparatus from enemy targets to distant objects of scientific interest. The first idea along these lines—to detect the avalanche of ions produced in the atmosphere by energetic cosmic rays—occurred very early in the war when a young British cosmic-ray physicist got into the control room of a Chain Home receiving station. Through the late 1930s "cosmic-ray showers" had been at the focus of attention of a large fraction of those physicists, both experimental and theoretical, concerned with the *most* fundamental physical processes. Now, Bernard Lovell (1993; also, 1975, p. 34; 1983, pp. 93–94; 1984, pp. 193, 200; 1990, pp. 149–155; 1991, p. 4) surmised that such cosmic-ray showers were revealing themselves in their full extent to radar operators as spurious echos caused by the reflection of radar pulses by those avalanches of ions. The idea and calculations proving the effect detectable were approved by P. M. S. Blackett, with whom Lovell had been a junior associate at Manchester University before the war, and the proposal was published (Blackett and Lovell, 1941). The serious defect (by excess, of course) in Lovell's estimate of observability was not recognized until 1946, by which time the intent to make such observations had provided the principal motivation for establishing a field station of Manchester University for radio astronomy at Jodrell Bank.

The first *successful* application of radar to the discovery of new knowledge came about serendipitously in the midst of the war when British radars were tilted up to the skies to track Germany's V-2 ballistic missiles. Spurious radar echoes were observed, which, as J. S. Hey (1973, pp. 19–23) was able to establish in the spring of 1945 with coordinated observations supported by the British Army Anti-Aircraft Command, were due to the reflection of the radar pulses from the trails of ions created not by cosmic rays, but by the passage of meteors through the atmosphere. This line of scientific investigation, continued in the late forties and early fifties especially by Bernard Lovell's group at Jodrell Bank, produced massive and decisive data on the numbers, velocities, orbits—and hence origins—of meteors (Hey, 1973, pp. 32–34, 124). Thus this was not only the first, but also the most important, "full" application of radar hardware and technique in the postwar decade.

Of considerably less scientific significance, but considerably greater appeal to the popular imagination, was the training of radar upon the moon. Radar "detection" of the moon was conceived toward war's end in several national radar laboratories—always primarily as a technical *tour de force,* however much it may have been represented to the public as a scientific inquiry. [This circumstance, so familiar in the late twentieth century, has been illustrated in striking detail by DeVorkin (1989) for balloon ascents in the 1930s and 1940s.] A proposal originating at the US Naval Research Laboratory in 1944/45 to bounce centimetric radar off the moon caused "a lot of excitement in certain quarters of the Navy" (Haddock, 1983, p. 116). The US Army's Evans Signal Laboratory in Belmar, New Jersey (a component of the Army Signal Corps' Fort Monmouth laboratories) was the first to achieve this success—in January, 1946—and to announce it, as subsequent successes in this line would also be announced, in the news media.

The endeavor, employing a modified version of the SCR-271 early-warning radar used so successfully at Pearl Harbor, was conceived and promoted by the commanding officer of Evans Laboratory, Lt. Col. John H. DeWitt (DeWitt and Stodola, 1949). In civilian life, as chief engineer of a radio broadcasting station in Nashville, Tennessee, DeWitt had before the war already attempted to detect reflections from the moon of signals broadcast by a radio station transmitter (Clark, 1980). DeWitt's men at Fort Monmouth, after four months' work, obtained what was quickly to become the "most widely published cathode-ray trace in history" (Mofenson, 1946, p. 92).

The story was carried by the *New York Times* on January 24, page one, column one, with followup articles on 24, 26, 27, 28, and 29 January. Although hailed as a significant scientific achievement and as beginning a new field of research, very little was subsequently done, or could be done, with that apparatus. In particular, the antenna, rotatable in azimuth only, could irradiate the moon only for half an hour at moonrise. The one

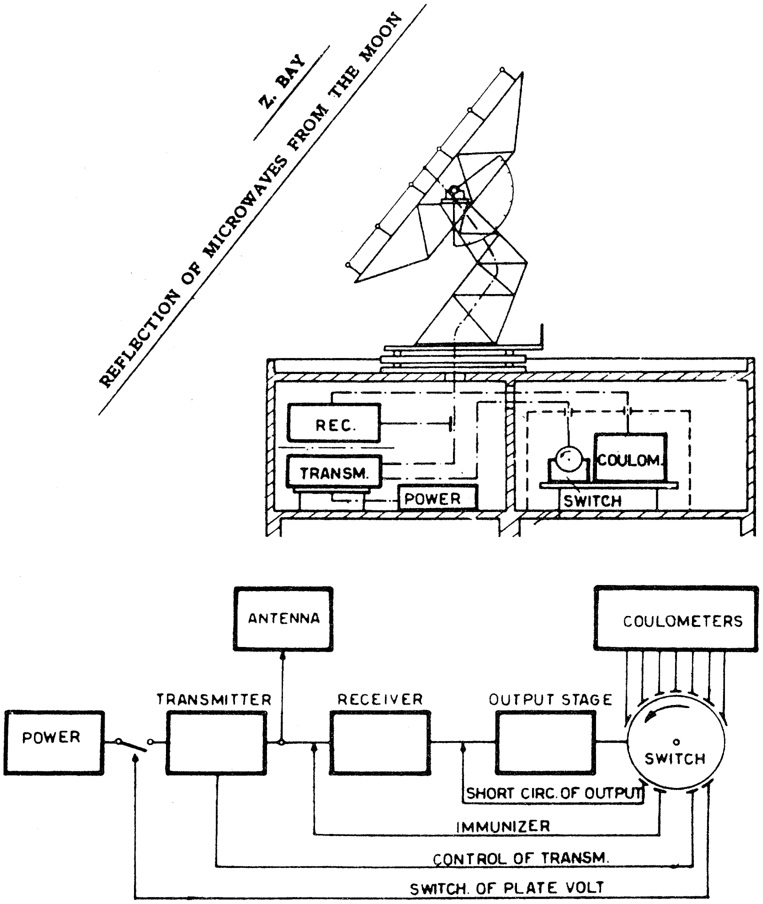

FIG. 9. Zoltan Bay's detection of radar signals reflected from the moon, Budapest, 1946. Collage of title and figures 1 and 2 from Bay (1947).

scientifically interesting fact implicit in those observations—the magnetic rotation of the plane of polarization of the radar pulses in passing through the ionosphere—remained unrecognized in the signal fading it occasioned (Evans, 1962, pp. 446–449; Kerr, 1984, p. 136).

The same accomplishment of detecting radar signals reflected from the moon was claimed immediately after in the Soviet press, without, apparently, a subsequent publication of details. It was also achieved—though relatively little noticed at the time—by a group in Hungary, led by Zoltan Bay (1947a, 1947b; Fig. 9). The difficult circumstances under which Bay had to work made his accomplishment truly remarkable. An excellent account of this work is given in the book by Sean Swords (1986, pp. 143–147, 173). With a good training in physical-chemical theory and experiment and some years as professor of theoretical physics at the University of Szeged, Bay became in 1936 director of the research laboratory of the United Incandescent Lamps and Electric Company (Tungsram) in Budapest. During the war he led the team that designed and developed a surveillance radar for the Hungarian military—without the assistance of Hungary's nominal ally, Germany[34]—and in 1944 began to assemble an apparatus for radar detection of the moon. This he and his collaborators achieved in the spring of 1946, having persisted with the project through the chaos of the final year of war and the first year of Soviet occupation (Wagner, 1985).

B. Radio astronomy

Radar astronomy was perhaps the fullest and most direct application to scientific investigation of the hardware and technique created in and for the Second World War. *Radio* astronomy, on the other hand,

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[34] Bay (1991, pp. 29–30), who in this memoir of the years 1940–1947, written 1950–1951, shows himself to have possessed an exceptionally vivid and detailed memory, unfortunately chose to say almost nothing about his scientific and technical work, describing only this refusal on the part of German authorities to allow Hungarian engineers to visit Germany to inform themselves about radar.

though lumped together with radar astronomy in historical accounts, and though pursued by some of the same individuals, with some of the same instrumentation—most notably at Jodrell Bank—owed its rapid, stepwise rise to several indirect factors. Our glance at prewar radio astronomy has shown that the failure of the scientific world to enter upon this field of investigation was due only in small part to limitations in the available high-frequency technology.

Indeed, radio astronomy's postwar emergence was associated with research carried out almost entirely below 300 MHz, and it was only the detection in the spring of 1951 of the 21-cm line—hydrogen's hyperfine transition at 1420 MHz, of which more will be said below—that pushed the field up into frequency ranges opened by radar research in World War II. Thus the most advanced radio telescope on the drawing boards in 1951, the 250-ft diameter steerable dish for Jodrell Bank, had not been intended to go below one meter wavelength. The detection of the hydrogen line compelled its redesign to perform at frequencies ten times greater, notwithstanding the considerable increase in cost (Hey, 1973, p. 58; Agar, 1994).[35]

Prior to World War II, while the available high-frequency technology might have supported radio astronomy as a research field, professional astronomers with a knowledge of radio engineering were lacking. The Jansky-Reber case argues as much. The postwar world, however, was a much freer one—much freer with resources and, with those greatly increased resources, supportive of many more programs of knowledge production, which were less discipline delimited. Consequently, postwar, radio astronomy no longer needed the astronomers in order to emerge as a research field. The rise of the Radiophysics Division of Australia's Council for Scientific and Industrial Research (CSIR)—i.e., Australia's radar R&D laboratory, established on the grounds of Sydney University in 1939—as a radio-astronomic powerhouse, after 1945, is a good example. As its then Scientific Director, E. G. Bowen (1984, pp. 85–87), recalls, "there was not a single astronomer. . . , nor, for that matter, anyone who had done a university course in astronomy" among the very large staff remaining with the organization after the war. By Bowen's account, this staff turned, and as a body, to radio astronomy.

Radio astronomy was not, of course, the only option available to radar-trained scientists. Postwar development of particle accelerators—to be discussed below—offered Australian radio physicists an alternative in the field of fundamental physics, to say nothing of geophysics, atmospheric physics, etc. Although Bowen (1984, pp. 85–87) implies that the majority of the wartime staff of the Division of Radiophysics went over at war's end directly to radio astronomy, Sullivan (1988, pp. 309, 312–313) and Robertson (1992, pp. 29–31) maintain that radio astronomy was initially the least of the Division's programs and only gradually acquired staff from other programs as its successes repeatedly warranted. In Bowen's initial, July 1945, program for the postwar Radiophysics Laboratory there was but one sentence—"a humble sentence" (Robertson)—calling for radioastronomic observations; 30 months later, however, one half of Bowen's December 1947 summary of the Lab's research activities was devoted to radio astronomy.

World War II created a large number of individuals with a personal orientation toward research who were well familiar with radio science and radar engineering. Their scientific careers either had not previously taken a definite direction or had been unsettled by years away at war. In the case of Australia, there was not an extensive infrastructure of research universities and industrial firms capable, as in Great Britain and the United States, of absorbing an exodus from the military research laboratories (Robertson, 1992, p. 29). This situation was clearly appreciated by Joseph Pawsey, Bowen's second-in-command in the leadership of Australian radio physicists, who in 1953 explained the breadth and energy of his nation's effort in radio astronomy as due to radar research's having created "a group of able young physicists with experience of radar techniques. At the conclusion of the war these men found themselves without definite commitments and anxious to establish themselves in scientific research."[36]

Here, then, was a pool of potential entrants into the field of radio astronomy (Lovell, 1964; Graham-Smith, 1986; Hanbury-Brown, Minnett, and White, 1992), and, when they did enter, they made use of what lay at hand in the way of radar antennas, receivers, cables, etc.—whether this was surplus of their own military or of their allies, or the abandoned equipment of a defeated enemy.

Of these last-mentioned equipments the single most important—available, however, only to Europeans, not to Australians—was the 7.5-meter diameter parabolic dish of the Würzburg Riese radar (Fig. 10). These antennae were quickly appropriated for would-be radio astronomers in Britain, France, and the Netherlands (Sullivan, 1984a, pp. 242, 303, 408) and, as Sullivan (1982, p. 327) has emphasized, "played an important role in the experiments of many early radio astronomy groups." The group at the Ecole Normale Supérieure was created in

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[35] The reorientation of radio astronomy effected by the discovery of the 21-cm line in 1951 is testified to in many of the papers in a special issue of the *Proceedings of the IRE* on radio astronomy (Haddock, 1958, pp. 5, 230, 234, 239, 240, 250).

[36] Pawsey as quoted by Lovell (1977, p. 161; 1983, p. 100). Something of this same effect obtained in Canada (Middleton, 1981, pp. 120–121; Covington, 1984, pp. 317–319) and even in Britain (Lovell, 1984, 1990, 1991). Edge and Mulkay (1976), surprisingly as they approached "the emergence of radio astronomy" as sociologists, failed to address the question of disciplinary origins of the practitioners of this newly emergent scientific field.

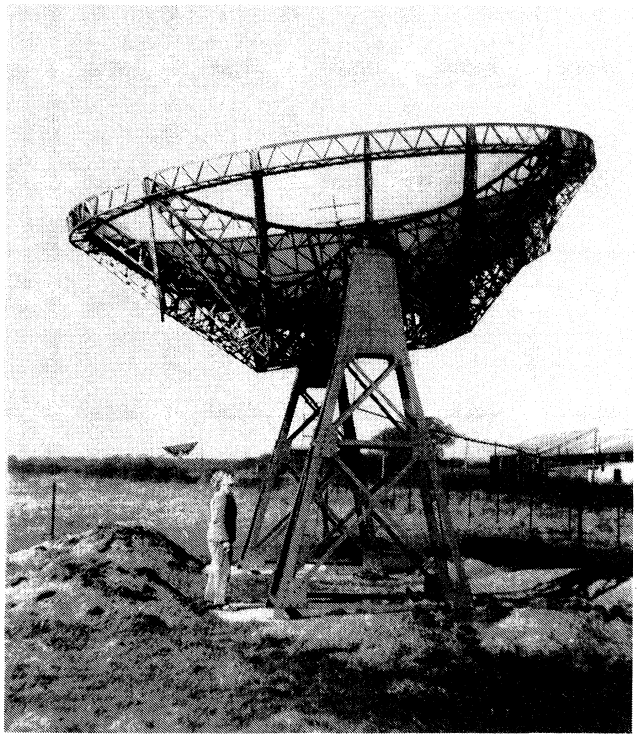

FIG. 10. A pair of antennae from the German Würzburg Riese radar in service as a radioastronomic interferometer at Cambridge University. Reproduced courtesy of F. Graham-Smith.

1945 around a pair of such dishes that Yves Rocard, who for the two previous years had been working with the British intelligence services, received from his British friends (Denisse, 1984, p. 303; Pestre, 1994, pp. 410–412). In the Netherlands it was a foresightful chief engineer of the Radio Department of the Dutch Post Office who "had rescued a few of the 7.5-meter Würzburg antennas, which had been part of a German radar chain along the coast during the war, from destruction and had them repaired for radio-astronomical purposes" (Muller, 1980, pp. 65–66). And as late as 1950, with some high-level diplomacy, Sweden received from Norway five of these Würzburg dishes to be used for radio-astronomic purposes—the initial equipment of the Onsala Space Observatory on the west coast of Sweden (Hård, 1993, p. 380).[37]

However, the choice of radio astronomy as a focus of major scientific effort is far from fully determined by the existence of a pool of uncommitted radio physicists and a pile of surplus radar hardware. Returning to the Australian case, an important factor in the rise of radio astronomy was the decision—stressed by Bowen (1984, pp. 85–87) and by Robertson (1992, p. 29)—of the Council for Scientific and Industrial Research, including its Division of Radiophysics, to commit itself postwar wholly to nonmilitary research (see also Sullivan, 1988, pp. 311, 341). The United States, by contrast, made a laggardly entrance into the field of radio astronomy until the recognition in the early 1950s of the national security significance of extremely sensitive radio telescopes. Only then, but very quickly, were Britain's and Australia's radio astronomy facilities surpassed by outsized American installations.[38]

C. Linear accelerators

The significance of wartime radar development for the advance of linear accelerator technology was stated concisely but comprehensively by John Clarke Slater (1948) in introducing his extended theoretical discussion of "The Design of Linear Accelerators" in *Reviews of Modern Physics:*

> The linear accelerator is not a new device. Sloan and others in the decade before the war worked, with moderate success, on similar instruments. The production of high-energy, high-frequency power sources such as the magnetron during the war, however, has given the possibility of feeding enough power into a linear accelerator, operating as a pulsed device, so as to promise really high accelerations and to make it a promising field for further work. Two main lines of development have resulted since the war, closely related to each other, and both really stemming from radar work. First, Alvarez, at the University of California, has been working on the design of a positive ion accelerator, working at a frequency of about 200 megacycles, using radiofrequency

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[37] Although several thousand Würzburg Riese radars were deployed in and around Germany during the war, all, apart from the few taken as scientific spoils, were destroyed within a short time. The Würzburg Riese dish recently obtained by the Deutsches Museum, Munich, had survived because it was employed as a radio astronomy antenna in the Netherlands.

[38] The United States' slow entrance into (and eventual predominance in) radio astronomy, relative to the U.K. and Australia, is indicated bibliometrically by Edge and Mulkay (1976, p. 52). It is the theme of Malphrus (1991). The question of its dependence on the perceived pertinence of the enterprise to national secuirty interests is considered implicitly by Emberson and Ashton (1958, pp. 26–27) and explicitly by Lovell (1977), Forman (1987, p. 228), Needell (1987, p. 281), and Sullivan (1988, pp. 310, 336–338). In Germany in the early 1950s militarily relevant advanced radar research colud not yet be pursued quite openly, and radio astronomy was promoted as a cloak for such (Eckert, 1993, p. 220). Against such sociological explanations of the national courses in this scientific development, R. V. Pound (1993b) argues that the British and the Australians had been immersed in meter-wave radar, which was the appropriate wavelength for the kind of radio astronomy that could be done—and was done by them—before the detection of the 21-cm hydrogen line. Similarly, Pound argues, the Americans, whose radar work had been centimetric, became strongly interested in radio astronomy only after and because of the detection of a centimetric line.

> equipment designed for radars of that frequency. Secondly, a number of laboratories have been working on electron accelerators, using high-power magnetrons, generally at about 10-cm wavelength, developed for radar purposes.

One of these electron "linac" projects, as Slater continued immediately to say, was his own, at MIT's Research Laboratory of Electronics—formed from remnants of the wartime Radiation Laboratory turned back to MIT and funded jointly by the Army and the Navy (Slater, 1975; Leslie, 1993).

Fundamental to both main lines of linac development is the complete conversion from an "exterior," electric-charge-based conceptualization of linear resonance accelerators in the prewar decades to an "interior," cavity-field-based conceptualization in the postwar schemes. That reconceptualization of ultra-high-frequency oscillations had been developed before the war at several sites—notably at Stanford, due to W. W. Hansen's interest in accelerating electrons with such cavity fields, and at Bell Labs, due to George Southworth's interest in waveguides. It became a matter of course only in consequence of the development of microwave radar.

As Slater obliquely acknowledged, the concept of an electron linac based on electrons propelled by a microwave, traveling down a (loaded) waveguide, was later in coming forward in the physicists' consciousness than was Luis Alvarez's proposal for a linear accelerator of protons, or heavier ions, in which the huge acceleration structure, conceived as a single resonant cavity sustaining a standing wave, was a direct translation of the prewar linac into a cavity oscillator. As late as the spring of 1946, neither the fact of Sloan's (1941) patent application, nor the concept it embodied of electrons "surfing" in a disc-loaded waveguide, was known even by those most intimately involved.[39] Indeed, Alvarez's first proposals were for the acceleration of *electrons*. It was, as he recalled (1987, p. 154), only the invention soon after, there at Los Alamos, of the synchrotron principle—by E. M. McMillan, Alvarez's only serious competitor for Lawrence's favor—which, as a "better" way to accelerate electrons to high energies, brought Alvarez to switch from electrons to protons as the particles to be kineticized by the standing wave in a mammoth, 200-MHz resonant cavity (Alvarez, 1946; Alvarez *et al.*, 1955). This frequency he set as that of the U.S. Army's obsolescent SCR-268 radar [Fig. 11(a); [anon], 1945], its first model to go into mass production and a model of which large numbers were expected to be declared surplus.

To this scheme of a high-energy acelerator driven by *many* surplus radar transmitters Alvarez devoted much thought and enthusiasm in the sring of 1945, while he, like everyone at Los Alamos, was still nominally engaged full-time in building the bomb. A thirty-page proposal for a linear accelerator was written and sent to E. O. Lawrence and a considerable lobbying effort launched (Seidel, 1983). Alvarez—or, rather, his prewar and assured postwar boss, Ernest O. Lawrence, the entrepreneurial head of the Berkeley Radiation Laboratory where this device was to be built—recruited the emphatic support of the President of the National Academy of Sciences, Frank Jewett (1945), the Director of the Office of Scientific Research and Development, Vannevar Bush, and diverse other notables (Edward Bowles, Lee A. DuBridge, I. I. Rabi). With their support he obtained from the Army Signal Corps the pertinent components—notably, the modulators and transmitters—of some 750 dismantled SCR-268 radars, with the promise to make from them a 1-billion-volt (i.e., 1 GeV) proton accelerator, 1000 feet long. In this aspect Alvarez's project has a good claim to being, in a quantitative sense, the *fullest* application of radar hardware to fundamental research.

In fact, Alvarez and team never got beyond the first, prototypical forty-foot section (Fig. 11b), which employed about thirty transmitters from their enormous stockpile "in an Oakland Port Terminal warehouse, eventually totalling some two thousand" (Alvarez, 1987, pp. 155–157). With these 30 they accelerated protons to an energy just over thirty million electron volts. And here, typically, it was not long before the Berkeley physicists decided—once they had accustomed themselves to applying in their postwar researches the spare-no-expense imperatives of their wartime research projects—to discard even those 30 surplus radar transmitters and replace them with oscillators custom built to their needs. "I never visited the warehouse," Alvarez recalled "and we used few of the surplus units; my radio engineers decided they could build better oscillators on their own." Alvarez's sense that "for the first few years after the war we had unlimited financial backing" concurs with that of his lieutenant on this project, W. K. H. Panofsky (Trower, 1987, pp. 72–74).

