# THE EARLY DAYS
# OF
# WHEELER AND HAZELTINE CORPORATION
# — PROFILES IN
# RADIO AND ELECTRONICS


BY

HAROLD ALDEN WHEELER

Chief Scientist
Hazeltine Corporation



1982

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the Author—1982

# FOREWORD

by Sal J. Nuzzo
President and Chief Executive Officer.

Hazeltine Corporation is today a high technology company serving a diversified spectrum of markets through the design and manufacture of products and systems springing from technologies whose origins go back to the beginning of the electronics industry. The story of Hazeltine is remarkable for its continuity of both its name, since 1924, and its engineering in the fields of radio, television, communications and data processing. The story of Hazeltine is also unique in that it parallels the history of the radio industry, beginning with its "Neutrodyne Circuit" patent which contributed to the "commercialization" of AM radio.

We are fortunate that Dr. Harold A. Wheeler, who is Chairman Emeritus of Hazeltine Corporation's Board of Directors and actively serves as its Chief Scientist, has elected to record for us the history of those early days. Harold Wheeler started work at Hazeltine in 1924 and has served as an engineering leader and technological innovator for the Corporation since that time. He is uniquely qualified to report on the early days of Hazeltine and some of the major factors which helped mold the electronics industry.

This book is the history of the early days (pre-World War II) of Hazeltine Corporation and of the early career of Harold A. Wheeler. As the story is developed, it becomes obvious that these two subjects are so intertwined that there is really a common theme. Harold Wheeler was Hazeltine Corporation's first employee and was a contemporary inventor, with Professor Hazeltine, of radio circuits such as the Neutrodyne—the patent on the Neutrodyne being the first corporate asset.

The first part of this book gives a personal account of Harold Wheeler, which when combined with the story of Professor Hazeltine*, provides insight into two of the individuals critical to the development of Hazeltine. This is followed by 24 short sketches of persons important during the pre-World War II era. These vignettes give an interesting understanding of the personalities involved in the early life of Wheeler and Hazeltine Corporation. Of particular note is the story of William A. MacDonald, the first Chief Engineer of the Company, the man who pulled Hazeltine Corporation through the Great Depression and who eventually became Chairman and Chief Executive of the Corporation.

It should be noted that while the book covers history, its organization is somewhat like a reference book, with groupings of subjects, generally chronological within each group. The next group of subjects covered are technical area topics, the first five being ordered according to the time of each major engineering effort. The first technical area of interest was TRF (Tuned Radio Frequency) Receivers, starting with Professor Hazeltine's original Neutrodyne in 1922. Then came Diode AVC, which encompasses a set of ideas

*"Hazeltine the Professor," by Harold A. Wheeler

developed by Wheeler while still a college student and reduced to practice in his home basement laboratory. The Superheterodyne Receiver follows starting in 1930, and it, in conjunction with the screen grid tube, threatened the very existence of Hazeltine Corporation. The next areas treated are the early TV work and the work on FM.

The major technical area of test equipment is then covered. Hazeltine Corporation developed new technology for making quantitative measurements on receivers in order both to evaluate overall performance, and to analyze operation of specific individual circuits. This required development of precision test equipment, frequently specialized for each technical area addressed. The corporate capabilities and services in measurement and improvement of licensee receivers were critical items in both developing a corporate reputation for engineering excellence and survival through the Great Depression.

Most of these special test equipments have outlived their days, but two are worthy of special mention because of their uniqueness and legacy. One, the Piston Attenuator, permitted a signal generator with direct reading of attenuation in decibels; the other, the Direct Reading RF Inductance Meter, is still in use in updated form in Hazeltine's Research Laboratories.

In all of the technical areas discussed, including test equipment, Dr. Wheeler was a major technical driving force, whose many innovations contributed significantly to Hazeltine's technical strength and lasting technology base.

Since the days recalled in this book, Dr. Wheeler has continued his prolific stream of innovative ideas. He has been, and still is, acting both as a teacher, so others may contribute, and as an individual contributor to the electronic engineering world. His works are generally tutorial in nature, starting with a basic understanding of engineering fundamentals. Some of the technical areas in which Dr. Wheeler has made such basic and fundamental contributions since the days recalled in this book include: small antennas, stripline RF techniques, VLF (very low frequency) antennas and propagation, phased array impedance, Microwave Landing Systems, and remote sensing energy management systems, plus a proliferation of charts for a wide range of engineering subjects and parameters.

I am very appreciative of this monumental reference book by Dr. Wheeler on the common theme of early days of Wheeler and Hazeltine Corporation. A study of the details of history and why things happen is important background for the future. We are very fortunate for the gift of this background in writing, first by Dr. Wheeler's book, "Hazeltine the Professor," and now by this book. All of us at Hazeltine are indebted to Dr. Wheeler for his many continuing technical contributions, literary contributions, and guidance through the Board of Directors.

**THE EARLY DAYS OF WHEELER AND HAZELTINE CORPORATION — PROFILES IN RADIO AND ELECTRONICS BY HAROLD ALDEN WHEELER**

## CONTENTS

Page

## CONTENTS (cont'd)

Page

## CONTENTS (cont'd)

Page

**Figures** **Page**

**Figures (cont'd)** **Page**

**Figures (cont'd)** **Page**

**Figures (cont'd)** **Page**

## Photographs

# Section 0. Introduction

## 0.0 Introduction.

The central theme of this account is "Opportunity". In my home life and then also in my profession, I have been presented with much more than one man's share. I have not made the most of my opportunities, but I have derived much pleasure and much satisfaction from what I have been able to accomplish. In reporting some memories of the first half of my life, it is my pleasant obligation to give credit to the many people and circumstances that contributed to my own growth.

Outside my families, my life has been inseparable from the history of Hazeltine Corporation. That Company's story and mine were so rich in events that it has been a challenge to present a fair sampling in such a relation as to form an adequate picture. The course of events followed remarkable continuity until the beginning of World War II, so I have chosen the time before that date as the period of this account.

That period in history started before World War I and continued through the Great Depression. It saw the rapid growth of radio communication from the turn of the Century, which reached an explosive transition with the advent of radio broadcasting from 1920. My chosen field of radio engineering experienced an evolution which was timed, as if by plan, to my individual program. Television was "just around the corner" when the war changed everything.

To my parents and later to my partner for life, I owe a debt of gratitude beyond what can ever be repaid. In my professional development, in a very different sense, the same is true of Professor Hazeltine, William A. MacDonald and Daniel E. Harnett. It is my intention to place in perspective my indebtedness to them and to many others, and my gratitude they deserve.

The predecessor to this volume was my small book entitled, "Hazeltine the Professor". It is a prerequisite for full appreciation of this story. It is duplicated herein, only to the extent needed for continuity. It was my debut in the field of historical writing. The generous enthusiasm of its reception was a spur to the present story of much greater scope. That writing was my first experience in recording personal accomplishments in my field of technology.

**0.1 Scope.**

For this account, I have chosen the period of my life before World War II. The story is based on my memories, supplemented by other available information. This period is introduced by my background before Hazeltine Corporation (1903-23), followed by my close affiliation with the Company in a time of continuity (1924-42). While this overlaps our early work related to the war, that will be mentioned only incidentally. These years in the Company are related to current U.S. history as the years just before the Great Depression, then during the depression, then just after.

This account has so many facets that I have not perceived any one scale of continuity on which all could be presented in a readable form. Therefore I am using the monograph style wherever it appears to be most helpful. The various facets relate to chronology and logistics, Company activities, technical topics, and my personal observations.

It may be helpful to think of this account in terms of these four phases, each about one decade:

| | |
|---|---|
| 1907-16 | My early education in Mitchell, S.D. |
| 1916-25 | My later education in Washington, D.C. |
| 1923-30 | My early work with Professor Hazeltine and Hazeltine Corporation in the Hoboken and New York laboratories. |
| 1931-41 | My later work with the Company in the Bayside and Little Neck laboratories, before the war. |

There follows a brief overview in just enough detail to establish continuity.

**0.2 Overview 1903-23.**

I was born near the University of Minnesota, where my father was teaching in the School of Agriculture. Shortly we moved to Brookings, S.D., where he continued teaching in the S.D. State College. In 1907, we moved to Mitchell, S.D., where he became manager of a newly organized seed company. My clear memories date from that year, at age 4.

In Mitchell, I was privileged to have a happy childhood and excellent schooling through Grade 7. My natural bent was clear and I chose radio as my profession.

Then we moved in 1916 to Washington, D.C., where my father was called to the Department of Agriculture. This opportunity grew to fill his remaining years until retirement.

In Washington, I graduated from the new Central High School in 1921. On a scholarship from there to George Washington University, I continued while living at home, and graduated in 1925. The next 3 years I attended graduate courses in the Physics Department of Johns Hopkins University in nearby Baltimore. I was near my home in Washington, where I married Ruth Gregory in 1926. Then we lived in an apartment near Johns Hopkins where our first child was born.

In the summers of 1921-22, I was employed as laboratory assistant in the Radio Laboratory of the Bureau of Standards in the Department of Commerce. At the same time, I was doing laboratory work at home. In an accidental meeting with Professor Hazeltine in 1922, we found a common interest. He shared with me his royalties on an invention which led to the formation of Hazeltine Corporation. Also he employed me as one of his assistants in the summer of 1923 at Stevens Institute of Technology in Hoboken, N.J.

**0.3 Overview 1924-42.**

Professor Hazeltine's invention came to be known as the Neutrodyne type of radio broadcast receiver. First marketed in 1923 by a small group of licensees, its success in terms of royalties required a base of management. This was provided by the formation of Hazeltine Corporation in 1924. I was the one original employee, though only part-time on a retainer until I finished college in 1928.

My family moved to an apartment in Jackson Heights, N.Y., where our other two children were born. Then we built a house in Great Neck, further out on Long Island, where we resided for 40 years until 1970.

Hazeltine Corporation started in 1924 on Feb. 1, a public company listed on the Curb Exchange. It was incorporated in Delaware, with headquarters in Jersey City, N.J., near Hoboken and New York City. The executives had an office at 120 Broadway.

The Company was formed by Professor Hazeltine's patent attorney, Willis H. Taylor, Jr. He engaged William A. MacDonald as Chief Engineer. They started a laboratory in the attic of the building which housed the Stevens E.E. Department, headed by the Professor. This Hoboken laboratory was the scene of the Company's engineering activities in the formative period of the first 5 years. I spent the summers working there.

MacDonald provided the leadership for building the engineering activities of the Company. As time went on, his leadership grew to include patent licensing, then management during World War II. He was Chairman and Chief Executive Officer when he died suddenly in 1961. Before the war, patent royalties were the support of the Company. They covered engineering services to licensee manufacturers, as well as new developments for their use.

In 1928, the Neutrodyne was superseded by the screen-grid tube from General Electric Co. However, the Hazeltine laboratory continued with other improvements in broadcast receivers. In 1929, it moved to larger quarters on 333 W. 52 St. in New York City. A New York subsidiary, Hazeltine Service Corporation, was formed for the laboratory work.

The New York laboratory, under the direction of Daniel E. Harnett, was devoted mainly to assisting our licensee manufacturers. In 1930, another laboratory was started in Bayside on Long Island. Under my direction, its mission was research and advanced developments for radio receivers. Here the Company's television activities were started by Harold M. Lewis. They grew to place the Company in a position of engineering leadership before the war. Television broadcasting on a large scale was delayed until after the war.

With the growth of television engineering, the Bayside laboratory was superseded in 1939 by a larger laboratory in a new building in Little Neck. Harnett was designated Chief Engineer, and I was designated Chief Consulting Engineer. This was in full operation when needed for war work from 1941.

Taylor and his firm, Pennie, Davis, Marvin & Edmonds, conducted litigation to validate the Neutrodyne patents and to collect back royalties from unlicensed manufacturers. This was completed in 1934.

When the Neutrodyne became obsolete, various other improvements were under development. The one destined to become most important was automatic volume control in the form decribed as "diode AVC and peak detector". It dated from my work in 1925 and was first marketed by Philco in 1929. It found universal use by 1932 when the first patents issued. Most manufacturers, including RCA, were licensed by 1937. In 1941, after favorable decisions in the lower courts, the U.S. Supreme Court refused to validate my patent. This decision became moot, as the war was upon us.

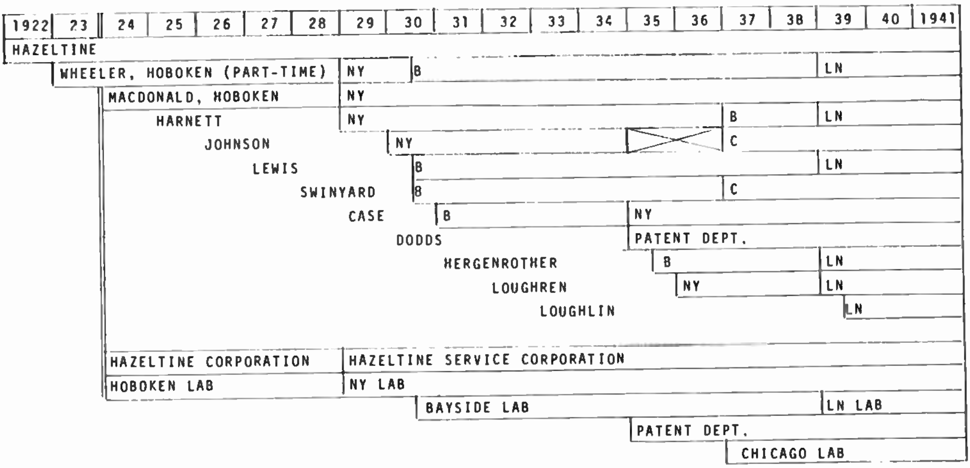
## 0.4 Chronological Outline.

| | |
|---|---|
| 1903 | I was born in St. Paul, Minn. |
| 1907 | We moved to Mitchell, S.D. |
| 1916 | We moved to Washington, D.C. |
| 1917-21 | I attended Central High School. |
| 1919 | I attended National Radio Institute (evening course). |
| 1920-22 | I operated Amateur Radio Station 3QK (at home). |
| 1920 | My father initiated the Radio Market News Service in the Dept. of Agriculture, after which he was appointed a member of the National Radio Conferences (1922-25). |
| 1921-22 | I worked two summers in the Radio Lab of the Bu. of Standards (Lab Assistant). |
| 1921-25 | I attended George Washington University, the Engineering School (B.S. in Physics). |
| 1922 | Working at home, I independently invented neutralization for a TRF amplifier, then accidentally met Prof. Hazeltine who had earlier made this invention. He designed the Neutrodyne receiver and licensed the member companies of IRM. |
| 1923 | I accepted Hazeltine's offer of a share of his royalties in return for assignment to him of any related patent rights I might have. He employed me to spend the summer working in his lab in Hoboken. The Neutrodyne receiver achieved market success in the growing field of radio broadcasting. |
| 1924 | Hazeltine Corporation was formed by Taylor to manage the royalties on the Hazeltine inventions. I was the first employee, part-time on retainer while continuing in college. MacDonald was employed as Chief Engineer to start a laboratory in Hoboken. |
| 1924-28 | I worked summers at the Hoboken lab. |
| 1925-26 | I invented the diode AVC and peak detector, and demonstrated it at my home in Washington. |
| 1925-28 | I attended Johns Hopkins University, for post-graduate studies in the Physics Dept. |
| 1926 | I married Ruth Gregory and we occupied an apartment near JHU. |
| 1927 | I spent much of the summer at Howard Radio Co. in Chicago, working on my AVC. |
| 1927 | The IRM disbanded and RCA licensed the IRM member companies, who continued also as Hazeltine licensees. |
| 1928 | At the end of my studies at JHU, I went to work full-time in the Hazeltine lab and we moved to Jackson Hts. in nearby L.I. |

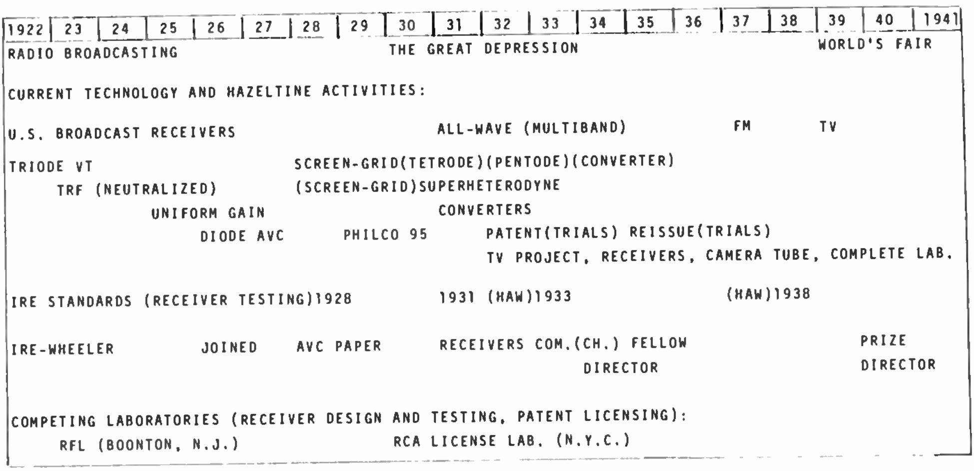
| | |
|---|---|
| 1928 | The Hazeltine neutralization was superseded by the screen-grid tube. Our lab continued with other improvements for broadcast receivers. |
| 1929 | The Hazeltine lab moved from Hoboken to N. Y. City. |
| 1929 | I designed the Philco 95 receiver, the first to use my AVC. It was an immediate success. |
| 1930 | RCA added to its license agreements, the superheterodyne, which had previously been excluded. We concentrated on improvements for new models from 1931 on. |
| 1930 | We started another lab in Bayside for research and advanced developments, under my direction. |
| 1930 | My family moved to a new house we had built in Great Neck. We had three children. We lived there the next 40 years. |
| 1932-41 | My AVC patents were the subject of litigation, which progressed favorably until the Supreme Court finally ruled that there was no invention. |
| 1932-41 | We engaged in a program of television studies and developments, which established our group as among the leaders in the field. |
| 1936 | At the Bayside lab, we made a TV picture tube comparable with the RCA iconoscope which was not available to us. |
| 1939 | The Bayside lab was superseded by the Little Neck lab. Harnett and I were named Chief Engineer and Chief Consulting Engineer. |
| 1939 | I was elected a Director of IRE. (I served 1940-45.) |
| 1940 | I received from IRE the Liebmann Prize. |
| 1941 | We converted to war work. |

## 0.5 Chronological Charts 1922-40.

### Chart I. Engineering Leaders and Their Locations.




### Chart II. Current Technology and Company Activities.



## 0.6 Editorial Practices.

In this account, I am using some practices of presentation for reasons I wish to be understood.

Shortly after World War II, I realized that the prevalent forms of writing the date were illogical and were not standardized. I recalled a form I had seen in some records of Ralph H. Langley, which I perceived as an example of the one logical form. Therefore I adopted this form and some variations which conform to the same logic. These are used herein. Here are the principal variants that are found instructive or useful:

| | | | |
|---|---|---|---|
| AD 1934 | DEC | 23 | |
| 1934 | DEC | 23 | |
| 1934 | 12 | 23 | (19)341223 |
| 34 | 12 | 23 | 341223 |

H.A.W., "A logical date code for communications or records", Jour. Industrial Engg., vol. 18, no. 4, pp. ix-x; Apr. 1968.

Another area of editorial practice is abbreviations. Before World War II, the IRE had enlightened editorial direction under the leadership of the Editor, Dr. Alfred N. Goldsmith, who was one of the three founders in 1912. One result was an orderly and logical set of practices in abbreviations. The subsequent technology explosion brought in many members who were ignorant or unappreciative of those practices.

With respect to the abbreviations of word groups, there was a period of confusion. I finally influenced the IEEE to adopt the simple and tolerable practice of the nontechnical press, namely, a set of capital letters. These are examples of this form, which I use:

| | |
|---|---|
| IF | intermediate-frequency (noun) |
| IF | intermediate-frequency (adjective) |
| AM | amplitude modulation |
| RMS | root-mean-square |
| SSG | standard-signal generator |
| TRF | tuned-radio-frequency |

With respect to the abbreviations for units of measure, the confusion was compounded by international regulation, later routinely mandated by IEEE. I reject such counterproductive abridgment of freedom of expression and progress by natural selection. I choose to use herein the time-honored practices of IRE wherever I regard the changes therefrom as a step backward. Here are some examples:

| **IRE (I use)** | **IEEE (Mandatory)** |
|---|---|
| db | dB |
| Kv | kV |
| μf | μF |

I have retained the contemporary usage of frequency in "cycles" (per second) because it was the IRE standard and hence prevalent in the references from the subject period in time. This does not reflect any personal objection to the present term "hertz", which was used in Germany long before.

For the natural unit of attenuation, I use the spelling "napier", which I regard as correct. It was established when it was first needed. [A][C] I adopted it as soon as I had a need. The descendents of John Napier adopted this spelling when English literacy progressed to the point of requiring consistency. [D] It is the base of the accepted adjective "napierian". The change to "neper" as the IRE Standard and eventually the International Standard was a step backward, in my opinion. [B] Fortunately there is no confusion caused by either spelling.

The keying of references has presented a problem of correlation between close support and the comprehensive lists appended. Here are some rules that I have adopted.

- The keys used in my previous book (Hazeltine the Professor) have the same meaning where used herein:
  - [H1]-[H14] Articles by Hazeltine.
  - [P1]-[P36] U.S. Patents to Hazeltine.
  - [1]-[26] Other References (1 or 2 digits).
  - [A1]-[A12] References in Appendix 1.
- These further keys may be used in this book:
  - [W1] . . . Articles by Wheeler.
  - [WP1] . . . U.S. Patents to Wheeler.
  - [WL-1] . . . Wheeler Labs. series of reprints, from 1947. Various authors.
  - [WM-1] . . . Wheeler Monographs, series of 19, 1948-54. Author, Wheeler.
  - [R1] . . . Hazeltine lab reports in the Hoboken-NY series from no. 1.
  - [R1W] . . . Bayside-LN series from no. 1W.
  - [R1Y] . . . Chicago series from No. 1Y.
  - [101] . . . Other References (3 digits).
- References in each chapter are keyed alphabetically:
  - [A][B][C]
- Illustrations in each chapter are keyed numerically:
  - Fig. 1, etc.

Any mention of a chapter reference in another chapter requires identification of the host chapter.

A U.S. Patent is identified by its serial number (7 digits) and its issuing/filing dates may be stated in the form 341223/330112.

[A] K. S. Johnson, "Transmission Circuits for Telephonic Communication", Van Nostrand; 1924. (The natural attenuation unit, napier, p. 9.)
[B] IRE, "1933 Standardization Report", 1933. ("A napierian unit called the neper", p. 76.)
[C] E. A. Guillemin, "Communication Networks", vol. II, Wiley; 1935. (The napier, p. 76.)
[D] H.A.W., "The Spelling of the name Napier", IRE Trans., vol. CT-2, p. 219; Jun. 1955.

# Section 1. Personal Account

## 1.0 Personal Account.

The following 12 chapters deal with my life before Hazeltine Corporation and with my continuing activities outside the Company. This account starts with my place in the family of my parents, continues through my education, and closes with the formative years of my own family. Here are the background and events that provided the preparation and support for my participation in the Company.

**1.1 The Family I Joined 1903.**

My father and mother were both outstanding people and made a happy home. Each one lived to age 92, overlapping many of their great-grandchildren. This contrasts with their parents in more hazardous times, with less stability and opportunity, none of whom lived past age 65. The last survivor of my four grandparents died in the first year of my life. However, they must have left a rich heritage of strength and talents. I and my four (younger) sisters and all our children and grandchildren are living and active.

It is not clear, from what line came my aptitudes in math, science and engineering. The only traceable heritage came from my Grandfather Wheeler, who built and operated flour mills powered by water wheels and made mechanical inventions. His only other grandson was my cousin Leon Wheeler (12 years .older), who graduated from U. of Washington in mechanical engineering. In World War I, he enlisted, was commissioned in engineering work relating to munitions, and was lost by torpedo. My father had superior talents and education in science.

My father was William Archie Wheeler, (18)760628-(19)681008. He was called "Archie". His parents came from N.H. and were married in Minn. In the early years of New England, Wheeler was the most common of family names. My father's growing years were strenuous. He was the youngest of three children, having one sister 6 years older and one brother 12 years older. He was fortunate in parental care and good health. When he reached the new Minn. School of Agriculture (a prep school) and then Minn. College of Agriculture, his talents were recognized and he was offered real opportunities to earn his way. His M.S. in Botany (and election to Sigma Xi) in 1901 were real accomplishments in that day. He served as instructor in the School for 2 years, then 4 years as professor in the S.D. State College in Brookings. Then he moved to Mitchell as one of the founders of Dakota Improved Seed Co. (DISCO), where he served as secretary and manager, 1907-16. He was recognized as the pioneer in the breeding of hardy alfalfa. At age 40, he went to the U.S. Dept. of Agriculture in Washington. He started in charge of the Hay, Feed & Seed Division of the newly organized Office of Markets, which grew to the Bu. of Agricultural Economics. He remained active 30 years until retirement. His specialties were marketing, reporting, standardization, inspection and regulation. After retirement, he published four monumental volumes, the last at age 89.

- Forage and Pasture Crops, Van Nostrand, 1950. A comprehensive handbook written from his experience.
- Grassland Seeds, Van Nostrand, 1957. A compilation of articles and information from many sources.
- Wheeler-Alden Family, 1962.
- Alden-Shedd Families, 1965.

The last two are geneologies of the families of my father and mother. They are monumental compilations of the family tree and biographical sketches which were his hobby after his marriage to a direct descendant of John Alden and Priscilla. In the Wheeler-Alden book, we are fortunate in having a 26-page autobiography of my father. (Also it contains an 8-page autobiographical sketch I wrote about 1961.) More recently, I wrote a booklet entitled, "My Memories of Mitchell 1907-1916". [W2]

I remember my father as tall and slender, with a kindly stoop and an engaging smile. He had blond hair, which he retained to the end. He worshipped my mother, and never said an unkind word to her or about her. Everyone who knew him liked him, and did not mind some immature mannerisms. He fell asleep at some inopportune times, a trait that I inherited. He never drove a car. He was in good health and mentally alert, and took care of my mother, until his brief last illness at age 92.

My mother was Harriet Maria Alden, (18)790910-(19)720428. She was called "Hattie". Her parents came from western N.Y. and were married in Minneapolis. Her ancestors in the early days of New England, John Alden and Priscilla, were later made famous by Longfellow. She was one of a large family of children, older and younger, who were outstanding in their talents of various kinds. Her schooling continued through High School level, and she did very well in school. Instead of college, she worked 3 years as a secretary to help support the family. She had five children. She gave my father her full support without interference in his work. She was a conscientious home-maker, perfectionist to the extent of her strength. She was a born teacher and excelled in bringing up children. She excelled in cooking and sewing. In the years just before and after her marriage, she sang solos and played the piano (accompaniment). Her health was generally excellent, but required some attention. Her thoughtful economies and hard work were a major factor in the family's standard of living.

I remember my mother as not very tall, trim and erect. She had a light complexion, beautiful features, dark hair and bright eyes. She was assertive but not aggresive. Everyone liked her and admired her talents and her natural dignity. She never drove a car. She took care of my father until their last move to a retirement home, when her mental capacity was slowly failing. She worshipped her children, grandchildren, and great-grandchildren, until her last illness at age 92.

My four younger sisters were:

| | | |
|---|---|---|
| (1) Helen May Wheeler | 050425 | Brookings, S.D. |
| (2) Margaret Wheeler | 080213 | Mitchell, S.D. |
| (3) Catherine Wheeler | 131216 | Mitchell, S.D. |
| (4) Harriet Wheeler | 160831 | Washington, D.C. |

They were bright girls, did well in school, and contributed much to our family life. Each one has 2, 3 or 4 children. Helen and Margaret graduated near the top from GWU. Helen made a career. before and after marriage, in the field of economics and statistics. Margaret continued to a master's degree in physics and math, then taught these subjects in Washington high schools until she was married. When her two sons finished college, she returned to GWU as Assistant Professor of physics. Catherine and Harriet were less motivated to scholorship. Catherine graduated from GWU, while Harriet changed course after two years in GWU. Margaret was the most talented in music and science. Her elder son excels in music, also his wife and children. Her younger son excels in science (Ph.D. in organic chemistry).

My father and I were of such ages as to miss military service in World War I and World War II. My father did important Government work during both wars, as I did during WW-II. Other members of our family have served in various ways.

- Catherine's first husband, William Lines (the father of her 3 children) was an Army Reserve Officer, advanced to Captain early in WW-II, and lost his life in a ship explosion at Pearl Harbor in 1944.
- Harriet's second husband, Ralph Hobdey (the father of her 4 daughters) saw much active duty in the RAF of U.K., 1940-47, advancing to Flight Lieutenant (navigator).
- Margaret's elder son, Arthur Montzka, was drafted 1957-59 and played the violin in the Seventh Army Symphony Orchestra on its goodwill tours in Germany and all over Europe.
- Catherine's elder son, Richard Lines, chose a Navy career. He enlisted in 1959 and has advanced to Lieutenant Commander. On Navy scholarships, he received the degrees of B.S. and M.S. in C.E., then transferred to the C.E. Corps. In 1971-78, he served at Guantanamo Bay, Okinawa, Puerto Rico and Guam.
- Catherine's younger son, Bill Lines, enlisted in the Air Force 1962-66. He served as radar operator in U.S. bases (the Sage network), and at Clark AFB in the Philippines (service in Thailand and S. Vietnam).
- Helen's son, Harold Richards, volunteered in the Air National Guard 1965. He served as aircraft mechanic in the USAF (servicing the F-100 a year in S. Vietnam) until he left with Sergeant rank in 1969.

My parents were married in 1901, at the same time my father received his M.S. degree from the U. of Minnesota. They lived nearby the next 2 years, while he worked as an instructor in the School of Agriculture, a preparatory school for the College of Agriculture. Shortly after I was born in 1903, they moved to Brookings, S.D., where he served 4 years as professor of botany in the S.D.

State College. Then they moved to Mitchell in 1907, with me and my first sister, Helen. That move is my earliest memory. They were fortunate in finding a nice frame house which served our family very well for the next 9 years. It was fairly new, and modern by the standards of that day. My second and third sisters, Margaret and Catherine, were born in that home.

Our father and mother were a wonderful team and our family relations were as nearly ideal as could be imagined. Kindness and love, respect and admiration, were the cornerstones. There was a spirit of cooperation and enough discipline. Good habits and cleanliness were taken for granted, by the example of our parents. No alcohol, no tobacco. In some other areas, their strictness may have gone to extremes. No playing cards, which were identified with the saloon and gambling. Sunday was a day for dressing neatly, going to church and making friends with neighbors. The cooking for Sunday dinner was done the day before.

My father was dedicated to his work, and his salary was adequate for a comfortable home life. He was manager of the newly organized Dakota Improved Seed Company (DISCO) in Mitchell, which grew and prospered. His office was near the Milwaukee station. Their warehouse and shipping dock were on a siding by the Omaha tracks. He travelled some, but usually not far. At home, he did most of the repairs and maintenance, also many improvements. He was very handy with tools, after working with his father. He varnished the floors, painted the woodwork, and papered the walls. He would have enjoyed more work outside on the lawn and garden, but he regarded that as a pleasure he could forego.

My mother was dedicated to the home and family. She was very bright in school, but gave up college so she could help her family. Before her marriage to my father, she worked 3 years as a secretary in some professional offices. She must have been extremely skillful, but I do not recall her even owning a typewriter afterward. Housekeeping for a growing family was a fulltime task in those days, and she was a perfectionist. She found time to curl her hair and dress attractively. Her cooking was wonderful. We ate our daily dinner at noon on a linen tablecloth with linen napkins in initialed rings. She sewed much of the clothing for herself and for us children. In the Fall, she made canned goods (in glass jars) for a treat during the winter. Otherwise, our menu was seasonal, except for meat and dairy products all year.

My younger sisters looked up to me. I did not fully appreciate them until later years. I wish I had. I envied any boy who had an older sister.

Most of the years in Mitchell, my father's sister lived with us. Aunt May Wheeler was older than my father, and never married, so she had no other home. She was retiring and conscientious, and was crazy about us children. For a while she worked as housekeeper for a family of boys who had lost their mother. Her presence at home was an emotional strain for my mother, sharing my father with his sister. On the other hand, Aunt May took good care of us for a few weeks, the one time my parents took a long trip (to the East).

As our household in Mitchell grew to 6 or 7, the 3 bedrooms were hardly adequate, so I slept on the screened back porch. It was consistent with the fresh-air principles of that day, but rather strenuous in the winter. The screens were covered with canvas in cold weather.

Our move to Washington, D.C., in 1916 completely altered our family pattern but not our close relationship. From then until I married in 1926, it was still my home, and I was very fortunate. My father had found a stucco house in a new development in Chevy Chase, D.C., near the fashionable suburb of that name in Maryland. It was 5 miles from my father's work in the Dept. of Agriculture, near the White House. There was good street-car service, third-rail in town and the Connecticut Ave. trolley out to Chevy Chase Circle. Our house was a half-mile walk from the trolley. The address was 5503 33 St. N.W. The phone number was Cleveland 1238.

My father's salary in his new job was much more dollars, but expenses were much greater in the city. My fourth sister, Harriet, was born soon after we moved. World War I was boosting prices. The first epidemic of the flu (Spanish influenza) was a terrible blow to Washington, wiping out whole families. We were fortunate, taking it in two shifts, so we could care for each other. First my father and the school children brought it home. Then my mother, Aunt May and the babies had their turn. Each of us was in a coma for a couple of days with a fever around 105. I remember reviving. My first question was, "What day is it?" My second was, "When do I eat?" I was recoverig rapidly, but left with one scar, the beginning of a bald spot on top.

Surviving this period was a severe struggle for our parents, from which we children were largely protected. Our father had regular hours and half-day on Saturday, so he shared some of the house work. Chain stores for food were just starting downtown, so he carried home much food, standing on the crowded streetcar. My mother hardly left the house for a few years, and seldom afforded any new clothes. Few people could afford a car, although our home had a garage in back. Our life style was simplified by some meals on the kitchen table, closer to the cooking. Our greatest economy was our mother's skill in preparing delicious meals from raw materials on the gas range.

That was the time when technology was reaching the home. The highlight of our first Christmas in Washington was a wind-up phonograph with disc records. The kitchen table was supplemented by a Kitchen Cabinet, with electric toaster and electric mixer. The icebox was replaced by the Frigidaire. The coal fire in the furnace was replaced with a Quiet May oilburner controlled by an electric thermostat. We had an electric fan for the hot summers. (It was a change from Mitchell, with a dry climate and cool nights.)

My sisters were wonderful girls. They did well in school and were much help to our mother. I did not do my share at home, because I was always preoccupied with my projects in addition to school. Later they proved to have been a good investment.

The church affiliations of my family had three phases:

- In Mitchell, the Congregational Church, which was the least ritualistic of the several churches in town.
- In Chevy Chase, the first five years after moving, the Presbyterian Church, which was the nearest to our home.
- In Washington, from 1921 for many years, the All Souls Church (Unitarian), which was chosen as most hospitable to the views of my parents.

Each of these served a purpose.

In Mitchell, we attended church and Sunday School regularly and participated in social activities. The building was new, attractive and pleasant. It was the nucleus of one circle of friends, the other circle being our neighbors. Sunday School was fun and we were given some introduction to our heritage. My father served as Superintendent for a year or two. The annual picnic was an event.

In Chevy Chase, our parents were overloaded with housework, and seldom "went to church". Here again, it was a nice building, the Presbyterian Church facing Chevy Chase Circle, within easy walking distance. We children attended Sunday School and made some friends in addition to our public school. The narrow views of the Minister and the denomination did not bother me, because I had self confidence and no particular sensitivity. It came to bother my sister Helen. When my parents became aware of this problem, they decided to go to another church, though much less convenient to our home.

As a result, on 210123, our parents took the family to All Souls Church in the city. It was a new experience we all enjoyed. The church was ostracized from the orthodox Protestant community of churches, then and for many years afterward. Ex-President William Howard Taft (then Chief Justice of the Supreme Court) was its most famous member and a regular attendant. The Minister, Dr. Ulysses G. B. Pierce, was a brilliant historian and philosopher. He was a most engaging preacher and teacher. The pastoral organization was active in many ways — religious, civic, social.

In 1921, that church was in transistion from an old location to a new building to be located at Sixteenth and Harvard Streets (two miles north of the White House). During the transition period, the Sunday morning church services were held in a large theater. First it was the Knickerbocker Theater (movies), at 18 & Columbia Rd., until that building collapsed under heavy snow on 220128. Then it was Keith's Theater (vaudeville) near the White House. From 1924, the new building provided spacious quarters for all activities. Social events, including dancing in the social hall, were a special feature which gave us much pleasure. Ruth and I were married there 260825.

After moving to Washington, we found a piano teacher so we could continue piano lessons on our old Franklin upright piano. After a leisurely pace for a few years, we found Mrs. Elizabeth P. Prindle, an inspired teacher with excellent training. My first lesson was 200221, and I made rapid progress. My talent was memorizing the classics, which I enjoyed. My lessons were crowded out after graduation a year later, but the results gave me much enjoyment for many years. After some income from my Hazeltine affiliation, I bought a new piano, a Steinway Model A Grand, on 240221. This occupied a focal place in our home from that time. After 55 years, it is now being used in the home of Margaret's son, Arthur Montzka, whose family is very active in music.

Late in 1924, with my Hazeltine income, I was able to buy a car for our family. To carry the family of seven, we needed the Buick sedan with jump-seats. The salesman gave me a couple of driving lessons on Mass. Ave. (there was little traffic). I taught some other members of the family, especially my second sister Margaret who became the alternate driver. This was my last year at GWU. I used the car a lot for transportation to school and for taking people around, especially Ruth.

In those days, a vacation was not taken for granted. Our entire family of 7 enjoyed one short vacation 250908-10 for my mother's birthday. In 3 days, we drove 450 miles in Virginia, visiting Luray Caverns and Monticello. On one straight road, we drove 60 miles per hour, the only time I remember in that car.

In our Chevy Chase home, we entertained guests frequently for dinner or an evening party. Our only relatives in Washington were my cousin Alden Grimes from Minneapolis and his family. We had a few friends from our past in Minnesota and S.D. We gained numerous friends from our contacts in the neighborhood, in my father's work, the church, high school and college. Large groups assembled in our large living room for special occasions. We had various groups of young people for dancing to the phonograph and later to the radio. When we were showing off my radio in the early days of broadcasting, we had as many as 30 guests at a time to hear KDKA on the "loudspeaker" (a Baldwin earphone attached to the phonograph horn). For some who were not present, I held the radio earpiece against the telephone mouthpiece. We had fun.

My four younger sisters all did well in school. We attended the new Central High School (or later Western High School). The GWU offered 7 scholarships, Some things you will not believe. The grocery clerk called at the door for our orders, to be delivered later the same day. He had a one-horsepower vehicle. This system recognized that not everyone had a phone or a vehicle. (We had a phone but our only vehicle was a one-boypower four-wheeler. I never had a bike.) We had sidewalks. The roads had some ruts but no potholes. The doctors made housecalls as a rule, not as an exception. The more prestigious doctors were getting electric or gasoline cars. Babies were delivered at home. For a "specialist", we travelled 300 miles by train to Minneapolis.

We walked to school twice a day, with midday dinner at home. It was little more than half a mile, but the snow might be several feet deep or making a blizzard. The temperature was seldom below -20°F, but sometimes also windy. The school had long hours but no homework. (Imagine, no books to carry!) We bought our books and enjoyed the pride of possession.

The public schools in Mitchell were excellent. They used the 6-2-4 system, which is now the preferred plan. Grades 1-4 were in a neighborhood school with a bell. Grades 5-8 were in a central school, with departmental teachers in 7-8, also cooking and carpenter shop. I did not stay for the high school, grades 9-12. The classes were orderly and not crowded. The teachers in grades 1-8 were young women, mostly not married yet, who were friendly and dedicated. Before high school, all studying was done at school, with no homework except a special assignment now and then (such as a relief map or a report on the Panama Canal). The nearby Carnegie Public library had Encyclopedia Brittannica. (Later we had at home the first popular-priced edition, a reprint of the 1911.) I spend much time at the library.

I had a few weeks at a private kindergarten. Then I was kept out of the first grade for reasons of health, which I have never understood. I fear it was an overprotective reaction by my parents. When I started in first grade a year late, the teacher had enough in a few days and put me directly into the second grade. My penmanship was beautiful and my spelling grade for the year was 100%. I worshiped my teachers. When we had any special performance, I was always given the key role (such as George Washington or Uncle Sam). My mother made the costume. On Memorial Day, all the schools marched in the parade out to the cemetery, led by the few surviving Civil War veterans. We were paced by the volunteer brass band of the town, which played regularly in the bandstand in the City Hall park.

There were several churches in town. We attended the Congregational Church and Sunday School, which was a healthy organization. I always wanted to ring the bell, but was never allowed to.

Every home had an upright piano. My mother played the piano to accompany singing. I took lessons from the daughter of a family who were near neighbors. It came easy to me.

Out of school, especially during the long summers, we were outdoors most of the time. Sometimes it was voluntary and sometimes "directed". My hair was clipped at the beginning of each summer, which made for a thick (warm) mop the next winter. The four wheels from my first "express wagon" served seasons of soap-box racers. The tires were iron hoops, with no natural affinity for the spoked wood wheels. My right knee and my left sole took the most wear-and-tear. Our favorite coasting route was the sidewalk of a long hill down to the bumpety crossing of the Milwaukee tracks (main line, no protection). (Now it is an unexciting overpass.)

My happiest hours were spent in the rail yards of the Omaha terminal, the end of the line. I loved the steam locomotives, the roundhouse where they were serviced, the freight cars on which we raced along the ridge, and the passenger cars with plush seats for a siesta. It was before the days of too many lawyers and liability suits. There were no fences. An engineer would let me ride in the cab, pull the whistle and ring the bell. It was dangerous; there were one or two passenger trains per day, plus the freight. The cinder bedding in the hot sun was a handicap when I was barefoot, as I usually was.

I guess my second choice was the waterworks by the reservoir, where the pumping was done by a large, one-cylinder, horizontal steam engine with a big flywheel and a beautiful centrifugal governor. It was a showpiece, with polished brass and everything so clean. The rhythm of the pumping station could be heard when we watered the lawn.

After dinner on a long summer evening, we usually played games until dark. The night skies were dark with brilliant moon or stars. There were a few days in 1910 when we were treated to a rare sight from our front porch. Shortly after sunset, Halley's comet was a pillar of light in the western sky. (I hope to see it again in 1986.) Of course, it rained sometimes, but only on the Fourth of July or the Sunday School picnic.

My idol was the boy next door, who was one year older and the nearest I had to a big brother. His name was Paul Reamer and his father was a dentist. One Christmas when I was about 9, he had a new construction set, what we would now call Erector. (His was the American Model Builder, which competed with the higher quality Meccano from England. Soon afterward came A. C. Gilbert's Erector, which eventually acquired both of the others.) From the moment I sat on the floor with that set, I had no other interest, not even dinner. Paul was less interested. From that time, my father provided me with a generous complement of such sets, usually whatever I wanted, because my demands were modest. The parlor floor became my workshop. After soapbox racers, those sets were my second experience in engineering. I learned to do with the limited supply of parts, and I learned the meaning of stresses and tolerances. I soon graduated from copying the pictures in the instruction books. I made models of more sophisticated design and electric power, until high-school days in Washington. The experience gave me insight into mechanical problems encountered in my later work. Also I played with Erector again for a few years when our son reached an age that gave me an excuse and some motivation.

The Reamers next door had an empty stable behind their house. They bought an early gasoline car to live in the stable. It was a two-seater with no roof or doors. It had a windshield and acetylene headlights. My first ride in a car was one frosty morning when Dr. Reamer called to me and said, "Get in, we'll go for a ride". We raced along a straight dirt road in the country, up to 35 miles per hour. Two other doctors in town had enclosed electric cars, which their wives could enjoy driving.

When I was 10, I was invited to a daily afternoon job carrying papers. It was the Mitchell Daily Republican. I was one of a dozen residential carriers, with about 50 customers on a two-mile beat, $1.25 for 6 days of the week. After school every day, I watched the papers being printed, stood in line to count my quota as they left the press, and carried them (not very heavy) in a shoulder bag. It was wonderful exercise and discipline (after dark in the winter). We moved just at the end of a contest of carrier performance, and I won the first prize, a $20 gold coin that I was allowed to keep through the FDR dynasty, and still have. That job gave me lots of time to dream about things in one of the catalogs I stowed in my bag (along with some candy or cookies). my customers were my friends, including some of our close neighbors. One played the accordion, so I sometimes rested on his steps. Another was my piano teacher. Sometimes I even read the paper. World War I was just starting. The news came by wire telegraph (Western Union).

My closest boy friend, in the same grade, was very different from me. Malcolm Ronald was the son of the newspaper publisher. We often walked home from school together. He was a literary type, who read or recited poems to me. One sea ballad, I could still repeat. He listened intently to my plans for making models, such as a wooden submarine. He stayed in the West for college and returned to Mitchell to carry on with his father's paper. He was more liberal, so it became the Mitchell Daily Republic. I kept in touch with him and visited him and his family in later years.

Scientific education was brought to Mitchell by itinerant lecturers. I remember four, on the topics of the gyroscope, liquid air, ultraviolet light, and wireless. The last was radio signaling the length of the stage, from a loud spark transmitter to a coherer receiver. It was about this time that I was deciding on radio engineering as career.

Just before we left Mitchell in 1916, an older boy in town afforded an amateur station with a fairly powerful spark transmitter. He was Max I. Black, who was to become a close friend in later years. He built a radio remote control, which I saw him demonstrate in front of the local theater. It was controllng a Lionel electric train. On the Fourth of July, just after we left, he demonstrated remote control of a 5-foot model boat at the nearby picnic grounds (Tobin's Park, on the James River, a narrow, winding stream). I missed the show but later read about it [C] Max went to the Navel Academy in Annapolis while we lived in Washington, so we became close friends. He went into Naval Avaition.

From Mitchell, our contacts with the outside world were a few trips. I remember a couple of trips to Sioux Falls, where we had friends, and to Minneapolis, where we had numerous relatives on my mother's side. I enjoyed the train trips, and especially wandering around Minneapolis alone on the trolleys. On one business trip, my father took me along to Chamberlain for a trip up the Missouri River to the lower Brule Indian Reservation. The steamer was a stern wheeler.

In these travels, my father noted that I retained a natural sense of direction. Then we traveled to Minneapolis, Chicago and Washington by train. The last lap was an overnight trip on a sleeper. Arriving in Washington the next day, I was disoriented and never again had any sense of direction.

[W2] "My Memories of Mitchell 1907-1916"; 1978.

[A] "Mitchell — the Corn Palace City of the World", Educator School Supply Co.; 1912.

[B] "The World's Only Corn Palace", The Goin Co., Mitchell, S.D.; 1978.

[C] H. C. Van Benthuysen, M. I. Black, "A radio controlled model boat", Electrical Experimenter; Oct. 1917.

[D] The Daily Republic, Mitchell, S.D. 57301; July 10, 1981. (Centennial special issue.)

[E] H.A.W. Letter to Editor, The Daily Republic; Sep. 5, 1981.

**1.3 Washington 1916-26.**

We moved to Washington, D.C., the middle of 1916, when I was 13 and had completed grade 7. That was my home base until I was married in 1926, after my first year at Hopkins. This was a momentous decade in my life, which saw my transition from a beginner in electrical experiments to a professional in radio engineering. I joined the IRE in 1926 as an Associate, the grade provided for college graduates while gaining experience.

Here I shall outline the circumstances leading to my activities as a radio amateur, my studies at George Washington U., my two summers at the Bu. of Standards, my introduction to Prof. Hazeltine, my employment in Hazeltine Corp., and my studies at Johns Hopkins U. All of these topics are covered individually.

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| 1919-23 | Radio Amateur. |
| 1921-25 | GWU. |
| 1921-22 | Summers at Bu. of Standards. |
| 1922-23 | Prof. Hazeltine |
| 1924 . . . | Hazeltine Corp. |
| 1925-28 | JHU. |
| 1926 | Marriage. |

Our move from Mitchell to Washington was a step forward in the opportunities for advanced education. It was a step backward in the grade schools. Washington was not a progressive city before World War I. In grade 7 in Mitchell, I enjoyed a departmental system similar to Junior High School, 4 teachers with small classes. In grade 8, in a high-class suburb of Washington, we had a crowded classroom with a single teacher who was also the Principal of an 8-grade school. I was well prepared for the transfer.

However Washington was improving. The next year, I took the trolley to the new Central High School, which was an outstanding school for grades 9-12. I elected a college-entrance curriculum with most of the available technical courses. In some degree, these were incompatible, I took French and Spanish. I should have had Latin, French, and German. High school, not college, is the right time to learn a language. I took Chemistry and Physics, but I missed Biology. I had good preparation in English and Math. In my junior year, grade 11, I headed a debating team which did very well. I graduated near the head of my class. Later I enjoyed the class reunions after 40 and 50 years.

The most beneficial activity I experienced in High School was the High School Cadets. It was "compulsory", and I had no conflicting activities. It was mostly outdoor marching and discipline. I enjoyed the marching and the uniform. It did wonders for my health and posture. In the annual competitive drill, I was in the first-place company the first year, and the second-place in the second and third years. In the fourth, I was disappointed not to be appointed captain of a company. Instead, I was appointed one of two captains on the regimental staff, the quartermaster. In retrospect, I recognize that that was a wise decision by the faculty and it gave me more freedom for other activities, especially social and radio. Every cadet company had an annual dance in the armory. Being on the staff, I had to go to every one. We marched in the inaugural parade of President Harding.

The Cadets provided the principal social activity in the high school, although there were athletics and various lesser activities. Any of the others did not enjoy as wide a base, which was a majority of all boys. The Cadets introduced me to ballroom dancing, but not until my last semester. My parents' background and the Chevy Chase church had frowned on dancing. Then two events changed all that. On 210123 we transferred to the Unitarian Church, which encouraged dancing among social activities. On 210211, my mother was invited as one of the two chaperons for the first cadet dance of the season, in the school armory, and she saw what wholesome fun we were missing. So I had a few lessons on the basic steps of the day (waltz, fox trot). The teacher was not much, but I got along. I enjoyed the music and the rhythm. During the rest of the semester, I never missed a dance. Consistent with the code of that day, I invited my best girl only half the time, and others the other half. The music was some group of high-school boys, moonlighting (no vocal, no loudspeakers). It did wonders for social poise. I really enjoyed the dancing of that day, and I regret that I had only marginal instruction. After our marriage, we had Arthur Murray instruction in New York (in the days when it was still "individual") and dancing was our greatest pleasure for many years.

The Washington high schools held a competitive examination each year for 7 scholarships to George Washington U., each one providing 4 years tuition (then $170 per year). I ranked third, close behind a girl who was a brilliant student and a boy with a photographic memory, both friends of mine. (In our English class play, Thelma Hunt had played opposite me in Barry's one-act play, "The Twelve Pound Look".) (Alan Boetcher went on to the Patent Office, for which he was particularly suited.)

Living at home while attending GWU proved to be an important advantage in my development. In enabled parallel activities which gave me a head start in my chosen career. Also I enjoyed social activities that contributed much to my future. I sometimes wonder what would have happened if I had gone away to a higher ranked college (such as MIT) and thereby had missed some of the events which contributed so much to my progress and to the support of our family.

In Mitchell, my exposure to engineering had been working with mechanical construction sets ("Erector") and watching steam locomotives. On moving to Washington, I spent the first summer in the "old museum" of the Smithsonian Institution and in the Public Library. Both were convenient by streetcar.

The museum contained a large collection of the working models that the Patent Office used to require for every invention. Some were exquisite mechanical models made of hardwood parts (including wheels and gears). Some were electrical devices, such as telegraph. Some were early aeronautical devices (gliders and flying machines).

The library had a section devoted to electrical experiments and model aeroplanes. I read all the books on those subjects (perhaps a few dozen) and dreamed the completion and operation of many devices. My favorite books were a few by Thomas M. St. John. He told how to make many electrical experiments with only the simplest tools.

Our house had a one-car garage in back, which we did not use until 1924. It was designated as my workshop. The furnishings were one large packing box (from our moving) as a workbench under the window, and a few smaller boxes piled up for shelves. The garage had a small door toward the house, so the large door was left closed. It faced the back alley which served as a driveway for the houses in our block. The temperature variation was tolerable, except in cold weather. After one or two summers, my shop moved into the basement, which had a door at ground level in the rear.

My garage workshop attracted some of the neighbor boys, who happened to be younger. I set up elementary electrical models, such as telegraph key and sounder, made of the most elementary materials plus a dry cell and a spool of insulated wire. My first motor was a tribute to my mentor, St. John. It was a bipolar armature (2 iron bolts, wound with magnet wire) a permanent-magnet field (a crude magnet from an obsolete telephone magneto), and a commutator tied onto a wood-dowel shaft with thread and glue. The bearings were two wood screws in holes in pieces of a tin can. It turned at a terrific speed, for fun but not for useful load. It made sparks. I still have it. The rough wood base is painted black to resemble the hard rubber then used for high-grade insulation.

Next I made simple galvanometers for measuring weak currents. One type compared the acidity of potatoes and apples by inserting two wire probes, one copper and one iron, to form an electrolytic cell.

Unfortunately, the makeshift methods of construction held a peculiar fascination for me. It was years before I fell from grace and adopted metal-working drills, soldering and storage batteries, all of which were available for amateur use. Shortly I did make a coil winder with hand crank, which was needed for thousands of turns of fine wire (as on a small spark coil).

While interested in model aeroplanes and some other things, electricity had captured me, in transition to the radio which I read about. From my early introduction to surveying, I made a plane table on my father's tripod (for camera) and an alidade (of wood). Then I surveyed our house and lot. My construction sets occupied most of my spare time in the winter. I had a generous kit of parts and an electric motor, from which I made some interesting models.

My radio work naturally centered in my bedroom, which was not uncommon among amateurs. A high aerial was desired, and the bedroom was closer to high points for mounting. During World War I, the Government prohibited radio reception by anyone not authorized. That was a silly rule which could not be enforced. Like many of my friends, I connected a crystal detector and headphones between a metal bedspring and a radiator pipe. This was my first experience receiving signals. The high-power Navy station (NAA) in nearby Arlington, Va., sent time signals at noon and 10 PM. After the war, I strung an aerial between our house and the next house, over the intervening vacant lot. That was used for many experiments in reception, and especially for my amateur transmitter. On 210924, it was superseded by a long wire on the opposite side of the house, and my radio work was moved to my laboratory on that side of the basement.

At the end of the war, when restrictions were removed, there was a rash of amateur activity. It fell into two categories. One was reception of all kinds of signals, especially in a struggle for long distances (socalled DX). The other was two-way communication with an amateur license. This was before radio telephone broadcasting, so code reception was the rule. I first made crystal receivers, then an amateur transmitter, then many kinds of receivers for the broadcasting which was just starting.

I have retained only two of my log books, one for 1922 and one for 1923. I have diaries from 1920, which cover the period of my amateur station 3QK. They begin in the middle of my junior year in high school (grade 11).

At that time I was using the famous Circular 74 of the Bu. of Standards for radio theory and computations. I was visiting the famous Frederick A. Kolster, then head of the Radio Laboratory at the Bureau, to talk about a summer job. (Instead, I worked one more summer in my father's office.) I was reading the periodicals, Electrical Experimenter, Wireless Age and Radio News. A neighbor of ours was the famous Cdr. A. Hoyt Taylor. At his home 200229 I first saw an advanced Navy receiver operating on a loop antenna. My station (3QK) was licensed 200424. I was receiving the experimental phone transmissions from the Bureau (WWV) and exchanging messages with my spark transmitter. Later that year, I became acquainted with Mr. Kolster's successor, Dr. John Howard Dellinger, who became a lifelong friend of mine and also of my father and mother. I bought a used typewriter, a Corona portable.

Toward the end of 1920, my father was inaugurating the Radio Market News Service of the Dept. of Agriculture. I received the first message 201215. This event was a factor in my employment two summers at Bu. of Standards and in my introduction to Prof. Hazeltine.

One of my father's special interests was market reporting on farm products. He became Chief of a new Division of Marketing Information.

Watching my amateur activities with code transmission, he perceived an opportunity for broadcasting his market reports to agents distributed over a farm area. They could rely on local amateurs for the code reception, then post the reports in public places. In 1920, he planned this service, which became common in the midwest in 1921. It used code transmission from the Post Office network of medium-power stations. In that year came the rapid growth of radio telephone broadcasting (the AM service, as we know it today). Then code reception was superseded by voice, which anyone could receive.

My father had many good ideas, and he was effective in getting action. He conceived a plan for getting me a summer job at Bu. of Standards right after graduation from high school. It may have been regarded as a recognition of my part in the Radio Market News Service in Dept. of Agriculture and the cooperation of Bu. of Standards in that project. He got Dept. of Agriculture to employ two Laboratory Assistants for assignment to the Radio Laboratory for work helpful to the News Service. The appointments went to Herbert F. Harmon and me, starting in the summer of 1921. He had just graduated from Grove City College, near KDKA in Pittsburgh, so he was 4 years my senior in formal education. We became good friends, although we were much different in our approach. My work during that period is the subject of another section. It led to my independent invention of the neutralization which was Prof. Hazeltine's earlier invention.

Radio broadcasting of sound programs goes back to 1919, with transmission from Westinghouse experimental station 3XK in Pittsburgh. As KDKA, it was the first to be licensed for broadcasting, just in time for the Harding-Cox election returns 201102. In 1921, 30 more stations were licensed, and the next year the number grew to hundreds. They were all nominally on two frequencies (360 and 400 meters, or 833 and 750 Kc). This made a bedlam of interference after dark, when long-distance propagation occurred.

Radio licensing was under the jurisdiction of Bu. of Navigation, Dept. of Commerce. It was apparent that some more regulation was needed. Pres. Harding called a National Radio Conference under chairmanship of Herbert Hoover, then Secy. of Commerce. Each of the Departments was invited to appoint a representative. In view of my father's Radio Market News Service, he was designated to represent the Dept. of Agriculture. The first of four annual sessions convened 220227. This was in the winter between my two summers at the Bu. of Standards. My father took me the first two days as a visitor. Among the representatives were several expert consultants from the profession. My father and I were already acquainted with Dr. Dellinger and with Prof. Jansky from U. of Minn. Also he introduced me to other leaders with whom I was to become acquainted in years to follow: Dr. Goldsmith from CCNY and IRE; Edwin H. Armstrong from Columbia U.; Prof. Hazeltine from Stevens. These conferences initiated a pattern of broadcasting on about 100 channels (540-1500 Kc). With little change, this is the AM service of today.

One of the features of Washington was the close association with centers of activity in radio. Soon after WW-I, I visited the Navy's key transmitter in the wilderness of nearby Arlington, Va., reached by trolley. It had three towers (heights 600, 450, 450 ft.) supporting a grid of wires. Its frequency was 2650 meters (113 Kc). The large rotary spark gap was a spectacle. A recent improvement was the "quenched gap" we captured from Germany. It was quiet and doubled the efficiency. (After WW-II, the Russians took the German "Goliath" transmitter, then the highest power of any in the world.) We were welcome visitors there and at the Radio Laboratory of Bu. of Standards. Washington had an active Radio Club which I joined and attended rather regularly. I was the speaker early in 1923 to describe the Neutrodyne receiver when it was the latest thing.

The year 1920 saw increasing activity in phone transmission by radio. The year ended with KDKA in Pittsburgh setting the stage for broadcasting programs. I was using a crystal detector until the end of the year, and did not receive KDKA (200 mi.) with my short antenna. I was receiving phone on experimental transmissions from nearby WWV of Bu. of Standards.

In the next year, 1921, I was beginning to use the vacuum tube in a regenerative receiver. I started working at Bu. of Standards. I moved to my new long antenna. My first reception of KDKA at home was 210823. (Also I began to receive KDKA on my simple receiver with crystal detector, 210927.) I added an audio amplifier and made my first loudspeaker. In the manner of that day, I attached a phone receiver to the horn of our phonograph in the living room. The Baldwin ear phone was the first to use an armature driving a nonmagnetic (mica) diaphragm, so it was used for this purpose. That was 211231, the end of the year. By then, there were 30 broadcast stations.

The popular interest in broadcast reception was building rapidly, but long-distance reception was mainly the province of the amateurs. The loudspeaker was uncommon. I dramatized the current progress by inviting large groups to our home for an evening of listening to KDKA on the loudspeaker. On 220325, 28 and 31, we had 3 groups totaling 80 friends from our neighborhood, our church, and other associations. Many others were treated to similar programs over the telephone. I held the radio earphone to the mouthpiece. In addition to KDKA, I heard an increasing number of other broadcasters:

| | |
|---|---|
| WJZ | Newark (200 mi.) |
| WBZ | Springfield (300) |
| WGY | Schenectady (300) |
| WWJ | Detroit (400) |
| KYW | Chicago (600) |
| WSB | Atlanta (500) |
| WHAS | Louisville (500) |
| WOR | Newark (200) |

Occasionally I heard KDOW, the S.S. America, in person-to-person conversations with New York.

Around this time, as a personal favor, I made demonstrations or installed receivers for some of the officials in my father's organization. On 220607, I installed a 3-tube set in Secretary Wallace's home. His interest had been aroused by a demonstration I had given at a garden party he attended 220318 at the home of Dr. Taylor in Falls Church, Va. On 220511 I installed a similar set in Mr. Pugsley's apartment. My choice for these purposes was the Clapp-Eastham regenerative receiver with audio amplifier and a horn loudspeaker. It required an outside-wire antenna, storage "A" battery and dry-cell "B" battery. I made a number of crystal sets as presents to some friends and relatives. Two years later, I made a 4-tube Neutrodyne (for dry-cell operation)for Admiral Gregory (Ruth's father). It was installed 241106.

My transition from the crystal detector to the vacuum tube was not something I planned. I had an unfortunate mindset against spending money on some "luxuries" such as a tube (at $5 each). The result was a few years delay in my experience with radio receivers, just at a critical time in history. The first tube I used, I borrowed from my amateur friend and neighbor, Leonard May, on 201118 (just after KDKA started). I connected it in a regenerative circuit. The first tube I owned was a surplus Navy tube handed to my father 210104 by Mr. Kolster, then chief of the Radio Section of Bu. of Standards. (I had talked with him a year before, about a summer job, which did not materialize.) It was a Moorhead "soft" tube intended for use as a sensitive detector. My next tubes were a few surplus Army tubes (Western Electric VT-1) from my friend, George B. Lacy, 210818. (Then I was working at Bu. of Standards.) With my new long antenna, I first heard KDKA on 210823. Shortly thereafter, I was given a simple receiver for test at home (ABC set from Wireless Equipment Co.) and it included a few of the latest RCA tubes (UV-200 detector and UV-201 amplifier). I was using these tubes by 210920. On 211219, I received 2 French tubes, which I may have obtained from a "club" order by a group at work. If so, these were the first tubes I bought, but at a reduced price.

I had an interesting "consulting" job for Major Casey of the duPont Company in Wilmington. He made beautiful models with the latest colorful plastics of his company. I do not recall how he happened to come to see me 231014. He wanted to make a high-grade receiver, so I advised the Neutrodyne with some luxuries beyond the models for sale. After another meeting 240201, he made the set, saw me again 241008, and invited me to see the set at his home in Wilmington 241107. It was working very well. He drove me past a store which had the latest loudspeaker (Western Electric 540AW cone type) so I bought one and took it home. Soon afterward, he called on John Dreyer at the Hoboken lab, to see if he had any further suggestions. When John saw the unusual features in the diagram, he was astounded, and asked how they happened. Major Casey told Dreyer that he had had some advice from a college boy in Washington, named Wheeler.

I received quite a few unsolicited offers of employment around the time I graduated from GWU. Especially I remember Westinghouse (Mr. Roberts and Capt. Semmes, patent attorney) and the Signal Corps at Camp Vail (Capt. Edwards, Capt. Andrews).

It was typical of our Washington location that we had a generous supplement of activities beyond our school work. We entertained many visitors to Washington, mostly relatives and friends from the midwest (Minneapolis, Mitchell, etc.). In 1921-25, I had a very busy social calendar. This was the time from my last year of high school through my last year at GWU. First I had many dates with quite a number of girlfriends. Also I took my parents and my sisters to many functions. Then I got acquainted with Ruth Gregory in GWU. After our first date (240321), I had few dates with other girls. Transportation was by street-car (or sometimes in a friend's car) until I bought a family car 241022. After that, I devoted much time to transporting family and friends and visitors around the city. I enjoyed it, and my efforts were appreciated. There was seldom any problem of traffic congestion or parking space.

Vacations were not taken for granted, but my work left some freedom. On 240802 I went for two weeks on Star Island in the Isles of Shoals off Portsmouth, N.H. I was a guest as a delegate from our church in Washington. Before we were married, I took a few brief vacations to visit Ruth. From 240831, I visited Ruth a couple of days in Lincoln Park, N.J., where I was a guest of her Grandmother Roome on her farm. It was my last chance to meet her mother's mother, while she was still in good health. From 240725, I visited Ruth a week at Richmond, Mass., where I was a guest of her elder sister, Esther Wilson, who lived there with her family.

Washington was my home base during several activities which are recounted in separate sections. From my days in Central High School, my first career award came as a happy reminder. At the annual reunion on 370101, I was the youngest one of four to receive a Certificate of Distinction from the Alumni Association.

### 1.4 Radio Amateur (3QK) 1920-22.

I operated an amateur radio station at my home in Washington after World War I. My call letters were 3QK, which happens to be unusually rhythmic in code:

...-- --.- -.-

I assembled my transmitter and receiver from components, some home-made and some purchased. I could send and receive 12 words per minute. I joined the Radio Club in Washington, but I was never an amateur at heart. My motivation was primarily in experiments and secondarily in communication activities.

When we moved to Washington in 1916, World War I was in progress in Europe. Woodrow Wilson (Democrat) barely won a second term by promising to keep us out of war. A month after he was inaugurated, we joined the war. The Government made typical blunders. They outlawed the teaching of German in the schools. They prohibited all radio amateur stations and even all receiving sets.

During the war, we all violated the latter prohibition, in word though not in spirit. In my case, a nearby station was NAA, the Navy's high-power key station with three towers in nearby Arlington, Va. Its code transmission could not be kept secret, because it could easily be heard on a covert receiver. I used a common practice, connecting a crystal detector and headphones between a bedspring and a radiator pipe. (I should have added a tuning coil.) I heard the time signals transmitted daily for 5 minutes just before noon and 10 P.M. It gave me the thrill of reception of code signals, with the broadcast preamble:

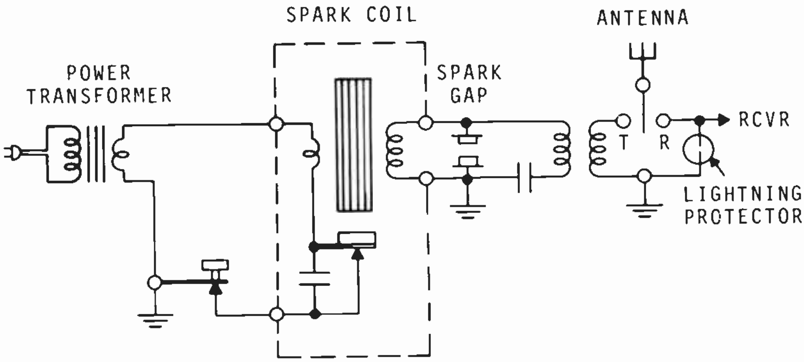
QST QST QST DE NAA NAA NAA

After the wartime restrictions were removed, I prepared to qualify for a station license. This required an operator's license obtained by passing a test on some regulations, sending and receiving 5 words per minute, and a secrecy oath. Then it required a station registration with some description. The licenses were issued by the Bu. of Navigation, Dept. of Commerce.

Instead of the bare qualification for an amateur operator's license, I attended the National Radio Institute in the fall of 1919, the evening course for commercial operators. (I was in high school, grade 11.) The lecture class work was elementary, mostly operation and maintenance. My grade was perfect. The code instruction was aimed at 20 words per minute for first-grade, or 12 for second-grade, commercial operator's license. I passed the latter, which was customary before professional experience. I received, "License to Radio Operator, Commercial Second Grade", No. 12040, Dec. 15, 1919.

Then I applied for an amateur station license, which was issued 200422 with call letters 3QK. The address was 5503 33 St. NW, Washington, D.C.

In our house, I occupied a small bedroom in the NE corner of the second floor. To the north, this overlooked a vacant lot, then the next house. My




1.4 Fig. 1 — Transmitter circuit of Station 3QK.

antenna was 2 parallel aluminum wires 40 ft. long, between my corner and the corner of the next house, about 25 ft. above ground. My transmitter was located on a wood shelf on the hot-water radiator (with manual key on extension cord for operation on a nearby table). My receiver was located nearby in a beautiful walnut-and-pine bookcase (made by my Grandfather Wheeler long before I was born).

Fig. 1 shows the transmitter circuit, which was typical of a low-power amateur station. The power was rated as "1.5-inch spark coil". Using an AC transformer instead of a DC storage battery, I realized less than half the rated power. The spark frequency was 120 pulses per second. From input about 20 watts, I probably radiated about 1 watt at a frequency somewhat above 1500 Kc. I exchanged messages with a few friends as far as a few miles.

Any details of the receiver are not shown in Fig. 1, because it was the subject of many experiments. I was always interested in the possibilities of the elementary crystal set with only tuner and headphones. Also I was experimenting more and more with vacuum tubes in a regenerative detector, an AF amplifier, and later a tuned RF amplifier. Concurrently, the motivation shifted to sound reception at all distances, with the growth of broadcasting.

Herewith is a photo of my amateur station and its operator, taken to represent the reception of my father's first radio news report (sent in code) 201215. This event was a milestone in my father's career in U.S. Dept. of Agriculture. It led to a series of incidents which brought me to the attention of Prof. Hazeltine and launched my career in Hazeltine Corp.

Up to that time, I had summer jobs in or near my father's office. The last summer (1920) I worked with George B. Lacy, whose mother was an employee near my father's office. He also was a radio amateur, and we became close friends, with many radio and social contacts for years afterward. He loaned or gave to me some postwar surplus vacuum tubes (Western Electric VT-1) which I used in some of my experiments with the neutralization of feedback in a tuned RF amplifier.

In high school, I had a few amateur friends, but none who remain in my memory. One time, my physics teacher asked me to give him a shopping list of components he should order for some radio experiments in his lab.

In the summers of 1921-22, after finishing high school, I worked at the Radio Lab of the Bureau of Standards. On 210709-19, I borrowed a Kolster "decremeter" and tuned up my 3QK transmitter to operate more efficiently and on a known frequency (1500 Kc, or 200 meters, assigned to amateurs).

After graduating from high school and starting in GWU, my radio station was promoted from my bedroom to a nice shop I had in the basement. On the south side of our house, the basement was at ground level. Under one window overlooking the corner vacant lot to the south, I had constructed a workbench with shelves nearby. An opposite wall was provided by an interior closet my father had built. There was an electric light overhead and a wall outlet. My shop was well lighted and ventillated, and usually not too hot or cold.

I strung a 130-foot antenna wire from the gable of our house, south to a tall tree across Livingston St. (There was no use of the land to the south and east of our house.) That was one of the few times I ever climbed a tree. My 3QK transmitter was moved 210924. It was much more efficient with the larger antenna. However, I made little use of the transmitter after that time. (Apparently I prepared a renewal application for 3QK, 220922 or 221127, but I have no record of renewal.) The large antenna was a help in reception on a crystal set without an amplifier. I heard broadcast stations as far as WGY (Schenectady).

I have retained some small notebooks in which I kept an informal log of my amateur activities. They are supplemented by my personal diaries from 1920. My two-way operations with my amateur transmitter are dated 200226-220312.

[A] E. E. Bucher, "Practical Wireless Telegraphy", Wireless Press; 1917. (An early guide for operators. Author from Marconi Co. of America.)

[B] E. E. Bucher, "Wireless Experimenters Manual", Wireless Press; 1920. (An early guide for amateurs. Author from Marconi Co. of America.)

## 1.5 My Two summers at the Bureau of Standards 1921-22.

Around the time of my first year in college, I experienced an opportunity that gave me a headstart of several years and fortuitously shaped my future. It started in the first year of radio broadcasting as a service to the home, giving me an early introduction to the problems and opportunities in receivers. It reached a climax in the following year, when my experiments at home led to my independent invention of the neutralized TRF amplifier for broadcast reception. Then my accidental meeting with Prof. Hazeltine led to our association and my career in Hazeltine Corp.

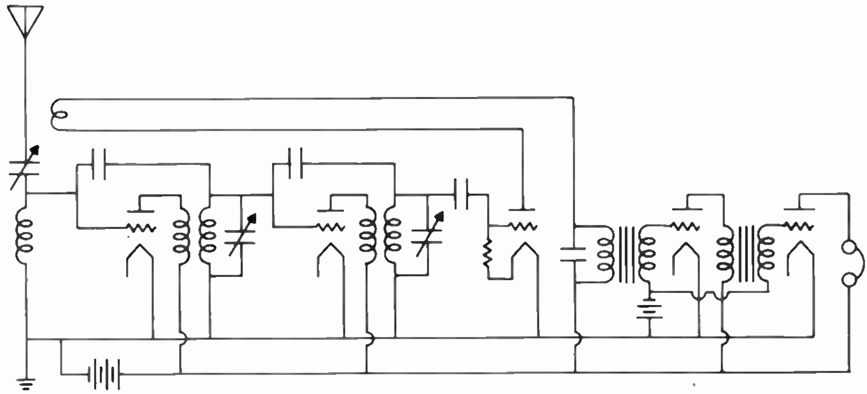
I worked in the Radio Laboratory of the Bureau of Standards, the summers of 1921-22 and half-time the intervening winter during my first year at GWU. I was one of two laboratory assistants employed by Dept. of Agriculture for their purposes. My pay came to about $1,000. It was a plan proposed by my father. It was related to his Radio Market News Service, which had benefited from cooperation of the Radio Lab. The other employee with this status was Herbert F. Harmon, a recent graduate of Grove City College (near Pittsburgh). I do not know how he happened to be selected. He worked fulltime over the same period. We were good friends. He had more formal preparation, the value of which I did not appreciate at the time.

Our function was some contribution to the reception of radio reports for farm purposes. After some experience in reception, I designed some elementary receiving sets that could be made at little cost. Harmon undertook to make some equipment for testing the performance of such a receiver. (This was to become a specialty of mine, from 1928.)

The Radio Section of the Electrical Division of the Bu. of Standards of the Dept. of Commerce was located at the Bureau, half-way out on Connecticut Ave. (by trolley). The Radio Laboratory was housed in a low building between two towers supporting its transmitting antenna. Its call letters were WWV for experimental transmission of various kinds, including voice. The first floor was occupied by the Radio Section. The two halves of the basement were occupied by Signal Corps and Navy laboratories.

One end of the first floor was devoted to "standards", which meant routine measurements for customers. One could send for testing or calibration, any of several devices, for a nominal fee. The test was made by comparison with a standard. A "wavemeter" was a frequency tester made of a fixed coil and a variable condenser with a fine scale. The resonance calibration was a large graph. The lab had secondary standards for calibration of a submitted wavemeter by comparison. The signal generator was a rather powerful oscillator (a few watts). The current indicator was a thermocouple with DC meter. After setting the oscillator to a frequency by the standard wavemeter, a calibration point was obtained on the wavemeter under test. I became familiar with the methods and made some tests.

One small, dark room was occupied by an experimental cathode-ray



1.5 Fig. 1 — Proposed 5-tube receiver with neutralization (220817 HAW).

oscilloscope. Some years later, its descendents were to become one of the most powerful tools in electronics technology, and the picture tube of the television receiver.

For stable operation of vacuum tubes, a central storage battery of hundreds of volts was wired to outlets.

There was a central cafeteria for the Bureau, but it was not close to the Radio Lab so we carried our lunches.

The Radio Lab was headed by Dr. John Howard Dellinger, assisted by Laurens E. Whittemore. Both were friendly to me and tolerant of my inexperience. My supervisor was John L. Preston. He performed a valuable function in translating my reports to a format suitable for publication as Bureau documents. I worked there from 210623 to 220930.

There were frequent staff meetings in the Radio Section, from which I learned a lot. One meeting I recall because I disputed a point made by Mr. Preston. As usual, the misunderstanding came from a difference of viewpoints, which no one saw how to resolve at the time. My criticism was not justified, and I was out of line in persisting. I wish I could apologize, but it is too late. Also I attended some meetings of the Electrical Division, but I do not recall their subjects.

The Radio Lab was recieving various kinds of radio receivers for testing. Some were Navy receivers designed during World War I. Others were home receivers responding to the rapid growth of broadcasting in 1921. All were intended to use a rather long wire as an antenna. To get away from the electrical noise (as around the Bu. of Standards) we needed a site far from dense population.

There was a vacant lot nextdoor to Mr. Whittemore's house in nearby Chevy Chase, Md., near the corner of Brookville Rd. and Shepard St. There we erected a Signal Corps field antenna between two 30-foot poles with guy wires. The foreman was Norman Snyder, a young engineer with unusual physical strength. I and one or two others were his helpers. One afternoon we hurried to anchor the last guy wires in view of an approaching electric storm. Just after we finished, a nearby strike induced sparks across the insulators in the guy wires we had been holding a few minutes before. A small shelter near the antenna was provided to house the equipment and the operator.

That location was a mile or two from my home. I walked there many days during the first summer. I was learning by operating the latest Navy receivers. The best one was the SE-1420, a double-tuned regenerative receiver. I was to learn later that it was designed by Prof. Hazeltine, with whom I was destined to be closely associated. It is the subject of one chapter in my book. [W1] I received code signals from all over the World. Today we would use a small antenna with a superheterodyne receiver.

My first task was the design of a simple receiver with crystal detector, which could be built by anyone with inexpensive materials and common tools. The result was a single-tuned set capable of receiving KDKA in Washington (200 miles) with a wire antenna of moderate length and height (say 50 ft. and 20 ft.). The tuning was simply a coil with 1-turn and 10-turn tap switches for 0-700 micro-henries of inductance. It tuned over the new broadcast band with an antenna wire between 40 and 80 feet in length. I made the first model by hand and tested it around 210927. As for sensitivity and distance, it was about as good as any crystal set. It would tune in the strongest signal, but might be unable to select a weaker signal. I wrote a description. After revision by Mr. Preston to proper format, it was anonymously published 220424 as Circular No. 120 (C-120, price 5 cents from GPO). [E] It was reprinted and handed out from the radio department of some stores (for example, Woodward & Lothrop in Washington).

I designed and we published some more advanced designs for easy construction. The second was double-tuned to enable selection of the weaker of two strong signals. The others used vacuum tubes, but I do not recall their details. They introduced me to the analysis of tubes by the Taylor series, which I found readable though I was not taught Calculus until the next year in college. I made some measurements on audio amplifiers with various transformers. Only crude equipment was available. I finished a few elementary reports before I left after the second summer.

I recall an incident at the lab on 220109. Mr Ould told us about a new textbook by Prof. Morecroft at Columbia U., and I joined a club order for this monumental volume (which I still have).

On 210702, a few days after I started work, we all listened to a daytime broadcast of a boxing match, the first in history. It was the Dempsey-Carpentier fight. The blow-by-blow description was sent from a special transmitter at the Lackawanna Terminal in Hoboken, across the river from New York. We received it on the Grebe CR-5 or CR-9 receiver, made in Richmond Hill on Long Island. This was a single-tuned regenerative receiver designed for 100-2000 Kc. It was a beautiful design, just before the rash of home receivers for broadcasting. It was connected to a small horn loudspeaker.

In late 1921 and early 1922, the single-tuned regenerative receiver appeared in some inspired designs for home reception of broadcasting. Each one was promptly sent to the Bu. of Standards for testing. I was priveleged to borrow each one for evening testing at home, because the long-distance reception occurred after sundown.

The first and most famous was the Westinghouse RC (RA + DA units). It had a single dial for tuning the antenna circuit by variation of both inductance (L) and series capacitance (C). A moderately long wire antenna was required. A "soft" tube (UV-200) was used with feedback for a regenerative detector. Two "hard" tubes were used in an audio amplifier adequate to drive a small loudspeaker. The tuner was described by Frank Conrad, the founder of KDKA. [F]

The second I recall was the Clapp-Eastham set. (Their company in Boston became the General Radio Co.) It lacked the single-dial tuning feature but offered more versatility for best reception. It was my choice for several installations I made for friends.

At home, I built such a set to suit my fancy. It was similar to the Clapp-Eastham set. It used 3 UV-201 tubes. On 220708, I took it to work to show it to my friends. Prof. Jansky happened to be there and was complimentary to my design.

This home-made set was the starting point for numerous experiments I made soon afterward. My work is tracked in my diary and my current Notebook No. 1. It led to my invention of various kinds of neutralization for a tuned radio-frequency (TRF) amplifier. One kind proved to be most useful. I was soon to learn that it had been invented on paper a few years earlier by Prof. Hazeltine, though I was the first to reduce it to practice. Here is how it happened.

In and out of my work in the Bu. of Standards, I had learned the need for RF amplification ahead of the detector in a radio receiver. For lower frequencies, it was in the Navy receiver I had seen in Cdr. Taylor's house. I had learned that it was subject to a severe limitation, especially in a tuned amplifier for the broadcast frequency band. The inherent grid-plate capacitance provided feedback that might be sufficient to cause oscillations (squeals and whistles in a sound receiver). [C]

Following in the footsteps of the famous Hartley of Western Electric Co., I first tried inductive coupling to neutralize the capacitive coupling in the tube. [G] On 220712, I operated this system. I retained regeneration by inductive "tickler" coupling around the TRF stage, from detector to antenna tuned circuit. After adjusting the neutralization at one frequency, this circuit was effective, but only when tuned to that frequency.

Here I introduced a procedure for setting the neutralizing coupling. I disabled the RF amplification by turning off the filament of that tube, while leaving the capacitive coupling. By applying a strong signal in the antenna circuit, the residual coupling could be cancelled by setting the neutralizing coupling. On turning the filament back on, the feedback coupling remained neutralized. Later Prof. Hazeltine adopted this procedure, with generous credit to me.

The next day, I conceived the solution to the problem of frequency dependence, and I tested it the evening of 220714. For the neutralizing coupling I used an equal capacitance on a reverse transformer (a pair of like coils, closely coupled). Then the neutralization set at one frequency was effective at all frequencies over the tuning range. That afternoon, I described it to Dr. Lewis M. Hull at the lab. The following day, Harmon came over to see it working.

I described it to my friend, Elmer Stewart, who was a patent attorney, and he advised me to write a disclosure and get it witnessed, which I did. He came over to see it 220718. Radio was a hobby but not his specialty. However, he made a search in the Patent Office, and two days later told me the two closest prior patents that had issued to date. They were Hartley and Rice. [G][H] Hazeltine's had not yet issued. On 220729, we conferred and he advised me that it was doubtful if I had made an invention over those patents.

Fig. 1 is a copy of my notebook sketch under date of 220817, the last I made before the trip on which I visited Prof. Hazeltine. On 220829, in his office, I described my neutralization to him. Then he showed me his earlier patent application on the same kind of neutralization. That was the beginning of another branch of this narrative.

It is notable, how much Fig. 1 was a preview of the first Neutrodyne receivers to be made to Hazeltine's design. The most famous was the Freed-Eisemann NR-5 (Neutrodyne Receiver, 5 tubes). Like Fig. 1, it had these features (5 tubes):

- Two TRF stages, each with capacitive neutralization using a close-coupled transformer tuned by a parallel condenser.
- A grid-leak detector.
- Two AF stages, each with a transformer.

My circuit differed in the manner of tuning the antenna circuit. Also I made available a regenerative feedback by inductive coupling, for maximum sensitivity to a weak signal. The similarily is remarkable, but might be traceable to some existing 5-tube receivers using two wideband RF stages with little gain.

Here we should be reminded of the Government policy on an invention made by an employee. He was encouraged to file a patent application. Under any patent to issue, the Government could obtain a royalty-free license, but any other benefits accrued to the inventor. For an invention made in the course of assigned work, the Government patent attorney would prepare and file the application. These provisions were applicable to the Hazeltine invention, but not clearly to mine because I did the work at home.

The trip on which I visited Prof. Hazeltine was not planned for that purpose. I accompanied my father to visit Rochester, N.Y., 220822 for advising the Post Office there on reception of the Radio Market News Service. A few days later we visited New York and Hoboken, where we accidentally encountered the Professor.

From my work at the Bu. of Standards, one of the greatest benefits to me was my early encounters with men who were already active in the field of radio. Some were outstanding leaders with whom I was later to have close associations and personal friendship. They were always appreciative of my work. Here are the names of people I knew in the Radio Lab, and I worked with some of them.

Frederick A Kolster (1883-1943) Radio Section Chief, 1912-21. (F'16)
Dr. John Howard Dellinger (1886-1962) Section chief, 1921-46. (F'21) (Pres. '25).
Laurens E. Whittemore (1892) Radio Section, 1917-24. (F'27). Later, AT&T.
Elmer L. Hall (A'48).
John L. Preston.
Herbert F. Harmon (1899) (M'58). Later, AT&T Co.
Harold A. Snow (1898) (F'56). Later, Radio Frequency Labs.
Norman Snyder. Later, GE.
Morris S. Strock.
Richard S. Ould (A'19).
Francis H. Engel (A'25). Later, RCA.
Hoy J. Walls (M'28).
Dr. Richard T. Cox (1898). Later, JHU.
Miss Elizabeth M. Zandonini (M'46).

With the exception of Kolster and Dellinger, these were young people (in their 20s) recently out of college.

Following are the names of prominent radio men whom I met at the Radio Lab.

Dr. John M. Miller (1882) (F'20) (Medal of Honor '53). Later, Atwater Kent, NRL. [C]
Lawrence C. F. Horle (1982) (F'25) (Pres. '40).
Dr. Charles B. Jolliffe (1894) (F'30). Later, RCA.
Dr. Cyril M. Jansky, Jr. (1895-1978) (F'28) (Pres. '34). Later, Jansky & Bailey.
Dr. Lewis M. Hull (1898) (F'28) (Pres. '33). Later, Radio Frequency Labs., Aircraft Radio Corp.

Note: The code (F'16) etc. denotes the latest grade of IRE-IEEE membership (Fellow, Member, Associate) and the year granted.

Between my two summers at the Bu. of Standards, I studied in my first year at GWU. That was a full course load, which was 34 semester hours (some in evening classes). My grades were A's and B's. School, half-time work, home radio and a busy social calendar added up to an overload on my strength. This was relieved the next school year, free of the half-time work.

My association with the Radio Lab at the Bu. of Standards gave me a headstart of great value. Its effect was to start my professional career 4 years early (at the end of high school instead of college). I owe this opportunity to my father and his friends who were sympathetic to his plans for me. We might say also that he was indebted to me for introducing him to the possibilities of radio. That knowledge inspired him to organize the Radio Market News Service, which in turn led to his appointment on the National Radio Conference. There he met Prof. Hazeltine and introduced me to him. A later accidental encounter brought us together and set the course for my career in Hazeltine Corp.

[A] "Radio Instruments and Measurements". Bu. of Standards, Circular No. 74; Mar. 1918. (C-74).

[B] Bu. of Standards, "The Principles Underlying Radio Communication", Signal Corps Radio Pamphlet No. 40, 1 ed.; 1919. (2 ed. 1922) (Editor, Dr. J. H. Dellinger).

[C] J. M. Miller, "Dependence of the input impedance of a three-electrode vacuum tube upon the load in the plate circuit", Bu. of Standards, S-351; Nov. 1919.

[D] J. H. Morecroft, "Principles of Radio Communication", Wiley; 1921.

[E] (J. L. Preston, H. A. Wheeler) "Construction and operation of a simple homemade radio receiving outfit", Bu. of Standards, C-120: Apr. 24, 1922. (The crystal set I designed.)

[F] F. Conrad, "Radio receiving equipment", Proc. IRE, vol. pp. 426-439; Dec. 1922. (The Westinghouse RA tuner.)

[G] R. V. L. Hartley, U.S. Pat. 1183875; 1918. (Neutralization by magnetic coupling, critical to frequency.)

[H] C. W. Rice, U.S. Pat. 1334118; 1920. (Incomplete neutralization, without close coupling between the reverse coils.)

### 1.6 George Washington University 1921-25.

When I started in George Washington University, my father was supporting our numerous family on the salary of a Government employee of intermediate professional grade. I had some savings from summer jobs. When I finished 4 years later, I was established in a promising career with compensation that was generous even for an experienced engineer. The principal contributing factors were the following:

- I lived at home, near GWU, where I enjoyed the best care and the space needed for experiments.
- I worked the first two summers at Bu. of Standards, where I learned much that was not taught in school.
- The GWU course in Physics was not "intensive", so I could make all A's and B's with time to spare.

I often wonder what would have happened if I had gone instead to a more demanding school (such as MIT) with more strains (with respect to health and money) and without these "advantages".

My choice of GWU was based on elementary considerations of cost. I won a scholarship for 4-year tuition (then $170 per year) and I could live at home. (Incidentally, GWU had no dormitories at that time.)

A fortunate circumstance was the fact that GWU was just building a Department of Physics, in which I wanted to major (on the excellent advice of my father). It was in the College of Engineering. I received the first award of the degree of B.S. in Physics. The primary purpose of the department was to enable Government employees to take evening courses for M.S. in Physics. They came mainly from Bu. of Standards and Patent Office. The department could not support two levels of the basic physics courses, so I took all the physics courses that would have been required for a Master's degree.

Mechanics & Heat
Mathematical Physics
Light & Sound
Electricity & Magnetism

The evening courses were naturally more tolerant in evaluating a student's performance. Furthermore, the department head, Prof. Thomas B. Brown, was also interested in radio so we had a common bond.

GWU was a struggling, growing university without a "campus". The administration and the classes in arts and sciences occupied converted stone residences in the "Foggy Bottom" region west of the White House. The hospital and the medical school (where we went for chemistry classes) were east of the White House. During my 4 years, two new buildings were constructed in the west location, the Law School and Corcoran Hall (arts and sciences). My graduating class was 186. I ranked first, largely as a result of my teachers' lenience and their evaluation of my ability as well as academic performance. With one exception (General Economics, second semester) my grades were all A's and B's (about 90%) and 3/4 were A's (above 95%).

The first year, my classes were easily scheduled, mostly daytime TU, TH, SA, and a few late on MO, WE, FR so I could work at Bu. of Standards those days. My mother prepared a nice lunch which I carried in a bag. The school had no food service.

The only English course I had in college was the freshman "Rhetoric" for all students. It was an "inspirational" course with required 150-word "themes" on a list of about 30 topics (the same every year). The teacher was Dean Wilbur, an aged professor removed from reality. Composition was easy for me, so I could type several themes in an hour before breakfast. Being typed and hence easily legible, the student assistant always gave me A. I made the honor roll, with one on which I "let myself go". It was a reaction to the painting, "Diana of the Tides", which we visited in nearby Corcoran Art Gallery. I suppose the practice contributed to my facility of writing later on, but the course was not oriented toward factual reporting and logical interpretation. At the end, we had one long, last paper on a topic of our choise. I chose the topic, "What is wrong with this course?" Grade, A.

In the Physics curriculum, chemistry was disposed of in the first two years. I found it easy enough but it did not attract my best effrort. In the third year, I enjoyed Physical Chemistry, a course of lectures by Prof. H. C. McNeil.

In the four years of Math, I did very well and enjoyed it. In the second year, I took naturally to the Calculus, with the inspired teaching of Dr. Howard Lincoln Hodgkins. I did not take naturally to differential equations. These courses were all that were available for the Master's degree. Dr. Hodgkins became President of GWU. Shortly before the time for my graduation, he called me to his office and suggested that I compete for the Ruggles Prize in Math, a gold medal awarded to one or two graduates each year. On a brief but diversified written test, given by Prof. Erwin, I rated first and a Master's degree man rated second. I wore the medal as a "watch fob" for many years.

Two years of German was something I should have had in high school. At the later time, I was even weaker in memorizing vocabulary. Incidentally, the old German characters had not been modernized, so that was another impediment. My performance was not outstanding, but I did acquire the ability to translate to English with the aid of a dictionary.

After the first year, 2/3 of my courses were Physics and Math. The more advanced courses were designed for the Master's level. The only "social studies" were Economics and Commercial Law.

As my faculty advisor for the degree in physics, Prof. Brown went out of his way to give me every opportunity. Our common interest in radio brought us together, even in the first year before my first Physics course. His lecture course in my second year (1922-23) was outstanding in clarity and lecture demonstrations. I had little need for further study of the textbook. In our first test, he told me I was the only A. I was already skilled on the sliderule and had the experience of two summers at the Bu. of Standards. I was impressed with the nice organization of the laboratory experiments under Prof. Cheney. He also gave me individual attention and encouragement.

The same year, I was permitted to take an advanced course in Radio, using a fairly recent textbook (Lauer & Brown). It required Calculus, which I was just learning at the same time. On 230508, I lectured to the class on the Hazeltine Neutrodyne receiver.

One day Prof. Brown called me in and showed me the first issue of the new Bell System Technical Journal, and suggested that I subscribe. Now I have a complete set, which is collector's item. The first few issues contained the image analysis of wave filters (Campbell, Zobel) in which I was to specialize in later years.

It was during this second year that my association with Prof. Hazeltine was developing, and Prof. Brown was much interested. He gave me all the freedom I needed for this outside opportunity.

Early in 1923, I redesigned my handwriting in some details, especially to facilitate my signature in cursive script. Later I came to appreciate that my father had made a similar change many years before, so our writing became quite similar.

In my third year (1923-24) I was employed as student assistant in the physics lab. A second-year liberal arts student, Ruth Gregory, called on me for help, and then transferred to the lab period where I was assistant. A year later, we were engaged to be married.

In my third and fourth years, Advanced Laboratory Physics was intended to include some experiments beyond the basic instruction. The school was equipped for some, while others could be made outside (as at Bu. of Standards). In my case, I had been making unusual experiments at home, notably the invention of neutralization for a TRF amplifier. Prof. Brown therefore let me plan and execute small research projects at home for lab credit at school. He visited my home lab on 240412 and perhaps other times. My fourth year work at home would have qualified as a Doctor's thesis.

My laboratory at home was located in the basement at a window on the south side. (5503 33 St. NW, Washington, DC.) We are here concerned with the time after my first summer in Hoboken (1923). At first (from 231110) my only meters were small DC table models (Weston 280) for voltage (full-scale 1.5, 15, 150 volts) and current (full-scale 0.1, 1, 10 amp.). Soon afterward (before 240315) I acquired a sensitive wall galvanometer (Leeds Northrop, Type P) the kind we used in the Physics Lab at GWU. It cost about $20. It had a suspended coil (126 ohms) and mirror, with a telescope and a long paper scale in a metal frame. It was cumbersome but more versatile than any portable meter for some years to come. Mine was in use at home before 240315, with suitable resistors as shunts and dividers for damping and scale multipliers. It was used for measurement of electron current in a vacuum tube. I had a fair assortment of the usual radio components.

Among the experiments I performed in my third year (1923-24), some were a preview of work that would soon be performed in the Hoboken lab of Hazeltine Corp. The most notable was the measurement of an RF amplifier stage, which I made on several kinds. The frequency range was the broadcast band (550-1500 Kc). Naturally, I tested the neutralized tuned RF stage from the (Freed-Eisemann) Neutrodyne receiver. The test was most meaningful on that kind. I also tested 6 varieties of wideband (socalled "untuned") RF stages made by 4 prominent companies. On those kinds, the test was idealized, disregarding the feedback coupling across each stage. However, it did give interesting insight into the design.

The measured voltage ratio of amplification in one stage was graphed in terms of $\log_{10}$ of the ratio. One unit was what would later be termed 20 decibels. With the latest (UV-201A) triode, the neutralized TRF stage measured more than one unit, while the untuned stage measured somewhat less over the broadcast band. The behavior of the latter in practice was complicated by feedback so its performance could not be predicted.

In my fourth year (1924-25) I developed a method of analysis of a triode-vacuum-tube circuit which was powerful and has seen little or no application since then. By the use of DC coupling (or the conceptual equivalent) I tracked the cyclic variation in an amplifier, used as an amplifier or an oscillator. The variation could be tracked slowly (point-by-point). The advent of the dynamic trace on a cathode-ray oscilloscope removed the need for the slow tracking, which probably explains the lack of further exploitation of the DC model. My anaysis started from the DC properties of the tube. It was able to predict the distortion or other properties of an amplifier or oscillator.

A DC amplifier is a resistance-coupled amplifier in which the usual coupling capacitor is replaced by a battery of equal voltage. This is a simple expedient if all cathodes are filaments connected to the same battery. Various alternatives have been devised, but need not be considered here.

First I used this model to analyze the behavior of an AF amplifier. I found that a one:one transformer would enable one tube to drive the next grid positive with little distortion, and thereby to enable more useful variation of current output. This principle I pursued the following summer (1925) in the Hoboken lab.

Then I used this model to analyze the behavior of a tuned (AF) oscillator. A two-stage DC amplifier was provided with regenerative DC feedback by connecting its output circuit to its input circuit. This was an early example of a "flip-flop" circuit. It was stable at either of two extreme points of equilibrium. It became an AF oscillator when I added a tuned circuit of parallel capacitance and inductance. By connecting an adjustable voltage to the input circuit, there was observed a DC curve with a region of negative slope, representing the negative conductance needed for oscillation. One could deduce the approximate amplitude of a sine-wave oscillation. This was verified at frequencies around 1 Kc. Other peculiarities of an oscillator were explored.

These experiments with DC coupling of triode tubes were a preview of some of the practices that have become common with transistors many years later.

The freedom I was accorded in laboratory work may have left a deficiency in the usual advanced lab experience, but GWU was not well equipped beyond the basic instruction. The result was experience in taking the initiative and performing experiments out of the ordinary. This put me ahead in my chosen field.

When the Hazeltine Neutrodyne receiver was on the ascendency, Prof. Brown arranged for us to demonstrate my Freed-Eisemann set to a meeting of the Washington chapter of Phi Beta Kappa (of which he was a member, though there was no chapter yet in GWU). In the University Club on 240317, I made this demonstration with the aid of a Western Electric 10-A audio amplifier and horn speaker. In daytime, we received the local station (WRC). I connected an indoor wire antenna. Then I discovered that the wire added little to the magnetic-field pickup by the first coil in the (unshielded) set. That was my first lesson, that a steel-frame building shields the electric field while the magnetic field threads in and out.

Until the last year, I used public transportation, so I spent much time on the streetcar between my home and the school. The Bu. of Standards was in between, where I worked half-time the first year. Early in the last year, my Hazeltine income enabled the purchase of a car, which I used part of the time in place of the streetcar. That saved some time, but was extravagant in cost. It gave me more freedom, in the days when driving was not impeded by congestion and there was room to park anywhere.

Until the last year, my social activities were little associated with the University, but mostly related to friends from earlier affiliations including high school and the church. After Ruth and I started going together, we enjoyed a very busy social calendar including many activities associated with the University. One circle was the sororities. She was a member of Gamma Beta Pi (a local which was later inducted into Kappa Kappa Gamma). My first tuxedo was required for the annual Panhellenic Prom of all sororities (240425). In our last semester, we attended 8 large dances related to the school, the last being my Senior Prom. By that time, we were engaged (from 250323). Those were the delightful days of ballroom dancing, before it was diluted by subversion and polluted by amplifiers.

A few of my graduation gifts have stood the test of time. I am still using the gold-plated Gillette razor from the Laudicks (our next-door neighbors). My parents gave me a beautiful Hamilton pocket watch, which has been superseded by the wrist watch. We are still using the binoculars from our friend, George Edler.

I graduated 250603 at the top of a class of 186. The Commencement was an evening ceremony in the Washington Auditorium. My degree was the first award of the B.S. in Physics. I was one of two to receive the Ruggles Prize in Mathematics. My Valedictory was a short speech (memorized) at the Class Day exercises in the (new) Corcoran Hall, the morning of the same day.

I am grateful to the inspired teachers and the dedicated professionals who gave me an excellent introduction in my chosen field. This background in physics and mathematics proved to be more-than-adequate preparation for my post-graduate studies to follow. The tolerance of these friends and their personal consideration for me made possible the outside activities in the Bu. of Standards and the Hazeltine affiliations, which provided a headstart of about 4 years in my career.

