RADIO REMINISCENCES: A HALF CENTURY

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INTRODUCTION

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INTRODUCTION

THE VOCABULARY OF RADIO

It is impossible for one who has spent a half century in scientific and engineering work, most of it in a specialized field, to dispense with the use of at least a limited number of technical terms. Since this modest effort is neither a text book nor a complete history of radio, I have no intention of going into technical details, but I must make use of a limited amount of special radio vocabulary in order to avoid endless circumlocution. To my colleagues and friends of the radio engineering profession, I can say that it is entirely unnecessary for them to read this introductory chapter. To these who have no radio vocabulary and to those who have no idea of the real meaning of words in the radio vocabulary commonly used by the public, I extend an invitation to acquire, by as painless a process as possible, at least an elementary knowledge of the most commonly used radio terms.

In the first place, the word radio itself was not born with the discovery by Heinrich Hertz, in the late 1880's, of these particular electro-magnetic waves which we know now as radio waves. For nearly ten years these electro-magnetic waves were studied by various people in many countries but without any very practical applications.

When the first applications of radio for communication purposes were made, people spoke of "wireless telegraphy"; the reason for this was obvious. Telegraphy was well known and, since early radio was all dots and dashes controlled by a hand-operated key similar to the telegraph key, it was natural to call it wireless telegraphy. A little later the whole general field of investigation was called the field of "wireless".

Within a decade it became obvious to any casual observer that the term wireless was a misnomer, because far from being free from wires, wireless telegraph equipment was pretty completely composed of them even to the antenna which radiated the signal.

The thing that distinguished wireless telegraphy from land line or ordinary telegraphy was the fact that there was no wire connection between the transmitter and the receiver, and that a part of the energy from the transmitter reached the receiver in the form of a radiated wave motion which passed through space without the aid of wire. Therefore, it was proposed and agreed upon by all interested parties, that the term wireless should be dropped and that wireless telegraphy should henceforth be called radio telegraphy.

The word radio is derived from the same roots as the word radiation. In the course of time, the public dropped the word telegraphy from radio telegraphy and telephony from radio telephony and referred broadly to the whole field of activity as radio. So the word became a noun instead of an adjective.

Since the essence of radio lies in the fact that it progresses from one place to another by radiation instead of by direct contact, it is evident that the first thing to understand clearly is the nature of this wave motion by which the transfer of energy from place to place comes about.

The most primitive way for man to apply energy is by the direct application of force by laying hands on an object and pushing or pulling. This is the transfer of energy by mechanical means with direct contact. A further stage in this same process occurs when a man attaches a wire cable to an object and pulls on that cable. As the object is moved it has received energy from the man, not by direct contact with his hands, but through an intervening mechanical connection. Thus this process is only a variation of the first one.

If, instead of pulling on the wire attached to the object, the man oscillates the hand holding the wire cable rapidly back and forth sideways, he will create a transverse vibration in the cable. This passes down the cable to the far end which is attached to the object. This object, in turn, may be twitched laterally back and forth, thus absorbing some of the energy which the man has expended by putting a wave motion on the cable. This is still a mechanical process, but the mechanism of the energy transfer has become a wave motion.

The most commonly observed wave motion of the mechanical type is, of course, wave motion in water. Close observation of water waves will disclose that, except on a shelving beach, the water itself does not move forward bodily, although the motion of the wave definitely does. The energy put into the waves, usually by the wind, although occasionally by an earthquake or a large explosion, is thus transferred from point to point without the water itself moving bodily from one place to another.

If an observer were standing on the end of a pier observing a regular set of incoming waves, he could count the number of the crests which passed by him in every minute. This number would be termed frequency and would be measured as the number of wave cycles per minute. We have the same thing in radio except that, since the frequency count is very high, we take the second as a standard time interval and measure our frequencies in cycles, kilocycles, (1000 cycles) or megacycles (1,000,000 cycles) per second. True, you cannot see the radio wave as you can the water wave, but there are instruments for observing this performance which are even more accurate than the human eye.

Our observer on the end of the pier will notice that some waves are bigger than others, that is, the crests rise higher and the troughs go deeper. The term "amplitude" describes this property of a wave. It measures the extent of the oscillations of the individual particles of the medium, such as the water, for example. Water waves, whose individual particles of water have an elliptical motion, are rather complicated. I have used them as an illustration because almost everyone has observed them. There are other waves which are very much simpler.

Sound waves are not a lateral or a vertical oscillation in the media through which they travel, but are pulsating or compressional oscillations. Those waves are not directly visible to the human eye but can be made visible by a rather simple equipment. Fortunately we have an ear drum which is sensitive to the impact of the sound waves coming through the air. Thus energy in the form of sound vibrations creating sound waves in the air is delivered to the listening human ear in the form of noise, music, speech, etc. Of course sound waves travel through any substance which has compressional elasticity, gas, liquid or solid. Indeed, you have only to put your head under water when swimming and have somebody knock a pair of stones together under water, to find out that sound will travel to your ear remarkably well through water.

The wave motions, and their accompanying transfers of energy which I have so far discussed, require the presence of some material such as earth, water, air, etc. to exist and be propagated to some distant place. Sound for instance, will not travel through a vacuum, as can be easily demonstrated by operating a bell in a space from which all the air has been evacuated. However, there is a huge wave motion family, known as the electro-magnetic waves, which not only are perfectly able to pass through completely empty space but, indeed travel better in such space than anywhere else. They differ from mechanical waves and sound waves in that they do not involve the motion of particles of matter, but only the setting up of certain oscillating electric and magnetic conditions in space. If the space be populated with a certain amount of matter it influences, to a considerable extent, the behavior of these waves, but does not prohibit their formation or play any vital part in the formation of the electro-magnetic wave motion. It is to this great family that radio waves belong, with light waves, heat waves, x-rays, ultra-violet rays and infra-red rays.

One thing all these waves have in common is that they are electro-magnetic disturbances, or undulations, of a transverse character, always at right angles to the direction in which the wave is moving forward. In their general physical properties, in their effect on human senses, they differ enormously. They differ in frequency from zero to so many uncounted megacycles that it would take a whole blackboard to express the result. The radio waves are lower in frequency, that is, longer in wavelength, than the infra-red rays; very much lower in frequency than the visible light waves, and enormously lower in frequencies than x-rays. Theoretically there is no lower frequency limit to radio waves, but for practical communication purposes we can safely place 10,000 cycles or 10 kilocycles per second as the lower limit. This corresponds to a wavelength of 30 kilometers from crest to crest, which is a distance of 18 3/4 miles. The shortest radio waves, definitely classified as such, although the border line is largely a matter of opinion, are 3 millimeters in wavelength. This means that their frequency is one hundred thousand million cycles or one hundred thousand megacycles.

I am sure a good many people are skeptical as to whether the radio scientists can really measure any such frequencies. Such people often say "Surely such oscillations cannot be actually counted". As to that, nobody can actually count, with the unaided human ear, the number of oscillations per second of a 440 cycle tuning fork used in tuning a piano, but the frequency of that fork has been measured and is accurately known. So it is, that by the aid of physical instruments which it is not my purpose to describe here, the radio scientist or engineer is able to make extraordinarily precise observations. Indeed, such observations can be certain to a few cycles in one hundred million. Thus, in several hundred million cycles one might be a little wrong but the error would be a very minute percentage of difference from the true figure.

To generate these radio waves we need first a device to produce alternating current of the same frequency as the desired wave. All currents in metallic conductors, either direct or alternating, can be thought of as a movement of negatively charged particles which are commonly referred to as electrons. These particles are so small that in a metal conductor they can, when properly driven to do so, move about with a considerable degree of freedom. Of course, in bumping around they do heat up the conductor to a certain extent, depending upon whether it is made of good or very poor material.

If then, we produce a machine, which we call a <u>transmitter</u>, to apply electric voltages rapidly alternating from positive to negative, to the base of a vertical rod called an antenna, this transmitter will cause negative charges to run up toward the top of the rod, then down, up again, and so on in rapid succession, so that the top of the rod, originally uncharged or neutral, is charged negatively when it has a lot of electrons at the top, and positively when most of them are at the bottom and there is a deficit of electrons at the top.

When our <u>antenna</u> is thus charged, it extends electric strains into the space in the vicinity of the antenna. These strains also alternate in direction. Ultimately a condition is set up in the surrounding medium which results in goodly portions of these electric strains moving off away from the antenna into the surrounding space. This is as simple a picture as I can draw of the mechanism of <u>radiation</u>. It is in this manner that the electrical wave, automatically accompanied by its magnetic component, is whipped off into space and starts out for distant horizons.

It is now time to speak of the <u>velocity</u> of the wave. Mechanical waves can have very slow velocities, or in some cases, reasonably high velocities. Water waves vary tremendously in velocity, according to the height or amplitude of the wave; the long big waves travel much faster than ripples. The great waves created in a recent earthquake in the Aleutians were supposed to have reached a velocity of nearly 300 miles an hour. Sound waves in air travel approximately a thousand feet a second which would be 720 miles per hour. This is very slow indeed for an electro-magnetic wave, which in free space, or even in ordinary air, is capable of traveling fast enough to go almost eight times clear around the world in one second. The velocity of radio waves is, as far as we know, the same as that of light waves, or 300,000 kilometers which is 18,000 miles per second. This is the highest velocity of which scientists have any knowledge.

These velocities are subject to exact measurements which are not particularly difficult, because nowadays it is not much harder to measure a millionth of a second than it used to be to measure one second. Thus the <u>velocity</u> of any wave, radio or otherwise, may be defined as the distance, taken perpendicular to the wave front, which is covered by the advancing wave in one second of time.

Whenever any kind of wave motion passes from one medium into another and different medium, interesting things happen to it. The radio waves are no exception to this rule. They may be reflected, as at the boundary surface between air and metal; they may even be partially reflected from a tree or anything which differs from the medium in which they normally travel. They may also penetrate into the objects which they encounter, the penetration being deeper when the reflection is less perfect. Obviously, if a wave is almost completely reflected, as it may be from a copper or silver surface, it can only penetrate skin deep, because there is nothing left to go on into the metal. But there are media, such as the ionosphere, which do not abruptly reflect the wave but allow it to enter the medium, changing the velocity of the wave in the process. The ionosphere is the region of the high atmosphere which, due to the fact that it possesses large numbers of free electrons, is capable of modifying the velocity and direction of travel of radio waves which enter it. Since we live on a curve earth, all radio rays which reach beyond the optical horizon depend on the action of the ionosphere for keeping from escaping into outer space by bending them so that they return to the earth. Thus long distance radio communication is wholly dependent on the physical properties of this interesting region.

If a cone of rays strikes the ionosphere at an oblique angle, the upper edge of the cone will be accelerated faster than the lower edge, because it enters that medium first. Thus the direction of advance of this cone is altered and it tends to bend, finally turning downward in many cases, so as to return at a distant point to the earth. A later chapter will shown how important this is to radio communication.

We can define the transmitter as a device creating high frequency currents which can charge the radiating antenna, whose duty it is to whip off some of this energy into space in the form of radio waves. The reverse of this process happens at the receiving end, where the advancing radio wave encounters an antenna and the electrical effect of the wave is to produce high frequency currents up and down this antenna. These in turn can be collected in a device known as a receiver and made audible for the human ear, or visible to the human eye. This defines the receiver.

The absorption of a radio wave is its loss of energy in passing from one place to another, due to the presence in the medium in which it travels of particles of matter which are capable of robbing it of some of its energy and thereby weakening it. Radio waves, in general, also suffer an attenuation with increasing distance due to the simple fact that the energy is spread out over a wider and wider area as the distance increases. Therefore there isn't much energy at any one spot where a receiving antenna may be located.

Radio waves may also be persuaded to travel along wires, and inside of hollow conductors if the waves are of sufficiently high frequency. Many useful applications of this have been made.

The term "phase", imported from the older field of alternating current engineering into radio, is applied both to the wave motion itself and to the radio frequency currents which produce the wave motion. Going back to our analogy of water waves, let us look at two wave crests separated by one wavelength, that is two successive wave crests. It will be seen that the actual motion of water particles is identical at these two points, although they are separated by a wavelength. In each case the water particle has risen to its highest point of uplift and will have a very brief instant of rest before it starts to fall down again. These two particles of water are said to have the same phase.

If we now consider two wave troughs separated by a wavelength, and a particle of water in each trough, we see that these two particles are also, momentarily at least, at the lowest point of their downward swing. They too, are said to be in the same phase. If, however, we compare the particle at the bottom of the first trough with a particle on either the preceeding or the following wave crest, we see that such a pair of particles are in opposite <u>phases</u>.

Another way of expressing it is that they differ in phase by 180°, exactly as the two opposite ends of a straight line, one pointing east and one pointing west, would differ in direction from each other by 180°. Obviously, there are an infinite number of other water particles on the slopes of the waves which can have intermediate phases differing from each other by any amount from 0° through 180° and back to 360°, which is equivalent to zero phase. A particle of water 90° out of phase with the particle in the crest of a wave would be approximately half way down the slope to the trough; the particle having 45° phase difference would be half way between this last named particle and the crest. This same term phase can be applied, with the same general meaning, to the radio frequency currents flowing through resistances, inductances and capacities. It may be noted that all of the points in an extended wave crest are in the same phase of motion, which leads to a definition of the <u>wave front</u>, as the continuous line drawn through a large number of contiguous points in the same phase of oscillation.

It is obvious that if a large number of waves of the same frequency arrive by different paths from the transmitter and strike the receiving antenna simultaneously, they will not add up and produce an extremely strong signal unless they are substantially all in phase. If we simplify this problem by considering only two such waves, it can be seen that two of them in phase will combine for double the effect on the antenna, but if two of them arrive in phase opposition, each one produces an opposite influence on the antenna and each one will cancel the other with a resulting zero signal. This will be discussed in a later chapter under the heading of "Fading". <u>Interference</u>, therefore, means the combination of radio waves of identical frequency but of different phases. Interference may be destructive or it may be additive.

We now come to the names and functions of a few of the well known component parts of radio apparatus. Every coil of wire, whether it has iron in it or not, possesses a property of retarding the phase or choking back high frequency currents attempting to advance through the coil. This property is called <u>inductance</u>. A device known as a <u>capacity</u> or <u>condenser</u>, which consists very simply of two or more surfaces in close juxtaposition but insulated from each other, tends to advance the phase of high frequency currents rather than retard them. The capacity and the inductance may be so combined in a circuit with suitable vacuum tubes as to permit the development of high frequency currents from the energy supplied by direct currents to the tubes. The frequency can be regulated by the proportions of inductance and capacity used. Both inductance and capacity are frequency determining elements.

All electrical devices possess at least some resistance. In other words, the ebb and flow of electrons within these devices is not wholly free, but it is somewhat impeded by collisions between the electrons and molecules. This results in heating the equipment and in the reduction of the efficiency of the machine involved. Thus resistance is a measure of the losses to be expected in the parts of radio transmitters and receivers.

A detector is a device which, when supplied with high frequency energy derived either directly or after amplification from the antenna, converts the energy of these currents into a low frequency, some of which can either be passed through the telephones and heard by the human ear, or can be used to actuate a pen on the recorder, thus making a record visible to the human eye.

The amplifier is a device for building up weak radio frequency currents into much stronger ones. This is commonly done by the aid of suitable vacuum tubes, particularly the shield grid tubes, associated with suitable circuits. Tuning of the receiver is the process whereby the inherent frequency of the receiver circuit is made identical with that of the incoming wave, thus giving the circuit enormously greater efficiency in picking up the signal. The selectivity of a receiver is the measure of its ability to discriminate between and untangle two signals which are fairly close together in frequency. With poor selectivity these signals cannot be unscrambled and hence interfere with each other. With good selectivity they both can be used without mutual interference.

In many cases the final detection of the signal is brought about by combining with it a local signal from a weak high frequency oscillator. When both of these signals enter the detector at the same time, one a distant signal, one a local, two new frequencies are produced, one of which is the difference between these first two signal frequencies and the other the sum of these two frequencies. Usually the difference frequency is used, in order to extract out of the combination a tone that can be heard in the telephone, because it is low enough in frequency, whereas both the incoming signal and the local signal, or heterodyne as it is called, are too high in pitch to be put through a telephone and couldn't be heard if they were.

The term crystal used in radio may mean a crystal such as silicon, galena, iron pyrites or carborundum, which is used as a detector. On the other hand, it may mean a quartz, tourmaline or occasionally some other new crystal which is capable of exactly controlling the oscillations in a transmitter and is used for the purpose of high stabilization of the frequency. We may say of a receiver that it uses a crystal detector, or of a transmitter, that it uses a crystal controlled master oscillator, but we mean very different crystals and very different actions in the two cases.

The expression coupling is used to indicate the proximity of one circuit to another without necessarily having any direct physical connection. Due to this close proximity, currents from one circuit will transfer part of their energy into the nearby coupled circuit just as the 2000 volt primary of a 60 cycle transformer transfers energy to the 100 volt, 60 cycle circuit which is used in the home. These two circuits, namely the house circuit and the supply circuit, are not actually in contact with each other but they are coupled to each other due to their close proximity within the transformer.

A radio circuit may be any combination of the fundamental elements, resistance, inductance, capacity and may, or may not, be connected to various types of vacuum tubes. Vacuum tubes are devices for creating a flow of electrons between certain elements built into the tube in such a way that the tube may be used as a detector, amplifier, transmitter, frequency multiplier or modulator.

Modulation is the process of putting information on the back of a radio wave so that it may arrive at its destination in a receiver and be interpreted. Pure, unmodulated or continuous waves may be detected, but they carry no intelligence except to indicate that someone has put radio frequency energy on the air somewhere. If a key is inserted in the continuous wave transmitting circuit and dots and dashes are produced, the simplest form of modulation is produced, which requires knowledge of the telegraphic code to interpret.

To place music or the human voice on the "carrier wave" is a much more complicated process but, as everyone knows, it is done in these days very successfully and by several means. The first process is by amplitude modulation, where the amplitude, or strength of the wave, is caused to vary in accordance with the variations in music or human speech, or in television, with the light value of the picture. It may be resolved at the receiver by aid of the detector, the amplifier, and finally the loud speaker or the television projector screen.

The second commonly used method of modulation is to keep the amplitude or strength of the carrier wave constant but to vary its frequency above and below normal value at a rate corresponding to the vibrations in music, speech or television as required, and by an amount proportional to the intensity of those effects. This is known as frequency modulation.

The term fidelity for a radio system is a measure of the exactness with which it reproduces music, voice, television, or what not, put into the transmitter and finally arriving at the receiver so as to be heard or seen. A very high fidelity system nowadays reproduces very closely what is actually put into the transmitter. The term linearity is closely allied with fidelity but differs from it, since it refers only to the ability of the system to deliver, say, twice as loud a signal if the transmitter is actuated with twice as strong a modulating impulse. Cheap receivers are often non-linear, that is, they saturate easily and are only good to certain levels, above which they no longer effectively reproduce the variation in intensity. Early broadcast transmitters had faults in this same way and even now it is seldom that the extremely wide variations of intensity, such as actually occur in musical performances are correctly reproduced, especially by amplitude modulation. Frequency modulation can do a better job.

There are, of course, a great many other highly technical terms used in radio but they are of extreme interest only to the scientists and engineers working in that field. They are not all necessary for a general understanding of the vocabulary of radio.

The extensive application of radar during the late war was accompanied by the introduction of a number of new terms in the vocabulary of radio.

partially reflected, returns to the radar receiver and is detected and displayed on the scope. All this happens between the time that one pulse leaves the transmitter and the time the next pulse starts out.

The term pulse length refers to the time of duration of one of these pulses measured in microseconds. The term pulse recurrence frequency means the number of these pulses which the radar sends out in one second of time. If a radar sends out a thousand pulses in each second of time, we say the pulse recurrence frequency is 1000. If each pulse lasts for one microsecond, we say the pulse length is one microsecond.

Early radars commonly used the A scope method of display. As explained more completely in a later chapter, the cathode ray oscilloscope, called "scope" for short, has a screen on the outer end facing the observer, which becomes a luminous record of passing electrical events when a sharp pencil of flying electrons or cathode rays moves over the rear face of this screen. If nothing is happening, the cathode ray pencil strikes the center of the screen and produces a small bright spot.

The sweep circuit of the scope is an electrical connection which causes this bright spot to move from left to right rapidly so as to produce a luminous trace in the form of a horizontal straight line across the face of the scope. If only the sweep circuit is supplied to a scope, the central bright spot simply becomes a luminous horizontal straight line. Really it is a moving pencil, but to the eye it appears continuous. The radar receiver, however, also has a connection to this scope so that the echo pulses received from the target will appear on the scope as a vertical pip or blip. The pip is a vertical deflection of a small part of the originally undisturbed horizontal line. Its position accurately indicates the exact range of the target.

The position of the pip measured from left to right along the luminous horizontal line, indicates the time which it has taken for a pulse of energy to go out to the target and return. Since we know that radio and radar radiations travel 300,000 kilometers per second, it is very simple to calibrate the horizontal line in terms of range, either in kilometers or in thousands of yards or in miles if you prefer them.

Thus a typical A scope on a search radar shows a large pip on the extreme left of the horizontal bright line, indicating the zero time or zero range position when all pulses start out from the radar. This pip is produced by the direct and local influence of the radar transmitter on the receiver. If, however, other pips are seen farther out on the line toward the right, they indicate distant targets which return their echoes to the screen at a later time. These pips are, of course, smaller than the initial pip which indicates the outgoing pulse. In general, the farther away the target, the smaller or weaker the pip becomes.

The bearing is obtained by rotating the directional antenna back and forth and noting when a given target pip appears and disappears.

In fire control radar this is done in a different way and with a far higher degree of precision, which is necessary for accurate control of missiles.

Radar is a branch of radio and a direct outgrowth of the development of radio. The word radar was coined, to the best of my knowledge, by Captain Sam Tucker of the Office of Naval Operations, who, at the time the word was first proposed, was a lieutenant attached to the Bureau of Ships, in the Radio Section of the Radio Division of that Bureau. The keen interest and foresight of this young officer had a very great effect on the growth of radar within the United States Navy.

Radar means radio direction finding and ranging - the ra in radar stands for radio, the d for direction finding, the a for and, and the r at the end of the word for ranging. The main difference between the much earlier direction finders and radar lies in two things: first, it is not necessary that the target of the radar have any radio equipment on board or radiate any radio waves. It must only be a target of such a nature that it will reflect some of the radio energy which is sent against it by the radar transmitter. Second, the radar set obtains practically instantaneously both the bearing, or angular position of the target with reference to the transmitter, and the range or distance between the radar set and its target.

When speaking of bearings we must consider that we are dealing with both vertical bearings, or altitude angles, and horizontal bearings which are azimuth angles. The bearings in the horizontal plane may be obtained either by reference to the true north or by reference to the heading of the ship carrying the radar. They may, indeed, be obtained by both methods simultaneously. If the bearings are referred to the true north, they are called true bearings; if they are referred to the ship's heading, they are called relative bearings.

Broadly speaking, radars fall into two classes. A search radar is intended for very long range warning purposes. A fire control radar is of more moderate range but capable of far higher accuracy both in range determination and in bearing determination. Both types of radars are capable of operating under conditions of zero visibility so that they have completely changed the standard doctrines of Naval warfare.

The radar transmitter is a radio transmitter with a very special form of modulation. It sends out a tremendously strong but very brief burst of radio energy and then, after a relatively long rest period, another burst of energy, followed in turn by another relatively long rest period. This sequence continues as long as the radar transmitter is in active operation. These bursts of energy are so short that their time of duration must be estimated in microseconds. When we realize that a microsecond is a millionth of a second, we see that these pulses are of very short duration indeed. But while they last they are of tremendous power, often of many hundreds of kilowatts. We may think of an ordinary radio transmitter at very high frequency directed into space by a fairly narrow beam, as being similar to a garden hose throwing a fairly steady and concentrated continuous stream of water, whereas the radar is more like a machine gun which periodically emits bullets, each individually having a very high energy level.

The term pulse refers to this brief burst of radio energy which periodically leaves the radar transmitter, rushes through space, usually directed by a beam antenna with a rather narrow angle, hits the target, is

The power of the radar is usually estimated in terms of the peak power of the individual pulses. This is many times greater than the average power of the radar transmitter. For instance, suppose we have a radar whose pulses are one microsecond in length and whose pulse recurrence frequency is one thousand per second; the radar is actually radiating in a second of time only a thousand pulses which, if placed close together would have a total length of a thousand microseconds, which is only one thousandth part of one second. Thus the radar is actually alive only a very small fractional part of the time. The ratio between the total time of power emission in a second to the value of the second itself is thus 1000 to 1 so that such a radar is said to have a duty cycle of one tenth of one percent. It is for this reason that the radar supplied with only a few kilowatts of power can be used in these brief short pulses with peak powers running into many hundreds of kilowatts. We see then, that radar equipment does not require extremely large power supplies because the nature of the equipment is such that it can take power continuously at a low rate and store it up in the modulator, letting it go for brief bursts at very high energy levels. Nevertheless it is the peak power which is most important in the operation of a radar rather than the average power, which has only a minor influence.

In order to concentrate this power on a distant target, it is important that highly directive antennae be used to form the radar beam. Sometimes these are arrays of metallic conductors properly phased and sometimes a single small antenna backed up by a parabolic reflecting mirror made of metal.

Beam width is defined as the angle covered by the slowly diverging beam of the radar as it leaves the antenna. If we say the beam width is 6° we mean that the greater part of the energy is confined within that angle. Search radars usually have wider beams than do fire control radars.

It is possible to make the radar beam narrow in horizontal angle coverage and wide in vertical angle coverage, or the radar may be designed for just the opposite effect, namely wide horizontal angle and narrow vertical angle. Such a radar beam is called a fan beam.

It often happens, sometimes intentionally and sometimes due to faulty design, that the radar beam has more than one principal direction of energy output. The radar is then said to have more than one lobe. When a radar has two lobes, for instance, they may be intentionally adjusted to equal intensity or one may have considerably more intensity than the other.

A radar is said to scan a given sector of space. This is usually accomplished by turning the radar antenna system about a vertical axis when it is scanning in azimuth, and about a horizontal axis when it is scanning in altitude angles. The scanning in azimuth may be a complete 360°, and very commonly is, with search radar. On the other hand, the radar may be swept back and forth over a given sector, say for instance 60° wide, and so concentrating its attention on a region of space from which the enemy is reasonably expected to approach. This is called sector scanning.

Besides the A scope already described, the radar information may be displayed on a cathode ray screen which is known as the plan position indicator, called PPI for short. In this type of scan and display, the radar scans continually, frequently over the entire 360°, but sometimes covering only a sector. The sweep circuit in this type of radar deflects the cathode ray beam radially outward from the center of the scope toward the periphery. This goes on very rapidly as the radar continues to scan and does not fill up the screen with a lot of bright lines because a mechanism is provided to blank out the bright spot, which otherwise would trace scanning lines all over the picture, until a radar echo is received. The receipt of this echo momentarily turns on the bright spot so that instead of a pip appearing, a small bright spot appears on the picture indicating the angular position of the target. The range of the target is measured by the radial distance from the bright target spot to the center of the PPI scope. Thus the PPI method of presentation presents a sort of crude television picture or map of surrounding reflecting objects both near and far. This is, of course, a very rapid and convenient way of getting a lot of information simultaneously on one picture. It is very useful in observing the movements of a large number of airplanes, for instance, whether they be our own planes or the enemy's.

Early in the development of radar, I insisted that we must, from the very outset, develop radar that could transmit and receive with the same antenna. The early British systems did not do this even on board ship, but I felt that installation difficulties would greatly handicap our Fleet if we did not solve this vexatious question. To the best of my knowledge the first duplexers, as we called them, were built by the Naval Research Laboratory. These may be defined as devices which automatically protect the receiver from being burned up by the tremendous power put into the antenna although the receiver is continuously connected to the antenna. When echoes from fairly distant objects come back in a few milliseconds, there is obviously no time for mechanical switching operations from send to receive position. The duplexer not only protects the receiver but prevents the diversion of power from the transmitter into the receiver lines. Thus, a few microseconds after the initial pulse has left the antenna, the receiver is ready to pick up the returning echo. Later when the National Defense Research Committee got into the picture, their engineers called a similar device the TR box. TR, of course, stands for transmit-receive.

The term IFF means identification, friend or foe. This term was invented by the British and to them must go the credit of having the first device which could be put on friendly targets in such a way that friendly radars would receive a particular kind of indication which would distinguish friend from foe.

The resolving power of a radar means its ability to separate or distinguish between adjacent targets. We may speak of the resolving power in terms of range or in terms of angular position.

These then, are the most important words in the vocabulary of radar which have grown up in a very few years' time. A few of them are newly coined words, but most of them are specialized meanings for terms which have been previously used in a broader sense.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER I - 1890-1900

EARLY "WIRELESS"

CHAPTER I

1890-1900 - Early "Wireless"

It is customary for every author to present his excuses for writing a book, especially if it departs radically in nature from any of his earlier publications. I really have no excuses; it has simply occurred to me that it might be interesting to put down a running account of some of the experiences in the field of radio wherein I have been either an interested bystander or an actual participant. By the time this manuscript gets into print, if it ever does, it will cover a period of very close to a half century. Not only will this account be as non-technical as possible but it will be non-controversial; therefore, it cannot be used as a basis for deciding who did a certain thing first or whether he did it better than someone else. It is my earnest desire to make the book of interest to the general public as well as to my technical colleagues. Of course, they may find it especially interesting, since many of them will be named in connection with certain of the events of which I propose to give an account.

These preliminary remarks must make it clear that I will unavoidably have to recount much of my own personal history, for which, again, I make no apologies.

I was born of New England parentage, but in the city of Chicago, my parents having gone West many years before I was born. My father, who was an advertising man, had little interest in, and not much understanding of, technical things. The attraction which scientific and engineering developments have held for me ever since I can remember is probably inherited on the one hand from my maternal grandfather, who owned and operated a bolt factory in Lowell, Massachusetts, and from my paternal grandfather, who was a builder and constructor in a small town near the border between Massachusetts and New Hampshire.

Long before I ever had any higher schooling, I was constructing simple Voltaic cells with zinc and copper plates in acidulated water, stringing a number of them up in series, trying to make a carbon arc and an induction coil. At this time I was living in a small village named Wilmette, a few miles north of Chicago city limits. Wilmette was then a town of only a few hundred people and was a very rural community indeed. I attended high school, in the neighboring city of Evanston where I sopped up all the mathematics, physics and chemistry I could get hold of.

During this period I obtained a couple of telegraph sounders and keys and with a few homemade batteries finally constructed, with the cooperation of a neighborhood boy friend, my first telegraph line. I remember very well that when everything was completed and should have started off properly with the first closing of the keys, nothing happened. For days we struggled with this difficulty and were almost in despair. It was then I had my first lesson in ground connections. My friend had a well at his house, with an iron pipe going deep into the ground, and therefore had good ground connections. Our well was operated with a bucket pump, which had only a wooden pipe, and my parents refused me permission to lower a metal plate to the bottom of the well for fear of contaminating the water. I was forced to use a lead pipe which connected with the cistern pump. Somehow or other I discovered the difficulty was due to the ground around that pipe being thoroughly dried out in the midsummer heat. A few buckets of water remedied the difficulty.

Later my family moved to the eastern part of the small town and I got a group of 8 or 9 of the boys together to construct a telegraph which pretty well ran all over the town. Some of us became expert operators in the old Morse code - good enough so that we could lounge at the window of the telegraph office of the railway station and read the stuff that came over the Western Union line. Of course the whole idea of the telegraph had been suggested by that telegraph office. We boys were always hanging around railroad stations and could tell any engine on that branch of the Northwestern line by its whistle while it was miles away. The railroad was the most technical thing we had in town.

With this background it will be seen why, after I finished high school, I wanted to go to a college where I could study electrical engineering. However, the family finances didn't run into attendance at any college which was so far away that I couldn't live at home, so in 1896 I matriculated at Northwestern University for a special course. My father felt that I couldn't look forward to graduation but that he could manage to send me for a year anyway. This special course had all the college physics, chemistry and mathematics for which they would allow me to register. This was as close as I could get to experimental electrical work. Northwestern University, Evanston Campus, did not then have the splendid school of engineering that it now possesses; however, in the long run I had no occasion to regret the circumstances that forced me to this course. Though I had to learn my engineering the hard way, I acquired thereby a better background for it.

I soon saw that I must partly support myself through college. By that time electrical installations in our little village had advanced as far as electrical door bells and some burglar alarms. The man who handled much of the building in our town was a good friend to me because he gave me plenty of jobs putting in electric bell circuits, and I got some repairing of buglar alarms. This work was done mostly at night, since I was busy in the school all day and nobody worked on Sunday in those days.

It is well to recall that the year 1896 was the year that Professor Roentgen's experiments on X-rays were announced. I can well remember with what enthusiasm and interest everyone received the news of these discoveries. Heinrich Hertz had completed, in 1886 and 1887, his epoch-making discoveries of electromagnetic radiations, predicted between 1861 and 1865 by Clerk Maxwell of England. It was very interesting to note that the frequencies or wave-lengths used by Hertz in his very early work were almost exactly the same as those used by the Navy in its first long range search radars during World War II. By the middle of my junior year in college I was repeating some of the experiments of Hertz. It was during this same period that Edouard Branly of France, brought out a coherer and that Rhigi, at the University of Bologna in Italy, did his work on waves about 2 cm in length, produced by sparks between two metal spheres in oil. These experiments I had the pleasure of repeating, to some extent, not many years after they were first done. I got up an experimental transmitter and receiver which worked through a few hundred feet and gave a well attended demonstration at the University, at which many people marvelled at the way these radiations would go through brick and wooden walls.

It is interesting to note that during this same period, Sir J. J. Thompson, whom I afterward visited in 1908 in England, was beginning his classical experiments which led to the discovery of the electron, which was first called a corpuscle.

Sir Oliver Lodge, in the London Electrician for 1894, discussed the work of Hertz and described his own experiments with electromagnetic waves (radio) and commented upon the phenomena of resonance for tuning, as exemplified by his famous syntonic jars experiments. The jars in question were Leyden jars which had a coating of tin foil on the inside and another on the outside. With the glass dielectric between, this constituted a capacity. Nowadays when we tune our broadcast receivers to pick out the frequency of our favorite station, we can hardly realize that there was a time when people knew nothing about tuning and hardly grasped its implications.

In my early experiments in college at this period, I know I had a vague knowledge of resonance as we called it in those days, and knew it had some vague connection with the size of the discharge plates and the general physical structure of the apparatus, but my own adjustments to produce resonance were largely empirical. By patient trial the adjustments were continued until the oscillations radiated by the transmitter were picked up and detected at the receiver. This wasn't any too easy, since the first detector I had to work with was a copy of the Hertz micrometer spark gap.

By 1898 Lodge had applied for a tuning patent, covering means of bringing the transmitter and receiver into resonance. Sir Wm. Henry Preece, then engineer of the British Post Office, had been interested in wireless communication since 1854, but the type of communication at that time was by conduction currents rather than by electromagnetic waves. Later, in 1877, he tried his experiments by means of magnetic induction using large loops of wire.

From time to time, taken through the years, these methods have had some use, especially in military work, but they have, of course, never been able to step out to respectable distances. In 1896, however, Marconi had gone to England looking for some support for his experiments and was put in touch with Preece and the resources of the British Post Office. I did not meet Marconi until many years later but I followed his work with interest. I have always felt that he was a star of the first magnitude in practical development, but that others deserve more credit for pioneering, inventions, and painstaking investigations which laid the foundations for present day radio. The practical success of the Marconi experiments in those years greatly stimulated my imagination, but led me to make a very unfortunate prophesy. I presented a paper in a Seminar at the University, bolstered with quite a long mathematical calculation, which proved to everybody's satisfaction, that important as wireless communication (it wasn't called radio in those days) was destined to be, it could never span the Atlantic. A very few years later Marconi accomplished that miracle. From then on to this day I have been very wary of making predictions as to what radio cannot do.

Of course we had no notion in those days of the role which the upper layers of the earth's atmosphere play in radio communication, making it possible for radio waves to travel to great distances by gliding around the curvature of the earth or bouncing about between the surface of the earth and the upper atmosphere in such a way as to make progress to their ultimate destination. We thought that the only way such radiation could travel was in a straight line, like light waves. We thought that it would have to bore down through the bulge in the earth which protruded itself between the start and the finish in a long distance path. We figured rightly that the absorption of the waves in passing through that much of the earth's crust would be too great to permit long range communication.

During the same period Slaby in Germany was doing work very much parallel to that of Marconi; in fact, he came to England in 1897 to witness some of Marconi's tests which had far exceeded his own in distance. These tests were made over a range of eight miles. In collaboration with Count von Arco he developed the Slaby-Arco system which ultimately led to the development of the Telefunken Company of Germany.

It was in this period, following his appointment in 1899 as electrical adviser to the British Marconi Wireless Telegraph Company, that Fleming went to work on his experiments leading to the development of the Fleming valve to replace the cumbersome and erratic coherers which were used in those days to receive radio waves. The Edison effect, upon which the action of the Fleming valve was based, was discovered in 1883 but no practical use made of it.

Sir Wm. Crooks was also working in England at this time, principally on the broad subject of discharge of electricity through gases. However, he did conduct original investigations in radio. Crooks described the luminous streams in cathode ray tubes as streams of projectiles and called them a fourth state of matter - he didn't know what the particles were. It remained for Sir J. J. Thompson after long experiments to bring that out. His work lies in the background too of cathode ray tube development which has been so important for radio and radar during the late war.

It is evident that this decade, 1890-1900, was a period in which a number of very interesting changes in our national habits, ways of thinking and ways of living were either well on the way or were started. It was a period of peace, except for the brief flare-up of the Spanish War. A number of my boyhood friends went into the Navy and some into the Army. I was very nearly drawn into it but listened to the counsel of my family to finish my education.

During this period, the first talking machine or phonograph began to make its public appearance in a very scratchy and irritating form. Telephony had definitely arrived, but was by no means a common thing in every household; wireless telephony was still in the future. Transatlantic communications were either by letter or by cable. Very jumpy and uncertain movies had arrived but were by no means common even in a good sized town. In 1896, there were four automobiles in the United States, none of them in private hands. Two years later, four cars were sold to the public by Winton in Cleveland. Electric light and power were common in the big cities but by no means used universally. There were more streets lighted by gas than by electricity. The series arc light for street lighting, and to some extent for store lighting, had arrived. The distribution of gas for lighting purposes had not touched many of the smaller towns, where the kerosene lamp was still supreme. Gasoline was known, but considered rather worthless. Kerosene sold for 10 cents a gallon. Not many miles of roads outside the cities were paved and in the cities there were still plenty of cobble stones used for pavements. Macadamized roads were beginning to be considerably used. The electric railroad had definitely arrived both for urban and inter-urban transportation. The use of gas for heating homes was hardly known except in the vicinity of a local gas well. It was also relatively rare for cooking purposes. Electric cooking had not appeared on the scene at all. In the small towns and the rural districts, fast transportation was by horse and buggy or horse and sleigh in the winter. Bicycles were very popular and very widely used. Most of the larger towns and cities had cycle clubs and century (100 mile) runs.

The use of tandems, triplets and quads, was far more common than today and six seater bike races were held. Explorations in the field of radio activity, x-rays and electromagnetic radiations were paving the way to a future of atomic power, radiocommunication and many other things.

To me the most important influences of this era were - first, the fact that I had come into contact with and under the influence of a man who was a remarkable investigator, a fine teacher and an uncompromising exponent of scientific veracity and integrity. He was also the most militant Quaker I ever knew - Professor Henry Crew, then head of the Department of Physics of Northwestern University, now Professor Emeritus. The second thing was that my imagination had been stimulated and fascinated by the developments in electromagnetic wave theory and applications thereof, a fascination which has never left me to this day.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER II - 1900-1903

MICHIGAN AGRICULTURAL COLLEGE

CHAPTER II

1900-1903 - Michigan Agricultural College

In the summer of 1899 the family's finances were such that I saw no hope of finishing my senior year in the University. I went to work for the Western Electric Company, Chicago. About this same time, Lee deForest was also working there. He was in the dynamo division and I was in the division dealing with telephone cables and switchboards so that I didn't meet him until a good many years later. Near the end of this summer an aunt living in Lowell, Massachusetts, heard of my predicament and advanced me a small sum of money which enabled me to go back to the University for the opening of the fall semester.

In the year 1900, when I had only one more semester to go before getting my B.S. degree, I ran out of money again and had to look around for a job. Through the kindness of Professor Crew I was put in touch with a job as an instructor of physics at the Michigan State College of Agriculture and Mechanic Arts, in Lansing, Michigan. This school was doing very good work in the field of agriculture but the Mechanic Arts end of it, aside from a very practical course in mechanical engineering, didn't go very far and had very small appropriations with which to function.

When I arrived in Lansing, or rather at the college, which was four miles away from the center of the city, I found that the course in electrical engineering, a short course given to junior and senior students, was given in the Physics Department, not in the Engineering Department. Furthermore, since the new head of the Physics Department who had arrived six months before, not only knew no electrical engineering but would not touch it with a ten foot pole, I was expected to teach the course. The fact that I had never had such a course myself didn't seem to make any difference. I was very fortunate in having a young junior student appointed as my special assistant for this course.

It was soon apparent that while my background and interest in the electrical side of physics gave me certain theoretical advantages, in a practical way this assistant knew a lot more about the work than I did. Between us we made a reasonably good team and sat up nights rigging up experiments for the class and boning on the text book so as to keep one jump ahead of the assignments. Thus I got my start in electrical engineering by the hard way. Incidentally, this assistant was H. T. Brainerd, known as "High Tension" in Lansing days who, the last I heard, was high up in the electrical engineering staff of the Allis Chalmers Company.

It wasn't long before we found an old induction coil without any interrupter which we put into operation after I had invented a cheap electrolytic device to take the place of the broken mechanical interrupter. This gave us a source of high voltage which we promptly connected to a condenser made of window glass and tinfoil plates immersed in a tin of kerosene; this in turn was provided with a spark gap, directly connected to an antenna on one side of the gap and to ground on the other, as in Marconi's earlier experiments.

We had no proper measuring instruments, no idea as to what frequency we were transmitting, or even how much energy actually went into the untuned antenna. This antenna was an insulated #14 wire running up through a hot air register, through the lecture room, through a skylight in the roof, and thus to the flag pole. We did our experimental work mostly after 5:00 P.M. One evening the instructor in military tactics borrowed our lecture room, since it was after the lecture hours of the Physics Department, and assembled his students at about 5:00 o'clock. Before the instructor entered, the students began a little general rough house and started playing with the antenna, swinging it from side to side. Brainerd, in the basement under the hot air register, perceived what was going on and was very indignant. He promptly put the current on the induction coil; the result was that three or four students were knocked endwise and we were subjected to an investigation. However, we were not forbidden to continue with our experiments and proceeded with our next test which was to develop a suitable receiver.

I should perhaps explain that during this period at Lansing, I did not have access to any good scientific library; the only technical periodical available was the Physical Review. This was available because Professor Atkins, Head of the Physics Department, was a member of the American Physical Society and I had the use of his personal library. I was unfortunately not aware of important developments in this and foreign countries which would have been of great interest to me during this period. For instance, Fessenden had developed a very good electrolytic detector in 1902 and John Stone had published papers on tuning coupled circuits.

When I started my work as a student at Northwestern University I used the coherer and was familiar with some papers published on it, notably by Dr. K. E. Guthe and Dr. A. Trowbridge with whom I was very shortly to be associated at the University of Wisconsin. Dr. Carl Kinsley had also published a paper on the coherer. I met him a few years later, during a visit to the University of Chicago. I knew very little of the work of G. W. Pierce and very little of the work then going on in England. Max Abraham, whom I was to meet within a few years and with whom I worked in Germany somewhat later, had started publishing important papers in the Annalen der Physik. In this same scientific periodical, interesting papers were published by Kiebitz, not only on the action of the coherer but on resonance problems of various sorts, the knowledge of which would have been very helpful to me at this time.

During this period Professor Zenneck, whom I came to know later, published a paper on damping of electrical waves and Becker, also in Germany, a paper on interference tubes for electrical waves. These tubes had a rather close relation to the cavity resonators used for radio and radar development during World War II. Dr. Braun of Strassburg was working in 1902 on standing waves on wires. Max Wien had published a very important paper on applications of resonance in wireless telegraphy. Dr. Paul Drude had published many papers dealing with electric waves but none of these things came to my attention until a little later.

During this period a number of interesting things happened of which I had only what vague knowledge I might have gathered from newspaper accounts. By January 1900 Marconi equipment was being installed on British channel steamers and a decision had been made to equip a number of ships in the British Navy. As early as November 1899 the U. S. Navy experimented with Marconi equipment installed on the cruiser NEW YORK and the battleship MASSACHUSETTS. The system was tested up to a distance of 36 miles and apparently gave satisfactory communication up to 16 1/2 miles. The board putting on the experiments recommended that the Marconi system be given a trial in the Navy. The American Marconi Company was started in December 1899.

In January 1900 the U. S. Navy decided that the terms demanded by the Marconi Company were unsatisfactory and Admiral Bradford, then Chief of the Equipment Bureau, U. S. Navy, said that the Navy would develop its own system. About the same time, General Greely, Chief of the Signal Corps, U. S. Army, indicated that the Army expected to do the same and that their experiments were going on in a satisfactory way. During this same month, the American Lighthouse Service started to use wireless.

In May of 1900, Russia announced that by the end of the summer the Black Sea Fleet would be equipped with radio. The British and German Navies started equipping a great many ships. During the first half of 1900, the North German Lloyd Company experimented with radio connecting Bremerhafen with certain lighthouses and light ships. The company accepted paid messages. In the same month the Chief Signal Officer of the U. S. Army announced plans to equip with radio the harbor of San Francisco and certain harbors in Puerto Rico and the Philippines.

It is interesting to note that the early support for practical application of radio came from the world's military establishments, in particular from the Navies. This was natural since when a Navy task force put out to sea in those days, it was very soon out of touch with all land communications and often, especially under the stress of bad weather, the task forces would lose trace of each other - perhaps be entirely out of communication until they reached some prearranged point of rendezvous.

In November 1901, the Marconi Company exhibited in New York some equipment for the Japanese Navy and in January 1901, France announced that the Mediterranean Fleet was to be equipped. The same year the Atlantic was spanned by signals from Clifden, Ireland to Glace Bay, Newfoundland. The French had previously used radio in July 1900 in the naval maneuvers off Cherbourg with ranges up to forty miles. Some of the leading figures in France were Professor Blondell, Captain Ferrie, Ducretet, Tissot, Lecarme and Popoff, a Russian who was also active in Russia's radio program. It thus appears that the military forces, particularly the naval forces, of England, Germany, Russia, France, United States and Japan all started using radio at very nearly the same time. Italy had actually started considerably earlier, with Marconi's work, but for lack of support he was forced to turn to England for backing.

The situation as regards navies of those days was such as to cause two diametrically-opposed views to the use of radio in naval service. The majority of the officers, particularly the alert younger officers, seized upon the idea with great enthusiasm but a lot of the old time admirals and captains didn't like the idea of putting radio on their ships at all. A Naval Task Force Commander, in those days, sailed on orders, sometimes sealed orders, but once he was on the high seas, he was absolutely his own boss and could use his own judgment to an extraordinary exgent. Nobody could reach him to change his orders or to tell him what to do. Some of those old skippers were a pretty independent lot and they weren't by any means one hundred percent in favor of having their ships equipped with radio for fear it would curtail their power and authority. Of course, they were in the minority, but even in the progressive United States there were some men, and good men too, of this type.

In March of 1901, Nikola Tesla, then in the United States, experimented with wireless transmission of power, proposed to send signals to Mars and claimed he had apparatus quite capable of doing it. His theory was that when the Martians were irradicated with his signals, they might be beings of such a nature that they would be able to devise some way of communicating back to the earth. A very amusing comment on this extravaganza was published in the London Electrician. Tesla was advised to try his first experiments on an ant hill 100 feet or so away. "Probably", said the editor, "the wisdom of the ants is likely not to be any more different from similar human attributes than that of the Martians. At least he would have the advantage of knowing that the ant hill was inhabited". Tesla was advised to "go to the ant, study her ways and be wise."

I wrote a brief paper, which was published in the Physical Review in 1902 on my electrolytic interrupter. This paper was of no great value, but for me it had important results, since I had a personal letter in connection with the matter from the editor of the Physical Review, Professor Ernest Merritt (now Emeritus of Cornell). It gave me a great lift to feel that so experienced and distinguished a teacher and scientist was interested in my feeble effort. I may say it also led to a life-long friendship.

The papers on the coherer that had come to my attention directed my interest to the single contact coherer. Now the coherer principle is very old. It was made practical by the investigations of Branly in 1895 and greatly improved by Marconi to give a higher degree of reliability. In essence the coherer consisted of two silver plugs in a glass tube with a narrow gap between which was filled with metal filings, generally nickel with the addition of some silver filings. Marconi had improved the coherer by slightly amalgamating the surface of the silver plugs and evacuating the whole device, which prevented oxydation of the filings. This instrument was hardly a quantitative device, was still very cranky, and was commonly used to operate a telegraphic instrument when the electromagnetic waves produced currents through the coherer which lowered the resistance of these filings and made them conducting, thus closing the circuit of the telegraphic instrument or relay. Means had to be provided for jarring the coherer to render it again non-conducting and thus ready for receipt of the next signal. It seemed to me that it might be possible to get a single contact device which would not require this continual tapping process to restore it to sensitivity and which could be used with a telephone instead of a relay. Experiments already made and with which I was somewhat familiar, indicated this possibility.

The receiver which I evolved for the early experiments at Lansing was a very curious affair, consisting of a telegraph relay with the secondary contacts reversed and made of nickel. The primary and secondary were placed in series so that the thing was hooked up exactly like an electric bell. When a battery was applied to it the movable armature would vibrate back and forth exactly as an electric bell. A telephone was placed in series with the battery. With the addition of resistances the current through the device was reduced to such a point that the contacts, although touching very lightly, just stopped vibrating. A very delicate spring, replacing the usual spring on the relay, facilitated this adjustment. One side of the nickel contacts was grounded and the other connected to the antenna.

Of course this very crude device, which was described in 1902 in the Physical Review, was extremely microphonic so it had to be suspended very carefully with rubber bands and couldn't be used in any room where there were loud sounds or vibrations without continually giving false indications. With this device, Brainerd and I succeeded in picking up our signals out in the fields a mile or so away from the transmitter. We then decided that we were ready to send signals from the college grounds to the city of Lansing, four miles away, where I was living. We made crude attempts to adjust transmitting and receiving equipment to resonance. We were very far from successful.

This curious receiver was evidently sensitive, as the following incident will indicate. I had a second floor room almost across the street from the State Capitol. The house had a central heating system so that I had a ground connection of sorts, but I couldn't get permission to run an antenna up to the roof. So I used an inverted antenna, anchoring it out of my window a few feet short of the ground. This reminds me of what we did many years later in airplane work. Perhaps this was the first use of the inverted antennae.

One Saturday afternoon when I was listening in, trying to pick up signals from the college, I heard a regular series of clicks which gradually faded away. I already knew from experiments at the college that this detector was very sensitive to local disturbances such as turning off an electric light in the vicinity or, in fact, opening any electrical circuit with its consequent sparks at the moment of opening, I diagnosed this as a momentary electromagnetic radiation from the circuit. The house in which I was living at Lansing was somewhat isolated from nearby houses and had no electrical equipment, the lighting system being gas. After these clicking signals were repeatedly received I rushed to the window one day and discovered that Mr. Olds, who had his first factory in Lansing, was driving a two-cylinder car down the street past the house. This curious detector picked up those ignition disturbances for about a block and a half. Perhaps this was the first discovery of ignition interference on a radio receiver. As everyone knows, this is a particular abomination for radio reception in automobiles and airplanes. Many years later I came up against this problem when working for the Navy. Certainly it has given engineers many headaches and it was a long time before successful countermeasures against it were evolved.

My paper on the single contact coherer was published in the Physical Review in 1903. This work was very laborious and painstaking, on account of the susceptibility of the device to shock, vibration and local electrical sparks. I had to work at night since, on account of heavy class work, I had no time to work in the daytime. Brainerd gave me what help he could. We both lost a good deal of sleep over it. Furthermore I was reprimanded by the president of the university for wasting my time on such useless work and burning up my efficiency as an instructor.

During this period at Lansing, I spent two summer school terms at the University of Michigan, not in the field of radio, but in the fields of philosophy, sociology and organic chemistry. This enabled me to take this work for credit to Northwestern University. Finally I was awarded my Bachelor of Science Degree in 1902.

By this time I was pretty much discouraged with my attempts to carry out scientific investigation in a place where there were so few physical and library facilities and where the attitude of the powers that be were so discouraging. At the end of the first semester in 1903, I resigned and accepted an appointment at the University of Wisconsin as instructor in Physics with the assurance that at least half of my time could be devoted to research.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER III - 1903-1908

UNIVERSITY OF WISCONSIN

CHAPTER III

1903-1908 - University of Wisconsin

On arrival at Madison, Wisconsin in February of 1903, I found the Physics Department headed up by three interesting men. The chairman of the Department was Professor Snow, who had worked under Rubens in Berlin in the field of Physical Optics. He ridiculed the idea of investigations in the field of wireless telegraphy (radio) as the sort of work that might be done in a trade school but hardly in a high class college department. He advised me to take up research work in some other field. Snow spent all his time in teaching college elementary physics and gave an enormous amount of time to the preparation of lectures and experiments. I usually helped him with these preparations, learning in the process many new things and many new techniques.

Dr. C. E. Mendenhall, who after Snow's death succeeded him as head of the Department, did his own research work in the field of physical optics and high temperature measurements, but I found him very sympathetic towards research work in any field and was indebted to him for many suggestions. He had, indeed, a very charming personality.

Professor Augustus Trowbridge had worked with Rhigi in Italy; he had charge of the work in the electrical side of physics and had a good deal of sympathy with my desire to work in the field of very high frequencies, that is, radio. However, he advised me to get into precise electrical measurements, holding that in doing so I would be able to merge this field with some high frequency measurements that would satisfy my aspirations. So this was the way it worked out.

Between 1903 and 1908 I published, in the Physical Review, five papers in the field of electrical measurements, particularly in the field of alternating current measurements. This was an excellent foundation for going on later in the field of radio measurements. However, I did fix up one rather spectacular radio experiment for Snow's lecture course which demonstrated resonance in aerial systems and was allowed to publish a paper on it in 1904.