In the immediate postwar years only Lawrence's laboratory had the staff, the amplitude of funds, and the political pull to undertake a project so large and so uncertain as the Alvarez proton linac, even in its drastically scaled back, 40-foot form. Several other U.S. laboratories, however, felt themselves to be able to undertake the far more modest task of constructing an electron linac with energy in the range one to five megavolts and saw in the postwar environment adequate prospects for funding. Thus, from 1946 to 1948, Purdue, Bartol, MIT, Yale, and Stanford reported their plans and achievements at meetings of the American Physical Society.[40] The

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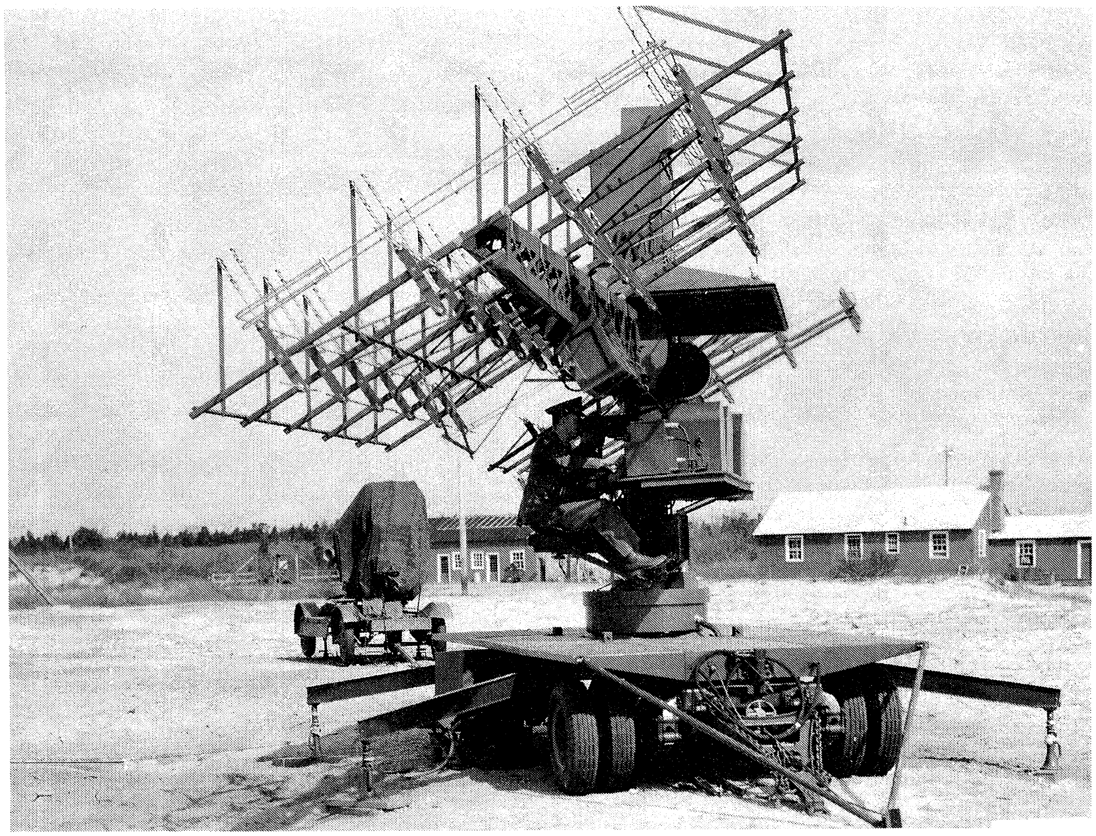
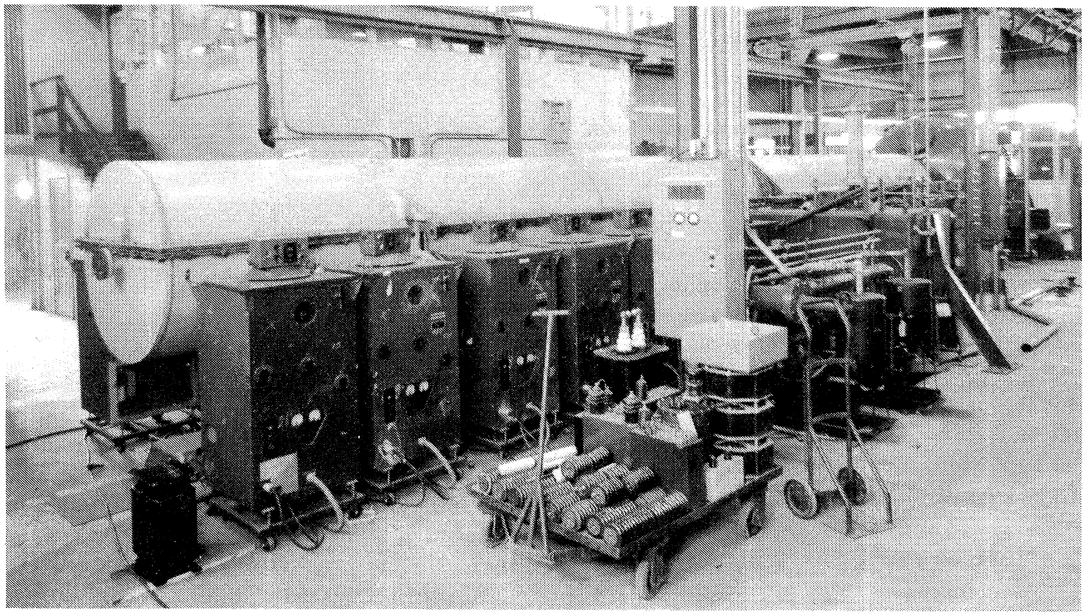
[39] Wathen (1946), writing to Hansen from the patent department of the Sperry Gyroscope Company five weeks after Sloan's patent was issued, testifies to this explicitly, as do implicitly the electron accelerators of the Australian radio physicists referred-to below and the initial proposal of J. C. Slater's group at MIT (Halpern *et al.*, 1946), the abstract of a paper presented at the April 25–27, 1946 APS meeting, Cambridge, Massachusetts.

[40] 1946: Phys. Rev. **69**, 50 (Purdue), 255 (Purdue), 671 (Bartol), 688 (MIT); **70**, 797 (Purdue). 1947: Phys. Rev. **72**, 347 (Yale). 1948: Phys. Rev. **73**, 657 (Bartol), 730 (Stanford), 1259 (Yale); **74**, 1243 (Yale).



(a)



(b)

FIG. 11. Radar used to power atom-smasher: Above (a), U.S. Army Signal Corps radar, model SCR-268; below (b), the proton linear accelerator built by Luis Alvarez (with *many* others) at the University of California, Berkeley, 1945–1947, powered by SCR-268 transmitters. Photographs reproduced courtesy of US Army Communications-Electronics Museum, Fort Monmouth, and Lawrence Berkeley Laboratory, University of California.

Stanford group, reporting late in 1947, had a three-foot length of waveguide fed by pulsed power from a Raytheon HK-7T 10.5-cm magnetron[41] (Ginzton, Hanson, and Kennedy, 1948, p. 107). Likewise reporting use of an HK-7 were the Bartol group (Miller and Amsterdam, 1949). The Purdue group reported an unspecified 10.6-cm magnetron (Errett *et al.*, 1949). For all, presumably, surplus radar equipment—especially 10-cm magnetrons—was important at the outset of their work.

As active as the Americans were, it should be noted that British and Australian laboratories were the quickest to push forward in this line of development, likewise exploiting the pulsed, high-power, S-band (10-cm) magnetrons. The group at Australia's CSIR Radiophysics laboratory was the first to achieve an operating electron linac, followed by that at Britain's Telecommunications Research Establishment (Fig. 12), pursuing the more demanding traveling-wave approach.[42]

However, by the early 1950s, as it became clear that such devices could be important for elementary particle phsyics only at energies above 100 megavolts, only one laboratory, Stanford, had the vision, the cadre, and the commitment to continue in this direction (Chodorow *et al.*, 1955; Galison, Hevly, and Lowen, 1992).[43] All the others, American as well as British, gradually dropped out of the competition.[44] As Sloan (1941) had, with remarkable prescience, anticipated, Stanford's relentless advance was based not on magnetrons—surplus or new, magnetron oscillations were just too unruly for many to contribute in unison to a traveling wave—but on locally developed, experimental, super-power klystrons (Chodorow *et al.*, 1953). And while they were nominally developed to power an electron linac, the pertinence of such super-power klystrons, with their high-frequency stability, to advanced air defense systems was always borne clearly in mind in those early cold-war years—borne clearly in mind as a case of basic research "repaying its debt" to radar technology.

D. Cyclical accelerators

Cyclical accelerators present much the same contrast with linear accelerators that radio astronomy presents with radar astronomy, namely, they have much less direct dependence on radar hardware and microwave technique, however much they may owe indirectly to experience gained in radar research, such as general familiarity with radio and electronic engineering and with large-scale, collaborative research and development projects. In this case, however, the balance between learning and teaching was more nearly equal. In 1940 it had been a significant consideration in turning over the development of microwave radar in the United States to physicists—for it was preeminently *nuclear* physicists who were recruited to the MIT Radiation Laboratory (and gave color to its camouflage name)—that all who had built or operated cyclotrons had already acquired a working knowledge of radio engineering.[45] More than that, as Heilbron and Seidel (1989, pp. 493–496) have forcefully argued, those responsible for mobilizing science

> knew that cyclotroneers understood how to work at the borders and edges of science and technology, and how to work in teams: cyclotroneers and their fellow travelers did not fear big projects, did not disdain to scrounge when necessary, did not insist on perfection or protocol. They were ideal people for crash programs.

Although drafted as experienced scroungers, over the course of the war these physicists discovered the joys of working without scrounging. Scrounging apart, the same

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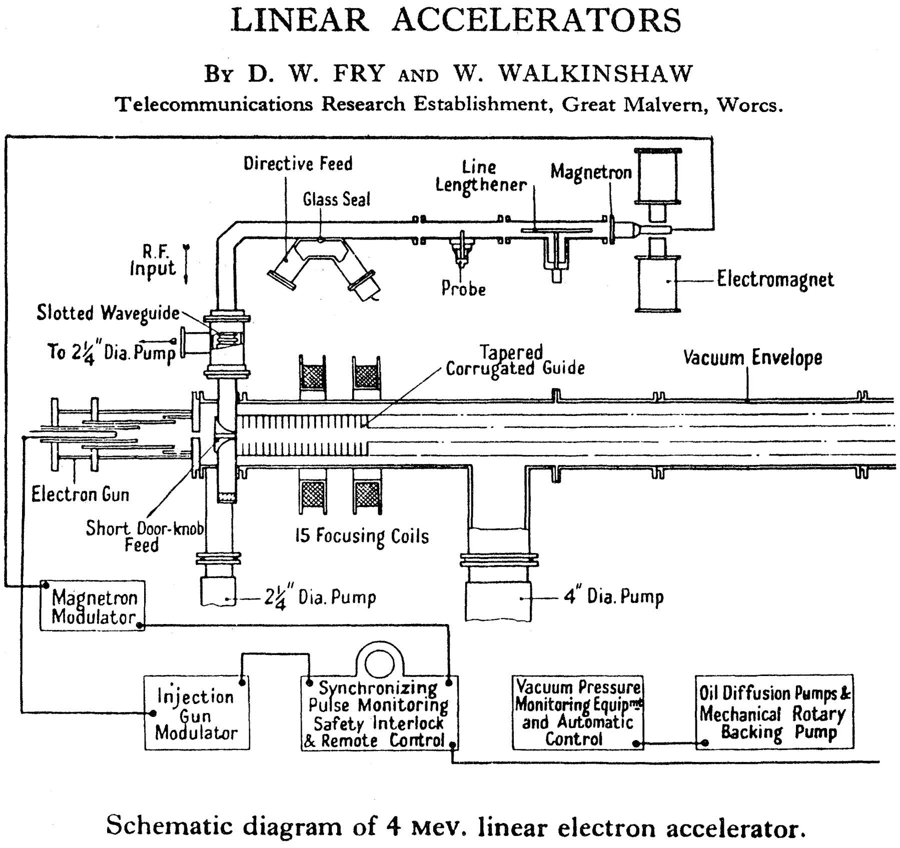
[41] According to Stitzer (1993), per Michael Cross, the HK-7 was used in the AN/CPS-4. This was a ground-based, height-finding "beavertail" radar for control of interception, of which 100 sets were ordered from General Electric in December 1943, "but none arrived in time for combat" (Guerlac, 1947, pp. 459–461, 1142).

[42] At Australia's CSIR Radiophysics Laboratory: Bowen, Pulley, and Gooden (1946); Allen and Symonds (1947). At Britain's TRE: Fry *et al.* (1947, 1948). TRE's first, some 40-cm long, yielded 0.5-MeV electrons in Nov./Dec. 1946; the later report (1948) is of the 2-m, 4-MeV linac (Fig. 12) first operated in Feb. 1948. The best overview of these early developments is that of Fry and Walkinshaw (1949).

[43] It should be emphasized that by 1947 Hansen and collaborators had already set their sights on a billion volts (1 GeV) (Ginzton, Hansen, and Kennedy, 1948, p. 105), so that they were then already in direct competition with Alvarez's project at Berkeley across San Francisco Bay. This is the real meaning of an oft-reproduced photo of W. W. Hansen and three co-workers standing, belly to back, with their 4-foot accelerator on their shoulders, which photo was a send-up of a seldom reproduced photograph of the 40-foot vacuum tank of Alvarez's accelerator with his ten times larger team perched upon it.

[44] In the 1950s Britain continued to project very large accelerators which, however, remained unrealized, e.g., the Alvarez-type proton linac for the Atomic Energy Research Establishment at Harwell (Pickavance, 1955).

[45] This point was, from early on, an important part of the self-conception of the MIT Rad Lab (DuBridge, 1946, p. 2; Massachusetts Institute of Technology, 1946, p. 12; Guerlac, 1947, pp. 259–261). It should, however, be noticed that cyclotrons (and hence cyclotroneers) operated at frequencies two orders of magnitude less than those at which they would begin their radar work, whereas the radio amateurs had already been working at much more elevated frequencies—at, roughly, the geometric mean between cyclotronic and microwave. Thus Alvarez (1987, pp. 40, 90), though he stresses that the first thing he did when he got to Berkeley as a postdoc in 1936 was to buy a textbook on radio engineering, also says of the first recruits to the MIT Rad Lab, four years later: "Jim [Lawson] had a strong amateur radio background. The rest of us, by contrast, had learned the few RF techniques we knew from building and operating cyclotrons. If we had been paid in proportion to our contributions to the success of the first microwave radar program, Jim Lawson would have earned more than half the monthly payroll."



LINEAR ACCELERATORS

By D. W. FRY and W. WALKINSHAW

Telecommunications Research Establishment, Great Malvern, Worcs.

Schematic diagram of 4 MeV. linear electron accelerator.

FIG. 12. Schematic diagram (detail, showing the back half only) of the two-meter long, 4-MeV traveling-wave electron linear accelerator constructed at Britain's Telecommunications Research Establishment, 1946–1948 (Fry and Walkinshaw, 1949, p. 114).

set of other qualifications, including how to work with electromagnetic oscillations in the range of 100 kilohertz to 100 megahertz—well below microwave frequencies—led to their being called upon once again after the war in the design and construction of "synchro-cyclotrons" [cyclically operating cyclotrons in which the frequency of the accelerating field varies continuously through the cycle (Richardson *et al.*, 1948; Cohen, 1959)] and of "synchrotrons" [annular accelerators operating on the same FM principle (Blewett, 1956)].

This happy idea for circumventing the energy limit of fixed-frequency cyclotrons had been conceived in the midst of the war, not only in the USSR (Veksler, 1945), where the mobilization of physicists was far from complete, but also, as mentioned above, at Los Alamos (McMillan, 1945, 1984), indeed at more than one Manhattan Project site.[46]

The existence in the United States in 1940 of a substantial pool of cyclotroneers—there were then some 23 cyclotrons in the U.S., while in all the rest of the world there were only some eleven more[47]—reflects the extraordinary investment of that nation's physicists in nuclear research in the course of the 1930s (Baracca *et al.*, 1986; Forman, 1987, p. 201; Eckert, 1993, pp. 189–190). Figure 13 shows that this investment was indeed reflected in America's increasing quantitative dominance in journal publication in nuclear physics over the decade of the 1930s. Naturally, at war's end the addition of the phenomenon of nuclear fission—and the new world of civil and military applications that it opened—further promoted American investment in facilities for nuclear and subnuclear research. It is thus not surprising that the synchro-cyclotrons—the super cyclotrons into which the push for the highest attainable accelerator energies was channeled immediately after the war—were built soonest, largest, and predominnatly in the United States (Table I).

On the Continent, the Dutch electrotechnical firm Philips' Gloeilampenfabrieken had dominated the particle accelerator scene in Europe before the war, for European physics institutes/departments, due to their relatively small size, were far less able than Amercans to build their own accelerators (Bouwers and Kuntke, 1937; Casimir

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[46] On Veksler and McMillan see, further, Hoddeson (1983, p. 6) and Seidel (1983, pp. 380–381, 388–389). Earlier than these two generally acknowledged inventors, Mark Oliphant, a British cyclotroneer, working on electromagnetic separation of uranium isotopes at Oak Ridge in 1943, conceived the principle and proposed a machine based upon it (Oliphant, Gooden, and Hide, 1947, p. 669; Cockburn and Ellyard, 1981, pp. 136–138). Much earlier still, the essentials of the principle of phase-stable acceleration had been grasped by the ingenious Leo Szilard (1934).

[47] Heilbron and Seidel (1989, pp. 317–352) give tabular lists of U.S. and foreign cyclotrons through 1940 and an extended discussion of the transfer of cyclotronics to Europe.

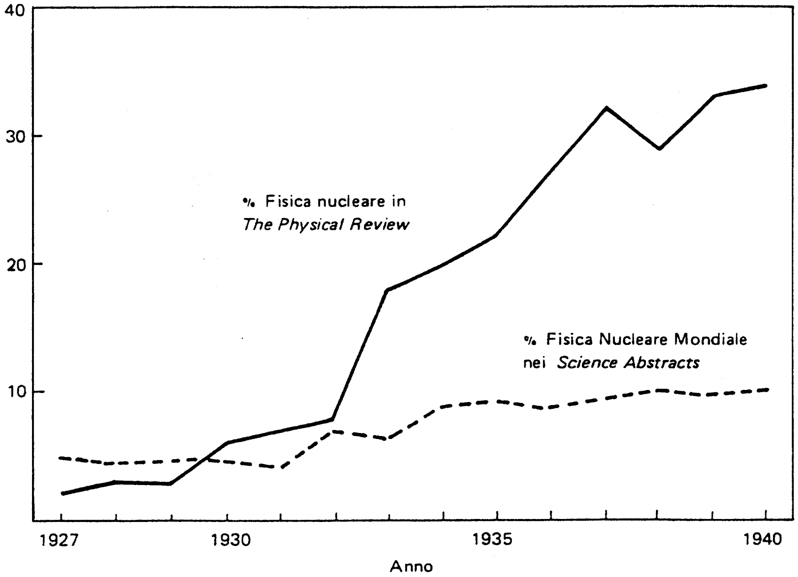

FIG. 13. The rise—absolute and relative—of nuclear physics in the United States. Reproduced from Baracca *et al.* (1986, p. 372), with permission of EUROMA-LaGoliardica Ed., Rome.

and Gradstein, 1966, pp. 27–28, 82–93; Heilbron and Seidel, 1989, ch. 7; Osietzki, 1989; Weiss, 1994). Philips recovered quickly from the German occupation (Philips, 1978; Schopman, 1988),[48] was the first to build and operate a synchro-cyclotron (Heyn, 1951), and subsequently built France's, Germany's, and Argentina's (Table I). Philips also had a large share in the design and construction of the rf system of Western Europe's largest synchro-cyclotron, that at CERN (Hermann *et al.*, 1987, pp. 19–21; 1990, pp. 97–108).

But a just comparison of the relative intensities of activity in this direction requires consideration not only of the numbers of synchro-cyclotrons constructed, but also (Table II) of the construction cost (approximately proportional to pole diameter cubed) and the construction rate (period into which the constructions were compressed). Considered from this perspective, and in light of the prewar disproportion, it is rather to be emphasized how quick and how large the European investment was in this direction, and more particularly, the British investment. Whereas before the war the ratio of particle accelerators in the US and the UK was probably about 10:1—at the start of the war Britain had two working cyclotrons and one under construction, against 23 operating in the US—in the first postwar years Britain had doubled her effort relative to America's, with one accelerator project for every four or five in the US.[49] The overwhelming priority that considerations of future weaponry gave to nuclear and elementary particle physics may plausibly explain the lack of interest and investment in radio astronomy among American researchers early in the postwar period. Nonetheless, that explanation fails to recognize that Britain and its scientists were, at the outset, making sizable and successful efforts in *both* directions.[50]

In this connection it should be noted that the most advanced accelerator concept at war's end—that of the proton synchrotron—was pursued at first *only* in Britain, by Mark Oliphant's team at Birmingham University (Oliphant, Gooden, and Hide, 1947; Gooden, Jensen, and Symonds, 1947; Hibbard, 1950). As Alvarez *et al.* (1955, p. 112) confessed at the end of the postwar decade,

> . . . for some time the feeling in this country was that, although Oliphant's plan was most attractive in many ways, so many unsolved and serious problems were involved in its practical realization that alternative methods of attaining high-energy protons should be explored.

Here as in the preceding five or six decades, British physics profited significantly from "colonial" reinforcements: a large fraction of Oliphant's team were Australian, as he himself was. These were radiophysicists who found no Australian accelerator projects to absorb them (Cockburn and Ellyard, 1981, pp. 136–143), but did not opt for radio astronomy. As Sullivan (1988, pp. 311–312) points out, initially the Vacuum Physics (i.e., accelerator) Section of the CSIR Radiophysics Division had a far larger research staff than did radio astronomy, but it "died away within two years." Colonial reinforcements could not be relied on for very long in the postwar world, and by the mid-1950s Oliphant's project and Britain had, in turn, been surpassed by the much larger—i.e., much more amply funded—American proton synchrotron projects at Berkeley and Brookhaven (Kirge, 1987).

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[48] In his autobiography, Casimir (1983), who became Director of Philips' research laboratories in 1946, never actually describes, or even asserts, the company's rapid recovery after World War II, but such is strongly implied by his two chapters on that period of his life.

