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MICROWAVE COMPONENTS

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ABSTRACT

This paper describes the development of some of the microwave components by the R-f Group at the Radiation Laboratory MIT. The examples are drawn mainly from the author's limited experience. Among them are the introduction of "broad-banding" and "preplumbing," the problem of crystal burnout, the design of crystal mixers, and devices using the Magic-Tee. Among latter were the frequency stabilization of klystrons and a related proposal for a narrow band duplex microwave communication system. With the war's successful conclusion, much of the experience was carried over into fundamental research, with microwave spectroscopy, radio astronomy, and nuclear and electronic magnetic resonance as relatively direct beneficiaries.

This talk will describe some of the development of Microwave Components at the Radiation Laboratory, necessarily limited mainly to the area of my own experience and involvement. Initially, there was no group with full responsibility for the design of microwave components, except for magnetrons, antennas, and scanners, this last especially for the initial aircraft interception project. The interconnections of the basic elements of systems were made with coaxial transmission lines fabricated as required, with center conductors supported by polystyrene beads. The Fundamental Development Group, commited to extending the art from the original 10-cm. wavelength to 3 cm. and eventually to 1 cm. of course mainly used hollow waveguides. However, as transmitter power increased at 10-cm, the use of plastic bead supported, "R. L. Standard" 5/8" diameter coaxial lines to interconnect transmitters, antennas, and receivers ran into troubles. Electrical breakdown, especially in airborn equipment became common. It was worsened by the large standing wave ratios that could result from the bead reflections. There was a helpful algorithm designed by J. L. (Jim) Lawson for positioning beads to make their reflections cancel between beads and groups of beads. Early systems were also provided with numerous adjustable tuners and it was found difficult, even in the laboratory, to develop a procedure for setting several tuners whose effects tended to be highly interactive. Even if an overall setting seemed best for power output and receiver sensitivity it would often involve high standing wave amplitudes between tuners and so contribute to losses and breakdown. Once so damaged, the line was useless.

Although I had visited the Indicator Group in 1941, I joined the Radiation Laboratory's newly organized R-f Group 53 full time in March, 1942, headed by Jerrold Zacharias who had very recently returned from an extended visit to the Bell Telephone Laboratories. My background for working on microwave components was helped greatly by my hearing, initially as a visitor, the lectures of Dr. W. W. Hansen. Bill Hansen was a physicist with a then very rare experience in microwave technology, arising from his role in the development of Klystrons with the Varian brothers. Hansen came up from Sperry Gyroscope Company on Long Island every week for several years to present the lectures. From him I learned a lot about transmission lines, "circle diagrams," "Smith charts," waveguide modes, resonant cavities, and many other things, presented in a way not dependent on immersion into the theory of networks as employed by engineers.

At the time our R-f group was organized it was decided that future 10-cm. systems should use 7/8" diameter stub supported 50-ohm transmission lines. The rigid center conductor of such a line is supported at appropriate places by a short-circuited side stub effectively a quarter wavelength long. At its resonant frequency such a stub causes no reflection because it adds a null admittance in parallel with the main line. My first specific task was to determine experimentally the correct resonant lengths for such stubs (the line diameter being not negligible compared to the wavelength, end effects meant a sizable departure from the nominal length) for the three sub-bands, at 9.1 cm, 10.0 cm, and at 10.7 cm. The procedure would be to construct a sample stub with a shorting plug soldered at a chosen length. It would be introduced between a matched termination and a standing wave detector (slotted section) and the reflection determined for various soldered positions of the plug. I was supposed to do this for each sub-wavelength because a 10% departure from the resonant wavelength would result in a standing wave ratio of 1.2 per stub. Instead of proceeding with the task, however, I proposed a combination of resonant structures that could be so configured as to cancel their sensitivities to frequency. The broad-band stub, using a fatter center conductor, of lower characteristic impedance, a half-wavelength long centered at the stub did the job. It is a form of coupled resonator with three poles. We also de-

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signed and distributed to the systems groups a device incorporating the idea but that could be used as either a straight through stub support, or, by soldering the shorting plug in another arm, as a supported right angle bend. This was more complicated to design as there was found to be no simple length of stub that would show no reflection on a bend, so in that configuration the stub had to behave also as an impedance matching transformer. These units became known as "GOZANYWHERE" stubs. The Patent Group developed a patent application in which we were led to propose many other devices to which the "broad banding" concept could be applied, and the resulting patent application included 98 claims. Only 27 were granted (No. 2,446,982)

