Atomichron®: The Atomic Clock from Concept to Commercial Product

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Invited Paper

The first half of this paper (Sections I–III) gives an overview of the development of atomic clocks from the earliest suggestions that atoms could provide superlative frequency and time standards to the realization of a cesium-beam device (1955) capable of assuming this role in a national standards laboratory. The second half (Sections IV and V) describes in considerably greater detail J. R. Zacharias' program of atomic beam clock development from its inception early in 1953 in MIT's Research Laboratory of Electronics to its striking practical success late in 1956 in the National Company's first commercial model.

INTRODUCTION

In the inaugural session of the International Congress for Chronometry in Lausanne in the summer of 1964 the keynote speaker declared that "we are watching a nearly explosive development of the domains touching upon chronometry":

> Where, previously, it was easy to group under the heading chronometry the astronomical determination of the hour on the one hand, and the art and technology of the clockmaker on the other hand, the prodigious scientific and technical evolution of the past two or three decades has shaken the tradition of chronometry. From the fields of physics and electronic engineering the principles of time determination and time keeping, which had remained nearly unaltered for centuries, have been overthrown [1].

Another speaker in this session contributed an example in point, observing that at the time of the previous congress, in 1959, the hydrogen maser had not yet been invented, yet here today there was a commercial model on display. Those five years had also seen the introduction of the first optically pumped alkali vapor clocks—indeed, by several different manufacturers—and also the first compact, portable atomic beam clocks [2].

Thus by the summer of 1964 the atomic clock had definitely arrived, and not merely as practical instrumentation,

but equally from the point of view of the conceptual bases of timekeeping. The "unanimous desire for an atomic definition of the unit of time" was then already being urged on the International Committee of Weights and Measures, and the formal adoption of the cesium hyperfine transition to define the second was only a matter of time [3].

The purpose of this paper is to tell the story of this scientific/technical development only in small part and only in outline. In particular, the rapid expansion and acceleration of this development in the late 1950s and early 1960s must be left to the book on which I am at work. Through the early history too I will follow a narrow path here, attending neither to masers nor to optically pumped vapor cells, in order to describe in greater detail, yet still all too incompletely, the creation of the first atomic beam clock and the first commercial atomic clock, 1953–1956 [4].

This initative of J. R. Zacharias at MIT's Research Laboratory of Electronics was carried through to a neatly packaged, plug-in-and-run device in collaboration with the nearby National Company, Inc. The story of the Atomichron, as Natco trade-named its cesium-beam atomic frequency standards, is, I surmise, in some respects typical of the process of creation of new technologies out of basic science in the period since the Second World War—anyway, in the vicinity of Cambridge, MA, USA. Certainly Zacharias himself thought so, maintaining at the first public announcement of his undertaking that "Development of the Cesium Atom Frequency Standards is a first-rate example of the conversion of post-war basic science into technology. And it is an example of how the combination of theoretical work, experimental work, and industrial production can yield something of use in many fields which will make further research possible" [5].

I. FROM ASTRONOMY TO ATOMS

Astronomers' Time

The increasing accuracy of clocks and of astronomical observation in Newton's day led John Flamsteed, the first Astronomer Royal at Greenwich, to test the earth as timekeeper against a pair of Thomas Tompion's pendulum clocks. Flamsteed found no evidence that the earth's rotation was less than perfectly uniform, but succeeding gener-
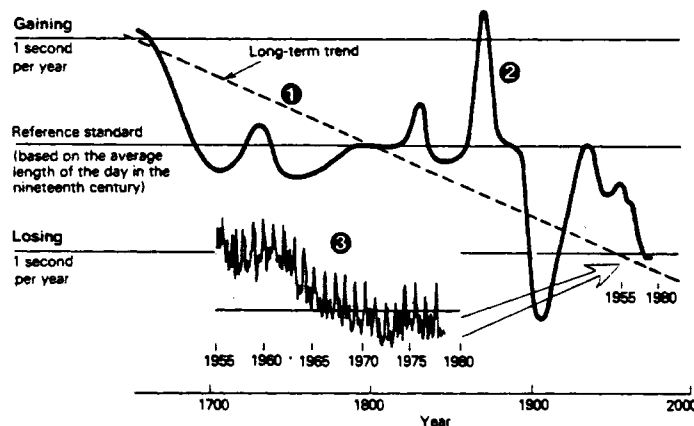
Fig. 1. *The Earth as a clock.* ① Long-term trend. ② Fluctuations. ③ Seasonal variations. (© D. Howse, National Maritime Museum, London.)

ations of critical astronomers have compiled an ever lengthening list of irregularities in the length of the day. By the beginning of this century a secular decline in the earth's rate of rotation, due to tidal friction, was an accepted fact. Stellar observations had established that the earth's axis of rotation, in addition to a regular nutation, executed an irregular "wobble." Lunar and planetary observations indicated, moreover, the existence of short-term accelerations and decelerations of the earth's rotation—indications confirmed in the following decades [6] (see Fig. 1).

Thus astronomy, not horology, had revealed such deficiencies of the earth as a time standard as were known at the beginning of this century. By the mid-1930s, however, the pendulum clock, which had continued to be improved by various electrical appliances and especially by the physical separation of the pendulum from the escapement, was finally at the point of making seasonal irregularities in the earth's rotation perceptible. But just at this moment of horological triumph, the pendulum clock was overtaken by the quartz clock [7].

The possibility of using the piezoelectric properties of quartz to control the oscillations in an electronic circuit was first demonstrated about 1920. By the end of the decade quartz clocks surpassing the stability of the best pendulums had been developed in several laboratories. The seasonal variations in the rate of the earth's rotation, which the pendulum was barely able to detect, were made fully apparent by quartz oscillators in the years before the Second World War [8]. Still, the evidence tended rather to be overlooked by both astronomers and horologists unready for the conceptual revolution which it implied [9].

Even twenty years later, in the mid-1950s, astronomers remained disinclined to cede primacy in time determination to physicists and engineers. The steady improvement of quartz oscillators and the advent of the first atomic frequency standards pushed astronomers to try to reestablish uniformity in astronomical time by shifting from the earth's daily rotation to its annual revolution to define the second. "Emphemeris Time," as it was called was formally adopted in 1960 by the General Conference of Weights and Measures [10].

This redefinition of the second, conservative in intent, was certainly retrograde from the point of view of the ready availability of the defined time. Even with the mean rota-

tion of the earth as measure of time, the official second of time was available only months in arrears, after analysis by the Bureau International de l'Heure of observations from several observatories. To achieve the same precision in the determination of Ephemeris Time, even with the short cut of deriving it from the moon's motion about the earth, required observations over a period thirty times as long—because of the moon's thirty times slower apparent angular motion. The ephemeris second remained the SI unit for just seven years; indeed atomic clocks had already rendered it obsolete at the moment it was adopted [11].

Physicists' Ideal

However stable, beyond all imagination, a quartz oscillator or pendulum may be, it will still fail to meet the most important desiderata for a conceptually satisfying time standard; namely, independent reproducibility anywhere—on earth, at least—and invariance with time and place. These were already the desiderata of the 18th century French scientists who constructed the metric system [12]. The development of the atomic theory in the 19th century, and in particular the conclusion that the atoms of a given chemical element were identical to one another—"as alike as manufactured articles"—led inevitably to the suggestion that not the earth but the atom be taken as the basis for our system of physical units and standards.

The first to come forward with this suggestion was evidently Maxwell, who in discussing units of length and time at the outset of his *Treatise* (1873), observed that:

In the present state of science the most universal standard of length which we could assume would be the wave length in vacuum of a particular kind of light, emitted by some widely diffused substance such as sodium, which has well-defined lines in its spectrum. Such a standard would be independent of any changes in the dimensions of the earth, and should be adopted by those who expect their writings to be more permanent than that body.

The unit of time adopted in all physical researches is one second of mean solar time... A more universal unit of time might be found by taking the periodic time of vibration of the particular kind of light whose wave length is the unit of length.

Kelvin, disregarding Maxwell's sarcasm, seized upon the suggestion enthusiastically:

The recent discoveries due to the Kinetic theory of gases and to Spectrum analysis (especially when it is applied to the light of the heavenly bodies) indicate to us natural standard pieces of matter such as atoms of hydrogen, or sodium, ready made in infinite numbers, all absolutely alike in every physical property. The time of vibration of a sodium particle... is known to be absolutely independent of its position in the universe, and it will probably remain the same so long as the particle itself exists [13].

Although, from a logical point of view, Kelvin was right to give priority to the atomic unit of *time*, from a practical viewpoint Maxwell was wise to give precedence to an atomic standard of *length*. With Michelson's invention of the interferometer (1881) *this* goal was almost immediately accessible

in principle, although technical and political difficulties prevented international agreement on an atomic wavelength standard until well after the Second World War [14].

The atomic standard of time, by contrast, remained an unapproachable ideal for some sixty years. Even today the counting of *optical* oscillations is a tour de force at the very limits of the technically possible [15], while the *microwave* energy differences, on which all past and present atomic frequency standards operate, remained unknown until the 1920s. These atomic energy states in the microvolt range first came to the physicists' attention as hyperfine structure of optical lines [16]. Not until the mid-1930s did physics and electronics begin to explore the microwave region of the electromagnetic spectrum, and thus to give some direction to the notion of an atomic clock [17].

The earliest technique suggested for the physical realization of an atomic frequency standard is also the one which has remained the most accurate and most widely employed to this day: "The Molecular Beam Resonance Method" (see Fig. 2). This was but the most important of a series of improvements made during the decade 1930–1940 by I. I. Rabi and his collaborators at Columbia University upon the molecular beam technique invented ten years earlier by Otto Stern. Stern showed that the magnetic energy states of atoms could be determined by deflection of a beam of such atoms in an inhomogeneous magnetic field. Now Rabi introduced in the path of the beam a radio-frequency field to stimulate the beam atoms to make transitions between magnetic energy states, and thus turned what had been a merely semiquantitative apparatus into a high-precision spectrometer [18].

Rabi foresaw, moreover, the possibility of a reversal of the analytic procedure, whereby the molecular beam resonance apparatus would function not as spectrometer but as frequency standard. Indeed as early as 1940 Rabi pointed to the hyperfine structure of Cs^{133} as the transition of choice
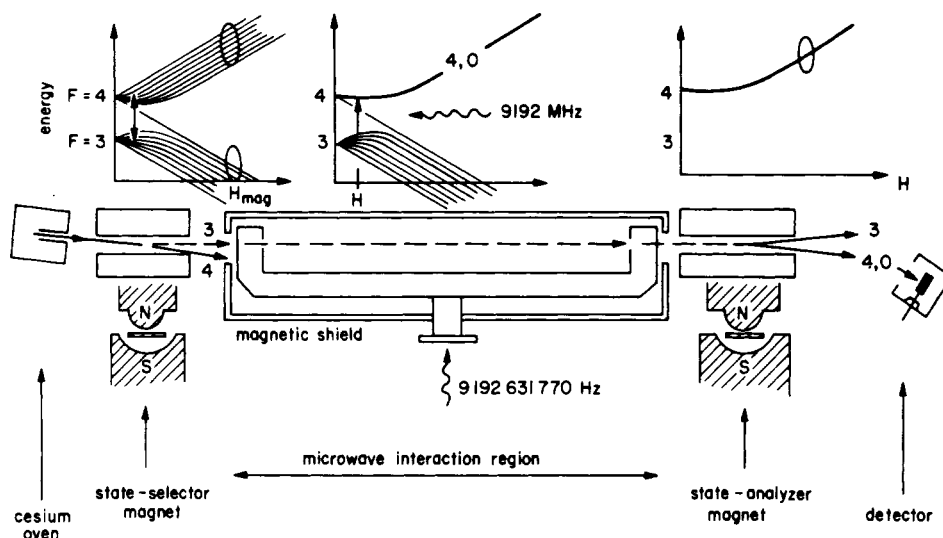

Fig. 2. *Molecular beam magnetic resonance method.* To avoid multiplying illustrations and explanations, we present the method here as it will be applied after 1950 to cesium. The cesium nucleus has an angular momentum of 7/2 (in atomic units); consequently the cesium atom, with its one valence electron, has a total angular momentum, F, of 4 (= 7/2 + 1/2) or 3 (= 7/2 − 1/2), depending upon whether the electron spin is pointing "up" or "down" relative to that of the nucleus. With the angular momenta of the electron and the nucleus are associated magnetic moments of opposite sign whose energies of interaction in the "up" and "down" orientations are represented on the ordinate of the upper left graph, the so-called Breit–Rabi diagram. This is the cesium hyperfine structure separation, 9192 MHz. In an external magnetic field, H_{mag}, these energies will have the various trajectories indicated, depending upon the 9 (= 2 × 4 + 1), respectively 7 (= 2 × 3 + 1), quantum mechanically allowed orientations of F with respect to the magnetic field.

A ribbon of cesium atoms, in states $F = 3$ and 4 in nearly equal numbers, emerges from the oven and enters the state-selector magnet. Those atoms with $F = 3$ lose energy with increasing field, as the Breit–Rabi diagram shows. Hence they will be drawn up the gradient of the magnetic field toward the convex pole, where the field is strongest, and so deflected as to continue through the apparatus. Conversely, nearly all the atoms with $F = 4$ will be deflected downward and rejected.

In the central portion of the apparatus, shielded from ambient magnetic fields, a small, well-defined magnetic field, H, is maintained (as indicated in the central Breit–Rabi diagram) so that the energies of $F = 3$ atoms in their several orientations will also be well defined and separated. The transition of choice, the so-called "clock" transition, is that in which atoms whose total angular momentum is oriented perpendicular to the magnetic field—and whose energies are consequently only weakly influenced by the field—absorb a microwave quantum, flipping the electron spin up, but leaving the orientation of the atom with respect to the magnetic field unaltered ($F = 3, M = 0 \rightarrow F = 4, M = 0$).