[49] See Table I and Goldsmith (1948, pp. 47–53), who lists 12 as "proposed or under construction" in the UK, and 53 in the US.

[50] David Edgerton (1991) has identified an "R&D" tradition in British national security policy that helps explain this circumstance.

TABLE I. Synchro-cyclotrons constructed 1945–1958. Data sources: Krause (1957), Behman (1958).

	Pole Diameter (m)	(pole diameter)^3	Energy (MeV protons)	Year completed
USSR	6.0	216	?	1949
Canada	2.1	9	100	1949
Netherlands[a]	1.8	6	28	1949
Sweden	2.3	12	200	1951
Argentina[a]	1.8	6	30	1954
CERN[a]	5.0	125	600	1957
France[a]	2.8	22	155	1958
West Germany[a]	1.9	7	33	1958
United Kingdom				
Harwell	2.8	22	175	1949
Liverpool	4.0	64	400	1954
United States				
Berkeley	4.7	100	350	1946
Cal. LA	1.0	1	21	1948
Rochester	3.3	35	240	1948
Harvard	2.4	14	168	1949
Columbia	4.2	74	385	1950
Chicago	4.3	79	460	1951
Princeton	0.9	1	20	1951
Carnegie	3.6	46	440	1952

[a]Built wholly or partially by Philips' Gloeilampenfabrieken.

E. Microwave absorption spectroscopy of gases

Molecular spectroscopy through the absorption of microwaves in gases is, unquestionably, the premier example of a flourishing field of physical research created—in every sense—by radar. Although during the decade of the thirties Neil Williams at the University of Michigan had sponsored several microwave-spectroscopic investigations of the energy states of gas molecules, that work in Ann Arbor stood virtually alone in the prewar period.[51] Certainly interest in radio-spectroscopic methods as an alternative to optical spectroscopy for measuring the smallest differences in the energy states of atoms and molecules was much heightened by the results achieved by Rabi and co-workers with their molecular-beam magnetic resonance method—and all the more so as they put their work forward quite explicitly as investigations of "The radio-frequency spectra of atoms" (Kusch, Millman, and Rabi, 1940). That this work opened to Rabi's contemporaries the prospect of a new, unbounded field of atomic and molecular spectroscopy is suggested by Harnwell and Ridenour (1940, p. 91) speaking in the same breath of "the molecular-beam technic of Rabi and his colleagues" and of "molecular vibration frequencies that occur in the range 10^{10} to 10^{11} cycle/sec," i.e., ten to a hundred times higher than the highest frequencies explored by Rabi and Company.

But, as emphasized above, these results first began to appear in 1938, and Europe was already at war before their importance was widely recognized. They did not, in any case, alter the fact that the applicability of such radio-spectroscopic methods to gases remained remote. On the one hand, the pertinent transitions were of much higher frequency, far beyond the range of stable, precisely tunable oscillators, and, on the other hand, collisions between the gas molecules so broadened any resonance that, even had such oscillators been available, it would have been impossible in all but a few exceptional cases to ascribe the resonance to a definite transition.

Radar development during World War II altered every aspect of this situation: technology, motivation, theory. First and most important, from the technological side, radar was pushed repeatedly up in frequency, roughly by an additional factor of three with each step. Although by 1943, equipped with 3-cm (X-band) radar, the armed services really had everything they needed, neither they nor the virtuosic physicists could resist taking another, albeit slightly smaller, step up in frequency: "K-band radar at

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[51] In 1948, surveying this "already large and rapidly growing new field in pure physics," Walter Gordy (1948, p. 668) stated that "so far as I know, only one paper on microwave spectroscopy exists in prewar literature," namely, that stemming from C. E. Cleeton's 1934 Ph.D. dissertation at the University of Michigan. Donald Coles (1950, p. 306) could add a second, unpublished, Ann Arbor dissertation, that of H. S. Howe in 1940; these two works together were discussed by Howe as "so far as the writer is aware. . .the only attempts at microwave spectroscopy prior to the Second World War." Townes and A. L. Schawlow (1955), in their bibliography of well over one thousand works, add only two additional prewar references directly related to microwave spectroscopy, both to work in Germany, the later negating the earlier.

TABLE II. "Intensity" of construction of synchro-cyclotrons, 1945–1958. The intensity of work on a postwar synchro-cyclotron is taken as proportional to the total financial outlay (roughly proportional to the cube of the diameter of its magnet) and inversely proportional to the construction time (roughly from war's end to completion date).

	A Number of machines	B Median cubed pole diameter (m^3)	C Years under construction[a]	D "Intensity" $\left[\frac{A \times B}{C}\right]$
United States	8	41	4	82
Britain	2	43	6	14
Rest of World	8	11	7	13

[a] Year of completion minus 1945.5.

one centimeter." In introducing this, the twentieth chapter of his history of the M.I.T. Radiation Laboratory, Henry Guerlac (1947, pp. 507–508) is distinctly apologetic about the effort put into development of this radar system and unconvincing about its potential uses. Nonetheless, the fact that a K-band apparatus was perfected to the point of serial production allowed physicists, after the war, to take control of the frequency domain in which Cleeton had planted their flag twelve years before.

As luck would have it, the frequency chosen for K-band radar lay squarely in the middle of a water molecule resonance. Consequently, the operational range of these sets—they were intended for use in the final theatre of the war, the South Pacific, a region of high humidity—was most disappointing. Here, then, was an urgent motivation for the development of techniques for microwave spectroscopy, namely, to find just where the problem with K-band radar lay and to provide the means of avoiding such problems in the future.[52] Ironically, it was the military uselessness of K-band apparatus that contributed most effectively to the rapid takeoff of this new field of physical investigation after the war. Here was a problem of undoubted practical import, and there was surplus hardware available to researchers—available precisely because that hardware was of such little practical value (Townes, 1952, pp. 272–273).[53] Investigations were begun in several laboratories well before the end of the war (Guerlac, 1947, pp. 507–524), and their quite practical motivation warranted the pursuit of microwave spectroscopy in industrial as well as academic settings when the war was over.[54]

The earliest investigations, naturally, simulated operational conditions—e.g., atmospheric pressure—and the resonances were consequently very broad. This question of the width of the absorption line was addressed theoretically at the MIT Radiation Laboratory by John Van Vleck (1942, 1945, 1947a, 1947b; see also Bleaney, 1982; Anderson, 1987; Fellows, 1990) with occasional collaboration by Victor Weisskopf. They would publish their work, before war's end (Van Vleck and Weisskopf, 1945), in the Niels Bohr Festschrift issue of *Reviews of Modern Physics,*[55] where they represented it, as the occasion required, as motivated not by urgent practical problems—which in fact it was—but by a logical inconsistency in the existing theories of collision broadening.

Beyond the well-known and intuitively obvious decrease in the width of the resonance curve as the pressure of the gas decreases, the Van Vleck-Weisskopf theory also contained as an implicit, but unnoticed, counter intuitive consequence, that with decreasing gas pressure the height of the resonance curve—i.e., the maximum value of the absorption coefficient per unit length of path through the gas—does not decrease but remains constant

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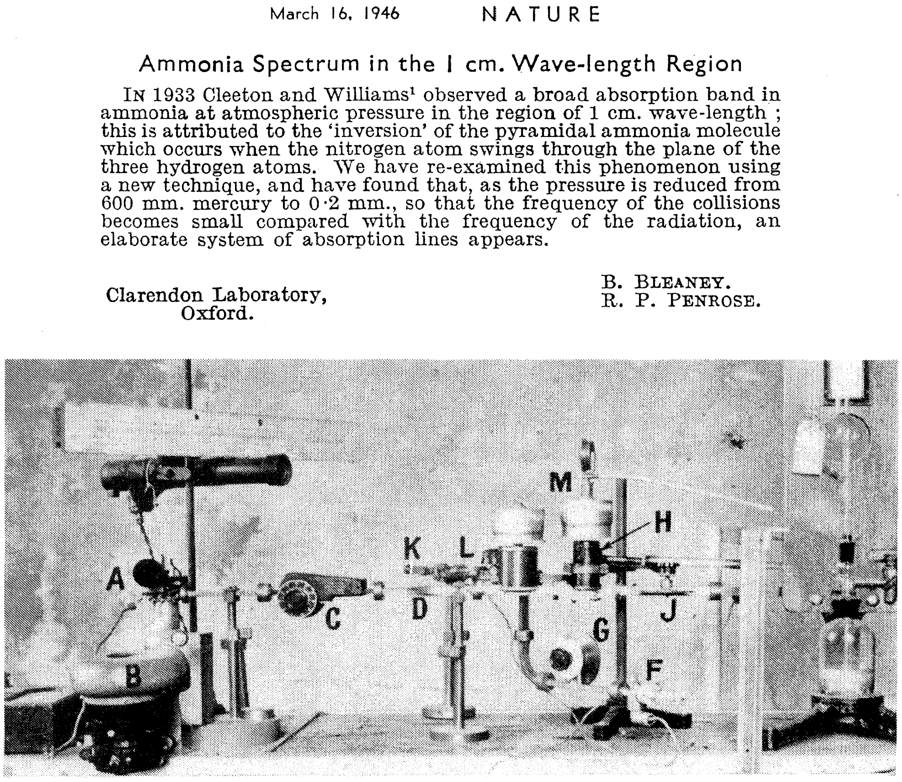
[52] E. M. Purcell (Rigden, 1986, p. 441) had suggested that military personnel were the source of alarm about the water vapor problem, causing it to be taken "more seriously than in retrospect it deserved to be." Further details may be expected from the book being prepared by Robert Buderi.

[53] Just how much K-band hardware was on hand at war's end is unclear. Guerlac (1947, p. 507) states categorically that "no production sets were built" and that no military use was made of it, while on the following page he describes it as "being held in reserve—except by the RAF which used K-band radar on a small scale to bomb Berlin in the closing days of the European war."

[54] In the US the prominent academic centers were Columbia (W. E. Lamb, Jr., C. H. Townes, B. P. Dailey), Duke (W. Gordy, W. V. Smith), Harvard (J. H. Van Vleck, E. B. Wilson, B. P. Dailey), MIT (M. Strandberg), Ohio State (D. Williams), and Yale (R. Beringer), while the industrial research labs supporting work in microwave molecular spectroscopy were Arthur D. Little, Cambridge, MA (G. W. King, R. M. Hainer), Bell Telephone Laboratories, Murray Hill, NJ (C. H. Townes), General Electric Research Laboratory, Schenectady, NY (A. H. Sharbaugh), RCA Research Laboratories, Princeton, NJ (D. W. Hershberger), Westinghouse Research Laboratories, Pittsburgh, PA (W. E. Good, D. K. Coles). Townes (1968) has emphasized, as an index of the shortsightedness of industrial research laboratories, that none continued to support such work beyond about 1950—whereas by 1955 the maser had emerged from his own work at Columbia University.

[55] Many details relating to the Bohr-Festschrift issue of *Reviews of Modern Physics* are to be found in Pauli (1993).



March 16, 1946 NATURE

Ammonia Spectrum in the 1 cm. Wave-length Region

In 1933 Cleeton and Williams[1] observed a broad absorption band in ammonia at atmospheric pressure in the region of 1 cm. wave-length ; this is attributed to the 'inversion' of the pyramidal ammonia molecule which occurs when the nitrogen atom swings through the plane of the three hydrogen atoms. We have re-examined this phenomenon using a new technique, and have found that, as the pressure is reduced from 600 mm. mercury to 0·2 mm., so that the frequency of the collisions becomes small compared with the frequency of the radiation, an elaborate system of absorption lines appears.

Clarendon Laboratory,
Oxford.

B. Bleaney.
R. P. Penrose.

A. Reflection klystron oscillator
B. Air blower
C, G, J, L. Variable attenuators
D. Waveguide " twist "
E. Cavity wavemeter
F, K. Crystal rectifiers
H. Resonant cavity
M. Micrometer head

FIG. 14. Microwave absorption spectroscopy of gases. Collage of first report of an investigation of a gas at low pressure (Bleaney and Penrose, 1946) and a photograph of the apparatus employed, as published in the first review of such studies (Bleaney, 1947). Reproduced with permission, Macmillan Magazines Limited (copyright 1946), and Institute of Physics Publishing Limited.

[Van Vleck, 1945, Eq. (18); Van Vleck and Weisskopf, 1945, Eq. (9)]. In the course of 1945, physicists in several laboratories recognized in this implicit consequence the possibility of a microwave spectroscopy of gases of a previously unanticipated precision.[56]

In the earliest postwar investigations, the apparatus was very simple (Fig. 14). The gas was confined in a resonant cavity, as in this first work of Bleaney and Penrose (1946) and in other early microwave-spectroscopic studies—e.g., those of Roberts, Beers, and Hill (1946). Although some later studies employing magnetic fields—e.g., Beringer and Castle (1950)—followed this method, practitioners generally moved quickly to relatively lengthy, nonresonant, waveguide absorption cells. A leading consideration here was to attain a readily measurable total absorption of the input microwave while avoiding saturation: even quite moderate rf power densities drive a significant fraction of the molecules into the upper of the two energy states connected by the transition in question, thus defeating just that crucial consequence of Van Vleck's theory on which the precision of the spectroscopic technique depended.

It is perhaps well to emphasize here that, however important magnetrons were, on account of their high power, in postwar particle acceleration, it was klystrons that were used in nearly every spectroscopic application because of their stability (frequency homogeneity) and tunability. Among all the high-frequency tubes the magnetrons—again on account of their transmitted power—have received by far the greatest attention in the history of radar, but we should remember (Pound, 1993) that radar requires a receiver as well as a transmitter, and that all radar receivers required a local oscillator able to match the transmitter in frequency while having good noise figures, stability, and tunability. To meet these requirements at centimetric frequencies, intensive effort was put into the development of klystrons during the war (Hamilton, 1947; Bell Telephone Laboratories, 1949, pp. 578–650; Callick, 1990, pp. 77–90.

Microwave spectroscopic investigations (Fig. 15) took two relatively distinct directions: a "chemical" direction (pursued also by chemical physicists in chemical laboratories), aiming to elucidate the structure of molecules, and a "physical" direction, aiming chiefly to ascertain properties of nuclei from their expression in molecular spectra, and therein competing very successfully with the molecular-beam magnetic resonance method.

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[56] This recognition *may* have motivated the work of Bleaney and Penrose (1946) at Oxford. William Good (1946), at Westinghouse, seems to have discovered it in the course of his experiments. Perhaps, indeed, only Charles Townes (1945) entered upon experiments with a clear theoretical expectation of this phenomenon.

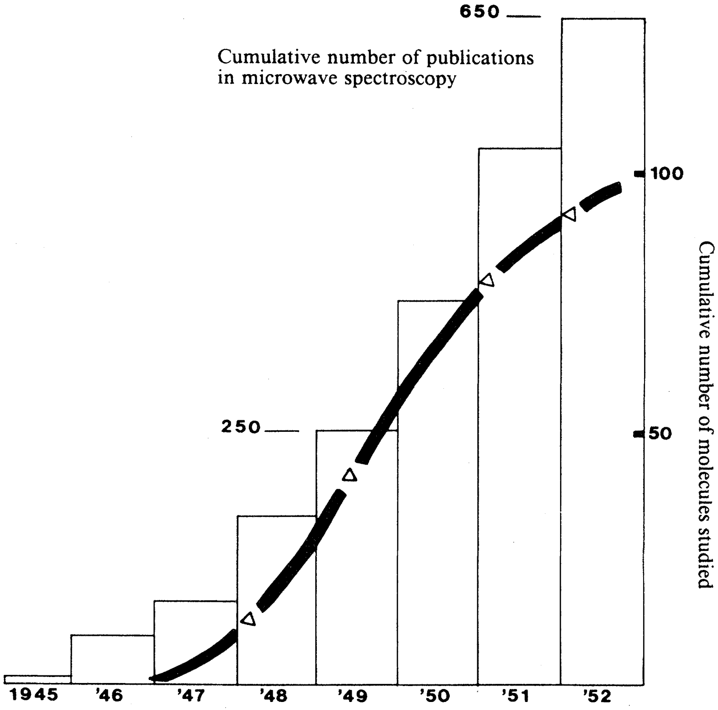

FIG. 15. Cumulative number of publications in, and molecules studied by, microwave spectroscopy. The publication count is based on the chronological bibliography in Townes and Schawlow (1955, pp. 649–682) and on the year-by-year totals reported by Gordy (1952). Reproduced from Forman (1987, p. 203), © 1987 by the Regents of the University of California, by permission.

F. Molecular-beam magnetic resonance spectroscopy

We saw in the course of our baseline survey that before World War II important physical quantities—notably, many hyperfine energy-level separations, but particularly that of hydrogen at 1400 megahertz—remained inaccessible to direct measurement by the molecular-beam magnetic resonance method, due to limitations of the high-frequency technology then available. The importance that attached to improving substantially upon the prewar precision of 5% will be appreciated if one recalls that as only one-electron (hydrogenic) atoms are described by rigorously soluble wave equations,[57] so also the hyperfine splitting is rigorously calculable only in the case of hydrogen—calculable, to be sure, in terms of the measured "anomalous" value of the magnetic moment of the proton (or deuteron) and the unquestioned value of the magnetic moment of the electron, $eh/4\pi mc$.

The high priority of this measurement, bearing directly on the fundamentals of atomic physics, led both Rabi and Jerrold Zacharias—who had accompanied Rabi from Columbia to the MIT Radiation Laboratory and had remained at MIT after the war—to choose it as the first problem to tackle upon returning to research. Even though each was well aware of the other's undertaking it, the importance of the measurement seemed to justify the risk of being scooped (Goldstein, 1992, pp. 85–86). Rabi and his postwar protégés reached the goal first (Nafe, Nelson, and Rabi, 1947), and with greater precision as well—1/1000%—so Zacharias and *his* protégés published only a very brief description of their own experiment (Nagle, Julian, and Zacharias, 1947).[58] The fuller report by Rabi's students (Nafe and Nelson, 1948, p. 723) made clear their reliance upon World War II radar hardware: "Frequencies in the range 1400–1800 Mc, required in the experiments on hydrogen, were obtained from a grounded grid oscillator (APR-20)"—i.e., a radar countermeasures search receiver—"using a 2C40 tube"—i.e., a high-frequency triode of so-called "lighthouse" construction developed during the war by General Electric.[59]

Thus we appear to have in the precision measurement of the hydrogen hyperfine splitting a clear example of the advance of physics as an "autonomous" activity: post-World-War-II physicists applied radar hardware to achieve a measurement whose desirability was previously recognized, but for which the instrumental means were not available.

Hydrogen and its isotopes are uniquely interesting;

---

[57] A clear, concise, chronological exposition of the stepwise refinement of the theory of the hydrogen spectrum is given by Series (1957).

[58] An overview of postwar atomic-beam experiments on the hyperfine structure of hydrogen is provided by N. F. Ramsey (1956, pp. 263–270).

[59] For the signification of APR as a radar countermeasures search receiver, see Guerlac (1947, pp. 1121–1122) and Suits (1948, p. 31). For APR-20 as one such, see *Electronics,* Jan. 1946, p. 94. For the 2C40, see McArthur and Petersen (1944), McArthur (1945), Guerlac (1947, pp. 328–330), Bell Telephone Laboratories (1949, pp. 379–382).

their hyperfine structures bear directly upon the theory of nuclear forces and, through the anomalous magnetic moments of the proton, the neutron, and—to everyone's surprise—also the electron, bear indirectly on the principles of quantum electrodynamics and on the theory of elementary particles. Apart from the hyperfine structure of hydrogen, however, MBMR spectroscopists showed little inclination to push beyond the prewar frequency range. And when they did begin to do so—to reach the hyperfine splitting of cesium at 9200 megahertz—their motivation was not the disinterested desire to understand, but rather the very practical purpose of constructing an atomic clock, i.e., a frequency standard based on this especially advantageous atomic transition.[60]

The results of both Rabi's and Zacharias' measurements of the hydrogen hyperfine splitting showed a small disagreement with the calculated difference in energy levels—absolutely small, but relative to the precision of the measurements and of the calculations, "violent" (Nagle, Julian, and Zacharias, 1947; Goldstein, 1992, p. 336, note 29). Rabi's results were available in time to be presented at the historic Shelter Island Conference, a gathering of the theoretical physics elite early in June 1947—the first such gathering since war's end (Schweber, 1986a; 1994, Chap. 4).

At the Shelter Island Conference were presented also the initial, and still more unsettling, results of yet another Columbia University atomic-beam experiment on hydrogen. Willis Lamb, a theorist trained by Oppenheimer, had been drawn so far into microwave development work at Columbia during the war that, in returning to fundamental physics, he chose to devote himself to microwave measurements, with a graduate student skilled at experimental work as collaborator. As he said in their first publication:

> The great wartime advances in microwave techniques in the vicinity of three centimeters wavelength make possible the use of new physical tools for a study of the $n=2$ fine-structure states of the hydrogen atom (Lamb and Retherford, 1947).

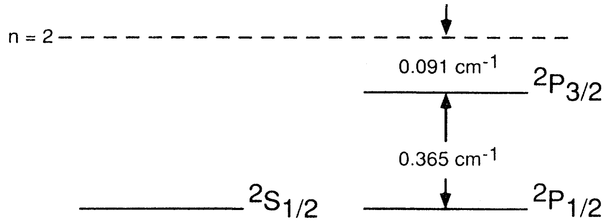

FIG. 16. Fine structure of $n=2$ levels of hydrogen according to the Dirac theory. The dotted line indicates the position of the $n=2$ level according to the Bohr theory.* From Lamb and Retherford (1950, p. 550) as redrawn by Schweber (1994, p. 208).

While Rabi and Zacharias were applying themselves to measuring the separation of the hyperfine energy levels in the ground state of the hydrogen atom, in order to compare that number with accepted theory, Lamb's object was to investigate in detail the agreement between experiment and the (still more firmly established) theory of the fine structure of the hydrogen atom (Fig. 16), those energy levels upon which the hyperfine structure is superposed (Lamb, 1946, 1955, 1983; Trigg, 1975, pp. 99–119; Schweber, 1994, Chap. 5). Theory required, namely, that among the three sublevels of the first excited state of hydrogen, $2^2S_{1/2}$, $2^2P_{1/2}$, and $2^2P_{3/2}$, the energy difference between the first two states, with equal total angular momentum, be precisely zero, and that between the two P states be 10 950 MHz (multiplied, of course, by Planck's constant h). In fact, as Lamb would find, the $2^2S_{1/2}$ state does not lie where theory had placed it, but 1000 MHz above the $2^2P_{1/2}$ state.