It was from Trowbridge that I received my principal inspiration and when I look back and see what a callow inexperienced youngster I was, I often wonder why he was so kind and helpful. I sat in on all of his graduate courses in electromagnetic theory as well as in some of Mendenhall's graduate courses. Within two years I was not only teaching engineering physics but the courses in direct current and alternating current measurements as well. I was made assistant professor and had a few graduate students for advanced degrees, working under me, though I didn't yet have a post-graduate degree myself.

When I first went to Wisconsin I was assigned a research room which had been recently vacated by Dr. R. W. Wood, who even then was a physicist with most remarkable ingenuity and ability. There were many mementoes in this room of his earlier work in physical optics. He had left Wisconsin to go to Johns Hopkins just before I arrived. I did not meet him until many years later. Trowbridge did his research work in an adjacent room and I often acted as his assistant, particularly in connection with some experiments having very curious and very unusual types of high frequency detectors.

Recently some Italian investigator had reported on a device which can be described as a variety of electrolytic detector. This consisted of a glass tube drawn out to a very fine point and bent up at the lower end, the whole being lowered into a glass of acidulated water. This tube was then filled with mercury to a sufficient height to force itself up to the lips of the capillary at the lower end of the tube. The mercury came just to the surface and had some contact with the acidulated water. A very small battery voltage was applied. Gas was formed on the surface of the tiny mercury globule, thus breaking the surface tension and allowing a spurt of mercury to spill out of the end of it and drop to the bottom of the glass of liquid. By carefully adjusting the pressure of the mercury column and the battery voltage, the situation could be arrived at where the mercury was on the verge of spilling out but did not quite do so. Then a high frequency impulse from a radio signal was connected between the mercury and a metal plate put in the liquid. The high frequency impulses would succeed in breaking the surface tension so that every time the signal came the mercury would spill out. How this curiosity could be used practically I am sure I do not know, but it was an ingenious device and just goes to show how many different things can be used to detect radio waves. The non-technical reader should remember that radio waves in and of themselves have no effect on the human senses unless the human subject is within a very short distance of a high power transmitter. In that case the waves passing through his body set up induced currents in the body which cause heat to be developed and the body temperature to rise. This effect is what is used in electrotherapy with the so-called inducto-therm machines. As far as we know today, in spite of occasional letters from cranks claiming otherwise, there is no noticeable effect on human bodies or any of the senses of man from feeble radio waves coming from any considerable distance. So it was necessary to invent these radio detectors to transform the effect of the radio waves into something such that the end result could appeal to the human senses either aurally or visually.

As mentioned earlier, Rutherford had done pioneer work on the magnetic detector in 1895 and Marconi had greatly perfected it and put it into practical use in about 1902. I helped Trowbridge experiment with a rather unusual type of this device. Scientifically it was very interesting. Practically it could hardly compete with already existing magnetic detectors.

These magnetic detectors operate roughly in the following fashion. A magnetized wire will have its molecules more or less shaken up by being subjected to the influence of currents produced by radio waves, usually by passing those currents through small coils wound around the wire. Jolting these molecules with high frequency changes the magnetic condition of this magnetized wire; this in turn induces in another coil around the wire a low frequency current which can be heard in an ordinary telephone. The Marconi variation consists of a continuous magnetized wire passing close to the poles of a magnet and then through the necessary coils to perform the operations I have described.

The Trowbridge experiment was done with a straight iron wire about three feet long clamped at the top. This wire was surrounded with a magnetizing coil which gave it longitudinal magnetization. After the wire was twisted at the bottom, the magnetization became twisted up; some of it went out sidewise through the coil and affected another coil connected to a telephone receiver. Trowbridge was interested principally in what we called the torsional magnetic hysteresis of this iron wire, but I discovered in some way that the presence of high frequency influence, or radio waves, affected the output of this thing and made a detector of it. I spent some time on this device and tried to get a simple portable form that would have practical uses, but found that its sensitivity was so low that I lost interest in it and never published anything on the subject.

The Physics Department had an excellent scientific library. I was able to catch up in my scientific and technical reading and find out what was going on in this and other countries in the field in which I was interested.

Most engineers today would say that the crystal detector succeeded the coherer; however, as far as practical application in radio is concerned, the magnetic detector and the electrolytic detector came earlier. The Fleming valve was also in the picture. It was certainly one of the first, if not the first, vacuum tube detector. The electrolytic detectors seemed to have been almost simultaneously developed by Ferrie, Fessenden, Nernst and Sloemilch. Such a device as a rectifier for low frequencies was evidently used by Dr. Pupin at a much earlier date. As indicated earlier, the Fessenden device came out in 1902. Ferrié was at that time captain in the French Signal Corps. Later he became the first commandant of the famous Eifel Tower Radio Station in Paris. I came to know him very well when he was a general, and head of the French Signal Corps in World War I.

During this same period, crystal detectors came into being. Dr. F. Braun of Strassburg was working with such detectors and suggested them to the Telefunken Company of Germany. Work was being done in this country by Pickard, Austin and G. W. Pierce. The first American patent was taken by General Dunwoody of the Army Signal Corps in December 1906. Eccles in England carried out elaborate studies of these detectors which at that time presented somewhat of a scientific puzzle. In essense, they are valves which let high frequency currents go through much more easily in one direction than in another, or else they are devices which develop pulsating currents of thermal origin when excited by currents produced from radio waves. These pulsating currents can then be heard in the telephone. Due to the low cost and simplicity of these devices, they soon became more widely used than any other detector, and it is interesting to note, since the advent of the extremely high frequencies, they are still widely used today. I became familiar with all this and with many papers covering resonance and tuning of coupled circuits, etc. which I should have had at my disposal while I was at Lansing.

The first use of radio in Naval warfare appears to have been in 1904 during the war between Russia and Japan. Admiral Togo kept in communication with his advanced squadron although the main fleet was out of sight of Port Arthur.

At this same time, in April 1904, there appears the first instance of the use of radio by war correspondents. The New York Times bought a Chinese ship called HAIMUN and equipped it with radio. The Times apparently had at least the tacit consent of the Japanese for this venture. Reporters were close enough to observe some of the battles and sent their dispatches, in spite of noticeable interference from the Japanese naval transmission, to a shore point, probably in Japan, whence it could be put on the cable to New York the same day. This procedure greatly annoyed the Russians who announced that if they captured the ship, they would hang the correspondents at the Yard Arm for violation of neutrality and for disclosure of positions of Russian units. There was a great deal of discussion over this incident, the opinion generally being that such activities on the part of the correspondents would usually have to be submitted to military regulations and censorship.

So far all high frequency oscillators capable of generating radio waves involved a condenser and spark gap. The condenser was charged and then discharged through the spark gap, setting up these oscillatory currents which were then coupled into the antenna from whence the radio wave went out. But it would not go out continuously. The waves died away completely between sparks and they even died away during the time the spark was functioning; in other words, they were waves that started strong and then tailed off very rapidly. Furthermore, they were emitted in groups corresponding to the way the spark gap tripped off the discharge. The better theoretical foundation now available on matters of tuning and resonance clearly pointed out that many things would be improved if these radio waves could be made more continuous and less intermittent. Tuning operations could be more precise and adjacent stations would be able to operate with enormously less interference.

During these years then, various physicists and engineers were interested in the problem of producing continuous or more properly, undamped waves. Duddell in England experimented with an arrangement of carbon electrodes in the form of an arc and produced oscillations of fairly high frequencies in this way. These oscillations approximated very closely to being continuous waves. Poulsen, in Denmark, improved the Duddell arc and his work is described in his Danish patent of 1902 and his German patent of 1903, paving the way to revolutionary changes. Incidentally it may be said that they paved the way for the arrival of radio telephony. Of course the spark transmitter by no means faded out of the picture at this time and many attempts were made to improve it so that the oscillations did not fade away so rapidly.

Experiments with high frequency alternating current machines for radio began near the end of this decade.

During this period at the University of Wisconsin, I met many interesting scientists who became internationally known figures. Among them were Dr. Michelson, then head of the Physics Department of the University of Chicago, and Dr. R. A. Millikan who was teaching electrical measurements at Chicago. Millikan was even then getting into the field of atomic physics and setting up experiments which led to his measurements of the radio of charge to mass for the electron. I can well remember the comment which these disclosures occasioned at the time. Millikan was one of the most genial, enthusiastic and likeable men I met in those years; simply bubbling over with energy and enthusiasm for his work.

During this period I met Sir Ernest Rutherford, of Magill University, who was doing revolutionary work in radio activity, which led to the first experiments in the transmutation of the atom. During an informal meeting someone asked him how he managed to turn out such a tremendous volume of work and how he was ever able to go to sleep and forget these epoch-making experiments. (It was well known that he worked long hours and never spared either himself or his assistants.) He replied that he had about a hundred paper-backed thrillers, more or less of the Diamond Dick type, which he never remembered for more than two or three days, so he was able to read them again. Every night he read one of these through and put himself to sleep.

A man who later had a marked influence on my career was Max Abraham who, during this period, published his famous volumes on electron theory. When he visited Wisconsin I was a member of the party which entertained him. I was tremendously impressed by his brilliant mind and incisive way of going at things. This resulted later in a friendship which ripened after I went to Germany. It was during these years too, that I came in contact with a Dr. G. W. Pierce of Harvard, now Professor Emeritus; this contact also led to a long friendship and I hardly have the words to express the admiration I have always had for his versatility, thoroughness and experimental ability. If anybody had offered me a job to work under him at Harvard I would have taken it at the drop of a hat, but I couldn't afford to pay my own way.

During the last year, 1907-1908, that I spent at Madison, Trowbridge went to Princeton where he ultimately became dean of the graduate school. I missed him very much, but it gave me an unusual opportunity since there was no one else to teach his courses, both graduate and under graduate. I was given the job. I must have struggled through it some way because in the summer of 1908 I was given a year's leave of absence with pay, provided I would use that advantage for graduate study. I then determined to go to Germany. On the way East I stopped at Northwestern University to see my old friend, Professor H. Crew, to take his advice as to where to go. I told him what I had been doing and the field I would like to get into. He looked me up and down and said, "Taylor, what you need is to study spiritualized electrical engineering", which was his way of describing engineering research. "I would advise you to go to Dr. Braun at Strassburg or else to Dr. Simon at the University of Goettingen".

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER IV - 1908-1909

GERMANY

CHAPTER IV

1908-1909 - Germany

Dr. Braun of Strassburg had already published a number of papers dealing with radio, particularly on crystal detectors, various circuits, transmitters and receivers, and was investigating the directional properties of an antenna arrangement in the form of tuned loops both for transmission and reception. He had also incorporated the Braun Wireless Telegraph Company and had assumed a prominent position in radio matters in general.

Professor Simon was best known for his work on the theory and application of what he called the dynamical characteristic curves of various electrical devices. He had developed the theory of the alternating current arc, including the high frequency Poulsen arc, which was then the most satisfactory source of undamped or continuous electromagnetic waves.

There were many others working in radio matters in Germany. Professor Max Wien was very active in both theoretical and experimental work involving radio circuits. The idea of attempting to improve the emission from ordinary fixed spark gaps led to the use of the rotary spark gap. This rotary gap was considered more efficient than the fixed gap and gave a much more musical note, which could be received after passing through a suitable detector, in a telephone receiver. Due to its clearer tone it could be much more readily distinguished from casual electrical disturbances and from other interfering signals, which might readily be of a different tone. It should be remembered that the very early spark sets gave a very rough, unmusical and irregular noise, when heard by way of head telephones.

In the search for more efficient spark systems, the idea of impact excitation appeared. The purpose of impact excitation was to get a device which would let loose a lot of high frequency energy in a circuit of such character that it would very rapidly decay. In the impact system the spark gap accelerated the decay by opening up the primary circuit when the intensity of the oscillation reached a low level. The antenna, or radiating system, was not connected directly to this high impact circuit, but to a coupled secondary which managed to absorb a reasonably high percentage of the primary circuit energy and continue oscillating for a considerable period of time, relatively speaking, after the primary circuit had been extinguished. While this did not give a continuous wave, it did give a wave which sustained itself much longer than with ordinary spark gaps and allowed much sharper tuning, thus permitting competing stations to approach each other more closely in frequency without mutual interference.

In Germany, Lepel had introduced the so- called Lepel gap and the Telefunken Company was about to put in Count von Arco's quenched spark. Peukert produced and patented a rotating disc gap in 1907 which had quenching properties. Many other investigators in Germany could be mentioned, but on the whole the work at Strassburg seemed to me the most interesting.

I left Madison in midsummer of 1908, going to Montreal and taking a few side trips. I sailed for Liverpool, intending, after a tour of England, to proceed to Strassburg. I met a man on board ship who had worked with Braun at Strassburg. He told me that Braun was a very difficult man to get along with and did not have much patience with foreign students. It is likely that Braun desired to keep many of his investigations secret because of his connection with commercial radio. At any rate, I decided to try Goettingen instead of Strassburg.

After arriving in Liverpool, about the first of September, I purchased an English bicycle, sent most of my belongings on to Germany and proceeded to tour England, Scotland and Wales on a bicycle. This tour was mostly recreational, with plenty of hard exercise. I made no attempt to visit places of scientific interest except on two occasions. After leaving Edinburgh, I crossed through the highlands to Glasgow and decided that I would like to see the laboratory at the University made famous by the work of Lord Kelvin. Kelvin, of course, was dead at this time, but Professor Gray had succeeded him as head of the Physics Department. Having read a number of his papers, I was bold enough to look him up and introduce myself. He was very kind to me, took me through the laboratories, and gave me a pass through the Kelvin commercial works where I spent a busy half day. It was amazing to note the number of different branches of physical science in which Lord Kelvin distinguished himself. He had a distinct flare for engineering application and he was a great man to build models.

Those models represented almost everything under the sun and a good many that were not under any sun but were just ideas. A large number of them had to do with the structure of matter. I was taken into one room of the Kelvin Laboratories where shelves, reaching to the ceiling, were loaded with hundreds of papier-mache models of which, Gray told me, Kelvin had made many with his own hands. Among these was one rather large affair which looked like a replica of some strangely-shaped combination of hills and valleys. I couldn't imagine what it was, but Gray related an interesting story about it. Willard Gibbs, the famous mathematician at Yale, had visited Kelvin some time before Kelvin's death and Kelvin showed him through this same model room. He picked up this particular model and showed it to Gibbs, asking him what it was, Gibbs looked it over carefully but admitted he couldn't imagine what it represented. Kelvin smiled and said it was a "Gibbs" thermo dynamic surface. This shows how differently the minds of famous men may work - to Gibbs the thermo dynamic surface was a mathematical conception with which he was entirely satisfied, but Kelvin couldn't understand and appreciate it until he made a model of it.

Later I came to Cambridge, and as I was wandering around among those beautiful old buildings, I got courage to see if I could locate Professor J. J. Thompson (later Sir J. J. Thompson) who has been mentioned as the discoverer of the corpuscle, which turned out to be the electron. I had no idea whether he would be at work, since classes were not yet started, but someone directed me to a building where he might possibly be found. This building was very poorly furnished and was not at all impressive. I wandered about from room to room, sighting no one. Finally I saw a man, in rather baggy looking clothes, on top of a step-ladder, screwing porcelain cleats to the ceiling for running an electric circuit across the top of the room. I took him for an electrician or janitor, so I approached him and asked him casually if he could tell me whether Professor Thompson was anywhere about. He looked at me for a moment, stepped off the ladder and said, "I am Thompson". He gave me something of a shock to realize that I was face to face with a man who was considered the foremost physicist of Europe, yet was forced to do his own electrical wiring for want of adequate appropriation and help. He was very nice to me, even inviting me to dinner. I tactfully declined, since I had only outing clothes with me, the rest having gone ahead to Germany. He asked me if I was thinking of working in the Cambridge Laboratory. I told him that I didn't feel particularly qualified for that line of work, much as I admired it, was frankly more interested in high frequency work in the field of wireless telegraphy, and expected to find it in Germany.

I left England near the end of September. After about a week of riding around in Holland, I took the train for Goettingen, arriving there the sixth day of October. In due time I matriculated at the University and registered for work in pure mathematics, mathematical physics and applied electricity. I had no expectation of staying more than one year, so did not announce myself as a candidate for a doctor's degree.

Goettingen at that time had a good many men famous in their respective fields. Minkowski had published his famous paper on time and space. He died of pneumonia later that year. Max Abraham was lecturing on Electron Theory; Professor Riecke, who was then quite an elderly man, headed a group doing excellent work in radio activity; Professor Herman Voigt, famous for his work in magneto optics, crystal optics, etc., was giving lectures in mathematical physics and thermodynamics. Professor Prandtl had just finished the first wind tunnel in the world and was still arranging experiments in aerodynamics. Professor Hilbert who, next to Poincare' of France, was the most famous mathematician in Europe, specialized on theory of complex variables.

Professor Simon was head of the Institute of Applied Electricity. He was assisted by Max Reich and Hans Busch, both of whom published a number of papers along radio lines. The Simon Institute had an experimental military radio station where research work was going on under the direction of Professor Simon. Simon had a good setup, pretty much on his own, to run in his own way. He was a man of extraordinary force and character, full of enthusiasm, but very impetuous. He could at times explode quite violently, but he was a marvelous teacher and very popular with his class of twenty graduate students. He was a close friend of Max Abraham although no two men differed so completely in their method of approach. Abraham couldn't put two wires together without forgetting to take off the insulation, but could look over a man's shoulder and tell him what was wrong with a circuit in a few minutes - in other words, he was a mathematical physicist with emphasis on mathematics, whereas Simon didn't believe in anything that he couldn't somehow visualize, or at least of which he could form a mental picture, but he was a wonderful experimentalist.

I had several weeks, before taking up work seriously, in which to improve my German. I could get along with scientific German in the field of physics or electrical engineering well enough; that is, I could read it fluently, but I didn't know much about ordinary conversation. For this reason I picked out a room in a pension where no one spoke English. I had no trouble in making a number of interesting acquaintances, and since the only common language we had was German, I learned conversational German rapidly. Still, my first interview with Simon was pretty tough and, I will admit, I was rather scared. He insisted on talking in German, although I knew that he was able to understand quite a little English. Finally I got it across that I wanted to get into radio, or at least work with very high frequencies.

"Well", he said, "do you know what an electrical valve is?"

"Yes", I said, "we use such things in radio detection".

"Quite correct", he said, "how many different kinds of valves can you name"?
I named the crystal detector, the copper oxide rectifier, the aluminum electrolytic rectifier, the point to plane gaseous discharge and the Fleming valve.

"Well", he said, "that is pretty good How would you like to take as your problem the investigation of electric valves?"

I agreed to this readily enough. Simon then asked, "Which do you think would be the simplest in operation, therefore the easiest to investigate?" My reply to this was that, since we seemed to know a good deal more about conduction of electricity through liquids than through solids or gases, it seemed that an electrolytic rectifier such as the aluminum cell might prove to be simplest in operation. It should be noted that this rectifier is not very suitable for high frequency. I wasn't very keen to go to work on it. Simon replied that my conclusion was justified and I had better start with the simplest of valves and when through with that, I could go on with a more complicated one.

The result was that I spent practically the whole year investigating that one valve and finally published a paper on it in the Annalen der Physik. Although disappointed that I didn't get directly into radio work, I did get a good bit of radio information from the papers presented in the seminars and things I picked up from the people with whom I was directly associated.

At the time I went to Germany, that country was an acknowledged leader in scientific research and was well towards the front in engineering development, but by no means so in engineering production and practical application to the things of daily life. Telephones were rarely found in private homes. No German hausfrau would think of ordering her meat and vegetables over it even if she had a telephone. On the contrary, accompanied by the kitchen maid, she proceeded on foot to the market with a large basket, and looked over everything carefully before buying.

The general use of checks for payment of bills was practically unknown. Only the bank where you had an account would cash your check. The use of electricity for illuminating purposes was rare in the private home, at least in a city of 30,000 the size of the one in which I lived. Low prices, due to efficient mass production, had not arrived; so that many things, which even then we considered necessities in this country, were in the Germany of that year considered as luxuries, only at the disposal of the wealthy. On the other hand, railroads were very efficient and well run. The parcel post system, which was very efficient and cheap, had been introduced several years prior to my arrival. A German acquaintance of mine told me that he had visited a place on the North Sea and had taken advantage of the parcel post. He packed a nice mess of fresh fish in a box with plenty of ice, and shipped it by parcel post to some friends in southern Germany. A little later in the summer he visited these friends and asked them how they liked the fresh fish. They were very much embarrassed and replied that, although they had hung them up for two weeks in the meat room, they still weren't fit to eat. The average peasant German knew how to hang meat to cure it but had no idea that fresh fish should be eaten immediately.

In contrast to this, German scientists and engineers enjoyed a prestige and respect which was by no means equalled in our own country at that time. The school systems were efficient, well organized and well run. The universities, although much more similar to our universities than to English universities, differed quite radically in many respects. The head man, generally with the title of rector, in the Imperial university had a seat in the Reichstag, corresponding to our United States Senate. Compulsory military service was in effect and every man had to serve either one, two or three years according to his educational status. I had several friends at Goettingen who were making their one year of military service, at the same time attending certain classes at the University.

The lowest grade of work given in a German university corresponded roughly to the senior year in an American University. The majority of the students were potential candidates for the doctor's degree and therefore corresponded to American post graduate students. Women had for some time been admitted to attend the university classes but not allowed to come up for degrees. The year I arrived was the first year that the universities were authorized to give degrees to women students, but there were very few of them in residence. One of these was a very brilliant Russian girl who was specializing in higher mathematics. I knew of no German women taking courses in scientific work.

Perhaps the strangest thing about the German Imperial University was that there was not the slightest attempt to compel a student to study or to insure his attendance in classes or in the laboratory. There were absolutely no tests or examinations given until the final examination, which occurred after a man had declared himself as a candidate for a degree, and a committee had been appointed to examine him. If a man registered for a certain course, he had only to attend the first and last lectures of that course to be credited with full attendance. The idea of the University having any responsibility as to what the student did with his time or his efforts simply did not exist.

It was properly claimed at this time that not over one-third of the students ever came up for their degrees, because the final examination was extremely thorough and it was considered a disgrace to fail in the examination; therefore, men who were not sure of being prepared never applied for examination. Another third of the German students drank themselves to death; these were mostly the men attached to the semi-political secret societies such as Germania and Saxonia and, to a certain extent, those who belonged to the different student unions. The remaining third of these students became the future rulers of Germany. When Germany submitted to the rule of Hitler the paper hanger, she violated this old precept.

Student duels were frowned upon by the authorities, but no serious attempt was ever made to stop them. In general, the scientific students seldom belonged to either the "corps" or unions, and the duels were usually confined to these fraternities. Ordinarily they were fought, without any grievances being involved, between the groups of men of two corps whose qualifications were such that they could be appropriately matched against each other. Often the men didn't know each other until they came together to fight. These duels were not in the least dangerous, although rather bloody affairs. Foreigners were seldom allowed to see them but I happened to have enough influence with certain friends to permit me to witness quite a number of duels.

Limitations of the movements of the sword arm, and the bandages in which the contestants were swathed, prevented cuts being received anywhere except on the face or the upper part of the head. Since two surgeons and plenty of antiseptic were always present, there were practically no cases of infection. Wounds healed up very quickly unless the recipient, who was always proud of his wounds, rubbed salt in them to get a better scar. I knew one duelist, with seventeen scars on his head and face, who always kept his head shaved to display them properly. I never could make out whether a man took the most pride in defeating his opponent or getting scars himself. There was never any cheering from the side lines and everything was conducted with a high degree of decorum. A boy at the back door of the inn where they usually fought could be seen busily sharpening swords for the next round. The schlaeger, or sword with which they were fought, was a long thin flat blade with rounded point which was sharp as a razor at the point and six inches back. It was only the tip of the sword that ever came in contact with the victim.

The Germans insisted that this exercise was less objectionable than the American game of football, because the cuts on the head and face didn't disqualify a man from earning a living, whereas if a man got a broken leg or strained it from time to time in football, he might feel it the rest of his life. They admitted that either sport could teach a man fortitude, nerve and decision. Even my Chief, Professor Simon, was scarred up a bit from duels he had fought in his student days, but I know of none of the serious minded students at his institute who fought duels.

Dr. K. W. Wagner, who afterward became the head of a famous laboratory in Berlin and a very distinguished man in the field of electrical and radio engineering, practiced continuously with a saber. I asked him why he did this and whether he really approved of duels. He said he didn't, but of course, if someone insulted him, he would have to fight. I said I didn't approve them under any circumstances. "What would you do" he asked, "if someone insulted you?" I told him that if I was big enough, I would knock the man down. "Oh" he said, "that might be all right for a cab driver, but a gentleman can't do that sort of thing". So you see, it is all in the point of view. It is not very many years ago when many people in the United States talked the same way.

The duels that were fought as a result of an insult were much more serious matters and the authorities made a strenuous effort to stop them, but didn't succeed any too well. They were generally fought with a cavalry saber and were very savage affairs. I didn't see any of those and didn't want to. Still a third type was occasionally fought by army officers, generally over a woman, of course, and since they fought with Mauser pistols at 10 paces, one or both of them were usually killed. These duels were strictly forbidden by the emperor, but since the only penalty for fighting one was confinement for two years within the boundaries of the walls of a town or city, they still happened occasionally.

It was a fixed rule at the University that whenever a man was awarded a doctor's degree, or when he suddenly left the university for any reason, he had to give a feast, which was known as a Kneipe. These feasts consisted pretty much of beer drinking, songs and speeches, but since they always used local city brew with less than 2% alcoholic content, an astonishing number of toasts could be drunk without any ill effects. Indeed, in my whole year in Germany, I saw far fewer Germans intoxicated than I did foreigners, probably largely due to the fact that the German preferred beer and light wine and shunned highly fortified liquors.

The men with whom I studied were all interesting characters; Hilbert, a mathematician, was a red-headed, irascible Koenigsberger; he had a frightful dialect and was very difficult to understand in a lecture room, but he was definitely a creative genius in mathematics. Although the top flight mathematician in Germany, he couldn't add up numerals in his equations involving the simplest arithmetic without the aid of the students in his class. He never seemed to be quite sure whether 2 + 3 made 4 or 5. He suffered from a skin infection or irritation, probably some form of acne, which turned his bald head a brilliant red when he was exasperated or seriously perturbed.

Voigt, under whom I studied mathematical physics, was every inch an aristocrat and a wonderful lecturer. He was a handsome, gold-bearded giant, but rather aloof with his students. He was also very fond of music and a virtuoso on the violin. Once every year he organized, conducted personally, and financed, a Bach festival. He always maintained that no man since Bach had written music fit for a violin.

Max Abraham, with whom I studied electron theory, and with whom I was very friendly thanks to our former contact in Madison, was very suave, very slow and precise, in his lecture work. He was very friendly with students who were really interested in his work and often, after one of his two hour lectures, he would go with his students across the street to an inn or cafe, and over a sandwich and stein of beer, help us work out our lecture notes with his comments and assistance. He was, at that time, a fairly wealthy man and didn't hesitate to dismiss his classes a few weeks early in the summer if he thought it was unreasonably warm. Since the Professors got a considerable percentage of the fees for these different courses, he lost part of his fee by this procedure, but that never seemed to influence him. He had very little patience with people who were not genuinely interested in his work, or who had not the proper mathematical and physical foundation to appreciate it. I remember once he announced a course on very advanced electron theory (what we called fourth dimension stuff) which was a very tough course indeed, and for which we had no suitable text book. Abraham was somewhat famous, not only for his brilliant ability, but for the unusual way in which he treated his students; that is, unusual for a German professor.

When the class assembled for the first time there were twenty two of us. However, Abraham was sure that there were not twenty two students in the University qualified to take this course, so he looked us over calmly for a few minutes and then said he was sure there must be some misunderstanding; that the prerequisites for this course were so and so, and he couldn't believe that so many people possessed these rather unusual qualifications. He then suggested that we adjourn for fifteen minutes, talk the matter over among ourselves, after which those who though they were really prepared could then return and he would begin the lecture. So we all sheepishly filed out. The group immediately began to disintegrate; at the end of fifteen minutes six of us marched back. Abraham smiled broadly and remarked that this was very much better. He was more interested in working with a small group of well prepared and definitely interested people than in lecturing to the masses.

It was possible for a student who didn't wish to attend the lectures regularly to acquire the knowledge in another way, if he so desired. This was particularly useful for foreigners whose German was adequate for reading the language but not adequate for understanding the spoken word. There were a number of reading rooms adjacent to the lecture rooms. There would always be one particularly ambitious student in each class who would take great pains with his notes on the professor's lectures. He had the privelege of taking these notes to the professor (with the latter's consent, of course) and having them checked. If they were adequate, and the professor wanted it, he then deposited them from day to day in the reading room. Any student of the class, by paying a fee of one mark (25¢) could obtain a key to the reading room and consult these official notes. I know several students who never attended any but the opening and closing lectures, but kept up with the course very well by spending hours in the reading room. I never availed myself of this method as it eliminates a certain amount of inspiration which one obtains by listening to the lecture.

At first I tried to take down my lecture notes in English, translating the German words into English as I went along. Of course, I found I couldn't keep up with the speaker. So then I took the notes down in the best German I could muster, which was very poor at first, then translated them into English as soon as I got back to my room. I didn't make very rapid progress by this method either. Finally I took them down, in very abbreviated and primitive German, and then that evening I would transcribe them into good German, thus keeping my thoughts all in the same language. At the end of three months I was doing most of my thinking in German and got along almost as well as I would have with English lectures. When I found that I could dream in German more readily than I could in English I knew I had the language.

Not long after I arrived at Goettingen, Max Abraham called on me and invited me to join an informal group made up of what I would call assistant professors, instructors and advanced graduate students. It was a rather small affair. We met every evening in a quaint old inn, which could be reached only by way of a couple of alleys. Here the food and beer was very good. The session began at eight and ended at exactly eleven when Abraham, who was the unofficial president of the organization, finished his third glass of beer. This group was a free forum to discuss anything physical or metaphysical, military, or religious; nobody, not even the emperor, escaped criticism. One of the members was a student and also a lieutenant in the infantry, on service with the Goettingen regiment; another was a lieutenant in what would now be called the Luftwaffe and was interested in balloons. The German military ballon Parsifal came out while I was there and I saw it in flight; also the early Zeppelins.

The talk in this little group frequently touched on military matters. I well remember a session when I was assured that there was no good reason for future wars between France and Germany, as they were both beginning to understand each other very well. Indeed, at that time the Germans were adopting new French words into their language; these words, incidentally, were all kicked out again under Hitler. Only a few weeks later the Army lieutenant remarked that it was only a matter of time before war came between them and the English and when it did, they would smash the French in six weeks. This was in the fall of 1908 or spring of 1909. I said that seemed strange, when they had told me before that the feeling was good between France and Germany. The lieutenant shrugged his shoulders and said "That has nothing to do with it; when war comes France will be England's ally." It is evident that the Army had already planned to drive through Belgium to Paris - which was attempted by General von Kluck after 1914. If von Kluck's ammunition train had managed to keep up with him, he might not have been stopped by the Paris taxicab army and the time would have been exactly six weeks.

On another occasion a man, who afterward became very distinguished in the field of foreign affairs, asked me why the United States was not preparing now for war with Japan. I replied that we had no reason to go to war with Japan; that we were excellent friends; we had good trade relations with them, and no cause for friction. He laughed and said "What about the Pacific? The domination of that ocean will have to be settled between you two and the sooner you settle the Japs, the easier it will be for you". It is characteristic of the Germans, either in science or military affairs, to look a long way ahead. It is perhaps also characteristic, that if these long range plans have sufficient resistance to block them at some point, they do not readily extemporize new ones.

These discussions at the "Black Bear", as the inn was called, used to get so violent that I thought there would be some fighting over them, but the minute the session was ended, everybody grinned and went off arm in arm. Needless to say, participation in this sort of thing rapidly increased my knowledge of the German language and gave me unusual insight into some of the things of very large consequence of which the Germans were even then thinking.

There were altogether, in various courses, about forty in the American colony, not counting the Canadians who were far less numerous. The British had about a dozen; among them Dr. Robinson who, during World War I got into aircraft radio work for the British Air Force at about the same time that I was doing it for the Navy for our country. Later he made himself well known by his advocacy of the so-called Stenode broadcast receiver, which was supposed to reduce interference from static or other man made disturbances which we all experience on our home receivers. No doubt these disturbances were reduced, but so was the quality of the signal, which was too high a price to pay. However, Robinson worked in England on other subjects, particularly the crossed coils direction finder and in general, his work was excellent.

Dr. Hodges, who for many years served the British Admiralty in the same capacity in which I served the United States Navy, was also there, He was a quiet, unobtrusive man of great industry and excellent ability and understanding. I have heard from him several times during the late war through mutual English friends, but I have never seen him since we left Goettingen.

Of course, being a dyed in the wool experimentalist, the work which gave me the greatest happiness was that under Simon at the Institute of Applied Electricity. I managed to carry my experiments on the aluminum cell up to fairly high frequencies and was looking forward to further work on other electrical valves at radio frequencies.

Shortly after the first of the year 1909, Simon asked me why I didn't come up for a degree. I said I couldn't meet the three year residence requirement; he told me to go to see the dean, because if he, Simon, was satisfied, the dean ought to be satisfied. I didn't take the matter very seriously because the German regulations were known to be very strict. However, I went to see Dr. Karl Runge, who was an outstanding figure in the field of applied mathematics, especially in the field of mechanics, and had served as imperial exchange professor at Columbia University. In a very few minutes, to my great astonishment and dismay, I was accepted as a candidate. I realized that I would have to do some very hard work to prepare myself for this examination.

Robinson and I came up for our doctor's examinations within a week of each other. We formed an alliance of mutual assistance during the strenuous days of hard preparation which preceded the examinations. This worked very well because, due to my teaching experience and background, I was better qualified in physics; he on the other hand, was a far better mathematician than I could ever hope to be. So we coached each other to our mutual advantage.

The University then had some peculiar customs and ceremonies connected with taking a degree. For one thing, the candidate had to put on a silk hat, full dress coat and white gloves in the middle of the day and make a formal call on each member of his examining board. Fortunately the members were generally absent and you had only to leave a card, which fulfilled the necessary courtesies. Furthermore, when you reported for the examination, you wore the same regalia. If it happened to be a very hot summer day, it was something of a torture.

In spite of being so busy the second half of the year, I managed to find out a little of what was going on at the military experimental radio station under Simon's direction. They had a good quenched gap transmitter and a fair sized umbrella top antenna which radiated a frequency in the neighborhood of 200 KC. The site was in a flat meadow not far from the river. The antenna could be operated either with the usual ground connections or in connection with a counterpoise, which is a second antenna surface made up of wires stretched a short distance above the surface of the ground, and usually larger in area than the elevated portion of the antenna. The Germans were trying to increase the efficiency of their radio transmitters. The main problem centered around the evaluation of the different losses in the transmitter system. They wanted to separate out the losses, within the antenna and in the ground itself, especially under different weather conditions. There was probably considerable other work going on at the same time which I was not allowed to see, because it was jealously guarded by the military.

Not long before I came up for my examinations I made drawings of a rectifier, a device converting alternating current into pulsating direct current. I submitted it to the head mechanician before construction. The drawings showed a high vacuum, indirectly heated hot cathode rectifier, surrounded by a water-cooled hollow nickel plated anode. After a considerable delay, this drawing came back to me with a note from Simon disapproving the gadget, stating that there were other valves which should be investigated first. I always believed that the Germans realized what an important device this might come to be, and didn't want an American to get the credit for it.

I was so anxious to get a chance to work in the radio station and the laboratory attached to it, that I finally prevailed upon Simon to take me in on that work the following year. I think they made a considerable concession in order to do this. I had, in the meantime, with the permission of the University of Wisconsin, resigned my assistant professorship. I had learned to live economically in Germany, so that less than half of my funds were expended at the end of the first year. Thus I estimated that I could get in another year's work and still have money enough to get home. With the prestige of the added experience and a German degree, I should be able to pick up a good job in my own country. Actually it didn't work out that way, for reasons that will soon become evident.

Early in August of 1909, having arranged for the rental of a room closer to the Institute of Applied Electricity than the one in which I had been living, I departed for a bicycle trip, meeting a friend (who happened to be the son of the Consul in Hanover) at the city of Muenchen (Munich). From here we proceeded through the beautiful country of Tyrol and on to Switzerland. We used to spend a day or two walking up a steep mountain valley, pushing our wheels laden with our baggage, stay a few days on top and then in an hour, coast down to lower altitudes again. Sometimes we left the bikes and proceeded on foot, with pack sack, for short trips. Coming down the valley of the Rhone, we stopped at night in Martigny. There a cable from the United States caught up with me and changed all my plans. This cable offered me the position as Head of the Physics Department in the University of North Dakota. This position had just been vacated by my friend Dr. Walter Steward, who had gone to the University of Iowa. The salary was pretty fair for those days, so after thinking it over for fifteen minutes, I cabled back an acceptance, feeling that if the job didn't suit me I could at least stick it out for a year and come back again to Germany, with my finances in still better shape than they were at the moment.

I took the train for Goettingen as soon as possible, cancelled my arrangements for a second year, and by good luck picked up a second class steamer reservation to New York.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER V - 1909-1918

University of North Dakota

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1909-1918 - University of North Dakota

I arrived at the University of North Dakota, Grand Forks, North Dakota only a few days after the University had opened for the fall term. The President of the University was Dr. Frank L. McVay, a man of high ideals and standards and friendly to research. There was no work in the field of high frequency or radio in progress anywhere in the University.

From the first I had it in mind to start such work in my own department, which was left in bad shape by resignations. There was only one assistant, Professor J. M. Rysgaard, who a few years later went to Hamlin University in St. Paul and retired from there quite recently. He was a fine man and a fine teacher, but was badly overworked.

I spent the first year organizing the department, having neither opportunity nor money to build up work in the field of radio. I succeeded in getting Dr. B. J. Spence, now head of the Physics Department of Northwestern University, as my associate professor, and Dr. E. B. Stephenson, now Associate Superintendent of the Sound Division of the Naval Research Laboratory, as assistant professor.

I took special interest in the courses in electrical measurements, particularly in a theoretical course in alternating currents which didn't duplicate anything given in the Department of Electrical Engineering. In this course I succeeded in working in a good bit of general electro-magnetic theory. I mention this because I had in this course as students some men who afterwards became very well known in the field of radio. One of these was R. A. Heising, who for many years had been with the Bell System, and who has so many inventions to his credit that it is no use mentioning more than one as a sample, namely, the Heising System of Modulation. Another was Dr. J. B. Johnson, a physicist also connected with the Bell Laboratories. Arthur Kishpaugh was also one of my students. He too is on the engineering staff of the Bell Systems. Still another was Harry Nyquist, who came to us from a small sectarian school in Minnesota and surprised me by demanding credit for the first two years of engineering physics. I thought he was pretty cocky and told him that I would authorize credit for him if he would pass a written examination. That being satisfactory to him I presented him with a long list of questions covering the whole field of college physics. I thought that when he saw those questions he would quit and save me any further trouble. He sat down a little after eight and was still writing away at noon, never asking any help interpreting any of the questions. I suggested he go out and get some lunch, which he had apparently completely forgotten. He was back again in half an hour, continued to write and finally turned in the papers about five o'clock. He remarked that if I would let him continue the next day he would polish the papers up a little. I took this sheaf of papers home. It was after midnight before I finished them. I found I had in my hands an abbreviated but excellent test book of college physics with absolutely no mistakes in it. Harry Nyquist, after taking his doctor's degree at Yale, as did also Johnson, went to work with Dr. George Campbell of the Bell Systems.

Needless to say I take great pride in the fact that I gave to these men their first introduction to the field of radio. It was probably the best thing I did during the eight years that I spent at the University of North Dakota.

I have mentioned in the preceeding chapter only a few of the principal radio developments with which I was either directly in contact or in which I was much interested. It was a period of very rapid growth of radio. The items mentioned only constitute a small fraction of the total progress.

The preceeding decade also saw the start of an activity which was not encompassed by the field of scientific research, engineering, and development by large companies and naval and military radio. I refer to amateur radio, particularly to American Amateur radio. Amateurs are of two types: first those interested in setting up communications with friends far and near and making new friends by way of these radio contacts. The amateur also forwards messages from his acquaintances to their friends by way of some other amateur in a different location. For all of this he receives no pay, and being commonly a man or woman of limited means, he is obliged to put together most of his equipment with his own hands. The second type of amateur is one who is interested in the technical side of radio and likes to build equipment. But this type of amateur also likes to make contacts with other amateurs at a distance in order to test his equipment.

For many years the professional radio engineers sneered at the amateurs and would gladly have had them abolished, but those of us who have worked with them over long periods of time will surely admit that they have made many extremely valuable contributions to the field of radio. Now it was natural that amateurs should first spring up in those regions where commercial stations were first built in this country, along the Atlantic Coast. The amateur would start with a crude receiver. He would learn the dot and dash code and, for his own amusement, intercept the messages - not for the purpose of betraying secrecy or using them unlawfully, but simply as a matter of curiosity and technical interest. Most amateurs begin with receivers. There are records to show that there were some amateurs even in the early 1900's, when even commercial stations were still using coherers. It is extremely probable that until the year 1906 when work by Pickard, Dunwoody and various foreign investigators disclosed the properties of crystal detectors, there were few amateurs capable of intercepting messages more than a few miles away from the transmitting stations. Crystal detectors were easy to make and cost almost nothing. This put a tool in the hands of the amateur that, from 1906 on, permitted him to construct a receiver capable of intercepting messages from coastal stations and from what few ships were equipped, over very considerable distances. By 1910, the amateur fraternity was definitely established, if not well organized.

After having contented himself for a while with intercepting commercial messages, the average amateur began to think of making a transmitter, frequently starting with an old Ford spark coil, a few turns of wire and a spark gap. He contacted other amateurs in the same community pooling their interests and finally establishing two-way contact. In the early days there were no regulations restricting the amateur in any way or requiring him to have a license. Even after the London convention in 1912, the regulations requiring an amateur license and restricting them to wavelengths shorter than 200 meters, that is frequencies higher than 1500 K.C., were not rigidly enforced, except in the coastal areas, for some little time.

In addition to the unorganized amateurs there were a few colleges and universities which put in experimental stations, also unlicensed at first, but later licensed, either as educational or experimental stations. It was a station of this character that I finally succeeded in getting set up at the University of North Dakota, financed by a very small allotment of University funds, supplemented somewhat from my own income. As in the case of the amateur, the first set-up was for receiving purposes. It was started in the fall of 1910. There were no suitable wavemeters or indicating instruments available at the University. With Ray Heising's aid, I got together a few coils and condensers, making rough computations of the probable tuning ranges.

There were, to the best of my knowledge, almost no amateurs or other stations within the State of North Dakota or in nearby Canada or Minnesota, capable of being heard at Grand Forks in the day time. I found very early that the time of day and the weather had a lot to do with the propagation of radio waves, and from the nature of our situation I was forced to do a large part of my experimental work at night.

Our early experiments in the spring of 1911 were rather local in character. We had put to work an old induction coil and condenser with suitable discharge inductances and rotary gap, and had erected a flat top antenna between two of the University buildings. After we had built a wavemeter, we found we were operating at about 600 K.C. We began work at short distances with portable receiving sets and then enlisted the aid of high school students and others, in towns from forty to one hundred miles distant, by providing them with crystal detectors and instructions for making simple receivers and setting up antennae. We were soon able to pick up, at night, certain commercial stations on the Great Lakes both in Canada and United States, but only in mid-winter could we hear stations on the seaboard.

I realized that greater distances would have to be spanned by longer wavelengths or lower frequency, and so built receivers for this purpose which were successful insofar as they were able to bring in, even in day time, low frequency signals from both east and west coasts. In the meantime we had purchased a suitable transformer from the Wireless Specialty Company and later a Thorardson transformer, which enabled us to step up the power of our transmitter and increase our signals to a night range, in the winter at least, of about 1000 miles.

The first station I worked both ways over long distance was the University of Michigan, Ann Arbor, Michigan. Neither of us were licensed at that time; the Michigan station signed the call UM and my station UND. This UM station was operated by the Department of Electrical Engineering, University of Michigan and employed an ex-commercial operator who gave me a good many pointers when I visited him. I don't think I will ever forget the thrill of the first long distance contact. It was pretty much home-made gear at both ends.

At that time there was no broadcasting in the sense that the term is used today. The word broadcast was used very early by the Navy for any code message which was sent to all stations; in other words, broadcast in all directions. The present broadcasting industry, which sends unpleasant advertising and some excellent entertainment in all directions, inherited the word, which gradually came to mean the broadcasting of speech, music and television.

Very early in the existence of the UND station later licensed as 9YN I started regular broadcasting of time signals and weather forecasts, for the purpose of making more interesting contacts with our increasing number of amateur listeners. It soon turned out that these broadcasts were of great interest to isolated communities in the nearby states, where such information was not generally available. The weather broadcast particularly, served a very practical purpose for our isolated communities of North Dakota. Time signals sent from our own master clock were checked daily with the time signals from the Navy Station at Arlington, Virginia which, by that time, we were able to receive, especially the night signals. Many amateurs visited the station and some of them had sufficient technical ability to cooperate in various experiments, particularly with those to do with wave propagation and weather. I tried to use the University of Michigan Station for some of this work but was not too successful, contact with them being a little too intermittent for systematic observation.

In those days when there was little or no regulation, some very amusing messages could be picked up occasionally, even from commercial stations. On one occasion when I was running an overnight test on an hourly schedule, I discovered that certain Canadian Marconi Stations on the Great Lakes called each other regularly during the night at the beginning of every other hour and exchanged traffic. Between my own schedules I used to listen to these Canadian stations, particularly to Port Arthur and Sault St. Marie. On this particular evening, these two stations got together on the hour and each signified he had no traffic for the other but the Sault station then remarked "I have a terrible toothache. It is driving me crazy". Port Arthur was silent for a minute and then came back with "Try a hot brick". The Sault station said "Thanks" and signed off. Two hours later he called up and announced that the hot brick worked fine. One cannot imagine such conversation going on in these days.

On another occasion I heard the high powered Marconi station in the Hawaiian Islands send, on long waves, a message to San Francisco which read something like this: "Tell father, James and I are getting married. Please sned money", signed, Madelein. The operator put in parentheses at the close of this message, ("She surely wants a swell wedding"). This practice among the commercials arose from the fact that during the slack hours there wasn't enough traffic to keep them busy and they saw no reason why they shouldn't talk with each other. Needless to say such talks were not paid messages. All this was finally stopped by international regulations.

There were a goodly number of stations on the coasts in many parts of the world. A good many ships were equipped. Experiments had begun on the transmission of photographs and drawings by radio. The foundations for radio telephony, particularly with the continuous wave stations such as those of the Poulsen arc, were being laid. The sinking of the TITANIC in 1912, after collision with an iceberg, was a tremendous influence for the legislation compelling passenger carrying merchant ships to use radio in the interest of safety at sea.

In the year 1913 the Lackawanna Railroad made experiments with radio on trains. Extensive plans for extending trans-Pacific and trans-Atlantic long range traffic were under way in 1912. A high power Fessenden set using the rotating synchronous quench gap was installed by the Navy at Arlington, Virginia and at the same time, a 30 K.W. Poulsen arc. The Fessenden set at Arlington operating on 2500 meters (125 K.C.) was the one from which I received the time signals. A 25 K.W. Fessenden set was also installed at Key West, Florida, on 1800 meters (166 K.C.). The Navy was well on the way towards installation of much higher powered Poulsen arcs on much longer wavelengths or lower frequencies, thus acquiring an immerse radio network extending to Guam, Samoa and Cavite in the western Pacific, to Balboa in the Canal Zone and to Alaska and Dutch Harbor, Aleutian Islands in the northwest. By the end of 1914, the United States Navy had fifty coastal stations and two hundred and fifty ships equipped with radio, using both arc and spark on some of the shore stations. The Navy system, indeed, was the largest radio system under one control in this country and possibly anywhere in the world.

The Federal Poulsen telegraph system, started in the United States, was beginning to equip west Coast stations and ships for commercial service. I knew about long wave high power unmodulated stations but had no means of receiving them at first. It didn't take long to fix up a Poulsen tikker which could be switched into the place of the usual crystal detector, for continuous wave reception. Fortunately this was a very simple and inexpensive device, consisting of a small rotating metal wheel near the edge of which a thin wire appeared to have a continuous contact which actually was intermittent, because the pressure of the contact was very light. This curious device, substituted for the usual crystal detector, received these continuous wave high power stations and produced in the telephones a scratchy note which could be read as dots and dashes if there wasn't too much static or atmospheric interference.

About this same time, since I had acquired an experimental license with call 9XN, I decided to experiment with lower frequencies for my rotating gap spark transmitter which by that time was a 4 K.W. set. My big problem was to get a much larger antenna. The only way to do this was to string it between the top of Science Hall and the top of the power house chimney, an elevation of 125 feet. The span between the two points of support was 800 feet. There was a three wire antenna, twelve feet wide at the near end, and twenty feet wide at the far end. To get this antenna attached to the chimney top was a problem, since there was no ladder inside or outside the chimney. I solved this difficulty by building a large box kite. After a number of trials, when the wind was just in the right direction, I succeeded in dropping the kite string across the chimney top. By night fall the wind died down, the kite came down and I was able to pull up on the far end of that string and get a light rope across the chimney top. This in turn was followed by a 5/8 inch flexible steel cable carrying a block and hoisting cable. The 5/8 inch cable was then tied down on both sides of the chimney at the base and with block and hoisting cable at the top I was thus able to raise and lower the antenna for repairs. The high North Dakota winds took it down more than once.

With this new antenna I was able to transfer our transmissions quickly from 600 K.C. to 200 K.C. Later on I used this antenna for the Poulsen arc transmissions on 100 K.C. All this was in the interest of comparing results over various distances, aided by increased amateur contacts, studying the effect of varying seasons and weather, and for comparing night and day results. My first paper dealing with wave propagation based on this work was published in the Physical Review in 1914. It was called "Radio Transmission and Weather".

In the course of these experiments I didn't escape considerable criticism from my co-workers at the University. A large part of the wiring in Science Hall was open wiring, not shielded by conduit. It picked up pretty heavy currents of high frequency, superimposed on the 220 volt direct current supply, especially when I was operating at full power on some frequency that happened to resonate part of the wiring system. I well remember that the Medical Department, on the third floor, had a centrifuge which was driven by a small 200 volt direct current motor. This was started and stopped by a small switch on the wall. One day, just after this device had been loaded to capacity for urinary analysis test, I happened to cut loose on the radio with a frequency that built up a strong response in the wiring system and a spark jumped over this switch. It started the motor before things were ready, spilling the contents of the centrifuge in every direction. Naturally this created considerable complaint. The only way I could get permission to continue operating was to guarantee against any future repetition of this performance.

In the process of putting resistance and capacity filters at different points on this line in order to break up their resonances, I learned a good many useful lessons. Also I got material which led to publication of a paper in the Electrical World in 1913, on the subject of Local Absorption in Radio Telegraphy.

Shortly after this the University Extension Service asked me to give some lectures on radio in several towns in different parts of the state. As a result I put together some portable long wave receiving equipment which had been provided with an oscillating tube or heterodyne receiver. I had no loud speaker or amplifier, but I hooked up ten or twelve head telephones in series and, after giving a lecture on the present status and possibilities of radio, I would invite the audience to come up and listen to the signals. This sounds very uninteresting from the present view point. Even then it appeared to be very dubious that the people in isolated rural communities, who constituted the largest part of North Dakota population, would care for such things. I would, of course, expect to interest a few amateurs and induce others to get into the game to help in my experiments, but I didn't expect the average North Dakota Scandinavian farmer, many of whom could hardly understand the language, to be interested in such talk.

On one occasion I was required to give a lecture in a small town about thirty miles from Minot, in the western part of the state. I brought along some student assistants and in spite of the weather being 30° below zero, we managed to get up a good receiving antenna and to install our gear in a barn of a place, with only one large stove for heat. It was so cold that night that I had to give my lecture in a fur overcoat and yet, these same farmers, whom nobody expected to be interested, came driving in this bad weather from points some of which were thirty miles away, to attend the lecture. They kept me up until midnight with questions and answers and showed me that people in any walk of life like to hear about something new, even if it be very far removed from their own daily life.

A little later I was to get up a portable transmitter so that whenever the town where I was to give a lecture had alternating current power, I was able also to set up a transmitter and usually able to exchange messages with the University. Often we put some of the students in communication with parents who were present in the audience. This was good advertising for the University but was a tough job. I didn't care to give these lectures in the summer when disturbances due to atmospheric conditions were heavy because I couldn't be sure of communication. On the other hand, to put up a good antenna in a few hours in a North Dakota winter gale was not exactly a picnic.

All of the earlier work at North Dakota was done without the benefit of amplifiers but by 1914 I was able to get hold of a few DeForest audions and built audio amplifiers also using audions for detectors, thus securing more quantitative measurements than could be carried out with the crystal detector.

These early audions generally had some residual gas in them. They were somewhat cranky to adjust for best output, but the very fact that they had this gas enabled an amateur with patience to work out trick adjustments, sometimes aided by small movable magnets placed near the tube, and to arrive at extraordinary sensitivity. In these days we do not need such sensitivity. We would rather have a robust long life tube for a detector since it has very stable adjustment and can be used with an amplifier. But in those days funds were scarce and the amateur with more than one tube was considered a plutocrat. We had to get everything we could out of one tube. So far our purposes the residual gas in the tubes was often an asset rather than a defect.

Many times I was able to copy European signals, using a single tube where nowadays we think we have to have anywhere from four to ten tubes to do the same job.

In the latter part of 1914, I managed to get together a few DeForest audion tubes and construct a one stage amplifier to follow my audion detector. This was a great improvement but the amplifier didn't work to advantage on continuous wave signals which I picked up on the Poulsen tikker. These were rough scratchy signals anyway and amplification didn't improve the signal to noise ratio. One day when receiving from long wave Poulson arc stations on the coast, I switched over from the tikker to the audion detector with its amplifier in a rather hopeless attempt to see if anything at all would come through this combination. After juggling around with filament voltage and other adjustments, I suddenly picked up a clear pure note whose pitch was determined by the setting of the tuning condenser on the receiver. I was quick to realize that I was getting beat frequency oscillations and that a local oscillation was produced in some way by my two audions. I was able to reproduce this condition over a considerable range of frequency by using careful adjustments. I don't think I realized that I had an accidental feed back from the amplifier tube to the detector by way of the Fork spark coil which I was using as a coupler between detector and amplifier. I sent in a paper to the Wireless World called "The Double Audion Receiver". This was published in March 1915. This paper led to my first direct contact with Dr. DeForest who kindly sent me a couple of extra tubes to further my experiments.