[A] Black, Davis, "College Physics".
[B] E. Edser, "General Physics for Students", MacMillan; 1922. (1 ed. 1911)
[C] E. Edser, "Heat for Advanced Students", MacMillan; 1920. (1 ed. 1899)
[D] E. Edser, "Light for Students", MacMillan; 1923. (1 ed. 1902)
[E] S. G. Starling, "Electricity and Magnetism for Advanced Students", Longmans Green, 4 ed; 1924. (1 ed, 1912)
[F] H. Lauer, H. L. Brown, "Radio Engineering Principles", McGraw-Hill, 1920. (Signal corps)
[G] S. M. Barton, "Theory of Equations", 2 ed., Heath; 1903 (1 ed. 1899)
[H] J. McMahon, "Hyperbolic Functions" 4 ed., Wiley; 1906 (1 ed. 1896)
[I] W. A. Granville, "Elements of the Differential and Integral Calculus", Ginn; 1911. (1 ed. 1904, 3 ed. 1939)
[J] W. W. Johnson, "Ordinary and Partial Differential Equations", Wiley; 1889
[K] T. B. Brown, "Foundations of Modern Physics", Wiley; 1940. (A later textbook by my professor.)

**1.7 Johns Hopkins University 1925-28.**

Following the good advice of my father, I planned to pursue post-graduate studies in the wider field of physics, as a preparation for a career in engineering. My employment with Hazeltine Corp. provided generous support for whatever plan I might wish to follow. I did wish to continue my close ties with Washington. The closest Physics Dept. with a recognized standing was that of Johns Hopkins University (JHU) in Baltimore, only 40 miles from Washington. Hopkins was even better known in the field of medicine and chemistry. The Physics Dept. was located on the beautiful, spacious Homewood campus on the north side.

The School of Advanced Studies at Homewood was remarkable for its atmosphere of dignity and intimacy with a relaxed pace. The Physics Building contained all offices, classrooms, laboratories and the library for the department. It was only for math lectures that I went to another building. I was assigned a small laboratory space on the top floor, which I used for the 3 years.

For my postgraduate studies in the Dept. of Physics of Johns Hopkins University, the faculty included a number of talented professors. The number was marginal to cover the growing scope of physics and mathematics, my elected major and minor. The number was adequate for the rather small enrollment. I did not call on the additional faculty of Engineering. There was no pretense of further "liberal" education.

Dr. Joseph Sweetman Ames (1864-1943) was a graduate of JHU (Ph.D. 1890) who stayed as professor of physics and later served as President (1929-35). My first year at Hopkins was his last year of teaching. I was most receptive to his presentation (the classical theory, then being enriched by quantum and particle theories). In his course on Electricity and Magnetism, I scored high. He was characterized by a gruff but friendly manner suggestive of a British heritage, though he was born in Vermont. He became best known for his leadership as chairman of the National Advisory Committee on Aeronautics (NACA) during a period of rapid growth (1927-39). The Ames Research Center of the later NASA was named in his honor.

Dr. Robert Williams Wood (1868-1955) was a graduate of Harvard U. and a graduate student at Hopkins. He returned a few years later to make a long and distinguished career as Professor of Experimental Physics, specializing in Physical Optics (the textbook he wrote). He was honored by several Doctor's degrees and other awards from many nations. He inherited and improved the Rowland machine for ruling optical gratings.

Dr. A. Herman Pfund (ca 1880) was a graduate of U. of Wisconsin. He came to Hopkins (Ph.D. 1906) and stayed as a teacher. He was my adviser.

Dr. Karl Ferdinand Herzfeld (1892) came from Austria. He succeeded Dr. Ames as head of the Physics Dept. at JHU (1926-36), then went to Catholic U. in D.C.

Dr. Francis Dominic Murnaghan (1893) came from Ireland. After receiving the Ph.D. from Hopkins (1916) he stayed as a professor (until 1948). He was an inspired teacher of math and physics. I learned much from him, and we were joint authors of one published paper.

Dr. Gregory Breit (1899) came from Russia and received the Ph.D. from Hopkins (1921). When I knew him as a lecturer there, he was employed at the Dept. of Terrestial Magnetism, Carnegie Institution, which was located near my home in D.C. He became famous in the team of Breit & Tuve, who pioneered in sounding the ionosphere height by pulse reflection. Dr. Tuve was receiving his Ph.D. from Hopkins (1926) and became Director of the Dept. of T.M. (from 1946).

Dr. Cohen was an excellent teacher of mathematics.

I enjoyed some association with other professors, including Dr. J. C. Hubbard during my last year.

I worked on three notable projects, which are described individually:

- The interrupted light beam was designated my research project. As byproducts, it yielded two other papers relating to random noise in an electron current.
- The wave filter determinant was a joint paper with Dr. Murnaghan, for which he contributed the application of difference equations.
- The heart amplifier was a laboratory instrumentation I made for experiments by Dr. Andrus, which we published jointly.

During my years at Hopkins, I was active in Hazeltine Corp. and IRE, both mainly in the New York area. The first year, I continued work in my home lab in Washington. There I demonstrated my AVC invention which was destined to become the keystone of the patent portfolio of the Company. My IRE activities included attendance at some meetings, committee work, and the presentation of a few papers.

One measure of my participation in the Physics Dept. was my talks to some of the weekly seminars identified as the Physics Journal Meeting. It was held on Thursday afternoon, followed by tea in the British tradition of Johns Hopkins. It was attended by most of the faculty and graduate students. These were my topics:

260218 — The Schottky effect. This was the shot effect which causes random variation (noise) in an electron current free of space charge. I found a simple derivation, which was the subject of a later brief talk at the American Physical Society in D.C. 270422.

260429 — The orthophonic phonograph. This was the current design of an efficient mechanical pickup and horn, based on the mechanical analog of an electrical filter. It was soon superseded by the inefficient electrical pickup, amplifier and cone speaker (with less wear on the record).

261129 — Wave filter determinants. This was the mathematical formulation of repeating sections, with special reference to the "loaded string". Prof. Murnaghan saw an opportunity for "difference equations", which led to our joint paper in Philosophical Magazine (England) and my brief talk at APS in D.C. 270423.

271117 — The brownian movement of electricity. This covered the thermal agitation of electrons in a conductor, with a simple "derivation" I had devised. I got acquainted with J. B. Johnson of BTL, and I made measurements in agreement.

280310 — The "woofle bird". (Not a regular meeting.) At a meeting of Gamma Alpha (graduate scientific fraternity) I demonstrated an electronic synthesizer of various audio sounds, in bursts at a repetition rate set in a "blocking oscillator".

280322 — The heart amplifier. I demonstrated the amplifier I had made for Dr. Andrus for heart research in JHU Medical School. The electric pulses in the body were amplified to actuate a mechanical counter and timer.

In the first term of my second year at JHU, I was appointed Lecturer and gave a course entitled "Electron Tube Theory" (261007-270120). It was scheduled on Tuesdays and Thursdays at 1330-1430. This opportunity was suggested by by friend, Vernon Whitman, and Prof. Ames liked the idea. My compensation was free tuition for that term. It was attended by 8 students (Physics and E.E.) and Prof. Murnaghan. Among the students were:

Vernon E. Whitman, Bu. of Standards, later Hazeltine Corp.
Francis M. Defandorf, Bu. of Standards.
Charles E. Dean, BTL, later Hazeltine Corp.
Julian D. Tebo, later BTL, active in IEEE.

Looking back today at my course of 50 years ago, I think it was an excellent course. The principal deficiency was reliance on the blackboard without lecture notes.

The textbooks were R. W. King (BSTJ, 1923) and van der Bijl (McGraw-Hill, 1920). These were augmented or supplemented by what I had learned from Prof. Hazeltine and what I had been doing. Most of the presentation dealt with the triode, its properties and its applications (amplifier, oscillator, detector). Also I described the diode rectifier for power or linear detection.

One of the incidents I remember was a conversation on 251009, leading to a "pulse transformer". That day was the first lecture of Dr. Breit's course on Advanced Atomic Theories. Afterward, I was included in a group for dinner and we continued our discussions in Merle Tuve's room in the dormitory. He was in the last year for his Ph. D. Breit and Tuve were working with the Dept. of Terrestrial Magnetism of the Carnegie Institution in D.C. They were developing their proposal for measuring the height of the ionosphere by reflection of radio pulses. They asked me if a pulse could be put through an impedance transformer with any ratio of turns. From my experience with Prof. Hazeltine on the "clock receiver" in 1924, I was able to assure them that it could, and I outlined the design of a "pulse transformer" for that purpose. Their experiments with pulse reflection were a preview of some later techniques. One was the radio-pulse altimeter for aircraft. Another was radar. During World War II, the term "pulse transformer" was classified "Secret", 15 years after I had named it and designed one (perhaps the first one).

I was offered several academic opportunities which I was sorry I could not accept. After the two papers I presented at American Physical Society on 270423, Prof. Randall offered me a fellowship at the U. of Michigan in Ann Arbor. Early in my third year, Prof. Lee in Hopkins asked me to teach the course in radio for seniors in the E.E. Dept. Early in 1928, I was invited to give some lectures at Yale.

The Gamma Alpha Graduate Scientific Fraternity had an active Johns Hopkins Chapter. It brought together the Homewood and Medical faculties and students. The functions were lectures and social gatherings. Vernon Whitman and I were introduced on 260416 by our friend Walter MacNair (also in the dormitory). We were soon invited to membership. I attended the meeting occasionally. Ruth and I enjoyed their party on 280310, where I demonstrated my "woofle bird" as a humorous stunt anticipating the electronic synthesis of sound effects. At another meeting, 280323, Dr. Andrus and I demonstrated the "heart amplifier" I had made for his use. On one trip to New York, 280424, I was invited to that chapter by Dr. J. B. Johnson to hear a talk he was giving. In the spring of 1927, I was elected to Sigma Xi, the honorary scientific fraternity. (My father was a member from the U. of Minnesota. Each of us wore this key on his watch chain, so long as a vest-pocket watch was used.) I was a member of the Johns Hopkins Club, housed in an impressive Colonial mansion on the campus. I joined the American Physical Society.

During my three years at Hopkins, I was very fortunate in having comfortable living accommodations near my school work. On the Homewood campus, there was one dormitory, mostly for graduate students. It was a luxurious two-story brick building conforming to the Colonial architecture of the campus. I lived there the first year, in a two-room corner suite on the second floor (A-2). I shared it with my friend, Vernon Whitman, who was commuting part-time from Washington while working at the Bu. of Standards and living in D.C. One room was a study and the other a bedroom, with nearby lavatory for a few rooms. The windows were decorated with beautiful figured drapes made by my mother and my fiancee. We enjoyed table service (table d'hote) for all meals in a spacious dining room. It was a short walk from the college post office (box 956), the Johns Hopkins Club, the coffee shop and all classes. The campus was served by double-decker buses on adjacent Charles St.

The second and third years, just after we were married, Ruth and I lived in the Hopkins Apts., at St. Paul and 31 Streets, just a block from the campus. Our apartment (314) was a very nice place for a young comple. The nearest stores were an easy walk. Our first child (Dorothy, 271024) was born in Johns Hopkins Hospital, the other side of town. For such a special occasion, I borrowed the family car from Washington for a few days. We were well equipped for dinner guests, and Ruth was hostess to many groups of relatives or friends. For radio broadcast reception, the Stromberg-Carlson Co. twice loaned us their latest model of Neutrodyne (late 1926, then early 1928). It was a shielded receiver with indoor loop antenna and cone speaker. The later one featured rectified socket power to replace all batteries, in the days shortly before AC tubes. Records were played on a mechanical turntable by an electric pickup with the receiver amplifier. I bought a Steinway upright piano, which served our home for the next ten years.

There were few organized social activities in the graduate school. The dormitory had an annual dance, for which Ruth came over on 251120. It was common for faculty and student couples to exchange visits. We were close to the Murnaghans. Our best friends were the MacNairs. (They attended our wedding in Washington, and we attended theirs in Baltimore on the way home from our honeymoon.) On New Year 1927, they went with us to call on the Ames' at home.

While attending JHU, I spent much time away from Baltimore. The first year, I went home to Washington (by train) every weekend. After we were married, we often spent the weekend with my family in Washington. All this time, I frequently made trips connected with my employment by Hazeltine Corp., in addition to summers in New York. During the third school year, I was called to New York 13 times (mostly for matters of patent litigation) and to Rochester 4 times (our work for Stromberg-Carlson Co.). For these and other reasons, my attendance at classes was somewhat irregular, and I did not spend enough time studying. The graduate courses were not graded, but I think my learning would have met the test. Summers were spent on work for Hazeltine Corp.

After three years at Hopkins, I should have come away with degrees M.S. and Ph.D in Physics. I did not complete the requirements for these degrees. It was a failure of discipline and planning on my part. The faculty reminded me what was expected, but my motivation was diluted by my rich variety of activities, in and out of school. I was fearful of the language test in French and German, but I was probably prepared by my courses in CHS and GWU. It was a requisite for either degree. Instead of one research project, duly reported, I performed several projects and did not write a thesis on any one. Admittedly the third year saw increasing need for my activities in Hazeltine Corp., largely my participating in patent contests which could not be scheduled. In my chosen field of endeavor, what I learned at Hopkins was forever helpful, and the lack of the Doctor title was little loss among working engineers. Sometimes I was obligated (and a little embarrassed) to remind someone that I had not received the degree. I was elected to the Sigma Xi. I did come away with a liberal education, many happy memories, the beginnings of a new family, and a headstart in a productive career in the field of my choice. I am happy that I went to Hopkins for the reasons I had considered, and I am grateful to the faculty. That experience was made possible by the mutually beneficial arrangements for which I am indebted to Hazeltine Corp.

[A] R. W. Wood, "Physical Optics", MacMillan; 1911. (Rev. 1924, 3 ed. 1934.)
[B] G. Breit, M. A. Tuve, "A test of the existence of the conducting layer", Phys. Rev., vol. 28, pp. 554-575; Sep. 1926. (Pulse reflection from the ionosphere.)

**1.8 The Interrupted Light Beam 1926-28.**

I planned a thesis topic for the degree of Ph.D. from JHU. I developed the topic, performed the experimental work, and demonstrated the result to faculty members. I published the results in abstract form but did not complete the dissertation. The technique was a major advance which is now in regular use on airports.

The topic was the detection of weak currents from a light beam on a photocell (or X-rays on an ionization chamber). The contribution was the use of periodic interruption of the beam to give a weak alternating current which can be amplified and detected more readily than direct current. The sensitivity was naturally related to surmounting the background electrical noise, which was the thermal noise in electrical resistance.

Here I take the title, "Interrupted Light Beam", to identify the essential feature of the technique. Implicit is the study of the thermal noise which must be surmounted.

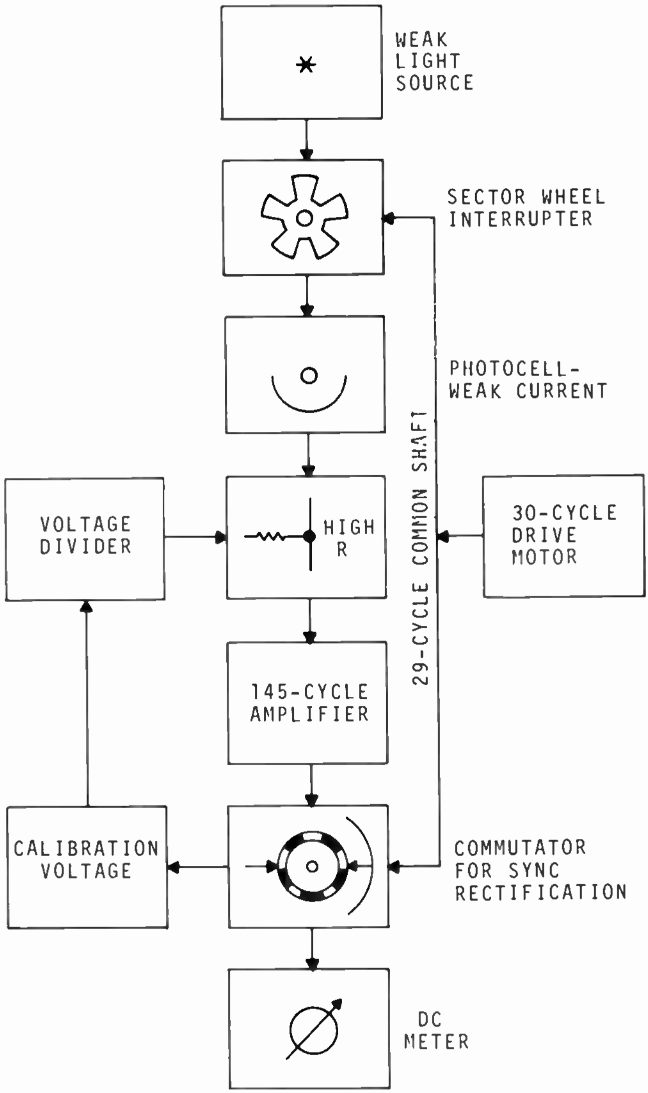
My topic evolved from discussions with Dr. Gregory Breit (lecturer) and Walter A. MacNair (a fellow student just completing his dissertation for Ph.D., living in the dormitory). They were interested in detection of weak X-rays in an ionization chamber. The detection of a weak light beam in a photocell was seen to present a similar problem and opportunity. My diary shows a discussion with MacNair 260120 and a description to Dean Ames the next day. I have notebooks entries from 260126. A synchronous motor and photocell were ordered soon (260312) and received a few weeks later. My adviser was Prof. Pfund, and also Prof. Lee was interested. Intensive work was sidetracked until the end of 1926, in view of exams, summer work, my wedding and settling in our Baltimore apartment.

Synchronous detection of a weak alternating current became a primary feature of my technique. My first notes on this feature (261219) show this concept implemented by a meter of the electrodynamometer or wattmeter type. The sector wheel for beam interruption and a similar commutator are mounted on one shaft driven by the synchronous motor. The commutator is used for synchronous excitation of the meter field.

The use of a simple DC meter was enabled by a suggestion of Dr. Breit (270928). He pointed out that the commutator could be used for synchronous rectification of the AC output of the amplifier. This was my final circuit.

Here we may outline some of the principal components of my demonstration. See Fig. 1.

- Self-starting synchronous motor, single-phase, 1800 RPM, 1/8 HP, Westinghouse, received 260409.
- Sector wheel (for light interruption) and a duplex commutator (for synchronous rectification and calibration on one shaft), each 5 sectors, belt driven from motor.
- Vacuum photocell, KH type, G.E. Co., received 260428.




1.8 Fig. 1 — Interrupted light beam.

- High resistance, 10 megohms, to develop input voltage for amplifier from small current in photocell.
- Known DC voltage and decade divider for calibration through the high R.
- Amplifier, tuned to 145 c., 2 stages UX-222 screen-grid tube, output stage UX-171A low-mu triode, final circuit 280524.
- DC meter, Rawson "Multimeter", Type 501, used at 0.1 ma full-scale, description 280514.

In the spring of 1926, I collected the necessary components and noted some ideas. (This was just after my demonstration of AVC in Washington, and that topic was attracting much of my attention.) The principal special components were the synchronous motor and the photocell.

Synchronous detection of the amplified AC was noted 261219, to be achieved by a product meter (dynamometer or wattmeter). The field was to be excited from DC interrupted by the synchronous commutator. Early in 1927, this was implemented with a Leeds & Northrop dynamometer. The effective bandwidth was determined by the meter. It was about 1 c. (between ±0.5). The amplifier was broadly tuned to 150 c. the frequency of interruption.

Synchronous detection shows random noise as fluctuation both sides of the average on the meter. Working in the dark, I sampled the meter fluctuation by a short pulse of light, then retaining the optical image for entering on the data sheet. This method caused eye strain which necessitated a few days of rest. This method was used for evaluation of noise, with due care to avoid a repetition of eye strain.

Early in my third year, Dr. Breit made his suggestion of synchronous rectification, which set the course for the rest of my work. The first half of 1928 was occupied with experiments on my thesis topic (along with some other topics, notably the heart amplifier).

I submitted the following "Outline of Research Problem" to the Physics Dept. on 271013.

**Amplification of Photo-Electric Currents.**

The object of this work is to detect and measure the weak currents of the photo-electric cell or ionization chamber, with a sensitivity on the order of $10^{-14}$ ampere, possibly extending the work to embrace the thermo-element problem.

The method proposed (suggested by Dr. Breit) is to interrupt the beam of radiation at a frequency of (say) 150 cycles, to amplify the alternating component of the resulting photo-electric current, and to rectify the amplified current by a commutator run synchronously with the interrupting sector wheel.

The special problems involved are (1) the amplification of the current by a multi-stage electron tube amplifier, on the order of $10^{10}$ times, and (2) the selective rectification to minimize the interference due to any disturbing currents, such as the thermal agitation of electrons in an electric circuit. With reference to both of these problems, the proposed work is thought to embody marked improvements over previous work along this line.

The new arrangement should be useful to supplement and in some cases replace the electrometer as used at present in the measurement of photo-electric currents.

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Referring further to Fig. 1, we may consider the various features of the equipment. We need not be concerned in detail with various practical problems which were solved as they were anticipated and encountered. Light shielding and electrical shielding were simple. The more difficult was protection of the amplifier (with microphonic tubes) from the mechanical and acoustic vibration of the motor and the driven rotary shaft. At the operating frequency, none of these problems diverted much effort.

The light source was a flashlight bulb operated at reduced power so it was barely visible. The photocell was insensitive to red, so its excitation was reduced even further. The light source and photocell did not need calibration, only the weak current from the cell. The photocell was the latest KH (potassium hydride) type. The emissive electrode was a hemispheric coating on the inside of a glass bulb. The light source was interrupted by the interposed sector wheel.

The choice of frequency was a compromise among these objectives:

- Low enough for ease of mechanical design of the rotary interrupter;
- High enough for ease of electrical design of the amplifier (tuning to the frequency of interruption, filtering the DC circuits, etc.);
- High enough to reduce the extra noise from the flicker effect in the amplifier;
- Avoid harmonics of the power frequency and the motor frequency.

A tentative choice of 150 c. avoided harmonics of 60, but not of 30, the motor frequency. Later the belt drive was changed slightly to give 145. That was a compromise in avoiding 120 and 180 by a large margin, and 150 by some margin. The margin of 25 or 5 may be compared with the meter-response low-pass cutoff near 0.5 c. The amplifier should be linear to the extent of the total amplified amplitude of any harmonics, so the tuning should be sharp enough to reduce their output to an amplitude comparable with that of the amplified residual noise in the narrow band of meter response. This requirement was not explored in the tests, but there was not perceived any evidence that it may not have been satisfied.

The weak current from the photocell was converted to voltage for amplification, by using a high resistance (10 megohms). For calibration, a known voltage was provided through a divider, likewise interrupted by the synchronous commutator. That voltage, divided by the high resistance, provided the calibration in terms of equivalent current from the photocell. This calibration was independent of shunt capacitance or other loading by the amplifier input circuit, but their effects were rather small.

The amplifier was intended to amplify only the desired frequency component, so it was tuned to 145 c. Fixed tuning in each stage was provided by a mica capacitor in parallel with an iron-core inductor (with airgap).

The amplifier comprised two tuned stages using the UX-222 screen-grid tetrode for "voltage amplification", followed by one stage using the UX-171A low-mu triode for power output. The screen-grid tetrode had just become available, and gave a gain-per-stage much greater than the high-mu triode. The gain-per-stage was over 40 db, obtained with high-impedance tuning at the low audio frequency. Around the operating frequency of 145 c. the nominal bandwidth was about 30. Therefore the tuning was not critical and the two tuned circuits gave some rejection at 60, 150 and 180. The output AC was connected to the meter through a current-doubling transformer, subject to rectification by the commutator. The final amplifier was completed before 280524, the date it was described in my notebook.

The commutator operated by alternating short-circuit and open-circuit on 5 pairs of segments. One contact was used for synchronous rectification. It was adjustable for phase angle to yield maximum output. Another contact (on the opposite side) was used for interrupting the calibration voltage. The two functions were independent.

The output meter was a large table model that was sensitive by the standards of that day. Its lowest scale was 0.1 ma in a 50-ohm coil, with a parallel damping resistance about 500 ohms. Its inertia and suspension gave a response with low-pass cutoff at 0.49 c. toward random noise. I measured this response for use in measurement of noise. In the observation of rectified output at 145 c., the effective bandwidth toward random noise was 1.0 c. (145 ±0.5).

The essential feature of this device was this small frequency bandwidth, obtained by synchronous rectification rather than sharp tuning. Furthermore, the observed output had equal fluctuations in both directions with no bias of the average.

The measure of the current sensitivity of the amplifier was the residual fluctuation caused by random noise. It came almost entirely from the thermal noise in the high R at room temperature. A much weaker contribution came from the amplifier. Therefore the RMS error of single observations could be predicted. This offered an opportunity for measuring and interpreting the observed random noise.

In the high R at room temperature, these conditions are typical of thermal noise:

R = $10^7$ ohms
temperature = 290 K
bandwidth = 1 cycle
thermal RMS voltage = $4 \times 10^{-7}$ volt
equivalent RMS current = $4 \times 10^{-14}$ amp

An equal RMS alternating-current component would be obtained by interruption of a direct current about twice as great, or $8 \times 10^{-14}$. Therefore this is the standard deviation of single observations of photo current. The random noise contributed by the amplifier was trivial when added in quadrature.

The square-wave calibration of the entire amplifier (from high R to meter) was measured to be:

$$\frac{\text{output rectified DC in meter}}{\text{input interrupted DC in high R}} = 4 \times 10^7$$

On the lowest scale, one division was 1 microamp, corresponding to input current of $2.5 \times 10^{-14}$. Therefore the RMS fluctuation was about 3 divisions.

My conclusions from these experiments were summarized in the following abstracts, which were read by title and printed in the program of the Minneapolis meeting of the American Physical Society, 281130-1201. They were published in the minutes of that meeting, Physical Review, vol. 33, pp. 114 and 124; Jan. 1929.

**Amplification of photoelectric currents.** Harold A. Wheeler, Johns Hopkins University. A convenient and rugged amplifying apparatus has been developed for the direct measurement of very small photoelectric currents (or x-ray ionization currents). The light beam is interrupted by a rotating sector wheel at a frequency of 150 cycles per second; the resulting 150-cycle alternating component of the photoelectric current is amplified more than $10^8$ times by a three-stage, broadly resonant, vacuum tube amplifier; the amplified current is rectified by a synchronous commutator with adjustable brushes; and the rectified current is measured by a portable micro-ammeter whose deflection is directly proportional to the photoelectric current. The factor of proportionality is determined by introducing a known current into the photoelectric cell circuit from a second set of brushes on the same commutator. The only time lag in this measurement is the period of the microammeter, about two seconds. The accuracy of measurement of very weak currents is limited by fluctuation phenomena in the amplifier. In selectivity against fluctuations and other disturbances, this system is equivalent to a sharply resonant amplifier with a resonance curve only one-half cycle in width. In the experiments performed, the fluctuations caused a "probable error" of between $10^{-14}$ and $10^{-13}$ ampere (for a single observation). Improvements have been outlined which should reduce this error to below $10^{-15}$ ampere.

**Measurement of electrical fluctuation phenomena.** Harold A. Wheeler, Johns Hopkins University. In experimental work on "shot effect" and "flicker effect" in vacuum-tube amplifiers and on the "thermal agitation" in electric conductors, it has been customary to amplify and rectify the fluctuations, and to observe directly their mean square amplitudes. An alternative method of measurement has been developed in which the fluctuations are amplified and made to cause a proportional agitation of the indicator of a galvanometer. The average deviation of the indicator from its zero position is determined by a series of observations, and the mean square amplitude of the fluctuations is computed therefrom. This method is especially applicable to the measurement of fluctuation components with frequency bands less than one cycle wide, at audio or sub-audio frequencies. The flicker effect has been measured in two different amplifiers, in one case within a frequency band 0.4 cycle wide at 0.4 cycle resonant frequency, and in the other case within a frequency band 1.0 cycle wide at 150 cycles. These observations indicate that the flicker effect may be caused by disturbances of several seconds duration. The thermal agitation in a resistance of $10^7$ ohms was measured by the latter amplifier; the agreement with the theoretical value was well within the probable error of the experimental value (8%).

**Note.** The lower-frequency measurement of the flicker effect was made with the Heart Amplifier described in another section. The center frequency of 0.4 and the bandwidth of 0.4 resulted from the product of the low-frequency reduction factor in that amplifier and the low-pass factor in the sensitive DC meter.

About the time I started this project, I was interested in the related topic of the "shot effect". This term denotes the "noise" in a current of electrons emitted and collected at random, free of any interaction (as by appreciable "space charge").

There has been much confusion between the "shot effect" and the "thermal agitation" of electrons in a resistive medium. The latter is subject to space charge, which leaves less fluctuation of the current. Both are random fluctuations which may be amplified to hear as "white noise". Both were first described in 1918 by the famous German scientist, Walter Schottky. He named the "shot effect" after the sound of the shower of lead pellets in the old "shot tower", likewise random and free of "space charge". The thermal agitation was further studied and published in 1926 in English by J. B. Johnson of Bell Labs., so it has been mistakenly distinguished as "Johnson noise".

In 1925, my attention was called to the shot effect by the classic paper of Hull & Williams (G.E. Co.) in Physical Review, 1925 Feb. They measured the shot effect by means of a tuned RF amplifier. The amplifier was the introduction of the screen-grid tube which was destined to supersede the Hazeltine neutralization. The fluctuations of the shot effect were predicted by a theorem said to be "familiar" in statistics. It was not familiar to me. On 260309, I wrote to Dr. Hull, expressing interest in his work and asking for a reference for this theorem. He replied but could not readily give me a reference. In the same letter, I proposed a simple derivation which had physical reality and therefore appealed to me. He replied that he doubted its validity.

My derivation started with the charge of one electron arriving on the capacitor of a resonant circuit. The result is a free oscillation of known behavior. I reasoned that a current of electrons arriving at random would yield the mean-square voltage of many such oscillations, superposed with random phase. The rule for such superposition is quadrature addition. The mean-square voltage becomes proportional to the average current, not its square. From the known properties of the resonant circuit, I derived the spectral density of random fluctuations in the current of electrons. This reasoning led to the correct result with each step having physical significance.

On 260218, I spoke before the seminar of the Physics Dept. on the subject of the "Schottky effect". The topic was the shot effect, and I may have presented my simple derivation.

After getting Dr. Hull's reaction to my derivation, I became convinced of its validity and sent it to the American Physical Society for their next program. I presented it on 270422 in Washington, D.C., and the following abstract from the printed program was published in Physical Review, vol. 29, p. 903; June 1927.

**Theory of the shot effect.** Harold A. Wheeler, Johns Hopkins University. The shot effect, described by Schottky, is the phenomenon of current fluctuations in a stream of electrons limited by random emission, as from a hot filament. Previous derivations of the magnitude of shot effect have been based on equations derived in the abstract by the theory of probability. In the present paper, a simple derivation of the equation, $(I_o^2)_{mean} = eI_a/2RC$ ($I_a$ = average space current), is given in terms of the familiar discharge current in a simple series circuit (R,C,L). This is followed by a Fourier integral derivation of the continuous frequency spectrum of the current fluctuations: $d(I^2)_m/d\omega = eI_a/\pi$. The simple derivation is thereby linked with former work and the correctness of the assumptions verified.

In the discussion period after my talk, Dr. Hull stated his doubt as to the validity of my derivation, though it gave the correct result. Another expert, who happened to be a friend of mine, rose to defend my thesis. Later I learned that a prominent theorist in BTL had concurrently published in the Journal of the Franklin Institute a deviation like mine. On 270606, I wrote a brief paper on the subject, of which I have a copy. It was never published, beyond the abstract, so the simple derivation has gone unnoticed.

By some reference dated 1926 Dec., I became aware of Dr. J. B. Johnson and his studies of thermal agitation in a conductor. I called on him at BTL in New York on 270111, initiating an acquaintance that brought us together at various times, both at meetings and by correspondence. In his letter of 270120, he described the concept of thermal agitation in the space charge region of a vacuum tube.

On 271117, I spoke to the Physics Dept. seminar on this subject, under the title "Brownian movement of electricity". It was based on the 1918 viewpoint of Schottky, the degrees of freedom including a resonant circuit as one. Here again, this was a simple derivation that appealed to me because it had a physical basis. Thermal agitation causes equal average kinetic energy in every particle in a gas, and an immersed resonant circuit becomes one particle. From this principle, one can derive the spectral density of thermal voltage attributable to the resistance in the tuned circuit, or in general.

The interrupted light beam is a powerful technique for detecting a weak source of light in the environment of a stronger continuous light. If the weak source cannot be modulated, a nearby external modulator is required. The sector wheel I used is an elementary example and the synchronous commutator is a natural elementary rectifier to be used in conjunction with the modulator.

A practical application of this technique is now used in airports for observing the height profile of haze or any other partial obstruction of visibility. It is termed the "ceilometer", referring to its measurement of the ceiling of an overcast. A fixed vertical telescope defines a vertical "beam" in space. From a base separated by about 500 ft., a light beam is scanned in elevation angle to intersect the vertical beam at a height varying with angle. The light beam is electrically generated and interrupted at a low audio frequency. The telescope has a photocell with an electronic synchronous detector for selectively sensing the scattering of the light beam. During each vertical scan by the light beam, the time variation of the detected signal gives the height profile of the haze. The intersection of the beams can be tracked in ambient daylight, when it may not be visible to the eye. It can be tracked through a moderate amount of intervening haze. Very little time is required for each vertical scan. This technique has proved extremely valuable.

So far as I have learned in the 50 years since my experiment at Hopkins, that was the first demonstration of the synchronous detection of a modulated light. It is the most powerful technique for detecting a weak source through much stronger ambient light.

All of these studies and experiments culminated in the following prospectus which I intended as a guide in preparing my thesis for the Ph.D. degree.

May 22, 1928 Thesis.

**Subjects to be treated:**

Amplification of small currents.
Thermal agitation limitation, measurement.
Electrometer limitation (thermal agitation in leakage).
Special tube designs (small gas and leakage).
Grid-induction shot effect.
Probable error of average fluctuation.
Measurement of galvanometer spectrum.
Methods of interrupting light beam or source.
Methods of synchronous rectification.
Calibration of amplifier by small condenser.
Math of rectifying system.
Math of shot effects.
Amplifier design.
Advantages of amplifier versus electrometer.
Double Fourier integral requirement of amplifier selectivity and lack of distortion in last tube.
Effect of commutator in reactive circuits.
Iron-core distortion.
Proposed arrangements, remote control.
Applications to Xray and photo experiments.

In retrospect, this prospectus was overly ambitious, especially considering that my allotted 3 years at JHU was drawing to a close. I did intend to write such a thesis and earn the degree, but that course was crowded out by other activities. I completed work on the heart amplifier, then vacated my lab space on 280616. On Sunday 280701, I took Ruth and baby Dorothy for a farewell call on Dean Ames and Mrs. Ames. July and August were occupied with some Company work in New York and Hoboken, some visiting in Washington, and moving from our Baltimore apartment to a larger apartment in Jackson Heights (not far out in Queens on Long Island). Thereafter, I was very busy with Company work. Our second child was due in December. In summary, however, I must admit that my failure to write a thesis was attributable to a deficiency in self-discipline and planning.

[A] G. Ferrie et al, "Amplification of weak currents and their application to photo-electric cells", Proc. IRE, vol. 13, pp. 461-470; Aug. 1925. (Includes interruption of light beam, followed by AF amplifier.)

### 1.9 Wave-Filter Determinants 1926-28.

The wave filter comprising number of repeating sections was a familiar form of frequency-selective network. I had been attracted to it from the beginning of BSTJ in 1922-23. In the Fall of 1926, I made a further study of the subject, with reference to some circuit configurations and the determinants for mathematical description of their behavior. I chose this topic for 261129, my third talk before the Physics Dept. seminar. As usual, the staff was well-represented at this meeting, including Prof. Murnaghan. I do not have a record or a clear recollection of the scope of my talk. However, it did lead to a joint paper we published afterward.

After my talk, he introduced me to "difference equations". It was a math technique suited to the evaluation of my determinant in a general form for any number of repeating sections and any terminations at the ends. With his advice, I wrote a paper on its applications to various types of wave filters.

A simple example is the wave filter shown in Fig. 1. It has (n-1) repeating sections with "mid-termination" between any generator and load. Its response ratio is computed from the following determinant, in which w may be a function of frequency.

$$D_n(a,b) = \begin{vmatrix} w+w_a & -1 & 0 \dots & 0 \\ -1 & 2w & -1 \dots & 0 \\ 0 & -1 & 2w \dots & 0 \\ \dots & \dots & \dots & \dots \\ 0 & 0 & 0 \dots & w+w_b \end{vmatrix}$$

This I termed, the wave-filter determinant. It has two distinctive properties:

- The main diagonal and the adjacent diagonals are repetitive, representing the repetition of like sections.
- The "minor" or "cofactor" of the first row and last column is simply unity, which leaves the overall response ratio dependent only on the main determinant.

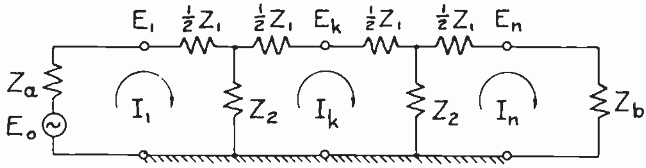
In the seminar, I presented this situation and the resulting behavior of several types of filters.

Prof. Murnaghan taught me the evaluation of this determinant by "difference equations", a technique found in some math textbooks. Defining $D_n$ for $w_a = w_b = w = \cosh\Gamma$, it is found that

$$D_n = \frac{\sinh(n+1)\,\Gamma}{\sinh\,\Gamma}$$

$$\frac{D_n(a,b)}{w_a + w_b} = \exp(n-1)\Gamma + \frac{(w_a - \sinh\Gamma)(w_b - \sinh\Gamma)}{(w_a + w_b)\sinh\,\Gamma}\sinh\,(n-1)\,\Gamma$$

These are general expressions for any number (n-1) of like sections. The latter formula gives the effect of inserting this number of sections; it is unity for zero number.




Ratios: $G_1 = \dfrac{Z_b}{Z_a + Z_b}$, $\quad G_n = \dfrac{E_n}{E_0} = \dfrac{w_b}{D_n(a,b)}$

Substitutions:

$$\cosh \Gamma = w = 1 + \frac{Z_1}{2Z_2}, \quad w_a = \frac{Z_a}{Z_2}, \quad w_b = \frac{Z_b}{Z_2}$$

1.9 Fig. 1 — Wave filter with mid-series terminations, (n-1) sections.

The "propagation exponent" or "transfer exponent" per section is Γ, which depends on the circuit configuration and parameters of each section. Its frequency variation is introduced through the related term w in the determinant, as exemplified in Fig. 1.

This direct solution for any number of sections and any termination was an accomplishment. It implicitly includes the effect of multiple reflection between the terminations. I applied it to a number of examples, computing and plotting the frequency variation of overall response.

A series of like sections has limited practical utility, because different sections can be combined for more economical satisfaction of any particular overall requirements. For example, some types are better suited for the terminations. Therefore the repeating sections are of interest mainly in demonstrating the wave concept in a line of many like sections.

A different application of difference equations is better suited for this purpose. Each intermediate row of the determinant gives a difference equation in terms of the currents in three consecutive sections. This application is similar to the differential equations for a continuous line, which are used to derive the wave propagation on the line. This solution had been published a few years earlier by G. A. Campbell in his U.S. patent no. 1227113 (1917), and reported by G. W. Pierce in his book (1920). It provided the background in terms of the wave concept. I was familiar with the classic 1922 paper by Campbell in BSTJ, which did not make reference to either of these earlier articles, and did not utilize the wave-filter determinant.

Because I was not sufficiently familiar with the earlier publications, I did not emphasize the distinction between that solution and ours. The latter contained some concepts and formulas having theoretical interest, but also I included too much explanation that was a duplication of the BSTJ articles. The Physical Review rejected our paper, probably because it was too long for the extent of its original contributions. Prof. Murnaghan sent it to the British journal, Philosophical Magazine, in which it was soon published.

[W6] H. A. Wheeler, F. D. Murnaghan, "The theory of wave filters containing a finite number of sections", Phil. Mag., vol. 6, pp. 146-174; Jul. 1928.

The manuscript of this paper contained many lines of math. I typed it on my Hammond Mathematical Multiplex portable typewriter. I had become familiar with that machine during my first summer in Hoboken, where it was used to type the Professor's lecture notes. I bought one in New York on 250904 for use at Hopkins. It was a versatile but weak mechanical design with interchangeable type shuttles. It was the predecessor of the later Veritype machines and the present IBM Selectric. It became a collector's item, so I presented mine to the Smithsonian Museum in Washington.

I presented this paper briefly before the American Physical Society meeting in Washington, D.C., 270423. Afterward, I met Prof. Randall of the U. of Michigan (Ann Arbor) and he offered me a fellowship (which I was not free to consider). The following abstract was published in the minutes of that meeting, Physical Review, vol. 29, p. 914; Jun. 1924.

**Analysis and applications of wave filter determinants.** Francis D. Murnaghan and Harold A. Wheeler, Johns Hopkins University. In the study of the electric wave filter comprising a finite line of recurrent sections, the simultaneous equations in currents and voltages yield determinants of a known type. By the method of difference equations, various typical determinants are evaluated and expressed in terms of convenient substitutions. Special attention is given to the finite cases involving (1) terminal conditions and (2) two recurrent sections in alternating succession. Procedures are given and simple formulas developed applying the determinants to (1) free state response of finite, non-dissipative Campbell filters, terminated by any values of resistance, as a function of the frequency of the applied alternating voltage. Proceeding to the infinite line by a novel method, the iterative impedance and propagation exponent are derived. The definition of complex electric impedance is outlined to show the fundamental differential equations involved and thereby make the work equally applicable to other systems with similar differential equations.

## 1.10 The Heart Amplifier 1927-28.

At the Johns Hopkins Hospital in 1927, Dr. Edward P. Carter and Dr. E. Cowles Andrus were investigating the "refractory period" of the heart. It is related to the malfunction known as fibrillation. They perceived an opportunity for some application of the advancing electronic techniques, so they approached the Physics Department for cooperation. The result was my development of the device I have termed the Heart Amplifier, which enabled a great advance in their experimental technique.

On 270603, Dean Ames called me in and told me something of their problem. He said he had selected me as best prepared to help them, and I was pleased to tackle this unusual problem. Dr. Andrus invited me to lunch with him at the Hopsital on 270702, which was the beginning of a close friendship and an exciting collaboration. Beyond a few notebook entries, my active work was delayed until Fall.

In an experimental animal, electric probes at separated points in the exposed heart yielded action currents that could be observed. They were recorded photographically from a sensitive string galvanometer with fast action. It responded in a few milliseconds. This was the advanced electrocardiograph (EKG) of that day. The research physicians became skilled in its use.

Dr. Andrus told me they needed a timer that would trigger an electric shock at a specified time interval after a heartbeat in the normal rhythm. The latter would be indicated by one of the repeating pulses of action, after much amplification. I designed a falling arm with a magnetic release and a mechanical trip switch for the delay timer. It was easily made and simple to understand and to use. (Today we would use an electronic clock as the timer, and its operation would be visible to the extent of displayed numbers.)

A few days after my first meeting with Dr. Andrus, I was in New York (270706) and told Prof. Hazeltine about the problem. (I had worked with him in the summer of 1924 on the radio-electric clock, which had been my introduction to a magnetic device operated from a pulse amplfier.) He told me I would need a laminated iron core in the release magnet for quickest response to the pulse of current from an amplifier. Therefore I used the E-core of an AF transformer, with a thin sheet-iron armature.

In 1928, Jan. & Feb., I built and tested the amplifier and the delay mechanism. I had accumulated most of the necessary parts. The resulting device is best described in my article in Journal of Experimental Medicine, vol. 1, pp. 364-367, dated 300301. It was an Appendix to an experimental report by Andrus & Carter. The electrical and mechanical design are not of primary interest here, but some features are notable. For purposes of testing, I made a key-operated simulator for generating a pulse like that specified for operating the equipment.

The entire device was intended to operate on an electric pulse of 0.5 - 2 mv with duration of at least 0.02 sec. The amplifier was provided with charging and discharging features as follows:

- Parallel capacitors to give a total charging time constant of about .03 sec, to protect against AF and RF interference.
- Series capacitors to give a total discharging rate of about 10 napiers per sec, to protect against low-frequency fluctuations (especially the "flicker effect" in a vacuum-tube amplifier).

A reversing switch on the input voltage was provided to select the pulse polarity for the more distinct output, as seen in the meter.

The amplifier was resistance-coupled with 5 stages as follows:

- 4 stages of voltage amplification by high-mu triodes (UX-240, mu = 30);
- One stage of current amplification by a medium-mu triode (UX-112A, mu = 8).

The latter delivered current pulses to a small 10-ma DC meter and the 10,000-ohm release magnet of the timing mechanism. The 6-volt A battery was common to all filaments. The 90-volt B battery was duplicated for the earlier and later stages to avoid feedback by resistance coupling in a common battery.

The timing mechanism was designed to trip a switch at a specified time (0.05 - 1 sec) after one pulse of action current. It included these features:

- A pulsing magnet to initiate the timing process. It was a quick-response magnet made of a laminated E-core from an AF transformer with many turns (10,000 ohms). The armature was made of thin sheet iron.
- After a starting switch, this magnet clicked off the heart pulses. By means of an escapement, the first pulse was skipped and the second pulse released a trigger to initiate the timing. This feature was insurance against the accident of erroneous timing relative to the first pulse.
- The trigger released a pivoted horizontal balance arm, which was loaded by two adjustable weights. Their unbalance determined the speed of falling on one end.
- An adjustable fixed arm carried a trip switch for the desired stimulating shock. The calibrated angle of this arm, together with the balance of the weights, determined the time lag.
- The stimulating shock was a short pulse from the secondary of an induction coil, on opening the DC circuit through the primary.
- The trip switch was reset on lifting the balance arm, then tripped at the specified time after its release.
- On lifting the arm, the sequence was:
  (i) While leaving a short-circuit on the secondary,

(ii) close the DC circuit in the primary, and then
(iii) remove the short-circuit on the secondary.

- In response to the falling arm, the sequence was:
  (iv) Open the DC circuit to generate the shock, and then
  (v) short-circuit the secondary to protect the stimulating electrodes until the next shock.
- For a few positions of the balancing weights, the time delay was calibrated and graphed so the mechanism could be set for any specified value.

The month of March in 1928 was largely occupied with demonstrations I made, after which Dr. Andrus and his team were able to use this device for their experiments This result was a credit to their laboratory skills and to the basic simplicity and visibility of the design.

In the usual demonstration of this equipment, a person provided the heart-action voltage from external electrodes on his arms. Each electrode was a wet plate on the forearm or a cup of water for the hand. In spite of the relatively weak coupling to the heart region, and the exposure to electrical fields in the environment, the amplifier would actuate the magnet to click off the heartbeats. On lifting the balance arm, the escapement would release the arm and trip the switch.

The following is a list of demonstrations of the complete equipment, conducted at that time (see my Notebook No. 10, p. 108):

280301 At JH Hospital, to Dr. Andrus, Dr. Carter, and other members of the staff, using arm electrodes.
280314 At JH Hospital, to Dr. Andrus, on the exposed heart of an experimental animal, the first test of the timed stimulus.
280320 Ditto, the first records of experimental observations.
280322 My afternoon talk and demonstration to the Physics Journal Meeting at Homewood (the seminar of the Physics Dept.) using arm electrodes and simulation of heart pulses.
280323 To Dean Ames in my lab, using simulation.
280323 My evening talk and demonstration to the regular meeting of Gamma Alpha, using arm electrodes.
280331 At JH Hospital, with Dr. Andrus, a demonstration to memebers of the Interurban Clinical Society, on various members using hand electrodes.

Then the equipment was left at the Hospital for use by Dr. Andrus.

A few days later, Dr. Andrus phoned me to report that he was using the equipment. He requested a brief description suitable for immediate publication. This and a later description were published as follows:

[W5] H. A. Wheeler, E. C. Andrus, "A device for determining refractory period of the mammalian heart during normal sinus rhythm", Proc. of the Soc. for Experimental Biology and Medicine, vol. 25, pp. 695-696; 1928. (An abstract with brief description of the heart amplifier and timer.).

[W9] E. C. Andrus, E. P. Carter, (H. A. Wheeler), "The refractory period of the normally beating dog's auricle", Jour. of Experimental Medicine, vol. 51, pp. 357-367; Mar. 1, 1930. (Appendix by H. A. Wheeler, "Amplifier and timing device", pp. 364-367. A complete diagram of the "heart amplifier and accessories", dated 290422.)

## 1.11 My Early Association with Hazeltine 1922-25.

My first contacts with Prof. Hazeltine occurred while I was living in Washington and working summers at the Bu. of Standards.

The first contact was anonymous. During the summer of 1921, my first experience in the Bu. of Standards was testing some professional radio receivers. One was the SE-1420 Navy Receiver. I spent many hours experimenting with that receiver at the field station in Chevy Chase, Md. I was impressed with its design and performance. Only later was I to learn that Hazeltine had designed this receiver while serving as consultant in the Washington Navy Yard in 1918. Furthermore, his invention of neutralization had occurred as a byproduct of that design.

My first personal contact was in Washington 220227. That day was the beginning of the National Radio Conference, chaired by Herbert Hoover, then Secy. of Commerce. My father was the representative from the Dept. of Agriculture, and he took me to the first session. Prof. Hazeltine was one of several experts from outside the Government. My father introduced me to him and others.

A few months later, my first meeting with Hazeltine resulted from an accidental encounter. My father had a business trip to New York City and he took me along. On 220809, we were lunch guests of a friend of his in Hoboken at the Lackawanna terminal. That was a nice dining room not far from Stevens, so the Professor often walked down there for lunch. As he passed our table, he spoke with my father and me, and invited us to call on him later in the afternoon. When we sat down in his office, I started telling him about the neutralized TRF amplifier I had been operating (since 220714). Then he showed me his pending application for a patent on the same invention. His was a "paper" invention, so he asked me how it worked. Neither of us was ever the same again. So far as we know today, we two were the only people in the World who were then working on this particular form of neutralization. It was the form destined for extensive use in broadcast receivers. The accident of our meeting was one of those rare coincidences and it turned out to be fortunate for both of us. I was 19.

Two months later, 221026, I received a letter from the Professor, suggesting that we discuss an arrangement for cooperation. He visited us in Washington 221103-04. I showed him my work and he offered me some relationship that would include a small but substantial fraction of his royalties on his invention. I had the advice of my father and my patent attorney, Elmer Stewart.

Here we must recognize some rules of U.S. patents. The intent is to issue a patent to the "first inventor", if he promptly makes application in the Patent Office. There is no "second prize". A successful "reduction to practice" is not required. In the case of Hazeltine's invention of neutralization, I independently but later conceived the same invention, and then made the first reduction to practice (subsequent to his filing date). Therefore we recognized that I had no claim to patent rights.

The day after the Professor talked with us in Washington, I proceeded to invent another form of neutralization which was an alternative to his. It was termed a "capacity bridge". It was a useful alternative, though deficient in two respects. It yielded somewhat less amplification and its circuit was less economical to build. (I was finally awarded some patents on this invention, after losing the broadest claims in an interference contest in the Patent Office.) Because this was a competitive invention which I could claim, it became a substantial factor in our negotiations.

We proceeded with further correspondence and meetings in Washington with Hazeltine, his patent attorney (Willis H. Taylor, Jr.), my father and myself. We reached a final agreement on 230328. I was to assign my related patent rights to Hazeltine and in return to receive 10 percent of his royalties. (This yielded to me about $17,000 before it was superseded by Hazeltine Corp. 240201.) This agreement established for me a continuing relationship with the Professor and then with the Hazeltine Corporation soon to be formed. He employed me to work with him the following summer in his laboratory at Stevens in Hoboken.

Between the summers of 1922 and 1923, Hazeltine had put his invention into a receiver design for manufacture by some companies who were the members of IRM (Independent Radio Manufacturers, Inc.). They had an exclusive license under his pending patents on neutralization. The sets were sold under the trademark "Neutrodyne", which was an inspiration of his patent attorney (Willis H. Taylor, Jr.). The Professor described his design to a regular meeting of the Radio Club of America at Columbia Univ. on 230302. He demonstrated the Freed-Eisemann model, which was one of the first to be marketed. This presentation was published the next month in QST, the journal of the ARRL. On 230406, I spoke to the Radio Club in Washington, D.C., describing the Hazeltine Neutrodyne in general terms.

During the summer of 1923, I had a nice room in the Theta Xi house adjacent to the Stevens campus. It was convenient to the Electrical Engineering Dept. headed by Prof. Hazeltine. I worked with two others as his assistants that summer. They were Stevens graduates who were instructors on his staff, namely Robert E. Poole and John F. Dreyer, Jr. We became close friends for life. The Professor gave us several lectures relating to our work, and also we took turns speaking to the group.

The inspired engineering in the Neutrodyne design was a revelation to me. Its simplicity and its refinement in performance were typical of the Professor's genius. It was characterized by three like dials on the front panel for tuning the antenna and two RF amplifier stages. Five UV-201A triodes were used for the 2 RF, detector, and 2 AF stages. The neutralization was accomplished by the procedure I had used in my first tests, for which the Professor always gave me credit. For adjusting the neutralizing capacitor in one stage, the filament of the tube was disconnected (cold) and the capacitive coupling was balanced out by adjusting for signal cancellation.

On my arrival for the summer, I soon became familiar with the Freed-Eisemann set which was then being sold. On the annual Alumni Day at Stevens, there was Open House, and my function was to demonstrate that set to visitors in the E.E. lab. It was a pleasure.

I still have the two notebooks of my work that summer. One is a bound notebook containing orderly entries of my theoretical studies and some experiments. The other is a small log book containing a sketchy record with many details. I learned a lot and I contributed my share. My salary was $140 per month, which was generous in those days, but my principal compensation was my share of the royalties being received.

I was assigned two principal areas of work. One was, learning more about the problems of the new models of Neutrodyne, and helping to solve them. The other was, trying out a novel scheme which may be termed the one-tube Neutrodyne. The Professor had conceived how neutralization might be used to enable the use of one tube as a reflex TRF amplifier and detector. I built and demonstrated this circuit in several models, and it performed as predicted. However, it was too "intensive" a design for practical use.

The Professor gave us about 8 prepared lectures on subjects relating to vacuum tubes and amplifiers, especially TRF amplifiers. He introduced me to the use of voltage and parallel circuits in circuit analysis, an advance over the customary current and series circuits then taught. He used the small power factor (p) as a measure of dissipation in a tuned circuit (rather than 1/Q). I spoke to two meetings on the subject with which I was most concerned: the capacity-bridge form of neutralization which I had previously invented; and the one-tube Neutrodyne which I had developed during the summer. I typed some of the Professor's lecture notes on the Hammond Mathematical Multiplex machine which was the forerunner of the Varityper and the IBM Selector typewriter. (I later acquired a portable model of this Hammond for use at JHU.)

Hazeltine's patent work was handled by the prominent New York firm of Pennie, Davis, Marvin and Edmonds (PDME) of which Taylor was a member. There I became acquainted also with R. Morton Adams and Baldwin Guild. All three were Stevens alumni and former students of the Professor, with whom I was to work closely in years to come. My first patent application was prepared by Guild. Its subject was the capacity-bridge form of neutralization which was included in my agreement with Hazeltine.

One of my privileges that summer was meeting the leaders in some companies who were members of IRM: I. P. Rodman of Garod Corp. (Garrison & Rodman); Joseph D. R. Freed of Freed-Eisemann Radio Corp., R. E. Thompson of R. E. Thompson Mfg. Co. Other prominent visitors were A. C. Maney, A. D. Silva (Wireless Improvement Co.) and L. C. F. Horle (who had engaged Hazeltine to design the SE-1420 receiver).

I bought a Freed-Eisemann Neutrodyne (NR-5, S.N. 868) for use in my home in Washington. Joe Freed presented me with a Western Electric horn speaker for use with it. That set is now an exhibit in my office.

My first summer away from home was made especially happy by attentions of my associates at work: the Professor often took me out to lunch or dinner; John Dreyer and his parents in Brooklyn entertained me several times at their home; Bob Poole, his father, his fiancee and her younger sister, who all lived near Hoboken, made many occasions for my pleasure. John Dreyer arranged for me to occupy a nice room on the top floor of the Theta Xi house next to the Stevens campus. I am grateful to all of them.

Between the summers of 1923 and 1924, the principal event was the organization of Hazeltine Corporation, a public stock company. Mr. Taylor negotiated with my father and me for my part in this venture. With my acquiescence, my royalty sharing agreement with the Professor (Hazeltine Research Corp.) was superseded by a contract with the new Company. I agreed to assign my related inventions in return for a nominal consideration (500 shares, $5000, and a retainer of $5000 per year). It contributed further to the support of my continuing education and it established my continuing relationship with the Company and the Professor.

In the summer of 1924, my second in Hoboken, I lived at the Brooklyn Central YMCA. I was mainly occupied with work for the new company in their lab at Stevens, and also with their patent attorneys, PDME at 165 Broadway in New York City. However, I did do some work for the Professor on one project in his lab. Also I was away for two weeks vacation at Star Island off Portsmouth, N.H.

The Professor and John Dreyer were designing a special receiver for T. S. Casner, Chief Engineer of the Radio Electric Clock Corp. Its function was the use of time signals from Arlington (NAA) for automatically setting a master clock at noon and 10 PM. It was later installed in the Statler Hotel in Boston. It became obsolete with the advent of the Telechron. Its description was published by two other students and instructors from Stevens.

The receiver had a fixed-tuned neutralized RF amplifier for operation on 112 Kc. Its passband was very narrow for selecting the signal carrier from noise. Its width was about 1 Kc, so it even reduced the AF sidebands (± 1 Kc) of the transmitter. Each tuner was enclosed in a shield box with its inductor, which was a "pancake" coil of stranded (litz) wire.

The most unusual feature was the mechanical pulse decoder operated from a magnet at the end of a pulse amplifier. The time signal was a coded succession of pulses every second for 5 minutes just before the hour. The Professor designed the decoder to make use of the information and redundancy in this code, so there would be 5 chances for a valid timing signal to set the clock to the nearest minute. This was my introduction to a pulse amplifier with resistance coupling and capacitors in series and parallel. I think I contributed to its design for this purpose, when I was left in charge during the vacation absence of Hazeltine (at Point O' Woods on L.I.) and Dreyer. I learned that the multistage RC amplifier could be peaked at the pulse frequency and would actually oscillate 1 or 2 times after a pulse. (Four years later at JHU, I applied this knowledge in the Heart Amplifier. Many years later, such techniques have become common in integrated circuits made of RC and transistors.)

This receiver was to be demonstrated in the Hudson Terminal Building, where there was much radio noise from electric circuits. A wire antenna was located on the roof. It was tuned and coupled to a shielded two-wire transmission line connecting to the receiver on an intermediate floor. This may have been the first use of a matched line to protect against electric noise in a building. The line was multiplexed with a phone connection for use in tuning the antenna circuit far from the receiver. The various features of this receiver yielded reliable setting of the clock, day or night, even when the NAA signal was too noisy to read the code by ear.

The next summer was 1925. I had graduated from GWU and the Professor was retiring from his position in Stevens. He had finished his textbook, "Electrical Engineering", which served me as a reminder of much I had learned from him out-of-class. From then until 1933, he spent much of his time away from New York, but still participated in some Company activities.

My continuing association with the Professor was mainly incidental to Company activities relating to patent contests. Most of these involved the issuance and validation and infringement of his patents and mine, over a period from 1926 until 1941 (the beginning of World War II). They are recounted in other chapters. Our typical relation was, one of us serving as the inventor, a fact witness, and the other serving as the supporting expert witness.

There was one subject which built on the Neutrodyne and therefore attracted the attention of both Hazeltine and myself. It is covered under the heading, "Uniform-gain circuits". One of the problems in the Neutrodyne was the variation of gain with frequency of tuning in each stage of RF amplification, and in the antenna circuit. In the summer of 1925, I tackled this problem and devised some promising countermeasures. The Professor discovered that some similar circuits were already being used (but without neutralization) by a former student of his, Carl E. Trube. Hazeltine became fascinated with this subject and proposed some designs somewhat different from mine. Also he contributed some ideas in the related high-inductance antenna circuit invented by MacDonald. Trube's patent rights were purchased by the Company and he was employed in 1927 but shortly afterward was killed in an accident while making some experiments. It was remarkable that the related inventions of Trube, MacDonald and myself survived the obsolescence of the Neutrodyne because they were found useful in screen-grid TRF amplifiers.

Only MacDonald's survived in the simple superheterodyne which became common in the Depression. In 1930-31, I published an account of these developments, and the Professor added a note on the contribution of Trube.

See [W1] [H/P] and [W10].

## 1.12 The Family I Started 1926.

Ruth Gregory was born in Portsmouth, N.H., on 1904 FEB 26. Her father was Luther Elwood Gregory, a career officer in the Civil Engineer Corps of U.S. Navy. When he retired in 1930, he was Rear Admiral. Her mother's maiden name was Anna Ryerson Roome, from Lincoln Park, N.J. Her mother died a few days after Ruth was born. Ruth had four elder sisters. She was brought up by her father's elder sister, Miss Elma Coit Gregory, who took good care of the daughters and was like a mother to Ruth. Ruth attended public schools in Portsmouth, Newark and Boston. She started in Simmons College, then started again 1922 in George Washington Univ., Washington, D.C. She was an excellent student and graduated with the degree of B. A. from GWU Columbian College in 1926.

Ruth was a slender brunette with beautiful features. She was medium height, just right for ballroom dancing, which came naturally to her. She walked with a slight stoop, which went with her relaxed approach to people. She would casually converse with anyone in a friendly way, and remembered people. She had good taste in selecting from the contemporary styles, which were becoming to her.

The spring of 1924 was just after the beginning of Hazeltine Corp. In my third year at GWU, I was student assistant in the physics lab. In her second year, Ruth was a student in that lab. We soon became close friends and I took her to the spring social functions. A year later, shortly before I graduated, we became engaged to be married. After she graduated, we were married 1926 AUG 25 in All Souls' Church (Unitarian) in Washington. Then we traveled in New England three weeks and stopped in New York a few days on the way home.

During the year of our engagement, my time was severely taxed but I was well and happy. It was my first year at Hopkins, living in the dormitory, and my first year away from home. It included my third and fourth summers in the Hoboken lab. I initiated our work on "uniform gain" and "automatic volume control". That Christmas holiday, I built the first diode AVC receiver in my basement lab at home. I shuttled between Washington and Baltimore for social activities and family affairs.

In the fall of 1926, we occupied a nice apartment near JHU. It was in the Hopkins Apts. (314) at St. Paul and 31 Streets. We enjoyed a happy two years there, until the end of my 3 years at Hopkins. We had friends among the university staff and other graduate students. Ruth was the perfect hostess for dinner guests. I was near enough to come home for lunch and much of my studying.

In June of 1927, I took Ruth to South Dakota and Minnesota, just 11 years after I had moved east. We visited my many friends still in Mitchell and my many relatives in Minneapolis, followed by a boat trip east on the Great Lakes.

The first summer in Baltimore (1927) my job took me to Chicago most of the time. I was working with the Howard Radio Co., a Hazeltine licensee.

Ruth's aunt lived with us that summer and kept her company.

Our first child, Dorothy, was born 1927 OCT 24 at the Johns Hopkins Hospital. She and Ruth were in good health and it was the least strain one could expect. The hospital staff were especially cordial to the family of a young student.

In the fall of 1928, we moved to a five-room apartment in Jackson Hts., a short distance out on Long Island. It was on the fifth floor at 78 22 St., near the elevated station on Roosevelt Ave. We were happy there for another two years.

From there I traveled to work in three different locations, all fairly convenient. The first few months were the last at the Hoboken lab. Then I was convenient to the N.Y. lab until late in 1930, when I took charge of the Bayside lab further out on L.I.

Our second child, Caroline, was born 1928 DEC 17 in the Harkness Pavilion of the Columbia Presbyterian Medical Center in upper Manhattan. Again the mother and baby had excellent care.

Our third child, Alden Gregory, was born 1930 NOV 17 in the same place, near the end of our stay in Jackson Hts.

On both occasions, the Harkness Pavilion afforded a view of the construction of the George Washington bridge.

In 1930, we found a lot in a developing residential community ("Smart Country Homes") in Great Neck, just beyond the City Line in Nassau Co., on the North Shore of L.I. It was 18 Melbourne Rd. in Russell Gardens (later an incorporated village). It was a half-a-mile south of the LIRR station. There we built a luxurious house which was to be our home for the next 40 years. (Our architect was my boyhood friend from Mitchell, Clarence B. Litchfield, then practicing in N.Y. City.) We moved there in December.

We selected Great Neck for one reason. It was the nearest to the Bayside lab that would be outside N.Y. City. The schools in Bayside were excellent but Great Neck schools were reputed to be the best. Our children went through high school in Great Neck. Caroline was near the head of her class. Unlike my high school in Washington, the Great Neck High School was not a center of social activity, and our children were handicapped in that respect. The teaching was excellent and there were a few courses that were unusually valuable.

Russell Gardens had about 100 homes, middle to upper class, occupying about one-half the available lots. The Russell Gardens Assn. owned and managed a central wooded park with brook and swimming pool, and some nearby tennis courts. Our house was on a corner lot facing the park. It was a beautiful setting and well drained. It was in the Depression, and very few more houses were built until after the war. I served one year as President of the Association. Then Russell Gardens became an Incorporated Village, and I served a three-year term as Trustee. The Association had some social affairs. Some of our neighbors became our close friends for life. The most notable were the Davises (Emerson and Suzanne). He became our family attorney and later helped me to organize and to operate Wheeler Laboratories, Inc. (after the war, from 1947). Typically our neighbors had children around the ages of ours.

During our years in Baltimore and Jackson Hts., we were able to save enough to build a house. My early royalties from the Professor and my compensation from the Company were generous. Also Ruth inherited a substantial sum from her grandmother's estate in Lincoln Park, N.J. When we planned our new house, it was just the beginning of the Depression. We had investments sufficient to cover the entire cost. Then I made a large loan to an old friend who was trying desperately (in the "dust bowl") to pay the depositors in his bank in S.D. Our house cost a little less, with falling prices, but the dollar value of our savings plummeted. We finally required maximum first and second mortgages to pay for the construction. We were fortunate in continuity of my employment and salary from the Company. To meet my needs for insurance to protect our growing family, I invented the progressive type of policy that is common today. I started with maximum term insurance and gradually shifted to ordinary mutual life with increasing cash value. It was new to my agent but he cooperated to achieve the result I wanted.

After settling down in Great Neck, we were eligible to vote and became interested in Government. Our first presidential votes were cast for Herbert Hoover against Franklin D. Roosevelt in 1932. Later we learned that FDR had been elected by the less fortunate people who did not have phones and were more susceptible to empty promises. We who did have phones ended up paying most of the taxes he spent to keep himself in office for a disastrous reign of 4 terms. Locally, we encountered a situation that reflected the excessive Republican control of Nassau Co. The Belgrave Sewer District was spending money but not building sewers. I joined a nonpartisan committee which succeeded in electing one Republican who was not the party nominee. One result was an improvement in the party organization. Another was sewers, to which we connected about 3 years behind schedule. A small mistake by the Republican Party was soon corrected. The big mistake of the Democratic Party (from FDR to date) still prevails in Congress and the White House.

With our large house, our family and frequent dinner guests and house guests, we afforded a resident maid. Usually one girl stayed long enough to be a real help. One in particular (Julia Kristoff) lived with us several years and became attached to the children. Ruth and I were free to go out and we entertained often. A typical party was 4 or 6 couples for bridge. It was the time of transition from Auction to Contract, so everyone had to learn new rules. Ruth and I played bridge a lot, but just as a medium for social contacts. We did not play very well, and did not enjoy the discipline of memory and strategy. Our friends had little or no use for alcohol, in the Roosevelt era when it was becoming a fad. With help at home, I was free to travel. My work required frequent short trips and just a few long trips. Ruth's aunt lived with us some winters. She was especially helpful in assuming the responsibility whenever Ruth and I were away for a few days.

We were fortunate in having a car during the Depression. From 1930, we had a Buick sedan, which was luxurious for that day. A symmetrical pair of spare wheels graced the running boards at the front fenders. They were in no danger of being stolen. It had a 6-cylinder engine. From 1936, we had the smallest Ford sedan (65 HP) with V-8 engine. In 1939 we tried the Oldsmobile with semi-automatic transmission. From 1941, we had the Olds with automatic transmission, much like the mechanism that is prevalent today. To the Bayside lab, I had an easy 6-mile drive, with no traffic on Northern Blvd. (Route 25A). Before the children went to Sunday School, we often celebrated Sunday morning by the whole family going for a ride. Sometimes there was a specific attraction such as Roosevelt Field (to see the flying), Port Washington (to see the Bermuda clipper flying boat arrive or depart), the model boat basin (Port Washington), or the Bayside lab (for the latest in radio receivers). (There was no air terminal closer than Newark.)

The public schools in Great Neck lived up to our expectations. First Lakeville School, then Junior High and Senior High. Dorothy did well enough and Caroline was a top student. Alden had some handicap so we sent him for a few years to Wildwood, the youngest branch of the Woods Schools in Langhorne, Pa. (not far from Trenton, N.J.). There he made excellent progress. He was away from home 1936-40, then continued from grade 4 in Lakeville. Ruth was active in the PTA at Lakeville, and was elected President.

We became interested in the Community Church of Great Neck. It was 3/4 mile from our house. It was a struggling, rather informal chuch making do with a parish house and plans for a future sanctuary. From my New England heritage, I liked the concept of the community organization. At first, our principal interest was the Couples Club with its parties in the conventional format of catered dinner and dancing in the parish hall. We especially enjoyed the ballroom dancing to dignified music (with no amplifiers). We made friends. Then we sent our children there to Sunday School when they attained ages 8, 7, 5.

There was one period when the church enjoyed a wholesome growth and we all enjoyed its various activities. For six years, beginning in 1937, the minister was Hiram William Lyon. He was a sincere and gracious human being, and he was well prepared for all aspects of his task. In his sermons, he delivered an honest message in few words. When we joined the church, he did not require me to sign something I did not understand. One winter, Ruth enjoyed teaching a class of teenage girls and they were attracted to her. We both participated in some other functions, including the rejuvenated Couples Club. Mr. Lyon was not married. He died a year after Pearl Harbor, from a sudden illness brought on by the fuel shortage. The church never recovered.

We enjoyed taking our children to interesting places around town. Among the favorite attractions were the Circle Line (boat around Manhattan), the Bronx Zoo, the American Museum of Natural History, the Metropolitan Museum of Art, the Empire State Bldg., and Jones Beach (just new). The first World's Fair in Queens was convenient (1939-40). We entertained several pairs of nephews, nieces or cousins, and Ruth or I would take them to some attractions, which they never forgot.

From Great Neck, before the war, Ruth and I had several vacations or brief trips in New England, where she had relatives. Our two principal vacations were in 1938 and 1940.

- In 1938, Ruth and I enjoyed a three-week vacation in the west, traveling by train. On the west coast we visited National Parks and relatives. On the way home, we visited my many relatives in Minneapolis and lastly the Chicago lab.
- In 1940, we took our only ocean cruise. We left from Hoboken on the New Amsterdam and visited Havana, the West Indies and Venezuela. (The Germans invaded Norway.)

On these two trips I used my new Bell & Howell 8-mm movie camera, mostly with color film. Previously I had taken many photos with a Zeiss Ikon bellows camera. It was before the days of Hertz car rental, so we got around on tours or in cars provided by our hosts.

Our move to Great Neck and my assignment to the new Bayside lab ushered in a period (1931-38) which was the happiest in my association with the Company. The technical staff was about a dozen graduate engineers with diversified training and origins. Most of them were a little younger than I. We formed a congenial group for work and social gatherings. There were frequent occasions when we entertained several couples, or perhaps a whole family for dinner with our family. Ruth was the perfect hostess. They reciprocated by many favors to us. We were all fortunate in surviving the Depression. Then came the Little Neck lab and expansion, so the greater number of employees diluted our intimacy with the individuals. We often entertained at home for visitors (or a visitor with his wife) who were business guests of the Company. Sometimes it was a lunch group, sometimes a dinner party, and sometimes overnight.

In this growth period of my career, my family was a major factor in providing some of the essential ingredients. They contributed immeasurably to my health and happiness and to my standing in the community of my peers. Not the least was moral support and motivation for anything I chose to undertake.

[A] W. A. Wheeler, "Wheeler-Alden Family"; 1962

# Section 2. The Company

## 2.1 The Company 1924.

**Hazeltine Research Corporation** was the company formed by his patent attorney Taylor to handle the Professor's Neutrodyne invention and its royalties in 1923. My first agreement for royalty sharing was made with that company. It was superseded by Hazeltine Corporation, to which the Professor and I then assigned our inventions relating to neutralization in an amplifier.

**Hazeltine Corporation** was the company formed by Taylor to handle the increasing activities of the Neutrodyne and to provide for future growth and diversification. It was organized as a public stock company listed on the N.Y. Curb Exchange (now the American Stock Exchange). It started in business 1924 FEB 01 and has continued to the present under the same name. In 1960, its listing was changed to the more prestigious New York Stock Exchange, with the symbol HZ.

The initial financing of the Company was directed to the purchase of the Hazeltine invention. The immediate prospects of earnings were royalties on this invention. The financing was handled by Foster, McConnell & Co. Some of the first investors were close associates of Herbert Hoover before he became President of U.S.

The Company's first President was R. T. Pierson from Stromberg Carlson Co. in Rochester. In 1927, he was succeeded by Edgar Rickard (a Hoover associate) who held that office for many years. This was one of his various interests. Soon after the Company started in business, Jack Binns was employed as Treasurer. He became a close friend of MacDonald, the Chief Engineer, who was a few years younger. Each of them advanced to the highest offices and continued active until his death.

The Company's stock paid regular dividends from the beginning until a period of stress beginning in 1965. In the 1930's, its market price was very low, reflecting the depression of the market, but then recovered.

The Company was incorporated in Delaware and its residence was in New Jersey. Its corporate office was 15 Exchange Place, Jersey City, just across the Hudson River from New York City. The executive office was at 42 Broadway, N.Y. City. This was near our patent attorneys, Pennie, Davis, Marvin & Edmonds (PDME) at 165 Broadway. The President and Treasurer, with a small supporting staff, were located at the executive office.

My contacts with the Company started from my negotiations with Taylor and the Professor. I was employed under a retainer until I finished 7 years of college, then fulltime. The retainer was generous in comparison with a contemporary fulltime salary for an experienced engineer. It left me with much freedom of activity, including summers at the Hoboken lab.

I spent many days in the offices of PDME, in which Taylor was a member of the firm. My work was related to patent applications, interferences and litigation.

My work at the Hoboken lab, where MacDonald was Chief Engineer, was rather informal at first. I had a headstart with the Company, while Mac was feeling his way. By the time of the fifth summer (1928), when I started work fulltime, we had arrived at close cooperation. The Professor had left his teaching (only to return later) and Mac was providing very thoughtful guidance of my work. He always left me much freedom for exploratory work of my choice.

**Hazeltine Service Corporation** (HSC) was formed as a subsidiary before the lab moved to New York in 1929. It was a New York corporation formed to conduct the engineering business of the Company, while leaving the royalty income in New Jersey. MacDonald was President. HSC continued under this name until 1942, when royalties were stopped by the war and our income was henceforth derived from work in New York. I was an employee of HSC for its duration. On moving to the Little Neck lab in 1939, Dan Harnett was designated VP and Chief Engineer, and I was designated VP and Chief Consulting Engineer. At times, we were invited to the Board meetings of the parent company.

**Hazeltine Electronics Corporation** (HEC) was formed in 1943 by renaming Hazeltine Service Corporation, for conducting the engineering and manufacturing business of the Company in New York.

**Hazeltine Research, Inc.** (HRI) was formed in 1946 as a subsidiary to conduct the U.S. patent business of the Company when licensee activities were resumed after the war. Its office was located at the Chicago lab, which henceforth has operated in this subsidiary, performing our engineering services to licensees. Bill Swinyard became Chief Engineer of HRI, later President, and lastly Chairman on his retirement from regular employment in 1969.

**Latour Corporation** was a company formed to hold the many U.S. patents of the famous French inventor, Prof. Marius C. A. Latour. In 1925, an 80% interest was acquired by Hazeltine Corp., and later it became a wholly owned subsidiary. I had little to do with that situation. Its principal value was as a supplement to our patent portfolio in negotiating a comprehensive royalty agreement with a licensee. Some of his patents were litigated but none were adjudicated.

## 2.2 The Hazeltine Laboratory Locations.

Before World War II, the laboratories of Hazeltine Corp. operated at these locations:

Hoboken, N.J. (1924 MAR — 1929 JAN).
New York City (1929 FEB — 1942 OCT).
Bayside, L.I., N.Y. (1930 OCT — 1939 APR).
Chicago, Ill. (1937 to date).
Little Neck, L.I., N.Y. (1939 APR — 1970).

**The Hoboken laboratory** was located in the Electrical Engineering building of Stevens Institute of Technology, where Prof. Hazeltine was head of the E.E. Department. The address was 521 River St. This was the old Navy Building overlooking the waterfront on the West bank of the Hudson River. It was one of the southernmost buildings of the campus. It had been built as a temporary building for Navy barracks during World War I. After the war, the E.E. Dept. occupied the South wing and the Professor's office was in the SW corner of the second floor. Other offices, classrooms, laboratories and the model shop occupied the rest of the first and second floors.

When Hazeltine Corp. was formed and MacDonald was employed as Chief Engineer, he and Taylor rented some space in the third-floor attic. The NE corner of the S wing was partitioned to form 3 small rooms as the Company's first laboratory. The corner room served as office for Mac and a part-time secretary. I shared that office during the few months between my assumption of fulltime work and our move to the N.Y. lab. The few engineers and supporting staff occupied the other two rooms, equipped as laboratories. The second room was later partly filled with a workbench in a screened enclosure for shielding from radio interference. The third room was later equipped with a South Bend lathe, a signal event in Mac's efforts to provide the lab with equipment for professional grade of work.

I remember two secretaries. The first was Mrs. Teeney. The second was Jean Giusti whose elder sister was married to Carl DiMartino in our attorney's office (PDME). Our first machinist was Arthur Brillat (from 251201) whose brother later worked in the Bayside lab.

The Hoboken lab served our purpose during the years of the Neutrodyne. It was a small overhead expense relative to the current royalties. The engineers of our licensee manufacturers beat a path to our door. Mac established the lab as a valuable service to our licensees. The professor was mainly concerned with teaching but was nearby and available when needed.

The Hoboken location was fairly convenient to New York City. It was a half-mile walk from the Lackawanna Terminal, which had an excellent restaurant on the second floor. The Terminal was connected by the Hudson Tubes (now PATH, for Port Authority Trans-Hudson) with the midtown Penn. Station and the downtown Hudson Terminal. Lackawanna ferry service connected the Terminal with three points across the river in New York.

With the increasing activity, it became apparent that we needed more quarters in a more accessible location.

**The New York laboratory** was our second location. All of the lab activities were moved to New York City, after forming a New York State corporation as a subsidiary to handle the engineering functions. It was designated Hazeltine Service Corp. and served this function until World War II.

In a loft building at 333 West Fifty-second Street in Manhattan, we occupied the top (fifteenth) floor and the penthouse above. This was between Eighth and Ninth Avenues, near local subway stops on the Seventh Ave. BMT, the Broadway IRT (and later the Eighth Ave. IND). In the easy traffic of that day, one could park all day in front of the building on the one-way street. We were just two blocks north of Madison Square Garden, then in its second location. My favorite lunch room was Child's in the Garden. In the days before self-service elevators, our elevator was manned around the clock. Our neighborhood was known as Hell's Kitchen, but we had no fear of going there at any hour of day or night. (I sometimes went back alone around midnight for tests related to patent trials.)

MacDonald made a very orderly plan for utilization of the space. On the fifteenth floor, a corridor joined the front entrance (at the passenger elevators) with the freight elevator in the rear. On the left side was a foyer with two secretaries, then an office for a few engineers, then Mac's office and conference room. On the right side was lab space, including a spacious screen room for shielding receiver tests from outside interference. The penthouse above, with access by stairs and freight elevator, housed the model shop. It was equipped with the basic machine tools, operated by one or two machinists. On bright days, we relied on outside lighting. On hot days, we relied on open windows with a breeze (and dust).

As senior secretary, Miss Luella A. Horton was in charge of the office (from 290901). Mac had known her at RCA, and she was a great help. She was assisted by a bright young girl, Helen Harms (291209-310711). She was my first secretary, very conscientious and skillful.

At some time after we occupied the N.Y. lab, the floor below (fourteenth) was added to our space for office functions. Jack Binns had a corner office just below Mac, and they had a private "intercom" of keys and buzzers in the tradition of radio operators. When the Company formed a Patent Department, it was first located on this floor. Later, these office functions and the Corporation headquarters occupied a separate location in the General Motors Bldg., 1775 Broadway, not far away.

As the N.Y. lab soon became crowded with work for licensees, Mac saw that we needed more space, preferably separated to encourage forward-looking engineering work. In the second year of the N.Y. lab, he started a new research lab in Bayside and placed me in charge.

**The Bayside laboratory** was a supplement to the N.Y. lab and represented a major expansion of engineering activities (about double). It was a bold venture at a time when the Company was in a transition period and the Great Depression was steadily developing.

MacDonald and Taylor found an old mansion (the Cornell place) at 209-71 26 Ave. in Bayside out on Long Island. It was close to Taylor's estate overlooking Little Neck Bay, and not far from Mac's new house in Little Neck. In anticipation, I was building a house in Great Neck, a little further out. The three-story (12-room) frame house had outlived its usefulness as a residence, but offered much freedom for conversion to our use. The front porch had some years left before surrendering to the termites. The owner was not in a hurry to take his profit on the land in a developing community. It was adjacent to a nice neighborhood around Bayside High School, and across the street from the Bayside Links, a public golf course. It was near Bell Blvd., about a mile north of the Bayside station on the L.I.R.R., which offered frequent service about 20 minutes from Penn. Station. It was surrounded with trees on a large plot.

After its preparation for our use, we started work in the "new" Bayside lab in 1930 OCT. The preparation had been efficiently managed without diverting much effort on my part. The Master's bedroom became my office. Like each of the principal rooms, it had a fireplace. The hall was my secretary's office with telephone switchboard. She was Mrs. Alice H. Lynch, whom I had just selected for the position. She did all the office work for the entire group over the next few years. Across the hall was Harold M. Lewis, a friend of Mac with much competence and valuable experience in our field. Other engineers' desks, workbenches and test equipment were distributed among the other rooms. The kitchen and cellar became the model shop. Few of us had cars, so there was no problem of parking space. In the rear was the old stable and the caretaker's cottage.

After 8 years of service, the Bayside lab was superseded by the Little Neck lab in 1939.

**The Chicago laboratory** was planned as a supplement to the N.Y. lab. It was intended for close support of our licensee manufacturers in the Chicago area.

In 1937, Kelly Johnson was re-employed to start a licensee laboratory in Chicago. Bill Swinyard was transferred from the N.Y. lab to serve as his assistant. The new lab was located on one floor at 325 W. Huron St. It was equipped to test receivers in the same manner as the N.Y. lab. Its services have contributed much to the relations between the Company and the licensees in that area.

In 1946, the Company's patent activities were transferred to a newly formed subsidiary, Hazeltine Research, Inc. (HRI) with headquarters at the Chicago lab.

The Chicago lab has continued to serve various functions with a succession in personnel. In 1957, it moved to 5445 W. Diversey Ave.

**The Little Neck laboratory** was the successor to the Bayside lab, and later also to the N.Y. lab. Building 1 was planned by MacDonald and occupied in 1939 APR. It was located at 58-25 Little Neck Parkway, just north of the Grand Central Parkway (Northern State Parkway) in Little Neck on Long Island. This is just inside the city line of Queens in N.Y. City. Around World War II and afterward, several other buildings were added near Building 1. This location became the headquarters of the Company and continued to be until it was vacated in 1970.

Building 1 had the shape of a four-family dwelling. This was Mac's plan, so it could be converted to that use if later desired. Instead, the war-time need for our services soon dictated expansion.

The internal arrangement of Building 1 provided spacious laboratory rooms at the ends and smaller rooms on the connecting corridors. The latter provided private offices for engineers, some with workbenches. There were two floors and a basement at the level of the rear parking space.

Dan Harnett was in charge as Chief Engineer of Hazeltine Service Corp. I worked closely with him as V. P. and Chief Consulting Engineer. Our offices were on the first floor in the south wing. His was near the central general office. Mine was at the SE corner with my secretary in the adjacent room. At the time we moved, my secretary was Jean Kalenius (later Mrs. Brouthers). The general office was in the charge of Mrs. Madeleine McGuinness, who stayed with the company for many years.

This building was outstanding as a pleasant place to work. The separate rooms offered privacy with windows for light and air. One room was our first technical library. The rule was informality until the war changed everything.

## 2.3 The Hoboken Laboratory 1924-29.

In its five-year history, the Hoboken lab grew to a small group of qualified engineers with a good start in the testing and improvement of Neutrodyne receivers.

MacDonald employed one assistant, Rudolph W. Ackerman, whom he had known before. Ackerman stayed 3 years and was very helpful in this beginning. They assembled simple equipment for single-stage tests of the neutralized TRF amplifier. Mac got acquainted with the licensees (IRM) and offered them some assistance.

In the second year (1925), John Dreyer worked in this lab. He brought his prior knowledge of the Neutrodyne from working with the Professor summers. He worked with Stromberg-Carlson Co. to design the first shielded Neutrodyne receiver, with 3 stages of TRF instead of two. It had only 2 tuning dials instead of 3, one for the antenna tuner and one for the 3 stages. It was the greater performance of this set that impressed on me the need for AVC. This set became the pattern for the "second-generation" Neutrodyne.

From the Signal Corps and RCA, Mac was acquainted with Fred E. Johnston, a contemporary engineer whom he brought to our lab for 1926-27. Fred was a great help in that period of increasing competition among radio broadcast receivers. After the end of IRM, he left to become Chief Engineer of one of our new licensees, Crosley Radio Corp. in Cincinnati.

Another friend of Mac in the Signal Corps was Jackson H. Pressley, who worked in our lab for the year 1928. He also was a very competent radio engineer. Much of his effort was directed to uniform gain, the subject of a separate chapter. He left to become Chief Engineer of another one of our new licensees, U.S. Radio & Television Corp. in Marion, Ind. He was a Fellow of IRE from 1931.

I remember Fred Johnston and Jack Pressley for their very friendly and helpful attitude toward me when I was getting experience. Their experience and my education found a common denominator in our mutual interests.

Another outstanding employee was Lincoln Walsh, Stevens M.E. 1926, who was sent to us by the Professor. We spent much time together in the summers of 1926-27.

From the Signal Corps, Mac employed his older friend Lucien J. Troxler. Beginning in 1928, Trox was a valued member of our engineering staff for many years, continuing in the N.Y. lab, the Bayside lab and the L.N. lab. He introduced me to my first assignment on fulltime employment, the standard-signal generator covered in a separate chapter. Later he was most helpful to me on that and other projects.

In the later years of the Hoboken lab and the earlier years of the N.Y. lab, John F. Crowley and Nicholas V. Fedotoff were competent members of our staff.

The fourth year in the Hoboken lab saw the demise of IRM, the group who had been the sole manufacturers of the Neutrodyne. An RCA license had become necessary and available. Our royalty base was soon increased by other manufacturers, such as:

All-American Mohawk Corp.
Crosley Radio Corp.
Charles Freshman Co., Inc.
A.H. Grebe & Co., Inc.
Philadelphia Storage Battery Co. (Philco)
United States Radio & Television Corp.

There was more demand for our engineering services.

The fifth year saw the end of the Neutrodyne monopoly on TRF receivers, with the advent of the screen-grid tube. Our patent situation became confused and our attraction of licensees became even more dependent on our engineering services. We had established a reputation for quality and we needed the facilities for more quantity.

During 1925-28, our typical fulltime staff was 4 engineers, including MacDonald. The typical yeild was 7 reports per month, of which about 5 went to individual licensee companies.

In the beginning of 1929, the Hoboken lab had a staff of 6 men and one secretary, just before the additions for the move to the N.Y. lab. The Hoboken lab had served a useful purpose in establishing engineering services and receiver improvements as a function of Hazeltine Corp.

[A] W. A. MacDonald, "The importance of laboratory measurements in the design of radio receivers", Proc. IRE, vol. 15, pp. 99-101; Feb. 1927. (Discussions, pp. 329-340; Apr. 1927.)

## 2.4 The New York Laboratory 1929-42.

On moving from Hoboken to New York, MacDonald doubled our staff to about 10 engineers plus a few supporting personnel. That was shortly before the stock-market crash and the Great Depression.

The most important action was the employment of Daniel E. Harnett to head the engineering staff and to take charge of the laboratory. We could not have anticipated how well he was suited to that post and how much he would contribute to our organization in the next two decades. Dan was named Chief Engineer 10 years later, in 1939.

In 1930, for the beginning of the Bayside lab, Mac doubled our staff again, to about 20 engineers plus support. More than half were located with me at Bayside. The two photographs show the division between New York and Bayside early in 1931, which changed little in the next few years.

The first year in New York saw a major improvement in our testing equipment, described in separate chapters. This was my primary responsibility, and we advanced beyond any other laboratory we knew. We were not satisfied with the equipment we could have bought from General Radio Co. or other companies. It was a major investment which proved to be wise for our purposes. This status supported the quality and quantity of our services to licensee companies.

The year 1929 was the second year of Philco in the manufacture of receivers. Again they sought our advice. One result was the Philco 95 model which I designed to use my diode AVC. That was the introduction of this feature, which shortly came into universal use.

From 1930, the New York lab concentrated on service to licensee manufacturers as distinguished from advanced developments in the Bayside lab. There was not a sharp line of demarcation, and both labs contributed much to both objectives. During 1931-38, the typical yield from N.Y. lab was 12 reports per month, mostly to individual licensee companies.

To work with Harnett, J. Kelly Johnson was employed 1930-34. He was outstanding in the functions of the New York lab. When he left, he was succeeded by Nelson Case from the Bayside lab. Kelly Johnson later organized the Chicago lab in 1937.

Late in 1936, Nelson Case became head of the New York lab when Dan Harnett moved to the Bayside lab. Nelson held this post until the end of the New York lab, when it merged into the Little Neck lab in 1942. Nelson also was well suited to this position, and benefited from previous experience with me in the Bayside lab.

The New York lab was mainly concerned with the progression of types of broadcast receivers. The superheterodyne became universal. One form was the multiband receiver for "all-wave" reception. Another was the small and inexpensive table model to meet the needs of the depression. Then came the beginning of television.

After the AVC trial in Wilmington, some major companies became licensees. First RCA settled that case and became a licensee 371215. They were followed in 1938 by:

General Electric Co.
Stewart-Warner Corp.
Sparks-Withington Co.

They all called on our engineering services.

When the Bayside lab was superseded by the Little Neck lab in 1939, there was little change in the New York lab.

The notebook records were kept in a numbered series that Mac had initiated in Hoboken. One was assigned to the current work for each licensee company. Others were assigned to projects.

The notebook was a bound volume with hard black covers and red leather binding, containing 150 pages with blue quadrille ruling. For my studies not necessarily keyed to companies, I had an individual series that ran from 1 to 13 before the Bayside lab.

At the time of starting the Bayside lab, Mac adopted a carbon-copy notebook with 75 pages in duplicate. It had a printed form on each page for signatures of the writer, two witnesses and a notary. At frequent intervals, the carbon copies were removed and forwarded to a file for patent records. It was a terrible mistake, presumptively on advice of our patent attorneys. It greatly inhibited the entry of rough notes in the bound volume. Most of my notes were kept in ring binders and have been lost. This system was used until 1942, when I designed a practial form for economy and for encouragement of use in daily work. That successor form is still in use in the Company.

A numbered series of reports had been initiated by Mac in Hoboken and was continued in the N.Y. lab until it was terminated. The text pages were typed with several carbon copies for different files. It was a laborious process which encouraged the secretaries to minimize errors and erasures. Some math forms had to be entered by hand on every copy. The drawings were reproduced by a cumbersome blueprint machine. The photo-offset process was the latest development, available only at a few professional agencies. We used it for special purposes, such as the few reports for general distribution to all licensees.

In addition to MacDonald and myself, the following list includes most of the engineers who were active in the N.Y. lab. They are listed in their order of appearance on the N.Y. staff.

John F. Crowley (from Hoboken)
Lucien J. Troxler (from Hoboken) (to Bayside later)
Daniel E. Harnett (to Bayside later)

Richard L. Waring
Charles E. Dean (to Bayside soon)
Vernon E. Whitman (to Bayside soon)
Robert T. Hintz
Walter Lyons
J. Kelly Johnson (Chicago later)
Nelson P. Case (from Bayside)
William B. Wilkens
Frank A. Hinners
Clifford O. Siegelin
Paul H. Taylor
Clyde K. Huxtable (from Bayside)
Joseph L. Hurff (to Bayside soon)
William O. Swinyard (from Bayside) (to Chicago later)
Arthur V. Loughren
William F. Bailey
Robert J. Brunn (to Chicago later)
Alden Packard
Lloyd M. Hershey
Rudolf E. Sturm (from Bayside)

Our Patent Department was started in 1935 at the location of the New York lab. It was headed and organized by Laurence B. Dodds, and became a very effective part of the Company.

[A] D. E. Harnett, N. P. Case, "The design and testing of multirange receivers", Proc. IRE, vol. 23, pp. 578-593; Jun. 1935.

[B] Hazeltine Service Corp., "These up-to-date laboratories serve Hazeltine licensees", Radio Broadcast, p. 31; Nov. 1929. (5 photos in N.Y. lab, list of licensees.)

## 2.5 The Bayside Laboratory 1930-39.

The beginnings of the Bayside lab are best seen in the photos of the old mansion and the staff early in 1931. I headed a group of 11 engineers plus 4 machinists and one secretary. When the Bayside lab was superseded by the Little Neck lab, the staff was about the same in number but almost entirely succeeded by other competent individuals.

Under the supervision of Frank Corbett, the old house had been prepared for our use, with the installation of electric power and outlets and the furnishing of office and laboratory equipment. Then we duplicated the principal models of test equipment that were in use in the N.Y. lab. To these we added much more test equipment developed in Bayside. We were soon ready for development work on the superheterodyne receivers which were just becoming universal under RCA license.

My office was in the master's bedroom and Lewis was across the hall. My secretary was Mrs. Alice H. Lynch, who sat at a desk and switchboard in the hall. She was my age and was extremely competent. She took care of the entire staff until 1935, when Marcelle Berg was employed as her assistant. Miss Berg was a bright young girl who became my secretary for a while after Harnett came to Bayside and I moved upstairs. She was succeeded by Jean Kalenius (later Mrs. Brouthers) who served me very well until she left some time after we moved to Little Neck.

In the days of the Bayside lab, a report was typed with 10 carbon copies. This was a real challenge to a secretary. Unusual math forms had to be filled in by hand. Prints of illustrations were made by a crude blueprint process.

In the Bayside lab, I started a parallel series of report numbers with W suffix. This series continued in the Little Neck lab. In 1931-38, the typical yield was 9 reports per month.

Very few of us had cars, so we provided a station wagon as a shuttle between the lab and the Bayside station of LIRR. Most of us carried lunch boxes and, in good weather, had lunch on the lawn, followed by horseshoe pitching.

The engineering developments are mostly covered in separate chapters. At first they related to receivers for broadcast reception in the standard band (0.55-1.5 Mc) and in other bands. Variations and refinements were the next in line. Then Lewis started our television project, which came to utilize most of our efforts.

For testing a loudspeaker, we built (on the porch of the house) a "dead room". It was a small room with sound absorbing material on the walls. We used it for development of some loudspeaker improvements. Then we developed the acoustic recorder (described in a separate chapter) for testing the performance of a loudspeaker in the living room of the house. This work was conducted by Whitman, Swinyard and Sturm.

For testing an "all-wave" antenna, we erected (on the side yard) a square of 40-foot poles with a fifth pole in the center to support test equipment and an operator. An antenna was suspended between opposite corners and tested at its terminals in the middle. The observations were phoned to a nearby room in the house for computations and plotting.

In our TV project, the most notable event was the employment of Hergenrother and his construction of an excellent camera tube (iconoscope). That was needed for our testing on moving subjects rather than just a still subject (monoscope). With the help of Frank Corbett, Hergie equipped the necessary tube laboratory, suitable for such a large tube. It was used for various other experiments, such as electron-multiplier tubes.

In 1936, my intensive activities in the AVC litigation made it advisable for Dan Harnett to take charge of the Bayside lab. This occurred late in that year, and gave me more opportunity for studies relating to FM and TV. One of my principal activities was participation in the classes relating to TV.

We had many visitors, both from our licensee companies and from other groups. Our high-fidelity broadcast receivers, and later our TV demonstrations, made the Bayside lab a showplace of the Company's engineering achievements.

In addition to Harnett and myself, the following list includes most of the engineers who were active in the Bayside lab. They are listed in their order of appearance on the Bayside staff.

| | |
|---|---|
| Harold M. Lewis | Rudolf E. Sturm |
| Charles E. Dean, Ph.D. | Paul H. Taylor |
| Vernon E. Whitman, Ph.D. | Joseph L. Hurff |
| Madison Cawein | Emil T. Joss |
| William O. Swinyard | Walter Lyons |
| James E. Brock, Ph.D. | Rudolph C. Hergenrother, Ph.D. |
| Nelson P. Case | Philip J. Herbst |
| William R. Hynes | Arthur V. Loughren |
| Clyde K. Huxtable | Benjamin F. Tyson |
| John E. Curran | Walter D. Frere |
| Lucien J. Troxler | Robert L. Freeman, Ph.D. |
| Frank P. Corbett | John C. Wilson |
| John F. Farrington | Leslie F. Curtis |

Many continued in the successor Little Neck lab.

The Bayside lab was a happy family in the suburbs. Most of us resided within a small radius. We had much in common with much diversification of background. We enjoyed many social events at our homes. We carried fond memories to our superior new quarters in Little Neck.

## 2.6 The Little Neck Laboratory 1939.

The Bayside lab moved to Little Neck just when our television work was building to a climax. It was just before the 1939 World's Fair in nearby Flushing Meadow. That was probably the outstanding Fair in history, and was repeated the next year. It was the debut of TV and air conditioning. It brought many visitors to our neighborhood, to our home and to our laboratory. It was the end of the Great Depression.

Dan Harnett continued in charge as the group moved from Bayside to Little Neck. He was performing that function extremely well, and was named Chief Engineer the same year (1939). I was named Chief Consulting Engineer and VP of the operating company, Hazeltine Service Corp. We cooperated closely. Our L.N. lab had plenty of space. which served to establish a nucleus for our war-time expansion soon to come. The photo shows the new building.

We were demonstrating quite a few models of TV receivers, some that we had made and some made by our licensees with the benefit of our advice. We could receive what signals were sent from RCA on the Empire State Building. Also we could show the signals from our experimental pickup.

The office facilities were gradually increased to handle an avalanche of reports. We graduated to duplication by carbon-back typing followed by the "Ozalid" dry duplicating process. For internal use, we had also the "Ditto" gelatin process with colors. The photo-offset process had become common for larger quantities, such as reports for general distribution.

One series of our reports was prepared in 1939-40 by Dean for our licensees, to introduce the subject of TV and to communicate our progress. It was entitled "Television Principles" and was printed in a bound volume 1944. The series was ended after 13 chapters by the priority of war work.

We organized the Consulting Group of professors for monthly discussions of current problems.

Our first report relating to war work was my (CONFIDENTIAL) report 1107W, 400608, "Outline for airplane identification set". It was a hint of the IFF program which was destined to be our major contribution in World War II. By the end of that year, we were largely committed to war work, long before Pearl Harbor. Our first development to go into service was the SCR-625 Mine Detector, designed by Curtis under my direction. Early in 1942, a few handmade samples were flown to North Africa, without waiting for deliveries from the first production order (awarded to another company).