In the prewar decade these predictions were examined and reexamined by optical spectroscopists, working here, once again, at the limits of precision of their techniques. "The various spectroscopic works," Lamb and Retherford (1947) noted in their initial publication,

> have alternated between finding confirmation of the theory and discrepancies of as much as eight percent.

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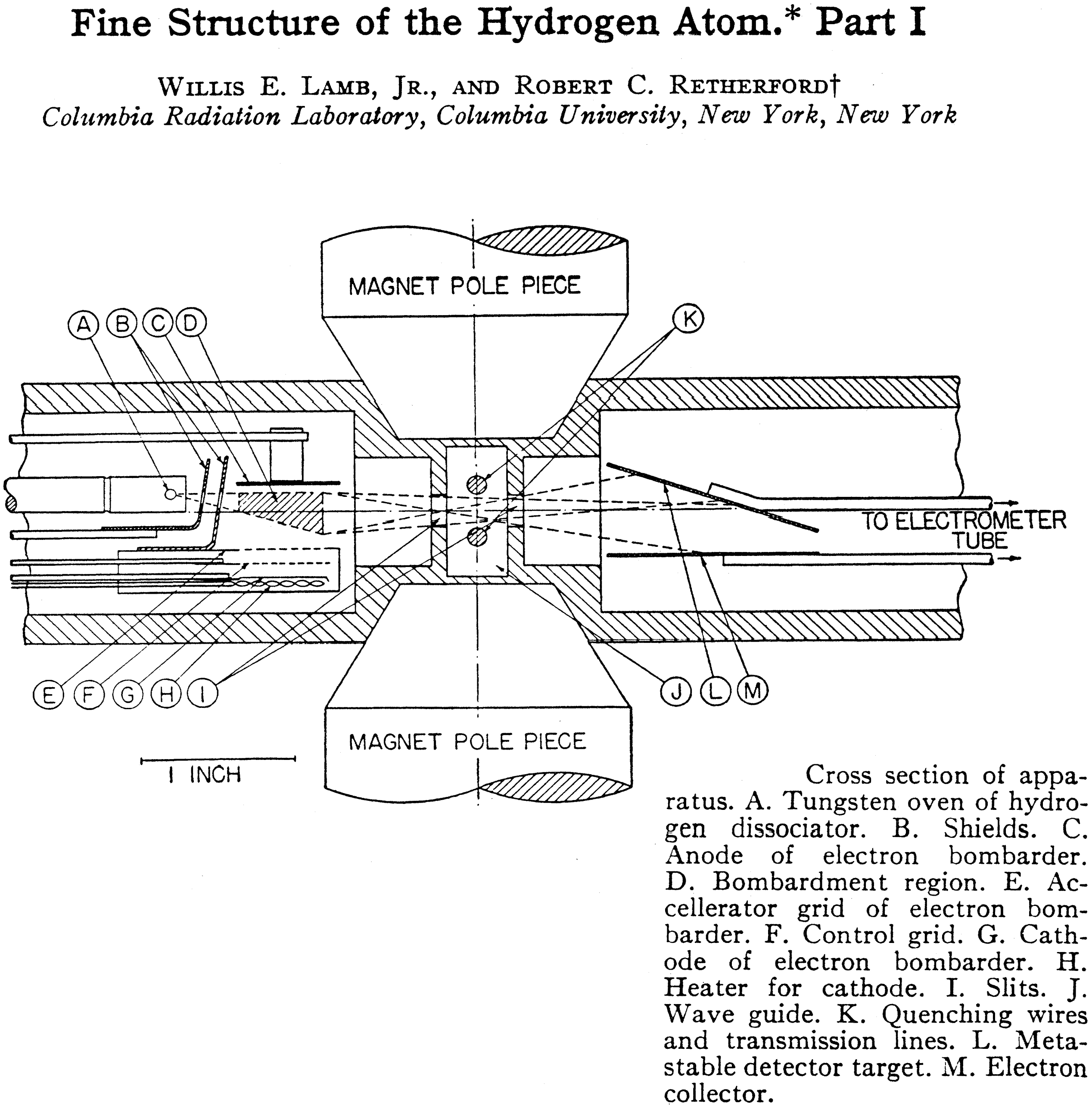
[60] The first to undertake such an atomic-beam frequency standard was Harold Lyons at the U.S. National Bureau of Standards in 1948—with detailed guidance by Rabi's lieutenant in the Columbia molecular-beams lab, Polykarp Kusch. In the early 1950s this project faltered at NBS, but the task was taken up successfully by Zacharias at MIT and by Louis Essen at Britain's National Physical Laboratory (Forman, 1985, 1986, 1995b). Essen's apparatus (Essen and Parry, 1957), like those of Lyons and Zacharias, took advantage of an important modification of the MBMR method discovered by Norman Ramsey (1949; 1956, pp. 124, 139–140; 1980; 1990): spatially separated rf regions. This made possible a great increase in the precision of measurement of the resonant frequency.

*The impressively successful theories of the hydrogen atom devised in the latter 1920s—first, although not wholly unambiguously, the solutions of the Schrödinger equation, and then, free of all ambiguity, the solutions of the Dirac equation—specified the energies of the several states of hydrogen's one electron, first the sequence of principal energy levels, $n=1,2,3\ldots$, and then, superposed upon these, the fine-structure levels, whose energies depend on the angular momenta of the atom's one electron. Of particular interest is the fine structure of the first excited state, for which the principal quantum number $n=2$. (This level, $n=2$, is that into which hydrogen's sole electron falls in emitting the atom's visible spectrum, the so-called Balmer series, and the fine structure of this level, being much less fine than that of the higher excited levels, effectively determines the fine structure of those visible lines.) These fine-structure levels are designated $2^2S_{1/2}$, $2^2P_{1/2}$, and $2^2P_{3/2}$. In this spectroscopic notation the letters S and P signify that the electron has, respectively, zero and one unit of orbital angular momentum about the nucleus, while the subscripts indicate the magnitude of the electron's total angular momentum, resulting from the vectorial addition of the electron's intrinsic one-half unit of angular momentum.

> More accurate information would clearly provide a delicate test of the form of the correct relativistic wave equation, as well as information on the possibility of line shifts due to coupling of the atom with the radiation field and clues to the nature of any non-Coulombic interaction between the elementary particles: electron and proton.

The apparatus Lamb and Retherford (1950) conceived (Fig. 17), however little it looked like one of Rabi's, was inspired by, and conceptually paralleled, the molecular-beam magnetic resonance apparatus. Thus a "beam" of hydrogen atoms issues from an oven/dissociator. (Dashed lines in Fig. 17 represent paths of hydrogen atoms from the dissociator *A* to the detector *L-M*.) By electron bombardment a small fraction of these atoms is excited from the $1^2S_{1/2}$ ground state to the $2^2S_{1/2}$ first excited state, which state is metastable. The metastable $2^2S_{1/2}$ atoms then pass across an X-band waveguide, which, like the transition region in Rabi's method, is in a uniform magnetic field. If the frequency of the microwaves in the waveguide corresponds to the energy difference between the magnetic sublevels of the $2^2S_{1/2}$ state and those of the $2^2P_{1/2}$ or the $2^2P_{3/2}$ states, transitions will be induced to those states. Since neither of the



Fine Structure of the Hydrogen Atom.* Part I

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Cross section of apparatus. A. Tungsten oven of hydrogen dissociator. B. Shields. C. Anode of electron bombarder. D. Bombardment region. E. Accellerator grid of electron bombarder. F. Control grid. G. Cathode of electron bombarder. H. Heater for cathode. I. Slits. J. Wave guide. K. Quenching wires and transmission lines. L. Metastable detector target. M. Electron collector.

FIG. 17. Apparatus of Willis Lamb and Robert Retherford (1950, Fig. 19).

2^2P states is metastable, i.e., since these atoms will decay spontaneously very rapidly to the ground state, the number of metastable atoms reaching the detector is decreased—again, just as in an MBMR apparatus. Lamb's detector is analogous to, but the inverse of, the surface ionization detectors used by Rabi and Company: the metastable hydrogen atom surrenders not its electron but its energy to the metal surface of the detector, from which an electron is then ejected.

The first step was to demonstrate the metastabilty of the $2^2S_{1/2}$ state of hydrogen is not destroyed by stray fields and the effectiveness of the proposed detection technique. For this, Lamb and Retherford (1950, p. 564) improvised an apparatus in which they made use of "permanent magnets of a type once used for K-band magnetrons"—with which Columbia, as a locus of wartime K-band magnetron development, was amply stocked. In the actual experiment (Lamb and Retherford, 1950, p. 566),

> Frequencies between 3000 and 10,000 Mc/sec were obtained directly from 2K41, 2K44, or 2K39 klystrons. Those frequencies near 12,000 Mc/sec were obtained by doubling the frequency from a 2K44 klystron in a 1N23 crystal multiplier. Frequencies from 1500 to 2600 Mc/sec were obtained from a 2C40 lighthouse tube oscillator.

All—klystrons, crystal multiplier, and lighthouse tube—were products of World War II radar research and development.[61]

What prewar optical-spectroscopic techniques could only hint at, Lamb was able to measure to an accuracy of better than 1%. This served as the essential stimulus for a reconsideration of the foundations of quantum electrodynamics by American theorists and the development of methods of calculation that could match the precision of measurement step for step. These same perspectives led to a reconsideration from the theoretical side of the previously unquestioned magnetic moment of the electron. A theoretically predicted 0.1% increase (i.e., by a factor $1+\alpha/2\pi=1+e^2/hc$) then found a quick experimental confirmation (Kusch and Foley, 1947, 1948; Schweber, 1994).

G. Magnetic resonance in condensed matter

If one were obliged to choose the one experimental technique most pervasive in physical research of the postwar decades, there can be little doubt, I think, that it must be the technique of magnetic resonance. The brilliant success Rabi and Company had made of magnetic resonance in conjunction with the molecular-beam method led to its adaptation to numerous other experimental situations in the immediate postwar period. Thus, for example, Lamb's experiment revealing the shift of the $2^2S_{1/2}$ state relative to the $2P$ states in hydrogen was not merely a molecular-beam experiment, but also a magnetic resonance experiment, however little the shift itself was intrinsically a magnetic phenomenon.

When one looks beyond the extremely demanding molecular-beam method—which although practiced more widely after the war than before, was then still to be found in only a very few laboratories—when one looks even beyond the many other applications of the technique to processes in rarified matter (e.g., Bitter, 1949; Brossel and Bitter, 1952; Brossel, 1969; Stroke, 1969), when one looks, finally, to magnetic resonance as applied to condensed matter, the proliferation of magnetic resonance investigations becomes staggering. Indeed, the origins of the magnetic resonance technique lay in prewar studies of condensed matter, and it was in studying condensed matter that it found its most extensive application after the war. Within but a few years, these applications had so swamped all others that the term itself was no longer commonly taken to refer to work with molecular beams such as was done by Rabi and his students.

The scope of application that just one of these postwar techniques—nuclear magnetic resonance—had achieved by 1960 was exuberantly sketched by one of its most energetic practitioners, Anatole Abragam (1961, preface):

> Since the first successful detection of nuclear resonance signals late in 1945, nuclear magnetism has developed at a pace which after fifteen years still shows no sign of slackening. Besides its first and obvious application to the measurement of nuclear moments, it has become a major tool in the study of the finer properties of matter in bulk. Structure of molecules, reaction rates and chemical equilibria, chemical bonding, crystal structures, internal motions in solids and liquids, electronic densities in metals, alloys, and semiconductors, internal fields in ferromagnetic and antiferromagnetic substances, density of states in superconductors, properties of quantum liquids, are some of the topics where nuclear magnetism has so far provided specific and detailed information.

By 1962 the rate of publication of papers on magnetic resonance—excluding, as in any case numerically insignificant, the application in molecular-beam experiments—was already above 500 annually (Norberg, 1965). Over the next fifteen years, 1961–1975, Bloembergen, Purcell, and Pound's (1948) "classical" exposition of nuclear magnetic resonance received 1145 citations in the scientific literature, as surveyed by the Institute for Scientific Information, leading every other physical, chemical, and mathematical paper of the 1940s (Garfield, 1980, pp. 54–60).[62]

One can thus understand the melodramatic expression of one conference organizer (Coogan, 1970, p. vii), who

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[61] Regarding the 2C40, see note 59, above; regarding the 1N23, see Scaff and Ohl (1949); for the 2K39, 2K41, and 2K44—all Sperry klystrons developed during the war—see Hamilton (1947, p. 36).

[62] Marsden and Rae (1990, p. 121) cite data indicating that by 1964 16% of all chemistry papers reported use of NMR, almost as large a proportion as the then reported use of ultraviolet spectroscopy, but still only half the proportion using infrared spectroscopy—on the adoption of which, see Rabkin (1987).

remarked that those first successful detections of nuclear resonance signals amounted to "the detonation of an explosive wave of research on magnetic resonance which has spread throughout the world and shows no sign of abating." Almost simultaneously the editor of the newly founded *Journal of Magnetic Resonance* prefaced his first volume with a declaration that "there can be no doubt that the field of magnetic resonance has grown with a rapidity almost unprecedented in science" (Brey, 1969). And all this before there was more than the barest hint of the possibility of magnetic resonance imaging (Hahn, 1990; Marsden and Rae, 1991; Wehrli, 1992).

1. Nuclear magnetic resonance

Like microwave spectroscopy of gases, nuclear magnetic resonance—the phrase was evidently coined by Rabi (Gorter and Broer, 1942)—is a technique rather than a field of natural knowledge. At Harvard, Purcell, Torrey, and Pound (1946), using magnetic fields between 1000 and 10 000 gauss and frequencies of a few to a few tens of megahertz, detected the slight drop in the quality factor Q of a resonant cavity filled with the nuclear magnetically resonating substance—paraffin, in their first experiments—while Bloch and his collaborators at Stanford (Bloch, 1946; Bloch, Hansen, and Packard, 1946a, 1946b) detected the slight radio-frequency signal induced in a pickup coil by the precession of the nuclear magnetic moments in water (Gerstein, 1994; Rigden, 1986).

The Harvard group—now Bloembergen, Purcell, and Pound (1948)—in giving NMR technique its "classical" form, moved closer to the methods of the Stanford nuclear induction group, abandoning the resonant cavity of their initial measurements. Their apparatus (Fig. 18) is, essentially, an rf bridge. The identical resonant circuits to be balanced against each other are, respectively, that in the shielded box to the right of the signal generator (30 MHz) and that in the box below the magnet, but including the coil surrounding the sample. The distance from tee A to tee D being a half wavelength greater via the latter circuit, off resonance the two signals nullify each

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Relaxation Effects in Nuclear Magnetic Resonance Absorption*

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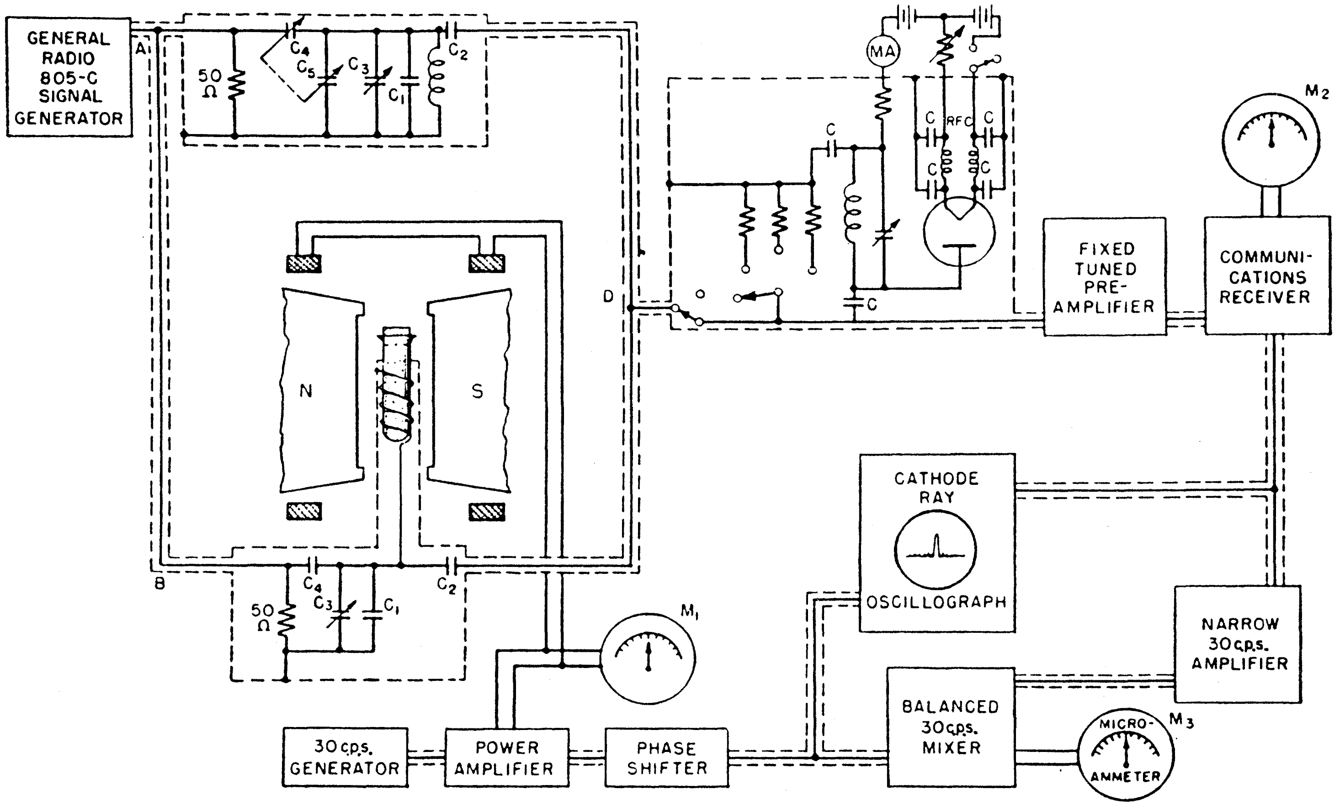

FIG. 18. Nuclear magnetic resonance apparatus in use by the Harvard group some two years after their discovery of the effect. Reproduced from Bloembergen, Purcell, and Pound (1948, p. 684).

other to within 1%. On resonance—i.e., when the (varied) magnetic-field strength is such that the rate of precession of the pertinent species of nucleus in the sample equals the frequency of the rf supplied by the signal generator—the NMR sample absorbs rf energy, reducing the Q of the coil and unbalancing the bridge. This is detected using a commercial shortwave receiver, such as was used by radio "hams," the rectified output being read on meter M_2. More precise—higher signal-to-noise—measurements are effected by the further detection system based upon 30-Hz modulation of the magnetic field and narrow-band, synchronous (lock-in) amplification and detection, with the output being displayed on the oscilloscope and meter M_3.[63]

The NMR technique, unlike microwave spectroscopy, say, owes no specific and inescapable debt to radar hardware. It is, however, generally indebted to the advance of radio-frequency instrumentation and methods over the course of the war—and to theoretical as well as experimental physicists' familiarity with them. Felix Bloch (1952, p. 209), the prime mover in the Stanford discovery of nuclear magnetic resonance, who had already had a first-hand acquaintance with the elaborate prewar experiments to measure the magnetic moments of proton, deuteron, and neutron,[64] stated in his Nobel Prize lecture that "the acquaintance with radio techniques during the war suggested to me still another and much simpler way, that of detecting the reorientation of nuclear moments through the normal methods of radio reception."

That Bloch thought in such conventional terms is surely connected with the atypicality of his war experience (Hofstadter, 1990): Bloch had not been enlisted during the recruitment for the MIT Radiation Laboratory (nor did he last long at Los Alamos), but was drawn by his Stanford colleague, the electrical engineer Frederick Terman, into the Harvard Radio Research Laboratory, devoted to radar and radio countermeasures (Suits, 1948, pp. 9–48; McMahon, 1984, pp. 200–205). In such work a relatively larger role is played by the radio reception techniques he then employed to detect precession of nuclear magnetic moments. Bloch shared his prize with Edward Purcell (1952, p. 232), who pointed in his lecture to "the development of new microwave techniques" and declared that "this experience turned out to be very valuable." Nonetheless, it should be recalled that Purcell had come into physics with an E.E. degree, and he later confessed to Van Vleck (1970) that he had been at war's end "rather fed up with microwave apparatus since he had worked with it so long during the war," and so turned back with relief to the conventional radio-frequency techniques NMR demanded.

Thus the war had not merely brought new frequency domains into the laboratory, but had provided far more flexible and powerful techniques for dealing with familiar ones. This is certainly suggested by Cornelis Gorter's repeated failures, 1936–1942, to observe nuclear magnetic resonance. The able, young Dutch experimental physicist had been the first to successfully observe resonant absorption of radio-frequency energy by paramagnetic ions in a solid (Gorter, 1936a, 1936b, 1937; Keith and Quedec, 1992, pp. 414–415). He then sought to observe the analogous resonant absorption by nuclei (Gorter, 1936c; Rigden, 1986, p. 442). Visiting Rabi at Columbia in September 1937, Gorter had pointed out how easily and well his own technique—driving the precession of the nuclear moments with a rf field perpendicular to the constant magnetic field—could be applied to Rabi's molecular beams (Gorter, 1947, p. 126; 1967). And Rabi's immediate success with the molecular-beam magnetic resonance method inspired Gorter's continuing but unsuccessful pursuit of nuclear magnetic resonance in Holland during the war (Gorter and Broer, 1942), using an rf detection method "rather similar" (Gorter, 1947, p. 126) to those successfully employed immediately after by the Purcell and Bloch groups. Presumably here, where it was not at all a question of previously inaccessible frequency domains, it was rather a question of refinement, sensitivity, and convenience of the equipment that the war had made available in long-accessible frequency domains.

Both the Harvard and the Stanford group approached their experiments as contributions to fundamental physics, and in particular to the precise measurement of nuclear magnetic moments, especially that of hydrogen and its isotopes—for which purpose it was indeed immediately used (Arnold and Roberts, 1947; Bloch, Nicodemus, and Staub, 1948; Gardner and Purcell, 1949). Regardless, however, of the motives and interests of its originators, the NMR technique was quickly "turned around" to give quantitative measures of the magnetic fields, and hence the physical and chemical environment, in which the precessing nuclei are situated (Purcell, 1948; 1952, p. 225; Rigden, 1986; Teichmann and Szymborski, 1992, pp. 288–289). In this unanticipated way NMR has become arguably the most widely diffused and most powerful of the analytical techniques emerging from twentieth-century physics.