It wasn't until Dr. Armstrong published his famous paper in the 1915 September issue of the "Proceedings of the Institute of Radio Engineers" that I had any clear understanding of the process of detection, regeneration, and oscillation. Armstrong was then a student at Columbia, working under Professor Morecroft. I didn't know him then but have had many friendly and interesting contacts with him in later years.

It was during this period that I joined the Institute of Radio Engineers which was organized in the year 1912, absorbing two earlier organizations, namely, the Society of Wireless Telegraph Engineers and the Wireless Institute. The Society of Wireless Telegraph Engineers had John Stone for the first president in 1907; Lee DeForest in 1909 and Fritz Lowenstein in 1911-1912. The Wireless Institute, between 1909 and 1912 was presided over by Robert Marriott. The first president of the Institute of Radio Engineers was Marriott and the next one Greenleaf W. Pickard. It has been my good fortune to know all these gentlemen and to have many interesting contacts with them. At this writing they are all alive except John Stone and Fritz Lowenstien.

I continued night work at this time, specializing with the signals of the two Navy stations at Arlington and Key West. I was especially interested in changes which could be correlated with diurnal and seasonal weather conditions. I worked out a correlation between the gradual fading out of the signals in the spring with the retreat of the snow and ice sheet towards the north and west. This resulted in a paper in the Physical Review, May 1913.

During this same time I frequently worked with a station in St. Louis operated by Washington University. The operator was Mr. Blatterman. We became very firm friends by way of radio. We even published a joint paper dealing with radio transmission phenomena in the IRE in 1916, although we didn't meet each other until after the First World War when he was a captain in the Army and I a lieutenant commander in the Navy.

It was this paper, presented at a New York meeting of I.R.E. which led to my first close contacts with the United States Navy. During that trip East I met Admiral Bullard, who was the first Director of Naval Communications and was responsible for building the famous Arlington Station. I met Lieutenant Hooper (later Rear Admiral) at the same time I met Bullard. Charles J. Pannill, now of RCA, was then civilian assistant to Admiral Bullard. I had met Dr. Austin, the principal Navy Radio Scientist, considerably earlier. Admiral Bullard offered me the cooperation of any suitable existing Navy stations in the furtherance of my studies on radio propagation. Since the Great Lakes Naval Station near Lake Bluff, Illinois, was the nearest station (600 miles) to mine, it was arranged that I should stop over there to see the authorities about working them into our tests.

By this time I had laboriously built a small Poulsen arc which operated on about 100 K.C. and was somewhere around 5 K.W. in power. In order to get this thing to operate, I had to tie up the whole laboratory in order to get together enough high voltage to operate the Poulsen arc. Such an arc, particularly one like mine, was entirely unstable unless the supply voltage was at least 500 volts D.C. Our direct current supply at the University was 220 volts, but I had a Fort Wayne three phase alternator driven by 220 volt D.C. motor. I also had a three phase 220 volt connection which had recently been brought up from the City of Grand Forks, so I could get this motor generator up to speed running the 220 volt motor off the direct current line. When I got it synchronized, I would cut on the 220 volt, three phase, supply in parallel with the motor generator, then open the 220 volt switch to the motor and let the alternator drive the motor as a direct current generator, adding this voltage to our 220 volt direct current line thus getting 440 D.C. By hooking together all of the storage batteries in the Physics Department in series, I was able to add about another 130 volts. In spite of several minor explosions which fortunately did no great damage, I managed to keep this arc going. The fact that it had a very bubbly note didn't matter to my amateur observers because they used the Poulsen tikker, which gave a rought note anyway, but the people at Great Lakes Naval Station had trouble copying it since they used a heterodyne receiver with oscillator tube which was supposed to give a clear musical note. We started our tests with Great Lakes using this arc.

About the same time I made a first and very feeble attempt at what would nowadays be called broadcasting, namely, radio propagation of voice and music. I succeeded in getting a small output by Poulsen arc at about 700 meters (430KC). This was very loosely coupled to an antenna which, when properly tuned, gave a current of only three quarters of an ampere. By putting a unit of three or four microphones in parallel, all of them in series with the antenna and ground, somewhat as Majorana had done in Italy, I succeeded in getting audible and somewhat intelligible voice signals at my home in the city of Grand Forks, four miles distant. Later I brought out a Columbia phonograph and fixed up a microphone arrangement in the place of the ordinary mica diaphragm the phonographs had in those days, and turned it loose. I didn't have much faith in this getting out very far and did it more as a matter of amusement and for lecture demonstration than anything else. Somewhat to my surprise, I found that a youngster one hundred miles away had picked up the music on a galena detector and was wondering where it came from.

During the latter part of my stay in North Dakota a very amusing incident occurred which led me to be offered the important position of Medicine Man in the Turtle Mountains Tribe of the Chippewa Indians. It came about in this way: Professor Frederick H. Koch, Head of the Department of Dramatic Literature, had started writing and putting on pageants. Later he followed this work up at the University of North Carolina where he founded the Carolina Players. I used to help him out with his pageants, furnishing some of the mechanical and electrical effects, such as artificial thunder and lightening. Aside from this we were very good friends and together we took many week-end skiing trips.

At the time I have in mind, he was putting on "The Pageant of the Northwest", which involved a scene where the French explorer LaSalle met certain Chippewa Indian Chiefs on the banks of the English coulee and signed a treaty with them. The University persuaded a famous old Chief, with some of his people, to come down from the Chippewa Reservation in the Turtle Mountains. This Chief, Marchebenas, was a very noble and intelligent man and possessed as much dignity and poise as any person I have ever met. He was accompanied by a Sub-chief named Little Boy, much younger, say about forty years old, and by a half-breed interpreter named Wellington Salt. On the day that the pageant was supposed to be first presented one of Dr. Koch's assistants called me up and asked if I could give the Chief a little show over in the Physics Department. I told him to bring them over at about 3 o'clock. When they arrived after being formally introduced I escorted them to the lecture room where I had set up an X-ray outfit with a fluoroscope. I asked the Chief if he would like to look at his own bones. I think he must have been startled but he didn't show it; he merely nodded acquiescence, so I showed him how to look into the fluoroscope and put his hand up in front of it. He played with his fingers and wrists very intently for a few moments, and then slid his arm a little farther in front of the fluoroscope, gave a grunt and said something in Chippewa to the interpreter. "What does he see?" I asked. "Oh", said the interpreter, "he has just located an arrow point he got in a fight with a Sioux back in 1878." A few minutes later he located a bullet he got in another battle of the 80's. Before he got through he located seven assorted spear points, arrow points, and bullets.

After that we went into the transmitter room and I told him I was going to show him the apparatus with which I talked to the Great White Father in Washington. Then I turned loose the 5 K.W. rotary gap which, not being enclosed, made a terrific din. All Indians, in a way, understand code signals, although they don't use the Morse code, but signals with drums and smokes. Considering the racket this spark made, I suppose the old Chief believed that it might be heard in Washington.

Then we went into the receiving room and I let him listen to some long distance signals with the headphones. Now this was an absolutely clear summer day and yet there was considerable static or atmospheric disturbance. The Chief had no difficulty hearing the dots and dashes come through and recognized them for the communication of intelligence that they were, but he also noticed the static and he asked, through the interpreter, "What is that little crackling noise I hear besides the signal?" "Well", I said, "that is due to a distant storm." "How far away?" he asked. I said "Perhaps a moon's travel from here". He listened again and then asked "Is it coming this way?" Since I had noticed that the static had been building up all the afternoon, I answered "I think it is." After another brief period of listening the Chief asked "When will it get here?" Having committed myself so far I thought I might as well go the rest of the way so I said "I expect it a little after sundown." So he thanked me cordially and went out. That night, at twenty minutes after sundown, we had a terrific and almost tropical storm with a remarkable display of lightning and rain, so that the pageant had to be postponed. Two days later when the Chief went by the building in which I worked, the Science Hall, he pointed his thumb at it and said "That is the house where the gods live". Later on when he told his story to the tribe back at Turtle Mountain, they thought he had lost his mind and were going to appoint a new Chief. However, when Chief Little Boy told them he too saw his own bones and witnessed these things, the Chief was retained and he ruled the tribe until he was past ninety with greater prestige than ever. I am sure that he would readily have given me the position as Medicine Man.

During the year 1915 I was fortunate enough to overhear the radio telephone experiments which succeeded for the first time in sending the spoken word across the Atlantic Ocean, as well as westward as far as Honolulu. Wireless telephony up to about 1914 had been confined pretty much to experiments with modifications of the Poulsen arc or to some few attempts with high frequency alternating current machines, neither of which were any too easy to modulate. Of course a continuous wave generator was necessary because the usual spark set, even if modulated with speech, would chop it up at the spark repetition rate and the speech would be quite unintelligible. Telephony didn't begin to come into its own and lay the foundations for the great present day broadcasting program until various engineers, notably Dr. Lee DeForest, worked out a means of causing the radio vacuum tube to oscillate, thus generating a steady and unbroken stream of radio frequency energy. This encouraged the development of transmitter tubes as well as receiving tubes. This work has been going on at a great pace ever since.

The great advantage of the tube oscillator or tube transmitter is not only that it gives continuous waves in a very convenient way, but it is a mechanism which can very readily have a speech modulation impressed on it so that when this wave is detected in a distant receiver, the speech will pass into the telephones or, in modern days the loud speaker, and become audible.

In 1915 the United States Navy put the Arlington Radio Station near Washington at the disposal of the Bell Telephone Systems engineers for a series of experiments in long distance radio telephony, which were destined to mark a great milestone in radio progress. The transmitting tubes used were not very powerful and not of long life. A large number of them had to be used in parallel to get an adequate amount of power and there was a continual bother due to replacement of burned out tubes.

Arrangements were made to observe these signals in various parts of the United States, particularly by the Naval stations. In spite of the first World War being on in Europe, the French gave the American engineers the use of the Eifel Tower listening post during certain limited hours. Neither the power nor the wavelength were suitable for crossing the Atlantic in the day time. The experiments were made at night; they were successfully observed at the Eifel Tower in Paris and as far west as Honolulu. It was my good fortune to accidently pick up these signals in Grand Forks. It was the first radio telephony that I had heard with the exception of my own very amateur experiments. It gave me a big thrill.

Among the many other interesting developments of this period, was the steady increase in the use of devices to aid navigation at sea. Some practical use had been made here and there of radio for aircraft. The Germans, of course, used radio on their Zeppelins which were bombing London. There was some spotting work done, particularly by English craft on the Western Front, signals being sent by light weight airborne transmitters with receivers using the crystal detector. Dr. Frederick Kolster in this country, had started his long series of studies on the radio compass and our Navy was decidedly interested. These early compasses depended upon the directional properties of the wire wound loop, used instead of a vertical antenna with which to collect the signal.

The development of the radio direction finder, as it is better called, was very slow due to the fact that the use of the tube amplifier didn't come until about 1914, and it was some years later before it was commonly in use. I do not refer to the radio frequency amplifiers so common in our broadcast receivers today, but to the audio amplifier which is much older. We possess both types in the modern receiver. The wire wound loop doesn't collect radio frequency energy anywhere near as effectively as an elevated antenna. Until the amplifier came along we couldn't get strong enough signals to make practical use of the loop, although the fact that it could be rotated so as to point to the direction from which the signal came had been known for at least a decade.

Early in 1916 I wound two large loops, suitable for picking up low frequency stations, on the walls of my bedroom. By combining these two loops through a switch, I could point the field of reception north and south, east and west, or northwest and southeast or northeast and southwest. With a suitable receiver, detector, and amplifier I was able to pick up long wave stations even from across the Atlantic. I sent in a paper in 1916 to the Institute of Radio Engineers on the "Possibilities of Concealed Receivers" - concealed in the sense that no external antennae were visible to betray their presence. Relations were getting rather strained between this country and Germany at the time so my good friend, Dr. Goldsmith, Editor of the Proceedings of the Institute of Radio Engineers, referred this paper to the Navy Department. The Navy Department requested that I postpone publication until after the War. This was one of my earliest contacts with the Navy. They were afraid the device might give aid and comfort to a German spy who would be able to set up a very simple equipment, unsuspected by his neighbors, and receive instructions from Germany. The Paper was finally published in 1919.

By the middle of the year 1916 I was obliged to break off my wave propagation experiments with the Great Lakes Naval Station because it was too busy with War preparations. Among other things, it was organizing amateurs in the country, who by this time were very numerous, into various drill chains. I was asked to assist in this work. I took up the organization of a drill chain which extended from Duluth to Denver, in almost a straight line, and had about 50 amateurs receiving the drill messages which were sent to me from Great Lakes on their 30 K.W. arc and which I then relayed at hours in the evening suitable for amateur reception, both on my spark and my arc sets. The amateurs cooperated very enthusiastically and 90% of these amateurs went into the Navy - some of them appeared in my own command later.

In February 1917, I was asked to give a lecture on radio in Chicago. On the way home I stopped off at Great Lakes to talk matters over with Lieutenant McCauley; the upshot of this interview was that he persuaded me to apply for a commission in the Naval Reserve. I took the medical examination before leaving. A week or two later I was given a commission as a lieutenant in what they then called the Coast Defense Reserve. This didn't mean much because very shortly nearly all of us volunteered for general service anywhere in the Navy. On the 28th of March I was ordered to active duty, reporting at Great Lakes just a few days before War with Germany was declared.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER VI - 1917-1918

WORLD WAR I

GREAT LAKES AND BELMAR

CHAPTER VI

1917-1918 - World War I - Great Lakes and Belmar.

At the time I was commissioned I had practically no knowledge of the Naval Service except that I knew the Navy was progressive and doing excellent work in the field of radio. One of the first things that impressed me was the appalling discovery that any reserve line officer, such as myself, was automatically expected to have executive ability and be able to take administrative charge of any shore station or project, whether he previously knew anything about it or not. This was a state of affairs very different from anything I had ever encountered in civilian life. The curious thing is that it works out successfully in many cases. Perhaps this is because the officer knows that he faces a court martial if he fails to burn the midnight oil to find out all he can about his project, so that he can administer it successfully. I still cannot believe this system to be the best, because I have known it to break down on a few occasions with disastrous results. Nowadays there are a great many Naval officers who are given technical duties for which they are well trained, and a much greater effort is made to fit the man and the job together. By my background and experience I should, under modern conditions, have been doing engineering duty only (EDO), but throughout more than five years of active service as a Naval officer I remained a line officer.

Many of us, in 1917, had no opportunity whatever for indoctrination courses, which would have been of great help. Thus I had to acquire knowledge of the customs, etiquette and procedures in the Navy by actual experience. In this I was greatly helped by a medical officer by the name of Commander J. B. Kaufman, (later Admiral Kaufman) who happened to room in the same house where I first lived at Great Lakes. He was kind enough to give me a lot of hints and some actual coaching, for which I have always been extremely grateful.

I found that my assignment at Great Lakes was that of District Communication Superintendent. Lieutenant McCauley and his aide had been ordered to sea before I arrived. This job involved handling all communications, by wire and radio for the station itself, where there were about 13,000 men on board and, in addition, the management of the entire Great Lakes system of radio stations (as far east as Buffalo), which the Navy had just taken over from the American Marconi Company.

These stations were manned by pretty green reserves, except for the Great Lakes station itself, which had a number of regulars. In addition, I was supposed to be in command of the radio regiment, which included about 2000 men under training as prospective radio petty officers. I had a junior reserve lieutenant named M. B. West as assistant communications officer, and since he had reported earlier than I, he had received considerable instruction from Lt. McCauley. He was, and is, a very able man, especially on administrative work. Without his help I certainly couldn't have swung the job.

The Naval District, over which I was supposed to exercise communication control, stretched west to the Mississippi, south to the Kentucky border, east as far as Buffalo, and north to Canada. Scattered through this district I had a number of reserve officers and several retired officers who had been called back to duty in charge of outlying stations.

In addition to organizing and supervising our own communications, we were supposed to suppress any illicit amateur or spy communication in this entire district. Also each individual station had to detail some of their personnel to mount guard day and night against possible sabotage. It was considered very important, on account of the Great Lakes ore shipments, to keep these Great Lakes stations in operation. Only the Great Lakes station itself was in first class condition. The others had rather antiquated and inferior equipment. It took a lot of tinkering to keep them going. In particular, the Calumet Station on Lake Superior took a lot of fixing up. When I first visited it on an inspection trip, I made up my mind it wouldn't have worked at all except for the ingenuity of a young first class radioman named L. C. Young. I kept my eye on this young man and wherever I went in the Navy, he went with me, becoming one of the principal pioneers in Navy radar and Associate Superintendent of the Radio Division at the Naval Research Laboratory.

Another man, who is now Superintendent of the Ship and Shore Radio Division of the Naval Research Laboratory, was a radio petty officer at Buffalo - he was L. A. Gebhard. Both of these men are well known members of the Institute of Radio Engineers. Mr. Young is a Fellow of that organization. There was a young petty officer named Howard Booth on duty at the Great Lakes Station. I was to have many contacts with him through the succeeding years. He is now Commander Booth, U.S.N.

The stations at Duluth and Calumet had no supervising officer, so I got the Commandant, at that time Commander Moffet (later Admiral Moffet) who afterwards laid the foundations of Naval aviation, to authorize the appointment of a friend of mine from Minneapolis, named Marc Frazer, as a junior lieutenant in the Naval reserve. He was to have cognizance over Duluth and Calumet and keep his eye open for unauthorized radio stations in the Minneapolis-St. Paul area, with his headquarters at Minneapolis.

In connection with the confiscation of illicit transmitters, Frazer nearly got me and himself into trouble. He had traced down an illicit transmission to the home of Ex-Governor Lind, who was a close personal friend of President Wilson. Lieutenant Frazer had requested the Linds to seal the set and stop using it. Since the transmissions still continued, Frazer took a couple of radiomen to the house and confiscated the set, in spite of very vigorous protest from Mrs. Lind. Mrs. Lind, who knew the commandant, called him on long distance telephone and made a violent protest. The commandant called me into his office and asked "What kind of man is this Lieutenant Frazer you have in Minneapolis?" I told him "He is a very fine man, and is very good officer material, in my opinion". "Well" he said, "Mrs Lind said he was very insulting, and broke into their house, carrying off her son's radio transmitter". I had been advised by Frazer as to what had happened, so I told the Captain that he was acting under my orders when he confiscated the set and that he couldn't have insulted Mrs. Lind, because he just wasn't that kind of man. Captain Moffett looked at me sharply for a moment and then said "Well, all right, carry out your orders, I will square it up somehow". This was characteristic of Captain Moffett; he watched any man who had a new job for a week or so; then he either made up his mind that he could do the job and proceeded to back him up, or else he promptly fired him and got another man in his place. Once he had made up his mind to back a subordinate, he would go the limit for him. I had the good fortune to be associated with him later, directly and indirectly, and to be at sea with him on the battleship MISSISSIPPI. I never saw a commanding officer who had a higher degree of enthusiastic loyalty from his officers and men, in spite of the fact that he ran a very taut ship. Later I recommended Frazer for overseas duty and he was with Admiral McCulley in France, receiving two promotions.

With the exception of the transmitters at Great Lakes, all of the district equipment was spark equipment and the receiving sets were provided with crystal detectors. At Great Lakes we had a very good spark set which had a Lowenstein quenched gap. When these multiple gaps were kept clean and operated from the 500 cycle generator, our signals had a clear musical note.

Great Lakes, in addition, had a 30 KW Poulsen arc which was unfortunately assigned for most of its work to a frequency of 30 KC. The self-supporting steel towers were not over 400 feet high and not very far apart, so that the T shape flat top antenna was not really big enough for this low frequency. In order to tune it we had to use a tremendous system of loading coils. Three large cylindrical coils of wire were housed in the radio station building. The result of this was that the antenna worked at a very high voltage. This high voltage also existed on one end of these loading coils. This fact was painfully brought home to me on a number of occasions when, on inspecting the station, I happened to be standing on the cement floor underneath thses coils. Although they were six or eight feet above my head, they induced sufficient currents in my body to cause sparks to jump through the metal nails of my shoes into the concrete, with a not very comfortable sensation. Captain Moffet accompanied me on one of these inspections and disregarded my request that he stand away from the loading coil; finally he couldn't stand it any longer and walked on to the rubber floor matting remarking that he guessed maybe I was right, it wasn't a healthy place to stay. This was my first experience with anything even approaching high power long wave radio.

About the time I had succeeded in getting district radio affairs running smoothly and the extensive station activities properly taken care of, something occurred which was destined to result in my leaving Great Lakes for another job. This came about in the following way. The Navy, on account of its interest in submarines, had been on the lookout for improving underwater communications with these ships. In 1909, Mr. George Clark, radio sub-inspector for the Navy, conducted experiments in the Potomac River near the Navy Yard, Washington, and also at the Navy Yard, Norfolk, Virginia, which demonstrated the receipt of radio signals over short distances, using antennae which were submerged underwater, except for the wires which connected the receiver with the underwater antenna. This receiver was in a launch. These connecting wires were shielded.

In December of 1916 Admiral William S. Smith and Lieutenant Commander S. C. Hooper inspected a system of reception of radio signals on underground or buried antenna wires, which was demonstrated by Mr. J.H. Rogers at Hyattsville, Maryland. Clark's experiments had to be done with a crystal detector and without the benefit of amplification. Amplification was known in 1914 and was beginning to be adopted in ships of the Navy and in some shore stations. Mr. Rogers' experiments had the benefit of an audio amplifier. He demonstrated that trans-oceanic signals could be read as received on these buried single wire antennae. Further experiments of a similar nature with the wires underwater had been made by Rogers, and observed by Dr. Austin, then Chief Radio Scientist for the Navy. This work was done at Piney Point, Maryland in March, 1917. Other experiments were undertaken by Lyon (who had been previously associated with Rogers) and Lieutenant Commander E. H. Loftin, who was then District Communications Superintendent at New Orleans.

In May of 1917 I received a dispatch from the Navy Department requesting me to report to the Chief of the Bureau of Steam Engineering for temporary duty. On arriving, I was put in contact with Lieutenant Commander (later Admiral) Stanford C. Hooper. Clark, by that time, was Radio Aide for the Bureau of Steam Engineering. We spent some time in the latter part of May observing the Rogers experiments. Mr. Rogers deserves credit for having first demonstrated to the Navy that effective reception on such buried or submerged conductors was possible.

In all of these early experiments the receiving system had a marked directivity, but strangely enough none of the early investigators, including myself, realized that this was the property of the system which made it of special interest. There was no doubt that the buried or submerged conductors, while giving much weaker signals than elevated antenna, often gave more readable signals, since the ratio of signal to static disturbances was more favorable. The fact that some stations did not give better readability on the underground wires than our elevated antenna was not understood. It was supposed by the early investigators, Clark, Rogers, Lyon and Loftin, that for some reason the penetration of the signals into the ground or water lying over the wire occurred in such manner that the static disturbances were more heavily absorbed than the sustained energy of the signal. I doubted this myself although I wasn't smart enough to see the correct explanation at that time.

There was no doubt that for trans-atlantic signals as received in Washington, the Rogers System gave considerable improvement. I therefore recommended that the Navy carry out further investigations along these lines. Lieutenant Commander Hooper asked me if I would be in a position to carry on such investigations at Great Lakes. I told him that I thought the matter could be arranged, if he would put the proposition up to Captain Moffet, and that I would very much like to do it. I then went back to Great Lakes and received the necessary authorization to put a group to work on this project. This work is all recorded in papers I published in the Institute of Radio Engineers in August and December, 1919.

This experimental group at Great Lakes was a special group which had no other duties. It was headed by Alfred Crossley. Since he is a well-known engineer who was connected with the Navy for many years, and published a number of papers in the Institute of Radio Engineers Proceedings, it may be of interest to state how I happened to first come in contact with him.

While I was at the University of North Dakota, I had a letter from one of my former students, Mr. Cleary McGuire, who had gone to work for the DuPont Company. As far as I know he has been continuously with them until this day, except for a hitch in the Navy during the First World War as a lieutenant, in which capacity he served as electrical officer of a battleship.

McGuire knew of my interest in radio, and when he became acquainted with Crossley, who was operating a limited license radio station for DuPont, he put him in touch with me. Crossley had served a four-year hitch in the Navy as Chief Radioman and had some commercial experience with the United Fruit Company, but had been badly handicapped by his lack of suitable technical background. For financial reasons he had been unable to finish his education. I found a job for him in the University of North Dakota in my own department, where he was of great assistance to me. He worked all day for me and did half again as much work as the average student at night. He knew just what he wanted and went right after it.

At the time I left North Dakota I had an emergency class of young men under training for radio work in the armed services. Crossley wanted to get back into the Navy at the same time I went in, but I persuaded him to stay on until he could finish with this radio class. In the meantime, I persuaded the Commandant at Great Lakes to commission him. I believe he was eligible for the Fleet Reserve, on account of his previous experience. We had him called to active duty at Great Lakes to take charge of this work on underground antenna.

Another good man on this work was Matthews, who was then a Chief Petty Officer in the Reserve and stayed in the Reserve for many years, although not as a petty officer, but as a commissioned officer. Anyone who knows the early days of Zenith Radio has heard of Matthews. I pulled L. C. Young down from the Calumet Station, and L. A. Gebhard from the Buffalo Station. With the aid of a few radio strikers (enlisted men under training for radio) the work was started. We did most of this work down on the beach, because it was easier to dig in the sand of the beach than in the hard clay of the bluffs. We also made quite extensive experiments with wires at different depths under the surface of the water, demonstrating, as we expected, that attenuation of signals with depth was far less with fresh water than with average salt water, and less than with underground antenna.

The Great Lakes experiments showed little or no improvement in readability of signals on the higher frequencies then in use, but on the low frequency high power long distance stations they showed a small improvement which I found could be made even greater if we could get a more ideal location where the wires could be so run that we could take better advantage of their directive properties. As a result of our report to the Bureau of Steam Engineering, the Navy Department decided to try out this method of long distance reception for trans-Atlantic signals.

The Navy had the responsibility of keeping open the communications to France for our forces in Europe. Furthermore, it was figured that the German submarines could cut enough of the cables to make us largely dependent on radio. Therefore we needed everything we could get in the way of good reception from Europe.

In October of 1917 I received a surprise visit from the Assistant Director of Naval Communications, Commander (later Captain) Fawell. He inspected the Great Lakes Station very thoroughly, inquiring minutely into the affairs of the different district stations, looked over the work on the underground wires, and then abruptly informed me that in ten days I could expect orders to come East for an assignment to duty on trans-atlantic work.

This was something of a jolt, as I had just succeeded in renting a small house in Lake Bluff, getting my family down from North Dakota and settled within four miles of the Great Lakes Station. There was nothing to do but ship the family down to Indiana for my wife's people to take care of, and proceed to Washington for further assignment, leaving my duties at Great Lakes, temporarily at least, in the hands of Lieutenant West.

On arrival in Washington I found that the Director of Naval Communication, Captain David W. Todd, was my new boss. I found also that I would have close relations with the Bureau of Steam Engineering, dealing with Lieutenant Commander LeClair of the Bureau, who had relieved Lieutenant Commander Hooper as Head of the Radio Division.

It also appeared that the Navy plan was to set up a receiving and control center at Belmar, N. J. This was the very beautiful site of a radio receiving center which had been set up by the American Marconi Company in 1913-14, though not yet much used. The American Marconi Company was under the guidance of President Nally, assisted by Winterbottom and other men who are now well known in the present RCA hierarchy. The Navy had taken over this station, had a rapidly increasing complement of necessary personnel, and had started the installation of private Navy leased wires to Washington and to certain high power stations capable of doing trans-Atlantic work. These stations were also in the hands of the Navy. I was given the title of TCO (Trans-Atlantic Communications Officer). As far as I know, I am the only Naval Officer who ever held that title. I was also Commanding Officer of the Belmar Station, with general supervision of the trans-atlantic network. I was given my pick of any men I wanted to pull into this work, as the Navy considered it of the most urgent importance. I had a number of the best men at Great Lakes immediately ordered to Belmar, including Young, Gebhard and Meyer. Some of them actually arrived before I did.

Meyer was, and is, one of the most interesting men I have ever known because, starting as a Yeoman 3/c of the Navy, with absolutely no knowledge of radio, he ultimately became a high grade radio engineer who is well respected by everyone who knows him. He has for years been in charge of the Transmitter Section of the Radio Division at the Naval Research Laboratory, and is now Head of Systems Integration Section of the Ship and Shore Radio Division of the Naval Research Laboratory. Mr. Meyer had been yeoman for me at Great Lakes and became, for a number of years, a confidential secretary and an all around right hand man, although he didn't begin to take on technical duties until about 1920. As for Young, who was the best man technically, we have been closely associated for thirty years.

Crossley was left at Great Lakes, to proceed shortly to Norfolk to install an underground receiving system. Later he was attached to the Radio Division of the Bureau of Steam Engineering and still later to the Naval Research Laboratory.

I might say, in passing, that the word "Steam" in the title of this Bureau was a matter of Navy tradition and history and an anachronism. Engineering in the Navy really began with the advent of steam, so it was natural that the word steam should appear in the name "Bureau of Steam Engineering".

When I first entered the Navy we had no Bureau of Aeronautics, or even a Department of Naval Aviation, although we did have an interest in aviation. We had a Bureau of Ordnance, a Bureau of Steam Engineering and a Bureau of Construction and Repair, which had to do with the design, building and repair of ships. The Engineering Bureau had the machinery and gadget end of it; the Bureau of Ordnance had to do with the guns, torpedoes, mines, etc; the Bureau of Yards and Docks had charge of all shore construction and buildings. We had a Medical Corps, a Dental Corps, a Corps of Civil Engineers and a Chaplain's Corps. Now the organization is very different; the Bureau of Steam Engineering, very shortly after I first knew it, became the Bureau of Engineering. During this war the Bureau of Construction and Repair and the Bureau of Engineering were merged as the present Bureau of Ships. Organized Naval Aviation started as Department of Naval Aeronautics and was graduated later into the present Bureau of Aeronautics. Engineering Officers who do only engineering work are simply known as line officers, EDO (engineering duty only) regardless of what branch of service they are concerned with.

Coming back to the plan for Belmar, it appeared that it was to have control of the high power transmitters taken over from the Germans some time before we declared war on them, namely, the station at Tuckerton, N.J., and the station at Sayville, L. I. We had also taken over the Marconi Station at New Brunswick. Our wire connections with Washington terminated within the Office of Naval Communications. Outgoing messages for Europe were sent to us over our leased wire system and then forwarded by radio to Europe. Received messages were sent by wire to Washington. First, all of our traffic was with France, working with the Eifel Tower, but soon was handled by the newly completed high power station at Lyons. A little later the new Rome station opened up. I transmitted the first radio message direct from this country to Rome and received the reply. These messages were an exchange between the Minister of Communications in Italy and President Wilson.

The German transmitter stations at Tuckerton and Sayville had been operated with high frequency alternating current generators which, I believe, were of the Goldschmidt type. We never did get the one at Tuckerton to operate satisfactorily. By the time I arrived on the scene, we had replaced it with a 60 K.W. Poulsen arc. This operated into an umbrella-like antenna whose center support was an insulated steel tower 800 feet high. The ribs of the umbrella were supported by a circle of smaller towers. The station was located on what was practically marsh land, a few miles back from the ocean. Warrant Officer (Radio Gunner) Hessler was in charge of the Tuckerton station. Very good ground connection was obtainable at Tuckerton but at Sayville, L. I., the dry sand under the antenna presented so high a resistance that the Germans had erected an extensive counterpoise, about twenty feet high, underneath the antenna; this was used in place of a ground connection.

We operated the alternator at Sayville for some time but had a great deal of trouble with it, largely on account of variations in transmission line voltage of the sixty cycle supply system of the station. This alternator operated at about 11,000 cycles or 11 K.C. It was a reflection type alternator, with a complicated system of very carefully tuned low-loss circuits, creating a strong third harmonic at 33 K.C. which was approximately the frequency used in the antenna. It was impossible to keep these reflection circuits properly tuned if the speed of the drive motors varied even a small amount, due to the supply voltage variations. We therefore finally installed a 350 K.W. Poulsen arc.

Mr. Haraden Pratt, who is now one of the leading men in the International Telegraph and Telephone Company, was, at that time, Expert Radio Aide for the Navy and our specialist on Poulsen Arcs. He certainly was one of the most competent civilian engineers that the Navy ever had in the Service. I have always had the greatest respect for him, both as an individual and as an engineer. I well remember visiting the Sayville Station after receiving notice that they were about to turn the power on the Poulsen arc; this was probably some time in the late fall of 1917.

The usual method of installing the Poulsen arc was to connect the water cooled terminal to the copper strip leading to ground, since the circulating water more or less grounded this terminal in any case. The other terminal was then connected, through suitable tuning coils of very large cross section, to the antenna. The adjustment of these tuning coils determined the wavelength, or frequency, of the emitted wave. We found it impossible to get more than about 20 amperes into the antenna, in spite of the fact that we had connected the counterpoise to the ground terminal. Since we knew we should have more than 300 emperes, considering the power available, it was evident that something was radically wrong.

Fortunately before I went up to Sayville I had taken the trouble to study the use of the counterpoise in connection with antennae. I told the engineers that it would be necessary to tune the counterpoise as well as the antenna. This required a tuning coil that would carry very large current, but need have only a very small inductance. We had no such coil, but found a copper strip about 3/4" in thickness and 3" wide. We hurriedly built this strip into a spiral by simply nailing it to a couple of planks in the form of a cross, in order to hold it in shape. We didn't need any better insulation, since the voltage of the counterpoise would be very low, although it might rise to well over 100,000 volts on the antenna.

After soldering the heavy leads from the counterpoise to one end of this strip, we fixed up a heavy flexible lead to the arc terminal, with a large copper clip which could be slipped along our spiral, thus permitting a crude, but very effective, form of tuning.

Starting the arc at low power, we adjusted this clip until we found the resonance point. I then ordered the power pushed up to the maximum. The current in the antenna went up to 400 amperes, which was more than any one had seen in an antenna at that time. I am sure that Mr. Pratt will remember this incident, and how astonished our helpers were when we had finished adjusting this crude contrivance.

I knew about counterpoise action because of my experience at New Brunswick. New Brunswick had originally been equipped for the American Marconi Company with a huge 300 K.W. rotary spark gap installation. I wish I might have seen this installation in operation; it must have been quite a sight, provided you had plenty of cotton in your ears to shut out the noise.

By the time I arrived, the first practical Alexanderson alternator was in operation at New Brunswick. The invention of the Alexanderson alternator, to my mind, was another milestone in radio progress. This has been well recognized by the fact that Dr. Alexanderson has received many honors for this and other notable contributions to the field of radio engineering. It was here that I first met "Alec" and started a warm friendship, based not only on our common interest in radio matters, but on the fact that we would both rather sail in any kind of craft than do anything else.

Alexanderson had his famous multiple tuned antenna connected to this 50 K.W. alternator. This antenna, instead of having one vertical down lead, or connection to the transmitter, had six of them, and each lead was separately tuned, not only to a ground connection directly beneath it, but to its counterpoise. Thus all of the six down lead currents, after proper adjustments of counterpoise leads had been made, operated in phase, and the equivalent antenna output was actually six times as great as the feed current from the alternator. This current was of the order of 300 amperes. It was the war-time experiments with this alternator on daily traffic that encouraged the development of the much larger 500 K.W. alternator which has been the backbone of RCA long distance telegraphic communication for many years. The alternator itself is a marvelous piece of mechanical and electrical engineering. It was this alternator that handled the first communication with Rome. So evidently, before I helped to tune up the Sayville Station, I knew how to tune a counterpoise.

An interesting thing about the New Brunswick Station was the fact that due to its fairly high power and the relatively low antennae, there existed a very powerful electrical field under the antenna. Since this antenna installation was nearly a mile and a half long and the reservation not very well fenced, the Commanding Officer of New Brunswick had continuous patrols, especially during the night hours, under this antenna, to protect against possible sabotage. At first the sentries were armed with rifles with bayonets, but on a dark night you could see blue sparks coming out of the tip of the bayonet a good deal farther away than you could see the sentry. In the winter, when the sentries wore gloves, they suffered no great inconvenience, but in the summer when they were bare-handed, the induced currents burned their fingers in a very annoying fashion, and we were forced to substitute side arms for rifle and bayonet.

The gasoline filling station was almost under the antenna, so all automobiles had to be grounded when parked at the filling station. The nozzle of the gasoline hose had to be grounded as well. There would have been serious accidents, had these precautions not been taken.

The Navy planned to erect another station to supplement this high power coastal system. This was to be an arc station. It eventually became the Annapolis high power station, but was not in operation prior to the Armistice. On the other side of the Atlantic, the Navy undertook the construction of a gigantic station at Crois D'Hins, near Bordeaux, France. This was equipped with a pair of 1200 K.W. arcs and, as I recollect, eight 800-foot towers. When we laid these plans before the French, they threw up their hands and said "My heavens, you expect to erect the equivalent of eight Eiffel Towers". The work was done largely with the aid of German prisoners of war, but was not completed until several years after the Armistice. Commander Sweet, who was an officer of long experience in radio and an expert on Poulsen Arcs, planned this work.

Our receiving center at Belmar was assisted by two other receiving centers; one, a Marconi receiving center on Cape Cod, at Chatham, which was in charge of Radio Gunner Burke, and the other the Navy Bar Harbor Station, under Lieutenant Alessandro Fabri.

When I arrived at Belmar, reception was being carried on with a few small elevated antennas, but we immediately started installation of a system of buried wires laid in the Shark River inlet, pointed away from the receiving station towards the northern part of Europe. The receiving house was located on the verge of the Shark River, which was a broad estuary at that point, only a few miles from the ocean, and very shallow. This was just right for our underwater wires, because the water was fairly salty. If the wires had been put at a much greater depth than two feet, the signals would have been too weak to receive, even with considerable amplification. We ran into this very difficulty at Chatham, because there we had to lay the wires out to the sea, and when a four foot tide came in, the signals got so weak that they were unusable.

The layout in the rugged country around Bar Harbor Station was not suitable for ground wires so reception there was usually most satisfactory with some form of loop antenna. Radio Gunner Raymond Cole did some remarkably good work with loop antennae combined with elevated antennae. A number of other antennae, some of them long low wires, were also in effective use at Chatham and Bar Harbor.

These stations were connected by leased wire to Belmar, so that whenever we had difficulty in receiving from France or Italy, we would call on Chatham, and particularly on Bar Harbor, for help in the reception. Bar Harbor not only had the advantage of being several hundred miles nearer to Europe, but due to high latitude, was much freer from atmospheric disturbances (static). Bar Harbor location was for years most valuable for handling European long wave signals.

Too much credit cannot be given to Lieutenant Fabri and his aides for developing this station and especially to Fabri for contributing, out of his own pocket, many things which added to the efficiency of the operators at this place. His principal aide was Warrant Officer (we called them Radio Gunners in those days) Raymond Cole. He is now a commander in the Navy, has had a long and distinguished career in radio, and has done magnificent work as the Head of the Radio and Radar School at the Naval Research Laboratory, which work has been recognized by his receipt of the Legion of Merit medal.

In addition to taking the French and Italian code messages, we had to run continuous intercept on the high power German alternator station at Nauen. This station spent most of its time broadcasting propaganda in English, much of it undoubtedly designed to influence the German population of the United States and stir up trouble in Mexico. It usually transmitted on a wavelength of 12,600 meters, (about 24 K.C.) but at certain periods of the day, for about twenty minutes, it would suddenly go off the air. We often wondered what the Germans did during this interval, so I ordered a receiver man to go on search and explore all possible bands to see if he came upon any other frequency, and particularly to look for the double frequency, which would be at 6300 meters, or somewhat less than 48 K.C. Sure enough, we found Nauen broadcasting a very queer four letter code for twenty minutes during these intervals. There was no question but that this was a special code directing the operation of submarines. We copied thousands of code words and forwarded them to Washington, but I am of the impression that this particular code was never broken, although other German codes certainly were.

The fact that we had to copy so many messages in a very difficult code meant that we had to have extremely high accuracy on the part of our receiving operators. True, we could call on Chatham and Bar Harbor for corrections, and occasionally on Tuckerton and Sayville, when they were not busy transmitting and could stand receiver watch, but this took time. The result was that we picked our operators with great care. We had our choice of the best men from each graduating class of the Harvard Radio School. Men who weren't careful and didn't show signs of speed and accuracy were promptly transferred, either to sea or to the armed guard, at New York. To insure copy on especially important messages, at times when the static was heavy, we usually doubled the operators on a given watch.

One very great advantage of the buried wire system of reception is that wires thus buried will not resonate, but rather are aperiodic and can be simultaneously used on a great many different frequencies. Thus for the first time the Navy had multiple reception. We were able to put as many different receivers, on as many different frequencies as we wished, on one of those underground or underwater wires. A further advantage lay in the fact that we had no fear of making copy during violent local thunder storms, whereas the elevated antenna would usually have to be grounded and cut off from the receiving set to prevent injury to set or to personnel. In fact, the local thunder storms never caused us any serious interruption.

Some particularly outstanding operators I would like to mention by name: Miller was perhaps the best, because he could copy forty words a minute on a typewriter and carry on a conversation with a bystander at the same time. Pfeifer, Snell, Heberling and Stokes were all good. After the Belmar Station was closed up, Snell and Pfeifer remained in the Navy, on duty at the Naval Communications Center, Navy Department, Washington. They both became officers in the Naval Reserve; Snell served as a captain during the late war, and Pfeifer as a commander. Meinholz, now with the New York Times, was head of our wire line department. Irving Vermilya, who claims to be the No. 1 amateur in the United States, was also in this department. Woods, now deceased, later became the Manager of RCA Radio Central, in New York. "Pop" Weaver and Bill Taylor also joined RCA and were on duty at the same office. Many others ought to be mentioned, but the list can't go on indefinitely.

I found early in my stay at Belmar that good radiomen could usually be separated into two groups; first, expert operators, second, material men who were mainly interested in the technical side of radio. It is not efficient to try to make an operator out of a technical man and it is usually impossible to make a technical man out of one who is primarily interested in operating. Snell was an exception to this - he was equally at home in either field. We found it best not to burden the operating people with the adjustment and upkeep of the equipment. I appointed a permanent material watch, headed by L. C. Young and L. A. Gebhard and assisted by Dutton, Jeffries, Bartsch and L. M. Clausing, to look after equipment. One of these material men was on watch at all times, day or night. They tuned in signals at the beginning of the watch and periodically plugged in head telephones, listening to the quality of the signals, making minor adjustments, or changing batteries without interrupting the copy.

After things were running smoothly, we decided to try a 2000 foot long underground wire, in line with the sea wires, but pointing in the opposite direction. This required the digging of a narrow trench, 7 feet deep and 2000 feet long. By this time we had 100 operators, a marine guard and various other ratings on the station. When we had some infractions of discipline, most of them, fortunately, rather minor infractions, I would hold a deck court and sentence the unlucky personnel to dig so many days on the trench. Still the trench proceeded very slowly. Thereafter, instead of sentencing a man to so many days on the trench, I designated so many feet - the sooner they dug it, the sooner the sentence was over and they were back on regular duty with their usual privileges. Thereafter the trench was very rapidly completed.

Had we known what we knew shortly afterwards about the action of these buried wires, we wouldn't have dug this trench. The whole advantage of the buried wire lies in its directivity and its lack of resonance, but since the trench wire, of necessity, pointed in the wrong direction for the signals in which we were primarily interested, it gave very poor results on European copy. It was pretty fair for making copy from San Diego, but this was only test work and not a regular job.

At this time, German submarines had appeared off the coast, not far from the station, and had sunk several ships and barges. We used to get a lot of anonymous calls from various shore resorts telling of strange lights seen at sea. When our men arrived with night glasses in hand, there was never anything to see. However, we did get a little excitement at one time, when Snell discovered strange signals on a considerably higher frequency than any we were normally receiving, and coming at very irregular and very brief intervals. These were pure continuous wave signals, yet could not be connected with any of the harmonics from our own or any other nearby stations. We could not help but feel that some of these might have been an attempt to communicate between German submarines and spies on shore, because everyone felt at the time that the German subs were being refueled, or at least given food, from our side of the Atlantic.

I dispatched Young on the motorcycle, with a portable receiver, and he cruised all of that part of New Jersey in an attempt to find where these signals came from, but couldn't pin them down. After coming back, he built a portable loop direction finder with an attached receiver, mounted in an old truck, and located the source of the trouble at our own station at New Brunswick. They came on only when the Alexanderson alternator was working and were relatively weak. We never found out just what caused them, but decided it had something to do with the speed regulating mechanism on the alternator, which kept it exactly on the same frequency. Young got a broken arm out of this business while he was attempting to crank the old truck. This direction finder was probably one of the first Navy portable radio direction finders.

At this time the French complained about our reception of their station at Lyons, so General Ferrie' sent over Lieutenant Paternot, assisted by Sergeant Deloy, and a trunk load of French amplifiers to show us how to receive their stations. Both of these gentlemen were able radio engineers, especially Deloy. He was the son of a French family which was able to finance early amateur experiments before he went into the French Army. These gentlemen lived with us for some weeks in the naive belief that a six-stage French amplifier would solve all of our troubles. The amplifier certainly was good, and did give us unusually strong signals, but unfortunately, it built up the static, or atmospheric disturbances, just as fast or faster than it built up the signals. Lieutenant Paternot finally threw up his hands and said they never realized what static was until they came to this country; they had nothing like it in Europe. The difference in latitude explains a good deal. As I have already pointed out, the static level at Bar Harbor was very much lower than at Belmar, and the receiving centers in France and England, in higher latitudes, were very much better off in that regard than we were at Belmar. Nevertheless, we were very grateful for their visit and made a lot of interesting and valuable experiments with the amplifier. The French were pioneers in the field of radio frequency amplifiers.

During this period we had a number of interesting visiting engineers or scientists who had gadgets that the Navy Department though might help reception. Hoxie brought his photographic recorder, which made a record of the incoming signals on moving film which was developed as it ran along. This was really an interesting instrument, permitting good discrimination between signals and static, and permitting higher speed of reception than you could possibly get with the human ear. But it had a bothersome defect, inasmuch as it took some minutes to develop the film. It also took a separate set of operators to read the film, because such operators, or film readers, had to be specially trained for the job. Ordinarily, in such work, if the operator loses a word he "breaks" the transmitting station. This is done by opening up one of our own transmitters at the touch of the key, and sending a prearranged signal which causes the distant transmitting operator to stop his transmission and listen. We then tell him to go back to the last intelligible word we have correct, and continue on from there; thus errors in reception can be corrected at once and filled in, whereas with the Hoxie recorder this was impossible, because we had to stop and read the film. By that time the transmitting station had gone many words ahead. Nevertheless, the Navy bought some of these recorders and we learned a lot from them; they had their good points.

Another device to improve reception was brought down by Dr. F. K. Vreeland. This consisted of a very sharply tuned audio frequency filter, or the equivalent of it at least. Since continuous wave reception involves listening to pure tone, and since the ear has considerable power of discrimination against the rough noise of static, a filter such as this would favor the tone against the static and would seem at first glance to be very valuable. When Vreeland's device was first connected to a receiver, to the casual listener there seemed to be a marked improvement, but when I brought two of our best operators to copy fifteen minutes, first without the device, and then with it, we found that both made better copy without the device. The difficulty was due to the fact that the static caused the filter to "ring" so that the static had a musical note of the same approximate pitch as the signal. The ear was not able to distinguish well enough between these two things. I think we all found out, a few years later, that this filter device had to be applied with considerable caution. The filter should not be too sharp, or ring too easily, else it will do more harm than good.

Another interesting device was brought to the station by Dr. Alexanderson. This was a resistance coupled amplifier, with especially made G.E. tubes which had extremely high impedance. The amplifier didn't help much, although it gave good signals, because it was too microphonic and would amplify static just as much as it would the signals. I am not likely to forget the time Alexanderson and I were testing it in the basement of the main building. We had brought out a lead to the 2000 ft. ground wire buried 7 ft. deep. I chose that wire because it would have plenty of static, as well as plenty of signal on it. In the middle of our experiments, a violent thunder storm came up. One of the 450 ft. towers, a few hundred feet from the building, was struck by lightning. If anyone thinks that a wire 7 ft. under ground cannot pick up a violent surge of current, they are very much mistaken; sparks four inches long jumped out of the lead wire coming into the basement, although it was shielded almost to the receiver. I had just put the receivers down, but Alexanderson still had the receivers on his head. He got a pretty lively shock. Even this didn't cause him to quit the experiment.

During the period when I was on duty at Belmar, the Bureau of Steam Engineering had made arrangements for Mr. Roy Weagant to continue his work on balanced loops, which had been underway before the Navy took over the station. Weagant's theory was that static originated from overhead, while signals came pretty much along horizontal paths, and that it should be possible to set up two large loops, suitable for long waves, at a considerable distance apart and balance their outputs in such a way as to cancel out the atmospheric disturbances, or static, and yet permit reception of signals. Really, what Weagant had was what should be more properly known as the binocular loop system. In the opinion of most engineers, his theory of the origin of static was quite erroneous, but the system did show definite advantages in reception which could be traced to the high directivity which a binocular loop system possesses over and above the directivity of a single loop.

Weagant was a man of great ingenuity; a clever and able experimenter. He filed patents on his system of reception, while I filed patents on what we called the Belmar balanced ground wire system, wherein I combined in balancing arrangements, the advantages of both loop and buried wire. This system of balancing buried wires, or balancing a buried wire against a loop, was described in a paper which I published in December of 1919 in the IRE. It followed up the paper published in August of the same year. The filing of my patent, at the suggestion of the Navy Department, precipitated an interference with Weagant; as a matter of fact, there was triangular interference, involving Weagant, John V. L. Hogan, and myself. This dragged out for a number of years, but I am happy to say that it never involved any acrimony or rupture of friendly relations between the three engineers involved.

The issue was finally settled by a compromise, wherein the patent was issued to me and was purchased by RCA. RCA then licensed the Navy for all rights to manufacture and use apparatus under this patent and under related RCA patents. The Navy obtained this privilege for a small sum of money. Everyone was satisfied with this except the lawyers, who were getting good fees and would have been glad to continue the fight indefinitely.

We did not do much high speed work with Europe, but now and then we got good transmission conditions and stepped up, with automatic senders, operated by punched tapes, to fifty or sixty words a minute.

During the Winter of 1917-1918, along in February, I think, we ran out of coal, on account of some misunderstanding about our coal deliveries. As this happened at the same time the temperature went down to 13° below zero, we were in a tough spot. I had to keep that station going, as it was the nerve center of the trans-oceanic system. I went down to Belmar and Asbury Park and bought a supply of kerosene and all the kerosene heaters I could locate, plus a large number of saws and axes. I practically commandeered these things, since I had no paymaster on the station, accounts being carried in New York, and I couldn't wait for red tape to unroll. Every man on the station not on watch, marine or gob, had to get out and chop wood to feed the furnaces in order to get up enough steam to run our main power plant, so that we could keep the generators going. Since it takes about four cords of wood to equal one ton of coal, and we were normally burning four to six tons of coal a day, it was a sizeable order. At the end of four days we had sawed all of the dead timber on the reservation. I then commandeered a car load of forty tons of coal, intended for heating the schools in Asbury Park. I called the Mayor and told him what I had to do. He said, "I guess it is more important for you fellows to keep going, than to keep the schools open". The bills for that forty tons of coal followed me personally around the country for years before the Navy finally O.K.'d my action and paid for it. In the meantime I had gotten some action in Washington, and 600 tons of coal were on the way by Pennsylvania Railroad. I was calling up the station master each day, and finally he said "You can't get it, because the Bill of Lading hasn't arrived". I said "Look here, the Railroad is in the hands of the government and I am working for the government - furthermore, we are freezing here. Will you give me that coal, or do I have to send the marines down to get it?" He finally agreed to give us the coal, but I had the Marines go down anyway, just to make sure.

By the middle of the summer of 1918, we were definitely certain that reception on all trans-atlantic frequencies was entirely satisfactory at Bar Harbor. The proper thing to do was to put the received signals straight through to the Navy Radio Center in Washington. We did this over our leased wires and hooked up all transmitting stations with the Washington desk in the same way. Thus we eliminated delays in forwarding outgoing messages to Belmar and incoming messages to Washington. Everything was centered in the Navy Department, Washington, D. C.

By this time I had been promoted to Lieutenant Commander. In July of 1918 I received a dispatch asking me to report to the Chief of the Radio Division, Bureau of Steam Engineering in Washington, as soon as possible. At that time I had unlimited travel orders; that is to say, I could travel on my own initiative, on these orders, without waiting for a command from any one, to any point in the United States, if my duties and interests so required. I grabbed my travel orders and hopped a train to Washington to find out what I had done wrong. I found out that I had merely worked myself out of a job. Lieutenant Commander LeClair told me that the aircraft radio program at Hampton Roads was not progressing as it should. He wanted me to go down and take charge of it. Orders were written up promptly. I returned to Belmar for two days, to turn over the rest of the work to subordinates, and proceeded to the Naval Operating Base, Hampton Roads, where was located the air station then under the command of Lieutenant Commander P.N.L. Bellinger, now vice Admiral Bellinger.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER VII - 1918-1919

AVIATION RADIO

HAMPTON ROADS

CHAPTER VII

1918 - 1919 Aviation Radio - Hampton Roads

For the next four years I was to be attached to Naval aviation activities and transferred to the Class V, or Naval Aviation Reserve. I didn't suspect this at the time, thinking of the job at Hampton Roads as an interesting but short interlude. The sole interest of the Bureau of Engineering in sending me to Hampton Roads was to stiffen up the work on aircraft radio. But once there, it was up to the Commanding Officer of the air station to determine what my job should be.

My only knowledge bearing on aviation came from a French treatise on the laws of aero-dynamics as applied to aviation. True, I had seen a few barn storming planes flying around, but I had never been close enough to one to know what it looked like. I felt that I did know something about radio and that I might be able to work out some way of improving its performance on board aircraft.

