Historical Perspectives on Microwave Field Theory

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I. Introduction

MICROWAVE FIELD THEORY furnishes the foundation for all of the microwave circuitry that made possible the great advances achieved by the microwave field. It is important to understand that microwave field theory is only a portion of electromagnetic field theory in general, and to appreciate how it is distinguished from it. In addition, the *network* formulation of microwave field theory has been fundamental to the rapid progress made by the microwave community; the formulation in network terms has been so intertwined with the development and understanding of the field concepts and behavior that the network formulation should be viewed as an integral part of microwave field theory.

The first part of this introduction explains the *point of view* indicated in the above paragraph. The second part of the introduction outlines the *scope* of the historical review presented here.

A. Point of View

Microwave field theory is a subset of electromagnetic field theory in general, and it possesses two main features that combine to distinguish it from the more general discipline. These features are:

1) it applies to structures for which the dimensions are of the order of the wavelength; *and*
2) the primary applications are to problems involving guided waves and resonances, rather than to many other problems such as general scattering in open spaces.

A very important corollary aspect is that these microwave field theory problems can be *rigorously* phrased in terms of appropriate transmission lines to represent guiding regions and lumped elements to represent the effects of geometrical discontinuities, resulting in what has been called *microwave network theory*. It is in fact this capability of phrasing microwave field problems in terms of suitable networks that has permitted the microwave field to make such rapid strides. This capability furnishes the rigorous underpinning for all of our many systematic and accurate microwave circuit designs.

The *first* of the two features mentioned above that characterize *microwave* field theory is that the *structural dimensions are of the order of the wavelength*. This stipulation follows from the practice (almost a requirement) that guiding structures are operated in the dominant mode (or sometimes the lowest mode of the orthogonal polarization). Under such conditions, the wavelength is of the order of the *cross-section* dimensions. Resonant structures, such as resonant cavities or filters, usually have elements with *lengths* which are again of the order of a wavelength.

In some other areas of electromagnetics, the dimensions are much greater than the wavelength (as in ordinary optics), or much smaller than the wavelength (for example, scattering by small particles, or the performance of low-frequency networks). For these examples, approximations may be made in the electromagnetics that greatly simplify the mathematics. In microwave field theory, no such simplifications are possible, and the full complexity of the mathematics must be faced. From this viewpoint, microwave field theory corresponds to the branch of electromagnetics that is the most challenging, but also the most interesting in terms of complicated phenomena, such as resonances, coupling effects, etc.

It is the *second* feature of microwave field theory mentioned above (the applications to *wave guidance* rather than to arbitrary scattering situations) that permits systematic simplifications and makes the electromagnetics tractable. For example, we may then take into account the geometrical regularity of the guiding structures, or the fact that the higher modes that are necessarily excited at geometrical discontinuities are generally below cutoff in the connecting waveguides, so that the discontinuity effects may be considered as lumped. In addition, the systematic formulations of microwave networks permit the reduction of field problems to transmission-line and lumped-element phrasings, and allow us to apply the *full range of network methods* to these problems.

It is also important to appreciate that since 1970 or so two new, but related, areas have emerged in the optics field: fiber optics and integrated optics. In both of these areas, the cross sections of the guiding structures are now of the same order as the wavelength, in contrast to the usual situation in optics where the dimensions are much greater than the wavelength. Interestingly, people trained in classical optics had to learn new techniques, whereas those trained in microwaves felt immediately comfortable and quickly made many contributions to these new areas. A similar situation prevailed in surface acoustic waves, where the wave types were not even electromagnetic. We may draw the very interesting and significant conclusion

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that microwave electromagnetics involves a well-developed body of techniques intended for problems involving guided waves, resonance effects, coupling effects, etc., in which the structural dimensions are of the order of the wavelength, *and* that this body of techniques can be applied not only to the microwave frequency range, for which it was developed, but also for other frequency ranges and even other wave types when the basic conditions are appropriate.

B. The Scope of this Historical Review

Even though microwave field theory is less inclusive than general electromagnetic field theory, it is still too large a topic to permit reasonably full coverage in a short presentation. This historical review therefore adopts the following approach. The development of microwave field theory is divided into two broad stages:

1) The formative years, which are summarized in Section II, and comprise the periods before and during World War II.
2) The period from the end of World War II to the present, during which the microwave field went through several more levels before reaching its present relative maturity. This period, discussed in Section III, saw the development of many new techniques, as well as the deepening of understandings and the systematization of the basic approaches.

During the formative stage, there were relatively few major figures, and a systematic brief account becomes possible. During the period after World War II, however, there were many significant contributors, so that a corresponding systematic summary becomes very difficult. The treatment in Section III, therefore, selects several of the most important developments during this period that are believed to be of general interest, but yields to a somewhat personalized account of events in many cases. Although the coverage in Section III is therefore necessarily limited in scope, and corresponds to areas in which the writer has had personal involvement, the presentation is so phrased as to indicate how the microwave field matured in certain important ways. The concluding remarks in Section IV contain some broad observations with respect to how this field has developed.

II. The Formative Years

A few basic investigations involving guided waves were conducted during the early years, mainly concentrated in the first decade of the 20th century, but the real history of microwaves, and therefore microwave field theory, begins during the decade of the 1930's. Enormous impetus was given during World War II because of the need to develop radar in a hurry, and great progress was made during that short time. By the end of World War II, the foundations for microwave field theory had already been established, although very much was accomplished after that. In this section, we view the periods up to the end of World War II as comprising *the formative years*, and we consider those periods below in three stages: before 1930, during the 1930's, and during World War II. Some of the further important accomplishments after World War II are considered in Section III.

A. Early Investigations on Guided Waves

It is customary to recognize that James Clerk Maxwell, in his original memoirs and in his *Treatise on Electricity and Magnetism*, published over 100 years ago, presented the formal foundation on which the complex edifice of electromagnetic theory was later built step by step. There were many contributors to the general features of electromagnetics even before 1900, but the history of *microwave* field theory begins around 1900.

We may start with John William Strutt, Lord Rayleigh, who succeeded Maxwell as Cavendish Professor at Cambridge. The prolific Lord Rayleigh, who seems to be first with nearly everything, made basic contributions to all sorts of topics in classical physics, including the resolving power of gratings, an explanation of why the sky is blue, a host of new results on the theory of sound, and the discovery of argon, for which he received the Nobel Prize. In microwave field theory, he is the first to discuss in detail (in 1897) the electromagnetic modes that can propagate through metallic tubes [1], and the scattering of electromagnetic waves by circular apertures and by ellipsoidal obstacles [2]. The latter work provided the foundation for the highly useful "small aperture" and "small obstacle" methods which were revived and developed further during World War II and later (see Section II-C). The former work actually contains the fundamental ideas of mode propagation and cutoff in waveguides, and needed to be rediscovered during the 1930's.

The second major figure is Arnold Sommerfeld, who made many contributions over the years to basic electromagnetics and to many other areas in physics, and whose first contribution to guided waves seems to be in 1899, on a study of the electromagnetic waves guided by a lossy cylindrical wire [3], a rather complex problem in its own right. Although he himself never received the Nobel Prize, a number of his students did, the most prominent of whom are H. A. Bethe, P. Debye, and W. Heisenberg.

Other early contributions to guided waves were by D. Hondros [4] and by D. Hondros and P. Debye [5], the latter paper being the first to contain an analysis of the modes guided by a dielectric rod. I had the privilege of taking a graduate course in chemical physics at Cornell University from Professor Debye; he was a wonderful lecturer, who clearly explained the simple parts and then slickly bypassed the difficult parts, a polished technique that I recognized only when I later went over my notes.

When G. Marconi, in 1901, first demonstrated that wireless communication between Great Britain and the United States was possible, many physicists speculated as to the possible mechanisms for such guidance around the curved surface of the earth. One mechanism was the guidance of an electromagnetic surface wave along the interface between air and an imperfect, but good, conductor. The guidance of such surface waves by a plane interface had been to some extent investigated earlier, but it was J. Zenneck [6] who recognized their relation to the radiowave propagation problem and in 1907 reexamined such surface waves in a thorough manner. There was no proof, however, that the so-called Zenneck wave could in fact be excited by a radio antenna. It was later shown conclusively that a different mechanism, reflection from the Kennelly-Heaviside layer in the ionosphere, furnished the correct explanation. That mechanism was offered in 1902 by A. E. Kennelly [7] and by O. Heaviside [8] independently; they speculated that if the outer atmosphere of the earth were electrically conducting, radiated waves could successively bounce between that outer atmosphere and the earth, and be confined to the region between them. The theory for such propagation between conducting layers was provided by G. N. Watson [9], and experimental confirmation of the existence of a conducting layer was later provided by various researchers.

In 1909, in a famous and controversial paper, Sommerfeld [10] attempted to verify the surface wave mechanism by solving rigorously the problem of radiation from a vertical electric dipole over a flat, finitely conducting earth. This paper led to what is probably the most famous, confused, and long-standing controversy in the history of electromagnetics. Part of the confusion lay in an error in sign that was not corrected until a later paper in 1926 [11]. Also complicating the analysis is the fact that the two open regions above and below the interface require four sheets in the Riemann surface representing the wavenumber plane, and that there are various ways in which the branch cuts can be chosen. After deformation of the integration path, the two branch point contributions were identified with space waves in the upper and lower regions, and a pole contribution was viewed as representing the desired surface wave. Different branch cuts taken by different writers, however, led to different surface-wave solutions. The controversy involved many prominent names, notably H. Weyl [12], and it grew particularly strong in the 1930's, leading even to an experiment performed in a boat on a lake [13].

B. The 1930's Period

The investigations described briefly in Section II-A, and other ones involving guided waves on various structures, were sometimes conducted out of curiosity and sometimes as an attempt to explain certain physical events, such as Marconi's success. They were, however, isolated solutions that did not lead further to anything practical at that time. The first *systematic* development of microwave field theory occurred during the 1930's, in connection with hollow metallic waveguides.

1) The Real Beginnings of Waveguides: The possible use of hollow waveguides for guiding electromagnetic waves was investigated independently during the early 1930's by two groups, one at the Bell Laboratories under George C. Southworth, and the other at the Massachusetts Institute of Technology under Wilmer A. Barrow. We are very fortunate that Dr. Southworth has written a book [14] detailing his personalized history of that period (and also his earlier work); it is very revealing not only with respect to technical details but also the attitudes of the time. He points out that since Marconi found that longer wavelengths were more effective for long-distance transmission, shorter waves were neglected. However, by the end of the 1920's, the ship-to-shore and transoceanic telephone projects became a practical reality, and the techniques below 25 MHz became commonplace, so "there was an urge everywhere to explore the frequencies beyond." Accordingly, in 1931, Southworth began a few "homespun experiments."

In 1920, Otto Schriever had performed experiments on guided modes on dielectric rods, verifying the earlier theoretical paper [5] by Hondros and Debye. Southworth had to rely at first on the paper by Schriever as a guide to his work, even though he concentrated on hollow metal guides, because he was unaware of Lord Rayleigh's analysis [1].

Southworth (who was one of the founders of the Microwave Theory and Techniques Society, and who was later made an Honorary Life Member of MTT-S) first built two oscillators which gave wavelengths tunable from 123–200 cm, but these wavelengths were too long for experiments on air-filled waveguides. He therefore filled his waveguides with water ($\epsilon' \approx 80$) so that the guide diameters could be reduced by a factor of about nine. The first measurements were made on water-filled copper pipes of circular cross section, and also bakelite pipes of the same cross-sectional size. Soon afterwards, he ordered some triodes from France which provided Barkhausen oscillations at wavelengths as short as 15 cm, thereby permitting measurements on air-filled pipes only five or six inches in diameter.

The Bell Laboratories were skeptical of not only the value but even the validity of his experiments. He reports that "one of the leading mathematicians of the company... had doubted its feasibility." As a result, he was ordered to "be assigned to more constructive work." Since they were slow in carrying out these orders, Southworth continued his measurements, and in the meantime the mathematician found an error in his earlier results and sent a correcting memorandum. Because of the change in the mathematician's opinion, and the success shown by Southworth in his experiments, he was eventually transferred to the Research Department.

Sallie P. Mead, one of his associates in the Research Department, had meanwhile reviewed Lord Rayleigh's analysis and had extended it to take metal attenuation losses into account. At almost the same time, Sergei Schelkunoff, then a relatively new acquisition in the Mathematics Department, came out with a similar analysis. As a byproduct, they both had discovered, in the early 1930's, that for one of these modes, later designated the TE_{01} mode, the attenuation *decreased* as the frequency is increased. This discovery led to the low-loss, circular electric mode, oversized-waveguide project undertaken after World War II by the Bell Laboratories and others around the world.