In the state-analyzer magnet on the right the $F = 3$ atoms which have made this transition to $F = 4$ are again deflected downward, but now into the detector, while those that have not "flopped" are rejected. (From R. F. C. Vessot, "Lectures on frequency stability and clocks...," in *Experimental Gravitation*, B. Bertotti, Ed. New York: Academic Press, 1974, pp. 111–162 (see pp. 129–131).) *Cf.* Fig. 5.

[19]. Norman Ramsey distinctly recalls discussion in Rabi's group of measuring the gravitational red shift by placing such apparatus on a mountain and in a mine shaft—and joking that he as graduate student would be assigned the subterranean apparatus while his seniors were enjoying the mountain-top view. Ramsey further recalls that at about that time they spoke with National Bureau of Standards staff, proposing the development of an atomic beam clock or magnetometer. (It was in this latter role that the molecular beam resonance apparatus as it then existed could most readily be cast.) [20].

But such a suggestion, though prescient, would have been premature. In 1940 the techniques for the generation and control of ultra-high frequencies, as well as the electronics required for frequency multiplication, division, and synthesis, were still in a very primitive state. *Real* prescience would have been required to foresee then that in just six years, wartime radar development would make microwave electronics a familiar technology, documented in detail in a 3-ft shelf of books [21].

II. The First Atomic Clock

Even Rabi could not have foreseen that in 1945 he and his crew would stand at the center of a technical development which was the basis of a radar industry larger than the prewar automobile industry [22]. If in those hectic years potential applications of the molecular beam resonance method were far from Rabi's mind, they were effectively recalled late in 1944 by the announcement of his Nobel Prize. Rabi made use of the notoriety gained in order to give publicity to the idea of an atomic clock [23] (see Fig. 3). And although I have not found other proposals for atomic clocks in print, they must have been frequently discussed. Certainly that is strongly suggested by Charles Townes' reference just a few months later to "the atomic clock which has been so often postulated in theoretical physics" [24].

Microwave Spectroscopy and Stabilization

The determination of microwave energy levels of atoms and molecules—by measuring the absorption frequencies of a gas-filled waveguide—was a burgeoning field which owed its growth, indeed its existence, to the wartime development of microwave hardware and technique. Microwave spectroscopy offered, moreover, a much simpler realization of the atomic clock than did the notoriously difficult atomic beam technique, which had been practiced in only two or three laboratories in the United States before the war. In fact, the molecular beam resonance method had never actually been pushed beyond some hundreds of megahertz [25], where there were few spectral lines and the traditional methods of frequency control still worked relatively well. The microwave spectroscopists, by contrast, were immediately at work in the region above a gigahertz, where the transitions were plentiful and, equally significant, the problems of frequency stabilization were severe [26].

Thus this new field of physical research quickly coupled in the minds and laboratories of several physicists with the very practical problem of calibration and stabilization of microwave oscillators. Townes at Bell Labs may have been the first to suggest this application; or it may have been R. V. Pound at the MIT Rad Lab. The first to construct

'COSMIC PENDULUM' FOR CLOCK PLANNED

Radio Frequencies in Hearts of Atoms Would Be Used in Most Accurate of Timepieces

DESIGN TERMED FEASIBLE

Prof. I. I. Rabi, 1944 Nobel Prize Winner, Tells of Newest Developments

By WILLIAM L. LAURENCE

Blueprints for the most accurate clock in the universe, tuning in on radio frequencies in the hearts of atoms and thus beating in harmony with the "cosmic pendulum," were outlined yesterday at the annual New York meeting of the American Physical Society, at Columbia University, by Prof. I. I. Rabi, who delivered the Richtmyer Memorial Lecture under the auspices of the American Association of Physics Teachers.

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Fig. 3. *Earliest publicity* (see note [23]).

apparatus effecting this application were: William V. Smith, who at the end of the war moved from the Rad Lab to Duke University to participate in a new Air Force financed program in microwave spectroscopy; Ewan W. Fletcher, who had moved from the Rad Lab to Harvard's Cruft Laboratory in a microwave research program under an ONR contract; W. D. Hershberger at RCA's Princeton Research Laboratory, where he had worked through the war; and Townes, possibly the first of all, at Bell Labs. All four stabilized klystrons in 1947/1948 with the 24-GHz inversion transition of the ammonia molecule, then still the only known line strong enough for the purpose [28], [29].

This work sufficed to demonstrate, and patent, the principle of frequency control of a microwave oscillator by feeding back the phase-discriminated output from a gas-filled waveguide [30]. Thus in a loose sense, the Duke, Harvard, RCA, and Bell Labs apparatuses were atomic clocks. In a more rigorous sense, however, a stabilized microwave oscillator becomes a clock only when its awkwardly high frequency is divided by the factor of a thousand required to bring it within the range of established frequency standards, in terms of which it is then calibrated, and, conversely, which it then serves to control and correct. Or, more rigorously still, a frequency standard becomes a clock

only when its highly stable and well-calibrated oscillations are actually counted, thus establishing a measure of time. The considerable effort involved in these further steps is not likely to be undertaken without a good, practical reason.

NBS and Microwaves

In the summer of 1942 the Joint Chiefs of Staff established within the National Bureau of Standards an Inter-service Radio Propagation Laboratory. Reconstituted in May 1946 as the Central Radio Propagation Laboratory, it was to encompass all radio work within the Bureau. While administratively the CRPL was just the 14th Division of the Bureau, its program was determined by a Radio Propagation Executive Committee on which sat representatives of the military services and of the several civilian agencies interested in radio [31].

Most of the work of Division 14 dealt indeed with radio propagation. One section, however, under W. D. George, was responsible for radio standards, and another, under Harold Lyons, for microwave standards. The importance of the CRPL within the Bureau of Standards, and the standards work within the CRPL, was strongly emphasized in the Bureau's April 1947 report to the President's Scientific Research Board (Steelman Board). "Radio propagation" appeared there as the first of the "subjects on which the Bureau feels an expansion of its present activities is most urgently needed."

> The impetus given to the use of new bands of the radio frequency spectrum, extending to over 30,000 megacycles, has given rise to the need for new frequency standards and calibration service. So far, only 14 of some 150 required standards are available. The American people, industry and Government will spend billions of dollars in the next few years for radio equipment. Primary standards of measurement and an adequate understanding of propagation phenomena will save from this vast sum far more than the few millions a year they will cost, and if war comes again, may mean the difference between obliteration and survival as a nation [32].

Chief among the standards already achieved by Lyons and George during the last year of the war was an apparatus providing frequencies in the range of 300 to 30 000 MHz with an absolute accuracy of one part in 10^8. In three relay racks of electronics the 100-kHz output of the "National primary standard of frequency"—a bank of nine quartz crystal oscillators—was multiplied up by a series of tube circuits and silicon crystal rectifiers to give 900 fixed frequencies with continuous interpolation between them [33].

The Ammonia Clock

The Bureau of Standards had the motive and the equipment to make an atomic clock—if only it had a microwave oscillator stabilized on the ammonia line. In the spring of 1947 Lyons set to work in this direction; in August 1948 he had a working clock. Though shown privately in the following months, its public debut was delayed until January 1949 (see Fig. 4). In the interim its operation was improved, both in reliability and stability, and it was given the packaging that made it look and sound so much like an atomic clock [34]. The fourth estate was impressed, to a
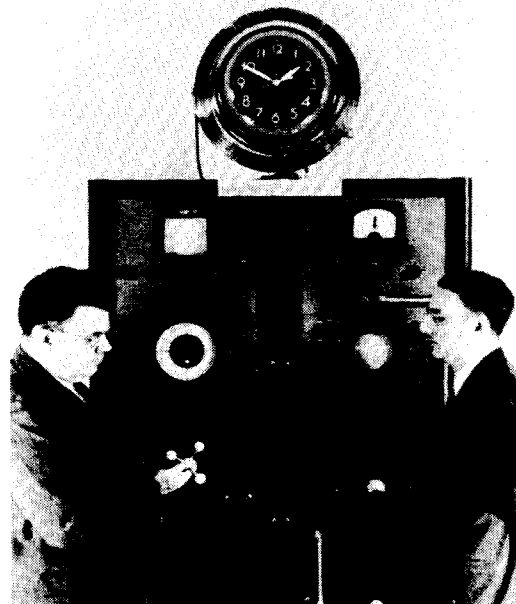

Fig. 4. E. U. Condon, H. Lyons, and the first atomic clock, Jan. 1949. Condon holds in his hands a model of the NH_3 molecule. Thirty feet of ammonia-filled, gold plated K-band waveguide, forming the absorption cell, are coiled around the electric clock at top; the absorption dip appears on the oscilloscope in front of Lyons. ("The atomic clock," NBS Tech. Rep. 1320, 1949; reprinted in J. Horological Inst. America, pp. 11–20, Feb. 1949, and pp. 7–14, Mar. 1949.)

degree unequaled by any other clock. Time, Newsweek, Business Week, numerous newspapers, Edward R. Murrow, and Voice of America—all were enchanted with this apparatus that took the pulse of the atom. Only the sophisticated New Yorker magazine found it a bit too much [35].

Stabilization on a molecular absorption line was certainly the simplest, most direct way to construct an atomic clock. Unfortunately, the method was seriously limited in the performance that could be attained [36]. The ammonia molecular inversion line, the only known transition in the centimeter range strong enough to serve the purpose, was intrinsically quite sufficiently narrow—though not so narrow as intra-atomic transitions based on magnetic, rather than electric, interactions. In practice, however, the line was severely broadened by collisions and thermal motion. Moreover, because the two states of the molecule are so close in energy, they are nearly equally populated under conditions of thermal equilibrium. Consequently, to achieve an adequate signal it was necessary to employ long absorption cells containing gas at relatively high pressure and microwaves at relatively high flux. For this poor sensitivity a price was paid in both stability and accuracy. By 1952, Lyons' group could claim stabilities of one or two parts in 10^8—still not significantly higher than that of the earth's rotation [37]. Strenuous efforts by Shimoda at the University of Tokyo brought the stability and accuracy of the ammonia absorption clock up another order of magnitude, but generally this direction did not seem sufficiently promising to be pursued further [38].

III. BACK TO ATOMIC BEAMS

It was clear from the outset that the atomic beam resonance method could obviate most of the deficiencies of the ammonia absorption clock: the atomic hyperfine transitions

were intrinsically very narrow, and collision broadening was absent, as was doppler broadening in directions transverse to the beam. Moreover, the sensitivity of the technique was enormously higher because of the initial spatial separation (by magnetic gradient) of the atoms in close energy states, and the subsequent conversion of a single microwave quantum into motion of a massive ion. The technique of atomic beams, esoteric and difficult before the War, had become more familiar in these following few years with the establishment of laboratories at MIT, Brookhaven, Harvard, Berkeley, and the Canadian National Research Council, in addition to the pre-war labs at Columbia and Carnegie Institute of Technology (see Fig. 5).
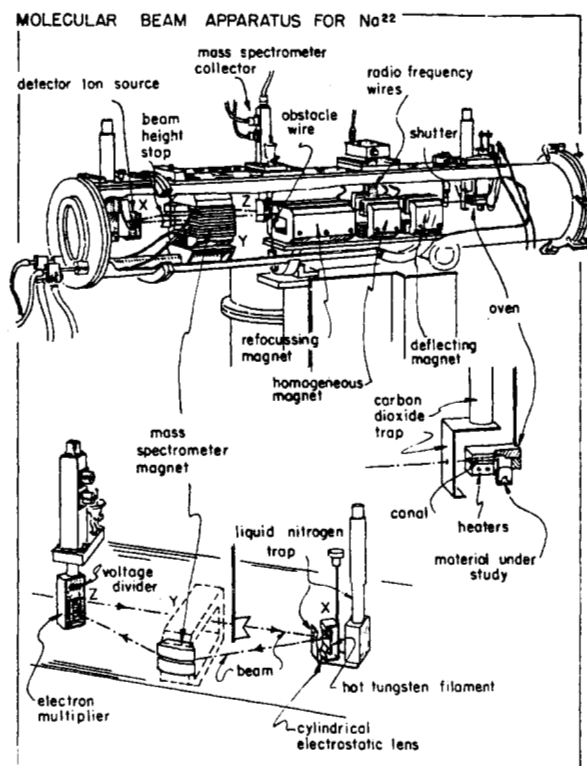

Fig. 5. *Typical postwar atomic beam apparatus.* MIT Molecular Beams Laboratory, 1947. This is a modification of an apparatus built in 1946/1947 for experiments on hydrogen. (MIT, RLE, Quarterly Progr. Rep., p. 18, Jan. 15, 1948.) *Cf.* Fig. 10.

Separated Oscillatory Fields

In those years following the war the ABRM was improved by many technical devices, but most significantly by a new principle of wide application: Norman Ramsay's method of separated oscillatory fields [39]. Ramsey devised the method while seeking to circumvent the problem, then becoming acute, of maintaining the uniformity of the magnetic field in the transition region of an atomic-beam apparatus as that region was being lengthened in order to increase the narrowness of the magnetic resonances. In classical terms, the frequency resolution of the apparatus is the reciprocal of the time a vibrating atom spends within it. In quantum mechanical terms, the reciprocal of the transit time through the transition region gives, via the Heisenberg uncertainty principle, the spread in energy of the atomic transition as observed in that apparatus. Since there was little effective

control over the average velocity of the atoms in the atomic beam, as this was largely dictated by intrinsic parameters such as melting point and by considerations of beam intensity, increased transit time meant longer transition regions. But to realize the potential of a lengthened apparatus required a corresponding increase in the uniformity of the magnetic field in the transition region. Thus the necessity of maintaining ever more uniform fields over ever longer regions was making serious problems for the molecular beam spectroscopist.