On reporting to the Captain of the Air Station, Lieutenant Commander Bellinger, now Vice-Admiral, I presented my orders. He looked them over and said: "Have you looked around the station?" I replied that I had only just arrived. "Suppose you take a couple of days to look over the station. Then come back and talk to me about a job. Here is an organization chart which you might look over. In the meantime, make yourself at home anywhere on the station." I saluted, went out, and got myself assigned to quarters with twenty two aviators who were keeping bachelors' mess in a house supposed to be a duplicate of that of the Governor of Virginia. It has been erected a few years earlier as part of the Jamestown Exhibition. This and other houses had been taken over by the Naval Operating Base, of which we were a part. On looking over the organization chart it was apparent that the air station activities were roughly divided into two equal parts, convoy and patrols on the one hand, known as the Operations Division, and on the other hand, experimental, upkeep and modification work, known as the Experimental Division. There were sixty officers and twelve hundred enlisted men on the station. Many convoys were leaving from Hampton Roads for Europe. These were accompanied, up to seventy to one hundred miles off the coast, by planes or small blimps as a protection against German submarine attack. There was a daily routine patrol of coastal areas where German submarines might be expected. Some of these patrols went as far south as Hatteras and Beaufort, others toward Cape May.

The Experimental Division handled, besides the radio activities, various other things such as all modifications for the seaplanes, many kinds of aileron controls, speed and climb tests on new models, etc. Routing engine overhaul was also handled in the Experimental Division. The officers and men were about equally divided in the two divisions.

I wandered about the station seeing something new and interesting every few minutes. Then I reported back to Captain Bellinger. He asked me how I like the station. I told him that I found it very interesting indeed. He inquired if I had looked at the organization chart. I said, "Yes I studied it carefully". "Well", he said, "you notice this station is organized in two divisions: the Experimental Division needs to be completely reorganized. You are the new boss. Go ahead and do it". "But Captain", I protested, "I know a little about radio, but I really know nothing about aviation. This is the first time I have ever been close to an airplane." "Oh, that's all right. It is a fine day. Go out, hunt up some of your pilots, and get a hop." It was the old Navy system. A line officer, even a reserve, had to be able to manage anything, whether he knew anything about it or not. I knew I was in for it, so I saluted, said "Aye, aye Sir," and went out and hunted up a pilot near the west pier and hangar, where the experimental planes operated. A twin pontoon plane, which I think was known as an "R" boat, was just taxying up to the landing. Warrant Officer Haliburton was the pilot. I stepped up to him and said, "I am your new boss. I would like to get a hop". After inquiring if I had ever been up before, he grinned and said, "Get in". We taxied out, took off, and went up to 4000 feet for a trip around Norfolk and Newport News. Then Haliburton did about everything to that plane he could do without wrecking it. It wouldn't stand very much compared to modern planes, but we did come down in a tight spiral which, with a little more, would have been a tailspin. I got dizzy until I managed to get my eye glued on the water, at a point coinciding with the axis of the spiral. Then the dizziness left me. He taxied up to the beach and I got out safely without having lost my lunch. Haliburton grinned again and asked, "How do you like it?" "Oh fine", I said. I never held this against Haliburton; in fact we became very good friends.

The next few days were spent in sampling different kinds of craft, even including the kite balloon, 1200 feet up on a steel cable. This was the only flight I didn't enjoy. One feels very helpless when swinging up there without any sense of power, motion and control, and I was very glad to get down again. I had been warned by experienced aviators that very few people like the kites.

Then I took over my desk at the Experimental Division Headquarters, called in the department heads, most of them lieutenants or lieutenants, junior grade, and told them that I had been pitch-forked into this job and thought I was going to like it, but that it should be obvious to any of them that I knew little about it. I promised them that if they would do their jobs well I would back them up to the limit. They took me at my word and made practically no protests at the reorganizations that I carried out within the Division. After we got rid of the routine engine overhaul, which didn't belong to the Experimental Division any way, everything went remarkably well and I was then able to put in some real time on radio.

I found Ensigns C. B. Mirick, George Eltz, Malcolm Hanson, Ley, Herb Rodd, C. D. Palmer and Lieutenant Davis all active in aircraft radio work in the Experimental Division. O. C. Dresser, now head of the Design and Drafting Division at the Naval Research Laboratory, then Electrician 1/c assisted in this work. I had succeeded in getting R. B. Meyer, Chief Yeoman, to act as my secretary, and Chief Radioman L. C. Young, who has been mentioned earlier, as a technical assistant.

Active work was in progress on the testing of various transmitters and receivers, both on the bench and in flight. A great deal of work was also being done on radio direction finders installed within the planes, so that bearings could be obtained on transmissions from shore stations.

The particular form of radio direction finder of most interest at that time was designed by Dr. Robinson, whom I mentioned earlier as having been closely associated with me in Germany. Robinson did this work on the cross coil loops for the British Air Service. Owing to the very high acoustic and electrical noise levels in planes of those days, a radio direction finder could not be used on board a plane in the same way as ashore, where great care was taken to put the installation in a quiet spot. The single loop compass, as used on shore, gives a very broad maximum, but a very sharp minimum. The signals actually vanish at the minimum position if everything is in good order. On the plane, the signals vanished for a long ways on either side of the minimum, because the noise conditions prevented the weak signals being heard at all. Therefore the bearings were very inaccurate.

Robinson's invention consisted of using two loops on the same rotatable frame, the planes of the loops making an angle of about 60° with each other. A manually operated switch connected first one coil and then the other to the receiver. The device was then able to operate in the maximum position. A bearing was obtained by rotating the loops so that the loudness of the signal was the same whichever way the switch was turned. Thus the bearing was obtained by comparing two loud signals rather than by determining the vanishing point of a very weak signal. For the conditions under which we had to work in those days, this was indeed a great improvement. Robinson deserves a great deal of credit for initiating this idea.

In the year 1918, it was considered that the most important function of radio in aircraft was to permit communication two ways: between plane and shore base and between plane and ship. It is, therefore, not out of place at this point to give a brief sketch of the status of Navy radio as I knew it at this time. As previously indicated, the long distance point to point and the long distance shore to ship communication relied principally on the powerful Poulsen arc long wave stations scattered throughout the United States possessions. Our ships were by no means always able to send messages directly to the United States without relay, sometimes through a foreign country.

The Navy coastal stations of moderate power relied mainly on spark sets of the quenched gap type. In 1902, the Navy had purchased two German sets; one a Slaby-Arco and another a Braun-Siemens-Halske. They also obtained a French set designed by Rochefort and Ducretet, a Lodge-Muirhead set from London and some DeForest equipment manufactured in the United States.

Tests were then made between Annapolis and Washington, between Annapolis and a ship in Chesapeake Bay, and finally between two ships at sea, a considerable distance from the land. Thereupon the Bureau of Equipment ordered twenty of the Slaby-Arco or Telefunken sets. Before long, besides Telefunken sets, the Navy was using quenched gap sets of American design, using the Lowenstein gap and others, particularly the sets made by Simon. The Navy showed an early interest in the transmission of voice, or radio telephony. Tests with the modulated arc transmitter of the Poulsen type were made on a number of ships in 1906. The CONNECTICUT and VIRGINIA were equipped with wireless telephone equipment in 1909, for experimental use during the round the world cruise of the Fleet. Mr. Lawrence J. Hazlett, who was clerk in the Radio Division of the Bureau of Engineering between 1903 and 1934, states that these experiments in 1909 were considered so important that some boxes of accessories were dispatched after the sailing of the Fleet, in order to intercept it at a South American port. The Commander-in-Chief of the Fleet quite disapproved of this, as he stated that he was at a loss to know what had prompted the Bureau to forward freight at a time when the Fleet's attention was fully occupied in matters of diplomatic importance, and when he himself was calling on the President of a South American country.

The CONNECTICUT and VIRGINIA experiments were not very successful. Satisfactory telephony, as pointed out earlier, had to await the development of oscillations in tubes, which were easy to modulate with human speech. The 1915 experiments from Arlington, by the Bell Telephone engineers in collaboration with the Navy, evidenced the further interest of the Navy in telephony.

In the Fall of 1916 tests were carried out from Secretary Daniel's office to the Captain of the NEW HAMPSHIRE, cruising between Hampton Roads and the southern drill grounds. The Commandant at Mare Island Navy Yard also took part in these conversations with the Captain of the NEW HAMPSHIRE, using land lines of course, from Mare Island to Arlington, thence by radio to the NEW HAMPSHIRE. The talk from the NEW HAMPSHIRE went by radio to Norfolk Navy Yard, then by land line to the Navy Department and to Mare Island. The NEW HAMPSHIRE evidently had a tube set at the time. The Bureau of Engineering then arranged with the Western Electric Company to produce a few telephone sets for installation in the Fleet. These were called Simplex-Multiplex sets. An attempt at secrecy was made by modulating the emitted wave at a super-audible frequency and then modulating that frequency in turn by the voice. This made it difficult to pick up communications at long range on an ordinary receiver although any good receiver could always be so tuned that at close range such double modulation could be resolved into intelligible speech. These sets furnished by Western Electric had special receivers, suitable for double modulation at all attainable ranges.

An amusing incident occurred when in 1917 the WYOMING, with several other American ships joined the British Grand Fleet in Scapa Flow. Mr. T. McL. Davis, who for many years has been head of the radio receiver work at the Naval Research Laboratory, was the radio operator on the WYOMING. When they were approaching the British Fleet, he rushed into the ward room, grabbed a small portable phonograph, brought it to the radio room near the microphone of the transmitter, and put on Cohan's song called, "The Yanks are Coming". This practically disrupted wireless communication within the British Fleet for some time, as the British operators had never heard anything like this and all who were listening called the WYOMING asking for more music.

Subchasers and some destroyers were equipped with a small five watt radio telephone set made by the Western Electric Company, known as the CW-936. This little set had a remarkable war and postwar record and was, above all, the set that popularized voice communication in the Fleet. When the Squadron commanders began maneuvering ships in the early 1920's, using this equipment, an ever increasing demand for voice communication arose in the Fleet. The Navy standby in 1918, on both ship and shore, was the quenched gap code transmitter with the exception, as previously noted, of the very long haul point to point and ship intercept. The ships were equipped with long wave receivers to copy these high power arc stations at great distances.

Perhaps it should be pointed out that the Italian Navy made successful radio telephone experiments within their Fleet in 1914, with Marconi tube equipment.

The United States Navy had been interested in radio direction finding, formerly called radio compasses, for some years. The WYOMING, for instance, had one in 1915. The Navy's shore compasses didn't amount to much until after the first World War. A good many stations were installed on the Atlantic Coast by the end of 1918. The Pacific Coast soon followed.

The Navy's interest in aircraft probably dates back to at least 1911, when the Navy acquired a Wright bi-plane which was operated from a small landing field at Annapolis. The first flight with radio equipment in a Navy plane was made in this plane, either very late in 1911 or early in 1912. The earliest Army experiences with radio in a plane were done at about the same time. The Navy pilot was a Lieutenant Rodgers, afterward Admiral Rodgers. Rodgers, much later, was lost for a considerable time in a sea plane which was bound from the Pacific Coast to Pearl Harbor. He saved himself and his crew by managing to navigate his ship, on the surface of the water, some 300 miles to the Hawaiian Islands. He was a commander at that time. Later he was killed in an airplane accident.

The receiver used in the 1911 flight was one of the earliest models of ship's receivers, known as the IP-76. The transmitter was a Ford spark coil, connected to a short trailing wire and to the fusilage and guy wires of the plane. The radio operator was a Navy Electrician named Range, who retired as Chief Electrician in 1923 after twenty years of service in the Navy, but has for a long time been on duty in the Washington Navy Yard in a civilian capacity. Range is well known to the old timers of Navy radio, especially for the period when he was on duty at the Arlington Radio Station. He states that after being lashed into his seat on the plane, they strapped this Ford spark coil and the transmitting key to one knee and the IP-76 receiver to the other; the telephones, of course, he wore on his head. The power was a small storage battery, which Lieutenant Rodgers objected to very strenuously, because it weighed too much.

Two-way communication was attempted with Annapolis Radio Station "NAK"; only a few miles' range was obtained. The communication was by code, not by voice. Later in 1912 Lieutenant Towers, now Admiral Towers, Commander-in-Chief of the Pacific, relieving Admiral Nimitz, was the pilot. The radio operator was Ensing Charles H. Maddox, now Captain Maddox. Electrician Range again assisted in this installation. This was a better installation, because the National Electric Supply Company of Washington designed and built a small 500 cycle generator to power the radio transmitter. Mr. Range believes that the receiver was still the IP-76.

Rodgers and Towers were primarily interested in flying, but Maddox has always been interested in radio. Although I haven't seen him for some time, I am sure he still retains that interest.

When I arrived at Hampton Roads, I found the main reliance of the patrol branch in the Operations Division was on a spark transmitting equipment known as the type SE-1300. This was a 200 watt set, the generator of which at least, was patterned after a French set. This operated usually on a frequency close to 600 kilocycles. The instruction book claims that it was good for communication up to 100 miles. The transmitter proper, with spark wheel and generator, was mounted in a streamlined case and placed in the slip stream of the propeller of the plane. The driving force was obtained from a small air driven propeller on the radio assembly. This was a wooden propeller, intended to give about 5000 revolutions a minute when the plane was cruising at 65 miles an hour. At higher speeds, as when the plane was diving or opened wide, (it seldom went over 120 miles) the propeller rotated more rapidly. The power of the set then went up and the pitch of the spark signal rose. By listening to one of these sets you could tell pretty well how fast the plane was moving.

This set made by the International Radio Telegraph Company, was soon supplemented by the SE-1300, which had 500 watt power in a similar design. Later, radio set power supply was operated with a single bladed constant speed Deslaurier propeller.

Another popular set was a quenched gap set built by Cutting and Washington, based on the quenched gap invented and named after Professor Chaffee at Harvard. This C.P.1110 was a very reliable set, but required careful adjustment. The National Electric Supply Company made a spark set called the CN-1105, and the International people one called the CQ-1115. These sets all used a trailing wire antenna leading out from an insulated reel, which carried a wire with a streamlined lead weight at the outer end. If this wasn't promptly reeled in before landing it was snapped off. Sometimes it jerked off anyway, as once when we were over at Norfolk, when this streamlined lead weight plummeted down through three floors of a home and imbedded itself in the cement floor of the basement.

Later on, Mr. C. B. Mirick, who was attached to the radio outfit at Hampton Roads as a junior lieutenant, invented a hollow shell weighted with fine shot. When released, the shell popped open, spilled the shot and fell harmlessly. I remember an occasion when a solid weight missed a policeman on the street by only one foot, flattening itself against the cement pavement after knocking out quite a hole. The policeman calmly brought it back to the air station, saying he thought maybe we wanted it. Mirick's form of the antenna weight was never officially adopted, because with the improvement of aircraft radio within a few years we were using principally small fixed antenna, not requiring weights.

In the early days we used an antenna suitable for transmitting if we were forced down on the water in a seaplane. Unless there was severe engine trouble, one could taxi along the water and still keep the radio generator turning over fast enough to get something out of it. This skid-fin antenna was also used for short range transmissions in the air when it was not necessary or advisable to reel out a long antenna.

The first time I went up in a plane and listened to signals from the ground, I felt pretty hopeless. The places assigned for the radio operator were in no way shielded from the tremendous noise of the engines. Engine mufflers were not too efficient. In addition to this, the ignition interference from the spark plugs in the engine was terrific. In order to shut out the engine noise we experimented with all kinds of radio helmets, the telephone receivers being buried in the ear caps of the helmet, surrounded with a sponge rubber device, which was supposed to close up the ear against extraneous sound. Only too often this rubber had become stiffened with age and perfectly useless, because it no longer conformed to the contours of the ear and head. A four hour flight under such conditions usually left the operator completely exhausted, with his hearing level so far down that he could hardly converse with people when first off the plane.

I made a special helmet, which padded the ears right next to the telephone receiving caps with a soft rubber arrangement made out of a bath sponge. This conformed easily and comfortably to the head and ears, and was the best and most comfortable helmet I ever saw. It was loaned to Lieutenant Rodd when he went across the Atlantic in the NC4 and never returned.

The conquest of ignition disturbances in planes is not completed, even at this time. I very early advocated complete shielding of the ignition system, but it was very difficult to carry this out, as the operating people were very fussy about their ignition, for fear of engine failure. Our smaller planes were all single engine planes, and engine trouble generally meant something pretty serious. It was nearly as bad with the big sea planes, the F-5-L's, because, although they had two engines, they couldn't maintain themselves very long in the air with one engine, and it took a terrific rudder effect to keep them going straight. The greatest bug-bear of flight was engine trouble. In those days, all planes, badly underpowered, were able to fly only a little faster than necessary to take them into the air. Sometimes, in a storm, they went backward with the wind faster than they went forward over the ground. This lack of sufficient power also meant that if the planes got into dangerous attitudes, it was very difficult to pull them out again.

As far as I know, the first radio telephone set installed at Hampton Roads was designed for the H-16 class of twin motor flying boat. It was a Marconi set known as the SE-1100. This set was able to work continuous wave telegraph, with a theoretical range of 150 miles, and voice communication with a range of 60 miles. The set, as installed, including all components and a receiver, weighed 210 pounds. Tone modulated telegraph transmission could also be used. This transmitter used two large G.E. Plyotron tubes, one as an oscillator and the other as a modulator. This set gave us lots of trouble and was never particularly reliable, although when in first class condition, it would operate and the range obtained was very good. This set was driven from a battery and a dynamotor, but it was very difficult to keep the batteries properly charged, as there was a heavy drain on them. It was able to transmit when the plane was down on the water, disabled, by erecting a small telescopic mast, which was stowed away in the tail of the airplane when not in use. The sole source of power was from the storage battery.

A much lighter weight telephone set (complete installation weight 87 pounds) was the CG-1104, a set using small tubes and manufactured by the General Electric Company. This set could be used for either telephony, continuous wave transmission, or tone modulated code transmission. It too was supplied by storage battery and dynamotor, being a low powered set. The life of the battery was much longer. The range of the set was not very great, especially on telephony, since the modulation was not at a sufficiently high level. Nevertheless it was a very useful set.

Another set of higher power was the CAG 1295, designed for any type of plane or dirigible. It weighed 94 pounds and had voice transmission.

There were several other sets, but the ones mentioned will give an idea of the sort of equipment we had to work with at the time. The microphone was subject to much experimentation, again due to the high acoustic noise level in the plane. Microphones were designed that could be strapped in front of the operator's lips and provide partial shielding against engine noises. These gave some improvement.

Extensive experiments were made with chest and throat microphones, which were little capsules built to contact the operater's body. It was protected from outside noises by pads, or by the operator's clothing. These would give very much lower noise level in the speech, as received on the ground from the plane in flight, but they did not give good quality of modulation, on account of distorting resonances of the chest and throat cavities of the operator. Furthermore, the different frequencies of the voice were differently attenuated in getting through the throat or chest to the microphone.

Naturally we had some crashes in the process of testing all this equipment. One of these I remember particularly well. We had received from the General Electric Company a new 50 watt tube set intended for voice and telegraphic communication. Mr. Kenney, for many years associated with that Company, came down to participate in the test. This was the only model of this particular transmitter. The test was run in the fall of the year. The weather was none too good and the sea pretty rough. The flight was made in a twin engined flying boat which was forced down by engine trouble, some eight or ten miles off the shore of Mobjack Bay. The plane had a hole torn in the bottom of the hull in landing and rapidly filled with water, sinking to the lower wing. The crew, including the people on the tests, scrambled up on the wing - all except Kenney, who finally emerged from about six feet of water, struggling up with the transmitter, which he was determined to save. It was upon this model that production was soon to be started. Furthermore, he made another trip to the bottom of the plane, finally succeeding, working under water, in disconnecting and salvaging the dynamotor. The lashed this equipment to the wind of the plane and sat shivering while awaiting help. Since they had not been able to send an emergency message, we at the air station knew nothing of their trouble. After an hour or two, the water seeped into the lower wing of the plane and that surface went under the water. When the crew had scrambled with the equipment to the upper wing, things looked pretty black. There was no help in sight and the weather was rapidly getting colder. Kenney then made a famous remark, which those of us who knew him will never forget. He looked around and said: "Well boys, this looks like the finish, but if I have to go, I am glad I am going in such darn good company". A half hour later, a fisherman hove in sight. After frantic signaling they attracted his attention. He brought them in to Hampton, where they telephoned for a boat from the air station in which to cover the four miles across Hampton Roads. They arrived after dark, but had no sooner set foot on shore than Kenney remembered that he had left the dynamotor at Hampton on the dock. In spite of the fact that we was soaking wet and cold, no one could persuade him from going back immediately to get that dynamotor. I am glad to say that, aside from a rather bad cold, he suffered no serious ill effects. After passing suitable tests, that set went into extensive production and was widely used on our Naval flying boats.

On another occasion one of our flying boats, which was coming in from a patrol over the Atlantic, outside the Virginia Capes, had a water circulation connection fail. The pilot was afraid he would not be able to get in on one motor, so he made a landing in order to fix this radiator connection. Unfortunately, the planking in the bottom of this seaplane was rather rotten. When the landing was made the sea, which was rather rough, tore a hole two feet wide and six feet long in the bottom of the boat. With rare presence of mind, the pilot opened both motors wide and dragged the plane off the water again before it could fill. He immediately let out his radio antenna and sent out the following message to the station: "Please have doctor and speed boat ready as I will sink immediately on landing". Of course he burned up one motor, but he managed to get in, landing in front of the station, almost on top of the doctor's launch. Flying planking cut an artery in the leg of one of the mechanicians and he might have bled to death if we hadn't had the doctor there. As it was, in a few days he was back on duty.

Incidents such as these went a long way towards selling the pilots the new gadgets of radio. With planes so under powered and unwieldy the pilots could hardly be blamed for resenting the addition of every new piece of equipment.

Radio had its war-time inhibitions as far as operations were concerned. One of our three-man dirigibles, accompanying a convoy, including several destroyers, went well out to sea, but shortly after it left the station we heard no more radio communcations. Indeed, for three days we heard nothing of this ship whatever and gave it up for lost, with all hands. It appeared, however, that the Commander of the convoy had ordered radio silence, so we had no means of knowing that the blimp had a jammed rudder about 70 miles out to sea. In spite of all efforts to free the rudder, it was only able to cruise in a circle. With the wind off shore this didn't do much good. Finally they came down on the water, close to a destroyer, and were picked up. About all they salvaged was the radio equipment and a few other instruments. The destroyer, not being able to break radio silence, had not told us of this. A week later the destroyer turned back from the convoy and, to our great relief, brought the party home.

We took advange of this opportunity to study the effect of salt water immersion on the radio set. We discovered that after soaking in warm fresh water for a few hours and thoroughly drying out the set, it was fully operable after we cleaned it up and replaced one or two very minor parts.

Of the men who were with me in radio at Hampton Roads, I can report that Mirick was head of the Aircraft Section of the Radio Division, Naval Research Laboratory for a number of years and is still at the Naval Research Laboratory. Ensign Rodd, later Commander, was unfortunately killed some years ago. Lieutenant Davis remained with the Navy a number of years and I believe he was called back to duty in the late war. He is probably on the

West Coast now. Dresser is still at the Naval Research Laboratory. Palmer was retired some years ago, but called back during the late war as a commander. Palmer was one of the best test pilots I knew. Most of our people say they felt more secure flying with him than with anyone else. I have lost track of Ensign Eltz.

Ensign Hanson later became a civilian employee of the Naval Research Laboratory, Aviation Section. Hanson was a man of remarkable experimental ingenuity, with a tremendous drive, and no respect whatever for any regular working hours. His genial personality made him welcome in any circle. If there was anything that he particularly loved, it was experimental work which involved a liberal amount of hazard, as when he ran away from the Laboratory as a stowaway on Admiral Byrd's ship on the way to Spitzbergen, whence the Admiral's North Pole flight took off. Later he accompanied Admiral Byrd to Little America. I will have something to say in another chapter about the tests run by that expedition with the Laboratory, with Hanson at the Little America end.

After a number of years when he was on duty at the Air Station, Anacostia, he later went into commercial work. He came back into the Navy as a reserve officer serveral years before this country got into World War II. His important work in lining up development and procurement in aviation radio and radar just before and during the early part of the War, when he was on duty in the Naval Bureau of Aeronautics, deserves a great deal of credit.

The radar in use in the Aleutians by the Navy in the early days of the war was an absolute necessity in that fog-bound country for purposes of navigation if nothing else. Commander Hanson made a trip into the Aleutians, to see what he could do about improving radio and radar facilities for planes, and was killed in an experimental flight when the plane crashed into the side of a mountain in a dense fog. No one can be certain, since there were no survivors, but the accident must surely have been due either to engine failure or to failure of the radar which Hanson was attempting to improve.

In the fall of 1918, I was ordered to the Naval Air Station, Anacostia, D. C. for the purpose of organizing an aircraft radio laboratory in Washington. I still had additional duties at Hampton Roads and still directed the radio work there, but not the whole experimental division. I spent a considerable percentage of my time at Hampton Roads, being there at the time of the Armistice.

After conferences in the Bureau of Engineering with Admiral Robert Griffin and Lieutenant Commander Hooper, it was decided that aircraft radio would be better off near the Navy Department, where interested Bureau officers could have easy and frequent contact with the work. Therefore plans were formulated to put up some kind of a building at the Air Station, Anacostia, for housing this activity. In the meantime, Admiral Griffin made arrangements with Dr. Stratton, Head of the National Bureau of Standards, to temporarily locate a group of men under my direction at the Bureau of Standards. Room was made for us on the third floor of what was called the East Building. Although we reported only to the Navy, the Bureau of Standards took care of us very well until we could get our little laboratory at Anacostia established.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER VIII - 1919-1923

AVIATION RADIO NAVAL AIR STATION, ANACOSTIA

CHAPTER VIII

1919-1923 - Aviation Radio, Naval Air Station, Anacostia

By the first of January 1919 the Aircraft Radio Group was established temporarily at the Bureau of Standards and a World War I Type A barracks was in the process of being converted into a laboratory at Anacostia Air Station.

At the Bureau of Standards, Chief Radioman L. C. Young was the outstanding technical man. He was assisted by Walter Parks, O. C. Dresser, Atkins and Bernard, (later of the Radio Communications Products Corporation). Chief Yooman R. B. Meyer was still acting as my secretary. Lieutenant Nelson USNRF, was with us for a short time but he was released from active duty at his own request and returned to the Bell Laboratories. I was very sorry to lose him.

Up to August 1919 work was carried on at both the Bureau of Standards and Anacostia Air Station. After August all activities were located in Anacostia.

The Bureau of Standards was not in a position to build gadgets for us, as their mechanicians were heavily loaded with their own work. I therefore requested the Navy to detail me a good Chief Machinist Mate. I finally obtained a man named Richmond who, as it happened, had never handled any machine work on anything much smaller than heavy engine parts, pieces of shafting, or the armature of a big generator. He was sure that he couldn't possibly do the fine work which we would require, but I persuaded him to put himself under the Chief Mechanician in the Bureau of Standards shop, who did a wonderful job of training. Richmond turned out to be one of the best mechanicians with whom I have ever dealt. He was later attached to the Naval Research Laboratory. After his enlistment expired he was taken on as a civilian employee, remaining with the Laboratory for many years. I regret to say that he died not long after his final retirement.

After this group reached Washington, I found that the Bureau of Engineering, Navy Department, relied upon me as a general consultant in all radio matters so that from this time on my work was by no means confined to aviation radio, although that continued to be the main business of the Aircraft Radio Laboratory.

One of the first problems set up at the Bureau of Standards involved the further study of engine ignition interferences on radio receivers. A 400 horse power Liberty engine was obtained, which could not be run under its own power within the Laboratory. After pulling the pistons out of the engine it could easily be driven with a small electric motor, the ignition system being set up and operated much as in a plane, except that the spark plugs did not function under compression. The set up gave a fair simulation of ignition disturbances. We had hoped that we would find them principally on certain frequencies, so that their effects could be easily filtered out. On the contrary, interference was strong over a wide band of frequencies, even up to 1000 megacycles, which is 30 centimeter wavelength. By putting in suppressors in the spark leads the effect could be reduced, but since this also reduced the intensity of the sprak, we knew the pilots and aviation engineers would not accept such a solution. We therefore reiterated my earlier recommendations that complete shielding of sprark plugs and ignition harness be adopted.

One of the transmitters studied while at the Bureau of Standards was a 250 watt telephone and telegraph transmitter intended for operation from ship or shore to aircraft. It was while working with this transmitter that I conceived the idea of maintaining a certain element of secrecy in speech while using the transmitter simultaneously for both telephone and telegraph messages. This was done by arranging the telegraph key in such a way that, when a dot or a dash was sent, the frequency of the transmitter changed by a very considerable amount, perhaps as much as 100 kilocycles. If then telephony was going on at the same time, the speech as heard on a receiver, tuned to either the up key or down key frequency, would be badly chopped up by dots and dashes and practically unintelligible. At the receiving end, which was at the Anacostia Air Station, two receivers were tuned to these two waves and their outputs combined in such a way as to get intelligible speech.

In connection with this device I filed an application for a patent. In due time the Patent Office sent me a reply, throwing out the case because they said it was inoperable. I wrote a letter stating that we had been operating between the Bureau of Standards and Anacostia Air Station and, if they would send their patent experts to look it over, we should be able to convince them that it was operable. They didn't accept this invitation, but shortly thereafter rescinded their action and issued the patent. This sort of system has been proposed by others but it has no real value now, for the simple reason that radio channels are too valuable to tie two of them up for a channel of communication, especially in the band where this device had to work.

Another set of experiments undertaken at the Bureau of Standards had to do with very long range aerial navigation. The Navy was getting ready for the flight which resulted in the first crossing of the Atlantic by an airplane. The Laboratory was asked to explore the possibilities of installing a loop direction finder which would tune to long wave high power stations. It was hoped that these signals could be used to guide planes over seas for great distances. Loops were installed on a plane but we did not have very suitable amplifiers, so were forced to make the loops so large that they were no longer dirigible. The plane had to change course in order to get bearings.

Rather crude bearings were obtained on the German station at Nauen on 12,600 meters, (about 23 kilocycles) while flying in the day time. A few years later the Navy made use of such very long range airborne radio direction finders using small rotatable loops and amplifiers. Suspecting that the bearings might be uncertain at night, I set up a suitable rotatable loop, with receiver and audio amplifier, in the laboratory at the Bureau of

Standards, and took a large number of observations on various stations, including the Annapolis high power station which was in operation at this time on 17 kilocycles, and several RCA stations on somewhat higher frequencies, particularly New Brunswick, Sayville and Marion. We carried out these observations continuously through night and day, finding the most extraordinary variations of the night signals, the errors in bearing changing with great rapidity and sometimes amounting to as much as 90°.

This work attracted a good deal of attention and discussion. Since I was the guest of the Bureau of Standards, I thought it would be well to let them publish my paper on these experiments in the "Bulletin" of the Bureau of Standards, which was a very high grade publication. When I turned this paper over to Dr. Rosa, he said he would be glad to look it over. I found out later that he called in Dr. Frederick A. Kolster who, since 1912, had been the direction finding expert for the Bureau of Standards, and asked him to read the paper. Kolster and I have always been good friends, so I therefore do not think he will take it amiss if I quote him on his story of what happened. After he reviewed the paper he reported to Dr. Rosa, stating that he had never observed any such crazy variations of any long wave station. His compass was set up in the field, well away from the influence of the neighboring buildings. He thought perhaps my extreme variations of bearing were due to working inside of a building. Dr. Rosa told him he had better have a talk with me and persuade me to withdraw the paper from publication.

When we got together I told Kolster that I was well aware that all my bearings required a correction of about 12° due to the influence of the building, but that did not in any way account for the rapid variations of bearing which occurred at night. I asked him if he had taken any observations after dark. He said no, he always worked during the regular hours of the laboratory. I then suggested that he put a watch on all night. The following evening he was at his outdoor station while I put a third class Radioman on my set, both taking observations all night long on the same transmitter, namely, New Brunswick. The next day the Radioman brought in a curve of variations which were as bad as any I had seen. Kolster had a similar curve, which coincided very well with mine when slipped over 12° to allow for the building deviation, due to my indoor location. Dr. Kolster stated that had he not personally taken these observations, he would never have believed the thing possible.

Erratic behavior of compass stations on nocturnal bearings occurs only within certain very important ranges, namely, the ranges where a sky wave from the Kennelly Heavyside Layer (ionosphere) plays an important role in the transmission. This effect, ever since its appearance has been known as the "night effect" on radio direction finders. My work initiated extensive experiments on other frequencies, especially the standard coastal compass frequencies of 375 kilocycles. It is obvious that even at this frequency irregularities of bearings can occur as the distance to be traversed becomes too great. While on a Naval vessel at sea I have directly confirmed this point. It a ship at sea requested a bearing at some distance in excess of 200 miles or thereabouts, the shore stations would usually refuse to guarantee their accuracy. I may say that modern methods of taking "flash" bearings have been evolved to get around this difficulty, but it has only happened within the last few years. Needless to say, Dr. Rosa finally accepted my paper and it was finally published in the Bureau of Standards Bulletin.

In the spring of 1919 my group worked out what we called a compound heterodyne long wave receiver, which was connected to a loop antenna and installed on the roof of the Navy Department for use of Navy Radio Central. The location was such that highly directive long wave equipment, especially that using long wires, could not be used, so we had to content ourselves with the moderate directivity of a loop. A very small antenna was coupled to this loop, which gave it a heart shaped figure of directivity. This device has very broad directivity in the direction of the maximum signal but the direction of zero reception is very sharp. Thus it can be directed against interfering stations and atmospheric disturbances so as to largely exclude them. This device was used successfully in improving reception from our long wave station at San Diego.

The cascaded double heterodyne principle was applied in order to get sufficient amplification to work with the loop. It should be remembered that multistage cascaded amplifiers were not used in those days as they are now. They had to await the invention, by Dr. Hull of the General Electric Company, of the shielded grid receiving tube (tetrode). There have been many attempts to use triodes or three element tubes, having only filament, plate and grid, for such purposes. Since the tendency of such devices is to be regenerative, they usually develop oscillations which destroy the clarity of reception. These can be suppressed with so-called "loss" resistances, but this cuts down the sensitivity of the device and partially defeats its purpose.

This compound heterodyne idea was for the purpose of changing the frequency of the incoming signal twice before it finally got into the telephones, permitting two sets of amplifiers on these two different frequencies. An ingenious circuit arrangement permitted this to be done with one auxiliary oscillator or heterodyne tube.

During this same period I was directing the radio work at the Air Station as well as at the temporary laboratory at the Bureau of Standards. One of our most interesting jobs was to assist in the equipment of the NC boats with which the Navy made the first trans-atlantic flight in midsummer of 1919. Lieutenant Mirick, who had come up from Hampton Roads, was put in direct charge of the installations on the NC boats, the work being done at Rockaway, L.I. These flying boats were the most awkward looking craft anyone could hope to see, but one of them did succeed in crossing the Atlantic.

The NC/4 craft, biplanes with about 154 foot spread of the upper wing, were equipped with four Liberty motors. A pair of these were mounted on each wing, back to back, one propeller pushing and another pulling. They were slow and sluggish, but they did have a large carrying capacity. One of them, off Atlantic City, took up a party of fifty people for a short flight.

The first part of the trans-atlantic flight was from Rockaway to Trepassy, Newfoundland; the second leg was from Trepassy to the Azores; the third leg was from Azores to Lisbon, Portugal, and the fourth from Lisbon to Plymouth, England.

As the installation was made under Mr. Mirick at Rockaway, the radio receivers were operated from a separate battery, which greatly reduced the ignition disturbances in the receiving system. One of the planes, the NC4, got into trouble on the flight to Trepassy, but after repairs at Cape Cod, rejoined the others at Trepassy. The flight leaders began to worry about weight and threw everything overboard that wasn't absolutely needed.

The radio receivers were put on the same batteries that supplied ignition for the plane. This made ignition disturbances very much worse and, later on in the flight, had important consequences.

The Commanding Officer of the flight was Commander Towers, now Admiral Towers, in the NC3, whose pilots were Commander Richardson and Lieutenant McCullough. Lieutenant Commander Reed (afterward Rear Admiral) had charge of the NC4, which got through to England, and Lieutenant Commander P.N.L. Bellinger (now Vice Admiral), commanded the NC1. Lieutenant Commander (now Vice Admiral) Marc Mitscher was one of the pilots of the NC1, assisted by Lieutenant L. T. Barin, later killed in a crash on the West Coast. Lieutenant Sadenwater was radio operator under Bellinger; Commander Lavender was operator under Towers on the trip from Trepassy to the Azores. The crew of the NC4, which successfully crossed the Atlantic under Lieutenant Commander Reed, consisted of Lieutenant Hinton and Lieutenant Stone, pilots; Lieutenant Rodd, radio operator; "Smoky" Rhodes, Chief Machinists mate and Lieutenant J. G. Breeze, pilot-engineer. Lieutenant Rodd, in particular, hung up some excellent communication records up to 700 miles from Bar Harbor, which station was his contact point on this side. When the receivers were connected to the trailing antenna, sufficient signals arrived to get through strong ignition disturbances. Signal level on the radio direction finder loop was so much lower that it was very difficult to get bearings at a useful range.

When the three planes landed for refueling at the Azores, they found heavy seas and low heavy clouds. The NC1 landed in heavy seas about 100 miles west of Flores, sustaining such damage that she could not take off again. Before coming down she had broadcast a message: "From NC1, lost in fog" which was picked up by the destroyer MELVILLE at Ponta Delgado. After taxying on the surface for five hours, the NC1 was discovered by the SS IONIA and the crew rescued. The plane was taken in tow, but the line parted and the NC1 disappeared beneath the surface.

The NC3, after alighting on the water, was able to receive radio, but not transmit. From 3:00 in the afternoon until the following morning they struggled, in spite of heavy damage to the plane, with difficult surface navigation. She finally made her way, after a 205 mile surface journey, into the harbor at Punta Delgado, after having refused aid from the destroyer HARDING, which offered assistance.

Reed, however, had the good luck to find a small hole in the overcast, got a sight on a mountain peak, and was able to land in more sheltered waters in farily good shape. After about five days at the azores, waiting for the sea to moderate, which it did only partially, Reed took off for Lisbon.

Now the Navy knew that this was going to be a hazardous flight so it had a line of destroyers out across the Atlantic to mark the course. Upon these destroyers the planes were supposed to take radio compass bearings. The seas were still fairly heavy when Reed took off. His magnetic compass was jolted and damaged, so that he was actually headed for the Coast of Africa instead of Lisbon. Not finding the next destroyer in line at the calculated time, he called upon Lieutenant Rodd for radio bearings on the nearest destroyer, which happened to be the WILKES. At first Rodd couldn't get sufficient signals to get a decent bearing, although he was in communication with the WILKES by using the antenna. He kept calling on the WILKES for more power. Chief Radioman Wiseman, on the WILKES, who died recently, told me that in response to that repeated request for more power he coupled up two 500-cycle 5 kilowatt generators, synchronized and operated them in parallel, and practically burned up the quenched spark gaps of his transmitter, but did succeed in delivering a signal upon which Rodd could get a bearing. That bearing was not dead ahead of the NC, but 60° off to the port side. Lieutenant Commander Reed told Rodd to take another bearing in fifteen minutes. All this time of course, the WILKES was not visible.

The next bearing showed the WILKES at 90° on the port side. Reed then knew that he was off the course, believed the radio compass was right, and made a right angle turn so that in twenty minutes he flew over the WILKES. Thereafter he had no serious trouble in getting to Lisbon. Later, on the way to Plymouth he had to make a forced landing, on account of engine trouble, and put into a small Spanish port over night while it was repaired. He and his crew finally arrived safely at Plymouth.

It will be seen then that radio played an important role in the flight, but that due to ignition trouble it very nearly failed. We used this as a further argument for better shielding of the planes' ignition system and for independent radio batteries.

We soon had an H-16 twin engine flying boat, well shielded, equipped, not with the crossed loops mentioned earlier, but with a single loop which naturally was a simple installation. We correctly estimated that with improved receiving conditions we should be able to successfully use this single loop for radio navigation.

In order to prove this point it was decided to send a Naval ship to an unknown point and require the plane to locate it. The ship was to transmit on request by the plane, and the plane was to use radio bearings to locate the ship and fly over it. An experimental F-5L installation was used, with Lieutenant Palmer at the controls and Mr. L. A. Gebhard at the radio compass. It started out from the Anacostia Air Station one morning in the summer of 1920 to locate the battleship OHIO. The OHIO was then our experimental radio ship. Her guns had been taken off, and under Captain (later Admiral) Halligan, she was used for a few years as a floating radio laboratory. The unfortunate disarmament conference in 1921-1922 cost us the use of this ship. She was, of course, of no military value, but in spite of this England and Japan insisted on her being scrapped.

I had boarded this ship at Annapolis the day preceding the flight, so the crew of the plane knew that we probably would be somewhere in the vicinity of Hampton Roads. As a matter of fact we were lying in Lynnhaven Roads during the first day's flight. A few minutes after the plane took off Gebhard reeled out his antenna and called the OHIO requesting signals upon which he might take a bearing. Captain Halligan left the OHIO part of the show entirely to me, so I directed the test from her radio room. The plane kept taking bearings successfully all the way down to Hampton Roads. Just before it arrived at Hampton Roads it signalled that it would come out and pick us up in the morning. It was going into Hampton Roads for gas that evening. There wasn't much night flying done in those days except by the air mail people and some of that was pretty disastrous. The country wasn't covered with radio directional beacons and radio blind landing systems as it is today. With low powered planes we had trouble enough bucking bad weather in the day time without risking it at night.

On the Ohio we decided to make a surprise move, so without making any departure report we pulled up anchor during the night and went 100 miles outside the Virginia Capes, where we lay to at about the 100 fathom line. At 9 o'clock in the morning the plane took off from Hampton Roads Air Station, called us for a bearing, headed for us, and arrived in an hour and five minutes. The sea being a little rough the plane didn't venture to land. We talked over the test and advised that the pilot go back to Hampton Roads. On turning again for shore the operator called up the station on Cape Henry and asked for signals, took a bearing on them, and got home in short order. The actual plot of this flight shows a slightly curved line due to the effect of the lateral wind, as would be expected. In those days this was considered quite an achievement and did a good deal to popularize the use of direction finders on board planes.

In 1919 Lieutenant Mirick, aided by Mr. Dresser, was loaned to the Air Mail Service. They installed the first direction finder loops on a Curtis "R" type land plane at College Park and made a number of flights with this equipment as far as Philadelphia. Radio directional navigation was fairly successful; they found the exact location of the station at the Philadelphia Navy Yard and obtained very good bearings on Norfolk when over Point Lookout, Maryland.

During this period we were constantly working to improve radio transmission between ship and shore and planes as well as between planes themselves, with a good deal of emphasis on voice transmission. Quite early in the aircraft radio picture, the Army pinned its faith largely on radio telephony, partly, I think, because the pilots hated to have to learn the radio code. The Navy didn't graduate a man from Pensacola as a Naval Aviator unless he had a fair knowledge of the code.

In the course of experiments with voice transmission we put together a transmitter at Anacostia which became the first broadcasting station in the city of Washington, and, except for the very early experiments of DeForest, one of the first broadcasting stations in the country.

In order to demonstrate to the Navy Department the possibilities of voice communication with planes, on the 7th of March 1919 we arranged for a test involving a plane, a transmitter at the Navy Yard, and an extension telephone to the Navy Department. Mr. Gebhard on this day talked over short ranges with Commander Hooper of the Radio Division of the Bureau of Steam Engineering, and with Admiral Griffin, Chief of that Bureau. Three days later, Lieutenant Sadenwater talked from a plane 60 miles down the Potomac to Admiral Griffin at the Navy Department. We thought we were all set, so the next day we invited Secretary Daniels to talk, but something went wrong and only a few words were successfully exchanged. On the following day communication was very successful and Secretary Daniels, Admiral Griffin, as well as Captain Todd, Director of Naval Communications, talked successfully to Sadenwater who was in the plane, 60 miles away. The set in the plane was the one made by General Electric and referred to in the previous chapter. Mr. Kenney, of the General Electric Company, was with us during this test. Not very much later we were able to have officers of the Navy Department give orders to a pilot 160 miles away.

In the effort to improve the range of telegraphic airborne radio equipment, two transmitters were developed known as the 1375 and 1385 respectively. These transmitters, for telegraph only, gave a clear 500 cycle note. One of them, using two 5-watt tubes, was engineered by Fred B. Monar, who afterward was employed at the Naval Research Laboratory and later, for many years, at the Navy Department. He is now retired. The second one, which used two 50-watt tubes, was engineered by Mr. L. A. Gebhard. These two Transmitters were the backbone of reliable Navy airborne communications for at least five years. Some of them probably lasted a good deal longer. The smaller set was used in embryonic fighter planes; the larger one in the big flying boats. One of the first transmissions by teletype from plane to ground was carried out, in this same period, suing such transmitters.

Since nearly all our test planes were sea planes, taking off from and landing on the water, we usually flew down the Potomac in making a performance test on a new piece of radio equipment. Naturally from time to time one of these planes would get into trouble and made a forced landing upon the water. On one occasion the landing was made on Buckner Creek, which is an estuary of the Potomac about 55 or 60 miles, as the crow flies, from Washington. The pilot got close enough to the shore of the creek, which is rather shallow, so that the crew could wade ashore and look for a house from which to telephone back to the station for certain spare parts, which could be flown down in order to make the necessary repairs.

Now it happens that there is a very beautiful old colonial mansion, known as "The Glebe", very close to the banks of Buckner Creek. At the time, this mansion was owned by Mr. Wetherill. Someone from the house had seen the plane land and met the crew on the shore, inviting them in to use their telephone and subsequently to a fine dinner. It took a day or two to get the spare parts and make the emergency repairs. In that interim our people were very well entertained indeed. It is a very curious fact that thereafter, whenever any of our planes had engine trouble or other difficulties, they were almost always found at "The Glebe".

Late in the fall of 1919, Lieutenant Bush, Captain of the Air Station, with Lieutenant Palmer and myself, made the trip down the river in connection with the test of a radio transmitting and receiving equipment. It seemed a pity to get down into such good quail hunting country without taking advantage of our opportunity, so the Captain (Bush) took along his hunting dog and shot guns and shells. Upon completing the test we landed in Buckner Creek but, on going up to the mansion, found no one home. In the meantime, Mr. Calhoun, from a neighboring estate, accompanied by his daughter, Gentry Calhoun, having seen the plane land, rode over and intercepted us. They insisted on fixing up a hunting party and providing us with a splendid meal, which, in order that we could keep an eye on the plane, was actually prepared and served at "The Glebe". The Calhouns had rounded up the robust old negro mammy, who was a wonderful cook, and a few other servants. By three o'clock, when we got back from our hunt, we all sat down, including the Calhouns, to one of the finest meals I ever tasted. In those days these people were relatively isolated. Good roads, which since then have been built into this country, make the situation very different nowadays, but at that time they were very happy to have visitors and certainly treated them royally.

Unfortunately, we got a rather late start coming back. The Potomac is famous for its bumpy air, due to the alternating land and water as the flight cuts across various headlands, but that night, although there wasn't a breath of wind stirring and the night was chill and clear, the air was the most turbulent that any of us had ever experienced. We had another difficulty which properly equipped planes do not have in these days. The surface of the water was such a perfect mirror that it could not be seen. Only the reflections of the stars showed up, so it was quite impossible to more than guess at the plane's altitude. When we came down for a landing we leveled off three times before we actually hit the surface of the water, and then we hit it too hard and bounced many feet into the air before we came to rest. Unfortunately, the dog, who was with me in the front cockpit, had been distressingly sea sick (or air sick) all the way home.

Landing difficulties such as these are completely obliterated with modern altimeter equipment, but the old standard barometer type of altimeter, which was the sole equipment in those days, could not be relied upon to much closer than 50 feet and therefore was nearly useless for coming down on the water at night or in a fog.

In order to improve the efficiency of our transmitter at the Air Station, we set up a miniature reproduction of the Alexanderson multiple tuned antenna, small enough to use on the frequency which we commonly used for communication with the planes, by either code or voice, and for broadcasting. This was about 820 kilocycles.

About this time the Navy's Lakehurst Station for dirigibles called on us to devise an antenna installation of very low height for that station. The high radio towers usually used were considered to be a hazard to the landing of big dirigibles. We therefore built a transmitter especially designed for a long, low, multiple tuned antenna system, operating in conjunction with a counterpoise, which was necessary on account of the sandy ground at Lakehurst. Mr. Gebhard was largely responsible for the design of this transmitter and its installation. It was quite successful in operation but, since it had four down leads it wasn't too easy to change quickly from one frequency to another. Lakehurst used this transmitter for some time.

During one of our flights at 7 or 8 thousand feet above the Anacostia Air Station, the radio operator in the plane reported a complete fading out of the signal when he was immediately over our antenna. We made an extensive investigation of this matter, which we called the cone of silence effect, and found that, in general, if the plane was not too low the signals would very nearly vanish during a time interval which depended on the altitude and speed of the plane. This amounted to quite a number of seconds for high altitude. Thus a plane approaching in overcast weather could supplement its direction finder bearing with the cone of silence data and locate itself with respect to the landing strip with a certain amount of accuracy, not good enough for complete blind landing, but at least very useful.

On the 30th of April, 1920, Lieutenant Palmer, with Mr. Gebhard as radio operator, made a flight down the Potomac to Colonial Beach, demonstrating the possibility of carrying on a conversation between a submarine and an airplane.

Lieutenant Palmer and I made a flight down as far as Nanjemoy Inlet on a fine June day in 1920 for the purpose of testing a very simple altimeter which was proposed for use by planes under conditions similar to those which occurred on the flight from "The Glebe". This device consisted of a small flexible wire, weighted at the end, which could be let down anywhere from 10 to 50 feet beneath the plane. When this weight struck the water it turned on a small red light on the dashboard. This gave the pilot warning in coming down that he was within a certain distance, say 25 feet, of the surface of the water. The device wouldn't be of much use with a modern high-speed plane, but worked well enough for the sluggish craft we were accustomed to. We took along a basket of pigeons for an experience flight and sent out several with messages to the Air Station. Since there was supposed to be good bass fishing in Nanjemoy, we thought we might as well take along a couple of fishing poles and some bait. After releasing the basket of pigeons, we landed on the broad surface of the creek and taxied in close to the shore, where we encountered an ancient darky with a decrepit row boat, which we promptly rented for the balance of the day. I am sorry to say that we didn't locate any bass and spent as much time bailing that boat as rowing it. Toward evening, when we got back to the plane, which had been anchored out a little way from the shore, we found it very low in the water. This plane was a small Italian sea plane - a two seater, in fact - known as "Macchi". Palmer had warned the mechanic who had readied the plane for us to be sure to put in a bilge pump. However, a thorough search of the plane disclosed no pump. The plane had been standing out in the sun without use for some time, so of course the hull was dried out and the seams had opened up, making it leak badly. Under these circumstances Palmer decided to take the plane off without bailing out the water. We failed to get it off the water, but succeeded in overheating our oil. The oil tank on this plane was in the leading edge of the upper wing, so a geyser of hot oil poured down over our heads and backs. Why we weren't badly burned I don't know, but the result was only a couple of badly soiled uniforms and caps. We then decided that we would have to bail out, using a lunch box as a bailing can. Finally we got enough water out to get off the water and start back home, arriving just about dusk. I made up my mind that thereafter I wanted no more trips in a plane not equipped with some form of radio.

We were requested to develop emergency equipment for transmitting from a seaplane forced down to the surface of the water, due to engine failure or other accident. The requirement was that the emergency message should be received at least 100 miles. I told Commander Hooper that he was asking for the impossible. He replied that I had done the impossible before and the 100 miles stood as the desirable figure.

As a result of this assignment we figured that we would have to use the plane's most powerful transmitter. For the larger planes, where this device is particularly necessary, we reasoned that usually both motors didn't go out of commission at the same time. Although the plane might be down on the water, it would probably be able to run one motor, at least at low speed. We therefore rigged up a conical canvas wind tunnel, which took the air from the slip stream of either motor and concentrated it at a diameter equal to that of the radio generator propeller (about 2 feet). Thus, an idling motor would give a considerable breeze on the radio set propeller, by ways of this canvas cone. Special emergency mountings were rigged up in a few minutes, in order to shift the radio generator to the proper place behind the cone.

The remainder of the problem consisted of finding a suitable antenna. We tried carrying small gas filled balloons, but they were no good in anything but a dead calm, as the wind blew them down too close to the water when they were being towed by the plane. We then devised a series of kites for light, medium and strong winds, these kites being very light and very easily stowed. With this equipment on board, Lieutenant Palmer set out for a point in the Chesapeake, 100 miles distant, with instructions to come down on the water in a simulated emergency landing, rig the emergency set, and communicate with us. Palmer was a little too realistic, nearly getting us into trouble, since when he got the gear rigged, he sent out an S.O.S. followed by the words "forced landing" and giving his position. This call sent on the standard distress wavelength, was not only heard by us, but by Quantico, Norfolk and Baltimore. In fifteen minutes he had rescue ships or planes coming from several directions. After receiving a warning from us that he had better cancel that call, he sent another message to the effect that emergency repairs had been completed and that he was taking off. We narrowly escaped an investigation and reprimand. It was a very realistic demonstration, and actually the test gave ranges considerably in excess of the required 100 miles. Navy seaplanes used this equipment for many years.

During this period at Anacostia, John Hayes Hammond, Jr. was developing a radio-controlled torpedo. Radio control of a torpedo is not an easy proposition, because the torpedo must run under water. Any overhead antenna carried by the torpedo would not only betray its approach, but would exercise a heavy drag. The idea was conceived of carrying a trailing wire behind the torpedo which would create less drag and be invisible if used as an antenna. This meant that the control signals would have to be very strong, and on a rather long wave in order to penetrate through several feet of water. I was called upon to assist in the design of such a transmitter for an airplane, operating at a rather low frequency, and to develop a suitable antenna that would efficiently radiate this wave. To get a large enough antenna in the form of a single trailing wire was hardly feasible, as the wire would have to be entirely too long and would require a long time to reel in when landing. Moreover, it would exercise a lot of drag.

Mr. Gebhard conceived the idea of using a double antenna, which really consisted of a huge trailing loop, which could be reeled out from one end, once the plane was in the air. The tests, which I believe were made up near Gloucester, Massachusetts, were reasonably successful, but only by the use of such a long training wire from the torpedo that its range and maneuverability were seriously handicapped. It was never put into general use. Jack Hammond also brought down a double modulation system which was supposed to give a certain amount of secrecy in communications.