After further successful work, Southworth wanted to publish his results but his superiors were reluctant because of a fear that the work was fallacious and that "the Company might be made to appear ridiculous." It was only after it was learned that Barrow of M.I.T. was doing similar work and that he was about to publish that Southworth was given permission.

Although both Southworth and Barrow independently indicated that their investigations overlapped and that the other person was also about to publish, the Bell Laboratories material appeared in print several months earlier than Barrow's paper. It appeared as a pair of companion papers in the *Bell System Technical Journal* in April 1936, the first by Southworth [15] on general considerations and experiments, and the second by Carson, Mead, and Schelkunoff [16] on the theory. Both papers referred to Rayleigh's much earlier paper [1] as the first to present the idea of "critical frequencies," above which propagation occurs and below which there is no transmission. They also referenced the work on dielectric rods by Hondros and Debye [5].

The stress in both papers was on modes in circular hollow metal guides, for the fields, the cutoff frequencies, the guide wavelengths (expressed as relative velocity), and the attenuation constants. More briefly, they also considered cylindrical dielectric guides, for the fields and the variation of guide wavelength with frequency. Southworth's paper [15] presented the field distributions, and curves comparing theory and measurement; the theory paper [16] presented the equations and their derivations. Southworth also described some of his measuring equipment: oscillator, tunable resonator, crystal detector, wavemeter, etc.

W. L. Barrow's paper [17] was published in the *Proceedings of the IRE* in October 1936. Although Barrow treated only circular hollow metal pipes, the overlap with the Bell Labs papers [15], [16] was surprisingly large. Barrow's paper [17] contained both theory and measurement, with the information usually presented in a more useful engineering format. Some material was not included in the Bell Labs papers; for example, Barrow also discussed radiation from the open end of the guide, and excitation from a coaxial line feed.

Following the publication of these papers, and corresponding presentations at a joint meeting of the Institute of Radio Engineers and the American Physical Society, Southworth and his colleagues delivered several semi-popular talks on waveguides. The first of these was given on February 2, 1938 before the Institute of Radio Engineers in New York, and it stressed different modes of transmission and their respective cutoff frequencies. A photograph taken on that occasion appears in Fig. 1.

It is most interesting that all of the experiments and also all of the theoretical analyses performed on hollow metallic waveguides during the first half of the 1930's period involved *circular* cross sections. On the other hand, *rectangular* hollow waveguides turned out later to be much more practical and also seem so much simpler to us, because the field distributions are simpler, and the analyses involve trigonometric functions rather than Bessel functions. When was attention first paid to *rectangular* hollow metallic guides?

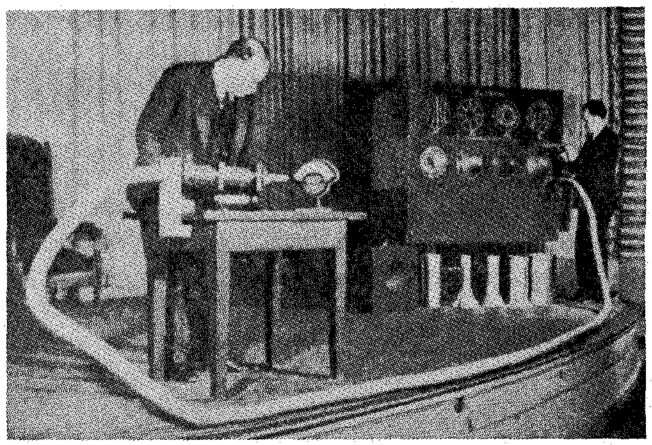

Fig. 1. Photograph of George C. Southworth (foreground) demonstrating different waveguide modes of transmission and their cutoff frequencies on February 2, 1938. This was the first demonstration of waveguides before the Institute of Radio Engineers in New York. (From [14]. Courtesy Gordon and Breach..)

It turns out that Lord Rayleigh was again the first [1]. The second seems to be L. Brillouin, who independently published an analysis [18] in 1936 that considered lossless waveguides only, as did Rayleigh [1]. The first consideration of wall attenuation in rectangular waveguides is due to Schelkunoff in 1937, as part of a larger paper [19]. Finally, a comprehensive paper [20] by L. J. Chu and W. L. Barrow appeared in 1938, again performed independently, and in fact part of Chu's doctoral thesis. Their paper contained detailed theoretical results for the attenuation properties, as well as for the fields in lossless guides, and also presented field patterns, structures for exciting various modes, and results of experiments.

During the second half of the 1930's period, a variety of additional papers on waveguides were published. Southworth published several more experimental papers, among them one [21] that described in detail his earlier experiments, including those on water-filled pipes, mentioned above. Chu published an analysis of hollow waveguides of elliptical cross section [22], and considered the transition from those of circular cross section. Actually, the first such analysis, although for lossless guides only, and incidental to other studies, was by R. C. MacLaurin [23] in 1898. Schelkunoff, who was a very active contributor during this period, published several noteworthy papers, but one [19] should be mentioned in this context since it contains general expressions for attenuation constants due to dielectric and wall losses, with application to tubes of circular and rectangular cross sections, plus field distribution pictures for several guides, including ones with triangular cross section.

During this period, confusion grew with respect to how to name these modes, particularly the distinction between what we now call TM (or E) modes and TE (or H) modes. Rayleigh [1] referred to these, respectively, as "oscillations of the first and second kind." Southworth [15] called the first type "electric wave," Carson, Mead, and Schelkunoff [16] called it "E-wave," Barrow [17] described it as "longitudinal wave," Chu and Barrow [20] selected "E wave" for it, and Schelkunoff [19], off on his own, introduced the term "transverse magnetic wave." Soon afterwards, the word "wave" was replaced by "mode" most of the time, but the designation TM or E became a matter of taste. The British systematized their choice to E-mode.

A perusal of several books written during the 1940's reveals the following usage: Stratton [24] (1941) employs E wave; Slater [25] (1942) uses TM wave; Schelkunoff [26] (1943) prefers TM-mode; Ramo and Whinnery [27] (1944) use TM wave and TM mode most of the time; Watson [28] (1947) is ambiguous, employing both TM-wave and H wave in different places; Montgomery, Dicke, and Purcell [29] (1948) are unbiased, employing both types simultaneously as E-mode (TM-mode), or sometimes one or the other; most, but not all, contributors to Ragan [30] (1948) use TM-mode; and Marcuvitz [31] (1951) systematically employs E-mode. From the last three books, which are from the M.I.T. Radiation Laboratory Series, and from other books in that series, it is clear that there was no unanimity of opinion there.

My personal preference is for E mode, partly because I feel it is more sensible to characterize the mode in terms of a component that is there rather than one which is absent, and partly because below-cutoff (or evanescent, as the British prefer) E modes and H modes contain electric and magnetic stored energy, respectively, so that, for example, a waveguide discontinuity that stores E modes will be capacitive, thus keeping the conceptions consistent and simple. Today, most people use TM mode instead of E mode; I therefore use it too, because I believe that in the interests of clarity it is better for everyone to employ the same notation, even if it is not optimum. On the other hand, we should always be grateful when a clear improvement in notation is introduced. A notable example, which few people recall today, was the introduction by H. A. Wheeler (a major contributor to the microwave field in many other ways and an MTT-S Microwave Career Award recipient in 1974) of the word "port" in the 1950's as a replacement for the cumbersome "two-terminal pair." Port is universally employed today in microwave networks.

2) The Beginnings of Microwave Network Theory: As indicated in the Introduction, microwave network theory is based on the rigorous formulation of microwave field problems in terms of transmission lines to represent the guiding regions, and lumped elements to represent the effects of junctions and other geometrical discontinuities. That formulation went through many stages, involving slow advances on the part of many contributors, in many cases with their contributions hidden in the context of the applications.

The *importance* to be attached to these developments is expressed very effectively by H. G. Booker in an important summary paper [32] (called an "integrating" paper by the British) that appeared in the issues of the *Journal of the Institution of Electrical Engineers* of Great Britain that reported the proceedings of their Radiolocation Convention in 1946:

> In the teaching of electromagnetism to engineers there appears to be a remarkable hiatus between presentation of the theory of transmission lines and that of wave propagation in general. The theory of transmission lines is normally approached from the point of view of circuit theory, which is developed in a straightforward engineering-like manner in terms of the impedance concept. The theory of more general forms of wave propagation, however, is approached, if at all, from the point of view of Maxwell's equations, and the impedance concept is often not used. The hiatus between these two methods of approach inevitably forms, for many people, the limit of their knowledge of electromagnetism. In 1941, when the technique of handling centimetre wavelengths was being rapidly developed for application to radar, this hiatus was forming an unnecessary *barrier to progress*. A paper was therefore prepared, and issued by the Telecommunications Research Establishment in secret form, with a view to bridging the gap and demonstrating that anyone who knows the theory of transmission lines is already well equipped to understand the elements of wave propagation both in the open air and in hollow pipes.

Booker's original 1941 secret report, summarizing the *impedance concept*, was published [32] after the war in "substantially its original form," even though, as he points out, those viewpoints "have since become fairly widely understood in the radar world." The paper unfortunately includes no history, but is only a teaching vehicle, summarizing the knowledge as of 1941.

The term "impedance" was first introduced by Oliver Heaviside in 1886 as the ratio of V to I in a circuit comprised only of a resistance and an inductance. It was soon afterwards extended to include capacitance, and later to complex values of Z so that phase was also taken into account. Still later, it was applied to transmission lines, developing concepts such as the distributed series impedance and shunt admittance of the line, and the propagation constant and characteristic impedance. There were also some attempts to represent wave propagation in space in terms of an intrinsic impedance of free space, but the impedance concept usually applied to the medium rather than the wave. It was also recognized that propagation in waveguides could be represented in terms of transmission lines, but the concepts were still fuzzy during the middle 1930's.

S. A. Schelkunoff is the person generally credited with extending the impedance concept to fields and waves in a systematic way. His key paper [33], in 1938, first recognizes that the impedance concept had already been generalized to some extent in mechanical systems and in acoustics, and that it could correspondingly be extended to electromagnetic waves. He then proceeded systematically along the following lines: Z is extended from circuits to radiation fields; impedance is regarded as characteristic of the field as well as the medium, so that the impedance to a plane wave in space is not the same as that to a cylindrical wave in space; direction is assigned to impedance; and the transmission line concept is generalized to encompass three-dimensional guided waves, since only the transmission direction is important.

This paper influenced the thinking of many researchers, who each contributed to a deeper understanding of these concepts, so that *the representation of guiding regions by* *transmission lines* was already quite well understood by 1941 or so. Booker, in his paper [32], also indicated that he followed Schelkunoff's approach.

The second major aspect of the microwave network formulation of microwave field theory is the *representation of waveguide discontinuities by lumped elements*. What progress was made during the 1930's along these lines?

It was understood *qualitatively* that waveguide discontinuities could be viewed in terms of lumped elements since higher modes were excited at the geometrical discontinuities which could not propagate away because they were below cutoff. It was even recognized that some lumped elements were capacitive and some inductive, depending on the discontinuity geometry and the incident mode. But it was *not* known how to evaluate the lumped elements *quantitatively*. Some small progress in the direction of quantitative calculations was begun around 1940 or so, but the real advances were made during World War II, partly in England and Canada, but primarily in the U.S.A. at the M.I.T. Radiation Laboratory. In 1943, Schelkunoff published a book [26] that was based on notes for a course, and summarized much of his research over the years. In my view, it was a tour de force; since the material presented was primarily his own work, it is remarkable how many advanced problems were solved. Towards the very end of the book, a few examples are presented on quantitative evaluations of some waveguide discontinuities, showing that Schelkunoff also undertook that challenge in the early 1940's.

The pace of Schelkunoff's activities slowed down considerably after World War II, but he still contributed. In 1954, he and I were both members of a discussion panel on "Mode and Field Problems in Non-Conventional Waveguides" (with Marcuvitz, Chu, and others); during the discussion, we had disagreed mildly on two points, one involving transverse resonance procedures and the other relating to the spectral nature of leaky waves. A few months later, we met by accident on Flatbush Avenue in Brooklyn, and he eagerly picked up the discussion, spending about an hour with me. I was highly honored, being only a young man then, but the episode also demonstrated the intensity of Schelkunoff's devotion to the field. At an URSI symposium a few years later, in 1959, I believe, it was learned that that meeting could be the last one he planned to attend; as a result, Schelkunoff received a most impressive spontaneous standing ovation.