2. Electron magnetic resonance

This phrase, in contrast with "nuclear magnetic resonance," has never been widely used. It serves well, how-

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[63] Regarding this method of lock-in detection, see Fig. 23 and discussion, below. Further, as regards experimental procedure, it may be remarked that, in contrast with the MBMR technique, where the C field was held constant and the oscillator frequency varied, in NMR and the other forms of magnetic resonance in condensed matter discussed below, the frequency was fixed and the resonant condition attained by varying the magnetic field.

[64] Bloch, who before the war had conceived and, with Luis Alvarez's collaboration, achieved a measurement of the magnetic moment of the free neutron by observing, in a cyclotron, the magnetic resonance depolarization of a neutron beam, now emphasized (Bloch, Nicodemus, and Staub, 1948, pp. 1025–1026) that all his ideas in this direction, the later as well as the earlier, were independent of Gorter and Rabi.

ever, to encompass several magnetic resonance phenomena observed in condensed matter, phenomena which, although each goes by a different name, are all electron-spin analogs of nuclear magnetic resonance: "electron paramagnetic resonance (EPR)," "ferromagnetic resonance," and electron spin resonance at so-called color centers in crystals. In contrast with NMR, electron magnetic resonance was directly dependent upon wartime radar hardware for its discovery, since in this case, at equal magnetic fields, the resonant frequencies are about 1000 times greater[65]—a few to a few tens of *kilo* megahertz.

The first observations of the magnetic resonance phenomenon that would soon come to be known as EPR—Gorter had vainly sought this magnetic resonance phenomenon as well—were made by Evgeny Zavoisky, 1944/45, at Kazan State University. Zavoisky's (1945a) original intent was to extend to liquids Gorter's observation of resonance/relaxation of paramagnetic ions in solids. Here, however, Jacob Frenkel (1945), a most capable theorist, entered the picture. Frenkel was then in Kazan in consequence of the evacuation of scientists from Leningrad (Frenkel, 1974; Peierls, 1994), and he turned Zavoisky's attention to electron-spin resonance. Zavoisky's (1945b) first experiments in search of this phenomenon were made at a frequency of about 10 megahertz, so that no absorption peak was observed, but only a maximum at zero magnetic field; i.e., the "peak" was lost in the few oersted of remanent magnetism. The experiments were then transferred to Pyotr Kapitza's institute in Moscow (Fig. 19), where a 10-cm magnetron did the trick (Zavoisky, 1946).

That radar was the source of any 10-cm magnetron in Kapitza's Moscow institute seems more than likely. It was manifestly so for David Halliday at the University of Pittsburgh. After a stint at the MIT Rad Lab, and still under 30, Halliday had returned to his alma mater, now as an Assistant Professor. Aware of Gorter's failure and Zavoisky's initial lack of success—"earlier experiments which were limited by the unavailability of oscillators of sufficiently high frequency" (Cummerow, Halliday, and Moore, 1947)—Halliday sought and found the effect (Cummerow and Haliday, 1946). The military radar origin of *his* hardware is very clearly stated: "the microwave oscillator was a Navy 3-cm test set, type AN/APM-3A" (Cummerow, Halliday, and Moore, 1947, pp. 1235–1236), its oscillator tube a Bell Labs 723A/B klystron.[66]

Halliday's work was followed up vigorously only in Holland and Britain, chiefly by Brebis Bleaney's group of microwave spectroscopists at Oxford.[67] It seemed "strange" to Van Vleck (1970, pp. 7–8) that American physicists, who had the requisite microwave equipment, "did not make a big splurge right after the war" in EPR. This is certainly a circumstance calling for explanation, perhaps in terms of the rather special (and complicated) paramagnetic compounds in which the effect was first observed and studied—Halliday's collaborator, G. E. Moore, was recruited as a physical chemist with the needed esoteric knowledge.

Whereas both prewar and postwar studies of paramagnetic relaxation were in search of a resonance phenomenon, the discovery of ferromagnetic resonance was an unexpected result of the continuation, with microwave radar technology, of a long and broad tradition of permeability measurements on ferromagnetic materials at high frequencies. A review completed in the last year of the war (Allanson, 1945) listed more than 50 such studies over the preceding quarter century. Among the measurements quoted, those at the highest frequencies were not at all recent, but were, rather, among the very earliest cited. This was the pioneering work of Glagoleva-Arkadieva's husband, V. K. Arkadiev, between 1910 and 1925, using Hertzian oscillators.[68]

Standard operating procedure in such investigations was to measure lossiness, $\mu\rho$, at a given frequency as a function of applied static magnetic field—not in search of a resonance of any sort, but as the means to deconvolve the electric resistivity ρ from the magnetic permeability μ, by extrapolating to the high-field limit, where $\mu=1$. Although devised without a thought of magnetic resonance phenomena, this experimental procedure duplicated exactly that used to search for nuclear-spin and electronic-spin resonances.

J. H. E. Griffiths, whose later career would be as an academic administrator, spent the first half of the war at Oxford directing the microwave group at the Clarendon Laboratory in the development of progressively shorter-wavelength klystrons—first 10 cm, then 3 cm, then 1.2 cm and 0.8 cm—and spent the second half of the war in London as Secretary of the Communications (i.e., camouflaged) Valve Development committee (Callick, 1990, pp. 3, 7, *et passim;* Morrell, 1992, pp. 298–301). At war's end, Griffiths returned to Oxford, to research, and "made use of surplus military apparatus" (Bagguley and Bleaney, 1990, p. 35) in order to do what occurred

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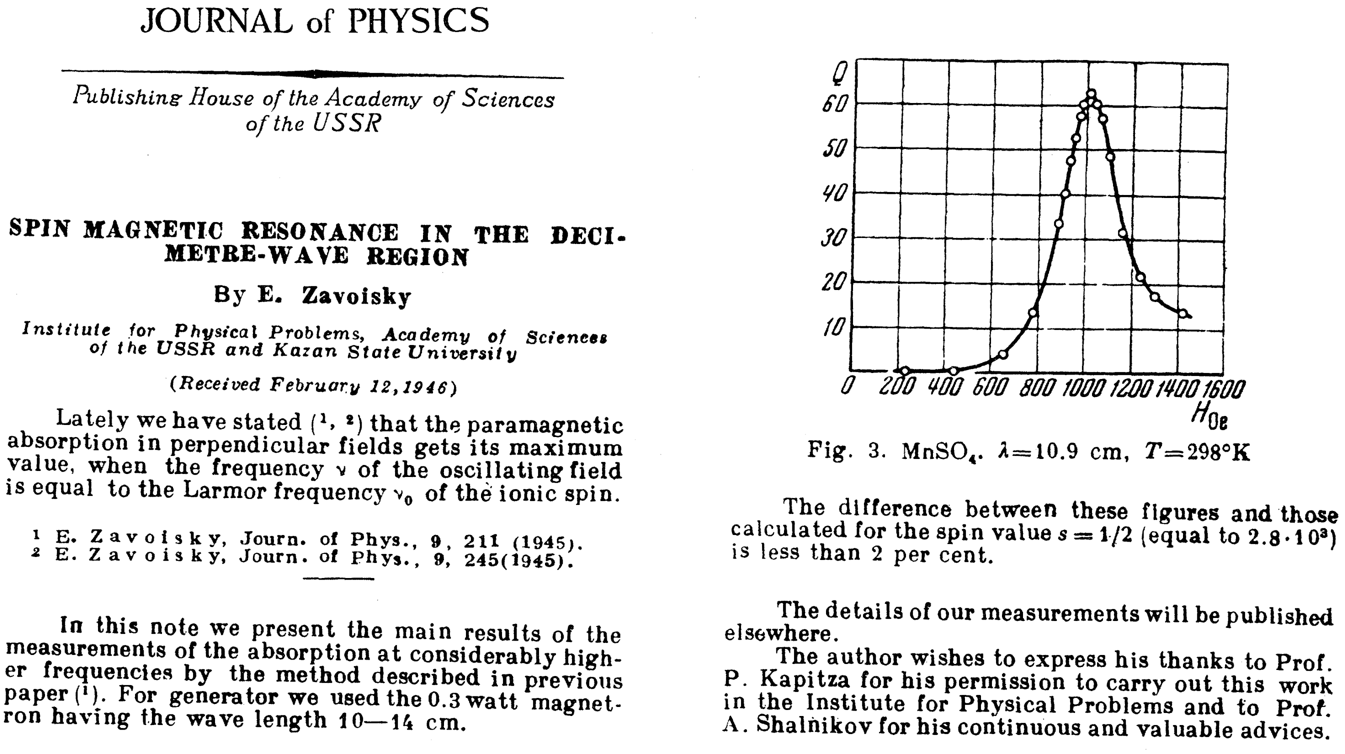
[65] Were it not for the proton's anomalous magnetic moment—featured above in the sections on molecular-beam magnetic resonance—this factor would be nearly 2000. Regarded as elementary particles with intrinsic (spin) angular momentum $\frac{1}{2}h/2\pi$, the proton and the electron would be expected to precess in a given magnetic field at rates inversely proportional to their masses (1836:1).

[66] "The 723A/B oscillator served as the beating oscillator for all radar systems operating in the 3-centimeter range until late in the war, when the 2K25 supplanted it" (Bell Telephone Laboratories, 1949, p. 590).

[67] Bleaney and Stevens (1953) provide an extensive review, which, however, fails to mention the work of Zavoisky or Frenkel; Bleaney (1993) does.

[68] See Dorfman (1970) and the discussion of Glagoleva-Arkadieva in Sec. I. The remarkable accuracy of the results that Arkadiev attained is indicated by the publication between 1923 and 1931 of some six researches disagreeing with his finding of a decline in permeability with increasing frequency, followed then by a vindication of his results (Hoag and Jones, 1932), with a response by Arkadiev (1933).



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SPIN MAGNETIC RESONANCE IN THE DECIMETRE-WAVE REGION

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(Received February 12, 1946)

Lately we have stated ([1], [2]) that the paramagnetic absorption in perpendicular fields gets its maximum value, when the frequency ν of the oscillating field is equal to the Larmor frequency ν_0 of the ionic spin.

[1] E. Zavoisky, Journ. of Phys., 9, 211 (1945).
[2] E. Zavoisky, Journ. of Phys., 9, 245(1945).

In this note we present the main results of the measurements of the absorption at considerably higher frequencies by the method described in previous paper ([1]). For generator we used the 0.3 watt magnetron having the wave length 10—14 cm.

Fig. 3. $MnSO_4$. $\lambda = 10.9$ cm, $T = 298°K$

The difference between these figures and those calculated for the spin value $s = 1/2$ (equal to $2.8 \cdot 10^3$) is less than 2 per cent.

The details of our measurements will be published elsewhere.

The author wishes to express his thanks to Prof. P. Kapitza for his permission to carry out this work in the Institute for Physical Problems and to Prof. A. Shalnikov for his continuous and valuable advices.

FIG. 19. Electron paramagnetic (or, spin) resonance, as initially observed, 1945–1946. Collage from Zavoisky (1946).

to many others too, viz., to follow the expected monotonic decline of permeability with length of electromagnetic wave, down that ladder of wavelengths which he had himself helped to construct.

To his surprise, Griffiths (1946) found peaks at 500 to 1000 gauss when using 3-cm microwaves, which moved up to 3000 to 5000 gauss when using 1.2-cm microwaves. The title of Griffiths' paper, "Anomalous high-frequency resistance of ferromagnetic metals," is revealing of the mental set from which he initially approached his experiments, i.e., shows how little he was looking for a magnetic resonance phenomenon. But as with every surprising observation, not every observer is able to make it: Griffiths' letter to *Nature* was followed immediately with one by J. B. Birks (1946), who found no resonance phenomenon. Neither had I. Simon (1946), working at the Charles University, Prague. In Griffiths' experiments the ratio $h\nu/H$ ranged from 3 to 14 Bohr magnetons, not the 2 to be expected from the precession of electron spins. The discrepancy was quickly cleared up by America's fastest-rising solid-state theorist, Charles Kittel (1947, 1948), who showed that the effective field was not H but the geometric mean of H and B ($=\mu H$).

Finally, brief mention may be made of the spin magnetic resonance of electrons trapped in those defects in ionic crystals known as color centers. As Teichmann and Szymborski (1992, pp. 288–289, 314) describe in their excellently crafted contribution to (Hoddeson *et al.* (1992), the discovery of electron-spin resonance in paramagnetic crystals led theorists immediately to surmise the existence of an analogous effect in crystals "colored" by radiation damage. Experimental discovery of the effect awaited, however, an effective collaboration between a microwave spectroscopist and such experts on radiation damage as were to be found only around nuclear reactors–not yet a widely available research tool.

H. Superconductivity and Fermi surfaces[69]

Both of these lines of research—superconductivity and Fermi surfaces—are highlighted as applications of radar

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[69] As the reader may perhaps have noticed, this section is the first in this paper to be titled not with a technique of physical research, but with a subject (object) of such investigations. Various of the preceding sections have indeed described significant additions to physical knowledge resulting from the application of the entitling technique, and it would be possible here to continue to hold to that principle, labeling this section not "Superconductivity and Fermi Surfaces," but "Measurements of Electrical Resistivity at High Frequencies and Low Temperatures." Alternatively, this, the preceding, and the following section might all be lumped together under the substantive rubric "Physics of Condensed Matter" or, as that expression is anachronistic, "Solid State Physics." In the period with which we are concerned even this latter expression seemed to many physicists to refer to no coherent body of natural knowledge; a significant party held out for the more restrictive, but conceptually and socially better founded research field known as "Physics of Metals" (Weart, 1992).

hardware and technique by Hoddeson *et al.* (1992, pp. 543–553, 210–221) in their chapters on the band theory of solids and on collective phenomena. Those authors quote A. Brian Pippard, a principal contributor in both these directions, who stands also at the focus of the present discussion, as stating to them that "the war turned me into a physicist rather than a chemist... it taught me about microwave techniques.... I came back [to Cambridge University] from the [British] Army with first-class techniques at my fingertips." In the United States, John Slater and his collaborators at MIT, Paul Marcus and Emanuel Maxwell, as well as William Fairbank at Yale, had likewise worked on radar during the war and contributed immediately afterwards to the first of these two areas of inquiry—namely, the great unsolved problem of superconductivity—by studying the penetration of high-frequency electromagnetic fields into metals at low temperatures. These studies took up directly from like investigations conceived and pursued in the late 1930s by Heinz London (1940), at the University of Bristol, who had used the highest frequencies available to him, 1500 MHz.[70]

Returning after the war to Cambridge, Pippard associated himself with David Shoenberg at the Mond low-temperature laboratory and began a program of researches published under the general title, "The surface impedance of superconductors and normal metals at high frequencies." As Pippard (1947a, p. 370) said in introducing the first of that series of papers,

> The experiments described... were designed to employ some of the recent advances in radio technique, arising directly from the wartime development of Radar, in order to obtain more accurate data than was possible at the time of London's work.

Pippard and his students, Robert Guy Chambers and Eric Fawcett, obtained their microwaves from a series of "CV" tubes—i.e., products of the British wartime Communications Valve committee (Callick, 1990, pp. 1–15)—and in addition, for the later work at higher frequencies, borrowed equipment from the Radar Research and Development Establishment at Malvern.[71] In their early experiments electrical resistance was determined by forming the metal into a "hairpin" loop connected to the terminals of the oscillator and measuring the frequency bandwidth—i.e., $1/Q$—of this resonant structure as a function of temperature. In later experiments (Fawcett, 1955; Pippard, 1957) at K-band frequencies, the surface of plane, single-crystal specimens was irradiated by microwaves issuing from a waveguide, and the surface resistance was measured by the heat generated.

Initially Pippard (1950b, 1953) focused on superconductivity—the development and criticism of the Fritz and Heinz London two-fluid model with distinct moieties of normal and superconducting electrons. But gradually his attention turned in the direction taken by Chambers (1952)—the application of these techniques to exploring the topology and topography of the Fermi surface in normal metals.

Barring all complications—all that makes a metal real and thus makes one metal different from another—the Fermi surface, i.e., the locus in momentum space of those electrons chiefly responsible for electrical conduction in a metal, is simply a sphere of radius proportional to the square of the momentum of the conduction electrons. The momentum here in question is not the very small momentum imparted to the electrons by the applied electric field and constituting the electric current, but the large momentum that they have in the absence of an applied field as a consequence of other electrons' "filling up" all momentum states of lower energy (i.e., all those interior to the Fermi surface), in conformance with the Pauli exclusion principle. For any real metal the Fermi surface departs in a more or less complicated way from a sphere (Fig. 20), the resulting form containing all the electrical properties and many of the magnetic properties of the metal. Speculation about those complications was an important part of the development of the band theory of solids in the decade immediately before, and in that immediately after, the war. But as late as the early 1950s the determination of the actual form of the Fermi surface of any real metal appeared to confront almost insuperable difficulties (Chambers, 1952, p. 408; Pippard, 1954, p. 282; Hoch and Szymborski, 1992, pp. 210–221).

The reorientation of Pippard's research program toward determination of Fermi surfaces was based on a return, once again, to Heinz London's (1940) interpretation of the fact that the resistivity of normal metals measured at a high frequency did not continue to fall with decreasing temperature to the extremely low values reached by the dc resistivity, but "bent over" to a constant value, independent of temperature (Pippard, 1947b). London attributed this "anomalous skin effect," as it came to be known, to the circumstance that at any given high frequency the depth to which the electric field penetrates into the metal decreases with temperature, owing to the rapid fall in resistivity at low temperatures, while the same decreased resistivity is, by definition, a great increase in the mean free path of the conduction electrons. At sufficiently low temperature that mean free path becomes many times greater than the skin depth, and under such conditions only a small fraction of the conduction electrons are able to contribute effectively to the current,

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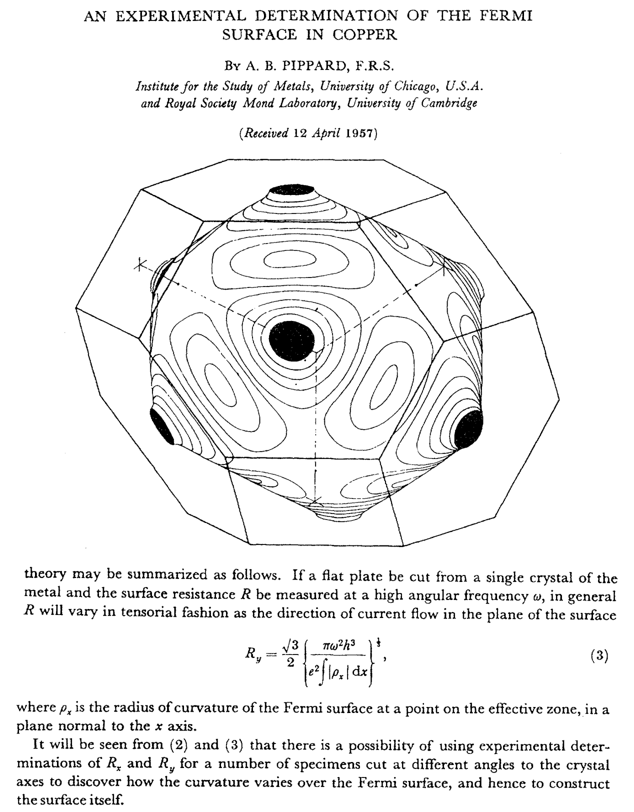
[70] On the London brothers, Fritz and Heinz, see Everitt and Fairbank (1970), Shoenberg (1971), Heims (1991), Dahl (1992, pp. 225–238), and especially Gavroglu (1995).

[71] Pippard (1947a, p. 373) and students began at 1200 MHz from a CV-90 disc-seal triode, similar to the General Electric "lighthouse tubes," continued (Chambers, 1952, p. 483) at 3600 MHz with a CV-67 S-band reflex klystron, at 9400 MHz with a CV-87 X-band reflex klystron (Pippard, 1950a, p. 101), and, at 36000 MHz, with a VX-357 klystron (Fawcett, 1955, p. 519), which, presumably, was a further development of the VX-302, a Clarendon Laboratory experimental K-band reflex klystron. For all the above, see Callick (1990, pp. 81–89).



AN EXPERIMENTAL DETERMINATION OF THE FERMI
SURFACE IN COPPER

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(*Received* 12 *April* 1957)

theory may be summarized as follows. If a flat plate be cut from a single crystal of the metal and the surface resistance R be measured at a high angular frequency ω, in general R will vary in tensorial fashion as the direction of current flow in the plane of the surface

$$R_y = \frac{\sqrt{3}}{2}\left\{\frac{\pi\omega^2 h^3}{e^2\int|\rho_x|\,\mathrm{d}x}\right\}^{\frac{1}{3}}, \tag{3}$$

where ρ_x is the radius of curvature of the Fermi surface at a point on the effective zone, in a plane normal to the x axis.

It will be seen from (2) and (3) that there is a possibility of using experimental determinations of R_x and R_y for a number of specimens cut at different angles to the crystal axes to discover how the curvature varies over the Fermi surface, and hence to construct the surface itself.

FIG. 20. A Fermi surface determined. Collage of title, text, and figure 18, from Pippard (1957).

namely, that fraction whose Fermi velocities (positions on the Fermi surface in momentum space) are very nearly parallel to the surface of the metal. Such conduction electrons will be the more numerous the greater the radius of curvature of the Fermi surface, and as a first, qualitative result, Chambers (1952) drew attention to the inverse relation between the height of the residual average surface resistance and the area of the Fermi surface.

Tackling the problem generally and quantitatively, Pippard (1954) derived from this "ineffectiveness concept" more exact relations applying to each of the principal axes of resistance of single crystals in the case that the conductivity is not isotropic—and it is not if the Fermi surface is not spheric: if x and y are the principal axes in the surface of the crystal, the radius of curvature of the Fermi surface about the y direction is proportional to the inverse cube of the residual surface resistivity in the x direction (Fig. 20). Since, however, any actual measurement is of a resistance—an integrated resistivity—"the curvature appears only in integral form, and the integral is in any case to be taken around a path which can only be determined once the shape of the Fermi surface is known" (Pippard, 1957, p. 326). The construction of that surface out of the resistance measurements thus promised to be an extremely tedious one, involving much trial and error (Pippard, 1954, p. 282). But Pippard accomplished it, with the help of the very refined facilities for preparing imperfection-free metal surfaces at the University of Chicago's Institute for the Study of Metals, a postwar establishment, building upon the Metallurgical Laboratory of the Manhattan Project (Feffer, 1992).