I became acquainted with the Hammonds, father and son, at this time. They were both interesting men, but Mr. John Hayes Hammond, Sr. particularly impressed me as one of the finest men I had ever met. He was greatly interested in pigeons. Since one of the main Navy lofts was at the Anacostia Air Station, he spent the better part of an afternoon, with Mrs. Hammond, at the Air Station, so I had a chance to have an informal talk with him.

The Hammond Secrecy System was tested both between the Station and portable equipment on land as well as with other equipment in planes. Mr. Young promptly devised a method whereby the messages could be easily read with an ordinary receiver, but in a few weeks Dr. Chaffee of Harvard, who was working with Hammond at this time, brought down a modification of the equipment which provided it with a frequency wobbler so that we could no longer receive the signals without the special Hammond receiver. This system was undoubtedly of great interest. It always intrigued me, but unfortunately, when everything necessary was done to guard the secrecy, it occupied rather too wide a band in the radio frequency spectrum, which rather overbalanced its advantages.

One of the most interesting personalities visiting us in those days at Anacostia was the old Prince of Monaco. He was getting well along in years, even at that time, but was still tremendously interested in anything that had to do with either aviation or radio. He had a special Lepel spark gap set on his yacht. This was rigged up with a sort of keyboard so that the modulation tone of the spark could be changed in such a way as to play simple music. I didn't hear this myself, since it was done while the ship was at anchor off New York, but there are probably many still alive who did hear it at that time.

When the Prince was on a visit to Washington, he requested permission to visit the Naval Air Station. We didn't have much warning, but decided to put on a little show for him. The Prince arrived in the afternoon with some of his staff. After looking over the planes in the hangar and seeing some of them in flight, he was taken to the Radio Laboratory. There I showed him our spark and voice transmitters and various airplane sets, and then asked him if he would like to talk to a plane in flight. The idea delighted him. Lieutenant Palmer took off with a small plane in which a voice transmitter had been huddiedly installed. After contact was established, I asked the Prince to take the microphone and talk to the pilot.

Now it happend that we hadn't had a chance to shield the ignition system of this plane, so there was considerable interference present in the receiver. The Prince spoke excellent English, but with a rather strong French accent. Lieutenant Palmer had great difficulty in understanding his remarks; in fact, he could clearly get only a few words but, being a good showman, he didn't betray this situation, always replying after this fashion, "Yes, I hear you, Prince, you are coming in very well. Sorry you are not up here to enjoy the fine weather and fine air - etc". Fortunately

Palmer's remarks came in with great clarity and I don't think the Prince ever realized that Palmer wasn't getting very good reception from him. In the meantime, he had been so interested that he had overstayed two important diplomatic engagements in the city before his staff finally managed to drag him away. He grumbled that he would rather spend the rest of the afternoon with us.

The bitter argument as to the relative advantages of ships and planes in Naval warfare had its beginning with General William Mitchell of the Army who was for scrapping most of the Navy immediately. The Navy, on the other hand, while willing and eager to support aviation, wished to make Naval aviation an integral part of the rest of the service, and didn't wish to scrap heavy ships until they were definitely known to be useless. In order to give the Army and Navy aviators a chance to see what they could do, it was arranged to give them an opportunity to drop bombs on a number of ships, some of them captured German vessels, some obsolete American warships.

I witnessed two of these tests. In the first test, the targets were not under way. It was not desired to destroy the ships during this particular test, but to determine something of the accuracy with which bombing could be carried out; therefore the bombs were made of a composition material, mostly concrete, which would disintegrate considerably on impact. In the tests I saw, explosive bombs were not used. Bombs were dropped from only a few thousand feet, but in spite of this low altitude, the percentage of hits was not at all impressive. The second test that I witnessed was carried out with the IOWA, which was under way and radio controlled from the OHIO, from whose bridge I watched the tests. In this case we were able to maneuver the IOWA, although at very slow speed, so as to make her a more difficult target. On this occasion the IOWA received very little damage. Having been an aviation officer myself, I would be the last to minimize the achievements of the aviation arm of either the Army or Navy, but I think the record of the Navy in the Pacific in World War II demonstrates clearly enough that we did well not to scrap all our heavy ships and put all our eggs in the aviation basket.

RADIO REMINISCENES

A HALF CENTURY

CHAPTER IX - 1919-1923

ANACOSTIA - DEVELOPMENT IN NEW FIELDS

CHAPTER IX

1919-1923 - Anacostia - Development in New Fields

Due to the fact that the Navy Department used me as a general consultant in all fields of Navy radio, my group at Anacostia was drawn into a number of interesting investigations, several of which had no direct connection with aviation radio.

The Bureau of Engineering was interested in clearing up its status with respect to reception of continuous wave signals with the heterodyne. The heterodyne is a local oscillator tube in or near the receiver, which produces a continuously emitted wave of frequency differing somewhat from that of the incoming signal. This local wave and the incoming signal wave combine in the detector to produce a new oscillation, whose frequency may be either the sum or the difference of the two wave frequencies. Usually it is the difference frequency which is of interest. By suitable adjustment of the frequency of the local wave, this difference frequency may be brought to the range suitable for perception by the human ear, which means usually, to a frequency somewhere between 250 and 2000 cycles, corresponding roughly in pitch to a tone range from middle C on the piano to the third octave above middle C. This is a very effective means of receiving telegraphic continuous wave signals.

The Navy's patent situation in regard to the use of this device was not considered very satisfactory; on the other hand the Navy had, at that time, acquired patents covering the Poulsen Arc system of transmission, so Commander Hooper was attempting to negotiate with one of the large corporations interested in radio a trade which would be attractive to that corporation as well as to the Navy. He found that this company took the attitude that the Navy could not possibly get along without the use of the heterodyne and was inclined to value its rights to that device very highly. Commander Hooper then called me to the Navy Department and told me to get up a device which would receive long distance low frequency continuous wave telegraphic signals without the use of the heterodyne and without using either the Poulsen tikker or the complicated tone wheel device which had been developed by the Germans for Sayville. Commander Hooper stalled off negotiations for a month while I went to work, aided principally by L. C. Young.

Before the month was out we had built and tested on trans-atlantic signals, a receiving set which met the specifications. The incoming signal was passed through the usual tuning circuits and a suitable amplifier, but was not detected in the usual fashion. Following the receiver was a Wheatstone bridge which, instead of having resistances in the four arms of the bridge, had two of these resistances replaced by three element vacuum tubes. The receiving telephones, or other indicating instruments, were connected across the usual balance points of the bridge. Balance was obtained by adjusting the remaining two resistances of the bridge. The amplified signal was then delivered to the grid of one of the tubes in another arm of the bridge. Thus the changes in the effective resistance of this tube would upset the balance of the bridge.

The supply current to the bridge was obtained from a 1000 cycle electrically driven tuning fork so that, when the bridge was unbalanced by the signal, a pure 1000 cycle tone appeared in the telephones. This arrangement had to be pretty carefully shielded from stray electrical fields. We then sent a report to the Bureau of Engineering and Commander Hooper renewed his negotiations with representatives of the company telling them that the Navy was not dependent on the heterodyne for continuous wave signals, but had another method of receiving continuous waves. He refused to divulge the details of that method, but invited the company to send engineers to the Anacostia Station to see it in operation. When the engineers arrived they were very skeptical, because the received signals sounded very much like signals received by the heterodyne method. However, there is one way by which heterodyne signals can always be identified, namely, by varying the different controls on the circuit. When this is done, tone of the received signal is varied. I allowed the engineers to manipulate all the controls without being able to change the signal tone, so they convinced themselves that there was no heterodyne involved. This experiment saved the Navy a substantial sum of money.

As soon as Commander Hooper satisfactorily completed his negotiations, I dismantled this bizarre equipment, since it wasn't anywhere near as convenient for reception as was the heterodyne method.

During this same period the exhaustive studies of long wave propagation and static, carried out by Dr. L. W. Austin for the Navy, led him to speculate on the possibility of there being a falling off of static or atmospheric disturbance intensity if waves much longer, or much lower in frequency, than those commonly in use could be exploited. His investigations had extended to wavelengths of about 23-1/2 kilometers, a frequency of about 12.8 kilocycles.

I undertook to modify the high power arc equipment at Annapolis for extremely long waves, for this purpose collecting all the large heavy duty copper loading coils that I could find in the Washington area. The powerful Poulsen Arc at Annapolis, unfortunately, couldn't be run at low enough power to operate with stability on this conglomeration of loading coils and antennae, so I obtained another arc, of 5 or 10 kilowatts capacity, carrying out an experiment on 28,000 meters or 10.7 kilocycles, Bar Harbor being the receiving station, at a distance of about 700 miles. Bar Harbor, in spite of our having only 8 or 10 amperes in the transmitting antenna, received the signals successfully, establishing a record for extremely long wave communication. However, the expected reduction in atmospheric disturbances did not materialize. I then wound some iron core inductances and performed a similar experiment from Annapolis on 54,000 meters, a frequency of about 5-1/2 kilocycles. In this case the radiation efficiency of even the big Annapolis antenna was, at this frequency, so low that it was all we could do to pick up the signals in Washington and Bar Harbor couldn't hear them at all. Judging from what we learned from the Washington reception there seemed little likelihood of there being any particular advantage in trying to transmit with gigantic antennas on such low frequencies.

Shortly after my arrival at the Bureau of Standards, in 1919, I became acquainted with Dr. J. H. Dellinger, Head of the Radio Division of the Bureau of standards. Throughout the years we have had many friendly and interesting contacts which have given me a high opinion of Dr. Dellinger's scientific ability and his skill as an administrator. Between 1921 and 1922, the U.S.R.I. (Union Scientifique Radio Internationale) had been organized by General Ferrié, who was its first president with Dr. Goldschmidt of Belgium as his first secretary. Dr. L. W. Austin, of the Navy, was the first Chairman of the American Section and Dr. Dellinger, for many years secretary of the American Section, is at the present writing the Chairman. This organization concerns itself with scientific radio experiments which require international cooperation as, for instance, the studies of long range propagation phenomena which obviously have to be made in several countries simultaneously. The Director of Naval Communications and the Chief of the Army Signal Corps are ex-officio members of the Executive Committee, on which the principal American radio companies engaging in research are also represented. There are other well-known scientists on the executive committee who may or may not have connection with any particular laboratory or corporation.

I have been official Navy representative on the executive committee of the American Section ever since its formation. The American Section is formed under the auspices of the National Research Council. Through my work with this organization I have come to know many distinguished scientists, both in this country and in Europe. Naturally this organization suspended operations during World War II, but is now planning resumption of the old custom of one annual meeting held jointly with the I.R.E. The American Section owes a great deal to the hard work put in by Dr. Austin up to the time of his death, and by the continuing efforts over a long period of years of Dr. Dellinger. The work of Dr. Dellinger's group on ionosphere research is well known to all radio scientists.

During this period I became very well acquainted with the late Charles Francis Jenkins and, with the approval of the Navy Department, placed our Anacostia Station at his disposal for experiments with radio facsimile transmission. Jenkins had been an indefatigable experimenter and inventor ever since 1895, when at the age of 23 he decided to take up invention as a profession. Indeed he had been inventing even before that, because in 1892 he was producing moving pictures, and in 1893 had a machine which some considered to be a forerunner of the motion picture projector. As early as 1894 he had outlined a scheme for the electrical transmission of pictures, and in 1923, using our transmitter at Anacostia, he transmitted pictures of President Harding, by radio, from Washington to Philadelphia. Later on Jenkins turned to television, using a mechanical scanning system with a revolving disc whose rim contained a tapered lens equivalent to a long series of small lenses.

Jenkins told me that the only invention from which he made any real money was a paraffin cardboard container which he thought up in five minutes, and which financed his laboratory for a number of years. He was, I think, always principally interested in keeping that laboratory going and didn't care much for money for other purposes.

Jenkins had a radio station of his own at the time he contacted us, but it was of very low power and not too well modulated. I don't think I even did get him to realize the necessity of having very wide band modulation when it came to television, but he wasn't the only engineer in those days who had that opinion.

Two developments, important to the Navy, were started at this time in Anacostia. One was the so-called coupling tube multiple reception system. The other was a multiple transmission system. Even at this time the Navy was making such extensive use of radio at sea that there weren't enough antennae to go around. Furthermore, it was increasingly important that ships should be able to transmit on certain frequencies and receive on other frequencies on board the same ship, and at the same time.

Thanks to the development at the Washington Navy Yard of a small multistage radio frequency amplifier using the Western Electric "N" tubes, we were able to consider the reception of weak signals from an untuned Antenna. Mr. Young and I worked out a system permitting a large number of such receivers, provided with the new amplifiers, to operate from a single small antenna which could be put in the forward part of a large ship while transmitters in the after portion of the ship could be operated on antennae which were somewhat separated from the receiving antenna, thus reducing interference from the ship's own transmitters on her own receivers. This system I took to sea on the WYOMING, trying it out very successfully in the Caribbean and Pacific waters in the early part of 1923.

In parallel with the multiple reception development we made attempts to transmit several frequencies simultaneously from one antenna. In this case we didn't use an untuned antenna, which would have made the efficiency too low to be useful, but used the nodal point system suggested by the British Navy's acceptor-rejector system of reception. Three frequencies were transmitted simultaneously from one antenna at the Anacostia station, one from a small Poulsen Arc on rather low frequency, the other two from tube sets on two different frequencies. One of the transmissions was voice modulated. These were observed by various Naval stations and created considerable interest at the time.

In the meantime our people in Naval Communications were asking for more and more channels of communication, so we began the exploitation of frequencies higher than those commonly used by military and commercial services. It should be remembered that the theory of wave propagation accepted at that time indicated that as the frequency went from low to high, the absorption losses over the route from transmitter to receiver rapidly increased. This was the reason that the amateurs were assigned frequencies in the neighborhood of 1500 kilocycles. It was considered that frequencies in this range and higher wouldn't be useful for military or commercial communication. Actually an enormous number of tests which substantiated the theory had been carried out. What nobody realized at the time was that the existing theory was invalid for frequencies much higher than 1000 kilocycles. The reasons for this will be given in the following chapter.

In spite of the fact that theory indicated that we would get only moderate ranges on 1500 kilocycles or higher, Mr. Young and I thought that there were many instances where the Navy did not need long distance communication, but would be able to use communication of moderate range on channels entirely separated from, and not interfering with, longer wave communications.

The amateur fraternity was the principal organization which was actually operating on frequencies of 1500 kilocycles or higher. Therefore, we tuned up a transmitter at Anacostia to 1500 kilocycles and started contacting amateurs in various parts of the country and, with their assistance, studying wave propagation phenomena. Many an amateur still operating will remember the work he did with us when we operated under the call letters NSF and NOF, the NOF call being used in the latter part of the Anacostia period for all broadcasting and amateur communication.

We took part in the fading tests organized by the American Radio Relay League, Mr. Young devising a system of automatically repeating the test signals from a local amateur, 3XF, so that our signals went out simultaneously with those of 3 XF but on a sufficiently different frequency to prevent interference. Thus the relative fading effects on two nearby frequencies could be judged from the reports of near and distant amateur stations.

These early experiments in cooperation with the amateurs threw an interesting light on wave propagation effects in this little explored band. Many times extraordinarily long ranges were obtained with very limited power, especially at night in the winter. This close cooperation between the Navy and the amateurs endured for many years. In fact, it endured until the Navy had developed a sufficient number of high frequency stations on shore and shipboard to get adequate observations within its own service. The Navy owes the amateurs a great debt of gratitude for the hearty cooperation they gave in those days, when so little was known of frequencies beyond 1500 kilocycles.

In the midsummer of 1919 I was sent by the Navy Department to New Orleans to determine whether the installation of buried wires for receiving antennae would improve the very difficult circuit between the Naval Station, which was across the river from New Orleans, and the Canal Zone. Since this was a long range circuit, low frequencies were used. Transmission by Poulsen Arc worked very well in the winter, but in the summer the atmospheric disturbances rose to a terrifically high level, particularly during the usual afternoon thunder storms, and communication was completely knocked out. New Orleans was able to receive the high power arc at Balboa only fairly well, but the 30 kilowatt arc at New Orleans was totally inadequate for reception at Balboa. I can't say that the ground wire experiments indicated any great gain; in fact I did not recommend that they be installed. On the contrary, I recommended that the proper thing to do was to put in a high power arc at New Orleans. While there I conducted, with buried wires, certain other experiments on frequencies between 250 and 500 kilocycles in connection with the circuit from New Orleans to Pensacola. In this case we got considerable improvement, due expecially to the fact that during the afternoon storms we were not obliged to ground the antenna and stop reception. Thus we were often able to work with ships easier than could Pensacola, even when the ships were closer to Pensacola than to us.

At approximately this same period I made a trip with the scouting fleet, being attached to the Battleship MISSISSIPPI, Captain Moffet commanding. By this time the ships had a number of tube sets. One of my principal duties was to observe the action of these sets under the shock of gun fire and to make notes on the proper design of such sets in order to resist these shocks. We did a lot of shooting with guns of all calibers, some of which I witnessed from the main top of the MISSISSIPPI. In those days the ships had the old style high cage masts. Our shock tests, curiously enough, showed that the worst shock came from the three inch anti-aircraft guns firing upward at a high angle. This gave a quick sharp shock, straight down through the decks, and jarred the transmitters worse than a salvo of fourteen inch guns, which simply seemed to push the ship bodily over to one side.

During the trip I wound a small radio direction finder loop and hung it on an improvised yardarm close to the main top. With this I was able to take approximate bearings on the airplanes that were cooperating with the Fleet. We were always worried in those days about the planes getting into trouble and coming down without our knowing where they were, making rescue difficult. They were not yet all provided with emergency transmitting equipment.

In midsummer of 1920 I spent a month on the old battleship OHIO. Lieutenant Mineratti was the Executive Officer, First Lieutenant and Radio Officer combined. We had trouble getting the Navy Department to allow us a sufficient crew to operate, so the ship was manned very largely with Philipino mess boys who, under Mineratti's direction, really did amazingly well. Mr. Young was with me on this trip. Our main interest was in long wave direction finder variations. We had equipped the OHIO with a dirigible loop suitable for long wave reception and found that, if we disregarded all except bearings from the intermediate range between 100 and 300 miles, we could navigate very well with bearings on high power long wave stations. By the time a ship would get into the zone where variations in bearings were likely at night, it would not be far from the region where the coastal compass stations on higher frequencies could give a position. Moreover, we found that the nocturnal variations, such as I described in the preceding chapter, were not nearly as bad over sea as over land. This is due to the fact that the ground wave, or part of the wave that has propagated itself nearest to the earth or water, was far less attenuated over salt water than over land. This had been expected since the days of Austin's measurements in connection with the Brant Rock station more than a decade earlier. Captain Halligan stated that he would be able to lock up his magnetic compasses and take the OHIO across the Atlantic on radio bearings alone. On this cruise I had the pleasure of occupying the Chief of Staff's cabin, which had been used by Alice Roosevelt when the OHIO visited Japan on the round the world cruise of 1909. One ancient warrant officer on board had made that trip and remembered many interesting incidents in connection with it.

In connection with our search for useful new radio frequency channels for the Navy, Mr. Young and I pushed our experiments at Anacostia to frequencies of 60 megacycles (60,000 kilocycles) and even a little higher. Considering the design of the tubes with which we had to operate, this was a difficult achievement. Certainly our circuits were not regarded as at all orthodox by engineers accustomed to working on lower frequencies. Dr. Edwin Armstrong's famous paper on what later came to be called the superheterodyne appeared in the February 1921 proceedings of the Institute of Radio Engineers. By this time we had radio frequency amplifiers, but none that were very good on frequencies higher than 1000 kilocycles. The Armstrong device provided for a transposition of frequencies to a region where they could be amplified and therefore gave us a wonderful tool for developing receivers for the higher band. It should be noted in passing that this work of Armstrong was started for the Signal Corps when he was in France in 1918. Young built our first superheterodyne shortly after Armstrong's paper came out.

By 1922 we had a 50-watt tube working as a transmitter on 60 megacycles, and a suitable receiver to go with it. It was during field tests of this equipment that the idea of Radar had its humble beginning. Working across an arm of the Potomac, we noticed that, due to the signals being reflected by passing vessels, we could detect their presence and approximate location. In September of 1922 I wrote a letter to the Bureau of Engineering requesting that I be allowed to put certain equipment on a number of destroyers, take them to sea at night and prove that hypothetical enemy vessels, represented by other destroyers, could not filter in between separate destroyer groups at night without detection. I pointed out that the device should work in a smoke screen, fog, or under cover of darkness. I am sorry to say that nothing was done about this at that time. We called it then "the detection of moving objects by radio" and a little later "the detection of enemy ships and aircraft". It should be noted that this early system only functioned if there was relative motion between observer and target. A fixed object, that is, fixed with respect to the observer, could not be detected. Therein it differs from modern pulse radar, development of which started later.

As pointed out earlier, our experiments with voice communication with planes had given us a keen interest of radio telephony. Since 1919 we had had fairly good radio telephone transmitters at Anacostia. It was natural that, to find out how far our transmission was stepping out, we should find it convenient to use the amateur stations with whom we were already in liaison.

Early in the Anacostia period we were asked by a Washington chess group if we could help them conduct a game of chess between one of their experts and a member of a chess group in Chicago. We contacted Matthews at 9XN in Chicago. (Matthews has been mentioned earlier as having been with us at Great Lakes and was known as an enthusiastic amateur.) The Washington man named his moves over his own telephone and we plugged them into our voice transmitter thus putting him on the air. The game went off successfully. This may have been the first time the game of chess was played with the aid of radio telephony.

In order to facilitate our field tests at a short distance from the station and observe alterations of our signals when we made certain changes, Mr. Young equipped his automobile with a very small receiver using three Western Electric "N" tubes. This receiver was adjustable over what we now know as the broadcast band. The filaments of the tubes were connected to the automobile battery. Plate supply was provided by a light portable dry battery. No loud speaker was available, but the set was provided with two pairs of head telephones, so the driver and one passenger could hear the signals. This was certainly a very early auto radio.

Besides the amateurs, our experiments were frequently observed by other stations having experimental licenses. More than once we worked two ways with Frank Conrad's station, 8 XK Wilkinsburg, Pa. As everyone knows, this station soon grew into the famous Westinghouse pioneer Station KDKA, which was licensed by the Department of Commerce in October of 1920.

We very soon found from our amateur reports that we had a wide circle of listeners whenever we operated at night. In order to increase this group and their interest in the work, we started broadcasting music in 1920. At first we operated with my old Columbia phonograph and a few very old records. Within a year or so we had fan mail from some twenty eight states and a number of people sent in new disc recordings because they got tired of hearing the same old records on every transmission. In particular, it is recalled that Senator France of Maryland, sent in a record "Maryland, my Maryland" requesting that it be played on a certain evening when he was holding a gathering at his home. Later on, the Washington Radio Group of which Young was a member, presented him on Christmas in 1921, with a phonograph with one hundred records, provided he would use it at the Anacostia Station.

Our station was operated entirely by volunteers who received no pay for their night work. Since we had a heavy daylight program on aircraft work, previously described, we didn't feel we could put in more than two evenings a week on such work. We fitted up a crude studio, using canvas drapes to cut down the reverberation time. Our first broadcast of music, other than phonograph music, was arranged by Miss Bird Mock. A piano was moved in and a program, consisting of piano solos, singing with piano accompaniment, and piano and violin music, was put on the air. Later on we repeatedly broadcast Marine Band Concerts, involving as many of the Band as we could get into our small studio.

Miss Mae Cross (now Mrs. Pope, Assistant Head of Field Service Branch, Office of Naval Research) participated in one of these concerts in 1922, singing a song entitled "Spirit Flower".

During this same period, we were approached by the Public Health Service. With the permission of the Navy Department, we began to broadcast public health lectures twice a week. Surgeon General Cummings, as I recall, made the first broadcast, but most of them were made by Mr. Heath. One of these broadcasts was on venereal diseases which, in those days, were not mentioned in the newspapers or talked about over the radio. Secretary Denby, who was visiting friends in Chevy Chase, heard this broadcast and was indignant with us for letting such material to go out over a Navy Station. We evaded censure by stating that, under the orders of the Navy Department, we had merely put the facilities of the station at the disposal of the Public Health Service and had no control over the subject matter of the lecture.

An amusing incident occurred in connection with one of our broadcasts. A Press Club group called up and asked if we could put on a broadcast which they might receive while President Harding was their guest. Neither Mr. Young, Mr. Gebhard or myself were at the station at the time this request came in, but several of the radio men who had been working with us were on the station, so they attempted to put it in operation. They finally succeeded, but not before they had done a lot of pretty tall swearing while they were trying to locate and close the various switches necessary of its operation. Unfortunately, a lot of this language came thru to the listeners of the Press Group, but it was treated as a good joke and we were not reprimanded.

This station was the first to put the voice of a President on the air, or for that matter, the voice of a Chief Justice, Senator or Representative. President Harding and Chief Justice Taft were both put on the air, thanks to the cooperation of the Chesapeake & Potomac Telephone Company, on the 30th of May 1922 during the dedication of the Lincoln Memorial. Mr. Lodge, of Massachusetts, was the first senator to broadcast. He gave a talk to a group in his home town Nahant, Massachusetts, speaking from his home in Washington. It was received fairly well but, unfortunately, a good many other people besides the people in Nahant heard the talk. The seantor was quite indignant that it was not restricted to his own town. Albert Beveridge, ex-senator from Indiana, also spoke over this station. Representative John L. Cable, of Ohio, spoke on the 10th of February 1922. There were others, but I am not able to recall them now.

President Harding requested that a receiving set be installed at the White House. This set with a suitable loud speaker was made up at the Navy Yard, Washington. I had the pleasure of supervising the installation in the President's office.

The Anacostia Station was the first to broadcast from the House of Representatives. This again was done through the cooperation of Mr. Creasy of the Chesapeake & Potomac Telephone Company, who arranged for a special set of high fidelity telephone lines.

In the year of 1920 I conceived the idea of picking up distant stations and rebroadcasting them on our transmitter. We called it "re-radiation" in those days. The first rebroadcasting of European signals was done in November of 1920 and was picked up as far West as Chicago. The system we devised covered also the broadcasting, or re-broadcasting, of voice or signals coming in over telephone wires. Unknown to me, Colpitts of the Western Electric Company was working on the same thing. Since we both took out patent applications, this resulted in a long conflict in interference which was finally settled, correctly I think, in favor of Colpitts. I believe the system I disclosed was a little better than the one he disclosed, but there is no doubt in my mind that he conceived the idea several months earlier than I did. Therefore he was entitled to the patent. Neither knew that the other was interested in the subject.

The last historically important broadcast from Anacostia occurred during the early part of December 1922, when for the first time in history a President's message to Congress was put on the radio. The broadcast was sent out on a multiple tuned antenna, with an input of about 1 kilowatt, on a frequency of approximately 700 kilocycles. From this time so many special programs were requested that work was beginning to interfere with research. The Navy Department therefore proceeded to install a suitable transmitter at Arlington. Thereafter all broadcasting, either in the public interest or specifically in the interest of the Navy, was carried on from Arlington, the first broadcast from there being approximately January 3, 1923. The Anacostia Station continued contact with amateurs, but the station was thereafter used for the advancement of research and development only.

In 1921 I was appointed technical advisor to the Navy delegation to the Inter-Allied Post War Radio Conference, held in Washington, with civilian, military and Naval representatives from England, France, Italy, Japan and the United States. It was in connection with this conference that I first met General Ferrié, Chief of the Franch Signal Corps, who had had a long and brilliant technical and military career. He was easily the outstanding man among the foreign delegates. I had many contacts with General Ferrié at this time which formed a basis for long continuing friendship. The General understood English well enough, but greatly disliked speaking it. I had the same attitude toward French, so our conversations were usually carried on by the use of French on his part and English on mine.

The principal commercial radio organizations of this country specifically interested in the problems of international communications were, at that time, the Bell Telephone System, the General Electric Company and the Radio Corporation of America. These interests felt that the foreign delegates to the conference should be shown something of the commercial radio picture, since no international conference on radio can decide on the allocation of frequencies for military use without due cognizance of the commercial frequencies in use all over the world. All long range communications must be regulated by international agreement or chaotic interference and inferior communications will result. The American companies pooled their interests and provided a special junket for the leading delegates. To do this they chartered two or three Pullman cars which started from Washington, took in the Bell Telephone and RCA activities in the New York area, and then proceeded to Albany, where transportation by automobile was furnished to Schenectady in order to visit the General Electric plant.

The giving out of the highly ornate invitations to this affair was in the hands of a local business agent of one of the big corporations. He issued invitations to almost all of the foreign delegates and to the ranking officers of our Army and Navy who were interested in communication. Neither Dr. Austin nor I received an invitation, which didn't particularly surprise us, as we well knew how these things were generally done. Nevertheless we both went on this trip. It came about in the following manner. The committee organizing the trip were particularly insistent that General Ferrié should make this trip, since he was one of the dominating figures of the whole conference, a man of remarkable keenness of mind, complete integrity and devotion to duty, and of very distinguished appearance. When the General received his invitation he sent back a note to the Committee, asking to see the list of guests before he decided whether he wanted to go. Thereafter nothing was heard from him up to the day on which the departure was scheduled. The Committee then sent representatives to call on the General, to find out why he had not accepted the invitation.

The General replied that no doubt it was a distinguished group, but that he came to this country because of his interest in radio communication. Two of the people to whom he most earnestly desired to talk he didn't find on the list of guests, namely Dr. Austin and Commander Taylor. He wasn't particularly interested in talking things over with the military men, who were in administrative control but not themselves doing research and development work. The Committee then promised the General that Austin and I would be there. So they called us over the telephone that afternoon. Having made other arrangements for the week-end, I told them I was sorry, but had other plans. They then send a delegation out to see me, arriving about an hour before the special train started. In the meantime, Dr. Austin had called me stating that he thought we ought to go out of courtesy to the General. Of course I agreed with him.

I have never had the pleasure to taking such a beautifully managed trip, or one where the guest had so little to think about. We would walk out of our car, say at New York, get into a taxi, arrive at a fine hotel and find our overcoats, brief cases and any other luggage miraculously there in our rooms ahead of us. I never found out how this was managed. In New York the delegation was put into a long line of cars. General Ferrié grasped my arm and insisted that I ride with him at the head of the procession. We had 30 motorcycle policemen clearing the streets ahead of us, stopping all traffic and whizzing us through the heart of the city at 30 miles per hour, with thousands of people on the sidewalk wondering who all these important people might be. I have never had connection with any such "pomp and circumstance" either before or since.

In the latter part of May 1922, I was sent to the Naval Hospital, then in the Georgetown District, and after experiencing two major operations during the week, I remained there for the next three months. During this period, namely on the first of July 1922, I was put on the inactive, but not retired, reserve list. The next day I was appointed in a civilian capacity, with duty still at Anacostia, as Expert Radio Aide.

Two amusing incidents occurred in connection with this appointment. Commander Hooper was anxious that I should continue work at the same pay that I had been receiving as an officer, and so recommended to the Navy Department. In those days these matters were all referred to the Assistant Secretary, who at that time was Theodore Roosevelt, Jr. (later General Roosevelt, who died in France during the late war). When Roosevelt saw the figure set for my salary he said, "This will never do, it is practically as much as I get myself", so they knocked $500.00 off the salary and the Assistant Secretary was satisfied. It should not be inferred that I held this against him as I regarded him highly, but the incident is quite typical of the official attitude of those days. Almost any appointed executive in a department would have had the same point of view toward a Civil Service employee.

The second incident occurred in connection with the Civil Service Commission. When the Commission was asked to give me a suitable examination, they replied that they had no one on their staff capable of writing up a set of questions for so important a position. They asked the Bureau of Engineering to write out the questions for them. The Bureau of Engineering felt they didn't have anybody suitable to write the questions either, so Lieutenant Commander Kaufman (later Admiral Kaufman) of the Bureau of Engineering asked me to write up the questions for my own examination. I therefore prepared a list of about thirty questions, forwarded this list to the Bureau of Engineering, who in turn sent them with a forwarding letter to the Civil Service Commission. The Commission then forwarded a copy of the questions to me, with a request that I answer them by letter. I did this, but was only able to make a grade of 89% on my own questions. I never found out who marked the answers. Since there were no other applicants, I was number one, and received the appointment.

It was late in the fall of 1922 before my physical condition permitted very serious work. By that time Young and I were working rapidly on the so-called coupling tube system, permitting a large number of receivers to be used on shipborad connected to one antenna. I went to sea with this system, as previously mentioned, during the first three months of 1923. This system was long used in the Navy and a modified form is still used today.

One other activity carried on in this period deserves mention as evidence of our continued interest in radio control projects. This led directly to radio controlled target ships, the IOWA being the first thus equipped and the first to be sunk (Panama Bay, 1923). A target destroyer was next equipped. In 1922 Mr. C. B. Mirick started work on pilotless target planes, known as "drones". To those who know anything about honey bees, the significance of the term will be clear. The drone has one happy flight and then dies. I believe I am responsible for this name for pilotless target planes.

Before concluding this chapter it may be interesting to give some details of the earlier mentioned cruise in the Caribbean and Pacific Waters.

The WYOMING was the flagship of what was then called the Scouting Fleet. Admiral John MacDonald commanded the Fleet and Captain Laws commanded the battleship WYOMING; Lieutenant Ruble (now Captain Ruble, Retired) was Scouting Fleet Radio Officer.

In addition to the multiple reception system worked out by Mr. Young and myself, the WYOMING had just installed, at the New York Navy Yard, the first vacuum tube set ever put in the Fleet whose power was equal to or better than that of the old 5 kilowatt spark set. It covered approximately the same frequency band as the older set, namely from about 175 kilocycles to 700 kilocycles. This transmitter had two Western Electric water cooled tubes supplied with a maximum of 8000 volts alternating current from a suitable transformer. The motor generator previously used on the spark set supplied the 500 cycle current. In order to stabilize the radio frequency, the oscillating circuit of the transmitter was coupled loosely to an independently tuned antenna circuit. The transmitter was not suitable for voice transmission, as the output was at all times completely modulated by the 500 cycle supply, and the signals could be recieved, without the use of heterodyne or autodyne reception, by any of the receivers with which the fleet was equipped, provided they covered the frequency bank. This set would be regarded as a crude affair nowadays, but it was a big advance over the old spark set, creating less interference on other nearby radio frequency channels of communication, and permitting very sharp tuning and loose coupling of receivers, so that atmospheric disturbances were not quite so troublesome.

On the way down to Guantanamo, we were involved in war games. Our little fleet was allowed 48 hours start, after which a group of destroyers, starting from Newport, was supposed to locate us and simulate an attack with torpedoes. Of course the destroyer outfit knew that we would ultimately go to Guantanamo Bay on the south side of Cuba, because that was the rendezvous, but they didn't know whether we would hug the coast, passing close to Hatteras, or swing widely out to sea, or go straight down past Salvador and around the eastern end of Cuba. As a matter of fact, we did go down by practically the shortest route which could be followed by ships leaving Norfolk. I believe we stopped at Hampton Roads for a day or two before the war game started. The destroyers made a wide sweep to the eastward and then tack towards the coast coming down past Cape Hatteras, missing us by about 100 miles. We were allowed a standard speed of 12 knots, which was about two-thirds of the speed of a battleship for those days, and the destroyers were allowed a maximum speed of 25 knots.

An interesting incident occurred in connection with these maneuvers. The WYOMING had a radio direction finder shack on top-side and I had been amusing myself during the trip down by taking bearings on various shore stations and commercial shipping. At the time the destroyers were off Hatteras, I heard a Navy supply ship open up and ask "What destroyers are those I see on my starboard bow"? Of course he got no reply from the destroyers, because both sides were operating under the rule of radio silence during this part of the game. Very likely the destroyers would have liked to blow the offending ship of the train out of the water. I promptly got a bearing of the ship which asked the question and reported the matter to the Admiral, who then knew he was perfectly safe in continuing straight down towards the eastern end of Cuba. We were pretty near our goal and would have made it without doubt before the destroyers discovered us. But about this time a submarine attached to our force broke a shaft and had to open up her radio to ask for assistance. The Admiral was then forced to break radio silence and order two destroyers to give the submarine help. This, of course, disclosed everybody's position. The two groups then joined up in Guantanamo for a number of weeks, going outside into the Caribbean only for torpedo practice. Even the battleships carried torpedoes in those days.

Before the Scouting Fleet started for Panama, I transferred to the destroyer BROOKS, Lieutenant Commander O. M. Reed (later Admiral Reed) commanding. I had with me one of the new amplifiers which the Radio Test Shop had been largely responsible for developing. This contained four stages of radio amplification and two of audio. We had used amplifiers of this type, which were known as Universal amplifiers, on our multiple reception installation. It was a spare amplifier that I took to the BROOKS and connected up to the ship's radio direction finder, which was then mounted aft, on top of the torpedo repair station.

There was a Chief Petty Officer on board the BROOKS who was very much a live wire and keen to get some experience with this amplifier. He was soon busy taking bearings over distances which were so much greater than any he had been able to reach before that he was very enthusiastic about the setup, spending most of his time, day and night, with the direction finder. At three o'clock one bright sunny afternoon in Frebruary we started out into the Caribbean for the trip to Panama, with a standard speed of 25 knots. Within a half hour nearly everyone on the ship was seasick. There wasn't any storm, but under the influence of the northeast trade winds the Caribbean is frequently pretty rough, and the BROOKS was an old, small destroyer. There were three other destroyers in the company. Fortunately, I am seldom affected by seasickness, so I was able to carry on my experiments as well as I could with the roll indicator on the ship indicating 30° roll to port and 35° to starboard all the way across the Caribbean. We were getting the seas on the port after quarter, and quite frequently green water came over the decks amidship. The radioman on watch on the main radio under the bridge was completely overcome by seasickness, and the chief, who was an old timer, and quite immune, stuck by his radio compass back aft.

I took the watch myself that night, as the radiomen who went on duty promptly collapsed. This was good practical experience which I quite enjoyed. It was a beautiful moonlight night, and since I couldn't sleep anyway with such violent motion going on, I spent the night either on the bridge or in the radio room.

Early in the morning we split off from the other three destroyers and struck down toward Porto Bello. It seems that a tramp steamer with disabled steering gear had drifted in on this rocky coast, where the mountains come almost down to the sea, and in sheer desperation had dropped both its anchors. By good luck they caught and held on a small pinnacle on which he reported subsequent soundings of eighteen fathoms of water. This had been reported to the Navy Hydrographic Office and we had been requested to locate this small ridge, which had never been marked on earlier charts. We arrived off the coast just before dawn. After daylight we made three ten-mile sweeps, parallel with the coast, at distances of approximately five miles. On the third third sweep we picked up, with the sound gear, this small ridge which wasn't much over a quarter of a mile long, and checked its depth to be eighteen fathoms as reported. This was the first practical demonstration I had seen of the operation of the sonic depth finder, which does echo ranging under water by timing the echoes sent out by an underwater sound projector. Incidentally, that tramp steamer certainly had luck, because there was no other ridge in this whose area that could have held her anchors, and this one was of very small dimensions indeed.

We caught up with the other destroyers in time to go through the Panama Canal with them, but we didn't get to our anchorage on the south side of the Canal, in Panama Bay, until midnight. In the meantime we had become so short of fresh water (a characteristic weakness of this particular type of destroyer) that we didn't have enough that morning to wash our faces. In passing through Gatun Lake, the squadron leader made the standard speed 21 knots. The BROOKS put down a scoop and gathered 8,000 gallons of water out of Gatun Lake. This was the only thing we could do to keep up with the procession.

After a breathing spell in Panama Bay during which I visited a large number of ships and the shore radio installations at Balboa, we took part in another war game. The battle fleet, coming down from San Pedro, California, was supposed to make an attack and simulate a landing which would menace the Panama Canal. The scouting fleet was supposed to locate this attempt in time to block it. For five days these two fleets maneuvered around in the Pacific, running at night entirely without lights without contacting each other. This may seem strange to the land lubber, but not to the sea-going man who appreciates the vastness of the ocean's spaces.

During this period I was on board the battleship UTAH, but couldn't do much radio work on account of the prevailing radio silence. I did find out that one corner of her after radio room had a temperature of 130° in the tropics, being located directly behind the boiler room bulkhead and right next to the double bottoms. The radiomen couldn't long remain alert under such circumstances.

The Scouting Fleet was on the losing side of this war game because of failure of radio communications. This failure came about in an interesting way. Our Fleet Radio Officer, Lieutenant Ruble, was not allowed to have any information as to the exact disposition and missions of our subordinate forces. Since the radio equipment on different types of ships varied greatly, it was impossible to make an intelligent plan of communication.

We had a considerable number of F5L flying boats to use for reconnaissance work. The LANGLEY acted a mother ship for this group. Incidentally, I got quite a thrill when I flew over the Panama Canal in twenty minutes on board one of these planes, reflecting on the story of Balboa's forty day march across this Isthmus to be the first white man to see the Pacific. These planes were equipped with a 1/2 kilowatt spark set which could be counted on for a 150 mile range. One of these planes discovered the attacking Fleet in plenty of time, but the only ship near enough to him for communication purposes was a ship of our train, not equipped for receiving aircraft frequencies, not supplied with a transmitter adequate for replying to the aircraft, and totally unable to relay his information 400 miles to the main body of the Fleet.

Admiral Hilary Jones was Commander in Chief at that time, the MARYLAND carrying his flag. Lieutenant "Tam" Craven was the United States Fleet Radio Officer. Lieutenant Arps, on the CALIFORNIA, was Radio Officer for the Battle Fleet.

During the post-war conference on board the MARYLAND Admiral MacDonald, who had refused to take our advice to so dispose his ships that a communication network could be successfully set up, stated frankly that he now realized that he had lost the battle because of his failure to grasp the importance of the radio communications. The reports that went in on this war game stimulated the Navy Department to make better and more complete radio communication installations in the future.

The crowning incident of this cruise was the sinking in 600 fathoms of water, of the old battleship IOWA, operating under radio control in Panama Bay, by gun fire from the MISSISSIPPI. The IOWA was an old coal burner, but had been given an oil burning installation for this mission because it was so much easier to control by remote signals. The IOWA could be run at not over 10 knots, usually somewhat less, and could be maneuvered and stopped by radio, with no personnel on board. In the first experiments, special five inch shells were used which were designed to prevent severe damage and deep penetration, as it was not desirable to sink the ship before a large number of tests had been carried out. However, the damage was so severe that the emergency crews were up all night putting collision mattresses over the side, plugging holes and pumping water out of her. The final sinking was accomplished by much longer range firing from five of the MISSISSIPPI's fourteen inch guns. For this operation the seaplanes acted as spotters. I was on the CALIFORNIA, flagship for the battle fleet stationed about 1000 yards away from the IOWA. The first salvo went just over the IOWA and the plane reported "300 yards up". The next salvo was about 150 yards short, some of the shells ricochetting and doing considerable damage; planes reported 100 yards down. The third salvo crashed into her with a very spectacular effect; it seemed as though bits of iron and other scrap rained into the water for as much as a minute. Some of them fell close to the CALIFORNIA. Twenty minutes after firing commended, the IOWA was on the bottom of Panama Bay.

I came back to New York on the MARYLAND, where I learned a great many things about the complicated installation of flagship radio. I arrived in Washington on the 4th of April to find that my people at the Naval Air Station had been warned of the approaching move to the Naval Research Laboratory, and were disconnecting and boxing up equipment. I was detached from the Naval Air Station and ordered to the Naval Research Laboratory.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER X - 1923-1926

THE BIRTH OF HIGH FREQUENCY COMMUNICATIONS - NRL

<u>CHAPTER X</u>

1923-1926 - The Birth of High Frequency Communications -

Naval Research Laboratory

In 1923 the Bureau of Engineering had three laboratories in the District of Columbia engaged in radio research and development. Named in the order of the date of establishment they were: first, the Radio Test Shop at the Washington Navy Yard under the direction of Radio Gunner William A. Eaton, who served during world War II, on duty at the Bureau of Ships, as Commander; second, the Radio Research Laboratory located at the Bureau of Standards, but directed by Dr. L. W. Austin for the Navy; third, the Aircraft Radio Laboratory at the Naval Air Station, Anacostia under my direction.

The Laboratory under Dr. Austin was largely interested in long wave propagation phenomena and in precision measurements at radio frequencies. Dr. J. M. Miller, attached to that laboratory, was interested not only in the field of precision measurements but in vacuum tube theory and measurements, in which field he became an authority. The work in the Aircraft Radio Laboratory has been described in the preceding chapters. The work at the Radio Test Shop consisted principally of the problems which came up in connection with maintenance, repair and operation of Naval stations afloat and a shore but included a considerable amount of research and advanced development. Various studies carried out in connection with the Poulsen Arc, the development of early radio frequency amplifiers suitable for Naval Service, and early work on the development of radio receivers deserve especial mention.

In addition to these laboratories the Bureau of Engineering maintained radio forces, partly civilian and partly military at most of the Navy Yards. Their principal work was installation, maintenance and alterations of equipment in accordance with Bureau plans. Nevertheless some of these yards, particularly the Mare Island Yard under the leadership of Mr. Rice, did some research and a considerable amount of development which was very well worth while.

For some time Commander Hooper (now Rear Admiral, Retired) of the Radio Division, Bureau of Steam Engineering, and Commander A. J. Hepburn, (now Vice-Admiral Retired) both of whom had had a life long interest in radio, felt that the radio research and development should be concentrated in one laboratory under one leadership. Admiral J. K. Robinson, who was at this time Chief of the Bureau of Engineering, did not agree with Commander Hooper, thinking that the Naval Experimental Station at Annapolis was about all the Navy needed in that line. He changed his mind within two years and once confided to me that, while he had originally opposed the establishment of the Naval Research Laboratory, he had come to see that it was destined to become a most valuable adjunct to the Navy.

On the 7th of July 1915, Secretary of the Navy Daniels wrote to Mr. Thomas A. Edison, stating that one of the most important needs of the Navy was for machinery and facilities for utilizing the natural inventive genius of Americans to meet new conditions of warfare; that the Secretary intended to establish a department of invention and development to which all ideas and suggestions from either service or civilian inventors could be referred for determination as to whether they contained practical suggestions for the Navy to take up and perfect. The Navy, he stated, had no present means of handling inventions received from the public, except by sending them to the various bureaus of the Navy, which were overcrowded with routine work and could not always give them the attention they deserved. The Secretary felt that Naval officers on sea duty were in a position to note improvements, but that they had neither the time, space, ability nor, in many cases, the natural inventive mind needed to put new ideas into definite shape. The Secretary had in mind a general plan of organizing a department for the Navy which met with the ideas of Mr. Edison, as set forth in an interview by Mr. Edward Marshall and published in the New York Times. He therefore asked Mr. Edison if he would be willing, as a service to his country, to act as chairman of such a Board.

On the 13th day of July 1915 Dr. M. R. Hutchison, personal representative of Mr. Edison, visited the Secretary in Washington, advising him that Mr. Edison had consented to head such a Board. The Secretary and his Aide afterward visited Mr. Edison at Orange, N. J. and discussed the salient features of the Board. The Secretary then wrote to the presidents of the eleven largest engineering societies of the United States, asking them to nominate two members each, to serve on this "Naval Advisory Board", a title which was afterward changed to "Naval Consulting Board of the United States". The original members of the Naval Consulting Board were: Mr. Thomas A. Edison and Dr. M. R. Hutchison, selected by the Secretary; Dr. L. H. Baekeland and Dr. W. Whitney, selected by the American Chemical Society; Mr. Frank J. Sprague and Mr. B. G. Lamme, selected by the American Institute of Electrical Engineers; Mr. R. S. Woodward and Dr. Arthur G. Webster, selected by American Mathematical Society; Mr. A. M. Hunt and Mr. Alfred Craven, selected by the American Society of Civil Engineers; Mr. B. M. Sellers and Mr. Hudson Maxim, selected by the American Aeronautic Society; Mr. Thomas Robins and Mr. Peter Cooper Hewitt, selected by the Inventor's Guild; Mr. Howard E. Coffin and Mr. Andrew L. Riker, selected by the American Society of Automotive Engineers; Mr. Wm. L. Saunders and Mr. Benjamin B. Thayer, selected by the American Institute of Mining Engineers; Mr. Lawrence Addicks and Professor Joseph W. Richards, selected by the American Electro-Chemical Society; Mr. W. L. R. Emmett and Mr. Spencer Miller, selected by the American Society of Mechanical Engineers; Mr. Elmer A. Sperry and Mr. Henry A. Wise Wood, selected by the American Society of Aeronautic Engineers. Mr. D. W. Brunton, Chairman of the War Committee of Technical Societies, was appointed to the Board, after its formation, by the Secretary of the Navy. I am indebted for this information on the History of the Board and its original organization to a book published by the Government Printing Office in 1920, entitled "Naval Consulting Board of the United States", written by Lloyd N. Scott, formerly captain, U.S.A. and liaison officer to the Naval Consulting Board and War Committee of Technical Societies.

It is further stated in this reference that at the organization meeting of the Board on the 7th of October, 1915, the members realized the necessity for the construction of a Naval laboratory in order to get the best results from the work which they proposed to do along lines of science and invention, it being realized that the Navy Yards and their facilities were fully occupied with the active work of construction and maintenance of the Fleet as their primary function. A committee was formed, consisting of Edison, Baekeland, Whitney, Woodward and Coffin, to make a study of the subject of a Naval laboratory.

Nearly all of the numerous recommendations made by this committee were ultimately carried out, except the first one, which was that the laboratory should be located on tidewater of sufficient depth to permit a dreadnaught to come to the dock. The majority of the committee was in favor of establishing the Laboratory at Annapolis, but Edison made a minority report in favor of Sandy Hook. The present site on the old Bellevue Magazine Grounds on the Potomac, at the south end of the District of Columbia, was considered as a possible compromise. Edison was apparently somewhat displeased that his suggestions of locating the laboratory at Sandy Hook was not finally adopted. I am sorry to say he never visited the Laboratory, although his son, Charles Edison, when Secretary of the Navy, visited the place a number of times and was much interested in it. Many other members of the Board also visited the Laboratory from time to time, particularly Baekeland, Saunders, Robins, Whitney and Maxim. Subsequent administrations made little or no use of the Naval Consulting Board and it was finally abolished. It is the opinion of many of the old timers at the Laboratory that had the Board been allowed to take a more active interest in the Laboratory during its very early history, and its advice followed, the growth of the Laboratory would have been greatly accelerated. The Laboratory definitely owes its existence to the work of the Board, particularly to its Chairman, Thomas A. Edison, who, even as early as 1910, had recognized the necessity of a research organization within the Navy. Congress appropriated the money for its establishment in 1916.

Due to the unusual situations arising from World War I, the Laboratory had not been completed. The project was revived after the War and finally money became available to go ahead with it. I regret to say that the Naval Service as a whole was not very keen about this matter. Had it not been for the devoted work of Admiral William Strother Smith, Commander Hooper and some others, it perhaps would never have been started, or perhaps never occupied after it was finished. Admiral Smith was appointed the first Director but retired before the Laboratory was actually functioning. The Laboratory was sufficiently completed to start operations in 1923 under the leadership of Captain E. L. Bennet.

Commander Hooper determined to unite the research and development work done in the three Washington radio laboratories at the Naval Research Laboratory as the Radio Division, under my direction as superintendent, with Dr. J. M. Miller as Assistant Superintendent. Dr. L. W. Austin, even then not in robust health, had resigned, to the great regret of all in the Navy who knew him. Dr. Austin formed a special laboratory under the Bureau of Standards, for continuing the studies on long waves, and spent a large part of his time in the affairs of the International Scientific Radio Union. Thus he avoided a long drive through the heart of the city to the Naval Research Laboratory.

In the month of May 1923 the Radio Division started work. The Sound Division arrived about the same time under the leadership of Dr. Harvey C. Hayes as superintendent, aided by O. E. Dudley (now Commander U.S.N.R.F.), F. W.

Struthers and E. E. Brock. It had been located at Annapolis within the Naval Experimental Station. Most of Dr. Austin's laboratory and personnel were moved down and merged with my personnel from the Naval Aircraft Laboratory at Anacostia. In September of the same year, the research and development personnel of the Radio Test Shop joined us at the Naval Research Laboratory.

The Radio Division started with 23 civilian employees. It finished World War II with 1000. The Radio Division had a group on Precision Measurements headed by Dr. J. M. Miller, aided by Charley Rowe and Raymond Owens; a group on Transmitters headed by L. A. Gebhard; a group on Aircraft Radio headed by C. B. Mirick; a group on General Research headed by L. C. Young; a group on Direction Finders headed by W. B. Burgess, and a group on Receivers headed by T. McL. Davis.

The Director of the Laboratory in those days was not in residence but had an office in the Navy Department, reporting through the Admiral in charge of Navy Yard activities to the Assistant Secretary of the Navy. The Director was also aide to the Secretary for Inventions. Commander E. G. Oberlin (later Captain USN Retired) was our first Assistant Director. Since he was in residence, it was upon his able shoulders that the administration of the laboratory mainly fell.

From the very beginning the organization of the Laboratory has differed radically from other organizations within the Naval structure. The civilian division superintendents are charged with full authority and responsibility for the work within their divisions. They report only to the Director of the Laboratory. The Director normally has staff officers with interests in various fields, but they do not directly control activities within the division. This gives the scientist sufficient freedom of action and at the same time responsibility for his decisions, which is necessary if research is to be untrammeled. This basic principle of operation has been supported by every director of the Laboratory to date. It is largely responsible for the growth and development of the Laboratory and for its ability to meet effectively the stress of war-time conditions.

A division of ballistics (later merged with the present Division of Mechanics and Electricity) was established late in 1923. The Heat and Light Division (later called Physical Optics) under Dr. E. O. Hulburt, was established in 1924. The same year the Radio Materiel School for selected officers and petty officers from the Fleet was established. The Divisions of Chemistry and Physical Metallurgy were established in 1927; the Division of Interior Communication in 1934. Other divisions were added later.

The Laboratory was officially opened on the 1st day of July 1923. Assistant Secretary of the Navy Theodore Roosevelt, Jr. being the leader in the ceremonies.

The Radio Division went to work some time before the Laboratory was officially opened, in spite of the fact that temporary wires had to be run here and there and portable radio generator sets had to be used for power supply. The wiring of the main building #1, in which all laboratories were then housed, was not yet completed.