It was in March 1948 that Ramsey, while teaching a course on physical optics, was led to consider whether there might be some analog in the atomic beam apparatus to the principle of the Michelson stellar interferometer [40]. The resolving power of such an interferometer is twice as great as a telescope of equal diameter, even though—or rather because—the interferometer has thrown away all the light collected by the telescope except in two narrow, diametrically opposed segments [41]. If the telescope lens is less than perfect, its performance is still further degraded relative to that of the interferometer (see Fig. 6), which depends on the quality of the lens only in those two narrow segments.
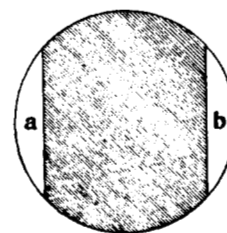

Fig. 6. *Telescope into interferometer.* (A. A. Michelson, *Light Waves and Their Uses.* Chicago, IL, Univ. of Chicago Press, 1903, p. 33.)

Ramsey discovered that the application of the transition stimulating oscillating field could be limited, as with the interferometer, to the two ends of the transition region, and indeed with a significant gain in resolution. Better still, the uniformity of the magnetic field in the intervening space was no longer material; its average value determined the frequency of the magnetic resonance, but its point-by-point variations had no effect upon the width of the resonance. Moreover, the new method obviated another problem which though not yet acute would become so as molecular beam spectroscopists followed the available microwave technology down into the centimeter range: how to maintain the stimulating fields in the same phase over the entire length of meter-long transition regions. In Ramsey's method all that was required was a fixed phase relationship between the microwaves in the two, widely separated, but short, oscillatory field regions.

A Cesium Beam at NBS

It is perhaps no coincidence that the initiation of atomic clock programs at both the National Bureau of Standards and the British National Physical Laboratory occurred during the relatively brief tenures of scientifically highly distinguished and personally very forceful Directors: Edward U. Condon and Sir Edward Bullard [42]. Condon, quantum theorist (and war-time microwave theorist) and industrial

research director, breezed into the Bureau late in 1945, urging his staff to "Think big!"—and to his superiors proposing a fivefold increase in the Bureau's budget. Although Condon did manage gradually to double the Congressional appropriation over the next six fiscal years, a great expansion of the Bureau's military programs multiplied its total expenditure much faster. By the time Condon left in the summer of 1951, funds transferred from the Department of Defense made up 80 percent of the Bureau's budget. Included therein was the funding of the Central Radio Propagation Laboratory [43].

It was in the spring of 1948, or possibly earlier, while still putting his first ammonia absorption clock together, that Lyons secured approval for an ambitious program of atomic clock development in his Microwave Standards Section. The program called for the development of additional molecular absorption clocks, including a deuterated ammonia clock, and complementary work in microwave spectroscopy. It also called for the development of an atomic beam clock [44].

Lyons, capable and flexible, but unschooled in these fields, turned for assistance to Rabi and his colleagues at Columbia. There research with atomic beams, under Rabi's lieutenant and succesor, Polykarp Kusch, and microwave spectroscopy, with Charles Townes just moving uptown from Bell Labs, was pursued under an administrative umbrella called Columbia University Radiation Laboratory. (This paper laboratory, created in 1942 following the MIT model, served after Rabi's return to Columbia as a vehicle for joint Army–Navy support of the more or less radar-related research of his Physics Department.) Townes and Kusch went on the NBS payroll as consultants—Townes remarking that the small fees were compensated by the interest of the program [45].

From summer 1948 to summer 1949 Lyons and members of his staff made several visits to Columbia, remaining for periods of up to six weeks to learn techniques. A conceptual design for the Bureau's atomic beam clock was prepared by Kusch late in 1948 and described in a symposium on atomic frequency and time standards (organized by Lyons, Kusch, and Townes) at the spring 1949 meeting of the American Physical Society [46], [47].

The choice of cesium as working substance was foregone. Beams of alkali atoms are by far the most easily and efficiently detected. They had been intensively studied in Rabi's laboratory before the war, and the hyperfine ground-state separations of all the alkali atoms had been carefully measured, either directly, or, if the frequencies were too high, by extrapolation. Cesium, namely, has the widest hyperfine separation among the alkali atoms: 9.2 GHz. Now the stability to be expected from a resonator is proportional to its Q (the ratio of its frequency to the width of its resonance). As the intrinsic width of these hyperfine transitions is negligible, the effective width of the resonance is simply the resolution of the atomic beam apparatus. Thus cesium, with the highest hyperfine resonance frequency promised the highest stability for any given design of the apparatus. Moreover, cesium offered the great technical advantage of occurring naturally in only one isotopic form, Cs^{133}.

Work on the cesium-beam apparatus began at the Bureau in the summer of 1949 with the fabrication of the vacuum chamber and supports. In September, Jesse Sherwood joined

Lyons' staff, bringing experience he had gained assisting Emmanuel Estermann in molecular beam work at Carnegie Institute of Technology. The apparatus was built up in a small hut under a large radio tower—for it was then still believed that a molecular-beam apparatus had to be carefully isolated from all mechanical (and electromagnetic) disturbances. While Sherwood labored over vacuum and beam, Robert McCracken, one of Lyon's most skilled electronics men, constructed the frequency multiplier chain and klystron driver providing the stable, pure, calibrated 9192 MHz to stimulate the atoms [48].

In the summer of 1951 the apparatus was finally all together (see Fig. 7) and preliminary measurements made of
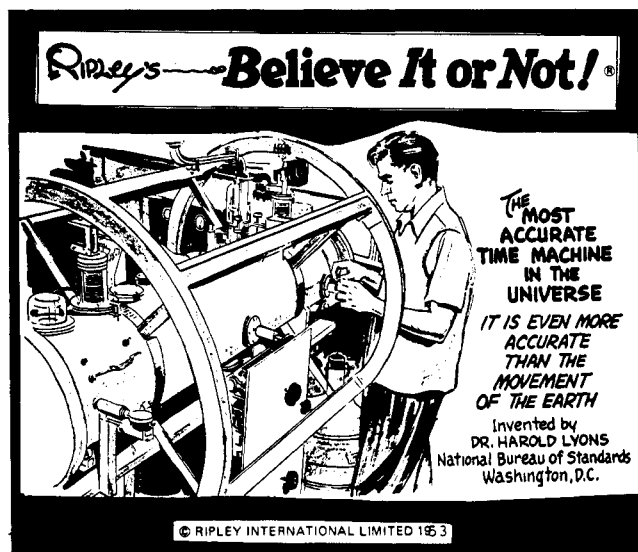

Fig. 7. NBS cesium beam apparatus. The drawing, taken from a 1951 Bureau photo, shows technician J. P. Maloney with NBS-1. Ripley's attention had been drawn by a piece on the accomplishments of Harold Lyons, in the New York Herald Tribune, Mar. 15, 1953.

the cesium hyperfine separation—the first direct measurement of this quantity by atomic beam techniques, and indeed the first application of centimeter waves in this field. Results obtained through January 1952 were reported at the New York meeting of the American Physical Society [49]. These initial measurements were made with but a single, 1-cm-long transition region: the cesium atoms, passing through two holes in a shorted X-band waveguide, were exposed within it to the 9.2-GHz magnetic field. However, the apparatus had been designed to take advantage of Ramsey's technique of separated oscillatory fields. Indeed it was in connection with the design of the NBS apparatus that Kusch had put Ramsey's concept to its first experimental test [50].

In the spring of 1952, a waveguide terminating in a very broad "J" was installed in the apparatus with provision for the beam to pass through slots in the upturned toe and the ankle. With this 50-cm separation—the first application, once again, of the Ramsey technique at microwave frequencies—the width of the resonance was reduced, as expected, by a factor of 100, but the pattern suffered chronically from asymmetries. In December and January they obtained 9192.6318 and ...19 MHz for the hyperfine

splitting, but no publication ensued [51]–perhaps in part because this was an improvement of only one order of magnitude over the 9192.632 they had earlier derived from the much wider Rabi-type resonance. Sherwood left the Bureau early in 1953 and Lyons, rolling up his sleeves, struggled to bring the balky apparatus into operation until, finally, the removal of the CRPL to Boulder in the summer of 1954 enforced an end to his efforts [52].

A Cesium Beam at NPL

Louis Essen had distinguished himself at Britain's National Physical Laboratory through his development of the annular quartz resonator used in observatories throughout the world, and through his development of extremely narrow-band, cavity-stabilized oscillators which he himself used for precision microwave measurements [53]. Alert to the potential of atomic clocks, he followed the work at NBS with keen interest. In June 1950, shortly after E. C. Bullard took over as NPL's Director, Essen had him writing to Condon to request full information about the Bureau's program and, in particular, Kusch's designs for a cesium-beam clock [54]. The personal interest Bullard expressed in the Bureau's atomic clocks, and his intention to begin work in this direction at NPL, were both doubtless genuine. However, the demands upon the Laboratory for defense work, and perhaps also the priority Bullard accorded to the development of electronic digital computers, stood effectively in the way of such an initiative [55].

In February 1953, Essen returned from a tour of American laboratories with renewed enthusiasm for the cesium-beam clock, and confidence that it could be built with the existing staff of the Electricity Division, notwithstanding their lack of experience with molecular beam techniques. Having gotten Bullard's backing, Essen was assigned J. V. L. Parry and they set to work in the summer of 1953. Though guided by Kusch's design, Essen and Parry kept the evacuated volume small by placing only the pole pieces of the deflecting magnets inside the vacuum chamber. At the end of April 1954 they began to braze and solder. After an embarrassing collapse of the chamber when first evacuated in November, the apparatus came together with impressive swiftness (see Fig. 8). Single-cavity resonance was observed early in May 1955 and the Ramsey resonance less than two weeks after. Two more months of reliable operation gave them enough data to publish [56].

Their 9192.631 83 MHz was based upon the provisional uniform time scale maintained by the Royal Greenwich Observatory, a scale derived from the earth's rotation, but employing quartz clocks to smooth its seasonal variations. William Markowitz, Director of the Time Service Division at the U. S. Naval Observatory, knew this was not good enough. His program for the determination of Ephemeris Time from the moon's motion had been under way for three years, and he had been looking in vain for someone in the United States able and willing to collaborate with him on the determination of an atomic or molecular frequency in terms of the astronomers' very best time. Markowitz had followed Essen's progress closely, and in August, 1955, at the International Astronomical Union
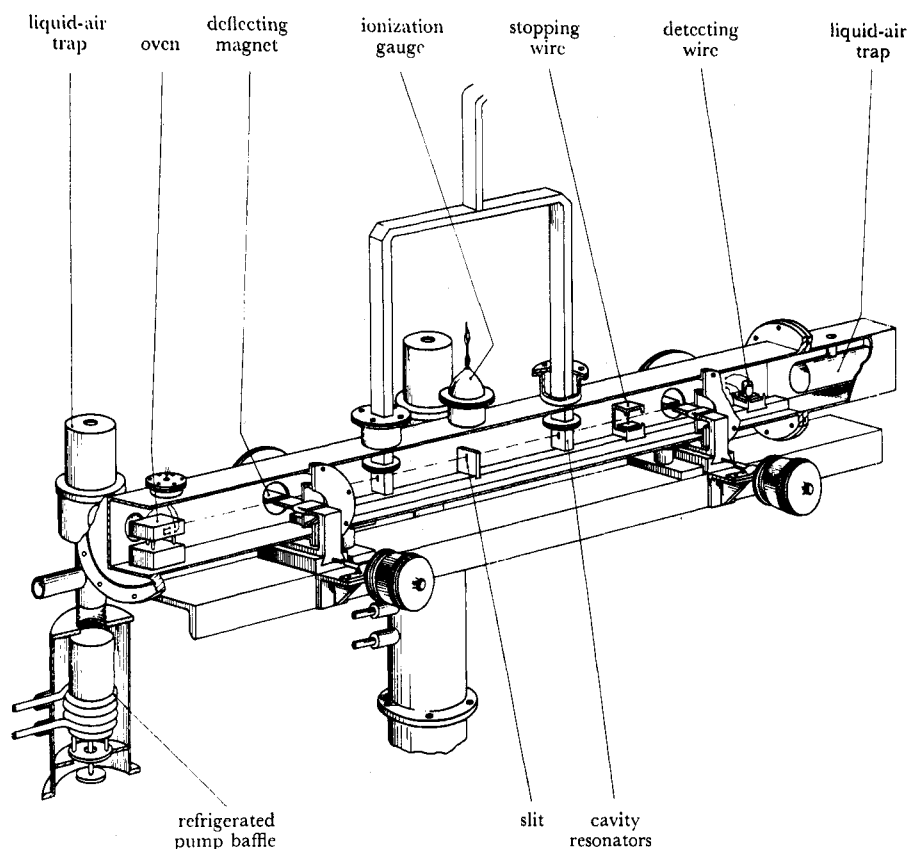

Fig. 8. *NPL cesium beam apparatus.* This drawing was prepared in 1953/1954 while the apparatus was being constructed. It remained in 1957 still sufficiently representative to be included in the definitive publication. (L. Essen and J. V. L. Parry, "The caesium resonator as a standard of frequency and time," *Phil. Trans. Roy. Soc. Lond.,* vol. A 250, pp. 45–69, 1957.)

meeting in Dublin, they agreed to undertake the task. The 9192.631 770 MHz which they reported after three-years labor was adopted nine years later by the General Conference of Weights and Measures, and remains today the official definition of the SI second [57].