I. Superfluidity

Among the phenomena responsible for that redrawing of the boundaries within physics such that "solid-state physics" was widened to "physics of condensed matter" is the superfluidity of liquid helium. Whereas electrical superconductivity of metals was recognized (1911) within three years after the liquefaction of helium (1908) made possible experimentation at 4 K and below, the recognition of a distinct state of helium itself below 2.2 K, "helium II," and in particular the recognition of its superfluidity, was a gradual, halting process, extending over three decades (Gavroglu and Goudaroulis, 1990). The very term "superfluidity" expresses the circumstance that when such a state finally gained recognition in the late 1930s, the strange behavior of liquid helium was perceived as closely analogous to superconductivity in metals. But however analogous to superconductivity, the phenomenon is neither electromagnetic nor ultrahigh frequentic. It owes its presence in this survey to the fact that some postwar investigations of superfluidity—namely, those of John Pellam on the velocity of sound and "second sound"—involved a direct application of radar techniques, and even radar hardware, *without* the use of microwaves.

"Second sound" is one of the extraordinary properties of liquid helium below 2.2 K. Whereas ordinary sound is pressure-difference waves propagated through the fluid, "second sound" is temperature-difference waves, or, equivalently—and it was from this direction that the idea first arose—differences in the concentration ratio of the normal and the superfluid moities of sub-2.2 K helium. The phenomenon was postulated by Laszlo Tisza (1938), stimulated by the work of Fritz London (1938; 1954, pp. 59–60, 77), in Paris, where the flight from Nazism had brought the two together. Second sound here appeared as a consequence of taking quite literally the two-fluid implications of London's (1938) suggestion that what happens in helium as the temperature is lowered below 2.2 K is a Bose-Einstein condensation, in which the liquid separates into two phases—not in actual 3D space, but in momentum space—with the superfluid portion increasing as the temperature falls toward absolute zero (Gavroglu and Goudaroulis, 1990, pp. 70–72).

The notion found ready acceptance in a rapid-fire series of experimental revelations of superfluid helium's astonishing viscous and heat-conduction properties, 1938–1941, among which Pyotr Kapitza's (1938) discoveries in the USSR were the most important (Shoenberg, 1985; Hoddeson, Schubert, Heims, and Baym, 1992, pp. 511–518, 564–565, 608–609). Lev Landau, jailed on political grounds in 1938, was released the following year on Kapitza's recognizance—in response to

Kapitza's appeals to Stalin that Landau was needed to interpret these experiments on helium (Boag, Rubinin, and Shoenberg, 1990, pp. 60–64, 348–350). Landau threw himself into the task and, disdaining Tisza's theory, obtained second sound, and much more, from a general conception of quantized hydrodynamic excitations of the superfluid (London, 1954, p. 88). It was for this work that Landau would be awarded a Nobel Prize in 1962.[72]

Wartime disruptions notwithstanding, second sound was demonstrated experimentally in 1944 in the USSR by Vasily Peshkov (London, 1954, pp. 80–82; Lifshitz, 1968; Andronikashvili, 1989, pp. 110–113). And here arose the possibility of discriminating between the two theories/theorists. Tisza had predicted that "somewhere between 0.6 and 1 K, helium II gradually becomes a homogeneous liquid and the thermo-mechanical effect vanishes" (Tisza, 1948). Landau's theory, as elaborated during the war years by Evgeny Lifshitz, also predicted a decline of the velocity of second sound in the vicinity of 1 K, but then a rapid rise at lower temperatures (Tisza, 1947b; London, 1954, pp. 97–99; Pellam, 1960). Acrimonious controversy among the theoreticians about the proper approach to a detailed, quantitative theory of second sound gave a filip to experimentalists' efforts at more precise measurements of second sound's dependence on temperature.

John R. Pellam had joined Harvard's Underwater Sound Laboratory in 1941, shortly after completing this bachelor's degree at MIT at the mature age of 26. In 1942 he transferred to the US Navy's Operations Research Group, under MIT theorist Phillip Morse, serving through the war in Britain, Africa, and the Pacific. At war's end he returned to MIT—where Tisza had landed in 1941. There, as a graduate student, Pellam worked with low-temperature physicist Charles F. Squire, who had himself served during the war as scientific consultant to the Secretary of the Navy. Between them, the two physicists had in sonar and radar a set of concepts and tools for engaging and gauging the physical environment. These they applied, first, to the measurement of the velocity of ordinary ultrasonic waves in liquid helium (Pellam and Squire, 1947b) and then (Pellam, 1949) to the velocity of second sound (Fig. 21).

> At that time Samuel Collins was completing the first model of his famous helium liquefier, which later became the mainstay of most low-temperature physics laboratories. Pellam enjoyed recounting the story that the first successful run of his thesis experiment was carried out inside Collins' first liquifier—on the day of its first successful run.[73]

These pulse techniques promised higher precision than the "classical" continuous-wave methods employed during the war by Peshkov and shortly after it by Cecil Lane and his students William and Henry Fairbank at Yale. And they appeared at first to confirm the support that those previous measurements gave to Tisza over Landau.[74] Indeed Pellam's (1949) final results, submitted in December 1948, were quite consistent with Peshkov's. Nonetheless, he declined to make any judgment between his MIT colleague Tisza (1949) and Landau: "A decisive answer apparently awaits the application of pulse methods to paramagnetically cooled liquid helium" (Pellam, 1949, p. 1192). Continuing these experiments, using the same radar techniques, at the National Bureau of Standards, where apparatus for cooling by adiabatic demagnetization of paramagnetic salts made it possible to reach much lower temperatures than were obtainable simply by pumping off the vapor from liquid helium (as the technique then was), Pellam and Scott (1949) came to the emphatic conclusion that "*The velocity increases markedly with decrease of temperature below 1 °K.* This is in line with the predictions of Landau."

J. Signal-to-noise and all that

By tradition, the American Physical Society held its annual spring meeting in the last week in April, in

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[72] "But Landau's greatest contribution to physics," in the estimation of Evgeny Lifshitz, Landau's closest and most constant associate, "was the theory of quantum fluids" (Kapitza and Lifshitz, 1969, p. 153). This judgment he reaffirmed in his introduction to Landau's collected papers (reprinted in Khalatnikov (1989, p. 22). Landau's Nobel prize diploma is reproduced photographically in Khalatnikov (1989, facing p. 192). Landau (1941a) opened his original exposition of this theory of quantum fluids with a scathing denunciation of the physical assumptions underlying Tisza's theory, which appraisals are only slightly moderated in the précis "Dau" published in America (Landau, 1941b, preceded by Kapitza, 1941). The Landau-Tisza controversy is the particular subject of Gavroglu and Goudaroulis (1988).

[73] Quoted from Pellam's obituary (Craig, Reines, and Shaw, 1978). For the Collins cryostat see Brickwedde, Hammel, and Keller (1992, pp. 370–371, 496–497).

[74] Lane *et al.* (1946); Lane, Fairbank, and Fairbank (1947). Pellam and Squire (1947a) reported their method and their results with ultrasound at the APS meeting in New York City, February 1, 1947, at which session the Yale group presented two papers on second sound (Fairbank, Fairbank, and Lane, 1947; Lane, Fairbank, and Fairbank, 1947), both explicitly confirming Tisza, who himself delivered a paper on the subject (Tisza, 1947a). Tisza's (1947b) fuller exposition, laying out the case for his theory versus Landau's, provoked Landau (1949) to a slashing attack on Tisza's theory, root and branch: "The experimental data which are available at present are yet insufficient to disprove Tisza's assertion. . . . But I have no doubt whatever that at temperatures of 1.0–1.1 K the second-sound velocity will have a minimum and will increase with the further decrease in temperature."

Washington, D.C. Apart from *the* annual meeting, held in New York City late in January each year, the Washington meeting was always the largest and most important. In 1946, and in that year only, the APS broke with tradition and held its late April meeting in Cambridge, at Harvard and MIT. As the society's perpetual secretary, Karl Darrow (1946), reported, "With eleven hundred registrants, this proved to be the greatest meeting we had ever held by ourselves." Moreover, "The circumstances of the time conspired to make this the outstanding 'radar and microwave meeting' of the Society." The titles of the three topical symposia, and more particularly those of the papers invited for them (Fig. 22), give a clear indication of what physicists returning to research saw as most pertinent in the results of the war's radar R&D.

Between papers on the several types of microwave "tubes," on the one hand, and those on amplifiers and other electronic circuitry of general applicability, on the other hand, was a symposium on "Random Processes and Noise."[75] Here the speakers described, not hardware and technique in the familiar sense, but a new sophistication about the conditions of physical measurement that had resulted from their wrestling with the problem of "threshold signals," trying to discern in the noisy output of a radar receiver the faint reflection of a real target (Middleton and Van Vleck, 1946; Lawson and Uhlenbeck, 1950). These principles of experimental design and analysis relating signal, noise, bandwidth, etc., are, of course, in no way specific to radar or microwaves—even less specific than the oscilloscopes, crystal diodes, polyethylene cables, and electronic circuitry that, as general laboratory utilities, have been largely omitted from this survey. Nonetheless, these general principles for the analysis of instrumental performance have gained a place in this survey because physicists first became imbued with these principles during the Second World War, and became so through their involvement with radio electronics, especially radar (Pound, 1993).[76]

For that generation of "Rad Lab" veterans—both physicists and radio astronomers—the single piece of equipment that affected most strongly their awareness of noise as a factor in experimental design was, unquestionably, the microwave radiometer (Fig. 23) designed and employed by Robert Dicke (1945; Dicke *et al.*, 1946; Sullivan, 1982, p. 105) to address the absorption of K-band microwaves by atmospheric water vapor. Dicke drew upon and combined several incompletely integrated concepts—thermal noise in an electrical circuit, thermal radiation equilibrium in a cavity, and the noise power received by a radio antenna (Burgess, 1941). With these fundamental considerations Dicke further combined in an ingenious way the technique of "lock-in detection."

"Lock-in detection" refers to a signal-processing technique whereby an otherwise indiscernible object of observation, or response of an experimental system, is made to stand out from the enveloping noise by directing the instrument alternately at the signal source and at the same source in the absence, total or partial, of the signal. If this alternation is at a low audio frequency, it is then possible to subtract out the noise by means of a multiplier/rectifier locked by a synchronous reference voltage to the desired signal. Moreover, the combination of synchronous reference signal and multiplier, followed by a low-pass filter, functions as a narrow bandpass system, yielding the sought-for signal, free of a great part of the systemic noise (Meade, 1983; Forman, 1995c).

In Dicke's instrument (Fig. 23) a synchronous motor rotated a disk, appropriately shaped and partially inserted into the waveguide between antenna and receiver (comprising local oscillator, balanced mixer, wide-band amplifier, and second detector), such that during one-half of the 30-Hz cycle the receiver "saw" the signal source, and during the other half "saw" the disk, which thus served as a 300-K noise source. To function as a radiometer—i.e., as a microwave noise meter—the re-

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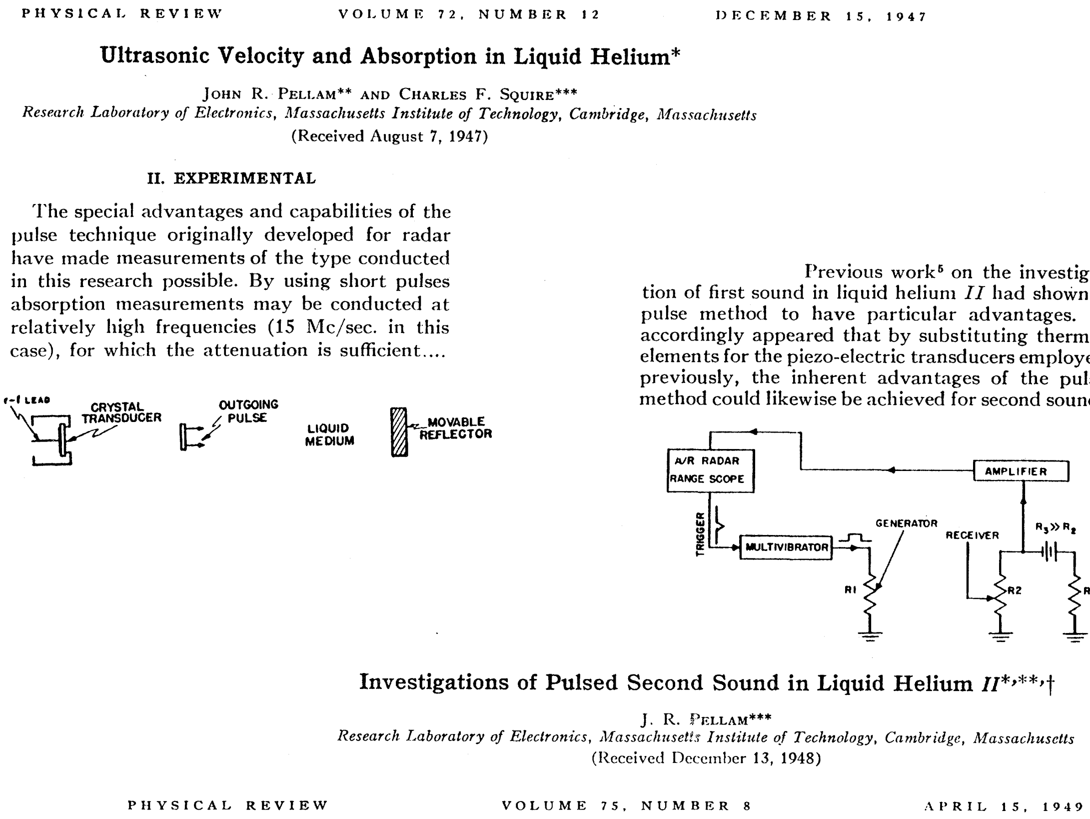
[75] The third main grouping of invited papers at this April 1946 Physical Society meeting, "Circuits for precision measurements" (see Fig. 22), is also noteworthy as a statement of physicists' expectations about the facilities resulitng from radar R&D that would prove most valuable in postwar physical research. This expectation was stated even more specifically and categorically by Guerlac (1947, p. 507), viz., that the research tool supposed to be of the greatest value to postwar physics was "the precision methods evolved during the radar program for the accurate measurements of extremely short time intervals." In retrospect this expectation seems very far from the mark.

[76] An early and important example is the account that the Stanford group (Bloch, Hansen, and Packard, 1946b) gave of their "nuclear induction" experiment and the reasoning through which its design was developed. Whereas I have not encountered a single prewar book giving prominent or extended consideration to signal and noise in the context of technique of physical experiment, the leading postwar guide to experimental technique, Fretter (1954), begins—chapter one, section one, page one—with "NOISE" and continues, section two, with "TIME-BANDWIDTH PRODUCT" and, section three, "GAIN-BANDWIDTH PRODUCT." In his preface to the 1968 reissue of this book, Fretter expressed satisfaction that through all the changes of those 15 years in physics and its experimental methods, just these topics—"the basic principles of experimental physics"—have remained valid. A further indicative example is Smith's (1957) book on the detection and measurement of infrared radiation. Smith, who was head of the Physics Department of the British Radar Research Establishment, Malvern, chose to devote two chapters, i.e., "a considerable part of the book" (p. vi), to a discussion of fluctuations and the limits they set to observation.



PHYSICAL REVIEW VOLUME 72, NUMBER 12 DECEMBER 15, 1947

Ultrasonic Velocity and Absorption in Liquid Helium*

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(Received August 7, 1947)

II. EXPERIMENTAL

The special advantages and capabilities of the pulse technique originally developed for radar have made measurements of the type conducted in this research possible. By using short pulses absorption measurements may be conducted at relatively high frequencies (15 Mc/sec. in this case), for which the attenuation is sufficient....

Previous work[5] on the investigation of first sound in liquid helium *II* had shown a pulse method to have particular advantages. It accordingly appeared that by substituting thermal elements for the piezo-electric transducers employed previously, the inherent advantages of the pulse method could likewise be achieved for second sound.

Investigations of Pulsed Second Sound in Liquid Helium *II**,**,†**

J. R. PELLAM***
Research Laboratory of Electronics, Massachusetts Institute of Technology, Cambridge, Massachusetts
(Received December 13, 1948)

PHYSICAL REVIEW VOLUME 75, NUMBER 8 APRIL 15, 1949

FIG. 21. Superfluid helium as acoustic medium investigated by radar techniques minus the microwaves. Collage of titles, text, and figures from Pellam and Squire (1947) and Pellam (1949).

ceiver had to have a wide frequency acceptance, for the signal sought was the noise power received across this spectrum. In order, however, to retain this signal while greatly reducing instrumental noise in subsequent stages of the detection process, the output of the wide-band receiver is followed by an amplifier accepting only a narrow band around the 30-Hz alternation frequency. The output of this bandpass amplifier is fed into a balanced mixer, where it is multiplied by a synchronous 30-Hz reference signal from a generator driven by the same synchronous motor rotating the 300-K noise disk. The result is that all signal components coming through the detection system synchronous with the reference signal produce dc outputs, while nonsynchronous (i.e., instrumental noise) components appear at harmonics of 30 Hz. Putting the output thus obtained through a low-pass filter, one gets a dc measure of the desired quantity, the difference between the noise temperature of the signal source and the noise temperature of the antenna, and, moreover, a measure that is immune to drifts in the instrument's operating parameters ($1/f$ noise) occurring on a time scale greater than 1/30 second.

The employment of Dicke's radiometer as a "telepyrometer" is illustrated by Fig. 24. These measurements of the microwave temperature of landmarks seen from an MIT rooftop were evidently made by holding the horn antenna of the radiometer at a fixed angle from the vertical, and then rotating it in azimuth through 360°. Thus for each setting of zenith angle (75°, 77°, . . . , 91°) there is a tracing in the upper diagram of "Antenna temperature"—i.e., the microwave noise temperature of the object towards which the receiving horn was pointing at each azimuth. One sees the relatively low noise temperature of the atmosphere (125 K at 75° from the vertical), and the relatively high noise temperatures of the buildings—with the exception of that housing MIT's Van de Graaff accelerator (F), whose aluminum dome reflects the low sky noise into the radiometer.[77]

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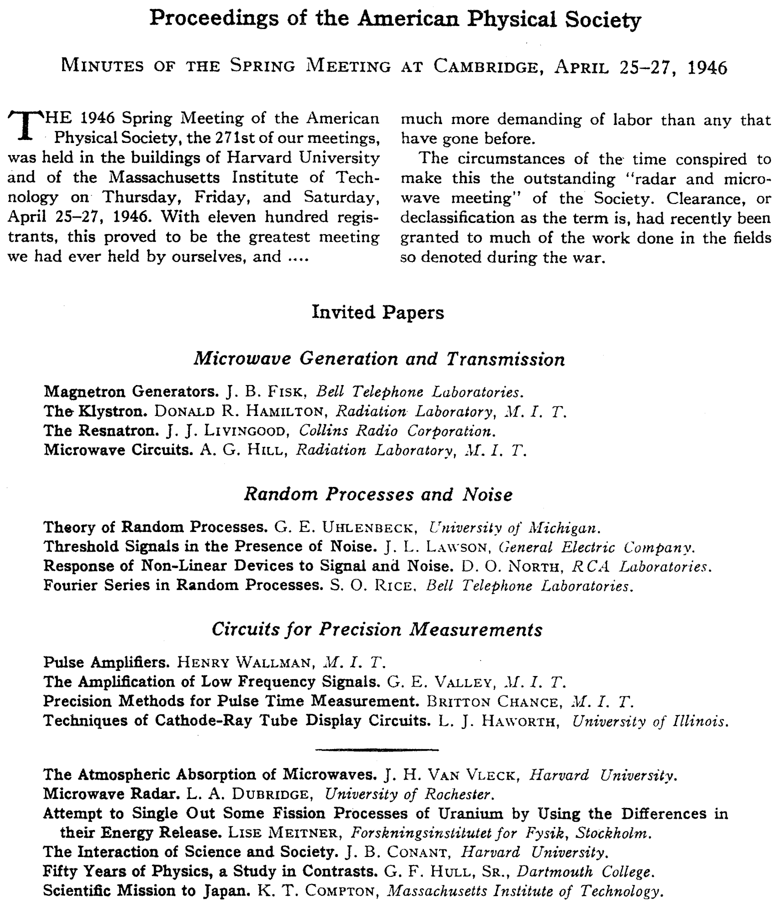
[77] Lawson and Uhlenbeck (1950), who give very few references, do not credit Dicke with these measurements or the instrument used to make them, let alone the concepts lying behind it.



Proceedings of the American Physical Society

Minutes of the Spring Meeting at Cambridge, April 25–27, 1946

THE 1946 Spring Meeting of the American Physical Society, the 271st of our meetings, was held in the buildings of Harvard University and of the Massachusetts Institute of Technology on Thursday, Friday, and Saturday, April 25–27, 1946. With eleven hundred registrants, this proved to be the greatest meeting we had ever held by ourselves, and .... much more demanding of labor than any that have gone before.

The circumstances of the time conspired to make this the outstanding "radar and microwave meeting" of the Society. Clearance, or declassification as the term is, had recently been granted to much of the work done in the fields so denoted during the war.