One of our first jobs was the development of an intrafleet set covering a number of frequencies between 1500 and 2500 K.C. This was a vacuum tube transmitter intended to replace the cumbersome British motor-buzzer spark sets which we had copied during World War 1. These British sets were supposed to be capable of operation without interfering with receiving work on other circuits, but it was almost impossible to keep them in such condition that this hope was realized. It was evident that a modulated continuous wave set would create far less havoc in the way of interference.

After this set was completed, largely by Mr. Young and myself, we modified the original model to go to nearly 3000 kilocycles. Since this set went down as low as 1500 kilocycles we were able to contact plenty of amateurs with it, especially at night when amateurs were on the job. We would then slowly raise the frequency, asking amateurs to observe the strength of the signals. Most of them didn't have equipment that could go very high in frequency but we encouraged them to build new coils and test again another evening, when they could reach higher frequencies. Thus we coaxed a good many of them up into the 3000 kilocycle band.

It was about this same time that Dr. Conrad, of Westinghouse, started his remarkable long range communication experiments on 3000 kilocycles. When Conrad attended the London conference in 1924, he staged a dramatic demonstration by picking up in London these high frequency signals directly from Pittsburgh. In England, Marconi was building gigantic beams for nearly the same frequency and beginning to work great distances with them. In the fall of that same year, 1923, some amateurs were very active in the 3000 kilocycle band. Fred Snell (later Captain USNR) and John Reinartz (later Commander USNR) had succeeded in getting the first signals on that frequency band across the Atlantic before the close of the year 1923. Our station at the Naval Research Laboratory, operating under the call NKF, also worked two ways with similar stations in Holland and was heard in various other countries. The distances obtained with this frequency band in the day time were nothing remarkable, but at night were astonishing, quite out of line with the theory that had so far held good up to 1500 kilocycles.

Although in the late fall of 1923 we did not realize the tremendous possibilities for the use of high frequencies in the field of Naval Communications, we did see that they would certainly be extremely valuable, provided we could sufficiently stabilize transmitters and receivers to make the use of such frequencies practical under Naval conditions. The amateurs using these frequencies usually applied what was called "raw alternating current" to the plates of the vacuum tubes involved in the transmitter, resulting in a highly modulated emitted wave. It wasn't too hard to locate such signals with a good receiver and to follow them even if they wobbled a bit in frequency. But this type of emission was just the thing we did not want on board our ships, for fear of interference with neighboring channels of communication. We could get a pure unmodulated wave by supplying the transmitting tubes with high voltage direct current. The receivers in all cases were at that time supplied with direct current. The difficulty was that the emitted wave fluctuated considerably in frequency, rendering it hard to follow if the method of CW (continuous wave) reception was employed.

The CW, or heterodyne method of reception, as explained earlier, involves the generation of a local frequency, usually within a thousand cycles of the frequency of the incoming wave, for the purpose of mixing it with the incoming signal, and extracting the difference frequency as an audible note in the telephone. This is not difficult if the wave frequency received is low, but when it is as high or higher than 3000 kilocycles, which is 3,000,000 cycles, it is not so easy. We have then the situation of an incoming frequency of several million cycles and a local frequency of the same order of magnitude. If either of these high frequencies wobbles a little, the difference of the two frequencies may very many thousands of cycles, thus often getting completely out of range of the human ear.

A great many things may bring about those fluctuations of high frequencies. If transmitters and receivers are not very carefully shielded, the movement of the hands or the body of the operator may produce changes of frequency of tens of thousands of cycles. So we learned how to build shielded transmitters and receivers. But in addition to the "body effect" we had to deal with effects within the transmitters and receivers themselves, variations in temperature, applied voltage, humidity, and finally with variations caused by movements of the antenna with which the signal is either sent or received.

It was clear to us in those days that something drastic would have to be done to control frequencies to a far higher degree of precision than hitherto had been attempted, if we were to succeed in selling the use of high frequency to the Fleet. The standards of reliability for Fleet work are vastly different from those so easily accepted by amateur communicators.

Some time before we moved to the Naval Research Laboratory, the use of the so-called power amplifier circuit for transmitters had started. This circuit provides as stable a frequency control as can be had with a small transmitting tube of very low power, the whole very carefully shielded and supplied with constant voltages. The output of this circuit is then applied to the grid of an amplifier of higher power thereby determining the frequency in the output circuits. The output of the power amplifier is then coupled into the radiating element, or antenna. It should be remarked in passing, that these extreme frequency stabilizations are not as necessary in telephonic transmission and reception, although they can by no means be neglected.

When L. C. Young and R. B. Meyer built the high frequency transmitter for the big dirigible SHENANDOAH, they used a 7 1/2 watt tube for master oscillator and a 50-watt tube for power amplifier. Taking the best possible precautions in the way of shielding and constant supply voltage, the power amplifier, if operated on the same frequencies as the master oscillator which excited it, had to be balanced or neutralized to prevent it from going off on its own and oscillating on same frequency, perhaps differing widely from the desired frequency and, in any event, on a frequency which was wholly unstable. As soon as we arrived at frequencies notably higher than 3000 kilocycles we were forced to use these neutralizing circuits, being particularly attracted to the circuit worked out by Dr. Rice of the General Electric Company. A good many other varieties of balancing circuits were later worked out at our Laboratory.

In an effort further to explore the behavior of higher frequencies, Mr. Young and I built a transmitter with a frequency of about 5700 kilocycles.

This had a rigid pole for an antenna, operating in connection with a counterpoise several feet above the ground. The transmitter consisted of a master oscillator, followed by a pair of 1 kilowatt Western Electric tubes which fed a high frequency transmission line leading to the antenna and counterpoise. The filaments of the transmitting tubes and the motor generator, supplying up to 2000 volts direct current, were both operated from storage batteries, in the effort to do away with voltage fluctuations. This transmitter showed a daylight range of 500 miles and a night range in excess of 5000 miles. It was easy to determine this because we not only had large numbers of amateurs in the United States cooperating with us, but had good contacts and help from amateurs all over the civilized world. The frequency stability of this transmitter was good, but by no means perfect. The next step in the program was to apply crystal control to high frequency transmitters.

The crystal referred to is not the usual receiving crystal, but is the piezo-electric crystal. Now these crystals had been known for nearly forty years, but their ability to oscillate mechanically and thereby control high frequency electrical oscillations was a relatively recent discovery. The Laboratory was soon actively engaged in preparing these crystals, ultimately setting up a crystal grinding shop which for a time supplied our entire needs. After this work could be reduced to a stereotyped procedure, the crystal grinding shop was moved to the Washington Navy Yard. Finally, after manufacturing concerns in the United States went into the crystal business on a sufficient scale, it was done away with altogether. For a long time, however, we had to make our own crystals.

The best substance for such crystals was at that time quartz, but only certain kinds of quartz were useful. To find out whether the quartz was useful or not required examination by optical methods. Cracked or flawed quartz can be discarded after observing with the unaided human eye. This is not enough to determine whether the crystal is going to be good as an oscillator. To do this, one must examine the crystal carefully with polarized light and exactly determine the principal crystal axes, so that slabs can be cut out at appropriate angles. In those days we usually used crystals about one inch square. For high frequencies the thickness of the crystal determined the frequency. Thin crystals operated at a higher frequency than thick ones. A crystal operating at 4,000,000 cycles, that is, 4000 kilocycles, would be roughly 3/4 millimeter in thickness.

In the usual vacuum tube oscillator circuit one usually finds tuning or frequency determining elements connected to the grid of the tube. Another set of such elements is connected to the plate circuit. To use the quartz crystal, it is substituted for the grid tuning element. The tube will then oscillate and produce radio frequency current only at the particular frequency of that quartz crystal. In order to change the frequency of such a transmitter, a suitable number of crystals must be switched in, corresponding to the number of frequency channels. Thus the frequency is not continuously variable but can only be altered in jumps, the number of frequencies available being determined by the number of crystals supplied. Although this is a handicap, in those days it was far more than over-balanced by the extraordinarily precise frequency control resulting from its use.

In the summer of 1924 Dr. Walter G. Cady and Dr. Karl VanDyke worked with us on crystals. Since these two gentlemen are, as far as I know, the two best authorities on quartz crystals, we naturally benefitted greatly by their association with us.

In the meantime Dr. Miller and his aides, with special assistance from Eisenhower, (who afterward became head of the crystal grinding section, later went to the Navy Yard crystal shop and still later into the crystal business on his own), rapidly developed the application of crystals to the standardization of frequency meters and the precise measurement of radio frequencies. This work resulted in a long line of frequency meters and precision frequency crystal calibrators being pioneered at the Naval Research Laboratory and finally supplied to the Fleet by the Bureau of Engineering.

The first high power crystal controlled transmitter in the world was built in 1924 by Mr. Gebhard, aided by Matthew Schrenk and Edwin L. White, and for a long time handled nightly broadcasting of official business from the Navy Department to the American Embassy in London. This set put 10 kilowatts of radio frequency power into an antenna consisting of a 1/4 wave vertical iron pipe working in connection with a star shaped iron pipe counterpoise all mounted on top of the machine shop. The frequency used on the London circuit was 4015 kilocycles.

In the meantime, new and better receivers of higher and higher frequency were needed. Mr. T. McL. Davis and Mr. Edwin L. Powell had completed the design of new low frequency and intermediate frequency receivers, known as the RE and RF. These two receivers were designed specifically for the coupling tube circuit of multipe reception and had a long and very honorable history in the Navy. It was many years before a better receiver, suitable for Naval conditions, was produced by anybody.

Our first high frequency receivers were strictly what we called "soap-box models", several of them being built by Mr. Young. They were sensitive enough, but not simple enough in operation or sufficiently robust to warrant sending any of them out into the service. Malcolm P. Hanson designed the first high frequency receiver that was sufficiently sturdy, simple and compact for practical Navy use. One of these was carried by the dirigible SHENANDOAH in 1924 in her flight to the West Coast and return. Some ten or twelve others were made up in our shops and distributed to ships and stations that we wanted to indoctrinate in high frequency and were in a position to make valuable observations on our signals. This receiver was finally worked over by Davis and Powell and got into the service as the RG.

We had a difficult job in teaching Navy personnel to handle high frefuency receivers. When receiving CW (continuous wave) the operator in search of a signal may move his dials so fast that he will pass over a dozen signals without hearing any of them. Due to the fine tuning required for high frequency reception, the CW method requires very deliberate and very careful movement of the dial. Anyone who has tuned a modern all wave broadcast receiver knows how much more carefully it must be tuned for high frequency European broadcasting stations, say in the 9000 to 12,000 kilocycle band, than for the ordinary

American local broadcasting stations, operating between 500 and 1500 kilocycles. If you multiply this difficulty by a figure of about 10, it can be seen how careful one has to be in receiving such frequencies by the CW method. Nevertheless, the CW method is the method par excellance for receiving and transmitting telegraphic code.

Cooperative experiments by NRL (Naval Research Laboratory) with the amateurs soon brought to light a new and remarkable fact in regard to the behavior of relatively high frequencies. We had noticed for some time that frequencies between 2000 and 4000 kilocycles were generally good for communication during the hours of darkness, but didn't travel as far during the daylight hours. On the other hand, frequencies between 4000 and 12,000 kilocycles were particularly good for long range daylight work, although there were certain regions where they were not well received.

In order to get at the reasons for these peculiarities, Mr. Young and I started experiments between Washington and Hartford, Conn., or rather a suburb, a few miles out of Hartford, where Mr. John L. Reinartz, an ardent amateur, had an experimental station. Commander Reinartz, USNR, was on duty here at the Naval Research Laboratory during part of the late war. We had two transmitters of two or three different frequencies available simultaneously at NRL. Reinartz had a continuously variable transmitter with which he was able to cover a very wide band of frequencies. Our method of procedure was to make contact on moderate frequencies which commonly went through without difficulty both ways. Then Reinartz would slowly raise his frequency while we made observations and notified him when his signals ceased to come through. Usually the signals would increase in intensity until, upon reaching a certain rather high frequency, which varied somewhat day by day, and certainly varied from season to season, the signals would suddenly cease altogether. Beyond this critical frequency, they never reappeared until the next day.

While these experiments with Reinartz were going on, we tested with another amateur, Wm. Justice Lee in Orlando, Florida, who frequently operated back and forth with us and made observations. Captain Lee, USNR, served for a good many years in the Office of the Director of Naval Communications and was retired relatively recently. Reinartz also served several years in that office. It so happened that Lee was listening in while we were conducting some of these experiments with Reinartz. He repeatedly noticed that after we had reported cessation of Reinartz' signals from Hartford to Washington, they were very successfully received many hundreds of miles farther away in Orlando. Lee reported this to us at once by radio. Thereafter, when all three stations were on the job, we took many observations, from which it was soon apparent that a zone of silence existed on certain frequencies. In this zone no signals could be received from the transmitting station, although beyond that zone of silence the signals were very strong.

This then was the discovery of the "skip" distance effect. In other words, the signals were capable of skipping over a good many miles; in fact, sometimes more than 1500 miles as we found later, then coming down with excellent intensity at points beyond. This is a phenomenon totally outside of the predictions of the older wave propagation theory and led to a modification of that theory which was published by Dr. E. O. Hulburt, of the Division of Physical Optics, and myself, in the Physical Review for 1926. Earlier I had published papers on the "skip" distance effect in QST and in the Proceedings of the Institute of Radio Engineers. The Hulburt-Taylor paper laid the foundation for modern high frequency wave propagation theory. The original Larmor theory was modified by the addition of new terms in the equations which contributed so little to the end result at low frequencies that this theory was still good on the low frequency bands. The added terms permitted a reasonably clear understanding of the new high frequency phenomena.

The behavior of the high frequency radiation leaving a non-directive antenna can be briefly explained as follows: The radiation goes out in all directions, and at all upward angles, from the horizon very nearly to the zenith. The waves which are sent out parallel with the surface of the earth constitute what we may call the ground wave. This wave is earth bound and, to some little extent, follows the curvature of the earth, bending around it and extending its range somewhat, but not a great deal, beyond what may be called "the line of sight". The more elevated rays leave the surface of the earth and proceed into the upper atmosphere until they reach a region called the ionosphere, because it is a region where ionization of the rarified gases has been brought about by solar radiation of ultra violet light.

It is difficult to describe the exact upper and lower limits of the ionosphere since conditions vary so much from season to season, from day to night, and are influenced by the sun spot cycle. All of these factors have a tremendous influence on the proper choice of frequency for use in high frequency communication. Suffice to say then, that this region extends roughly from approximately 70 miles above the surface of the earth to a point several hundred miles higher. Naturally this is a region of very rarified gas. It is because of this that the ultra-violet rays from the sun are able to penetrate deeply and to disrupt neutral molecules of gas, producing a very considerable number of free electrons. The tendency to produce electrons is counteracted by the tendency of the electrons to recombine. Since the chances are greatly against their recombination with the same positive ions that they were originally mated with, we may describe this situation in the upper atmosphere as a constant succession of marriages and divorces between the negative electrons and the positive ions.

In the middle of the day the ultra-violet light is strong in these upper regions, so more free electrons are produced then in the middle of the night when the recombinations which take place very greatly reduce the free electron population. In general, there are more electrons on a midsummer day than on a mid-winter day. The smallest number may be expected in the middle of the winter, late at night.

Since it is these electrons which make high frequency communication possible, it will be well to stop a moment and describe what happens to some of the different rays emitted from a transmitting antenna. The ground ray, earth bound, is rapidly attenuated, following approximately the low attenuation described in the older theory, so that at very high frequencies the earth bound ray proceeds only a few miles from a fairly strong transmitter before it is practically all absorbed. In other words, this ray, vibrating very rapidly, with its feet continually tangled in the earth or sea over which it travels, quickly expends its energy in the form of earth currents. At high frequencies this ray gives very limited range communcation indeed, whereas at very low frequencies it is capable of going to immense distances.

If we now consider a ray leaving the antenna at an angle of 20° upward from the horizon, we see that it will enter the ionosphere at a rather oblique angle. The action of this region, with free electrons in it, is to gradually turn the ray down almost as though it were reflected from a large mirror. Since the ray has struck at an oblique angle and since, in reaching the surface of the earth, it has to catch up with the curvature of the earth in order to come all the way down, it must reach the earth's surface quite a long distance away from the transmitter. If we consider another ray, say at 45° from the horizon, we see that it strikes the ionosphere at a sharper angle, but under suitable conditions it too, will be turned back to the earth and will reach the surface at a much closer point with reference to the transmitter than would the ray at 20° angle. If now we consider a ray which goes upward close to the vertical we see that if it were reflected down, it would come very close to the transmitter, so that there would be no skip distance. This is what actually happens with only moderately high frequencies, when used in the daytime. Supposing the effective part of the ionosphere is, at the moment, 100 miles high. Our measurements difinitely showed that at 15 miles horizontal distance, at a frequency of 4000 kilocycles, the ray which had traveled over 200 miles, bouncing down from the ionosphere, was very much stronger than the highly attenuated ground ray which had traveled only 15 miles. In other words, the attenuation on the trip up and down from the ionosphere was very small. Indeed the higher the frequency, up to frequencies on which measurements were then available, the less was this ionospheric attenuation. However, since we do get skip distance effects, it is seen that when the upward rising ray of a sufficiently high frequency strikes the region of the ionosphere at too sharp an angle, that is, too near the vertical, the ray is not bent rapidly enough to ever catch up with the curvature of the earth and come down again. The ray which shoots high enough to just get down again marks the beginning of the first zone of reception. There may be a gap of hundreds of miles in some cases, between the end of the ground ray range and the beginning of strong sky wave signals.

By carefully measuring skip distances under various conditions and at many different frequencies, we were able not only to get up good practical communication charts as guides to the use of high frequency, but to determine fairly closely the number of free electrons per cubic centimeter in the ionosphere under different seasonal and diurnal conditions.

It was plainly evident that high frequency communication could not be used by the Navy in the same way as low frequency communication had been used. Hitherto the Navy had assigned ships to certain frequencies, in the middle frequency range not affected by skip distances, for continual use day or night, winter or summer, for specific purposes. This could not be done on the high frequencies. A frequency in these higher bands might give the ship excellent daylight communication, especially over long ranges, but at night-time, due to the very small number of electrons in the ionosphere, this wave might never be led to the earth so that night-time communication would be limited to a very few miles indeed, namely, to the range of the ground ray. On the other hand a lower frequency might be excellent for night use, but be too heavily attenuated for long range daylight use.

In spite of these difficulties, the fact that atmospheric disturbances were enormously lower on the high frequencies and that it was possible to get long range communication with sets of ridiculously small power, say 1% of the average Naval set, made it worth while to try to sell the operating agencies of the Service the idea of making practical use of high frequency communication.

I have already mentioned the 10 kilowatt set on 4015 kilocycles which was put into operation in 1924, a crystal controlled set, used for a long time on the broadcast to the American Embassy in London. In spite of occasional failures due to unusual magnetic disturbances, sun spots, etc. this set hung up such a good record of effective, although not perfect, communication that the Office of Naval Communication, then under Admiral Ridley McLean, was very definitely interested.

In 1924 it was decided to send the great dirigible SHENANDOAH across the continent and back. I have already mentioned that we built the high frequency equipment for the SHENANDOAH. When she started on her trip Mr. Young and I arranged to work her continuously across the continent, using a high frequency transmitter at the Laboratory and making arrangements to send her messages over the wire to the Navy Department as fast as they came in. It was soon evident that the SHENANDOAH's high frequency transmitter was far more effective in maintaining communication than anything else she had. We were in touch with her on nearly the whole flight. In between her schedules with the Laboratory she worked effectively countless amateur stations, all over the country. Lieutenant Palmer was the operator on board the SHENANDOAH. L.C. Young and R.J. Colson did the operating at the Naval Research Laboratory.

We had alerted the amateurs in various parts of the country, giving them the SHENANDOAH's frequency, which was about 3200 kilocycles, asking them to listen in and make reports. In several instances this was very helpful during the night flight as the SHENANDOAH, at certain times none too sure of her position, was able to call up amateurs in all the nearby towns. Sooner or later they would find an amateur who could see the SHENANDOAH lights. On one occasion of uncertainty, Palmer requested an operator in a town of whose identity they were not quite sure, to flash the headlights of his automobile rapidly for purposes of identification. The word evidently got around, because in a few minutes the whole town was ablaze with flashing automobile headlights that gave the SHENANDOAH a positive check on her position at that moment.

It may be recalled that the SHENANDOAH was built originally for hydrogen, but on account of various disasters to hydrogen filled ships, the Navy decided to use helium. Helium has about 10% less lift than hydrogen. Therefore the SHENANDOAH was not able to fly at anything like the altitudes obtained by hydrogen filled Zeppelins. She took the southern route across the continent, through Arizona, and had to go through a mountain pass near Bisbee. That took the ship so close to the rocky walls that one of the boatswain's mates demanded a boat hook to keep the mountain goats from jumping on board. I got this from Palmer at the time it was said to have happened; of course I cannot personally vouch for the truth of this story. This flight, at any rate, helped a good deal in selling high frequency communication.

The SHENANDOAH started on her last and fatal flight at 3 o'clock in the afternoon, September 2, 1925. Commander Zachary Lansdowne was her skipper and Lieutenant Commander (now Admiral, Retired) Rosendahl was her navigator. Commander Raymond Cole, then Warrant Radio Officer, was in charge of radio, assisted by Chief Radioman George C. Schnitzer. The ship was expected to make a somewhat round-about flight to arrive finally in St. Louis, Missouri. Cole took the first radio watch, continuing until midnight, when Schnitzer relieved him. The radio had been functioning well and a great number of weather reports had been received. These reports, in those days, did not carry upper air observations as they do today. In any case, the intensity of the violent disturbances associated with an approaching cold front was not indicated in the report.

Commander Cole owes his life to the fact that because he had had a very long watch his relief did not call him at 0400 in the morning as previously agreed. Just before dawn, probably about 0500, a terrific squall hit the SHENANDOAH and Cole rushed into the midships passage on his way to the control car up forward. He says that the ship was even then much higher in the bow than in the stern, but that he wasn't especially alarmed. Fortunately for Cole, he saw the cook pouring out hot coffee and stopped to get a cup before proceeding to the control car. Before he could finish his coffee the ship broke up. It is estimated that the height at the time was about 3600 feet. The control car was detached from the forward section and plummetted to the earth with disastrous results to those stationed there. The forward section, freed of the control car sailed for a number of miles before coming to the earth. Many of its personnel were saved. Out of a total complement of about 42, nearly 60 percent escaped with their lives. The rear section traveled some little distance and came down gently. All this was happening in the general region about 40 miles north of Marietta, Ohio. The middle section was badly broken up, settling very rapidly. Cole had been injured by a heavy steel wire, which snapped and struck his head. A broken girder had pierced and held his heel. He was unconscious during the drop, which he made hanging by his pierced heel, but after the rescue parties freed him from the wreckage, he was able, with assistance, to get to an awaiting ambulance. Schnitzer dropped with the control car and was killed, as also were about half of the people still in the middle section when it dropped.

It is interesting to note that the high frequency radio receiver built by NRL was recovered uninjured and ready to operate as soon as batteries were connected to it. This receiver is now a museum piece on display at the Navy Department.

Very shortly after this Mr. Young and I had pushed some interesting long range daylight experiments into the 15,000 kilocycle band. At that time a great many amateurs had already entered this band, so we were able to get plenty of contacts, not only in this country, but in others. We even worked two ways to Australia on one occasion with only 50 watts of power. We had so many interesting reports of strong signals in Central and South America that, after talking the matter over with Admiral Ridley McLean, we shipped one of the small high frequency single circuit receivers, engineered by Malcolm Hanson, down to the Balboa Receiving Center in the Canal Zone with instructions as to how to operate. Day after day we tested with a little 50-watt transmitter at NRL. Each day the report came back "nothing heard". I was sure that it was due to inexperience in the tuning of such a receiver, which had to be done with great care. However, Commander Raguet, later Admiral Raguet, who was our liaison man with Admiral McLean's Office, was very patient and we kept on trying. Finally, one day a report came back "Signal strength 10" (the most he could give us on the signal scale) "Go ahead and send some traffic". We never had any particular trouble reaching Balboa with this set after they once found the signal. We had rigged up a radio relay station at Washington Navy Yard, which had wire connections to the Navy Department, so that the Navy Department could send the messages. This was an early incident of radio repeater work on high frequency. Before long we had a special set of lines run down to NRL from the Navy Department, which the Office of Communications found very useful. Balboa replied to these communications with a low frequency high power arc. The usual procedure was for the Navy Department to use the high power set at Annapolis, also on low frequency, but day after day in midsummer Balboa would send a message "Annapolis, high power unreadable. Please use the 20 meter (15,000 kilocycle) set". Now since Annapolis was using 250 kilowatts and we were using 50 watts, a difference of a factor of 5000 in favor of Annapolis, these practical communication tests made a very considerable impression on the Navy Department.

It was then decided to make an experimental installation on the Flagship of the Fleet which sailed to New Zealand and Australia in 1925. I persuaded the Navy Department to call Fred Snell back to active duty and assign him to the staff of the United States Fleet Radio Officer, who was then Commander Hooper. Hooper, at that time, was not all sold on high frequency. When he came back from that cruise, he was enthusiastically in favor of it. We converted the crystal control set which we had used at the Laboratory and operated on 5,700 kilocycles, and helped Snell, during a preliminary "get ready" period at the Laboratory, to put his personal amateur transmitter in good shape, arranging it so as to cover a wide band of frequencies. We also arranged with amateurs all over the world to cooperate in these tests. For official business, NRL was designated as the principal receiving station, with a man to relay the messages at once to the Navy Department. The station was manned by Mr. Young and myself, aided by Malcolm Hanson and Paul Sheram. Sheram, in particular, was one of the most remarkable operators I have know. He had infinite patience with weak and difficult signals and an almost uncanny ability to read them when other people could scarcely hear them at all.

The high frequency transmitters and receivers were put on board the USS SEATTLE at San Francisco and a considerable number of tests made while the Fleet was enroute to the Hawaiian Islands. It was immediately noticeable that operating conditions for high frequencies on board ship were more difficult than ashore. Many electrical devices on board ship emitted high frequency radiation which interfered with reception of weak signals. In addition to this, vibrations and pitching and rolling movements of the ship introduced difficulties in the handling of very sharply tuned high frequency apparatus. In spite of these and other difficulties, communication with high frequency was fair. After the lay-over period in the Hawaiian Islands, when the operators had had more experience, it improved rapidly.

The official report and recommendations of the Fleet were printed in the monthly Radio and Sound Report of the Bureau of Engineering, Navy Department early 1926. While this report very definitely recommended the addition of high frequencies to the Fleet's communication system, it was more conservative than the plan recommended by the Bureau of Engineering, backed by the experiments and technical advice of the Naval Research Laboratory. The Fleet report, among other things, recommended the immediate reservation of suitable blocks of high frequency bands to cover the possible needs of Fleet communications. The Fleet naturally had the most difficulty with the highest frequencies, such as were suitable for long range daylight work, because the high frequency receivers provided were highly experimental and not very stable in adjustment. Naturally they gave the most rouble.

The Bureau of Engineering and the Laboratory immediately went to work to remedy this difficulty by providing high frequency receivers robust enough to stand shock and vibration, at the same time having the required degree of sensitivity. The ranges obtained by the Flagship during this cruise exceeded all expectations, there being little difficulty in handling direct communication with Washington, by high frequency both ways, when the Fleet was at Wellington, New Zealand and later on at Melbourne, Australia, nearly 10,000 miles away. True, this couldn't be done at all hours of the day, being most effective between 11:30 p.m. and 8:00 a.m. Washington time. To get communications through such distances seemed almost a miracle, especially in view of the extremely low power of the equipment used. San Francisco and Honolulu, as well as Balboa, had been equipped with high frequency receivers. Thus the Fleet's report was not based entirely on the results obtained in Washington and in the Fleet.

Normally on a cruise like this, the Flagship, say at Wellington or Melbourne, would have centered communications on her long wave Poulsen arc equipment transmitting to Samoa, whence it would be relayed to San Francisco or San Diego, whence it would be sent to Washington. In Melbourne, for some ten days, the long wave static was so heavy that the Flagship was entirely out of communication by long wave. Had they been forced to put this traffic on the cable, it would have cost a good deal of money. The high frequency, on the other hand, was entirely unimpaired by this condition and handled the traffic with ease, directly to Washington.

The Fleet recommended that the shore stations be equipped to trasmit to the Fleet on frequencies not higher than 9000 kilocycles, because they were afraid that the instability inherent in high frequency receivers and the high noise level from electrical disturbances on board ship would jeopardize the reception of higher frequencies. Fortunately the Bureau of Engineering disregarded this recommendation and went right ahead with its plans for equipping, as soon as possible, 28 shore stations with high frequency equipment going all the way to 18,000 kilocycles. In a few years the United States Fleet had far better communications than any Fleet in the world and, thanks to crystal control and precision receivers, more stable high frequency equipment. Needless to say this splendid equipment scored heavily for us in World War II, so that the Navy was not forced, with the approach of the war, to rush into the production of huge quantities of new high frequency communicating equipment. Much of the equipment could be rapidly duplicated by industrial agencies already familiar with such production. The Navy was thus able to spend much of its resources for pushing the radar program.

An interesting outcome of the tests in the Hawaiian Islands, while the Fleet was at anchor before proceeding to Australia and New Zealand, was the discovery that at certain hours the 50 watt signals sent by Lieutenant Snell on about 7500 kilocycles could be perfectly copied at Johannesburg, South Africa. This was reported to me by an English amateur, who sent me his complete log of messages, copied from the SEATTLE. These signals came in for two and a half hours every morning and again every evening, but not at other times. This is of great interest, because if we look at the globe we see that Johannesburg is exactly on the opposite side of the world from the Hawaiian Islands. Signals can go half way around the world by any one of an infinite number of paths, all of approximately the same length.

Now the frequency used by Snell at this time and observed in South Africa, was suitable for long range nocturnal communication, therefore they didn't go around the world on the side exposed to daylight. However, there were two periods in the day when the path between Johannesburg and the Hawaiian Islands lay in darkness. These two periods corresponded to the morning and evening hours, when our Johannesburg correspondent received such tremendously strong signals and such good copy. It is interesting to note that analysis showed that during one of these periods the signals came over the Indian Ocean and inland to Johannesburg; during the other period, over the Atlantic Ocean and inland to Johannesburg. This shows that high frequency signals of very low power can, under just the right conditions, penetrate to such great distances that they must be used with considerable discretion if the Fleet does not want to have them intercepted.

By the time the Fleet was well along with the cruise, Lieutenant Snell had rigged up a low power radio telephone set. He amused himself by talking to amateurs in the United States and other countries, first by code, then getting them to listen for his voice communication. This led to an amusing incident while the Fleet was at Wellington. Wellington is almost on the opposite side of the world from London so, at certain hours of the day, communication between the two places is extremely easy even with low power. Snell had been trying out his voice set with a British amateur one day when a party of visitors, friends of the Governor General of New Zealand, arrived on board. One of them, a young Englishmen attached to the Governor's staff, although not an amateur in any sense, was tremendously interested in radio. This gentleman spent a lot of time in the radio room. Snell asked him if he wouldn't like to overhear a conversation with this British amateur, who also had a voice communication transmitter. So the Englishman listened in, but only remarked "Oh, this is just some bally Yankee spoof - you have got a little radio phone set concealed somewhere in the Fleet and that is what I am hearing". This somewhat irritated Snell so he remarked "Well if you have any relatives in England I will get this man to send a message from you which will require a reply stating whether it was correctly received or not". The Englishman then said "Oh I have a brother in London". So Snell handed him the microphone.

"Tell this British amateur anything you like". The Englishman then spoke into the microphone, approximately as follows: "They say I am hearing voices from England. It isn't true, is it? Please reply by cable". A day or two later he came rushing on board waving a cablegram in his hand. It seems that his brother had cabled him collect to the tune of many pounds of good English money; "Of course it is true you bally ass, now pay for this". I will say the man was a mighty good sport, because he didn't begrudge the money at all, being delighted with his experience.

The rapid expansion of Navy high frequency had a marked influence on commercial activities in this country. Commercial activities were under way and doing excellent work also, but the Navy was definitely ahead in practical experience. It was very much to the interest of the Navy to get commercial people into the game, because we had to find somebody to build a large quantity of high frequency equipment.

During the year 1925, the Laboratory built a crystal controlled transmitter covering a group of frequencies in the 4000, 8000, 12,000 kilocycle bands. The Laboratory had found a way to efficiently multiply frequencies, so that with a crystal controlled master oscillator of, say, 4000 kilocycles we could get output in the power amplifier of 8000 and 12,000 kilocycles. With two amplifiers cascaded, giving the possibility of two or three fold multiplication in either one or both, we could arrive at almost any desired output frequency in which we were then interested, up to 20,000 kilocycles.

The first shipboard crystal controlled master oscillator set operating on more than one frequency was the XA, a 500 watt set for CW communication only. In order to give this set a good trial, it was installed in June of 1925 on board the cruiser MEMPHIS. That was the same cruiser that brought Lindberg back from France the next year. Captain, later Admiral, Lackey, commanded the MEMPHIS. The installation was made at the Philadelphia Navy Yard. Tom Marshall and I went to sea with it. Marshall, now a Lieutenant Commander in the Navy, was then a Chief Radio Electrician. He was not only an expert communicator, but had remarkable experimental ability, having worked for some time with us at the Naval Research Laboratory, contributing a number of important improvements to high frequency gear, especially to high frequency receivers. Personally Mr. Marshall was one of the most pleasant men I have worked with.

We had just finished the installation of the set when we departed from the Philadelphia Yard to Newport. We were able to send our departure report and make a number of other tests on the way up to Newport. We had permission to work amateurs as well as Naval stations on this cruise. At Newport we took on board Admiral Gleaves and an Army general whose name I cannot recall, whom we were taking to St. Nazaire, France, for participation in the ceremony connected with the dedication of Ella Payne Whitney's statue to commemorate the landing of the first American soldier in France in 1917. We sailed the Great Circle route at about 18 knots.

We had hardly stuck our nose out of Newport before we ran into a northeast gale which, off the banks of Newfoundland, had the intensity of 60 knots, accompanied by heavy seas. This lasted for three days. While it didn't bother anybody seriously, in spite of heavy green seas coming over the foredeck, we did have to slow down to 12 knots for six or eight hours. It gave the radio equipment a good shaking up, which it withstood very well indeed.

Besides the XA transmitter, we had on board a 50 watt transmitter with wide range of frequencies, a laboratory affair, fastened down on a small table top. This turned out to be useful for experimental work to supplement the 500 watt set. We took it along just as it was, table and all. This set afterward journeyed in the same way to Balboa and was the first high frequency set they had there.

In spite of the difficulty in getting around the ship in foul weather, Marshall and I went right ahead with our communications. We worked a number of French stations shortly after we left Newport and soon had many satisfactory test contacts for both day and night operation.

When we were beyond the middle of the Atlantic, at a point almost on the opposite side of the world from New Zealand, we had very excellent communication with amateurs in Wellington, New Zealand. One night at 11:00 P.M., as I was about to turn in, I passed the door of the Captain's stateroom and he called out to me "Doctor, how are the radio tests going"? I told him we were working practically anybody we wanted to and had just been talking with a chap in Wellington. I think the Captain didn't believe me, but he quietly remarked that the ship had been on a cruise to New Zealand the previous year and he had lots of friends in Wellington; perhaps he could get messages to some of them. I said: "Certainly, will you write them out?" So he wrote out three messages, one to the Governor General of New Zealand, in whose home he had been entertained. We had a 2 o'clock A.M. schedule with the Wellington lad, so I got the messages up to Marshall. He promptly put them through on schedule. Since it was 2 o'clock in the afternoon New Zealand time, they were delivered immediately to the addresses. Within an hour we had replies and the Captain found them on his desk at 8:00 A.M. the next morning. Perhaps that is one reason why he wrote the Bureau of Engineering that, as far as he was concerned, all the other transmitters could be taken off his ship - he didn't need anything but the XA.

Marshall and I left the MEMPHIS at St. Nazaire and sent the small 50-watt set up to Antwerp. I spent ten days in Paris, where I met Captain Taylor Evans, son of Fighting Bob Evans, the hero of Manila Bay. Since I had a diplomatic passport I was able to help him out a little when we crossed the French-Belgium border. We lay in Antwerp on the PITTSBURGH for some time and practically every day succeeded in working directly with Washington. On the way back to the States we worked two way communications with the MEMPHIS, then lying at anchor in the Thames River, England, communication being good up to 2500 miles, both by day and night.

An interesting incident occurred as we were coming out of the English Channel headed westward. Two carrier pigeons, apparently in a rather exhausted condition, lit in our rigging and stayed with us all day. They frequently left the ship but always came back again. I gave instructions to one of the sailors to climb up and capture them after dark, when they had gone to roost. We found that they carried English bands and had evidently been part of a long distance race in which they had lost their way. I believe the flight was from the south of France to some place in England. I took the numbers off the bands and contacted an English amateur, requesting him to notify the owner that his pigeons were with us; not that he could do much about it, but because the owner would probably be interested to know what had become of them. The amateur replied he was sorry but he couldn't deliver such a message as the British laws didn't permit it. He could only communicate on technical matters for the purpose of testing his equipment. He could not handle either paid or unpaid messages.

I was very much interested in these pigeons, having kept homing pigeons myself, and was concerned as to how we should get them food and drink. Pigeons can't thrive on salt water although they would repeatedly leave the ship and try to drink some of it. I talked this over with Captain Evans and he promptly produced two small silver bowls from the equipment of his cabin, one holding fresh water and the other one rice. By this time the pigeons had become accustomed to our attention, would approach quite closely and finally accepted food and drink. I am sorry to say that one of them made the mistake of roosting in the lower rigging one night and the ship's cat caught it, but the other one was still with us when we got to the Brooklyn Yard. The last I saw of it, it was making off in the northeast direction. Whether it gave up the effort to reach England or not I do not know.

On the outgoing part of this trip, when we were somewhere in mid-Atlantic, we received repeated calls from an unauthorized station using a non-registered or "bootleg" call. At first we paid no attention, but finally I told Marshall to give him an answer to see what he would say. The reply immediately came back in four letter Navy code, to our very great surprise. When this long code message was deciphered, it turned out that we were listening to a transmitter located in the American Legation in Constantinople. There was trouble in that region at this time and the Turks did not permit the use of radio or cable communication without censorship. They didn't know that the Legation had any radio equipment. The fact is that all they had at first was a receiver for high frequency, which only required a small indoor antenna. This kept them in touch with what was going on in the rest of the world. They wanted to get some confidential messages back to the State Department of this country, so a radioman who had been detailed to duty at the Legation went down to the SCORPION to see if he could find materials for the transmitter.

The SCORPION was an old coal burning and very small destroyer, which had been sent over to the Mediterranean Waters during World War I and wasn't considered fit to make the return trip to the United States. I believe ultimately this ship was turned over to the Greek Government, because she remained at Salonika for a long time. At any rate, the radio man from the American Legation managed to get a 50 watt transmitting tube, a small transformer to give him some high voltage, a few coils of wire and a condenser. With this he manufactured a high frequency transmitter operating somewhere between 7000 and 9000 kilocycles.

A transmitter requires an outdoor antenna but they didn't dare put one up at the Legation for fear the Turks would see it. This radioman conceived the idea of making a 50 ft. insulated wire a part of the flag halyard, so that when the flag was pulled down at night, the antenna went up. Thus he was able to communicate at night when his frequency would have the greatest range. That 50 ft. wire and homemade 50-watt transmitter was what we had been hearing. No wonder it sounded wobbly and ragged. The upshot of it was that we forwarded a great many code messages to Washington, the contents of which I was not aware of as it was not our business to decode them. They were evidently of interest to Washington, because we were urged to keep contact, which we did practically all the way across the Atlantic.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER XI - 1926-1929

THE NAVY ADOPTS HIGH FREQUENCY

CHAPTER XI

1926-1929 - The Navy Adopts High Frequency

One of the most annoying disadvantages of high frequency for communication purposes is the tendency of the signals to "fade"; in other words, the signal is frequently not of constant intensity but rises and falls in volume, sometimes disappearing entirely for brief intervals. These changes may go on slowly but they can often occur a good many times in a minute. Smaller variations of amplitude can even occur many times a second.

These effects are partly due to the fact that a large number of different rays arrive at the receiver at the same moment, having traveled slightly different paths to the ionosphere and back to the earth. Many things point to the fact that the ionosphere is a region capable of considerable turbulence. The electron population varies considerably from moment to moment and the whole region may be likened to an uneasy sea of ionized gas. This gives rise to these different rays. The fact that they have small differences of path from transmitter to receiver means that these rays do not all arrive in the same phase of oscillation. Thus they do not always add up and reinforce each other. On the contrary, destructive interference between rays may result in momentary cancellations. Another curious thing about high frequency waves is the way they behave with respect to their polarization angle.

All the electro-magnetic waves, radio, light or heat contain both electrical and magnetic components. These are Siamese Twins which are both promptly killed if they are separated. In other words, a radio wave may be detected and evaluated equally well by either its electrical or its magnetic component, but neither component can be isolated from the other so as to have a separate existence. The radio wave leaves a vertical antenna with its electrical force oscillating approximately in a vertical plane. The wave is then said to be vertically polarized. On the other hand, if the antenna is a horizontal wire or rod, the electrical force in the oscillations is in the horizontal plane and the radiation is said to be horizontally polarized. These remarks are only important for high frequencies. At the low frequencies it is not possible to create horizontally polarized waves which will not be rapidly absorbed, due to the action of the earth or water over which they must travel. When working over very short distances within the range of the ground wave, it requires a vertical antenna to receive a vertically polarized wave because if the antenna were horizontal, the electrical forces in the wave, being at right angles to the rod, would produce no oscillations in current along the rod. Similarly, a horizontal rod would have to be used for horizontally polarized waves.

The ionosphere acts upon the wave as doubly refracting crystals act upon light waves, at least to the extent that it rotates the plane of polarization, and the velocities of motion of horizontally and vertically polarized electrical waves are slightly different. It is impossible to tell what the polarization angle of a wave will be when it comes down after having passed into and out of the ionosphere. Furthermore, it constantly shifts its plane of polarization from moment to moment due to uneasy movements and changes within the ionosphere itself. This also causes signals to vary at the receiver because sometimes the polarization angle favors the receiving antenna; at other times, just the opposite happens. I mention these matters in order to make it clear why in those days everyone in radio was thinking about devices which would smooth out these difficulties and render reception easier. Various anti-fading devices were originated at NRL and tried out with varying degrees of success. Some of these devices were applied to the transmitter so as to rapidly alternate or rotate the wave polarization angle, but the simplest was probably the use of a long low receiving wire, a number of wavelengths in extent, which could integrate the effects of a great many down-coming rays. Both the Radio Corporation of America and the Bell Telephone Laboratories had made many noteworthy contributions in this field.

In spite of these difficulties the very great ranges obtained with simple equipment and a small amount of power appealed greatly to the Naval Service.

The Laboratory was soon busily engaged in building a 5 kilowatt crystal controlled multi-wave transmitter for San Francisco. At that time we couldn't get any commercial company to undertake such a transmitter. We were reluctant to take on this job without permission to go out to San Francisco with our engineers, make the installation and indoctrinate the personnel in the proper way of handling the set. Money for civilian travel in the Navy is always tight in peace time; we were told that they couldn't afford to send us to the West Coast. Fortunately Mr. Brown, of the Mare Island Navy Yard, was spending his vacation in the East. We arranged to give him a couple of months' duty at the Naval Research Laboratory to take part in the building of this transmitter and to learn how to install and operate it. This was successfully done and this transmitter became the first high power crystal controlled high frequency transmitter on the West Coast. Gebhard, Fetsch and Willoughby engineered this job.

Another peculiarity of high frequency transmission for Naval use comes into play when it comes to the broadcasting of important messages which must go to long distances and to a number of different points of the compass. Suppose such a broadcast is to be put out at 6:00 P.M. To go out in the Atlantic we must use a frequency preferably well under 12,000 kilocycles, because the Atlantic is in darkness; but if, at the same time, the broadcast has to go to San Francisco or Honolulu, both of which lie in daylight at this hour, frequencies between 16,000 and 20,000 kilocycles are needed. If the same message is required at Panama, an intermediate frequency is desirable. Now, since all these conditions change a bit with the seasons, it is necessary to have four frequencies going simultaneously to do a thorough job.

The Naval Research Laboratory built for such broadcasts the first four frequency sets for Arlington. Mr. Gebhard was in charge of the engineering of both Mare Island and Arlington sets, being assisted by Mr. J. T. Fetsch, Mr. R. B. Meyer, Mr. Edwin L. White, Mr. O. C. Dresser and others. For many years these sets at Arlington were in operation; usually they were used separately, but for these special broadcasts they were operated, with only one key, from the Navy Department. These were crystal controlled sets operating in the 4000, 8000, 12,000 and 16,000 kilocycle bands.

The Bureau of Engineering asked us to build two 5-kilowatt crystal controlled multi-wave transmitters to be operated from Crocket-Wheeler 7500 volt direct current generators. One of these sets was the first high power crystal controlled set in the Canal Zone; the other was the first high power high frequency set on board a battleship, the USS TEXAS.

The installation on the TEXAS was made by Mr. R. B. Meyer. When he tested the set on various frequencies, he found that induced voltages of considerable magnitude were produced at various points on the ship, particularly on the 12" guns. A spark strong enough to slightly burn a person coming in contact with it could be obtained at the muzzles of these guns and measurements showed very considerable voltages even around the breeches of the guns, inside the gun turret. This was alarming because there was danger of setting off premature explosions during the loading of the guns. These explosions have happened several times in the Navy with very disastrous results and heavy loss of life although, so far as we know, they were never caused by radio.

The discovery of this effect on the TEXAS started a long chain of investigations on what we called "radio frequency hazards" which were found to be very real. Suffice to say that we were able to recommend certain installation changes and special precautions to be taken while transmitters of considerable power were being operated. Actually the results of tests showed that the increase in range due to the use of 5 kilowatt instead of 1 kilowatt was not worth the extra size, cost and complications of the set. Thereafter the Navy standardized on 1 kilowatt as the maximum power for high frequency shipboard transmitters.

The aircraft carrier people became alarmed about the possible radio frequency hazards. Again extensive investigations resulted in outlining the special precautions advisable to preclude these hazards.

In connection with the matter of radio frequency hazards it should be pointed out that high frequency currents, unlike those of low frequency, do not shock the individual who comes in contact with such circuits. They are not, therefore, dangerous to life but they are capable of creating severe and painful burns. Being a member of the high voltage club myself, I speak from experience. This group includes all people who have contacted 1000 volts or higher and have survived. There are several members of the 2000 volt group still at the Laboratory and, I am happy to say, alive and well although some of them carry a few scars.

The 5 kilowatt high frequency transmitter for Balboa was installed, with suitable antennae for the different frequencies, by Mr. O. C. Dresser. We found difficulty in keeping the 7500 volt direct current motor generator in operating condition on account of the high humidity in the tropics. This affected the insulation of the brushes and armature of this machine. The difficulty at Balboa was solved by building a box around the motor generator with a small heating unit inside. This box was kept closed and heated to above the temperature of the surrounding atmosphere when the set was not in operation, thus doing away with the excessive humidity. During operation the cover of the box was thrown open and a small ventilating motor turned on, because while operating the heat generated in the machine kept the temperature at a suitable level.

The control center in the Isthmus of Panama is at Balboa, but the actual location of the transmitters is several miles away on the shores of Gatun Lake, at Darien. While this installation was going on at Darien, Mr. Dresser, who is an ardent fisherman, sampled the waters of Gatun Lake in the evening when he was off duty. I don't know how many fish he caught but he did see some huge crocodiles at clase range and made up his mind to capture one of them. He had the blacksmith at the station make a huge steel hook of 3/4" rod with a sharpened point. He baited this hook with a large chunk of pork and attached it to a piece of 5/8" flexible steel cable. He threw this out from the shore of the lake and fastened the end of the cable around the trunk of a large stump fully expecting to have a crocodile the next day. But when he went down to the shore the next morning he found the ground around the stump trampled down as though by an army, the steel cable wound many times around the stump, the hook pulled out quite straight, and no crocodile. The crocodile evidently was hooked but in his struggles had gone around and around until the cable was wound tight around the stump. He then succeeded in bending that great steel hook nearly straight and thus released himself. This was too much for Dresser. He made no more attempts to catch crocodiles.

During this period the high power water cooled transmitting tube made its appearance, fostered in this country mainly by the General Electric Company, the Bell Telephone Laboratories and the Westinghouse Company. Due in part to pressure brought by NRL, these tubes were rapidly improved to go to higher and higher frequencies so that during the latter part of this period we could get excellent power out of two 20 kilowatt tubes at frequencies as high as 20,000 kilocycles.

I can mention only a few of the many activities in which the Radio Division of the Naval Research Laboratory was engaged at this time. We made a very large number of reports to the Bureau of Engineering and a good many papers were published by the personnel of the Division. Dr. Hulburt, of the Physical Optics Division, then called the Heat and Light Division, took a very prominent part in the problems of wave propagation. The Bureau of Engineering quoted some 66 of our publications and reports in their "Radio and Sound Reports" in the short interval of four years - many of them being quoted in full.

During this period we built the first high frequency sets for the Coast Guard Ice Patrol. For the first time they had satisfactory communication with Washington when they wanted to report an iceberg. With the use of intermediate frequencies they had been forced, before this, to resort to relay by way of the nearest ship. Many of the reports were dangerously delayed because no ship was near enough to receive their signals and relay them on to Washington, where they were broadcast on high power to all ships. The North Atlantic is none too easy a region for communication at any season of the year.

During this same period we built for the Army, at their request, a multi-wave 1/2 kilowatt crystal controlled transmitter similar to the XA equipment which we had used on the MEMPHIS except that, although crystal controlled, the power amplifier used alternating current at 60 cycles instead of direct current, thus broadening the tuning a little, giving it a rather rough note but making it easier for inexperienced operators to receive. We got the impression at the time that this was the first finished set of the crystal controlled type that the Army had used. They copied it and several of this type were used on the Army networks.

The first MacMillan Expedition to the north of Greenland had Lieutenant Commander Byrd, afterward Rear Admiral Byrd, in charge of the planes carried by the ship. This Expedition sailed in 1925. It utilized amateur communications in the 1500 kilocycle band, maintaining contact with this country with considerable difficulty, but much better than could have been done on lower frequencies. When the second MacMillan Expedition left in 1926, John L. Reinartz went along as operator of high frequency equipment. Besides working amateurs pretty well all over the country, he made special tests for the Laboratory. We succeeded in maintaining excellent two-way contact, even in the daytime, by using very high frequencies for those days, that is, between 17,000 and 18,000 kilocycles.

In that same year Admiral Byrd made his historic flight over the North Pole, leaving New York by ship, in April of 1926, for Spitzbergen, starting his flight with Floyd Bennett from a small island and making a round trip of 1600 miles. The only connection our Laboratory had with this flight was that we loaned Malcolm P. Hanson to the Byrd Expedition for the purpose of installing the radio equipment in the planes and some of the equipment on board the ship. Hanson begged for leave of absence to make this trip, but we felt that it would take him away from the Laboratory for such a long time that he was refused permission. Hanson couldn't resist the lure of adventure. He was sent to New York to make some last minute changes in the installations. No one noticed that he did not come ashore until the ship was well out at sea and it was too late to put him off. I think he hoped to go as radio operator on the flight over the Pole but he didn't realize this hope. Theoretically we should have fired Hanson, when he got back to the Laboratory, for disobedience of orders; actually we all had a sneaking sympathy for him and we just overlooked it.

The following year Hanson and Hyland installed the equipment for Byrd's four passenger flight to France. It will be recalled that Byrd arrived over Europe in very bad weather and had difficulty in locating LeBourget Field in Paris. He was close enough to the field of several occasions so that his motors could be heard, but the weather was very thick and his gas getting low so, rather than take any chances, he went back to the coast and deliberately dropped into the sea close to Ver-Sur-Mar, France.

I don't mean to give the impression that all the work of the Radio Division was in the field of high frequency, but so many things could be done in that field as, for instance, the use of directive antenna for sending and receiving and the accomplishment of various things already described that we did put most emphasis on that work. One low freguency job we developed and finished during this period was the differential recorder. This was my invention. The device was for the purpose of copying low frequency signals on tape recorders in such a way as to balance out to considerable extent the annoying static disturbances. This device went into some little production in the Navy, was tried out at sea on board the USS SEATTLE and at San Francisco was used for the automatic reception and relaying of Arlington time signals.

The work on radio controlled pilotless airplanes started at Anacostia was continued, under Mr. C. B. Mirick, at the Naval Research Laboratory. In the winter of 1923-1924, Mirick tested his various radio control devices by the use of a small three-wheeled cart which came to be dubbed the "electric dog". The front wheel of this cart was improvised from a small boy's velocipede. This cart could frequently be seen wandering around by itself over the streets in the Laboratory grounds, stopping and starting and turning corners under the control of an operator who was watching it from the top of a building. Incidentally, we discovered a picture of the "electric dog" recently. When Lieutenant Commander Luke's son, now an engineer at NRL, saw this picture he remarked sadly, "Yes, that was my velocipede".

Mirick developed a control switch which operated, in a way, similarly to the control stick of an airplane. Incidentally, this control stick was almost identical with that employed in connection with some German radio controlled missiles. This device demonstrated both simultaneous and independent operation of control circuits. After many tests at Dahlgren, where Mirick worked under the immediate direction of Lieutenant (now Rear Admiral J. J. Ballentine, Naval Aviation Officer at the Proving Grounds, a successful flight without pilot was made September 18, 1924. It is recalled that the Dahlgren personnel were very much worried lest the plane should get out of control and crack up over land or head for some of the buildings. The aim was to take the plane off the water, fly it around over the water and bring it back down. Lieutenant Ballentine wanted to be sure that the plane could be put out of business if anything went wrong so he had a pilot in another plane fly just over the "drone" with a load of bricks which he could drop into the propeller if the drone persisted in going in the wrong direction. The flight was entirely successful, although they had one or two bad moments when the control seemed to stick, but the plane took off too hard and cracked a pontoon which caused it to nose over and sink after landing, damaging most of the gear. In 1925 another attempt was made with a new plane and equipment. This plane was lost in an accident in the take-off for which the radio equipment was not responsible. The project was not resumed for several years.

These early experiments with the radio controlled plane used the teletype selector switch. Although this was demonstrated to be unsuitable for flight control, it was later used very successfully in the control of the two target ships, the destroyer STODDERT and the battleship UTAH.