In addition to Harnett and myself, the following list includes most of the engineers who were active in the L.N. lab during the two years before preemption by war work. They are mostly alumni of the Bayside lab, and those are listed in order of their appearance in Bayside.

| | |
|---|---|
| Harold M. Lewis | John A. Rado |
| Lucien J. Troxler | Rudolf E. Sturm |
| Emil J. Joss | Robert J. Brunn |
| John E. Curran | Richard W. Jansen |
| Rudolph C. Hergenrother | Kazmier Wysocki |
| Benjamin F. Tyson | John F. Farrington |
| Robert L. Freeman | Leonard R. Malling |
| Leslie F. Curtis | John A. Hansen |
| John C. Wilson | Arthur V. Loughren |
| Harold L. Blaisdell | Jasper J. Okrent |
| William F. Bailey | Bernard D. Loughlin |

This staff in Little Neck and our remaining staff in New York, together with their adequate facilities, prepared us for major participation in war work. The transition was completed before Pearl Harbor.

Just after the war, several of these engineers were invited to write chapters in this handbook:

H. Pender, K. McIlwain (eds.), "Electrical Engineers' Handbook", 4 ed. Wiley; 1950.

This revision (from 3 ed. in 1936) was organized by Knox McIlwain, who joined our staff during the war and was afterward my successor as Chief Consulting Engineer of HEC. These chapters were contributed by our staff:

| | | |
|---|---|---|
| 5-26 | HAW | Transients in networks. |
| 7-56 | C. J. Hirsch | IF amplifiers. |
| 8-16 | LFC | FM receivers. |
| 8-26 | BDL | Distortion and interference in FM systems. |
| 9-02 | HAW | Pulses and pulse systems. |
| 9-13 | JJO | Pulse circuits. |
| 9-28 | HAW | Delay lines. |
| 11-43 | WOS | Measurement of AM and FM broadcast receivers. |
| 20-02 | AVL | Television principles and theory. |
| 20-47 | WFB & RJB | Television receivers. |
| 20-64 | AVL | Other forms of television. |

[A] (Photo) Little Neck laboratory, Proc. IRE, vol. 27, p. 153; Feb. 1939. (Harnett, MacDonald, Wheeler in front of lab under construction.)

[B] C. E. Dean, "Index of general Hazeltine Reports by Subjects", Rep. No. 1710-WP; Oct. 27, 1944. (About 200 from 1936 under 19 subjects.)

Photo 1 — House in Mitchell 1914.
Address: 518 North Duff Street.
My Home 1907-16.

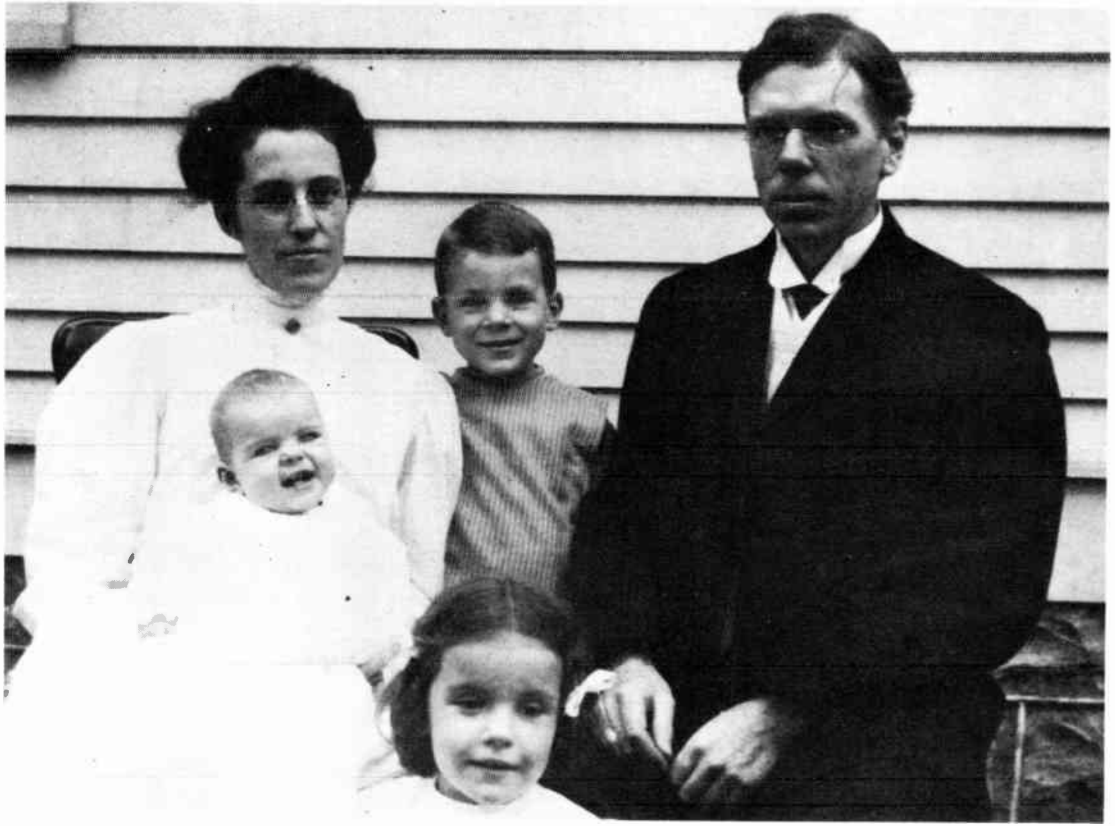


Photo 2 — My family in Mitchell 1908.
Father, Mother, Harold, Helen, Margaret.

Photo 3 — House in Washington 1916.
Address: 5503 Thirty-Third Street N.W.
My Home 1916-26.

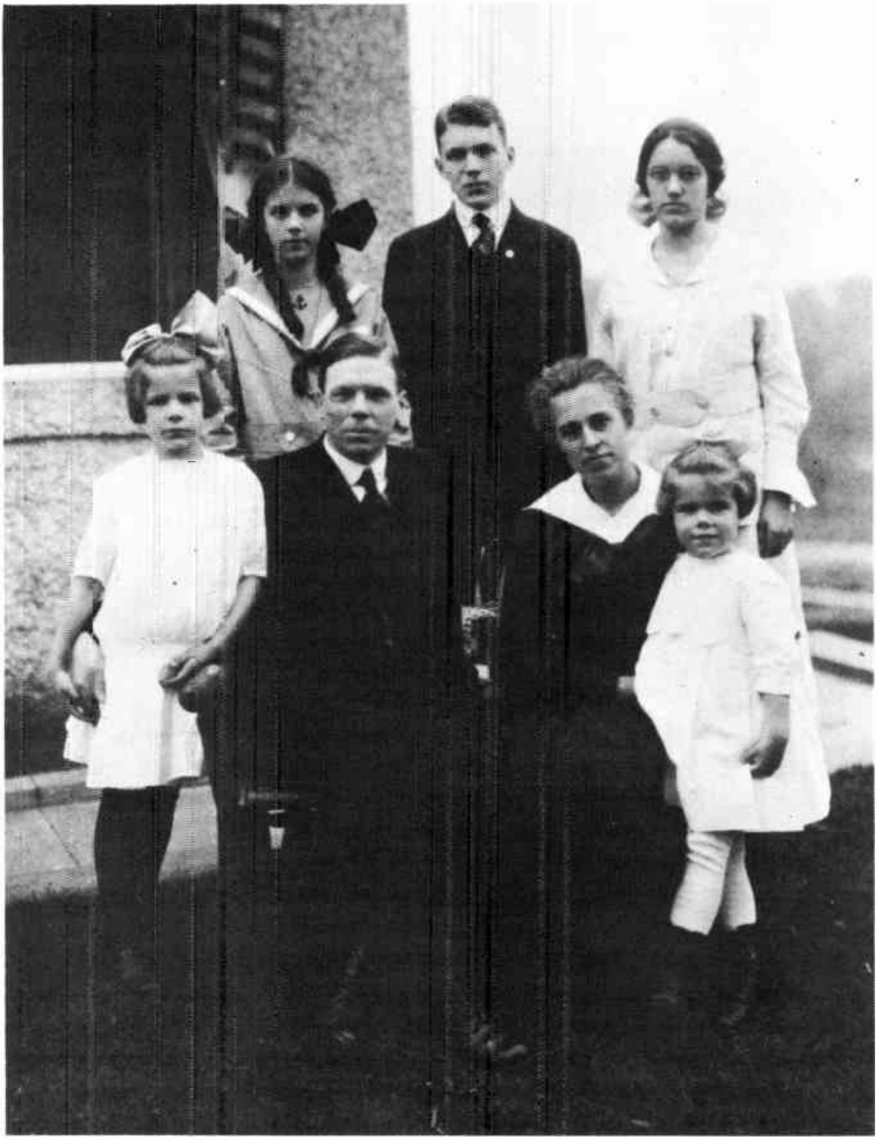


Photo 4 — My family in Washington 1921.
Father, Mother, Catherine, Margaret, Harold, Helen, Harriet.

Photo 5 — House in Great Neck 1936.
Address: 18 Melbourne Road.
Our Home 1930-70.

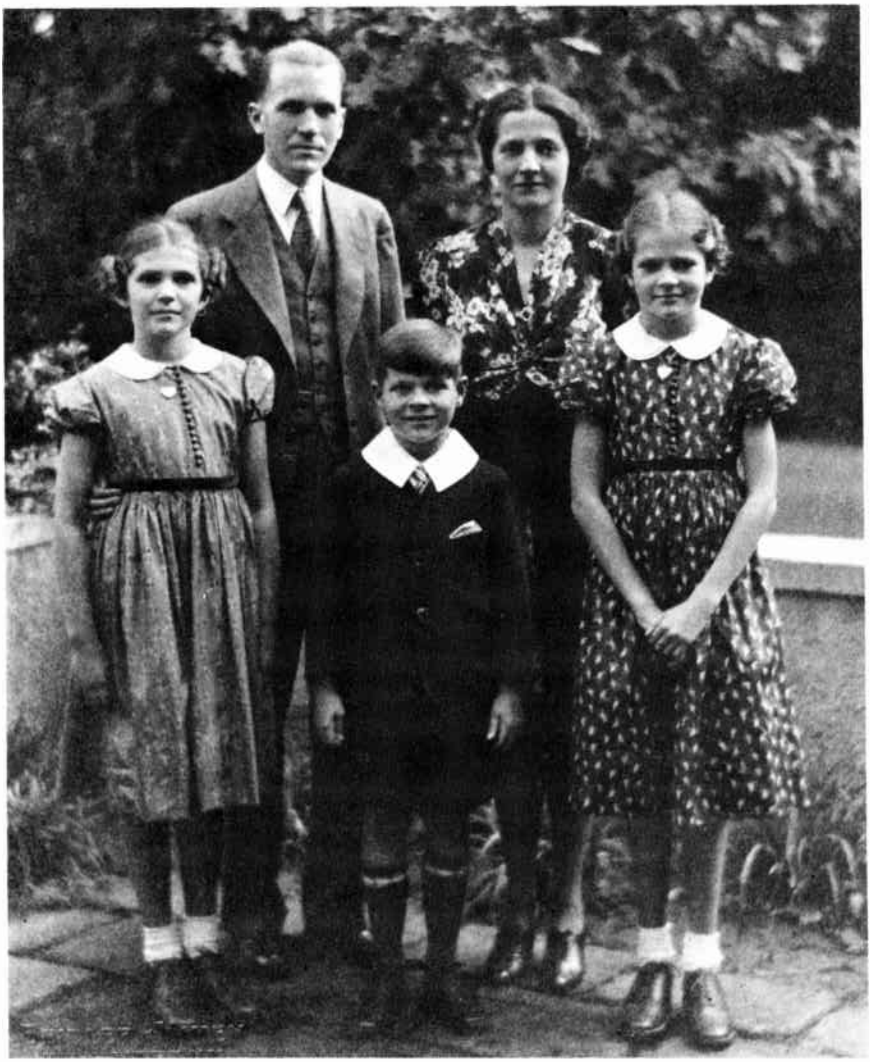


Photo 6 — My family in Great Neck 1936.
Harold, Ruth, Dorothy, Alden, Caroline.

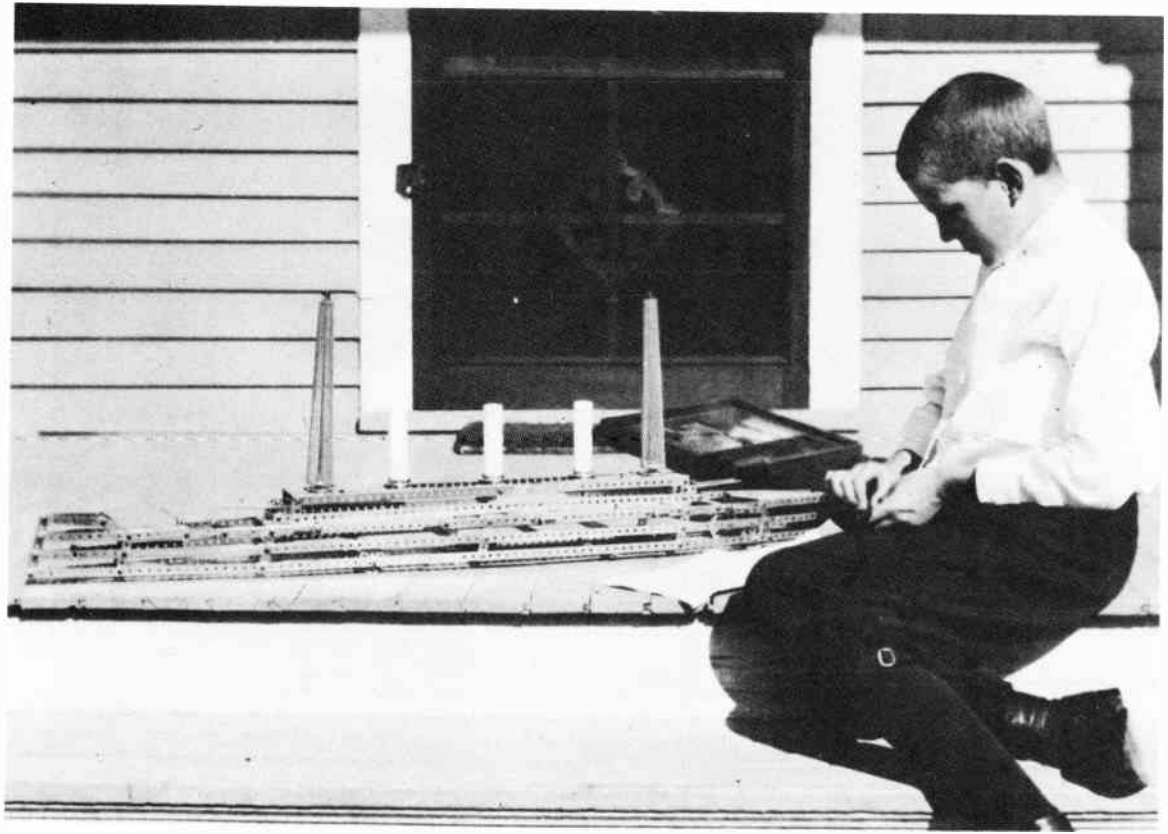


Photo 7 — Battleship model 1915.

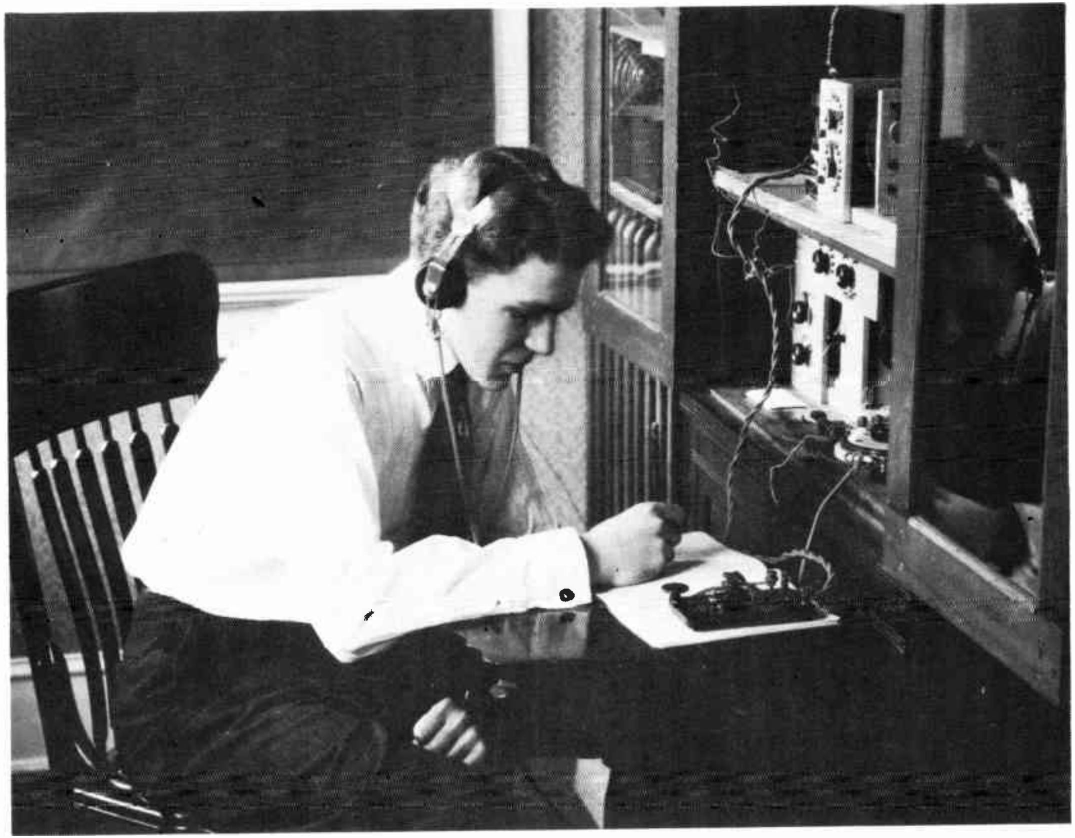


Photo 8 — Radio station (3QK) 201215.
Receiving first broadcast of Radio Market News Service.

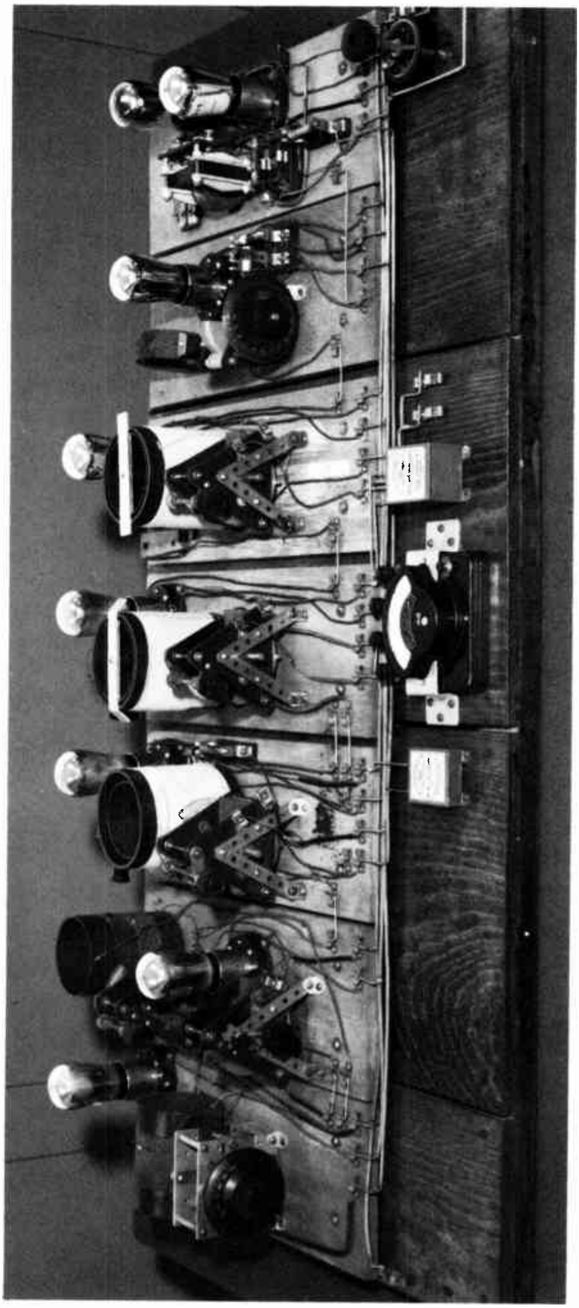


Photo 9 — Washington receiver with diode AVC 1926.

Photo 10 — Bayside laboratory 1931.
Address: 209-71 Twenty-Sixth Avenue.

Photo 11 — New York laboratory staff 1931.
Front row: Horton, Johnson, MacDonald, Harnett, Troxler.
Back row: Harms, Hintz, Waring, A. Brillat, Crowley, Uban, Corbett.

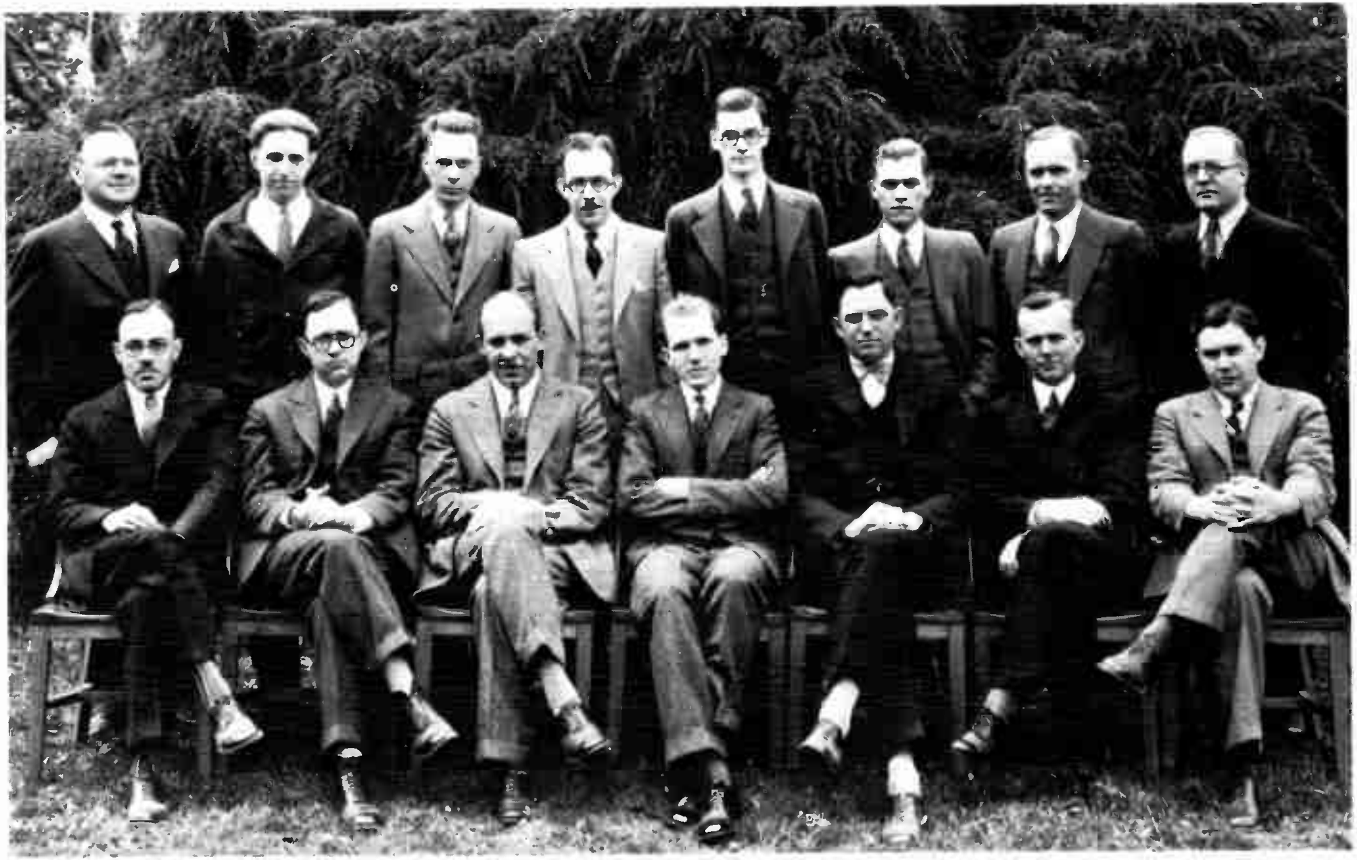


Photo 12 — Bayside laboratory staff 1931.
Front row: Case, Dean, Lewis, Wheeler, Brock, Whitman, Cawein.
Back row: E. Brillat, Quebe, Swinyard, Hynes, Huxtable, J. Curran, Frossard, Muller.

Photo 13 — Little Neck laboratory 1939.
Address: 58-25 Little Neck Parkway.

Photo 14 — William Alexander MacDonald 1895-1961.

Photo 15 — Harold Alden Wheeler.

# Section 3. Names

## 3.0 Names.

Here are brief introductions to two dozen of the people more prominent in Hazeltine Corp. before World War II. The order of listing is guided by their order of appearance. One purpose of each sketch is to give some perspective outside the main line of activities reported in this account. For each individual who contributed more than 5 patents from the period before the war, the number is noted beside his name (in parentheses).

| | | |
|---|---|---|
| Prof. Hazeltine (17) | Harnett (12) | Farrington (14) |
| Taylor | Dean | Dodds |
| Wheeler (122) | Whitman | Hergenrother (11) |
| Dreyer | Johnson (14) | Loughren (10) |
| MacDonald (27) | Cawein (17) | Freeman (11) |
| Binns | Swinyard | Curtis (8) |
| Rickard | Lewis (39) | Wilson (34) |
| Troxler | Case (33) | Loughlin |

### 3.1 Professor Hazeltine.

Louis Alan Hazeltine was born in Morristown, N.J., 1886 AUG 07. He died at age 78 in 1964, shortly after I received the Medal of Honor from IEEE. In my book, "Hazeltine the Professor" (1978) I have recorded much of his life and achievements. Here I shall duplicate that account only to the extent that it may contribute to the emphasis or continuity of the present account. His career, like mine, became inseparable from the history of Hazeltine Corporation.

Hazeltine graduated from Stevens Institute of Technology at age 20 in 1906. He taught in the Electrical Engineering Dept. in Stevens from 1907 to 1925, advancing to Professor and Department Head. Then he "retired" to a different pace in other activities. He married a second time in 1930 and shortened his name to Alan Hazeltine. He returned to Stevens for a second teaching career, 1933-44, concentrating on graduate courses in mathematics and physics. From the beginning of Hazeltine Corp. in 1924, he served as consultant and some years as a Director. During the period of this account, his activities on behalf of the Company were close to mine and are mentioned in context.

Here we may be reminded of some highlights of Prof. Hazeltine's activities in relation to the main lines of this account.

- 1918 The invention of the neutralization destined to be used in a TRF amplifier in a broadcast receiver.
- 1922 The design of the Neutrodyne receiver for broadcast reception, which was the outstanding type in 1923-28.
- 1924 His part in the organization of Hazeltine Corp. and in the Neutrodyne patent litigation until its conclusion in 1934.
- 1926 His interest in the "uniform-gain" inventions of his former student, Carl E. Trube, which were acquired by the Company.
- 1936 His part as our expert witness in the litigation on my diode AVC patent.
- 1936 President of IRE.

On his return to Stevens in 1933, the Professor received the honorary degree of Doctor of Science. I received a similar recognition from Stevens, the honorary degree of Doctor of Engineering, in 1978, just 55 years after my first summer working with him in Hoboken. In 1937, Hazeltine was the first to be awarded the Armstrong Medal by the Radio Club of America. I was accorded this honor in 1964. We both received from the National Association of Manufacturers in 1940, the Modern Pioneer Award for inventions.

Since writing my book about the Professor, I have thought of one trait to which I did not give the attention it deserved. I appreciate it because it is one I lack. In working on a new problem, it is my custom to probe each different approach, or barge ahead in a mathematical derivation, to see first if it offers some promise. If so, I return to test its validity and to fill in the details. Therefore my notebooks contain many a false lead followed by reconsideration. On the opposite extreme, the Professor performed this process in his mind, and then reported the finished product. A page in his notebook is filled with very small, orderly script, math and diagrams, meticulously identified and dated. When teaching a new course in his "second career", he did the same thing on the blackboard before class, then it was photographed afterward for reproduction as lecture notes. This intensive presentation set a pace that could be followed by the brightest students and presented them with a welcome challenge.

The Professor adopted an unusual practice in numbering the pages in his bound notebook. With the book open, he wrote first on the right-hand page, then on the left-hand page. This enabled him to see the latter while writing on the next right-hand page. It was a great help in continuing a mathematical development. (Also it prevented defacement of wet ink on turning a page.) I have used this practice in most of my career, because I am likewise attracted to a logical and unusual feature.

In my book, I describe the Professor's design of the famous SE-1420 Navy Receiver about 1918. Since writing the book, I have had some interesting experiences relating to that receiver. I have been amazed and impressed to be reminded of the prominence of that design in the memories and records of later years. Among the old timers who have read my book, every one seems to have been familiar with the old SE-1420. I have become aware of quite a few historical museums of early radio, and every one seems to have one of these sets. Few if any of the surviving old timers had been aware that Hazeltine designed that set, and they have been glad to learn. In looking over the early volumes of Radio News, I have seen photographs of typical receiving installations of professional grade, and the SE-1420 is usually in a prominent position. These are examples I noted.

Photographs in Radio News, Showing the SE-1420 Navy Receiver.

1924 APR, p. 1410, in the radio room at Lakehurst, N.J.
1924 MAY, p. 1569, in an experiment at NRL, D.C.
1924 SEP, p. 297, two in the radio room on S.S. Leviathan.
1924 OCT, p. 476, in Radio Mail Headquarters, D.C.
1924 NOV, p. 669, in a Government experiment.

1925 DEC, p. 787, being used by Dr. A. H. Taylor at NRL, D.C.
1926 JUL, p. 15, in a facsimile experiment.
1928 APR, p. 1109, in the radio room on S.S. Southern Cross.

It is always a privilege and a pleasure to recall the Professor's profound contribution to my early development and the launching of my career with a headstart. From the time of our accidental meeting in 1922, he indoctrinated me in his imaginative and creative approach to any problem. It usually led to a useful solution and real insight into the problem. It was an approach that naturally appealed to me, and I have enjoyed its practice. I am forever indebted to him for his advice and encouragement. I have attempted to show my gratitude by likewise contributing to the growth of younger engineers.

See [W1] [H/P]

[A] G. Clark, "The SE-1420/IP-501 receiver," Antique Wireless Assoc., The Old Timer's Bulletin, vol. 22, no. 3, pp. 14-18; Dec. 1981. (History and photos, written ca. 1940.)

### 3.2 Taylor.

Willis H. Taylor, Jr., was born in New York City 940817. He was a student of Prof. Hazeltine in the Class of 1916, graduating with the M.E. degree from Stevens Institute of Technology. During World War I, in U.S. Army Signal Corps, he rose to the rank of Captain. He worked with the famous Major Edwin H. Armstrong and became acquainted with Lieut. William A. MacDonald. After the war, he served as patent attorney to both Armstrong and Hazeltine. He was a member of the prominent firm of Pennie, Davis, Marvin & Edmonds (PDME) with offices at 165 Broadway in New York City. He brought other Stevens graduates into that firm.

It was Bill Taylor who set the stage for Hazeltine Corporation.

- In 1920, he filed the second Hazeltine patent application on neutralization in a triode amplifier, and took over the prosecution of both applications. The second was later divided.
- In 1922, he brought together the Professor and the Independent Radio Manufacturers, Inc. (IRM), which led to the design of the Neutrodyne receiver.
- He planned and organized the Hazeltine Corporation, starting in business 240201. He negotiated my employment from the beginning.
- He started the laboratory and employed MacDonald as Chief Engineer. MacDonald was destined to head the Company.
- He planned and conducted the litigation which protected the IRM royalties until its end in 1927, and subsequently collected some back royalties from another company.

Taylor and I became good friends in our common interests in the Company. I worked with him on various patent matters, including expert testimony in the Hazeltine litigation. My first visit on Long Island was as an overnight guest at his waterfront home in Bayside. It was near there that he found the old mansion we converted to the Bayside laboratory in 1930.

As a Stevens alumnus, Taylor became active in the affairs of the Institute. He served as Chairman of the Board of Trustees for many years, and he was awarded the honorary degree of Doctor of Engineering in 1956. This was the degree conferred earlier on the Professor and later on me. Taylor was a Senior Member of IRE and a member of Sigma Xi.

## 3.3 Wheeler.

I, Harold Alden Wheeler, was born 030510 in St. Paul, Minn. I received from George Washington University in 1925 the B.S. in Physics. I attended postgraduate courses 1925-28 in the Physics Dept. of Johns Hopkins University.

My activities before World War II are the subject of this book. After working with Prof. Hazeltine in 1923, I was employed by Hazeltine Corp. from its beginning 240201. That was part-time work until 1928, the end of my college studies.

I left the Company in 1946, just after World War II, and formed Wheeler Laboratories, Inc., in Great Neck on Long Island. I was President 1947-68. I negotiated the acquisition of my company by Hazeltine Corp. in 1959, so my accomplishments during that period innured to the benefit of Hazeltine Corp. and have been regarded as continuity of employment. The remaining staff of my company was merged into the Research Laboratories of the parent company in 1971. My office was moved to their new location in Greenlawn.

I was named a Director of Hazeltine Corp. in 1959 and have continued in this function. I was Chairman of the Board in 1965-77, and served as Chief Executive Officer in 1966 during a transition in management. I retired from line employment in 1968 at age 65 and since then have served as consultant and Chief Scientist.

I have been active in the IRE since I joined in 1926, and later in AIEE and their successor IEEE (from 1963). First I wrote and presented papers and served on technical committees concerned with standardization. I qualified as a Fellow in 1935. I was elected a Director for 1940-45, the war years. I initiated these practices, among others:

- Allocation of all members to Sections based on geographical boundaries.
- Including in the annual directory a section of Fellow Biographies.

After the war, I was one of the three founders of the Long Island Subsection (later the L.I. Section) and I was its second chairman. I was named a Fellow of AIEE (1946) and Radio Club of America.

I wrote a series of 19 technical monographs in 1948-54. The first eleven were collected in a book entitled "Wheeler Monographs, Vol. I", 1953. In 1978, I wrote my recollections of "Hazeltine the Professor" in a small book for complimentary distribution by Stevens Tech and the Company. That book was the starting point for writing the present account.

I have been generously accorded many awards over the years. These have been significant to me in the context of their timing:

1916 First prize (a $20 gold coin I still have) in the contest of "paper boys" on the Mitchell Daily Republican (S. Dak.).
1921 Scholarship to GWU (four years' tuition), one of 7 from Central High School (D.C.).
1925 Ruggles Prize in Mathematics (gold medal), GWU.
1927 Sigma Xi, JHU.
1937 Certificate of Distinction from CHS Alumni Assn.
1940 Modern Pioneer Award from NAM, for inventions.
1940 Morris Liebmann Memorial Prize from IRE, for papers relating to TV.
1947 Navy Certificate of Commendation, for work on IFF antennas during WW-II.
1956 Tau Beta Pi, Polytechnic Inst. of Brooklyn.
1960 Distinguished Engineering Alumnus Award, GWU Engineer Alumni Assn.
1960 Distinguished Achievement Award, one of 7 from the Seven Colleges on Long Island.
1964 Medal of Honor, IEEE (formed by merging IRE and AIEE).
1964 Armstrong Medal, Radio Club of America.
1969 Alumni Achievement Award, GWU Alumni Assn.
1972 Doctor of Science (hon.). GWU.
1978 Doctor of Engineering (hon.), Stevens Inst. of Technology.
1980 Fellow, Polytechnic Inst. of NY.

[A] Who's Who in America (since 1958).
[B] Who's Who in Engineering.
[C] World Who's Who in Science (1968, from antiquity).
[D] American Men of Science.
[E] Edna Yost, "Modern American Engineers", Lippincott; 1952, 1958. (One of 12 names.)
[F] Harold Alden Wheeler, "Microwave People", Microwave Jour., vol. 3, no. 9, pp. 10-17; Sep. 1960.
[G] Wheeler Laboratories, "Company Profile", Microwave Jour., vol. 4, no. 4, pp. 100-103; Apr. 1961.
[H] J. F. Mason, "Harold Wheeler: an innovator in the world of communications", Electronic Design, vol. 24, no. 8, pp. 80-83; Apr. 12, 1976. (AVC story, color photo of Washington receiver, notebook page 94.)
[I] "Leaders in Electronics", Electronics Magazine; 1979.

3.3 Wheeler.


[J] Harold Alden Wheeler, "Great Innovators", Electronics, vol. 53, no. 9, p. 424; Apr. 17, 1980. (One of 11 living names, in fiftieth anniversary issue.) Also "Classic Circuits", p. 438, automatic volume control, 1926, one of 12 featured.) (Also "The living record of a technology", p. 17.) (Hazeltine Corp. adv., photo of Neutrodyne receiver, p. 400.)

[K] [WM. . .] [PR-71] Wheeler Monographs; 1948-54.

[L] [WL. . .] [PR-71]

**3.4 Dreyer.**

John F. Dreyer, Jr., was born in Brooklyn 1901 FEB 28. He lived in a nice residential area with excellent public schools, in which he skipped two grades in view of his top standing. He was an outstanding student of Prof. Hazeltine in Stevens Institute of Technology, receiving the M.E. degree in 1921. He continued as an Instructor under the Professor, and we worked together in the Summer of 1923. We became very close friends.

John was employed in the Hoboken lab during its second year, 250202-260420. That was a year of great progress in the design of Neutrodyne receivers, largely as a result of his preparation under the professor and his competence in practical design. It was the beginning of my work on automatic volume control and Dreyer came to Washington to see my receiver.

He went to work in the Atwater Kent Manufacturing Co. in Philadelphia. There he was the leader of an outstanding engineering group, designing and manufacturing the largest number of broadcast receivers for some years. They made the first "piston attenuators", as reported in my chapter on that subject.

Later he worked for other companies, including RCA Radiotron in Harrison, N.J., and Electronic Communications, Inc., in St. Petersburg, Fla.

Dreyer was a Fellow of Radio Club of America and a Senior Member of IRE. He now resides on Cape Cod, Mass.

See 4.1[E] [I], 9.8[C]

### 3.5 MacDonald.

William Alexander MacDonald was born in New York City 1895 DEC 27. He died suddenly of a heart attack 1961 AUG 11. His parents were William Fuller MacDonald and Sue E. (Watson) MacDonald. I know little of his early years. He was active as a radio amateur for about 2 years, perhaps during high school. After finishing high school, he was employed as follows:

| | |
|---|---|
| 1913-15, | Marconi Wireless Telegraph Co., N.Y. City, radio operator on ships. |
| 1916-17, | Western Electric Co., laboratory assistant. |
| 1917-19, | U.S. Army Signal Corps., second lieutenant. |
| 1919, | University of Paris (Sourbonne), completed communications course in radio. |
| 1919-20, | Signal Corps, Camp Alfred Vail, N.J., assistant radio engineer. |
| 1920-24, | Radio Corp. of America, engineer. |
| 1924. . ., | Hazeltine Corporation, Hoboken, N.J., Chief Engineer. |
| 1934. . ., | Also Vice-president. |
| 1952. . ., | President. |
| 1957-61, | Chairman and Chief Executive Officer. |

In the Army in France, he was a Second Lieutenant in the distinguished team headed by Major Edwin H. Armstrong, who was becoming the most famous of radio inventors. Other members of that team were:

Civilian Harold M. Lewis, who later was one of the engineering leaders in Hazeltine Corporation.

Sergeant Harry W. Houck, who later founded Measurements Corp. in Boonton, N.J.

Civilian Jackson H. Pressley, who later worked for Hazeltine Corp. a year and became Chief Engineer of two large companies.

MacDonald was elected a Fellow of IRE (1953) and a Fellow of Radio Club of America.

In 1922, he married Dorothy Belder. She had a son by a previous marriage, Baldwin, who took the name of MacDonald. He is married and they have a daughter. He died recently. The MacDonalds adopted a daughter, Doris Diane, who became Mrs. McLaughlin, and they have three children. The most outstanding personal trait for which I remember MacDonald was his devotion to Dorothy and their children.

MacDonald was a tall and handsome man with a neatly trimmed mustache that was becoming to him. He walked with a slight stoop. He had an engaging smile and a twinkle in his eye. He was left-handed and wrote in a position that is common with that trait. His writing was good and his notebooks were neat.

He was strong and enjoyed physical activity. During a few years in the 1930's, he succumbed to the current fad and had a farm in nearby Connecticut. He reported that the tenant farmer, who was much older, could outwork him any day. In contrast, MacDonald cultivated an indoor hobby of needlepoint, and made some beautiful pieces. Later in life, he played golf, but I doubt if he enjoyed it.

The MacDonalds built a nice home in Little Neck on Long Island, where they lived for many years until after World War II. Then they moved to Bayville Rd., Locust Valley, where they lived until he died in 1961.

In preparing this account, I have come to appreciate more and more that it was MacDonald who guided the Company through the troubled waters. He pursued one pattern in the days before World War II, when the Company was supported by royalties from manufacturers of broadcast receivers. He deserves much credit for our survival through the Great Depression. Then he adapted to the war by a novel plan that made best use of our engineering expertise in combination with a number of our licensee manufacturers who had relied on us for this expertise. He strived for post-war activities that would support the Company without relying on royalties, after the Courts had gone far in outlawing the intent of the patent system. In the post-war period, the problems were too much for any one man, so the program of the Company gradually suffered from some lack of continuity in direction. He was not able to develop an adequate successor team for leadership and management. He was tired when he died suddenly in 1961 from a heart attack brought on by excessive physical exertion.

MacDonald's strength was his persistence in pursuing a course and finding ways to overcome obstacles. His ways were sometimes inept, especially in internal administration. His business dealings with the heads of other companies won their confidence and respect.

MacDonald's weakness was his inability to surround himself with men who could supplement his strengths by filling some gaps of his weaknesses. He evaluated an associate by what he wanted him to be, and then he was disappointed with his performance. I know firsthand, because I was in that position after he welcomed me back to the Company in 1959.

MacDonald was lonesome, as the prime mover in the Company. He was timid about taking sole responsibility. He needed someone to share it. This need came to be filled by Jack Binns, who was 11 years older. I do not know how he happened to be employed as a Company officer in 1924. He was famous as a radio operator and later as a radio editor in New York. He came from a British background, and was a very friendly partner who talked the same language. MacDonald shared with him the primary responsibility of the Company for many years. Jack Binns was also friendly with our licensees and helpful to MacDonald in the Company relations with them. He was always very kind to me, but concerned with different functions in the Company.

MacDonald's timidity was in contrast to his commanding height and physical build. It was most evident in his worries for the Company, usually justified. In testifying as a witness in a patent trial, he did not have the confidence to give a simple unqualified "yes" or "no" reply. I suspect that his timidness reflected his struggle in his earlier years and his awareness that his education was largely acquired in the "school of hard knocks". I admire such education but it did leave a weak link in communication between him and me.

In those days, it was not customary to address one's senior by first name. Some of MacDonald's friends called him "Bill", but more called him "Mac". He used the latter nickname in conversation with me, so I soon came to call him Mac. His seniority of 8 years over me naturally shrunk in significance with the lapse of time, but I always treated him as my senior. He always treated me as an equal. His favorite compliment to me was, "Harold, you have forgotten more than I ever knew". He was particularly complimentary to the way I kept my laboratory notebooks.

The years of this account saw the transition from Prohibition to the fad of Drinking. Some service men from World War I were the prime movers. This category included MacDonald and some of the company heads with whom he negotiated, so they had a common denominator. My generation of young engineers were outside this category, so we were separated by a gap in the social structure.

In terms of technical knowledge, MacDonald had acquired much in the years of college age. In his experience as an operator, he learned a lot while using radio transmitters and receivers. Around the end of World War I, he had a wonderful opportunity in the Paris laboratory of the Signal Corps. He was working with outstanding young men in Armstrong's group. With some of them, he completed a radio course for a year in the University of Paris (Sourbonne).

While he contributed much in ideas and sound advice in the laboratories, there were few notable examples of his originality. The outstanding one was the "high-inductance primary" for the antenna tuned circuit of a receiver. This is detailed in the section on "uniform gain". Prof. Hazeltine was attracted to this feature and made a thorough analysis of it. In the 1930's it was used in nearly all broadcast receivers that were designed for a long-wire antenna.

MacDonald's patent was the subject of one infringement suit, which was decided against the patent. The subject was difficult for the Court, and I think the decision was a mistake.

With this general introduction, I wish to chronicle some of the principle events in MacDonald's guidance of the Company.

At the beginning of the Company (240201) there was a trio bound by close ties. Professor Hazeltine provided the invention and the engineering genius that made the success of the Neutrodyne receiver. Taylor was close to Hazeltine, as his former student and patent attorney who provided the opportunity for this success. I was close to Hazeltine by virtue of our common language in engineering, and the coincidence of our separate inventions related to the Neutrodyne. Taylor had seen the need and opportunity for Hazeltine Corporation, then he had found the financial base for a stock company listed on the Curb Exchange. All three of us were profiting by the success of the Neutrodyne.

The Company was intended to manage the royalties and to satisfy the increasing needs of our licensee manufacturers for engineering assistance and ongoing improvements. For the latter purpose, Taylor set out to organize an engineering laboratory. Two things happened, but I do not know which came first.

Taylor set out to hire a Chief Engineer. After his graduation from Stevens (1916) Taylor had served as a Captain in the U.S. Army Signal Corps near Paris. There he had become acquainted with MacDonald, a Lieutenant serving under Major Armstrong. MacDonald was then employed by RCA and lived in Little Neck on Long Island. Taylor employed MacDonald as Chief Engineer of the new Company, effective 240315, 6 weeks after its beginning.

One or both of them found a location for starting a laboratory. The Professor's E.E. Dept. was located in the "Navy Building" at Stevens, which had been built during World War I as a Navy barracks on the waterfront. (It was a "temporary" building, which was used for many years until recently demolished.) There was idle space in the third-floor attic of this building. The Company rented a back corner, overlooking the Hoboken waterfront, and partitioned three small rooms for office and laboratory.

MacDonald was joining a group whose backgrounds were very different from his. He employed as lab assistant, a friend of his, R.W. Ackerman, and engaged part-time secretarial help.

I was a problem to MacDonald. Our first meeting was his visit to me in Washington 240416, a month after he started work. We found little in common. He did not talk my language and I had no way of perceiving the value of his practical experience. I did not accept him as my boss, and he was not demanding in this respect. I needed some good fatherly advice, which my father did not provide in this case. On some later occasions, Mac tried to be helpful to me, and succeeded in some degree. I was always respectful.

One impediment to communication between Mac and me was the difference in our attitude toward money. He had gone to work after high school, and had struggled to get ahead with a good mind but without a college education. As a result, he stood in awe of money. Several times I have heard him say of someone, "He must be good because he got a lot of money". This "bottom-line" logic has a weak link which I have appreciated more with years of observation. His respect for the dollar was essential in carrying the Company through the Depression. With the advice of Jack Binns, Mac made all decisions for the budget and payroll of the laboratories. I was not even consulted for rating the employees under my direction. The result was fair enough, and it relieved me of some worries. I always enjoyed the support I needed in expenditures and personnel.

MacDonald's background was in contrast to my "sheltered" life, which was never a struggle for money. There were times when I did not spend much, and even should have spent more on my experiments. My parents made the struggle for the family, which was barely sufficient to carry us until I began to receive a liberal income from the Professor and the Company. The continuity of this income supported my education through 7 years of college and carried us through the Depression, with no struggle except the unexpected costs of financing our new home. I was subject to one test which Mac applied to some individuals: "He never had to meet a payroll". I have often remembered this test, especially in respect to youthful protesters and public officials. At least, I did sympathize with Mac's problems and cooperate in his approaches.

During my next summer (1924) in Hoboken, I worked mostly in Mac Donald's lab, but some in the Professor's lab on the second floor. Mac was making some progress in setting up lab tests of radio circuits. I remember one incident when he was stumped by some misbehavior of his "VT voltmeter". I took one look, diagnosed the problem as a "parasitic oscillation" at a higher frequency, put one finger on one screw and stopped the trouble. It was not always so easy. Mac was also good at troubleshooting, from his range of experience.

The next 3 summers, Mac had increased the staff with some friends of his and some Stevens graduates, and I worked for him in the lab. He was confronted with the usual problems of getting an adequate budget from the Company's Board of Directors.

Mac used to tell me that my talents needed management. I did not resist this notion, but only with time have I fully appreciated its significance. From one viewpoint, he provided me with adequate support so I was relieved of such burdens. Futhermore, some of my most productive work resulted from problems he assigned to me. I needed even more guidance, because I was inclined to dig deeper in the current problem rather than to shift to another problem. I was always free to pursue problems that attracted me, and with good support. This was MacDonald's way of expressing general appreciation of my work.

The primary function of the lab was to assist our licensee manufacturers. MacDonald was at his best in this function. He was sympathetic to their needs and understanding of their problems. He was cost conscious in a way that I have never been. He made good use of our staff for this function. Our work was appreciated and it made us happy. We made close friends of our peers in the other companies.

From the fall of 1928, when I moved to New York for fulltime work, Mac gave me definite assignments which were well chosen from the viewpoints of the Company and myself.

From that time, for the next 4 years to 1932, MacDonald was confronted with his first major challenge. The Neutrodyne was superseded by the screen-grid tube, so the Company had no patents then in use. Our licensee group, the IRM, disbanded but most continued as licensees. They were joined by some other companies previously excluded. We had some patent situations in prospect. Also we had improvements which were useful with screen-grid tubes. MacDonald held together our licensee situation by superb salesmanship and engineering services.

In this period of transition, Mac pushed forward in expansion of our engineering activities. The first major advance was the move to a new laboratory in New York City. Early in 1929, the N.Y. lab occupied the top floor and "penthouse" of a loft building at 333 West 52 St. These were palatial quarters by comparison, and Mac enjoyed the grandeur. The floor below was later occupied by Jack Binns and the accounting staff, formerly located at 120 Broadway. A subsidiary incorporated in New York State was formed for the lab organization. It was named Hazeltine Service Corporation (HSC).

Jack Binn's office on the 14th floor was just under MacDonald's on the 15th, and they had an interconnecting key and buzzer for code signaling. One day Mac had had enough of one caller and casually played with his key to signal Jack Binns to take him away. It turned out that the caller was also a radio operator, and he told Mac that he was leaving anyway.

The N.Y. lab became famous for service to manufacturers, in competition with the RCA License Laboratory, also in New York City. In professional circles, their staff and ours were governed by mutual respect and personal friendship.

The next year in this period of transition, Mac perceived the need for separation if advanced developments were to be pursued in addition to licensee engineering. He and Taylor selected an old mansion (no longer habitable) near Taylor's place in Bayside, and bent the zoning a little for our purpose. It was equipped for a separate laboratory, and occupied late in 1930.

Mac put me in charge of the Bayside lab (at age 27) which was a major promotion and a great compliment. As I look back, I am embarrassed to remember how little I showed gratitude and appreciation for this opportunity. It would have made Mac happy. In anticipation, I was building a new home in Great Neck, 5 miles beyond Bayside. The lab was a mile from the Bayside station on the L.I.R.R., which was convenient to the Penn. Station and the subway to the N.Y. lab.

When the Bayside lab was started, Mac made a serious mistake, perhaps on advice of our patent attorneys. He provided a new style of lab notebooks designed for a carbon copy of every page. Every so often, the carbon copies from each notebook were removed and filed in a separate clip binder. For record purposes, this procedure was logical. However, the nuisance and formality of making a carbon copy caused a serious resistance to the entry of working notes. At best, an engineer must be disciplined to work in a bound notebook rather than on loose sheets (as in a looseleaf notebook). The quantity and variety of my notes could not have been kept in the carbon-copy format. As a result of the carbon-copy system, most of my notes were kept in looseleaf notebooks and have been lost. In 1942, I designed a new style of bound notebook (without carbon copies)) which was intended to offer least resistance to keeping working notes therein. This style is still in use in the Company.

1930 was early in the Depression. Soon Mac decided we should take a pay cut, in keeping with the times. He and I took 25% while the others were cut only 10%. This small cut and the avoidance of any reduction of staff made our Company unique in those difficult years.

In 1934, the last litigation over the Neutrodyne patent was dragging out, in a tedious accounting of the amount that the Atwater Kent Co. should pay Hazeltine Corp. for past infringement. Mac set up a personal meeting with Mr. Schwank, the AK general manager, and they arrived at a formula for settlement. AK took a license for future use of our patents and paid us

$680,000. The former became worthless, because they went out of the radio business. The latter was extremely valuable at the bottom of the depression. A substantial fraction was distributed among the employees as an extra bonus.

Late in 1934, Mac had to make the most difficult decision of his career. Our patent attorneys were the firm of P.D.M.&.E., of which Taylor was one of the later partners. They had enjoyed a monopoly on our patent work, which was a natural result of Taylor's initiative in forming the Company. The work included two activities. One was the filing and prosecution of patent applications in the Patent Office. The other was the conduct of litigation to enforce our patents and to protect the monopoly due to our licensees under patent law.

The litigation required continuity, especially in the adjudication of recently issued patents on my AVC invention. It was being conducted by Taylor and Adams, with participation of the senior partner, William H. Davis.

However, Mac and I were unhappy with the processing of new applications. We thought the results were not commensurate with the costs. Mac presented our views to Taylor in a couple of meetings I attended. It was extremely difficult for both of them. They were close friends and Taylor had brought Mac into the Company. Each one was loyal to his organization and sensitive to his obligation. The outcome was Mac's decision to form a patent department in the Company for the processing of new applications. Taylor was hurt and never fully recovered, although he and his firm continued the litigation which they were then conducting for us. Years later, Mac reminded me of his agony in making this decision, which he regarded as his responsibility in the Company. In retrospect, I do not understand how it came to be necessary, but I believe it was. The new patent department was inaugurated in early 1935, with the employment of Laurence B. Dodds from General Electric Co.

The next year, Mac made another wise decision, in which I did not participate although I welcomed the move. It involved Dan Harnett, who was then in charge of the N.Y. lab. At the end of 1936, Harnett was transferred to Bayside to take over from me the direction of that laboratory. Two factors made that move advisable. First, my efforts had been increasingly preempted by our litigation, mostly relating to my AVC patents. Secondly, the growth of our television activities under Lewis had suffered from my primary interest in other developments that had progressed under my direction. The result was a closer, happy association between Harnett and myself.

The growth of our TV activities in the Bayside lab impressed Mac with the need for more facilities. He planned the construction of a new building near his home in Little Neck. This was a bold step and he was sensitive to the risks.

Therefore the building was so designed that it could easily be converted to a 4-family dwelling. This never became necessary. The new Little Neck laboratory was occupied in 1939. Harnett was in charge, succeeding Mac Donald as Chief Engineer, while I worked closely with him as Chief Consulting Engineer. The readiness of this facility gave us a headstart in our work for World War II.

This account closes with the transition to a different mode of operation after Pearl Harbor. In 1942, Mac built a large one-story building next to the Little Neck lab and closed the N.Y. lab after 13 years. He had moved to Little Neck, where he shared an office with Dan Harnett. This was unfortunate in some respects. Mac did not enjoy an easy relationship with our engineering staff, in view of the communication gap. Dan had served as an effective buffer, and had been sufficiently removed from Mac to enjoy the necessary freedom of leadership. Both of these relations were lost, and the result was an excessive strain on Dan. After the war, Dan and I embarked on different courses outside the Company. My subsequent accomplishments in Wheeler Laboratories. Inc., were acquired by Hazeltine Corp. in 1959 when Mac negotiated the purchase of my company. Mac died in 1961. Later my group was merged into the parent Company (1971) where I have continued serving in various functions.

[A] See 9.2[A].
[B] W. A. MacDonald, "Considerations in screen-grid receiver design", program of Eastern Great Lakes District Conv., Rochester, Nov. 18, Proc. IRE, vol. 17, p. 1671; Oct. 1929.
[C] W. A. MacDonald, "European reception conditions", program of Rochester Fall Meeting, Nov. 10, Proc. IRE, vol. 19, p. 1704; Oct. 1931.

### 3.6 Jack Binns.

John Robinson Binns (who used the name Jack Binns) was born in Lincolnshire, England, 1884 SEP 16. He came to U.S. in 1912 and became a U.S. citizen in 1923. After age 14, he attended a technical school for telegraph operators and worked as an operator, then he went to sea as a wireless operator until 1912. In New York, before World War I, he was a reporter for New York American. During the war, he served in the Royal Air Force of Great Britain and the Royal Flying Corps of Canada, becoming an instructor in flying and radio. On the New York Tribune in 1919-24, he became editor of the weekly radio supplement. After joining Hazeltine Corp. as assistant treasurer in 1924, he worked closely with MacDonald and rose to president in 1942. He was Chairman of the Board from 1952 until he died in 1959. He was a Fellow of the Radio Club of America.

Jack Binns became famous in 1909, when he was the wireless (radio) operator on the S.S. Republic. That ship was rammed in a fog off Nantucket. The 1500 passengers were immediately transferred with great difficulty to the other ship, also damaged but still controllable. Jack Binns sent the distress signal (then CQD, later SOS) which called help via the nearby shore station. In spite of dense fog and darkness, radio communication guided the nearest liner, Baltic to the scene in time to take the total of 3000 passengers from the other ship and then to save the crew of the Republic before it sank. By then, the fog had cleared, revealing many other ships which had heard the radio dialog and had come to help if needed. Their searchlights illuminated the last act, rescuing from the water the Captain and his Second Officer. This incident was the first sea rescue by radio.

A. M. Caddell, "CQD — the story of the first sea rescue by radio as told by Jack Binns", Radio Broadcast, vol. 4, pp. 449-455; Apr. 1924.

Directory, Radio Club of America; 1959.

Editorial, "60 years ago", Technician-Engineer; May 1969. (1909, Jack Binns.)

Editorial, "Progress since the Republic's CQD", Proc. Radio Club of Amer., Golden Jubilee, p. 78; 1959. (Jack Binns, 1909, account written in 1939.)

## 3.7 Rickard.

Edgar Rickard was a mining engineer, active in the American Institute of Mining and Metallurgical Engineers. During World War I, he was one of the prominent engineers on the international Commission for Relief in Belgium (C.R.B.) headed by Herbert Hoover. He later served as administrative assistant to Mr. Hoover in all of Mr. Hoover's emergency war and post-war organizations. He was recognized by several honorary degrees, the last in 1936, Doctor of Laws, from U. of California.

Mr. Rickard served as a Director of Hazeltine Corporation from 1925 until his death in 1951. He was President 1927-42 and Chairman 1927-51. He was a tall, handsome man with great poise and self-assurance. His office was never near mine, so I had few contacts with him, but I enjoyed every opportunity.

### 3.8 Troxler.

Lucien J. Troxler, Jr., was born (18)900730 in Louisiana and received his B.E. degree 1915 from Tulane U. in New Orleans. He started in Hazeltine Corp. as an engineer at the Hoboken lab 280216. He was brought in by MacDonald, who had worked with him in the Signal Corps Lab. at Ft. Monmouth, N.J. Troxler worked with me in the N.Y. lab, then at Bayside. He continued at L.N. until retirement 640319 after 36 years. He was a Senior Member of IRE-IEEE.

Trox, as he was fondly known to his associates, was 13 years my senior, and I always looked up to him. He was tall and slender, with a friendly, retiring manner toward everyone. He was the most conscientious worker I have ever known. Much of his work was designing, assembling and calibrating test equipment to meet our exacting requirements in the laboratory. His accuracy and attention to detail were well applied to such equipment. He is now living in nearby New Jersey.

## 3.9 Harnett.

Daniel E. Harnett was born in Elmira, N.Y. 1899 JUL 20. He received from Columbia U. the A.B. in 1922 and E.E. in 1925. He started in Hazeltine Corp. 290201, served as chief engineer in 1939-46, and left in 1948. He had previously worked for Pacent Electric Co., W. J. Murdock Co., and Acoustic Products Corp. Subsequently he worked for Emerson Radio and Phonograph Corp. 1948-49 and then General Electric Co. in Syracuse, from 1949 until he retired in 1964. He was a Fellow of IRE (1942), AIEE, their successor IEEE, and Radio Club of America. He died 750911.

Dan Harnett was my best friend. He was a real friend to every one of our engineers and other employees. He was my senior by a few years of age but I was senior to him by a few years in the Company. First we cooperated closely when he was a charge of the N.Y. lab and I was in charge of the Bayside lab. After he moved to the Bayside lab in 1936, most of my activities were under his direction and I owe much to his guidance.

Dan was handsome and nearly as tall as Mac. The three of us have been dubbed, "the Hazeltine giants", with me the least in age and height (just under 6 feet). Each of us was a little stooped in posture. The one time we were photographed together was in front of the Little Neck lab while under construction early in 1939. [A]

Dan had a remarkable balance of the best traits of a chief engineer. His manner was quiet and unassuming, while supported by a sense of purpose and a high level of engineering education, competence and experience. Many times he painlessly steered me back on the track when I strayed because of carelessness or narrow vision. Those incidents may have been individually trivial but their sum made a great impression on me.

In our growth period of the 1930's, Dan was able to cooperate closely with Mac, in spite of their marked difference of background. Perhaps his greatest achievement was serving at once as a buffer and a connecting link between Mac and our ambitious young engineers (including myself). Mac admired and trusted him, while he recognized Mac's objectives and problems.

Dan's greatest engineering strength was not so much personal invention as the ability to perceive an opportunity and to stimulate others. His practical experience in previous jobs and in contact with our licensee manufacturers was a powerful background for his direction of our engineering activities. This continued in our work for the Government during World War II, when he enjoyed the full confidence of our customers. All of this added up to a truly great Chief Engineer. This was my view while working with him and I hope I found ways of letting him know it.

Harnett deserved more awards than he received. After the war, he did receive:

1945, Certificate of Appreciation from Office of Scientific R&D.
1947, Certificate of Commendation from U.S. Navy Dept.
1954, Certificate of Appreciation from RETMA (now EIA).

[A] Photo of W. A. MacDonald, D. E. Harnett, H. A. Wheeler, in front of L. N. lab (under construction), Proc. IRE, vol. 27, p. 153; Feb. 1939.
[B] Obituary, IEEE Spectrum, vol. 12, no. 12, p. 78; Dec. 1975.

## 3.10 Dean.

Charles Earle Dean was born (18)980523 in South Carolina. He received these degrees:

1921, A.B., Harvard U.
1924, M.A., Columbia U.
1927, Ph.D., Johns Hopkins U.

After some work with the Telephone Co., he was employed by MacDonald and started work 290320 at the beginning of the N.Y. lab. Dr. Dean was our first employee with a doctor's degree, and his was excellent preparation in physics and math. He worked at the Bayside lab, the Little Neck lab and the Plainview lab, until he retired in 1963. He resides near Washington, D.C. He was elected a Fellow of AIEE and later IRE, both of which became IEEE. He also was elected a Fellow of Radio Club of America.

We knew Dean as Earle or Charlie. Vernon Whitman and I became acquainted with him when we attended some of the same classes at JHU, a few years before he came to our company. He had a wide range of knowledge, and became more familiar with radio. He was assigned some unusual tasks. One was the cataloguing of 100 current radio receivers of many makes, which were intended to yield a perspective on the current market and practices. They were described in Bayside reports in the block 100W-200W, around 1932. (It is unfortunate that this collection was not preserved.) He was particularly helpful with exhibits for litigation.

During the time before World War II, Dean's most notable contribution was composing the series of 13 reports which was later printed in the volume entitled "Television Principles", 1944. He was skillful in working from notes or drafts with the understanding needed to yield an orderly text. In later years, he edited two other volumes of Company articles relating to color television, 1955 and 1956. He wrote a comprehensive chapter on radio receivers for the McGraw-Hill "Radio Engineering Handbook", edited by Keith Henney, fourth and fifth editions, 1950 and 1959.

### 3.11 Whitman.

Vernon Eleazer Whitman was born in Washington, D.C., 1900 APR 30. He received the degrees:

1922, B.Sc. in Electrochemical Engg., M.I.T.
1924, M.Sc. in Phys., M.I.T.
1924, M.A. in Phys., J.H.U.
1928, Ph.D. in Phys., J.H.U.

He was employed as physicist in the Bureau of Standards, D.C., 1923-29. Then he was employed by Hazeltine Corp. 1929-37. For the next year or so, he worked for Carl Zeiss, Inc., in New York City. In the Fall of 1939, he went to Folmer Graflex, Inc., Rochester, N.Y. as assistant to the president. That company became Graflex, Inc., and he became Director of Research & Development, during World War II. He continued with successor companies until he retired in 1963.

In the Bureau of Standards, he worked in the Aeronautical Instruments Section, also in the Capacitance and Inductance Section and the Instrument Section of the Electrical Division.

I met Vernon Whitman in 1922 when we were both ushers in All Souls Church (Unitarian) in Washington, D.C. From then until he went to Rochester, we were close companions. Working at his home, with my advice, he built a sophisticated superheterodyne receiver, at a time when it was not common. In our avocations, we had two common interests. One was the piano, playing the classics by memory. The other was ballroom dancing, likewise the classic forms. He was the more skilled in both.

In 1925-26, our first year at Johns Hopkins, he shared my dormitory room while time-sharing with his job in Washington. It was his suggestion to the department head (Prof. Ames) that I be appointed to teach a one-semester course on electron tubes, which I did in our second semester. He was an excellent student, as proven by his record at M.I.T. and J.H.U.

When I was married in 1926, Vernon served as my "best man". Afterward, he was a frequent guest in our home. When he was married in 1930, I served as his "best man".

At my suggestion, Vernon was employed in the N.Y. lab 290601. He was transferred to the Bayside lab late in 1930, when it was started, and continued working with me. His superior education and his experience with laboratory equipment enabled him to do a professional job on the projects assigned to him. I learned a lot from him.

I do not recall that he was especially inventive although ingenious in theoretical and experimental work. However, one incident stands out in my mind.

Sometime in the middle 1930's, he proposed to me a radio altimeter using pulses reflected from the ground. He was motivated by his previous specialty of aeronautical instruments, including the barometric altimeter. I proved that his suggestion would not work, because the short pulses would require much more bandwidth than I could see in the frequency spectrum. I lacked the foresight that much higher frequencies in the future would make available the greater bandwidth. Soon afterward, RCA released a pulse altimeter (more properly termed, a terrain-clearance meter). (An assistant to Whitman, Rudolph Sturm, recently reminded me that he also made that proposal to me, with similar response.) The pulse altimeter was a forerunner to radar.

In the late 1920's, Vernon became an avid amateur photographer. It was the advent of the Leica with many frames on a 35-mm film, which started the era of the "shutterbug". He snapped many candid shots of our first baby at various ages, and Dorothy was a beautiful model. He made a deal with a New York agency so he could borrow unusual equipment for experiments. I recall a close-up of a dragon fly, which he enlarged to show the sophisticated structure of its wing. Nature had evolved a triangular girder of three tapered tubular members with just enough separators to avoid collapse. Man has never equaled that design. Vernon did all his (fine-grain) development and enlarged printing at a time when such services were not common.

As Vernon's interest in photography grew steadily, it appeared to me that he would advance furthest in that field. It was a difficult decision for me, but I had a logical basis for it. Not long afterward, he was launched on a second career at Graflex. There he combined his love of photography with his multilingual experience in travel.

After his retirement, Vernon and his wife, Irene, continued living in Rochester. More recently, they have moved to Pomona, Calif., and we keep in touch with them. Some conversations with him have been helpful to me in preparing this account.

### 3.12 Johnson.

John Kelly Johnson was born 030127 in Oskaloosa, Iowa. He received these degrees:

1923, B.A. and B.S., Penn College, Iowa.
1924, B.A.; 1925, B.S.; 1927, E.E., Columbia U.

He was a member of Sigma Xi and Tau Beta Pi. He was a Fellow of Radio Club of America and IRE (later IEEE.). He worked in the N.Y. lab of Hazeltine Corp. 300201-341001. After working at Wells-Garder, he was called back to Hazeltine in 1937. He formed the new Chicago lab, where he was in charge until 1942. From then, he was engaged in consulting work. He is retired and now resides in Vermont.

Kelly Johnson was a tall, handsome man with an attractive, outgoing personality. Our common interests in our work occupied many happy hours together. He served us very well as a talented engineer with excellent preparation. He was especially helpful in our work with licensees, which led to his recall for forming and managing our Chicago lab.

## 3.13 Cawein.

Madison Cawein was born 040318 in Louisville, Ky. He received the B.S. in Physics 1924 from U. of Kentucky, and he returned as an instructor and postgraduate student 1926-30. He was elected to Phi Beta Kappa.

He joined Hazeltine Corp. 300716 in the N.Y. lab. After a year or so, he went to the Bayside lab to work with Lewis. There his work related mainly to multiband receivers, TV and associated test equipment, as recounted in other chapters. As a result of his previous acquaintance in 1924-25, he brought Hergenrother into the Company in 1935, which was a major event in our TV program.

After 1938 he worked ten years in Farnsworth Television & Radio Co., Fort Wayne, Ind. Since then, he has been active in various other companies and consulting work.

Cawein has about 60 U.S. Patents, of which about one-third were assigned to Hazeltine Corp. In 1954, he was named a Fellow of IRE (later IEEE). He has been living in Denville, N.J.

**3.14 Swinyard.**

William O. Swinyard was born in Ogden, Utah, 040717. He received the B.S. in math and physics in 1927 from Utah State U. He was married and moved East, graduating from RCA Institutes. In 1930, he was employed as an engineer in Hazeltine Corp., where he served in various functions until his retirement in 1969.

| | |
|---|---|
| 1930-34, | Bayside lab. |
| 1934-37, | N.Y. lab. |
| 1937. . . | Chicago Lab., Hazeltine Research, Inc. (HRI) |
| 1942. . . | Chief engineer, HRI |
| 1958. . . | V.P. and Director, later President and Chairman, HRI. |

He was one of the founders of National Electronics Conference, annual in Chicago since 1944, and has served NEC as President and Chairman of the Board. He was elected a Fellow (1945) in IRE (now IEEE) and also a Fellow in Radio Club of America and in AAAS.

In the early days of the depression, Bill Swinyard lived in Bayside. One day in 1930, he saw some work in progress on the old mansion which was being fitted to serve as the Bayside lab. He applied for carpenter work. When we soon learned of his college degree, we offered him a position as engineer. This was the beginning of the longest continuous service of any engineer in the Company (40 years). After "retirement", he and his wife moved to Salt Lake City, but he travels to Chicago as a Director of HRI.

Bill Swinyard was an assistant to Vernon Whitman, while they were doing valuable work on some projects of particular interest to me. He brought a natural aptitude for engineering, together with college education in science and excellent learning ability.

When Kelly Johnson left the N.Y. lab in 1934 to become Chief Engineer of Wells Gardner in Chicago, Bill Swinyard was transferred to the N.Y. lab as his successor in service to licensees.

In 1937, the Chicago lab was organized as a function of HRI, which was the patent holding and licensing subsidiary of Hazeltine Corp. Its purpose was services to licensees in the Chicago region. Kelly Johnson rejoined the Company as Chief Engineer of the Chicago lab, and Bill Swinyard went there as his assistant. When Kelly left in 1942, Bill became Chief Engineer. He held that post until 1957, when he became President and Chairman of HRI.

Bill enjoyed working under Kelly Johnson in services for our licensees in that area, and he was very popular with the engineers and managers of those companies.

Bill was active in the IRE and other professional affairs, especially in Chicago. His term as IRE Director (1945-46) overlapped the last year of my term (1940-45). He later served as chairman of the Technical Committee on

Radio Receivers (1946-48), a post that I had held (1935-38).

On our first trip to the West Coast, by train in 1938, Ruth and I visited Kelly Johnson and Bill Swinyard at the Chicago lab, 325 W. Huron St.

[A] W. O. Swinyard, "Measurement of loop-antenna receivers", Proc. IRE, vol. 29, pp. 382-387; Jul. 1941. (Biog. p. 412.)

[B] W. O. Swinyard, Board of Directors — 1945, Proc. IRE, vol. 33, p. 822; Dec. 1945. (Biography.)

### 3.15 Lewis.

Harold Miller Lewis was born in Geneva, N.Y., 1893 JAN 12. He returned there in his last years and died in 1978 at age 85. He graduated from Union College, Schenectady, in 1916 with degree of B.E. After a year in Research Lab of G.E. Co., he served in the U.S. Signal Corps during World War I. He was located in the Paris laboratory 1918-19 and attended the Radio Engineering Course at Sorbonne. He continued at Camp Alfred Vail, N.J., in 1919-21 and again in 1927-30 after it became Fort Monmouth. In the meantime, he was President of Radio Service Labs., and then President of LeMor Radio. At the Bayside lab of Hazeltine Corp., 1930-37, he was in charge of the Company's initial work in television. After that, he worked in various functions for Hazeltine Corp. and Signal Corps. He was a Fellow of Radio Club of America.

"Lui", as he liked to be called, was a natural inventor with a good balance of theoretical training and intuitive sense. He was issued about 60 U.S. patents.

The one I remember best is a method of generating single-sideband modulation. A radio signal comprises a carrier on a fixed frequency, supplemented by sidebands representing the modulation. Lewis combined amplitude and phase modulation of the carrier to yield only one "single sideband" representing all the modulation. It is still used. Currently this principle is relied on for one of the proposed systems of "AM Stereo", which is in use and may be adopted as standard in the near future. This system is supported by Hazeltine Corp. and I have been working with Barney Loughlin on some circuits based on the Lewis invention.

Lewis was an active amateur in his formative years. After joining the Signal Corps in World War I, he was a Sergeant in Major Armstrong's group in the Paris laboratory. That is where Armstrong invented the "superheterodyne", a circuit which is still used in all kinds of radio receivers and will never be obsolete. Armstrong described it to Lui on a page of notes, for Lui to make the first model. I have a print of those notes. This must have been a powerful stimulant to Lui's inventive bent.

In the early years of the Bayside lab, Lui and I did not enjoy a smooth relationship. He was employed by Mac as an old friend (from the Signal Corps in World War I) whom he knew to be competent. He was assigned to the Bayside lab which was under my direction. He had seniority in age and experience. I had seniority in the Company. I was not experienced or talented in the utilization of another employee of proven genius. Lui was sensitive and headstrong, but not in excess, as I see it in perspective. To cap the climax, he favored Roosevelt versus Hoover the engineer president, in what was probably the bitterest presidential campaign in history. Sometimes we shared ideas and mutual respect; sometimes we fought. In later years, we relaxed and became the best of friends.

In the early 30's, there was an incident which is still vivid in my memory. My invention termed the "piston attenuator" had been built into two signal generators by Lewis and Cawein. The second used the type characterized by coaxial coils in a pipe. The attenuator was a "primary standard" but they committed the "heresy" of checking it by measurements. They found and identified a weakness in this type, because it did not use what we would now call the "lowest mode" in the pipe. With this stimulus, I proceeded to invent another type, characterized by coplanar coils in a pipe, which did use the lowest mode and hence was free of this weakness. This was a spectacular case of the need for bringing together different viewpoints to combat complacency (mine, in this case).

In the early 30's, Lui perceived that television was the growth area for our lab and initiated an intensive program in TV. I do not know how much he may have been influenced by others, such as Mac and Dan. I was somewhat indifferent, largely because I was immersed in other areas that were (and still are) dear to my heart. Lui and his assistant, Madison Cawein, did pursue television, and I did allocate supporting effort in circuit development. In circuit design, Lui favored the "transient" viewpoint whereas I was accustomed to the "steady-state" viewpoint. Both were ultimately found essential. Most emphasis on TV was assured by Harnett's transfer to Bayside late in 1936. Gradually it became my principal activity.

### 3.16 Case.

Nelson Perry Case was born in Canon City, Colo., 040717. He received the B.A. in physics in 1924 and E.E. in 1926, both from Stanford U. The next few years he worked on geophysical research, then at Bu. of Standards, then at U. of Michigan. He joined Hazeltine Corp. in 1931 and continued until 1945. Then he went to Chicago as Chief Engineer of Hallicrafters. He retired to Arkansas, where he lived until he died in 1978. He was a member of Phi Beta Kappa and Sigma Xi. His functions in Hazeltine Corp. were:

1931-34 Bayside lab.
1934-42 N.Y. lab. (from 1937, in charge).
1942-45 L.N. lab.

Nelson Case joined me in the Bayside lab after excellent preparation and some experience. He was a very competent engineer and a valuable member of our team. When Kelly Johnson left N.Y., Nelson was transferred to the N.Y. lab to work with our licensees, for which he was well suited. During the last five years of the N.Y. lab, he managed that operation.

### 3.17 Farrington.

John F. Farrington was born 1895 in New York. His radio career was as follows:

| | |
|---|---|
| 1907-11, | radio amateur. |
| 1911-15, | radio operator on shipboard. |
| 1916, | Bliss Electrical School, honor student. |
| 1916-29, | W. E. Co. and Bell Telephone Labs., development work on war radio equipment, then radio-telephone services. |
| 1929-31, | International Communications Labs., New York, engineer-in-charge, radio department. |
| 320929, | Joined Hazeltine Corp., Bayside lab., remained until about 1939, recalled 1942 for some war work. |

He retired to Laconia, N.H., doing consulting and some work at Raytheon. He died 1964. In IRE, he was a Fellow from 1931. Also he was a Fellow of Radio Club of America.

John Farrington joined me at the Bayside lab in 1932. He was tall and slender, and his manner reflected his wide variety of interests and background. He was employed by MacDonald for his wealth of experience in some of the leading groups working on radio. I was 8 years younger and lacked such experience, so I was not prepared to make best use of his talents. He made some efforts to help me with guidance, but I am doubtful how well I may have responded. Under my direction, he did excellent work, including one or more projects in which he was prime mover. I enjoyed our association and we learned much from each other.

### 3.18 Dodds.

Laurence B. Dodds was born 020911 in Nebraska. He received these degrees:

1925, BS EE, U. of California (Berkeley).
1930, JD, George Washington U.
Sigma Xi, Tau Beta Pi, Phi Delta Phi, Order of Coif.

After experience as a patent attorney with G.E. Co. in Schenectady and Washington, MacDonald employed him 341221 to form our new Patent Dept. as an adjunct to the N.Y. lab. In 1939, he was elected a Director and VP of Hazeltine Corp. He held various other offices in later years. His service was part-time after 1960 and he retired in 1975. He is now living in Boulder, Colo.

Larry Dodds was tall and handsome, with a deserved manner of self-confidence. He and I worked together a great deal. He and his group prepared and filed more than 100 of my patent applications, with unusual competence. He conducted the litigation which forced the Patent Office to issue to me a patent on the "tuning meter", a feature associated with my AVC. Most of the litigation he conducted was after the war, when I was not directly involved.

**3.19 Hergenrother.**

Rudolph C. Hergenrother was born in Chemnitz, Germany, 030905. He received these degrees:

1925, B.A., Cornell U.
1928, M.S. in Physics, Penn. State Col.
1931, Ph.D. (cum laude), Calif. Inst. of Tech.

Then he spent 2 years on a research fellowship at Washington U., St. Louis, and a year at Farnsworth Television Laboratories. He joined Hazeltine Corp. in 1935 to work in the Bayside lab on development of television tubes. He continued in the Little Neck lab on microwave circuits and measuring equipment until 1945. Then he worked in Raytheon on various kinds of tubes until he retired in 1968 and went to live in Florida. He has published numerous articles and has numerous patents. He is a Fellow of IRE (1956) (now IEEE).

Dr. Hergenrother was brought to our lab by Madison Cawein, who was a pal of his at Cornell 19 years before. He was known as "Hergie" and fitted naturally into our staff for the next 10 years.

His first and most important task was the design and construction of a TV camera tube. The RCA iconoscope, for this purpose, could not be purchased, and we aimed to make a complete TV system for demonstration and further developments. Hergie had to equip a tube laboratory for glass working and chemical processing. The first camera tube was completed abour a year later, and its performance fulfilled our most ambitious expectations. The associated circuits were made by Lewis and Cawein, who were already working on such equipment. The U.S. Patent to Hergenrother and Lewis (2,169,840) related to this first camera tube.

On various subjects relating to tubes and microwave circuits, Hergie contributed much to our work.

[A] R. C. Hergenrother, "The television picture tube", Proc. Radio Club of Amer., vol. 17, no. 5, pp. 1-11; Jun. 1940.

### 3.20 Loughren.

Arthur Vivian Loughren was born 020915 in Rensselaer, N.Y. He graduated from Columbia U. with the degrees of BA in 1923 and EE in 1925. He was elected to Phi Beta Kappa, Sigma Xi and Tau Beta Pi. His background of professional experience came from 5 years at GE Schenectady, 4 years at RCA Mfg. Co. Camden, and 2 years at GE Bridgeport. He was active in IRE committees, where I became acquainted with him.

Art Loughren joined Hazeltine Corp. 360201 at my invitation. He was a Columbia friend of Dan Harnett. He worked first in the N.Y. lab and then in the L.N. lab until he left in 1956. We became close friends, and had much in common. He was most active in television, monochrome before the war and color after the war.

In the Company after the war, Art Loughren succeeded Dan Harnett as Chief Engineer, 1946-49, and was VP in charge of research, 1951-56. In the latter period, he directed the Company's group which made pioneering contributions to color TV. His participation was recognized by these awards:

1953, David Sarnoff Gold Medal (SMPTE)
1955, Morris Liebmann Memorial Prize (IRE)

He was elected President of IRE for 1956. He was a Fellow of IRE-AIEE-IEEE, SMPTE and Radio Club of America. He was awarded about 30 U.S. Patents, mostly assigned to Hazeltine Corp.

After leaving the Company, he went to Airborne Instruments Lab. of Cutler Hammer as VP in charge of Applied Research Div., 1956-62. Then he became President of a new company, Key Color Lab., 1962-75. He is now retired and lives in Hawaii.

In 1981, Art Loughren was awarded the Armstrong Medal by the Radio Club of America. I was invited to make the presentation at the annual meeting in New York.

## 3.21 Freeman.

Robert Lee Freeman was born 090201 in Cleveland, Ohio. From Stanford U. he received the degrees of AB, EE and Ph.D. in 1931, 33, 34. He was a student and friend of the famous Professor Frederick E. Terman. In 1934-37, he worked first at Crosley Radio Corp. in Cincinnati and then at Farnsworth Television, Inc., in Philadelphia.

Bob Freeman came to work with me in the Bayside lab 3709XX and continued in the L.N. lab until he left the Company 4508XX. He had excellent preparation and it was a pleasure working with him. I recall that he taught us to measure RF impedance by observing the standing wave on a slotted line. On Fred Terman's third visit to our lab 400127, we had a lunch reunion with Bob Freeman and myself. During the few years just before the war, Bob worked on various problems relating to broadcast receivers and television.

Bob left us for a short period as Chief Engineer of Lewyt Corp. in New York, then went to A. C. Nielson Co. in Chicago 1946-69. He retired to Cape Coral, Fla.

### 3.22 Curtis.

Leslie Forrest Curtis was born (18)881102 in Hanover, Mass. He died 1974. Some of his activities before joining Hazeltine Corp. were as follows:

| | |
|---|---|
| 1910 | BS in EE, Tufts Col. |
| 1910-12 | G.E. Co., Schenectady. |
| 1912-20 | U. Of Washington, Seattle. |
| 1912-17 | Instructor. |
| 1916 | MS in EE. |
| 1917-20 | Asst. Prof. of EE. |
| 1920 | Chairman, Seattle Section, IRE. |
| 1921-30 | American Bosch Magneto Corp., Springfield, Mass. |
| 1924-30 | Chief Engineer |
| 1930-37 | United American Bosch Corp. |
| | Chief Engineer, Radio Division. |
| ---- | Fellow, AIEE-IEEE. |

He joined Hazeltine Corp. 370801 to work with me in the Bayside lab. He stayed until the 1950's, when he continued for some consultation while living in Mass.

Les Curtis was a thoughtful and considerate professor of moderate stature and a friendly twinkle in his eye. It belied his stature as a climber on Mt. Rainier. He taught me a lot and I built on his teachings. I wonder if my enthusiasm with further progress left me with full appreciation of my headstart from him. This question arises over the two topics I recall. One was the divided-band transformer forming a continuous-band filter. (See Antennas and Lines.) The other was the feedback control of the coupling coefficient between two tuned circuits. (See XPS.) His later studies of FM behavior followed theoretical representations much different from mine, and I doubt if either one of us appreciated the other.

Our principal collaboration occurred early in the war, when we made a mine detector for the Army Corps of Engineers with support from NDRC (later OSRD). It was the SCR-625 design used in World War II and even later in Korea. After the war, he published a description of the mine detector. [B]

Curtis left little writing. He did write the chapter on "Modulation and Detection" in the McGraw-Hill "Radio Engineering Handbook" edited by Keith Henney, the third edition 1941 and the fourth 1950.

[A] "Electrical interference in motor car receivers", Proc. IRE, vol. 20, pp. 674-688; Apr. 1932. (Biog. p. 752.)

[B] "Detectors for buried metallic bodies", Proc. Nat. Electronics Conf., vol. 2, pp. 339-351; Oct. 1946. (Biog. p. 694.)

### 3.23 Wilson.

John Charles Wilson was a remarkable addition to our staff. He had a headstart in TV with Baird Television, Ltd., in England. From his experience there, he had written the first textbook on "Television Engineering" (Pitman, 1937). It was based on their pioneering broadcasts for BBC from 1929. On the title page, his credentials are stated as follows:

Fellow of the Royal Society of Arts
Fellow of the Physical Society
Fellow of the Television Society
Member of the Institution of Electronics
Graduate of the Institution of Electrical Engineers
Diplomate in Radio and High-Frequency Engineering
Member of the Television Department of
the Columbia Broadcasting System, Inc.

After coming to U.S., Wilson worked a short time in the TV department of CBS (Columbia Broadcasting System, Inc.).

He joined Hazeltine Corp. 371001 and stayed with us until his untimely death 411205 after a serious illness. I do not recall how he happened to come to us. He brought to us another dimension in experience and viewpoints. He was a true scholar in the British tradition. I enjoyed every minute of our many discussions. He wrote many internal reports but I recall only one outside presentation (of which I was joint author):

> "The influence of filter shape-factor on single-sideband distortion", (summary) Proc. IRE, vol. 28, p. 253; May 1940. (IRE Annual Convention, Boston, 400629.)

**3.24 Loughlin.**

Bernard Dunlevy Loughlin was born in New York City 1917 MAY 19. He was living in E. Orange, N.J., when he graduated from Cooper Union Institute of Technology. He received these degrees:

1939, B.E.E., Cooper Union.
1945, E.E., Cooper Union, Professional Degree.
1946, M.S.E.E., Stevens Institute of Technology.

Working at home while attending Cooper Union, he made a small television receiver to pick up the experimental signals from the RCA transmitter on the Empire State Building. Then he conceived and made a "vector response indicator" which won first prize in the student paper contest conducted by the N.Y. Section of AIEE. That developed into the "phase-curve tracer" which is the subject of a separate chapter.

Shortly after his graduation, Barney Loughlin applied for employment at the Hazeltine lab in Little Neck. Dan Harnett, Les Curtis and I interviewed him, and we were so favorably impressed that we employed him before he left the lab that day. He started work 390619, living near the lab until he was married in 1943.

Barney was steered into the field of radio by his uncle, Charles A. Wingardner, a CU graduate working on switching equipment at Bell Telephone Labs. in N.Y. City. While in high school, he became active as a "ham", with operator's license dated 330721 and then amateur station license W2GQX. Using a short-wave CW transmitter, he was qualified in the ARRL network.

During World War II, he worked on IFF equipment in the Navy programs at Hazeltine Corp. For a few years thereafter, he worked on development of FM and superregeneration and their combination. Then he became active in the conversion of TV to color. His concepts and inventions in that field form the basis for the present color TV signal. They have been accorded many honors, the most significant being Loughlin's selection by IRE in 1952 as the first to receive the Vladimir K. Zworykin Award. He has been elected to the National Academy of Engineering (NAE).

Loughlin has been awarded more than 100 U.S. Patents, which are assigned to Hazeltine. He has assumed increasing responsibility in the Company, serving now as a Director and VP, Technology.

Since my first meeting with Barney Loughlin 40 years ago, we have enjoyed a delightful relationship as close friends and as leaders in our chosen field. He was most receptive to my indoctrination in the years just after college. I was soon learning from him. After the war, our fields of specialization diverged but we always talk the same language. I derive the greatest satisfaction from my part in providing an early opportunity for his development. The Company turned out to be a fertile field for his monumental contributions to color TV. The period before World War II saw only the first development of his career, the Phase-Curve Tracer.

# Section 4. TRF Receivers

## 4.0 TRF Receivers.

A TRF receiver was one which relied on a Tuned-Radio-Frequency amplifier for some gain ahead of the detector. Such an amplifier had one or more stages, each individually tuned to the signal carrier frequency. The tuning enabled substantial gain per stage and contributed selectivity in favor of the desired signal. These characteristics distinguished the TRF amplifier from the familiar AF amplifier which was the original triode amplifier and was not intended to be selective in frequency.

The TRF receiver was an improvement over the Armstrong regenerative receiver, which was prevalent in all services around 1920, the beginning of sound broadcasting. That receiver obtained the required gain by feedback in the tuned detector circuit. There was no amplifier stage ahead of the detector. A critical adjustment of the feedback was required for maximum gain, which then yielded spectacular sensitivity to a weak signal. An improvement was needed to avoid the critical adjustment ("tickler") and to obtain more selectivity toward the increasing number of broadcast stations.

A TRF receiver was recognized as a desirable improvement but was not exploited around 1920 because it was subject to an unsolved problem. The triode vacuum tube was the type available and it contained inherent capacitance between the input electrode (control grid) and the output electrode (plate or anode). In an RF amplifier, this grid-plate capacitance caused feedback coupling which was not desired and interfered with the desired amplifying action. This problem had been analyzed by the famous Dr. Miller of the Radio Laboratory in the Bu. of Standards, so I became familiar with it in 1921 when I started working there.

The full utilization of the triode in a TRF amplifier required neutralization of this capacitive coupling to remove the feedback action. Prof. Hazeltine and I found the same circuit for this purpose, and used it in a TRF amplifier for broadcast reception. He made an inspired design, the Neutrodyne receiver which achieved commercial success in 1923.

Another solution of the same problem was the insertion of a "screen-grid" between grid and plate. It could be designed to shield the capacitive coupling while offering little obstruction to the desired electron stream. The resulting "tetrode" superseded the triode about 1928 for use in a TRF amplifier. It yielded more gain and avoided the need for neutralization. The screen-grid tube was used in all broadcast receivers until superseded by the transistor several years after the war. It was used in a TRF amplifier or in the IF amplifier in a superheterodyne receiver.

The TRF amplifier was commonly used in a broadcast receiver from 1923 until 1930 when RCA offered to all companies a license under the Armstrong superheterodyne patent. The triode with neutralization was prevalent in 1923-28, then the screen-grid tetrode in 1929-30. Here we shall report first on the neutralized triode and then on our "Uniform Gain" improvements which were useful in either a triode or a tetrode circuit.

### 4.1 The Hazeltine Neutrodyne Receiver 1922-29.

This topic is the centerpiece of my previous book [H/P]. It is susceptible to a narrative order of presentation. It had a beginning, a climax, and an end. Here I aim to give a critical review. Its occurrence marked the beginnings of Hazeltine Corp. and my association therewith.

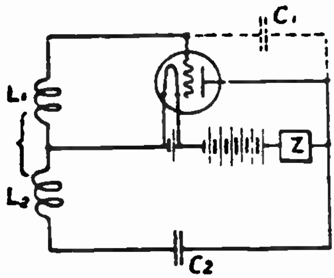
The Hazeltine invention of neutralization happened for a purpose that proved to be trivial. While designing the Navy Receiver (SE-1420) in 1918, he encountered undesired capacitive coupling between a small coupling coil and a large tuning inductor for a low frequency (down to 40 Kc). Only inductive coupling was desired, and the capacitive coupling permitted leakage of interference from a spark transmitter on much higher frequency. He considered a "Faraday shield" in the form of another coil covering the first. Then he perceived that a reverse coil closely coupled with the first would not only decrease the inherent coupling but would also give a reverse coupling that could be designed (by the turns ratio) to cancel the first. Furthermore, this "neutralization" would be independent of the frequency of operation. This invention was found unnecessary in that receiver, so it was left out in the production design.

At the same time, Hazeltine was concerned with the design of a high-gain AM amplifier for code signals, usually peaked at 1 Kc like the most sensitive earphones for reception. He was aware of a feedback problem caused by the grid-plate capacitance of a typical triode vacuum tube. It was not too severe for AF. He later noted that neutralization could be accomplished in either of two arrangements shown in Fig. 6.

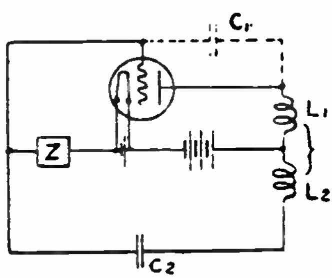
- A neutralizing capacitor between grid and a reverse coil in a transformer in the plate circuit, later termed "plate-circuit neutralization" (PCN);
- A neutralizing capacitor between plate and a reverse coil in a transformer in the grid circuit, later termed "grid-circuit neutralization" (GCN)

His first patent application [P2] was filed 190807 by the Navy patent attorney (E. H. Loftin). It showed one example of a VT amplifier with GCN, with no reference to AF or RF, and no statement of the independence of frequency. The latter was implicit, and was not appreciated as a distinguishing feature of the invention.

Here we may note that we have learned of only one other inventor who conceived this feature about the same time as Hazeltine. That was the famous Raymond A. Heising of Western Electric Co. (and later Bell Telephone Labs.). The Professor learned of that by accident some years later when looking at Heising's notebook in a contest on a different subject. Then Heising told him that it had been abandoned, so they had elected not to contest Hazeltine's

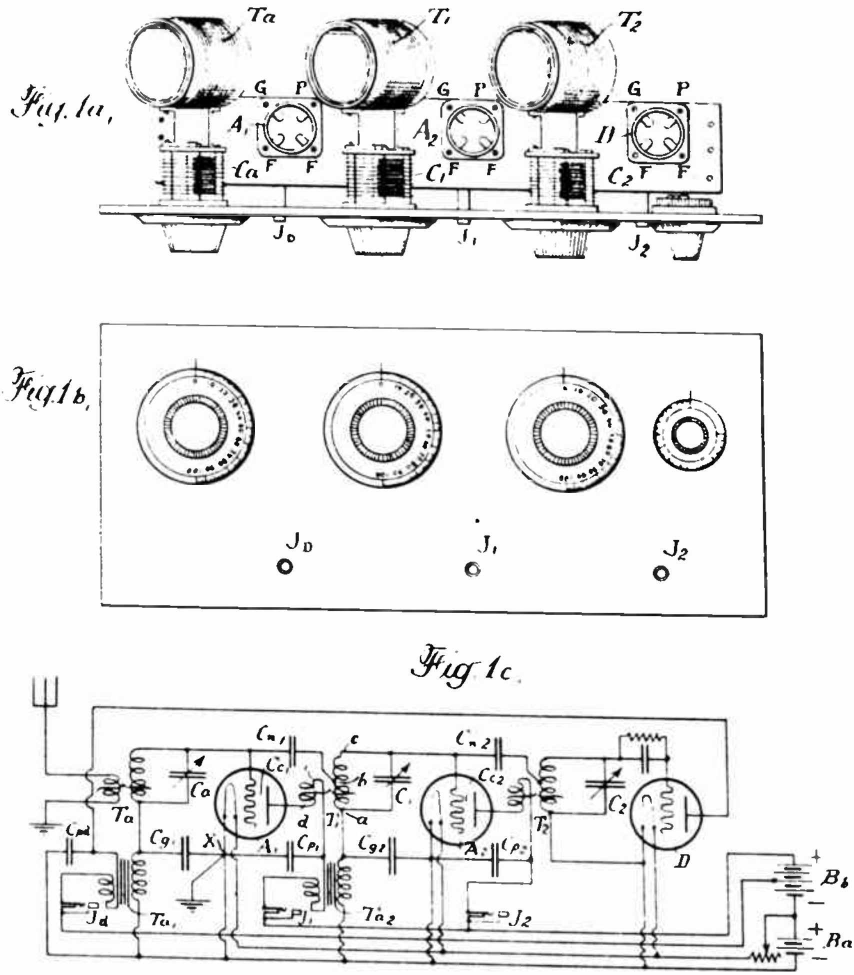



(a) Grid-circuit neutralization.




(b) Plate-circuit neutralization.

4.1 Fig. 6 — Grid-circuit and plate-circuit neutalization, as described in one patent [P4].




4.1 Fig. 7 — The original Neutrodyne receiver, as described in one patent [P6].

patent rights. Heising was primarily interested in an RF power amplifier fixed-tuned in a transmitter.

As Hazeltine learned later, the earliest invention of neutralization was useful for that purpose. The famous Ralph V. L. Hartley (of the same Company) had earlier proposed a reverse magnetic coupling (not capacitive coupling) which was effective when adjusted for one frequency. [1]

Also Hazeltine learned later of another earlier invention which gave "incomplete" neutralization independent of frequency. The famous Chester W. Rice of G. E. Co. had described a circuit that was useful over the frequency range of a tuned receiver. [2] It corresponded to Hazeltine GCN, but without close coupling to the reverse coil.

In later contests, the Courts held that;

- Hartley and Rice were earlier inventors of useful forms of neutralization, and were infringed by the Hazeltine receiver design (using PCN);
- Hazeltine PCN [P3] [P4] was a patentable improvement as used in his receiver design. The scope of PCN was decided to cover some equivalents lacking the refinement of close coupling with the neutralizing coil, or complete neutralization.

Both Hartley and Rice had set out to solve the problem of feedback in a triode RF amplifier, whereas Hazeltine had not.

Hazeltine's second patent application [P3] [P4] was filed 201228 by his personal patent attorney (a former student, Willis H. Taylor, Jr. of PDME). It was a "continuation-in-part" of the first, including the same disclosure and some more. It was the first to show both GCN and PCN for a VT (AF) amplifier. See Fig. 1. Also it showed an RF power amplifier with no reference to tuning or frequency independence.

After World War I, professionals were again joined by amateurs in the progress of radio. Both were oriented to transmitters and receivers. The transmitter used components which had to be purchased. The receiver used a crystal detector or a vacuum-tube (VT) detector. The former could be made at home, while the latter required the purchase of the VT and batteries. The "A" battery was a 6-volt storage battery (like the one used in automobiles). The "B" battery was a series of dry cells in multiples of 15 (22.5 volts). The triode VT had a filament (cathode) heated by the A battery, a plate (anode) connected to the B battery, and an intervening grid (for control).

The common VT receiver used the Armstrong regenerative circuit with the detector. It was extremely sensitive by virtue of RF amplification obtained by critical adjustment near the point of oscillation. With the customary outdoor wire antenna and sensitive headphones, it enabled reception at long distances. The newly organized RCA owned the Armstrong patent rights. A few manufacturers already had licenses but RCA refused to issue more licenses. An amateur used it without a license in his home-made set, or bought one of the licensed receivers, which were very well designed.

I was a licensed amateur (3QK) with a low-power spark transmitter and a crystal receiver. My first VT was a surplus tube presented to me on 210104 by Mr. Kolster who was then head of the Radio Lab in the Bu. of Standards.

Radio telephone broadcasting was born on 201102, Election Day. Westinghouse station KDKA in Pittsburgh was then licensed for broadcasting, the first to be licensed. Its VT transmitter radiated little power, so its range was a few miles to a crystal receiver. Its power was increased in steps. On 210927, using my long wire antenna at home, I was able to hear KDKA on a crystal set in Washington (200 miles).

The phenomenal growth of broadcasting in 1921 and the RCA refusal to license Armstrong regeneration were the two factors which led to an opportunity for Hazeltine.