Invited Papers

Microwave Generation and Transmission

Magnetron Generators. J. B. Fisk, *Bell Telephone Laboratories.*
The Klystron. Donald R. Hamilton, *Radiation Laboratory, M. I. T.*
The Resnatron. J. J. Livingood, *Collins Radio Corporation.*
Microwave Circuits. A. G. Hill, *Radiation Laboratory, M. I. T.*

Random Processes and Noise

Theory of Random Processes. G. E. Uhlenbeck, *University of Michigan.*
Threshold Signals in the Presence of Noise. J. L. Lawson, *General Electric Company.*
Response of Non-Linear Devices to Signal and Noise. D. O. North, *RCA Laboratories.*
Fourier Series in Random Processes. S. O. Rice, *Bell Telephone Laboratories.*

Circuits for Precision Measurements

Pulse Amplifiers. Henry Wallman, *M. I. T.*
The Amplification of Low Frequency Signals. G. E. Valley, *M. I. T.*
Precision Methods for Pulse Time Measurement. Britton Chance, *M. I. T.*
Techniques of Cathode-Ray Tube Display Circuits. L. J. Haworth, *University of Illinois.*

The Atmospheric Absorption of Microwaves. J. H. Van Vleck, *Harvard University.*
Microwave Radar. L. A. DuBridge, *University of Rochester.*
Attempt to Single Out Some Fission Processes of Uranium by Using the Differences in their Energy Release. Lise Meitner, *Forskningsinstitutet for Fysik, Stockholm.*
The Interaction of Science and Society. J. B. Conant, *Harvard University.*
Fifty Years of Physics, a Study in Contrasts. G. F. Hull, Sr., *Dartmouth College.*
Scientific Mission to Japan. K. T. Compton, *Massachusetts Institute of Technology.*

FIG. 22. What about radar seemed important to physicists turning to research. Collage from Darrow (1946).

With Dicke's microwave radiometer as exemplar, lock-in detection was quickly and widely adopted in postwar physical experimentation, becoming standard operating procedure perhaps first in nuclear magnetic resonance, but soon followed by microwave spectroscopy.[78] This is illustrated clearly in the nuclear magnetic resonance apparatus of Purcell and collaborators (Fig. 18). The authors there state (Bloembergen, Purcell, and Pound, 1948, p. 687) that their lock-in amplifier was "copied after one designed by R. H. Dicke." Similarly, Herbert Gutowsky (1975, p. 284) recalls of his own NMR experiments just at this time over in Harvard's Chemistry Department, "On February 21, of 1948, [George] Pake and I made our first low-temperature run. . .with an rf bridge system, using 30-cycle modulation of the magnetic field with a lock-in detector"—i.e., just what Purcell and Pound were using in the Physics Department. In short, these principles came in the postwar period to be quickly and widely accepted as essential to the design of experiments and to drawing conclusions from them.

In line, however, with the method of this survey—viz., proceeding with an eye to prewar-postwar continuity—it must immediately be said that, although they had not been integrated and set out as a foundation for physical experimentation, all of these concepts had been developed, and to some degree had also been used, piecewise, in sectors of physics (Moullin, 1938) and electronic engineering (Cherry, 1951; Bell, 1953; Okwit, 1984) over the previous quarter century. The most important locus for such studies was, unquestionably, Bell Telephone Laboratories, where, through the 1920s, Carson, Hartley, Johnson, Llewellyn, and Nyquist interacted fruitfully, de-

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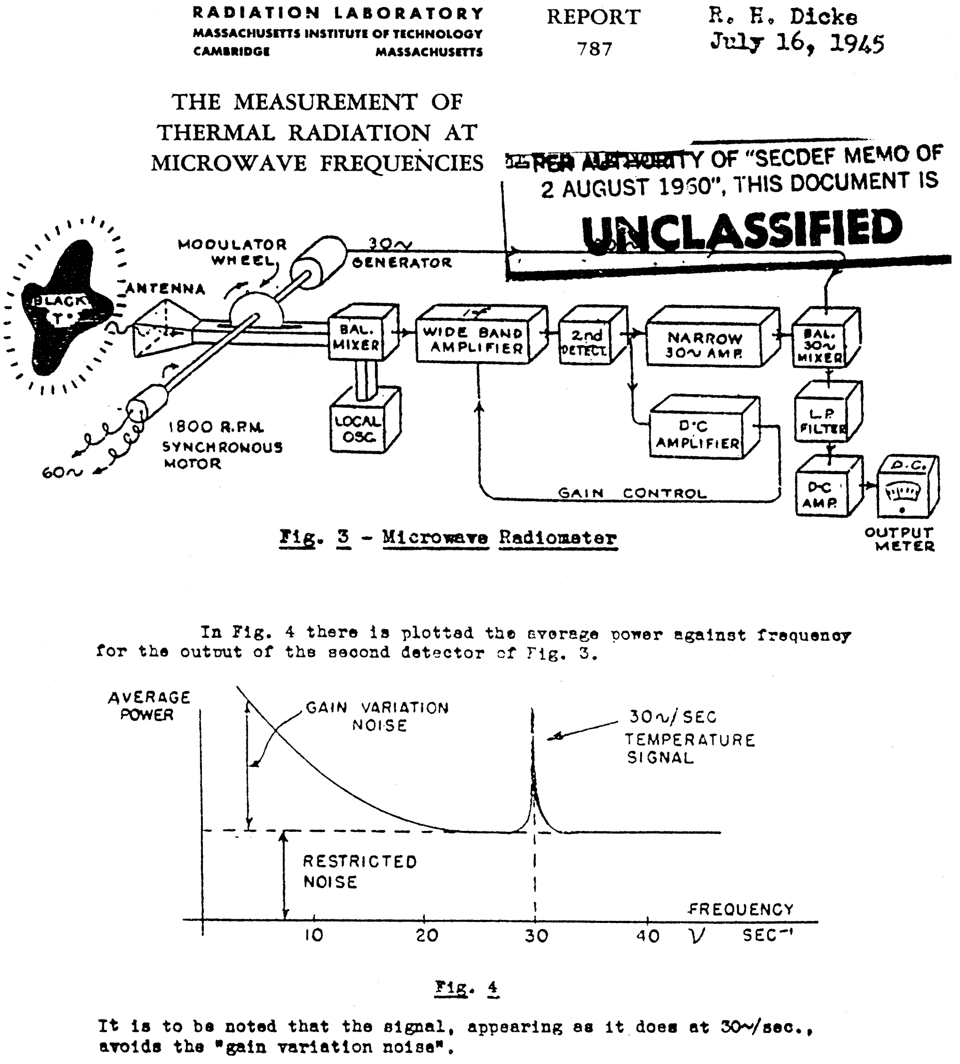
[78] That lock-in detection was standard operating procedure in both these fields is evident in Fretter's (1954, pp. 249–250, 270–271) treatment. Pound (1993b) indicates his role in introducing the Harvard microwave spectroscopists (i.e., E. B. Wilson's group in the Chemistry Department) to lock-in detection. Beringer and Castle (1950, Fig. 2) give a well-illustrated example of the incorporation of lock-in detection in a microwave-spectroscopic apparatus. When Theodore Forrester raised "the possibility of observing beat frequencies between lines in the visible spectrum" (Forrester, Parkins, and Gerjouy, 1947), it was Dicke's (1945) technique that made experiment in this direction a possibility: "The detection of an effect so heavily overwhelmed by noise demands that a modulation be imposed on the signal in such a way that the noise remains unmodulated. In a technique which is now common [Dicke cited] the modulated signal, after detection, is passed through a very narrow band amplifier, preferably phase selective, before registering on the indicator" (Forrester, Gudmundsen, and Johnson, 1955, p. 1693).

veloping the concepts of information content and its logarithmic measure, of frequency bandwidth, signaling speed, noise power, spontaneous voltage fluctuations, noise temperature, and antenna temperature (Burgess, 1941; Gabor, 1946; Gilbert *et al.*, 1984).

Further, the lock-in amplifier/detector had a brief but important prewar history. The first detection instrument based upon synchronism between signal and reference voltage was, apparently, that devised by C. R. Cosens (1934) for, and manufactured by, the Cambridge Instrument Co., Ltd., 1932–1934. This was a balance detector for alternating-current bridges, which was sensitive only to the ac frequency applied to the impedances to be balanced. During the later 1930s some geophysical and astrophysical experimenters incorporated the principle of lock-in detection into apparatus to observe weak signals against an irremediably noisy background, e.g., observation of the sun's corona in daylight (Skellett, 1940; Hufbauer, 1994) and measurement of light scattered by the upper atmosphere from a searchlight beam (Johnson, Mock, and Hopkins, 1939; Johnson *et al.*, 1939). The refinement of the lock-in amplifier as a frequency-sensitive and phase-sensitive detector was taken on especially by Walter C. Michels and his students at Bryn Mawr College for women (Michels and Curtis, 1941; Michels and Redding, 1948).

These early developments await their historian. I shall not attempt to explore them further at this late stage in a survey of postwar applications of radar. Nonetheless, having begun this survey with Gustaf Ising's (1924) ingenious concept for the acceleration of ions, I want now in approaching its conclusion to draw attention to his insightful anticipation of a significant piece of this complex of concepts.

When last we were with Ising in the Physical Institute of the University of Stockholm he was attempting, neither confidently nor successfully, to demonstrate experimentally his idea of repeated acceleration of a bunch of charged particles. At the same time, however, Ising (1932) was confidently and tenaciously engaged in demonstrating the limit set by Brownian motion to the sensitivity of galvanometers, a phenomenon potentially



RADIATION LABORATORY
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
CAMBRIDGE MASSACHUSETTS

REPORT
787

R. H. Dicke
July 16, 1945

THE MEASUREMENT OF
THERMAL RADIATION AT
MICROWAVE FREQUENCIES

PER AUTHORITY OF "SECDEF MEMO OF 2 AUGUST 1960", THIS DOCUMENT IS UNCLASSIFIED

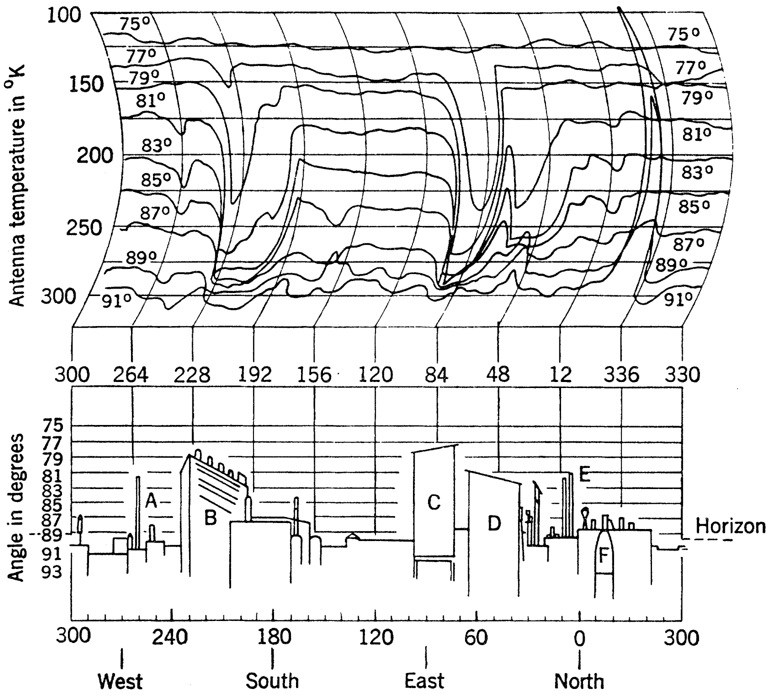
Fig. 3 - Microwave Radiometer

In Fig. 4 there is plotted the average power against frequency for the output of the second detector of Fig. 3.

Fig. 4

It is to be noted that the signal, appearing as it does at 30∿/sec., avoids the "gain variation noise".

FIG. 23. Microwave radiometer with lock-in detection. Collage from Dicke (1945).



Optical and microwave radiometer panorama of the Cambridge, Mass., skyline from a point on Building 20, MIT. The labels on the curves of the upper diagram are the antenna angles measured from the vertical. Similarly, the angles indicated on the left side of the lower figure are measured from the vertical.

(A) Chimney of MIT powerhouse
(B) Radiation Laboratory, Building 24
(C) and (D) Sheds on Building 20
(E) Lever Brothers Company chimney
(F) Electrostatic accelerator, MIT

FIG. 24. The surroundings of the MIT Rad Lab as "seen" in microwave noise. Reproduced from Lawson and Uhlenbeck (1950, p. 107).

important in geophysical measurements, his longstanding interest. Circa 1920 he had begin to publish, in Swedish, theoretical studies along these lines (Beckman, 1960). But it was the publication in the *Philosophical Magazine* in 1925 of a paper by two distinguished Dutch experimentalists reporting small ceaseless fluctuations of the zero position of an ultrasensitive galvanometer, which they attributed to microseismic perturbations, that moved Ising (1926) to take up the question energetically. Citing and recapitulating his earlier papers, Ising reconstrued the Dutch observations as being, rather, of "nothing else than the *Brownian fluctuations* of a suspended system, theoretically announced by M. v. Smoluchowski" in 1912. And then he set about proving this by experiments of his own devising (Ising, 1932; Moullin, 1938, pp. 215–217).

While fluctuations—whether of electric currents or of galvanometer null positions—and their connections with statistical mechanics were familiar to physicists before the war, these phenomena and their theoretical interpretation were not generally seen to be connected with the studies by communication engineers on the problem of evaluating the information content of a signal in relation to the diverse sources of noise in the communications system, i.e., in the transmitting channel and receiving apparatus. Moreover, the physicists, tended to regard such statistical-mechanical considerations as questions of fundamental principle rather than practical matters of experimental technique. From quite a different direction, however, physicists, especially the atomic and nuclear physicists, had already been dealing in their daily practice with the problem of distinguishing signal from noise. As particle counters, they had in these same interwar years begun to employ statistics to develop the relations between the number of discrete events recorded and the certainty with which they could be distinguished from random events, i.e., could be construed as a signal rather than noise.[79]

Indeed it was from this angle that physicists made contact with, and made sense for themselves of, the earlier studies in the field of electrical communications. Dicke (1945) did—he had come to the Rad Lab fresh from a "particle-counting" dissertation at the University of Rochester. So also did Zoltan Bay (1947a), who is best known among physicists for his early adaptation of the electron multiplier to particle counting (Bay, 1941; Wagner, 1985). Bay used just such considerations from the counting of discrete events to design an experimental protocol for detection of radar reflections from the moon (Fig. 9). In order to bring the signal above the noise lev-

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[79] Elementary discussions are given by Rutherford, Chadwick, and Ellis (1930, pp. 167–173) and Rasetti (1936, pp. 34–35), who is explicit on the question of "the relative accuracy attained." Advanced discussion by Peierls (1935).

el, Bay and collaborators pulsed their apparatus at three-second intervals and integrated the receiver output for thirty minutes by switching it, after each pulse, sequentially to ten coulometers (volta-meters, electrolytic cells), corresponding to ten "range bins."[80]

If, then, we distinguish, on the one hand, that direction of physical inquiry represented by Ising's studies of Brownian motion of a sensitive galvanometer (i.e., pushing the sensitivity up to the point where measurement is defeated by statistical fluctuations), and on the other hand, that represented by the nuclear physicists' particle-counting experiments (i.e., using statistics to effect a measurement, fluctuations notwithstanding), we may say that it was the melding, in the context of designing radar systems, of these two, theretofore largely distinct approaches to the problem of measurement that created a much wider appreciation among experimental physicists of the importance of such considerations in the design of their own experiments and instruments.

At war's end Dicke accepted a faculty position at Princeton and took there with him one of the three microwave radiometers constructed at the Rad Lab (Dicke, 1983). He imagined himself turning this device upon the heavens above that university, but he found no support there for such radio-astronomic interests. Dicke and Beringer (1946) consequently reported their "quite incomplete" wartime microwave-radiometric observations of the sun and moon, "since it seems unlikely that the authors will be able to continue this work," and each then turned his interests in another direction. Touring the U.S. two years later (1948), Joseph Pawsey, radio astronomer with Australia's CSIR, reported back to E. G. Bowen that, "Meanwhile, the [U.S.] physicists, who at the close of the war had the skill and the inclination to undertake the radio side [of eventual programs of radio-astronomic research], but failed to interest the astronomers then, now have other interests."[81]

Radio astronomers working with meter waves—especially Martin Ryle's group at Cambridge University—were soon employing and extending "Dicke switching" between signal source and noise source (Ryle, 1950, pp. 190–193; Sullivan, 1982, pp. 114–115). It remained, however, for Edward Purcell and his student Harold Ewen, at Harvard University, 1949–1951, to make an astronomical discovery of outstanding importance employing a microwave radiometer. The observation in question was of the radiation emitted in the transition between the two hyperfine energy levels of the hydrogen atom in its ground state—the $\Delta\nu=1420$ MHz that has figured repeatedly in the atomic-beam researches reviewed in this paper. Indeed, the postwar atomic-beam measurements of this quantity by Rabi & Co. (and by Zahcarias & Co.) greatly facilitated this search by locating $\Delta\nu$ within ten kilohertz, rather than, as prewar measurements had, within fifty megahertz.[82]

Ewen and Purcell's (1951) radiometric detection technique involved comparison, using a lock-in detector, of the "noise temperature" of the sky in the direction of the Milky Way, at the frequency $\Delta\nu$, with that at a neighboring (i.e., nonresonant) frequency. Although no details of their experimental methods were ever published—perhaps because Ewen wished to hold them proprietary to the firm he was then forming[83]—Sullivan (1982, pp. 325–327) points out that the line is very much broader than the Harvard physicists had assumed (not ten, but a thousand kilohertz), so that even when stretching their frequency switching interval to 75 kHz, Ewen and Purcell (1951) were detecting only "the tip of the iceberg."

The idea that a search *should* be undertaken for this 21-cm microwave spectral line came from elsewhere: from Jan Oort and his student, H. C. van de Hulst (1945), working through the war in the occupied Netherlands. Oort's group was also the first to initiate the search, with the Würzburg Riese antenna put at his disposal by the Dutch Post Office's Radio Department, and with receivers to which Philips Research Laboratories, Eindhoven, contributed importantly.[84] Success, however, was finally attained by Oort's group only with a radiometer modeled after that of Ewen and Purcell:

> Our first experiments with part of the receiver had clearly shown that the stability of a simple non-switching receiver would be insufficient, and from the literature I had available, it was clear that it would be necessary to use some form of Dicke receiver in which the input was

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[80] Vajda and White (1976) emphasize the importance in the subsequent development of radar astronomy and radio astronomy of Bay's technique of lifting signal above noise through integration.

[81] Quoted by Sullivan (1988, p. 337), who also points out, pp. 313, 315–316, that work paralleling Dicke's on noise temperature of sighted objects was done at the Radiophysics Laboratory in 1944/45 by Ruby Payne-Scott and Pawsey, and that such microwave-radiometric observations formed the only line of "radio-astronomical" investigation included in the Radiophysics Laboratory's 1945 July postwar program paper.

[82] Lang and Gingrich (1979, p. 634) state that "Because Ewen's original detection equipment had a tuning range of only ±200 kHz, the wavelength of the transition needed to be known with a fairly high degree of accuracy."

[83] Ewen Knight Corporation, Needham, Mass. By 1957 Ewen had as employees Frank Drake, Hays Penfield, and Peter Strum, all of whom reported on radiometric methods in the special issue of *Proceedings of the IRE* on radio astronomy (Haddock, 1958). In his sole further publication on the original 21-cm experiment, a popularization, Ewen (1953) gave no technical information.

[84] Muller and Oort (1951). Perhaps of some importance in gaining these resources for Oort's effort was the support of C. J. Bakker, who, already a leading researcher at Philips before the war (in particular with studies of voltage fluctuations in conductors), joined Oort in interesting himself during the war in Jansky and Reber's work and afterwards became one of the most influential figures in Dutch (and European) science.

switched periodically between the antenna signal and the signal from a noise source. A synchronous detector would then measure the difference between the two signals. Then the letter arrived with the proper solution to our problem: the noise-source signal would be replaced by an antenna signal at a different frequency.[85]

IV. CONCLUSION

To conclude this survey we must return to the question posed at the outset: to what extent were the directions of inquiry that physicists pursued after World War II—pursued using the technical means that wartime radar development and deployment had put into their hands—already established in the prewar period as problems of acknowledged importance? This bears on the question of the autonomy of modern physics: Was the science of physics following its own logic (or star, or inertia)? A continuity of research direction and problems from the late 1930s to the late 1940s, with radar providing only the technical means to address previously recognized but inaccessible problems, would certainly be evidence that it was. And indeed in the course of this survey we have found, in almost every case, that the investigation exploiting radar hardware and technique in the postwar period had already been pursued as a direction of inquiry or effort in the prewar period.

But we should not, I think, be overly hasty in drawing the conclusions of "continuity" and "autonomy" from this circumstance. First of all, the bias toward the discovery of connections between the subsequent and the precedent is so strong in the practice of history, that one may almost take the multiplication of such connections to be the measure of the historian's insight.[86] Moreover, as I have stressed at various points above, the specific method of this historical inquiry has been to seek the ramifications of prewar problems in postwar researches. We should keep in mind that, of the cases considered in this review, two of the three directions of research that had in the postwar period the greatest growth and widest spread, and established themselves most successfully as fields of fundamental research—viz., radio astronomy, and microwave spectroscopy of gases—were in the prewar years marginal, poorly established, and little acknowledged within their respective sciences of astronomy and physics. (The third major direction, magnetic resonance in condensed matter, must be regarded as not yet quite extant prewar.) Thus it is clear that important revaluations of the directions of physical research took place in these postwar years, revaluations that this survey has tended to underemphasize, due to its focus on previously recognized problems now found addressable with the hardware and technique developed for radar during the Second World War.

We should also realize that, by looking only at experimental undertakings of the *immediate* postwar period—for only in this period is evidence of a *direct* connection with wartime radar hardware and technique to be found—our survey is inevitably skewed toward continuity: in returning to research at war's end, physicists would, as Rabi (1945) implied, have tended to look, first, back to the problems they had put aside when they pitched into their nation's war effort. A striking instance is Bernard Lovell's failure to recall, on returning to research in 1945, "the glamorous ideas" for radar detection of cosmic-ray showers that had occurred to him in 1939/40 when he was first pulled from his university laboratory into a radar station. Until shaken awake by his chief, Patrick Blackett, Lovell had proceeded on the unquestioned assumption that his task was to take up his cosmic-ray studies just where he had left off in 1939, viz., with his old cloud chamber "now covered in thick dust" (Lovell, 1990, pp. 109–110). A further factor working in the same direction in nearly every academic physicist engaged for years in war work under conditions of restricted autonomy were feelings of exhaustion and of some degree of disgust with the objects and instruments of such work. To such feelings E. M. Purcell was quoted as testifying in our discussion of NMR, above. Thus the return to researches left lying like Cincinnatus' plough was also, for many physicists, a turning away from activities that, however exhilarating early in the war, had gradually become distasteful.