In the mid-summer of 1925, Dr. Gregory Breit and Dr. Merle Tuve of the Carnegie Institution of Washington, approached us with the idea of cooperating with them in connection with a new method of directly measuring the height of the refracting layers in the ionosphere.

Our determination of ionospheric heights had been based upon skip distance meaurements on a number of different high frequencies. Dr. Edward Appleton (later Sir Edward Appleton), in England, had developed a method using what may be called the sliding frequency device. He used an emitted wave which rapidly changed frequency during emission, so that the fequency of the wave being emitted at the moment could be compared with the frequency of the reflected signal from the ionosphere, which came from a wave emitted slightly earlier.

Breit and Tuve proposed that we use short high power pulses, fairly well separated, to determine by direct measurement with time delay between the sending out of a pulse and the arrival of its echo from the ionosphere. On the 22nd of June 1925 after I had given my approval to the project, Dr. Breit and Dr. Tuve with Mr. Gebhard went into ways and means of producing such a high power pulse transmitter.

Mr. Gebhard, aided by Mr. Matthew Schrenk, built and put on the air the first high power, high frequency pulse transmitter which could be keyed from a trigger circuit developed by Dr. Breit. L. C. Young and Dr. Tuve in the meantime, were working up the receiver. These experiments not only started the pulse echo method of ionosphere measurements but had an extremely important bearing on radar developments a few years later. This pulse method of measuring the ionosphere is used by many ionosphere stations in various parts of the world today.

This work was done first in the 4000 kilocycle band. The receiver could be located at the Carnegie Institution for the Study of Terrestrial Magnetism, since both the ground wave and the sky wave could be received at that point. It was easy to compute the height of the ionosphere from the difference in the arrival times of ground waves and sky waves.

When we went to higher frequencies, however, the ground wave did not reach that laboratory, so the receiving work had to be done within a few hundred yards of the transmitter and on NRL grounds. Young and Tuve succeeded in working up devices that would prevent the receiver from being paralized by the outgoing pulse from the nearby transmitter. The transmitter and receiver, at this time, used separate antennae.

As early as 1925 the Patent Section of the Judge Advocate's Office of the Navy Department, then under Lieutenant Commander (now Captain) Robert Lavender, urged the Laboratory to file patent applications on new devices. This resulted in a good many members of the staff filing for patents with the understanding that highly confidential material would, temporarily at least, be held in a "secret" status, that some applications would not be accepted because they might not be of sufficient interest to the Navy, but that the remaining patents would be prosecuted by the Navy Department, assisted by the Department of Justice if necessary. The commercial rights on these patents would be in the hands of the inventor to dispose of as he saw fit. The Navy retained complete rights to use and manufacture patented devices. This system was a decided advantage to us for many years in the days of relatively low salaries and slow promotions. We were able to retain a good many competent men who might have left us to go into commercial organizations which, in those days, paid much higher salaries than did the Navy. It was probably not the best method of holding good men.

It was as a result of this situation on patents that I became acquainted with Mr. C. W. Hough, President of Wired Radio, Inc., a subsidiary of the North American Company. With the consent of the Navy Department, I entered into an agreement with Wired Radio to turn over to them my commercial rights on all patents and, to a certain extent, to act as their consultant. Mr. Hough made arrangements for acquisition of commercial rights of patents with a good many other members of our staff. Of course, patents and inventions are a big gamble and some people like this feature. However, a man who is primarily interested in research considers invention merely a side line and doesn't want to be bothered with the legal and business side of the process of acquiring, selling and defending a patent. The proposition offered to the members of our staff by Wired Radio relieved us of these annoyances and in addition to that, provided us, free of charge, with legal counsel to expedite the preparation of our cases in cooperation with the Patent Office of the Navy Department. The Navy too, profited greatly by this arrangement. Certain commercial concerns objected to the Laboratory turning over most of its patents to one organization but I told their representatives that we would be equally ready to deal with any organization which offered us equal inducements.

The Laboratory continued to cooperate, as necessary, with the exploring expeidtions, who by this time had found out that high frequency and light weight sets were capable of keeping them in touch with civilization from almost any point on the globe. We gave some advice on the proper use of frequencies to the University of Michigan for their Greenland Expedition under Professor Hobbs, whom I had known at the University of Wisconsin, but who at this time was Head of the Geology Department in the University of Michigan.

We also were in contact with the early planning of the Wilkins Expedition to Alaska which was to attempt the flight from Point Barrow over the North Pole. We advised both of these expeditions to make use of existing amateur communications for their contacts. However, we profited greatly by the immense amount of information brought in about the peculiarities of high frequencies when used in very high latitudes where the ionosphere undergoes some strange variations, especially when it is upset by the aurora.

Sir Hubert Wilkins seemed to me to be a very fine leader and certainly deserved better luck than he had on the Point Barrow expedition.

In 1927, when the International Radio Conference took place in Washington, I was appointed Technical Adviser to the Navy Delegation. This conference had representatives from 76 different countries. Many internationally known figures were present. General Ferrie was again representing France as the head of their delegation; Dr. Van der Pol was the leading technical member of the Dutch delegation and certainly one of the keenest men in attendance, I have met him repeatedly in subsequent years and hope to meet him again in the near future. He was, and still is, one of the leading engineers with the famous Philips Corporation of Eindhoven, Holland. He and many other Eindhoven people were forced to work for the Germans while they held Holland, but they managed to keep their output pretty well down by a cleverly conceived system of sabotage. We lost track of Van der Pol for some years. Only recently have I heard that he is all right and will soon visit in this country again. I remember Dr. Dye as one of the British technical advisers. He was then Head of the National Physical Laboratory at Teddington, England, an institution similar to our Bureau of Standards. A little later I arranged with Dr. Dye to transmit high power crystal controlled high frequency signals to England for a series of precision measurements for the purpose of comparing the absolute standards of frequency in this country and in England. We did this on a frequency of 20,000 kilocycles.

It was at this 1927 conference that the world went over to the designation of radio waves in terms of frequency instead of wavelength. I was one of those who most urgently put forward this idea. There wasn't much difficulty in persuading the Germans, French, Belgians and Italians, but the English who are always prone to cling to archaic systems of weights, measures and coinage, were rather bitterly opposed. Actually the Navy had started using kilocycles, instead of meters of wavelength, a year or two earlier.

It is interesting to see a big conference like this at work. The main assembly gets nothing done, indulging only in generalities. Committees are appointed to consider groups of subjects. These committees, in turn, appoint sub-committees. There are even sub-sub-committees. It is the last two categories of committees that do the real work. Their decisions are usually accepted without question by the main assembly.

I found that people from the different nations were very different to deal with. The British bluntly lay down their demands in such a way as to give you the impression that no possible compromise is acceptable. The only way to deal with them is to meet them on their own ground with an equally emphatic statement of your own stand. They will then, after a short period of deadlock, begin to indicate that there are possibilities of a truce or compromise. The French are always suspicious no matter how generously you may act towards them. They never will agree to anything without very prolonged search for the "nigger in the woodpile". When they do not find one, I think they are actually disappointed. One of the leading men in the Italian Delegation was Count Montefinale, a Captain in the Navy. He was a fine man to deal with, almost always succeeding in lining up the Italian Delegation on our side of the fence. He was a technical man too, well versed in radio communication practice and theory. The Dutch too, were always reasonable to deal with and, I thought, very fair minded. Curiously enough, the Japanese, in an international argument, nearly always lined up with the United States. Captain Isasaki, one of their leading members, had been educated at Harvard and had a daughter who had been through an American Conservatory of Music. He was a genial man and a wise one, very friendly towards the United States. I don't know what has become of him, but I am sure he would not have wanted to see his country and ours at war. Another Japanese officer, however, was of a different type. He was a forceful and versatile man, speaking English without a trace of accent; he spoke very good French and German as well. One thing that sticks out at such a conference is that the American delegates are very short on foreign languages. Van der Pol, for instance, could write technical papers with equal ease in French, German, Dutch or English. He spoke with a strong Cambridge accent, having spent several years in England. The official language adopted at this conference was French and everything had to be finally put in that language.

There were present, in one capacity or another, during this Washington Radio Conference, a good many people who were interested in radio wave propagation. I succeeded in getting a small group of these, including Sir Edward Appleton of England, Dr. Mesny of France, Dr. G. W. Pickard and Dr. Hulburt to meet informally at my home for a discussion of wave propagation problems and other things of general interest to radio people. Later I entertained Dr. A. Meisner. Dr. Meisner, according to my latest information, is still with the Telefunken Company. I found him to be a very modest but at the same time a very competent radio engineer, representing the best class of fair minded and intelligent German citizens. When my son visited him in Berlin eleven years later, just about the time of the Munich conference, he found him distinctly anti-Nazi and very much worried about the German situation. Dr. Meisner was vice president of the Institute of Radio Engineers in 1929, the same year that I served as president. While the Institute has always had an American as president it has, for a long time, elected as vice-president some distinguished engineer from another country.

I cannot leave the subject of this international conference without a brief comment on the difficulties of running a conference with a lot of European delegates in a place where prohibition is in effect, as it was at that time. We managed to hold most of our committee meetings in one of the embassies which had diplomatic immunity. It was really astonishing how much easier it was to get people to make compromises and concessions when their minds had been properly lubricated with a little good liquor. I remember in particular a very prolonged session of a committee involving General Ferrié and another officer representing France, Captain Montefinale representing Italy, and two Britishers, one a Naval captain and one a civilian, representing England. Captain Isasaki and a Japanese commander representing Japan were our hosts for the evening. Tam Craven, then Lieutenant Commander USN and recently of the Federal Communications Commission, represented, with my assistance, the American delegation. We argued in the arid state for two and a half hours without being able to get any agreement on the issues involved; in fact the proceedings got a little beligerent. The French were on the fence as usual. Japan and United States were opposing England in the argument, with Italy inclined to stay with the United States but not wishing to offend the English. Things were really at a deadlock when the Japanese brought in some really fine old French brandy. After a round or so of this lubricant it was amazing to see how the atmosphere changed - the beligerency disappeared and concessions and compromises were promptly made. Within an hour the whole difficult question was satisfactorily settled. Wishing to be sure to keep my own head perfectly clear, I indulged in these refreshments very mildly, surreptitiously attempting to dilute my brandy with water. Unfortunately General Ferrié detected me. He was horrified, shouting that such actions simply could not be allowed - that it was no less than a sacrilege and an insult to France to dilute such brandy with water. However, he finally forgave me, since before the conference ended I was a guest of honor at a dinner over which he presided.

The advent of shield grid tubes for receivers in 1927 and for transmitters in 1928, marks a definite epoch in radio. It will be recalled that radio tubes started with the Fleming valve which, having only two electrodes, a hot filament and a cold plate, can be called a diode. Dr. DeForest put in the control grid, between the hot filament and the cold plates. It was called a grid because it was an arrangement of wires something like a screen with plenty of holes through which electrons could pass from the heated cathode to the cold plate. This tube is called a triode because it has three elements within it. The grid element permitted the control of the flow of the electrons from the hot cathode to the cold plate and back through the high voltage battery or generator connected from the plate to the cathode. Control of this current by small voltages impressed on this grid not only permitted more efficient detection of incoming signals, but it permitted the tube to be used as an amplifier, thus rendering it possible to deal with much weaker signals than could be handled with a simple detector. Furthermore, with feed-back, or regeneration, which is a coupling up of the plate output circuit with the grid input circuit, it becomes possible to make the tube oscillate, thus acting as one of our most satisfactory generators of radio frequency current either for receiving or transmitting purposes. However, a very small amount of regenerative coupling will often cause such a tube to oscillate when oscillations are not wanted. Therefore there was always difficulty in using triodes as amplifiers because they were liable to start oscillating and become generators, which ruined their action as amplifiers.

The invention of the shield grid tube which must, I think, be credited to Dr. A. W. Hull of the General Electric Company, removed this hazard because when suitably by-passed with capacity and connected into the operating circuits of the tube, this shield grid would largely prevent regenerative coupling within the tube. Therefore proper shielding of external circuits could effectively suppress all tendency to oscillate.

It is true that Dr. Schottky in Germany produced a tetrode, but it would not function as a shield grid tube because the lead wires through the base of the tube to the shield grid were so long that magnetic coupling was set up within the tube, prohibiting the tube from functioning as a non-regenerating amplifier. Apparently Schottky's tetrode was produced for another purpose. Had it been properly constructed it might have functioned as a shield grid tube. Dr. Hull, however, definitely intended to produce a shield grid effect and succeeded admirably. Thus we acquired stable amplifiers, amplifiers that did not require neutralizing or balancing, amplifiers which could consist of a number of tubes cascaded one after the other and could operate at either radio or audio frequencies. Since practically all modern transmitters and receivers use such tubes, it is hard for engineers today to appreciate what a tremendous impetus the shield grid tube gives to the radio art in the late 1920's.

The General Electric Company was kind enough to give us samples of these tubes very early, even before they gave them out to the industry. Naturally we pounced upon them with avidity and immediately incorporated some of them into the transmitters that we were building for the Bureau of Engineering. At this time Commander (later Captain) Ruble was head of the Radio Division in the Bureau of Engineering. When he read my enthusiastic report as to the possibilities of these new tubes, he promptly informed the Laboratory that since these tubes were not standard tubes, we were not authorized to use them in any new design. Fortunately we paid no attention to this edict. This new transmitter worked so well and eliminated so many tricky controls that had been necessary in balancing triode amplifiers that they were promptly adopted and began to appear in all the new receivers and transmitters built for the Navy.

I think I ought to note at this point that the Naval Research Laboratory had, from the very beginning, maintained cordial relations with the Army Signal Corps and, as soon as it was established at Wright Field, with the Army Aviation Radio Laboratory. Frequent visits were exchanged between NRL and this Laboratory, as well as with the Army's Signal Corps Laboratory at Fort Monmouth. However the liaison went further than that because, in 1925, Major Maughbourne and Captain Hill were actually stationed, for some considerable time, in a room across the hall from my office, as Army liaison officers. Maughbourne afterward became Chief of the Signal Corps. Both of these gentlemen were ardent experimenters and tremendously interested in the advance of radio, Maughbourne, particularly, being a man of very great ingenuity.

This practice of maintaining a permanent liaison office was discontinued somewhat later, through no fault of ours. I am glad to say that the situation again exists today, Major E. G. Witting being the Signal Corps representative and Captain G. B. Fanning the Army Air Corps representative. The Navy has liaison officers at various Army activities such as Fort Monmouth which is now called the Squier Laboratory.

Since everyone in those days was pushing into the study of higher and higher frequencies, we soon found that substances like glass, porcelain, rubber, marble, etc. which had hitherto been considered good enough insulators, were no longer adequate. Even bakelite, so long first in the field because it was convenient, machineable, sufficiently strong and of good appearance, was worthless in strong high frequency fields such as would occur in parts of a transmitter. In a high power transmitter bakelite parts would actually swell up, burn and completely carbonize. Thus started the long and patient search for better and better insulators, a search which is still going on today as frequencies still move upward.

During the period of which I write, high grade pyrex insulators were produced by Corning Glass Works. Micalex, a British product originally, was improved and put out by the General Electric Company. We had to learn to use different types of resistors because most of the older types used in radio circuits no longer acted as pure resistors at these high frequencies, but acted as though they had either inductance or capacity associated with them. All types of capacitors had to be redesigned and reinsulated. The wire used in winding coils underwent a radical change. Up to 1200 kilocycles or thereabouts, very finely stranded copper wire reduced the high frequency losses and performed much better than solid conductor. But for frequencies of several thousand kilocycles we soon found it far better to go back to solid copper conductors. Since, however, these very high frequency currents travel entirely in the skin of the conductor, it was unnecessary to use a solid conductor because a hollow one did just as well. Thus many radio frequency coils, used in constructing receivers and transmitters, were wound of thin copper tubing and in some cases, in the transmitters, these copper tubing coils were water cooled.

In all of these matters the industry cooperated with us hand and glove. We had to push some of them into the game by assuring them that if they got good products we would only be too glad to test them and that the rapidly opening future of radio would find an excellent market for new and better high frequency components. Not only did we get cooperation from Westinghouse, General Electric Company, RCA and Bell Telephone Laboratories, but from many other institutions too numerous to mention.

In the year 1927, Mr. Young and I became very much interested in round-the-world radio signals and long distance echo signals. At certain hours of the day radio signals on frequencies that are very commonly used in long distance communication, say between 12,000 kilocycles and 21,000 kilocycles, will arrive at a receiving station from two directions, one of them being the shorter great circle route to the transmitter and the other a longer path around the other side of the world. Thus, if a station at Lisbon is being received in Washington, the direct signal comes in from a northeasterly direction. It is necessary to look at a globe rather than a map to see that this is true. At certain hours of the day it would often occur that another signal would come in from the southwesterly direction. Having traveled very much farther to get here it would arrive as much as one tenth of a second later than the direct signal. Thus if the station was sending dots and dashes with a fair speed, a lot of extra dots and dashes would come in between the dots and dashes of the message, hopelessly scrambling it.

It might be thought that the message coming over the greater distance would be so weak that it would not be very annoying but, curiously enough, conditions frequently arise which make that longer path signal louder than the direct signal. The use of highly directive receiving equipment can exclude either one of these signals and prevent them from interfering with each other. At these frequencies such equipment is not practical to install on shipboard.

In the process of studying these effects we actually measured signals of very excellent intensity which had traveled twice completely around the world. This was accomplished by setting up our own crystal controlled 20 kilowatt transmitter on 20,000 kilocycles and equipping it with an arrangement for sending relatively short pulses or dots. We then got permission to set up a receiving arrangement at the Cheltenham Magnetic Observatory, about eleven miles from the Naval Research Laboratory grounds, right out in the open Maryland countryside. Receiving conditions were particularly good because no electric wires were permitted to come anywhere near this observatory. Therefore obnoxious interferences were at a very low level. Since we were only using receiver equipment operated from batteries, we were able to assure the Observatory that the very small amount of power that we needed would in no way affect the Observatory installations. This was substantiated by actual tests. Thus we were able to send signals at convenient intervals and carry out systematic experiments using directive receiving antenna at Cheltenham.

Based on the fact that the signals which had traveled twice completely around the world and come back to the Washington area were still strong enough to actuate a receiver with no great amount of amplification, I predicted that it should be possible to get signals reflected from the moon. We even made half-hearted attempts to do this on the frequency of 32,000 kilocycles, using a fixed directive antenna which was pointed broadly upward. We took only a few shots because we had to wait for intervals when the moon came into the right position. We didn't have enough power to get through, only a matter of 500 watts. We couldn't use the 20 kilowatt set because its frequency was too low for a conveniently sized directive antenna. About that time the depression hit us, with curtailment of all finances. We didn't feel justified in diverting time and money to what most people of the Navy considered a hairbrained experiment. I am glad that the Army, a few months ago, succeeded in actually receiving signals sent to and reflected from the moon. That work was done at the Camp Evans Laboratory at Belmar, N.J. This is the same station of which I was Commanding Officer for the Navy in World War I.

In the process of these investigations, Mr. Young and I discovered not only the signals which traveled clear around the world in .138 seconds, (between 1/7 and 1/8 of a second) but a lot of other echo signals of much shorter duration, but longer than would be expected for reflections from the ionosphere. These we called splash-back signals, based on the theory that they came back from the first, second, third, etc. zones of reflections of a high frequency wave with a long skip distance. These results were published in the Proceedings of the Institute of Radio Engineers in two papers, one in May 1928 and the other in September 1929.

While we were carrying out these experiments, we were assisted from time to time not only by Chief Radioman Wiseman, who was permanently attached to my staff, but by young petty officers detailed for part time duty from the Radio Materiel School, which had been established at the Naval Research Laboratory in 1924. Now these experiments were carried on in a small wooden building, well out on the edge of our reservation in those days. We had no janitor service, so we all took turns at cleaning up. I was personally much interested in this work and therefore spent a good deal of time at the field house. One morning I went out at about 9:00 o'clock, finding Young and Wiseman busy making some slight changes in the 20 kilowatt transmitter in order to get it ready for some special tests. While I was waiting for them I picked up a broom and began sweeping the place out. Suddenly, a young radio man appeared in the door and asked if this was the radio field house. I told him: "Yes, come on in". This young man had never seen a water cooled tube or high power transmitter and hadn't had much to do with high frequency. He stepped up to the 20 kilowatt set and began looking it over with a good deal of awe. Seeing that he was interested, I walked up to him, still holding my broom, and went over the different parts of the set, explaining what they were and what they accomplished. Shortly thereafter the Chief took charge of him and put him to work. When this young man reported back to the Officer-in-Charge of the Radio Materiel School he remarked, with awe in his voice, "What kind of a place is this anyway? Even the janitors know all about radio". This young man was Arthur Godfrey, the well known radio announcer. I hope he remembers the incident.

Admiral Byrd's expedition to "Little America" and the South Pole sailed on the 16th of September 1928. Malcolm P. Hanson was loaned to the expedition by the Navy and for a long time prior to the date of sailing was busy with installing and gathering equipment. Hanson remained on the payroll of the Laboratory. This expedition gave us an excellent opportunity to study wave propagation on various frequencies in a part of the world for which there were very little high frequency data available. Some data on the 3500 kilocycle band and on medium frequencies had been obtained by the Ross Sea Whaling expedition.

The Naval Research Laboratory had regular schedules with the ships going down and coming back as well as regular schedules with the Camp at "Little

America". An immense amount of information of value was obtained. The frequencies studied varied from 8000 to 30,000 kilocycles. During the seasons of the year and hours of the day when the signal path was largely in daylight, our high power 20,000 kilocycle set proved most effective, with an equally high frequency at the Little America end.

It is perhaps worth noting that Byrd's planes were also equipped with radio. While he was making flights in the neighborhood of the South Pole itself, some 600 miles from the base, the signals were picked up and relayed directly to the New York Times Station in New York. In this particular test we were not involved but listened with interest.

When the two ships of the expedition, the NEW YORK, an old whaler and the ELEANOR BOLLING, a small steamer, were on the way back from New Zealand, they encountered extremely calm weather near the Marquesas and for weeks couldn't get out of the doldrums. Of course the BOLLING could have gone ahead, but she didn't have coal enough to make Panama with the NEW YORK in tow. The result was that they stuck it out for weeks before reaching Panama. This was probably an uncomfortable period for those on board but offered us opportunities to make all sorts of experiments.

Our three o'clock schedule was greatly bothered by round the world signals which interfered with reception. All we had to do at out end was to put up a directive antenna which enabled us to receive Hanson satisfactorily, but he had a bad time on the NEW YORK. We finally recommended that he run a long wire, a few feet above the deck the full length of the ship, receiving from one end of it. When the ship happened to be headed in the right direction, this gave him a certain amount of directivity and greatly improved reception. Usually by half past four or five this situation would clear up and we managed to get through pretty regularly. The NEW YORK was then 3000 miles from Washington.

We managed to rig up a voice attachment on rather low power and tried radio telephony one way quite successfully, sometimes getting through on as high as 28,000 kilocycles. One day the Captain of the NEW YORK was asleep in a chair up on deck where he could get the first breath of a breeze. Hanson ran an extension cord to a loud speaker, placing it behind the Captain's chair. He then instructed us to call the Captain and tell him the breeze was on the way. So I called: "Hello NEW YORK CAPTAIN. Washington calling - there is a breeze on the way". The Captain had never had anything to do with long range radio telephony. He woke up with a start, staring around the deck, thinking someone on board had been playing a joke on him.

At this same time I arranged to have Mrs. Hanson come down to the station at the Naval Research Laboratory, bringing Hanson's first son, who had been born a few months after Hanson left the United States. We didn't tell Hanson ahead of time. The communication was a little difficult that day but finally we connected at about 3:15 in the afternoon. I asked Hanson to stand by for voice communication. His wife then talked to him and asked him to wait a minute, following which she got the baby close to the microphone. Now this little rascal had been prattling around in great style while we were getting ready, but when once in position to do his stuff, he refused to make a noise of any sort. Finally in desperation, Mrs. Hanson pinched him a little, whereupon he set up a lusty squall. Hanson came back immediately in code: "Is that Malcolm Jr. I hear"? It wasn't every man in those days who could hear his son's voice for the first time from a distance of 3000 miles. I thought this yarn would be a good publicity story for the newspapers but Captain Theleen put his foot down and "wouldn't listen to any such nonsense". The Navy always had the reputation of being a silent service but sometimes I think it was too silent for its own good.

During this same period Mr. Mirick and Mr. MacGregor of the Aircraft Section built the first temperature controlled crystal controlled transmitter for heavier-than-air craft. At approximately the same time the first transcontinental reception from an aircraft in flight was carried out. The test was arranged with Lieutenant Palmer, who flew from North Island Air Station near San Diego, communicating with us using a frequency of 3500 kilocycles, transmitted by the first airborne crystal controlled set called the ME, which went into production by Westinghouse. This had to be a night flight, since this frequency was not at all suitable for daylight communication. It naturally attracted a good deal of attention, since it demonstrated the immense distances which it was possible to cover from an aircraft equipped with a relatively small low power transmitter.

The introduction of high frequency radio in the Navy was of particular interest to the submariners. A submarine, which must be ready to crash dive at any moment, obviously cannot carry large massive transmitters and elaborate radio antennae. During World War I, our submarines were equipped with transmitters operating in the middle frequency band, between the low and high frequencies, that is, approximately between 200 and 1200 kilocycles.

Unfortunately, the restricted conditions on board a submarine prevented the use of antennae of adequate size to give these ships sufficiently long range communications. Moreover, there wasn't room in the crowded interior of a submarine for high power radio equipment. As far as receiving was concerned, the submarines were in a better situation because, thanks to adequate amplifiers, a comparatively small antenna would bring in comfortable signals on almost any frequency. On the whole submarine communication up to 1927 was distinctly limited and, from a military point of view, extremely unsatisfactory. From the very nature of their work, submarines commonly make very long cruises, often widely separated from other Fleet units and they really require extremely long range communication facilities. Since the high frequencies can get out to immense ranges with very low power and very small antennae, they offer obvious advantages.

In 1927, Mr. A. M. Trogner and Mr. R. B. Meyer of the Naval Research Laboratory designed the XE transmitter which put out approximately 200 watts at various frequencies between 2000 and 18,000 kilocycles. This was done at the request of the Bureau of Engineering after conferences in which the power, dimensions, frequency range and general performance of the set were outlined. These sets were intended for the V boats, that is the lagger size submarines. The Bureau gave us, supposedly, the exact dimensions of the hatches so that there should be no difficulty in getting these sets inside of the ship. These sets were put into small scale production by the General Electric Company in 1928 and in the early summer of that year Mr. Meyer proceeded to the Mare Island Navy Yard (San Francisco) for the purpose of assisting in the installation of the first two sets on the V1 and V2.

Apparently insuperable difficulty was encountered at the outset when it was found that the actual dimensions of the V boats' hatches were some two inches smaller than the dimensions given by the Bureau of Engineering. It was impossible to reduce the dimensions of the transmitters without entirely rebuilding them.

These ships were part of the command of Admiral Ridly McLean, formerly mentioned as Director of Naval Communications, but who at the time in question was on the HOLLAND, the mother ship of the Pacific submarine outfit. His radio officer was Lieutenant Harry Hill, an officer with very wide knowledge of radio and particularly well informed concerning its application to submarines.

As I have pointed out earlier, Admiral McLean had from the start been intensely interested in high frequency. Harry Hill was decidedly a high frequency "fan". The Admiral decided that those sets had to go aboard the V boats somehow. They were gotten aboard by removing the soft patch over the engine room. Down below, however, Mr. Meyer was faced with the problem of lugging this set, weighing over 500 pounds, along the catwalk of the engine room with an overhead clearance of not over five feet. This was accomplished by a gigantic rigger from the Mare Island Yard, who took the load on his back and crawled the length of the engine room catwalk on hands and knees with two men steadying him, one in front and one in the rear. When they arrived at the after bulkhead of the engine room, the set was too big to go through the door, so blow torches had to be brought into play to cut an opening for the passage of the transmitter. To get from the control room to the radio room, they had to chisel the combing loose from the deck plates in order to provide access. From there it was lowered into the radio room.

Mr. Meyer completed this installation while enroute to Honolulu. Various tests were run while ships were lying in Lahina Roads, including two-way tests with the Naval Research Laboratory both by day and by night. This was done with the ships on the surface. Limited range communication up to 15 or 20 miles was also possible while the submarines were running at periscope depth. These were possibly the first experiments in the Fleet itself with the small so-called periscope antenna which need not be unrigged when the ship makes a dive. Lieutenant Hill was largely responsible for this antenna.

In the meantime, the Laboratory was busy with efforts to obtain communication from submarines completely submerged including emergency communications from a submarine lying on the bottom in 100 feet of water. It should be noted that the action of salt water is such as to not only nearly short-circuit the radiating antenna when immersed but to heavily absorb the energy which is radiated into the water. This absorption increases rapidly as the frequency increases. Very limited range communication might be theoretically possible if the submarine could use huge loops and a tremendous amount of power on very low frequencies. Unfortunately, the submarine is just the kind of ship that has no room for these things.

Although I realized that high frequency would suffer a tremendous absorption, it seemed worthwhile to make a few experiments based on the assumption that if we could get even a small amount of high frequency energy above the surface, it would proceed to the ionosphere and down again to a ship at a very considerable distance without actually having to pass through the water itself, except in the initial process of getting away from the submarine. In other words, if we had a submarine 20 miles away from a surface ship, it would not be necessary to send high frequency signals 20 miles under water. We might be able to send them through a few feet of water from a submarine which was under but near the surface. Once the signals broke through the surface, the ionosphere would see to it that they would reach the ship in question.

In 1928 and 1929 we were allowed to use the submarines S28 and S29, operating out of Key West and in the neighborhood of the Dry Tortugas. Chief Radio Electrician Tom Marshall was sent down by the Laboratory with a soap-box model of a small 50-watt set capable of operating from 4000 to 17,000 kilocycles.

The problem of an antenna was a severe one. These ships at that time had heavy insulated wire loops, commonly used for mid-frequency transmissions. These cables ran up over the A frames, were grounded at the ends and had leads through water tight stuffing boxes into the radio room. Although not ideal for high frequency work, it was determined that these antennae were worth trying. They were very sturdy, being lashed to the clearing lines, and didn't have to be unrigged when diving.

When ready for the tests the ship would start operating while on the surface. We had no difficulty in establishing two-way communication with the Laboratory day and night, although the distance was 1000 miles. Naturally we used the 12,000 and 16,000 kilocycle bands in the daytime and the 4000 and 8000 kilocycle bands at night. As contact was established, the ship would slowly submerge by winding up on the anchor chain. During this time the ship had a little positive buoyancy so that the depth could be exactly regulated. The signal diminished very rapidly as soon as the top of the A frames was flush with the water. At three feet under water the signals would practically vanish. It was possible, however, for these ships to communicate with Washington when the periscope was housed and everything was a few feet under water.

Further tests between the two submarines showed that they could communicate with each other on suitable high frequencies over distances of as much as 50 miles, both ships being just barely submerged. Under certain conditions such communication could be extremely valuable.

For rescue operations, that is getting signals out of a ship which lies helpless on the bottom, it was soon apparent that it was highly desirable to have some part of the antenna at or above the surface of the water. One scheme was to rig a small vertical antenna with a float, the whole thing weighted so that when it arrived at the surface it would be substantially upright. A flexible rubber covered antenna uncoiled from the deck when a catch was released and the buoy floated up to the surface. We had considerable success with this device. For a time it was regarded by the submariners as a first rate emergency communication system. We also tried a sponge rubber snake, with a wire in the core, which floated on the surface and was connected with the ship by a long flexible cable in the same manner as the buoy had been. This gave moderate ranges but was not as satisfactory as the buoy. Needless to say one couldn't expect the power of the set to pass up to the antenna through 100 feet or more of cable immersed in salt water without experiencing considerable loss. Nevertheless enough got out to be very useful indeed.

A person who has never had experience on board a submarine can hardly realize what a complicated and crowded mass of machinery fills her hull. In those days the radio room was so small that it was extraordinarily difficult to service the sets once they were installed. The layout of the installations were generally planned in the Bureau of Engineering after due consultation with the Bureau of Construction and Repair on all matters concerning ships' spaces.

The Bureau of Engineering requested us to make some full scale imitation booths in accordance with plans of various submarine radio rooms. We made these models out of wooden frame work covered with wire screening. Several of these models were located out in the field near one of our buildings.

One day officers from the Bureau brought down a load of light wooden replicas of the various radio and sound equipments which, their plans showed, could be installed in the radio room of a certain submarine. After we had fitted all these units in place, it was found that the only way the operator could be put into place was to build him into the room and seal him up tight so that he could never get out. All through the years we had to fight a battle for space and weight allowances for radio and radar equipment for submarines as well as for aircraft. In these days the allotted spaces are much larger but on the other hand, the number of gadgets has greatly increased so the radio and radar compartments in both airplanes and submarines still look pretty crowded.

Due to international and national competition for radio frequencies and a never ending struggle of commercial and military interests for the exclusive use of frequencies, the Navy had to make many readjustments of its frequency assignments and was always looking for new frequencies to be exploited in order to meet the ever increasing demands of the military branches for more and more communication facilities.

At the Laboratory we knew that the frequencies above 25,000 or 30,000 kilocycles had rather restricted use due to the fact that they were not all turned down to the earth again by the ionosphere.

There was an increasing demand in the Fleet for more channels of communication of very limited ranges from ship to ship. We realized that this demand could be satisfied by the use of frequencies much higher than those hitherto employed in the Fleet. While these frequencies would be of very limited range, involving distances not much greater than horizon distances, it was already evident that these same frequencies would have much farther ranges if used for communications to and from airplanes for the simple reason that the airplane usually flies high enough so that its horizon distance is very much greater than a horizon distance between ships. It must be realized that the limitation of horizon distances is due to the curvature of the earth. If we lived on an infinite flat plane, as some of the ancients believed, radio communications would behave very differently indeed.

From what has preceeded it should be clear that high frequencies are able to evade the horizon distance law because they can be reflected or refracted from the ionosphere, thus reaching many thousands of miles beyond the horizon. If however these frequencies are too high, they will not be reflected back to the earth but will escape to outer space. At the same time these very high frequencies may be used for very limited range communication between points on the surface of the earth and over much more extended ranges between airplanes and points on the surface of the earth.

The 1927 International Convention classified all radio frequencies below 100 kilocycles as low frequencies; all frequencies between 100 kilocycles and 1500 kilocycles as medium frequencies; all frequencies from 1500 to 30,000 kilocycles as high frequencies and frequencies above 30,000 kilocycles as super-frequencies.

Up to 1924 the low frequency band had been the standard band for both Navy and commercial long haul and international circuits. The band between 100 kilocycles and 1500 kilocycles included the present national broadcast band between 500 and 1000 kilocycles; the standard shore station compass frequency of 875 kilocycles; the standard 500 kilocycle ships' frequency which was also the international distress frequency, and a large number of assignments in all nations for military purposes. The Navy had many frequencies in use on shipboard between 175 and 1500 kilocycles.

After high frequency was adopted by the Navy, some of its older channels were surrendered because they were not so well adapted to the ships' need as the high frequencies and because various other interests were anxious to get them. Up to 1928, the super-frequency field had been very little exploited. Therefore the Laboratory proposed to the Bureau of Engineering that we build two transmitters, covering part of this band, which could be sent to the Fleet for test and approval.

Since in this discussion frequencies are going up so as to make quite a mouthful of kilocycles, I will change the terminology by often referring to frequencies in megacycles instead of kilocycles. Since the megacycle is a million cycles per second and a kilocycle is only 1000 cycles per second, this means that instead of saying 30,000 kilocycles we can say 30 megacycles.

The Laboratory contacted two of the industrial radio corporations in regard to building these transmitters. They both stated that they had so little experience in this field that they preferred not to undertake the work. We therefore, proceeded, in May of 1928, to build two "XP" transmitters covering the range from 14 to 75 megacycles. These transmitters were crystal controlled and used intermediate stages as frequency multipliers. This work was under immediate direction of R. B. Meyer who also supervised their installation on the TEXAS and CALIFORNIA about the 6th of December 1929.

It will be noted that the frequencies of these sets overlapped the high frequency band and were capable of coping with a wide variety of communication conditions. These sets were an immediate success, but the Fleet, in the interests particularly of communications with aircraft, requested that they be supplied with a modulator unit which would permit them to transmit voice as well as code. The aircraft carrier SARATOGA and the battleship WEST VIRGINIA received two XP-1's; the LEXINGTON a later model known as the XP-2. Later the XP-3 was designed for carriers, with an extra band covering 6.6 megacycles to 8 megacycles put on especially in the interests of aviation communication. These sets all had voice modulation. Some little time after the SARATOGA got her set, she was sent down to Guantemala to take part in rescue work incident to an earthquake. While she was down there she communicated very successfully by voice with the Naval Research Laboratory.

The Laboratory took advantage of the presence of these super-frequency transmitters in the Fleet to collect a lot of information on the attenuation of the ground wave over salt water which, as expected, turned out to be very much lower than the attenuation over earth. This was of great interest in connection with out never ending studies of wave propagation phenomena.

The battleship UTAH made a good will trip to South America in 1928, the most distant port being that of Montevideo, 5700 miles from Washington. This ship carried a good many notables and a large number of newspaper men so that it was necessary to handle a large volume of official and commercial traffic. This was all done on high frequencies, official traffic going directly to the Navy Department which controlled the transmitters at Arlington, and most of the commercial traffic to the RCA station at Tuckerton, N. J. The very successful communication maintained on this trip, in comparison to what had been obtained on earlier trips with low and medium frequencies, made a great impression on the Navy Department. The Laboratory assisted the Navy Department from time to time in handling difficult signals because of more favorable receiving conditions existing at the Laboratory than at the Navy Department.

By this time the skip distance and reliable range tables, published by L. C. Young in the McGraw-Hill Radio Handbook, had been distributed widely in the Naval Service as a guide to high frequency radio communication. Thanks to a vigorous, at the same time practical, research program on the part of the Naval Research Laboratory, by the end of 1929 the Navy had adopted high frequency communications on a large scale for shore stations, surface ships, submarines, airplanes and dirigibles. Some super-frequencies had also been adopted by planes and surface ships.

During the first six years of the Laboratory's existence, 1923-1929, the Laboratory had had three directors, Captain E. L. Bennett, Captain Paul Foley and Captain D. E. Theleen; none of these directors had been in residence. The main burden in launching this new Naval enterprise had fallen upon Commander E. G. Oberlin, now Captain USN Retired, whose office was at the Laboratory. The Laboratory had had four successive executive officers, namely: Lieutenant H. P. Sampson, Lieutenant Commander H. H. Little, Lieutenant Commander Benjamin Pearlman, and Lieutenant Commander Alex D. Douglas. All these officers now hold the rank of captain.

Between 1926 and 1929 an officer was appointed to be in charge of general plant facilities and the machine shop. The first incumbent was Lieutenant T. L. Ryan (now Commodore) who is well remembered by many of the old timers in the Radio Division for his cooperative spirit, energy and ingenuity. Since such a shop is not a production plant and seldom has two jobs of the same character, the personnel manning it must have unusual ingenuity and ability. The able cooperation of the Shop Superintendent, Mr. George Jacobson, and of machinists like Emil Kaiser, W. H. Dyer, J. O. Payne, C. R. Richmond, W. L. Foster and E. E. Brock was as important to the progress of work in the Radio Division as the efforts of the engineers and scientists.

The practice of having especially trained technical officers associated with the divisions was not started until May of 1929 when Lieutenant John F. Madden (now captain) reported for duty in connection with certain activities connected with submarines. However, Lieutenant Sampson, the first executive officer, really acted in this capacity for both Sound and Radio activities.

The Radio Materiel School has been closely associated with both Radio and Sound Divisions since it was founded in August of 1924. The first Officer-in-Charge was Lieutenant W. M. Tinsley (now Commander, Retired) who was followed by Lieutenant Martin Dickinson (now captain). Lieutenant Clarence Bence was the first Officer-in-Charge of the Warrant Officers Radio School. There was very close cooperation, in the early days of the Laboratory, between the School and the Radio Division. This was certainly of great mutual benefit. Graduates of this school went out to the Fleet with information on the latest equipment and on what was likely to come in the way of future equipments.

There had been some changes in the Radio Division. We had had a small increment in personnel so that by the end of the period covered by this chapter we had about 33 physicists and engineers. Dr. J. M. Miller, to our great regret, resigned in 1925 to go with Atwater Kent as Chief Radio Engineer. Many years later he returned to the Laboratory. He was superseded by Dr. Lynde P. Wheeler, a very able physicist who came to us from Yale University. Dr. Wheeler, left us in the early 30's to go with the Federal Communications Commission and retired only a few months ago. He has been intimately connected with the affairs of the Institute of Radio Engineers as president, member of the Board of Directors and member of important committees of that Institute.

Since the Laboratory started to function in a post-war period, finances, right from the start, had been very tight and, indeed, since we were entering a depression in 1929, they were still tight at the end of this period. Had it not been for the encouragement and support of the Bureau of Engineering, the Laboratory would not have been able to open at all, much less continue to function. The direct appropriation from Congress was small, little more than covering the overhead and operating expenses. Moreover the new Divisions which had been established, namely, the Division of Heat and Light (later called Physical Optics), Chemistry, and Physical Metallurgy had to be supported from the direct NRL appropriation. Thus the Radio and Sound Divisions were entirely dependent for support on the Bureau of Engineering which, in spite of the unhappy state of its own finances, saw to it that the Radio and Sound Divisions at the Laboratory were kept going. Perhaps it is futile to speculate on what might have happened if during this period we had had five or ten times as large a technical force at work with suitable support in the way of equipment and shop facilities. Personally, I am certain that such an investment would have paid off many times over during World War II.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER XII - 1929-1933

NEW DEVELOPMENTS AND SUPER-HIGH FREQUENCIES

CHAPTER XII

1929-1933 - New Developments and Super-High Frequencies

During this period radio investigators were getting into more and more desperate straits for lack of suitable insulators which would stand up under the influence of strong high frequency fields. At this time we had Pyrex which even at reasonably high frequencies stands up well, but it had to be cast or molded. It was very difficult to produce it to the exact dimensions which might be required. Moreover, it could not be machined or worked up into small parts for special purposes. We also had Micalex, previously mentioned. To some extent this could be machined but was not suitable for making up into small parts. Neither was it as good an insulator for high frequencies as we needed. We also had quartz, both of the crystalline and fused form, but although this substance is one of the best of insulators, even at very high frequencies, it is so hard that it is impossible to machine it at all except by long drawn out grinding operations. In the desperate search for a new high frequency material, we were ready to try almost anything.

An interesting incident occurred in connection with this matter. It led to the development of a substance which, temporarily, at least, enjoyed great popularity not only with the Naval Research Laboratory but with the commercial organizations who were equally concerned with its use. Later this substance was supplanted by still better insulating substances so that it is no longer widely used. The possession of it in the early 1930's permitted us to go ahead in the high frequency field so that its development, although not leading to a permanently useful product, was of a very great importance to the Navy and to the industry. This substance was finally named Victron.

The discovery of the properties of Victron came about in this way. Dr. Bichowsky, then head of the Chemistry Division at the Naval Research Laboratory, visited the Naugatuck Laboratories, a subsidiary of the U. S. Rubber Company. One of their chemists showed him a translucent substance which could be produced in a great variety of colors. It was a bi-product of the manufacture of alcohol and ought to be useful to somebody except that it was a little too expensive to use for hair brushes and tooth brush handles and such gear. Also it had a rather low melting point. Bichowsky asked if they had tested the properties of this substance as an insulator. They replied that they had no facilities for making such tests. Bichowsky then asked for a few samples to turn over to the Radio Division at the Naval Research Laboratory, since it was known to be on the lookout for anything new in the way of insulators.

Our tests on this product showed it to have lower high frequency losses, that is less heating effect, than any insulator that we had so far tested except quartz. However, its low melting point was a decided objection to its use in high frequency transmitters which were bound to get pretty warm. Moreover it wasn't as strong as such an insulator should be.

We immediately contacted the company's engineers asking for a conference to see what we could do about remedying these difficulties. The result was a long series of joint experiments. The company impregnated this material with various substances such as powdered quartz, mica and other things to see if they could raise the melting point and give it more mechanical strength without destroying its excellence as an insulator. Curiously enough, the most satisfactory substance for impregnation turned out to be lithopone, although this substance of itself is a very poor insulator indeed. This gave the material a somewhat higher melting point which, although not fully satisfactory, made the substance useful. At the same time the impregnation improved the mechanical strength. So Victron was born and in the process changed its color from a pale translucent amber to white. This substance was easily machined and the company engineers were able to supply us with sheets of varying thickness as well as with rods which could be made up into whatever forms we desired. While it didn't have either the resistance to high temperatures or the mechanical strength ultimately desirable, it was a very important and valuable stop-gap, permitting progress in the developments in high and superfrequency fields which might have been stalled without it. Of course further experiments with non-machinable insulators such as isolantite, steatite and other ceramics were vigorously continued, again with the close cooperation of the industrial agencies.

This continual pushing into new fields required constant effort in the development of new instruments. A radio engineer needs for test purposes an instrument called a signal generator. This is really a low power but very precisely calibrated transmitter which can be used to test and calibrate receivers and other gear. There were no such instruments available for the higher frequency fields in which the Laboratory was already working.

We were fortunate in being able to push some of the instrument makers into this field. We were obliged to build some instruments ourselves to prove that such instruments were actually feasible. Melville Eastham of General Radio and the members of his staff cooperated very closely with us after I made a special visit to Cambridge, Massachusetts and applied all the persuasion of which I was capable. Ferris was already in the field of signal generators so that company also began to push into the high and super-frequency fields. Many special contracts were let with such people for the sole purpose of getting them interested. I know that this helped not only the Navy, but the whole radio industry. Mr. Eastham used to say it didn't seem that he could ever satisfy us because, by the time he got one signal generator designed, we already had on the board another one of much higher frequency. I am sure today he would be the first to admit that we were the first to see the tremendous demand that would come from all directions for such instruments.

I have mentioned in a previous chapter the investigations which we carried out in connection with high frequency hazards which might come from sparks, induced by high frequency transmitters, which could in turn set off explosives or gasoline. By this time this particular ghost was pretty well chained. Proper procedures and countermeasures had been developed so as to render this hazard far less than other hazards which are daily faced by Naval ships.

There was another type of hazard which created some worry at this time. It had been noticed by all of us who worked in close proximity to high power high frequency circuits that our hands and arms, reaching out to make adjustments in the transmitter, would get very hot although not in contact with any circuit. Further than this, we noticed that those engineers who worked for hours in the neighborhood of such high power equipment, often complained of slight headaches and a feeling of lassitude. Dr. Whitney, of the General Electric Company, about this same time pointed out that exposure of the human body to high frequency electro-magnetic fields could produce an artificial fever and recommended this for the treatment of certain diseases. The medical profession was definitely interested. It wasn't clear whether these induced currents in the human body, which were not a result of contact with the circuit but were transferred to the body by the process of induction or coupling, were solely heat effects or whether there was some abstruse electrical effect in addition.

The Navy has always been anxious to minimize the exposure of its personnel to unnecessary hazards. We therefore asked the Navy Bureau of Medicine and Surgery to cooperate with us in some practical tests.

At this time we had a new type of water cooled transmitting tube, made by the General Electric Company, which was capable of putting out approximately 30 kilowatts of high frequency energy at 50 megacycles. If this energy was fed into a vertical rod a quarter wavelength long (approximately 5 feet) it was capable of creating a very strong field of electrical force anywhere within a range of 10 or 20 feet. Indeed, if the top of this antenna were touched with an insulated metal rod which was subsequently withdrawn, it would burst into a brilliant flame which streamed off into the air. Unless this discharge was started in this manner there was no visible effect. Engineers working in the vicinity of this equipment experienced headache and lassitude within an hour. The measurements made by the doctors from the Bureau of Medicine and Surgery showed that their body temperatures went up sometimes to as high as 103°. The Laboratory furnished a number of engineers, including Dr. L. P. Wheeler, then assistant Superintendent of Radio, Mr. L. C. Young, Mr. O. C. Dresser and several others to act as human guinea pigs. Some real guinea pigs and rabbits were also provided. The latter were exposed to very strong fields and were pretty thoroughly ruined. The outcome of the investigation showed that these effects were not permanently disabling to a man of normal health. Indeed, curiously enough, some of the volunteers stated that although they felt miserable the day of the test, they felt very much better than normal the following day. Local temperatures in the arms, if the hand held a short metal rod, could be raised to as high as 107°, although this rod was not closer than 10 feet from the antenna.

It was finally concluded that with properly shielded transmitters there was no appreciable hazard to Naval personnel. I am glad to say that none of our human guinea pigs showed any unpleasant after effects as a result of these experiments. Today machines to deliverately produce this effect on a regulated scale are commonly used by both doctors and chiropractors.

As these machines have come to be widely used they have developed a real problem for the Federal Radio Commission (later Federal Communication Commission) because they are in effect transmitters which violate the law, since they operate without licenses and without keeping to assigned frequencies.

Mr. Young and I made exhaustive experiments with one of these machines using it as a transmitter to communicate satisfactorily from Washington to Seattle. I have without great difficulty picked up by radio receivers in Washington machines which were operating in Europe. The European machines used 50 cycles supply current, whereas all American machines at that time used 60 cycles except a few on our West Coast which used 50 cycles. A directional receiver would readily differentiate between these West Coast machines and those of Europe.

It was definitely proved that these machines could operate without interference with world wide communications if they were operated in a shielded room. The shield could be made of copper screening and still have good ventilation in the room. Suitable filters had to be put in the alternating current supply leads to the machine in order to prevent the high frequency currents of the machine from backing up into the service lines for electrical supply and finally radiating out into space.

Harvard University cooperated with us in a test which proved that this could happen. We heard in Washington a shielded machine at Harvard without difficulty, using a not particularly sensitive receiver. With suitable filters in the supply line the signals could not be heard. The circuit of the machine used in these tests was provided with a key by which identifying dots and dashes could be transmitted.

For machines used in hospitals this Laboratory had always recommended complete shielding and filtering of supply leads. Unfortunately, however, this doesn't solve the entire problem since many of these machines are portable and the doctors like to bring them to the homes of bedridden patients where treatment is required. The legal question arises as to whether or not it is in the public interest to improve communication by suppressing portable machines. There is a way out of this. By international agreement a set of precisely fixed frequencies could be allotted to these machines so that they could be operated without shielding, but this in turn means a more expensive machine. Naturally this is vigorously opposed by the manufacturers of these devices as well as by the customers. Nevertheless I think this is the final answer to this problem.

Communication at this time advanced tremendously, both in the Navy and in commercial circles. The point had been reached where simultaneous transmission and reception was a necessity rather than a convenience. The commercial centers, like RCA and Bell Telephone Laboratories, solved the problem by physically separating the transmitter and receiver sites by a good many miles, but the Navy was very slow to follow this procedure because, it must be remembered, we were in a depression period. Personnel in the Navy was at a low level and money was scarce; it would require extra personnel and more money to operate a separate receiver site. I had steadily kept pressure on the Navy since 1919 to adopt the separate receiving center with remote control of transmitters but it was a good many years before I succeeded in getting the backing necessary to put it over.

On board ships the condition was difficult indeed, since adequate separation simply could not be obtained. However, we did accomplish a great deal by localizing the receiving antennae which, thanks to amplifiers, could be small in dimensions, in one end of the ship and the transmitting antennae in the other end. Suitable filtering circuits and the right type of receivers soon permitted us to do a great deal of "duplexing". This means the operation of transmitters and receivers simultaneously although not necessarily on exactly the same frequency. The speeding up of communications resulting from the Navy's adoption of duplexing operations on large ships was a notable achievement.

Mr. L. A. Gebhard made an extended trip on the ARIZONA in March, 1931 when she cruised to Puerto Rico and the Virgin Islands. He checked up on the performance of her radio equipment and arranged for duplexing. President Hoover, who was on board during this cruise, inquired whether he could be put in contact with his son in Asheville, N.C. where he had an amateur radio station with radio telephone attachment. Thanks to the cooperation of Lieutenant Commander (later Captain) C. F. Holden, the Communication Officer, and Lieutenant (later Captain) H. A. Tellman, Radio Officer, Mr. Gebhard was able to put a telephone attachment on the XAF standard high frequency transmitter with which very satisfactory exchange of conversation was obtained with young Hoover at Asheville. In the President's cabin, the loud speaker on his broadcast receiver was connected to a receiver for amateur frequencies and a microphone was installed for the President's use, so that the conversation was carried on from his own quarters.

The commercial stations took the lead in the development of highly directive equipment on high frequencies for their long haul point to point work. The trans-atlantic telegraph and telephone circuits operated by RCA and the Bell System were able to put up large directive installations for both receiving and transmitting. This was possible because they worked over fixed circuits from this country to cities in other countries. Therefore a reasonable number of these directive systems could cover all necessary circuits. The Navy lagged in this matter, probably because there was no way of applying these cumbersome antennae to our ships. The Laboratory had directive equipments which were used as mentioned in a preceeding chapter in the communications with the Byrd Expedition to the Antarctic and on many other tests. Finally our shore stations began to put in directive equipment over their principal circuits.

General adoption of what was called the Cornet System, which the Naval Research Laboratory originally installed at Arlington, soon followed. The Cornet System permitted simultaneous broadcasting of four frequencies in the 4, 8, 12 and 16 megacycle bands, all these frequencies being in harmonic relation. This greatly improved our intercept schedules sent out to all ships. A report on the Shanghai incident of 1932 refers to the efficiency of this system. At that time we had a number of very small shallow draught gun boats going up and down the Yangtze River as far as the Gorges. These ships, originally equipped with low frequencies, were never able to maintain decent communication with our Asiatic Fleet or with the Legation at Peking. We advised the Bureau of Engineering of the type of high frequency sets the ships could carry. After they received this equipment they never had any serious difficulty in maintaining communications. The PANAY, later deliberately sunk by the Japanese long before the opening of the War, was one of these ships.

Concerning the directive antenna used by the Navy Shore Stations, particular mention should be made of those developed for RCA by H. H. Beveridge and his assistants at Rocky Point, and to the Bruce antennae with variations thereon, developed by the Bell System, and adopted by the Navy.

The matter of fading of high frequency signals due to recurring destructive interference between the different rays arriving at a receiver continued to give us a good deal of worry. Here too, the commercial interests had much at stake. To them must go the greatest credit for the "diversity" systems devised to greatly ameliorate this nuisance.