C. The World War II Period

The development of the magnetron in Great Britain furnished a reliable source of centimeter waves and made radar feasible. It was the tremendous push to improve radar during World War II that led to striking advances in such a short time for the microwave field as a whole, and for microwave field theory in particular.

Many laboratories in the U.S.A., such as those at Harvard, Stanford, Columbia, and Brooklyn Polytechnic, contributed to some phase of the overall radar program, but the center of the activity and the most famous of these laboratories was the Radiation Laboratory at the Massachusetts Institute of Technology. Also active in radar development was the Telecommunications Research Establishment in England and McGill University in Canada. The grouping together of prominent and highly capable individuals under these special war-time circumstances produced an unusually stimulating and productive working environment.

The following story of how N. Marcuvitz was recruited for his role at the M.I.T. Radiation Laboratory is illuminating in several respects. He was then a graduate student at the Polytechnic Institute of Brooklyn, and Dr. Ernst Weber, then a Professor there, was attempting to establish a collaborative effort with the M.I.T. Radiation Laboratory. In this connection, F. W. Loomis and I. I. Rabi, Associate Directors of the Radiation Laboratory, came down on a Saturday in January 1942, a month or so after Pearl Harbor. Dr. Weber made a presentation, and Marcuvitz, his prize student, demonstrated his project on the famous Sommerfeld problem, complete with copper ground plane, antenna, and near-field measurement setup. By Sunday evening, Marcuvitz was on a train to Boston, and soon afterwards Weber received a contract. This was typical of the activity at the time: quick decisions and quick action afterwards.

At M.I.T., Marcuvitz was made responsible for precise *measurements on waveguide discontinuities*; that responsibility involved two requirements: first, the development of an accurate measurement setup, and second, the evolution of a measurement procedure that would permit the network parameters of geometrical discontinuities to be determined with great precision. With respect to the measurement setup, it was often not easy to tell whether or not power was coming through. To check for power, they often placed their cheeks next to the open end of the waveguide; if the cheek grew warm, the power was on. When the result was ambiguous, they sometimes used their eyes! They were unaware then of microwave biological hazards, but it seems no one suffered permanent harm.

Much creative attention had to be paid to the precision measurement procedure, but the method finally adopted turned out to be taken from a paper published in 1942 in Germany by A. Weissfloch [34]. That method has been called the "D versus S procedure," where S is the distance from the discontinuity output reference plane to a short circuit in the output, and D is the distance from the input standing wave minimum to the discontinuity input reference plane. It has also been referred to as the "tangent method," since the D and S values are related by tangent functions. A description of the method appears in a publication [35] and in the *Waveguide Handbook* [31, sec. 3.4]. The method was systematically used, and it furnished very precise results for many discontinuity structures.

I had wondered, since this method was clearly valuable, why it was published during the war and whether or not the Germans actually used it. I learned that after the war Weissfloch had moved to France, and that he was working near Paris in a laboratory of a French affiliate of ITT. In connection with a trip to Europe in 1956, I wrote to Weissfloch and arranged to visit him. He informed me that during the war his colleagues in Germany felt his method to be without merit and did not care whether or not he published it; he was in fact not permitted to do anything further with it on the job. He also commented that not until much later did the Germans understand the value of radar, and that they had discouraged war-time research in microwaves.

Verification of this surprising situation is given in Southworth's book [14, pp. 174 and 175], where several paragraphs are quoted from a 1959 letter to Southworth from Dr. H. Mayer, then Vice-President of Siemens and Halske. Mayer's laboratory in Germany had received, in 1943, some equipment that came from "an English plane shot down near Amsterdam, Holland." The letter continues:

> For a considerable time this piece of equipment was quite a riddle to us, especially the strange components such as waveguides, magnetrons and the like, indicating that microwaves were used. But for which purpose? At that time, microwave techniques were badly neglected in Germany. It was generally believed that it was of no use for electronic warfare, and those who wanted to do research work in this field were not allowed to do so.

Marcuvitz was in the Fundamental Development Group, headed by E. M. Purcell; others in the group included C. G. Montgomery and R. H. Dicke. Their function was to supply information to the various applications groups, who then used the network parameter results in their designs. They would usually make rough calculations based on these results and then adjust or optimize by cutting and trying. It was felt, however, that the major contribution made by these network parameter results was in creating a *way of thinking*, so that waveguide plumbing could be designed in network terms.

The Fundamental Development Group was in fact a training ground for many, since they stayed in it for awhile, absorbing the point of view, and then moved over to one of the applications groups. T. Saad, the Editor of this Special Issue and an MTT-S Honorary Life Member, was one of those individuals. Marcuvitz says that he was invited to Saad's wedding during this period, and that Saad's mother made "the best baklava he has ever tasted."

Dr. Marcuvitz is best known, of course, as an extremely able microwave field theorist, rather than an experimentalist. This transition from experimentalist to theorist was made easier because of his close association with Julian Schwinger. Soon after his arrival in Cambridge, MA, Marcuvitz, together with R. Marshak, who later became President of the City College of New York, rented a house near Harvard Square. Some of the rooms were rented to others who worked at the Radiation Laboratory, and Schwinger was one of those people. This arrangement lasted for only a year, but Marcuvitz and Schwinger became friends.

Schwinger worked during the night and slept all day. Marcuvitz would wake him up at 7:30 P.M., and they would go to dinner. After that they would often discuss their research problems until midnight, after which

Fig. 2. Photograph of Julian S. Schwinger (left) and Nathan Marcuvitz discussing M.I.T. Radiation Laboratory research problems during World War II. (Courtesy N. Marcuvitz.)

Marcuvitz would go home to bed and Schwinger would begin his work. A photograph of Schwinger and Marcuvitz on one of these occasions is shown in Fig. 2.

Schwinger was in the Theory Group, which was headed by G. Uhlenbeck. Despite the brilliance of many in that group, Schwinger's contributions stood out above the rest. The principal challenge faced by Schwinger and the others was how to quantitatively characterize waveguide discontinuities in terms of lumped elements. As indicated in the previous subsection, people already understood the lumped-element concepts in a qualitative sense, but methods had to be developed which yield numerical values as a function of the geometrical parameters. It was necessary to solve the "diffraction" problem posed when the wave incident on the discontinuity excited the higher modes in unknown proportions. Schwinger's contribution was that he established an *integral equation formulation* of the field problem, and then developed *various methods for its solution*. It was a giant step forward.

Various *approximate* methods were developed for solving the integral equations for different geometrical discontinuities. One of these methods involves techniques for manipulating the static kernel rather than the dynamic one, based on the recognition that for almost all the higher modes the cutoff frequencies are much larger than the operating frequency, so that for those higher modes the operating frequency can be set to zero. A second method, termed the equivalent static method, employs the same static kernel but then views the problem as an electrostatic one that can be solved by a conformal mapping to a simpler geometry. Another procedure recasts the integral equation into a variational form, from which the normalized susceptance is obtained via a judicious field assumption and appropriate integrations. In addition, more accurate results were obtained in some cases by finding the field first by means of one of the first two methods and then inserting that field into a variational expression.

One morning sometime later, Marcuvitz arrived at his desk and found on it a note from Schwinger which read "A

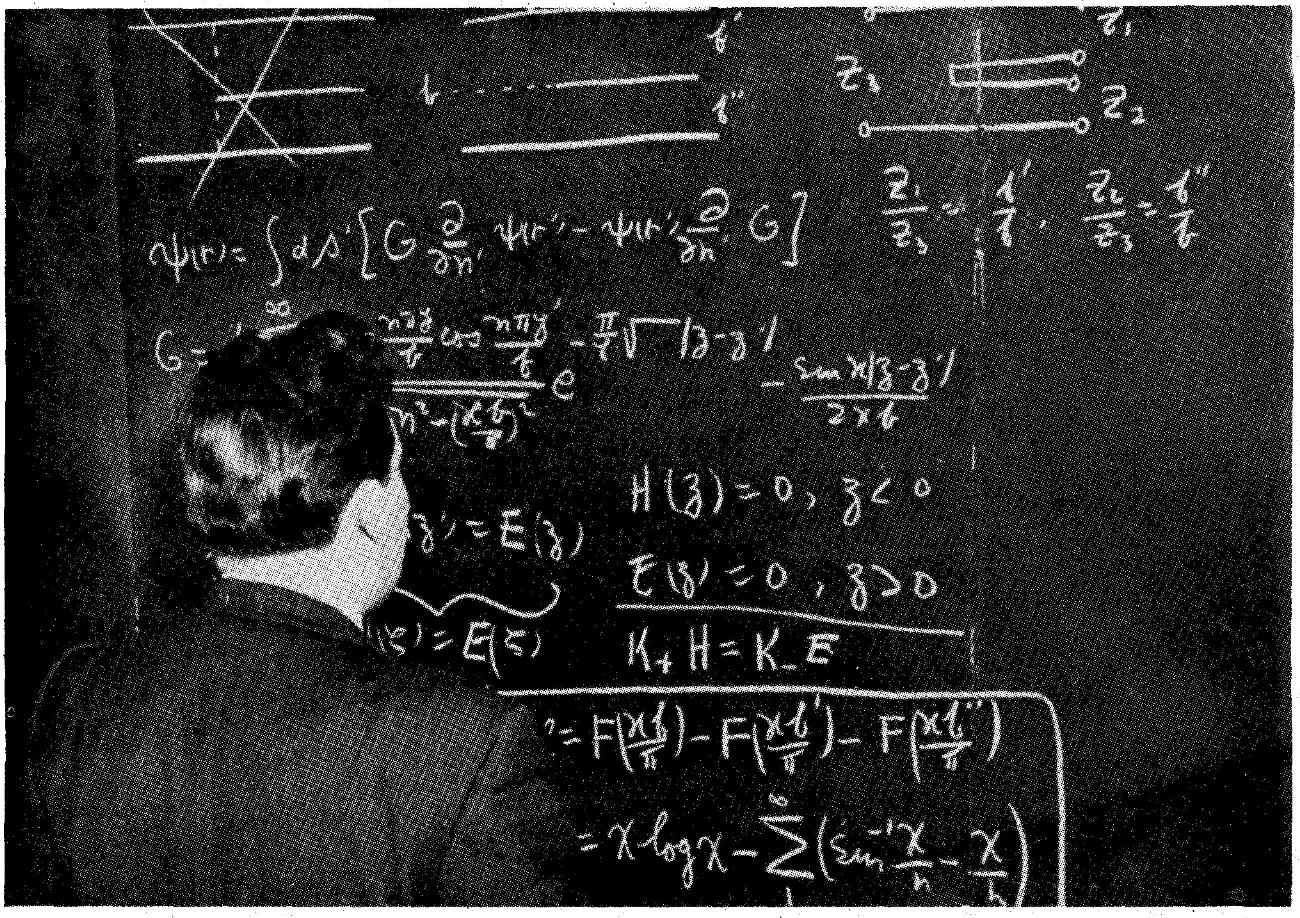

Fig. 3. Photograph of J. S. Schwinger at the blackboard during one of his lectures at the M.I.T. Radiation Laboratory during World War II. (From the official M.I.T. Radiation Laboratory Book, *Five Years*.)

new era has dawned." While working during the previous night, Schwinger had realized that certain types of discontinuity could be solved using the *Wiener-Hopf technique*, which provided *exact* solutions for those geometries. The previous solutions, while accurate, were only approximate. Here was a breakthrough of another type. Weiner-Hopf solutions for different structures were first obtained by Schwinger and later by Carlson, and then by Heins.

In order to interact with others in the group, Schwinger reluctantly got up a little earlier and presented seminar lectures during the afternoon. Figs. 3 and 4 show photographs of Schwinger at the blackboard. Notes on these lectures were taken by D. S. Saxon [36], who later became President of the University of California. These notes became famous, and were reprinted or copied privately by various groups.

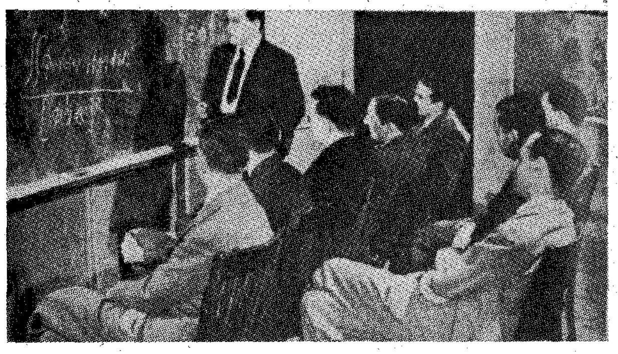

Fig. 4. Photograph of J. S. Schwinger and part of the group that attended his lectures at the M.I.T. Radiation Laboratory during World War II. (From the official M.I.T. Radiation Laboratory Book, *Five Years*.)