A few small companies, who were making crystal sets, saw their business increase with broadcasting. Then they saw the threat of oblivion by competition from the few other manufactures with Armstrong licenses. They banded together to form the Independent Radio Manufacturers, Inc. (IRM) in the hope of finding a new opportunity. I do not know the sequence of the events which followed. They engaged as their secretary a patent attorney, Walter Russ, an associate of Taylor in PDME. Taylor took the problem to the Professor in 1922, and they saw an opportunity for his invention to compete with Armstrong regeneration.

Incidental to this sequence of events, was my accidental meeting with Hazeltine on 220829. It is recounted in context of my second summer at the Bu. of Standards. It was there that I told him of the first operation of neutralization (in the form to be known as PCN). I do not know, to what extent my visit influenced his course of action soon afterward.

In the fall of 1922, Hazeltine designed a prospective broadcast receiver using a TRF amplifier with his invention in the form of PCN. He gave a print of his notebook pages to a young engineer, Joseph D. R. Freed of Freed-Eisemann Radio Corp. (one member of IRM). Joe Freed made a receiver from that description, took it to the Professor, and it operated as predicted. This became the second time that he had made an inspired design on paper, which was still unusual. The members of IRM rushed to manufacture this design. With minor changes, it was marketed in the spring of 1923.

Hazeltine's patents on neutralization were licensed exclusively to IRM through the medium of Hazeltine Research Corporation (HRC). The IRM grew to 14 members, then it was closed to further membership.

Fig. 7 is a description of the first model, taken from his subsequent patent application on improvements beyond the neutralization. [P6] The first-generation models were characterized by the 3 like dials on the front panel. These knobs controlled the tuning of the antenna circuit and the 2 stages of TRF amplifier, each with PCN. The one triode in common use was the UV-201, which was soon superseded by the much superior UV-201A. Its price was five dollars, so the Professor used the reflex principle to enable the 2 stages to do the work of 2 RF and 2 AF. The reflex feature proved to be troublesome, so the must common model had 2 RF plus detector plus 2 AF, a total of 5 tubes.

This receiver was trademarked the "Neutrodyne", a name coined by Taylor in the tradition of the "Greek-O-Schenectady" practice of the day. It was an inspired term that implied the peculiar feature of neutralization.

The Neutrodyne receiver was described and demonstrated by the Professor on 230302 before the Radio Club of America at Columbia U. [H4] He became famous for this development.

In 1923, the licensed manufacturers were supported by advice from the Professor. The Neutrodyne was superseding the Armstrong regeneration, not just competing. With the increasing burden of managing the licensing business, and the growing opportunity for expansion, Taylor saw the need for a dedicated organization.

For this purpose, Taylor organized a stock company, Hazeltine Corporation, to supersede HRC and to accommodate expansion. That Company started in business 240201, with stock listed on the Curb Exchange (now the American Stock Exchange or AMEX). It is the subject of another chapter. A laboratory for this Company was started soon afterward, in rented space in the same building with the Professor and the Stevens E. E. Dept. The personnel and functions of that Hoboken laboratory are the subject of other chapters.

The Neutrodyne receiver was the subject of extensive testing and further development, under the Chief Engineer MacDonald and his staff. The second-generation design was a shielded Neutrodyne with 3 TRF stages tuned by one of two dials. The other dial tuned the antenna circuit. It was developed by John Dreyer, working with Ray H. Manson, Chief Engineer of Stromberg-Carlson. Like the previous designs, it was intended to operate on a wire antenna. With its greater gain and its shielding, this design was also adapted to a rotary loop antenna mounted on top.

The Neutrodyne reigned supreme in the years 1923-28. One reason was the RCA refusal to license the Armstrong superheterodyne (until 1930). After 1928, the Neutrodyne was superseded by the screen-grid tube for an RF amplifier (the UX-224).

During the life of the Neutrodyne, there were some developments not directly related thereto. There was a tortuous transition from batteries to AC socket power, which culminated in AC-heated cathodes and rectified AC for plate current. I shall mention some other developments which were directly related and were missed in the early Neutrodyne models:

- Filtering battery leads.
- Coils in cans.
- Unicontrol.
- Antenna coupling tube.
- High-L antenna circuit.
- Uniform gain.
- RF and AF gain controls.

**Filtering battery leads.** Common A and B batteries were used for all tubes. Their use for a high-gain RF amplifier was a new experience. The common leads and resistance of the B battery caused feedback and oscillation in an early 5-tube set. It did not occur in the Professor's original 3-tube set because the reflex AF circuits blocked RF currents in the B battery. Removing the reflex was a refinement which left the other problem. We soon perceived this problem and it was easily solved by a bypass capacitor. It became a more difficult problem for the AF amplifier, but that also was soon solved.

**Coils in cans.** There was a need for shielding the RF coils. Early in 1926, our competitor RFL introduced the practice of putting the coils in cans mounted on a metal chassis. This practice came to the Neutrodyne via Stromberg-Carlson, who were also a licensee of RFL. See the chapter on the Neutroidal Coil. This practice was one of the great advances in technology, which we then adopted.

**Unicontrol** of the several tuners was an obvious opportunity in the Neutrodyne with several dials reading nearly alike. As early as the winter of 1924-25, the Trube "Thermiodyne" receiver (without neutralization) had unicontrol of 3 or 4 tuners, with trimmers on each for fine tuning if needed. The Neutrodyne designs skipped this phase. About 1925, we went to two dials, one for antenna and the other for all RF stages. About 1928, we went to one dial with no trimmers, by using a makeshift of weak coupling to the antenna tuner. In the meantime, we should have considered some better compromises. For example, my notebooks showed several arrangements for unicontrol of all tuners, supplemented by axial motion of the same knob for antenna trimming. This would have required only one hand instead of two, at some slight expense in mechanical design. Mediocrity in this respect persisted until the day of an integrated loop antenna with a superheterodyne receiver (in the mid-1930's).

**Antenna coupling tube.** An alternative to the antenna tuning was the substitution of an RF amplifier tube between the untuned antenna circuit and the first tuner. This is described further in the chapter on Uniform Gain. It left all tuners alike so it was ideal for unicontrol. It was used in 1927-28 but suffered from some kinds of interference and noise so it was deprecated. With other advances that came alone, it might have offered a net advantage. The concept was revived in later years and is now used in the socalled "active antenna" (with a transistor instead of a VT).

**The high-L antenna circuit** was an improvement in weakly coupling the antenna with its tuner. It is described further in the chapter on Uniform Gain. It was introduced by MacDonald, studied by the Professor, and put into our designs for licensees early in 1928. This also enabled unicontrol, and without the addition of an antenna coupling tube.

**Uniform gain** in general, and especially in a TRF amplifier stage, is the subject of another chapter. Without neutralization, it was first used in the Trube Thermiodyne receiver in the winter of 1924-25. We did not appreciate it until we independently developed the concept and engaged in its development. We acquired Trube's patent rights and incorporated it in neutralized TRF amplifiers in 1927-28. That was just before the Neutrodyne was superseded by the screen-grid tube. Before the refinement of uniform gain, we missed some simple opportunities for equalizing the gain and achieving a better compromise in our designs.

**RF and AF gain controls.** The original Neutrodyne had no gain control designed for that purpose. The earphone could be plugged into any one of three jacks, giving none, one or two stages of AF amplifier. It lacked the knob which later came to be designated the "volume control". Previously, there had been no problem of signals too loud. In any case, some misadjustment could be used to reduce the volume. In the Neutrodyne, some judicious detuning met the usual needs. Therefore we were slow to ask the question, what amounts of RF and AF gain would yield the best result.

In the second-generation (shielded) Neutrodyne, the "volume-control" knob reduced the filament temperature of the first tube. From this we learned that we were hearing the random noise from the first tube, and that it actually increased when the filament temperature was slightly reduced. Gradually we arrived at a compromise wherein one knob controlled the RF gain by reducing the antenna coupling and also the plate current in the first tube (but not the cathode temperature).

At the same time, we were coming to appreciate that the detector operated best at a signal level intermediate between weak and overload. Therefore the amount of AF gain was designed to operate the detector at the best level when delivering normal power to the loudspeaker.

It was only after the Neutrodyne that AVC operated to give the proper proportions of RF and AF control. The AVC set the RF gain to give the best level at the detector, and the "volume control" knob could set the AF gain to give the desired loudness from the loudspeaker. Before AVC, we should have provided separate controls for RF and AF gain. I do not know of any receiver design that provided this freedom.

From these retrospective comments, we are reminded of the need for consciously exploring alternatives with an open mind. The following is a particularly relevant example.

The Neutrodyne was a countermeasure for an undesired property of the triode as an RF amplifier. The grid and plate were used as input and output electrodes, with common filament (cathode). The inherent grid-plate capacitance caused feedback which was a handicap in an RF amplifier. If instead the grid had been used as the common electrode, it would have acted as a shield between input and output electrodes (cathode and plate). Then a stable RF amplifier would have been possible (but with less gain per tube). This was proposed early, but was not appreciated until World War II (the "lighthouse" triode for higher frequencies).

This concludes my critical review of the Hazeltine Neutrodyne receiver, from beginning to end. It enjoyed an auspicious beginning in the inspired initial design by the Professor. The engineering services to our licensees (the IRM) were a credit to the Hoboken lab staff and were appreciated by those companies. We became close friends of their engineers, some of whom were members of our staff at earlier or later times. It is a sobering exercise, to recognize now some opportunities we missed or even resisted at that time. If we had one fault, it was our striving for maximum performance without giving due weight to the benefits obtainable by compromise.

[A] [H/P] [W1] "Hazeltine the Professor", Hazeltine Corp.; 1978.

[B] Figs. 6 and 7 are taken from [P4] and [P6].

[C] [P2] [P3] [P4] [P6] U.S. Patents to Hazeltine, see [H/P].

[D] The Neutrodyne receiver was designed for the broadcast band of 550-1500 Kc, which was standardized in the period 1922-24. Channels were assigned on multiples of 10 Kc.

[E] J. F. Dreyer, Jr., "Solving the problems of the Neutrodyne", Proc. Radio Club of Amer., vol. 3, no. 3, 7 pp.; May 1924. (Overlooked in [H/P] references.)

[F] H. T. Friis, A. G. Jensen, "High frequency amplifiers", BSTJ, vol. 3, pp. 181-205; Apr. 1924. (Overlooked in [H/P] references.) (TRF triode amplifier, shows Hazeltine GCN with credit to Rice.)

[G] R. V. L. Hartley, U.S. Pat. 1,183,875; 1918. (Neutralization by magnetic coupling, critical to frequency.)

[H] C. W. Rice, U.S. Pat. 1,334,118; 1920. (Incomplete neutralization, without close coupling between the reverse coils.)

## 4.1 The Hazeltine Neutrodyne Receiver 1922-29

[1] J. F. Dreyer, Jr., R. H. Manson, "The shielded Neutrodyne receiver", Proc. IRE, vol. 14, pp. 217-247; Apr. 1926. Discussion by L. A. Hazeltine, vol. 14, pp. 395-412; Jun. 1926.

### 4.2 My Capacity-Bridge Neutralization 1922.

A tuned radio-frequency (TRF) amplifier using a triode vacuum tube needed neutralization of its grid-plate capacitance. Prof. Hazeltine and I had independently invented one form of neutralization for this purpose, which came to be known as "plate-circuit neutralization" or the Neutrodyne. On 220829 during my first meeting with the Professor, I learned of his priority to this invention. Our second meeting was in Washington 221103-04.

On 221105, the day after our second meeting, I invented another form of neutralization, which came to be known as the "capacity bridge". It was one of several forms which were invented around that time. It came to have special significance to me for two reasons:

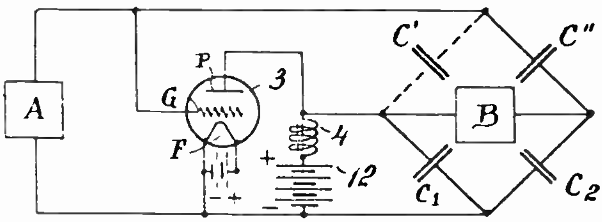
(1) It was a significant consideration in my agreement with Hazeltine, which led to my career in Hazeltine Corp.; and
(2) It was the subject of the first of my many U.S. patents.

Fig. 1 shows the two forms of capacity-bridge neutralization (CBN) with respect to a triode vacuum tube (VT). By analogy with the two Hazeltine alternatives, they may likewise be termed (a) PCN for "plate-circuit neutralization" and (b) GCN for "grid-circuit neutralization". They were natural alternatives in applying the same concept. I promptly showed them to the Professor and he was much impressed that I had come up with a workable alternative to his invention. Only later did we learn that they were both descended from a common line.

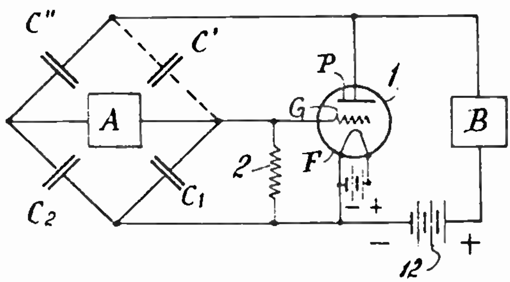
He pointed out to me, one peculiarity in which my CBN fell short of his transformer circuit with close coupling. He obtained true neutralization of the grid-plate capacitance. By comparison, I obtained neutralization of the coupling between the tuned circuits associated with the grid and plate. My neutralization left some untuned feedback by the grid-plate capacitance, which had the effect of a shunt conductance on the grid (for PCN) or on the plate (for GCN). Either was undesirable because it would decrease the available gain per stage.

On 230203-04, in my lab at home, I operated my CBN in both GCN and PCN. On 230731, I described my CBN to our summer group (Hazeltine, Poole, Dreyer) in one of our regular meetings. At the request of the Company (Mr. Pierson, then President) I built two receivers at my home and sent them to the Hoboken lab:

240428 Completed one receiver with 2 TRF stages using CBN (GCN) on UV-199 triodes (a small tube for dry-battery operation);
240512 Completed another receiver, same except UV-201A triodes (the prevalent type).




(a) Plate-circuit neutralization.




(b) Grid-circuit neutralization.

4.2 Fig. 1 — Capacity-bridge neutralization, as described in my patents [A].

I demonstrated these at the Hoboken lab on 240801 to a Company group (Pierson, MacDonald, Ackerman, Taylor, Hazeltine). They served their purpose in proving the usefulness of my CBN.

On 240922, nearly two years after my invention, we filed a patent application on my CBN. A few months later, for some reason that was not apparent, we filed a "continuation-in-part", in which we claimed the "output bridge" (PCN) form. The latter application issued after 5 years, with claims to that form. I do not recall any contest in that case, although 5 years was an excessive lapse of time in the Patent Office. It was the first of my many U.S. patents.

In the meantime, the "parent" application became involved in an "interference" declared by the Patent Office among a number of copending applications, each of which supported the same assigned set of claims. In this interference, I was one of four applicants:

- Wheeler (Hazeltine Corp., attorney Guild);
- Ballantine (Radio Frequency Labs.);
- Miller (Atwater Kent);
- Round & Willans (British assignors to RCA).

Each one had an opportunity to prove that he had first conceived the invention and/or had met some other criteria so he was entitled to a patent. My depositions were taken 290917-20 at PDME, 165 Broadway. My testimony was corroborated by my father and my attorney Elmer Stewart. Stuart Ballantine attended. Dr. John M. Miller was a famous scientist who had published the first analysis of the feedback problem to which neutralization was directed. Capt. Round was a famous British inventor from World War I.

I do not have a record or a clear recollection of the scope and outcome of that interference. My second patent issued a few years later. It did not contain broad claims to CBN, or to the "input bridge" (GCN) form. Therefore I infer that another party (probably Ballantine) proved earlier conception of CBN in that form, and was issued a patent with broad claims thereto. My patent retained claims to GCN with restrictions to some relations that I had described as preferred, and also to some other associated features.

In summary, my first invention to reach the Patent Office and to yield a U.S. Patent was valuable to me in various ways. It gave me experience with the game of inventing and patenting. So far as I know, it has never been accorded the compliment of practical use, although it was "useful". In that respect, it may be classified with most of my inventions and patents.

[A] H. A. Wheeler, U. S. Patents.
Pat. No. Issued/Filed
(1) 1757494 300506/250227/cont/240992
(2) 1896500 330207/240922

Note (1) Neutralization by capacity bridge in output circuit (PCN).

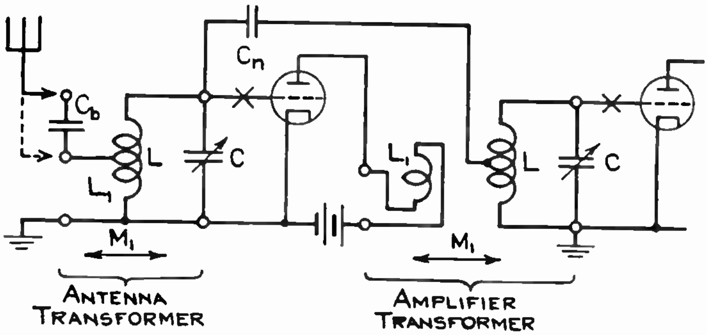
Note (2) Neutralization by capacity bridge in input circuit (GCN) with some restrictions.

**4.3 Uniform Gain 1924.**

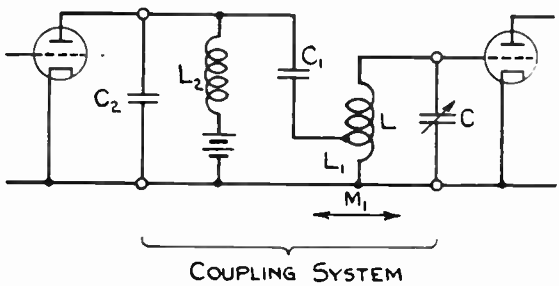
This topic has unusual interest from several viewpoints, so I am presenting it in some detail. It was my first intensive activity after the Company was founded. It became a major factor in carrying the Company's royalty base through the transition from the Neutrodyne to later developments (around 1929-31). In the Hoboken lab, during a period of two weeks in the summer of 1925, I independently invented the basic feature and every variation that became significant. In the following winter, while I was at Hopkins, the Professor became aware of the earlier work of his former student, Carl E. Trube. The Company acquired his rights and filed a patent application under guidance of the Professor, who did not have full knowledge of my work. The most significant claims in that case were later lost to Kolster (and then assigned to the Company) and to other parties, so it was never published in U.S. (only as a British patent). The untimely death of Trube in 1927 may have been a factor in the loss of some of his claims, by weakening our proofs. Some of my inventions anticipated some attributed to Trube and the high-inductance antenna circuit attributed to MacDonald. Outstanding designs were made in 1928 by Jackson H. Pressley in our lab. The later form which achieved widest application was in my first notes and was never claimed. There was much deficiency in cooperation among all parties in the Company and our attorney's office. I do not know how much was attributable to my lack of diligence, but I was involved in various stages. My 11 related patents were mostly specific to trivial variations. The technical perspective on the Company's activities was well presented in my IRE paper of 1930-31. So what was this development?

The subject of Uniform Gain is here taken to comprise the various countermeasures addressed to a problem encountered in the original Neutrodyne receiver. The problem was the large variation of amplification or gain with tuning over the broadcast frequency band (550-1500 Kc). This problem was most severe in the TRF amplifiers which antedated the superheterodyne. Also it was severe in the design of antenna tuners for a wire antenna, until superseded by the integral loop antenna. Therefore this subject is concerned most with the unicontrol TRF receivers which were most common about 1927-31.

The Professor had elected the rotary capacitor (C) as the variable for tuning each circuit to any channel in the broadcast band. A fixed inductor (L), as the other part of each tuned circuit, was easy to make with low loss in a convenient size comparable with that of the capacitor. The capacitor had much less loss, which did not depend on its size. He mounted the inductor (coil) on the frame of the capacitor to form a modular tuner. One was used for the antenna and each RF amplifier stage. The reactance of each part was proportional to the frequency of tuning. With nearly uniform loss power factor

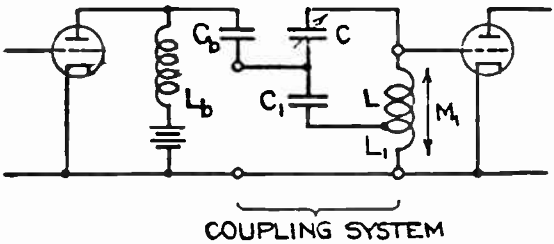



(a) Constant-ratio transformers of the Neutrodyne receiver.

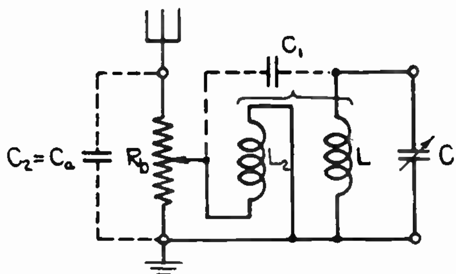



(b) The first variable-ratio transformer by Trube, without neutralization.

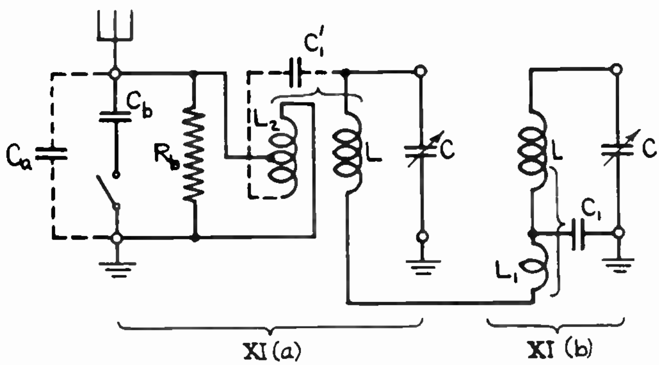
4.3 Fig. 1 — Elementary RF transformers to illustrate the voltage ratio.




(a) The basic interstage transformer by Trube, without neutralization.

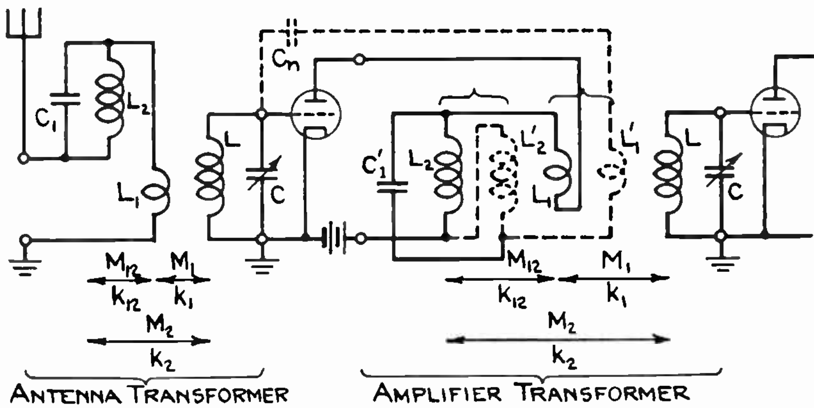



(b) The high-inductance antenna circuit.

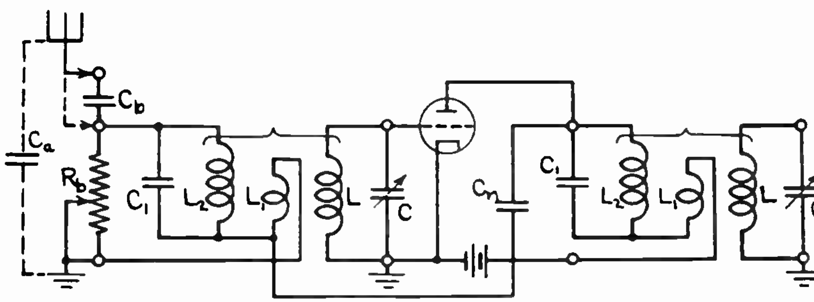



(c) A combination of these in a double-tuned antenna circuit, as in Philco 95.

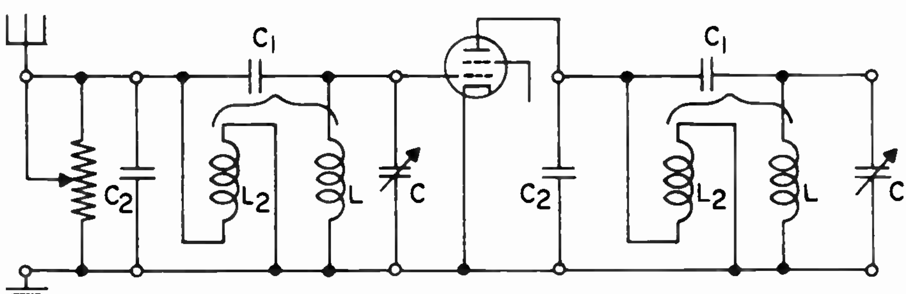
4.3 Fig. 2 — Variable-ratio transformers with addition of two parts.




(a) Design by Hazeltine, for triode with plate-circuit neutralization.

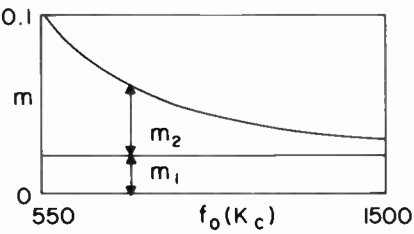



(b) Design by Wheeler and Pressley, for triode with grid-circuit neutralization.

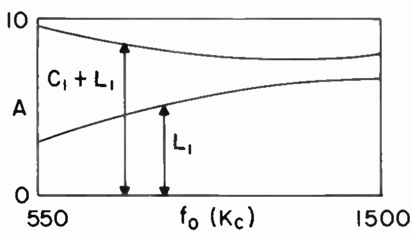



(c) Design for screen-grid tetrode.

4.3 Fig. 3 — Variable-ratio transformers (antenna and interstage) with inductively coupled low-frequency primary circuits.




(a) The variable ratio made by adding two parts.




(b) Uniform gain made by contributions of two parts.

4.3 Fig. 4 — Graphs of operation over tuning range, Fig. 2(a).

(p = 1/Q), the parallel-resonant impedance was therefore proportional to frequency. Using such a tuner in 3 simple transformers left a great variation of overall gain. The gain was less at lower frequencies, where the antenna also was less efficient. It was greater at higher frequencies, where it was more difficult to prevent oscillations from a small amount of overall feedback. This was the problem.

Figs. 1-4 illustrate the principal concepts and developments relating to uniform gain. They are taken from my comprehensive paper presented in 1930 in Toronto and published in Proc. IRE in 1931. [A]

Fig. 1(a) shows for reference the simple TRF transformers used in the early Neutrodyne receivers. The simple transformer had a fixed ratio, which left the problem of gain variation over the frequency range of tuning. The amplifier stage had "plate-circuit neutralization" (PCN). The screen-grid tube was not yet available.

Several techniques were learned or devised for reducing or solving this problem. The more significant ones, and some related topics, will be discussed under these headings:

- The antenna coupling tube.
- The high-inductance antenna circuit.
- Variable-ratio coupling circuits.
- Applications with neutralization.
- Applications with the screen-grid tube.
- A historical review.
- Applications to oscillators.
- The three-coil transformer.

The time period is about 1924-32. The principal features were fully described in a paper I presented in Toronto 300820, which was published in 1931.

The antenna tuner presented the more difficult problem, for several reasons:

- The efficiency of the wire antenna was less at lower frequencies, because its length was a smaller fraction of the wavelength;
- The antenna was mainly capacitance (rather than resistance) so it became a part of the tuned circuit;
- As such it affected the tuning range and the tracking with the other (amplifier) tuners (as for unicontrol);
- The antenna was not standardized, so a wide range of size had to be tolerated.

One ingenious alternative was the avoidance of this problem by the provision of an extra tube between the antenna and its tuner.

**The antenna coupling tube** was proposed to our licensees early in 1927. The idea may have come from MacDonald. He had a patent application filed 250526, which included this idea in an unusual (reflex) arrangement. The antenna was coupled to the first tuner through an extra tube instead of transformer coupling or a direct connection. This tube and the first tuner became another TRF stage and the antenna was not tuned. This stage could be neutralized, like the others. It tracked the others so it was ideal for unicontrol.

Connecting the antenna directly to the grid of the coupling tube was "efficient" for a short wire, whose capacitance may have been little greater than the tube capacitance. It did not make best use of the greater capacitance of a long wire.

The coupling tube added much thermal noise, as compared with the coupling circuit. Some size of antenna would have picked up external radio noise of about the same strength, so the tube noise would have been unimportant. In recent years, this concept has been exploited in a later version of the antenna coupling tube. It is the socalled "active antenna", made of a small antenna connected to a transistor. The FET operates in the manner of the old triode, but contributes much less noise.

In 1928, I used at home a Howard "Green Diamond" Neutrodyne model which had the antenna coupling tube and one dial for unicontrol of the 3 tuned circuits. I used an indoor wire antenna.

The antenna coupling tube was sensitive to a kind of interference termed "cross-modulation". All signals reached the first grid, regardless of frequency selection in the followng tuners. This was a form of distortion by the "nonlinear" behavior in the coupling tube. This fault was perceived only in receivers located near a transmitter, but it led to the rejection of this feature in later models. The later "variable-mu" tube would have greatly reduced this problem, but I do not recall any revival of the antenna coupling tube.

Today I can see how the antenna coupling tube should have been designed and operated. Then it might have survived for unicontrol until the wire antenna was superseded by the integral loop antenna in the 1930's.

- The antenna wire should be long enough to reach outside the shielding and/or electrical noise in the building.
- The antenna wire should exceed a specified minimum length (say 10 ft).
- The first grid (including PCN) in parallel with the minimum antenna should be resonated at the low end of the tuning range by a parallel small coil of high inductance.
- A high resistor should be connected in parallel, with a value just low enough to widen the peak of that resonance.
- Another resistor (without bypass capacitor) should be connected in series with the cathode of the first tube.
- An RF gain control should be provided by one knob causing opposite variation of these two resistors.
- In later years, when it became available, the variable-mu screen-grid tube should be used as the first tube.

The result of this design would be:

- Maximum gain from a small antenna, especially at lower frequencies where most needed; and
- A gain control which would combine two functions to reduce cross-modulation, namely, decrease the voltage on the first grid and decrease the distortion in the first tube.

Why did we not think of these improvements, as a useful package?

**The high-inductance (high-L) antenna circuit** was another approach to the same problems peculiar to the antenna tuner. It was an example of the "variable ratio" concept which was the main line in developments toward uniform gain. It was a variant of my first inventions in mid-1925. MacDonald recommended the simplest form, on which he was awarded an "improvement" patent. Hazeltine advised a further refinement, on which he also received a patent.

In the original Neutrodyne design, the antenna was connected to a fixed-ratio transformer in the first tuner. The antenna coupling was much more efficient at higher frequencies, and had much more effect on the tuning of the first circuit and its tracking with the other circuits. See Fig. 1(a).

Fig. 2(b) shows the high-L antenna circuit ($L_2$) which provided fixed resonance of the antenna at a frequency below the tuning range. It was moderately coupled with the tuner (L). The result was more antenna coupling at lower frequencies, where it was needed, and less at higher frequencies, where it could be detrimental.

The high-L antenna circuit, in its simplest form, was simply a rather small multilayer coil of many turns, located coaxially in or near the grid end of the tuner coil. It was reversed so the incidental capacitive coupling ($C_1$) would add to the inductive coupling. To provide for an antenna of small capacitance, a fixed capacitor could be added in parallel to insure that the frequency of resonance would be below the tuning range. See Fig. 2(c).

The high-L antenna circuit could be shunted by a variable resistor as a manual gain control (all or part of the "volume control" function). If the resonance with a small antenna were to fall within the tuning range, this shunt resistor could protect the tuner against the otherwise excessive reaction. These features were found useful.

The high-L antenna circuit was recommended by MacDonald to our licensees around the winter of 1927-28. It marked the transition to unicontrol tuning in Neutrodyne receivers. Until then, a separate dial for antenna tuning had been thought necessary to accommodate different lengths of wire. (An accessible trimmer on the antenna tuning would have enabled unicontrol of all tuners, in the manner of Trube's designs in 1924-25.)

**Variable-ratio coupling circuits** offered the best approach to uniform gain. They were devised first for an interstage tuner in an RF amplifier. Later they were adapted to the antenna tuner, whose performance was influenced by different factors. The concept was a variable-ratio transformer or equivalent, with its ratio varying with tuning over the broadcast range (550-1500 Khz). Therefore the ratio variation would be designed to track the rotation of the tuning capacitor. Mechanical tracking of some circuit components was one way, but the mechanical linkage was resisted by electrical designers accustomed to wiring separate components. Electrical tracking was devised, once the problem was appreciated.

The first record of the variable-ratio concept dates from the early work of Carl E. Trube, a former student of the Professor (Stevens class of 1921). The words "varying the ratio" are found in his first patent, on an application filed 240109. [B] (These words might have been added sometime before it issued 300610.) He described one way of tracking a variable ratio, by connecting in series two rotary capacitors on one shaft. Together they served as the tuning capacitance across a fixed inductor. He showed a triode amplifier, in which the ratio variation with tuning could be used to control the variation of amplification or gain per stage. For example, the latter could be made nearly uniform, hence the term "uniform gain".

Here we designate the ratio that may be variable:

$$m = \frac{\text{primary voltage (on preceding plate)}}{\text{secondary voltage (on succeeding grid)}} < 1$$

This ratio is a measure of the amount of coupling from the plate current to the tuner which delivers its voltage to the next grid. For uniform gain, it should vary inversely with frequency.

Trube had also another motivation for his circuits. His employer did not have a license to use the Neutrodyne, so he was handicapped by the feedback in the triode. The company was the Shepard-Potter Co. of Plattsburg, N.Y., which in 1924 became the Thermiodyne Radio Corp. Oscillation in a TRF amplifier was known to be caused by the plate circuit becoming an "inductive" impedance under some conditions, such as a slight detuning. Trube proposed to include in the plate circuit some "capacitive" impedance, so the net impedance would never become inductive (or not too much so). This was the reason he proposed the two capacitors in the primary voltage divider (plate circuit).

In 1924 and early 1925, Trube designed three receivers that were sold under the trademark Thermiodyne. They featured TRF amplifiers using the UV-201A triode. In the first one, he did not include his variable ratio of two capacitors. He used the circuit of Fig. 1(b). It included a fixed ratio of two capacitors and a tapped inductor. A parallel high inductance ($L_2$) introduced a different concept for variable ratio. It provided in the primary circuit a fixed resonance at a lower frequency, somewhat below the tuning range. As a result, the voltage ratio (m) was about doubled at the lowest frequency of tuning. This was the earliest use of the primary resonance below the band, by means of high L in the primary circuit. Here the ratio variation did not directly involve the tuning C, but did track with frequency of tuning.

In the second and third of the Thermiodyne receivers, Trube used an improvement over his first idea of two capacitors. It was the circuit of Fig. 2(a). A fixed capacitor ($C_1$) in series with the tuning C gave one component of voltage ratio that was much greater at lower frequencies. By the addition of a few turns ($L_1$) at the end of the tuning coil (L), a fixed component was added to the variable component to give the total voltage ratio (m). This introduced the freedom of designing for any amount of variation between the extremes of tuning. This became the principal concept behind uniform gain. Trube was then able to design for whatever amount of gain that was usable without neutralization. His third receiver was the first to include the variable-ratio concept also in the antenna circuit.

Incidentally, all the Trube receivers featured unicontrol of all tuners, the antenna and 2 or 3 amplifier stages. Also each tuner was provided with a trimmer knob for fine tuning. (This is what we should have done in the early Neutrodyne designs.) In his third receiver, the similarity of all tuners was helpful in their tracking for unicontrol.

In summary, Trube had introduced in two forms, the concepts that were to be prominent in the development of uniform gain, namely:

- The low-frequency resonance in the primary circuit; and
- The combination of two components of coupling to give any amount of variation of the coupling ratio over the tuning range.

In the summer of 1925, at the Hoboken lab, I started work on the problem of uniform gain. From 250807, in a period of two weeks, I devised various circuits for obtaining a "variable ratio", and I used this term from the first day. I was not familiar with the Trube receivers, which had recently appeared on the market. I had no way of knowing the contemporary unpublished work of Kolster. The patent rights of both were later acquired by our Company. It was my work which alerted us to the opportunities of the variable ratio. Furthermore, I described its use with either form of neutralization, PCN (plate-circuit neutralization, as used in the Neutrodyne) or GCN (grid-circuit neutralization, also invented by Hazeltine). Early in 1926, on learning of Trube's earlier work, the Professor became fascinated with the subject, and made designs for its use with PCN..

Fig. 4 shows in graphs the concepts peculiar to variable ratio and uniform gain. They are based on the simplest form, Fig. 2(a), as used by Trube in his third receiver. Fig. 4(a) shows the variable ratio (m) as the sum of two parts, which can be proportioned at will. Their sum is a measure of the amount of coupling from the plate current to the tuned circuit, which may be needed and realized. Fig. 4(b) shows the resulting nearly uniform gain. It is expressed in terms of the voltage ratio (A) from one grid to the next. It depends on the coupling and other factors.

Figs. 2(b) and (c) are antenna circuits, unrelated to the kind of RF amplifier. The former (b) is the high-L form recommended by MacDonald and Hazeltine in the winter of 1927-28. The latter (c) is an adaptation of (a) and (b) to a double-tuned preselector, which I used in the Philco 95 in 1927. (The double-tuned preselector was needed to prevent cross-modulation in the sharp-cutoff UX-224 tetrode with AVC bias. It was not needed with the successor UX-235 featuring gradual cutoff or "variable mu".)

Fig. 3 shows some of the preferred forms which were adapted to antenna circuits and either neutralized triode or screen-grid tetrode. They have in common a high-L primary resonant at a low frequency and inductively coupled to the tuner. The latter feature gives them the versatility desired for the three applications. The last one (c) is the simplest of my original configurations. It corresponds to the high-L antenna circuit of Fig. 2(b).

**Applications with neutralization** were prevalent just before the triode was superseded by the screen-grid tube in a TRF amplifier. They fall in two classes:

- Grid-circuit neutralization (GCN) was involved least with the plate-circuit complications which were peculiar to the variable ratio. Hence I paid most attention to GCN and it was used most.
- Plate-circuit neutralization (PCN) had been preferred by the Professor. Hence he made an elegant design for variable ratio with PCN. It was the first to be used (in a Gilfillan receiver late in 1926) but was superseded by the simpler GCN.

Both kinds of neutralization were adapted to the variable-ratio transformer having low-L and high-L primary coils ($L_1$, $L_2$). Fig. 3(a) shows the Professor's design with PCN. Fig. 3(b) shows the simpler design for GCN. The latter avoids the bifilar duplication of both primary coils. Two varieties of the latter were designed in the Hoboken lab. My design located the low-L coil ($L_1$, used also for GCN) inside the main coil (L), and the high-L coil ($L_2$) outside or at one end. The other design (by Pressley) located the low-L coil outside (over the entire length of L) and high-L coil inside. Both designs were economical and effective. Both were designed for enclosure in shield cans, which had become the common practice.

With these designs, we achieved about the best compromise that was possible with the triode TRF amplifier. It was a major advance over Trube's pioneering designs, and bore little resemblance in circuit details. Unlike the Trube receivers, the tuning capacitors were all grounded and hence could be mounted on one metal shaft for unicontrol. Refinements in manufacture had enabled close tracking so trimming knobs were not required or used for all the RF stages. A separate dial was used for the antenna tuning.

**Applications with the screen-grid tube** in a TRF amplifier were characterized by these changes:

- Simplification and greater versatility by the omission of neutralization; and
- Higher primary impedance (by greater coupling ratio) and more gain.

Figs. 3(a) and (b) could be adapted by merely removing the neutralization circuit ($C_n$). Fig. 3(c) was a further simplification which came to be preferred for both antenna, as in Fig. 2(b), and RF amplifier. It was one circuit I favored in my first work in mid-1925.

**A historical review** is given here, for relating the various contributions to the subject of uniform gain. Trube and the Hoboken lab became the leaders in the field, with respect to inventions, practical applications and exposition. However, several contemporary inventors gave us real competition in the timing of their work. They were Kolster, Loftin & White, and Roberts of RCA.

The following chronological outline refers to the variable-ratio circuits. A triode stage without neutralization is presumed unless otherwise indicated (as by A for antenna circuit, also GCN or PCN for neutralized triode, or screen-grid tetrode).

| | |
|---|---|
| 240109 | Trube's first patent application. "Varying the ratio" by tuning C and another C on the same dial. Plate connection direct or by inductive coupling (transformer). Low-frequency resonance in primary circuit (not clear). |
| 2412XX | Trube's first "variable-ratio" receiver being manufactured. Transformer ratio varying by low-frequency resonance in primary circuit by $L_2$. Fig. 1(b). |
| 2503XX | Trube's receiver with $(C+C_1+L_1)$ being manufactured. Variable ratio by sum of two parts for uniform gain. Fig. 2(a). |
| 2504XX(A) | Same, used also in antenna circuit. |
| 250807(A) | Wheeler's independent conception of nearly all forms, entered in his notebook within 2 weeks. The first to include neutralization, showing both PCN and GCN. The simple high-L primary finally used with the screen-grid tube, Fig. 3(c). |
| 250827 | Kolster's application. Transformer has variable ratio in two parts for uniform gain. One part has low-frequency resonance. Fig. 3(a). |
| 260608(A) | Loftin & White paper received at IRE. Fig. 2(a). |
| 260615(A) | Hazeltine's report (213) based on information from Trube. Presented at IRM meeting in NYC. Transformer has variable ratio in two parts for uniform gain. One part has low-frequency resonance. Added PCN. Fig. 3(a). |

| | |
|---|---|
| 260630(A) | L&W paper presented at IRE meeting in NYC. Fig. 2(a). |
| 260702(A) | Trube's second application. The circuits of his receivers made in early 1925. Not issued in U.S. so presumed lost in interference in Patent Office after Trube's death. Issued as British Patent 273639. |
| 260820(A) | Wheeler's report (230) on variable-ratio designs with high-L primary and PCN. Included an improvement over Hazeltine's design. Without PCN, that one was later the type used most with screen-grid tubes. Fig. 3(c). |
| 2610XX(A) | L&W paper published in Proc. IRE. Fig. 2(a). |
| 2706XX | Trube died. |
| 2712XX(A) | MacDonald's report (311) on high-L antenna circuit. (May have been presented to IRM meeting 271102.) Fig. 2(b). |
| 280525(A) | MacDonald's patent application on high-L antenna circuit. Fig. 2(b). |
| 28XXXX(A) | Pressley & Wheeler made designs for uniform gain with GCN. They were manufactured in 1929 just as the triode was being superseded by the screen-grid tube for an RF amplifier. Fig. 3(b) and 3(c) plus GCN. |
| 280714 | Trube's third application (a continuation-in-part of his second) issued in U.S. 300610. Principal claims were later lost to Kolster. Fig. 3(a). |
| 290620(A) | Wheeler's first patent application, with GCN. Fig. 3(b). |
| 290803 | Pressley's patent application, with GCN. Fig. 3(b). |

From this chronological abstract, several conclusions seem to emerge:

(1) Trube was the pioneer in the concept of a "variable ratio" in a TRF circuit, especially in one tuned by a rotary capacitor. By variable ratio, we mean an effective transformer ratio which varies with the frequency of tuning by virtue of electrical reactance.

(2) He was the first to put this concept in use. He used the simplest type, with series C and a few turns of extra coupling, as in Fig. 2(a). He used it in both antenna and interstage tuners, but without neutralization.

(3) Independently I conceived all the varieties of variable-ratio circuits that have appeared in the literature. I did this in two weeks in the summer of 1925. I adapted them to antenna circuits and to a triode with GCN and PCN. I explored alternatives and led the activities of the Hazeltine labs in making useful designs, including neutralization. I prepared a comprehensive practical and theoretical summary for presentation and publication in 1930-31.

(4) Kolster devised one of the useful circuits about the time I was starting, but without neutralization. His filing date was a year ahead of Trube's second application, which was filed by his assignee (Hazeltine Corp.) and claimed similar circuits. Under circumstances that I do not know, and at a time several years after Trube's death, his assignee relinquished his claims that were similar to Kolster's, and acquired rights under Kolster's patent.

(5) Loftin & White were apparently unaware of Trube's receivers on the market, and later published a similar circuit for the same purpose. They proposed it for the unneutralized amplifier and the antenna tuner. They were the first to publish the concept of "variable ratio" as here understood.

(6) Around the time of their publication, was filed Trube's second application, which claimed these circuits. Trube lost his claims thereto, presumably in a contest in U.S. Patent Office. The interference proceeded about 1931, several years after his death, so we were handicapped in establishing proofs of his priority. No U.S. patent was issued on these claims, but a British patent on the same application did issue promptly.

(7) MacDonald promoted the early use of a high-L antenna circuit similar to mine, and it was commonly identified with his name. He was encouraged by some theoretical studies by Hazeltine. Both were issued "improvement patents" on this subject.

(8) The practical use of variable ratio with the triode was based on designs with GCN, made by Wheeler & Pressley in the Hoboken lab in 1928.

(9) After the triode was superseded in 1929 by the screen-grid tube, my circuits (rather than those of Trube, Kolster, Loftin & White) were used in TRF amplifiers and antenna circuits.

The Trube British patent deserves special attention because its corresponding U.S. application on 260702 was the first release based on his Thermiodyne receivers of early 1925. They were the first use of the variable ratio made of the sum of two parts as in Fig. 2(a). It was used for both antenna circuit and triode TRF amplifier (without neutralization). It was the basis for the acquisition of Trube's rights by Hazeltine Corp. in the spring of 1926. This application was filed by the assignee with the benefit of intervening studies by the Professor and myself. I have few notes and no clear recollection of its disposal. Claims to this arrangement must have been made and then lost in an interference in the U.S. Patent Office. A contiuation-in-part was filed later but did not show this arrangement. We might wonder whether Trube was entitled to those claims, and might have prevailed if he had lived to support the best proofs. Some delays in filing related cases (up to several years), and some other circumstances, suggest that we may not have been aggressive in the prosecution of applications relating to the variable ratio.

In my diary, I have fragmentary notes of a Trube interference in which I made a deposition in New York, a few days in the perod 310709-310805. Our adversary was RCA, whose experts were the distinguished engineers, John V. L. Hogan and Dr. Walter Roberts. I infer that Trube lost, because no patent issued to him after that date. Roberts was a prominent inventor in our field, so he may have been awarded the claims we lost. That was 7 years after Trube's invention and 4 years after his death. His invention was no longer needed so much for a TRF amplifier, but found some other applications.

Another note in my diary refers to an interference between Trube and White, which was in progress 330120. I do not know what was its outcome.

Also notable is the number, character and timing of my 11 patents in relation to the related patents of Trube, MacDonald, Hazeltine and Pressley. Most of my patents claim trivial variations. My first application (290620) was substantial in its coverage of GCN in a triode circuit as in Fig. 3(b). However, its filing date was already 3 years after my first inventions which anticipated all the other work by our team. Here again we might infer a lack of diligence in claiming our rights, and I do not recall to what extent I may have shared the responsibility. Our time and energy were shared by many activities.

**Applications to oscillators** occurred in various situations. The oscillator is essentially an amplifier with its output circuit coupled to its input circuit. In the triode, the variable ratio may be effective between the plate and grid of the same tube. I shall mention two applications, which relate to other chapters herein:

- Our first standard-signal generator; and
- The local oscillator in a superheterodyne receiver.

In a standard-signal generator, the first component is the RF carrier generator. It is simply an RF oscillator. I wanted uniform output over the tuning range (the broadcast band). It was 1928, 3 years after my invention of the variable ratio for uniform gain, so it was natural for me to use this feature in the oscillator. A negative ratio was required for oscillation. I selected a circuit otherwise equivalent to Fig. 3(c). It was easily designed for uniform voltage across the tuned circuit (L). That was coupled by a balanced coil to the following stage, which was a balanced modulator.

In 1930, we became active in designing superheterodyne receivers. Such a receiver requires a "local oscillator" and "modulator" for "heterodyne" frequency conversion from the signal frequency of tuning down to a fixed "intermediate frequency" (IF). The signal and oscillator tuners had to track with a (nearly) constant frequency difference. The circuit for such tracking turned out to be especially suited for the use of a variable ratio in the oscillator.

The tracking circuit included a large fixed capacitor in series with the tuning capacitor of the oscillator. I saw the similarity with $C_1$ and C in Fig. 2(a) so we immediately adopted that circuit for variable ratio in the local oscillator. It was easy to proportion the circuit for unform voltage in the oscillator, which was desired for the modulator.

Then we wanted to integrate the oscillator function into one tetrode with the modulator function. The best performance required a self-regulated oscillator which would not draw grid current. The variable ratio of the same type enabled a design with uniform amplitude, which was ideal for self-regulation. This was the first well behaved integrated oscillator-modulator in one tube. It was soon superseded by a specialized tube design from RCA, called the "pentagrid converter". The variable ratio offered an advantage in a local oscillator in any context.

**The three-coil transformer** is a topic incidental to neutralization and uniform gain. One may desire zero "leakage inductance" in one of the three coils, even if not in the other two. The reason is beyond the present scope.

In my notebook 260722, I derived a simple rule for designing three coupled coils so that the intermediate one would have zero leakage inductance. I included it in my application filed 290620, as incidental to a variable-ratio transformer for GCN. At an intermediate date (271031) the Professor included the same rule in a divisional application. He may have independently derived the same rule.

Let us identify the intermediate coil (sub-2) and the other two coils (sub-1, sub-3) and state the coupling coefficent (k) of each pair. If $k_{13} = k_{12}k_{23}$ the intermediate coil has zero leakage inductance. This condition can be satisfied by design in various configurations. It is approximated by three coaxial coils of similar shape. Hazeltine used flat coils, rather close. I used concentric solenoids with different radii.

Beyond our patent disclosures and my 1930-31 IRE paper [A] I do not recall any publication of this rule or the concept of its approximation. It is the kind of concept that has always appealed to the Professor and to me.

**Litigation on MacDonald patent.** There were two trials on the MacDonald patent 1913604 on the high-L antenna circuit. (The first dealt also with my patent on the Diode Peak Detector, so that chapter covers that part.)

- Hazeltine Corp. vs. General Electric Supply Corp. (on G.E. receivers) in the U.S. District Court in Baltimore, Md., (Fourth Circuit) before Judge Coleman. Trial 371027-1104 (7 days). Settled before a decision, by G.E. Co. taking a license 380414.

- Hazeltine Corp. vs. Crosley Radio Corp. (on Crosley receivers) in U.S. District Court in Cincinnati, Ohio, (Sixth Circuit). Trial 401112-19 (6 days). Decision against Hazeltine, about 410706.

In the first case, our attonrney was R. Morton Adams (PDME) and he relied on Prof. Hazeltine as expert witness. The defense attorney was Stephen Philbin who used Waterman as expert witness. MacDonald participated as the inventor. There was no decision because the case was settled.

In the second case, our attorney was Laurence B. Dodds, assisted by Henry Kilburn, relying on Prof. Knox McIlwain (U. of Pa.) as expert witness. The defense attorney was probably Samuel E. Darby, Jr., and the expert was Leo A. Kelley. MacDonald testified as the inventor. I was examined by Kilburn as the last witness. We lost the case.

**Litigation on Trube patent**. There were two trials on one of the Trube patents on the variable ratio for uniform gain.

- Hazeltine Corp. vs. General Motors Co. (on Delco receivers) in U.S. District Court in Wilmington, Del., (Third Circuit) before Judge Nields. Trial 400429-0507 (6 days). Argument 401018. Adverse decision, about 410509.
- Hazeltine Corp. vs. a distributor (on Sonora and Emerson receivers) in U.S. District Court in Chicago, Ill., (Seventh Circuit) before Judge Igoe. Trial 401128-29.

In the first case, our attorneys were William H. Davis and Adams (PDME) who relied on Prof. Hazeltine as expert witness. The defense attorney was Drury Cooper, who used Leo A. Kelley as expert witness. I participated and testified briefly on some tests of infringement. The adverse decision came at a time when the receiver industry was being stopped by the war.

In the second case, our attorneys were the same, with Prof. Hazeltine. The defense attorney was Samuel E. Darby, Jr. I testified briefly. I do not recall why the case closed on the second day, or what was the outcome.

**References** to published papers, patents and Hazeltine reports. All listed patents were filed and/or acquired by Hazeltine Corp.

[A] H. A. Wheeler, W. A. MacDonald, "Theory and operation of tuned radio-frequency coupling systems", Proc. IRE, vol. 19, pp. 738-805; May 1931. (Variable ratio for uniform gain, 3-coil transformer.) (L. A. Hazeltine, Discussion, C. E. Trube.) (Presented in Toronto, Aug. 20, 1930.)

[B] C. E. Trube, U. S. Patents (or British, B).

| | Pat. No. | Issued / Filed | |
|---|---|---|---|
| (1) | B273639 | 280116 / 260914 | / U.S. 260702 |
| (2) | 1762431 | 300610 / 240109 | |
| (3) | 1763380 | 300610 / 280714 | / cont. 260702 |
| (4) | 1798962 | 310331 / 300514 | / div. 280714 |

Note (1) Thermiodyne, Figs. 1(b), 2(a) herein.
Note (1) (3) (4) Fig. 3(a) without neutralization.

[C] H. A. Wheeler, U. S. Patents.

| | Pat. No. | Issued / Filed | |
|---|---|---|---|
| (1) | 1831431 | 311110 / 310116 | |
| (2) | 1844374 | 320204 / 300820 | |
| (3) | 1846701 | 320223 / 300528 | / div. 290620 |
| (4) | 1868155 | 320719 / 290620 | |
| (5) | 1878653 | 320920 / 300820 | |
| (6) | 1895091 | 330124 / 300819 | |
| (7) | 1907916 | 330509 / 300819 | |
| (8) | 1910870 | 330523 / 300910 | |
| (9) | 1913693 | 330613 / 310116 | |
| (10) | 1927672 | 330919 / 300910 | |
| (11) | 1943405 | 340116 / 300910 | |

Note (3) (4) Fig. 3(b) herein, GCN, rule for 3-coil transformer.
Note (10) Philco 95 receiver, Fig. 2(c) herein.

[D] W. A. MacDonald, U. S. Patents.

| | Pat. No. | Issued / Filed | |
|---|---|---|---|
| (1) | 1888278 | 321122 / 300719 | |
| (2) | 1913604 | 330613 / 321214 | / div. 280525 |
| (3) | 2014086 | 350910 / 250526 | |
| (4) | 2022514 | 351126 / 280525 | |

Note (3) Antenna coupling tube.
Note (2) High-L antenna circuit (restricted), Fig. 2(b) herein.

[E] L. A. Hazeltine, U. S. Patents.

| | Pat. No. | Issued / Filed | |
|---|---|---|---|
| [P12] | 1692257 | 281120 / 271031 | / div. 250227 |
| [P20] | 1852710 | 320405 / 301104 | |
| [P20] | Re. 19823 | 360114 / | / re. 301104 |

Note [P12] 3-coil transformer (rule added 271031 in div.)

Note [P20] High-L antenna circuit (improvement).

[F] Hazeltine Reports, Hoboken lab.

| No. | Date | Author | Title |
|---|---|---|---|
| 213 | 260615 | L. A. Hazeltine | Preliminary report on Trube variable-ratio system. |
| 230 | 260820 | H. A. Wheeler | Study of variable ratio R.F. transformer designs. |
| 267 | 2701XX | W. A. MacDonald | Antenna coupling tube. |
| 311 | 2712XX | W. A. MacDonald | Antenna coupling system. |

[G] F. A. Kolster, "Electrical coupling system", U. S. Patent 2089271; 370810/250886. (Took some claims from Trube 1763380.)

[H] J. H. Pressley, "High-frequency transformer", U. S. Patent 1829058; 311027/290803. (Triode TRF amplifier with GCN coil on outside.)

[I] W. A. MacDonald, "A system for uniform amplification", Radio Broadcast, vol. 15, pp. 235-236; Aug. 1929. (Antenna-coupling tube, high-L antenna circuit, shunt C or variable R.)

[J] W. E. Holland, W. A. MacDonald, "The Philco 95 screen-grid plus", Radio Broadcast, vol., pp. 111-112; Dec. 1929. (Diode AVC and linear detector, uniform gain. Designed by Wheeler.)

[K] E. H. Loftin, S. Y. White, "Combined electromagnetic and electrostatic coupling and some uses of the combination", Proc. IRE, vol. 14, pp. 605-611; Oct. 1926. (Received by IRE 260608, presented in NY 260630.) (For uniform gain in TRF amplifier, also antenna coupling.)

[L] F. E. Terman, "Radio Engineers Handbook", McGraw-Hill, pp. 165-169; 1943. (Reproduction of parts of [A].)

[M] G. L. Beers, W. L. Carlson, "Recent developments in superheterodyne receivers", Proc. IRE, vol. 17, pp. 501-515; Mar. 1929. (Shows a TRF preamplifier, 227 triode, Rice neutralization, high-inductance primary resonant at a lower frequency for uniform gain.)

## 4.4 Contests Involving the Hazeltine Neutralization 1924-34.

The Hazeltine neutralization was an improvement over earlier forms of neutralization. His third patent on this subject was specific to the form most used, after he put it into his design that became the original Neutrodyne receiver. This form came to be known as "plate-circuit neutralization" (PCN) because the reversing transformer was in the plate circuit rather than the grid circuit (GCN). This form is what I first reduced to practice, but later found to be anticipated by Hazeltine's pending patent applications. Its use covered the period 1923-28, after which it was superseded by the screen-grid tube which did not require neutralization.

The contests involving this subject will be reviewed under these topics:

- The infringement of prior patents..
- The infringement of the Hazeltine PCN patent by coils with magnetic coupling.
- The infringement of the Hazeltine PCN patent by coils with equivalent coupling.
- The infringement of related Hazeltine patents.
- The Jones contests.
- Interferences in the Patent Office.

**The infringement of prior patents** by Hazeltine neutralization was a topic close to the relationship between the Professor and the IRM group. In 1922, the IRM companies were denied a license under RCA patents, so they were prevented from manufacturing the set that was then most popular (the Armstrong regenerative receiver). Therefore the principal objective of IRM was survival without infringing the RCA patents (of which the most visible was Armstrong). This they accomplished for a few years (1923-27). Then, even before the Neutrodyne became obsolete, it became apparent that other RCA patents were infringed. At that time, RCA changed its policy and offered a license to any company. As a result, in 1927, the IRM companies needed and obtained an RCA license, so their exclusive Hazeltine license was terminated and the Hazeltine royalty rate was reduced to one-half.

The following course of events, in retrospect, established that the Neutrodyne receivers made by the IRM companies did not avoid infringement of all RCA patents. In response to infringement suits brought under some RCA patents, we of Hazeltine Corp. conducted the defense on behalf of our licensees. This process bought time for IRM competition until an RCA license was finally made available. On taking a license in 1927, each company was absolved from past royalties. In effect, the restrictive policies of RCA caused these results:

- Motivating the Hazeltine design of the Neutrodyne, which then superseded the Armstrong regenerative receiver..

- Payment of royalties from many companies to Hazeltine Corp. instead of RCA, until 1927 after which royalties were shared.
- Forcing other companies to use neutralization (until it became obsolete in 1928) by refusing licenses for the Armstrong superheterodyne receiver (even after 1927).
- Establishing Hazeltine Corp. as the principal competitor to RCA in making and licensing radio inventions.

On the last point, the final act was after World War II, when these two companies developed the two principal features which made possible the conversion to color TV.

The RCA earlier patents involved were Hartley and Rice neutralization. These two patents were found immediately by my patent attorney in 1922, and he advised that they dominated my invention of PCN. When Hazeltine's attorney was confronted with Hartley and Rice, he adopted the position that they were not dominant because they failed to accomplish Hazeltine's result (complete neutralization over a frequency range of tuning). This position was first accepted and later finally rejected in the Courts, in the following case.

In 1924, RCA recognized the success of the Neutrodyne receiver, manufactured without a license under RCA patents. Therefore RCA filed suit against Garod Corp., one of the Hazeltine licensees. Hazeltine Corp. was not a party, but conducted the defense for Garod. In the annual reports of the Company, there was no mention of this case until the decisions commenting on the Neutrodyne.

This suit was based on the Hartley and Rice patents. Hartley accomplished one-frequency neutralization by magnetic coupling in opposition to the inherent capacitive coupling. Rice accomplished partial neutralization over a frequency range of tuning, with a balanced grid circuit. The latter left some untuned coupling which was regenerative and might cause oscillation. I made models of Rice and of Hazeltine PCN and GCN for demonstration. They may have been used in the trial, but I did not participate. The trial occurred in the SDNY before Judge Inch.

On 260628, Judge Inch ruled that the Garod Neutrodyne did not infringe the Hartley and Rice patents. This decision was appealed. On 270502, the CCA reversed his decision and held that those two patents were valid and were infringed by the Garod Neutrodyne. In retrospect, I regard the latter as a correct decision. There was no basis for appeal.

As a result of these decisions, the IRM companies could no longer operate without a license from RCA. However, such a license was then made available. There was no further need for IRM, so their agreement with Hazeltine Corp. was terminated 270606. On 270824, a special letter to Hazeltine Corp. stockholders reported on these developments. They did not affect the validity of the Hazeltine patents, but the license fee was reduced to one-half. The Hazeltine license was no longer exclusive to IRM companies. Most of them continued under Hazeltine license and they were soon joined by several other companies:

Crosley Radio Corp.
A. H. Grebe and Co., Inc.
United States Radio and Television Corp.
Philadelphia Storage Battery Co. (Philco).
All-American Mohawk Corp.
Charles Freshman Co., Inc.

Philco was just entering the field of broadcast receivers, and relied on engineering support from the Hazeltine lab, then still in Hoboken. Grebe and Freshman took licenses in settlement of infringement suits.

In summary, the IRM in its short life (1922-27) was associated with some remarkable achievements:

- Instigating the application of the Hazeltine invention in the design of the Neutrodyne receiver.
- Obstructing and delaying the enforcement of the RCA monopoly by defensive litigation and by effective competition in the market place with a superior product.
- Superseding the prevalent Armstrong regenerative receiver which was made by RCA and very few other companies under exclusive license.
- Influencing RCA to relax its exclusive licensing policy, which was the reason for the formation of IRM.
- Leaving the legacy of a new company which continued as the principal competitor of RCA in inventions and engineering relating to radio broadcast receivers — the Hazeltine Corporation.

**The infringement of the Hazeltine PCN patent by coils with magnetic coupling** was the main cause of actions by Hazeltine Corp. against unlicensed companies. Unfortunately some of those companies might have been willing to operate under a license but were excluded by the IRM restriction of membership. The accused infringements ranged from "carbon copies" of the Neutrodyne, to some ingenious alternatives or subterfuges which were intended to avoid infringement. In the latter category were a group of licensees of a competing company, Radio Frequency Laboratories, Inc. (RFL in nearby Boonton, N.J.). Those will be reviewed as a separate group. Here we shall deal with infringements which were essentially the PCN with magnetically coupled coils as used in the Neutrodyne.

Hazeltine Corp. vs. Electric Service Engineering Corp. in SDNY before Judge Thacher. It is my recollection that this was a "carbon-copy" case, probably filed in 1924 or 1925. On 260701, it was decided in our favor, to the extent of ruling one Hazeltine patent valid and infringed. This ruling was not appealed, so it resulted in an injunction.

Hazeltine Corp. vs. A. H. Grebe & Co. Inc. in EDNY before Judge Moskowitz. It is my recollection that this also was a "carbon-copy" case, probably filed in 1924 or 1925. The trial was in 1926 MAY. On 270620, it was decided in our favor, to the extent of ruling two Hazeltine patents valid and one or two infringed. This case was settled by Grebe taking a license 270701, when it had just become available after the termination of IRM. Grebe was an outstanding manufacturer with a record of high-quality engineering.

Hazeltine Corp. vs. Charles Freshman Co. Inc. This case may have been held in abeyance, pending the outcome of the Grebe case. It was settled by Freshman taking a license in 1927 when it became available.

Hazeltine Corp. vs. Atwater Kent Manufacturing Co. was filed 260805 in Philadelphia but the patent trial on their receivers occurred in Brooklyn, as follows, against their distributor:

Hazeltine Corp. vs. E. A. Wildermuth and Co. in EDNY before Judge Moskowitz, filed 260820. On 280203, the Company leaders and our patent attorneys appointed me as the "expert witness" in future trials on the Hazeltine patents. I was 25, and I was to be pitted against experienced experts in the field. This case was my first. The trial occurred 280418-280501. One of my tasks was to prove (by theory and tests) that an old Signal Corps amplifier (BC-59A) did not have PCN. The opposing expert was our friend, John V. L. Hogan (one of the 3 founders of IRE in 1912). My position was accepted. The accused Atwater Kent receivers had neutralization by coupling in open space, equivalent to the usual added capacitive coupling. On 281115, the decision ruled in our favor, that the PCN patent was valid and was infringed by those receivers. This decision was appealed.

This case was tried by Taylor and Guild for Hazeltine. Mr. Davis argued our case before the CCA. The opposing counsel was Stephen Philbin, who was retained by RCA for their major cases. He was a prominent trial lawyer and one-time Yale football star.

The Atwater Kent appeal was decided in our favor 290708 by the CCA sustaining the decision of Judge Moskowitz. After further proceedings, this was made final 310202. The accounting was then assigned to a Special Master in Philadelphia. This proceeding went on and on, with participation of Adams and me on behalf of our Company. No end was in sight, and we were in the bottom of the Depression. Near the end of 1933, MacDonald arranged a meeting with Mr. Schwank, general manager of AK. They negotiated a settlement, which became effective on or about 340219. It must have been reported to stockholders on 340616 with the special distribution of $2.375 per share ($416,000 total) but was not described in the Annual Report. The settlement was $680,000 cash to Hazeltine, a license agreement and some other considerations. The license became worthless because AK was discontinuing its radio business.

Hazeltine Corp. vs. Radio Corp. of America (RCA) was filed in 1927 in SDNY. It was tried in 1930 and decided 310922. Judge Woolsey held that two models, Radiola 16 and 17, infringed PCN. (In their day, these were the most popular in the RCA line.) This decision was sustained by CCA 320606. The accounting before a Special Master in New York dragged out until 1937, when all disputes with RCA were settled after the Wilmington trial on the diode AVC patent.

This group of cases directly or indirectly influenced some major companies to take a Hazeltine license, and stopped some minor companies from further infringement. In a few cases, the defendant company may have been willing to take a license but was excluded by IRM. By the time these cases were decided, the IRM had become inoperative, an eventuality that was not anticipated though inevitable. The Atwater Kent case yielded mainly a cash settlement which was a small fraction of the royalties they had escaped, but came at a time when the money had unusual value to our Company and stockholders.

**The infringement of the Hazeltine PCN patents by coils with equivalent coupling** was a unique situation brought on by a competing group of brilliant young inventors. The Radio Frequency Laboratories, Inc. (RFL) was formed about 1923 by the Boonton Rubber Co. in Boonton, N.J., an hour out on the Lackawanna from Hoboken. The President was Richard W. Seabury and the technical leaders were Dr. Lewis M. Hull (V.P.) and Stuart Ballantine. (I was acquainted with Dr. Hull from my summers at the Bu. of Standards.) Their patent attorney was Blake Townsend, the son of a prominent patent attorney in Washington. Hull and Ballantine being from Harvard, they later used as their expert witness, the famous Prof. E. Leon Chaffee of Harvard. Hull and Ballantine had contributed an ingenious circuit which approximated the result of PCN but without magnetic coupling between the opposite coils in the plate circuit. It accomplished neutralization of feedback coupling from the tuned circuit, which was the main offender. Like the Rice circuit, it left unneutralized some untuned coupling between grid and plate. The latter was not enough to be detrimental. The ideal of "close coupling", advocated by Hazeltine, had been relaxed in our practical designs as a practical compromise for economy. One might regard the RFL circuit as the complete relaxation from this ideal, but yielding nearly the same result. It went further than just economy, to the extent of being slightly more expensive.

The RFL neutralizing circuit was offered to a group of prominent radio manufacturers who were excluded from IRM and thereby denied a Hazeltine license. They wanted to make sets to compete with the Neutrodyne. (This motivation may be compared with that of the IRM companies being denied an RCA license.) The result was a group of RFL licensees who were mostly superior to the IRM group. There was only one IRM member who also joined the RFL group, namely, Stromberg-Carlson, the most prominent member of IRM. Through our close cooperation with S-C, we learned early of some features of innovation by RFL. (One was the enclosure of a coil in a can for shielding.) Presumptively the RFL likewise learned early of some of our developments in the S-C designs. In the IRE, we were all good friends. Hull and Ballantine later served as President of IRE. The companies of the RFL group became Hazeltine licensees, at various times after the end of IRM and after the Neutrodyne was superseded by the screen-grid tube and the superheterodyne.

In order to maintain our patent position with IRM, we filed suits against some companies in the RFL group, alleging infringement of the Hazeltine PCN patent by the RFL circuit. There was an arguable defense, but our position prevailed in the Courts. It was essentially that the RFL circuit had indirect coupling between reverse coils in the plate circuit, which was equivalent to the Hazeltine magnetic coupling, in the sense of the patent claims. The following were the principal contests on this position.

Hazeltine Corp. vs. Grigsby-Grunow Co. (on the "Majestic" receiver) in SDNY before Judge Coleman. We sought a preliminary injunction, pending a trial of all issues. The contest occurred around the end of 1928. Opposing affidavits were prepared by me for Hazeltine, then by Chaffee for defendant. There was a hearing starting on 281224. We made demonstrations to show the equivalence of the RFL and Hazeltine circuits. Then there were more affidavits, opposing briefs and final argument. On 290325 we were denied a preliminary injunction.

Hazeltine Corp. vs. National Carbon Co. (on the "Eveready" receiver) in EDNY. A decision in 1930 held that these receivers infringed the PCN patent. This decision was finally upheld by CCA 310202. A settlement was reached 320210.

Hazeltine Corp. vs. United American Bosch Magneto Corp. (Models 28 and 29) in SDNY before Judge Knox. These receivers were similar to the Eveready. On 310224, they were held to infringe the PCN patent. On or about 350123, they settled by taking a Hazeltine license.

Some companies from the RFL group took Hazeltine licenses after the RCA settlement in 1937. Among them were Stewart-Warner Speedometer Co. (380318), Sparks-Withington Co. (the "Sparton" receiver) (380405) and Grigsby-Grunow Co. (the "Majestic" receiver).

In retrospect, the RFL situation was an unfortunate by-product of the exclusive policy of IRM, limited to 14 members. The RFL did not survive long after the adverse decisions in 1930-31.

**The infringement of related Hazeltine patents** was incidental to the main line based on PCN. It was one consideration in the continuity of licensing in the period (1928-32) between the obsolescence of the Neutrodyne and the issuance of patents on diode AVC. The Hazeltine "receiver" patent and its divisions claimed improvements invented in the design of the Neutrodyne receiver but also useful in the successor screen-grid receivers. For that reason, their definition and coverage was pursued with some diligence in the Patent Office and the Courts. Much time was lost. With an original filing date of 250227, the last to issue was a reissue of a division, on 360114. The principal topics were "high conductance" and "like tuning".

The feature of "high conductance" was a design relation in a triode TRF stage, especially one with neutralization such as Hazeltine PCN. I did not have it when I operated the first such amplifier, shortly before the Professor designed the Neutrodyne. He devised that relation for realizing the best compromise between gain and selectivity. In the Neutrodyne, "high conductance" referred to the use of relatively few turns in the primary coil of the tuned transformer in each plate circuit. It was described as "less-than-optimum" coupling. This referred to a slight sacrifice of gain in return for nearly the maximum selectivity possible with the tuned circuit. The practice of this feature gave a primary-circuit conductance (at resonance) substantially greater than the plate conductance of the triode. It was so described in the claims. It was practiced in the receivers accused of infringement of PCN. With the successor screen-grid tetrode, a similar relation was retained but in different form. The tetrode had such a low value of plate conductance that any sensible design would present relatively high conductance, though not by the use of few turns. Likewise it would obtain the benefits. Therefore we were ambitious to rely on this feature for extending our coverage over the tetrode receivers.

The feature of "like tuning" enabled unicontrol of several stages, although unicontrol was not used at first in the Neutrodyne. Like tuning was facilitated by high conductance and by neutralization but was not essentially dependent on either.

We considered these features to be major improvements amounting to invention over anything accomplished before. They could be regarded as engineering design, a matter of degree rather than kind. By care and ingenuity in definition, we secured the allowance of claims in the Patent Office. Some of these claims survived interference contests with copending applications. Afterward, none survived in the courts. One case may be mentioned as most relevant.

Hazeltine Corp. vs. Sears Roebuck & Co. in SDNY before Judge Coleman. Presumptively it was based on sets made by another company manufacturing a TRF receiver without a Hazeltine license. Of the three Hazeltine patents in suit, the Court ruled on 331226 that one was invalid and all three were not infringed. In 1935, the CCA sustained that decision. By that time, the diode AVC patent situation was building up with aggressive prosecution against RCA.

**The Jones contests** were a number of challenges by an inventor who had been working as a young engineer at the Washington Navy Yard when the Professor was there around the end of World War I. He independently but later invented PCN and some related features. His patent attorney was Maxwell James. They were hopeful that they might obtain some share of the patent coverage and royalties that were accruing to Hazeltine Corp. The scope of each of the following actions is not clear from my notes, but here is what I can report.

In the Patent Office, an interference between Hazeltine and Lester L. Jones ended 280604 with all contested claims awarded to Hazeltine.

In the Patent Office, there was another interference between Hazeltine and Jones. On 290211-13, depositions were taken from Edwin A. Armstrong, John F. Dreyer and me. I do not recall the outcome.

Jones obtained a patent with some claims that could be read on the PCN in the Neutrodyne. He filed suit in EDNY alleging infringement by receivers made by two members of IRM. These cases were defended by Hazeltine Corp. with me acting as expert witness. They were tried before Judge Campbell 290404-22. The defendants were Freed-Eisemann and Walthal (a distributor of Stromberg-Carlson sets). On 290907, the Court ruled that the Jones patent was invalid.

In some later interference proceedings, I recall a deposition taken late in 1930 at the new Bayside lab. I was required to testify on the operation of a receiver built by Jones, which was the basis for some of his claims. It had ingenious tuning by variable inductors rather than capacitors. We all recognized a theoretical reason for preferring inductors but they were not practical to make (in comparison with the rotary capacitors in common use). Jones used an ingenious but cumbersome rotary inductor made of interleaved flat coils, each one a pair of D-shaped spirals. The three tuners tracked remarkably well.

I have always regretted that the talents displayed by Jones were not better utilized. I lost track of him.

**Interferences in the Patent Office** were declared on the basis of certain assigned claims in some of the patent applications of Hazeltine. There was no significant challenge to Hazeltine neutralization.

One interference with Ballantine involved the high-conductance feature. I attended his depositions in Boonton 280210-16, where I first became acquainted with him. The Patent Office on 281101 decided in our favor, on the basis of Hazeltine's earlier dates.

The later interference with Jones has been mentioned.

**In summary**, the contests clearly sustained Hazeltine's claims to PCN, even to the extent of covering some equivalent circuits which were devised by competitors to avoid these claims. His claims to other forms of neutralization, and to related features incorporated in the Neutrodyne design, were left with significant restrictions of their scope. None of the latter claims were adjudicated valid and infringed.

# Section 5. Diode AVC.

"the 'idea' is great but 'volume' is inappropriate for a TV or radar receiver. 'Gain' is better.

## 5.1 Automatic Volume Control (AVC) 1925.

The most important invention of my career was the Diode Automatic Volume Control (AVC) and Peak Detector. I made it alone, though in the context of other developments in progress in the Hoboken lab. I built the first model alone, in my basement lab at home in Washington. I presented it to a N.Y. meeting of IRE and published it in the Proceedings. I designed the first receiver (Philco 95) in which it was marketed. That happened just when the Hazeltine invention was made obsolete. My invention became the keystone of the Company's patent portfolio in the 1930's, from which the royalties carried the Company through the Depression. My reissue patent was upheld in the Courts until their decisions were reversed in the Supreme Court. The invention was made for AM broadcast receivers using vacuum tubes. It is still used in all AM receivers, though the VT has been superseded by the transistor.

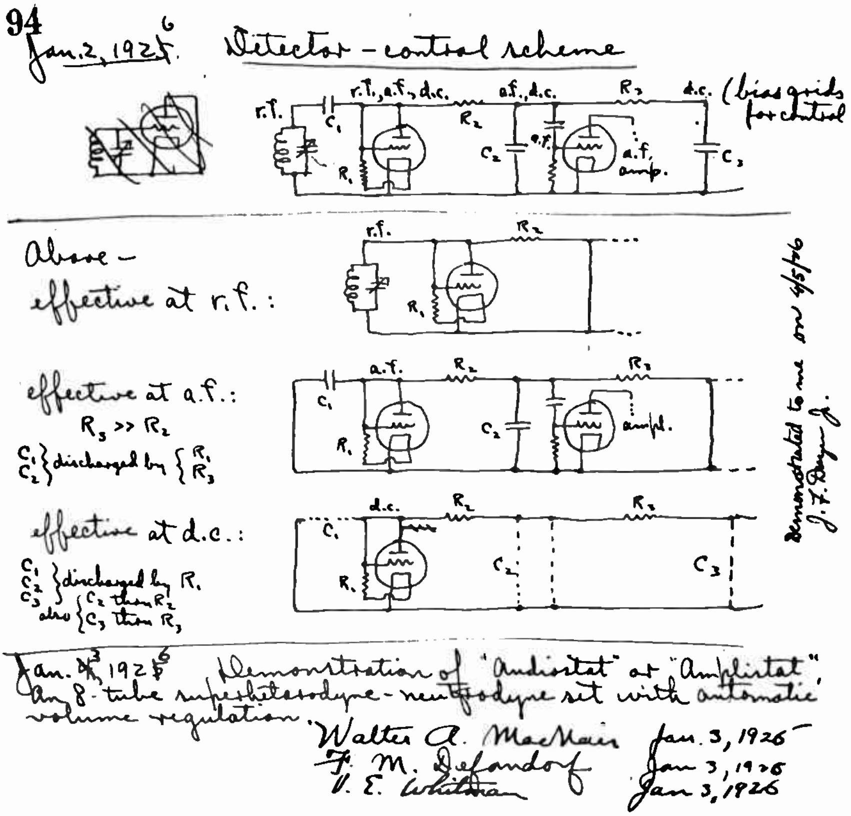
The preferred name for AVC eventually became "Automatic Gain Control" (AGC) which distinguishes from the remaining manual volume control. Here I retain my original designation, which was the one comonly used before World War II.

In 1966, I was one of 400 who were invited to contribute to the "McGraw-Hill Modern Men of Science", each one to describe his "most significant achievement". I chose this topic, so its story has been recorded therein. Mine was one of 13 in the field of electrical and electronics engineering.

In the early 1920's, automatic controls were not common. I remember two. One was the mechanical centrifugal governor used in a stationary steam engine of the type using a cylinder and flywheel. The other was the thermostat used in various forms for control of a heating system.

The summer of 1925 was my third in Hoboken. When I arrived at the lab, I was impressed with the greater amplification or gain that was available in the new "shielded" Neutrodyne receiver with three TRF stages instead of two. It was shown to me 250630 by Dreyer and MacDonald. There was a "volume control" knob for adjusting the RF gain and hence the loudness (volume of sound). While tuning to different stations, continual adjustment of the volume control was necessary to hear the weaker signals while avoiding excessive volume ("blasting") on the stronger signals. Both hands were needed on the two dials for tuning (antenna and RF stages). I told them that we should add an automatic volume control, to take the place of a third hand. The volume control had become nearly as critical as the tuning, but its function was subsidiary to the station selection. On 250709-11, I decided that the control should be responsive to the RF carrier of the signal, not to the AF output of the receiver, and I showed several ways of accomplishing this. Then I concentrated on other topics, including the invention of uniform-gain circuits.

My first studies showed how AVC could be accomplished in a receiver for AM radio reception. The modulated-carrier signal would be amplified in a few




5.1 Fig. 0 — Detector-control scheme

RF stages, whose gain could be decreased by an increasing negative bias on the grid of one or more of the stages. (In a superheterodyne, these would be intermediate-frequency or IF stages.) The amplified carrier would be rectified to develop a negative bias for this purpose. This bias would be applied to decrease the gain with increasing signal strength, thereby leaving little variation of the amplified signal. The automatic control would respond in a small fraction of a second during tuning or fading, so excessive loudness would be prevented. The preferred designation has come to be "automatic gain control" (AGC) to compensate for signal strength or tuning. Then the remaining manual "volume control" is required mainly for adjusting to the desired loudness.

The next fall I started at Hopkins. On 251208, I resumed my notebook studies of AVC. I laid out a modular plan for a superheterodyne receiver with a neutralized (fixed-tuned) IF amplifier. Each stage was to be enclosed in a metal shield box. I chose "grid-circuit neutralization" by a plate-to-plate capacitor of convenient size. I chose 300 Kc as the IF. This was much higher than previous practice, and was enabled by the neutralization not employed before in an IF amplifier. At home, I collected the required tuning capacitors and ordered the shield boxes to be made by a hardware store.

Over the Christmas holidays, I spent 15 days in Washington (251219-260103). The usual social activities were increased by our engagement. Ruth was tolerant of my time sharing. About one-half the time I spent in my basement lab, making and testing the AVC set.

My intention was to use a triode detector with plate-current rectification, to economize on the amplified signal voltage required on the grid. I was obsessed with the prevalent notion of using the least gain ahead of the detector. I had some trouble with the complication of deriving from the decreasing positive plate voltage an increasing negative bias for controlling the gain. My notebook also contained a plan for using a separate diode rectifier to derive this bias.

On 260102, I devised and implemented a scheme using the same diode for detection of the signal modulation and for rectification of the signal carrier. This was the first to give linear detection of the signal, combined with rectification of the carrier independent of the modulation. Using this diode circuit for automatic control of carrier gain yielded all the desired features of performance:

- Gain control by the rectified carrier was free of fluctuation with modulation;
- The gain control held the amplified signal at a nearly constant level such that the response to modulation was "linear" (free of distortion).

So far as we know today, both of these features were lacking in any earlier disclosures. They were lacking in receivers made a little later by the Telephone Co. (but not sold) and by RCA (sold as the Radiola 64).

I dubbed my AVC the "Audiostat" by analogy with "thermostat". On 260103, we had a party at my home, at which some of the guests were other graduate students at Hopkins (MacNair, Defandorf, Whitman). I demonstrated to them my 8-tube superheterodyne with the diode circuit and they signed my notebook.

I connected a small meter to indicate the plate current of the first IF stage, so we could see the operation of the control. I did not show this in my notebook. It was helpful in tuning and in showing signal "fading" which was largely compensated. This became identified as another invention, the "tuning meter". It was later used in professional receivers as an inverse indicator of signal strength (the socalled S meter).

Here I should explain my natural resistance to using a diode rectifier for detection of the signal. It had to be made of a triode, as by joining the grid and plate. The triode cost five dollars, so I was hesitant to abandon its intended amplifying ability. The diode was known to be an inefficient detector of a weak signal (less efficient than a crystal detector) but I came to appreciate its usefulness on a strong (amplified) signal. The indirectly heated cathode was just around the corner, and it brought greater efficiency.

(In retrospect, I should have used the "space-charge grid" connection for greatest efficiency. By applying a positive voltage on the grid, a "virtual cathode" could have been formed to make a diode of a large area close inside the plate.)

A feature of my circuit was the use of the same diode for both detector and control. This was economical because its linearity was desired for both functions.

My demonstration of AVC was spectacular, the more so because the superheterodyne offered much gain and I had a long wire as an antenna. It received on the loudspeaker, any signal sufficient to survive the interference in the crowded spectrum. Late at night, some West Coast stations operated the AVC and actuated the meter. The superheterodyne had only two dials for tuning the antenna and the local oscillator. With the AVC, I did not bother to provide a manual volume control.

Herewith is a facsimile of my notebook page 94 (260102) on which I first outlined the use of a single diode rectifier to deliver the modulation to the AF amplifier and a DC bias for gain control. It is the page signed by my guests at the first demonstration, in my basement lab while the party was in progress just above.

The day after my demonstration, I went to New York on one of my infrequent visits between summers. I told Dreyer about my AVC. He mentioned that he had heard of some work in progress in Western Electric Co. relating to that subject. Much later, we learned about it. They did not accomplish what I did. Three months later, on 260405, Dreyer visited me in Washington to see my AVC receiver.

On my arrival for the next summer in Hoboken, we were planning for patent coverage on my AVC invention, and the Company became aware that my contract did not include assignment of the variable ratio for uniform gain or the AVC. They were much relieved when I agreed to include all inventions with no additional compensation. This agreement 260623 further strengthened my bonds with the Company. It avoided the tensions that would have resulted from a large payment to me, however fair it might have been. The first patent applications on AVC were prepared by Baldwin Guild in PDME (a former student of the Professor at Stevens) and filed 270707.

The same summer, I went to Rochester 260728 with MacDonald for 5 days at Stromberg-Carlson. Among other things, I planned with them an adaptation of their latest Neutrodyne set to demonstrate the diode AVC and linear detector. They made it and sent it to Hoboken for demonstration 3 days from 260809. This was the first time MacDonald had seen AVC in operation.

On 260818, I demonstrated the set to Pres. Pierson and Jack Binns, and they were much impressed. The next day was one of the regular meetings of IRM engineers in New York. I reported progress on my AVC. Then I left for our wedding in Washington.

Why was it, that diode AVC was not immediately adopted in our designs of broadcast receivers? AVC was needed and I had demonstrated the utility of diode AVC. However, it was still expensive to provide enough RF amplification for its effective use. This came later with the screen-grid tube, by adding a wideband RF stage to drive the diode. Then the superheterodyne became available, with economical IF gain. The first broadcast receiver to have AVC of any kind was the Radiola 64, sold by RCA in 1928. [M] It was a superheterodyne, which was still exclusive with RCA. Its triode AVC circuit was superseded the next year by my diode AVC in the Philco 95.

In view of this problem, I devised various schemes for using a triode detector for AVC. An approach to linear detection was possible in a triode detector with grid-current rectification, operating on a signal voltage much less than required for diode AVC. The AVC circuit was more complicated, but operative. It required an extra triode as a "DC amplifier" to derive from the detector a negative bias for automatic control. I was doggedly pursuing this circuit as late as mid-1929, even when screen-grid tubes became available. I did arrive at one or more laboratory designs that performed well but were never manufactured and sold. In retrospect, that effort was misdirected, and could better have been directed toward earlier use of diode AVC (perhaps in 1928 instead of 1929).

On a personal trip to Omaha, between trains in Chicago on 260513, I visited the Howard Radio Co., one of our licensees. In talking with Mr. Austin A. Howard, Pres., and Leland H. Hanson, Chief Engineer, I told them about my AVC set, then operating in Washington. A year later, I visited them again 270610 on our vacation trip to the midwest. I must have further aroused their interest in AVC. On my arrival in New York 270707, Jack Binns told me that Mr. Howard wanted me to go to Chicago and make a receiver with AVC. The Company consented, so I worked in Chicago most of the time that summer between 270718 and 270917 (It was the first summer after our marriage.) There

I worked directly with Bertram A. Schwarz and Harold Jorgenson, and we became close friends. I lived at the Allerton House, which was very comfortable.

Here again, I tried to improve on the diode AVC by some kind using the triode detector. Finally I went to the diode AVC and quickly arrived at a working model. There was no thoughtful objective for a design, and I did not contribute the mature judgment that was needed. We ended up with an elaborate, heavy model with remarkable performance. It had 4 stages of shielded Neutrodyne TRF amplifier, which was sufficient to operate the diode AVC from a loop antenna mounted on top. The receiver with its cone speaker was housed in an ornate console. It was not suitable for marketing. If I had used some judgment or had been given some direction, the result might have been the first diode AVC receiver on the market. I did not know enough to ask the right questions.

As a direct result of the summer in Chicago, I prepared the first paper on AVC to be published in the Proceedings of IRE. It was a brief description of diode AVC and peak detector, based on the Howard receiver. It was entitled, "Automatic volume control for radio receiving sets". I prepared a draft and obtained a criticism by the Professor.

The arrangements for my first talk to IRE were most interesting for the differences between then and now. In 1927, the Institute of Radio Engineers was 15 years old and had a membership of about 4000. The monthly meeting in New York was the prinicpal meeting in the World. On 270922, I called at the headquarters, next door to the Engineering Bldg. at 33 W. 39 St. There I talked with Mr. Robert H. Marriott, who was one of the three founders of IRE and was currently Chairman of the Committee on Meetings and Papers. I told him my topic and he said he would schedule it for their meeting 6 weeks hence. On 271001, I finished my paper and sent it to him. Two weeks later, it had been set in type and preprints were mailed to the members for preparation of any discussion. Two members (one from Boston and one from N.J.) presented prepared discussions. In the evening of Wednesday 271102, I read my paper in the smaller auditorium of the Engineering Bldg. The chairman was either the President, Dr. Ralph Bown (later Pres. of BTL) or the Secretary, Dr. Alfred N. Goldsmith (another one of the three founders). The attendance was about 100, as usual. There was no microphone, nor was one needed. The prepared discussions had no lasting significance. Dr. Goldsmith made the only comments from familiarity with AVC, presumptively based on the current development of the Radiola 64 by RCA, with which he was associated. (At the time of my talk, my wife was in the Johns Hopkins Hospital recuperating after the birth of our first child.)

My paper was published in the Proceedings of IRE for 1928 JAN, together with the prepared discussions, only 3 months after its submission. It is reproduced herewith, together with the Contents and Contributions which disclose the distinguished authors in whose company I found myself (Marconi, Armstrong, Heising).

In 1928, we made little progress on AVC. It was the year Jack Pressley worked at the Hoboken lab. He and I were mainly concerned with variable-ratio transformers for uniform gain in a TRF stage using a triode with neutralization. When I was going to a full-time schedule, MacDonald on 280925 gave me two principal assignments. One was AVC developments. The other was testing equipment which we urgently needed. I had already been designated as our expert in patent trials relating to the Neutrodyne. I was kept busy.

The first real opportunity for diode AVC came in 1929, three years after its demonstration in Washington. The new screen-grid tube (UX-224) enabled RF amplification without the need for neutralization, and with more gain per stage. For their second year, Philco was relying on our advice and engineering for their receiver designs. Early in June, their Chief Engineer Walter E. Holland, and his assistant William H. Grimditch conferred with MacDonald and Harnett in the N.Y. lab recently occupied. They decided to try for AVC in the top model of their new line, which came to be known as the Philco 95. I was assigned the design of this receiver and I started work on my return 290623 from a trip to Rochester. The list of features and the circuit diagram I entered on that day were substantially a preview of the design they adopted. It relied on the diode AVC and peak detector.

The diode AVC and peak detector were substantially the circuit I had used 260102 in the Washington receiver and 1927 in the Howard receiver (published early in 1928). The principal parts of the receiver were:

(1) Unicontrol double-tuned antenna circuit and two-stage TRF amplifier (2 type 224 screen-grid tetrodes).
(2) An extra stage of wideband RF amplifier to provide the voltage needed for the diode rectifier.
(3) A diode rectifier (227 triode connected as a diode) used as a linear peak detector for recovering the AF modulation and for providing a control bias.
(4) Control bias on the two TRF stages and half-bias on the third stage to yield maximum RF voltage output for the diode rectifier.
(5) An AF pre-amplifier stage (227 triode) followed by a tapped resistor ("potentiometer") with a knob for manual volume control.
(6) Two-stage AF amplifier with transformer coupling (227 triode plus a pair of 245 triodes in push-pull).
(7) Cone loudspeaker.
(8) A "local-distance" switch for reducing the antenna coupling when receiving very strong signals on a long wire antenna.

The last feature (8) was required because the sharp cutoff of the 224 tetrode left some distortion of a strong signal. (This problem was later solved in the 235 tetrode with the "variable-mu" feature of gradual cutoff.) The complete circuit was published late in 1929. [G]

The Philco 95 was the pioneer of AVC in a broadcast receiver. It was the first to have these features, which came to be used in all AM broadcast receivers to this day:

- Diode peak rectifier for linear detection of AM;
- Same kind of rectifier for AVC bias, following the AM envelope to yield an average voltage dependent only on the carrier.

Typically these were provided by a common diode, but there were some refinements obtainable by using separate diodes.

Around the end of 1929, I designed the second model to use diode AVC. It was the 1930 Fada KA receiver. It was generally similar but with some improvements, as follows:

- The diode detector was inductively coupled with the untuned last stage of RF amplifier, so the input and output could be connected in series while retaining the grounded cathode of the diode.
- The high resistance was provided with an adjustable tap ("potentiometer") for use as the manual volume control (ahead of the first stage of AF amplifier).
- The TRF amplifier had an improved form of uniform-gain transformer.

This was the most advanced TRF receiver ever designed and manufactured.

In 1930, the RCA released the superheterodyne by licensing under the Armstrong patent. That circuit made available, in a practical design, any reasonable amount of amplification before the detector. Then it was easier to make AVC in a broadcast receiver at a reasonable price.

The cost of an extra triode, to be used as a simple diode, was soon avoided by locating one or two small diodes at one end on the same cathode as a triode. The latter could be used as the first stage of AF amplifier. Such tubes were introduced in 1931-32, for the specific purpose of diode AVC and peak detector, which had achieved universal use in the better (console) receivers for broadcast reception.

The RCA License Laboratory in New York City was becoming our competitor in releasing advanced developments to licensee manufacturers. All radio manufacturers had to have a license from RCA, but RCA and some others did not yet have a license from Hazeltine Corp.

A remarkable coincidence occurred in 1932. My first 3 patents on AVC were issued 320927. One covered diode AVC. A few days earlier, the RCA Lab had released a bulletin recommending diode AVC using their new tube type containing two diodes and a triode on one cathode (the double-diode triode). When they saw my patent, it spoiled their whole day. However, they did not rush to take a license.

During the next few years, there were two lines of development relating to my invention of diode AVC. One was its technical development in the new Bayside lab, yielding a great variety of automatic controls in a radio broadcast receiver. The other was the patent litigation which led to the licensing of all major manufacturers by Hazeltine Corp. under my patents.

The technical developments were mainly a variety of refinements based on the diode AVC and peak detector. These had little impact on marketed designs. Some will be described in relation to the subjects of other chapters, notably the "automatic expanding selector" (AXPS).

The AVC patent litigation came to a climax in 1936, after which RCA took a license and set an example for other companies not yet licensed. It came to an anticlimax in 1941, with the Supreme Court reversal of favorable decisions in the lower courts. The litigation will be reviewed in detail in another chapter.

The broadest claims to come out of my AVC invention were directed to a combination that was found in the Washington receiver. Patent No. 2041273 issued 360519 from my first application 270707 as a starting point. We had been slow to appreciate the broad concept of these two features together:

- A detector that would yield nearly linear response to modulation over a limited range of carrier voltage. This was true of the diode peak detector. Like any detector, it was subject to the lower limit approaching square-law response. Also it was subject to an upper limit imposed by saturation or overloading in the preceding RF amplifier.
- An AVC designed to hold the carrier voltage within this range for a wide range of signal strength. My AVC was designed to do this.

Other inventors were doing this with a triode detector operated at a moderately high voltage for a crude approach to linear response. I did it first, and better with the diode peak detector. I survived an interference in the Patent Office, but I do not recall the details. The other parties may have been RFL and RCA, who were working on this approach. This was clearly a new result so this patent might have survived in Court.

Looking back, I recall my AVC invention as a remarkable byproduct of my current opportunities in Hazeltine Corp., together with indoctrination by the Professor. In the latter half of 1925, I perceived the need and skimmed over various ideas which (as I was to learn later) had been invented by the outstanding engineers in the Telephone Co. They were much my senior and had access to the latest ideas, not yet published and not available to me. I ended up with the last step, which was far beyond their progress to date. My diode AVC and peak detector superseded their work and was the circuit universally adopted in AM receivers for all purposes. It is still in use 50 years later, with no hint of obsolescence. The U.S. Supreme Court made a mistake in deciding (against the evidence and the latest decisions of lower Courts) that it was not invention.

[A] [W3] "Automatic volume control for radio receiving sets", Proc. IRE, vol. 16, pp. 30-39; Jan. 1928.

[B] [W 21] "Design formulas for diode detectors", Proc. IRE, vol. 26, pp. 745-780; Jun. 1938.

[C] H. A. Wheeler, U.S. Patents.

| | Pat. No. | Issued | / | Filed | | |
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| (2) | 1879861 | 320927 | / | 301110 | / | div. 270707 |
| (3) | 1879862 | 320927 | / | 301110 | / | div. 270707 |
| (4) | 1879863 | 320927 | / | 301113 | / | div. 270707 |
| (5) | Re. 19744 | 351029 | / | Re. 340926 | / | div. 270707 |
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| (7) | 2013121 | 350903 | / | 340606 | / | cont. 320406 |
| (8) | 2041273 | 360519 | / | ren. 320827 | / | div. 270707 |
| (9) | 2050679 | 360811 | / | 331003 | | |
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| (12) | 2080646 | 370518 | / | 270707 | | |
| (13) | 2159240 | 390523 | / | 320622 | | |
| (14) | 2182329 | 391205 | / | 370623 | | |
| (15) | 2189848 | 400213 | / | 370715 | | |

Note: (4) (5) Diode AVC ("original" and "reissue").
Note: (1) AVC by relay.
Note: (2) (3) Triode AVC.
Note: (8) AVC in linear range of rectifier.
Note: (12) (13) (15) Visual indicator of tuning to resonance.

[D] HAW, Reports 476 (291007) and 507 (291203), the Philco 95 receiver with diode AVC.

[E] HAW, Report 553 (300317), the Fada KA receiver with diode AVC.

[F] R. E. Sturm, "Cathode-ray tuning indicator", Rep. 736W, 370702. See [C] (13)

[G] W. E. Holland, W. A. MacDonald, "The Philco 95 screen-grid plus", Radio Broadcast, vol. 16, no., pp. 111-112; Dec. 1929. (Photo and circuit diagram, diode detector and AVC, announced 290901.)

[H] "McGraw-Hill Modern Men of Science"; 1966. ("Most significant achievement" of each of 426 contemporary scientists. Wheeler, diode AVC.)

[I] J. F. Mason, "Harold Wheeler: an innovator in the world of communications", Electronic Design", vol. 24, no. 8, pp. 80-83; Apr. 12, 1976. (AVC story, photo of Washington receiver, notebook page 94.)

[J] S. Ballantine, H. A. Snow, "Reduction of distortion and cross-talk in radio receivers by means of variable-mu tetrodes", Proc. IRE, vol. 18, pp. 2102-2127; Dec. 1930. (Types 550, 551 — not RCA.)

[K] L. C. Waller, "Applications of visual-indicator type tubes", RCA Rev., vol. 1, no. 3, pp. 111-125; Jan. 1937. (Tuning indicator, 6E5 "magic eye". One-inch cathode-ray tube, RCA 913 in metal tube.)

[L] "Classic circuits", Electronics (Fiftieth Anniversary Issue), vol. 53, no. 9, pp. 436-442; Apr. 17, 1980. (AVC 1926 by Wheeler is No. 4 of 12 examples, p. 438.)

[M] G. L. Beers, W. L. Carlson, "Recent developments in superheterodyne receivers", Proc. IRE, vol. 17, pp. 501-515; Mar. 1929. (Radiola 64 circuits, AVC by separate triode rectifier.)

PROCEEDINGS OF

# The Institute of Radio Engineers

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## CONTENTS



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# CONTRIBUTORS TO THIS ISSUE

**Armstrong, Edwin H.,** Born December 18th, 1890. Was educated at Columbia University, E. E. degree. Research graduate under Professor M. I. Pupin in the Hartley Research Laboratory at Columbia University for a number of years. During the war a captain and later major in the Signal Corps of the United States Army in the Army Research Laboratories in Paris, France. Mr. Armstrong is universally known for his work in connection with the regenerative circuit, the superheterodyne circuits and the superregenerative circuit. At present he is doing research work in the Hartley Research Laboratory of Columbia University. In 1918 Mr. Armstrong was awarded the Institute Medal of Honor. He has been a frequent contributor to the PROCEEDINGS of the Institute, and is a Fellow of the Institute.

**Hazel, Herbert C.:** Born at Harrodsburg, Indiana, September 26th, 1899. Received A.B. degree Indiana University 1922; received M.A. degree, 1926. During research connected with this work on problems of measuring current and resistance at radio frequencies, the thermionic vacuum tube method of calibrating ammeters was devised. At present, critic teacher in physics in Bloomington (Indiana) High School. Member of Phi Beta Kappa and of Indiana Academy of Science.