Never was the NPL cesium-beam apparatus—any more than the NBS—operated as an atomic clock, or even in the feedback loop of a stabilized oscillator. Nonetheless, it is not without some reason that it is commonly cited as the first cesium clock. By establishing a value for the cesium frequency with all the precision of the most advanced astronomical time scales, Essen and Parry met the first precondition for the establishment of an atomic standard of time interval that could challenge and displace the astronomical.

IV. THE FIRST ATOMIC BEAM CLOCK

While Essen and collaborators, with Bullard's support, were pushing the cesium-beam program forward along the expected lines at Britain's National Physical Laboratory, another program was developing along quite unexpected lines under Jerrold Zacharias at MIT's Research Laboratory of Electronics.

MIT's RLE

The Radiation Laboratory, a deliberate misnomer, was set up at MIT by the National Defence Research Committee in the winter of 1940/1941 to work on microwave radar. Over the course of the war the Rad Lab acquired an unprecedented array of electronic equipment, as well as the specialized shop facilities and personnel to construct it. By the middle of 1944, with victory seeming assured, and scientists everywhere beginning to look to their postwar personal and institutional interest, there was much concern at MIT that the unique resources assembled by the Rad Lab not be dissipated. This concern crystallized around a proposal, originated by John C. Slater while on loan to Bell Labs (where interdisciplinary research was the norm), for the formation of an interdepartmental laboratory. Slater's proposed "Research Laboratory of Electronics," and its nominal program, the continued pursuit of microwave research, especially to higher frequencies, was sold without difficulty to the military. (See Fig. 9.) As with the Columbia University Radiation Laboratory, a liberal contract with the Army Signal Corps covered joint funding by both (subsequently, all three) services for a very diverse research program [58].

One of the first experiments performed with the facilities of the Rad Lab under their new auspices was a direct measurement of the cesium hyperfine separation by Arthur Roberts, Yardley Beers, and Albert G. Hill. This ingenious experiment used a 3-cm klystron stabilized by the cavity method recently devised at the Rad Lab by R. V. Pound. But Roberts loaded his cavity with cesium vapor. A variable magnetic field applied to the cavity cum vapor swept 14 hyperfine Zeeman lines successively through the cavity resonance. As the two resonances approached and receded the resonant frequency of the cavity swung up, then down and up again, along a dispersion curve, following the variation in magnetic susceptibility of the cesium vapor. This signature was recognized by mixing the output of the cavity with that of a separately stabilized 3-cm oscillator and passing the difference through an FM receiver and phase-sensitive detector.

Fig. 9. *RLE leadership, circa 1948.* The centrality of the RLE within postwar MIT is perhaps best indicated by the careers of its first three Directors. Left to right: Julius Stratton, Director 1946–1949, then Provost, then President of MIT; Albert Hill, Associate Director 1946–1949, Director 1949–1952, then first Director of Lincoln Laboratories; Jerome Wiesner, Assistant Director 1947–1949, Associate Director 1949–1952, Director 1952–1960, then J. F. Kennedy's Science Advisor, then Provost, then President of MIT. (Photo courtesy of the MIT Museum.)

Unfortunately, after yielding preliminary results, consistent with Kusch and Millman's pre-war 9192.6 MHz, the apparatus became obstinate, and Roberts followed the call of an academic appointment in Iowa. Though an equally preliminary publication drew attention to their method, which was important for the progress of microwave spectroscopy, no quantitative results were obtained until 1948. Then Beers returned to MIT for the summer, got reliable operation, and measured the frequency with the microwave frequency standard constructed at the Rad Lab during the war. Beers claimed for the 9192.51 MHz he obtained an order of magnitude greater precision than Kusch was able to do at the same time for the 9192.7 ± 0.1 MHz he obtained by an improved extrapolation from radio-frequency measurements on cesium atomic beams [59], [60].

Zach

When, late in 1945, Roberts was looking for an appropriate substance upon which to try the experimental technique he had devised, it was Jerrold Zacharias who suggested cesium, with its hyperfine transition exactly suited to the Rad Lab's 3-cm radar equipment [61] (see Fig. 10). Zacharias himself was then beginning to organize under the broad RLE umbrella a Molecular Beams Laboratory—just as Rabi's lab formed a major part of the CURL. Since at this time the objects of molecular-beam research were almost exclusively the properties of nuclei, so the technique itself and the practioners of it were generally assigned to nuclear physics. Indeed, just because Zacharias himself had been appointed Director of MIT's newly established Laboratory of Nuclear Science, it seemed to him best to find other auspices for his own research operation [62].

The career of J. R. Zacharias—"Zach" as he was familiarly known—well exemplifies the enormous difference between the physicist's world, and especially the Jewish physicist's world, before and after the Second World War.

Fig. 10. *MIT molecular beam apparatus, 1946.* J. R. Zacharias watches John McClean, one of his two mechanical technicians, sweat together the apparatus of Fig. 5. In background is student Darragh Nagle. (Photo courtesy of the MIT Museum.)

In nine years at Hunter College, 1931–1940, Zach had managed to rise from Instructor to Assistant Professor. But from five years at the Rad Lab and a brief tour at Los Alamos, Zach emerged as one of the most important people at MIT, with academic standing and administrative responsibilities unimaginable in 1940.

The war had given Zach a chance to display his energy and exercise his exceptionally quick and critical intelligence by dealing rapidly and decisively, but also imaginatively, with a wide range of technical and organizational problems. And the post-war world, that the physicists helped mightily to create, knew how to value those qualities and to give them scope. In the ten years following the war, in addition to his academic responsibilities as teacher, researcher, and administrator, Zach was active as an industrial consultant, and as an advisor to the military services. Most important and demanding were his roles as a leading member of the strategic study projects: Lexington, Hartwell, Charles, Lincoln, Vista, Lamplight. Subsequently, he would be a member of the President's Science Advisory Committee, and Chairman of the Physical Sciences Study Committee, which he called into existence to reform physics education [63].

Zach's Big Clock

Zacharias had not forgotten Rabi's vision of atomic clocks on mountain tops. Recognizing the potential importance of the atomic-frequency standard as both scientific and technologic instrument, Zach too had followed closely the Bureau's progress with its cesium apparatus. But to a physicist counting nearly twenty years experience with atomic beams, and especially to one with Zach's critical eye, the efforts of Lyons' Section could seem halting and amateurish. Zach was sure he could do better, but he saw no good reason to do just what the Bureau was doing [64].

Late in 1952, however, Zach was struck with an ingenious idea. While teaching an undergraduate honors seminar, he found himself explaining how the action of gravity upon a single atom could be demonstrated in a molecular beam apparatus—a conventional horizontal apparatus. Why not, he thought, construct a vertical apparatus, in which cesium atoms, flying upward from the oven, would be slowed by gravity, the slowest among them eventually coming to rest, then falling back down into a detector alongside the oven. The 9192-MHz "flopping" field would be applied about a meter below the top of the apparatus. Thus cesium atoms would spend a half second in rising and another half second in falling through this 1-m transition region. The 1-Hz frequency resolution to be expected from such an apparatus was several hundred times better than the Bureau's machine could possibly achieve [65].

There were obviously many technical problems posed by such a "fallotron." Even though held to a mere 5 m—two stories—it was an undertaking an order of magnitude larger than any ever tackled by a molecular beam laboratory. Because of the slowness of the cesium atoms in question, the vacuum requirements were even more severe than those derived from a comparison of beam path length and molecular mean free path. To compensate for the relative scarcity of the slow atoms, an enormous cesium flux had to be produced and efficiently focused into a beam. There was, moreover, a serious scientific question whether atoms with velocities as low as 10 m/s, with which a 5-m apparatus would operate, really existed at all in an atomic beam from a 400 K oven. To be sure, the Maxwell distribution assigned such slow atoms a finite probability, 10^{-6} that of the most probable velocity, 300 m/s, but the applicability of the formula to an atomic beam had never been put to so searching a test.

Late in December 1952, Zach initiated experiments to put the slow atoms in evidence, but these apparently were soon put aside, perhaps with the argument that only the "fallotron" itself could clearly demonstrate the presence of such atoms. The enthusiastic response of several of Zach's colleagues—notably Norman Ramsey up the street at Harvard, and Jerome Wiesner, Director of the RLE—decided the question. In March 1953 Zach set a first year graduate student, R. D. Haun, to work on the problem of cesium ovens producing highly collimated high-flux beams. By the summer of 1953 the project had advanced so far that his two skilled mechanical technicians began building up the apparatus through a hole cut in the ceiling of one of Zach's rooms in Building 20 (see Fig. 11), a typical war-time wooden structure of two stories that the RLE had taken over from the Rad Lab. The project went on for four years, absorbing a large fraction of the resources of the Molecular Beams Laboratory, especially its technical personnel. Rainer Weiss, who came into the Lab while still an undergraduate, became Zach's right hand on the big clock, and persevered to the bitter end (see Fig. 12). The fallotron was finally abandoned early in 1957 without having come close to working. The slow atoms were never found; apparently they were scattered out of the beam before escaping the vicinity of the oven [66].

Fig. 11. *Erecting the big clock, 1953.* Frank O'Brian, J. R. Zacharias' chief mechanical technician, bolting up the quadrupole magnet which was to serve the "fallotron" as both state-selector and state-analyzer. (Photo courtesy of the MIT Museum.)

Fig. 12. *"Fallotron" from above.* Rainer Weiss works on a cesium detector (temporarily placed in the transition region to search for the missing beam) while Zach looks up from the floor below.

Zach's Small Clock

The 5-m "fallotron," whose very name echos the glamorous accelerator projects of the post-war years, was by no means the be-all and end-all of Zacharias' atomic clock program. If Essen returned from America in February 1953 ready to attempt a cesium clock with limited and inexperienced staff, it was surely in part because he had found that

"At M.I.T. Professor J. R. Zacharias was, as he expressed it, definitely interested in clock making. He thought it was a fairly simple business to design an atomic beam equipment to serve as a clock or as a frequency standard... ." Indeed, by the time of Essen's visit, Zach had already decided to build, in parallel with the big clock, a "small clock," which would function both as a source of the cesium hyperfine frequency for the ultra-narrow line of the fallotron, and also as a frequency standard for his Molecular Beams Laboratory strung out through several corridors of Building 20. More, the design of this apparatus was to demonstrate the feasibility of its own further industrial development into a marketable atomic clock [67] (see Fig. 13).
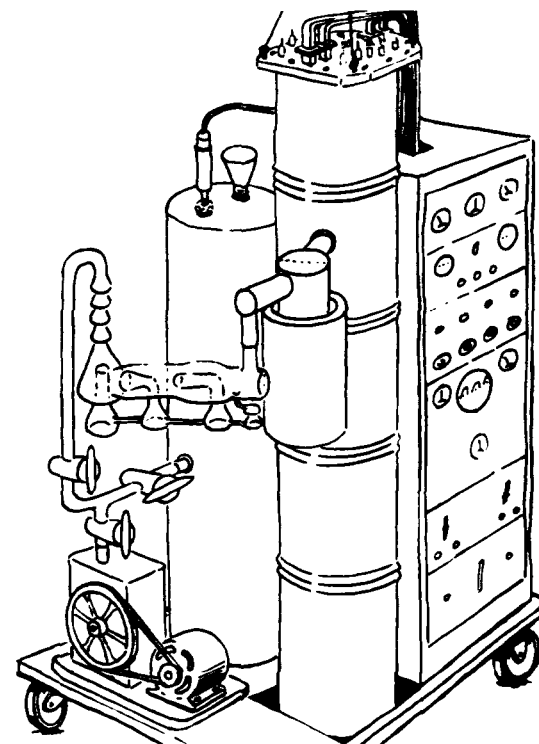

Fig. 13. *Concept of the small clock.* Drawing by F. O'Brian, probably in 1953, of Zacharias' concept of a semi-transportable cesium beam frequency standard for his laboratory.

From this latter perspective, the gravest delinquency of the extant atomic-beam apparatus was squandering the charge of their ovens. No evacuated apparatus which required to be opened at even infrequent intervals to be recharged with cesium could hope to make a place for itself outside specially equipped laboratories with specially trained staff. From the outset Zach had clearly in mind that the solution of this problem and that of a high-flux oven for the big clock were one and the same: both required emission of highly collimated beams, in the one case in order to reduce the total emission to the lowest possible value, in the other to increase the total beam flux by a very large factor.

Thus it was on oven design that R. D. Haun was set to work in March 1953. The very simple solution that finally emerged from Haun's experimentation, aided by the advice of RLE's ingenious machinists and technicians, was "crinkly foil." Strips of 0.001-in nickel foil were passed between a
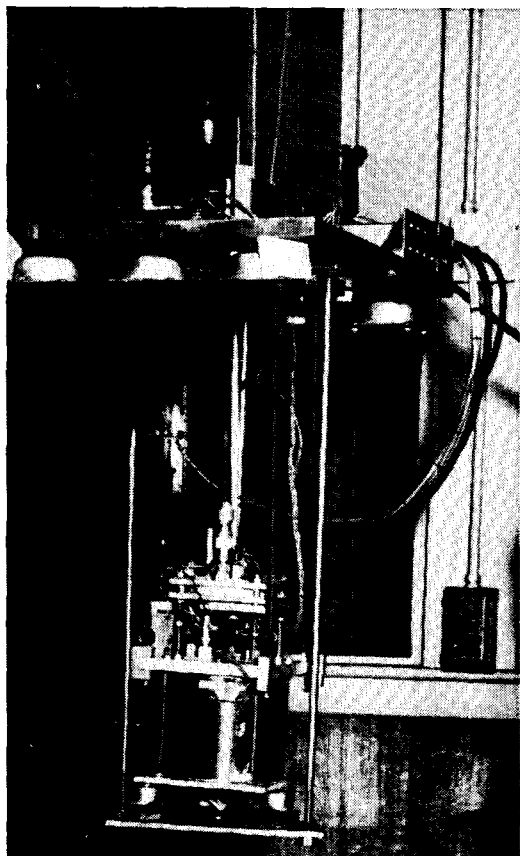
Fig. 14. *Yates–Rearwin 3-GHz oscillator.* Shown as mounted, antimicrophonically, on the small clock, Jan. 1955. (Photo courtesy of the MIT Museum.)

pair of finely toothed, interlocking rollers, resulting in a corrugated foil of wavelength about 7 mils and amplitude about 1 mil. Stacking alternate layers of crinkly and flat foil produced a structure of fine canals, which, clamped in the orifice of the oven, resulted in a highly collimated beam. For the small clock the stack was just $\frac{1}{40}$ in high and 1 in wide; for the big clock it was $\frac{1}{2}$ in high and 4 in wide [68].