The theoretical results derived by Schwinger and others in that group, together with experimental data taken by Marcuvitz' group on structures for which no theory was available, were systematically arranged and edited by Marcuvitz and published as the *Waveguide Handbook* [31], volume 10 of the M.I.T. Radiation Laboratory Series. It was characteristic of this period that everything was done so rapidly that no one paid attention to who did what first; they were not concerned with publication but with getting the task done. As a result, many people have never been properly credited for their contributions. Of course, Schwinger's contributions were so outstanding that he was the exception. He received a Nobel Prize later for work on quantum electrodynamics, but, considering the enormous impact his contributions to waveguide discontinuities made on microwave field theory, he would certainly have deserved such a prize if Nobel Prizes were awarded in this field.

Another very useful approximate technique for certain classes of waveguide discontinuities is *small aperture theory*, and its dual, small obstacle theory. Although the initial ideas were outlined by Lord Rayleigh [2] in 1897, H. A. Bethe [37] revived and generalized them, in effect introducing another method. The approach is based on the recognition that a wave incident on a small hole in a conducting wall produces a field in the hole equivalent to the sum of an electric and a magnetic dipole, the polarizabilities of which are given to a good approximation by electrostatic expressions if the hole diameter is small relative to wavelength and the hole is not near the waveguide wall. The great virtue of this approach is that very simple closed-form expressions can be obtained quickly. Later in the war period, Bethe left to head up the theoretical group at Los Alamos working on the atomic bomb, and still later he received the Nobel Prize for some earlier work on physical processes in the sun. I had the privilege of taking a course at Cornell University from Professor Bethe. He was a fine teacher, but he had one peculiar characteristic. He would walk backwards towards the door as he ended his lecture, so that after he completed his last sentence he disappeared.

After the war, Marcuvitz returned to the Polytechnic Institute of Brooklyn to complete his doctorate, and his thesis dealt with a reformulation of small aperture theory [38]. He showed that it was derivable from originally rigorous expressions, and he rephrased the results in practical network terms, thereby eliminating the need for the integrations appearing in Bethe's formulation if normalized mode functions are employed. Before issuing the *Waveguide Handbook* [31], Marcuvitz derived analytical expressions for a large number of waveguide discontinuities that were not considered at the Radiation Laboratory, in order to produce a more useful final volume. All of these additional derivations were based on small aperture or small obstacle theory; in fact, almost one-third of the solutions appearing in the *Waveguide Handbook* were obtained using this theory.

Others also contributed to the utility of this method after the war. For example, S. B. Cohn (an MTT-S Honorary Life Member and an MTT-S Microwave Career Award recipient in 1979) performed careful electrolytic tank *measurements* [39] to obtain the electrostatic polarizabilities for aperture shapes for which no theory was available. I extended Marcuvitz' results to apply to *longitudinal* small obstacles [40]; previously, only transverse obstacles had been treated. L. B. Felsen and W. K. Kahn [41] generalized the procedure further to cover *multimode* situations. Recently, there has been a revival of this approach. Several people, principally R. F. Harrington [42], have extended it to apply to thick walls, to conductance expressions, and to coupling to cavities.

During the war period, contributions to the quantitative description of waveguide discontinuities were also made by others not affiliated with the Radiation Laboratory. In the U.S.A., for example, analyses for discontinuities in parallel plate guide [43] and in coaxial line [44] were performed by J. R. Whinnery and his colleagues. Dr. Whinnery coauthored an excellent and widely used book [27] in this field, was an MTT-S Microwave Career Award recipient in 1976, and has been for many years a Professor at the University of California at Berkeley. In England, at the Telecommunications Research Establishment, G. G. MacFarlane developed a quasi-stationary field approach for a variety of discontinuities, such as capacitive and inductive irises and strips, and periodic strip gratings. The work was conducted in 1942, but published [45] only in 1946. As a third example, a group at McGill University in Canada under W. H. Watson [28] also worked on such problems, but concentrated on resonant slots. Had it not been for Schwinger's more extensive, more systematic and more accurate contributions, these other results, which were certainly significant, would have been better recognized.

In connection with solutions for waveguide discontinuities, note should be made of a book [46] by L. Lewin published in 1951. Lewin was aware of work done by others, including Schwinger [36] and MacFarlane [45], but, working alone at the Standard Telecommunications Laboratories, Ltd., in England, he rederived results for many of these discontinuities in somewhat different ways, employing different modal separations, and he obtained slightly different final solutions. He also presented solutions for new structures, such as the tuned post and tuned window. The book is also unusual in that the solutions are presented in great detail. Lewin himself is unusual in that he feels at home with both practical details (he once described to me his novel idea for winding a helix), and abstruse mathematics (he published a book on dilogarithms); he is presently a Professor of Electrical Engineering at the University of Colorado.

Lastly, some comments should be made on similar investigations in the Soviet Union. I do not know whether the work was performed during World War II or immediately afterwards, but papers were published shortly after the war. The most noteworthy contributions on waveguide discontinuities were made by L. A. Weinstein (or Wainstein, or Vainshteyn—different spellings have appeared, but the last one shown indicates how to pronounce his name), who applied the Wiener-Hopf technique to obtain rigorous solutions. He went beyond the Schwinger group in the sense of considering different incident modes and also multimode situations. His original papers in 1948, for example [47], [48], were in Russian, but a translation of his book into English was available later [49].

I met Weinstein, a serious, self-confident, and very knowledgeable person, twice during the 1970's (in the U.S.S.R), but my first contact was at an URSI General Assembly in Boulder, CO, in 1957. He was chosen as a show-piece young scientist by the Russian delegation, and he presented his thorough study of group velocity in dispersive media, a most complicated topic but one which A. Sommerfeld and L. Brillouin had treated in detail in 1914, although Weinstein did not know it. When he finished, B. Van der Pol boomed out, "This problem was solved years ago by Sommerfeld and Brillouin." Taken aback and not hearing completely, Weinstein responded meekly, "I am not familiar with the American literature"; in response, Van der Pol, who was Dutch, said triumphantly, "But Sommerfeld is German and Brillouin is French." I felt sorry for Weinstein; he had duplicated an already solved problem, but it was clear that he was talented.

III. The Maturing Years

It would be almost impossible to write in this issue a balanced and reasonably complete description of the developments in microwave field theory since World War II. For one thing, there have been so many fine investigators that to include their main contributions in a balanced fashion would require much more than the space presently available. In addition, the field has moved in a variety of important directions, creating a scope too great for a single paper. It has been necessary, therefore, to limit this section to a few areas that I believe represent some key developments in the history of microwave field theory. Even within those few areas, I have chosen to stress what was within my personal knowledge so that the information presented would be reliable. As a result, my own work and that of my colleagues may absorb a disproportionate amount of the coverage, but I believe that they are presented in a proper historical context. This somewhat personalized account should therefore serve to impart a flavor of how the microwave field has developed since World War II in certain important ways.

A. The Microwave Research Institute Shortly After World War II

Many of the contributors to microwave field theory at the M.I.T. Radiation Laboratory were physicists before the war, and most of them went back to physics research after it. It is interesting to speculate on whether or not the course of microwaves would have been altered if they had continued in microwave field theory, because many of these individuals contributed very significantly to topics in physics. Several of them, such as Schwinger and Purcell, received Nobel Prizes for their later work (Bethe and Rabi received them for earlier work).

Notable among those who returned to Electrical Engineering departments at universities was N. Marcuvitz, who became an Assistant Professor at the Polytechnic Institute of Brooklyn. Although Dr. E. Weber had earlier lost Marcuvitz to the Radiation Laboratory, the contract he received furnished the basis for the establishment in 1942 of the later world-famous Microwave Research Institute (M.R.I.) at Brooklyn Polytechnic (since 1973 the Polytechnic Institute of New York). Dr. Weber (who later became President of Brooklyn Polytechnic and the first President of the merged IEEE, and who received many honors including the MTT-S Microwave Career Award in 1977) was its Director for more than a dozen years, being succeeded in that position by Dr. Marcuvitz. During that period, Weber and Marcuvitz provided the leadership in the areas of microwave components and microwave field theory, respectively, raising M.R.I. to a position of world-wide prominence in both areas. In the process, the Polytechnic Research and Development, Inc. (P.R.D.) was established as a commercial spin-off, for a time owned by the school and later sold to industry.

M.R.I. had established, for a time, the reputation of being perhaps the most prominent university activity in microwave field theory in the world. For many years, it attracted post-doctoral researchers from around the world to spend a year or more, coming from such countries as Japan, France, U.S.S.R., Israel, Italy, England, Denmark, Sweden, Hungary, Poland, and Finland. Many of those researchers have since became famous in their own right. M.R.I. was also well known for its series of annual symposia on topics in the forefront of the electronics field, and for the symposium proceedings volumes, 24 in all, that accompanied them.

Not only did M.R.I. produce much important research in microwave field theory and on basic microwave components, but it also trained a whole generation of microwave engineers. The journal, *MicroWaves*, in an interview with many microwave engineers in 1968, asked them various questions, including from what school they received their microwave education. One of the article's conclusions was that more microwave engineers graduated from Brooklyn Polytechnic than from any other school, and that the second was M.I.T., with only half as many microwave graduates.

The most important contribution made by M.R.I. to microwave field theory, in my opinion, is the rigorous, systematic *reformulation of field theory in microwave network terms*, leading to a solid body of microwave network methods. These microwave network approaches greatly simplified both the setting up and the solving of many microwave field problems, and they helped to make the problems more transparent, thus also providing enhanced physical insight. The major credit for the systematic development of this reformulation into network terms goes to Marcuvitz, who pursued that goal with enormous zeal and effectiveness. Of course, much had already been accomplished along these lines by others, at the Radiation Laboratory and elsewhere. Many others were already using rigorous microwave networks by the end of the war or soon afterwards. The difference here is that Marcuvitz wanted to introduce greater simplification and systematization and to greatly extend the range of usefulness. He was seldom satisfied with the first phrasing; often the problem would go through three or four variations until the simplest version was achieved. After a few years, a methodology evolved which was systematic and hopefully close to optimized, and we have put it to extensive use in subsequent research problems.

For about a decade, we held a weekly seminar at which every applicable field problem was examined in these network terms. The organizer and prime mover was Marcuvitz, who presented most of the seminars himself, especially at the beginning, when he taught us what was known at the Radiation Laboratory. I joined M.R.I. in 1946, just when all this began, so that I was fortunate enough to have experienced it all.

Unfortunately, very little of all this was published. One basic paper [50], which Marcuvitz coauthored with Schwinger, appeared in 1951; it incorporated many of the simplifications in the phrasing employed, but it was compactly presented. Marcuvitz and Schwinger had agreed to coauthor a book with the title, *Theory of Guided Waves*, but it was never written. Marcuvitz and I had agreed to write a book called *Microwave Network Theory*; several chapters were written and some parts of them were issued as reports, but unfortunately the rest was never completed. Another book, planned as a sequel to the others, did actually reach a publisher, but many years later (1973) and in a rather different form from that originally intended. That book [51], coauthored by L. B. Felsen and N. Marcuvitz, covers an enormous number of topics in a highly compressed form. On the other hand, these methods were included in the many graduate courses we presented in electromagnetics generally and microwaves more specifically. Many students took these courses over many years, so that the methodology was widely disseminated. As a result, these network methods have subsequently appeared in many research papers, by us and by many others.

The research project on which we worked from 1946 into the early 1950's was concerned with *equivalent circuits for slots* located in various positions in rectangular waveguide. Marcuvitz was the principal investigator for the first three or four years, until he began to spend much of his time with the N.Y.U. Courant Institute; I then took over that function. The others who worked on that contract (which had the low number AF19(122)-3) and its successors, included H. M. Altschuler, J. Blass, L. B. Felsen, H. Kurss, and A. Laemmel. The investigations were both experimental and theoretical. In the experimental portion, we were in part concerned with developing improved methods for precision measurements of equivalent circuits; in that connection, we published diligently, and about a dozen papers emerged, written by Altschuler, Felsen, and me, either alone or as coauthors. During that time, G. Deschamps, at the University of Illinois, published a different precision measurement method, based on a scattering matrix approach in contrast to our approaches based on impedances.