**Heising, Raymond A.:** Born at Albert Lea, Minnesota, August 10th, 1888. Received the E. E. degree, University of North Dakota, 1912; M.S. degree, University of Wisconsin, 1914. With the Research Department, Radio Section, Western Electric Company since 1914 until the Bell Telephone Laboratories were organized. At present with the latter concern. Connected with the development of transmitting apparatus, designed and operated the Arlington Trans-Atlantic Telephone Transmitter, 1915; invented constant current modulation system used extensively in commercial broadcast work and by the United States Army and Navy. Since the war continued research and development work in connection with ship to shore operation, trans-atlantic tests, etc. Mr. Heising is a Fellow of the Institute. He has contributed numerous papers to the PROCEEDINGS of the Institute and is a member of its Board of Direction. In 1921, he was the recipient of the Institute's Morris Liebmann Memorial Prize.

**Marconi, Guglielmo.:** Born at Bologna, Italy, April 25th, 1874. Educated at Leghorn Technical School. In 1895 began series of experiments on communication by means of Hertzian waves at which time he was able to transmit intelligible signals to a distance of 1½ miles. In 1896 he took out the first patent granted for a practical system of wireless telegraphy. He transmitted the first signals across the Atlantic in 1901. Senatore Marconi has been associated constantly with the development of radio telegraphy and telephony through his connection with the British Marconi Company and its associated companies. In 1920 Senatore Marconi was awarded the Institute Medal of Honor He has been a frequent contributor to the PROCEEDINGS of The Institute of Radio Engineers and is a Fellow of the Institute.

**Wheeler, Harold A.:** Born May 10th, 1903. Assistant in tests of radio receiving equipment, Bureau of Standards, 1921–1922. Associated with Professor Hazeltine in the study of neutralization of capacity coupling in vacuum tubes, 1922–1923. Engineering, Hazeltine Corporation in development of Neutrodyne receivers, including automatic volume control, 1924 to present. Received the B.S. degree in physics, George Washington University, 1925. Assistant in Physics Department, Johns Hopkins University, 1925 to date. Associate member of the Institute.

9

# INSTITUTE ACTIVITIES

## 1928 Convention Plans

PLANS for the 1928 Convention have been completed. The Convention will be held on January 9th, 10th, and 11th, the Convention Headquarters being in the lobby of the Engineering Societies Building, 33 West 39th Street, New York City.

Papers on the following subjects will be presented: "A Digest of The International Radiotelegraph Conference;" several papers on "Audio Frequency Amplifiers"; "The Making of Talking Moving Pictures" (with demonstration); "Radio Picture Transmission Symposium on Inter-Electrode Tube Capacities," and several others.

The inspection trips will include a bus ride through the new Holland Tunnel, through the Experimental High Power Station group at Whipanny, New Jersey, and the National Broadcasting Company's Station, WJZ; an inspection of the technical equipment of Roxy's Theater; inspection of the studios of the National Broadcasting Company; inspection of the F. A. D. Andrea plant; inspection of Aerovox plant. The plans call for a dinner with entertainment on the evening of January 11th.

## December Meeting of the Board of Direction

At the meeting of the Board of Direction held on December 7th in the offices of the Institute the following were present:

Ralph Bown, President; A. N. Goldsmith, Secretary; Melville Eastham, Treasurer; L. A. Hazeltine, R. A. Heising, R. H. Marriott, R. H. Manson, Donald McNicol, and J. M. Clayton, Assistant Secretary.

The following were transferred or elected to higher grades in the Institute:

Transferred to the grade of Member; T. G. Deiler, B. A. Engholm, W. H. Fortington, L. J. Gallo, J. J. Stanley and John A. Victoreen.

Elected to the grade of Member; E. A. Beane, L. B. Root, and J. K. Skirrow.

One hundred and thirty eight Associate and eight Junior members were elected.

## Bound Volumes

Due to the increase in the number of pages in the Proceedings throughout the year it has been necessary to increase the price of the 1927 *Bound* Volumes. These volumes in blue buckram can

# AUTOMATIC VOLUME CONTROL FOR RADIO RECEIVING SETS* 30

By
Harold A. Wheeler
(Hazeltine Corporation)

*Summary*—*A receiving set is described in which the radio-frequency amplification is automatically controlled to give a nearly constant radio-frequency voltage at the detector, independent of differences in antenna signal voltage. This results in nearly uniform response at the loud-speaker from nearby and distant broadcasting stations and also reduces the effect of fading. The method employed consists in using the rectified carrier voltage to adjust the grid bias of the radio-frequency amplifier tubes. There are indicated the solutions of special problems that arise in carrying out this method.*

IN the present radio receiving sets employing high amplification, it is necessary to adjust carefully a "volume control" in order to reproduce signals of different intensities with the same audible intensity from the loud speaker. There are various devices which could be employed to regulate automatically the

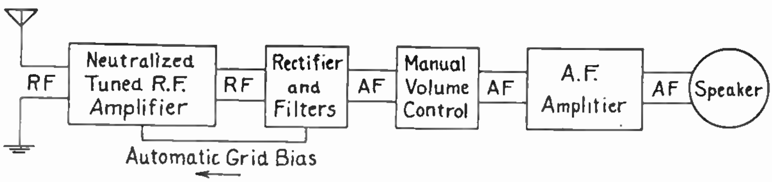



Fig. 1

amplification of the signal, some of which employ moving mechanical parts. It is the purpose of this paper to describe a simple electric circuit, without moving parts, in which the amplification is regulated automatically by the signal, and the loud speaker intensity reaches approximately the desired level for each signal, independent of the signal intensity and therefore irrespective of a reasonable amount of fading.

Any device to accomplish this object without introducing distortion of music or speech must operate by the signal carrier wave. Any variations in its intensity must be compensated by reciprocal variations in its amplification. The method to be described provides for controlling the radio-frequency amplifier, thereby maintaining the desired signal level in the detector or rectifier, audio-frequency amplifier and loud-speaker.

* Received by the Institute, October 6, 1927.
* Presented before the Institute of Radio Engineers, New York City, November 2, 1927.

Fig. 1 shows the outline of a set which has been constructed for broadcast reception, embodying this automatic volume control, comprising the following component sections. (1) A four-stage radio-frequency amplifier of the well-known Neutrodyne type, with UX 201-A tubes, the antenna circuit tuned by one dial and

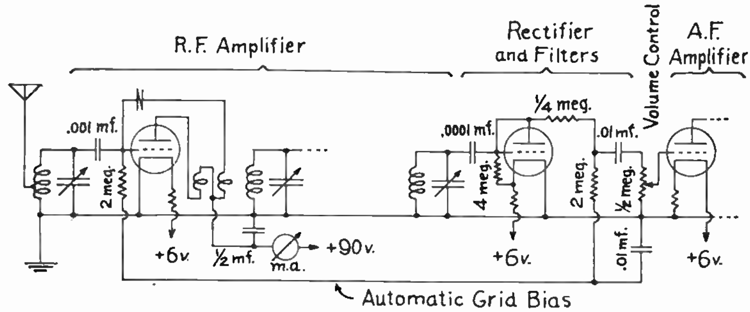



Fig. 2

the four coupling transformers tuned simultaneously by a second dial. The total amplification is controlled by varying the negative grid potential of the first three tubes. (2) A two-element rectifier with simple filter circuits to reject the radio-frequency currents

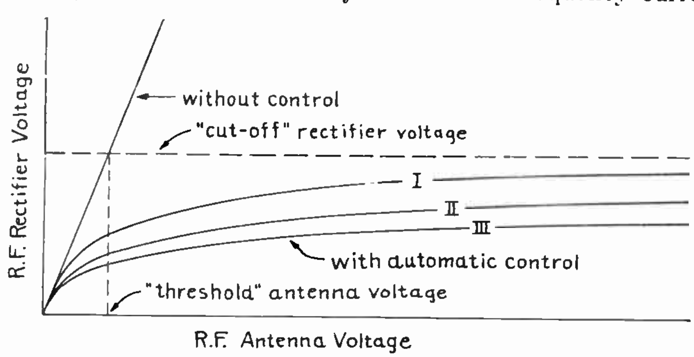



Fig. 3

and to segregate the direct and audio-frequency components of the pulsating rectified voltage. (3) A manual volume control in the form of a voltage attenuator connected to the grid of the first audio-frequency amplifier tube. (4) A four-stage audio-frequency amplifier and loud speaker. The entire set, excepting the last two audio-frequency stages, was enclosed in a grounded metal box divided into compartments, one for each tube with its preceding coupling circuit.

Fig. 2 shows the essential circuit details pertaining to the control system. The direct component of the rectified voltage, free of audio-frequency variations, is applied to the grids of the first three tubes. If the radio-frequency rectifier voltage could exceed a value of about ten volts, this automatic grid bias would thereby cut off the signal through the radio-frequency amplifier, so the rectifier voltage cannot exceed this value.

Fig. 3 shows graphically the comparison between the performance of the radio-frequency amplifier with and without the automatic control. With the system described, the rectifier voltage and audio-frequency voltages are nearly independent of the an-

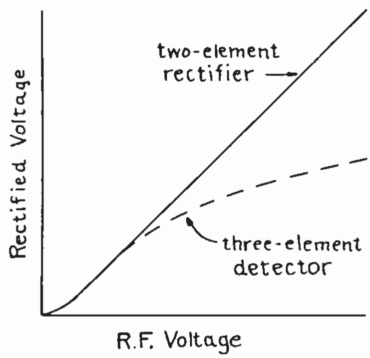



Fig. 4

tenna voltage, when the latter exceeds the threshold value. The curves I, II, and III show the performance of the system when the automatic grid bias is applied to one, two, or three tubes, respectively, of the radio-frequency amplifier.

The degree to which the signal can be cut off in one tube is limited by two factors. First, any error in neutralizing the grid-plate capacity permits signal current to pass through the tube, even when its mutual conductance is zero. Secondly, the sharp bend in the plate-current grid-voltage curve causes distortion of a strong signal on the grid, when the mutual conductance is reduced too far by the grid bias. In view of such limitations, it is undesirable to reduce the amplification ratio per stage below about 1/10 of its normal value. When controlling several tubes, these limitations become unimportant. The last radio-frequency stage is not controlled because it must supply as high as ten volts to the rectifier.

The properties of the two-element rectifier contribute largely to the simplicity of the control system. Fig. 4 shows the nearly linear proportionality between alternating and rectified voltages in this form of rectifier, as contrasted with the irregular performance of the three-element detector. The signal modulation is rectified without distortion. Also the average rectified voltage is equal to the rectified carrier voltage, while with a "voltage-squared" detector the average rectified voltage is proportional to the average total power of carrier and sidebands. This last feature is worthy of mention in connection with the control system, since the automatic grid bias should depend only on the carrier amplitude, independent of the modulation.

With the circuit constants shown in Fig. 2, the time constant of the circuit which connects the rectifier to the grids of the control tubes is 1/40 second, so that the control system comes nearly to equilibrium in 1/20 second. This time can be reduced further if necessary, but is ultimately limited by the allowable reduction of the signal modulation at the lowest audio frequencies.

In consequence of the automatic control action, it becomes difficult to tune the receiving set accurately by ear to a desired signal. The amplification of the controlled tubes is decreased as the response to the signal is increased by tuning, and vice versa, so that the point of resonance is indicated by minimum plate current in the radio-frequency amplifier. Taking advantage of this fact, a milliammeter (*m.a.*, Fig. 2) is connected in the plate circuit of the first tube, to be used as a resonance indicator, and also to give an indication of relative signal intensities.

There is an incidental problem in supplying the plate current to all tubes of the set described from a common rectifier and filter system. In the controlled radio-frequency amplifier tubes, when operating at low plate current, the signal carrier is modulated appreciably by small fluctuations in the plate voltage. Such fluctuations are caused by the plate current pulsations in the audio-frequency amplifier. In the presence of a strong carrier wave, these two effects may cooperate to generate a low frequency oscillation. This disturbance may be avoided by reducing the internal output impedance of the rectifier-filter, by decreasing the amplification at low frequencies in the audio-frequency amplifier, or by using separate rectifier-filter systems to supply the plate currents of the radio- and audio-frequency amplifiers, respectively.

The performance of the automatic volume control as described can be summarized briefly as follows. A maximum variation of signal voltage in the ratio of 1:1000, corresponding to differences in distance, fading, or tuning, results in a maximum variation of the rectified carrier voltage in the ratio of only about 1:3. This small variation, together with possible differences in the degree of modulation of different stations, can readily be compensated if necessary by adjusting the manual volume control for the audio-frequency amplifier, which also determines the "volume level" for the automatic volume control.

The name "Audiostat" has been selected for this device, by reason of its tendency to maintain the audible intensity at a constant value.

Attention might be called to British Patent 259,664 (Western Electric Co., July 14, 1925), in which a somewhat similar system is presented. This latter system is applied to a super-heterodyne receiving set, and is more involved in several respects than the system described in this paper.

It is desired to acknowledge the cooperation of the Howard Radio Company of Chicago, in whose laboratory the set described was assembled.

35 **Discussion***

**G. W. Pickard**: Mr. Wheeler has confined his bibliography to a single reference, British Patent 259,664 of July 14, 1925. May I suggest that the PROCEEDINGS of the Institute of Radio Engineers is in this case a more fertile field than patent files, for in a paper entitled "Short Period Variations in Radio Reception," presented before this Institute on December 12, 1923, and published in the PROCEEDINGS of April, 1924, will be found a description of the exact system of Mr. Wheeler's paper.

For convenience, I will give the quotation from my 1923 paper:

> So far as the varying intensity of the sound in the telephone receiver is concerned, there are several simple expedients which will markedly smooth out reception from a distant station. Thus, the grids in a radio-frequency amplifier train may be floated on a fairly large condenser, shunted by a high resistance. When reception is weak, the grids assume a small negative potential, and amplification is at a maximum. When the input rises, a large negative charge is built up on the grids, and amplification is reduced. *Similarly, a separate rectifier connected to the output end of the radio-frequency amplifier and to the grids will have the same effect.* (Italics mine. G. W. P.)

By accident rather than by design, some of the resistance-coupled radio-frequency amplifiers of 1917–1918 achieved a certain degree of automatic volume control. In these amplifiers rectification went hand-in-hand with amplification, with the result that a strong signal would build up so large a charge on the grids that amplification was greatly reduced.

Mr. Wheeler mentions the difficulty in tuning such a receiver by ear. This difficulty is due to his choice of a too-small time-constant for the rectifier-grid circuit. Inasmuch as the more important fading periods of broadcast reception are of the order of a minute or more rather than seconds or fractions of a second, it is not necessary or even advisable that the control should operate within a small fraction of a second. In my work with this form of receiver, I made the time-constant of the control about ten seconds, which gave ample time for tuning.

It is not likely that any system of automatic volume control will make a distant station behave just like a local. When one is located in the zone of most violent fading, that is, at such a distance from the transmitter that the direct and refracted waves are of the same amplitude, the field intensity at the bottom of a deep fade is usually below the disturbance level. Under these conditions, I have found that a receiver with automatic volume

* Received by the Institute, November 2, 1927.

control may maintain an approximately constant signal output from the loud speaker, but this will be periodically obliterated by the rising and falling noise level.

**E. Bruce**†: Several years ago during the ship-to-shore experiments conducted by the Western Electric Company, Mr. H. T. Friis, now of the Bell Telephone Laboratories, employed the automatic electrical gain control disclosed in British patent No. 259,664 to which Mr. Wheeler's paper refers. In this connection, a circuit suggested by Mr. Affel of the American Telephone and Telegraph Company disclosed in U. S. patent No. 1,511,015, October 7, 1924 is of interest.

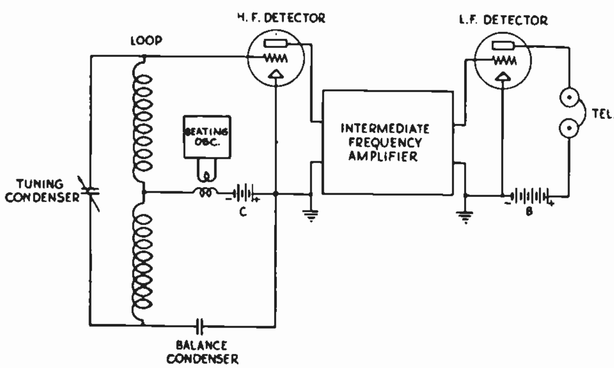



Fig. 1

The writer has devised a method of automatic gain control which is extreme in its constructional simplicity and at the same time avoids the range limitations experienced by Mr. Wheeler and also mentioned by Mr. Friis. Since this material has never been published, it seems appropriate, at this time, to present it to the Institute for consideration along with the method described by Mr. Wheeler.

The basic idea involved is to cause the rectified output to operate on the frequency changing device of a double detecton or "superheterodyne" system. Since the output of this device occurs at the intermediate-frequency, no signal output is possible when this device is made to be inoperative. In other words, we obtain a practically infinite operating range. This is in marked contrast to operating on amplifier tubes which require elaborate and accurate balancing schemes to prevent the signal from passing

Received by the Institute, November 10, 1927.
† Bell Telephone Laboratories, New York City.

via the inter-electrode capacities of the tube. Mr. Wheeler has pointed out that the range of such signal reduction is limited to the practical accuracies to which these balances may be adjusted.

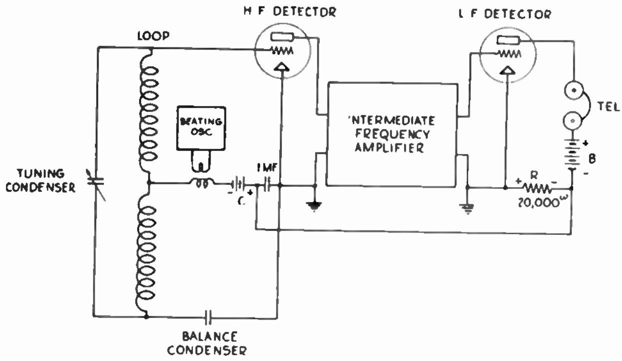



Fig. 2

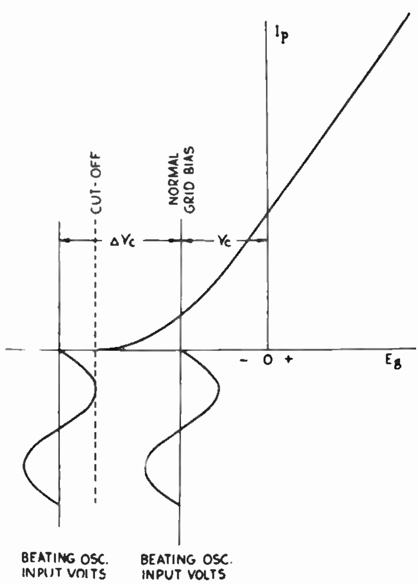



Fig. 3. Characteristic of High-Frequency Detector.

Fig. 1 shows an approved form of double-detection receiver. Fig. 2 indicates how it may be provided with an automatic control by simply adding a one micro-farad capacity and a 20,000-ohm resistance.

Fig. 2 shows that the rectified output of the low-frequency detector causes an increase in the IR drop across the 20,000-ohm resistance. This IR drop furnishes an additional negative bias to the high-frequency detector, driving that tube toward plate current cut-off. At cut-off the amplification of the receiver is totally destroyed.

Fig. 3 shows the change in negative bias necessary to reach cut-off. Referring to the figure,

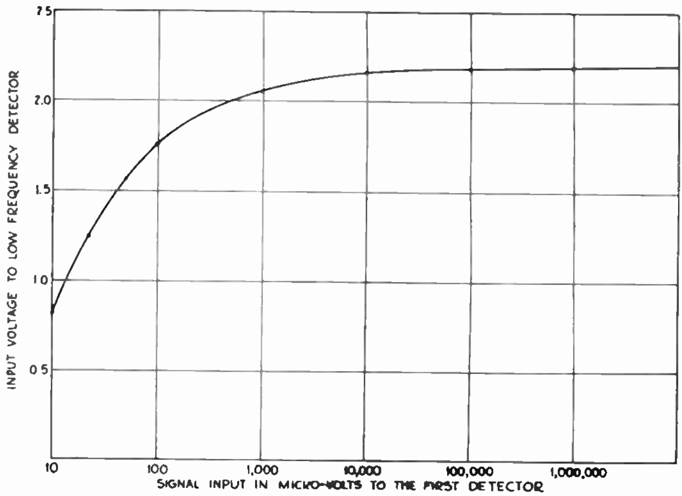



Fig. 4

$$\lim. \Delta V_c = V_s + V_{bo} + \frac{V_b}{\mu} - V_c$$

where $\Delta V_c$ = change in grid bias.

$V_s$ = peak signal voltage at detector input.

$V_{bo}$ = peak voltage of beating oscillator input.

$V_b$ = plate battery voltage.

$\mu$ = voltage amplification of tube.

$V_c$ = normal negative grid bias.

Let us assign practical values to these terms respectively in the above order, then

$$\lim. \Delta V_c = \text{negligible} + 1 + \frac{45}{6} - 6 = 2.5 \text{ volts.}$$

$\Delta V_c$ is the ultimate limit (signal voltage small compared with beating oscillator) of the biasing voltage that need be provided by the drop across the resistance $R$.

$$\lim. \Delta V_c = I_r R.$$

$$I_r = \frac{\lim. \Delta V_c}{R} \times 10^6 \text{ microamperes} = \frac{2.5 \times 10^6}{2 \times 10^4} = 125 \text{ microamperes.}$$

An average $N$ tube voltmeter characteristic shows that a change in plate current of 125 microamperes represents an input of 2.5 volts for a plate circuit load of 20,000 ohms. It is therefore concluded that the input to the low-frequency detector can never exceed 2.5 volts for any practical signal.

Fig. 4 is a measured characteristic of the signal input in micro-volts to the first detector vs. input voltage to the low-frequency detector. The intermediate frequency amplifier possessed a voltage gain of 120,000. If we start at a level of 1000 micro-volts and increase this signal 1000 times, the output will only increase by about 6 percent.

With the arrangement as described, the desired output may be manually adjusted by altering the value of the negative biasing battery marked $C$ in Fig. 2.

An automatic gain control of the kind described has been found to operate very satisfactorily. In the circuit used, the flat characteristic has been improved through the use of detector tubes having a voltage amplification constant of 30 instead of 6.

In the present state of development of the automatic gain control, there are two serious limitations: First—As the gain of the set rises and falls, in order to compensate for a fading signal, it is accompanied with a corresponding rise and fall of static and inherent set noise. This is quite disturbing to intelligibility. Second—It is common experience that speech quality is poor during the minimum of a fading period. An automatic gain control is incapable of remedying this situation.

## 5.2 The Diode Peak Detector 1926.

I was the first to use the diode peak detector for recovering the modulation of a signal or for developing a bias to control the gain in an amplifier. I first used a common diode detector for both functions, which came to be known as Diode AVC and Peak Detector. The concurrent evolution of both, and the functional interaction of their properties in practice, has detracted from the appreciation of the diode peak detector as a separate instrumentality. Here we shall consider the diode peak detector as a linear rectifier whose properties were found to be useful in various functions, especially in a radio receiver for a modulated-carrier signal. Its background was relevant to its evolution. The diode was a two-electrode device which was the predecessor of the triode, etc., with more electrodes.

The diode power rectifier was a concurrent development in a different context. Its only relevance was the common principle of rectification. It did use either of the two types of rectification that will be distinguished. Its functions were different, as follows:

- High-power, not received-signal.
- Low-frequency, not carrier-frequency.
- Constant-voltage, not modulated.

The result was separate lines of development. For example, the AIEE was the principal medium for the power rectifier, and the IRE for the signal rectifier or detector. I was active in the latter.

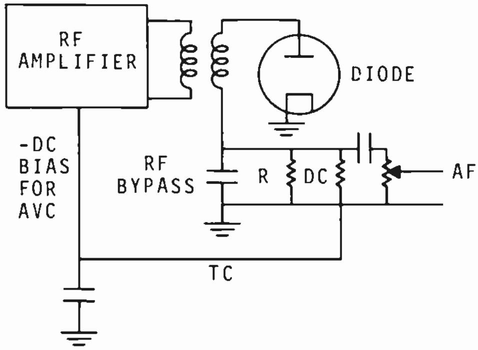
The diode rectifier in a radio receiver had two periods of development.

- The weak-signal detector, especially before the advanced development of the vacuum-tube RF amplifier. It was inherently "square-law" in response.
- The amplified-signal detector. It could be made "linear" in response. This feature became important for a radio-frequency (RF) carrier signal with amplitude modulation (AM) by audio-frequency (AF) sound.

The former period identified the diode in various forms. The latter identified different circuit applications. They were separated by a period of disuse of the vacuum diode in a radio receiver. This fact is relevant to the resurrection of the diode detector in the latter period.

In the early days of radio, the detection of a weak radio signal was accomplished by various weird and ingenious devices bordering on witchcraft. Then the diode rectifier emerged as the one destined to survive. It appeared in two principal forms:

- The thermionic diode or "Fleming valve" which was a rectifier by virtue of a hot cathode (filament) and a cold anode (plate) in an evacuated envelope (bulb or tube).



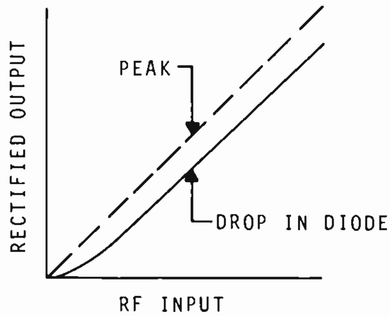

5.2 Fig. 1 — Diode peak detector.

- The crystal detector which was a rectifier by virtue of a junction at a contact between two dissimilar materials. A typical pair was a mineral (galena or silicon) and a phosphor-bronze (non-corrosive) wire or "cat-whisker". Years later, this was resurrected in solid-state physics. Its most common descendent today is the silicon junction diode (the nucleus of one line of transistors).

The weak-signal diode detector came before the advanced development of RF amplifiers. It was operated at such a low voltage that the rectification of a signal was inefficient. The rectified signal had to provide power to a telephone receiver (earphones). The latter was designed to present an impedance matching that of the diode so it would get all the available power. The physics of rectification dictated that a weak-signal detector had a "square-law" response, which gave lesser efficiency toward a weaker signal.

The square-law response was typical of the early diode detector and also of the triode detector which superseded the crystal detector in the early years of broadcasting (say 1920-25). For code reception, the distortion was harmless. With the advent of a carrier modulated by a telephone signal (voice or music) the distortion could be objectionable. The early broadcasting stations of highest quality held their modulation below 50 percent to reduce such distortion, at the cost of decreasing their useful coverage area.

Some triode detectors had a limited range of signal voltage over which the response was more nearly linear. This range was seldom exploited because the detector was usually followed by an AF amplifier while the signal on the detector was held down by a manual "volume control". This was the state-of-the-art in 1923-28 (which was the lifespan of the Hazeltine Neutrodyne).




5.2 Fig. 2 — Linear response.

Linear response was gradually recognized as an objective in the 1920's, mainly for sound reception but also for some other purposes. There were two distinct approaches to this problem, as follows:

- "Average rectification" of either half-wave of the signal voltage. While the diode current-voltage relation was not linear, it could be made to appear nearly linear by adding in series a rather high resistance.
- "Peak rectification" of the envelope of the signal voltage. Relying only on the unidirectional conduction of the diode, a rectified voltage could be obtained which would nearly follow the envelope. A (small) capacitor was charged nearly to the envelope voltage and was (slowly) discharged through a high resistance so the voltage would follow the modulation envelope.

By a remarkable coincidence, both of these were disclosed in adjoining articles in the same issue of the IRE Proceedings, 1928 JAN. (See the Contents of that issue, reproduced herewith.)

In one article, the famous Armstrong needed a linear detector for a keyed signal subject to interspersed noise pulses. He used a full-wave balanced rectifier of the "average" type. It was feasible at his rather low signal frequency (20 Kc). It would not have been so useful in the broadcast band (around 1000 Kc) because inherent shunt capacitance would have reduced the effect of this resistance in series with each diode.

In the next article, I disclosed the diode peak detector for AVC. It was destined to supersede the other type of linear rectifier. So far as I know today, that was the first publication of a peak rectifier to follow the modulation of a received signal.

I devised the diode peak detector for use in my AVC receiver 260102. It performed two functions, the signal detection and the carrier rectification for automatic gain control. The latter is detailed in the chapter on Diode AVC. The latter function was uppermost in my mind at that time. Soon I came to appreciate that linear response was essential to the best performance of both functions. It was provided in the circuit I used on that date.

The diode peak detector is shown in Fig. 1. This is a simple circuit which was first used in my design of the Fada KA receiver with diode AVC. The design was completed early in 1930. The peak detector was like the one I had demonstrated four years before, but with some refinements. The RF input voltage was inductively coupled in series with the output circuit, facilitating the removal of RF from the high resistance (R) of the output circuit. The rectified voltage (nearly equal to the signal envelope) was developed across R. It was utilized in two parallel paths, each presenting much higher resistance. Either or both could have been utilized from one diode circuit:

- The rectified AF modulation of the signal, free of DC, was coupled to a voltage divider (socalled "potentiometer") which was operated by a manual knob for "volume control". (A voltage divider of such high resistance was not available when I first made the diode peak detector.)
- The rectified carrier was conducted through another path to provide a negative bias for gain control in the preceding RF amplifier. This path included a "time constant" (of about 0.1 second) to remove the AF fluctuations.

Each of these paths was intended to take much less current than the high-resistance leakage path (R).

The peak detector was efficient, in that nearly all of the power taken from the RF input circuit was rectified and delivered to the output circuit. It was used as a "voltage-responsive" device, in that it took much less than the "available power" from the RF circuit. The output circuit delivered its voltage to "voltage-responsive" circuits. With the 227 diode, as used in the Fada KA receiver, typical values were 0.1 megohm for R and 1 megohm for each parallel path.

The linear response is graphed in Fig. 2. It shows the drop from the peak voltage, which was small as a result of the high value of the leakage resistance R. The response was nearly linear for voltages exceeding a fraction of one volt.

I found a way of computing the drop in the diode. It was given in Report 451-AW, 340709. It has not been published elsewhere so the resulting formula is given at the end of this chapter.

The first publication of the diode peak detector in a radio receiver was the preprints mailed before my talk to IRE 271102, followed by the Proceedings 1928 JAN. Its first use was in my designs for the Philco 95 and the Fada KA models (1929-30). My subsequent experience and studies were reported in 451W (331226) and 451-AW (340709). Some were published in a patent (340320) and an IRE paper (1938 JUN). In the meantime, some related studies were made by Dr. Charles Travis (RCA License Lab. 1931-35) and became available to me early in 1938.

My 1938 IRE paper explained and formulated various peculiarities of the diode peak detector in a practical circuit. It was a comprehensive treatment of the behavior of an ideal diode, described as one having zero resistance to current in one direction and zero conductance to current in the other direction. This simplification led to various relations involving the loading of the RF input circuit and some limitations on following the modulation in the output circuit. One result was the description of a constant-resistance output circuit in which the rectified carrier and modulation components could be separated. That was the subject of the second patent. I do not know of any use of that improvement. Another result was the description of the reaction of the AC output circuit on the modulation envelope of the input signal. That was to form the basis for some understanding of a diode mixer many years later, for use in a superheterodyne receiver.

Another application of the diode peak rectifier is the "AF Vacuum-Tube Voltmeter" described in a separate chapter.

With the transition from vacuum diodes to solid-state diodes, the peak detector was carried over intact. It became even easier to design with a high degree of refinement. The silicon junction diode was a great advance over the silicon crystal detector of half a century before. It became more nearly a complete rectifier, so the high-resistance leakage path could be realized. In AM receivers, it is the detector still in common use today and in the foreseeable future.

The diode peak detector was the subject of two patents. The first was based on my early use in the Washington receiver and the first AVC designs. The second covered refinements which saw no use, so far as I know.

There was one patent suit based on the earlier patent with the broader coverage:

> Hazeltine Corp. vs. General Electric Supply Corp. (on G.E. receivers) in U.S. District Court in Baltimore, Md., (Fourth Circuit) before Judge Coleman. Trial 371027-1104 (7 days). Settled before a decision, by G.E. Co. taking a license 380414.

In the same case we asserted our MacDonald patents on the high-inductance antenna circuit, described in the chapter on "Uniform Gain". That part is omitted here.

Our attorney was R. Morton Adams (PDME) and he relied on Prof. Hazeltine as expert witness. The defense attorney was again Stephen Philbin, who again used Waterman as expert witness. I participated but did not testify, so I infer that the story of my invention may have been stipulated from the Wilmington AVC trial the previous year.

The trial was confused by the two different subjects being interspersed in one presentation. The subject of the peak detector was further confused by the "tidal reservoir" analog which was the Professor's response to the Judge's groping for some understanding. The analogy was beautiful but equally elusive to understanding. Also the Judge invited Prof. Ferdinand Hamburger of Johns Hopkins as a referee to listen and advise the Court. This might have introduced views that we would have had no opportunity to challenge. There was no decision, because the case was settled by General Electric Co. taking a license under all patents of Hazeltine Corp.

**Computation.** Here is my formula for computation of a peak detector, as given in Report 451-AW, 340709. It was based on a close approximation for the diode average current, under these assumptions:

- Diode current-voltage relation following the 3/2-power law, which is the ideal for a unipotential (indirectly heated) cathode and current limited by space charge.
- Input from a pure sine wave.
- Output bypassed at input frequency.

The instantaneous diode current is taken to be:

$$I_a = H_a (E_a)^{3/2}$$

A reference voltage is defined:

$$E_1 = \frac{128}{9} (G/H_a)^2$$

Then the response ratio is:

$$E/E_c = 1 + \frac{1}{2}(E_1/E_c)^{1/2} - \sqrt{(E_1/E_c)^{1/2} + \frac{1}{4}(E_1/E_c)}$$

$$= \frac{1}{1 + \frac{1}{2}(E_1/E_c)^{1/2} + \sqrt{(E_1/E_c)^{1/2} + \frac{1}{4}(E_1/E_c)}}$$

in which:

$I_a$ = instantaneous current in diode.
$E_a$ = instantaneous voltage on diode.
$E_c$ = peak voltage of sine-wave input.
$E$ = rectified voltage.

$E_1$ = reference value of $E_c$, at which the tangential 3/2-power response intersects the asymptotic linear response.
$G$ = $1/R$ = leakage conductance.
$H_a$ = 3/2-power conductance of diode.

The second form of the quadratic solution (inverted) was not then found in the usual handbooks (such as Dwight) so I devised it for this purpose. (Later I found it in Hudson, an earlier handbook, also from MIT.) It is here the more useful, in avoiding a small difference. The tangential 3/2-power response for a small voltage is:

$$E/E_c = (E_c/E_1)^{1/2}; \quad E/E_1 = (E_c/E_1)^{3/2}$$

The asymptotic linear response for a large voltage is:

$$E/E_c = 1 \,; \quad E/E_1 = E_c/E_1$$

The relative drop from linear response for a large voltage may be expressed:

$$E/E_c = 1 - (E_1/E_c)^{1/2}$$

For the circuit values mentioned above, $E_1$ is so small that the 3/2-power assumption fails for small voltages. The reason is the thermal distribution of speed of emission of electrons from the hot cathode. The response to a small signal then becomes square-law below about 0.1 volt, and nearly linear above that value.

My computation came out in explicit closed form for the 3/2-power diode, which is the ideal space-charge model. This was a fortunate coincidence.

[A] [W21] "Design formulas for diode detectors", Proc. IRE, vol. 26, pp. 745-780; Jun. 1938. (Report 517W, 341201.)

[B] H. A. Wheeler, U.S. Patents.

| | Pat. No. | Issued | Filed | |
|---|---|---|---|---|
| (1) | 1951685 | 340320 | 310401 | |
| (2) | 2018540 | 351022 | 340217 | div. 310401 |

[C] HAW, Hazeltine Reports.

| Rep. No. | Date | Title |
|---|---|---|
| 451W | 331226 | Linear rectification using the diode. |
| 451AW | 340709 | Computed rectification of the diode. |
| 517W | 341201 | Design formulas for diode detectors. |

[D] See chapter 9.6 on AF VTVM.

**5.3 The Tuning Meter 1926.**

My first demonstration of automatic volume control (AVC) included a feature which was not previously needed. It came to be known as the "tuning meter". It was an indicator so connected as to display the change of amplification or gain in response to the automatic control. When the sharpness of tuning was blunted by the AVC, this indicator was a valuable aid in tuning, hence its name.

Among my many U. S. patents, only one has been adjudicated as to validity. It is my patent on the tuning meter.[C] (12) We challenged the Patent Office rejection of my claims for "lack of invention". We appealed to the U. S. Courts in Washington. The U. S. Court of Appeals for the District of Columbia finally declared my claims to be valid and ordered their allowance.

The tuning meter was used in the first commercial broadcast receiver with AVC of any kind, the RCA Radiola 64 in 1928.[M] This was soon after my IRE publication. In later years, the tuning meter was used in various forms, such as the RCA "Magic Eye".[K]

The original tuning meter was a small portable milliammeter (actually a "voltmeter" with full scale 15 ma., Weston Model 280). It was connected to indicate the plate current of the first stage of the bias-controlled IF amplifier. It was used in my first demonstration 260102. It did not appear in my notebook until later. It is shown in my first patents and my IRE paper.[A].

The meter so connected gave a compressed indication of the variation of gain during reception or tuning. Its reading decreased with control so it never went off scale. After tuning, its reading gave an inverse reading of signal strength, reacting to "fading". In later years, it was termed a "sensitivity meter" (S meter) and its reading was used as an inverse measure of the signal strength.

My first patent application on AVC, filed 270707, was eventually titled "Visual Resonance Indicator" and was issued 10 years later, 370518, to claim the tuning meter.[C] (12) The other features were separated in divisional applications, all of which issued sooner.

The Primary Examiner in the Patent Office did not find any prior disclosure of a meter connected to respond instantly to gain variation under automatic control. He took the position that it was not an invention. We maintained that is was a new and useful improvement, accomplishing a result not previously realized. It was the first time a meter was a convenience in tuning a receiver to one of various signals over a wide range of signal strength.

On 320125, we demonstrated my Washington receiver before the Board of Appeals in the Patent Office (Messrs. Thurber, Hopkins and Morgan). They sustained the rejection of our claims to the tuning meter.

Then we took our case to the Supreme Court of the District (D.C.), which had the status of a District Court of the U. S. That trial was conducted before

Justice Letts on 350228-0301 by Adams and Guild, with Ralph H. Langley as our expert witness. I testified as the inventor. That Court sustained the rejection by the Patent Office.

We appealed our case to the U. S. Court of Appeals for D. C. It was argued before the five Justices by Lawrence B. Dodds (head of the new Patent Dept. in the Company). Their opinion, written by Associate Justice Robb, supported a final decision in our favor on 361207. He ruled that I was entitled to a patent because I had "invented or discovered" a "new and useful improvement". This decision was an adjudication of the validity of my claims, which were finally issued 370518.

The tuning meter has been used in some form in various designs of high-grade or specialized receivers, but not in small sets. It has been used in various forms, some quite different from a meter. I proposed a simple light with brightness variation. By using that as the dial light, the operator was able to see the scale and the tuning indicator at the same place. The tuning light was a low-current filament or neon tube. In one type of light, it was designed to appear like a vertical thermometer with varying height of illumination.[C] (13) Two other ingenious forms were the following.

In 1936, RCA sold a special tube (Type 6E5) named the "Magic Eye".[K] A small fluorescent screen was seen in the end of a glass bulb. A bright circle was seen with a gap whose angle was controlled by a bias to act as a meter. The Magic Eye was used in many broadcast receivers.

In the Bayside lab early in 1937, we made a sophisticated tuning indicator using a small cathode-ray tube (screen diameter one inch, type 913).[C] (15), [F] It was especially suited for an AM receiver with AVC and AFC (automatic frequency control). The latter provides automatic fine tuning when the dial is set near the correct tuning. The bright spot was deflected in vertical and horizontal directions by voltages from the "discriminator" circuit of the AFC. The spot described a circle each time the receiver was tuned through the frequency of an AM signal. It was normally at rest on the bottom of the circle, and reached the top when the tuning was correct. This rotary motion of the spot was a spectacular indication of each station tuned in. This indicator was not usually used for the original purpose of the tuning meter, to show any change of the gain. The discriminator was usually preceded by a limiter whose output was insensitive to gain and signal strength.

In summary, the tuning meter was an interesting and useful accessory to AVC. It showed clearly the operation of the automatic control, which was its purpose in my first demonstration of AVC. It was found to be a valuable aid in tuning a receiver with AVC. It was found useful also as an inverse indicator of relative signal strength. The Patent Office ruled that it was not an invention. The U. S. Court of Appeals finally decided that it was an invention, and that I was entitled to a patent. Ten years elapsed from the filing of the application (my first on AVC) and the issuance of the patent.

The references for this chapter are the following, appended to the chapter entitled "Automatic Volume Control 1925".

[A] [W03], [C] (12) (13) (15), [F], [K].

## 5.4 The Diode AVC Patents and Litigation (1932-41).

The AVC litigation involved U.S. Patent 1879863 (the "original patent", nicknamed -863) and its successor, Re.19744 (the "reissue"). Their scope is what has been called diode AVC, based on my invention at the end of 1925. The patent was issued from a division of a comprehensive parent application disclosing various forms of AVC (Automatic Volume Control, now termed Automatic Gain Control or AGC).

The purpose of this chapter is to place on record my view of the litigation in which we were unable to successfully adjudicate the patent covering the one most important invention of my career. The result left me disillusioned and embittered toward our system of patent law and enforcement.

Presenting my view in proper perspective necessitates something like a retrial and argument, tempered with whatever understanding comes with review. I am sympathetic toward the human frailties involved at every stage. However, I do not profess tolerance toward a system that fails because it perpetuates error. It is my intention to give facts and interpretations that will bring out not only the soundness of our position but also the difficulties of the entire procedure.

My invention met all the definable criteria. It achieved a new result with a new circuit arrangement. I was the first to conceive it and I promptly reduced it to practice. I was working alone. I designed the receiver in which it was marketed and found commercial success. It was copied and went into common use. It was finally recognized by RCA, after a full trial, taking a license without waiting for a decision.

Hazeltine Corp. had rights to the invention. Our patent attorneys filed patent applications in which claims were allowed and issued. We asserted these claims against "carbon-copy" infringements.

Two cases in EDNY were decided against our patent. We appealed to CCA-2 and Judge Hand ruled that there was no invention.

In response to comments in one of the lower-court opinions, we had filed a reissue application with more specific claims. The reissue was allowed over all these adverse decisions, and issued shortly.

We asserted the reissue claims against a "carbon-copy" infringement by our most powerful adversary, RCA. The trial occurred in Wilmington. Instead of relying on the presumption of validity, we undertook to prove the fact of invention. By my testimony, we presented the making of the invention. By testimony of experienced engineers, we established that it was not obvious and was adopted over other attempts to solve the same problem. Without waiting for a decision, RCA settled the case by entering into a royalty agreement. Unfortunately, this settlement deprived us of a favorable decision and positive adjudication of the reissue patent in suit.

In spite of the acquiescence of most major companies, one still refused to take a license. That company was Detrola in Detroit. Against them, we reenacted in Detroit the Wilmington trial. The District Court ruled that the reissue was valid and infringed. It was upheld on appeal to CCA-6 in Cincinnati.

The Detrola case was appealed to the U.S. Supreme Court. They took the appeal as a conflict between CCA-2 and CCA-6, though the latter case was based on more specific claims and further factual evidence. The Supreme Court reaffirmed the earlier decision of CCA-2. My patent was struck down after 10 years of its 17-year statutory life.

The channels of rationale which led to this result are one subject that I wish to report as history. They were tortuous so their reporting is laborious. Especially I wish to report the events that left openings toward this result, however erroneous it may have been.

- From hindsight, we may see that the original claims could have been expressed more clearly and should have been keyed to the specification in the same words. The former was partially remedied in the reissue, but after damage had been done.
- From hindsight, we may see that we should not have relied at first on the presumption of validity, though all the tests were met. That reliance was unrealistic in view of the bias against sustaining a patent, which had been building in the courts.

In general, we relied on presentation of an honest position, based on technical reality and conscientious interpretations. The Court, being unfamiliar with the subject, gave too much weight to the simpler positions offered by opposing counsel, which were supported only by the advocative testimony of his expert witness.

Admittedly there is no absolute test of what constitutes an invention, especially a "patentable invention". I believe that the unfavorable decisions were based on errors of fact and conjecture, so they resulted in a mistaken final decision. My only hope of counteracting the adverse decision lies in a fair sampling of what I regard as significant errors in arriving at that end result.

The original patent with its 13 claims was allowed after the usual processes in the Patent Office. The first trials made us aware of the need for some more specific claims. These were filed in a reissue application, which was shortly issued with 13 claims (including a few the same). The significant dates were:

260102 Demonstration of invention.
270707 Filing of parent application.

301113 Filing of divisional application.
320927 Issuance of "original" patent.
340926 Filing of reissue application.
351029 Issuance of "reissue" patent.

The litigation to be reviewed occurred in four phases, as follows:

(1) On the original patent, several infringement suits were filed against "carbon copies" of my circuit. They were contested as to the validity of the patent and infringement claims. After two adverse decisions in the U.S. District Courts (EDNY), the Circuit Court of Appeals in the Second Circuit (CCA-2) ruled "that there was no invention". By that time we had filed the reissue application and it was about to issue.
(2) On the reissue, we filed suit against RCA, it was tried in Wilmington, and RCA settled while waiting for a decision.
(3) On the reissue, we then filed suit against Detrola, and it was tried in Detroit. Both the District Court and the Circuit Court of Appeals (CCA-6) ruled that the reissue was valid and infringed.
(4) The last decision was appealed to the U.S. Supreme Court as a conflict between CCA-2 and CCA-6. That Court ruled that I was not "the first inventor", reversing CCA-6, so the reissue claims became invalid.

**First phase**. Of the four cases in phase (1), two were tried on the merits, one before Judge Campbell and the other before Judge Galston. Our attorney was Willis H. Taylor, Jr., who relied on Ralph H. Langley as expert witness. The defense attorney was Samuel E. Darby, Jr., who relied on an expert witness who shall here be nameless. He was a young engineer who later became famous, but I shall be criticizing his testimony at one trial.

(1a) Hazeltine Corp. vs. Davega — City Radio, Inc. (on a Zenith receiver) was defended by Zenith. In a hearing 340103, it was postponed. It was settled with a consent decree limited to Zenith apparatus.
(1b) Hazeltine Corp. vs. R.E.B. Service Corp. (on a receiver, Kolster Model K-130) in EDNY before Judge Byers. Consent decree in respect to the validity of the patent and the infringement by this receiver.
(1c) Hazeltine Corp. vs. R.E.B. Service Corp. (on receivers of Bosch, Colonial and RCA-Victor) in EDNY before Judge Campbell. Decision 340910. After previous consent by this defendent on another receiver (K-130) the validity could not be challenged. Infringement was challenged. Decision was non-infringement.

(1d) Hazeltine Corp. vs. Abrams et al (on a receiver, Emerson Model 39) in EDNY before Judge Galston. Trial 340618-22. Decision 340806, invalidity. Appealed to CCA-2. Argument 350306 before Judges L. Hand, Swan and Chase. Decision (Judge Hand) 350729, invalidity.

Claims 1, 5, 6 and 10 of the original patent were selected by Taylor to form the basis for these suits. As an introduction to the language, claim 10 is quoted here. It is the most specific of the group.

(a) In a signaling system,
(b) a vacuum tube amplifier
having a cathode and a control electrode,
(c) a diode detector coupled to said amplifier,
said detector having an anode,
(d) means for maintaining said anode normally negative
relative to at least part of said amplifier cathode,
(e) means for causing said anode to become more negative
in the presence of an amplified signal,
(f) and a direct-current connection
between said control electrode and said anode,
(g) whereby the amplification of said amplifier
is regulated automatically.

This language is relevant to both validity and infringement, as will be discussed.

The diode AVC of my invention was essentially the AVC circuit that was peculiar to the receiver I demonstrated 260102 in Washington. It was not used by anyone before that date. I disclosed it in a patent application, in publications, and in practical designs for manufacture and sale under license from Hazeltine Corp. Unlicensed manufacturers copied these designs with only the minor variations that were characteristic of different models. Taking this set of circumstances as a matter of fact, the invention still required definition in the sense of patent practice and staking out a claim.

To this end, we attempted to define the invention in terms of a set of features which distinguished it from what had been disclosed before (the "prior art"). For example, claim 10 described a model that was, at least, different from all the prior art we knew then (or today) and was representative of my circuit. That alone is persuasive but not conclusive in establishing a patentable invention. There is no definite criterion, then or now.

**My invention.** Here I shall attempt to identify some features peculiar to my invention in a preferred embodiment, so they can be correlated with the closest prior art. Some are intended for a broadcast receiver in the home, rather than a point-to-point receiver in a professional station.

(1) An amplifier at the carrier frequency of the signal, subject to control by a negative DC bias.
(2) A linear rectifier or detector to derive an average negative bias proportional to the carrier but independent of the modulation.
(3) Applying the amplified signal to a rectifier, and using the bias to reduce the amplification, more for a stronger signal.
(4) Applying the control bias through a DC connection with a time constant that is small but sufficient to preserve the modulation.
(5) Holding the amplifier and rectifier output nearly constant over a wide range of signal strength, below the overload level and within the range of linear rectification.
(6) Accomplishing this result while tuning between a weak signal and a strong signal.
(7) Using a diode rectifier with a high resistance as the simplest linear rectifier or peak detector, responsive to the signal envelope.
(8) Using such a linear detector for recovering the modulation free of distortion.
(9) Using the same rectifier for control bias and detection of modulation.
(10) Placing the anode (output electrode) of the rectifier negative from the amplifier cathode, so no intervening extra B battery is required.
(11) Using a diode rectifier, with all cathodes (filaments) connected to a common A battery.

My Washington receiver 260102 combined all of these features. Its short designation was "diode AVC and peak detector". It was the principal subject of the original patent application, described with reference to Figs. 1, 2 and 8. This description supported all the claims. The visual indicator is ignored here, being covered in another chapter on the Tuning Meter.

**The prior art**. Before commenting on the two decisions, the "prior art" is here introduced. It was represented by the following five relevant U.S. patents and a number of other U.S. patents which showed only some components used in my invention. No other prior art was cited.

| | U.S. Pat. | Issued | Filed |
|---|---|---|---|
| Friis | 1675848 | 280703 | 241129 |
| Affel | 1574780 | 260302 | 211005 |
| Affel | 1511015 | 241007 | 220504 |
| Evans | 1736852 | 291126 | 230924 |
| Evans | 1869323 | 320726 | 230924 |

The following comments are relevant to timing.

- Friis was "published" 250714 in the form of British patent 259664 (not in any U.S. medium) a few months before I completed my invention. I learned of it afterward, probably while preparing my patent application.
- Friis was the latest to be filed. It was the only one shown in a radio receiver, directed to the problem of "fading".
- Affel-780 was the only one not cited by the Examiner in the Patent Office. It was published shortly after I completed my invention. The others were considered before my claims were allowed.
- The two Evans cases were based on the same application. Both were issued long after my filing date, so they were copending.

These three inventors were among the most prominent in the Telephone Co. They were much senior to me and had the advantage of familiarity with the latest developments not yet published.

Each one of these examples of the "prior art" was lacking most of the listed features of my invention. This summary is intended to place each one in proper perspective.

- Friis showed only (1) and (3), the latter in the form of a triode with extra B battery.
- Affel-780 showed only (10).
- Affel-015 showed none.
- Evans showed only (1), (3), (4) and (9), the rectifier being a triode with an extra B battery.
- Friis and Evans having (1) and (3), are operative for some purposes in a modulated-carrier receiver, but the two Affel circuits are not.
- Affel-780 is the only example of (10), the anode negative so no intervening extra B battery is required, and has none of the other features.
- All relate to some form of automatic control by negative bias.

**Judge Campbell** (1c) was presented only with a question of infringement, because the defendant had accepted validity by his consent in the previous case on the same patent (1b). The Judge adopted the position of plaintiff's attorney Darby, that the claims should be restricted in scope, in a manner which amounted to an attack on their validity. From a tortuous rationale, the Judge decided that the claims so interpreted were not infringed. I shall point to mistakes of fact in his decision, which presumptively influenced his decision. He paid no attention to our position, that the accused receivers were essentially "carbon copies" of my designs made in accordance with the patent disclosure. He did not dispute our position that our claims in suit, taken on face value, were infringed.

Among the accused receivers, the closest copy of my design was the RCA Victor. The most specific claim in suit was claim 10. I shall take these together as an example of the most extreme position adopted by the Judge.

The Judge said, "As we have seen from our consideration of the prior art, Wheeler's sole contribution to the art was teaching the use of a single three-electrode vacuum tube detector, the two cold electrodes (grid and plate) of which are connected together to form an output electrode of a diode detector, and which serves the double function . . ."

The patent made it clear that a diode or two-electrode rectifier can be made by so connecting a triode. The universal tube for receiver use at that time was the triode, so I used one as a diode. Actual diodes for receiver use were sold later, as a result of the use of the diode AVC I had introduced.

Making a distinction between a diode and a triode so connected was an argument advanced by Darby. It was a layman's fabrication and had no basis in the testimony of his expert.

The "double function" and everything else in that paragraph was clearly found in the RCA-Victor receiver, and the Judge did not dispute it.

In order to cover conceivable variations within the scope of the invention, the electrode from which the control bias was obtained was termed "output electrode". I shall point to one position taken by Darby and supported by his expert.

The RCA-Victor set used the latest RCA tube type for the diode. In one tube envelope, there were a diode and a triode with separate leads to pins in the base. Darby got his expert to testify that one tube could have only one "output electrode", and here that had to be the triode anode from which the amplified audio signal was derived. In other words, the diode anode was an "output electrode" if the diode were in a separate envelope, but not if it were housed with a triode. I could not understand how such a position could be asserted by a promising young member of my profession. It should not have affected the interpretation of claim 10, but the Judge made no distinction. He did not respond to our rebuttal testimony and argument.

One relevant patent in the prior art was Affel-780, which was not cited by the Examiner. It will be discussed with reference to Judge Galston's decision on validity.

Darby argued and the Judge adopted a rationale contrived to relieve the three different accused receivers from infringement, but for different reasons. Our rebuttal could not be made simple and it did not prevail.

I can appreciate the dilemma confronting Judge Campbell. He adopted Darby's position and gave little or no weight to our rebuttal.

**Judge Galston** (1d) was presented with the usual questions of validity and infringement. He ruled against validity.

We asserted the broader claims (1, 5, 6 and 10) so these were subject to his test of validity. Some others claims (such as 2 and 3) were more specific and might have passed the test.

There was supposed to be a presumption of validity, but the Judge did not accept that. Referring to the patents put in evidence by the defendant, he perceived that "greater insistence at the trial was placed upon those patents that were not cited" in the Patent Office. This may have implied an attack on the presumption of validity after issuance. The attack on validity was the usual "lack of invention" and "anticipation".

Affel-780, which was not cited in the Patent Office, was argued to consititute "a complete disclosure of the Wheeler patent". After some discussion, the Judge concluded:

"It would thus appear that Affel discloses a combination which in operation and result meet the alleged novelty of Wheeler's arrangement".

The subjective element of this conclusion was "alleged novelty".

In arriving at this conclusion, the Judge stated that, in Affel, the rectifier anode "becomes increasingly negative in the presence of an amplified signal". This was a feature of my patent but it was not true of Affel. His rectifier was not exposed to the amplified signal. It was exposed to the amplifier input signal, before amplification. At the time of this trial, all of us knew that this order would have been useless in a receiver for a wide range of signal strength. It would not have protected a preceding amplifier from overloading. On a strong signal, it would have completely cut off the succeeding amplifier, nullifying the signal output. This result was indicated in Affel's Fig. 8. The levelling result conceived in Affel's Fig. 5 was computed by a careful adjustment of rectifier and amplifier properties. It was predicted for only a small range of signal strength, in contrast to the large range encountered in a radio receiver.

Affel gave no description of the signal and no mention of how it would be affected by the bias control. He said he intended "no distortion" but he did not say what this meant or how it would be accomplished. While my asserted claims were silent on the character of the signal, my disclosure included a complete description of the signal and how it would be protected during control.

Judge Galston made several remarks, conditional on the claims being "narrowed", which he said was not "permissable". For one purpose, he said they would have to be narrowed to:

- a reception system as distinguished from the transmitter art;
- "regressive" as distinguished from "progressive"; and
- "time delay".

Our expert Langley termed as "regressive" control, the feature of controlling a preceding amplifier in response to the amplified output signal. This was a feature of my disclosure. The opposite was termed "progressive" control. It was shown in Affel-780. The former was useful in a receiver; the latter was not. The former was clearly indicated in my claims, specifying the control bias as "increasingly negative in the presence of an amplified signal".

The "time delay" was included in my disclosure as a time constant in the direct-current connection of the control bias. It was implicit in the term "direct-current connection". The time constant was what restricted this connection to a direct-current bias, free of fluctuation at modulation frequencies. This was clear in my disclosure, even to circuit values.

From his misconception of Affel-780 relative to my claims, the Judge decided they were invalid. His misconception was mainly the failure to appreciate the "regressive" feature in my claims, while recognizing that it was the opposite of Affel.

Judge Galston's decision rested on some misconceptions as to technical facts. Some of his comments were an invitation to remedy by reissue.

**Judge Hand**. We appealed to the CCA-2 from Judge Galston's adverse decision (1d). Referring to the opinion below, Judge Hand (for CCA-2) said:

> "We reach the same result as he, and though we follow a somewhat different course, we adopt his statement in general".

To me as a layman, any difference between the two opinions cast doubt on the rationale of either one. Only Judge Galston had seen the physical exhibits and the witnesses and had heard the testimony. Without the benefit of this experience, Judge Hand was more severe in his ruling against validity. His opinion prevailed by rank and ultimately reached the Supreme Court.

Judge Hand's opinion based on the asserted claims of the original patent was destined to carry undue weight against the revised claims of the reissue when the Supreme Court considered conflicting opinions form CCA-2 and CCA-6.

Judge Hand, even more deeply than Judge Galston, undertook to say what was invention or what was engineering skill, competent designing, common ability, patient and exhaustive experiment, etc. The following phrases, taken in context, are examples:

- "it does not appear to us how that choice was significant".
- "it certainly could not have required much ingenuity".
- "it took no ingenuity".
- "the mere notion was obvious enough".
- "quite naturally suggests itself as perhaps feasible".
- "nothing was nearer at hand".
- "there was no need of this".

- "it seems a natural expedient".
- "when, as in a receiving set, compactness becomes an end, why not eliminate the "B" battery necessary to a triode?"
- "Nothing of the sort is even intimated in the claims". (Comment: it was implicit in one phrase in the claim.)
- "the specifications do not themselves declare that the 'time constant' is an inevitable essential". (Comment: the specs were clear that it was essential.)
- "at best, Wheeler could not have regarded it as an essential feature". (The "time constant", same comment.)

His comment on the "B" battery is so naive that it could not have been a considered opinion. His comments on the "time constant" are inconsistent with my description. The latter were especialy relevant to the question of invention.

The Judge gave little weight to the history of the invention and its success, which may have been inadequately presented. He did admit:

> "the subject is all very unfamiliar to us; we must proceed quite in the dark, guided only by the interested advice of those whose conclusions we are personally unable to check".

Judge Hand expressed the opinion that "there was no invention to be saved" by limiting the claims, as by interpretation. This amounted to a withdrawal of Judge Galston's implied invitation to reissue. Our application for reissue had been filed just before Judge Hand's opinion was published. The reissue was allowed by the Examiner with knowledge of that opinion.

The keynote of Judge Hand's opinion was correctly stated:

> "None of these patents being complete anticipations, the question is as to the importance of the step from them to Wheeler".

He answered this question by conjecture unsupported by factual record. He decided there was no invention, so my original patent was invalid.

**Comments**. In retrospect, I have some comments relating to our prosecution of the patent application and the early litigation. I must share responsibility for some of the actions I may criticize.

Affel-780 was a surprise to us because it was not cited by the Examiner. Its relevance cannot be stated in a few words, but may be judged by its disclosure of only one concept (10) in my list of features. It showed a triode rectifier with extra B battery. It was useless for a receiver.

We based our suit on claims 1, 5, 6 and 10. These had a broad preamble, "In a signaling system", with no restriction to a modulated-carrier signal. I do not recall why we left out some more specific claims, which were clearly infringed and might have withstood the attack on validity. Some of them (2, 3 and 11) were carried over verbatim in the reissue, and included in the subsequent litigation. Their inclusion would have strengthened our case before Judge Campbell and Judge Galston, and might have affected their decisions.

As a result of our choice of claims, we found it advisable to argue some implicit restrictions which were explicit in some other claims. This left room for further challenge and confusion.

Some of the words and phrases in the claims were not used in the specification. They were descriptive of the disclosure and were used in the claims as we developed our concept of the invention. They should have been defined in the specification, as by amendments permitted during prosecution of the application. Examples of this deficiency were "control electrode", "output electrode", "diode", "direct-current connection", and the "negative" phrases. (This deficiency was not remedied in the reissue application, when it should have been recognized.)

Some confusion was caused and/or enabled by the description of several inventions in one specificaiton. They were covered in several patents, based on divisional applications. We did not find one common denominator for the various concepts of invention, which evolved during the prosecution in the Patent Office. Each patent should have been cleared of material not essential to its final scope, by amendment before issuance. (This was done in the reissue.)

**Second phase**. Phase (2) is identified as the suit against RCA, which was filed immediately after publication of the reissue 351029.

(2) Hazeltine Corp. vs. RCA Mfg. Co. (on RCA receivers) in U.S. District Court in Wilmington, Del., (Third Circuit) before Judge Nields. Trial 361109-20 (9 days). Argument 370921. Settled 371215 before a decision.

Our attorneys (PDME) made a special effort in this case, in an attempt to recover from our adverse decisions in the earlier cases. We attacked the most powerful adversary. William H. Davis (senior partner) conducted the testimony of Prof. Hazeltine as our expert witness. R. Morton Adams, a Stevens graduate who had studied under the Professor, conducted the preparation for trial and my testimony as the inventor.

The defense attorney was Stephen Philbin, assisted by his associate Barnes. He relied on a professional expert witness of long experience, named Waterman.

On the initial presumption of validity, we briefly presented our proof of infringement. The claims asserted were all, with the possible exception of those few relating to a visual indicator. There was no serious dispute as to infringement, because the AVC circuits were "carbon copies" of my patent.

Then Philbin and Waterman attacked the validity. The parties implicitly agreed that the Evans patents (both based ont he same disclosure) were the nearest approximation to my patent, in terms of the revised claims then asserted. (This was a major departure from the earlier cases based on Affel-780.) Another earlier inventor, Friis, testified that he had made a set different from his patent, and more like mine. That was still not as close as Evans, so it had no effect.

Then we presented an impressive case in rebuttal. We undertook to prove invention and success by factual testimony.

We displayed my Washington receiver, which was a hand-made model with an experimental appearance that disarmed any attack on its authenticity. It was made of six modular units, some in shield boxes. The triode connected as a diode was in plain sight, also the control-bias connection with the resistor and capacitor providing the time constant. That and my related notebook entries were never challenged as to content and date. We asserted 251217 as the date of invention of AVC by a diode, and 260102 as the date of completing the invention and demonstrating all features in the Washington receiver. We offered to call my three Hopkins friends who had signed my notebook as witnesses the next day. The defense stipulated what they would have testified. I was questioned by Adams for a whole day to present the complete story of the invention and some evidence of success. The latter was based on my comprehensive survey of all models using AVC, up to that date. It left little doubt of its "universal use". My cross-examination by Philbin, about two hours, did not make a dent in our story and served to emphasize its reality.

We went further and called a number of prominent engineers to support our position on invention and success. Mort Adams and I had visited many engineers to establish the facts as viewed by outsiders. The ones who testified on our behalf were mostly chief engineers or senior engineers in companies who were paying royalties on this and other patents of Hazeltine Corp. They were Maurice L. Levy, Wm. F. Cotter, Fred E. Johnston, Leslie F. Curtis, Wm. L. Dunn, Clair L. Farrand and David P. Earnshaw. In general, they stated that the use of a diode for AVC and linear detector was beyond the engineer "skilled in the art", they learned it from me, it had become a valuable feature of radio broadcast receivers, and they had used it in preference to other circuits proposed by others. We were indebted to all of them for supporting our cause.

Mort Adams questioned MacDonald on various topics relating to the Hazeltine engineering activities and the accused RCA receivers. Finally Mr. Davis questioned the Professor in the areas where we challenged the position of defendant, and he was cross-examined by Philbin.

Briefs were filed and oral argument occurred 370921. While waiting for a decision from Judge Nields, RCA settled the case by taking a license under all of our patents on 371215.

During the conduct of this case, Mort Adams and I spent much time together, especially in our travels (by train) to interview prominent engineers. He was impressed with the rapport I quickly developed with each of those engineers, representing a wide variety of background and viewpoint. I was impressed with his ability to steer me toward the essence of our case, from any angle. With his engineering education and mine, we communicated freely. We learned much from each other and became close friends.

**Third phase.** Phase (3) is identified as the suit against Detrola, which was filed after we were denied a decision in the RCA case.

(3) Hazeltine Corp. vs. Detrola Radio & Television Corp. (on Detrola Models 75 and 178) in U.S. District Court in Detroit (ED of Mich., Sixth Circuit) before Judge Lederle, filed 380308. Trial 390411-13 and 391025-1102 (total 9 days). Argument 391213-14. Decision 391226, my patent valid and infringed Appealed to CCA-6. Argument 401016 before Judges Hicks, Simon and Allen. Decision (Judge Florence E. Allen) 401209, affirmed (my patent valid and infringed).

This trial was essentially a replay of the Wilmington trial (2) about three years later against a different adversary. The defense attorney was Samuel E. Darby, Jr., who had prevailed against us in the earlier cases on my original patent, ending in CCA-2, 1935. This time he relied on a different expert, Leo A. Kelley, who was well qualified and fair.

Judge Lederle's first reaction was doubt as to whether we had a valid reissue, so this uncertainty was resolved before continuing the main trial six months later.

The revision of the claims in the reissue was responsible for a shift to the Evans patents as the closest "prior art". Their disclosure was first published 291126, long after my filing date. Unlike Affel-780, Evans had been cited by the Examiner, and he had still allowed all my claims in the original patent and the reissue. This history contributed to the presumption of validity. The claims remaining in the reissue contained the limitations which might have relieved some of the concerns expressed by Judge Galston.

Our supporting testimony of prominent engineers was stipulated, which deprived us of the dramatic effect of the Judge seeing and hearing the men.

With respect to both receivers (Models 175 and 178) we asserted all the claims not including a visual indicator. One receiver (Model 175) had a visual indicator (presumably the RCA tube called the "magic eye"), so we also asserted claims 6 and 7 on that feature. We did not assert claim 8 on a specific connection of the visual indicator. The Judge decreed infringement of all claims asserted. He ruled that all claims were valid.

With our briefs after trial, we submitted "Proposed Findings of Fact and Conclusions of Law". Our statement was a remarkable summary of the history of the invention and the factual testimony of our supporting witnesses. Our proof of invention was summarized in one "Conclusion of Law" based on 13 facts as follows:

- The fact that the Wheeler system of automatic volume control is different from any other system of automatic volume control,
- the fact that the diode used by Wheeler had not been in use in radio receivers for many years and previously had been rejected in favor of the triode,
- the fact that the diode had been modified to accomplish Wheeler's purpose by operating it at a high potential and connecting a high resistance across its electrodes.
- the fact that the diode after modification gave a new and improved result,
- the fact that other engineers and scientists working on the problem did not arrive at Wheeler's solution,
- the fact that the systems devised by others had defects that made them less satisfactory than Wheeler's,
- the fact that these other systems were devised by engineers in the foremost engineering and research groups,
- the fact that practicing engineers skilled in the art of radio receiver manufacture did not know of any successful automatic volume control system at the time of Wheeler's invention in December, 1925,
- the fact that Wheeler's system was at first regarded as a backward step by other engineers and manufacturers and later proved to be the best system,
- the fact that engineering laboratories and engineers at first recommended and manufacturers at first preferred to use other systems but finally adopted the Wheeler system after the others were found unsatisfactory,
- the fact that practically all receivers today that use any form of automatic volume control use the Wheeler system,
- the fact that the Wheeler system has gradually supplanted all others,
- the fact that defendant is licensed to use the other systems that were devised and nonetheless prefers the Wheeler system,
- the fact that the majority of other manufacturers are licensed to use the other systems that were devised but nonetheless prefer to use the Wheeler system,
- all constitute evidence showing that what Wheeler did amounted to invention.

The decision of Judge Lederle was mainly the adoption of our "Proposed Findings". It supported our position.

After Judge Lederle's decision in Detroit, Darby appealed to the Circuit Court of Appeals (CCA-6) in Cincinnati. Detrola became the Appellant and Hazeltine the Appellee. After submission of appeal briefs, this contest was argued 401016 before a panel of three Circuit Judges. Mr. Davis and Darby made the argument. During the questioning, it became apparent that Judge Florence E. Allen would be writing the opinion. She was well prepared and her questions to Darby were devastating. On the Court's decision 401209, her statement of the opinion was a classic in thoroughness and clarity. (She should have been appointed as the first woman on the Supreme Court.) She reviewed every essential topic, with remarkable clarity and continuity, and arrived at complete affirmation of validity and infringement. The decision was unanimous.

**Fourth phase.** Phase (4) is identified as the conflict which was accepted by the U.S. Supreme Court. Two Circuits had ruled on the same invention but based on different claims.

- Judge Hand (CCA-2) said there was no invention in the disclosure of the original patent, even if the asserted claims were to be limited in scope.
- Judge Allen (CCA-6) said there was an invention, properly described in all claims in the reissue patent, and they were infringed by Detrola.

We urged the latter position, based on the greater weight to which it was entitled after the further proof of facts in the last trial. The latter decision was made with due consideration of the former, but not vice versa.

Since this contest was an appeal from CCA-6, Detrola was Petitioner and Hazeltine was Respondent. After submission of briefs, the argument by Darby and Mr. Davis was made 410407 before the panel of all nine Justices. Their few questions did not disclose which one was likely to write the decision.

Here I must relate a painful assessment which may have been a determining factor in the decision. Mr. Davis was not at his best. He was involved in labor disputes based on the Wagner Act. He had been working for days, around the clock, in the negotiation of a labor dispute at Allis-Chalmers, and had come to Washington by train the night before. Our case was not fresh in his mind. His argument, and especially his response to some questions, showed the effects of fatigue. Darby was fresh and aggressive as usual.

The Court's decision 410512 was written by Associate Justice Owen J. Roberts. He adopted Judge Hand's statement for CCA-2 with no reference to the further factual evidence available to Judge Allen in CCA-6. Justice Roberts followed Judge Hand's rationale and went even further in making technical judgments unsupported by the record of other facts. These were generally taken from Darby's brief, but I do not have a detailed correlation.

The most specific error in Justice Robert's statement related to "linear response":

> "There can be no question that the patents cited as prior art disclose the accomplishment of linear response. The curve exhibited in Wheeler's drawings to illustrate the result of the use of his system is duplicated in similar curves by Affel and Friis. It cannot be claimed, therefore, that Wheeler has accomplished a new result. At most he can have obtained an old result by new means."

Here he confused "linear response" with some undefined "results" and treated the two as one:

- The "result" of levelling the output was illustrated also in similar curves by Affel and Friis.
- The result of "linear response" was not mentioned or disclosed by anyone else. It was shown by another curve in my patents (Fig. 8 in original, Fig. 4 in reissue).

My result was a combination of the two, which was a totally new result. No evidence or rationale to the contrary was found. Justice Roberts made a mistake.

The Supreme Court handed down a unanimous decision which was a reversal of a unanimous decision of the CCA-6 affirming the decision of the District Court which saw and heard the evidence. I draw a strong inference that the individual members of the Supreme Court did not give independent consideration to the merits of the case. If they had, I would expect some dissention, as there is in nearly every controversial case. I infer that they did not consider my case important enough for individual attention. If so, I say they should not have taken the case.

The decision of the Supreme Court affirmed the CCA-2 decision which went much further than the District Court in ruling against invention. Neither of those lower Courts had the benefit of all the factual testimony available to CCA-6.

The Supreme Court in 1941 had a history of non-enforcement of nearly every patent to reach its jurisdiction, over the past decade. President Roosevelt, around 1939, made a speech in which he condemned further inventions. He said that existing inventions might be a good thing but we should cut down on further inventions because they reduced employment. His speech was received on home radio receivers which used my diode AVC and peak detector, as covered in my patent soon to be struck down by the Supreme Court. That year we entered World War II. That war was won by inventions. I invented the mine detector which was used by our Army in all land theaters. I made numerous inventions in the Navy IFF program. One was the "lifesaver" antenna which was used on every surface vessel of the Allies during the later stages of the war.

The Roosevelt speech was not taken lightly by Industry. The National Assn. of Manufacturers (NAM) organized an elaborate program to recognize inventions and inventors. The main event was staged at the new Waldorf Astoria Hotel in New York on 400227. About 100 selected inventors, from all fields, were given the "modern Pioneer Award". Among the recipients were Prof. Hazeltine and myself. Also Affel, but not Evans or Friis, from the patents cited in the AVC litigation. I had received about 80 patents at that time.

When the adverse decision of the Supreme Court became known, the radio industry was shocked. There was general recognition of my invention, as implied by the actions of RCA and GE in taking a Hazeltine license without waiting for final decisions in the Courts. The adverse decision was yet another blow to the patent system. The resulting outrage apparently reached the Court. In the next patent case, they sustained a patent. They never reverse a decision as a remedy to the injured party, even when they later learn they made a mistake. In our case, the mistaken decision could have destroyed our Company, because the AVC patent was the keystone of our patent portfolio on which our income was dependent. We were saved by the war, which demanded and rewarded our engineering talents.

In the six months ending 260102, without knowledge of prior art, I had overcome the defects of earlier circuits and had invented the one AVC circuit which was copied for universal use. It was the diode AVC and peak detector. It accomplished a result that no one else had disclosed. It was claimed in my patents. The Supreme Court finally ruled that my patents were invalid for want of invention. I submit that their decision involved technical error and the rejection of factual testimony.

Justice Roberts proceeded in the same way as chairman of the first Pearl Harbor investigation. He was an early New Deal appointee.

# Section 6. Superheterodyne Receivers

## 6.1 The Superheterodyne Receiver 1930.

One of the timeless inventions in radio is the Armstrong invention which has come to be called the superheterodyne receiver. It enables a signal of any frequency over a tuning range to be shifted to one frequency for final selection, amplification, detection and further processing. This frequency is historically less than the signal radio frequency (RF) and greater than the modulation frequency so it has come to be termed the "intermediate frequency" (IF). Its benefit is greater at higher signal frequencies. When it was invented, the broadcast and amateur bands (up to 3 Mc) were the higher frequencies. Its first spectacular use was amateur communication across the Atlantic Ocean in 1922 (221211).

RCA acquired this invention and used it exclusively until 1930. They had a monopoly on a sophisticated broadcast receiver with sufficient gain to operate on a rather small loop antenna. Then RCA included the superheterodyne in its license to manufacturers of broadcast receivers.

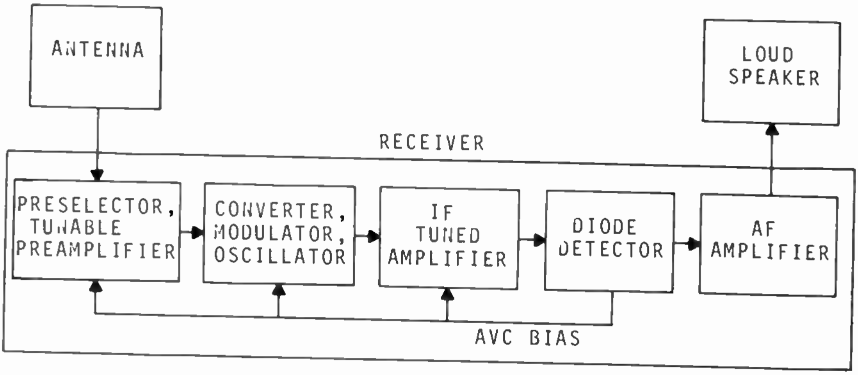
Without the superheterodyne, we had achieved a high grade of performance with the TRF amplifier. Using the triode, we exploited the Hazeltine invention of neutralization in the Neutrodyne (1923-28). Using the successor screen-grid tetrode, we further improved the TRF amplifier and added AVC (1929-30). Then we offered the opportunity for still further progress by adopting the superheterodyne and making improvements on it. Those developments are the subject of this chapter and some more specific chapters in this category.

The first superheterodyne I made was the Washington receiver in which I demonstrated the diode AVC and peak detector. This set anticipated some improvements we were to utilize after the unrestricted licensing of the superheterodyne. Some of these features were:

- A higher IF for further separation of the "image frequency" from the signal frequency. Previously the IF had been around 100 Kc or lower. I used 300 Kc. I accomplished this in a triode IF amplifier by using Hazeltine neutralization (GCN) to enable high gain per stage.
- Diode peak detection for linear response to modulation. This was facilitated by the availability of more gain before the detector.
- The use of such a detector also to control the gain and deliver to the detector a signal carrier voltage of a few volts, within the range of linear response.

In these respects, my design was superior to the latest RCA design to be reported about 3 years later, 1928-29.

From 1930, when the superheterodyne became available to our licensees, the Hazeltine labs in New York and Bayside concentrated on improvements in its design for broadcast reception. The transition from TRF receivers offered a




6.1 Fig. 1 — Superheterodyne receiver around 1931.

new opportunity because only RCA was then experienced in the superheterodyne. Fig. 1 is a block diagram of its essential components in 1931. The following features were among its peculiarities, including some to which we contributed innovative improvements:

- The preselector, whose principal functions were the tuning of the antenna for efficient reception and the suppression of the "image" response which was peculiar to the superheterodyne converter.
- The converter, whose function was the shifting of the signal frequency to the lower, fixed, "intermediate frequency" (IF). It required a "local oscillator" (LO) to be tuned in track with the selector, and a modulator (or mixer or "first detector") for combining the two variable frequencies.
- The tracking of the signal and oscillator tuning frequencies to provide the required constant difference with unicontrol.
- The IF amplifier, whose function was providing most of the amplification or gain ahead of the detector.

The preselector and the converter became specialities of our labs, so they are covered in separate chapters. Another chapter relates to the "expanding selector" that was possible in the IF amplifier, but was afforded in few models for sale. In general, the superheterodyne offered not only a great opportunity for improvements but also some problems that had not confronted the designer of a TRF receiver.

Not the least challenge was the concurrent need for an inexpensive receiver in the Depression. This challenge was met so effectively that small receivers accounted for a steady increase of the dollar volume of home receivers. One result was a total investment before World War II that met the needs for home broadcast receivers until the end of the war when their manufacture was resumed.

In the early 1930's a common receiver was a table model with the receiver chassis in the base and loudspeaker just above. A typical shape was patterned after the Gothic arch, hence the designation "cathedral" model. In the middle 1930's, at the depth of the Depression, we designed one of the lowest-price receivers of those years. It was a 5-tube superheterodyne with AVC, complete in a small rectangular box with enclosed loudspeaker. It was made by Belmont for Montgomery Ward and sold for about $8. It was notable that my diode AVC was afforded in this model. That feature was found to offer some economies, not just added cost. The same had been found true of the superheterodyne, especially with our contributions.

An essential option in a superheterodyne was the choice of the IF, somewhere between the highest AF (say 10 Kc) and the lowest RF of the broadcast band (550-1500 Kc). The background and the progress after 1930 is here summarized.

- 42 Kc was used in the first superheterodyne sold for broadcast reception. It was made by G. E. Co. for the RCA line in 1924.
- 180 Kc was used in the Radiola 64 in 1928, the RCA model which first used any kind of AVC.
- 175 Kc was commonly used in the models which appeared in 1931 after RCA released the superheterodyne.
- 262 Kc was initiated by Hazeltine labs for 1931 models, especially for motorcar receivers to be made by Philco (Transitone).
- 450 Kc became common in 1932, and led to the present standard 455. It was chosen as the highest with a reasonable margin below 550.
- 125 Kc was used in double-band sets for Europe, being just below the European band (150-285).

We contributed to the trend upward from 175, following the precedent in my Washington receiver (300). In particular, the 262 value was an example of an invention I made shortly after we started work on the superheterodyne. When tuned to one frequency channel in the broadcast band, it was desirable to place the image frequency midway between two adjacent channels far from the tuned channel. My invention was as follows, referring to the existing channels:

- IF an odd multiple of 2.5 Kc so the image would be separated by an odd multiple of 5 and thereby located between channels separated by 10.
- IF an odd multiple of 12.5, for the same relation to the cleared channels separated by 50.

As a compromise, we chose 262 (rounded from 262.5) as a moderate step up from 175. Another choice was 462, but that proved not to be critical because the image of most channels fell about the broadcast band with its orderly pattern. The present standard (455) is not an example of my rule. (The early IF at 175 was a "worst" choice, being an even multiple of both 2.5 and 12.5.)

My first notebook entry on our superheterodyne work was 300905. It showed an IF of 312 Kc for the purpose of my invention.

The tracking of the signal and oscillator tuning was accomplished by a novel method we learned from RCA. With two like variable capacitors, nearly constant frequency difference (IF) was obtained by supplementing the higher-frequency (oscillator) tuner with adjusted values of two fixed capacitors respectively in series and in parallel. We took advantage of the series capacitor to employ a variable-ratio circuit from our work on Uniform Gain. The result was nearly constant oscillator voltage, as described in that chapter. It is my recollection that only our designs utilized this feature.

In the New York lab, we devoted much effort to the reduction of the size and cost of the IF amplifier. Its tuning was fixed so it did not require space for a variable capacitor. Tubes were being made smaller. The principal other component was the interstage IF transformer. By some ingenuity, our staff developed these features:

- A small size enclosed in a small aluminum can as a shield.
- Double tuning of a coupled pair of coils on one paper-layer winding.

We could benefit little from the available iron-core materials, made of powdered iron. It was long before the ferrites that are now in common use.

The superheterodyne led to more appreciation of the need for two-signal tests of interference. I introduced them into the 1933 and 1938 IRE Standardization Reports. I was Chairman of the Technical Committee on Radio Receivers, which prepared these reports. As examples, they showed some tests we had made at the Bayside lab. The principal interference was at the image frequency but various other terms became apparent.

About half of the superheterodyne receivers sold in the 1930's were based on designs from our labs in New York and Bayside, and later Chicago. A complete implementation of the diagrams of Fig. 1 would have required about 9 tubes, including 2 double-purpose tubes and a power rectifier (double diode). The tuning would have used 3 rotary capacitors on one shaft. A skeleton implementation of all parts except the preamplifier yielded the low-price 5-tube model.

[A] E. H. Armstrong, "A new method of receiving weak signals for short waves", Proc. Radio Club of Amer.; Dec. 1919.

[B] E. H. Armstrong, "A new system of short wave amplification", Proc. IRE, vol. 9, pp. 3-11; Feb. 1921. (Superheterodyne receiver, IF at 50-100 Kc, signal above 500 Kc.)

[C] E. H. Armstrong, "The super-heterodyne — its origin, development, and some recent improvements", Proc. IRE, vol. 12, pp. 539-552; Oct. 1924. (The first superheterodyne receiver for broadcase use, sold by RCA.)

[D] G. L. Beers, W. L. Carlson, "Recent developments in superheterodyne receivers", Proc. IRE, vol. 17, pp. 501-515; Mar. 1929. (The Radiola 64 from RCA.)

[E] H. A. Wheeler, U.S. Pat. 1878614, 320920 / 310224. (IF an odd multiple of 2.5 or 12.5 Khz.)

### 6.2 The Converter in a Superheterodyne 1931-33.

In the Armstrong superheterodyne receiver, the frequency conversion required an extra tube and also made less effective use of one RF amplifier tube. The extra tube was used in the "local oscillator" (LO). The "modulator" tube yielded "conversion gain" much less than the amplifier gain obtainable from the same tube. We attacked the problem by finding ways to avoid the need for the extra tube. The oscillator and modulator functions were combined in one tube. This may sound simple, but it required ingenuity to preserve the normal operation in both functions.

We developed useful circuits from two approaches to this problem:

- Using the same essential electrodes for both oscillator and modulator, we wanted to avoid grid current in the modulator grid. An oscillator usually had grid current in an amount we considered excessive. I developed a circuit in which the oscillator was self-regulated to avoid grid current.
- Using added electrodes in the modulator tube, we provided separate grids for the two functions. Furthermore, by developing a "virtual cathode" between the two functions, we made the "emission-valve modulator" which could be subjected to bias control for AVC while maintaining normal oscillation.

The former was a makeshift but it was a valuable interim measure. The latter was a real solution of the problem. It happened that RCA concurrently found the same solution and brought out a new tube for the purpose. The single tube performing both functions was designated the "oscillator-modulator".

Several years earlier, I had recognized the problem of grid current in a self-oscillating modulator tube in a superheterodyne. These entries in my notebooks were directed to a self-regulating oscillator without grid current:

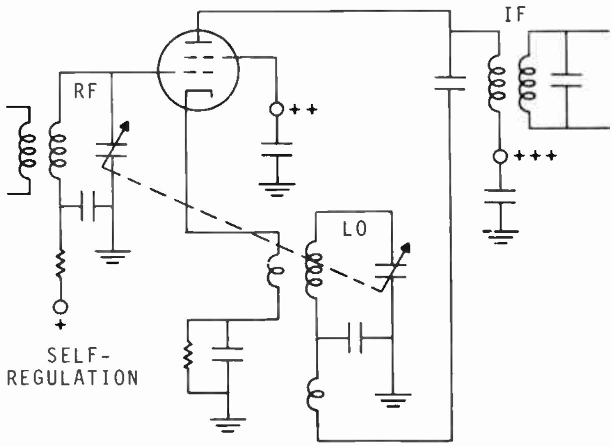
240906 A triode oscillator-modulator for a superheterodyne. A high resistance in the plate circuit regulated the plate current so the amplitude of oscillation on the grid was held less than a fixed grid bias. The resistance was bypassed with a small time constant to avoid periodic "blocking" of the oscillation.

270124 Another description of the same circuit for the same purpose.

At that time, I was not designing an economical superheterodyne, so the idea was not immediately utilized.

In 1931, I had another option as a result of the indirectly heated cathode. The regulating resistance could be connected in the cathode circuit, where it would offer two advantages:

- Much lower resistance, decreasing the loss of plate voltage and power;
- Increasing the grid bias with oscillation, as a further margin against grid current.



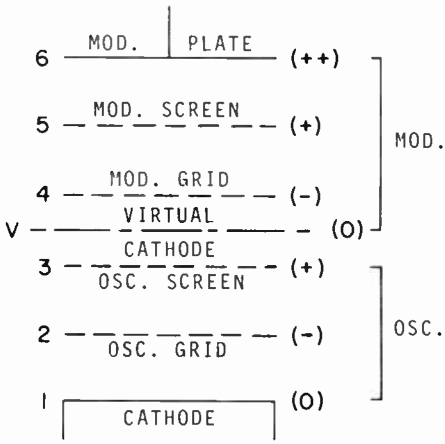

6.2 Fig. 1 — Converter with self-regulating oscillator.

The resulting circuits were described in my first publication but they were soon superseded.

Fig. 1 is a typical circuit of the converter in which I used the self-regulating oscillator to avoid grid current. See [A] (5) (6). It was developed and put in use in 1931. It usually used a tetrode, as shown, but could have used a triode or a pentode with the same essential circuit. The same electron stream was used for both oscillator (LO) and modulator. The LO used variable ratio, taking advantage of the series capacitor already present for frequency tracking. The variable ratio was designed for nearly constant oscillation voltage on the inductors and the cathode. The resistance in the cathode circuit was near the ratio mean of these two bounds:

- The greatest value with which the oscillator would start at all frequencies of tuning:
- The least value with which the oscillator would not have grid current.

These were always separated and the ratio of separation was a measure of the effectiveness of the regulation. The separation could be increased by circuit design. The required resistance was noncritical to the tube properties, so the circuit was practical. The time constant of the cathode resistor and its RF bypass capacitor was made somewhat less than the "ringing" time constant of the LO tuned circuit, so the regulation would be stable.



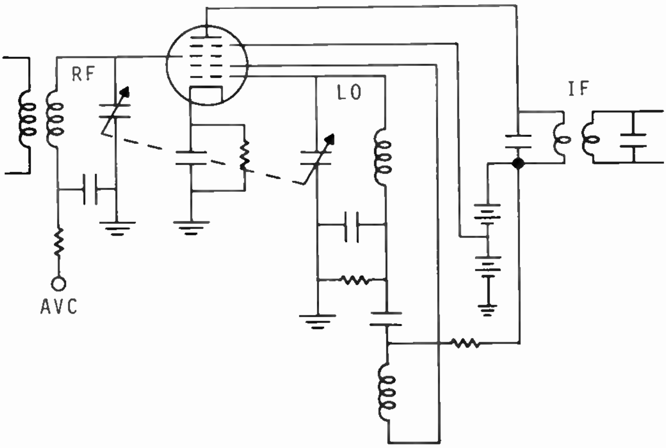

6.2 Fig. 2 — The hexode tube.

This circuit could not be subjected to bias control of its gain, because that would stop the oscillation. It could have been preceded by an RF stage subject to bias control. However, it was first used in a low-price 5-tube receiver which did not yet afford AVC, so the manual control could be obtained in the antenna circuit, ahead of the converter. There remained a demand for a single-tube converter that could be bias-controlled.

The activity and use of the Fig. 1 circuit was indicated by the following summary:

- 50 reports from the Bayside and N.Y. labs, 310411-330223.
- Bayside authors HAW, VEW, NPC, CKH; N.Y. authors WAM, DEH, JKJ.
- Manufacturers Philco and 10 other licensees.
- Single-band sets for U.S., double-band sets for Europe.

On or about 320912, there was a meeting in MacDonald's office in the N.Y. lab, attended by Bill Grimditch, Mac and myself. They said, how could we make a one-tube oscillator-modulator that could be bias-controlled for AVC? I made some sketches and came up with "hexode". It was formed from a tetrode by inserting two more grids next to the cathode. They were to be used for the oscillator, to modulate the electron stream before it reached the RF grid. Then the latter could be subjected to bias control. Shortly we named this function, the "emission-valve modulator", because the oscillator modulated the electron stream between its emission and its arrival at the RF control grid of the modulator.




6.2 Fig. 3 — The hexode converter circuit.

Fig. 2 shows the essentials of the hexode tube which I proposed. Between electrodes 3 and 4, the electron stream is decelerated and is mostly sent back to 3. This develops a "virtual cathode" (V) from which the outer electrodes derive their space current. Its potential is effectively that of the real cathode. The inner 3 electrodes were used for a triode oscillator. The virtual cathode (V) and the outer 3 electrodes were used for a tetrode (screen-grid) modulator. The result was essentially that of separate triode and tetrode tubes but gave even greater conversion gain in one tube envelope with only one cathode to be heated.

Philco was affiliated with Sylvania, a competitor of RCA in the manufacture of tubes. With great secrecy, Sylvania in Emporium, Pa., undertook to make samples of the hexode. The samples were sent to the Bayside lab for testing and for trial in circuits. One problem was more difficult than expected. That was the design of the modulator grid (4) for gradual cutoff ("variable mu") so it could be used for bias control of strong signals.

The usual grid for gradual cutoff operated on a real cathode. It was rather easy to use graduated spacing of the grid wires for securing the gradual cutoff. Operating on the virtual cathode proved to be much more difficult. I devised a mathematical rule for graduating the spacing, so smooth gradual cutoff could still be obtained.

Fig. 3 shows a converter circuit using the hexode oscillator-modulator with bias control for AVC. The first receiver using this converter was reported 330406. The converter was followed by:

IF amplifier (78 pentode);
diode detector, AF amplifier (6B7 diode-triode);
AF power amplifier (42 pentode);
AC rectifier (80 double diode).

This pattern survived in the most popular small sets.

Independently, and likewise under great secrecy, RCA arrived at a similar solution for a converter suitable for bias control and AVC. They went one step further and added a shield screen between the oscillator and the virtual cathode. The result was a heptode which they named the "pentagrid converter". It was announced 330214 (2A7, 6A7).

I spoke to meetings and published articles on the hexode ("emission-value modulator") in 1933 MAR and APR. Both RCA and ourselves had promptly filed U.S. patent applications. Ours was filed a few days earlier (330130). In the Patent Office, an interference determined that RCA had earlier dates of invention and reduction to practice, so we lost to them the broadest claims on the principle of the virtual cathode between an inner oscillator and an outer modulator. The RCA tube soon became available so we used it instead of marketing the hexode. This left to us the credit for first publication and the earlier experience in applications. We did not learn whether a leak of our progress had had the effect of expediting the RCA announcement.

Our earlier filing date in U.S. retained for us the priority on foreign rights under the Convention. We took advantage of this priority by applications in the principal European countries. Our patent situation in Europe was managed by AGA Baltic (Sweden) under mutual agreement. They established the validity of my oscillator-modulator patents in some European countries. In this process, a comprehensive study was prepared by that company in the Swedish language. It was entitled, "Wheelers Modulatorpatent" and was published in 1945. This was the only case where one of my inventions came to be adjudicated in litigation against an infringer. The infringement was the use of the RCA tube or equivalent. Our affiliation with AGA Baltic led to my life-long friendship with one of their leaders. Carl-Eric Granqvist. (We entertained him and his bride 371205 on their trip to U.S.)

The integral oscillator-modulator tube did not survive in a similar transistor. There was no longer a demand for double use of one cathode, and the triode became the common form of transistor.

[A] H. A. Wheeler, U.S. Patents

| | Pat. No. | Issued / Filed |
|---|---|---|
| (1) | 1931338 | 331017 / 320120 |
| (2) | 1958027 | 340508 / 330130 |
| (3) | 2015327 | 350924 / 330130 |
| (4) | 2016760 | 351008 / 330323 |
| (5) | 2022067 | 351126 / 310528 |
| (6) | 2022068 | 351126 / 311214 |
| (7) | 2034013 | 360317 / 330114 |
| (8) | 2042571 | 360602 / 350327 |

Note (1) Oscillator-modulator, IF output coil for plate DC.
Note (2) Hexode, some features left after disclaimer 400925.
Note (3) Hexode tube with gradual cutoff.
Note (4) Hexode tube, grid spacing for gradual cutoff.
Note (5) Oscillator with cathode coil.
Note (6) Oscillator-modulator with cathode coil, etc.
Note (7) Pentode oscillator-modulator, suppressor oscillator.
Note (8) Neutralization in hexode oscillator-modulator.

[B] [W 11] "The emission valve modulator for superheterodynes", Electronics, vol. 6, no. 3, pp. 76-77; Mar. 1933.

[C] [W 14] "Oscillator-modulators in superheterodyne receivers", IRE meeting N.Y.; Apr. 5, 1933. (Transcript, Rep. 596W, 351002.) (A comprehensive overview of tubes, circuits, and theory, including the hexode. Never published elsewhere.) (Discussion by J. C. Smith, RCA work on 2A7, 6A7, Proc. IRE, vol. 23, pp. 576-577; Jun. 1935.)

[D] [W 12] "The emission-valve modulator for superheterodyne receivers", Proc. Radio Club of Am., vol. 10, pp. 23-25; Apr. 1933. (Overview of all types, including the hexode. Presented 330308.) (Also in Radio Engineering, vol. 21, no. 4, pp. 12-14; Apr. 1933.)

[E] C. K. Huxtable, "Oscillator-modulator summary", Hazeltine Rep. 403W, 330427. (List of 80 reports from Bayside and New York labs.)

[F] E. W. Herold, "The operation of frequency converters and mixers for superheterodyne reception", Proc. IRE, vol. 30, pp. 84-103; Feb. 1942. (Comprehensive view of all types.)

## 6.3 Image Suppression in a Superheterodyne 1931.

When a superheterodyne receiver is tuned to a weak signal on one frequency, it is subject to interference from a strong signal on any of several other frequencies. The latter are termed "spurious-response" frequencies. One is at the "image frequency", which is higher than the signal frequency (RF) by twice the intermediate frequency (IF). The early designs used an IF of 175 Kc, so the difference was 350 Kc; for example:

| | | |
|---|---|---|
| Signal | RF | 600 Kc |
| | IF | 175 |
| Image | RF + 2 IF | 950 |

Here an attempt to receive a desired weak signal on one clear channel (600) would have been subject to interference from an undesired strong signal on another clear channel. The interference would have appeared as a whistle of pitch decreasing and then increasing while tuning through the desired signal. This interference was noticed because it was not present in the previous designs using TRF amplifiers.

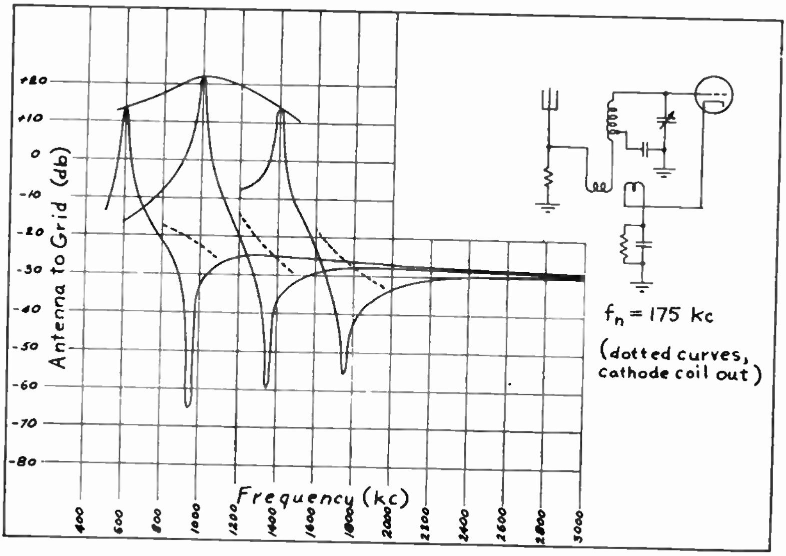
The reduction of such interference came to be measured by the "image ratio", a larger value indicating more reduction of interference. Any amount of reduction could be obtained by some number of tuned circuits ahead of the superheterodyne converter.

The reduction of the image response came to be called "image suppression". For economy, special selective circuits were devised to obtain image suppression more than the amount naturally provided by the number of tuned circuits. Such image-suppression circuits are the subject of this chapter.

We became aware of this opportunity from testing one of the first Atwater Kent superheterodyne receivers early in 1931. Several tuned circuits (3 or 4) were used between the antenna and the converter. The image ratio was greater than would be expected from the tuned circuits. We found the cause in the use of a tapped coil in one tuner. It was arranged so that one tuned circuit provided not only a peak of response at resonance, but also a valley of response near the image frequency. The valley tracked with tuning, at a constant frequency ratio. It fell on the image at one tuning point, but missed somewhat at other tuning points. We found simple ways to use this principle in our designs.

We soon realized that this feature was related to a much earlier (1928) patent to MacDonald. [C] (1)

With our background of variable ratio for uniform gain, I proceeded to apply that principle to track the valley near a constant frequency difference. Such image suppression gave roughly the effect of another tuned circuit, but without its space and cost.




6.3 Fig. 1 — Image suppression with tracking frequency difference.

Fig. 1 shows one circuit which we found most useful, and graphs of its performance. The circuit was based on these relations:

- The antenna was coupled to the first tuned circuit by additive inductive and capacitive coupling as shown.
- The two kinds of coupling were proportioned to give image-frequency coupling from antenna circuit to the first grid, proportional to the image frequency while tuning. This proved to be easy, to a close approximation.
- An extra inductive coupling was provided to give equal coupling to the first cathode. This cancelled the image response in the first tube.

The resulting cancellation tracked with frequency to give most image suppression, as appears in the graphs. The dashed lines show the behavior before the cancellation.

The wide peak around midband was obtained by tuning the antenna circuit near midband and adding the shunt resistor.