However important the oven problem in the long term for the success of atomic beam clocks, in the short term Zach needed even more a spectrally pure and stable source of 9192 MHz to excite the transition, as well as the discriminator and feedback mechanisms to lock that signal generator to the cesium line. Discussion with his RLE colleagues in electrical engineering, Wiesner and Henry Zimmermann, pointed toward a small planar triode, recently available from Western Electric, which gave high power gain up to 4500 MHz. If, as promised, an oscillator based on this 416B triode showed considerably less phase jitter than klystrons operating in the same range, its output could easily be doubled or tripled in a semiconductor diode to give the required 9192 MHz.

It was now early in the summer of 1953 and Zimmermann fingered one of his advisees who, having completed a year of graduate work, was looking for a master's thesis project. Zach invited Richard Rearwin into his office, and asked whether he had a few minutes to talk about his favorite project. Then with infectious enthusiasm Zach began describing the clock while sketching his ideas on an oversized

paper pad. Before the end of the summer Rearwin was at work on the problem of a cavity-controlled oscillator based upon the 416B [69].

At this point Zach had the good fortune to enlist the assistance of James Garrett Yates, who had recently arrived at RLE for an academic year, on leave from Cambridge University. Yates, Irish by birth and education, had won gold medals for firsts in physics and mathematics at Trinity College, Dublin, before five years of wartime radar work in Britain had made an electrical engineer of him. After the war, in Cambridge's Electrical Engineering Department, first as Demonstrator, then as Lecturer, Yates had a large influence on its new curriculum as well as on electronics research there. At MIT Yates' initial contacts included RLE Director Wiesner, who steered him toward Zach's clock project. Yates found it so intriguing that he made the small clock, in all its aspects, his own project. Though modestly depreciating himself as a mere engineer ignorant of modern physics, he impressed everyone in the Molecular Beams Laboratory with his ability and amiability [70].

When Yates came into Zach's lab at the end of September 1953 he found Rearwin measuring the impedances of the 416B, and he threw himself into the problem of oscillator design. From their joint efforts, and the assistance of one of RLE's exceptional machinists, came a simple but effective device—in the view of the younger engineer "a work of art" (see Fig. 14). Its operation was no cinch, however, with 27 separate steps just to get it to oscillate. By the middle of January, 1954, they had two such oscillators beating together, and inferred a stability of a part in 10^9 over 1 s. Further development continued into the spring, especially on the feedback cavity whose dimensions, thermally controlled through a resistive winding, determined the frequency of oscillation [71].

While Haun was at work on ovens, and Rearwin and Yates on the microwave signal source, the RLE shops were making up the components of the atomic-beam apparatus. Early in January 1954 the "insides" were brought around to Room 014, where the big clock already soared through the ceiling. With the components came a large low cart, or trolley, on which the clock and its racks of electronics were then erected (Fig. 13), for Zach was determined to demonstrate that an atomic beam apparatus could be made transportable. Where a laboratory molecular-beam apparatus is liberally equipped with viewing ports and arrangements for adjusting the positions of critical components from outside the vacuum chamber, Zach imposed upon Haun, Yates, and the technicians assisting them, an apparatus that had to be completely aligned by wires and jigs before being sealed inaccessibly into its windowless can—again, a measure intended to persuade the industrial engineers [72] (see Fig. 15).

Five months of work were required to get the small clock to the point of operation. At the end of June they found their first single-cavity "flop"—the graphic colloquialism for a hyperfine transition considered as reorientation of the valence electron—and on July 15 double-cavity, or Ramsey, flop was observed. At this junction, perhaps even somewhat earlier, Yates and wife, having already remained longer than they had anticipated in the American Cambridge, departed on an automobile trip through the States. Zach,
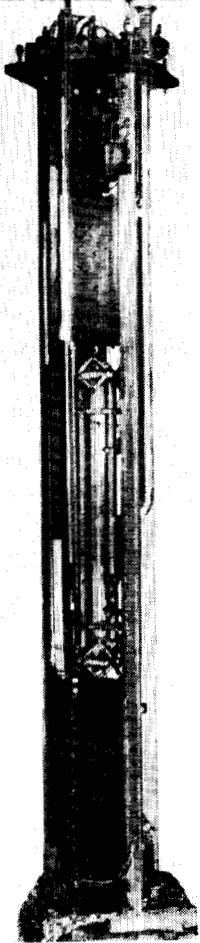
Fig. 15. *The insides of the small clock.* The cesium beam apparatus was lowered from above into its 6-ft vacuum can, upon whose lip the square plate at top rested. All electrical and microwave connections go through that top plate, as foreseen in Fig. 13. The rigid skeleton is formed by a pair of U channels bolted to the sides of the steel boxes containing the state-selecting and -analyzing magnets. The magnets are permanent but magnetized *in situ*, hence the heavy bus bars running down the left side. The oven is in shadow at bottom, the detector at top, with seven curved dynode plates of one of the electron multipliers clearly visible above the magnet box. A second multiplier is placed symmetrically to the rear, for the entire apparatus is essentially double: the oven produces two beams, the magnets have two sets of poles, and there are two sets of microwave interaction regions fed separately by waveguides on right and left (hidden). The primary purpose of this duplication was, presumably, to provide a check on the accuracy and stability by comparison of these two partially independent "clocks"; the secondary purpose was to measure by such comparison small physical effects upon one of the beams—in particular, observation of the effect of electric fields on the cesium hyperfine structure, which formed R. D. Haun's doctoral research.

perforce, became more involved in the laboratory work and was fortunate, moreover, to have gotten the assistance of Kerns Powers. An able electrical engineer in his late twenties and his first year of graduate study, Powers had applied to Wiesner for a summer job at RLE and was assigned to the clock project. As his first task Powers had designed and built the servo-electric circuits to lock the Yates–Rearwin

oscillator to the cesium resonance. This included a phase detector and a direct coupled, high-gain dc amplifier to meet the relatively high power requirements of a thermal control of the oscillator cavity [73].

In the latter part of July, Haun and Powers sought to get this control system to function in conjunction with Richard Daly's lock-in detection system based upon modulation of the excitation frequency. Zach, in anticipation of the requirements for feedback control and calibration of a clock, had had Daly develop this lock-in detector, and a system of frequency markers, for the high-precision measurements of gallium hyperfine structure which constituted Daly's doctoral thesis. Though this technique of frequency modulation of the transition field was to be essential in the next, industrial stage of development, in the summer of 1954 it added further degrees of complication to an apparatus whose operation was at all times precarious [74].

Finally, early in August, Haun and Powers took out the FM equipment and put the dc Ramsey flop signal directly into the lock-in circuit. In this way, on Friday the 13th, they locked the microwave oscillator to a cesium hyperfine transition and thus had the first atomic-beam clock—or their stabilized microwave oscillator would become a clock once they had gotten Powers' frequency division and comparison circuits into operation and could thus beat the Yates–Rearwin oscillator against signals from WWV, the Bureau of Standard's radio station.

That same day the frequency-modulated detection system was operated, and in the following days Yates rejoined the effort for two months of intensive work before sailing for England. Various electronic arrangements and modifications of the clock were tried. In particular, on the 23rd, the vacuum can was opened and an alteration of the beam geometry undertaken that would be fundamental for commercial cesium-beam clocks: the oven, magnets, and detector were taken out of line and placed at small, carefully calculated angles, so that only a small fraction of the beam, consisting of relatively slow atoms, found their way through the apparatus. Immediately, the width of the resonance was reduced from 750 to 350 Hz [75].

Haun, having served his laboratory apprenticeship, now would spend two more years using the small clock for measurements that could be regarded as a contribution to the science of physics and thus qualify him for a Ph.D. in that Department. Given the exigencies of their discipline, it is thus not surprising that nearly all the physicists in Zach's laboratory, both junior faculty and graduate students, saw themselves as pure scientists, devoted to understanding nuclear structure, and neither welcomed the influx of engineers nor applauded the application of the Laboratory's resources to technical development. This was certainly unrealistic on their part, though no more unrealistic than the notion of pure physics, altogether, in the post Second World War world [76].

V. The First Commercial Atomic Clock

In the spring of 1954, on top of all that was occupying him on MIT's campus, Jerrold Zacharias was testifying at Oppenheimer's AEC security hearing and negotiating an agreement for the commercial development of his atomic

beam clock by the National Company, of suburban Malden. Zach had already begun to solicit the interest of leading industrial research and development laboratories back in 1953, when the clock project had still been barely launched. However, no well-established advanced electronics firm would undertake a vacuum tube in which the working fluid was a neutral atom. Zach therefore turned to a far less glamorous local firm which, recently under new and ambitious management, was relying heavily upon his close friend Jerome Wiesner for technical advice and direction [77].

The National Company, Inc.

The National Company, Inc. had been founded in 1914 as National Toy Company. In the 1920s the firm moved into amateur radio equipment, and by the 1930s had a solid reputation in this field. During World War II and after National added to their line high-quality radio receivers and transmitters for the military services. With about 600 employees, including some 80 engineers in design and development, National produced in short runs and at modest profits a large number of specialized equipments. In the late 1940s, NATCO—as the company was addressed by cable and by its familiars—took a fling at marketing a television receiver. Quickly, however, the firm backed out of the unaccustomed field of hyper-commercial competition. Following the outbreak of the Korean War in June 1950, military procurement climbed steeply out of its postwar slump. By the end of 1951, Natco's backlog of government orders had risen to more than 10×10^6, roughly twice its annual gross sales. With some delay the company's profits also began to rise. But neither backlog nor increased earnings were well reflected in the stock market, which was slow to take cognizance of Natco's improved prospects [78].

The whole of Natco's unexciting but dignified development as an electronics firm had been under the benevolent hand of William A. Ready. As he wrote with pride early in 1953, in what was to be his last report to the stockholders, "during the entire history of the Company, there has never been any lost time as a result of a strike or walkout." Indeed, the "homey atmosphere" of the Ready era is commemorated to this day by its former employees at an annual Christmas party. Ready was aging, and stepping back in favor of his son-in-law, Charles Hornbostel, better known as middle-distance runner in the 1932 and 1936 Olympics than for any subsequent accomplishment [79].

Natco was closely watched by wealthy broker Louis C. Lerner. Of local and modest origins, educated at the State Teacher's College, Lerner in his early forties was already a millionaire holding stock in more than 200 companies. Early in the Korean War he had begun to buy up Natco's too. Now, however, neither he, nor other investors whom he mobilized, were satisfied with their return. Seizing the opportunity afforded by Ready's announced intention to resign the presidency, at the March 1953 stockholders' meeting, after an intense proxy fight, Lerner won control of Natco [80].

Within a year, National had more than 30 new men in its upper echelon. The talent search was mounted by the new Chairman of the Board, Raymond C. Cosgrove, previously Executive Vice President of Avco Manufacturing Corp., Vice President of Crosley Radio, and four times President of the Radio-Electronics-Television Manufacturers Association. At the same time the labor force was reduced by one quarter

following efficiency studies by Booz, Allen, and by the Work-Factor Company, whose senior partner, Joseph Quick, became Natco's President. The new leadership was inspired by the notion that this 5×10^6/year company could, in but a few years, gain a leading position in the 5×10^9/year electronics market. It was just a matter of vision and aggressive management. Indeed, where the Ready regime was above all amicable, "aggressive" was the watchword of the new regime, appearing in three of the first seven sentences of its first report to the stockholders [81].

But efficiency and aggressiveness were not the only ingredients in the new Natco's formula for profitable growth. Even greater importance was given to expansion and diversification of the product line, and of the requisite technical expertise. To this end Lerner, Cosgrove, and Quick leaned heavily upon local academic advisors from the Rad Lab alumni network. Malcolm M. Hubbard, an Assistant Director of Lincoln Laboratory who had worked with Zacharias during the war and for him after it, was the first to be drawn onto the Board of Directors. He was followed shortly by Wiesner, and by Oliver H. Straus, a New York department store heir and M.D. who never practiced (see Fig. 16). Straus had worked with Wiesner at the Rad Lab,

Fig. 16. Natco's technical team, 1954. From left: "Jerry" Wiesner, "Ollie" Straus, "Al" Pote, and Raul Frye, Vice President for Engineering when the clock project was undertaken. (From Natco, "Tuned to tomorrow," 1954.)

and now he took charge of audio engineering in this company in which he had invested heavily. Early in 1954 Hubbard resigned from the Board and was replaced by Zacharias [82].