Pursuing the elimination of tuning devices led to studies of the characteristics of such components as magnetrons and crystal diodes to determine the statistical distribution of their r-f characteristics, in production runs, in the interest of designing devices that would yield as near optimum results as possible without tuners. For example, once a successful a TR-box (transmit-receive switch) allowed use of a single antenna, it was necessary to establish a length to the "cold" magnetron that would result in a maximum received signal, coupled into the receiver at the branch point, i.e. the "duplexer." Originally a telescoping line section, often called a "trombone" because it was folded back on itself to allow a fixed overall geometry, was introduced between the magnetron and the duplexer. However we were able to design after measuring a large number of production tubes, "preplumbed" magnetron couplers that included both a quarter wavelength transformer to present the magnetron with the optimum load impedance when oscillating and with the best phase length for the branch to the receiver when "cold."

I have already referred to crystal diodes. These were found, initially by the British, to be by far the most sensitive detectors for microwaves available at the time. The earliest had gone back 20 years to galena, PbS, and "cat's whiskers" such as many of us had experienced in our youthful introduction to radio technology. During the years of the Rad Lab there was intensive study of the properties of semi-conductors, especially of silicon and germanium, at many places, both in Britain and here at the Bell Laboratories, Westinghouse, General Electric, Sylvania, and du Pont, as well as at the Rad Lab. Purdue University and University of Pennsylvania each had contracts from the NDRC for their studies. I emphasize that the need to obtain the best behavior from the point contact crystal diodes as frequency converters (mixers) in microwave Radars led to the understanding of doped semiconductors and laid the groundwork for the whole solid state revolution in electronics with which we now live.

Crystal diodes and their survival were among our major concerns. The protection offered by the TR-box, itself a developing art, often seemed inadequate and we kept hearing that operating systems were hardly ever up to the standards of performance that we knew they should have, and burned out crystal diodes usually proved to be the cause. Especially in the face of ever increasing transmitter power, we worried about the "feed-through" of the TR when its internal gas was in the broken down state. Measurements of the leakage power did not indicate there should be a problem, when compared to the diode's tolerance. After a number of false starts, we discovered that the culprit was the "spike," i. e. the very large but brief transmission before the TR gas broke down. Recognize that 'scopes of the era had very limited bandwidth and could not display nanosecond pulses directly. I had tried back in 1941 to examine the performance of a magnetron pulser but the trace on my 'scope, a Dumont 208, looked more like a tangle of string than a pulse, presumably from cross talk between X and Y axes. Rad Lab groups developed the Synchroscope to solve that problem, but it was not nearly fast enough to display the spike. The cure for the problem came in the introduction into the TR gas cell of a small d-c gas discharge, mostly shielded from the r-f field of the TR resonator but providing enough ionization to enable a fast start of the discharge induced by the r-f. This d-c powered discharge was in turn found sometimes slow to ignite, at turn on, so a small amount of radioactive cobalt was incorporated as a "keep alive of the keep alive." Additionally, crystals were subjected at their production to the discharge of a short, charged, coaxial transmission line before they were given their r-f performance acceptance test. These discharges were of shorter time duration than the thermal time constant of the diode contact and so were described by their energy delivered per pulse. For the early crystals the energy was 0.3 ergs and later "high burnout" crystals were required to withstand 2 ergs.

One of my assignments was to design crystal mixers. This was another part of the effort to eliminate tuning parameters. The introduction of production testing of crystals in fixed mounts brought with it the narrowing of accepted r-f impedance characteristics, so we developed for the 10-cm. band a loop coupled fixed tuned coaxial line mixer, that came to be known as the "Pound Mixer." Since it was directly coupled to the resonant TR cavity, itself of high Q, a single parameter of tuning was still present and that proved to be adequate. We went on to design waveguide based mixers for the 3-cm. bands and the 1.25-cm band, based on the crystal mounts of the production tests for the 1N23 series and the 1N26 crystals, respectively. These mixers, and the duplexers with which they functioned, became more complex as they came to include separate mixers using the common local oscillator working with a magnetron sample derived from a "cut-off attenuator," to provide for automatic frequency control of the local oscillator. The signal mixer itself came to have a second local oscillator controlled in frequency through a "beacon reference cavity" to allow the Radar to be shifted to receive navigational signals from a beacon, when activated by the Radar transmitted pulses. In all these cases an important property of the design was the provision of non-resonant matched loads for the local oscillators. In the earlier versions where the local oscillator fed into a highly mismatched line, there often were inaccessible regions of frequency owing to something I learned was called by engineers the "drag-link effect."