On the theoretical part of the program, on which most of the effort was concentrated, we employed variational methods primarily, and all of us contributed. We derived closed-form expressions for the equivalent circuit parameters of various slots, and we found very good agreement with measurements generally; in those days computers were not yet available, so that theoretical results not expressed in simple closed form were considered not useful. We derived expressions and performed measurements on rectangular slots, resonant and nonresonant, located at the end of rectangular guide, transverse inside the guide, coupling E-plane tees, coupling H-plane tees, and radiating from the top or side of the guide. The study was comprehensive and was summarized in two large (no longer available) reports [52], [53]. We were aware of and took into account the work of others, such as W. H. Watson [28], A. F. Stevenson [54], L. Lewin [46], and V. Rumsey and his group at Ohio State University, who were conducting competitive analyses.

Again, unfortunately, very little of this ever got published. It seemed there were too many of us, and by the time we agreed some had left. One of the few publications was one by me [55], on radiating series slots in the top wall of rectangular waveguide. That paper resulted from later consulting activities with the Hughes Aircraft Company, and served to explain theoretically some of their measurements. Building on the theoretical results obtained earlier by our group, I used variational approaches to extend the earlier work of Stevenson [54] by including reactive effects and considering off-resonance behavior as well; the theory yielded excellent agreement with various measurements both at and away from resonance.

By the early 1950's, the group broke up. Altschuler went later to the National Bureau of Standards in Boulder, Blass went into industry, Felsen worked on high-frequency scattering problems, Kurss joined the mathematics faculty at Adelphi, Laemmel went into communication theory, and I began to work on strip transmission-line problems and on surface waves. Sometime later, Marcuvitz got more into administration; after a few years as Director of M.R.I., he became Dean of Research and then Vice-President of Research at the Polytechnic, and he spent a year at the Pentagon as Assistant Director (Research) in ODDR&E.

B. Strip Transmission Line and Microstrip Line

1) The Competition Between Strip Line and Microstrip: In the years immediately after World War II, rectangular waveguide became the dominant waveguide structure largely because good components could be designed using it. By 1950, however, people sought components that could provide greater bandwidth, and they therefore examined other waveguides. Ridge waveguide offered a step in that direction, and a neat theoretical paper by S. B. Cohn appeared on it in 1947 [56], but it was not the answer. Coaxial line would have been very suitable, since it possessed a dominant mode with zero cutoff frequency, thereby yielding two important virtues: a very wide bandwidth, and the capability of miniaturization. The lack of a longitudinal component of field, however, made it more difficult to create components using it, although various novel suggestions were put forth. In addition, those components would be expensive to fabricate.

In an attempt to overcome these fabrication difficulties, the center conductor of coaxial line was flattened into a strip and the outer conductor was altered into a rectangular box. Components with such interiors were then fitted with connectors for use with regular coaxial line. After a few years, many excellent components became commercially available employing this approach.

At about the same time, others took a much bolder step; they removed the side walls altogether, and extended the top and bottom walls sideways. The result was called *strip transmission line*, or stripline. Different methods were used by different companies to support the center strip, but in all cases the region between the two outer plates was filled

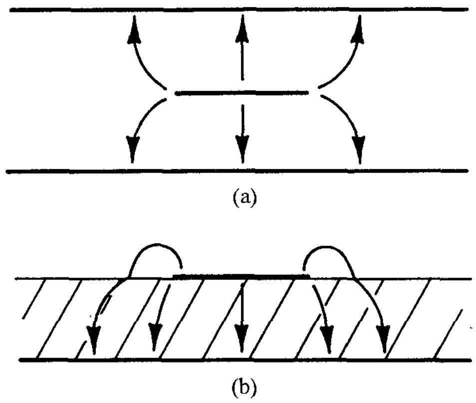

Fig. 5. Cross sections of (a) strip line and (b) microstrip line, showing the basic electric field lines.

(or effectively filled) with only a single medium, either dielectric material or air. A modification that emerged at roughly the same time involved removing the top plate also, leaving only the strip and the bottom plate, with a dielectric layer between them to support the strip. That structure was termed *microstrip*. The two different structures are illustrated in Fig. 5.

The inventor of the stripline concept is R. M. Barrett of the Air Force Cambridge Research Center (now called RADC, Hanscom Field). He was also the prime mover in its development, not simply furnishing contract money but also encouraging the various researchers in different laboratories to exchange information. Among the organizations he supported were the Polytechnic Institute of Brooklyn, Tufts College, and the Airborne Instruments Laboratory Inc. (now AIL). I interacted very closely with both of these organizations, particularly with W. E. Fromm and E. G. Fubini of AIL. Barrett also wrote a popular article [57] early on, in 1952, to encourage interest in this new type of waveguide. He stressed the simplicity of the structure, its printed circuit nature, and its many other virtues, including the fact that circuits based on stripline could be trimmed by applying a razor blade to the center strip. He also wrote a short historical survey of work on these lines as of 1954 [58]; in recognition of his vital contributions to the early stages of microwave printed circuits, however, he has been invited to present a more detailed history of these developments in this current issue.

Shortly after the appearance of Barrett's article [57], a group of engineers from the Federal Telecommunications Laboratories of ITT presented a series of three papers [59]–[61] on microstrip, the competing printed circuit line. They presented the concept, an approximate theory, and various components. The basic point of view and the intended virtues were similar to those propounded by Barrett; they differed in their choice of waveguiding structure.

Progress on stripline and microstrip proceeded so rapidly that a full-scale symposium on Microwave Strip Circuits was held in October, 1954, at Tufts College under the sponsorship of AFCRC. At this symposium, the Proceedings of which were published as a special issue of the IRE TRANSACTIONS ON MICROWAVE THEORY AND TECHNIQUES, in March 1955 [62], the extensive developments on microstrip were summarized in a paper by M. Arditi [63], but it became clear that only ITT was working on microstrip and that everyone else was using strip transmission line.

There were good technical reasons for the preference for strip transmission line. Because the region between the two outer plates of strip transmission line contains only a single medium, the phase velocity and the characteristic impedance of the dominant (TEM) mode do not vary with frequency; because of the symmetry of the structure, all discontinuity elements in the plane of the center strip are purely reactive. In contrast, the two-media nature of microstrip causes its dominant mode to be hybrid, not TEM, with the result that the phase velocity, characteristic impedance, and field variation in the guide cross section all become mildly frequency dependent. Because of the symmetry unbalance in microstrip, all discontinuity elements possess some resistive content and therefore radiate to some extent.

AIL stressed this last-mentioned point by calling their strip transmission line "High-Q Stripline." ITT became sensitized on this issue, particularly after Dr. Fubini made some sharp comments during the presentation of a paper. After that talk, Dr. A. Clavier, a Vice-President of ITT and a former Honorary Life Member of MTT-S, confronted Fubini on this matter; Fubini, who became an Assistant Secretary of Defense a decade later, backed off because it was not his intention to offend, and offered to modify AIL's nomenclature. By that time, however, the symmetrical form of stripline was the clear winner, and microstrip stayed in the background for another decade.

Sometime around the mid-1960's, microstrip began to appear again, but in a modified form. In the new microstrip, *the cross section was reduced substantially*, stressing the concept of "micro." This step greatly reduced both the reactive and the resistive aspects of discontinuity elements, thereby removing one of the prime objections to the original microstrip circuits. The resulting miniaturization also offered a more compact circuitry and stimulated the imagination in the direction of more elaborate microwave integrated circuits.

Today, microstrip has become the dominant waveguide type, and the symmetrical form of stripline plays a subsidiary role. It is indeed ironic that the roles have reversed so completely.

2) Theoretical Research on Strip Line:

The first theoretical challenge faced by researchers during the early 1950's in connection with the symmetrical form of strip transmission line involved the *characteristic impedance* of the dominant mode. (Many investigators worked on this problem; a summary of the principal contributions appears in one of my papers [64].) Early on, it was recognized that conformal mapping could yield a rigorous answer because the mode was TEM. The solution involved the ratio of elliptic functions, however, and in those days computers were not yet available. It was desirable, therefore, to have at one's disposal a simple formula (or formulas) that would permit an easy calculation of the characteristic impedance over the complete range of dimensional parameters.

I came up with a simple result of that type in which a wide strip was approximated by accounting for the fringing fields at the sides, and a narrow strip by choosing an equivalent circular rod. The two solutions overlapped to better than 1 percent at some intermediate geometry. Soon afterwards, I received a telephone call from W. E. Fromm of AIL (who later became President of AIL, and then Vice President of Eaton Corp.), informing me that he had just received a letter from S. B. Cohn, then at the Stanford Research Institute. The letter contained a simple result for the characteristic impedance, together with numerical curves. From the description, the solution was exactly the same as mine. I then recited some of my numerical values and Fromm indicated that they were identical with Cohn's. I contacted Cohn and proposed joint publication, but it was too late: he had already submitted a paper [65] to the IRE. (Over the years, my research results crossed with those of Cohn several times, including two patents on which we learned later that I happened to have had the priority.) Subsequent to that, Cohn proceeded to calculate the attenuation constant for strip line, but I concentrated on deriving equivalent circuits for *discontinuities*, stimulated by measurements being taken then at AIL, and encouraged by R. M. Barrett of AFCRC.

Although it was widely understood that reactive effects were associated with junctions and other discontinuities in rectangular waveguide or coaxial line, most designs in strip line at that time completely ignored such reactive effects. Some people did not realize that these effects could be important, and others simply did not know how to characterize them. With the aim of characterizing some common stripline discontinuities and assessing their significance, the group at AIL took careful measurements on two discontinuity elements in the center strip: a gap and a round hole. The group at Tufts College took measurements on other discontinuities, such as bends.

Realizing that a Green's function analysis of these discontinuity elements would present formidable difficulties, I concentrated instead on possible *approximate* approaches. I began with stored power considerations combined with an approximate model of the strip line, in which the fringing fields at the sides were compensated by extending the strip width and placing magnetic walls at the sides. That approach yielded good results for several discontinuities, but an even more successful approximate method involved an application of Babinet equivalences. By taking the Babinet dual of the approximate model mentioned above, the resulting structure became a parallel plate line of finite width; it was then possible to obtain the equivalent circuits for several additional discontinuities by taking the appropriate duals of existing solutions for corresponding discontinuities in parallel plate guide. By using these two methods in appropriate contexts, I derived simple expressions for the equivalent network parameters of a large variety of discontinuities in the center conductor of stripline, including gaps, holes, bends, changes in width, and tee junctions. Very good agreement was found with the measured results obtained by AIL and Tufts College.

These approximate approaches and network expressions, together with some comparisons with measurements, were presented at the Tufts College symposium mentioned above; a paper was then published in the IEEE TRANSACTIONS ON MICROWAVE THEORY AND TECHNIQUES [66] in 1955. Shortly afterwards, H. M. Altschuler and I constructed a very precise measurement setup in strip line to determine experimentally the parameters of additional strip line discontinuities. Simultaneously, we derived theoretical expressions for some additional discontinuities and we improved the parameters for some of the existing ones. Those theoretical and measured results were summarized in a paper in 1960 [67].

During the late 1950's, a group at the Stanford Research Institute (S.R.I.), headed by S. B. Cohn (our research paths crossed again here), took a variety of measurements on strip line tee junctions [68]. In the process, they presented an equivalent network for the tee junction that was preferable to ours [66], [67]. As an outgrowth of consulting work I performed for IBM, a detailed study was conducted with A. G. Franco on strip line tee junctions, where additional careful measurements were taken at IBM and comparisons were made with all other available data. We selected the S.R.I. network form [68] as the recommended one, and showed how available theoretical results could be recast to apply to the S.R.I. form. That study was summarized in a paper in 1962 [69].

I was awarded the Microwave Prize for the 1955 paper [66]. It is also my understanding that the equivalent circuit results presented there were widely applied in strip line circuit designs, and that the results were quoted in various books and reports. Since no one else derived comparable theoretical expressions, the only alternative would have been to compensate empirically for the reactive effects of the discontinuities.

After the rebirth of microstrip, these equivalent circuit expressions were applied to microstrip discontinuities, but with only limited success. It became necessary to derive comparable new expressions, but the task was more difficult for the microstrip geometry. I did not participate in those efforts, but the earlier investigations, in the late 1960's and early 1970's, made use of a modified approximate model for the line, and employed some of the network forms I introduced. A large number of people became involved in those investigations, and substantial success has been achieved. Because of the more involved microstrip geometry, however, many of the results are available only in the form of numerical curves.

C. Surface Waves and Leaky Waves

Most waveguide studies in the microwave field have been concerned with closed waveguides, or with modes guided principally by conductors even when the guides were not closed (for example, strip line). On the other hand, it was known from the early days that dielectric structures could also guide waves, and that the fields of those waves extended partly out into space. Those fields became associated in the minds of researchers with the air–dielectric interfaces, and the waves, therefore, became known as *surface waves*.