For a high-grade receiver with IF of 175, this preselector plus one TRF stage was adequate for image rejection. When the IF was raised to 450, the image-response problem was greatly reduced, for these reasons:

- Greater separation between tuned frequency and image;
- For most channels, the image fell above the broadcast band.

Therefore there was little or no use of image suppression in receivers after the mid-1930's.

The higher IF was nearer the broadcast band, so the spurious response at the IF became troublesome. The most intensive design we made was one for single tuning ahead of the converter, including these features:

- Double tuning the antenna circuit to give a wide double peak over the band;
- Including in this tuning an IF trap;
- Image suppression as described.

This was used in some low-price models with only two tuning capacitors on one shaft, the second for tuning the oscillator.

I was granted a patent on this circuit for image suppression, but it was not used long after it issued.

The work at the Bayside lab was skillfully performed in 1931-32 by Vernon Whitman, Nelson Case and Bill Swinyard. Much ingenuity was required in adjusting the circuits and in measuring the very large ratios of peak and valley.

The image suppression was most interesting as a development which accomplished a remarkable result and served a useful purpose in one stage of progress.

[A] [W 14] "Image suppression in superheterodyne receivers", Proc. IRE, vol. 23, pp. 569-575; Jun. 1935. (Presented in N.Y., 330405.)

[B] "Image suppression summary", Report 291W: 320719. (Abstract of 32 reports from NY and Bayside labs, 310506-320705.)

[C] U.S. Patents

| | Pat. No. | Inventor | Issued / Filed |
|---|---|---|---|
| (1) | 1680424 | WAM | 280804 / 260203 |
| (2) | 1875837 | HAW | 320906 / 310707 |
| (3) | 1883794 | JKJ | 321018 / 310707 |
| (4) | 1960984 | HAW | 340529 / 330405 |
| (5) | 2048527 | HAW | 360721 / 320201 |
| (6) | 2075683 | HAW | 370330 / 330405 |

Note: (1) (2) (3) (6) Coil tap or equivalent, one-point suppression.
Note: (4) (5) Cathode coil or equivalent, tracking suppression.

## 6.4 Multiband Receivers 1931.

In the Spring of 1931, there appeared various models for reception in other bands along with the U.S. broadcast band. All the bands may be summarized as follows:

- 150-300 Kc, low band used in Europe.
- 550-1500 Kc, broadcast band in U.S. and Europe.
- 1.5 — 4 — 10 — 23 Mc, three higher bands.

Ths subdivision of the three higher bands was arbitrary for receiver design. They included especially the international broadcast bands near 6, 9, 12, 15, 18, 21 Mc. The respective bands were selected by switching the inductors and trimming capacitors of all tuned circuits.

The first coverage of the low band appeared with the broadcast band in a two-band receiver. We designed such a receiver with TRF amplifiers using screen-grid tubes. Then we went to a superheterodyne with IF of 125, just under the low band. The TRF designs used some of our refinements for uniform gain. The superheterodyne designs used our developments in oscillator-modulators and image suppression. Designing for these bands was straightforward.

The first coverage of the higher bands was achieved by supplementing a broadcast receiver with a "short-wave converter". The receiver was tuned to one frequency and used for the IF amplifier to follow the short-wave converter. This combination provided experience in design and operation at higher frequencies with their problems. A high degree of refinement was not expected or realized. The remarkable result of transatlantic reception, however erratic, developed a demand for this feature integrated in a broadcast receiver.

The IF of 450 was high enough to enable the design of an "all-wave" superheterodyne receiver with a single converter to this IF. Any one of the four bands was selected by switches from a single knob. The problems of switching were added to those of the higher frequencies.

Concurrently we had to make new test equipment for covering the extended frequency range. That is covered in other chapters.

There were new problems of antenna design, especially to accommodate the broadcast bands at 6 Mc and higher. They used horizontal polarization of the radio wave, as distinguished from the vertical polarization used at lower frequencies. Our work on the antenna problems is covered in other chapters.

Our intensive work on multiband receivers occupied much of our staff in both Bayside and N.Y. labs in 1931-32. An overview was given in the paper presented by Harnett and Case 340530 at the annual IRE convention in Philadelphia. [A]

The technology of band switching involved hardware and options that are complicated to present. They dated back to the SE-1420 Navy receiver designed by Prof. Hazeltine just after World War I. That receiver may be compared with the all-wave receiver about 12 years later:

- SE-1420, 40-1200 Kc, ratio 1:30, 6 bands, each band 1:1.76 (nominal), two tuners, separate controls, antenna and regenerative detector.
- All-wave, 0.55-23 Mc ratio 1:42, 4 bands, each band 1:2.55 (more or less), two tuners, unicontrol, antenna and superheterodyne converter.

These two models may be compared for their features that were similar or very different:

- They were scaled about 1:16 in frequency.
- They utilized the prevailing Armstrong inventions of the earlier and later periods.
- They exemplified the progress from the versatility of separate tuning to the facility of unicontrol.
- The lower-frequency design was straightforward while the higher was approaching the frontiers of tubes and circuit design.

In the design of the switching circuits, I was not deeply involved, only for occasional advice. The results were a credit to our team and were utilized by many of our licensees.

[A] D. E. Harnett, N. P. Case, "The design and testing of multirange receivers", Proc. IRE, vol. 23, pp. 578-593; Jun. 1935. (Four ranges, 0.53-23 Mc.) (Presented at annual convention in Philadelphia 340530.)

## 6.5 Antennas and Lines 1934-38.

The Multiband Receiver brought the capability of broadcast reception in the U.S. band and the transatlantic bands. While a long wire was useful as an antenna for all bands, its behavior became less predictable at higher frequencies. Therefore there came to be a demand for a more sophisticated device termed the "all-wave" antenna system. We devoted much effort to the design of such a system, and the result was the most effective device available in 1935 or thereafter. The problems of installation were such that only the more ambitious transatlantic listeners would use it, so it saw little use, dying out by the time of World War II and not reviving thereafter. That is regrettable because a real service has been available.

The "all-wave" antenna system was directed mainly to broadcast reception in these bands:

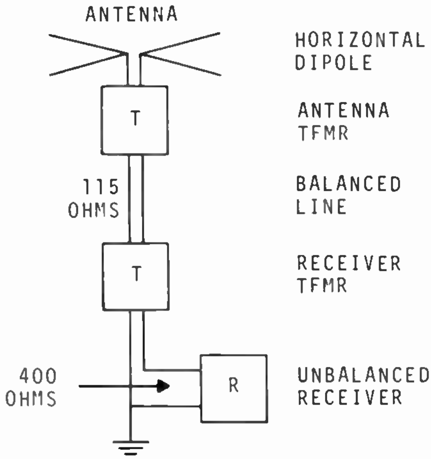
| | **Medium-wave** | | **Short-wave (SW)** | |
|---|---|---|---|---|
| | U.S. Standard | | Transatlantic | |
| Frequency | (0.54 - 1.5) | — | ( 6 - 18) | Mc |
| Wavelength | (550 - 200) | — | (50 - 17) | meters |
| Polarization of wave | Vertical (VP) | | Horizontal (HP) | |

In a typical receiver, the U.S. band was covered in one tuning range and higher frequencies were covered in three more bands by switching.

The short-wave band posed an added problem in directive reception, for two reasons:

- An adequate length of wire became comparable with the wavelength, so it approached resonance and its current distribution became dependent on its length relative to the wavelength.
- The horizontal polarization was best received on a horizontal wire, but only in the directions "broadside" from the wire. We adopted a horizontal wire of prescribed length, to obtain predictable current distribution and the best reception in the broadside directions. This result required a symmetric connection to the two halves of the wire at their junction. Such an antenna was said to be "balanced" relative to ground. It was termed a "doublet" or, more recently, a "dipole" antenna. For best performance, it had to be suspended in an open space with orientation broadside to Europe. Then it had to be connected to the receiver by a balanced two-wire line.

Some attention was being paid to the pickup of local electric noise on the antenna wire. For noise reduction, the active antenna wires were located outside the building and connected to the receiver by a transmission line designed to avoid noise pickup. This was convenient with the use of a balanced dipole outside.



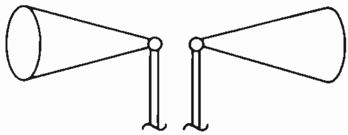

6.5 Fig. 1 — An all-wave antenna system using a double-V dipole.

Prof. Hazeltine in 1924 used such a line to avoid noise pickup in a large building. He located a rather small antenna on the roof and connected it to a receiver inside by a long line. The line was a balanced pair, as we came to use here. The system was the radio electric clock, receiving signals in New York City from Arlington, Va. I worked on it that summer. It was one of the first uses of such a system.

Fig. 1 is a diagram of the essential components of the system we developed. In general, it was similar to other systems proposed around 1934. Our contributions were made to the design for most performance. The components were:

Horizontal diapole antenna.
Antenna-to-line transformer-filter.
Balanced-pair transmission line.
Line-to-receiver transformer-filter.
Receiver.

Each will be discussed further, except the receiver. For present purposes, its significant property was its input impedance at the tuned frequency. It was taken to present 400 ohms pure resistance. That value had been selected by my IRE committee as typical of the mean impedance of a long wire at high frequencies.



6.5 Fig. 2 — A conical dipole.

The testing of this antenna system presented some unusual problems.

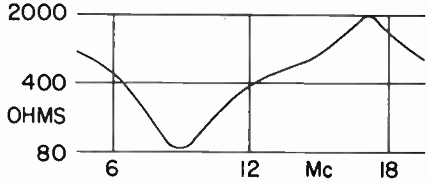
- The measurement of balanced impedance or admittance was accomplished by a specially designed Antenna Impedance Meter, which is the subject of another chapter.
- The measurement of response was enabled by our signal generators using the Piston Attenuator, described in another chapter. The output coil was not grounded, so it could be connected to give a balanced voltage.
- A dummy antenna was made of small components to simulate the measured impedance properties of the actual dipole antenna in space.

All of these were not available outside our Bayside lab, where they were developed. For measurement in place, a horizontal antenna was suspended between two 40-foot poles. The impedance meter and the operator occupied a "crow's nest" at the top of a third pole in the center. The operator telephoned the readings to a nearby room in the lab, where they were entered and processed during each run.

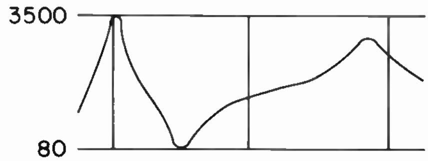
**The horizontal dipole antenna** was a subject new to us. I made a study of that and transmission lines, which I wrote up in Rep. 516W, 341121. My sources were rather old reference books: Bu. of Standards C-74 (1918), Steinmetz (1920) and Pierce (1920).

For operation over a wide band of frequency (6-18) I perceived some principles:

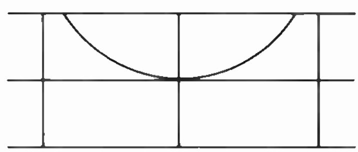
- The effective length of the dipole should not exceed about 3/2 wavelength, else the broadside radiation would be impaired. This meant 25 meters effective length at 18 Mc. Any practical length would be less so this was not a problem.
- For wideband "matching" to the line, the dipole should be made long for radiation resistance, then thick for reducing reactance.



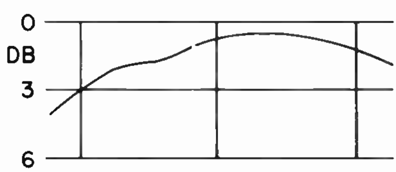

(a) The variation of impedence of the doublet.




(b) After wideband tuning.

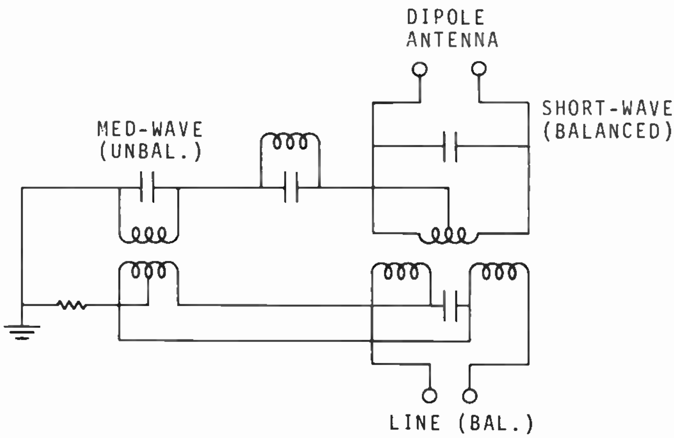


(c) The image impedance of the connected wave filter.

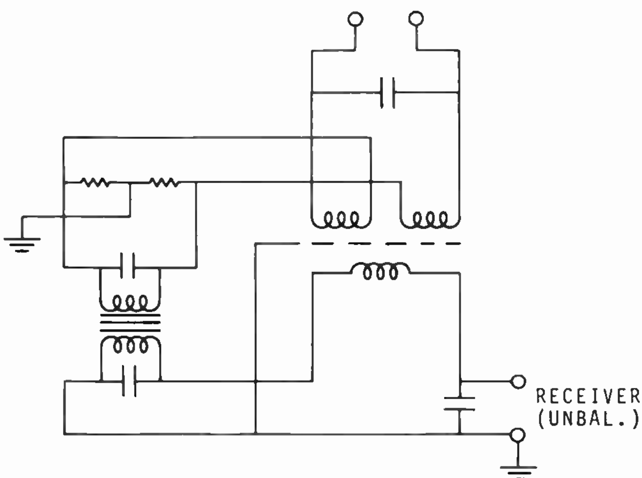



(d) The reflection loss at the junction.

6.5 Fig. 3 — Wideband matching of the 15-meter double-V doublet with a transmission line.

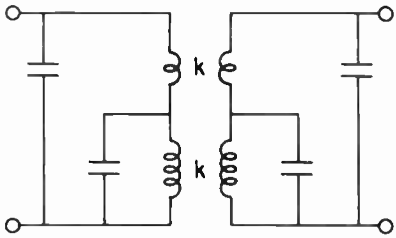



(a) The antenna-to-line filter.




(b) The line-to-receiver filter.

6.5 Fig. 4 — The transformer-filters at the ends of the balanced transmission line.

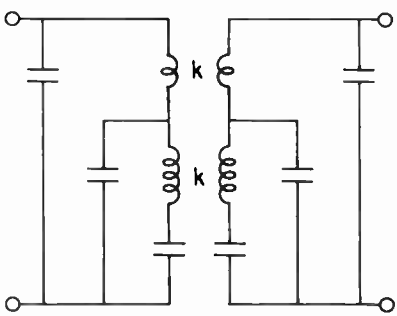



$f_1, f_2, f_3, f_4$

$$\frac{f_1}{f_2} = \frac{f_2}{f_3} = \frac{f_3}{f_4} = \sqrt{\frac{1-k}{1+k}}$$

$$k = \frac{f_2^2 - f_1^2}{f_2^2 + f_1^2} \qquad \left.\begin{matrix} f_1 = 0.54 \\ f_4 = 18 \end{matrix}\right\} k = 0.824$$

(a) The first type of double-transformer filter.




$f_1, f_2, f_3, f_4, f_5$

$f_1 = 0.54$, $f_5 = 18$, $k = 0.705$

(b) The second type, with an extra capacitor on each side.

6.5 Fig. 5 — Continuous-band double-transformer filters.

Toward the latter objective, I considered the following.

**A conical dipole** is shown in Fig. 2. I thought of this as an improvement for wideband operation. I was attracted to its simplicity and I was able to compute its impedance. In preparing a patent application, my attorney came up with an old book by Sir Oliver Lodge. He showed the conical dipole with a spark gap in the middle for use as a transmitter. He showed the surrounding spherical-wave pattern. So we did not file an application.

I had a friend a few years older, Philip S. Carter, who was an antenna expert in RCA out on Long Island. Unknown to me, he also thought of the conical dipole. Apparently he did not find the Lodge book and neither did the Patent Office, so he obtained patents on the conical dipole as a wideband antenna. Perhaps our use for it really did amount to an invention that was patentable over Lodge. My work was 2 or 3 years ahead of his filing dates.

As a practical matter, I saw a few wires in parallel as a step in the right direction. I computed that two wires in parallel would have the properties of one round conductor of this radius:

$$\text{effective radius} = \sqrt{\text{wire radius} \times \text{wire separation}}$$

After some experimenting, we chose as a compromise the two-wire "double-V" doublet or dipole, as shown in Fig. 1, with length of 15 meters (50 ft.) and end separation of 2 meters.

**The transmission line** was an open pair or a pair enclosed in an insulating sheath. We came to prefer the latter, with a wave resistance of 115 ohms. (The present 300-ohm line became available a few years later for FM antennas on higher frequencies.)

**The antenna transformer** provided coupling to the balanced lines in two modes:

- For the U.S. band, using both halves of the antenna in parallel by an unbalanced antenna circuit.
- For the short-wave bands, using both halves of the antenna in series by a balanced antenna circuit.

The transformer included frequency filters for the separation of these two modes at some frequency in between. They were tailored to the properties of a specified antenna.

**The receiver transformer** provided coupling from the balanced line to the unbalanced receiver circuit (nominally 400 ohms). In those days before ferrites, it was not feasible to make a transformer with close enough coupling for the entire frequency range. A double-transformer divided-band filter was made with available values of coupling.

**A tapered line** was considered as a balanced impedance transformer from dipole antenna to line. Other workers also were considering this approach. It was not used, but it is reported here. The ideal model would have an exponential taper of wave resistance by spacing of a pair of wires. That would be analogous to the exponential horn for sound waves. I analyzed this model as a highpass filter and described terminations which would give image impedance like that of a constant-K highpass filter. Also I described a lowpass filter with tapered impedance to behave like a bandpass filter. I obtained patents on these models, but I do not know of any use. They involved interesting concepts.

**Computations.** Measurements of complex impedance or admittance, as on the balanced dipole antenna, had to be converted and interpreted by computation of formulas. It was a laborious process by sliderule, using the usual trigonometric routine. I devised a shortcut for quadrature addition, using the addition or subtraction of unity. On the straight sliderule, this routine would often run off scale, requiring a resetting of the slide. I had a collection of sliderules, including a circular sliderule which would avoid running off scale. Therefore we used this circular sliderule for many computations. It was really primitive compared with the small personal calculator of today, which could be programmed to make such computations.

**A dummy antenna** was made to simulate the balanced impedance of the 15-meter dipole and its excitation by the horizontal electric field of an incoming wave. [A] This enabled the measurement of the overall performance of the system, from antenna to receiver.

**Comments.** The all-wave antenna system was a major project in the Bayside lab. It yielded design reports to our licensees. Our design was manufactured by Bosch, Hammarlund and Taco, and perhaps others. It had a moderate amount of use in competition with some products for the same purpose.

Outstanding work was performed by Vernon Whitman and Rudy Sturm under my direction. It required a variety of skills and ingenuity, and the result was a remarkable yield for the time invested.

The greatest future impact has come from the wideband adaptation of a resonant antenna to a long line. It was founded on the inclusion of the antenna reactance in some arms of a wave filter based on image design. I have made numerous applications of this concept, and my associates and I are constantly extending its use. It is too little appreciated outside my group and our alumni.

The implications of this exercise went far beyond its direct applications. Previously little attention had been paid to antennas by the designers of home receivers, like myself. Then some of us were exposed to antenna design for the first time. It became more of a challenge with the progress of the frontier of higher frequency. After SW broadcast reception came the FM and TV services, then the radar during the war. My introduction to antennas before the war became a specialty during the war and afterward. That was mentioned in my Navy Certificate of Commendation after the war, and later in my citation for the Medal of Honor from IEEE.

[A] [W 19] With V. E. Whitman, "The design of doublet antenna systems", Proc. IRE, vol. 24, p. 1257-1275; Oct. 1936. (Presented in Rochester, 351120.)

[B] Hazeltine Reports.

| No. | Date | Author | Title |
|---|---|---|---|
| 516W | 341121 | HAW | Doublet antennas and transmission lines. |
| 521W | 341218 | HAW | Reflection loss chart. |
| 543W | 350318 | HAW | Transmission lines with exponential taper. |
| 546W | 350329 | VEW | Simple doublets of various lengths. |
| 571W | 350613 | HAW | Design formulas for transformer filters. |
| 575W | 350621 | VEW | All-wave antenna system for 15-meter doublet |
| 606W | 351202 | VEW | Bosch KT9525 antenna system. |
| 623W | 360309 | VEW | All-wave antenna summary. |
| 637W | 360410 | HAW | All-wave dummy antenna. |
| 641W | 360430 | HAW | The design of doublet antenna systems. |
| 660W | 360723 | RES | Antenna impedance meter. |
| 731W | 370623 | RES | Balanced doublet antenna filter and receiver filter design. |
| 741W | 370720 | RES | Dummy doublet antennas. |
| 766W | 371020 | VEW | Transformer filters — part 2. |
| 766AW | 371020 | VEW | Transformer filters — part 3. |
| 805W | 380223 | HAW | Relation between band-pass and tapered low-pass filters. |
| 831W | 380401 | HAW | Transformer filters — part 4. |
| 1279 | 350625 | DEH | 15-meter all-wave doublet antenna. |
| 1292 | 350729 | CKH | Simplified antenna system for 15-meter doublet. |
| 1361 | 351218 | CKH | 15-meter double-V doublet antenna system. |

[C] H. A. Wheeler, U.S. Patents.

| | Pat. No. | Issued / Filed |
|---|---|---|
| (1) | 2064774 | 361215 / 350610 |
| (2) | 2064775 | 361215 / 350610 |
| (3) | 2081861 | 370525 / 350610 |
| (4) | 2092709 | 370907 / 351119 |
| (5) | 2204712 | 400618 / 370826 |
| (6) | 2206990 | 400709 / 390418 |
| (7) | 2239136 | 410422 / 380523 |

Notes (1) Relations between antenna and line; band-pass filter to match mean impedance in one band; balanced in higher band and unbalanced in lower band.

(2) (4) Divided-band transformer filter for antenna end of line, balanced and unbalanced operation.

(3) (7) Continuous-band transformer filter for receiver end of line, balanced-to-unbalanced operation.

(5) Tapered line as high-pass filter.

(6) Tapered low-pass filter as band-pass filter.

[D] O. J. Lodge (Sir Oliver), "Electric telegraphy", Brit. Pat. 609154; 1898 FEB 01 - 1898 AUG 16. (Biconical dipole for transmitter or receiver antenna, series inductance for like tuning.) (MacLaurin, 1949, p. 11.)

[E] E. L. Norton, U.S. Pat. 1954943; 340417 / 320729. (The basic reference on a symmetrical multitransformer continuous-band filter.)

[F] W. H. Bohlke, V. D. Landon, "Principles of noise-reducing antennae", RCA Radio Service News, vol. 1, no. 7; Feb. 4, 1935. (Multiple-transformer filter with C to ground from every coil terminal.)

[G] L. F. Curtis, "Automatic antenna kit, KT9525", United Amer. Bosch Corp.; Sept. 1, 1935. (Design from Hazeltine Rep. 1279.)

[H] [W 22] "Transmission lines with exponential taper", Proc. IRE, vol. 27, pp. 65-71; Jan. 1939. (Termination in a high-pass filter.)

[I] P. S. Carter, "Simple television antennas", RCA Rev. vol. 4, pp. 168-185; Oct. 1939. (Includes conical doublet for wideband operation.)

[J] F. E. Terman, "Radio Engineers Handbook", McGraw-Hill; 1943. (All-wave antenna system, p. 859, Hazeltine.)

[K] H.A.W., "Handbook of Electromagnetics", Wheeler Labs., Inc.; from 1968. (12.06.01 . . . , multitransformer filters, tapered lowpass filters. Design formulas from Reps. 571W, 766W, 766AW, 805W, 831W.)

## 6.6 The Expanding Selector (XPS) 1933-37.

The Expanding Selector (XPS) was a feature we developed for increasing the bandwidth of the IF amplifier in a superheterodyne receiver. It was first made with manual control. Later it was made with automatic control in response to signal conditions. Then it was termed the Automatic Expanding Selector (AXPS).

The need for the XPS came from the conflicting objectives which had to be compromised in practice:

- "High fidelity" (now HI-FI) was the growing ambition to reproduce the entire AF range of sound, especially music. The AM receiver could be designed to cover about 8 Kc which required a frequency bandwidth ±8 from the carrier.
- Selectivity was the ambition to receive one signal in spite of another on a nearby channel. The latter might be on an adjacent channel and/or might be stronger. This result might have required lesser bandwidth, perhaps ±4 or even less.

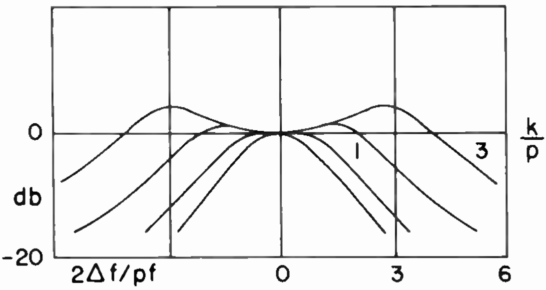
The conditions of interference differed greatly with such circumstances as:

- Frequency separation in terms of channel spacing;
- Relative power and distance of transmitters;
- Time of day and fading;
- Program material and tolerance of spurious sounds.

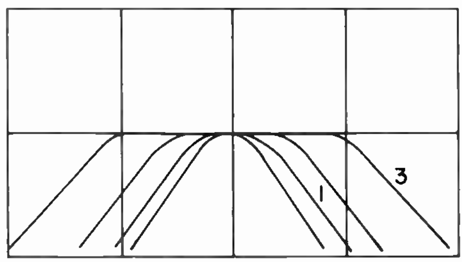
The objective in receiver design was the most pleasing compromise in any set of circumstances. This result required an adaptive control of the receiver bandwidth, either manual or automatic. So we undertook to develop practical receiver designs with this feature. It was a major project in the Bayside lab 1933-37.

Our designs for XPS and AXPS saw little or no use in broadcast receivers on the market. The demand for high fidelity did not reach a high level before World War II. Then FM broadcasting came to offer much greater bandwidth and also stereophonic sound. As a result, high fidelity in AM receivers could not complete.

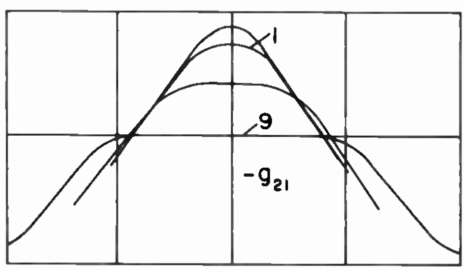
After the Armstrong superheterodyne became available in 1930 for all broadcast receivers, the first wave of improvements were directed to problems inherent in the superheterodyne. These are discussed in other chapters. The second wave of improvements were directed to the refinement of performance, notably the compromise between adjacent-channel selectivity and high-audio reproduction. Our XPS was intended to make available the most bandwidth that could be utilized in the reception of any one station. It was limited mainly by interference from adjacent-channel signals on one or both sides. Adaptation to the existing conditions could be accomplished most




(a) Two tuned circuits (p) with variable coupling (k).

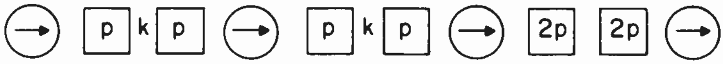



(b) Same with third tuned circuit (2p).

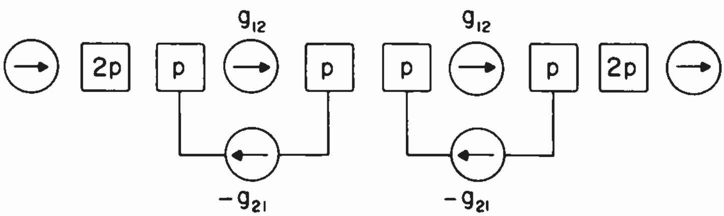



(c) Same except bidirective coupling (fixed $g_{12}$, variable $-g_{21}$).

6.6 Fig. 1 — Bandwidth control by variation of coupling.




(a) Control by two couplings (k).




(b) Control by two backward couplings ($-g_{21}$).

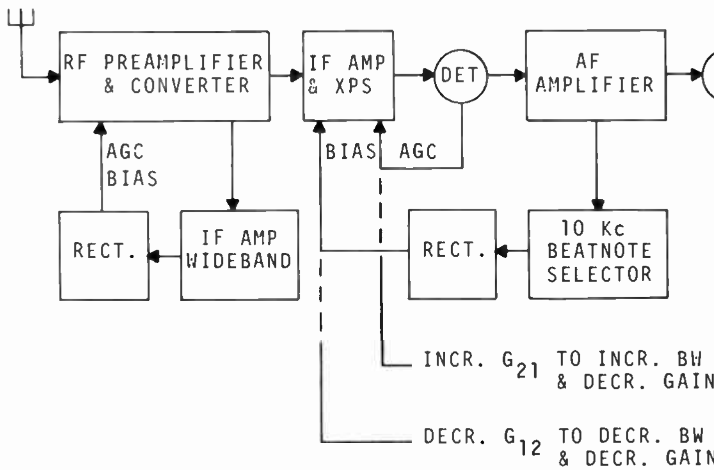
6.6 Fig. 2 — Bandwidth control in IF amplifier with six tuned circuits.

closely by a manual adjustment of XPS. Later we devised automatic controls (AXPS) which were able to yield nearly the closest adaptation while easing the demands on the users attention. Furthermore, the AXPS could adapt to changing conditions, such as "fading" of the desired or the interfering signal.

There were various ways to expand the bandwidth in the IF amplifier. Any one could be designed to be operated from an added control knob. The addition of a control knob on a "unicontrol" receiver was objectionable. One of our first developments was a solution to this problem.

Single-knob control of both tuning and bandwidth was provided by a mechanical device which utilized both rotary and axial motion of the knob:

- Rotary motion for tuning.
- Axial motion (outward from the panel) for bandwidth expansion.
- An interlocking mechanism for disengaging the tuning when the knob was pulled outward.




6.6 Fig. 3 — Superheterodyne receiver with AXPS.

The third feature was needed so the tuning would be adjusted with the least bandwidth, for which it was most critical. The first patent listed was issued to Nelson Case and myself on this feature. [D] (1)

In the Patent Office, we were to learn of a much earlier invention by L. J. Lesh, likewise using rotary and axial motion of one knob for tuning and another function. The other function was regeneration in the Armstrong regenerative receiver. It is remarkable that the other function, in that case, inversely adjusted the gain and the bandwidth in a manner we were to develop to a high degree of refinement. Hazeltine Corp. acquired the Lesh patent. [G] His application to regeneration was subject to a critical and unpredictable condition of minimum bandwidth, so the interlock feature could not have been used. The dual function of one knob would have been useful in various situations. When the Neutrodyne receiver had a second knob for the antenna tuning, I had proposed a unicontrol tuning by rotation, supplemented by axial motion for trimming the slightly different antenna tuning.

**A triple-tuned selector** was developed for bandwidth expansion in an IF amplifier. [A] [C] (477W) It was remarkable in requiring only one variable parameter for expanding the width of a bandpass response of three peaks at the same level. Its essential behavior is shown in Fig. 1. Its components are three circuits tuned to the same center frequency and related as follows:

- A pair of like circuits, each having a bandwidth dissipation factor (p or 1/Q), with a variable coefficient of coupling (k);
- A third circuit, alike except double the dissipation factor (2p).

Fig. 1(a) shows the response curves of the pair, which tend to have high peaks off center with increasing coupling (k/p). Fig. 1(b) shows the same but multiplied by the wider peak of the third circuit on center. The latter expands to three peaks approximating a flat response of variable width.

Fig. 2(a) shows how two of these sets were used in an IF amplifier with six tuned circuits. [A] Their bandwidth expansion was controlled by the variation of coupling (k) in two of the three double-tuned interstage transformers.

At that time (1934-35) I made a comprehensive study of the behavior of a pair of tuned circuits with coupling and/or symmetrical detuning. [C] (612W) I made the "discovery" that a symmetrical double-peaked response had the same shape for coupling or detuning. I do not know whether that was yet common knowledge among theoretical specialists in the field. Furthermore, I "discovered" that the product of a pair of coupled circuits (p,k) and a third circuit (2p) yielded a variable-width response of three equal peaks. I believe that this was a substantial invention at that time, and we made much use of it. I do not find any claim to it, but I do not recall any reason for not claiming it. With progress in network theory, we learned that this triple-tuned set could be described in terms of three poles on an ellipse on the plane of complex frequency. [J] Changing the coupling or detuning would merely expand the ellipse on the frequency axis.

**Automatic XPS** or AXPS was the application of automatic controls for adapting XPS to any set of conditions. Various approaches had occcurred to us and to others. The problems and developments fell in two categories:

- XPS circuits that could be subjected to control by a bias voltage (in the manner of the prevalent AVC);
- Signal-responsive circuits for developing bias voltages to obtain an acceptable compromise in XPS.

Progress in the former was most notable in feedback-filter circuits. Progress in the latter was most notable in its response to adjacent-channel interference.

**Feedback-filter circuits** were exemplified by a pair of tuned circuits with forward and/or backward coupling through a vacuum tube. The tube could be controlled by a bias voltage derived in whatever manner from the desired and undesired signals. The product of forward and backward coupling (in proper phase) could yield the same response as some value of the coefficient of coupling. Such circuits were not familiar so there was a fertile field for innovation.

This was one of the topics brought to my attention by Lester F. Curtis, probably around the beginning of 1936. He was Chief Engineer of United American Bosch Corp., a new licensee. He had a patent application pending, which related to this subject. [F] The resulting patent was later acquired by Hazeltine Corp. and reissued. Les Curtis joined us at the Bayside lab 370801.

In that patent application, Curtis showed a pair of tuned circuits with the usual inductive coupling supplemented by backward coupling through a tube controlled by the AVC bias. His filing date of 340921 was earlier than any other record I have found, showing this combination:

- A pair of coupled tuned circuits in an IF amplifier;
- A double-peaked response obtained by feedback through a backward coupling tube.

Also he showed automatic bias control (in response to the signal) for increasing the effective coefficient of coupling between the tuned circuits, and also decreasing the gain. I do not recall, to what extent his work may have influenced my subsequent development of bidirective coupling equivalent to mutual reactance between a pair of tuned circuits.

Building on my study of a coupled pair of tuned circuits, I developed rules and formulas for using directive coupling by vacuum tubes. [C] (612AW) This was a monumental study, probably far ahead of any work by others up to that time, 360203. It was based on the concept of bidirective coupling which could be described separately in the forward and backward directions. For example:

$+g_{12}$ = forward transconductance;

$-g_{21}$ = backward transconductance

From these I stated the equivalent coupling capacitance,

$$\omega C = \sqrt{g_{12}g_{21}}$$

and the midband power ratio of amplification,

$$A^2 = g_{12}/g_{21}$$

It is notable that increasing the backward coupling had the effect of increasing the bandwidth coupling coefficient (k) while decreasing the gain.

Fig. 1(c) shows this effect of bidirective coupling in the set of three tuned circuits previously described. It corresponds to the effect of symmetrical detuning as an alternative to coupling, because that likewise would decrease the gain while increasing the bandwidth.

Fig. 2(b) shows a pattern in which we used bidirective coupling in two pairs of a set of six tuned circuits. Bias control of various kinds was applied to the tubes in different functions.

We devised many schemes for AXPS with symmetrical or unsymmetrical expansion under various controls. Fig. 3 shows the pattern of a typical set with symmetrical AXPS. It has these features:

- Separate AGC (automatic gain control) bias on the RF preamplifier to protect the IF amplifier from excessive total strength of desired signal and any undesired signals.
- Usual AGC bias derived from the detector, but applied to increase the backward coupling ($g_{21}$) for decreasing gain and also increasing bandwidth.
- Extra AGC bias responsive to 10-Kc beatnote from the carrier of an undesired signal on either adjacent channel, and applied to decrease the forward coupling ($g_{12}$).

The last was the most unusual in that its result was a composite effect:

- Decreasing the forward coupling had the direct effect of decreasing the bandwidth;
- Also it forced a reduction of the usual bias (for restoring the signal level at the detector) which had the indirect effect of further decreasing the bandwidth.

By proportioning these bias controls, the beatnote was held below any prescribed level relative to the desired-signal modulation. A receiver along these lines was made by Farrington and described in his IRE paper. [B]

The most sophisticated AXPS had independent expansion of bandwidth on opposite sides of the carrier. These features formed one pattern that was demonstrated:

- AFC (automatic frequency control) for centering the tuning on the desired-signal carrier (duly protected against any near-channel interfering carriers);
- A "tuning indicator" for centering the AFC to assure its margin of effectiveness;
- XPS in the IF amplifier by symmetrical detuning in proportion to signal strength;
- Separate bias control responsive to interference on lower and higher channels, such that either reduced the bandwidth expansion caused by detuning toward that side.

It was a memorable experience to operate this receiver. The desired signal was selected by one knob. On nearly every channel, a signal was received. It was free of near-channel interference. It was heard with the highest audio response that was consistent with the margin of protection against interference. Naturally some forms of distortion could not be remedied, such as co-channel interference or selective fading.

This work was mostly conducted in the Bayside lab under my direction by Nelson P. Case, John F. Farrington, Clyde K. Huxtable, Joseph L. Hurff, William B. Wilkins, and others. Every one did excellent work to meet unusual requirements, and contributed in proportion to his experience.

We should recognize some of the contemporary work by other groups toward the same general objectives. It forms the background over which our work stands out. There were many approaches to manual XPS, that came with the wider use of the superheterodyne and the striving for higher audio fidelity. Advances in automatic XPS (AXPS) were later and we were among the leaders.

For manual XPS, our use of one knob was new with us and I do not know of this idea from anywhere else. The idea of rotary tuning with narrowband and then pulling out for wideband was not applicable in the receivers contemporary with Lesh, the only one known to have used one knob for tuning and another function.

For manual or automatic XPS, my relationship for a set of three tuned circuits was new and I do not know of this idea from anywhere else. It was basically the concept of:

- A pair of tuned circuits of equal dissipation factor (p) with double peak obtained by a single variable coupling coefficient (k) or the equivalent bidirective coupling or symmetrical detuning;
- A third circuit of double dissipation factor (2p) for levelling the double peak at the center level.

I believe this was a major contribution in 1934 and would have been used if XPS had appeared in competitive receivers (instead of being superseded by the FM service).

For AXPS, bidirective coupling of a pair of tuned circuits offered the most versatility for bias control. It was a major advance over Curtis's 1934 feedback circuit. My use of forward and backward tubes of opposite transconductance for coupling was the simplest approach. My analysis clearly presented its properties and opportunities. I believe it was new late in 1935 when I did it. There is some question how close was some related work by H. D. Ellis in England in 1934 and by Tellegen and vanLoon in Holland in 1935. A contemporary worker in U.S. was W. vanB. Roberts in RCA. It was a distinction to be competing with inventors of their caliber.

The greatest refinement in performance of AXPS was obtained with controls responsive to the adjacent-channel carrier on one or both sides. Of my patents listed, the second to be filed was directed to this feature. [D] (9) It was the last to issue, because it was challenged in U.S. Patent Office by an application of two prominent British inventors, N. R. Bligh and C. N. Smyth in General Electric Co., Ltd. Their Convention date was prior to my filing date. After my proof of earlier invention in U.S., I was awarded broad claims to bandwidth contraction in response to a nearby undesired-signal carrier. Those and other British inventors around 1935 may have anticipated my invention of automatic symmetrical contraction in response to an adjacent-carrier beatnote. I am not aware of anyone else showing independent-sideband or unsymmetrical contraction in response to near-channel interference on either side. [D] (5) (6)

The Expanding Selector was a challenge to our experience and ingenuity with the objective of making a broadcast receiver for best operation in a variety of practical situations. We made much progress, though our refinements were not afforded in the receivers of later years. The feedback amplifier with bidirective coupling was the one feature which has become more important with the passage of time.


[A] [W 15] with J. K. Johnson, "High fidelity receivers with expanding selectors", Proc. IRE, vol. 23, pp. 594-609; Jun. 1935. (Manual XPS by coupling variation in each pair of p circuits associated with one 2p circuit. Control by axial motion of tuning knob, interlocked for narrowband tuning.)

[B] J. F. Farrington, "Receiver with automatic selectivity control responsive to interference", Proc. IRE, vol. 27, pp. 239-244; Apr. 1939. (Contains numerous features from the Hazeltine work at Bayside lab. References 1934-36, early work.)

[C] Hazeltine Reports, Bayside Lab.

| No. | Date | Author | Title |
|---|---|---|---|
| 428W | 330901 | HAW | Operation of expanding selector. |
| 477W | 340514 | HAW | Expanding selector theory. |
| 518W | 341207 | HAW, JKJ | High fidelity receivers with expanding selectors. |
| 607W | 351216 | JFF | Triple superheterodyne AXPS receiver. |
| 612W | 360116 | HAW | A generalized theory of coupled circuits. |
| 612AW | 360203 | HAW | Directive coupling of tuned circuits. |
| 633W | 360407 | HAW | AXPS contracted by carrier beat note. |
| 635W | 360415 | JFF | Second AXPS receiver. |
| 651W | 360701 | HAW | Automatic expanding selectors. |

| | | | |
|---|---|---|---|
| 689W | 370129 | HAW | Proposed receiver with AXPS, AFC and antenuator. |
| 761W | 370928 | HAW | Feedback amplifier filters. |

[D] H. A. Wheeler, U.S. Patents.

| | Pat. No. | Issued / Filed |
|---|---|---|
| (1) | 2024017 | 351210 / 331107 |
| (2) | 2115676 | 380426 / 360620 |
| (3) | 2152514 | 390328 / 370618 |
| (4) | 2152618 | 390328 / 360721 |
| (5) | 2168461 | 390808 / 360721 |
| (6) | 2183980 | 391219 / 360128 |
| (7) | 2185388 | 400102 / 370729 |
| (8) | 2185389 | 400102 / 380509 |
| (9) | 2255690 | 410909 / 351022 |

Notes (1) With NPC. Rotary tuning knob with axial motion for XPS. Interlock for narrowband tuning.

(2) Triple superheterodyne for symmetrical detuning by a second local oscillator, used for AXPS.

(4) AXPS by bias control of bidirectional coupling.

(5) Independent-sideband AXPS, reduced in response to inter-channel beatnote.

(6) Independent-sideband AXPS, reduced in response to adjacent-channel interference.

(8) Feedback amplifier filter.

(9) AXPS preceeded by wideband AVC responsive to near-channel interference.

[E] J. F. Farrington, U.S. Patents to Hazeltine Corp.

| | Pat. No. | Issued / Filed |
|---|---|---|
| (1) | 2054412 | 360915 / 350507 |
| (2) | 2167400 | 390725 / 370121 |
| (3) | 2216998 | 401008 / 380427 |

[F] L. F. Curtis, U.S. Pat. 2033330 to United Amer. Bosch Corp. 360310/340921. Reissued as Re.21222 to Hazeltine Corp. 391003/370911. (Pair of tuned circuits with backward coupling tube to spread the peaks and reduce the gain, responsive to AVC bias.)

[G] L. J. Lesh, U.S. Pat. 1934722 to Associated Elec. Labs., Inc., 331114/240710. (Later assigned to Hazeltine Corp.) (One knob for two functions: rotation for tuning; axial motion for coupling variation. The latter for regeneration giving inverse variation of gain and bandwidth.)

[H] L. A. Hazeltine, discussion on "The shielded Neutrodyne receiver", by Dreyer and Manson, Proc. IRE, vol. 14, pp. 395-412; Jun. 1926. (Symmetrical detuning with simultaneous reduction of gain and expansion of bandwidth.)

[I] H. F. Mayer, "Automatic selectivity control", Electronics, vol. 9, no. 12, pp. 32-34; Dec. 1936. (By feedback through backward tube, giving symmetrical double-peak resonance curve.)
[J] H. A. W., "The potential analog applied to the synthesis of stagger-tuned filters", IRE Trans., vol. CT-2, pp. 86-96; Mar. 1955. [WM-15]
[K] H. A. W., "Handbook of Electromagnetics", Wheeler Labs., Inc., from 1968. (12.06.31 . . . , feedback-amplifier filters. Design formulas from Reps. 761W, 1014W.)
[L] Hazeltine Service Corp., "A variable selectivity superheterodyne", Electronics, vol. 8, pp. 180-183; Jun. 1935.
[M] D. E. Harnett, N. P. Case, W. Lyons, (ed.) "Automatic bass control and variable selectivity", Proc. Radio Club of Amer., vol. 12, p. 27; Sep. 1935.
[N] C. E. Dean, "Bandwidth factors for cascade tuned circuits", Electronics, vol. 14, no. 7, pp. 45-45; Jul. 1941. (Normalized graphs devised by Wheeler.)

# Section 7. Television

## 7.1 Television (TV) 1932-41.

Our activities in television (TV) started under Harold M. Lewis in the Bayside lab in 1932. He perceived the future of TV and was naturally motivated to explore this new field. He chose Madison Cawein as his assistant. From this autonomous group, the television work grew to be utilizing most of our expanding staff at the end of the decade. I was preoccupied with developments in the reception of sound broadcasting and in patent litigation, so I was slow to assume leadership in the TV work. The remainder of the Bayside staff was directly under my supervision, and gradually cooperated more and more with the television group. The entire staff was integrated under Dan Harnett when he took charge of Bayside late in 1936. From that time, my principal functions in television were conducting classes, making special studies, supervising some developments, and participating in general.

In this chapter, I shall give a story of our TV work up to the beginning of World War II, when it was interrupted. Some areas in which I was particularly active are mentioned here and reported in some detail in separate chapters under these headings:

- Television Images 1937.
- Paired Echoes 1938.
- Wideband Amplifiers 1937
- Magnetic Scanning 1938.

First I shall mention the background of TV progress up to the time we became active.

**Before 1932**, TV had progressed to the stage of mechanical scanning in the transmitter with electronic scanning in the receiver. The progress was proportional to the number of lines in one frame of the picture. There was little change in the frame frequency (24 or 30 per second). The 1932 experimental transmissions by RCA were characterized by these features:

- Going to a much higher carrier frequency (around 50 Mc) to enable the use of greater bandwidth for more lines.
- Transmitting from the top of the Empire State Building, the newest and tallest skyscraper in New York City, giving line-of-sight range about 30 miles.
- Going to 120 lines for electronic scanning in the receiver (on a cathode-ray oscilloscope used as the picture tube).

Subsequent progress went to 240, 343, 441 and finally 525 lines. The odd numbers were achieved by dividing each frame of lines into two sequential interlaced fields, whose frequency (60) reduced the flicker in viewing. About 1933, the RCA image-storage tube (the Zworykin "iconoscope") enabled efficient electronic scanning in the camera tube and thereby set the future course for all-electronic scanning. By 1938, the line frequency had progresse‹ to 441. The last major advance was about 1938, and it related to utilization of the channel width (finally 6 Mc). Instead of transmitting both sidebands of modulation (as in sound AM), it was found sufficient to transmit all of one sideband but only the inner parts of both sidebands. This was termed "vestigial sideband" transmission, which was not a description. (We sometimes called it "sesquisideband", also not a good description.) This feature enabled nearly double the useful bandwidth of the video signal.

**Lewis and Cawein** were the prime movers in our TV program from the beginning in 1932 until late in 1936, when Harnett was given the overall responsibility of the Bayside lab. They were joined in 1935 by Hergenrother who made the camera tube we needed for a complete system demonstration. To these three men goes the primary credit for the timely launching of our expanding TV activities. It happened in the period of greatest acceleration in TV development in U.S. The all-electronic system became established and it was ripe for intensive refinement in technology (understanding and engineering).

The most unusual function in TV was the sawtooth scanning of the picture frame (the raster of lines to form the image). The generation of a linear sawtooth waveform was based on new concepts but was not very difficult. It was used for cathode-ray oscilloscopes in laboratory experimenting and testing. The same was true of the synchronizing process ("sync"). The TV signal included special timing pulses for this purpose. One of the principal topics was the design of these pulses and their utilization in the receiver.

The most demanding function in TV was the amplification of the wideband signals required for the rapid transmission of the variations in the picture signal. In contrast to the bandwidth of about 10 Kc for an audio signal, a bandwidth of about 3000 Kc or 3 Mc came to be required for a video signal. This was triple the width of the entire broadcast band for 100 sound channels, and it had to be amplified all at once (not just a selected channel). Furthermore, the picture reproduction was sensitive to some kinds of distortion not noticeable in sound. These considerations introduced a whole new game in the technology of amplification and channel selection. This game attracted much of my attention.

Early in 1932, Lewis and Cawein planned and made an elementary receiver for the TV signals then being broadcast by RCA on an experimental basis. They brought to this project some recent experience with short-wave receivers in our labs. This TV receiver was demonstrated and another one was made for Philco. [C] (Their name had been changed to Philco Radio & Television Corp.) Late in that year, Lewis made a plan for TV development in the Bayside Lab.

From 1933, there was one line of development of the various kinds of test equipment needed for TV receivers. [B] Furthermore, we needed a camera tube for generating a video signal to be used for picture reception.

**The Camera Tube** which we made in the Bayside lab in 1936 was one of the principal achievements in our TV program. It was remarkable for its planning and its successful execution. It was essential to the complete demonstration we were assembling in our lab. Its beginning was a TV planning conference 350729 among MacDonald, Lewis, Farrington and myself. Among other things, we recognized the need for a camera tube in order to test and to demonstrate our TV receivers without relying on the RCA programs from the Empire State Bldg. They used the Zworykin "iconoscope" (image-storage tube) which was not available outside RCA. Therefore we would have to make one. It was one of the most sophisticated of electronic devices up to that time. We had no background or competence in making a vacuum tube of any kind. A fortunate set of circumstances led to a solution of our problem.

Madison Cawein had been a member of our engineering staff for 5 years and was then working with Lewis in the main line of our TV program. At Cornell U. in the mid-1920's, he had become acquainted with Rudolph C. Hergenrother, and their friendship grew while they were later both employed at Westinghouse in Bloomfield, N. J. In 1935, Dr. Hergenrother had been working on TV tubes in Farnsworth Television Labs, but they were reducing staff. Cawein invited him to join our staff, and we employed him to make the camera tube we needed. Hergie brought to us a powerful blend of all the education and skills that would be required. Those ranged from a Ph.D. in Physics at Cal. Tech. to practical experience in glass blowing.

Starting from scratch in the old Bayside lab, Hergenrother equipped a tube laboratory for the processes needed to make a camera tube patterned after the RCA model. It was a large cathode-ray tube with unusual internal structure and materials. In 1936, he made such a tube with excellent performance. It placed us in the position of being the only lab outside RCA to have an equivalent camera tube. It is doubtful if any other single engineer could have performed this task. He had one assistant and the incidental cooperation of one or more other members of our staff. It must have given him much satisfaction and we accorded to him the appreciation that he deserved.

The camera tube required various accessory circuits for beam forming, beam scanning and amplification of the video signal output. These were all within our range of competence by that time, so we soon had a picture signal generator. It was adapted for pickup from movie film or a live subject. It was used to modulate a carrier for complete testing of a TV receiver. At that time, other labs outside RCA had to use a stationary pattern in a simulated camera tube ("monoscope") for modulation in a signal generator. We were unique in capability and were established as the leader outside RCA.

**The main line of TV development** was a series of receivers embodying improvements as time went on (1936-40). [D] This occupied an increasing number of our staff, mostly at Bayside and later Little Neck but also in New York. My active participation in the TV program began with the design of RF, IF and VF (video-frequency) circuits with the first receiver in this series (1A). It was my introduction to the circuit problems peculiar to picture reproduction as distinguished from sound. This led to my intensive studies on these and other topics in TV.

As a "subcontractor" to Lewis and Cawein, I led a few of my group in making the wideband amplifiers required in this TV receiver (A1). Our designs were ingenious and sensible in response to the bandwidth specifications, and we provided some "phase correction" as an improvement over conventional circuits. We were soon to learn that the viewer demanded closer tolerances of amplitude and phase distortion in the wideband circuits.

Each receiver in this series was planned to meet a particular need and/or to demonstrate some features. One of our more recent models was usually installed in MacDonald's office in the New York lab for showing to visitors. Some were operated in our homes for a while to gain experience. At one time I took one of our receivers to the homes of several of my friends who were invalids and otherwise could not have seen TV.

The earlier receivers contained picture tubes with rather small screens for close viewing. With the advent of larger screens, the tubes became longer so the customery horizontal axis required excessive depth behind the receiver cabinet. Then two alternatives were developed:

- The mounting of a long tube with vertical axis, adapted for horizontal viewing in a 45° mirror above the cabinet.
- The design of a "short" tube for horizontal mounting and direct viewing.

The latter set the course for the future. It imposed severe demands on the internal design and the beam-deflection fields for such wide-angle scanning while maintaining the rectangular shape on the screen. The size of screen and the type of viewing are noted for some of the later receivers in the list. [D] For direct viewing of any size of screen, the axial length was designed as a compromise from various considerations.

During this period we benefited greatly from the addition of three brilliant and experienced engineers to our staff:

- Arthur V. Loughren (360201) from GE and RCA.
- John C. Wilson (371001) from CBS and previously Baird in England.
- Leonard R. Malling (381001) also from Baird.

I learned a great deal from each one and they contributed in very different ways.

Jack Wilson had just published the first textbook on television engineering. [L] It was a scholarly presentation based on his experience in Baird Television, Ltd., the pioneering company in England. He was familiar with analytical concepts related to TV and introduced them in our lab. A friend of his as Baird was Leonard Malling, who came to the U.S. later. Malling's genious was practical applications of new ideas.

Art Loughren and I were much interested in the basic relations in a TV picture. We developed some concepts which were related to the reproduction of a pictorial image by the scanning process. They are the subject of a separate chapter, "Television Images 1937". Our joint 1938 IRE paper on this topic became one of the basic references on this subject. It related the vertical and horizontal fineness of resolution as limited by the number of lines and the frequency bandwidth of light variations along the line.

The TV picture showed graphically some defects in the image which we could correlate with some peculiarities in the circuits. One such defect was the widening of a vertical line and fringing effects in its image. Some widening was unavoidable with a restriction of the frequency bandwidth (about 3 Mc in the channel width of 6 Mc). This was simply seen as a blurring of the image. Further visible distortion appeared as ripples either side of the line. These were identified with amplitude and/or phase ripples in the frequency response in the wideband circuits of the transmitter and receiver. The amplitude and phase distortion in the circuits was interrelated so its interpretation was complicated. I perceived a formula for independent interpretation in terms of "paired echoes", which is the subject of a separate chapter. My IRE talk in 1938 was the first publication of this formula and attracted much attention in the profession.

Wideband amplifiers presented one of the principal problems in the design of TV circuits. We had known that the shunt capacitance in a vacuum tube imposed a bandwidth limitation by virtue of its charging time. In my AVC receiver designs in 1929-30, I used one "untuned" RF stage for the bandwidth of the broadcast range (about 1 Mc). I obtained moderate gain with one tetrode. In TV, we needed much gain over much greater bandwidth (about 3 Mc) and it had to be more nearly level. About 1937, I perceived that there was a simple rule for stating the upper limit of level amplification that could be obtained from one tube over a specified bandwidth. I was the first to publish this limitation, in my talk to the IRE Annual Convention in New York 380618. It was entitled, "Wideband amplifiers for television". This topic is included in the separate chapter on "Wideband Amplifiers". This limitation was gradually reduced by improvements in the design of vacuum tubes, but remained as a real problem in TV and radar until after the war. Then the vacuum tube was superseded in a low-power amplifier by the transistor, which has much less capacitance because of its smaller size. Therefore the bandwidth limitation on gain per stage is no longer a problem in the TV receiver. It will always be a problem in the frontier of extremely large bandwidth for extremely short pulses. Therefore my rule has timeless interest. I have extended its scope in postwar studies and publications.

These three IRE papers, relating to TV, established my status as a leader in the field. They were a suitable subject for the Morris Liebmann Memorial Prize awarded annually by IRE. I noted in passing that I was not selected in 1939, but the prize was awarded to an outstanding member of the BTL staff, Harold T. Friis, who was my senior by several years. In the following year (1940) it was awarded to me for this work. I regarded it as a tribute to the Hazeltine laboratories, which had provided the opportunity and stimulation for my studies, and I said so in my brief speech of acceptance.

The wideband amplifiers with which we were most concerned fell in two categories:

- A video-frequency (VF) amplifier for the frequency band from near-zero to an upper bound between 3 and 6 Mc. One was used in the transmitter between the camera tube and the carrier modulator. One was used in the receiver between the picture-signal detector and the picture tube.
- An intermediate-frequency (IF) amplifier for the picture signal in a superheterodyne receiver. It was used between the converter and the detector. Its frequency band was typically 9-13 Mc for the modulated carrier.

Both of these kinds are described in the separate chapter on wide-band amplifiers. Guiding their design was one of my principal functions.

**Beam scanning** in the cathode-ray tube was one of the most active areas of innovation in the TV system. It was a particular challenge in the economical design of the picture-tube circuits in a receiver. A sawtooth wave of voltage or current was required for each direction of deflection, slow vertical and fast horizontal. Electric or magnetic deflection found application as follows:

- Electric deflection was achieved by a sawtooth voltage between an internal pair of deflecting plates, requiring a separate pair for each direction. The two pairs had to be located in different regions along the beam, which was wasteful of space and field energy. This form was adequate for a tube with moderate-voltage beam, a small screen and a small angle of deflection.
- Magnetic deflection was achieved by a sawtooth current in an external pair of deflecting coils, requiring a separate pair for each direction. The

two pairs could operate in the same space. They were integrated in a deflection "yoke" around the neck of the tube, which was constricted to economize in size. This form was preferred for a tube with high-voltage beam, a large screen and a large angle of deflection.

The circuit-design problems were very different for the two kinds.

Electric scanning required a sawtooth voltage. A nominal value (of the order of 100 volts) could be obtained directly from a vacuum tube, without a transformer. The sawtooth waveform could be generated by a simple "relaxation" oscillator. The charging current was small, so little power was needed to excite the deflection plates. This model was attractive for a small tube. It was neither simple nor economical if adapted to a large tube.

Magnetic scanning promised great advantages for a large tube but also presented less familiar problems in design. These problems were related to the field configuration from the coils and especially for wide-angle scanning with uniform speed in both directions on the picture screen. I devoted much effort to the circuit problems. My work is described in a separate chapter on "magnetic scanning". It contributed much to our understanding, as far as principles and concepts of design. The end result for wide-angle scanning required much further development by experimental techniques beyond the scope of theoretical methods.

In 1938, MacDonald organized a Television Planning Board to meet weekly at the New York lab. The original members were MacDonald, Harnett, Lewis, Cawein, Loughren and myself. Our usual meeting time was Thursday afternoon, followed by dinner at a nearby restaurant. Our first meeting was 380407, and our first topic was planning our first TV receiver to have a 12-inch picture tube (receiver no. 8). Presumptively it was a long tube, mounted vertically and viewed in a 45° mirror on top. I attended 13 meetings, but missed a few while Ruth and I went to the West Coast by train for a long vacation, July 9 — August 3. The last meeting in my diary was 380822, just after our return. That was our last summer at the Bayside lab. Those meetings did much to set the course of our TV work to continue in the new Little Neck lab.

It was our custom to hold a series of classes about once a year. Its purpose was continuing education and reporting on current developments. The classes were scheduled during working hours. One of my principal functions was organizing each series and conducting some or all of the classes. On the subject of television, we held six series, although the last was curtailed by the priority of war work.

- 361208-370120, Bayside, eight classes conducted by me. They were attended by all the Bayside engineers and a few from New York, a total of about 16. They were directed mainly to wideband circuit design.

- 370923-1214, Bayside, eleven classes at Bayside except the last which was demonstration in New York lab. The first was conducted by Harnett and Cawein. I did not conduct any classes because I was busy on litigation (including the Baltimore trial 371024-1104). I attended seven.
- 380308-0524, Bayside, eight classes. The first was conducted by Loughren and the second by Curtis and Freeman. I conducted the remaining six. This was the first presentation of my new concepts of paired echoes, wideband amplifiers and feedback filters. They aroused much interest and the attendance was most of our growing television staff.
- 391003-1205, Little Neck, nine classes, of which I conducted five. My participation was interrupted by litigation (including the Detroit trial 391025-1102). I spoke on sawtooth-current circuits for magnetic scanning. I demonstrated the pendulum model of the sawtooth-current oscillator for efficiency.
- 400109-0305, Little Neck, seven classes, on Tuesdays. The speakers and their topics were:
  Sturm, the antenna impedance meter.
  Tyson, the antenna circuit of a TV receiver.
  Loughlin, the phase-curve tracer for TV.
  Malling, an economical TV receiver.
  Curtis (2), frequency modulation.
  Rado, a curve tracer for TV.
- 401022-29, Little Neck, only two classes. Loughlin spoke on his phase-curve tracer and I spoke on shortcuts in computation (the reactance chart and the sliderule). Then I was diverted by litigation (Cincinnati trial 401113-19 and Chicago trial 401128-29) followed by war work.

These 44 classes in a period of four years contributed very much to the development of our engineering staff and to the result of our leadership in television outside RCA. My participation stimulated my development of the concepts which were my principal interest in relation to television.

Concurrently with these classes, we prepared a cumulative textbook entitled, "Television Principles". [F] It was written in 1938-40 by Dr. Charles E. Dean, who was experienced and talented in technical writing. He used material from our classes and other sources. He wrote 13 chapters in the form of laboratory reports which were serially printed and distributed to our engineers and our licensees. Then the work was terminated in favor of war work. I prepared the drafts for the last three chapters, relating to magnetic scanning. Other principal contributors were Harnett, Hergenrother, Loughren, Bailey and Wilson. In 1944, the collection was published in a bound volume for complimentary distribution to our staff and licensees. The result was a valuable collection of timely information, much of which has not been published elsewhere.

**The World's Fair** in New York 1939 saw the U.S. debut of TV broadcasting. The RCA transmitter at the Empire State Building was operating on regular schedule. There was a continuous demonstration in the RCA Building at the Fair. The most spectacular event was the TV broadcast of King George VI and Queen Elizabeth on their visit to the Fair 390610 from England. On that Saturday afternoon, we demonstrated some receivers at the LN lab to visitors. I took our family, including our daughters (ages 10 and 11) and some of their friends.

At the opening date of the 1939 Fair (390430) we had about eight receivers operating at the LN lab. In addition to some of our models, we had receivers made by GE, Stewart-Warner, Belmont and Stromberg-Carlson. The typical size of the picture tube was 7 or 9 or 12 inches. It was the time of transition from a vertical "long" tube viewed in a 45° mirror to a horizontal "short" tube viewed directly. The picture was so dim that the room had to be nearly dark for satisfactory viewing. The LN lab had just been occupied, so it was our showplace.

The only time I have ever spoken on a broadcast program was the sound broadcast of a short speech on the subject of television. It happened 391207 in Schenectady, N.Y. The General Electric Co. transmitted a serial called "The Science Forum". It was a half-hour program in which the feature was a ten-minute talk by an invited guest. It was broadcast twice. [A]

- At 4 PM on short-wave stations WGEA and WGEO to South America;
- At 7:30 PM on WGY (790 Kc) to a large area of U.S. (this hour being long after sunset).

I was the invited guest. My talk was a general statement on the remarkable features of moving-image transmission by the scanning process and on the future of its applications. It was heard from WGY by my family and some friends around Long Island, and by my father who was traveling on the B. & O. Capitol Limited train between Washington and Chicago.

As the invited speaker, I was a guest of honor of the General Electric Co. At the Mohawk Club I was the guest of the famous Dr. William D. Coolidge, who also hosted a lunch for me the next day. Among the lunch guests were Dr. Albert W. Hull (the screen-grid tube), Chester W. Rice (inventer of one form of "neutralization"), Harry B. Marvin, George F. Metcalf and Dr. Pollack (my personal escort). (Dr. Coolidge told me he wanted to measure the static charge on a moving belt. I suggested measuring the magnetic field of the current.) In the afternoon, I spoke to a large group of the staff of the Research Labs. I chose to present my recent concept of "speed of amplification" in a wideband amplifier. Dr. Hull, Dr. E. D. Cook and others joined in the discussion.

Toward the end of our pre-war TV program, we employed Bernard D. Loughlin, shortly after his graduation from Cooper Union. He had made a small TV receiver at home. He came to us with the ideas which developed into the Phase Curve Tracer, covered in a separate chapter. It was a major accomplishment which contributed to our standing in TV progress. His greatest contributions came later in the post-war development of color TV. [G] [H]

Near the end of the World's Fair 1939, Hitler invaded Poland to start World War II. TV in England was about a year more advanced than in U.S. because the Government had supported TV broadcasting by the BBC. The need for "radar" had been perceived, and it had been recognized as using much of the technology of TV. Therefore the TV engineers were assigned to radar. This gave England the headstart that was needed to have some form of radar ready for the German air offensive. Perhaps TV under Government sponsorship gave them the margin of advantage that was needed to win the "Battle of Britain" in the air. The implications of this event in history have much relevance to the importance of technological superiority and of Government sponsorship to this end.

Belatedly the U.S. Government organized various activies to expedite technological development. President Roosevelt was re-elected in 1940 to an unprecedented third term on his promise to keep us out of the war in Europe. However, we were committed to help England. In my personal diary, the first entry relating to war work was 400912, near the end of the World's Fair 1940. As a byproduct of our TV programs, Art Loughren had seen an opportunity for a miniature TV set in the nose of a bomb, to be used for guidance to a target on land or sea. He had made a small set and demonstrated this function at the LN lab on that date. About that time, the Government organized a group of selected scientists and engineers under the leadership of Dr. Vannevar Bush of MIT. It was first named the NDRC (National Defense Research Committee) and later the OSRD (Office of Scientific Research & Development). My first contact with that group was the visit of some members to our LN lab 401205. Among the visitors were Dr. L. O. Grondahl of Union Switch & Signal Co. and Dr. Julius Molnar of MIT. I think the principal topic was the TV bomb, on which we were shortly supported for further work and demonstrations. By 420817, our model had undergone a substantial development effort led by Loughren and had been tested at Eglin Field, Florida. It did not go into use during the war. More recently, that concept has been developed by others with Government support, and may now be a feature of some of our weapons.

**Standardization** is an early necessity in the utilization of a new service involving many parties. The standards for television in U.S. were generated, recommended and put in effect by these organizations:

- The RMA (Radio Manufacturers' Assn.) which first identified a Television Committee in 1929.
- The IRE (Institute of Radio Engineers) which identified under its Standards Committee a Technical Committee on Electrovisual Devices (including TV) around 1932. This became the Television and Facsimile Committee and finally (in 1939) the Television Committee.
- The NTSC (National Television System Committee) which formulated the standards for the TV service.
- The FCC (Federal Communications Commission) which adopted these standards.

Some members of our staff performed real service in this process, although RCA and GE were the leaders in terms of experience and current activities.

The first standards on TV were the few definitions composed by the IRE Technical Committee and published in the 1933 Report of the Standards Committee. The IRE did not directly contribute to the standardization of a system, but rather the IRE members participated in the RMA program. In 1940, we were represented by Lewis and Loughren on the IRE Television Committee.

The RMA Television Committee generated the standards which were finally adopted. Much study and testing was conducted by their member companies toward the objective of best utilizing one channel 6 Mc wide. Their preliminary report was the first article in the new periodical, "R.M.A. Engineer" (Nov. 1936). The result was their report released 380603. [N] It was complete and was changed later in only one detail (from 441 to 525 lines).

The NTSC was organized to satisfy the FCC that the engineering opinion of the industry was prepared to approve one of the competing television systems. [P] This was made necessary by just a few objections from companies other than RCA, who had also participated in generating the RMA standards. None of these objections survived the NTSC, but one change was made (441 to 525) which gave a slight improvement. This and the higher level of confidence and acceptance were worth the year's delay. The NTSC had about 15 members and an equal number of alternates, representing that number of organizations. One was IRE, for which I was alternate. Another was Hazeltine Service Corp., represented by Harnett with MacDonald as alternate. The deliberations were allocated to 9 panels, each including some committee members and some other experts in its assigned field. The most important was Panel 7, dealing with the picture reproduction by the scanning process. Its chairman was Harnett. It had 22 members, including Cawein and Loughren from our staff. Among its topics was the number of lines. The panel recommended 441 (then in use) but a review by the entire committee led to the change to 525. Our paper on the "fine structure" was one of the very few guides to either choice. The NTSC held its first meeting 400731 and its last 410308. The intervening months were filled with intensive activity by the most competent engineers in the field.

The FCC was satisfied with the report of NTSC and adopted that set of standards. As a result, the FCC permitted commercial television broadcasting from 410701. It functioned at a low level of activity until after the war.

As our TV program became more active, nearly all of our engineers became involved either full-time or part-time in that work. Their contributions included initiative, leadership, studies, concepts, test equipment, design of receivers, teaching our staff, advising our licensees, and writing for education. The names to be listed were especially active and may be grouped as follows:

(1) Senior members of our staff who brought experience to our TV program from its beginning;
(2) Experienced engineers who joined our staff with TV background or special interest;
(3) Other engineers who joined the TV program and were especially active in some phases.

A few names (*) are particularly notable because they carried over some expertise of our pre-war TV program to the post-war development of color TV.

| | | | | |
|---|---|---|---|---|
| (1) | | D. E. Harnett | | |
| | | H. A. Wheeler | | |
| | | H. M. Lewis | | |
| | | M. Cawein | | |
| | | J. F. Farrington | | |
| (2) | | R. C. Hergenrother | from Farnsworth | |
| | (*) | A. V. Loughren | from RCA and GE | |
| | | J. C. Wilson | from Baird and CBS | |
| | | L. R. Malling | from Baird | |
| | | J. A. Rado | from CBS | |
| (3) | | L. J. Troxler | (*) | C. E. Dean |
| | | B. F. Tyson | (*) | W. F. Bailey |
| | | W. B. Wilkens | (*) | B. D. Loughlin |
| | | H. L. Blaisdell | | |
| | | L. M. Hershey | | |
| | | R. J. Brunn | | |
| | | R. E. Sturm | | |
| | | R. L. Freeman | | |

The benefit to our company did not mature before TV progress was interrupted by war work. As compared with some other companies (especially GE) our war work benefited less from our TV expertise. After the war, however, came the intensive development of color TV. In that phase, RCA and our company became the prime movers in terms of innovation. Several members of our pre-war TV team continued to provide the nucleus of (monochrome) experience. This group and our established reputation were a valuable legacy from our pre-war TV program. It was a monument to the initiative of Lewis and Cawein. It was a credit to all of us who participated in our growth with TV.

[A] "Television", General Electric Science Forum, broadcast from Schenectady, 391207. (Hazeltine memo 800211, HAW-80-RL8158).

Twenty million people in this country now have television programs delivered to their doors from the broadcasting stations. The foremost of these stations is located on the Empire State Building and serves the population of New York City.

To these people television means that they now have available another avenue of communication between the outside world and their homes. It is something more than they have been getting from their home movies. It will bring them the sight and sound from a great variety of activities and that without any delay after the occurrence of the events. They will see headline news at the instant it is made. They will see the progress of a football game before the announcer has time to put it into words. They will have available a new form of dramatic entertainment which is comparable with the sound movies and which can be seen by everyone without delay.

When the King and Queen of England visited the World's Fair, I was not numbered among the crowd. I was sitting in the midst of a group of friends in the seclusion of a Long Island suburb watching the television receiver display the activites of those distinguished visitors. No one visiting the Fair could have seen them in their various activities as we did.

To the communication engineer, television is a triumph of high-speed operation. The progress of communication over the years has been measured by the rapidity with which signals could be transmitted from one point to another. To review the progress of communication is to summarize the history of increasing speed of operation.

Modern communications started with the Morse telegraph a century ago. This was the first use of electricity to carry messages. As we look back, we perceive that the electric currents did the best they could, but they were working in a crude harness. Electric currents were controlled by keys and the keys were controlled by hand. The speed of operation was only a few words per minute and was limited by the skill of the operator.

Half a century ago, the next major step was the advent of the Bell telephone. By transmitting speech directly it was possible to speed up the operation and to eliminate the crude devices of the telegraph. The telephone responds to sound waves. It transmits the personal inflections of the voice which could never be described by words over the telegraph system.

Fifteen years ago radio broadcasting grew up over night. By radio it became possible to transmit a hundred speeches through the same medium. Any one of these could be selected in the broadcast receiver. Therefore we were transmitting by broadcasting a hundred times as much as could have been transmitted over the medium of a telephone wire.

With the development of radio broadcasting came the beginnings of television. Looking back on the early demonstrations fifteen years ago, we are impressed with their crudeness as compared with our present technique. They used noisy and cumbersome mechanical devices rotating at a tremendous speed. They required special lighting, therefore outdoor events could not be picked up. In the short space of fifteen years television has been developed to the highest degree of refinement reached by any form of communication. We no longer use any moving mechanical parts. We use only the moving electrons to pick up the picture and to reproduce it in a reciver with all the details and motions of the objects in the picture.

You may have heard engineers say that a television picture tells the observer as much as could be told by a thousand persons speaking all at once. That is merely our way of saying that the speed of communication required in a television system is one thousand times as great as the speed required to reproduce the sounds of speech and music. We think of sound waves as occurring very rapidly because we cannot perceive, their individual vibrations. The electric currents which reproduce television pictures have to respond a thousand times more rapidly than the ear drum responds to sound waves.

In communication of all kinds the speed of operation is measured by the number of impulses which are transmitted in one second. It is the speed of these impulses which determines how rapidly the transmitting and receiving devices have to act. In the telegrph code transmission there were only a few clicks in one second. With the development of the telephone we were able to reproduce sound waves which require thousands of impulses per second. Even this rapid operation, which seems instantaneous to our senses of perception, is much too slow for television.

In television we break down the picture into many parallel lines and then build it up again line by line in the receiver. You get the same effect if you put a silver dollar on the table, covered by a sheet of paper, and trace lines across it with a pencil. The picture on the silver dollar is built up line by line on the paper.

This operation in television has to be performed so rapidly that an entire picture is taken apart and built up again in the twinkling of an eye. This requires that the lines be traced at extremely high speed. Such high speed is obtained by using not a pencil but a moving beam of electrons. This beam produces a single spot of the picture at the receiver but it moves over the entire picture almost instantly. Therefore the eye perceives not the spot which is produced by this electron beam but rather the entire picture traced by the spot.

In a television receiver we reproduce the motion by sending thirty complete pictures every second. This is exactly the same principle as used in motion pictures. In television, however, each picture is broken up into four hundred lines and each line is regarded as five hundred individual dots. The scanning spot must follow the instructions of the transmitter while it is making each one of these dots. There are 200,000 of these dots in a single picture which is reproduced in 1/30 of one second. Therefore the speed of operation is 6,000,000 dots per second, the number of dots in thirty pictures.

The extreme agility and precision of the electron beam which forms the television picture may be seen in a simple example. If the picture is six inches wide in the receiver, the bright spot moves across the picture at a speed of one mile per second. This is a speed of 3600 miles per hour or ten times the speed of a racing airplane. While the spot is moving at this tremendous speed, it is tracing accurately one hundred dots for every inch of motion. It has to supply five hundred dots while it moves across the picture just once. Like the distances of astronomy, this speed of operation is impossible to appreciate. The communication engineers visualize it only because they have watched it develop from the perceptible speed of telegraph code up to the imperceptible speed of sound transmission, and then further year by year.

When you are looking at your television receiver you will not have to worry about the severe demands you are making on the electrons. You will not have to worry about the years of patient effort by which the engineers have brought television to its present state of perfection. You will be witnessing a fitting climax to a century of electrical communication. It is a system which has no moving parts except the electrons. They have no choice but to follow the bidding of the transmitting station which is broadcasting the television signal. They will do their best to reproduce for you whatever you wish to see, whether it is your home team in the World's Series or the President's inaugural address.

[B] Hazeltine Reports on TV testing equipment.

| No. | Date | Author | Title |
|---|---|---|---|
| 374W | 330310 | HML, MC | Cathode ray oscillograph. |
| 429W | 330918 | HML, MC | Improved cathode ray oscillograph. |
| 472W | 340412 | MC | High frequency sensitive vacuum tube voltmeter. |
| 488W | 340606 | HML | Modulation generator for television test equipment. |
| 554W | 350423 | MC | Acorn tube V.T.V.M. |
| 652W | 360701 | LJT | Video frequency generator. |
| 653W | 360713 | AWB | Signal generator for 20-100 Mc. |
| 672W | 361209 | AWB | Television transmitter. |
| 673W | 361211 | WL | Video-frequency amplifier for 441-line transmitter. |
| 675W | 361217 | WL, MC | 343-line pedestal generator. |
| 676W | 361217 | WL | 441-line transmitter rack oscilloscope. |
| 680W | 361229 | WL | Direct vision camera preamplifier and booster amplifier. |
| 682W | 361229 | LJT | 441-line electrical timer. |
| 683W | 370122 | LJT | 441-line pedestal generator. |
| 687W | 370126 | MC | 441-line television transmitter rack monitor chassis. |
| 688W | 370126 | WL | Picture tube test equipment. |
| 697W | 370204 | RCH | Electron gun type DA for television camera tube. |
| 699W | 370224 | WL | Motion picture camera preamplifier. |
| 701W | 370329 | PJH | Phase meter. |
| 716W | 370506 | MC | Motion picture camera synchronizing control chassis |
| 717W | 370511 | MC | Motion picture tele-camera. |
| 733W | 370628 | MC, ETJ | Direct vision tele-camera. |
| 748W | 370820 | RCH | Specifications of television camera tube. |
| 794W | 380105 | RCH | Television camera tube — theory of operation. |

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|---|---|---|---|
| 802W | 380214 | PJH | Characteristics of camera tubes. |
| 812W | 380309 | LJT | Synchronizing signal generator. |
| 875W | 380720 | HML | New standard pedestal generator. |
| 933W | 381130 | LRM | Standard cross signal generator. |
| 937W | 381208 | LJT | 40-120 Mc signal generator. |
| 976W | 390411 | HLB | A method of testing scanning coils. |
| 996W | 390614 | JAR | Sweep generator. |
| 1157W | 401029 | BDL | A phase curve tracer for television. |
| 1573 | 370104 | HML | Special 441-line television oscilloscope. |
| 1843 | 380715 | AP | Camera preamplifier #79-N. |
| 1891 | 381205 | JAR | New 441-line television timer. |
| 1904 | 390113 | WFB | Monitor scanning circuits. |
| 1919 | 390221 | AVL,RWK | Television oscilloscope Mobel B. |
| 1925 | 390314 | RJB | Adjustable frequency television transmitter. |

[C] Hazeltine Reports on first television receiver.

| No. | Date | Author | Title |
|---|---|---|---|
| 266W | 320524 | HML, MC | Television receiver for the 40-80 Mc band. |
| 266AW | 320527 | HML, MC | Television receiver — AF system. |
| 266BW | 320729 | HML, MC | 37-70 Mc television receiver. |
| 293W | 320729 | HML, MC | 37-70 Mc television receiver for Philco Radio & Television Corp. |

[D] Hazeltine Reports on TV receivers by serial numbers.

| Ser. No. | Rep. No. | Date | Author | Picture Tube | (Viewing) |
|---|---|---|---|---|---|
| A1 | 656W . . . | 360824 . . . | JFF (VEW,JLH) | | |
| 4 | 738W | 370706 | MC | | |
| 5 | 725W . . . | 371111 . . . | MC (JLH,RJB) | | |
| 6 | 841W | 380425 | MC | | |
| 7 | 1808 | 380429 | AVL (MC) | 5-inch | |
| 8 | 863W | 380622 | MC | 12-inch | (indirect) |
| 9 | 1847 | 380727 | WBW | | |
| 10 | 861W . . . | 380609 | MC (BFT) | | |
| 11 | 893W | 380916 | BFT | | |
| 12 | 1897 | 381228 | AVL, WBW | | |
| 13 | 1955 | 390531 | WBW | 12-inch | (indirect) |
| 14 | 1922 | 390301 | LRM | | |
| 15 | 1930 | 390323 | LMH | | |

| | | | | | |
|---|---|---|---|---|---|
| 17 | 1049W | 391127 | JAR | 7-inch | (direct) |
| 18 | 1017W | 390909 | RJB (RLF) | 12-inch | (indirect) |
| 20 | 1086W | 400327 | JAH | 9-inch | (indirect) |
| 21 | 1079W | 400311 | LRM | 9-inch | (direct) |

[E] Hazeltine Reports on other TV topics.

| No. | Date | Author | Title |
|---|---|---|---|
| 331W | 321128 | HML | Television job numbers. |
| 332W | 321205 | HML, MC | Scanning chassis for cathode ray television. |
| 333W | 321129 | HML, MC | Philco-Farnsworth cathode-ray and scanning chassis. |
| 334W | 321130 | HML, MC | 30-70 Mc television receiver — diode detector. |
| 343W | 330320 | HML, MC | Television test at Philco. |
| 389W | 330331 | HML, MC | Saw-tooth current generators. |
| 396W | 330420 | HML, MC | Generation of the television modulated carrier. |
| 397W | 330421 | HML, MC | Status of television. |
| 471W | 340411 | HML | Television development program. |
| 720W | 370602 | HML | A synopsis of cathode ray television. |
| 752W | 370908 | HML | British television. |
| 804W | 380222 | JCW | Relaxation oscillators. |
| 1114W | 400622 | JCW | A target-viewing dirigible bomb. |
| 1115W | 400624 | AVL | Television equipment for military use. |
| 1185W | 410127 | LRM | Triode sawtooth current scanning generator with high voltage obtained from retrace pulse. |
| 1774 | 380215 | AVL | Television synchronizing signals. |
| 1824 | 380615 | WFB | Observations on RCA television transmission. |
| 1830 | 380621 | DEH | Television monitor pictures. |
| 1889 | 381203 | AVL | Synchronizing signal separation. |

[F] C. E. Dean, "Television Principles", Hazeltine Electronics Corp.; 1944.

| Chap. | Rep. | Date | Title |
|---|---|---|---|
| 1 | 1776 | 380217 | General aspects of modern television. |
| 2 | 1789 | 380330 | The mosaic camera tube. |
| 3 | 1822 | 380606 | Formation of complete video wave. |
| 4 | 1837 | 380630 | General requirements on the television receiver. |
| 5 | 1853 | 380816 | Frequency characteristics of the receiver. |
| 6 | 1877 | 381020 | Chief features of the television receiver. |

| | | | |
|---|---|---|---|
| 7 | 1894 | 381221 | Television pickup systems. |
| 8 | 1921 | 390228 | Alternative synchronizing and receiving methods. |
| 9 | 1924 | 390319 | The picture tube — part I. |
| 10 | 1932 | 390329 | The picture tube — part II. |
| 11 | 1945 | 390428 | Magnetic scanning coils. |
| 12 | 1997 | 391011 | Sawtooth-current amplifiers for magnetic scanning. |
| 13 | 2045 | 400227 | Sawtooth-current oscillators for line-frequency magnetic scanning. |
| Appendix | | | Bibliography of 1940-44 references. |

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[H] K. McIlwain, C. E. Dean (eds.), "Principles of Color Television", Hazeltine Labs. Staff, Wiley; 1956.

[I] H. M. Lewis, "Standards in television", Electronics, vol. 10, no. 7, pp. 10-13, 50-51; Jul. 1937. (Comparison of RMA proposed U.S. standards with British standards.)

[J] H. M. Lewis, A. V. Loughren, "Television in Great Britain", Electronics, vol. 10, no. 10, pp. 32-35, 60, 62; Oct. 1937.

[K] R. C. Hergenrother, "The television picture tube", Proc. Radio Club of Amer., vol. 17, no. 5, pp. 1-11; Jun. 1940.

[L] J. C. Wilson, "Television Engineering", Pitman; 1937.

[M] W. R. MacLaurin, "Invention and Innovation in the Radio Industry", MacMillan; 1949.

[N] A. F. Murray, "RMA completes television standards", Electronics, vol. 11, no. 7, 28; Jul. 1938.

[O] D. G. Fink, "Principles of Television Engineering", McGraw-Hill; 1940. (Rev. 1947)

[P] D. G. Fink (ed.), "Television Standards and Practice", McGraw-Hill; 1943. (N.T.S.C. report 410320.)

[Q] RCA, "Television", vol. 1; Jul. 1936. Vol. 2; Oct. 1937. Vol. 3 (1938-41); Dec. 1946. Vol. 4 (1942-46); Jan. 1947. (Collections of RCA papers.)

[R] H. A. Wheeler, J. C. Wilson, "The influence of filter shape-factor on single-sideband distortion", (summary), Proc. IRE, vol. 28, p. 253; May 1940. (IRE Annual Conv., Boston, 400629.)

[S] A. V. Loughren, "Frequency modulation in television; II — Interspersed F-M and A-M in a television signal", Electronics, vol. 13, no. 2, pp. 27-30; Feb. 1940.

[T] H. M. Lewis, "Wave form circuits for cathode ray tubes — Part II", Electronics, vol. 15, no. 8, pp. 48-53; Aug. 1942.

## 7.2 Television Images 1937.

The reproduction of an image by the scanning process is simple in concept. Any degree of fineness can be achieved by increasing the number of lines and scanning a small spot of light. However, the fast scanning for reproduction of a moving image requires some frequency bandwidth of a communication channel, which may not be available in the spectrum of radio frequencies. Television has proved to be a marginal case, so we have strived to utilize most effectively whatever bandwidth has been allocated. Conversely, we have aimed to request only the bandwidth required for the purpose. These were objective goals which we found difficult to quantify.

Around 1937, the essential relations had been studied and fairly well established by subjective visual testing. I set out to establish these relations by objective analysis. The result was our paper entitled, "the fine structure of television images". In preparing this analysis, I was greatly assisted by the perceptive reactions and suggestions of Art Loughren, my co-author. We were encouraged and stimulated by our associates, especially Harnett, Lewis, Wilson and Cawein. This paper became the principal theoretical basis for the TV standards finally adopted in 1941. It was supplemented by subjective observations which may have more closely represented the practical conditions.

The relation between speed of reproduction and the required frequency bandwidth is determined by a mathematical process called the "Fourier integral transform". It is simple in concept and familiar in application. If a very short impulse of current or voltage is applied to a circuit of limited frequency bandwidth, the response in time is widened. One may say that it is blurred by the sluggishness of the circuit which favors low frequencies and rejects high frequencies. The transform yields the relation that the effective widths in time and frequency ($2t_c$, $2f_c$) are inversely related:

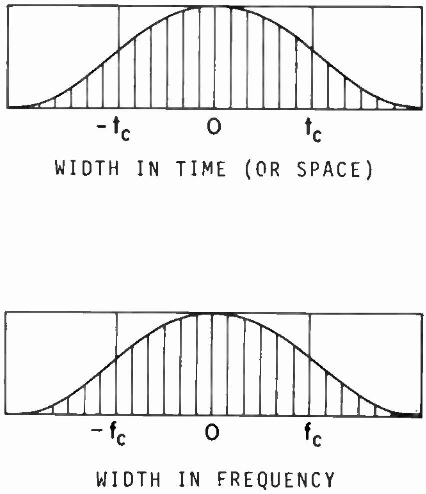
$$(2t_c)\ (2f_c) = 1$$

The center of each width is shown as zero relative to some reference;

- The time center is any time reference;
- The frequency center may be either
    - (i) zero for a VF (video frequency) low-pass circuit or
    - (ii) the carrier frequency for a selective bandpass circuit, either signal RF (radio frequency) or internal IF (intermediate frequency).

In the horizontal scanning process, the time width becomes the perceived width of reproduction of a thin vertical line. This is related to the width of a "picture element".

In Fig. 1, either width may be taken as an inverse measure of the other. For example, a specified width in time (and horizontal distance) inversely




$$(2t_c)(2f_c) = 1$$

7.2 Fig. 1 — The variation of voltage or light intensity along a horizontal scanning line.

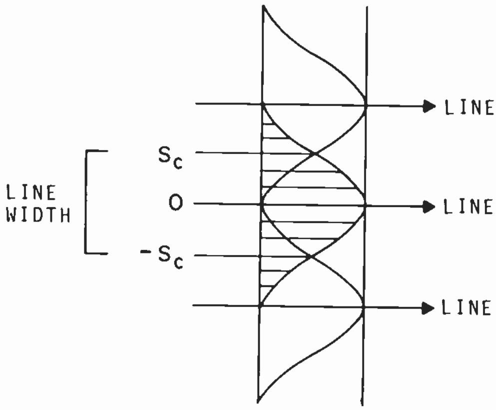
determines an equivalent frequency width. With respect to the perceived width, we are reminded that:

- The width of each scanning spot (in camera tube or picture tube) contributes to the overall "width";
- The restriction of frequency width in each amplifier (in transmitter or receiver) contributes to the overall "width".

In terms of either scale, all components can be "cascaded" and their overall effect can be computed by one of these rules:

- On the scale of time (or perceived width) the resultant of all widths can be approximated by their "quadratic sum",

$$w = \sqrt{w_1^2 + w_2^2 + \ldots}$$




7.2 Fig. 2 — The variation of light sensitivity or light intensity across horizontal scanning lines.

- On the scale of frequency, the resultant of all component response curves is simply their product, whose width is decreased by each component.

The time widths in scanning may be taken to include only the electronic components (from camera to picture tube), while regarding any blurring in the subject or the optical focusing system as a property of the subject to be imaged.

The foregoing factors are related directly to the horizontal scanning, in which the time width is perceived as the width of reproduction of a vertical fine line. It is effective alike on many horizontal lines. The horizontal lines have to meet other tests for reproduction of a flat field or a nearly horizontal line.

Fig. 2 shows the vertical distribution of the widths of adjacent horizontal lines of scanning. I took the simplest smooth curve that would overlap to give a uniform distribution, namely,

$$S = \cos^2 (\tfrac{\pi}{4} s/s_c) \qquad (-2s_c < s < 2s_c)$$

This "cosine-squared" distribution across each line can be generated by a circular scanning spot having a certain radial distribution.

- In the camera tube, this is the relative response to light intensity as measured by the variation of the generated output voltage.
- In the picture tube, this is the relative variation of light intensity in response to the applied voltage.

In either case, the lines combine to give a "flat field" of response.

The visual perception of a flat field is not sensitive to the line structure, so the overlapping in the picture tube is not critical. However, the reproduction of a fine line that is nearly horizontal is sensitive to the overlapping in the camera tube. If the overlapping is not designed on the basis of a flat field, the line appears to have periodic "beads". This is seen as the "moire effect" in a TV picture. If caused in the camera, it survives in the picture. Therefore I adopted the transverse distribution based on a flat field, as in Fig. 2.

Returning to the transform pair in Fig. 1, I found that the cosine-squared shape has a transform of very nearly the same shape. For practical purposes, the two could be used interchangeably. This fact suggested to me another similar shape whose transform has exactly the same shape:

$$T = \exp - (\tfrac{\pi}{4} t^2/t_c^2); \qquad F = \exp - (\tfrac{\pi}{4} f^2/f_c^2)$$

This is the bell-shaped probability curve (the socalled Gaussian function which is identified with the socalled Maxwellian distribution). For practical purposes, it is a useful approximation to the cosine-squared shape, and yields simple formulation for several relations:

- In the camera tube or the picture tube, a beam perfectly focused would have a circle of confusion caused by Maxwellian distribution. Scanning this beam would form a line with transverse distribution of the same shape.
- In the amplifiers, a response approaching this shape has a "minimum-phase" slope that is nearly linear, which is the condition for avoiding phase distortion. Minimum-phase is obtained in the simplest frequency-selective networks.
- The quadratic-sum rule, stated above, has its rigorous derivation based on this shape.

An essential contribution of our paper was my appreciation of the practical interchangeability of these two shapes, the cosine-squared and the probability. Either may be taken in Fig. 1 or 2 for present purposes.

The reproduction of a nearly horizontal narrow line is subject to widening in both camera and picture tubes. I noted that the widening along the line was between one and two times the width of one scanning line, and averaged about $\sqrt{2}$ times, which was an example of the quadratic-sum rule. This derivation was another contribution of our paper.

From these relations, we deduced a condition for equal "width of confusion" across vertical narrow lines and (on the average) across nearly horizontal lines:

$$f_c = f_p N_s^2 R/2\sqrt{2}$$

in which

$f_c$ = nominal cutoff frequency of the system
$f_p$ = picture frame frequency = 30 per second
$N_s$ = number of scanning lines
R = aspect ratio (horizontal/vertical) = 4/3

Examples: $N_s$ = 441; $f_c$ = 2.75 Mc
525 3.90

The utilization of a channel width is not susceptible of definite formulation because there is a practical compromise relating to margins beyond the nominal cutoff frequency. We presented a rationale for 441 lines being near optimum for a 6-Mc channel. That was recommended in the final report of NTSC. A further experimental study was made, which gave a slight advantage to 525, so that was finally recommended by NTSC and adopted by FCC in 1941.

The departure from our recommendation may have had a basis in practical departures from our model, such as:

- A camera-spot distribution with steeper sides (perhaps by defocusing rather than random dispersion) but still designed for a flat field;
- A picture-spot distribution with steeper sides and perhaps also too small for a flat distribution.

Either of these might well have been regarded as tolerable and even as a practical improvement. Our paper was found helpful because its theoretical approach could be compared with earlier and later studies based largely on subjective evaluation. My co-author, Art Loughren, was a member of the NTSC Panel 7 so our paper received due attention in their deliberations.

The principal presentation of our paper occurred 380406 at the monthly meeting of the New York Section of IRE. My talk was attended by about two hundred, an unusually large number for the smaller auditorium in the old Engineering Societies Bldg. on W. 41 St. Prof. Turner (Yale) presided. I spoke from slides and notes, having graduated from "reading a paper". Our paper was presented at several other section meetings by Art Loughren, Kelly Johnson or myself:

371116 Cincinatti
371117 Emporium, Pa.

371228 Indianapolis
380106 Conn. Valley
380217 Chicago

It was received by the profession with much interest. It was our debut in the literature of television.

[A] [W 20] with A. V. Loughren, "The fine structure of television images", Proc. IRE, vol. 26, pp. 540-575; May 1938. (Hazeltine Rep. 771W, 371105.) (Presented IRE, N.Y. Section, 380406.)

**7.3 Paired Echoes 1938.**

The frequency response in a wideband amplifier is subject to two kinds of distortion, which are distinguished as amplitude and phase distortion. These are interrelated and we did not know how to separately describe their effects. I developed a concept for this purpose, which I named "paired echoes". It was simple and came to be appreciated in both theory and practice. I published it promptly, and it is probably the one contribution for which I am most widely known today. [A] It is an essential teaching in any textbook on the transient response of frequency-selective networks. [D] [E] In television, it is related to the widening of a vertical thin line in an image formed by horizontal scanning.

The widening of a vertical line is partly attributable to the restriction of the frequency bandwidth in relation to the speed of the horizontal scanning. It is further attributable to any distortion in the frequency response within the allotted bandwidth. The unavoidable blurring of the line may be supplemented by an adjacent light or dark line, usually weaker. Such a result of frequency distortion is the subject of this chapter.

Our simplest encounter with frequency distortion was in the design of a video-frequency (VF) amplifier for about 0-4 Mc. Such an amplifier was used in the transmitter for the output from the camera tube and in the receiver for the input to the picture tube. This amplifier is taken for the present discussion, though the principles were applied also to a modulated-carrier amplifier.

The two kinds of distortion were distinguished as follows:

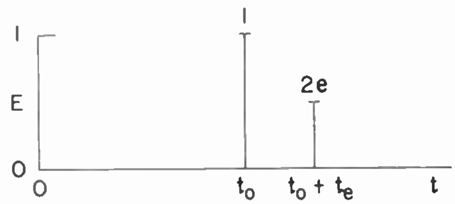
- Amplitude distortion was identified as a departure from the desired amplitude response, which had a gradual transition tapering from level in the passband down to near-zero outside this band. Any variation from this objective was known to cause symmetrical distortion of a pulse on the time scale, and of the perceived image of a vertical line.
- Phase distortion was identified as a departure from linear phase response over the passband. Linear phase was known to cause merely a delay in time, imperceptible to the viewer. Any variation from linearity was known to cause asymmetrical distortion.

There was a need for understanding and describing separately the effects of both kinds of distortion.

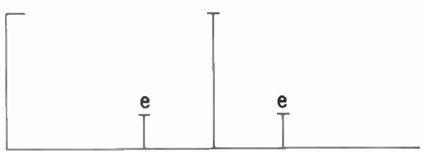
The latest textbook on the subject was Vol. II from Guillemin at MIT. [B] In relation to a delayed weak reflection in a transmission line, he described the association of amplitude and phase distortion of equal amount. Their measure was equal in terms of the compatible basic units.

- The napier for attenuation; and
- The radian for phase angle.

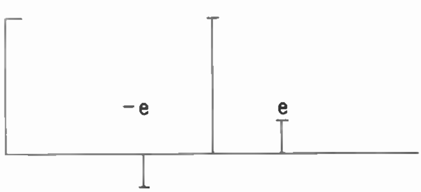
His presentation of one echo in these terms was the foundation on which I built.




(a) Single echo.

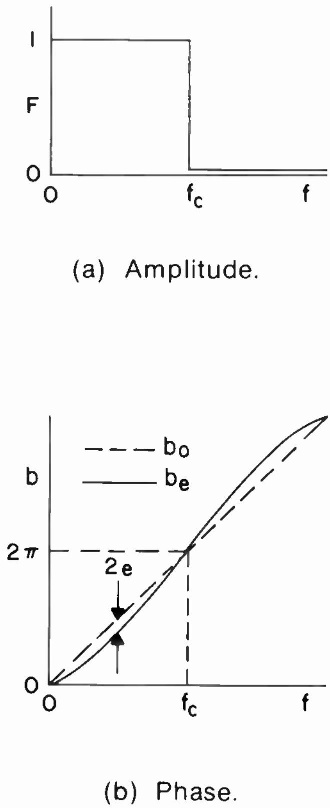



(b) Positive pair.

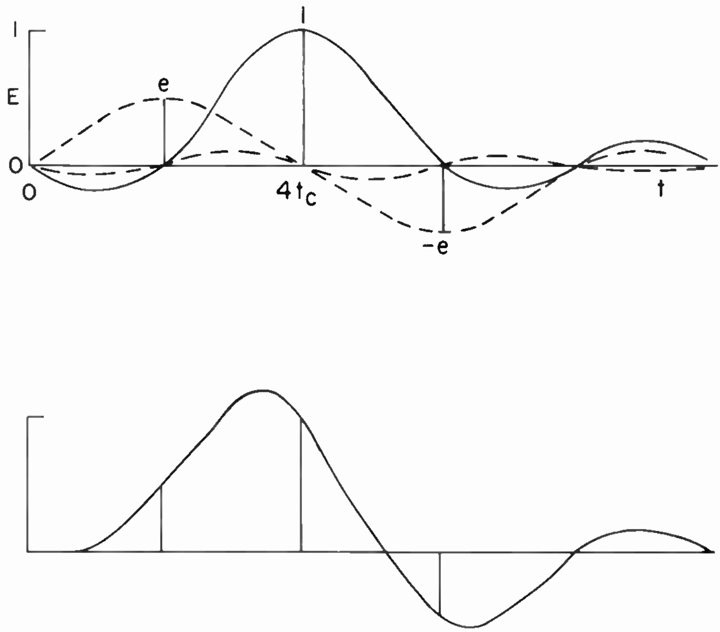



(c) Negative pair.

7.3 Fig. 1 — The amplitute and time relations between signal and echoes.




7.3 Fig. 2 — The frequency characteristics of an example.



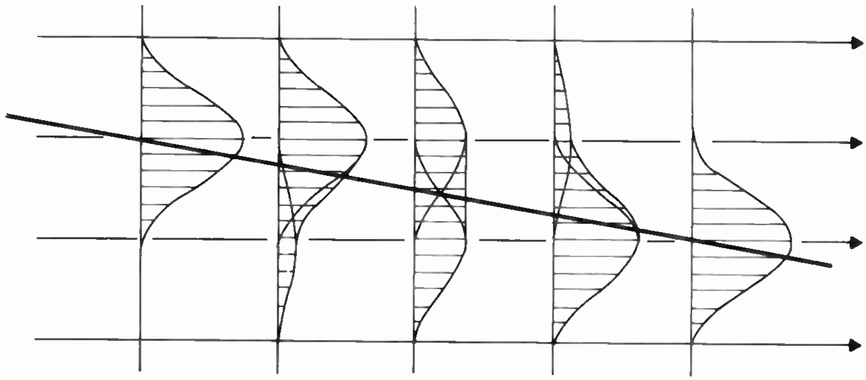

7.3 Fig. 3 — The application of paired echoes to evaluate the effects of the phase distortion.

I perceived that one echo could be analyzed as the sum of two pairs of equal value:

- One symmetrical "positive" pair; and
- One antisymmetrical "negative" pair.