The hybrid junction, which we called the "Magic-Tee" was introduced by R. H. Dicke, who was the first to point out that, if it was arranged to have every arm matched when the others were connected to non-reflecting terminations, all four arms behaved equivalently, splitting power equally between the adjacent two arms and coupling no power to the opposite. We discovered a way to produce this matching over a surprisingly broad band, and we designed "double balanced mixers" for 3 cm. and 1.25 cm. Each used three such Tees. The "double" referred to the inclusion of the AFC mixer and "balanced" referred to the fact that local oscillator noise was canceled by this design that used a pair of matched diodes. That became especially valuable at 1.25 cm. where the intermediate frequency, even though moved up from 30 to 70 MHz., was a small part of the signal frequency.

One memorable semi-emergency occurred when Harper Q. North of General Electric called our attention to the behaviour of his "welded contact" germanium diodes. The war had begun to look to be in its final stages and we were thinking that things then still in the laboratory could make little contribution to the war effort. However the "Battle of the Bulge" struck and the possibility that North's diode might significantly lower the "noise figure" of Radars was felt to be important. We undertook to find out how and why the crystals differed from those with which we had become familiar. We soon found that, when presented with suitable impedances on the r-f and i-f sides, a mixer could show conversion gain, rather than the usual nearly 6 db of loss. It was also found, however, that the output noise also increased under those circumstances and, overall, the result was little different from that we were obtaining with conventional diodes. I was able to show that the welded crystal could be so tuned that, in the presence of a local oscillator drive, it could reflect a larger power, at the 3-cm signal frequency, than that sent in. In other words the mixer was acting as an r-f amplifier. My colleague, H. C. Torrey found the explanation of this behaviour in the voltage sensitivity of the barrier capacitance and the smallness of the "spreading" resistance, as a result of the welding. In retrospect, one recognizes in this the discovery of the parametric amplifier. These later became important for achieving low noise in microwave receivers. At the time we often expressed a wish that we could find a way to add another contact to diodes to serve as an analog to the control grid of the triode vacuum tube, but it remained for J. Bardeen and W. H. Brattain, to achieve this and invent the transistor, first reported in 1948 (1).

One day in the fall of 1944 a lab member asked me if I had an idea of how to design a microwave frequency discriminator, such as was used to transduce f-m signals into amplitude variations for detection. I looked to the Magic-Tee as the way to solve all novel problems, and soon came up with a design, using a high-Q wavemeter cavity as the frequency sensing element. With this, I was in turn led to devise a circuit to derive an error signal sensing frequency fluctuations of an oscillator and to close a servo-control loop servo-control that became a very effective frequency stabilizer. A better system used i-f modulation, amplification and phase detection to generate the error signal, avoiding the instabilities common to d-c amplifiers. With these devices I was able to obtain surprisingly stable audio beats between pairs of klystrons, at 10 GHz, suggesting carrier purity to the order of a part in a hundred or a thousand million. Without stabilization klystrons at such high frequencies set close enough to one another in frequency to hear beats resulted in messy sounds, only briefly remaining within the audible range.

I suggested using such stabilized oscillators for narrow-band f-m duplex voice communication (2). The single oscillator served as both the transmitter as well as the local oscillator for receiving, with offsets between communicating pairs equal to the intermediate frequency. One virtue was the fact that the presence of the other station could be sensed at each, by the echoing of the operators own voice, just as one recognizes a telephone to be live, because the signal at the i-f discriminator varies identically with frequency shifts of either carrier. The frequency was modulated with excellent fidelity, via injection of the voice signal into the frequency stabilization loop.

This development, although not directly associated with on going projects at the Radiation Laboratory, became well known there. When Louis Ridenour returned from the Office of the Secretary of War, he felt it could be made the basis of a solution to a nasty problem in air traffic control at military airfields. Using directive antennas, initially aimed at a plane from information from the mapping Radar, but afterward following a given aircraft by locking onto to the aircraft signal in the receiver with the help of error signals derived from a conical scan of its own antenna on the ground, this system was planned to establish two way communication for airport traffic and to provide identification of the plane being followed by its frequency assignment. A one gigahertz band could provide at least a hundred thousand frequencies for such assignment. Ridenour came to my group and took tutelage for a few days on aspects of microwave design including the use of standing wave detectors to measure r-f impedances, prior to organizing a special new Group specifically to pursue this concept. That group made good progress until, in the late spring of 1945, it was ruled that it was not appropriate for the Radiation Laboratory, Division 14, NDRC, to undertake the development of communication systems. The project was transferred, along with some of its personnel, but not Louis, up the river to Harvard, where another division of NDRC supported the development of communication systems. There it soon died, and we suspected the NIH (not invented here) syndrome played a role. The IRE was kind enough to honor me in 1948 with the B. J. Thompson Memorial Award. That award claimed to recognize as the best paper published in the Proceedings of the IRE by an author under 30, my paper describing the frequency stabilization systems (3).