Surface waves were understood to propagate along these interfaces *without radiation*, a point made very clearly by H. M. Barlow and J. Brown in their excellent book on surface waves [70]. Another type of wave is the *leaky wave*, which is indeed involved with radiation, and which sometimes is linked with surface waves as a general category. These two wave types have much in common, but they differ sufficiently, particularly with regard to their history, that we shall treat them separately here. The history of leaky waves is particularly intriguing, in part because in its early phase people actually questioned whether or not these waves really exist, and in part because they were later found to play a key role in a variety of different physical phenomena, sometimes in quite unexpected ways.

1) Surface Waves:

The history of surface waves guided by dielectric structures or by interfaces between air and an imperfectly conducting medium goes a long way back. In Section II-A, it was pointed out that wave guidance by a dielectric rod was investigated in 1910 by Hondros and Debye [5], and that the studies by Zenneck [6] in 1907 and by Sommerfeld [10] in 1909 on surface waves supported by a slightly lossy earth formed an attempt to explain Marconi's experiments.

After World War II, attention was again paid to surface waves, partly because dielectric rods were being used as surface-wave antennas, but primarily because they were interesting and different. The renewed attention prompted the invention by G. Goubau of a new type of waveguide, and that accomplishment in turn stimulated even wider interest in surface waves and their properties.

The most extensive activity on this broad topic was carried out in England, principally by H. M. Barlow and his students, most of whom later became prominent themselves. Barlow was the dominant university figure in the microwave field in England, creating a center at University College London that became world famous. He was recognized for these activities in many ways, including election as a Fellow of the Royal Society and as a foreign member of the U.S. National Academy of Engineering. His three principal students in surface-wave studies were J. Brown, A. L. Cullen, and A. E. Karbowiak; Brown was Head of the Electrical Engineering Department at Imperial College for many years, became President of the IEE, and is now Technical Director of Marconi, Ltd.; Cullen succeeded Barlow as Department Head at University College, and was also elected an F.R.S.; Karbowiak went to industry for a time and then became a Professor and Department Head at the University of New South Wales, Australia.

The contributions of Barlow and his group spanned a wide range of topics involving surface waves. An important paper by Barlow and Cullen [71] covered in detail the basic properties of these waves. A surprising result on the excitation of surface waves was found by Cullen [72]; he showed that a source located some distance above the surface, rather than at the surface, offered optimum excitation efficiency because of cancellation effects between the direct and reflected space waves. Barlow and Brown published a major book, called *Radio Surface Waves* [70], and Brown issued a pioneering paper [73] on wave types near interfaces. Barlow and Karbowiak published experimental studies on cylindrical surface waveguides that were corrugated [74] and had capacitive surfaces [75]. Karbowiak wrote many theoretical papers as well, including ones on stratified surface waveguides [76] and guides with complex wall impedance [77].

In the U.S.A., G. Goubau placed a dielectric coating on a metal wire, thereby producing a reactive surface on the wire which then supported a surface wave. The reactive surface did not require the metal wire to be lossy, as did Sommerfeld's solution [3] for a bare wire; furthermore, the evanescent field outside the dielectric sheath did not extend out as far, and its extent could be controlled by the dielectric thickness. Goubau published several papers on this work [78], [79], and his colleagues termed his new guide the *Goubau line*.

The U.S. Army at Fort Monmouth, where Goubau worked, had high hopes that Goubau's dielectric-coated metal wire could furnish a simple single-conductor transmission line that was inexpensive and flexible. There was also speculation that the Goubau line could be strung on telephone poles and be used commercially in community-TV applications.

Alas, the extension of the field transversely into the air region outside of the dielectric coating rendered the line impractical. Careful experiments showed that the attenuation of the line increased substantially during exposure to rain or snow; in addition, initially puzzling sudden and erratic changes in input VSWR were finally traced to the presence of birds sitting on the line and then suddenly leaving it.

It is interesting that the same basic concept is present in the optical fiber, which became practical about two decades later. In the step-index fiber, for example, the central dielectric core of higher refractive index corresponds to the wire plus its dielectric coating, but the transversely evanescent field outside is trapped in the lower-index cladding instead of being exposed to the air. The total field is thus protected from the external environment in the case of the optical fiber. If a similar cladding were to be placed around the Goubau line, at UHF or microwave frequencies, the cross-sectional dimensions would be excessive. It is thus the much *smaller size*, corresponding to the much higher operating frequency, that makes the optical fiber feasible.

Much other work on surface waves was conducted in the U.S.A. during this period, but its impact was not as noticeable, being primarily in the form of gaining insight and understanding. Only two examples of contributions are mentioned here, to illustrate the fact that the field grew quite sophisticated in its inquiries. The first example notes that there were even a few studies of the effects due to *discontinuity structures* on surface-wave guides. Such problems are difficult, since these guides are open waveguides and discontinuities on them are both reactive and resistive. One notable study was by A. F. Kay, on the discontinuity between two semi-infinite plane surfaces, each possessing a different effective surface reactance. The structure was idealized, but it provided insight into the relative reflective and radiative effects of such discontinuities. Kay's analysis [80] employed the Wiener-Hopf method and therefore yielded an exact solution.

The second example of sophisticated inquiries involves F. J. Zucker, of the Air Force Cambridge Research Center (now RADC at Hanscom Field, MA), who was not only concerned with practical surface-wave antenna matters but who also participated with several different groups, including ours, in a variety of fundamental but also esoteric topics current at the time. Among those topics were the question of the "existence" of the Zenneck wave [81] and whether a finite length of surface-wave structure actually radiates all along its length, as some antenna designers maintained, or only at its two ends, the feed end and the termination [82]. Zucker's two comprehensive chapters, one [83] in H. Jasik's *Antenna Engineering Handbook* and the other [82] in Volume II of *Antenna Theory* by R. E. Collin and F. J. Zucker, were important contributions to the field, combining basic theoretical material with practical implementations. In addition to his role as a researcher, Zucker was also a contract monitor whose mode of operation deserves special mention. He followed closely the work of those he supported, interacting directly where feasible and pertinent, and he was always encouraging and always helping.

During the late 1950's and early 1960's, as a consequence of concerns about communication blackout with vehicles in space during part of the reentry phase, studies were conducted by various groups on surface waves guided by *plasma layers*. T. Tamir and I, at the Polytechnic Institute of Brooklyn, formed one such group. We found that a rich variety of wave types could exist on plasma layers, such as backward surface waves [84] and nonradiating complex waves [85] when the plasma is overdense, and leaky waves [86] when it is underdense. Some of these wave types were also found by others on plasma cylinders.

As a result of these studies, we developed what we felt was a thorough understanding of various wave types guided by interfaces. We wrote a pair of comprehensive papers summarizing much of this information in a systematic and lean style, and we sent it to the IEEE TRANSACTIONS ON ANTENNAS AND PROPAGATION for publication. The reviewers were favorably impressed but the editor requested that we reduce the length of the paper by 50 percent. We did not see how we could do that without greatly weakening the paper; then we remembered that the British, particularly the Barlow group, had been very productive in the area of surface waves, so that our "long" pair of papers might be better appreciated by them. We therefore submitted the papers to the *Proceedings of the IEE* (London), and they accepted them without any change whatever [87], [88]. About a year later, Tamir and I were notified that our pair of papers won for us the Institution Premium, the highest award of the IEE, and that we were the first Americans to receive that award. We had not gauged adequately just how much the British would appreciate our results.

An impetus of a different sort arose in the late 1950's and 1960's, at which time new surface-wave structures were invented, analyzed, and measured in response to a perceived need for low-loss waveguides for millimeter wavelengths. Two examples were D. D. King's "dielectric image guide" [89], consisting of a dielectric half cylinder of small radius on a metal ground plane, and the "H guide" of F. J. Tischer [90], comprised of a dielectric strip between parallel vertical metal planes, the combination resembling the letter H. This activity flourished for a short time only, however, and faded when the need for millimeter waves did not materialize.

During the past decade, needs associated with integrated optics gave rise to a variety of dielectric strip waveguides placed on a dielectric substrate. In addition, solid-state sources for millimeter waves had become practical, and the need for millimeter-wave systems reemerged. As a result, the surface-wave waveguides invented earlier were revived and modified, and new ones were invented. The dielectric image guide took on a rectangular, instead of half-cylinder, form and became popular. The H guide was modified by T. Yoneyama and S. Nishida [91], [92] of Tohoku University, Japan, to become less than a half-wavelength wide so that discontinuities would no longer radiate; the resulting structure became suitable as a candidate for millimeter-wave integrated circuits, and was renamed the NRD (nonradiative dielectric) guide. Among the new structures were several dielectric strip-type guides proposed by the group at the University of Illinois led by R. Mittra and T. Itoh [93], [94], and admittedly based on corresponding structures for integrated optics.

Two very useful and simple approximate theoretical methods emerged during the 1970's for determining the propagation characteristics of dielectric strip-type waveguides. One was developed for integrated optics use by E. A. J. Marcatili [95] of the Bell Laboratories; the results were presented in simple form but were not valid near to mode cutoff. The second method was really the first stage of a transverse resonance procedure, but was developed by independent thinking, first by R. M. Knox and P. P. Toulios [96] and later by the Illinois group [93]. The second method is called the "effective dielectric constant" method, and it is more accurate near cutoff than Marcatili's [95]. A later thorough theoretical study of this class of dielectric strip waveguides was conducted by the group at the Polytechnic Institute of New York led by S. T. Peng and me. We found [97], [98], to everyone's surprise, that some modes on a large class of these waveguides could *leak*, contrary to the predictions of the above-mentioned approximate methods which indicated that all modes were purely bound. More specifically, the dominant mode on these waveguides *is* always purely bound, but the lowest mode of the opposite polarization, and all higher modes, generally leak energy away in the form of a surface wave. Such leakage can be important in an integrated-circuit context because it produces unwanted cross talk. It was also found that the "effective dielectric constant" method yields accurate results for the phase constant under most circumstances; however, that method cannot furnish any information on the leakage behavior.

2) Leaky Waves:

W. W. Hansen [99] had proposed during the late 1930's that an antenna could be created by cutting a rectangular waveguide longitudinally, thereby producing a long slit in the side of the guide out of which power could leak away. The proposal was not pursued during that period because of the success of slot arrays, but the simplicity of the structure remained attractive, and it was reexamined during the early 1950's.

The early designs of leaky wave antennas did not produce good agreement with measurements because the theoretical basis was unclear. It was, however, recognized that the leaking waveguide possessed a complex propagation constant, with the usual phase constant and an attenuation constant due to the leakage (in addition to an attenuation constant due to wall losses). The field struggled with two basic problems:

1) Was the leaky wave truly real?
2) How should one determine the leakage constant theoretically?

Since it was obvious that leakage was being produced, why did people question the *reality* of the leaky wave? If one recalls simply that the squares of the wavenumbers in the three orthogonal directions must sum to the square of the free-space wavenumber, then, if there is decay longitudinally, corresponding to power leakage, the wave amplitude must increase in the transverse (or cross-section) plane. Since the cross section outside of the waveguide is unbounded, this implies that the leaky wave must *increase* transversely to infinity, yielding an unphysical result. The state of skepticism and uncertainty was so great that the Ohio State group, under V. H. Rumsey, made direct probe measurements [100]. They found that near to the waveguide the field did indeed increase transversely, but that some distance away it dropped off rather suddenly. These circumstances and the physical explanation for them are summarized in Fig. 6, where greater field strength is represented by a greater density of lines.

As the leaky wave moves along the longitudinal (z) direction in Fig. 6, the field intensity decreases exponentially along z. However, if one follows the dashed line vertically in the x direction, it is seen that the field *should increase* vertically away from the guide surface. After a certain value of x, however, related to the location of the source (the beginning of the cut), the field drops off, as found experimentally. The leaky wave, with its peculiar behavior, is thus defined only in the wedge-shaped region shown in Fig. 6.