The sum of these two pairs left a single echo as described by Guillemin.

Fig. 1 is a diagram of this concept, as introduced in my presentation. It was an elementary application of the idea of "symmetrical components", that was encountered in various situations common in electrical engineering (both power and communications). The central unit pulse in time is the signal that



7.3 Fig. 4 — The variation of the vertical width of the image of a nearly horizontal line.

would image a vertical line. The weaker pulses represent the echoes equally displaced either before or after the signal pulse. The single echo (2e) is the sum of the positive pair (e,e) and the negative pair (-e,e). The former is symmetrical so it represents amplitude distortion (by some test). The latter is antisymmetrical so it represents phase distortion (from linear phase).

The effect of amplitude distortion is not simple to define, because the essential bandwidth limitation of the frequency response is a form of amplitude distortion. Therefore the more interesting implications of paired echoes relate to the effect of phase distortion, as will be illustrated by an example.

Fig. 2(a) shows a hypothetical "square" amplitude response representing a limited bandwidth in frequency. Fig. 2(b) shows a sinusoidal phase ripple superposed on a linear phase slope ($b_o$) corresponding to a delay of $4t_c$. The amount of ripple is one radian ($2e = 1$). The increased slope around cutoff is typical of a frequency filter.

Fig. 3(a) shows the pulse response of Fig. 2(a) with the linear phase. The paired echoes corresponding to the phase ripple are shown in dashed lines. Fig. 3(b) is the sum, showing the opposite effects on the leading and trailing edges.

The amount of phase ripple is chosen to give little response before the rise of the leading edge. This would be typical of a "minimum-phase" network, as was usually used in a wideband amplifier. The result shows the overshoot on the trailing edge, likewise typical of such a network.

The echo amplitude (e = 0.5 radian) is rather large to be taken as an example of the theory based on small echoes. It is taken to show the principal effects of phase distortion and it is small enough to give a fair approximation.

The effect of phase distortion over the frequency range is naturally weighted by the amplitude response. In Fig. 2, the phase response beyond cutoff is irrelevant. Within the passband, the phase slope from zero is decreased by the ripple. In Fig. 3(b), the resulting pulse peak has less delay in time.

Tolerance of distortion is one consideration in the design of a wideband amplifier or the entire system of wideband networks. From the viewpoint of paired echoes, I was able to suggest that amplitude and phase distortion would be subject to equal tolerances if they were stated in terms of compatible (equal) units, such as the basic units which are the napier and the radian. It was customary to use the practical units, which consequently became related as follows:

$$(1 \text{ napier} = 8.686 \text{ db}) \quad = \quad (1 \text{ radian} = 57.30 \text{ deg})$$
$$\text{or } 1 \text{ db} = 6.60 \text{ deg}$$

This was a natural inference from Guillemin 1935. [B] So far as I have learned, I was the first to publish this helpful comparison. It was published again later by Bode 1945. [C]

In preparing the paper on paired echoes, I developed another remarkable concept related to the widening of a nearly horizontal thin line in its reproduction by scanning. Fig. 4 shows the concept of the transverse distribution of light intensity contributed by the two nearest scanning lines. Taking as a reference the cosine-squared distribution nearest the average distribution along the line, I regarded any departure from this shape as an error which had to be tolerated. From the considerations for a vertical line, I regarded the transverse distribution of the nearly horizontal line as equivalent to a pulse in time along the scanning line. Then I could express its departure from the average as equivalent to a variable distortion in frequency response. I regarded its amplitude and phase components as a measure of unavoidable distortion in the scanning process. There was no simple way to summarize the different shapes of distortion, but I did draw one conclusion:

- The quadratic sum of the weighted amplitude and phase distortion had a maximum value of about 0.3 (napier, radian).

I inferred that the same value would be a consistent specification for the weighted tolerance in the overall frequency response of the wideband networks. It could be stated as ±1.8 db and ±12 deg. These were a fair objective but probably have not been realized.

The concept of "paired echoes" has been applied to an "aperture" in any domain:

- In TV scanning, it appears in the inverse domains of frequency bandwidth and time (image) pulse width.
- In the radiation from a narrow-beam antenna, it appears in the inverse domains of aperture width and beamwidth (especially sidelobes).

In frequency-selective networks, the concept of "paired echoes" has offered the simplicity of equating some effects of small amounts of frequency distortion of either kinds, stated in compatible units.

I presented this paper 381115 at the Rochester Fall Meeting of IRE-RMA. [A] From all the papers at that meeting, it was selected for the next meeting of the New York Section 381207.

[A] [W 23] "The interpretation of amplitude and phase distortion in terms of paired echoes", Proc. IRE, vol. 27, pp. 359-385; Jun. 1939. (Hazeltine Rep. 829W, 380326.)

[B] E. A. Guillemin, "Communication Networks, Vol. II", Wiley; 1935. (Echo effects, p. 498.) (The natural loss unit, napier, p. 76. This is the original, authentic practice, so I use it in preference to the later, corrupted practice, neper. Compare the adjective, napierian.)

[C] H. W. Bode, "Network Analysis and Feedback Amplifier Design", Van Nostrand; 1945.

[D] BTL, "Transmission Systems for Communications", 4 ed., BTL; 1970. (Paired echoes, p. 721.)

[E] R. E. Kalman, N. DeClaris (eds.), "Aspects of Network and System Theory", Holt; 1971. (E. J. Baghdady, "Models for signal-distorting media", paired echoes, p. 343.)

## 7.4 Wideband Amplifiers 1937.

Television required amplifiers having a frequency bandwidth much greater than the bandwidth previously needed for any other service. I had been introduced to the problem in the earlier days when we wanted an untuned amplifier to cover the broadcast band. That bandwidth was 1 Mc. In TV we soon demanded 4 Mc for the picture signal, 6 Mc for one channel, and more for test equipment. This was a challenge to me, and I devoted much effort to establishing the fundamental limitations and relations, and then to devising practical designs. My contributions were mainly to the former, which could then be applied to the latter by our engineering staff. The former is the principal subject of this chapter.

The problem of a wideband amplifier came from the shunt capacitance in an amplifier tube. The essential part was internal and more was added in the mounting and leads. Its impedance was inversely proportional to frequency. As a result, the interstage coupling impedance in an amplifier was limited by the frequency bandwidth over which uniform gain was required. Because gain required coupling impedance, gain from one tube could be obtained only over a limited bandwidth.

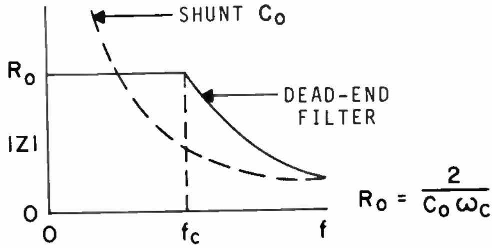
By this time, internal feedback had been removed by the "screen-grid", so there was no need for neutralization if a screen-grid tube could be used. In later applications for much higher frequencies, the triode was used in the "grounded-grid" connection, so its grid served as a screen grid.

Wideband amplifiers were required in a TV system for any of these functions:

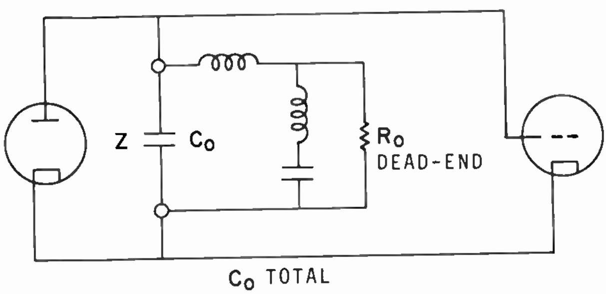
- A VF (video-frequency) amplifier from the camera tube to the transmitter modulator.
- An IF (intermediate-frequency) amplifier from the modulator to the frequency converter.
- An RF (radio-frequency) power amplifier from the converter to the antenna.
- An RF preamplifier from the receiver antenna to the converter.
- An IF amplifier from the converter to the detector.
- A VF amplifier from the detector to the picture tube.

They were also required for test equipment, especially the cathode-ray oscilloscope.

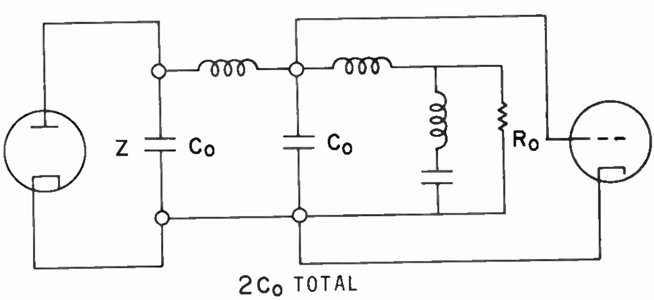
By the time I concentrated on the problem of wideband amplifiers (1937) it had received much attention in the TV groups active in England and in other U.S. companies, mainly RCA. Some circuits had been devised from a step-by-step progression toward improvements. This progress was slow to become known, so I had only a sampling of that knowledge. From what I have since learned, there was no statement of the ultimate limitations and how closely




(a) The impedance of a dead-end filter.

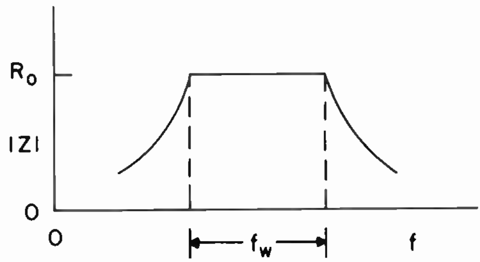



(b) Two-terminal self-impedance.

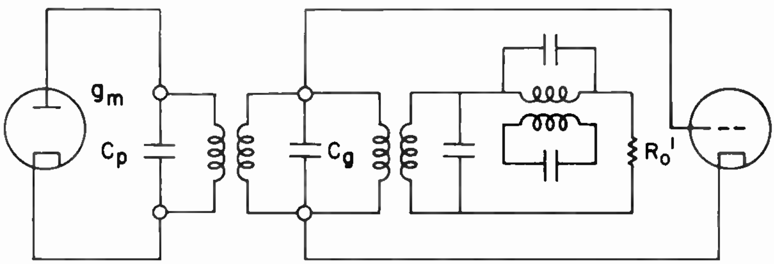



(c) Four-terminal transfer impedance

7.4 Fig. 1 — A lowpass interstage coupling impedance obtained by means of a dead-end filter.



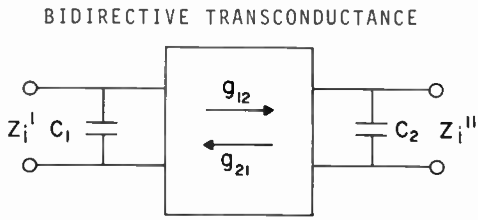

(a) The impedance of a dead-end filter.




$$A = \frac{2g_m}{\omega_w \sqrt{C_p C_g}}$$

(b) Four-terminal transfer impedance

7.4 Fig. 2 — A bandpass coupling impedance.




CUTOFF FREQUENCY: $\omega_c = \sqrt{\dfrac{-g_{12}\,g_{21}}{C_1\,C_2}}$

POWER RATIO: $A^2 = \left|\dfrac{g_{12}}{g_{21}}\right|$

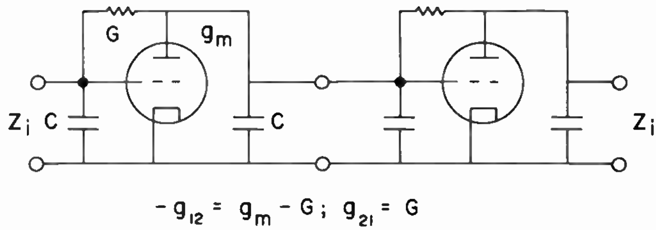
7.4 Fig. 3 — Feedback amplifier in a low-pass filter section.

they might be approached in practice. I perceived this opportunity and developed some basic concepts that were needed.

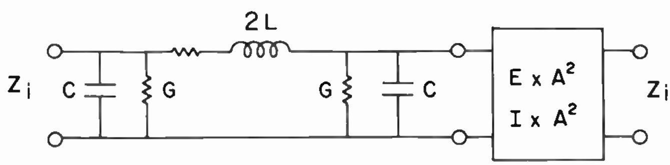
This account of my developments is directed mainly to these topics:

- The "dead-end filter" for making a uniform impedance across the shunt capacitance of an amplifier tube, over the greatest possible bandwidth.
- The "feedback filter" for integrating amplifier tubes into a wideband circuit.
- A feedback filter with only shunt C to make the equivalent of a low-pass filter with shunt C and series L.
- Various combinations of parallel and cascade connections in a relation to get the most gain and bandwidth from a number of amplifier tubes.
- The concept of "speed of amplification" as the real limitation of an amplifier tube with shunt capacitance.
- Various IF amplifier circuits using a bandpass filter for the picture signal, including traps to remove adjacent sound signals.
- An RF preselector for switching TV channels with equal bandwidths.
- Various forms of an excess-phase filter to compensate for phase distortion from a minimum-phase filter.

Only the first two were included in my first published paper. [A]



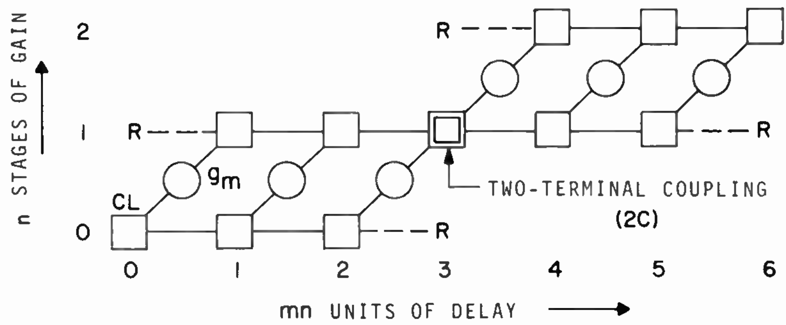

(a) Feedback amplifier.




(b) Equivalent filter.

7.4 Fig. 4 — Two-stage lowpass feedback amplifier and the equivalent constant-K filter section.

**The dead-end filter** was a simple concept for developing across shunt capacitance, an impedance that was constant over some bandwidth. It was derived from the wellknown constant-K wave filter of Campbell. The image impedance at either end of a section (or half-section) was defined for a mid-shunt or mid-series termination. In the passband, it was pure resistance varying with frequency. I perceived that a full-shunt or full-series termination would present (over the passband) an impedance of constant magnitude (and varying phase angle). I drew a reasonable inference that this gave the greatest constant impedance theoretically obtainable over a specified bandwidth, or vice versa. That inference came to be accepted as a valid theorem.




mn TUBES = m PER STAGE x n STAGES

MAXIMUM LEVEL AMPLIFICATION: $$a = \frac{mn\, g_m}{2\epsilon\pi f_w C} \quad \text{(NAPIERS)}$$

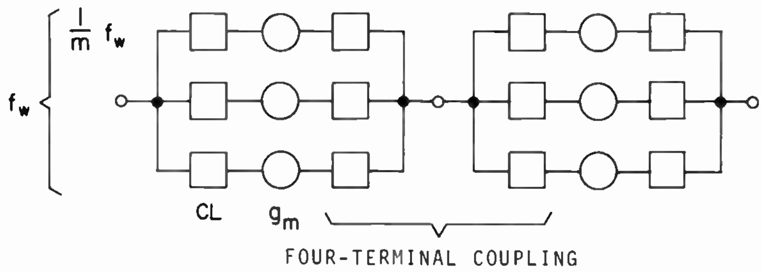
MAXIMUM SPEED OF AMPLIFICATION: $$\omega_n = \frac{g_m}{\epsilon\pi C} \quad \text{(NAP/SEC)}$$

7.4 Fig. 5 — The division of tubes in parallel and cascade by distributing them along dead-end filters.

The approximate realization of the uniform impedance over a bandwidth required some extension of the wave filter to a "matched" (nonreflecting) termination. Then the reflections inherent in the filter served to approximate the uniform impedance. I adopted the term, "dead-end filter", for the concept and the circuit.

Fig. 1 shows a simple example of the dead-end filter. It is a low-pass filter, made of shunt C and series L. A full-shunt arm is $C_0$. In Fig. 1(a), the dashed curve shows the impedance (pure reactance) of $C_0$ over a frequency range. The solid curve shows the impedance (Z) of constant magnitude, that theoretically can be developed across $C_0$ by a dead-end filter of some cutoff frequency ($f_c$). The dead-end filter serves the function of leveling the impedance over its passband.

Fig. 1(b) shows the simplest application to provide the interstage coupling impedance in an amplifier. It is a two-terminal self impedance. The dead-end filter is seen to comprise one section, made of a constant-K half-section




mn TUBES = m PER STAGE x n STAGES

MAXIMUM LEVEL AMPLIFICATION: $a = \dfrac{mn\, g_m}{\epsilon \pi f_w C}$ (NAP)

MAXIMUM SPEED OF AMPLIFICATION: $\omega_n = \dfrac{g_m}{\epsilon \pi C}$ (NAP/SEC)

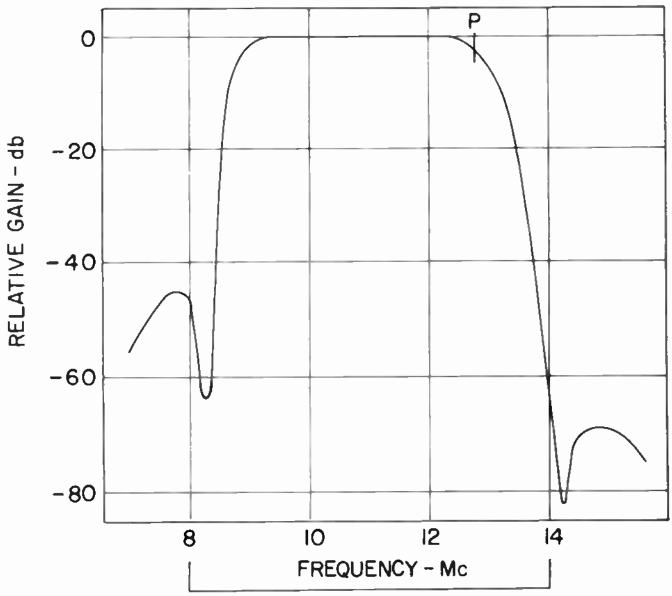
7.4 Fig. 6 — The division of tubes in parallel and cascade feedback filters dividing the frequency band.

followed by a socalled "M-derived" half-section of Zobel. The latter is designed to provide an image termination with closer approximation to the terminal resistance $R_o$. The resulting Z is nearly uniform over nearly all of the passband, a close approximation to the graph (a). The shunt $C_o$ is the total capacitance of preceding and succeeding tubes.

Fig. 1(c) shows the separation by an added constant-K filter section, so that each $C_o$ is one-half as great, from either one of the tubes. The resulting four-terminal transfer impedance is equal to the full-shunt end impedance, with the addition of lagging phase angle. The separation doubles the impedance.

Fig. 2 shows an example of the application to a bandpass filter. Fig. 2(a) shows the constant Z over a bandwidth $f_w$ (instead of $f_c$). The bandpass filter offers the additional freedom of a transformer ratio so the two full-shunt arms ($C_p$, $C_g$) need not be equal. The result is the greatest amplification (voltage ratio, A) obtainable over this bandwidth by a tube of transconductance $g_m$. It is formulated on the figure.

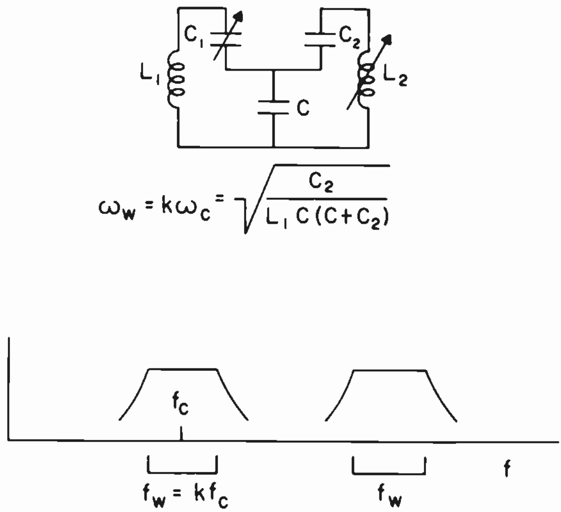
**The feedback filter** was a simple concept for including an amplifier tube in a wave-filter section. It enabled any number of stages to be served by a single dead-end filter, thereby minimizing the number of added circuit elements.




7.4 Fig. 7 — Picture IF wideband-amplifier response with sound IF traps.

Fig. 3 shows this concept for a low-pass filter. Between two mid-shunt arms ($C_1$, $C_2$) a feedback amplifier is connected. It is represented by "bidirective transconductance", made of forward $g_{12}$ and backward $g_{21}$. The result is a wave-filter section between image impedances ($Z_i$). The formula for its cutoff frequency ($\omega_c = 2\pi f_c$) has a real solution if the forward and backward are opposite in sign. The formula for its power ratio ($A^2$) gives a gain if the forward exceeds the backward. This "feedback filter" has the cutoff property of a filter and the gain of an amplifier.

There is no network of passive elements that has the same filter properties as the feedback filter in Fig. 3, because the bidirective coupling has opposite polarity forward and backward. A pair of stages removes this property. Fig. 4(a) shows a two-stage low-pass feedback filter that does have an equivalent filter section as shown in (b). Fig. 4(a) illustrates one way of making the bidirective coupling. The forward coupling is mainly the tube's mutual conductance ($g_m$), which is a negative transconductance. The backward coupling is a much smaller passive conductance (G). The latter, being reciprocal, also partially cancels the $g_m$, as indicated.




7.4 Fig. 8 — Preselector with channel switching and equal bandwidths

The equivalent network in Fig. 4(b) has the filter properties of one section of constant-K low-pass wave filter, as shown. Each feedback stage acts as an impedance inverter, so the intermediate full-shunt 2C becomes full-series 2L. The feedback (small) G is represented by (small) dissipation in the equivalent filter section. (This exemplifies the simulation of L in an active network containing only C.) The amplifying action is represented by a hypothetical box which multiplies both voltage and current by $A^2$ and does not affect the impedance. This equivalent network was described in Report 612AW and later was published in Wheeler Monographs, No. 5. [C] [M]

The box in Fig. 4(b) was a concept that was introduced a decade later as one of several unique components in network theory. Its matrix was then described and it was given various names. When I first found a need for it and defined it, it was a bold step because it cannot be realized as an isolated component.

Fig. 3, taken from my early paper, was probably the first publication of a wave filter including inversion by feedback. [A] The equivalent network in Fig. 4(b) exemplifies the simulation of L in an active network containing only C. This is now common in transistor applications.

I first devised and used the feedback stage (with bidirective transconductance) for AXPS, as mentioned in that chapter. [C] It was applied to a narrow-band selector. Its function was to enable automatic control of bandwidth, as well as gain. Later I came to appreciate its significance for a wideband amplifier behaving as a wave filter.

**Parallel and cascade connections** of amplifier tubes offer remarkable opportunities for a wideband amplifier. To the extent that a network is adequately described by transconductance and shunt capacitance, it is theoretically possible to obtain any amount of gain over any bandwidth by using a sufficient number of tubes. In other words, the properties of one tube do not place a limitation on the obtainable gain and bandwidth.

It is an essential that each stage yield some gain over the entire bandwidth. This is possible if a sufficient number of tubes are connected in parallel in each stage, according to one of these rules:

- In a "distributed amplifier", the tubes of one stage are connected between corresponding sections of an input wave filter and an output wave filter, each designed for the entire bandwidth.
- In a "divided-band amplifier", the tubes of one stage are connected in successive sub-band amplifiers, all of which are connected in parallel for the entire bandwidth.

The former had been devised by Percival and the latter had been approached in elementary forms by various other workers before I developed a refined theory of their common properties and theoretical limitations. This I finally published later in Wheeler Monographs, No. 11 (1949 July). [N] The above two rules are exemplified in Figs. 5 and 6.

**A distributed amplifier** is shown in each of two stages in Fig. 5. Each square represents a full-shunt arm of a wave filter. Each stage has an input dead-end filter and an output dead-end filter. Each filter has a dead-end termination R. Each circle represents one of the tubes in the distributed-amplifier stage.

By distributing the tubes along a filter, the coupling impedance remains the same for any number of tubes. Also the total phase lag is the same through each tube. Therefore the voltage ratio of amplification (A) is multiplied by the number of tubes in each stage (m). For any bandwidth, some gain is possible by using a sufficient number of tubes in each stage.

The distributed amplifier is useful for either low-pass or band-pass filter, because every shunt arm is the same.

Fig. 5 shows a two-terminal interstage connection. The output deadend filter of the preceding stage is connected directly in parallel with that of the succeeding stage, so the two level impedances are joined. The double impedance of a four-terminal interstage coupling is not available for a distributed amplifier.

A distributed amplifier stage is convenient for a low-pass amplifier, because every tube is connected the same and handles the entire bandwidth without transformers. However, a low-pass amplifier has its phase slope doubled by intervening series inductors. This will be seen to be relevant to speed of amplification.

**A divided-band amplifier** is shown in each of two stages in Fig. 6. The entire bandwidth ($f_w$) is divided into m sub-bands (each $\frac{1}{m} f_w$). These are amplified in m channels which are separated and recombined in each stage. The simplest form has a feedback filter section for each sub-band in each stage, as in Fig. 6. The entire amplifier forms one wave filter which is continued at one end to a dead-end termination (not shown). Each square represents one full-shunt arm in the filter for one sub-band. Each circle represents one tube with its associated feedback.

Using a sufficient number of tubes in one stage, some gain over any bandwidth is obtainable by reducing the sub-band width for each tube.

The separation and recombination follows the same principle as the divided-band transformer, which is described in the chapter on Antennas and Lines.

The entire bandwidth may be low-pass or band-pass. The lowest sub-band is low-pass or band-pass, but every other is band-pass.

As an alternative to feedback, the divided-band amplifier can be made with a dead-end filter completed in each interstage coupling. This is much more complicated. It is somewhat superior in performance, because it avoids the loss incidental to the feedback. It uses divided-band transformers.

Any number of tubes can be utilized in any one of the available patterns of parallel and cascade connections. Over a prescribed bandwidth, some allocation between parallel and cascade will yield the greatest gain, as will be described. The distributed amplifier in Fig. 5 has a two-terminal coupling impedance while that in Fig. 6 has a four-terminal. Therefore the latter gives the greatest gain possible over a bandwidth with a number of tubes. It is formulated in terms of the overall gain in napiers (a) for mn tubes, each having $g_m$ and shunt C alike on plate and grid. (The detrimental effects of feedback conductance G would decrease the effective value of $g_m$ and would add some loss.)

**The maximum gain** from a large number of tubes (mn) is obtained by allocation to some number of stages (n) with some number in parallel in each stage (m). This result requires that the number in each stage (m) be just sufficient to give a gain of one napier ($A = \epsilon$ ). Then the overall gain is n napiers ($A^n = \exp n$). Conversely, the number of stages is equal to the gain in napiers ($n = \ln A^n$). I soon arrived at this relation. It appeared that any one type of tube, used in a sufficient number, could yield any amount of gain over any bandwidth. A superior rating of the tube type would decrease the number of tubes required, and likewise the number of sections of envelope delay.

**The speed of amplification** was the concept I named to describe the quality that was determined by the properties of any one type of tube, and could not be increased by the number of tubes. I defined it in terms of the average envelope delay or phase slope over the nominal passband of a constant-K wave filter:

$$\text{Speed of amplification (napiers/second)} = \frac{\text{gain (napiers)}}{\text{envelope delay (seconds)}}$$

This depends not only on the tube type but also on some peculiarities of the circuit used. It is formulated in Figs. 5 and 6, the same for many tubes in a bandpass amplifier in either of these arrangements. This is true in spite of the latter giving double the gain (and double the delay) by virtue of "four-terminal" interstage coupling.

A low-pass distributed amplifier gives only one-half the speed, because it requires an extra inductor in series between the shunt arms. In a divided-band amplifier, only the lowest sub-band is subject to this handicap.

The concept of speed of amplification was related to stability criteria governing the amount of stabilizing feedback that could be used in an amplifier or "servo" system. In that context, the gain of one napier per stage was described in different terms. [G] [K]

**Selective IF amplifier circuits** were needed in a receiver to perform the dual function of amplification and frequency selection. The picture IF amplifier had to cover a wide band (say 4 Mc) while rejecting the nearby sound signals of the same channel and the adjacent channel. Fig. 7 shows this function. For practical reasons, the full realization of wideband gain was not affordable or necessary. However, we did devote much effort to selecting bandpass filter circuits which gave the most wideband gain that was consistent with simplicity and economy. [D] They will not be described in detail. The typical interstage coupling filter was double or triple tuned and contained a resonant trap on one or both sides. Much ingenuity was directed to the design and trimming of the circuit elements to achieve the objective of any one interstage circuit. Trimming some inductors, rather than capacitors, was preferable for greatest bandwidth. The powdered-iron cores of that day were adequate for this purpose, so they offered an alternative to capacitor trimming.

**An RF preselector** was required in a receiver, between the antenna and the first tube (preamplifier or converter). It was required to select any one of several TV channels, 6 Mc wide, including both picture and sound signal. We designed a double-tuned preselector that could be switched to any one of the seven channels then in use. They ranged from 50-56 to 102-108 Mc. Elementary coupled circuits did not offer the uniform bandwidth of 6 Mc that we desired.

I devised an unusual selector that inherently yielded a uniform bandwidth. [B] [E] [H] Its essentials are shown in Fig. 8. The primary circuit was tuned by its capacitor and secondary by its inductor. The coupling C was fixed. The primary fixed inductor ($L_1$) was available for coupling with a transmission line from the antenna. The secondary fixed capacitor ($C_2$) was the input capacitance of the first tube, for wideband response.

This was one of my favorite inventions. It was carried to a high degree of refinement. For example, the variable dissipation in the first tube (caused by transit time) was cancelled by feedback from an inductor in the screen lead. This selector was superseded by other types after the war.

**Phase correction** was needed to reduce the distortion of a pulse in time going through a wideband amplifier. This distortion was antisymmetric in time, as described by a negative pair of echoes. The distortion appeared in the widening of a thin vertical line in horizontal scanning.

For example, a video-frequency (VF) amplifier made of minimum-phase (ladder) coupling networks had phase slope increasing with frequency to a maximum near cutoff. An excess-phase section could be inserted to increase the slope more at lower frequency, so the resultant would be more nearly linear. Such a "phase-correcting" section was usually made with a negative circuit element, which had to be mutual inductance. This kind of section was termed M-derived with excess phase ($m > 1$). For a low-pass filter, it was familiar.

An IF bandpass amplifier, for a modulated-carrier picture signal, needed similar phase correction around its midfrequency . Such a section was not familiar, so I devised a network for the purpose. [B] (6) I have not seen it published anywhere outside my patent. It was complicated and probably has not been used.

I do not recall any practical use of my phase-correcting networks, nor do I know of any today. Their need has probably been reduced by using sufficiently gradual cutoff outside the passband.

**Comments**. In retrospect, my work on wideband amplifiers was largely conceptual. It contributed much to our understanding of the limitations, but the indicated refinements have been afforded only in moderation.

My work was original in the sense that I did it before some earlier work was available to me, as through publications. I like to appreciate what was developed earlier by some others. Their stature was such that I derived satisfaction from independently arriving at similar concepts. I wish to give credit which I now think was due to two prominent inventors, one in England (EMI) and the other in US (RCA). References to their patents appear in my papers. [A] [M] [N]

Dr. William S. Percival (EMI) was about my age and had a headstart in TV. He later published an abstract of his early work, but with little historical detail. [O] It appears that he devised the distributed type of extra-wideband amplifier in 1935. He used a dead-end filter with a full-shunt arm to take the capacitance of each tube. It had an M-derived matched termination. It was "published" 370125 in his British patent which was first to issue. I first saw it shortly before 380208, just when I was already preparing reports on these concepts. He was 2 or 3 years ahead.

Dr. Walter van B. Roberts (RCA) was 10 years older than I and well prepared at Princeton U. We were independently working on several of the same problems. Where we arrived at the same solution, he was usually ahead in time. He had the concept of a dead-end filter as early as 1929, but apparently not the concept of constant impedance across a full-shunt input connection. He may have been ahead of me with the concept of feedback filters in cascaded stages of a wideband amplifier. His work was published much later, after delays in the U.S. Patent Office, so I did not see his feedback filter until long after all my concepts had matured. He did not publish in the current literature.

I mention these parallel workers because the correlation of our work and its timing is a credit to all of us as leaders and a measure of its current significance in the early growth of TV technology. My U.S. patent claims contain some restrictions reflecting their priority in some respects. Also my allowed claims are a measure of my priority in other respects.

[A] [W 24] "Wide-band amplifiers for television", Proc. IRE, vol. 27, pp. 429-438; Jul. 1939. (IRE Convention NY 380618.)

[B] H. A. Wheeler, U.S. Patents

| | Pat. No. | Issued / Filed |
|---|---|---|
| (1) | 2153857 | 390411 / 380518 |
| (2) | 2167134 | 390725 / 380422 |
| (3) | 2167135 | 390725 / 380422 |
| (4) | 2167136 | 390725 / 380422 |
| (5) | 2167137 | 390725 / 380422 |
| (6) | 2177761 | 391031 / 380915 |
| (7) | 2181499 | 391128 / 371110 |
| (8) | 2185389 | 400102 / 370729 |

| | | | |
|---|---|---|---|
| (9) | 2196881 | 400409 / 381029 | |
| (10) | 2205738 | 400625 / 380816 | |
| (11) | 2247538 | 410701 / 400111 | |
| (12) | 2247898 | 410701 / 390929 | |
| (13) | 2609460 | 520902 / 460622 | |

Notes (1) (6) (7) Phase correction.
(2) (3) (4) (5) (11) Dead-end filter.
(8) Feedback filter.
(9) (13) Channel selector.
(10) Distributed modulator.
(12) IF selector with traps.

[C] H. A. Wheeler, Reports.

| No. | Date | Title |
|---|---|---|
| 612AW | 360203 | Directive coupling of tuned circuits. |
| 761W | 370928 | Feedback amplifier filters. |
| 795W | 380118 | Wide-band coupling impedance — theoretical limitations. |
| 803W | 380218 | Wide-band amplifiers — theoretical limitations. |
| 809W | 380304 | Feedback amplifiers and equivalent networks for use in filter design. |
| 859W | 380608 | Wide-band amplifiers for television. |
| 1014W | 390825 | Divided-band feedback-amplifier filters. |
| 1030W | 391011 | Filters for differentiation and integration. |

[D] Reports relating to IF selectors with traps.

| No. | Date | Author | Title |
|---|---|---|---|
| 915W | 381028 | ETJ | Wide-band i-f transformers with traps. |
| 984W | 390508 | BFT | Television i-f filter with low-frequency trap. |
| 1002W | 390712 | BFT | Television i-f filter with high-frequency trap. |
| 1010W | 390810 | HAW, KW | The design of i-f trap filters by half-sections. |
| 1015W | 390908 | BFT | Inductively tuned television i-f filter with low-frequency trap. |
| 1018W | 390911 | BFT | Inductively tuned television i-f filter with high-frequency trap. |
| 1028W | 391005 | BFT | Television i-f filter with high and low frequency traps. |
| 1117W | 400626 | BFT | Selector circuits for television receivers. |
| 1147W | 401002 | LRM | Economical television i-f amplifier using i-f filters. |

| | | | |
|---|---|---|---|
| 174Y | 390922 | JKJ | Television picture intermediate frequency permeability tuned interstage coupling circuit. |
| 181Y | 391030 | JKJ | Television picture intermediate frequency interstage coupling circuit. |

[E] Reports relating to RF preselector.

| No. | Date | Author | Title |
|---|---|---|---|
| 904AW | 381105 | HAW | Preselector with uniform bandwidth. |
| 944W | 381229 | BFT | A uniform band-width tuner for television receivers. |

[F] H. A. W., "The properties of directive mutual reactance obtained by the use of vacuum tubes", (program) Elec. Engg. vol. 56, pp. 383-384; Mar. 1937. (AIEE NY, Networks Conference, E. L. Bowles.)

[G] H. W. Bode, "Relations between attenuation and phase in feedback amplifier design", BSTJ, vol. 19, pp. 421-454; Jul. 1940. (A concept of one napier per stage, pp. 444-446.)

[H] B. F. Tyson, "A preselector circuit for television receivers", Electronics, vol. 13, no. 11, pp. 23-25; Nov. 1940. (IRE NY Section 400501.) (Switching channels with equal bandwidths.)

[I] H. A. W., "Equivalent networks for the three kinds of triode circuits", (summary) Proc. IRE, vol. 33, pp. 59-60; Jan. 1945. (Presented IRE Annual Conv., N.Y., Jan. 25, 1945.) (Repeater-transformer.)

[J] H. A. W., "Wideband amplifiers", joint meeting of IRE and AIEE Sections, N.Y., Apr. 19, 1945. (Two-hour talk, paired echoes, dead-end filter, parallel-cascade, speed of amplification.)

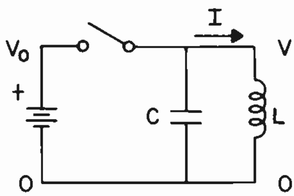
[K] H. W. Bode, "Network Analysis and Feedback Amplifier Design", Van Nostrand; 1945. (A concept of one napier per stage, p. 478.)

[L] E. L. Ginzton, W. R. Hewlett, J. H. Jasberg, J. D. Noe, "Distributed amplification", Proc. IRE, vol. 36, pp. 956-969; Aug. 1948. (Low-pass filter, one napier gain per stage.)

[M] [WM-5] "Generalized transformer concepts for feedback amplifiers and filter networks", Wheeler Labs., Wheeler Monographs, no. 5; Aug. 1948. (Feedback filter, repeater-transformer.)

[N] [WM-11] "The maximum speed of amplification in a wideband amplifier", Wheeler Labs., Wheeler Monographs, no. 11; Jul. 1949. (Distributed and divided-band amplifiers, one napier gain per stage.)

[O] W. S. Percival, "Some factors in the design of wide-band amplifiers for television", Proc. IEE, vol. 99, part IIIA, pp. 834-841; Apr. May 1952. (Dead-end filter, distributed amplifier, one napier gain per stage.)




7.5 Fig. 2 — Efficient sawtooth-current oscillator.

- With the switch closed, the constant voltage $V_0$ assures a constant slope of the sawtooth current during reversal.

The operation of the switch is not to be described here in detail. The switch current is carried in the opposite directions by two tubes, as follows:

- One tube is essentially a rectifier to carry the decreasing current during the first half of the trace.
- The other tube is essentially a grid-controlled rectifier to carry the increasing current during the second half.

The peak value of the sawtooth current is set by some operation involving the control grid.

Ideally the average current is zero so there is no power drawn from the source of constant voltage ($V_0$). In practice, some net power is consumed as a result of some loss during the retrace half-cycle and some extra voltage on the grid-controlled rectifier.

I obtained a continuous oscillation of sawtooth current by feedback. It could be synchronized by the timing of pulses of the signal.

We first used an inverted diode as the self-rectifier, but the resulting sawtooth waveform was distorted somewhat by the total resistance of diode and coil (ignored in Fig. 2). This was corrected by using an inverted low-mu triode with some grid modulation.

For the grid-controlled rectifier, we used a power tube of the pentode or beam-tetrode type.

This sawtooth-current oscillator was an excellent line-scanning circuit and was used in several models. It was described in later papers from RCA, who had received our reports as a licensee. [C] [D]

During one of our classes late in 1939, I demonstrated the sawtooth free oscillation by a long pendulum on a string. It was blocked by an obstacle on one side, making the pendulum much shorter during the retrace half-cycle.

The last 3 chapters in our book, "Television Principles", deal with magnetic scanning. They are the only publication of our work on the slope detector and sawtooth-current oscillator. Most of the experimental work on these topics was performed very well by Harold Blaisdell and Rudy Sturm.

I regard the sawtooth-current oscillator as one of my best inventions. It was the kind of concept and practical application that gave me most satisfaction. We were to learn shortly that this concept had been anticipated by the brilliant British inventor, A. D. Blumlein of EMI. After our first development of the sawtooth-current oscillator, I learned of his conception through his U.S. Patent which had issued late in 1936 with a British Convention Date in 1932. He was about 7 years ahead of me. So far as I know, he did not go as far as the inverted triode, which I used to obtain linearity of the sawtooth waveform. I was awarded the U.S. Patent on this improvement. [A] (5)

In a TV receiver, the line-scanning circuit was the last to abandon vacuum tubes for solid-state devices, because it made the greatest demand for voltage and current (volt-amperes). In current practice, the inverted diode is used and the little remaining sawtooth curvature is compensated by some means such as magnetic hysteresis associated with the load inductance. Blumlein's early invention survived but my later improvement did not.

[A] H. A. Wheeler, U.S. Patents

| | Pat. No. | Issued / Filed |
|---|---|---|
| (1) | 2206989 | 400709 / 390328 |
| (2) | 2226648 | 401231 / 390127 |
| (3) | 2235131 | 410318 / 391025 |
| (4) | 2242934 | 410520 / 381104 |
| (5) | 2250170 | 410722 / 390213 |
| (6) | 2255403 | 410909 / 390330 |
| (7) | 2264781 | 411202 / 390329 |
| (8) | 2347529 | 440425 / 410405 |

Notes (3) (5) Sawtooth-current oscillator.
(8) Slope detector.

[B] Reports.

| No. | Date | Author | Title |
|---|---|---|---|
| 868W | 380705 | MC | The design of scanning coils and amplifiers. |
| 879W | 380726 | JCW | Design of magnetic scanning circuits. |
| 894W | 380926 | HAW | Corrective networks for scanning amplifiers. |

[B] Reports. (Cont.)

| No. | Date | Author | Title |
|---|---|---|---|
| 895W | 381004 | HAW | Driving a scanning coil from an amplifier with shunt capacitance. |
| 916W | 381031 | HAW | Design formulas for sawtooth-current oscillator with inductive feedback. |
| 922W | 381109 | HAW | Relations between electric and magnetic scanning. |
| 928W | 381117 | JCW | Two-tube current-scanning oscillator. |
| 934W | 381130 | HLB | Tetrode-diode line-frequency sawtooth-current oscillator. |
| 950W | 390117 | HLB | Tetrode-triode sawtooth-current line-frequency oscillator. |
| 953W | 390131 | HLB | A two-tube field-scanning sawtooth-current oscillator. |
| 960W | 390217 | HAW | An efficient sawtooth-current oscillator. |
| 960AW | 390404 | HAW | Improvements in sawtooth-current oscillators. |
| 960BW | 390519 | HAW | Improvements in sawtooth-current oscillators. |
| 964W | 390227 | HLB | Fractional-mu characteristics of inverted triodes. |
| 966W | 390307 | HAW | A sawtooth-current oscillator for television vertical scanning. |
| 976W | 390411 | HLB | A method of testing scanning coils. |
| 986W | 390519 | HLB | Tetrode-triode sawtooth-current line-frequency oscillator. |
| 1003W | 390719 | RES | Three-tube sawtooth-current line-frequency oscillator. |
| 1006W | 390724 | HAW | An improved yoke for magnetic scanning. |
| 1007W | 390803 | RES | Field-frequency oscillator with output transformer. |
| 1025W | 390929 | HAW | A sawtooth-current generator with feedback for cancellation of curvature. |
| 1037W | 391030 | RES | Scanning and high-voltage supply chassis for magnetic deflections. |
| 1048W | 391127 | RES | Line-frequency sawtooth-current oscillator for 9" short picture tube. |

| | | | |
|---|---|---|---|
| 1061W | 400126 | RES | Television class lecture on scanning-oscillator output transformer. |
| 1064W | 400202 | RES | Field-frequency slope detector. |
| 1073W | 400227 | RES | Minimum energy storage factor of a scanning coil. |
| 1945R | 390607 | CED | Magnetic scanning coils. (Chap. 11) |
| 1997 | 391011 | CED | Sawtooth-current amplifiers for magnetic scanning. (Chap. 12) |
| 2045 | 400227 | CED | Sawtooth-current oscillators for line-frequency magnetic scanning. (Chap. 13) |
| 2132 | 410325 | RES | Line and field frequency scanning supply for color or black-and-white television. |

Note HLB other reports on sawtooth-current oscillators. 934AW, 934BW, 954W, 956W, 967W, 972W, 987W, 991W.

Note CED chapters in "Television Principles" 1944.

[C] A. W. Friend, "Television deflection circuits, Part 2", RCA Rev., vol. 8, pp. 115-138; Mar. 1947. (Sawtooth-current oscillator, tetrode and inverted triode.)

[D] O. H. Schade, "Magnetic-deflection circuits for cathode-ray tubes", RCA Rev., vol. 8, pp. 506-538; Sep. 1947. (Sawtooth-current oscillator, tetrode and inverted triode. Background of Blumlein and Wheeler.)

[E] H. W. Claypool, "Horizontal scan non-linearity in television receivers and the saturable reactor", IRE Trans., vol. BTR-7, no. 1, pp. 14-20; Apr. 1961. (Sawtooth-current oscillator, pentode and inverted triode. Author from Westinghouse.)

[F] A. D. Blumlein, "Sweep circuit", U.S. Pat. 2063025; 361208/330401/Brit. 320404. (Sawtooth-current oscillator, triode and inverted diode.)

# Section 8. Frequency Modulation

## 8.1 Frequency Modulation (FM) 1935-41.

In the few years just before World War II, the famous inventor Edwin H. Armstrong, single-handed, elevated frequency modulation from the discard to a growing new service now known as FM. We were watching from outside and contributing a little from our expertise. I made some theoretical studies. Our laboratories made signal generators for testing and designed a few models of home receivers. The craving for "high fidelity" of audio reproduction provided the primary motivation. The FM service matured after the war and FM was adopted for the sound channel in the television signal.

The radio signal used in the broadcasting services has a carrier which is modulated in either of two ways. The original service (1920) used amplitude modulation (AM), in which the amplitude of the signal is alternately increased and decreased at the audio frequency (AF). Then the degree of modulation is limited to ±100 percent. The later service (1940) used frequency modulation (FM), in which the frequency of the signal is alternately increased and decreased at the AF. Then the frequency deviation of modulation is not subject of any inherent limit, but may be many times the AF. Also the transmitted power is constant and does not vary with modulation, so the power amplifier in the transmitter can be used most efficiently. The FM being independent of amplitude, the amplified signal in a receiver can be "limited" at a fixed level so the FM detector yields the same AF output for any signal strength (above the noise background). There is no need for the AVC used in an AM receiver. In some other respects, however, the problems of design and operation are more difficult for FM. Most noticeably, the AF output of the FM receiver is distorted if not tuned on center, and there is no simple rule for tuning on center. Adequate solutions have been found for all problems of either system, so both AM and FM have survived in common use. The latter is used at higher carrier frequencies where greater bandwidth can be afforded for the wideband feature with its advantages. In broadcasting, this means the FM sound channels for sound only and for the sound with TV.

On 351106, I attended the regular monthly meeting of IRE on a Wednesday evening in New York. A group from our labs attended the dinner nearby before the meeting. Professor Hazeltine had just been elected President of IRE for 1936. In that meeting, Armstrong presented his system of wideband FM. We accorded it such importance that Vernon Whitman photographed the slides from the projection screen for our further study. The Professor made some remarks that showed some skepticism toward the principles of the system.

In retrospect, I might describe wideband FM as a system which made a good signal much better and a marginal signal worse. It required several times the essential bandwidth (say 150 Kc instead of 30). It utilized the greater bandwidth to obtain the equivalent of a much higher degree of modulation. Unlike amplitude modulation (AM), there was no restriction imposed by complete modulation (100%). Especially, the lower audio frequencies (AF) were allowed modulation to the extent of the entire bandwidth, rather than utilizing only a small fraction of the AM bandwidth. These overall principles were little appreciated at first. I trace their conception back to an earlier paper Armstrong presented 271005 (which appeared in Proceedings of IRE just ahead of my paper on AVC). There Armstrong used extra bandwidth for cancellation of noise pulses interspersed with the key pulses of a signal.

In his demonstrations of wideband FM, Armstrong emphasized the high-fidelity reception of sound with elimination of the background noise which was customary in AM reception. With respect to thermal noise in the receiver, this result was attributable to his wideband FM. With respect to lightning "static", however, this result was attributable to his use of much higher carrier frequencies, naturally free of such static. He made a mistake in his insistence that both of these features be included in one package. His system was recognized in 1940 when FCC allocated 42-50 Mc for channels of 200 Kc width. (This allocation was moved even higher after the war.)

One of Armstrong's problems was the description of his signal and its processing, because its peculiarities required unfamiliar concepts and terminology. It was much more complicated than AM. The proof of its noise reduction was especially elusive. After hearing his first talk, I formulated a description which went one step beyond his in translation to familiar terms. After his second talk (in Rochester two weeks later, 351119) I presented my formulation in a "prepared discussion". Armstrong was so impressed by my remarks, that he phoned me a month later (351230) to urge that I write up my remarks and send them for publication. After discussion with MacDonald and our patent attorneys, they advised me against publications, because we perceived an adversary situation developing and did not wish to "take sides". I respectfully declined Armstrong's request, but it brought us close for the first time. In retrospect, I am ashamed that we avoided such an opportunity for association with the truly great inventor. Some years after the war, his patent attorneys selected me to serve as their expert witness in the final litigation of wideband FM. My other obligations made it impossible for me to undertake that additional work load, but I was able to recommend another prominent engineer who did serve them. The litigation was concluded in favor of Armstrong, but not until after his tragic death by suicide in 1954 at age 63.

Our laboratory work began right after Armstrong's first papers on wideband FM. It was conducted mostly at Bayside and Little Neck, but also some in New York and Chicago. Little activity was reported until 1939-41. The leaders were Dan Harnett and Nelson Case. Among the others most active were John Farrington, Les Curtis, Bob Freeman, Ben Tyson and Lloyd Hershey.

Early work on wideband FM involved experimental transmission and reception on channels in the range of 38-44 Mc. In 1940, the FCC authorized the range of 42-50 Mc. After the war, that was moved up to the present 88-108. The channel width has been 0.2, allowing for FM deviation out to ±75 Kc. That was based on 5 times the highest AF of 15 Kc.

Preparing for the design of FM receivers, we first became familiar with the circuit problems to be encountered at the much higher IF (4.3 Mc) for this purpose. The principal problems were:

- The linear slope circuit for the FM detector;
- The preceding limiter.

Other problems of the receiver were:

- Stability of the tuning oscillator;
- Noise suppression while tuning between stations;
- Integrating the FM design with the AM design for economy.

Fortunately the tuning range was a small ratio so it could be covered easily with an expanded scale.

The FM service used horizontal polarization (HP) in preference to vertical polarization (VP). The following were significant considerations:

- An antenna for HP could easily be connected with a transmitter or receiver by a vertical balanced line.
- Propagation characteristics were more stable and predictable over ground of various properties.

Unfortunately, the horizontal dipole for a receiver has a figure-8 directive pattern, so it must be oriented broadside to the transmitter. During the war, we solved the problems of a vertical antenna. With this knowledge, we might prefer VP with the advantage of onmidirective (nondirective) reception. It would be connected with a shielded (coaxial) line.

The typical antenna for a receiver was a "folded" horizontal dipole connected with a 300-ohm balanced line.

Our work yielded quite a few reports to our licensees. Their scope ranged from specific problems to complete receiver designs. The former included test equipment and the design of the FM detector (limiter, slope filter and rectifier). This was growing up to a major activity when it was halted by the war.

In 1939-40, I organized a Consulting Group (described in another chapter) which held 16 meetings, each attended by 10-12 members of our staff and 7 invited professors prominent in our field. The first 2 meetings and 4 later meetings were devoted to FM, which was the subject of most common interest in the group. The result was useful in stimulating our thought and awareness in this field. Beginning 401014 in New York, the AIEE conducted a series of six classes on FM. I attended the first by Armstrong and one other by Nyquist of BTL.

I made an intensive theoretical study of some problems in FM reception. They were a challenge because some ingenuity was required in the statement of the problem and in the formulation of a solution.

My most significant study was entitled, "Common-channel interference between two frequency-modulated signals". I presented it at the Rochester Fall Meeting of IRE and RMA, 401112. It featured a mathematical model of the FM detector which avoided discontinuities of a limiter or a slope filter of limited bandwidth. The result left the essential effects.

My most advanced study was not published. It was entitled, "Impulse noise in an F-M receiver", Rep. 1144W, 400924. A byproduct was my collection "Standardized forms of steps and pulses", Rep. 1121W, 400715. I selected the hyperbolic secant (sech) form of pulse as mathematically attractive for this study. One of the most remarkable features of wideband FM was its tolerance of noise pulses.

In summary, our work on FM was mainly the performance of our usual function of service to our licensees. In that period, our list included the principal manufacturers and most of the others. General Electric Co. was a leader in the promotion of FM. We contributed much to the progress of FM receiver design in the other companies.

[A] Hazeltine Reports.

| No. | Date | Author | Title |
|---|---|---|---|
| 1005W | 390721 | JFF | A frequency-amplitude modulation receiver. |
| 1019W | 390919 | HAW | The patent situation on Armstrong frequency modulation. |
| 1043WR | 391114 | LFC | Effects of amplitude and phase characteristics upon a frequency-modulated signal. |
| 2018R | 400116 | DEB | Frequency-modulated standard signal generator. |
| 1080W | 400318 | DEB | F-M standard signal generator. |
| 1092WR | 400412 | DEH | The selection of an optimum derivation for a frequency-modulation system. |

| | | | |
|---|---|---|---|
| 1106W | 400608 | BFT | 14-tube frequency-modulation receiver. |
| 1121W | 400715 | HAW | Standardized forms of steps and pulses. |
| 1127W | 400802 | HAW | Two-signal cross-modulation in a frequency-modulation receiver. |
| 1144W | 400924 | HAW | Impulse noise in a frequency-modulation receiver. |
| 1128WR | 401023 | LCF, RLF | Loops and small doublet antennas for F-M receivers. |
| 1162W | 401111 | HAW | Common-channel interference between two frequency-modulated signals. |
| 2129 | 410313 | RES | Model 8-tube AC-DC superheterodyne for AM-FM broadcast reception. |
| 2109 | 410412 | LMH | Design of a 13-tube 3-range superheterodyne with F-M band and interstation noise suppression. |

[B] H. A. Wheeler, U.S. Patents.

| | Pat. No. | Issued / Filed |
|---|---|---|
| (1) | 2150044 | 390307 / 371001 |
| (2) | 2152515 | 390328 / 370618 |
| (3) | 2265826 | 411209 / 400812 |
| (4) | 2285957 | 420609 / 410329 |

Note (3): A practical embodiment of my mathematical model, which was an FM detector made of a pair of linear slope filters connected to a square-law balanced rectifier.

[C] [W 25] "Two-signal cross-modulation in a frequency-modulation receiver", Proc. IRE, vol. 28, pp. 537-540; Dec. 1940. (Presented in N.Y. 410205.)

[D] [W 26] "The solution of unsymmetrical-sideband problems with the aid of the zero-frequency carrier", Proc. IRE, vol. 29, pp. 446-458; Aug. 1941.

[E] [W 27] "Common channel interference between two frequency-modulated signals", Proc. IRE, vol. 30, pp. 34-50; Jan. 1942. (Presented in Rochester 401112.)

[F] H. A. W., "Handbook of Electromagnetics", Wheeler Labs., Inc., from 1968. (04.06.01 . . . , steps and pulses, from Rep. 1121W.)

[G] C. E. Dean, "Index of general Hazeltine reports by subjects", HEC Rep. 1710WP; 441027. (Under 19 subjects, about 250 titles, mostly 1938-40.) (30 titles related to FM.)

[H] IRE, "Standards on Radio Receivers — Methods of Testing Frequency-Modulation Broadcast Receivers"; 1947. (Chairman W. O. Swinyard; FM contributor, L. F. Curtis.)

[I] E. H. Armstrong (1890-1954), "A method of reducing disturbances in radio signaling by a system of frequency modulation", Proc. IRE, vol. 24, pp. 689-740; May 1936. (Presented in N.Y.U. 351106.) (Presented in Rochester 351119, followed by Wheeler discussion.)

[J] Ed., Wheeler discussion of Armstrong FM, Electronics, vol., no., pp. 11-14; Dec. 1935. (Photos of HAW, HML, JKJ, LFC.)

# Section 9. Test Equipment

## 9.1 Test Equipment.

The quantitative evaluation of performance is essential in engineering. It supersedes subjective testing, as by operating a radio receiver and listening to the sound. Much progress in the development of a radio receiver was made by astute observations and perceptive comparisons. However, the time came when the results had to be quantified. First some simple tests were devised for some components of a radio receiver, such as the detector. The headphones and the AF amplifier were already being tested in the telephone system. The regenerative circuit was difficult to test quantitatively. The concept of overall testing was developed gradually.

Engineering education has always emphasized laboratory experiments and testing. The advanced education of Hazeltine, after graduating from Stevens, was his year of employment in the G.E. Testing Department. In my experiments at home, much of my effort was devoted to devising and making tests. When I made the first neutralized TRF amplifier, I immediately devised a rather ingenious way of testing its gain. The quantitative results are graphed in my notebook.

From the beginning of the Company laboratory in Hoboken, my creative work was about evenly divided between the end product and the testing of its performance. The latter offered an opportunity for much ingenuity in the development of methods and the design of equipment for accuracy and ease of operation.

Here I am describing some of the more interesting tests and equipment which were developed and used in the early days of our laboratories. I derived much satisfaction from our ability to make tests that were not common and our ability to make the needed tests more accurately and more easily.

## 9.2 Testing a Radio Receiver.

In the years before World War II, my activities were directed to radio receivers, especially the broadcast receivers which were used so widely. Therefore the testing of a radio receiver was of direct concern, and much of my attention was paid to this operation. Here I shall discuss the objective testing of the overall performance of a receiver, as distinguished from the separate testing of its parts.

Some relevant topics are the following:

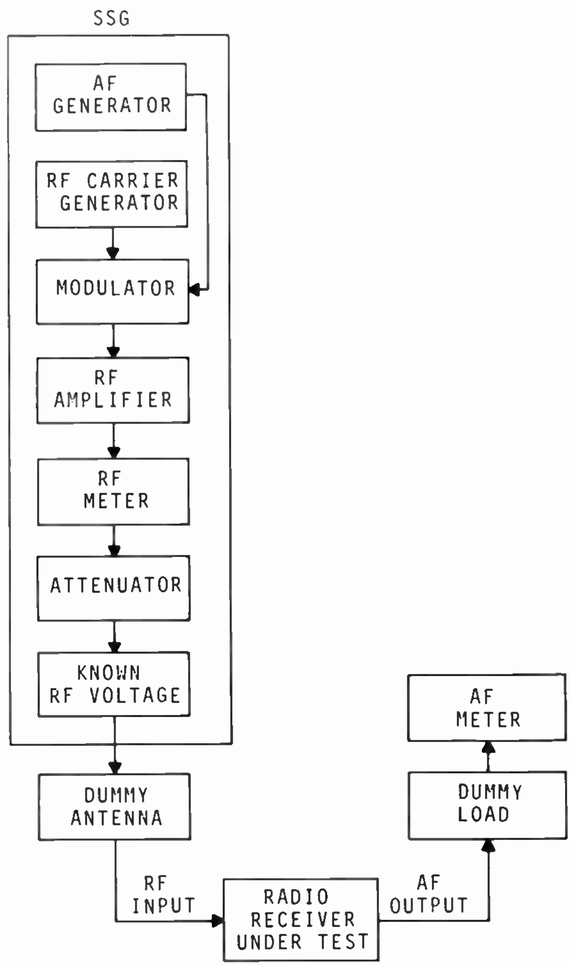
- The uncertainties of early receivers.
- My summers at the Bureau of Standards.
- The stable operation of the Neutrodyne receiver.
- The advent of IRE standards.
- My assignment to design a Standard Signal Generator (SSG).
- My conduct of the IRE Technical Committee on Radio Receivers.
- The "piston attenuator" and its use in several SSG.

**The uncertainties of early receivers.** In the beginning of broadcast reception, the sound-modulated carrier was usually received with the aid of one of these three instruments:

- A Pickard crystal detector acting as a rectifier.
- An Armstrong regenerative triode acting as an amplifying detector.
- An Armstrong superheterodyne, which was the forerunner of all broadcast sets in common use after 1930.

The third was not in early competition with the other two, and was monopolized by RCA until 1930, so it is skipped here. Each of the others was notable for instability and capricious behavior.

For sensitivity, the crystal detector relied on a ticklish setting of its "catwhisker", a light metal contact on a mineral surface we now know as a semiconductor. Such a contact was known to give rectification of a weak signal, which was one form of detection. Its physical basis was not understood, and a piece of natural mineral was selected by trial. The most common minerals were galena (lead sulfide) and silicon. The former is obsolete. The latter is now the base for most of the myriads of semiconductor devices. It is now known to rectify by virtue of traces of impurities (now controlled by "doping"), which accounts for the variation we found among samples in nature. The mystery of the old crystal detector, and its capricious behavior, left much uncertainty of performance. Its weak output, especially as usually used without an amplifier, was difficult or impossible to measure. With some luck, remarkable sensitivity (to earphones) was obtained at a contact point searched out for the most efficient rectification.




9.2 Fig. 1 — The equipment for testing a radio receiver.

For sensitivity, the regenerative detector relied on the feedback in a triode vacuum tube to nullify the losses in a tuned circuit, and thereby to respond at one frequency to a very weak signal. This required a critical setting of feedback coupling, dubbed the "tickler". Too little lost the sensitivity. Too much caused oscillation, with "squeals and whistles" (the beatnote with the carrier). The amount of regenerative amplification depended on several factors, especially the adjustment.

In spite of these peculiarities, some engineering leaders were developing concepts for describing and testing the sensitivity and other properties of a radio receiver for a sound-modulated carrier signal.

**My summers at the Bureau of Standards**. While I was working with another young lab assistant, Herbert F. Harmon, one of our assignments was to measure the sensitivity of a radio receiver. He was better prepared, having recently graduated from Grove City College near Westinghouse in Pittsburgh. He started to build an elementary signal generator, and I do not recall how far he progressed, during and after my employment there. I did not contribute much to the project, but it was a preview of what was to become one of my principal activities. The head of the Radio Section was Dr. Dellinger, who was to become the leader in standardizing some test procedures.

**The stable operation of the Neutrodyne receiver**. With Prof. Hazeltine's design of the neutralized RF amplifier in a receiver, we did have stable and dependable performance. We could predict the operation in some respects, by measuring the separate parts. This was accomplished in the Hoboken lab, albeit in an elementary way. At the New York convention of IRE, on 270112, MacDonald described our methods. An RF amplifier stage was measured between an oscillator of known voltage and a VTVM with voltage calibration. Its gain and resonance were observed. There was no rating of overall sensitivity. This article elicited a prepared discussion from G.E. Co., one of whose authors (Norman Snyder) had worked with me in the Bureau of Standards while I was there. From this we learned that their lab in Schenectady was then measuring the overall behavior of a receiver. It is interesting that they took as an example, a stabie receiver (a Neutrodyne, judging by the description and behavior) rather than one of the regenerative receivers they manufactured.

**The advent of IRE standards.** From its early days, the IRE had a Standards Committee which published an updated report every few years. The report included definitions of terms and test procedures. Beginning in 1928, this report contained some rules for testing a radio broadcast receiver. These were formulated by the Subcommittee on Radio Receivers. Dr. Dellinger was chairman, Prof. Hazeltine was one of the 24 members, and MacDonald was the member representing the Hazeltine lab and our licensees. Therefore

MacDonald had advance notice of this report, which evolved not long after his talk at the IRE. The 1928 report of this subcommittee (50 years ago) provided the rationale for all subsequent practices in testing a radio receiver. A signal was to be simulated by a standard signal generator (SSG), providing an RF carrier of known frequency and voltage, modulated a known amount by an AF generator of known frequency. Such a signal was to be applied through a prescribed dummy antenna to the receiver under test. In a dummy load, the AF output power was to be measured.

- In a sensitivity test, one would determine the RF input voltage required to yield a prescribed AF output power.
- In a selectivity test, one would graph the RF variation of sensitivity with fixed tuning.
- In a fidelity test, one would graph the AF variation of output.

Fig. 1 shows a set of equipment for making such tests.

The 1928 report went further in specifying practices for making and reporting these tests.

- Carrier-frequency range, 550-1500 Kc.
- Audio-frequency range, 40-10,000 c.
- Modulation 30% for each of the 3 principal tests.
- Modulation 400 c for all tests except fidelity.
- For a receiver designed for an outdoor wire antenna, simulation by a "standard dummy antenna" (200 pf, 20 $\mu$h, 25 ohms, in series).
- A set of 3 or 5 "standard test frequencies" for sampling the carrier-frequency range (600, 800, 1000, 1200, 1400), especially for any test requiring a graph.
- Selectivity test out to ± 100 from each test frequency.
- Input signal range (1 $\mu$v — 0.2 v).

These arbitrary but sensible practices have survived to this day (half a century), with adaptation to the common integral small loop antenna.

**My assignment to design a Standard Signal Generator (SSG).** When I started fulltime work 280817 at the Hoboken lab, MacDonald gave me two well chosen assignments. The more immediate was the design of a SSG as needed for tests in accord with the IRE standards just adopted. The design of this SSG is the subject of another section. The General Radio Co. was just releasing a SSG for this purpose, the first model to be sold. (See back cover of Proc. IRE, 1928 SEP.) In retrospect, we should have purchased this model immediately. It was an economical design with simple operation, and was used by most labs for many years. It did lack some refinements we wanted, which justified our design of a more sophisticated model, witness the fact that we made a duplicate of our model in 1931 for the Bayside lab and another in 1937 for the Chicago lab. The refinements in our design have never all been found in any other design.

**My conduct of the IRE Technical Committee on Radio Receivers.** After the 1928 report, the subcommittee was upgraded to the Technical Committee on Radio Receivers, under the successor chairman, E. T. Dickey of RCA. This committee prepared the 1931 report, which was a minor revision of its predecessor. In 1932, I was appointed chairman of the committee, and initiated an aggressive revision. I was supported by an outstanding group of 16 members.

There were two approaches to the formulation of standards:

- One was the description of accepted practices.
- The other was the pursuit of further practices that would be needed but were not yet in use.

I adopted the innovative latter approach, with the encouragement of my committee.

As a result, our 1933 report included several new concepts, such as the following:

- Two-signal tests for crosstalk and whistle interference.
- Random-noise test in terms of "equivalent noise sideband input" (ENSI).
- Expression of sensitivity in "db below 1V".
- Automatic volume control (AVC) test.
- Spurious response test, especially for a superheterodyne image ratio.
- Adjustment of a regenerative receiver.
- Tests for high-frequency (short-wave) receivers (1500-20,000 Kc).
- Specification of signal voltages for 3 sample tests (distant, mean, local).

I was continued as chairman until the 1938 report was issued. It was largely a clarification with integration of tests over the "all-wave" broadcast range (540-23,000 Kc). By then, the innovative tests were largely accepted practice, where needed. This report became famous. It was the first IRE standard to be translated to a foreign language (Spanish in Argentina).

**The "piston attenuator" and its use in several SSG.** One of the principal problems in a SSG was the attenuation of the generated signal from a measurable value to a small value for testing a sensitive receiver. In my first design, I used the customary step-attenuator made of a ladder network of resistors with a switch. The measured value was adjusted to fill in between the steps. In my thinking, I evolved the concept of an attenuator with these three features:

- Reactance in place of resistance;
- A field in space with a continuous variation of coupling, in place of a step switch.
- A rate of attenuation predictable by the dimensions, independent of the properties of the (homogeneous) medium.

These were realized in the form I named the "piston attenuator", from its structure in a cylindrical shield. Later it was re-invented at the M.I.T. Radiation Lab. during World War II, and became known as a "waveguide beyond cutoff".

Another section reviews this development, which is one of my favorite achievements. It is recounted in historical and technical detail in my 1949 monograph on the subject. It is notable that I described the idea of the capacitive type to Prof. Hazeltine in 1930, and he introduced me to the Bessel (cylinder) functions which were needed for computing the rate of attenuation. This enabled me to progress to the other two (inductive) types. In the tradition of the Professor, this was an invention in concept, simply described, and susceptible of mathematical analysis. Later I learned of background in Rayleigh's classical work on bounded fields, and in an earlier (experimental) approach by my friend John Dreyer at Atwater Kent in 1929.

This concludes my overview of the testing of a radio receiver, and how I was involved in the formulation of standards and the design of specialized testing equipment.

[A] W. A. MacDonald, "Importance of laboratory measurements in the design of radio receivers", Proc. IRE, vol. 15, pp. 99-111; Feb. 1927. (IRE Convention, N.Y., 270112.) (Hazeltine Corp.,) (Hoboken lab.) Discussion, pp. 329-340; Apr. 1927. H. D. Oakley, N. Snyder. (Overall receiver tests at G. E. Co., Schenectady.) J. H. Dellinger. (IRE Standardization Com., Receiving Sets Subcom.) (overall tests methods.)

[B] (E. T. Dickey, Chairman) "Standard tests of broadcast radio receivers", Report of the Com. on Standardization for 1928, IRE, pp. 106-126; 1928. (Subcom. on Radio Receivers, 26 members including Hazeltine and MacDonald.)

[C] (E. T. Dickey, Chairman). "Standard tests of broadcast radio receivers", 1931 Report of the Com. on Standardization of IRE, pp. 121-143; 1931. (Tech Com. No. 1, Radio Receivers, 13 members, including W. A. MacDonald.)

[D] (H. A. Wheeler, Chairman) "Standard tests of broadcast radio receivers", 1933 Report of Standards Com. of IRE, pp. 75-114; 1933. "Standard tests of high-frequency receivers", pp. 115-126.
[E] (H. A. Wheeler, Chairman) "Standards on radio receivers 1938", IRE, 58 pp.; 1938. (Also translated to Spanish for Argentina Section of IRE.)
[F] W. O. Swinyard, "Measurement of loop-antenna receivers", RMA Tech. Bul. No. 3, pp. 1-5; Dec. 1940.

## 9.3 Our First Standard-Signal Generator (SSG) 1928.

**Plan.** When I reported for fulltime work 280817, MacDonald gave me, as one of two assignments, the design of a Standard Signal Generator (SSG). It was intended to implement the test procedures just standardized by IRE. They were specified in the 1928 report of the Standards Committee, Subcommittee on Radio Receivers, of which MacDonald was a member.

**SSG by General Radio Co.** The first available SSG for this purpose was the GR Type 403, which was advertised on the back cover of Proc. IRE, 1928 SEP. This was shortly after my assignment, and we should have purchased one, but we never did. It included some ingenious features, but lacked some features we wanted. It was an inspired compromise design for general use.

**The design of our SSG.** For our first SSG, I made an electrical design with emphasis on features relating to known problems without conventional solutions. Some of these will be described. I made a layout which was reduced to a mechanical design by Nick Fedotoff, an experienced designer of electrical equipment. He did an excellent job, carrying it through a model shop (Foote Pierson in N.Y.C.). I had the skillful cooperation of Lucien Troxler in completing the electrical assembly, adjustments and testing. On moving to the N.Y. lab early in 1929, we built a screen room as a shielded enclosure in which the SSG was operated.

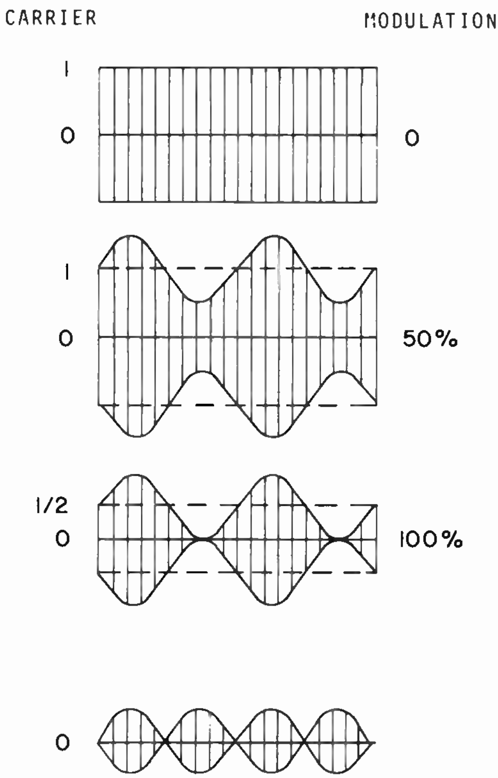
Fig. 1 in the preceding chapter shows the arrangement. The main unit includes RF oscillator, modulator, amplifier, meter and attenuator. The 5 tubes are type UV-171A, chosen for "low mu" and hence moderate DC voltage for high current and high transconductance. The cathodes are heated by 5 V DC from a storage battery. The high voltage is provided by rectified AC power.

**Shielding.** Testing a sensitive receiver on the bench next to an RF generator imposes severe requirements on shielding of the generator. Only the desired weak signal must be let out to reach the receiver. Our design shields the circuits and then encloses all in a separate outer aluminum box. Control shafts from the front panel have insulating sections.

**The RF oscillator.** The RF oscillator is tunable by a variable capacitor. Its feedback is "variable-ratio" in the manner of my contemporary circuits for "uniform gain" in an RF amplifier. It is designed for uniform RF voltage over the tuning range, which was unusual. The oscillator output provides the RF carrier in the modulator.

**The RF tuning.** A major feature of the design is the expanded scale of the tuning. The range is divided into 5 bands, each centered on one of the 5 standard test frequencies (IRE 1928):

500-600-700 Kc
700-800-900
900-1000-1100
1100-1200-1300
1300-1400-1600




9.3 Fig. 1 — The modes of maximum modulation.

Each band is expanded over a half-circle on the dial of a rotary capacitor. The range of the tuning capacitance is set for each band by a pair of fixed capacitors in series and parallel. The rotary capacitor is shaped to give nearly linear calibration in each band. A reference scale is used for experimental calibration in each band, after which its frequency scale is engraved on one of 5 semicircles on the dial. Direct reading on this scale is fine enough for all intended purposes, especially the selectivity test with RF variation. It was an expensive luxury but paid off in the many tests for which it was used. The associated power amplifier is separately tuned but is not critical and does not affect the measurements.

**The RF modulator.** An unusual balanced modulator uses a pair of tubes in opposition, only one of which is modulated by a voltage from a separate AF generator. The design gives linear modulation of the RF carrier up to 50%, which covers the typical requirement of 30%. The other tube is used to cancel 50% or 100% of the carrier, if desired. See Fig. 1.

- Half-cancellation of the carrier has the effect of doubling the % of modulation without distortion, so up to 100% modulation is available.
- Full cancellation of the carrier leaves only the sidebands of modulation, so their power can be measured to set the degree of modulation.

These features have not been used in any other SSG design, so far as I have learned. They were especially useful in that day when the cathode-ray oscilloscope was not yet in common use, as for showing modulation and distortion.

**The RF amplifier.** From the modulator, the weakened RF carrier is amplified to a power level sufficient for driving a low-resistance attenuator with RF carrier up to 0.2 volt. This is the highest value specified (IRE 1928, 1933, 1938) for standard tests with a dummy antenna. The input to this amplifier is adjusted to set the output voltage.

**The RF voltmeter.** A thermocouple and DC meter is used to set the RF carrier voltage to a measurable value for the attenuator. The solid-state rectifier was not yet available for this function.

**The attenuator.** A step attenuator is made of special low-resistance units. It is supplemented by a 100:1 unit at the receiver for weakest signal with least susceptibility to RF leakage.

**The dummy antenna.** To simulate a wire antenna, as was commonly used, the receiver is connected to the SSG through a standard dummy antenna. Its principal component is a series capacitor (200 pf per IRE 1928).

**Comment.** The most unusual features of this desgin were the direct-reading expanded scale of tuning and the balanced modulator. The latter was remarkable for enabling 100% modulation free of distortion, and for enabling separate measurement of the carrier and sideband power.

[A] [R422] L. J. Troxler, "Standard RF signal generator"; Jun. 1929.
[B] General Radio Co., "Standard signal generator, Type 403", Proc. I.R.E., vol. 16, no. 9, back cover; Sep. 1928. (500-1500 Kc, 1-200,000 $\mu$V).
[C] L. M. Hull, "Overall measurements on broadcast receivers", Proc. Radio Club Amer., vol. 5, pp. 85-93; Oct. 1928. (Based on 1928 report of IRE and General Radio Co. Type 403.
[D] "Rating of radio receivers", Gen. Radio Exp., vol. 3, no. 6, pp. 1-4; Nov. 1928. (Based on 1928 report of IRE and SSG Type 403.)
[E] C. T. Burke,"The standard-signal method of measuring receiver characteristics", Gen. Radio Exp., vol. 4, no. 10; Nar, 1930. (SSG, Type 403-C.)

### 9.4 Our First AF Beat-Frequency Oscillator (BFO) 1928.

For modulation of the SSG over the AF range, an AF generator is needed to provide uniform voltage with frequency variation over a specified range (such as 30-10,000 c). The AF variation is used for the fidelity test.

A beat-frequency oscillator was the only familiar circuit for this result. The General Radio Company announced such an instrument about that time. It was a good compromise design for practical purposes of that day. We should have purchased one, but never did. Perhaps we should have used it instead of designing a matching unit to go with our RF unit.

The beat-frequency oscillator can deliver an extreme ratio of AF as the beatnote between two oscillators at higher frequencies, one variable over a small ratio. I designed an AF generator utilizing the beat frequency between one oscillator at 40 Kc and another at a lower, variable frequency. The pair are made alike except for added variable capacitance on the tuner of the latter.

The frequency scale has two features:

- It goes to zero for "zero-beat" trimming of the pair of oscillators.
- It is nearly logarithmic in frequency calibration over the useful AF range.

These are achieved by a rotary capacitor with radically shaped rotor, over an angle range of 240°. An experimental calibration is engraved on the dial.

An unusual feature is the indicator for observing the zero beat. A circuit for a small panel meter is designed with a rectifier to give a greater reading at lower AF. Then the approach to zero beat is signaled by oscillation of the pointer, down to a fraction of one cycle per second. This enables a setting much closer than possible by ear, and by sight instead of sound. This method was original in this design, and found various applications later on.

A few years later in the Bayside lab, a motor-driven variation of this AF generator was made by Whitman, Swinyard and Sturm for a sweeping record of loud-speaker response.

For testing an AF amplifier, this AF generator was designed to give a measured output of any value from 10 V down to 1 mV.

This instrument was designed as a companion to the SSG. It was the same style and height for locating by the SSG on the left side. Its mechanical design and its electrical construction and testing were performed with much skill by Fedotoff and Troxler.

[A] [R754] L. J. Troxler, "Standard audio frequency generator"; 310724.

## 9.5 RF Vacuum-Tube Voltmeter 1929.

The measurement of a single stage of RF amplification requires an RF voltmeter meeting these criteria:

- Little loading of the output circuit (negligible loss and small capacitance that can be tuned out);
- Short lead between the circuit and the voltmeter;
- Fullscale about 1 V (RF, RMS).

A vacuum-tube voltmeter (VTVM) relying on anode-current rectification was commonly used. The RF signal was applied to a negative grid. To obtain greatest change of current by rectification, the grid bias and the anode voltage were related to give a moderate value of DC. This value was balanced out in a sensitive meter, so as to read a relatively small change of current by rectification. The result was a delicate balance, sensitive to drift and susceptible to unbalance which could damage the meter.

In 1928, one improvement was immediately available. The recent screen-grid tube was a departure from current tube design, in having a grid cap at the top instead of a grid prong in the base. This minimized the grid capacitance. Then we mounted the tube on a "gooseneck" so it could be supported close to the points of connection, with very short clip leads.

The UX-222 type (connected as a triode) was chosen, being the smallest screen-grid tube and requiring the least power for its DC filament-cathode (3 V, 0.12 A).

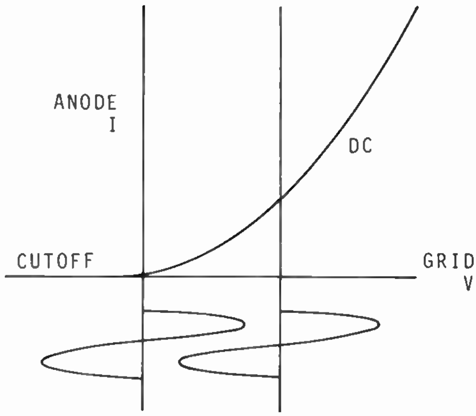
In order to reduce the initial anode current, that would have to be balanced, I used a principle that was new in a triode rectifier. It will be described with reference to Fig. 1.

The DC curve in Fig. 1 is determined by 3 principal phenomena:

- The cutoff is rounded slightly by the voltage and temperature gradients near the negative end of the filament, and by the granularity of the grid structure;
- The intermediate range of current is governed by space charge and the voltage gradient along the filament, so the change of voltage nominally varies with the 2/5 power of the current.
- The higher range of current is governed mainly by the space charge, so the change of voltage nominally varies with the 2/3 power of the current.

The ultimate saturation of current is so far removed that it can be ignored.

The result of these factors is a gradual transition of behavior which yields an intermediate voltage variation approximating the 1/2 power, which is commonly termed square-law. (Such a transition would not be obtained with the later unipotential cathode.)




9.5 Fig. 1 — The two modes of operation for square-law rectification.

There are two ways of using this curve for square-law rectification, as indicated. One is the full-wave rectification, well up on the curve, which was the practice. The other is half-wave rectification, which I perceived as preferable. The latter left very little initial DC to balance out, so the balance was not critical. The operating point was chosen for closest approximation to square-law calibration on the DC meter scale of rectified current (change of average current). The latest bench meter gave fullscale for 10 $\mu$A. After balancing out only 3 $\mu$A, the rectification gave full scale for about 1V (AC, RMS).

The combination of the gooseneck structure with the half-wave square-law rectification yielded an RF VTVM which was a great advance in refinement and ease of operation. It did require batteries, but very little power. By some standards, its degree of refinement has never been equaled by any other design. Later compromises yielded more features of convenience and versatility with a lesser degree of refinement.

[A] (R451A] HAW, "Volt-square meter", 2908XX.

## 9.6 AF Vacuum-Tube Voltmeter 1929.

The testing of a radio receiver requires an AF output meter for power (around 50 mw) or for voltage across a known resistance.

In late 1928 and early 1929, the measurement of AF power of voltage was typically made by a thermocouple and a sensitive DC meter. This suffered from these deficiencies:

- Danger of burnout of the thermocouple by slight overload;
- Sluggishness of the DC meter by reason of its sensitive design and the overdamping by the thermocouple;
- Square law (power, not voltage) which decreases the voltage ratio usable on a single scale of the DC meter.

Therefore there was a need for a meter free of these deficiencies.

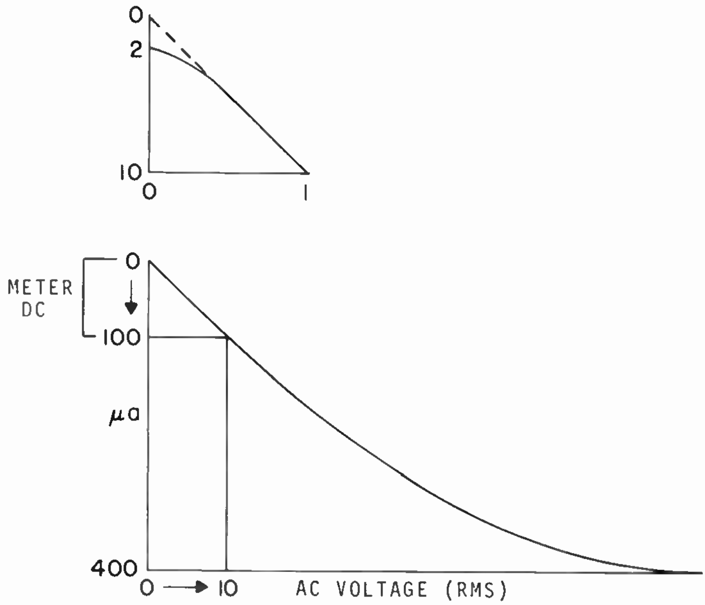
To meet this need, I designed a vacuum-tube voltmeter (VTVM) with a linear scale, which was suitable for AF measurement. This was a predecessor to the integral-rectifier type of AF meter, which became available in the next few years (using a bridge-type copper-oxide rectifier).

I used a peak rectifier, which is inherently linear. It was a byproduct of my diode AVC and linear detector. Instead of linear response to AF modulation of an RF carrier, it required merely a linear response to a steady AF voltage.

For the diode, I used the anode and cathode of a triode (171A) with a positive bias on the grid. This reduced the space charge and formed a virtual cathode near the anode. The result was greater current in the diode.

The common VT, with DC filament as a hot cathode, did not offer the very sharp cutoff desired in a peak rectifier. By using a high-resistance load (5 megohms), the small current met fairly sharp cutoff. The result was nearly linear rectification from 1 to 10 V (AF, RMS). A DC meter for 2 $\mu$a was not available, so I added a DC amplifier to give a current variation of 100 $\mu$a, the fullscale rating of a common DC meter with a large scale. A low-mu amplifier was desired, to handle 10 V with small DC voltage on the anode. For this I used a backward triode (171A, backward mu about 1/3). The rectified voltage was applied negative to the amplifier, so it reduced the current. The initial current was 4 times fullscale, and it was balanced out. Therefore the meter overload was limited to 4 times fullscale DC, which was a tolerable overload, so the meter was protected.

Fig. 1 shows the behavior of this VTVM, especially the matching of the rectified current with the meter scale for direct reading of AC voltage. The DC balance with zero AC voltage is set to 2 $\mu$a to compensate for the hook on the curve of linear rectification. The DC output is set by a series resistor so that the linear variation matches the meter scale from 1 to 10 V, with error less than 0.1 V + 1%. The slight curvatures of rectifier and amplifier tend to cancel. The circuit adds little to the free damping of the meter.




9.6 Fig. 1 — The linear response from the DC amplifier.

Batteries were used for stability. The drain was small. They were a small 6 V storage battery for the filaments and a small 9 V dry battery for the DC amplifier.

While this VTVM required a large meter, a box, and the batteries, it was located on the back of the bench and served very well. It overcame the deficiencies of a thermocouple.

For testing a receiver, the AF dummy load was provided by a resistor and the linear VTVM. The current taken by the latter was negligible. The required power level could be adapted to the voltmeter by an output transformer and/or a voltage divider.

[A] [R39W] MC, "Audio frequency output meter".
[B] [WP31] 1951685; 340320/310401. (Includes the only publication of the linear AF VTVM.)

**9.7 The RF Reactance Meter 1929.**

The Neutrodyne receiver featured several tuned RF circuits (typically 3) which had to track for maximum sensitivity and selectivity. The original 3-dial design required the operator to track all the dials when tuning from one station to another. With the advent of unicontrol, all tuned circuits were tuned at once from a single dial to a fair degree of approximation. There was a need for a systematic test to evaluate the small errors in tracking.

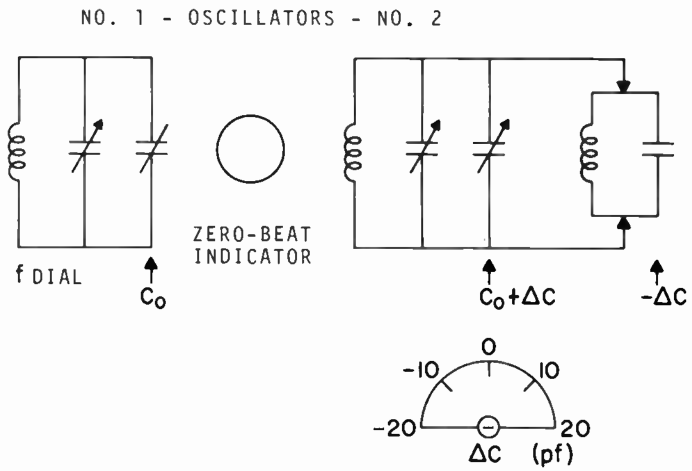
For this purpose, I devised the socalled RF Reactance Meter. It should have been named, a Differential Susceptance Meter, but the term "susceptance" was little used by others (other than the Professor and myself as his disciple). It proved to be one of our most useful instruments in testing all receivers prior to the superheterodyne. Its useful life for this purpose was therefore only a year or two, but it had further applications. The first one was built early in 1930 at the N.Y. lab. A copy was built for the Bayside lab. Our design was later manufactured by General Radio Co. They published an excellent description of its principles and applications. [H] [I]

The principle of the reactance meter relates to the behavior of a tuned oscillator. See Fig. 1. An added shunt susceptance (here measured by $\Delta C$ increment of capacitance) shifts the frequency of an oscillator (No. 2). Compensating for this increment restores the frequency, so its amount is a measure of the increment.

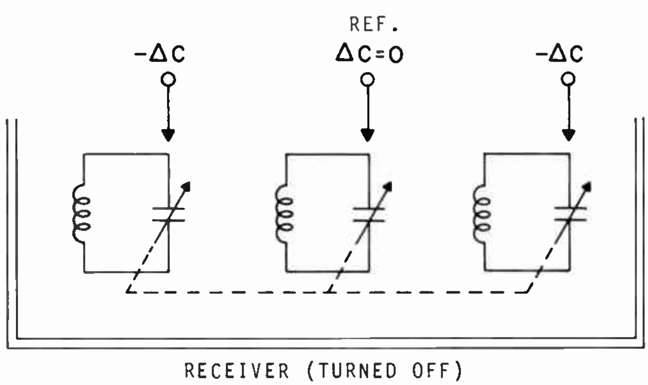
This operation ignores any moderate amount of shunt conductance (loss) associated with the added susceptance. This becomes a one-dimensional balance as distinguished from the two-dimensional balance in an AC bridge. Also it avoids "searching" for a peak of resonance, which is needed in the socalled "Q Meter". The tuning of the oscillator gives a valid reaction, even in cases where the measured susceptance is much less than its shunt conductance.

Referring to Fig. 1(a), the first oscillator is set to the test frequency. Then the $\Delta C$ dial of the second oscillator is set to zero, its test leads are disconnected, and it is tuned to the first by reference to the zero-beat indicator.

For a sensitive zero-beat adjustment, intercoupling of the oscillators is reduced nearly to zero by coupling through buffer amplifiers to a mixer. Instead of the customary audible beatnote, I featured a visual indicator combining two kinds of response. A triode connected as a diode rectifier is associated with a sensitive small meter, to form a rectifier meter like the integrated models which were soon to become available. This is coupled to give maximum response in the AF range of beatnote. As the beatnote becomes subaudible, it appears as a reduced response with a vibration of the meter pointer, so its frequency can be adjusted down to about 1 cycle.




(a) Pair of oscillators with zero-beat indicator.




(b) Test of tracking error in unicontrol receiver.

9.7 Fig. 1 — The RF reactance meter and its principal use.

Having set to zerobeat with the test lead disconnected, the lead is clipped to the test load. Referring to the triple-tuned receiver in Fig. 1(b), the lead is clipped on the one circuit and the receiver is tuned with ΔC set to zero. Then the lead is clipped on any other circuit. After resetting ΔC for zerobeat, this dial indicates the difference of tuning, which is an error in tracking. This is the test for which the reactance meter was designed.

There was one measurement for which the reactance meter was found especially useful. It was the tracking errors of the first tuned circuit in a unicontrol receiver, as affected by the differences of an associated wire antenna. Therefore it was used for TRF and superheterodyne receivers. Our 1930-31 paper includes several examples of this measurement. [W10]

This instrument is generally useful for measuring small susceptance or for setting an external tuned circuit to resonance at the set frequency. For example, I used it for some studies of the apparent capacitance and resonance of an RF choke coil. [C] [D] [E]. It formed the background for a related instrument, the direct-reading RF inductance meter (chapter 9.11) [R480W]

The reactance meter was remarkable as the first test set we made self-contained in the manner of contemporary receiver designs. It used AC tubes with indirectly heated cathodes (227 triode, 224 tetrode) and rectified AC for DC supply.

This project and its applications occupied several members of our staff at various times from 1929 to 1934, including N.V. Fedotoff, W. R. Hynes, F. P. Corbett, J. K. Johnson and W. O. Swinyard. In a later design by L. J. Troxler, the frequency range was extended below and above the broadcast band. [B]

[A] [R559] H. A. Wheeler, "R. F. reactance meter"; 300321. (550-1500 Kc) [R559A] W. R. Hynes, changes in Bayside model; 310312. [R559B] H. A. Wheeler, graphical method; 310624.

[B] [R1137] L. J. Troxler, "R.F. reactance meter"; 340822. (3 ranges: 125-350; 550-1500; 1500-4000 Kc.)

[C] [W10] with W. A. MacDonald, "Theory and operation of tuned radio-frequency coupling systems", Proc. IRE, vol. 19, pp. 738-805; May 1931. (Reactance Meter used to measure the relative detuning of antenna circuit.)

[D] [R479W] W. O. Swinyard, "Choke coil conductance measurement"; 340514.

[E] [R506W] H. A. Wheeler, "Design of RF choke coils", 341017.

[F] [W18] "The design of radio-frequency choke coils", Proc. IRE, vol. 24, pp. 850-858; Jun. 1936.

[G] [WP64] 2092708; 370907 / 350239. Re. 21176. (RF choke coil designed by Reactance Meter.)

[H] [R1061] L. J. Troxler, "General Radio reactance meter; type 421-A"; 331216.

[I] H. H. Scott, "The General Radio — Hazeltine reactance meter", General Radio Experimenter, vol. 8, no. 11, pp. 1-4; Apr. 1934. (Type 421-A, from Hazeltine design, 3 ranges: 125-300, 550-1500, 1500-4000 Kc.)

**9.8 The Piston Attenuator 1930.**

One of my favorite inventions is the exponential attenuator in a cylindrical waveguide below cutoff. In the days before the waveguide had been named, I called it the "piston attenuator", by analogy to the piston in a cylinder of an engine. Its practical attraction is its logarithmic scale of output voltage or power, which becomes linear in decibels. It was an invention in the tradition of Professor Hazeltine, combining a theoretical concept and a practical application. When I first mentioned it to him (early in 1930) he was fascinated by it and he steered me to two concepts: [A] [WM-8]

- For a circular cylinder, the cylinder functions (Bessel functions) which were not so commonly taught and used in those days.
- For minimizing the distortion by higher-order terms, the shaping of the electrodes.

As usually happens, the field theory could be found in the earlier literature, but not the applications and refinements. The piston attenuator is still in use and no successor is likely in the future.

In one form, the piston attenuator was independently invented a little earlier (1929) by my friend, John Dreyer, while working at Atwater Kent in a group of outstanding engineers. He also was a disciple of Professor Hazeltine. He did not go as far as a computed calibration and the use of the lowest mode in a cylinder. [WM-8] During World War II, the piston attenuator was re-invented at MIT Radiation Laboratory for use with microwaves.

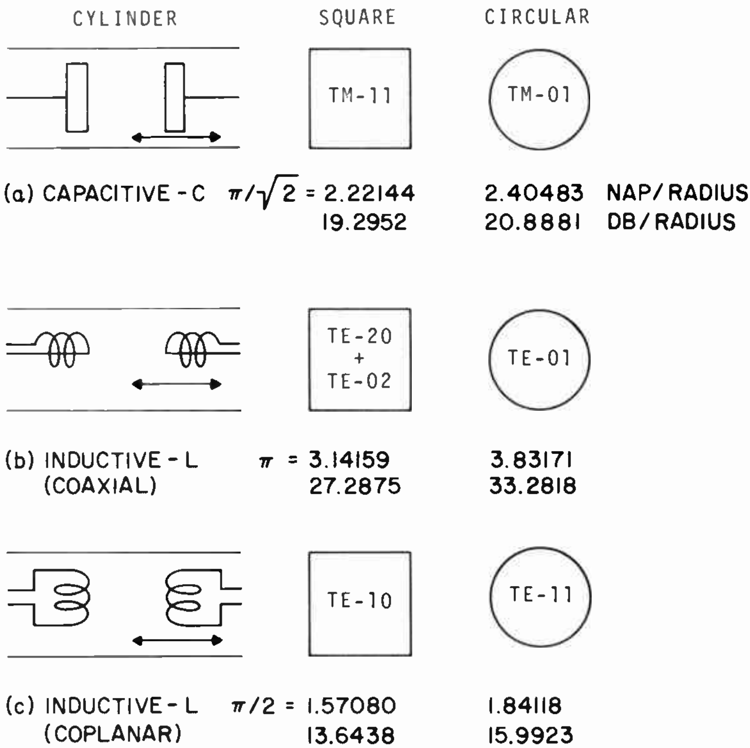
The piston attenuator is one subject which I have documented in its history, theory and applications. This I did in 1949, so I shall give here only the highlights as I see it in perspective. [WM-8]

The three different types were adapted to three SSG in the Bayside lab. These will be described here, because each one featured the piston attenuator.

Fig. 1 is a chart of the three types that have been found conceptually helpful and practically useful. Each type is identified by its field (C or L), its conventional mode number in a circular or square cylinder, and the rate of attenuation with axial separation. They are shown in the order of conception by me.

The C type relies on decay of E field along the length in a cylindrical pipe (shield) of conducting material. My original concept (in 1929) was a ladder network of series and parallel C, with a sliding contact. Then I perceived (early in 1930), that this could be realized continuously in a metal pipe, as shown. I computed the rate for a square pipe, derived from the images in the walls of a square pipe. The rate came from a definite integral I found in the one handbook then available.

Peirce, 490. $$\int_0^\infty \frac{\cos mx\,dx}{1+x^2} = \frac{\pi}{2} \bullet \; e^{-|m|}$$




9.8 Fig. 1 — The principal types of piston attenuator.

I soon told the Professor about it, and he directed me to the solution for a circular pipe, in the one available textbook on that subject (Byerly). (These references date back to Harvard U. before 1900.) The latter was my introduction to cylinder functions (Bessel functions) for boundary problems of a field in space. The presentation was based on heat conduction, which we translated by analogy to E and M fields. The rate of attenuation in a square pipe is related to pi, while that in a circular pipe is related to Bessel roots, as may be recognized on the chart. The Professor designed a pair of electrodes for a circular pipe, which would suppress higher-order terms and leave a close approximation to exponential variation of direct C.

The L type with coaxial coils was my second form, which I conceived (in 1931) by analogy between M field and heat conduction. Circular coils in a circular cylinder is the simplest shape, and I found in Byerly the basis for its rate of attenuation. Later I perceived how the rate in a rectangular cylinder (such as a square) could be derived by images. Following the Professor's example, I found diameter ratios, for circular pipe and coils, which would suppress the second and third terms. This left near-exponential variation of mutual L as close as the coils starting to overlap.

The L type with coplanar coils was my third form. It was developed in response to a fault in the second form. Experiments by Lewis and Cawein had shown that the coupling between coaxial coils far apart was sensitive to any departure from axial symmetry. Even with axial alignment, such departure was caused by the leads of coils with few turns. Their thoughtful tests showed me the nature of another mode having a lesser rate of attenuation, which I had not anticipated. With this stimulus, I came to realize that we should be using the mode having least attenuation, so any other mode would contribute relatively little to the coupling. From their tests, it appeared that the least rate of attenuation would occur between coplanar coils. I soon found simple models for computing the rate in a circular pipe (by Byerly) or a square pipe (by images). The latter proved to be the simplest model of all types of piston attenuator. Here again, I found shapes to suppress the second and third terms.

The L type with coaxial coils in a circular pipe was the first to be used by us in a SSG. (It was the type used earlier by Dreyer, but with experimental calibration.) It was used in a multi-range SSG made by Lewis and Cawein, completed early in 1932. A pair of plug-in coils was used for each range, with the inductance adapted to the frequency range. All pairs were about the same size, to operate in the same circular pipe, but differed in number of turns. (This was a freedom not available in the C type.) The frequency coverage was 0.1-80 Mc in 6 ranges. The design and operation was straightforward. One of the coils in a pair was used for the RF oscillator and was located in the near end of the pipe. The other was used to derive the output voltage and was mounted for axial motion with a linear scale in decibels. This model was the first use of a computed calibration (linear in decibels) in a SSG. It was useful for most tests, because the fault caused appreciable error only for a very weak signal at the very high frequencies (coils of few turns).