In addition to these exceptionally expert Board members, the new Natco obtained the services of an impressive constellation of consultants. From Harvard: R. V. Pound, one of the most fertile and versatile of the younger generation of physicists, and H. I. Ewen, still younger and still more energetic, who had recently made the first observation of the hydrogen hyperfine transition at 21 cm. From MIT: J. C. R. Licklider, authority on sound and hearing; H. J. Zimmermann, Assistant Director of the RLE, and A. J. Pote, Assistant Director of the Transmitter Division of Lincoln Laboratory, who indeed had already been drawn in as consultant in 1951. Initially this technical talent was used intensively, and their Saturday gatherings for brainstorming and criticism were memorable. Within a few years, how-

ever, all the scientists and engineers had left Natco's Board of Directors, as did most of the stars from its constellation of consultants as well [83].

Natco Undertakes the ABC

In the spring of 1954 the new National Company was headed full tilt toward its vision of itself as front runner. It signed a mutual licensing agreement with CSF, a leading French electronics firm, established an in-house research laboratory, and in the next twelve months would double its engineering staff. Several development projects were under way, the largest being a high-fidelity FM receiver, amplifier, and phonograph system for the commercial market, and, for the military, a system of over-the-horizon radio communication based upon tropospheric scattering. This would have been quite enough for a more conservative management, but Natco, relying heavily upon its staff of consultants not merely for advice but also for initiative, was on the lookout for any promising project. Thus the atomic beam clock, a wholly new departure, offered Natco the opportunity to occupy a novel field of electronic technology [84].

Zach, however, was unable to offer National anything but his expertise. The idea of an atomic beam clock had not merely been published by Rabi years before, but had already been patented by Hershberger for RCA. True, the concept had not previously been reduced to practice—and indeed Yates and Haun would not succeed in doing so for some months still. But the fact that the small clock was being built on MIT's campus with the government's money precluded Zach from acquiring a patent that could protect Natco from competition. Still the Board found the prospect exciting, and struck an agreement with Zach paying him consultant's fees and a 4-percent royalty on all the atomic beam clocks it sold [85].

That summer—again, even before the small clock ran—experimental work was begun at Natco by one of Zach's lieutenants: Assistant Professor John King was assisted at Natco by a student brought along from MIT and a couple of the company's engineers. The summer over, Richard Daly took over from King. The Physics Department, as it came to be known, started from scratch, quite literally with bare walls and a bare floor. At the outset much time was spent simply ordering equipment. Even in October when Eugene Grant—soon to be Vice President for Engineering—arrived at National he found "a rather hilarious situation . . . no microwave measuring equipment, no vacuum pumps." Worse, they wanted the engineers and technicians familiar with these essential technologies, so that throughout the project to a great extent they were "reinventing the wheel" [86].

Funds and Schedule

The common experience of R & D projects in novel technologies—that they take longer and cost far more than their proponents ever imagined—was here emphatically confirmed. Natco's Board of Directors, in approving the clock project in May 1954, appropriated $50 000, and seemingly expected salable hardware within a year. It was recognized, of course, that 50 Grand was but a fraction of the cost of even so rapid a development. Thus from the outset funds were solicited from all three military services, although not until the spring of 1955 were any contracts signed [87].

Among program officers at military funding agencies, Friedrich Reder of the Fort Monmouth Signal Corps Laboratory was the most interested and active in atomic clock development, with contacts through the entire field. By drawing in the Air Force, he was able to bring Natco $70 000 for development plus delivery of one clock. Later in 1955 Rome Air Development Center and Air Force Cambridge Research Laboratories came through with another $210 000 for six units. The Office of Naval Research provided $30 000 for research and another $50 000 for one, or possibly two, units, for the Naval Research Laboratory, which, early in September 1956, received the first National Atomic Frequency Standard to be delivered.

The fraction of this $360 000 for nine prototypes which is to be counted as support of research and development is, of course, largely a matter of accounting. When Zach first publicly announced his clock program in January 1955 —presumably in response to the sensation created by the public announcement of Townes' ammonia maser—he stated that Natco would have clocks for sale within the year at $10 to $20 thousand. By May the price had risen to $40 000 for a lot of one, and in June was fixed at $50 000. Rather than saying that the first eight or nine units were therefore sold to the military at a discount, we should of course say that a good fraction of the development costs—which Natco ultimately reckoned at nearly a million dollars—was included in the price of each unit [89].

Even more than the development costs, the development delays were distressing to Natco's management. By the spring of 1955 the company's financial picture had deteriorated badly. Backlog of government orders generated by the Korean War had disappeared. High-priced managers and expanded research and development staff had greatly increased overhead costs. The hi-fi system rushed onto the market in the summer of 1954 proved to have been incompetently designed and had to be withdrawn at considerable loss. Thus the National Atomic Frequency Standard came to have greatly increased importance in management's eyes, indeed was viewed as potentially the company's salvation [90].

But the atomic clock was not to help Natco's cash flow in 1955. Zach would not, as Quick requested and expected, devote himself full-time to expediting the project. In June, when the breadboard model was not yet quite finished, Daly was still hoping to complete the first batch of deliverable units by January, 1956. Naturally, it required intensive work to complete them by the end of that year.

Dick and Zach

In keeping with National's policy of relying heavily upon the consultants, Zacharias, now also on the Board of Directors, played an important role in the design and development of their commercial clock. Primarily responsible, however, for directing the work and shaping the product was his student Richard Timothy Daly, Jr. Born in 1925, and a ham since adolescence, Daly spent 42 months in the Army Air Force and two years at Sperry Gyroscope Company, before heading to MIT for undergraduate and graduate training in experimental physics. From this stiff course he emerged at the end of August 1954 with a highly successful thesis and overflowing self-confidence. Enthusiastically, the can-do irishman threw himself into the Natco project and sought to make it the vehicle of his own vision of an atomic-beam clock as it ought to be [91].

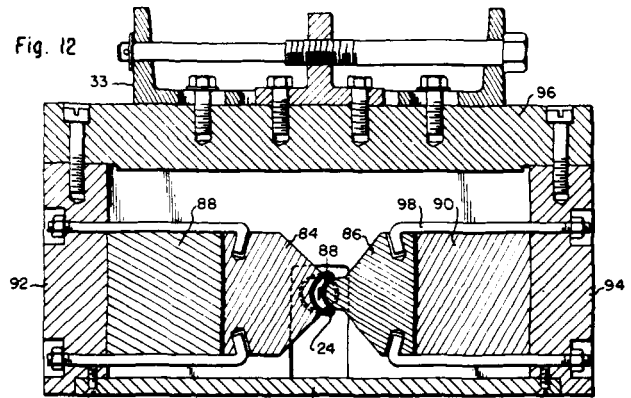Zach took a rather gradualist, evolutionary approach to the problem of industrializing the laboratory atomic beam apparatus. With the "small" clock he had begun to address the task, and had moved several steps away from laboratory design and techniques. Now, Daly argued for a radical departure, taking the industrially manufactured electronic vacuum tube as a model. The slim, sealed-off beam tube he envisioned contained only the cesium oven and detector within its glass vacuum envelope. It would be, moreover, a *short* tube, for, Daly reasoned, increased stability could as well be achieved in a shortened tube with its greater signal-to-noise ratio, as in a lengthened tube with its reduced line width [92].

Zach saw such radicalism as reckless, especially as he was committed to producing hardware in short order. Who, however, was in charge? Consultants Zacharias and King, or Physics Department head, Daly? Soon Zach was pulling in Wiesner to their weekly conferences, and brought Yates back from England for a brief visit, in order to rein in Daly's technical fantasy. At times appeal was even made to management's strong arm [93].

By the beginning of 1955 a tentative compromise design concept for the production prototype had been hammered out (see Fig. 17). The tube would be long, and it would be
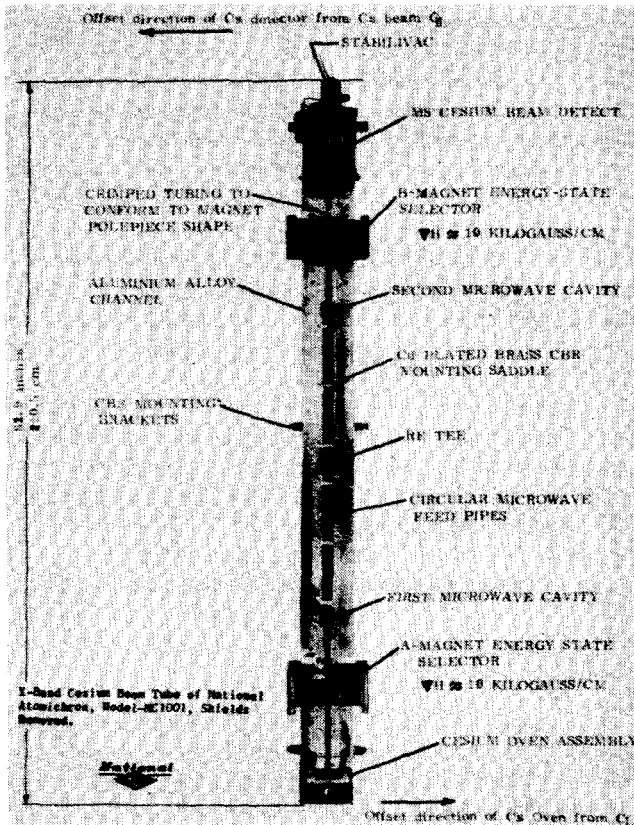
Fig. 17. *Production model, 1955/1956.* Illustrated is the cesium beam tube of the National Atomichron Model NC-1001, which was essentially identical to that of the prototype production model, NAFS. The labeling is due to J. George, "History," *op. cit.* (note [99]).

metal. But once baked and pumped it would be permanently sealed off. Moreover, it would be very narrow, and thus of minimum evacuated volume, with the magnets wholly external to the tube (see Fig. 18).
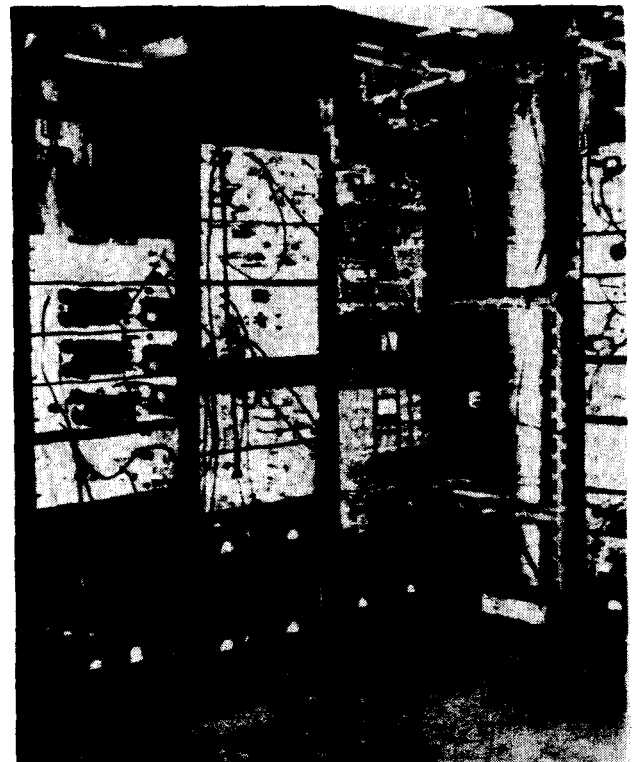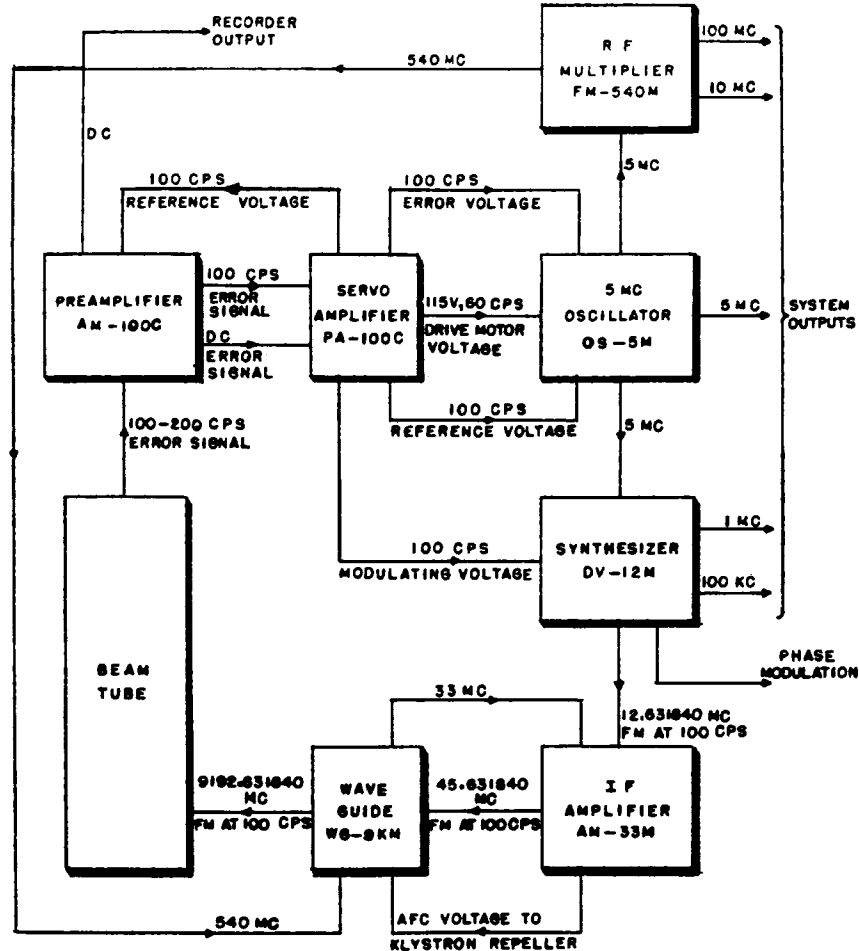
Fig. 12

Fig. 18. *Detail of Atomichron magnets.* This horizontal section through supporting channel, state-selecting magnet, and vacuum envelope shows Daly's slim beam tube at its slimmest point. (The short vertical line within the crescent, 24, represents the cesium beam.)