Beyond controlling the klystron frequency with a cavity, I hoped eventually to be able to tie it to a molecular or atomic spectral absorption line, and thereby to create an absolute frequency standard based on a fundamental physical property. This would be the basis of an "atomic clock." In the summer of 1945 I became especially excited by the idea of developing such a device as a step toward a post-war project to look for the relative stability of time scales based on differing physical phenomena, an issue that had some relativistic and cosmological implications. Another project took precedence, however, when E. M.Purcell brought up, with H. C.Torrey and me, while walking to lunch in Central Square, the question of the detectibility of an energy difference between the two states of spin orientation of protons, through their r-f absorption, when placed in a strong steady magnetic field and a resonant r-f magnetic field. The talk led to our joint decision to try an experiment to to detect such an effect. We were each occupied full time with writing parts of the books that were to form the 28 Volume Radiation Laboratory Series. I was able to borrow most of the necessary r-f equipment from my still intact laboratory and to have Charlie Rowe, my technician, machine the special resonant cavity for 30 MHz. There was no suitable large magnet available to us at M. I. T. Purcell was able to arrange the lone by J. C. Street of the magnet in his cosmic ray shed at Harvard, where before the war he had identified, using a large cloud chamber in the magnet, the mu meson, now called the muon (4). The magnet, constructed from the frame from a former generator of the Boston Elevated Railway (now the MBTA), had not been run for five years because Street had also been at the Radiation Laboratory, and its British Branch, since 1940. There were plenty of cobwebs to be knocked down before getting on with some modifications to the pole pieces and with efforts to calibrate that electromagnet. We worked mainly in the evenings on this "moonlight" project, intending to work at our writing commitments during the days at the Lab. In practice, I found it difficult to attend to that mission, because it was much more exciting to begin to get back to research science, as were colleagues who were departing the Lab to begin their post war careers. My stabilized oscillators were being used by others for a microwave spectroscopy project (5) in the new Research Laboratory for Electronics, established to carry on the projects enabled by the new technology from the Rad Lab, and I spent time observing and kibitzing. E. Bright Wilson, a distinguished physical chemist at Harvard, sought my help to introduce his postdoctoral assistant, Ben Dailey, to microwave techniques, as a step toward their undertaking microwave spectroscopic studies. Nevertheless, the real excitement came, when, on December 15, 1945, after failing a couple of nights before, and for a large part of that same Saturday afternoon, we succeeded in detecting the absorption of 30 Mhz power by the magnetically polarized hydrogenic protons in about a pound of paraffin wax filling our cavity (6). We had worked with many kinds of microwave resonances including cavities and such absorption lines as that of water vapor, gaseous ammonia, and oxygen and we had considerable benefit in understanding the quantitative limits set by noise sources on the sensitivities of detectors, from our Radiation Laboratory experience. Our new experiment actually was operated at 30 Mhz, the common intermediate frequency of our Radar receivers, although we designed the resonator as if it were a microwave device, although large by microwave standards. We believe our background in microwave technology played an important role in enabling our success, as well as that of our west coast competitors, Felix Bloch, W. W.Hansen, and Martin Packard several weeks later (7). We were not aware of a failed effort attempt in 1942 by C.J.Gorter (8) because scientific communications with Nazi occupied Holland had been cut off until then.

A post war field of research derived very directly from the advances during the war, and pursued especially actively by chemists, was the microwave spectroscopy of gases. Paramagnetic and ferromagnetic spin resonance studies owe their technologies largely to these same developments. Radio astronomy too, opened up from these advances. I played a part in the discovery of the hyperfine transition signal of interstellar atomic hydrogen, by E. M.Purcell and H.I. Ewen (9). The crystal mixer they used at 21 cm, I had scaled up from my 10-cm waveguide mixer described in my RL book (10).

I kept my eye out for a reference sufficiently precise in frequency to allow exploitation for a relativistic test, such as with an atomic clock. This affected my choice of applications of nuclear magnetic resonance for a while, but only in 1959, when Mossbauer's recoil free nuclear gamma ray resonance came along, did I find a way to carry out such an enterprise, the measurement of the effect of gravity on photon energy (11). Besides the purely technical accomplishments of the Radiation Laboratory, I like to assert that it revolutionized the economic and social structure of scientific research as it developed in the post war age.

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