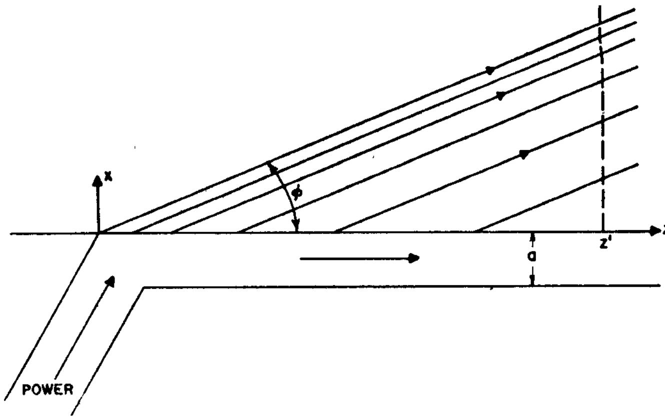

Fig. 6. Leakage from a closed waveguide opened at the top. The leaky wave is defined only within the wedge-shaped region shown. (From [118].)

Many people actually expressed surprise on learning that the wave type really exists, but its reality was no longer questioned. Still at issue, however, was how to compute the complex nature of the wave's properties. Several different methods, mostly perturbation approaches, were tried by various people, but most of them were incorrect, and they yielded poor results when they were applied to antenna structures. There was no sound theoretical basis for these waves until N. Marcuvitz [101], at Brooklyn Polytech, supplied it. He took his cue from radioactive states in nuclear physics, and he recognized that these leaky waves (or leaky modes) were pole solutions, as were all regular modes, but that these were located on the "wrong" Riemann sheet of the transverse wavenumber plane. (When the region is open to infinity, a branch point arises, and two Riemann sheets are present; on one sheet, the fields decay toward infinity when small loss is introduced, while on the other they increase to infinity. A spectral solution, containing properly behaved waves, is restricted to the former sheet; the leaky poles occur on the second sheet. The leaky modes are therefore not contained in the spectral (proper) solution, but they exist, being a rephrasing of part of the continuous spectrum portion of the spectral solution.)

Once the leaky modes were understood to be pole solutions, they could be obtained by applying the transverse resonance condition in the same way as for ordinary modes. When this approach was applied to antennas, the correspondence between theoretical and measured results was *spectacularly* good. This agreement was typified in a paper [102] by R. C. Honey, which demonstrated such agreement in detail down to -36 dB. The leaky wave antenna field moved briskly after that, with various theoretical papers appearing, and with many new antenna structures. It was found in many cases that the antenna performed so closely in conformity to the theoretical design that no further adjustments were required.

Leaky waves were later shown to play a significant role in the explanation of *many physical effects*, as diverse as radiation from plasma sheaths surrounding vehicles reentering the atmosphere, Čerenkov radiation, Smith-Purcell radiation, Wood's anomalies on optical gratings, blind spots in large phased-array antennas, prism and grating couplers in integrated optics, etc. Our group at the Polytechnic Institute of Brooklyn made many of these contributions.

Briefly, these effects may be summarized as follows. 1) T. Tamir and I found [86], as mentioned above, that when the dielectric constant of a plasma sheath lies between zero and unity (an underdense plasma), a leaky wave can propagate along the sheath; that leaky wave influences the nature of the radiation from antennas on a vehicle during reentry, and we computed [103] the resulting effects. 2) Čerenkov radiation occurs when an electron or a modulated electron beam travels near to or in a dielectric medium; Smith-Purcell radiation is produced when the dielectric medium is replaced by a metallic periodic grating. I. Palócz and I treated these effects by considering the radiating electron beam as a leaky space charge beam, and the resulting analysis yielded the first self-consistent solution of these effects [104], [105]. 3) Blind spots in the radiation patterns of large phased arrays constitute an important performance defect, and were first found unexpectedly on a full-scale antenna; they correspond to the array not radiating or receiving over a narrow angular range. Many people contributed effectively to the understanding of this phenomenon; the first correct explanation was in terms of leaky waves, given by G. H. Knittel, A. Hessel, and me [106]. 4) The well-known Goos-Hänchen beam shift occurs when an optical beam is incident on a dielectric interface at exactly the critical angle of total reflection; the reflected beam is shifted in position by some tens of wavelengths. When the beam is incident at the leaky wave angle, the resulting shift is much stronger, being as much as several hundred wavelengths. T. Tamir and H. L. Bertoni [107] described this beam shift and showed how it explains the high efficiency of prism and grating couplers for integrated optics.

Some of the history relating to our work on Wood's anomalies on optical diffraction gratings is interesting. A. Hessel and I were first exploring how radiation was produced from open periodic structures when the period was made sufficiently large (very roughly a half wavelength). To simplify the problem, we assumed a periodic reactive surface, and solved for the guided modes. When the period became sufficiently large, the guided modes became leaky. We obtained a rigorous solution [108], which was in fact the first such solution for any periodic structure. The book [70] by Barlow and Brown includes the derivation of that solution, and states that "it is of fundamental importance in the further development of surface-wave aerials." A few years later, at an international URSI Symposium, V. I. Talanov of Gorky University in the U.S.S.R. told me that he had independently analyzed a similar problem [109].

As a byproduct, and just for fun, we decided to also set up the rigorous solution for plane-wave scattering by the periodic reactive surface. That solution is actually easier to obtain than the guided-wave one since in the scattering problem everything is real, rather than complex. We handed the solution over to one of our computresses and asked for certain numerical results. (In those days, and it is not *that* long ago (early 1960's), we still used mechanical computing machines. If we had had present-day computers available, we might not have bothered to derive a rigorous analytical result.) We were handed back some peculiar-looking curves with sharp spikes which we did not understand, but finally had to believe after they were double-checked. When told about these unexpected results, our Air Force contract monitor, F. J. Zucker, asked "Could they be Wood's anomalies?" I replied that I didn't know what Wood's anomalies were, although I had heard of them, but I would find out.

The clue turned out to be invaluable. The paper that resulted from this study [110] contained an entirely new theory of Wood's anomalies, which for the first time showed the intimate relationship between leaky waves and one class of these anomalies. It also showed that some of the sharp variations in amplitude that Wood found experimentally were in fact just a special case of "scattering resonances" that could arise whenever the plane-wave angles and the leaky wave angles were the same. This work, to our disappointment, was ignored for almost a decade, but then it was "discovered" and was termed a "classic paper." For example, R. Petit, who founded a group (which has since become world famous) at Marseille during the 1970's devoted to diffraction theory, told me that the early work of his group was based heavily on our paper. Now our point of view is very widely applied, but, as often happens, the newer contributors to the field use the approach but are no longer aware of its origins.

D. Antennas That Are Best Viewed as Waveguides

Certain classes of antennas are actually modified waveguides, or consist of portions of waveguides. Those antennas can best be analyzed by viewing them in waveguide terms, and by applying microwave field and network approaches. Three very different examples of such antennas are horns, leaky wave antennas, and phased-array antennas.

The treatment of leaky wave antennas by analyzing them as open waveguides turned out to be particularly successful; it is safe to say that *the agreement between theoretical design and measured performance has been better for leaky wave antennas than for any other type of antenna.* Such excellent agreement did not occur when those antennas were first designed, however, because some fundamental uncertainties arose in connection with leaky waves in general, as explained in the previous section. Only when those basic points were correctly understood was it possible to make real progress.

Let us first review briefly the early history of some antennas that were viewed in terms of waveguides. One of the first was the *sectoral horn*, fed by rectangular waveguide and comprised of a section of rectangular waveguide with its top and bottom walls extending radially outward, so that it becomes a sector of a circle when viewed from the side. Barrow and his colleagues [111], [112] examined the sectoral horn thoroughly during the late 1930's both experimentally and theoretically. The propagation of waves in radially expanding regions was treated by Marcuvitz during World War II in terms of radial transmission lines [113]; the formalism was developed originally for the analysis of klystron cavity behavior. Although the radial transmission line phrasing is rigorous, it has not been widely used because it is more complex than uniform transmission line theory and because it is applicable to a smaller range of problems. It has been employed in various problems, however, including sectoral horns [114], where the junction discontinuities at the feed end and the radiating end were taken into account, and guided modes in partially dielectric-filled circular waveguides, a topic thoroughly treated by P. J. B. Clarricoats and his colleagues [for example, 115].

A second, but less successful, early example was the attempt to describe *dipole* radiation by viewing the dipole as a biconical horn fed at the origin of the spherical coordinate system. But then the wire structure needed to be tapered conically to correspond conceptually to the spherical system, and a substantial discontinuity was present when the dipole antenna ended in the radial direction. Much work was done on this approach by Schelkunoff [26], Stratton and Chu [116], and others.

The first *leaky wave antenna* was the one proposed by W. W. Hansen [99] in the late 1930's, which consisted of a rectangular waveguide that is cut longitudinally, thereby creating a long slit in the side of the waveguide out of which power could leak away all along its length. (During World War II, H. G. Booker made the interesting suggestion that "girders and trenches" be used to guide waves; we know now that such structures would leak, and behave as antennas.) Further work on leaky wave antennas did not resume until the early 1950's, and most of those antennas were also perturbations in one sense or another of closed waveguides. It was therefore logical to view these antennas in terms of waveguides which were somewhat modified. Because the theoretical basis was unclear, however, people tried different models for analysis but they met with varied lack of success. When it was finally understood that the leaky wave was a pole solution (even though the pole was nonspectral), the transverse resonance procedure was applied in the usual fashion, to derive a complex transverse wavenumber from which one obtained the complex longitudinal wavenumber, yielding the phase and leakage constants. The measurements finally (and suddenly) agreed with the theoretical designs, and the agreement was so remarkably good that in most cases it was unnecessary to do any trimming to optimize the performance; the design dimensions were the final dimensions.

The theoretical challenge then moved from understanding the nature of the leaky wave to solving for the discontinuities due to the various cuts or other guide perturbations, to be used in the transverse resonance procedure in its application to a variety of antenna structures. The two chief theoretical groups involved in such studies were ours and the one at Ohio State University headed by V. H. Rumsey, although others contributed as well, including A. L. Cullen [117] in England. Hansen's original longitudinal cut in the side of rectangular waveguide served as the starting point for new antenna structures. First, the cut was widened all the way to simplify the structure, and the result was termed the "channel" guide; its problem was that it leaked too strongly per unit length, resulting in a short antenna aperture length and, therefore, a wide radiated beam. Since the slit cut directly across the wall currents, even a narrow slit would leak rather strongly, consistent with the logarithmic nature of the slit discontinuity when viewed transversely. A contribution of the Ohio State group was the replacement of the slit by a periodic series of small holes, so that the wall currents would only be pushed aside instead of being rudely cut. The resulting antenna, which became known as the "OSU holey guide," permitted narrow radiated beams.

A large variety of structures were eventually analyzed and measured. L. O. Goldstone and I published two comprehensive papers on leaky structures based on rectangular waveguide [118] and on circular waveguide [119], with different longitudinal cuts and different incident guided modes. Our many careful measurements agreed very well with our theoretical results. Interestingly, we developed a theoretical perturbation procedure to simplify practical calculations because this work (1959) preceded the computer era. The Ohio State group published several papers [100], [120], [121] on various structures in rectangular guide, some similar to ours and some different, with both measurement and theory, and they included the effects of mutual coupling between neighboring parallel leaky wave antennas in a small array. Propagation along a slotted circular waveguide was studied earlier by R. F. Harrington [122]. A few years later, in a different context, P. J. B. Clarricoats and I examined the leaky wave performance of slitted cylinders containing a dielectric rod core [123].

Other structures were also investigated with respect to their potential utility as leaky wave antennas; these included perturbations of coaxial line, and periodic, rather than uniform, perturbations of both rectangular and coaxial guides. In all cases, the structures were viewed as waveguides whose perturbation produced a complex propagation wavenumber. Particular mention should be made of the *asymmetric trough guide* invented by W. Rotman, which does not begin with a closed waveguide. Instead, Rotman selected an open waveguide that is nonradiating by virtue of its dimensional symmetry, and he produced a controlled leakage by introducing asymmetry. In this case, the open waveguide was a "trough guide," a structure that is derived from symmetrical strip transmission line by bisecting it vertically and placing a metal plane at the midplane; the asymmetry is created by placing a metal insert in one of the halves of the line. Rotman took measurements of the structure and I provided the theory; the agreement between them was excellent [124], and designs based on the theory were employed in applications that covered various frequency ranges (I was told that in one case one could physically walk inside the structure).

Comprehensive summaries of the various types of leaky wave antennas and some of the associated theoretical background appear in F. J. Zucker's chapter in H. Jasik's Handbook [83], and in the chapter by T. Tamir in Part II of the book entitled *Antenna Theory* by R. E. Collin and F. J. Zucker [125]. In addition, a more detailed presentation of the field that describes both the structures and the methods of analysis is contained in the book *Traveling Wave Antennas* by C. H. Walter [126].