Daly's original design called for feeding the microwave cavities through a coaxial waveguide. Thus microwaves would fill the annular space, travelling out from a T at the midpoint of the guide, while the cesium beam traveled down the evacuated central tubular conductor. In the compromise design, this multiply connected structure was replaced by one in which the waveguide and microwave cavities formed part of the vacuum envelope. Consequently, the microwaves and the cesium atoms were in

Fig. 19. *Breadboard model, 1954/1955.* The can and its innards, as also those of the MIT small clock, are on display in the "Atomic Clock" exhibit at the Smithsonian Institution's National Museum of American History: Science, Technology, and Culture.

SYSTEM BLOCK DIAGRAM

Fig. 20. Block diagram of the frequency generation and control systems of the National Company breadboard and prototype production models. The waveguide unit contains both the germanium crystal completing the multiplier chain to 9180 (= 17 × 540) MHz and the reflex klystron local oscillator at 9147 MHz. It is here that the 33-MHz difference frequency is generated and, subsequently, the 9147 MHz is added back again to the amplified intermediate frequency, canceling the drift of the klystron frequency. (R. T. Daly for National Co., Inc., "Final Report. Atomic Frequency Standard," ONR contract 1707(00), NR375-750, p. 38, Nov. 15, 1956.)

interaction over the entire length of the microwave structure, from cavity to cavity. It proved impossible to calculate rigorously for this structure either the phases and standing-wave ratios, or the effect of the atom-radiation interaction upon the observed resonance. Much effort was spent both internally, and by TRG (Technical Research Group, Inc., in New York), under contract, estimating these effects. From such calculations, and others by Yates as well, it appeared that the effects would remain at or below a part in 10^{10}, and would thus not compromise the design specification of a part in 10^9. In the event, the effects proved to be extremely small [94].

Breadboard

While the design of the prototype production model was being vigorously discussed, work went forward on the construction of a breadboard atomic beam clock. From the outset of the development work at Natco under John King, the initial goal had been to build as beam tube a better-engineered replica of the MIT small clock. This was accepted by Daly too as a necessary first step, and would have been a straightforward one, except that it was begun without any vacuum or microwave equipment yet in house (see Fig. 19).

Far less straightforward was the development of the microwave and electronic control systems. This was also the more important task since the design developed for the breadboard was to be, apart from packaging, also that of the prototype production model. Much of the engineering work—some three man-years—was done by Dunn Engineering Company, a group of MIT graduates with whom Natco contracted. A large hand in the conceptual design was held, however, by National's Engineering VP, Eugene Grant, together with James Bagnall, while Project Engineers Michael Bovarnick (breadboard electronics), Walter Mainberger (NAFS electronics), and Arthur Orenberg (airborne prototype electronics) together put in some five man-years realizing it.

The frequency generation system was originally based on the Yates–Rearwin 3-GHz oscillator, which alone, with its thermostatic enclosure, occupied a full cubic foot. Soon

found insufficiently stable, it was discarded after all in favor of Lyon's more demanding direct multiplication of the frequency of a quartz oscillator in the megahertz range. However, after multiplication nearly 2000 times to 9 GHz, there was insufficient microwave power available to drive the beam tube. This, together with the necessity of synthesizing 9192.631840 Hz from the integral frequency (in megahertz) of a crystal oscillator, resulted in the rather complicated scheme of Fig. 20. The frequency of an auxilliary klystron is first subtracted from the weak output of the multiplier chain, then added in again after amplification of that intermediate frequency, thus rendering the final signal independent of the klystron frequency. This "drift canceling" technique, already in use among radio engineers, proved very successful. The weak link in the frequency generation scheme was the synthesizer/divider, for which Dunn Engineering had been largely responsible and which underwent repeated redesign in following years [95].

The accuracy/stability—for at this date the two concepts were still conflated—of a part in 10^{10} that Zach had attributed to the RLE small clock was a surmise, based upon the width of the resonance and the characteristics of the beam tube and control system. Now, in offering to supply clocks to the military and the commercial market, it became important to make some experimental test of the promised performance by comparison of two clocks. For this purpose resort was made to a peculiar design feature of the MIT small clock replicated in the breadboard beam tube. Namely, two parallel cesium beams issued from a single oven, and passed through the same deflecting magnets. Each beam, however, transited its own interaction regions terminating its own microwave structure. In the MIT clock, dual beams permitted the measurements that would give Haun a PhD in physics, viz., of the small shifts in the cesium hyperfine frequencies produced by strong electric fields applied to one of the beams. In the breadboard model, with the two beams treated as nearly alike as possible, and with a separate electronic and microwave system for each, dual beams approximated two independent clocks.

The breadboard beam tube diverged, however, from its MIT model in having only one detector (hot-wire ionizer, mass spectrometer, electron multiplier). This was feasible because full reliance could now be placed upon the gen-

eration of an error signal by "dithering" the frequency of the transition-stimulating microwaves back and forth on the resonance curve. Thus by using a different modulation frequency with the microwaves stimulating each beam, simple filtering of the detector output separated the signals due to each [96].

VP Gene Grant had actually invented this "dither" method for automatic frequency control of a microwave oscillator while working during the war at Westinghouse on airborne radar. Through this work, and his post-war employment at the Air Force Cambridge Research Laboratory, Grant was personally acquainted with Wiesner and Zacharias, who then were largely responsible for getting him to Natco. It was, however, apparently only here, now, at National that Grant himself, or Daly, or perhaps one of the project engineers, recognized a most important consequence of this dithering process, one which was to play an essential role in the success of the NAFS and in all subsequent passive atomic frequency standards. Namely, while the center frequency of the applied microwave signal is off the resonance peak, the output from the beam tube is at the modulation frequency; however, when the center frequency is right on the resonant frequency, the output signal is at twice the modulation frequency (see Fig. 21). This trick of "second harmonic detection" was patented by National and long regarded as one of their most valuable in the atomic clock field. It is not surprising, however, that so natural a consequence of an automatic frequency control technique widely applied in post-war electronics had been discovered and published previously [97].

Prototype

Work on the breadboard model—whose completion had originally been expected in the fall of 1954—continued through the summer of 1955. Thus although calculations had continued on the projected microwave structure of the prototype production model, final design and initial attempts at fabrication of the beam tube were apparently deferred until the autumn of 1955 [98].

The problems presented in fabricating this microwave structure/vacuum envelope, formed from several different materials, have been vividly recounted by James George, who joined the effort in September 1955 [99]. These prob-
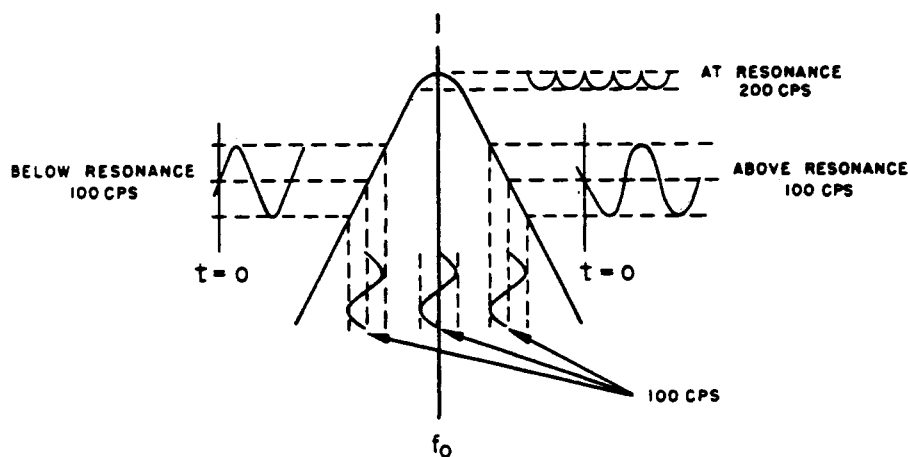

Fig. 21. *Phase discrimination and 2nd-harmonic detection.* (National Co., Inc., "Instruction manual for Atomichron Model NC-1001," p. 2-28, 1957.)

TL   TITANIUM FILAMENT LEADS
TG   TITANIUM FILAMENT
TC   TITANIUM CONNECTION
I   VYCOR INSULATOR
G   ELECTRON GRID
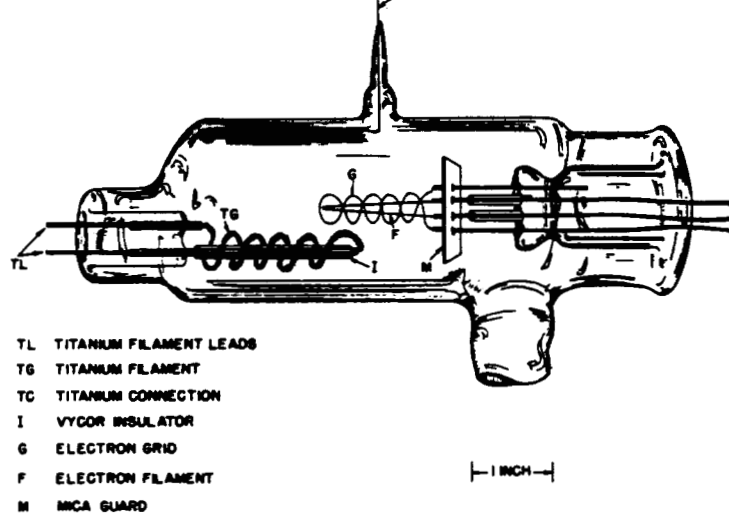F   ELECTRON FILAMENT
M   MICA GUARD

├─ I INCH ─┤

Fig. 22. *"Stablivac" titanium evapor-ion pump.* (J. G. King and J. R. Zacharias, MIT, RLE, Quarterly Progr. Rep., p. 62, July 15, 1957.)

lems would have been considerable even for experienced workers with full facilities. Here, as earlier, work at Natco was hampered by the lack of both. However, the ready availability of the RLE shops, as well as knowledgeable metallurgical firms in the vicinity, provided some compensatory advantages. Finally, in February 1956 as Daly recorded in his final report to the Signal Corps:

> After all the foregoing problems had been solved, a test beam tube containing only a cesium oven and detector was constructed, baked out under vacuum at 300° F. for two days, and sealed off. After seal-off, a barium getter was flashed and then, for the first time, a cesium beam was operated in a sealed tube.... [However,] After about 100 hours operation, the background signal due to the presence of the cesium atmosphere was so much larger than the signal from the beam itself that it was almost impossible to tell when the beam was on or off, much less observe the transition signal...[100].

Daunted, Daly took J. Yarwood's *High Vacuum Technique* (1945/1948) to hand. Scanning its final chapter on "The Properties of Materials Important in High Vacuum Technique," he found under the heading "Carbon" that colloidal graphite, adhered to glass or metal surfaces, "absorbs caesium strongly." And indeed, an alcohol-dag sold by Acheson adhered well to the tube's internal metal surfaces and effectively solved the problem of the cesium atmosphere. Now, however, the other half of the vacuum problem; namely, outgassing by the tube itself, its detector, and especially by the impure cesium commercially available, became acute. Barium getters proved mere bandaids: a pump of some sort was unquestionably required [101].

Zacharias had anticipated this problem from the outset, and regarded the intended elimination of all pumping apparatus as unrealistic. As Director of MIT's Laboratory of Nuclear Science, Zach was inevitably aware of the high-capacity titanium getter (evapor-ion) pumps being developed for accelerators [102]. From early on he had urged Daly to develop a small titanium ion pump. Daly resisted,

in accord with his vision and with the burden of development work already on his hands (which included a shortened version of the prototype beam tube, for the Air Force). Finally, in December 1955, as beam tubes were beginning to be assembled at Natco, Zach himself sketched an extremely simple "Jim Dandy Apparatus," and had it made up and tested at RLE. It worked surprisingly well. Thus in February, when the vacuum problem became an urgent reality, Zach had its solution almost in hand. With some further development, the "Stabilivac" (see Fig. 22), as it was soon tradenamed, proved quite sufficient for Daly's slim tube [103].

Vacuum was the most serious but by no means the last of the beam tube problems. However, the addition of Joseph Holloway to Daly's group in April 1956—immediately following completion of *his* MIT Molecular Beams thesis under Zach—helped greatly in surmounting the remaining obstacles. By the summer, complete and tested tubes were piling up in the Physics Department where the dozen production prototypes were to be assembled (see Fig. 23). The holdup was the electronics. Though, in principle, frozen the previous autumn, and merely to be repackaged to fit in the front half of a $2 \times 2 \times 7$ cabinet, the electrical engineers would not let well enough alone. Gradually, however, the chassis, cabinets, etc., fabricated in the Model Shop, came into the Physics Department for wiring, testing, and assembly. By the end of the year, the dozen prototypes were done—and Daly was on his way to TRG [104].