The C type in a circular pipe was the second to be used by us in a SSG. It was made by Lewis and Cawein in 1934. It covered 12-88 Mc in 3 ranges. The oscillator and a modulated power amplifier were tuned by a pair of plug-in coils and unicontrol rotary capacitors. The same piston attenuator was used, which was adapted for this ratio of coverage by a transformer ratio from the amplifier. The Professor's design was used for suppression of higher-order terms. This was the first use of the C type, and was computed as a linear scale in decibels.

The L type with coplanar coils in a square pipe was the third to be used by us in a SSG. The first model was made by Huxtable in 1934, the second by Troxler in 1935. The frequency coverage was 0.1-25 Mc in 6 ranges by switching fixed inductors. For the piston attenuator, I chose a square pipe with coils on rectangular forms to fit the pipe. Each coil was a few turns; one was adapted to the oscillator inductor and the other to an output inductor by a transformer ratio chosen for each range. Their shapes were chosen to suppress the second and third terms. The computed scale, linear in decibels, was predicted and tested to be valid nearly to contact. The small number of turns reduced any effect of incidental C coupling. This model proved extremely convenient in operation. This arrangement of coils has not been used in any later models by anyone known to me.

The L type with coplanar coils in a circular pipe was the fourth to be used by us in a SSG. It is the type which has since been used most. This SSG was designed and built by my microwave group in 1942, then manufactured by the Company in 1943 as Model 1050 operated at 900-1200 Mc. The oscillator was tuned by a coaxial-line resonator, which was coupled to the attenuator pipe through a hole. Its inner conductor served as the input "coil". The output "coil" was made of a single wire.

The 68 references from my monograph [WM-8] are included here as a historical sampling of the activities relating to the piston attenuator. The TE-10 mode of attenuation in a rectangular pipe was the first use of this mode, some years before it was named and became the most common mode in a waveguide. A piston attenuator using the lowest mode is still commonly used in a microwave SSG and there is no sign that it may be superseded.

[A] References from [B].

(1) *W. E. Byerly, "Fourier's Series", Ginn and Co., New York, 1893. (Trigonometric and Bessel series for exponential attenuation in rectangular and circular cylinders.)

(2) *Lord Rayleigh, "On the passage of electric waves through tubes, or the vibrations of dielectric cylinders", Phil. Mag. S.5, vol. 43, no. 261, pp. 125-132, Feb. 1897. (Theory of waves in conducting cylinders of rectangular and circular cross-section.) (Reprinted herewith.)

(3) Lord Rayleigh, "Scientific Papers", vol. 4, pp. 227-280, 1897, (Waveguide theory.)

(4) E. J. Townsend, "Functions of a Complex Variable", Henry Holt and Co., New York, 1915. (Exponential rate of attenuation between parallel planes, p. 127.)

(5) Jacob Kunz and P. L. Bayley, "Some applications of the method of images", Phys. Rev., vol. 17, pp. 147-156, Feb. 1921. (The electric field of a wire between parallel planes.)

(6) B. O. Peirce, "A Short Table of Integrals", third ed., Ginn and Co., New York, 1929. (No. 490, the case of exponential attenuation.)

(7) Alan Hazeltine, Notes to Wheeler, April 14, 1930. (Dimensions of the capacitive attenuator shown in reference 14, HC Fig. 1, upper diagram.)

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(16) G. C. Southworth, "Hyper-frequency wave guides — general considerations and experimental results", B.S.T.J., vol. 15, pp. 284-309, April 1936. (One of the original papers on waveguides, showing only various modes in a circular cylinder.)

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(21) S. A. Schelkunoff, "Transmission theory of plane electromagnetic waves", Proc. I.R.E., vol. 25, pp. 1457-1492, Nov. 1937. (Waveguides of rectangular cross-section.)

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(40) Harold Pender and S. R. Warren, Jr., "Electric Circuits and Fields", McGraw-Hill Book Co., New York, 1943. (The field of a line charge, p. 294.)

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(42) H. A. Wheeler, "The piston attenuator", presented before I.R.E., Jan. 28, 1944. (Summary and history of 15 years.) Program, Proc. I.R.E., vol. 32, p. 180, Mar. 1944.

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(45) Simon Ramo and J. R. Whinnery, "Fields and Waves in Modern Radio", John Wiley and Sons, New York, 1944. (pp. 321-323, 371-373, attenuation of TE and TM modes in waveguides below cutoff; p. 339, table 9.03, modes in circular guides; p. 343, table 9.04, modes in rectangular guides.) (p. 206, depth of penetration.)

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(48) D. M. Hill and M. G. Crosby, "Design of f-m signal generator", Electronics, vol. 19, no. 11, pp. 96-101, Nov. 1946. (Using piston attenuator at 54 - 216 Mc, with subsequent converter for 0.1 - 25 Mc.) Boonton Radio Corp.

(49) Eduard Karplus, "Components of uhf field meters", Electronics, vol. 19, no. 11, pp. 124-129, Nov. 1946. (Using piston attenuator at 300 - 1000 Mc.) General Radio Co.

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(55) *Harvard Univ. R. R. L. Staff, "Ultra-High-Frequency Techniques", vol. 2, 1947. (W. B. Wholey, piston attenuators, pp. 1016-1023.)

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(57) *R. E. Lafferty, "Piston attenuator chart", Electronics, vol. 21, no. 2, p. 132, Feb. 1948. (Nomogram for $TE_{11}$ and $TM_{01}$ modes in circular cylinder, showing variation of attenuation with frequency.)

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(65) General Radio Co., Signal Generators, piston attenuators used at 20 - 1000 Mc.

(66) Hazeltine Electronics Corp., Signal Generators, piston attenuators used at 900 - 1200 Mc.

(67) Hewlett-Packard Co., Signal Generators, piston attenuators used at 500 - 4000 Mc.

(68) Measurements Corp., Signal Generators, piston attenuators used at 2 - 1000 Mc.

**Notes**

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[B] [WM-8] "The piston attenuator in a waveguide below cutoff", 61 pp.; Jan. 1949.

[C] D. E. Harnett, N. P. Case, "The design and testing of multirange receivers", Proc. IRE, vol. 23, pp. 578-593; Jun. 1935. (Includes a description of Wheeler's piston attenuator, all 3 types. The only publication until years later.) (Discussion by J. F. Dreyer, Jr.)

[D] N. P. Case, "40-120 Mc signal generator", RMA Engineer, vol. 4, no. 2, pp. 9-12 and 15; May 1940. (Piston attenuator, coplanar coils in a circular cylinder.)

## 9.9 The Acoustic Recorder 1932-38.

The acoustic recorder was developed to make a photographic trace of the frequency variation of the sound output from a radio broadcast receiver. It was developed in 1933 at the Bayside lab. It accomplished an evaluation which was a great step forward in its day and which has seldom (if ever) been made more recently. The desired result of sound reception is a faithful reproduction (by some standard) of the sound being transmitted.

"Fidelity of reproduction" involves a number of contributing factors that are difficult to define, because each one is complicated and/or subjective in nature. The behavior of an audio (AF) transmission channel, between microphone and loudspeaker, could be simply described and could be designed to a high degree of refinement. The same was true of a microphone. On the other hand, the loudspeaker, its environment and the location of the listener presented problems that defied description. The loudspeaker presents difficulties in design, and its performance depends on its location, especially relative to the nearest walls (including floor and ceiling).

The purpose of our tests was to indicate the performance of the loudspeaker toward a listener in a typical living room. This distinguished from the customary "scientific" tests in a "free-space" environment simulated in a non-reflecting "dead room" (anechoic chamber). The most significant achievement in our tests was the combination of these two factors:

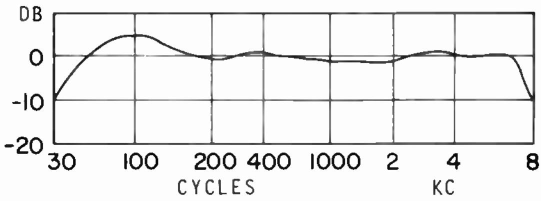
- A realistic placing of the loudspeaker in its cabinet, located in the corner or on one side of a room with floor and ceiling;
- A realistic placing of the listener's ear at some distance and direction from the loudspeaker (not too near any walls behind the listener).

The resonances in an ordinary room are so complicated that tests at spot frequencies are not useful. In the testing of a loudspeaker, it was (and still is) customary to sweep the AF and to record the response of a test microphone in some location relative to the speaker.

The acoustic recorder was designed to sweep the AF in 1 to 8 minutes, over the range of 30-7000 c on a logarithmic scale. The signal from the test microphone was recorded photographically on a linear scale of decibels. Fig. 1 shows a record made in 3 minutes with the following two refinements intended to smooth the graph and thereby to give a significant result in one graph:

- Frequency "wobble" 5 times per second, by an amount somewhat less then ±(10 c plus 5%);
- Smoothing to give an average over the "wobble" bandwidth.

The loudspeaker is in a corner of a living room. The listener microphone is located at a typical place in the room.




9.9 Fig. 1 — A record of acoustic response from a loudspeaker in the corner of a living room (average of three locations).

The frequency sweep is mechanical with a separate rotary wobble. The recorder is a mirror oscillograph on a photographic film on a rotating drum.

The record in Fig. 1 shows the performance of a special single-cone loudspeaker in a corner environment. The departures from level come mainly from these causes. The effects of room reverberation are largely smoothed out, just as the ear accommodates to its environment.

The acoustic recorder was developed in 1933 at the Bayside lab by Vernon Whitman, Bill Swinyard and Rudy Sturm, working under my direction. They were an excellent team for utilizing the available techniques. The recorder enabled an informative study of the loudspeaker problem in a living room and of the performance of various models. From tests by this recorder, more than 20 reports were issued to various licensees, 331214-380507. Most of these were made in 1934-35 by Whitman, Swinyard and Farrington. In 1935, we published a description of the acoustic recorder and its use.

[A] [W16] with V. E. Whitman, "Acoustic testing of high fidelity receivers", Proc. IRE, vol. 23, pp. 610-617; Jun. 1935. (Presented at Annual Convention, Philadelphia, 340530.)
[B] [R445W] V. E. Whitman, "Acoustic test procedure", 331214.
[C] [R495W] V. E. Whitman, R. E. Sturm, "Acoustic recorder", 340912.

## 9.10 The Antenna Impedance Meter 1934-38.

The antenna impedance meter was made for measurements on a "short-wave" balanced antenna. The frequency range (3.5 - 20 Mc) was much above the highly developed broadcast band, but much below the bands destined for FM, TV and radar. This range was too low to use "wavelength" dimensions in test equipment, but high enough to require unusual refinement in "circuit" techniques. The required circuit balance imposed further constraints on the measuring set.

The measurement of impedance or admittance implies the evaluation of real and imaginary parts. In any particular frequency range, there are encountered a particular set of problems.

Circuit balance is easier in a parallel measurement, so the two components of admittance were chosen for separate evluation. Then they are converted to the two components of impedance.

The "variation" method was chosen for shunt conductance. The unknown conductance is added in parallel with a known conductance, and the ratio of voltage reduction ($V/V_o$) is observed.

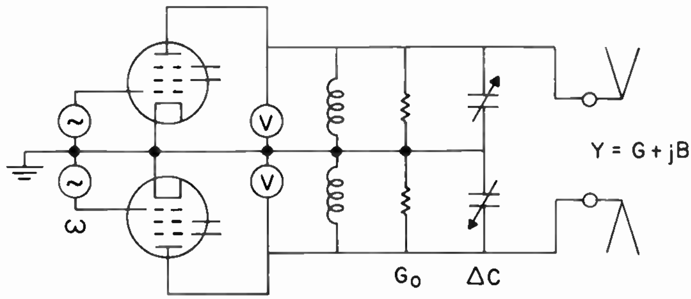
The "substitution" method was chosen for shunt susceptance. The unknown is connected across a parallel-resonant circuit with a calibrated linear scale of shunt capacitance. On retuning to resonance, the observed decrease of shunt capacitance ($-\Delta C$) is translated to positive shunt susceptance.

Fig. 1 shows the essentials of this circuit, and the formulas for evaluation of the admittance.

The measurement of rather small impedance or large admittance requires a rather large $\Delta C$ for susceptance. Two series pairs of capacitors were connected in parallel. The calibrated range of one series pair was made 1000 pf, the other 100 pf. They were connected across one of 4 plug-in pairs of inductors.

Direct reading of conductance was enabled by an extra scale on a square-law VTVM across the balanced circuit. At 1/4 scale, $G/G_o = 1$. At 1/36 scale, $G/G_o = 5$. On a large-scale meter, the latter was found usable. To cover the range of 0.5 m.mhos (down to 200 ohms) each plug-in pair of inductors was standardized to 1 m.mho. Then an extra shunt of 9 m.mhos (111 ohms) was provided to cover the range of 0-50 m.mhos (down to 20 ohms).

The greatest requirement of $\Delta C$ occurs on the low-frequency side of a series resonance, or peak of admittance. If the peak is 50 m.mhos, the susceptance of $\Delta C$ may be as high as 1/2 this value, or 25 m.mhos. This corresponds to about 1000 pf at the lowest frequency, which is available in the design. Therefore it is possible to measure a series resonance of 20 ohms. (The nominal radiation resistance of a straight dipole at resonance is 73 ohms.)




$$G = G_0(V_0/V - 1)$$
$$B = -\Delta C \omega$$

9.10 Fig. 1 — The antenna impedance meter for measurement of balanced parallel admittance.

The conversion from admittance components to impedance components was computed by slide-rule routine which I had devised. It has not appeared in any slide-rule instruction book. The quadratic sum required for a right triangle is performed by adding one to a ratio. In one setting of the slide, any two dimensions of a right triangle can yield the third. On the usual straight slide rule, this routine frequently runs off the end of one scale. I had a 6-inch circular slide rule which avoided this nuisance, so it was used with the impedance meter, especially for conversion from admittance components to impedance components. This was one of the few applications for which I ever found the circular rule to offer much advantage. Today we could use a small programmable calculator, such as the HP-97 with print-out, and avoid the labor of computation.

This equipment was developed in 1934 at the Bayside lab by Vernon Whitman and Rudolph Sturm working under my direction. Vernon had much experience in lab techniques and Rudy learned quickly after his recent course at Stevens Tech, so they made an excellent team.

Some results of measurements on horizontal dipole antennas were published in 1936. I do not recall any similar results from other laboratories being published at any time. Even today, I am not aware of any equipment in use for such measurements.

The antenna impedance meter was used in an intensive program for development of an "all-wave" antenna system. An elevated horizontal dipole (doublet) antenna was used for "short-wave" reception on horizontal polarization and also for broadcast-band reception on vertical polarization. The antenna was supported between two 40-foot poles on the grounds of the Bayside lab and the impedance meter was operated at the top of a third pole midway between the other two. For these tests in winter, Rudy wore an aviator's outfit and the observations were phoned into the lab building for recording. The measurements were used to enable the design of a dummy antenna for simulating the actual antenna in the design and testing of associated circuits. That is the subject of another chapter.

[A] References relating to the antenna impedance meter are found in chapter 6.5 on Antennas and Lines.
[B] J. R. Dempsey, "The Rotarule", Berkeley, Calif.; 1929. (A 6-inch circular slide rule.)
[C] [WM-2] "Slide-rule operations for radio problems", Wheeler Monographs, no. 2; May 1948. (The right triangle of impedance or admittance.)

## 9.11 The RF Inductance Meter 1936.

The RF measurement of an inductor (without iron core) has been accomplished by various makeshift methods, and still is to this day. The direct-reading RF Inductance Meter was one of my outstanding inventions, as measured by its simplicity and refinement. The first one was designed and built at the Bayside lab in 1936, then another was made for the N.Y. lab. It was too specialized to find a place in the catalog of test equipment. Its built-in calibration did not inspire the confidence of a direct measurement. Today it is used only in the Hazeltine Research Labs.

The purpose of my inductance meter was to measure an inductor (L) at a frequency near its operating frequency, but sufficiently low to avoid any substantial effect of its inherent shunt capacitance. At this frequency, its reactance would be great enough to measure easily and would include approximately the high-frequency skin effect in the wire and any nearby shield (such as a ground plane or shield can). Furthermore, the measurement was to ignore series resistance (loss), which is dependent on the frequency.

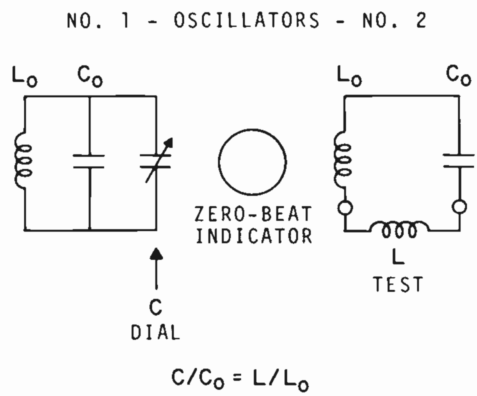
The common method up to that time (1936) was the 1000-cycle bridge. It requires a two-dimensional balance and the reactance at that frequency may be less than the resistance. It ignores the skin effect in the wire and (in some degree) the reduction of L by any nearby shield.

Another method was resonance with a known capacitor at a known frequency. This was a lab experiment for each test. A measurement at the operating frequency was provided some years later by the socalled "Q Meter" of Boonton Radio Corp. Tests at two frequencies are required to make a correction for shunt capacitance (usually substantial in its effect).

My new approach was a variation of the Reactance Meter. That instrument was used in 1934 to measure a small increment of inductance ($\Delta$L) in series with an inductor in a tuned circuit. This tuned circuit was so designed that the $\Delta$C scale (up to 20 pf) could read directly in $\Delta$L (up to 2 $\mu$h). This was the first time that the RF inductance of a short wire could be measured quickly and easily. The frequency was 1 Mc. [A]

Fig. 1 shows the essentials of the Inductance Meter. Two oscillators are alike and are first set to zerobeat. Then the L under test is connected in series with the second oscillator and a calibrated C is added to the first oscillator to return to zerobeat. The C scale is proportional to L, and the circuits were designed for calibration so L is a decimal multiple of the C scale. Four pairs of tuned circuits give operation in these ranges: [B]

| | | |
|---|---|---|
| 0 — 30 | $\mu$H | 540-470 Kc |
| 0 — 300 | | 180-150 |
| 0 — 3,000 | | 54-47 |
| 0 — 30,000 | | 18-15 |




9.11 Fig. 1 — The RF inductance meter.

The C scale is expanded to 3000 divisions by a main dial of 30 and a vernier dial of 100. The lowest scale has 1 division for 0.01 $\mu$h the inductance of a short wire.

The direct-reading feature is based on the fact that a linear scale is readily available in a rotary capacitor but not in any practical inductor. The oscillators provide a relation, $L/L_0 = C/C_0$, so the scale of L becomes likewise linear. There are incidental effects which cause a slight departure from the ideal relation. After all adjustments provided, the residual error is of the order of 1% or less for a pure L. The resistance in a typical inductor may cause a lesser error in the same or opposite sense. As in the reactance meter, the first-order behavior ignores the resistance, which affects mainly the amplitude and not the frequency of the oscillator.

Further development of the Inductance Meter (Model 10) was my first project in Wheeler Labs., Inc., after the war. [C]. Refinements were added to compensate for incidental capacitance in the circuits. Its range was extended to 5 decades. Each range was provided by a plug-in pair of inductors. The lowest was 10 $\mu$h. The C scale was provided by a General Radio Co. standard capacitor with 5000 divisions by a worm-gear drive, so one division on the scale was 0.002 $\mu$h.

This design was later tailored to a neat package (Model 20) which was described in 1947 at the Rochester Fall Meeting of IRE and RMA. [D] The GR standard capacitor (full scale 1000 pf) was teamed with a circuit box of equal size, and the pair of inductors for any range was plugged into a receptacle on top. One model 20 was sold to the Bureau of Standards and another to Boonton Radio Corp., who decided not to manufacture it. [E]

A set of standard inductors is required for trimming the plug-in circuits to realize the calibration. For each scale on the Model 20, the calibration is matched at full-scale and 1/10 of full-scale, by trimming the inductance and incidental capacitance of each of the plug-in coils. A decade set of coils in cans was available from Boonton Radio Corp., from 10 μh to 100 mh. Also needed was a standard of 1 μh. For this purpose, a computable standard was made of a rigid coaxial line 1.5 meters in length with short-circuit on the far end. Then a special short-circuiting plug at the near end enabled a predictble change of 1 μh. The inner conductor was made of a small copper wire, with a correction for the skin depth at the operating frequency for that inductance.

The Model 20 has an extra feature, the measurement of C by substitution across the standard C. Then the scale of the latter shows directly a reduction equal to the test C. The scale is readable to a fraction of one division (0.2 pf). Another GR standard C has 100 pf fullscale, with one division for 1/10 the C (0.02 pf). For small C, the highest frequency is used (1.6 Mc) so the zerobeat test is most sensitive. This scale is useful also for very small L up to 0.1 μh (one division = 0.0002 μh). This method gives the most sensitive test of small C or L that has been devised.

Paul H. Taylor worked on the Inductance Meter at the Bayside lab in 1936. The further development at Wheeler Labs. in 1947 was made by Robert Novick and John Irish.


[A] [R480W] W. O. Swinyard, "Measurement of low inductance"; 340516. (Use of RF reactance meter with external inductor.)

[B] [R647W] P. H. Taylor, "Direct-reading inductance meter"; 360601. [R647AW] 361117. [R647BW] 361120.

[C] [WL-1] H.A.W., "RF inductance meter", Electronics, vol. 20, no. 9, pp. 105-107; Sep. 1947. (WL Model 10.)

[D] H.A.W., "R.F. inductance meter with direct-reading linear scale", presented at Rochester Fall Meeting; Nov. 17, 1947. (WL Model 20.) Program, Proc. IRE, vol. 35, p. 1107; Oct. 1947.

[E] R. H. Schott, "Model 20 R-F inductance meter — operation", Wheeler Labs. Inc., Rep. 290; Feb. 1950.

## 9.12 The Phase-Curve Tracer.

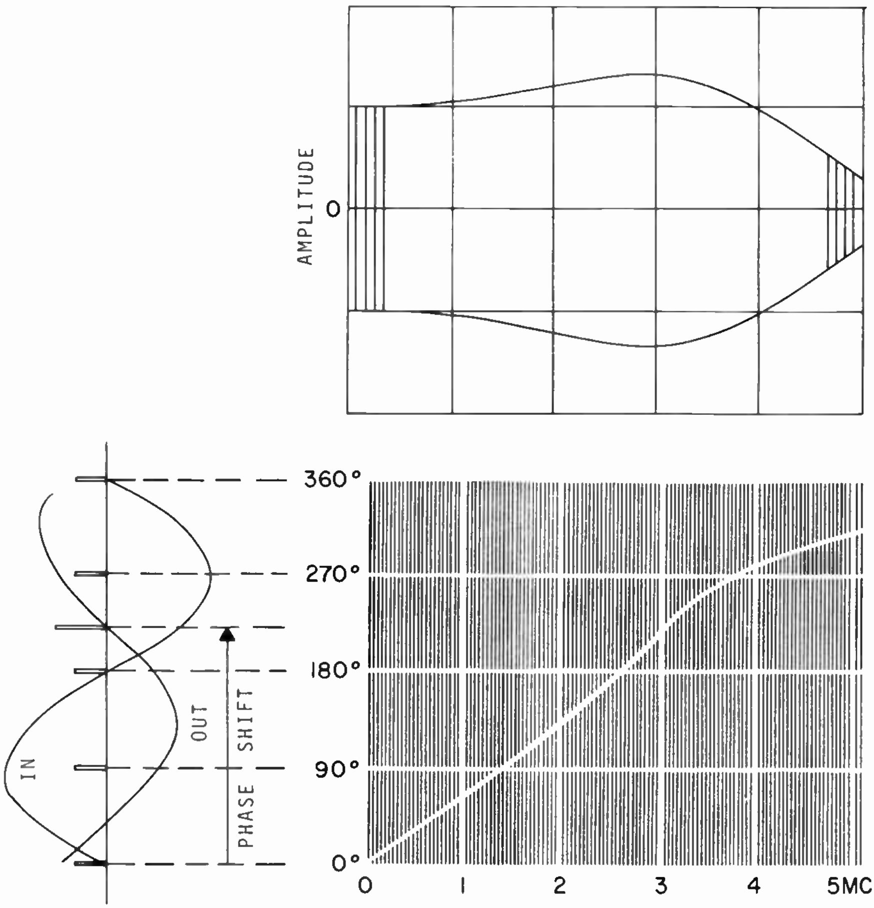
The Phase-Curve Tracer of Bernard D. Loughlin was a major event in his career and in the history of Hazeltine Corp. It was a milestone in the evolution of circuit testing techniques, from the frequency domain to the time domain, the latter being associated with television. It came at a time when television was making demands on network response, not only the amplitude but also the phase (which is not critical in sound reproduction). Previously there was no phase-curve tracer.

Barney Loughlin graduated from Cooper Union in 1939. He had built at his home in East Orange, N.J., a model of a circuit tester which could display not only the usual amplitude variation but also the phase variation over a swept frequency band. The amplitude variation appeared in the usual manner on a cathode-ray oscilloscope, while the phase display appeared as a line of bright dots on the amplitude area. This was a rudimentary phase tracer. He submitted a paper on this topic to the annual student-paper contest of the New York Section of AIEE, and won first prize, 390427. (In a later year, I served as one of the judges of this contest.) His paper was entitled, "Vector response indicator", to convey the concept of both amplitude and phase being displayed. It was presented first to the judges and then at the next annual convention of AIEE in N.Y. (400125).

With his degree (B.E.E.) and this development in electronics, Barney went out to find a job. Opportunities for an engineer were still few, in the slow recovery after the depression but before the acceleration of war work. He was interviewed by Dorman D. Israel, Chief Engineer of Emerson Radio and a good friend of Dan Harnett and myself in IRE. Dorm told him that the best opportunities in television engineering were at RCA and Hazeltine. Barney came to Little Neck for an interview, talked with Dan and me, and we immediately employed him, starting 390619. We were much impressed with his ability, especially as evidenced by the phase tracer he had made.

A few months after he started work, we assigned to him the project of building a more advanced phase-curve tracer for our TV circuits. This he did and prepared a description for publication. He first presented this paper at the Rochester Fall Meeting of IRE and RMA, on 401111, then at the IRE annual convention in N.Y. 410111.

In the meantime, Bell Telephone Labs. had learned of this development when a distinguished group from their staff had visited us at Little Neck on 400129 at our invitation. The group was Ronald M. Foster, Robert L. Dietzold, Dr. Sidney Darlington and Dr. Hendrik W. Bode, all prominent experts in the field of frequency-selective networks. On 400919, a TV group from BTL was attending a TV committee meeting at the Little Neck lab, so they saw the phase-curve tracer in operation. This group was Axel G. Jensen, Dr. Pierre Mertz, Maurice E. Strieby and Donald A. Quarles. As a result of that visit, Dr.




9.12 Fig. 1 — The display on the phase-curve tracer.

Mertz came again on 401206 with Estill I. Green and others to learn more about it.

With the increasing tempo of war work, our TV activities were being reoriented. The Signal Corps at Ft. Monmouth ordered a phase-curve tracer. This was delivered to them on 420318 by Loughlin and Duffy. I am not aware how it may have been used there. It was more needed for the more intensive work on radar at M.I.T. Radiation Laboratory, but I do not know of their making any phase-curve tests.

After World War II, TV and other activities made increasing use of the time domain, which was utilized in the phase-curve tracer. Also an improved model was designed and a few were made and used at the Little Neck lab.

Loughlin's home-made phase tracer introduced the feature of tracing the phase by a line of bright spots on a "raster" area formed by sweeping the frequency. In that case, the raster fast scan was the signal waveform making vertical scan in proportion to its amplitude, in the usual manner of amplitude display. The relatively slow horizontal scan followed the frequency sweep. The phase appeared as a bright spot marked on each vertical scan at a position corresponding to the amount of phase shift.

The phase-curve tracer developed by Loughlin at the Little Neck lab displays the phase (or amplitude) variation of a circuit, over a frequency range, in the time of one field of a picture sequence. It simultaneously displays a grid of coordinate lines which are derived from the same source and therefore are correlated. In the Loughlin-Hazeltine design, the frequency is swept over the video range (0.1 - 5 Mc) in 1/40 second, so the display can follow slow variations of the circuit parameters.

The principles are easily stated, and their implementation is engineering (beyond the scope of this story). These items can be followed with reference to Fig. 1.

- The test frequency is swept (0.1 - 5 Mc) on a frequency-tracking time base.
- The input and output signals of the sweeping frequency are converted to two signals of the same constant frequency (50 Kc) while retaining the amplitude ratio and phase difference. The constant frequency is made lower for a practical line frequency but high enough to follow the variations while sweeping.
- The phase difference between input and output yields a time shift of intercepts at the constant frequency.
- The intercepts are used to time the development of short pulses from the input, to serve as a reference, and from the output, to represent the phase shift.

- The phase curve is displayed on a raster of fast vertical line scan pulsed by the phase timing, and slow horizontal scan timed by the frequency sweep.

Any desired grid of rectangular coordinates can be displayed by developing from the input signal more pulses during either scan period.

The range of phase difference can be expanded by compressing the vertical scale and displaying several times one cycle in one line scan. A continuous curve of several cycles can be seen, with repetition every multiple of one cycle above and below. This is a graphic illustration of the N-cycle ambiguity of angle.

The same display could be plotted on paper by an ink stylus, which would require translation to a relatively slow read-out.

Further development of the phase-curve tracer could be made to display some derived quantity, such as phase slope or envelope delay.

The Phase-Curve Tracer was an outstanding advance in technology. This result is available today but only as one of several features in a more complicated device.

[A] B. D. Loughlin, "Vector response indicator", Trans. AIEE, vol. 59, pp. 355-356; Jun. 1940. (Based on first-prize paper in student contest.)

[B] B. D. Loughlin, "System for indicating electrical phase-shift characteristics", U.S. Pat. 2285038; 420602 / 400803.

[C] [R1157W] B. D. Loughlin, "A phase curve tracer for television"; 401029.

[D] B. D. Loughlin, "A phase curve tracer for television", Proc. IRE, vol. 29, pp. 107-115; Mar. 1941. (Presented at Rochester Fall Meeting 401111 and IRE Annual Convention in N.Y. 410111.)

[E] [R1291W] M. P. Duffy, "Operating manual for phase-curve tracer", 420423.

[F] [R1293W] M. P. Duffy, "Technical report and servicing instructions for phase-curve tracer", 420512.

[G] M. Levy, "Methods and apparatus for measuring phase distortion", Elec. Com., vol. 18, pp. 206-228; Jan. 1940. (A phasemeter for 0.5 Mc, by conversion to a fixed frequency.)

## 9.13 Test Equipment for TV 1932-41.

The introductory chapter on Television includes a listing of 38 reports on test equipment for our TV program 1932-41. Most of them came from the Bayside lab and the remaining few from the N.Y. lab. The number and variety of these instruments and procedures make it difficult to give them the credit due or to place deserved emphasis. I shall comment on a selected few.

The cathode-ray oscilloscope was not a common instrument before TV. It became essential with signal features for the timing of TV scanning and image display. Its development as test equipment was soon surpassed in its applications as a picture tube for the receiver. The subject period of time saw many advances in its utilization for both purposes. This was a preview of its explosive progress for use in radar during the war. We were not aggressive leaders in this history. but we made progressive designs for use in our laboratories.

The TV signal was specialized in ways that were complicated in contrast to the simplicity of a sound signal. It brought a very different set of demands and tolerances. Timing and transient response became important, in contrast to linearity and freedom from harmonics. Therefore signal generators had to be modulated in the waveform of TV signals. These were designed for timing the scanning and for picture reproduction. At first, the latter performance was inferred from test patters; then from the line scanning of motion-picture films.

Signal generators had to be made for higher carrier frequencies, all above the 30 Mc which had been our upper limit. They had to be adapted for much higher frequencies of modulation (VF or video frequency increasing to 5 Mc). For the most part, these changes could be made by extrapolation from the practices of "all-wave" broadcasting.

Our construction of a camera tube (iconoscope) in 1936 enabled us to modulate the carrier by the video signal from a live subject in moderate light. We were perhaps the first laboratory outside RCA that achieved this capability. It enabled us to assemble and to demonstrate a complete TV system with all the functions peculiar to the transmitter and receiver. We had a versatile "signal generator" from which to test a receiver design.

Magnetic scanning in the picture tube is the subject of a separate chapter, together with special tests for the required sawtooth current.

Toward the end of this period, we were equipped for complete testing of all characteristics of a TV receiver.

The phase-curve tracer of Barney Loughlin was a diagnostic tool of great value for future refinement of video circuits. It saw little use before the wartime hold but went into use when TV activity resumed after the war.

# Section 10. Other Topics

## 10.1 Inductance Formulas 1928

Prof. Hazeltine introduced me to a simple formula for a radio coil, and I became interested in this subject. I designed another simple formula which is still in common use. The computation of inductance became one of my specialties, and still is. It is a game which is fascinating as one application of field theory, so its significance goes beyond the simple formulas I shall recall.

In the period of this story, a low-loss RF inductor was made in the form of an "air-core" coil, meaning that no iron core was used. In the 1930's, a core of compressed powdered iron or iron dust became useful for a small coil but a rather large coil was needed for low loss in the broadcast band (around 1 Mc). From the beginning of radio, one of the challenges has been the computation of the inductance of a coil in any of the forms commonly used. The most common were the single-layer coil (helix, also termed solenoid) and the multilayer coil, as shown in Fig. 1. Either of these could be computed on the assumption that the nearest objects had no effect on its magnetic field. Such a formula could be modified for approximating the effect of a shielding enclosure (such as a box or a can made of copper or aluminum).

The Professor had derived a simple formula for a multilayer coil, which he gave to me sometime during our first summer (1923). I derived a related formula for a single-layer coil, which was simpler and gave a closer approximation. One of my earliest papers was a comparison of these two formulas. [W07] They were developed from two different approaches.

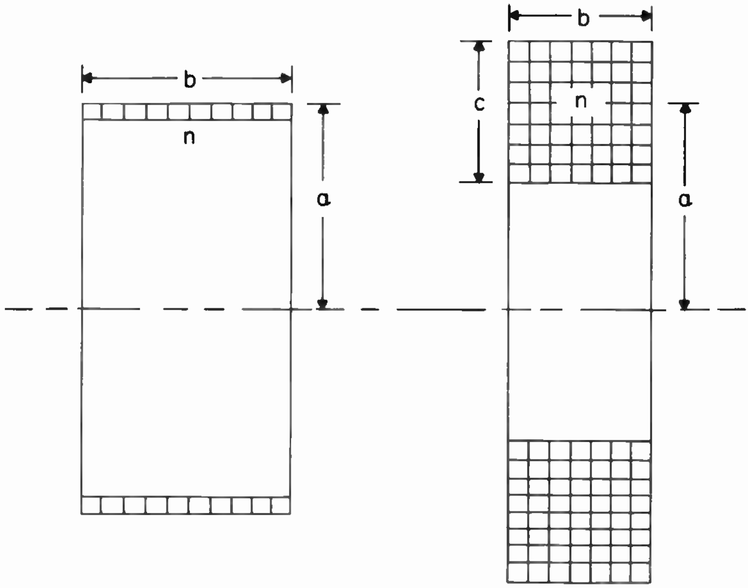
Hazeltine's formula for Fig. 1(b) is the following, in terms of inductance (L) in $\mu$h and dimensions in inches:

$$L = \frac{0.8\ a^2 n^2}{6a + 9b + 10c}$$

Its principal feature is the ratio of the 3 coefficients in the denominator. They are related to the coil shape that would give most L with a given length and size of wire. This shape was described in the familiar reference C74. [B] With the chosen coefficients, this shape would make the 3 terms in the denomintor nearly equal.

I was especially interested in the single-layer (helical) coil of Fig. 1(a). Its complete formula was found in C74, but required reference to a tabulated factor. I noted that the L for a long coil has a parabolic variation with length (b). Therefore I analyzed 1/L for its near-linear slope and intercept in terms of shape (b/a). I found that the asymptotic straight line gives a remarkably close approximation, so I wrote the formula for Fig. 1(a) in this form:

$$L = \frac{a^2 n^2}{9a + 10b}$$




10.1 Fig. 1 — The two common forms of air-core inductor.

The straight line yielded a ratio near 9/10 for the coefficients in the denominator. These numbers yielded unit coefficient in the numerator, from a peculiarity of the dimensional units. The result is an extremely simple formula with error less than 1% of L if $b/2a > 0.4$. It has been quoted in most handbooks.

It was common practice to express formulas for analysis (L from given dimensions) rather than synthesis (dimensions for desired L). But synthesis is the design process, so I soon found that either of these formulas could be "reversed" to give an explicit solution for synthesis, and I did that. This reversal has been rediscovered by various other workers in later years. A recent incident is a program for my formula, listed in the catalog for the HP-97 personal calculator. (02168D, from B. K. Murdock.) It solves for any one of the parameters from any sufficient set of three others. For example: from wire size, radius and L; the number of turns (and hence the coil length).

Over the years since my 1928 paper, I have come to view our two formulas in historical perspective.

Hazeltine's formula is similar to one published many years earlier by Perry in England. [D] Perry's coefficients in the denominator were roughly in the ratio 5:9:10, which is not significantly different from Hazeltine's. A variation of Perry's formula appeared in Electrical Experimenter, Apr. 1918, and I copied it in my notebook at the time (long before I met the Professor).

My formula first appears in my notebook on 200724, with essentially the same coefficients. I suspect that I forgot it and then derived it again, when it appears on 250808 in the final form. It was the latter which I chose to publish in 1928. Later I realized that my formula is a second approximation for a series expansion for a long coil, given by Rosa and Grover in S169. [C] Comparing their formula (79), it appears that my ratio 9/10 is an approximation for a theoretical value 8/3 $\pi$ or 0.85. The arrangement of my formula greatly reduces the effect of further terms in the series expansion.

These incidents are reminders of the continual challenge to build on the state of the art. I always welcome further knowledge of the background. This may come from diligent searches but often comes from accidental discovery in conversations and browsing in the literature. The yield from the latter is proportional to one's contacts with the outside world. Now I may never know whether Hazeltine independently developed a formula similar to Perry's, or just improved on it to suit his whims.

[A] [W 7] "Simple inductance formulas for radio coils", Proc. IRE, vol. 16, pp. 1398-1400; Oct. 1928.

[B] J. H. Dellinger et al, "Radio Instruments and Measurements", Bu. of Standards, C74; Mar. 1918.

[C] E. B. Rosa, F. W. Grover, "Formulas and tables for the calculation of mutual and self-inductance", Bu. of Standards, S169; Dec. 1916. (See formula (79).)

[D] J. Perry, "A formula for calculating approximately the self induction of a coil", Phil. Mag., vol. 30, pp. 223-227; Sep. 1890. (Multilayer formula in the form of Hazeltine's.)

[E] M. Brooks, H. M. Turner, "Inductance of coils", U. of Ill., Bul. no. 53; Jan. 1912. (Multilayer formula, same form as Hazeltine's but different coefficients.)

[F] W. H. Eccles, "Wireless Telegraphy and Telephony: A Handbook of Formulae, Data and Information", The Electrician, London; 1915. (Perry's formula, p. 68.)

[G] Electrical Experimenter, p. 321; Aug. 1917 (Single-layer formula, same form as mine but not as close approximation.)

[H] Electrical Experimenter, p. 840; Apr. 1918. (Perry's formula, copied from Eccles.) (This is where I first saw it.)

[I] B. K. Murdock, "Handbook of Electronic Design and Analysis Procedures Using Programmable Calculators," Van Nostrand; 1979. (Part 3-5, Wheeler's formula, p. 373.)

## 10.2 The Neutroidal Coil 1925-26.

This invention of mine had much in common with Hazeltine's Coil Angle which I featured in my previous book. [H/P] [P5] It was directed to nullifying the magnetic coupling between coils. It was a simple mathematical solution. It was superseded by coils in cans. Mine came about 3 years later than his. Unlike his, mine was not used before it was superseded. I named mine the "Neutroidal Coil" or "Neutroid" after the pattern of the name "Neutrodyne", because it operated by neutralizing the external magnetic field. The name reflected some similarity to the familiar toroidal coil or toroid, which was known to have no external magnetic field.

The toroidal coil was wound on a form of doughnut shape. The internal magnetic field was closed on itself, so it was contained. It was crowded so the losses in the surrounding coil were rather large for its size. It was difficult to construct, especially with the single-layer coil desired for a tuner at broadcast frequencies. It was seldom used.

A makeshift equivalent was the "binocular" coil, made of a pair of opposite coils side-by-side connected together. It was an ingenious device for practical reduction of external magnetic field. Its magnetic field was largely contained. It was used in AK receivers before shielding (the receivers which were later found to use Hazeltine PCN in a useful amount).

I perceived that a pair of concentric coaxial coils could be designed to neutralize the far field. Fig. 1(a) shows my invention. The pair is connected in series, with the outer coil grounded. My first description was entered 250711. The inner and outer coils were connected in opposite directions with equal $ND^2$ product.

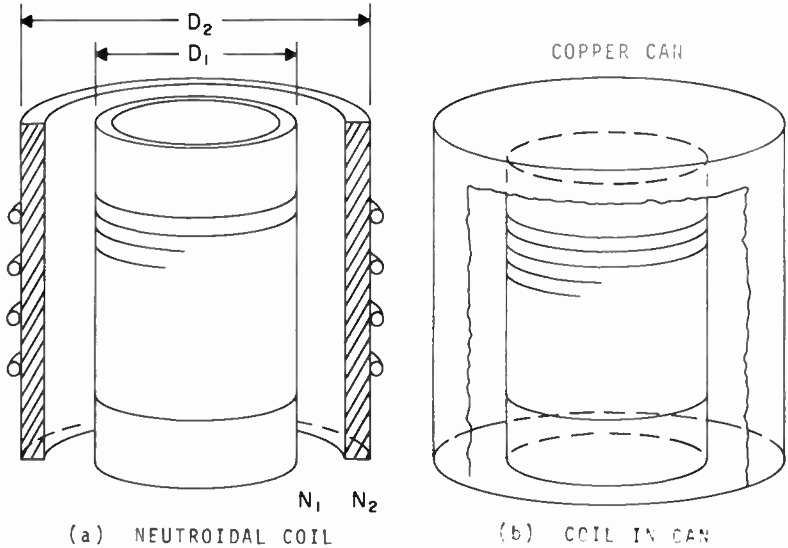
The coupling between a pair of inductors of any one of these types is proportional to the inverse n power of the distance, as follows:

| | |
|---|---|
| Simple coil (solenoid) | n = 3 |
| Binocular pair | n = 5 |
| Neutroidal pair | n = 7 |

This comparison shows that the Neutroid was the most effective in containing the magnetic field (short of the toroid). The outer coil also provided some shielding of the electric field.

A year after making this invention, I learned of coils in cans, as in Fig. 1(b). Our competitor, Radio Frequency Labs. (RFL) in Boonton, N.J., had a different group of licensees, which included our licensee Stromberg-Carlson. Through them we learned the RFL latest practice of shielding by enclosing the coils in copper cans mounted on a low chassis of metal.

On 260709, I made some measurements of a coil in a copper can. I recall vividly my surprise on discovering that the apparent RF resistance of the coil was actually decreased by enclosing it in the can. Also the inductance was




10.2 Fig. 1 — Comparable coils with small external field.

decreased (as I expected). The net result was little change of the magnetic-loss power factor (p = 1/Q). Furthermore, the copper can provided complete shielding of both magnetic and electric field.

I soon saw the reason for the decrease of coil resistance by putting it in the can. In a solid wire, the skin effect causes the current to concentrate on any part of the surface that may be exposed to the greater density of magnetic flux. In an open coil, the flux density is greater on the inner face of the wire, so the current concentrates on this face. Surrounding the coil with a can serves to concentrate the flux density also on the outer face. Then the total current in the wire utilizes more of its surface, thereby decreasing the coil resistance. The effective resistance includes also some dissipation in the can, but that makes a minor contribution. See Fig. 1(b).

It became clear to me that the Neutroidal Coil behaved much like a shield can, but fell short in the degree of shielding. The can also offered economy in cost and mechanical protection. We adopted coils in cans, on a low metal chassis. It was a major advance in technology.

It is interesting that one other inventor also devised a pair of opposing coaxial coils nearly like the Neutroid. He was Lester L. Jones, whom we encountered in contests involving some Neutrodyne features.

## 10.3 AF Power Amplifier 1929.

An invention I made in 1929 deserves attention although it never found a useful application. Its motivation was negative, and shortly evaporated. Its concept was remarkable, being a reversal of current practice. Its performance was excellent, and could have been improved with redesign of the triode to fit this application. It lacked the standby economy of power that was typical of its predecessor.

The AF power amplifier was used to deliver power to a loudspeaker for reproduction of sound. Freedom from distortion required "linearity" which was not typical of an amplifier tube (then the triode). Linearity was obtained from a pair of triodes in a balanced circuit termed a "push-pull" amplifier. In the push-pull circuit, economy of power was obtained in the socalled "class B" operation. In that mode, the standby current in each tube was small, and each tube delivered one "half-wave" of output current. A "low-mu" triode was used, which required a large negative bias on the grid to hold down the standby current. This bias prevented grid current so no power had to be delivered from the driving circuit (the preceding stage).

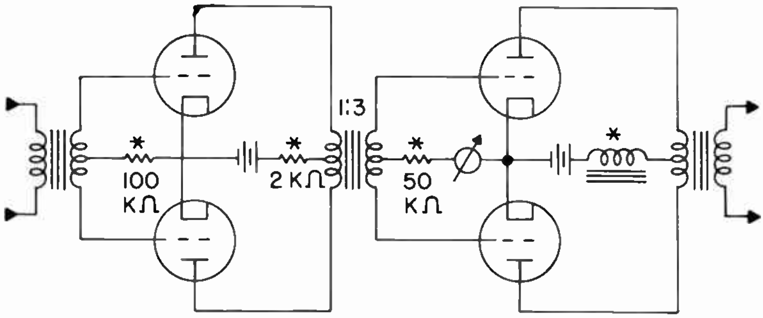
In the traditional push-pull amplifier (still the common practice) the input and output circuits were center-tapped transformers which effectively coupled both sides in parallel. While either tube was delivering its half-wave of current, the other drew no current and hence was harmless.

The Telephone Co. held a patent which contained basic claims to the use of a negative grid bias in a vacuum-tube amplifier. That patent was being used to exclude competition in the growing field of sound movies ("talking pictures"). The common push-pull amplifier required such a grid bias.

The Farrand Mfg. Co. and its principal, Clair L. Farrand, relied on our patent attorneys, PDME and specifically Willis H. Taylor, Jr. That company made one of the current loudspeakers and wanted to make a power amplifier to drive it. They wanted to avoid the grid-bias monopoly. Taylor brought them to me with this problem. He was aware that I had been working on an amplifier without grid bias.

My background dated from early 1925, my research work for credit at GWU. By an unusual method of analysis, I found a way to operate a triode as a power amplifier without grid bias. It was a medium-mu tube, the only one then available. I provided a moderate amount of input power, enough to drive the grid positive as much as negative. This was a "class A" amplifier, which was nearly linear without pairing tubes in a push-pull relation. In the Hoboken lab, I designed an AF power amplifier of this type for use of the common low-power triode. It was shortly superseded by the low-mu triode with large bias. Also it became common to use the medium-mu triode in a push-pull pair.

When the no-bias problem was brought to me by Taylor and Farrand, I immediately devised a way to use the medium-mu triode without bias in a




10.3 Fig. 1 — A push-pull AF amplifier with series operation of each pair of tubes.

push-pull amplifier. See Fig. 1. Instead of operating both tubes effectively in parallel, I proposed to operate them effectively in series. To this day, I have not learned of anyone else making this approach in a push-pull amplifier.

In a few days, beginning 290207, I entered in my notebook this concept and various circuits for its implementation. They were essentially like Fig. 1, which contains some refinements dating as late as 290521. During this period, numerous experiments were made in the Farrand laboratory in N. Y. City. (I was then working in our N. Y. lab.)

Fig. 1 shows a push-pull amplifier in which the input or output circuit of each stage is connected between the pair of grids or plates. Each common connection (*) carries little AF current.

In the prior art, a push-pull amplifier would have had a direct connection (or low impedance) in each common connection (*) and also a negative bias in the grid connection. That was parallel operation, the prevalent concept.

Half-wave operation in either side of my circuit was the result of two relations:

(1) The positive grid drew current and forced most of the voltage negative onto the other grid;
(2) The plate with the positive grid presented low resistance and acted as a return for the current variation in the other plate circuit.

Very little power was required to drive this grid circuit. Linear operation was assured by the symmetry as in any push-pull amplifier.

My patent applications were filed 290716 and they suffered no restrictions in the Patent Office.

For reasons I do not recall, it became unnecessary for Farrand to use my invention. Its only publication was the patents.

[A] H. A. Wheeler, U. S. Patents.

| | Pat. No. | Issued / Title | | |
|---|---|---|---|---|
| (1) | 1878740 | 320920 / | 290716 | Push-pull amplifier. |
| (2) | 1878741 | 320920 / | 290817 | Push-pull amplifier. |
| (3) | 1878742 | 320920 / | 290817 | Thermionic amplifier. |
| (4) | 1878743 | 320920 / | 290817 | Push-pull amplifier. |
| (5) | 1904185 | 330418 / | 270217 | Low-frequency amplifier. |

Note (5) The earlier work on AF amplifier without bias.

Note (1-4) The push-pull AF amplifier without bias.

## 10.4 The Consulting Group 1939-40.

Shortly after the research lab moved from Bayside to Little Neck, we organized a Consulting Group for interchange of ideas between our leaders and seven prominent professors in our field. We held 16 meetings from 391012 to 400606, every two weeks on Thursday, 4:30 — 7 PM, followed by dinner in a private dining room at the Hidden House in Great Neck. For each meeting, I sent the agenda in advance, I served as Chairman, and Curtis reported on the discussion.

The first meeting was attended by our leaders and all "senior engineers":

W. A. MacDonald, Vice President
D. E. Harnett, Chief Engineer
H. A. Wheeler, V.P. and Chief Consulting Engineer
N. P. Case, Engineer in charge of N.Y. lab
L. B. Dodds, Head of Patent Dept.
J. F. Farrington (on leave)
L. F. Curtis
R. L. Freeman (Ph.D., Stanford U.)
R. C. Hergenrother (Ph.D., Cal. Tech.)
A. V. Loughren
L. R. Malling
J. C. Wilson

The visiting Professors were:

From Moore School of E.E., U. of Pa.:
Carl C. Chambers
Knox McIlwain
From Stevens Tech.:
Herbert C. Roters
William L. Sullivan
From Columbia U.:
John B. Russell
From Cornell U.:
Vladimir Karapetoff (retired)

Their attendance was notable; all were present at 8 of the 16 meetings. Their arrival time was uniform because they came by train to Little Neck and we met them at the station. Our representation was usually most of the above and some others invited in view of their particular interest in the topic under discussion. We skipped 400414 because Ruth and I were taking a cruise in the Caribbean; we were in Havana on that day.

The Armstrong system of wideband FM was developing rapidly so it was the topic of most interest to the Group. Therefore it occupied 6 of the 16 meetings. Mainly, the Group provided a sounding board and discussion base for problems and ideas with which we were concerned.

Some of the professors were leaders in the theory of frequency-selective networks, so several meetings were devoted to such topics.

In meeting No. 15, Prof. Brainerd introduced us to their "differential analyzer", which was one of the first analog computers for solving an integral equation. It was an ingenious mechanical device which yielded the solution in the form of a graph on paper. It was soon to be superseded by the electronic analog computer which played a major role in the early progress of simulation and guided missiles. He showed us the solution for the oscillation in a resonant circuit with periodic variation of one parameter (such as capacitance). It went far beyond the usual solution for a circuit with fixed parameters.

One result of this Group was Knox McIlwain joining Hazeltine for war work and staying some time afterward. He and I became close friends, and we learned a lot from each other.

[A] Meetings of the Consulting Group

| Meeting No. | Date | Report No. | Date | Author | Topic |
|---|---|---|---|---|---|
| 1 | 391012 | 1033W | 391017 | LFC | FM |
| 2 | | 39 | | | FM |
| 3 | | 45 | | | Tubes |
| 4 | | 51 | | | TV |
| 5 | | 59 | | | FM |
| 6 | | 63 | | | FM |
| 7 | | 65 | | | Networks |
| 8 | | 71 | | | TV |
| 9 | | 76 | | | FM |
| 10 | | 81 | | | Receivers |
| 11 | | 90 | | | |
| 12 | | — | | | FM |
| 13 | | 1102W | | | Magnetics |
| 14 | | 03 | | | |
| 15 | | 10 | | | |
| 16 | 400606 | 11 | 400617 | LFC | |

### 10.5 Terman's Handbook

My first meeting with Prof. Frederick E. Terman was 350613 when he visited me at the Bayside lab. I met him at the LIRR station. He later confided that he was surprised to see me because he had expected me to be an elderly man with a beard. He was familiar with the few papers I had published. His visit was one of many he made to various workers and laboratories in collecting material for the handbook he was preparing.

He was already known in the profession for his outstanding textbook, "Radio Engineering" (1934) which he was using at Stanford U. Otherwise, he had published little and had seldom come to our meetings in the East.

Fred Terman and I became close friends from our first meeting. Each of us had an acute appreciation of the other's work. He was three years older, and more thoroughly prepared with Ph.D. from Harvard U. under the famous Dr. G. W. Pierce.

Fred visited our lab at Bayside again 380708 and at Little Neck 400127. In the meantime, we employed his former student, Bob Freeman (Stanford Ph.D. 1934). Terman was elected President of IRE for 1941 while I was a Director (1940-45) so I saw him often. He was a petition candidate, and became the first President from the West since 1915. My first visit to him at Stanford was 410528, on my first airplane trip to the West Coast.

During this period, he was preparing the "Radio Engineers' Handbook" (McGraw-Hill 1943). He was much interested in our developments so I gave him reprints and also some material not yet published. As a result, my name has the second greatest number of credit lines. The material he used came from our published papers (1928-42): [W3] [W7] [W10] [W14] [W15] [W16] [W18] [W19] [W21] [W24] [W27] and 9.8 [C] Harnett & Case.

After it was published, I reviewed this handbook. (Proc. IRE, vol. 31, p. 649; Nov. 1943.) I have found it extremely useful even to this date, for its wealth of information and its many references to sources.

# Section 11. Conclusion

## 11.1 Conclusion.

The preparation of this account has given me a perspective I have not had before. My life and the Company before World War II enjoyed a continuity which was remarkable and was beneficial to my career. Opportunities were exploited and some obstacles were surmounted.

The events which most influenced my future might seem to have happened as if by plan. Actually most of them were related to circumstances that were accidental. They set the course of my life while I exercised little choice beyond adaptation. One might say merely that I did not reject opportunities or resist my father's advice.

Here I shall give a chronicle of some events and circumstances which together contributed to a continuous growth of my career and Hazeltine Corporation. They are grouped under headings which are intended to emphasize their collective influence.

**Mitchell**

- For my health, I grew up in a small town with invigorating climate and clean air.
- For early education, our schools were outstanding.
- Modular construction toys (the predecessors of A. C. Gilbert's Erector) came out just when I was ready for the elements of structures and mechanisms.
- At age 13, I made an early choice of vocation, engineering and specifically wireless (later termed radio). That field developed with a timing well suited to my career development.

**Washington**

- We moved to Washington just when I was outgrowing the resources of Mitchell.
- The new Central High School opened just when I was ready.
- The Public Library and the Old Museum of inventions were my introduction to technology.
- The four-year scholarship to GWU made it easy for me to continue while living at home.
- By taking the new course in Physics major and Math minor, I had to take all the courses which were designed for evening students toward a Master's degree. The pace was easy for me so I had spare time.
- Living at home, I had the freedom of my workshop for radio experiments and my amateur station.
- Washington had a Radio Club of amateurs and the National Radio Institute, an evening course where I learned to be an operator.

## My Father in the Government

- My father, in the Dept. of Agriculture, was fascinated by my radio work and happened to see an opportunity for starting the Radio Market News Service as a service to farm communities.
- As a result, my father was appointed as delegate from that Dept. to the National Radio Conferences conducted by Herbert Hoover, then Secretary of Commerce.
- In those meetings, my father met the leaders in radio, including Dr. Dellinger and Prof. Hazeltine.
- My father took me to those meetings and introduced me to those leaders.
- Through Dr. Dellinger, Section Head of the Radio Laboratory in the Bu. of Standards, my father got me a job there as laboratory assistant for the summers just before and after my first year of college.
- We happened to live near the Bu. of Standards.

## My Invention of Neutralization

- In the Radio Laboratory, I used the latest receivers using the Armstrong regenerative circuit.
- Furthermore, I learned of the need for a TRF amplifier and the problem of internal feedback in the triode if used for that purpose.
- Working at home, I found a solution to this problem, by a circuit which came to be known as "plate-circuit neutralization"; then I assembled and operated a TRF amplifier with this circuit, the first time in history.

## My Meeting with Hazeltine

- Six weeks later, my father took me on a trip, my first to N.Y. State. We stopped in Rochester for me to advise the Post Office on installing an antenna and receiver for market reports. Then we went to New York City where my father had business in Hoboken. There we accidentally met the Professor at a restaurant during lunch and he invited us to call at his office in the nearby Stevens Institute of Technology.
- When I told Prof. Hazeltine about my neutralized TRF amplifier, he showed me his earlier patent application on the same principle. That did not disclose its application to a TRF amplifier as required in a broadcast receiver.

### The Neutrodyne

- It happened that RCA refused to license any more companies to make and sell the Armstrong regenerative circuit, which was common in broadcast receivers.
- A group of unlicensed companies banded together to form the Independent Radio Manufacturers (IRM) in an attempt to find some way to compete without a license.
- They happened to engage PDME as patent adviser, and a member of that firm as their Secretary (Walter Russ).
- Hazeltine's patent attorney (Taylor) happened to be a member of the same firm.
- They brought IRM to the Professor to see if he could design a competitive receiver without regeneration. This happened shortly after my first visit to his office.
- These circumstances led Hazeltine to design the neutralized TRF receiver which came to be known as the Neutrodyne.

### My Agreement with Hazeltine

- Shortly after my first visit to the Professor, I invented a different form of neutralization which came to be known as the "capacity bridge".
- Next, the Professor visited me in Washington and proposed that we "join forces", especially in view of my different invention. I was most receptive, so we made an agreement under which I was to receive 10% of his royalties. Also he invited me to work in his laboratory the next summer (after my second year in GWU).
- The generous return from his royalties gave me much freedom to work at home and to plan ahead.
- My first summer in Hoboken was my first job away from home and my first creative employment.

### The Company

- The Neutrodyne was an immediate success and provided the IRM with a superior product they could exploit without an RCA license. (Later the Courts ruled that the Neutrodyne was subject to an RCA license, but then such a license became available and the IRM disbanded.)
- Taylor perceived the need for a company to manage the Hazeltine licensees and to provide laboratory support, also to build for the future. To this end, he formed the Hazeltine Corporation, which took over Hazeltine's rights (including mine).
- One of Taylor's major contributions was the selection of MacDonald as Chief Engineer. It happened that they were friends (and neighbors on

Long Island) after association in the Paris laboratory of the Signal Corps around the end of World War I. No one could have foreseen the key role that MacDonald was destined to play in the future of the Company.

- MacDonald proved to be well suited for dealing with the engineering management in our licensee companies. Also he was able to obtain in the Company the support we needed for laboratory facilities and staff. Maintaining our royalty income and our investment in engineering was a major accomplishment in the transition after the Neutrodyne and in our survival through the Depression.
- It was this environment that enabled our staff to provide improvements. It enabled me to work on problems of my choice, with close support by our staff. I was encouraged to file patent applications, yielding more than a hundred U.S. Patents on my work before the war.

**New York City**

- Hazeltine, Taylor, and hence the Company, happened to be located in the New York area. New York was the focal point of business in general, and especially of the radio profession. The location contributed much to our growth and success.
- In my personal development, I benefited much from my proximity to the IRE, in the ways of learning, meeting the leaders of radio, and participation. As the headquarters of IRE, New York was its center of activities. New York had Columbia U. (the headquarters of Radio Club of America), CCNY (the birthplace of RCA laboratories, RCA, American Tel. & Tel. Co., International Tel. & Tel. Co., and Bell Telephone Laboratories. The monthly meeting was not just the N.Y. Section, but a meeting of the Institute, usually conducted by the President. I was nearby to serve on committees, to attend meetings, to present papers, and to be elected a Director.

**My Opportunities**

- The nature of our Company's business and the character of our work gave me an unusual number of opportunities for personal contacts.,We had frequent visitors from our licensees and other companies.
- My continuous employment, from the beginning of the Company, enabled my completion of seven years of college while contributing to the progress of the Company. It enabled my early marriage and my comfortable home wherever I was located. It offered me real opportunities for creative work through the troublous years of the Depression. It was an excellent foundation for my later career.

The phenomenon of Hazeltine Corp. before World War II could not happen again. The practices of that day enabled the Company to thrive as an engineering organization supported entirely by patent royalties. The technology of radio and television was simple enough for a small, independent group to contribute substantial innovation. We made the most of that opportunity.

**Acknowledgments**

Of the two dozen names accorded personal sketches, one dozen are surviving and have been helpful to me in preparing this account. To these and others I wish to express my gratitude for stimulating conversations and correspondence, all of which has contributed to my story.

John F. Dreyer, Jr.
Lucien J. Troxler
Charles E. Dean
Vernon E. Whitman
J. Kelly Johnson
Madison Cawein
William O. Swinyard
Laurence B. Dodds
Rudolph C. Hergenrother
Arthur V. Loughren
Robert L. Freeman
Bernard D. Loughlin
Clyde K. Huxtable
Rudolph E. Sturm
William F. Bailey
William B. Wilkens
Leonard R. Malling
Ernest G. Curran
Benjamin F. Tyson

I am grateful to Bernard D. Loughlin and Kenneth P. Robinson for reading and commenting on the draft. The figures were skillfully prepared by Walter Anderson and Joyce Gregory. Especially I am indebted to my secretary, Geraldine Fargnoli, for her thoughtful work on the text.

This work and its publication have been made possible by the support and encouragement of the management of Hazeltine Corporation, to whom I owe a debt of gratitude that cannot be expressed in words.

# Section A. Appendix.

## A.1 Abbreviations.

Proper Names

| | |
|---|---|
| AIEE | American Institute of Electrical Engineers |
| AK | Atwater Kent Manufacturing Co. |
| ARRL | American Radio Relay League |
| CCNY | City College of New York |
| CHS | Central High School, Washington, D.C. |
| GE | General Electric Co. |
| GN | Great Neck, N.Y. |
| GWU | George Washington University |
| IEEE | Institute of Electrical and Electronics Engineers |
| IRE | Institute of Radio Engineers |
| IRM | Independent Radio Manufacturers, Inc. |
| JHU | Johns Hopkins University |
| KDKA | Radio Broadcasting Station, Pittsburgh, Pa. |
| LN | Little Neck, N.Y. |
| NAA | Navy Radio Station, Arlighton, Va. |
| NAM | National Association of Manufacturers |
| NDRC | National Defense Research Council |
| NY | New York, N.Y. |
| OSRD | Office of Scientific Research & Development |
| PDME | Pennie, Davis, Marvin & Edmonds |
| QST | Magazine of ARRL |
| RCA | Radio Corporation of America |
| RFL | Radio Frequency Laboratories, Inc., Boonton, N.J. |
| RMA | Radio Manufacturers Association |

Word Groups

| | |
|---|---|
| AF | audio-frequency |
| AFC | automatic frequency control |
| AGC | automatic gain control |
| AVC | automatic volume control |
| BO | beating oscillator |
| BW | bandwidth |
| CRT | cathode-ray tube |
| FM | frequency modulation |
| GCN | grid-circuit neutralization |
| HP | highpass |
| IF | intermediate-frequency |
| LO | local oscillator |
| LP | lowpass |
| PCN | plate-circuit neutralization |

| | |
|---|---|
| RF | radio-frequency |
| SSB | single-sideband |
| SSG | standard-signal generator |
| TRF | tuned-RF |
| TV | television |
| VF | video-frequency |
| VT | vacuum tube |
| VTVM | VT voltmeter |
| XPS | expanding selector |

### Units

| | |
|---|---|
| c | cycle |
| Kc | kilocycle |
| Mc | megacycle |
| $\mu$h | microhenry |
| pf | picofarad |

## A.2 Sources

To support or to supplement my recollections, I have relied especially on some sources which fall into these categories:

- Wheeler Collection (WC) in the library of the Hazeltine TIC (Technical Information Center) which is across the hall from my office in Greenlawn Bldg. 1. This collection is about 3000 volumes I have collected since World War II. About one half are my property and the other half Company property. It is maintained under my direct supervision as a research collection, not to be borrowed. Most of it is valuable and irreplaceable. It covers the fields of my principal interests and activities, and some lines are kept up-to-date (especially the binding of many periodicals).
- The Hazeltine Collection in the TIC library, which contains more books and periodicals which are less concentrated in my fields of interest.
- Company records, especially laboratory notebooks and the ribbon copies of laboratory reports. They are complete for the period of this account (1924-42). They are stored under control of TIC.
- Several Company collections I have organized in binders or bound volumes, which are kept in my office. They have been related to my work.
- Personal diaries, photographs and other records, which are kept in my study at home.
- Inquiries to my friends who were active in the period before World War II, inside or outside the Company.

**The Wheeler Collection (WC)** comprise 28 stacks of bound volumes in these categories:

- Bound volumes of periodicals and some other collections.
- Books arranged on shelves by subject code, each code alphabetically by authors.

It has a card index in the usual form, by author, subject and shelf code. Each book is marked with a uniform label. The collection is registered by serial number, with my secretary who serves as librarian for this collection. It occupies a partitioned section of the TIC library.

**The bound periodicals** include the following series:

Proc. IRE, 1920-62, when succeeded by Proc. IEEE.
Trans. AIEE, Cum. Index, 1922-38, 1939-49, 1950-59.
Proc. Radio Club of America, from 1909 (nearly complete).
Wireless Engineer (British) 1923-62 (beginning to end).
Bell System Tech. Jour., 1922 to date (from beginning).

RCA Review, 1936 to date (from beginning).
Radio News, 1921-34.
Electronics, 1938 to date (technical articles).
General Radio Experimenter, 1926-43 (from beginning).
Jour. Applied Physics, 1931-68 (from beginning).
Reviews of Modern Physics, 1929-68 (from beginning).

**Proceedings of IRE** includes these features bound in the following annual volumes:

1943 Cumulative index 1913-42
1947 Cumulative index 1943-47
1954 Cumulative index 1948-53
1960 Cumulative index 1913-60
Separate volume, Yearbooks 1929-32, Members, Standards 1928, 1931.
1942 Yearbook, Members
1946 Fellow Biographies (first time)
1948 Fellow Biographies, Members.
1949 Fellow Biographies, Members.
1951 Fellow Biographies, Members.
1952 Fellow Biographies, Members.
1953 Fellow Biographies, Members.
1954 Fellow Biographies, Members.
Separate volume, Yearbook 1963 (last), Fellow Biogs., Members.

An annual review of progress was a feature of the following years. The pages are given for the part prepared by the Tech. Com. on Radio Receivers.

| | | | | | |
|---|---|---|---|---|---|
| 1935 | pp. 415-447 | 1940 | pp. 99-125 | 1945 | pp. 143-155 |
| 36 | 376-408 | 41 | 89-103 | 46 | 164-184W |
| 37 | 165-218 | 42 | 57- 71 | 47 | 399-424 |
| 38 | 277-307 | 43 | 127-131 | 48 | 522-549 |
| 39 | 161-183 | 44 | 125-129 | 62 | 793-798 |

The last item was this topic in the fiftieth anniversary issue.

**Proceedings of Radio Club of America** includes these features bound with the following annual volumes:

1929 Yearbook, members.
1930 Yearbook, members.
1931 Yearbook, members.
1934 25 Anniv. Yearbook, history, member biographies.
1937 Members.
1948 Yearbook, members.

1950 Yearbook, members.
1950 IBCG Commemorative Issue
1954 Yearbook, member biographies.
1959 50 Anniv. Yearbook, history, member biographies.
1964 Yearbook, member biographies.
1973 1975, 1977, 1979, members.

1959 includes these names from Hazeltine Corp:

Barber, Alfred W.
Binns, Jack
Crawford, John D.
Dean, Charles E.
Harnett, Daniel E.
Hazeltine, Louis Alan
Hirsch, Charles J.
Johnson, J. Kelly
Langley, Ralph H.
Lewis, Harold M.
Loughren, Arthur V.
Lyon, Walter
MacDonald, Wm. A.
Swinyard, W. O.
Walsh, Lincoln
Wheeler, Harold A.

**Historical books** (before World War II) are found on shelves of most of the subject codes, but especially RADIO. This tendency is a result of the wider scope of earlier books on radio, in contrast to the specialization of later books.

**Laboratory Notebooks** that I used individually are all located in my office bookcase. Also I used notebooks assigned to specific jobs or licensee companies. Those are kept in storage under control of TIC.

In 1917-24, I used some small notebooks with flexible brown cover.

1917-21 Formulas and math.
1923-25 Formulas and math.
211202-240215 Washington, radio log.
230611-230912 Hoboken first summer.

From then until 1930, I used bound notebooks with hard cover, which came to be numbered in the following series. (*denotes Hazeltine standard notebook, 1924-30, hard black cloth cover, red leather binding, blue quadrille ruling.)

1. 211231-221030 Washington, radio log.
2. 221211-240215 Washington, circuit descriptions.
3. 230613-230910 Hoboken, summer 1923.
4. 231118-241213 Washington, Hoboken.
5. 290107-260726 Hoboken.
6.* 250703-260701 Washington, Hoboken 1925-26
7. 260226-280513 JHU.
8.* 260701-270820 Hoboken (Chicago).
9.* 270820-280906 Hoboken (Chicago).
10. 280513-280617 JHU.

11.* 280906-290220 Hoboken.
12.* 290220-291214 N.Y. lab.
13.* 300131-301024 N.Y. lab.

From 1930 until it was superseded in 1942, I used the carbon-copy notebook prescribed in the Company. It had a hard cover with black cloth binding, blue quadrille ruling, and carbon copy on yellow sheets. I have retained the carbon copies of my notes in clip binders with flexible brown covers.

| | | | |
|---|---|---|---|
| 150 | 301028-310425 | 240 | 360224-380909 |
| 164 | 310427-310821 | 241 | 360319-370513 |
| 171 | 310903-320222 | 242 | 360412-371011 |
| 180 | 320301-321017 | 510 | 371126-381004 |
| 190 | 321006-330731 | 533 | 381014-390328 |
| 210 | 330721-341031 | 540 | 390330-400630 |
| 230 | 341121-360224 | 710 | 391012-420512 |

**Wheeler Report Binders** is the name of a collection of Hazeltine reports relating to my work, many written by me. It is located in my office bookcase. The reports are collected by subjects in clip binders with hard covers and black cloth binding. The contents are carbon copies of the reports, supplemented by reprints of published papers and patents. The following list includes those generated before the war. An index binder provides tables of contents to the collection.

Vol. 1 — Job 55W, All-Wave Systems, Part I
2 — Job 55W, All-Wave Antenna Systems, Part II
3 — Job 12W, Image Suppression
4 — Job 12W, Oscillator-Modulators
5 — Job 94, Filters, Part I
6 — Job 94, Filters, Part II
7 — Job 94, Filters, Part III
8 — Job 94, Filters, Part IV
9 — Jobs 150, 170; Television Scanning
14 — XPS Receivers
15 — Job 180, Frequency Modulation
16 — Jobs 16W and 17W, Diode Detectors and Automatic Volume Control
17 — Single-Sideband Receivers
18 — Job 250, Reference Material
19 — Miscellaneous Reports
29 — Test Equipment (Bayside Reports)
30 — Television Principles
31 — Miscellaneous Reports (Bayside and Little Neck)
32 — Miscellaneous Reports

**Annual Reports** of Hazeltine Corporation are collected in a few clip binders with flexible brown covers. These are located in my office letter files. They form a complete set from the beginning (1924) to date. For the first 25 years, they gave little more than the required financial abstract of Company affairs. Even a major event was usually reported only in general terms.

**Technical Monographs** have always come from various sources that may not be organized for ease of access in future years. Typically one is too long for the usual periodical, perhaps a college thesis. The same has been true even of articles which were published long ago and/or in a medium which was not well organized for reference. I have accumulated two collections which have been found useful.

- Monograph Binders form a file of various old articles, collected in clip binders by subjects of particular interest to me. These are located in 2 drawers of my office file cabinets.
- Bound volumes of reports from some prolific sources after the war. They are kept in my WC library, and mostly are identified by a gray cloth cover. Principal sources are MIT (RLE), Harvard (RRL), PIB (MRI), Sperry, JHU, SRI, NBS, U. of N.M.

**Bound Archives** of all publications are enclosed in order of date, in volumes with red cloth cover. Each has a complete table of contents with reference to source. Author biographies are included.

"Papers by Hazeltine Corporation Engineers".
1918-1938. Small size (IRE until 1938).
1929-1943. Large size (IRE from 1939).

"Papers by Wheeler and Associates".
1944-49, 1950-56, 1957-64, 1965-69. Large size.
Includes Wheeler Labs., some patents, name and subject indexes.

**Wheeler Patents** (assigned to Hazeltine Corp.) are collected in these forms, kept in office bookcase:

- U.S. Patents (about 180) in numerical order, in 3 clip binders with contents in terms of short titles.
- British Patents in 2 clip binders, ditto.
- U.S. Patents, grouped by subject, in 4 volumes, red cloth binding.

**Abstract Cards** from a collection of about 3000 items on 5" × 8" cards in 3 file drawers located in my office. They are classified by subjects of my principal interests, about 1920-60. They are white cards for publications, yellow for patents, and blue for Company reports. I designed a printed form for each color. They provide an extremely valuable classified sampling, especially for my reference. These are some of the principal subjects.

| | | | |
|---|---|---|---|
| Amplifiers | Detectors | Lines | Oscillators |
| Antennas | Fields | Modulators | Scanning |
| AVC | Filters | Networks | TV |

**Personal Diaries** which I have kept from 1920, are small annual volumes of daily notes. They contain cryptic entries which give an indication of all my activities. They are located in the bookcase in my study at home. They provide a continuous record, except for the latter part of 1929 and the year 1930. Some bridging of this gap has been possible from a dated photograph album and my complete series of bank check stubs. These diaries have enabled me to reconstruct many events in time, related to my activities and the people involved. In the back of each volume is a list of some dates having special significance. I have transcribed a continuous abstract of professional highlights over the years (in script) and also selective abstracts for the most active periods of Hazeltine neutralization and diode AVC. These have made a major contribution to my historical accounts.

**Company Archives** of various kinds are collected in the Headquarters office, in my office and in my files at home. The Patent Dept. has an orderly collection of all U.S. patents assigned to the Company. They are arranged in post binders in numerical order, and each one is tabbed with number and name of inventor.

**Hazeltine Laboratory Reports** were numbered in three continuous series, until some time after World War II. The system was initiated by MacDonald in the first laboratory. The ribbon copies were preserved, and are still kept in storage under control of the TIC. The few times I have called for specific numbers, they have been found and loaned to me. For the most part, I have relied on reference to index lists in two ring binders with canvas cover, kept accessible in TIC. Each number series is listed with date, title, and author (except the earlier numbers). The three series are:

From 1924; the Hoboken-NY series started by MacDonald.
From 1930, also the Bayside-LN series, with suffix W.
From 1937, also the Chicago series, with suffix Y.

**Names** of Company employees and others can be identified and documented from various sources. Here are some I have found helpful. Sometimes a survey of most of these sources has been required to put together even a brief sketch on one name.

- Who's Who in America, and other biographical directories, especially those for science and engineering.

- Terman's Handbook (1943) has a name index of 800 prominent authors in radio; they can be traced through the references to publications.
- Proc. IRE Fiftieth Anniversary Issue (1962 MAY) has sketches on 180 authors.
- Proc. IRE, from 1928, included sketches on the authors each month; also photos, from 1939.
- Proc. IRE, Cumulative Index, or any other including a name index of authors, gives a lead for tracing.
- IRE Yearbook (from 1946) has included Biographies of Fellows (some or all of: birth, college degrees, IRE service, awards).
- Radio Club of Amer. has in some years published sketches of all members.
- Book of biographies or history.
- Member directory (IRE, AIEE, etc.) for any year; past years may not be accessible.
- Hazeltine records. Authors in report index. Annual reports. Employee list 1924-mid'35. Telephone directory 440722.
- Hazeltine-IRM advertisement (late 1926 or early 1927) reproduced herewith.

# The men behind Neutrodyne

*Engineers, physicists, mathematicians, who have placed Neutrodyne in its present position of leadership—and who will keep it there*

IN SPITE of the fact that the Neutrodyne receiver of today represents the highest attainments in radio development, no expense is being spared, in the continuance of a policy of constant improvement.

At considerable expense the Hazeltine Corporation and the fourteen companies licensed to manufacture Neutrodyne apparatus maintain a large staff of technical men who devote their entire time to research and experiment in radio. These men, whose names are listed below, are the men who have brought Neutrodyne to the position of leadership it enjoys today. And these are the men whose ideas, designs and inventions will keep Neutrodyne in the forefront in the years to come.

These men are determined that Neutrodyne receivers will always combine the best in sensitivity, selectivity, ease and economy of operation, volume and perfection of tone reproduction.

PROFESSOR L. A. HAZELTINE: M.E. Fellow A.I.E.E. Fellow I.R.E. Fellow American Physical Society. Formerly professor in charge of the Electrical Engineering Department of Stevens Institute of Technology. Inventor of the Neutrodyne Radio Receiver.

R. W. ACKERMAN: C. E. Schools of Mines, Engineering and Chemistry, Columbia University. Active service U. S. Army Engineers, 82nd Division.

LEWIS M. CLEMENT: B.S. in E.E. F.I.R.E. Assistant Chief Engineer of high-power Marconi stations in Honolulu and San Francisco during construction and early operation. Nine years with Bell Telephone Laboratories in charge of radio receiver and special developments, including government transmitter, receivers, secret systems, etc.

J. W. FINK: M.E. Specialist in audio frequency and loud speaker development.

DONALD O. FRIEND: Massachusetts Institute of Technology.

S. W. GILFILLAN: Stanford University.

VIRGIL M. GRAHAM: I.R.E. A.I.E.E. University of Rochester.

LELAND H. HANSEN: I.R.E. Designer of radio apparatus since 1916. Formerly with American Marconi Company, 1911-1925. Active service U. S. Signal Corps in France.

FRANK A. HINNERS: F.I.R.E. Pratt Institute. Associated with radio development in all its phases since 1909. Among earliest workers on quenched spark telegraphy in this country. In charge transmitter design supplied U. S. Government during war. Chief Engineer extensive overland radio telegraph system.

F. E. JOHNSTON: A.I.E.E. Long Beach, Cal., Polytechnic and Sorbonne and Ecole Superieur de Electrique, Paris; First Lieut. Radio Intelligence Section U. S. Signal Corps. Croix de Guerre and U. S. Citation. Assistant Engineer in charge construction of high-power station at Warsaw, Poland. Engineer in charge of Riverhead, L. I., Transatlantic Radio Station.

C. T. JOHNSTON: E.E. University of Iowa.

THOMAS S. LEOSER: I.R.E. Lehigh University.

W. W. LINDSAY, JR.: I.R.E. Technical School, Hanover, Germany. Post-graduate work, Columbia University.

R. E. MACDOWELL: B.S. in M.E. and E.E.E. Electrical experimental work, Radio Telephone Officers' School, U. S. Government.

R. MACGREGOR: E.E. Provenside Acad. of Electrics, Glasgow, Scotland. Exp. work in British Navy, two years.

W. A. MACDONALD: I.R.E. University of Paris. Lieut. Sig. Corps U. S. Army in France. Engineer in charge development of airplane radio transmitter equipment Signal Corps U. S. Army (four years). Consulting Engineering Dept., Radio Corporation of America.

RAY H. MANSON: E.E. I.R.E. A.I.E.E. Member S.A.E. University of Maine. Chairman Technical Committee. Radio Sect., N.E.M.A. Committee on Communication. Chairman Electro-Acoustical Sub-Committee of I.R.E. Standardization Com. Member Bur. of Standards, Radio Advisory Committee. Member Component Part Committee of Tech. Sub-Committee, A.E.S.C. Sect., Committee on Radio.

BENJAMIN F. MIESSNER: I.R.E. Member American Soc. to Advance Science. Purdue University. Authority on electrical acoustics. Engaged in radio research since 1908.

JOHN W. MILLION, JR.: A.B. University of Michigan. Instructor, Mathematics and Physics, Des Moines University. Graduate in research, Washington University, St. Louis, Mo. Graduate work, Columbia University. Development work in vacuum tubes and radio reception in Bell Telephone Laboratories, July, 1923, to Jan. 1, 1925.

WILLIAM J. MURDOCK: Designer and manufacturer of radio apparatus since 1904.

J. N. NICHOLS: B.S. in E.E. First Imperial Light Science School, Petrograd, Russia. Engineer Instructor, Russian Imperial Army, and later French Air Service.

J. A. NEILSON: Copenhagen Technical School, Denmark. Research work in Danish Naval Radio Service.

BENJAMIN OLNEY: Electro-Acoustical Engineer. Specially engaged in audio frequency amplification and speech reproduction research.

LAWRENCE S. PHILBRICK: Phillips Andover Academy, Massachusetts Institute of Technology.

F. F. PRELAG: B.S. Vienna, Austria. Engaged in experimental research work in radio in Europe and this country.

R. X. RETTENMEYER: M.S. B.S. in E.E. Formerly in the Bell Telephone Laboratories in charge of work on high quality carrier broadcast systems.

A. W. SAUNDERS: E.E. Formerly in Bell Telephone Laboratories in radio receiver development design, including receivers used by U. S. Coast Guard.

ROGER W. SEMONS: R.E. Assistant Instructor, Harvard Radio School, 1917-22. Design Eng. of broadcast stations.

F. J. STRASSNER: B.S. in E.E. and E.E. Formerly with Bell Telephone Laboratories. Engaged in transmission maintenance matters. District plant engineer for New York Telephone Company for eight years.

HOWARD J. TYZZER: I.R.E. Designer of radio receiving apparatus since 1916.

LINCOLN G. WALSH: M.E. A.S.M.E. A.I.E.E. Stevens Institute of Technology Research work in Bell Telephone Laboratories. Chairman, Metropolitan Section, Intercollegiate Convention, A.S.M.E.

HAROLD A. WHEELER: B.S. in Physics, George Washington University. Graduate work in Physics, Johns Hopkins University. Engaged in special research work with Neutrodyne apparatus since 1922.

S. TRUBEE WOODHULL: B.E.E. University of Michigan. Formerly with the American Marconi Company. Designed transmitting apparatus for United States Navy and special apparatus for operation in the trenches during the war.

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INDEPENDENT RADIO MANUFACTURERS, INCORPORATED
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## A.3 U.S. Patents to Wheeler

| | Pat. No. | Issued/Filed | Subject |
|---|---|---|---|
| [WP1] | 1757494 | 300506/250227 | PCN by C bridge. |
| [WP2] | 1831431 | 311110/310116 | TRF variable ratio. |
| [WP3] | 1844374 | 320209/300820 | TRF variable ratio. |
| [WP4] | 1846701 | 320223/300528/ div. 290620 | 3-coil transformer. |
| [WP5] | 1855619 | 320426/280120 | Bridge filter for hum. |
| [WP6] | 1866687 | 320712/271208 | AVC by relay. |
| [WP7] | 1868155 | 320719/290620 | 3-coil transformer. |
| [WP8] | 1873236 | 320823/280922 | Volume control by shielding. |
| [WP9] | 1875837 | 320906/310707 | Image suppression. |
| [WP10] | 1878614 | 320720/310224 | Choice of IF in superhet. |
| [WP11] | 1878653 | 320720/300820 | TRF variable ratio. |
| [WP12] | 1878740 | 320720/290716 | AF push-pull amplifier. |
| [WP13] | 1878741 | 320720/290817 | AF push-pull amplifier. |
| [WP14] | 1878742 | 320720/290817 | AF push-pull amplifier. |
| [WP15] | 1878743 | 320720/290817 | AF push-pull amplifier. |
| [WP16] | 1879861 | 320927/301110/ div. 270707 | Triode AVC. |
| [WP17] | 1879862 | 320927/301110/ div. 270707 | Triode AVC. |
| [WP18] | 1879863 | 320927/301110/ div. 270707 | Diode AVC. |
| [WP19] | 1895091 | 330124/300818 | TRF variable ratio. |
| [WP20] | 1896500 | 330207/240922 | GCN by C bridge. |
| [WP21] | 1904185 | 330418/270217 | AF power amplifier. |
| [WP22] | 1907916 | 330509/300819 | Antenna Tuner. |
| [WP23] | 1910870 | 330523/300910 | TRF variable ratio. |
| [WP24] | 1913693 | 330613/310116 | TRF variable ratio. |
| [WP25] | 1927672 | 330919/300910 | TRF circuits, Philco 95. |
| [WP26] | 1930784 | 331017/310522/ div. 270707 | Filament rheostat. |
| [WP27] | 1931338 | 331017/320120 | Oscillator-modulator. |
| [WP28] | 1931596 | 331024/310409 | Tone control. |
| [WP29] | 1933402 | 331031/310716 | TRF, double band. |
| [WP30] | 1943405 | 340116/300910 | TRF variable ratio. |
| [WP31] | 1951685 | 340320/310401 | Diode peak detector, load. |
| [WP32] | 1958027 | 340508/330130 | Emission-valve modulator. |
| [WP33] | 1960984 | 340529/330405 | Image suppression. |
| [WP34] | 1997665 | 350416/330310 | AF push-pull amplifier. |
| [WP35] | 2000113 | 350507/331116 | IF relations in superhet. |

| | Pat. No. | Issued/Filed | Subject |
|---|---|---|---|
| [WP36] | 2001950 | 350521/310814 | Triode AVC. |
| [WP37] | 2013121 | 350903/340606/ con. 320406 | Double AVC. |
| [WP38] | 2015327 | 350924/330130 | Gradual-cutoff grid. |
| [WP39] | 2016760 | 351008/330323 | Gradual-cutoff grid. |
| [WP40] | 2018540 | 351022/340217/ div. 310401 | Diode detector |
| [WP41] | Re 19744 | 351029/340926/ re. 301113/ div. 270707 | Diode AVC. |
| [WP42] | 2022067 | 351126/310528 | Oscillator-modulator. |
| [WP43] | 2022068 | 351126/311214 | Self-regulating oscillator. |
| [WP44] | 2024017 | 351210/331107 | (NPC) Manual XPS, inter-locking |
| [WP45] | 2026075 | 351231/330405 | Image suppression. |
| [WP46] | 2034013 | 360317/330114 | Oscillator-modulator. |
| [WP47] | 2034014 | 360317/350404 | (VEW, NPC, WOS) Loud-speaker. |
| [WP48] | 2038253 | 360421/350410 | (WOS) Loudspeaker. |
| [WP49] | 2039136 | 360428/340531 | 10-Kc trap. |
| [WP50] | 2041273 | 360428/320827/ div. 270707 | Linear AVC. |
| [WP51] | 2042571 | 360602/350327 | Emission-valve modulator. |
| [WP52] | 2048527 | 360721/320201 | Image suppression. |
| [WP53] | 2048528 | 360721/330306/ div. 270217 | AF amplifier. |
| [WP54] | 2049147 | 360728/351119 | (DEH) Antenna coupling. |
| [WP55] | 2050679 | 360811/331003 | SSB receiver. |
| [WP56] | 2050680 | 360811/350227 | SSB receiver. |
| [WP57] | 2061991 | 361124/350522 | Unicontrol superheterodyne. |
| [WP58] | 2064774 | 361215/350610 | Antenna-to-line coupling. |
| [WP59] | 2064775 | 361215/350610 | Antenna-to-line filter. |
| [WP60] | 2073038 | 370309/340309 | Suspended diode AVC. |
| [WP61] | 2075683 | 370330/330405 | Image suppression. |
| [WP62] | 2080646 | 370518/270707 | AVC tuning indicator. |
| [WP63] | 2081861 | 370525/350610 | Multitransformer filter. |
| [WP64] | 2092708 | 370907/350329 | RF choke coil. |
| [WP65] | 2092709 | 370907/351119 | Antenna-to-line filter. |
| [WP66] | 2099156 | 371116/360125 | Double-superheterodyne AFC. |

| | Pat. No. | Issued/Filed | Subject |
|---|---|---|---|
| [WP67] | 2115676 | 380426/360620 | AXPS in triple super-heterodyne. |
| [WP68] | 2137318 | 381122/351019/ div. 310528 | Expanded tuning. |
| [WP69] | 2150044 | 390307/371001 | Frequency detector for FM. |
| [WP70] | 2152514 | 390328/370618 | AF automatic control. |
| [WP71] | 2152515 | 390328/370618 | Control by freq. and ampl. |
| [WP72] | 2152618 | 390328/360721 | AXPS by backward tube. |
| [WP73] | 2153857 | 390411/380518 | Phase-correcting low-pass filter. |
| [WP74] | 2154141 | 390411/370202 | VT with small plate C. |
| [WP75] | 2156137 | 390425/371016 | Triple-tuned double-trap selector. |
| [WP76] | 2159240 | 390523/320622 | Light tuning indicator. |
| [WP77] | 2167134 | 390725/380422 | Dead-end filter, C + G. |
| [WP78] | 2167135 | 390725/380422 | Dead-end filter, series X. |
| [WP79] | 2167136 | 390725/380422 | Dead-end filter, lowpass. |
| [WP80] | 2167137 | 390725/380422 | Dead-end filter, bandpass. |
| [WP81] | 2168461 | 390808/360721 | Control by 10-Kc beatnote. |
| [WP82] | Re21176 | 390815/390407/ re. 370907 | RF choke coil. |
| [WP83] | 2177761 | 391031/380915 | Phase-correcting bandpass filter. |
| [WP84] | 2181499 | 391128/371110 | Bandpass phase compensation. |
| [WP85] | 2182329 | 391205/370623 | Thermal-R preattenuator. |
| [WP86] | 2183980 | 391219/360128 | AXPS unsymmetrical. |
| [WP87] | 2185388 | 400102/370729 | Feedback in IF stage. |
| [WP88] | 2185389 | 400102/380509 | Feedback-amplifier in filter. |
| [WP89] | 2189848 | 400213/370715 | Rotary tuning indicator on CRT. |
| [WP90] | 2189849 | 400213/370729 | Low-AF feedback. |
| [WP91] | 2190816 | 400220/371020 | Horizontal figure-8 antenna. |
| [WP92] | 2192991 | 400312/381007 | Dead-end filter, tapered BW. |
| [WP93] | 2195438 | 400402/360909 | Bidirective coupling like $X_m$. |
| [WP94] | 2196881 | 400409/381029 | Tunable selector, uniform BW. |
| [WP95] | 2204712 | 400618/370826 | Exponential line, termination. |
| [WP96] | 2205738 | 400625/380816 | Wideband modulator. |
| [WP97] | 2206989 | 400709/390328 | Ripple neut. in scanning tfmr. |
| [WP98] | 2206990 | 400709/390418 | Tapered bandpass filter. |

| | Pat. No. | Issued/Filed | Subject |
|---|---|---|---|
| [WP99] | 2212173 | 400820/381021 | (JCW) Feedback of pulses. |
| [WP100] | 2217948 | 401015/371023 | (DEH) AVC for TV, pos. mod. |
| [WP101] | 2222387 | 401119/370216 | (JFF) L adjustable, iron core. |
| [WP102] | 2226648 | 401231/390127 | Correction for sawtooth curvature. |
| [WP103] | 2235131 | 410318/391025 | Sawtooth oscillator. |
| [WP104] | 2239136 | 410422/380523 | Multi-tfmr BP filter, extra C. |
| [WP105] | 2242934 | 410520/381104 | Sawtooth, load diagram. |
| [WP106] | 2247538 | 410701/400111 | Dead-end filter, dif. or int. |
| [WP107] | 2247898 | 410701/390929 | Bandpass filter with traps. |
| [WP108] | 2250170 | 410722/390213 | Sawtooth current, inverted triode |
| [WP109] | 2251966 | 410812/390607 | Field-sync pulses, reflecting line. |
| [WP110] | 2252148 | 410812/400905/ div. 390607 | Field-sync AVC. |
| [WP111] | 2255403 | 410909/390330 | Line oscillator, sync pulses. |
| [WP112] | 2255690 | 410909/351022 | AXPS by double AVC. |
| [WP113] | 2259538 | 411021/381206 | TV shade-level control. |
| [WP114] | 2264781 | 411202/390329 | Sawtooth-current oscillator. |
| [WP115] | 2264782 | 411202/390914 | AFC. |
| [WP116] | 2265826 | 411209/400812 | Linear FM detector. |
| [WP117] | 2271322 | 420127/400406 | Field-sync by double pulses. |
| [WP118] | 2278159 | 420331/330712 | Hexode modulator. |
| [WP119] | 2279543 | 420414/390607/ div. 381029 | Regen. by screen L. |
| [WP120] | 2280139 | 420421/410123 | BW decreased after AFC. |
| [WP121] | 2285957 | 420609/410329 | FM detector. |
| [WP122] | Re22112 | 420609/420303/ re. 400406 | Field sync. |
| [WP123] | 2293233 | 420818/400207 | FM-sync selector and AVC. |
| [WP124] | 2303184 | 421124/400615 | TV AVC ripple cancellation. |
| [WP125] | 2347529 | 440425/410405 | Rectifier for slope indicator. |
| [WP126] | 2488359 | 491115/400311 | AXPS in FM receiver. |

Note: This list includes only inventions made before World War II.

**A.4 Articles by Wheeler.**

[W1] [H/P] "Hazeltine the Professor", Hazeltine Corp.; 1978.
[W2] "My Memories of Mitchell 1907-1916"; 1979.
[W3] "Automatic volume control for radio receiving sets", Proc. IRE, vol. 16, pp. 30-39; Jan. 1928.
[W4] "Measurement of vacuum-tube capacities by a transformer balance", Proc. IRE, vol. 16, pp. 476-481; Apr. 1928.
[W5] with E. C. Andrus, "A device for determining refractory period of the mammalian heart during normal sinus rhythm". Proc. of Soc. for Exp. Biology & Medicine, vol. 25, pp. 695-696; 1928.
[W6] with F. D. Murnaghan, "The theory of wave filters containing a finite number of sections", Phil. Mag., vol. 6, pp. 146-174; Jul. 1928.
[W7] "Simple inductance formulas for radio coils", Proc. IRE, vol. 16, pp. 1398-1400; Oct. 1928. (Discussion, vol. 17, pp. 580-582; Mar. 1929.)
[W8] (W. E. Holland, W. A. MacDonald) "The Philco 95 screen-grid plus", Radio Broadcast, vol. 16, no. , pp. 111-112; Dec. 1929. (Photo and circuit diagram, diode detector and AVC, announced 290901.)
[W9] appendix, "The refractory period of the normally beating dog's auricle", Jour. of Exp. Medicine, vol. 51, pp. 357-367; Mar. 1930.
[W10] with W. A. MacDonald, "Theory and operation of tuned radio-frequency coupling systems", Proc. IRE, vol. 19, pp. 738-805; May 1931. (Discussion on C. E. Trube by L. A. Hazeltine.)
[W11] "The emission valve modulator for superheterodynes", Electronics, vol. 6, no. 3, pp. 76-77; Mar. 1933
[W12] "The emission valve modulator for superheterodyne receivers", Proc. Radio Club of Amer., vol. 10, no. 4, 3 pp.; Apr. 1933.
[W13] "The hexode vacuum tube", Radio Engg., vol. 13, no. 4, pp. 12-14; Apr. 1933.
[W14] "Image suppression in superheterodyne receivers", Proc. IRE, vol. 23, pp. 569-577; Jun. 1935. (Discussion on oscillator-modulator.)
[W15] with J. K. Johnson, "High fidelity receivers with expanding selectors", Proc. IRE, vol. 23, pp. 594-609; Jun. 1935.
[W16] with V. E. Whitman, "Acoustic testing of high fidelity receivers", Proc. IRE, vol. 23, pp. 610-617; Jun. 1935.
[W17] "R-f transition losses", Electronics, vol. 9, no. 1, pp. 26, 46; Jan. 1936.
[W18] "The design of radio-frequency choke coils", Proc. IRE, vol. 25, pp. 850-858; Jun. 1936.
[W19] with V. E. Whitman, "The design of doublet antenna systems", Proc. IRE, vol. 24, pp. 1257-1275; Oct. 1936.
[W20] with A. V. Loughren, "The fine structure of television images", Proc. IRE, vol. 26, pp. 540-575; May 1938.
[W21] "Design formulas for diode detectors", Proc. IRE, vol. 26, pp. 745-780; Jun. 1938.

[W22] "Transmission lines with exponential taper", Proc. IRE, vol. 27, pp. 65-71; Jan. 1939.

[W23] "The interpretation of amplitude and phase distortion in terms of paired echoes", Proc. IRE, vol. 27, pp. 359-385; Jun. 1939. (Presented at IRE-RMA Rochester Fall meeting 381115; IRE NY Section 381207.)

[W24] "Wide-band amplifiers for television", Proc. IRE, vol. 27, pp. 429-438; Jul. 1939. (Presented at IRE Convention NY 380618.)

[W25] "Two-signal cross modulation in a frequency-modulation receiver", Proc. IRE, vol. 28, pp. 537-540; Dec. 1940.

[W26] "The solution of unsymmetrical-sideband problems with the aid of the zero-frequency carrier", Proc. IRE, vol. 29, pp. 446-458; Aug. 1941.

[W27] "Common-channel interference between two frequency-modulated signals", Proc. IRE, vol. 30, pp. 34-50; Jan. 1942.

[W28] "Formulas for the skin effect", Proc. IRE, vol. 30, pp. 412-424; Sept. 1942.

[W29] "Constant-K multitransformer filter for covering a wide range of frequencies", "Driving a filter from a reactive generator", "The properties of directive mutual reactance obtained by the use of vacuum tubes", (abstracts) Elec. Engg., vol. 56, pp. 383-384; Mar. 1937. (Symposium, E. L. Bowles, "Network Analysis and Synthesis".)

# WHEELER MONOGRAPHS

Selected Topics in Radio and Electronics

Harold A. Wheeler and Associates

Numbers 1-11 of Wheeler Monographs have been published as a bound volume (Vol. I), available to technical libraries. A few sets of Numbers 12-19 have been made available (Vol. II).

WM-1. "Transmission lines and equivalent networks", Apr. 1948.

WM-2. "Slide rule operations for radio problems", May 1948.

WM-3. "A simple theory and design formulas for super-regenerative receivers", June 1948.

WM-4. "Geometric relations in circle diagrams of transmission line impedance", July 1948.

WM-5. "Generalized transformer concepts for feedback amplifiers and filter networks", Aug. 1948.

WM-6. "A simple theory of powdered iron at all frequencies", Sep. 1948.

WM-7. "Superselectivity in a super-regenerative receiver", Nov. 1948.

WM-8. "The piston attenuator in a waveguide below cutoff", Jan. 1949.

WM-9. "Measuring the efficiency of a superheterodyne converter by the input impedance diagram", Mar. 1949.

WM-10. "The transmission efficiency of linear networks and frequency changers", May 1949.

WM-11. "The maximum speed of amplification in a wideband amplifier", July 1949.

WM-12. "Formulas for electron tubes", Mar. 1950.

WM-13. "The limitations of a wideband power amplifier", June 1950.

WM-14. "The capacitance of two parallel wires of different diameters, Aug. 1950.

WM-15. "Potential analog for frequency selectors with oscillating peaks", June 1951.

WM-16. "Pulse-power chart for waveguides and coaxial lines", Apr. 1953.

WM-17. "Nomogram for some limitations on high-frequency voltage breakdown in air", May 1953.

WM-18. "Step-twist waveguide components", May 1954.

WM-19. "The transmission-line properties of a round wire between parallel planes", June 1954.

# Name Index

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## Subject Index

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