With the revival of millimeter waves within the past decade or so, new interest has been shown in leaky wave antennas specially suited to millimeter wavelengths. Because of the smaller wavelengths and higher waveguide losses at millimeter waves, these new leaky wave antennas have to be simple in structure, to ease the fabrication problems, and be based on low-loss waveguides. One class of such antennas involves periodic modulations of dielectric image guide, where the modulations may be caused by periodic metal strips on the top surface or by periodic grooves cut into the top surface. It is interesting that those structures were proposed during the 1960's [127] with no takers at that time, but they were reinvented during the 1970's. The only available theory for these antennas that can be used for antenna design is that by F. Schwering and S. T. Peng [128] for dielectric grooves; the theory works best for wide dielectric strips, but a modification has been developed for narrow strips [129]. New uniform leaky wave structures based on low-loss waveguides have also been investigated; among them are an asymmetric strip antenna in groove guide [130], [131], and a leaky structure based on NRD guide that is foreshortened on one side [132].

The third example of antennas that are best viewed as waveguides is that of *phased-array antennas*. Here, the case for a waveguide approach is not as evident as it was for leaky wave antennas or horn antennas. Most investigators did not, in fact, employ such an approach.

The application of waveguide approaches to phased arrays of slots or dipoles was both novel and successful. The array is viewed as a periodic structure, with each radiating element associated with a *unit cell* whose cross-sectional size and wall boundary conditions are dependent on the periodic array environment. As a result, the unit cell is equivalent to a waveguide with peculiar walls. Those walls have been termed "phase-shift walls," where the phase difference between opposite walls depends on the scan angle of the radiated (or received) beam. The immediate advantage is that mutual coupling effects are taken into account fully and automatically, so that the rest of the array can be ignored, reducing the array problem to one involving only a *single* waveguide. An illustration of this unit cell viewpoint is given in Fig. 7.

The first one to recognize the value of this waveguide approach was H. A. Wheeler in 1948 [133]. (Wheeler is well known for recognizing the essence of a problem and succinctly revealing its main features. He left Hazeltine Corporation shortly after World War II to found his own company, Wheeler Laboratories, that earned universal respect for the quality of its work and for the caliber of its

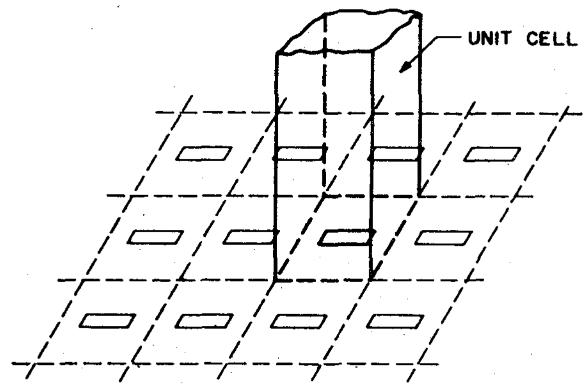

Fig. 7. Unit cell waveguide drawn around a typical radiating slot in a two-dimensional phased array. (From [134].)

Fig. 8. Photograph (from right to left) of H. A. Wheeler, S. B. Cohn, and A. A. Oliner, taken during the 1950's. All three individuals received the Microwave Career Award (in 1975, 1979, and 1982, respectively). Photograph courtesy of *Microwave Journal*.

personnel. He returned to Hazeltine during the 1970's and his company was absorbed into it; he later became Chairman of the Board and Chief Scientist of Hazeltine. A group photograph of Wheeler, S. B. Cohn, and me, taken during the 1950's, appears in Fig. 8.) Wheeler derived the radiation resistance of a typical dipole in an array of dipoles phased for broadside radiation by showing that it was equivalent to that for a dipole located in a waveguide. His treatment, though pioneering, was limited in that he considered only the resistive portion of the radiating impedance, and only for broadside radiation. About a decade later, interest in phased arrays became very strong in connection with anti-ballistic missile defense considerations. By then, Wheeler's paper had become lost in the literature, and no one picked up the idea.

The concept of unit cells was reinvented by S. Edelberg and me [134], [135] in a more complete study that then also took susceptance into account, and examined the influence of scan angle. The array we considered was the typical one in which each slot in the array was fed by a separate rectangular waveguide. The array is then regarded as the junction between two waveguides, the feed waveguide, and the unit cell waveguide shown in Fig. 7, where the walls of the unit cell waveguide are determined by the angle of scan. The properties of this junction can then be determined by purely waveguide methods, and the approach also furnishes considerable insight to those trained in waveguide techniques. Of course, there is a one-to-one correspondence between the antenna properties and the waveguide features; for example, the conditions corresponding to the onset of a grating lobe are those for which the next higher mode becomes propagating in the unit cell waveguide.

The above study on phased arrays was conducted as part of consulting activities for the M.I.T. Lincoln Laboratory. I recall that I had somehow hurt my back so that I could not walk, and I was forced to stay in bed for a few days. While propped up in bed, thinking about the phased-array problem, the unit cell idea suddenly came to me. I did not know about Wheeler's earlier work until a few years later when I came across it accidentally while searching for a paper on a completely different topic. The unit cell concept was put to excellent use a few years later by P. Hannan and colleagues at Wheeler Laboratories in connection with measurements on phased arrays. Measurements on a full-scale array, with the appropriate feeding, can be a formidable task, and measurements on a small array often yield only rough results. An alternative procedure is to *simulate* an infinite array by making use of the unit cell concept; the measurements are then made within a waveguide, but the structure then corresponds to a specific angle of scan only. The principle and the details were developed thoroughly by Hannan and colleagues [136]–[138].

The unit cell, or waveguide, approach to phased-array antennas has been described in detail in Chapter 3 of Volume II of the three-volume set on *Microwave Scanning Antennas*, edited by R. C. Hansen [139]. The approach has been widely employed since; in particular, extensive use of it has been made by A. Hessel and colleagues on arrays on curved surfaces and on other array structures on planar surfaces.

The waveguide viewpoint has also been helpful in the understanding of *blindness* effects in phased arrays, where the array cannot receive or transmit within a narrow angular range. The effect was first discovered during the early 1960's to everyone's great surprise (and shock) on a full-scale model of an array of dielectric-covered dipoles. As other arrays were examined more closely, more and more examples of blindness were found on arrays with different radiating elements. The first theories involved surface wave effects, but they were not correct. The first correct explanation for some of these arrays [140] employed a leaky wave interpretation; another successful explanation for other arrays was put forth by G. H. Knittel, who showed that cancellation effects can occur at certain scan angles between the propagating mode and a below cut-off higher mode in the unit cell waveguide. A summary of the types of array element for which blindness can occur, and an explanation in terms of the unit cell approach appears in the literature [141].

IV. Concluding Remarks

The partial history of microwave field theory presented here is admittedly selective with respect to both the topics chosen and the specific history covered. It would have been impossible to do otherwise because of the wide scope of the field, even though I limited the scope in the Introduction by distinguishing microwave field theory from electromagnetics in general. Within that framework, it was possible to present a reasonable coverage (in Section II) of the formative periods before World War II. Since that time, however, with the field moving in many directions and with so many investigators making important contributions, it was essential that the coverage be severely restricted. I have therefore selected several topics in Section III that I believe were of key importance to the development of the field, and also were within my personal knowledge. I have tried, in the process, to present a flavor of how the microwave field developed in certain important ways. I hope that other contributors to microwave field theory will elsewhere offer us their views in a similar fashion.

In the remainder of this section, I would like to present some broad observations with respect to ways in which this field has developed, recognizing that others may have made some of these points before.

1) The general availability of computers today has changed in many ways how one proceeds in microwave field theory. Computers have provided us with a very powerful tool, that permits us to obtain numerical values for problems which might otherwise be impossible to solve, except in very rough approximation. The principal stress today in microwave field theory therefore involves numerical methods. Before approximately 1970, the stress was on obtaining simple but accurate analytical solutions from which calculations could be made easily, and also on those few instances when it would be possible to derive exact solutions, against which the approximate solutions could be compared.

The pursuit of simple but accurate analytical expressions forced us to develop our physical insight, and to try to understand the essential features of any problem. One danger today is that the computer can make things a bit too easy for us, enabling us to obtain the numerical results without a proper understanding. I am reminded of the remark made around 1940 by W. W. Hansen in connection with the "impedance concept," a novelty then in electromagnetics: "It should not be used as a substitute for thought."

Another valuable feature of exact solutions is that they can provide unexpected new information or insight; an example was provided in Section III-C-2 in connection with Wood's anomalies. In an exact solution, peculiar results force us to rethink the problem and may lead to new understanding; in a purely numerical approach, peculiar results may be due solely to an artifact in the numerical method used for computation. A second danger today is that the motivation for exact solutions has diminished greatly, and one sees less of them (also partly because fewer problems are left for which exact solutions are obtainable).

A judicious combination of analytical methods up to an appropriate point and then numerical procedures thereafter furnishes the best arrangement, of course, permitting us capabilities far greater than we dreamed of in the past without diminishing our physical understanding.

2) As the microwave field developed, new ideas emerged or new structures were proposed, but they were not appreciated because the field was not ready for them. When the field later recognized the need for those ideas, they were sometimes revived but at other times the ideas were reinvented.

Within the history presented here, we saw examples of both kinds. Lord Rayleigh's early solutions for guided modes were not known to Southworth and some others during the 1930's, and they were reinvented. Wheeler's concept of a unit cell, or waveguide, approach to arrays was lost, but the concept was reinvented a decade later by Edelberg and me when the field had to consider that class of problems more seriously. On the other hand, an example of a revival is furnished by the theory proposed by Hessel and me, and applied to Wood's anomalies on optical gratings: that plane-wave scattering resonances for a large class of periodic structures occur when the plane-wave angle coincides with a leaky wave angle; the theory languished unappreciated for a decade but it was then picked up and is now widely used. Another example relates to the millimeter-wave guiding structures proposed during the 1950's and early 1960's; they were revived when the millimeter-wave field became active again over a decade later.

Such reinvention degenerates into sheer duplication at times when a new field unfolds; a striking example is furnished by the field of integrated optics during the 1970's, where the new practitioners in that field simply did not read the IEEE journals and proceeded to reinvent or rederive many well-known results, often less well than the original versions. Aside from such extremes, the general problem of reinvention is exacerbated today because more papers are published now than ever before; there are more journals and more symposia with multiple parallel sessions. It is now harder than ever to keep up with current work, let alone go back into earlier literature.

3) As a theorist, I would like to romanticize that theory opens up new vistas, and the practical people follow. In the microwave field, it usually goes the other way. A new structure is invented or a new need emerges, and a first-order design is made; then theory makes the design understandable and permits systematic improvements in design. Sometimes, however, when the development of the field reaches a new phase, and the understanding is fuzzy, theory is needed to provide even a decent first-order design. Two examples from the history presented here come to mind. During World War II, it was not known how to design the required components for radar systems until some information was furnished on how to characterize the waveguide junctions and other discontinuities involved. The microwave network point of view furnished by the theorists provided the required insight even before the numerical values were available. A second example relates to leaky wave antennas, where the designs were inadequate before the theory was properly understood, but became excellent after the correct theory was furnished.

4) Sometimes a small modification in the technology, possibly even in the dimensions of a structure, can change a previously unwanted or impractical structure into a practical and desirable one. There are surely many such examples, but we can here call to mind three of them. One involves the recent transformation from H guide to NRD (nonradiative dielectric) guide. The plate spacing in H guide was made greater than a half wavelength to lower the attenuation, but it also caused all discontinuities to radiate; as a result, H guide languished and appeared to have no future. NRD guide simply reduces the plate spacing to less than a half wavelength, causing all discontinuities to be reactive and producing a potentially practical candidate for millimeter-wave integrated circuits. A second example relates to the disappointment surrounding the Goubau line, when it was finally understood that the evanescent field extending into the air outside of the dielectric region rendered the line impractical. When the optical fiber was designed later, a dielectric cladding was placed around the central core region, and all the evanescent field was confined to the cladding, thereby avoiding the problems encountered by the Goubau line.

The third example is microstrip line, which was essentially rejected by the field because of its hybrid mode nature and the fact that discontinuities on the line would radiate. Then, some time during the 1960's, the cross section of microstrip was reduced substantially; this step, reducing all dimensions relative to wavelength, greatly lowered the radiative (and reactive) content of the discontinuities and also lowered the hybrid aspects of the mode, causing it to be more TEM-like. As a result, microstrip line grew more popular than symmetrical strip line, and a few years later became the dominant waveguide type in the microwave field. This simple but key change in the cross-sectional size of microstrip relative to wavelength resulted in what is probably the most dramatic inversion in the history of the microwave field.

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