Atomichron®

At the outset of the program at National, the atomic beam clock was known as just that: the ABC. "Clock," of course, was a vivid but rather loose term for what was to be merely a frequency standard: there was no good, practical reason to burden the instrument with the electronics and motors necessary to integrate the oscillations and display elapsed time. Hence the production prototypes, for which Natco was taking orders, had by the summer of 1955 come to be denominated the National Atomic Frequency Standard: NAFS. This was not an especially euphonious acronym.
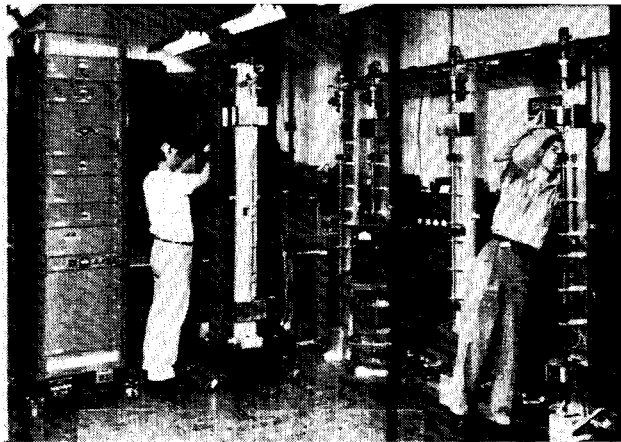
Fig. 23. *Atomichron assembly.* The photograph shows not the initial prototype series, but the subsequent assembly of the NC-1001 in Natco's Production Division.

As thought was given to publicizing and marketing this first practical atomic "clock," need was felt for a sexier name. Daly came up with "Atomichron," which National then made its registered generic tradename for all their atomic clocks [105].

The well publicized unveiling of Natco's Atomichron took place in a panelled room of the Overseas Press Club in New York on October 3rd [106] (Fig. 24). A month earlier, however, the first of the NAFSs had been delivered to the Naval Research Laboratory [107]. Subsequent production, under the model number NC-1001, was carried out in Natco's Production Division, with James George providing

Fig. 24. J. R. Zacharias and R. T. Daly (right) present their Atomichron® to the world at the Overseas Press Club in New York, Oct. 3, 1956.

these NASF/NC-1001 Atomichrons were made and sold [108]. The instrument, the first piece of quantum electronics equipment to be put into the hands of the "public," worked impressively well. Its stability and its accuracy were an order of magnitude better than originally specified and its operation was surprisingly reliable [109].

Though the instrument spoke well for itself, National continued its promotional efforts. These were misplaced, for there was in those early years no commercial market. Ninety percent of those first fifty Atomichrons were purchased by military agencies for their own installations and for loan to other government facilities, university-operated military laboratories, and university laboratories. The other 10 percent were purchased by other U.S. and foreign government agencies. Apart from their evident utility as frequency standards in laboratories engaged in research in that field, or responsible for the maintainance and transmission of standard frequency and time signals, there was at first no well-established application for these instruments. Soon, however, the urgency and priority of the problems of missile tracking and guidance would give new rationale and direction to atomic clock development [110].

For a full five years National had this limited market all to itself. After losing money heavily in 1955, Natco began to pick up in the following years, boosted by atomic clock sales and development funds. The $360 000 Natco had received from Army, Navy, and Air Force in 1955–1956 were followed by military development contracts for ten times that sum in 1957–1959 [111]. Their performance, however, was disappointing, and by the early 1960s the credit the company had gained for its pioneering work was largely dissipated. Through that decade, National oscillated between modest earnings and serious losses. Liquidating assets to stave off bankruptcy, in 1969 Natco sold its atomic clock line, know-how, and Atomichron® trade name [112].

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Fig. 4. *E. U. Condon, H. Lyons, and the first atomic clock, Jan. 1949.* Condon holds in his hands a model of the NH_3 molecule. Thirty feet of ammonia-filled, gold plated K-band waveguide, forming the absorption cell, are coiled around the electric clock at top; the absorption dip appears on the oscilloscope in front of Lyons. ("The atomic clock," NBS Tech. Rep. 1320, 1949; reprinted in *J. Horological Inst. America*, pp. 11–20, Feb. 1949, and pp. 7–14, Mar. 1949.)

Fig. 9. *RLE leadership, circa 1948.* The centrality of the RLE within postwar MIT is perhaps best indicated by the careers of its first three Directors. Left to right: Julius Stratton, Director 1946–1949, then Provost, then President of MIT; Albert Hill, Associate Director 1946–1949, Director 1949–1952, then first Director of Lincoln Laboratories; Jerome Wiesner, Assistant Director 1947–1949, Associate Director 1949–1952, Director 1952–1960, then J. F. Kennedy's Science Advisor, then Provost, then President of MIT. (Photo courtesy of the MIT Museum.)

Fig. 10. *MIT molecular beam apparatus, 1946.* J. R. Zacharias watches John McClean, one of his two mechanical technicians, sweat together the apparatus of Fig. 5. In background is student Darragh Nagle. (Photo courtesy of the MIT Museum.)

Fig. 11. *Erecting the big clock, 1953.* Frank O'Brian, J. R. Zacharias' chief mechanical technician, bolting up the quadrupole magnet which was to serve the "fallotron" as both state-selector and state-analyzer. (Photo courtesy of the MIT Museum.)

Fig. 12. *"Fallotron" from above.* Rainer Weiss works on a cesium detector (temporarily placed in the transition region to search for the missing beam) while Zach looks up from the floor below.
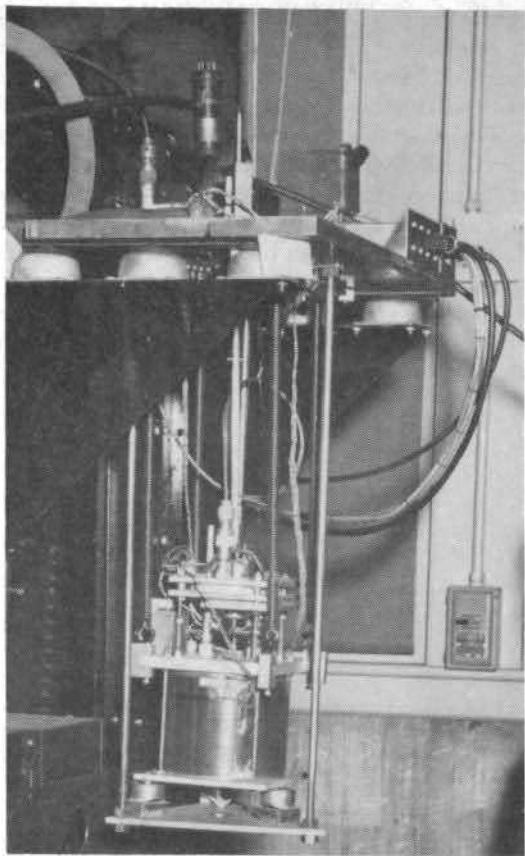
Fig. 14. *Yates–Rearwin 3-GHz oscillator.* Shown as mounted, antimicrophonically, on the small clock, Jan. 1955. (Photo courtesy of the MIT Museum.)
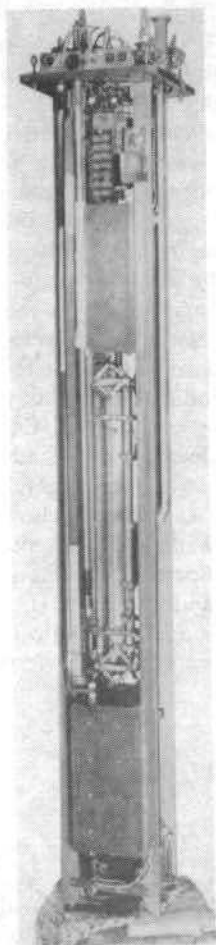
Fig. 15. *The insides of the small clock.* The cesium beam apparatus was lowered from above into its 6-ft vacuum can, upon whose lip the square plate at top rested. All electrical and microwave connections go through that top plate, as foreseen in Fig. 13. The rigid skeleton is formed by a pair of U channels bolted to the sides of the steel boxes containing the state-selecting and -analyzing magnets. The magnets are permanent but magnetized *in situ*, hence the heavy bus bars running down the left side. The oven is in shadow at bottom, the detector at top, with seven curved dynode plates of one of the electron multipliers clearly visible above the magnet box. A second multiplier is placed symmetrically to the rear, for the entire apparatus is essentially double: the oven produces two beams, the magnets have two sets of poles, and there are two sets of microwave interaction regions fed separately by waveguides on right and left (hidden). The primary purpose of this duplication was, presumably, to provide a check on the accuracy and stability by comparison of these two partially independent "clocks"; the secondary purpose was to measure by such comparison small physical effects upon one of the beams—in particular, observation of the effect of electric fields on the cesium hyperfine structure, which formed R. D. Haun's doctoral research.

Fig. 16. *Natco's technical team, 1954.* From left: "Jerry" Wiesner, "Ollie" Straus, "Al" Pote, and Raul Frye, Vice President for Engineering when the clock project was undertaken. (From Natco, "Tuned to tomorrow," 1954.)
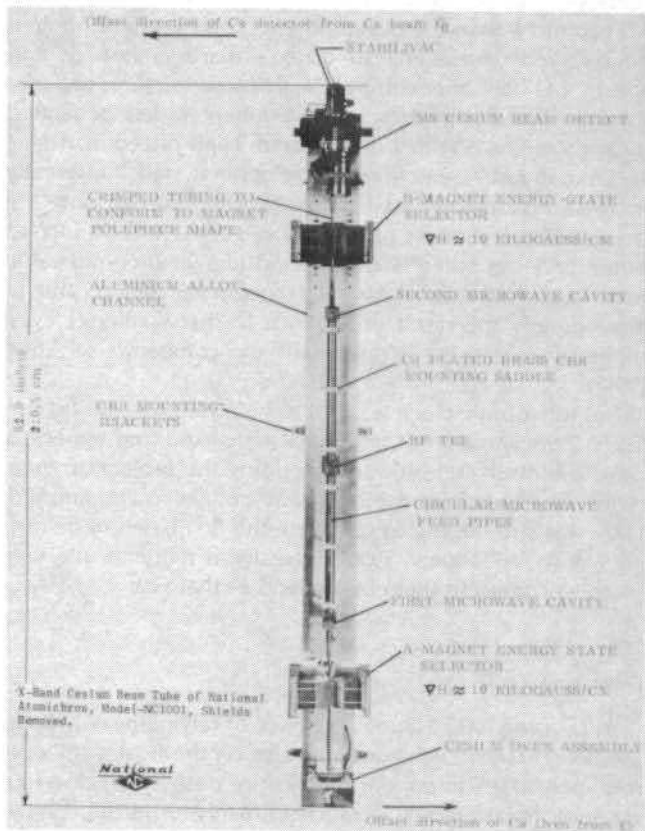
Fig. 17. *Production model, 1955/1956.* Illustrated is the cesium beam tube of the National Atomichron Model NC-1001, which was essentially identical to that of the prototype production model, NAFS. The labeling is due to J. George, "History," *op. cit.* (note [99]).
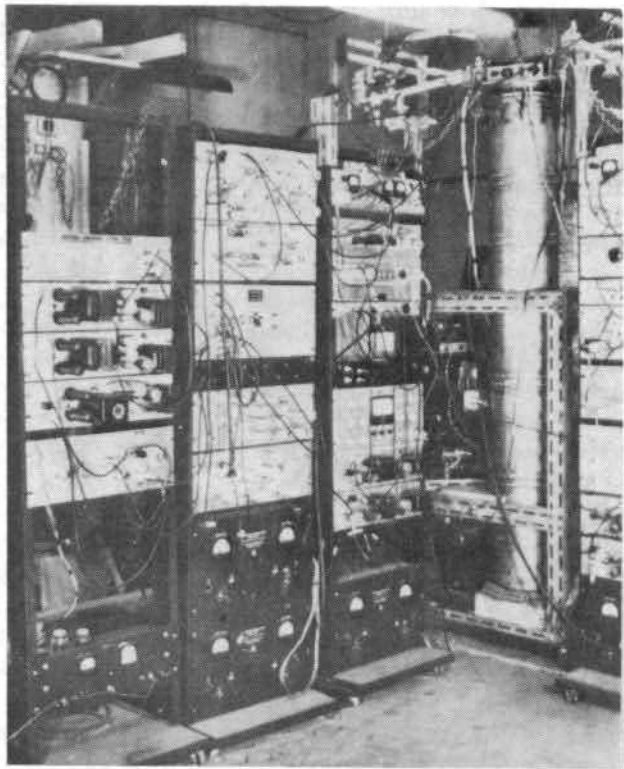
Fig. 19. *Breadboard model, 1954/1955.* The can and its innards, as also those of the MIT small clock, are on display in the "Atomic Clock" exhibit at the Smithsonian Institution's National Museum of American History: Science, Technology, and Culture.
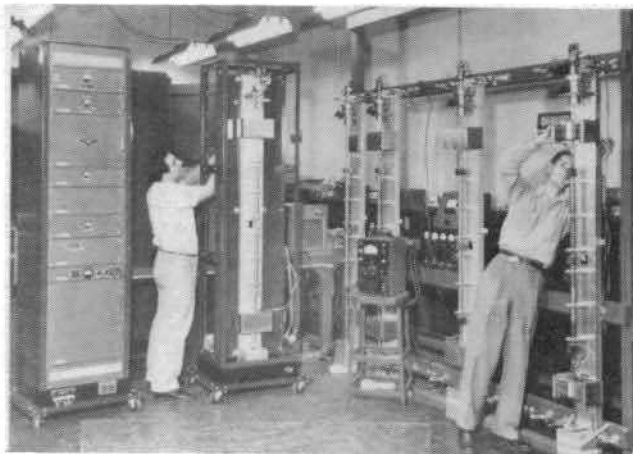
Fig. 23. *Atomichron assembly.* The photograph shows not the initial prototype series, but the subsequent assembly of the NC-1001 in Natco's Production Division.

Fig. 24. J. R. Zacharias and R. T. Daly (right) present their Atomichron® to the world at the Overseas Press Club in New York, Oct. 3, 1956.