Those effects of the war and of the postwar conditions tending to disconnect postwar research from its prewar roots could be expected to show themselves only weakly within a year or two, but strongly after five or so. This is indeed suggested by the fate of nuclear induction/magnetic resonance, which originated in "pure" physics with acknowledged precedents in prewar research directions, but which was quickly turned around in quite other directions, and very largely in quite other hands. And, as Sullivan has pointed out in his work on the origins of radio astronomy, the largest of the postwar research programs in this field, that of Australia's Radiophysics Laboratory, was by no means large from war's end. The recognition of the "obvious" opportunities created by radar, although it occurred within a few short years following the war, was nonetheless a gradual and unforeseen de-

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[85] Muller (1980). Sullivan (1982, pp. 325–327) points out, however, that aid was rendered reciprocally: van de Hulst, who was spending that year at Harvard, had persuaded Ewen and Purcell to use a much wider switching interval than they had initially assumed necessary. For Australian confirmation of the 21-cm line, see Kerr (1984) and Sullivan (1988, p. 314).

[86] Thus it would have been possible to develop a case for discontinuity between prewar and postwar physics by focusing on the prewar period and identifying active lines of research which atrophied or were abandoned in the postwar period. Such, for example, were the determination of nuclear properties through optical spectroscopy and the frequent involvement of nuclear and particle physicists with radiation therapy—on which see Weart (1979), Heilbron and Seidel (1989, pp. 389–414), and Feffer (1992).

velopment.[87]

Finally, it is important to connect this specific historical inquiry with the broad transformation of physical research in the postwar decades to which reference was made in the first pages of this paper. The diversity and amplitude of the scientific ramifications which our inquiry has found to spring from this one specific technology is itself a sort of demonstration of the circumstance central to most of the historical literature referenced in the opening pages of this paper: the overwhelming of science by its own techniques. Whether this is to be regarded as a development peculiar to the mid-20th century, or, on the contrary, as evidence in support of the contention (Harwit, 1981) that scientific advance, even scientific "revolution," depends primarily upon technical innovations, will require separate consideration. Nonetheless, as the geopolitical confrontation that was the Cold War recedes into history, and the awareness, obscured by that confrontation, of the cultural transformation that is postmodernity takes ever stronger hold, we can now begin to recognize knowledge production in recent decades as overwhelmingly instrumental, both literally and figuratively. For however steadily we may have been moving in this direction over the last half century, it has been only in the last half decade that we have been able to see and to accept this fact.

ACKNOWLEDGMENTS

This survey would not have been undertaken without the stimulus provided by the conference on the history of radar organized by Oskar Blumtritt at the Deutsches Museum, Munich, in December 1992. The conference papers more properly on radar's history have since been published (Blumtritt, Petzoldt, and Aspray, 1994). I am grateful to E. M. Purcell for valuable pointers at an early stage of research for this paper; to Ward Weathers, who, as Smithsonian intern, assisted me at a late stage; and to Louis Brown, David DeVorkin, Kai Handel, Martin Harwit, Lillian Hoddeson, Robert V. Pound, and Federik Nebeker, who provided valuable information and criticism.

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[87] A striking instance of an unanticipated discrepancy in fundamental physical measurements, "coming to light" in consequence of the development and wide deployment of microwave techniques, was the determination, by comparison of Shoran and geodetic measurements, and by measurements with cavity wave meters, that the accepted value of the speed of light—as previously determined by measurements on *light*—was significantly in error, by 15 parts in 300 000 (Essen, 1947; Aslakson, 1949; Petley, 1985, pp. 54–68).

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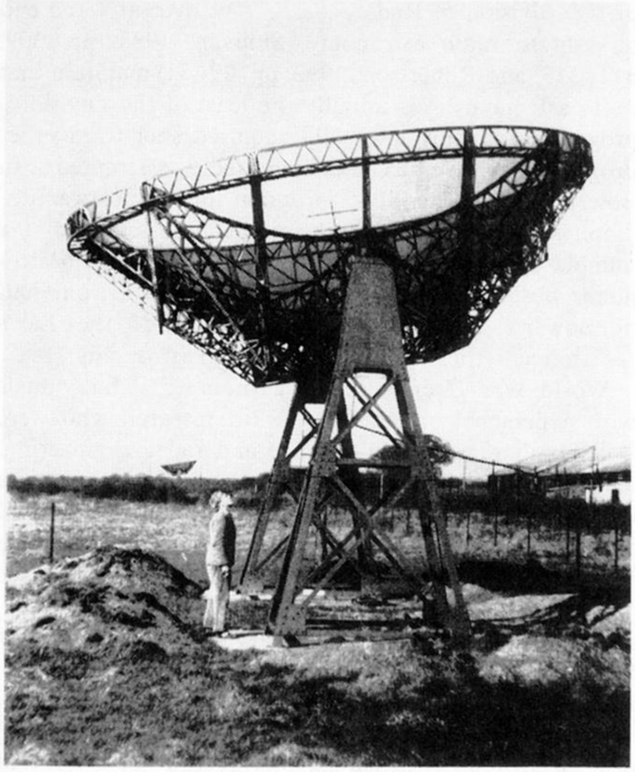


FIG. 10. A pair of antennae from the German Würzburg Riese radar in service as a radioastronomic interferometer at Cambridge University. Reproduced courtesy of F. Graham-Smith.

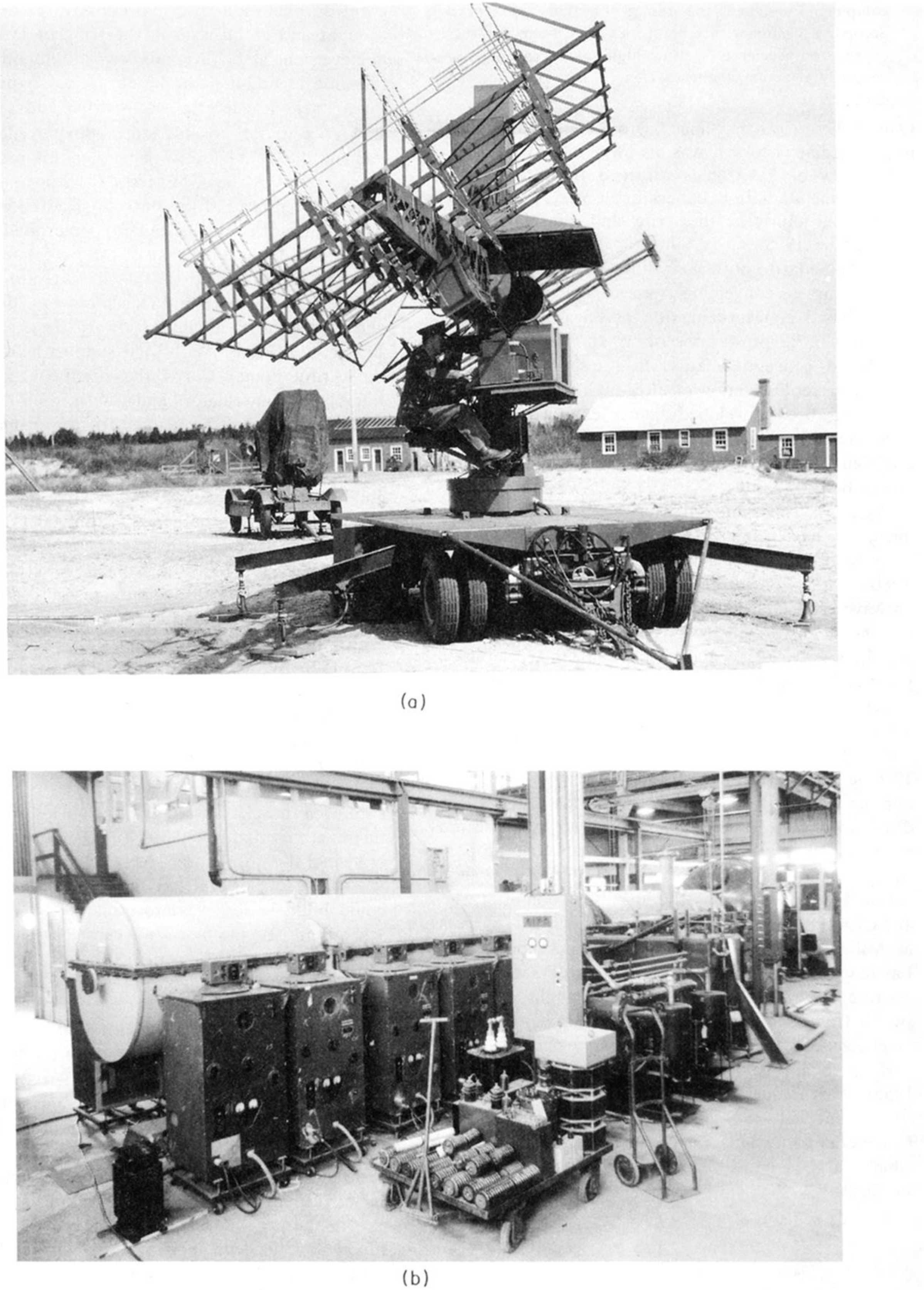
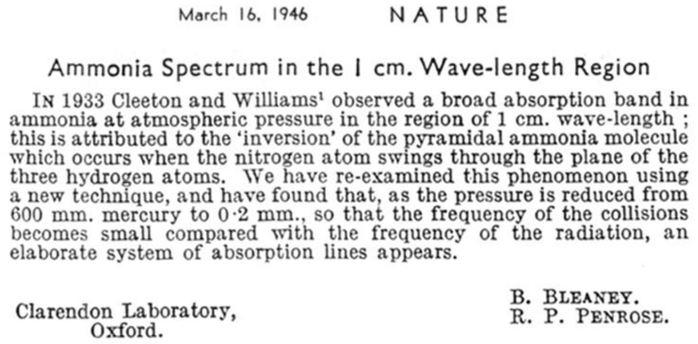


FIG. 11. Radar used to power atom-smasher: Above (a), U.S. Army Signal Corps radar, model SCR-268; below (b), the proton linear accelerator built by Luis Alvarez (with *many* others) at the University of California, Berkeley, 1945–1947, powered by SCR-268 transmitters. Photographs reproduced courtesy of US Army Communications-Electronics Museum, Fort Monmouth, and Lawrence Berkeley Laboratory, University of California.



March 16, 1946 NATURE

Ammonia Spectrum in the 1 cm. Wave-length Region

IN 1933 Cleeton and Williams[1] observed a broad absorption band in ammonia at atmospheric pressure in the region of 1 cm. wave-length; this is attributed to the 'inversion' of the pyramidal ammonia molecule which occurs when the nitrogen atom swings through the plane of the three hydrogen atoms. We have re-examined this phenomenon using a new technique, and have found that, as the pressure is reduced from 600 mm. mercury to 0·2 mm., so that the frequency of the collisions becomes small compared with the frequency of the radiation, an elaborate system of absorption lines appears.

B. BLEANEY.
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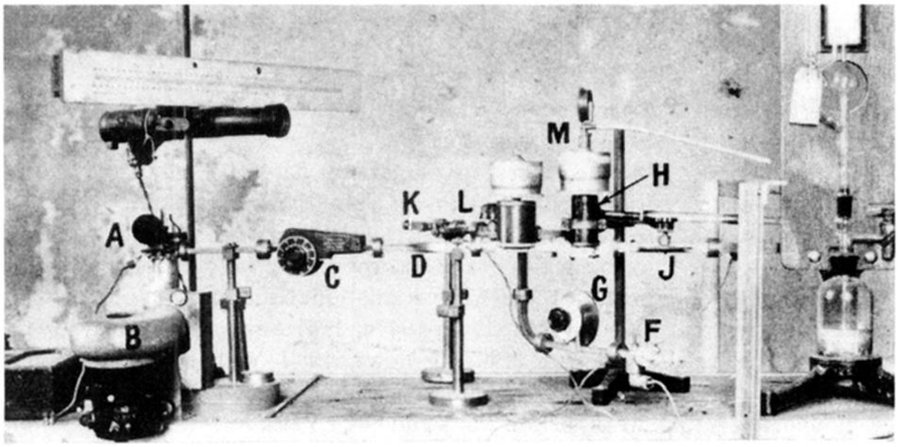
Oxford.



A.	Reflection klystron oscillator
B.	Air blower
C, G, J, L.	Variable attenuators
D.	Waveguide " twist "
E.	Cavity wavemeter
F, K.	Crystal rectifiers
H.	Resonant cavity
M.	Micrometer head

FIG. 14. Microwave absorption spectroscopy of gases. Collage of first report of an investigation of a gas at low pressure (Bleaney and Penrose, 1946) and a photograph of the apparatus employed, as published in the first review of such studies (Bleaney, 1947). Reproduced with permission, Macmillan Magazines Limited (copyright 1946), and Institute of Physics Publishing Limited.

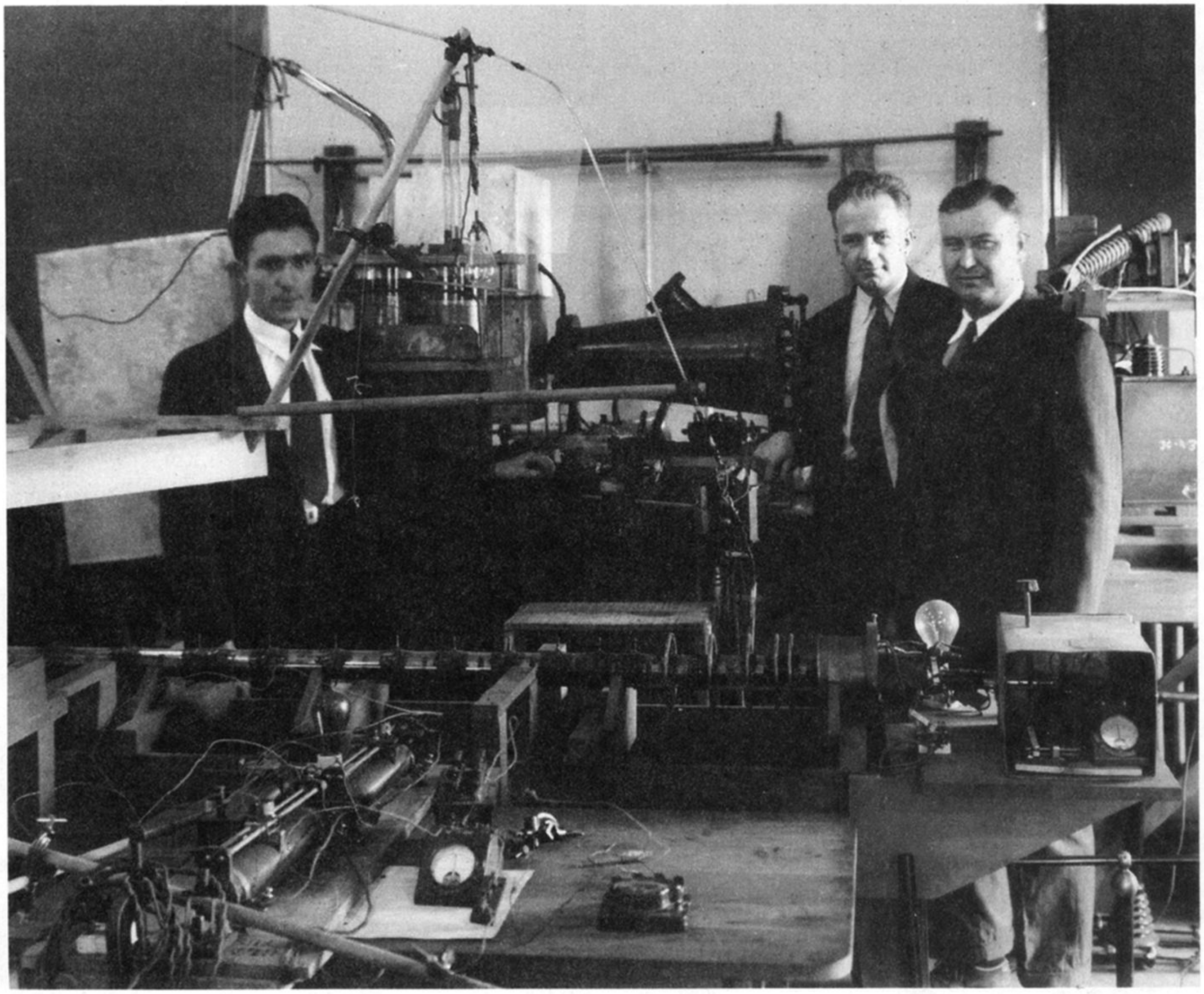


FIG. 3. Jesse Beams, right, and his collaborators at the University of Virginia stand behind their linear accelerator, October 30, 1936. (Science Service photo, Smithsonian Institution).

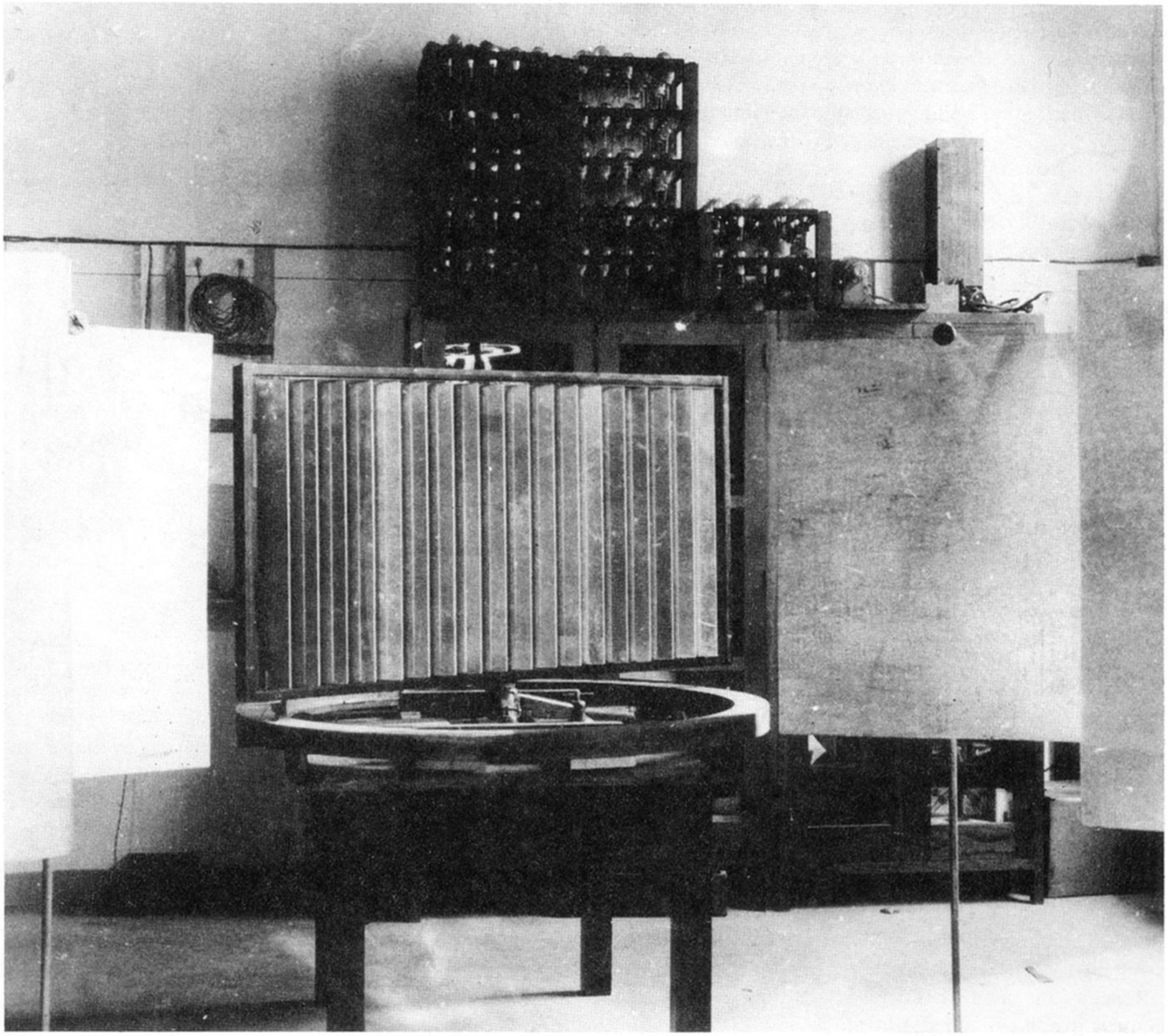


FIG. 7. Cleeton's diffraction grating and magnetron tubes. Photograph reproduced from Cleeton (1934, p. 29).

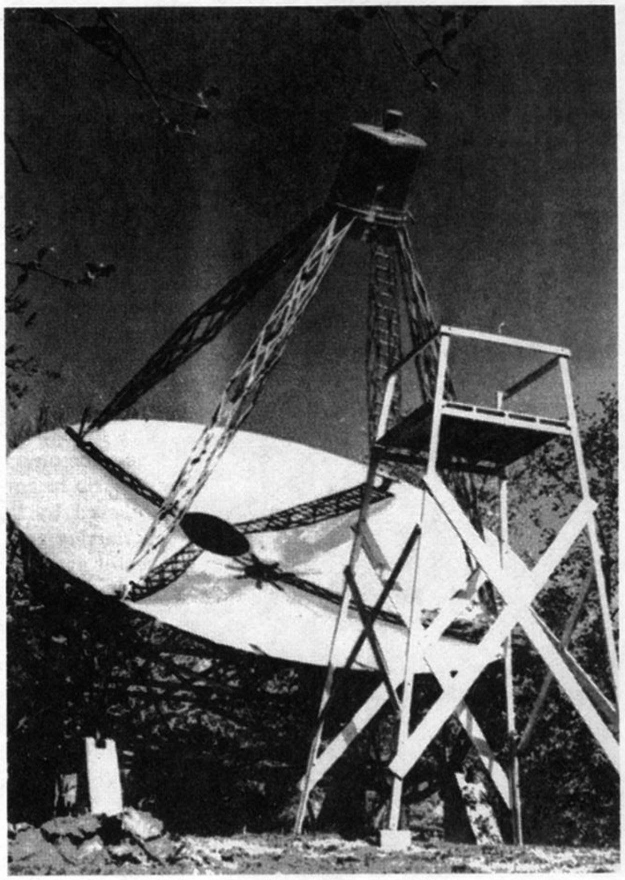


FIG. 8. Grote Reber's radio telescope at his home in Wheaton, Illinois, circa 1939. Reproduced courtesy of G. Reber.