A diversity system consists of a combination of the outputs of several receivers connected to fairly well separated antennae, in such a way that the system automatically selects the signal which is stronger. When receiving antennae are separated only a few hundred feet, the signals do not fade at the same instant on the different antennae. The Bell System accomplished this thing through a very ingenious arrangement for picking up the rays at varying angles of approach from the ionosphere. The first system may be called "space" diversity and the second "angle" diversity. Some of my readers may remember the extremely poor quality of the early high frequency broadcasts from Europe, which were rebroadcast on ordinary frequencies in this country. A great deal of distortion and fading was present and, except at rare intervals when things remained steady for a few minutes, it was very difficult to understand the speaker. If we contrast this with the extraordinarily good quality of rebroadcast European transmissions as received in this country today, we can realize the tremendous improvements brought about in high frequency long distance reception by the use of diversity systems. The Navy had adopted diversity in a number of its shore bases but it is not feasible to put it on board ship since it requires a very large area in which to install it.

The rapid increase in the use of high and superfrequencies introduced new and difficult problems in the field of radio direction finders. At low and intermediate frequencies the electric forces in the arriving radio wave are usually nearly vertical. A simple wire frame or loop of one or more turns may be so connected to a radio receiver as to permit the rotation of the loop to disclose the direction from which the signal comes.

In military and naval work the radio direction finders are useful not only to keep track of our own ships and airplanes but to permit, by observation of the direction of arrival of a wave from various shore stations, the accurate location of the position of our own craft with respect to the nearest shore. Furthermore, direction finding by radio is absolutely essential if we are to learn something of the enemy's activities and whereabouts. The extensive sweep of the early Japanese operations in the Pacific Islands during the late war permitted them to establish large numbers of direction finding stations on islands here and there so that they were able to give our units operating in those waters a great deal of trouble. They had an excellent network giving much advanced information as to the whereabouts of our various units. Finally these stations were systematically bombed out by our Army and Navy planes. This was none too easy a job. It had to be preceeded by elaborate photo reconnaissance and many stations had to be bombed out more than once, because the Japanese had a lot of spare parts. Unless the installations were pretty completely demolished, they would rebuild them in a few days.

High frequency direction finding required the development of new and special devices, since the high frequency waves arrived at the direction finder site with their electrical forces in all sorts of angular positions. Moreover the angular position of the electric force varied constantly from time to time. This sort of thing completely ruins the action of a simple loop direction finder, our main reliance on low and intermediate frequencies. It was necessary to devise an antenna system for the direction finding receiver which would respond only to vertical components which were sure to be present from time to time and of sufficient duration to permit the taking of bearings. The British scientists, particularly Dr. Smith-Rose, must be credited with the earliest and most successful attack on the problem of developing suitable direction finders for high frequency. The Adcock System of direction finder antennae is named after the British scientist who developed it. This system uses two vertical sets of rods which collect only the vertical components of the arriving wave. The connections between these two sets of rods, spaced some distance apart, are carefully shielded against radio influence.

Most of our high frequency direction finders were developed as variations or improvements on the original Adcock system although some of the arrangements worked out in this country, particularly the system devised by Dr. DeFriis of the Bell Telephone Laboratories, are sufficiently original to be considered new systems. Our early work on high frequency direction finders was carried on by Warren B. Burgess, aided by Mr. A. H. Moore and Mr. R. H. Worrall. A little later the problem was very aggressively attacked, under my immediate direction, by Mr. Harris Hastings and Mr. Ray Gordon with several assistants. Our particular effort at the time was to get a model that was readily transportable and could be set up on a suitable site in a half hour's time. We also hoped to use such an equipment on board ship, and did take several of such equipments to sea. However, the accuracy obtained on board ship with any high frequency direction finder devices, other than radar, which will be mentioned later, leaves much to be desired. At shore bases on the other hand, our semi-portable equipments were widely used with great effectiveness.

Since we had always gotten into trouble if we ran long leads from the vertical collecting rods to a receiver housed in some shelter, we decided, for this equipment, to put the receiver into the rotated and elevated equipment, with the operator on a little platform at about the same level.

This was, of course, emergency equipment and not intended for final permanent installation.

While we were working on the equipment, we often set these direction finders, mounted on their portable wooden tripods, out in the fields near the Laboratory while they were under test. This put the vertical collecting rods about ten or fifteen feet above the surface of the ground and gave reasonably good results when used on flat terrain. One day, while Harris Hastings was on the upper platform rotating the antennae assembly in order to take bearings on some European stations, Admiral Courtney and Captain Van Meter came down from the Navy Department to visit the Laboratory. Admiral Courtney, then Director of Naval Communications, was very keenly interested in the problem of high frequency direction finding. In those days, Naval officers in the Washington area were not wearing uniforms. When the Admiral saw this direction finder out in the field he recognized it for what it was, stopped his car, got out, walked over and called out to Hastings "Well young man, what are you doing here?" Hastings, who hadn't met the Admiral and knew that the work was considered pretty confidential replied, "Well, who the hell are you"? Admiral Courtney replied "I happen to be Admiral Courtney, the Director of Naval Communications". Hastings, of course, was very much embarrassed and started to make profuse apologies but the Admiral said "Never mind my boy, you made exactly the proper answer". After that they had a good get together and the Admiral mounted the platform and took a lot of bearings himself. From then on he was a frequent visitor whenever high frequency direction finding was in progress.

All of our earlier Navy receivers had been supplied with storage batteries for lighting filaments of the tubes and dry batteries for the higher voltages used on the plates and grids of the tubes. This was, of course, a terrible nuisance as batteries had to be continually replaced or recharged. The advent of an alternating current supply system for receivers required a general redesign of all Navy receiving equipment. Most of our earlier receivers required the simultaneous adjustment of at least two controls, generally the tuning control for the antenna system and the tuning control for the receiver circuit, which was loosely coupled to the antenna circuit. The adjustment of a third control was necessary if we wished to receive unmodulated signals with a local heterodyne oscillator. If there was any adjustment required while the operator was making copy, he was obliged to use both hands to make these adjustments and, therefore, lost some of the incoming messages while the adjustment was being made. Single dial control was badly needed and indeed, had arrived in broadcast receivers used by the general public.

The Naval Research Laboratory was called upon to develop the basic designs of the first Navy receivers to use alternating current supply and single dial tuning. Mr. T. McL. Davis and Mr. Edwin L. Powell deserve a good deal of credit for the thorough way in which this was done, resulting in a line of receivers, finally produced for us by the industry, which stood up a great many years. The people in the radio industry cooperated with us heartily in this program.

In order to hold our transmitters precisely on frequencies we had adopted the use of quartz crystal control to a very large extent. However, as the demands of the service for more and more frequencies and rapid changes in frequencies continued to increase, it was apparent that an appalling number of crystals would have to be issued to the Fleet in order to have all frequencies available which they might need. Furthermore, it was obvious that if we operated on exactly fixed frequencies, an enemy would know just where to look for our signals and have comparatively little difficulty in "jamming" them. This means rendering the signals unintelligible by superimposing on them enemy emissions of exactly the same frequencies.

Commodore Jennings B. Dow, Chief of Electronics Division, Bureau of Ships (then a Lieutenant) insisted that we try to get a high precision of frequency control with the possibility of continuous variation which could not possibly be had with the crystal control system. With continuously adjustable frequencies it would be much easier to avert deliberate enemy interference by prearranged and rapid shifts of frequency.

Dow himself proposed what came later to be known as the Dow circuit, which he worked out at the Laboratory with the assistance of Raymond Owens, L. A. Gebhard and R. B. Meyer. Our engineers went to work along the same lines. It meant producing mechanical arrangements for extremely accurate adjustment of capacities and inductances such as had hitherto never been attempted anywhere in the field of radio. Mr. Meyer was largely responsible for the development in 1931 of the TAD transmitters which had a continuously variable frequency from 2000 to 4525 kilocycles. He used the best ideas contributed by Lieutenant Dow and the Laboratory engineers. Commodore Dow should be given much credit for being the principal initiator of this program.

The Laboratory called in representatives of three large commercial radio interests and explained to them what we wanted and what we had worked out in the way of circuits. They all said the thing was impossible; they couldn't undertake it. So we built the first transmitter ourselves and then had these corporations send down their engineers and draftsmen in order to convince themselves that the system was feasible and workable. By the use of frequency multipliers we could step up the frequency to much higher bands than 4000 kilocycles once we had a stable master oscillator. Westinghouse got the first contract for this type of equipment, resulting in the TBF transmitter, a transmitter that has come through many successful battles and is recognized by all radio officers in the Fleet as one of unusual excellence.

Due to the changing structure of our ships it was becoming increasingly difficult to get enough transmitting and receiving antennae to meet the needs of radio communications. We were able to get along with very few receiving antennae after we learned how to put a lot of receivers on one antenna without interfering with each other. This Laboratory undertook to see if anything of a parallel nature could be done with transmitting antennae. Here the situation was very different. It really doesn't make much difference if you have a rather inefficient receiving antenna, because you can make up the difference with a good amplifier. This cannot be the case with transmitters, because high gain amplifiers in transmitters mean a very great expenditure of power. The transmitter with its associated antenna must be an efficient combination. Nevertheless the Laboratory did develop apparatus which permitted two and sometimes three frequencies to be delivered simultaneously from three different transmitters into one antenna, provided these frequencies were separated in different bands, differing rather widely in frequency. These "diplex" units as they were called, were installed on a great many ships.

Up to this point we did not have any too clear an idea of the efficiency of our high frequency transmitters; that is, we did not know exactly how much of the electrical energy put into the transmitter finally arrived in the antenna. With the assistance of Mr. Hastings I worked out a method of accurately determining this matter at least as far as water cooled transmitting tubes were concerned. Some success was obtained in extending these results to lower powered air cooled tubes. These results were reported in a joint paper by Hastings and myself, published by the Institute of Radio Engineers.

Equipment for submarines had been much in our minds. Some of the experiments we made with them have already been recounted. We continued to push this work for all frequencies, even superfrequencies, under the leadership of Lieutenant Harry Hill who was then at the Laboratory, assisted by Mr. Ray Gordon. It is perhaps appropriate to mention at this point that Mr. L. A. Gebhard made an extended trip to Europe in 1930 and came back with a lot of valuable information as to what was going on, particularly in France.

The use of automatic and teletype transmission and reception was steadily increasing. In teletype reception the message is printed out as though coming out of a typewriter instead of appearing on the tape in dots and dashes.

During this same period the Bureau of Standards had taken over very efficiently the systematic study of ionosphere heights, using variations of the pulse method. They also had started their broadcast of extremely accurate frequencies so that laboratories far and near could accurately calibrate their precision frequency measuring equipment. Both of these services nowadays are literally indispensable.

In the meantime our aircraft people had become definitely sold on the use of high frequency and superfrequency. They were using high frequencies up to 13,000 kilocycles and were using fixed antenna on fighter aircraft. In 1930, L. A. Hyland, now executive engineer for Bendix Radio, built the first 50 megacycle superfrequency set (XT) for an airplane, for code transmission only, and successfully tested it. Mr. Matthew Schrenk, aided by Harris Hastings built in 1931 the GL set for voice communication, operating between 50 and 60 megacycles. This set was the first set to derive all necessary power from an engine driven generator, the power being supplied through a filter to cut out ignition interferences and other disturbances from electrical equipment on the plane.

It was while Hyland was testing a high frequency direction finder on board a plane that he first observed the radio echoes from airplanes in flight. The Laboratory was emitting signals on 32.8 megacycles while Hyland had his direction finding equipment installed on an airplane on the ground at Anacostia Naval Air Station, two miles away from the transmitter. The antenna emitting the signals was horizontally polarized. Hyland employed on the plane a 15 foot horizontal wire with a connection to the receiver leading in from a point a little off from the center of this wire. By swinging the tail of the plane around he was able to get a very sharp minimum on the signals from the Laboratory. While this was going on, he noticed that whenever an airplane appeared in the air anywhere in the vicinity, the minimum was disturbed, indicating that additional signals reflected from the airplane were coming down to the receiver installed in the plane on the ground. These were distinctly fluctuating signals such as we had used for radio echo work in 1922. Theoretically we knew that all objects reflected radio signals to some extent at least, but this was the first positive proof that the magnitude of the reflections of an airplane in flight was sufficient to readily betray its presence and, to some extent, its position in space. When Hyland reported these results we felt that we were now in a position to get support for future work on the detection of enemy vessels and aircraft, which had been rather neglected for the past eight years. The Navy was very keenly alert to the significance of the rapid development of air power. We felt sure that they would be interested in any device which could betray the presence, and eventually locate, aircraft as well as ships. Incidentally, Young, Hyland and myself filed a joint patent at this time (which was long held in the secret status) for the location of moving craft either on the ground, on the water or in the air.

Further experiments on these airplane echoes were vigorously prosecuted, using principally the personnel of the Aircraft Section of the Radio Division, then under C. B. Mirick. L. A. Hyland, J. J. MacGregor and J. D. Wallace were active in this work. Experiments were continued with the installation on the plane used as receiver. The plane was parked on a compass rose belonging to the Army side of the Anacostia Field. One day, in the fall of 1930, Commander E. B. Almy, then Assistant Director of the Naval Research Laboratory, accompanied by some other officers, visited this set-up to witness the experiments. Two Army officers from the Army's side of the field came over to see what was happening on their compass rose and were much interested in the observations. I am sorry I cannot recall their names or ranks, but this is probably the first knowledge obtained by any one in the Army of our work on the location of airplanes at a considerable distance beyond the range of vision. I do not think they made any official report on the matter.

The experiments were expanded to cover a range of frequencies up to about 100 megacycles, using portable equipment which was operated in various locations within 30 miles of the center of Washington. Photographic records of these pulsating signals were made so that we could correlate the position and speed of the plane with the signal observations.

By 1932 we had worked out a complete system for the protection of an area. This involved a set of directional transmitters of moderate power, operating on superfrequencies. These transmitters were to be arranged around the periphery of a circle whose center was the center of the area to be protected; transmitters were to be located about 15 miles from the center of the area of protection. Each transmitter was to send out a fan beam so that a few miles further out these beams gradually overlapped each other, which was possible since the beams were not too narrow. Fifteen miles further out in the direction of these beams, another circle of receiving huts was to be erected. The output of the receivers was to be connected by wire with a central recording station for the entire system. We were able to demonstrate the efficiency of such a system of area protection by building only a few of the component parts and subjecting them to actual tests. Thus it was proved that we could detect the presence and the approximate location of many planes within fifty miles of the center of the area to be protected.

It is not my purpose to go into the mathematics of this system here. I will only state that a knowledge of the general direction of the beam, plus a continuous record on the recorders of the signal fluctuations, permitted a fairly good estimate to be made of the position of the plane, although in no way comparing with the great accuracy of subsequent radar equipment.

The difficulty with this system was that it was not applicable to Naval ships since it required fixed location of transmitters and receivers. Of course it was applicable to protection of Naval bases, but up to that time and for many years later, the protection of the shore bases of the Navy was largely the Army's job, as at Pearl Harbor on December 7, 1941. Therefore, I wrote a letter in 1932 to the Secretary of the War, via the Secretary of the Navy, calling the attention of the Army to the fact that we had developed this area protection system (it wasn't called radar yet) and that the system seemed far more adaptable to the needs of the Army than to those of the Navy. The Army was invited to send representatives to the Laboratory to witness a demonstration of the system in the field. This letter was signed by the Director of the Laboratory and resulted in a delegation of four Army officers visiting the Laboratory and witnessing the demonstration. For purposes of convenience, the transmitter for this demonstration was located at the Laboratory and the receiving equipment near Fort Foote, Maryland. Some time after this the Army asked permission to disclose this matter to the General Electric Company because they were considering giving them a development contract. The result was a conference at the Laboratory on June 20, 1935 between our engineers, Major Jackson, Captain Harding and Lieutenant Bell of the Army and Mr. Kenney of the General Electric Company, who has been mentioned in Chapter VII.

This contract was never completed for the simple reason that more effective developments were already under way, giving a more direct, more effective answer to the problem. It is interesting to note that this early type of radar, which can be called the FM (frequency modulation) type, was revived during the late War because it had the power of definitely discriminating between moving and fixed targets. It is still a live issue. With the much higher frequencies we now have available, and the very much sharper beams, there are possibilities that did not exist in the early 1930's.

It was during this period that we formed what we called the Special Research Section, organized principally to push forward in the field of what was, in a few years, known as Radar. We called it then "The Location of Enemy Ships and Planes". Mr. L. C. Young was made head of this section and in 1933 Mr. Robert M. Page was put on this work, followed a little later by LaVerne R. Philpott and R. C. Guthrie.

In 1933 Mr. Young proposed that we abandon, for the time being, the frequency variation or beat method and attempt to do the job by the use of high power pulses such as we had used in 1925 in getting range measurements on the ionosphere in collaboration with Drs. Tuve and Breit of the Carnegie Institution. I told Mr. Young that I would be glad to see this tried but warned him that it would be a much more difficult job than getting reflections from the ionosphere. Although the ionosphere was a long distance away, it was a very large and very perfect target, giving strong echoes, whereas the location of an airplane at a similar distance, say 100 miles, would require shorter pulses of very much higher power and new types of receivers. With this understanding we went ahead with the pulse method which was the basis of modern radar, having with us our background of having built high power pulse transmitters for the ionosphere work in 1925.

During the period covered by this chapter there had been several changes in the organization and leadership of the Naval Research Laboratory. The Laboratory remained under the Office of the Assistant Secretary of the Navy until 1931. At that time it was transferred to the cognizance of the Bureau of Engineering because that Bureau had most largely supported the Laboratory by the assignment of problems and the allocation of funds. Captain D. E. Theleen, Director from July 1926 to July 1930, had been succeeded by Captain (now Admiral) E. J. Marquart who, in turn, was succeeded by Captain E. G. Oberlin, former Assistant Director, in February 1931. He, in turn, in March of 1932, was succeeded by Commander (now Captain, Retired) E. B. Almy. Commander M. A. Libby who had been Assistant Director between December of 1929 and January 1930, died very suddenly and was succeeded as Assistant Director by Commander (later Captain) E. B. Almy, who served until March 1932 when he became Director. Lieutenant Commander W. J. Ruble (later Captain) served from March 1932 until May of 1933 as Assistant Director. Between 1929 and 1933, Lieutenant Commander (now Captain) A. D. Douglas and Lieutenant Commander (now Captain) B. W. Chippendale had served as executive officers. Lieutenant W. J. Holmes, who was Shop Manager between 1932 and 1933, later, after his retirement, became famous as an author of stories of the sea, particularly about submarines, which were published in the Saturday Evening Post. I remember him as a man of remarkable ability and keenness of mind with a completely absorbing interest in everything that had to do with submarines. He was called back to active duty after Pearl Harbor but I suppose he is now again on the retired list. I certainly hope that he will continue his stories in the Saturday Evening Post which he writes under the pseudonum of Alex Hudson.

Rear Admiral W. S. Parsons (then Lieutenant) and Captain E. H. Pierce (then Lieutenant) were attached to the Laboratory in 1933 as Technical Assistants. We owe much to these two. Commander Parsons was tremendously interested in the Radar program and gave us many practical suggestions and valuable ideas. He did everything he could to get money to support the program. He has been often mentioned in the press of late on account of his connection with the atomic bomb. He made the trip with the first bomb dropped on Hiroshima. Captain Pierce was one of the most effective radio and sound officers we ever had at the Laboratory. A man of unusual ability, wide understanding and excellent training and experience in radio and sound work.

Early in 1932, the Bureau of Engineering suggested that the Radio Division be separated into two divisions, one a small research group and the other an engineering and development group. The Director called me in and asked me what I thought of the idea. I told him that I didn't think much of it because it has always been my theory that to have a well balanced and enthusiastic organization one should permit at least some research work to go on in all divisions. I believed that the close contact between research workers and engineering development workers was good for both of them. The development engineers were sure to get a lot of new suggestions and ideas from the research workers who in return would certainly get better ideas of what the equipment would finally look like when it was sent to the Fleet. Thus they would be better able to direct their research work in such a way as to make it easier to take over when development came into the picture. "Certainly" I said, "There is no objection to having a special research group, but some research ought to go on everywhere".

Since the Bureau of Engineering held the purse strings at this time and they seemed rather insistent on trying out this reorganization, I told the Director that I was willing to try it out. He asked me which group I would prefer to head up. I told him I would prefer to stay with the small research group. Mr. L. A. Gebhard took over the Radio Engineering Division, although he too, was strenuously opposed to the split. I took over the Radio Research Division which consisted of only eight individuals.

The Radio Research Division had a reasonable appropriation for those days, no obligation to make frequent reports and a considerable latitude in choice of problems upon which to work. This split didn't last long. The Bureau of Engineering soon saw that the total output of the Laboratory was not as high under this arrangement and so restored the original divisional organization, under my direction, in January of 1934.

During the time the split was in effect, I think I had one of the happiest periods I have spent in this Laboratory. I had very few administrative headaches and a minimum number of technical reports to bother with so that I was able to spend most of my time on problems which I had wanted to undertake for some little time. Mr. Young and I went to work on a system of secret communication which would be difficult for the enemy to pick up and de-code. This system was based on the combination of two audible frequencies with which the transmitter was modulated, these two audible frequencies being kept in absolutely perfect phase relation to each other. This we could do by a device which is called a locked synchronism oscillator. We could, for instance, produce two frequencies one of which was exactly twice or three times the other, with fixed phase relation, or, we could produce two frequencies which bore the exact relation of two to three or three to four, four to five, etc.

These signals could be received on an ordinary radio receiver. After detection both of these audible tones could be heard, sounding like a major or minor chord as the case might be, but they could also be displayed on the screen of a cathode ray oscilloscope.

The oscilloscope deserves, at this point, a special description because it became so absolutely vital for our radar equipment during the late war, besides which it had many other uses. This instrument is a recording instrument wherein a pencil, so to speak, writes a luminous inscription on the screen of the tube. This record can be read by the eye or photographed by a camera. This pencil is not a mechanical pencil, but it is a narrow stream of electrons which are propelled through the so-called cathode ray "gun" of the tube and create a fluorescent spot wherever they hit a chemically prepared screen. Now if either magnetic or electric forces are caused to influence this beam during the flight of the electrons from the gun to the luminous screen, the beam will move, even at tremendous speeds, exactly in response to these influencing forces. The cathode ray tube is really very old but the earlier tubes were so difficult to use that they existed only here and there in physics laboratories. Direct visibility of the resulting pictures was not obtained with the earlier tube; on the contrary, this high vacuum tube had to be opened up and a photographic plate inserted every time it was necessary to record observations. Then the tube must be again evacuated.

During the late 20's and early 30's there was a very rapid development of cathode ray tubes in all countries, particularly in the United States. The development in this country was participated in by the General Electric Company, Western Electric Company, Allen DuMont Laboratories, Radio Corporation of America and others. These tubes were soon produced in all sizes from a one inch to twelve inch screen. They soon became so important to the radio engineer that I used to say that our engineers carried them around in their pockets at all times because they were as useful as a jack-knife. Certainly modern radar could not have reached the state that exists today without the cathode ray tube.

This device then has an index finger that will follow and trace the performance of rapidly changing electric currents, magnetic fields or voltages, even if they are changing so fast that time intervals have to be measured in units smaller than one millionth of a second. The development of the cathode ray tube to a practical tube for the everyday radio man marked another epoch in the advance of radio and radar. It is as important to radar as the telephone is to communication.

With this instrument then, we arranged to display the two tones in locked synchronism which we had put on our transmitter and received at a distant point on the receiver. One tone was arranged to cause the indicating cathode ray pencil to oscillate in a vertical plane. The other tone oscillated the pencil in a horizontal plane. The combination of the tones produced a very pretty picture on the screen.

The configuration of this picture depended on the frequencies and their relative phases. Frequencies in the ratio of 2 to 1 would produce a figure eight in one phase combination and a section of a parabola in another phase combination. In between, other phases would produce figures that gradually slipped from one shape to the other. Sometimes the bottom of the parabola would be on the right hand side of the picture and sometimes on the left. With other ratios as 2 to 3 or 3 to 4, etc. the figures were much more intricate but they too changed, if the starting phases of the frequencies were caused to differ by a small amount.

No matter how these phases were shifted, the tones remained exactly the same so that an enemy listening in with a head telephone would not be aware that information was being transmitted. We were able to work out a phase shift arrangement, operated by a set of keys, that would produce different figures which, incidentally, are called Lissajous figures, on the cathode ray tube without the tone quality of the signal being changed in any way. Thus we could make up a code and transmit information. We studied this system for some time. There were several objections to it. One was that the Fleet didn't have enough transmitters which were capable of taking such a very precise form of modulation; second, we finally decided that it would not take the enemy long to analyze the signals with a cathode ray tube and ultimately break the code. In any case, we put it on the shelf for further consideration. I now consider it a dead issue so do not hesitate to mention it here.

This particular study raised a number of interesting questions concerning the physics of the human ear and its relation to music. The question naturally arose as to whether changing the phase of two tones without changing their pitch would change the sound of the chord as heard in the loud speaker or telephone. When music is played by an orchestra, many combinations of tones occur but they are not like the tones that we produced since exact phase relationship and exact locking of frequency ratios is not possible with musical instruments. Two instruments cannot even be adjusted to the same frequency much closer than to within one beat per second which means that the phases would whirl around 360° each second. It is fortunate for our enjoyment of music that the human ear is not bothered by this matter of relative initial phases in complex sounds.

Since in a small way I am a musician myself, I became interested in this problem as a side issue and made a number of attempts to determine whether this change in phase without change of tones or the sound chord, could be detected by the human ear. None of us could detect this change unless it was done so abruptly as to produce momentary transients. We took care not to do this in our method of making the phase shifts. However, I had the pleasure of testing this matter out with a highly trained musician, namely Dr. Leopold Stokowski who spent several hours in the Laboratory with us, making experiments and discussing the physics of music in which he had always been much interested. Dr. Stokowski has a remarkable sense of absolute pitch but in addition to this he was definitely able to detect these phase differences in the component frequencies in a chord, although we arranged the experiments so that he couldn't see when we made the shifts. He was the only man tested who was able to do so. Perhaps other highly trained musicians have the same ability - it raises an interesting question.

In the process of testing out the range of this secrecy equipment, Mr. Young and I equipped a radio truck and started out on a trip of several hundred miles in order to take measurements both by day and by night at various ranges. This disclosed some data which was of very great interest to students of wave propagation.

At short ranges, where the ground wave signal predominates in intensity, we were able to get steady figures and very reliable communication in the daytime, with reasonably steady figures having only a little variation at night. At somewhat longer ranges the daylight communication was still good; after dark our figures on the cathode ray oscilloscope began to execute the most intricate and remarkable gyrations. When modulation is put on a carrier wave the carrier wave splits up into three components: first, the original carrier frequency; second, a side band which is higher in frequency than the carrier wave by an amount equal to the pitch of the modulating tone; third, a lower side band which has a lower frequency than the carrier wave by an amount equal to the pitch of the modulating tone. Since we were transmitting on a frequency of 250 kilocycles and our modulating frequencies were 1 kilocycle and 1 1/2 kilocycles it will be seen that our receiver was getting the carrier wave at 250 kilocycles, and two side bands, one at 251 kilocycles and at 249 kilocycles. In addition the other two side bands came in at 251 1/2 kilocycles and 248 1/2 kilocycles. The fact that these Lissajou figures change shape so rapidly at night means that there were uneasy movements going on in the ionosphere, which at the longer ranges controlled the predominating signal. These changes in the ionosphere did not affect all members of this group of frequencies in the same way but changed both their relative amplitudes and phases. This is the clearest possible proof of the uneasy turbulence which commonly exists in the upper regions responsible for long distance radio transmission. We decided that if the modulating frequencies could be lowered so that the whole family of frequencies would be in a very narrow band, we could get rid of this effect. We therefore modulated the transmitter with 40 cycles and 60 cycles simultaneously. True, this did reduce the rapidity and amplitude of the effects but it had no means cured it. This then was another objection to this system of secrecy transmission. It would be limited in usefulness to the ground wave range.

An interesting byproduct of this investigation was the development of a radio frequency wattmeter which performed satisfactorily up to 30 megacycles and permitted the direct measurement of the radio frequency power in the different component parts of the system or the antenna. This instrument was described in a paper published in the Proceedings of the Institute of Radio Engineers.

The increasing interest in aircraft radio problems, not only in the Army and the Navy, but in commercial aviation as well led, during this period, to the development of numerous direction finding and homing systems. The efforts of the Bureau of Standards, Civil Aeronautics Authority and the commercial interests succeeded in developing a low frequency system which was originally put into effect over large sections of this country by installing a number of ground stations capable of emitting radio signals of such a nature that the pilot was able to keep himself "on the beam", that is, "on course" by so flying his plane that the signals coming in from one side of the beam blended with the signals coming from the other side of the beam into a long dash when the plane was exactly on course.

The principal advantage of this system was that although it required large and rather delicately adjusted systems on the ground, the equipment in the plane was relatively simple. The disadvantage of the system lay in the fact that it set up fixed courses designed to take the plane from one airport to another. If, due to storm conditions, the plane was forced a long ways off the beam, it was difficult, if not impossible, to get on again.

As far as the Navy was concerned this system was not satisfactory because we could not use it for carrier bourne aircraft. It would be impossible to install these large ground systems on board ship and impracticable to keep them in proper position once they were afloat. Therefore the Navy's interest was centered on a homing system using superfrequencies.

During the period discussed in this chapter many experiments were made with frequencies between 30 and 100 megacycles, using beams putting out a relatively narrow radiation pattern. It was proposed to use these superfrequency beams for blind landing as well as general guidance. This sort of system also has the advantage of putting the more complex equipment on the ground or on the ship and the simpler equipment in the plane, where heavy and complex equipment is highly undesirable. The system also has the advantage of being small enough to put on board a ship.

In the meantime much work was done on the improvement of direction finding equipment on board the plane, particularly in the new superfrequency field. While these devices were not perfected in this particular period, the basic research and preliminary development was well under way.

It was at this same time that we became aware of the great influence which the sun spot cycle has on high frequency radio work. The increase and decrease of sun spots and magnetic storms follow approximately an eleven year cycle. It so happened that since the early work in 1923, 1924 and 1925 the passing of the years had brought us more than half way through this sun spot cycle. The Laboratory continued to take an active interest in wave propagation, cooperating with the amateurs and exchanging information with the commercial laboratories, the Bureau of Standards, the Carnegie Institution and others equally interested in this fascinating study. We concentrated our efforts during this period in the region between 20 and 40 megacycles because it is within this region that the sun spot conditions have the most marked effect. At one part of the sun spot cycle, frequencies above 25 megacycles are of very little use for long range communication, while at other parts they sometimes prove valuable, even well past 30 megacycles. During the right phase of the sun spot cycle we occasionally sent signals across the continent with very low power on 40 megacycles.

These questions were of great military as well as scientific interest, because the Navy wanted to know when it was safe to use high frequencies for a limited range communication without fear of being intercepted by a distant enemy. If we were building equipment to cover a period of more than a few years it became evident that for strictly limited range communication, communication which never was turned down to distant points on the earth by the ionosphere, we should have to push the frequencies well above 60 megacycles.

In determining the skip distances or zones of silence for frequencies between 20 and 30 megacycles we relied largely on picked amateur observers. As a result of their cooperation we were able to prove that these frequencies sometimes had zones of silence or skip distances as great as 1800 miles. This led to curious communication situations; for instance, there were periods when we could put strong signals on 28 or 30 megacycles into Denver, Colorado without having them heard anywhere between Denver and Washington except for a few miles outside of either city. These signals would bounce again from the ionosphere and come down on the West Coast, say in San Francisco, again giving excellent signals which were not observable at intervening points. These conditions were by no means regular and reliable. Neither were they particularly unusual at this time. Later, at a different phase of the sun spot cycle, it was quite impossible to duplicate them. All of these apparently erratic effects are explainable in terms of the varying ionization produced in the upper layer of the earth's atmosphere by the ultra violet light of the sun, which is greatly affected by sun spot bursts.

In looking over the record of visitors who came to the Laboratory during these years, I see the names of many scientists and radio engineers of national and even international reputation. There are also the names of many famous officers of the Navy and of the Army which have appeared in the headlines during the late war. The record clearly shows that we maintained very close contact with the Westinghouse Company, RCA, Bell Laboratories, DeForest Radio, General Electric, Allen DuMont Laboratories, Heintz and Kaufman Ltd., General Radio, Federal Telegraph Company, National Electric Supply Company, DuPont, United States Rubber Company, Isolantite Incorporated, Submarine Signal Company, International Nickel Company and many others too numerous to mention.

The policy of the Laboratory was not to attempt research and development which could be done for us by competent outside organizations. The policy was also to turn over to such organizations all except highly confidential information because in this way, if war came, there would be a large number of organizations familiar with Navy standards and Navy work. Thus they would be able to go into production for us when the emergency came. This policy paid off well when the time came when we needed huge quantities of radio and radar material.

Among the government departments we had very close contacts with the Army, Bureau of Standards, Lighthouse Service, the Department of Justice, the Coast Guard, the Department of Commerce, the Department of Agriculture and the Coast and Geodetic Survey. Within the Navy our contacts were mainly with the Bureau of Engineering, Bureau of Aeronautics, Bureau of Ordnance, Chief of Naval Operations, Office of Naval Intelligence, Director of Naval Communication, the Washington Navy Yard, the Naval Academy, Naval Experimental Station at Annapolis, the Marine Corps, the various Naval Fleets, the Navy General Board, the Navy Hydrographic Office and the Naval Aircraft Factory. I think that our close relations with the post graduate school at Annapolis, which continue to this date, have had a very beneficial effect. Many young Naval officers in this way come to know about the Laboratory and its work and know where to turn for help later on when they were faced with difficult problems in the field of radio.

One visitor, an amateur who had assisted us greatly in the determination of skip distances on high frequencies, was Arthur Collins of Amateur Station 9CXX, Cedar Rapids, Iowa, who called on us in October of 1930. Collins was then a very young man but I was very much impressed with his keenness and insight. I like to believe that the encouragement which we gave him to make radio his life work had something to do with the formation of the Collins Radio Company which has contributed no small amount of radio equipment for both Army and Navy during the late war.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER XIII 1933-1937

THE MARCH TO STILL HIGHER FREQUENCIES RADIO AND RADAR

CHAPTER XIII

1933-1937 - The March to Still Higher Frequencies - Radio and Radar

Radio started in 1887 with frequencies between 30 and 300 megacycles. It may seem surprising that more spectacular progress in the exploitation of higher and higher frequencies did not occur, once the tremendously useful properties of superfrequencies were discovered.

The reason for this lack of extremely rapid progress lies in the fact that practically every component of radio transmitters and receivers had to be radically improved or changed in order to function at much higher frequencies than those for which these components had been originally designed. I have already explained how this forced the development of new insulators.

Quite early in the development of radio communications it was known that high frequency currents tend to flow through the outer skin rather than through the whole body of a conductor. This means that the surface of the conductor plays the greatest role in carrying the current. This knowledge led to the creation of a conductor made up of a very large number of fine wires or strands, this being first accomplished by the Germans who put it on the market under the name of "Litzendraht". Even with this special conductor, which we used in the early days for winding coils, it was never possible to reduce the losses to the same point as the losses of the same conductor for currents of commercial electric lighting frequencies. Nevertheless the use of stranded conductor, the individual strands being well insulated from each other, was a great help.

Shortly after frequencies began to get higher than 2000 kilocycles it was discovered that the use of this finely stranded conducting cable not only was no longer an advantage but was actually a disadvantage. Radio engineers were then forced to go back to the use of non-stranded or solid conductors, but since only the outer shell of the conductor was of any use, we soon found ourselves making coils and circuits out of thin copper tubing. In other cases we used thin metal tubing electroplated with silver, since that metal is a still better conductor than copper. In other words, the radio engineers had to patiently learn a lot of new tricks. Considering the magnitude of the difficulties involved, the march toward higher and higher frequencies really went on at an astounding pace.

Perhaps the most critical component of the radio circuit, the one which caused us the most worry, was the vacuum tube upon which we depended for the production of very high frequency oscillations and for the detection and amplification of the corresponding high frequency waves in our radio receivers. Since in this country the Navy was the first to make very extensive use of high frequencies as well as superfrequencies (above 30 megacycles) we had to take the lead in bringing pressure to bear upon the radio tube laboratories of the country in order to persuade them to turn out vacuum tubes suitable for higher and higher frequencies. The Bell Telephone Laboratories, the General Electric Company, the Westinghouse Company, RCA, Eimac and others contributed in a highly important way to the program.

While all this work looking to future applications was going on, the Navy as a whole had consolidated its position in the high frequency field under 30 megacycles by providing the Fleet and the shore stations with a splendid line of transmitters and receivers. In the production of this equipment we had the very able cooperation and support of many organizations, notably, the Westinghouse Electric and Manufacturing Company, RCA, General Electric and the Bell Telephone Laboratories.

The Navy had decided, after experiments carried out on board the TEXAS by our engineers in cooperation with the ship's radio officers, that since we were mainly interested in telegraphic communication by the continuous wave method, it was unnecessary to consider standard equipment for even large ships which would put more than one kilowatt of radio frequency power into the antenna. This wise decision allowed us to avoid the use of a large and cumbersome power plant, water cooled tubes, and various accessories that go with higher powers.

In 1934, there occurred a very interesting instance of the use of very moderate high frequency power for very important purposes. In that year Mussolini apparently decided that, since the Japanese had been extremely successful in grabbing what they wanted in China, he might as well start something in Ethiopia. Ethiopia had protested to the League of Nations as early as May 13, 1934 about the pressure Italy was putting upon it. Our State Department was very anxious to have frequent and uncensored reports on the Ethiopian situation from Addis Abbaba. The State Department requested the Navy Department to provide radio communication from Addis Abbaba to Washington.

The Bureau of Engineering took a 100 watt transmitter capable of operating between 4000 and 20,000 kilocycles, procured a gasoline driven motor generator to supply the transmitter power, added the necessary receivers and shipped the equipment hurriedly, before the Italians got there, into Ethiopia. We also had a naval ship so stationed in the Mediterranean area that, if necessary, it could be used to relay the signals to Washington.

The circuit from Washington to the capitol city of Ethiopia is about 6000 miles in length. The direction from Washington is slightly north of east. This may seem strange to those who are more familiar with maps than with globes, but it is a fact that, although the Ethiopian capitol lies near the equator and Washington far north of it, the true bearing from Washington is a little north of east. In fact, the city of Johannesburg, South Africa, is actually, in bearing, almost exactly east of Washington. These things cannot be understood by examination of a flat map but only by recourse to the globe.

This circuit involved a time difference of over six hours so that during a large part of the twenty four hours the circuit was partly in sunlight and partly in darkness. This meant that during these hours frequencies suitable for night operation would fail to pass the daylight sector, because of high attenuation, while frequencies suitable for daylight operation would fail to pass the night sector because of not being turned down by the ionosphere. Direct communication with Washington must, therefore, be limited to a few hours, namely, during the summer months at least, between 11:00 A.M.

and half past three in the afternoon using a very high frequency, since the entire path would then be in daylight. During a similar period in the night hours from 11:00 P.M. to somewhere between three or four o'clock in the morning the whole circuit was in darkness, so that suitable night frequencies could be used.

The Naval Research Laboratory assisted the Navy Department not only in the preliminary tests which resulted in the proper choice of a frequency of over 19,000 kilocycles for the daytime circuit and a frequency not far from 9000 kilocycles for the night circuit, but we actually assisted with the reception of the signals, particularly in the daytime. We arranged to put up a simple but fairly directive antenna system at the Laboratory to aid in the reception of these very weak daylight signals.

The story of the struggles which this small Naval unit had in getting this equipment into Ethiopia and set up, is a tale of superhuman efforts in the face of almost insurmountable obstacles. The antenna had to be camouflaged, to some extent at least, and the power plant had to be muffled as much as possible so as not to betray the presence of the equipment. This set continued to operate for some time after the Italians arrived, as they did not immediately discover its presence. Thus the true story of what was going on in Ethiopia was delivered every day to the State Department when no other method of communication would have been possible without long delays and censorship by some foreign country. As I remember the situation, there was little need to relay the signals through the ship in the Mediterranean.

Since this transmitter in Ethiopia had only 100 watts of power, this constituted a rather remarkable achievement. Of course, the operators in Ethiopia didn't have so much difficulty with reception, since we had much more powerful sets with better antennae in Washington, so we could deliver them a much stronger signal. When one stops to consider that successful and important communications could be handled with no more radiated power than it would take to operate an electric heating pad or a 100 watt lamp, it seems, even in these days, a rather miraculous performance.

During this period the Navy became increasingly conscious of the growing importance of Naval air power. Several influential officers interested in Naval air power suggested to Captain H. R. Greenlee, at that time Director of the Naval Research Laboratory, that the Laboratory should recognize the situation by creating a separate aircraft radio division. Although I did not believe this as efficient as the older method of closely integrating airborne radio developments with those for ship and shore, I readily agreed to try out the idea. Accordingly Mr. L. A. Gebhard, who had had a great deal of experience in airborne radio work, was appointed to head the Airborne Radio Divisions in January of 1933. After a little over a year's trial, it was apparent that there was nothing to be gained by this separation and much to lose; therefore Mr. Gebhard recommended that the arrangement be dropped.

In March of 1934, the Aircraft radio activities again became a section of the Radio Division, under the leadership of Mr. Matthew Schrenk. Mr. Mirick, former leader, had remained with the Research Group, in the earlier 1932 split of radio into Engineering and Research, and after that engaged in specialized work on aircraft electrical power plants, working very closely with the Radio and Electrical Division of the Bureau of Aeronautics. The new leader, Mr. Schrenk, had had broad experience in various sections of the Laboratory and for some years had been very active in aircraft work. However, it should be noted that a great deal of the work, contributing directly or indirectly to progress in aircraft radio, was done by sections of the Radio Division other than the Airborne Radio Section. Notable among these sections for their contributions were the Transmitter Section under Mr. R. B. Meyer; the Receiver Section under Mr. T. McL. Davis, and the Measurements and Direction Finder Section under Mr. Warren B. Burgess. In the Special Research Section which had been formed in 1933, the first work on airborne radar was started with the construction of a pulse or radar type of altimeter operating on a frequency of about 500 megacycles. Mr. LaVerne R. Philpott was largely responsible for this device.

As Naval aviation grew, there were an increasing number of practice evolutions with groups of ships at sea, including the operation of carrier borne aircraft. Some of these exercises took the aircraft to distances of several hundred miles away from their carriers and from the main body of the Fleet. Of course, these planes were generally equipped with radio communication. It was at times possible to get bearings on them from the ships and to tell the pilots what courses to steer in order to get home. Even in good weather planes frequently became lost, because flying over the ocean is a very different thing from flying over land, where there is always a chance of picking up some familiar land mark. On the open sea it is very easy to fail to find a target, even one as large as that made by a group of ships. Since the planes are naturally provided with magnetic compasses, this may seem a little strange.

The main difficulty is, of course, the wind drift. A pilot flying a given compass course may, when the action of the wind is taken into consideration, be actually flying a course considerably different from the one he intends to fly. In other words, the aircraft does not travel over the surface of the sea in the direction in which it is pointed because it is so easily affected by the movement of the medium, namely the air, in which the plane travels.

It is true that an experienced pilot, provided the weather is sufficiently clear, can observe the waves beneath him and make a rough estimate of the "drift". Unfortunately, however, changeable weather conditions may arise where the waves do not by any means truly indicate the direction or magnitude of the wind. Perhaps this explanation shows why we became extremely interested in homing devices which would give the aircraft an absolutely sure course back to their respective carriers.

The development of the first of these devices, known as the YE equipment, started in 1934 with a rotating beam transmitter at a frequency of about 120 megacycles. This transmitted, as it rotated, an indicating signal showing in what direction it was pointed. In other words, this device acted exactly as would a rotating lighthouse beam if it changed color or some other Characteristic according to the sector toward which it pointed. The radio beam had the advantage of having a range of 100 to 150 miles, provided the aircraft was flying at a reasonable height so that the curvature of the earth would not cut off the beam. This transmitter was mounted on the roof of one of the Naval Research Laboratory Buildings. Many tests were made with planes from the Anacostia Air Station. The development of this transmitter was entirely in the hands of the Transmitter Section under Mr. R. B. Meyer, ably assisted by Mr. Oscar Dresser. The development of the receiver, which was to go on the aircraft and permit the reception of these signals, was turned over to the Aircraft Section under Mr. Matthew H. Schrenk, assisted by Mr. H. R. Miller.

In order to avoid a large amount of additional aircraft equipment, a very small and compact adapter, as it is called, was produced by the Aircraft Division. This could be connected in ahead of the usualaircraft receiver by the simple throwing of a switch, permitting simple and easy reception of the beacon signals.

The breadth of the beam of radiation sent out by the YE was considerable, so that as the beam rotated the pilot would usually hear at least three signals for three courses 10° to 15° apart. This was an advantage rather than a disadvantage, because it was very easy to pick the strongest signal and determine thereby exactly what course the pilot should fly in order to arrive at his carrier.

The YE transmitters on these carriers could be so modulated that it would be possible for a lost pilot to distinguish his own carrier or, in an emergency, to pick out another carrier and ask permission to land on it.

On many occasions the flights around Washington demonstrated the ability of this equipment to bring a plane home in a dense fog, but in spite of these successful demonstrations the Bureau of Engineering decided that the rotating beam antenna structure was too large to easily install on shipboard. We were requested to redesign the equipment for a frequency of 246 megacycles which would permit the reduction in the size of the antenna by a factor of nearly 4 to 1. It was this decision which so greatly delayed the development of these devices for, at that time, there were no vacuum tubes in existence which, at the frequency in question, would give even the moderate amount of power required for this job.

We managed to redesign the equipment for about 200 megacycles but stuck there for some time until new tubes, whose development had been hastened by the needs of radar, came into the picture. Fortunately for the Navy this development was finally completed and introduced into the Fleet in 1940 and 1941, in time to save many lives and many planes in the battles of the Pacific. In the long run, the decision of the Bureau of Engineering to double the frequency from 125 to 246 was justified, but that decision cost laboratory engineers many anxious hours. The work on the YE equipment shows how effective the cooperation was between different sections of the division in the development of equipment of the utmost importance to Naval aviation.

The development of suitable transmitting and receiving tubes for aircraft was a long, patient struggle. Radio equipment in aircraft has to withstand a good deal of shock during take off and landing, tremendous variations in temperature due to varying altitude, a great variation in humidity and above all, constant vibration. Many vacuum tubes which do well enough on the surface ship are utterly unfit for use on board aircraft. The persistent demands of the aircraft people for "non-microphonic" vacuum tubes, that is, tubes which would continue to function in spite of vibration and shock, finally resulted in the development of better tubes for both of the armed services and all branches thereof as well as for the radio industry as a whole.

The vacuum tube section of the Radio Division, under the leadership of Joseph T. Fetsch, aided greatly in the solution of these difficult problems. The manufacturers in the country cooperated whole heartedly and were continually producing new designs in line with our suggestions, sending them to us for a checkup. New methods of cushioning shock by special supports for radio gear also helped greatly in arriving at a practical solution of our difficulties.

As the frequencies rapidly became higher and higher it became increasingly evident that the problem of getting high frequency energy from the transmitter into the antenna was a very serious one, not to be solved by the expedients used prior to 1930. It was standard practice in Naval ships to put most radio equipment below decks, behind armor. The power from the transmitters was passed up to the antenna through heavy copper conductors. These conductors passed through what was called a "trunk", which was an opening, 8" or 12" in diameter, running straight up through the different decks of the ship to the top side, where it terminated in an insulated support to which the wire leading to the antenna could be attached.

When attempts were made to send very high frequencies to top side antennae, it was found that most of the energy was dissipated and very little ever arrived above decks. These large trunks, moreover, were a great hazard to a fighting ship in case the enemy should decide to use gas.

The radio industry, because of its interest in high frequency for communications and superfrequency for television, took an active interest in the prompt solution of this problem and played a leading role in providing suitable high frequency transmission lines. For some time we had been using what we call open wire lines, consisting of two parallel wires a few inches apart, supported by insulators and running from the transmitter to the antenna. The losses in such lines were not prohibitive until frequencies got above 100 megacycles.

In the meantime the Bell System developed and made wide use of what is called tubular transmission line. This was made of semi-flexible copper tubing through the center of which was strung a solid copper wire with large numbers of insulating beads on it to prevent the wire from coming in contact with the walls of the copper tubing. These beads could be made of glass or ceramic materials of low loss at high frequencies.

These copper tubing lines were a great advantage and, for a time at least, although not for very long, we could forget about the difficulties of getting power from a high frequency transmitter to an antenna say 200 feet or more distant from it. These copper lines had one disadvantage; they didn't work after they became well saturated with moisture. Thus they had to be filled with a dry inert gas like nitrogen and kept under a slight pressure. We will see later how these lines had to be replaced because of the difficulty of keeping them gas tight under battle conditions. Nevertheless, they solved our transmission line difficulties for the time being and permitted progress to go on. Other lines of a more flexible nature were soon produced by various organizations. Although some of them are not of as low a loss as the copper line, they are very useful in certain spaces were flexibility of lines is an important factor.

Early in February of 1934 a demonstration of radio echoes from an airplane was given to a group of congressmen from the Subcommittee on Naval Appropriations of the House of Representatives. On this Committee was a man who had a very great influence in the development of Navy radar - a man to whom the Navy owes a great debt of gratitude. This was the Honorable James Scrugham, later Senator Scrugham, from Nevada, who died fecently. Mr Scrugham had been educated as an engineer. He certainly had an engineering type of mind. He took a very keen interest in everything we were doing in the field of what we called microwaves, which then meant waves of any frequency higher than 100 megacycles. The demonstration we gave in 1934 was of the old radar system which used the beat method. It operated on 60 megacycles and successfully demonstrated radio echoes from airplanes at a very considerable distance. Mr. R. A. Gordon of the Aircraft Division was responsible for the equipment with which this demonstration was made.

The work on pulse radar which had been going on for a year in the Special Research Section had been strengthened by the addition of several very competent engineers, notably, Mr. R. C. Guthrie and Mr. Arthur A. Varela. From the outset, Dr. R. M. Page gave indications of possessing extraordinary ability and fertility of invention. These qualities resulted in his contributing more new ideas to the field of radar than any other one man. Still under Mr. Young's direction as head of the section, he was given very complete authority to go ahead with pulse radar as rapidly as possible.

This radar work had been supported by very pitiful funds diverted perhaps illegally, I will admit, from other projects. The time had arrived when we definitely needed more tangible support. With the consent and approval of Captain Greenlee, then Director of the Naval Research Laboratory, and of Admiral S. M. Robinson, then Chief of the Bureau of Engineering, I went to see Mr. Scrugham early in 1935, accompanied by Dr. Hayes of the Sound Division. We knew that Mr. Scrugham was even then the most influential member of the Naval Appropriations Subcommittee, which he headed the next year and for a number of years thereafter.

We put up a strong plea for a substantial addition to the small direct appropriation which the Naval Research Laboratory usually received from Congress, this increment to be earmarked for long time investigations, particularly in the field of microwaves and supersonics. Mr. Scrugham listened in silence, asked a few questions, but promised us nothing. We left his office feeling very much discouraged, but on the following Monday morning, he telephoned to state that the Committee had agreed to give us an extra $100,000.00 to be spent on this work. This looks like a small amount in these days but it looked like ten million dollars to us then.

It was some little time before this appropriation became available. In the meantime the Bureau of Aeronautics had given the Sound Division $15,000.00 to develop a sonic altimeter. The Sound Division believed that the new radio echo work was more promising for the solution of the problem.

With the consent of the Bureau of Aeronautics they turned $10,000.00 of this money over to the microwave groups in the Radio Division. The next year the Bureau of Aeronautics added about as much more to this fund, which kept pulse radar alive in a difficult period before adequate funds became available. Perhaps it is pertinent to add at this point that the House Appropriations Committee doubled and trippled our funds in the next two succeeding years.

Our first laboratory models of pulse radar used a circular sweep circuit instead of the A scope as described in the introductory chapter. It is interesting to note that the German searchlight and fire control radar used the same type of sweep throughout the war. In this type of display the scope has a bright line in the form of a circle. The pips produced by outgoing and reflected pulses appear as protuberances or "corona ray" effects on the outer periphery of this circle. These are generally so arranged that the outgoing pulse appears at the top of the display and the range is measured around the circle instead of along a straight line as in the case of the A scope.

When the time came to make a practical pulse radar on a scale that would permit a realistic test, Mr. Page argued that we ought to wait until we could do the job with a very much higher frequency. We had been doing this first work at frequencies not much higher than 30 megacycles. Mr. Young argued that since we knew from our earlier radar work of the beat type that ranges of 50 miles on airplanes were possible at 30 megacycles, we ought to start our pulse work at that frequency, which we knew would give adequate echoes, and then move gradually to higher frequencies. Furthermore, we already had a very large directive antenna supported by 200 foot towers, which would direct a 30 megacycle beam in a fairly narrow pencil. I backed Mr. Young up in this argument because I felt that the sooner we got an actual demonstrationoof this character to show to influential people in the Naval Service, the sooner would we get enthusiastic financial backing. I still believe the matter was decided correctly, because we were definitely able to get such satisfactory echoes with the 30 megacycle beam that we at once raised the frequency to 80 megacycles, also with a fixed beam but much smaller than the one on 30 megacycles. I think this step by step method in the long run gave the best results.

In these earlier experiments with fixed beams we had to have the pilots fly the target planes strictly in the course covered by the beam, which could not be rotated. We usually had radio communication with the pilot so that we could advise him when he was off the beam. One of the pilots got lost in the thick weather one day, twenty five miles south of the Laboratory. We told him to fly five minutes in an easterly course. He did so but we saw no pip on the scope indicating that he had crossed the beam. We then told him to fly ten minutes in a westerly direction. Soon the pip appeared, whereupon we told him he was on the beam. He came home safely. Probably this was the first instance of a plane being brought home by radar in thick weather. This was in the summer of 1936.

The contacts between the Naval Research Laboratory and the War Department during this period were fairly frequent and, I believe, very profitable to both Army and Navy. Mr. W. D. Hirshburger, who had formerly been employed in the Sound Division of the Naval Research Laboratory, was in the employ of the Signal Corps Laboratories at Fort Monmouth in 1936. Later he went to RCA. Mr. Hirshburger spent two days with us early in 1936, discussing various phases of microwave problems. Microwaves in 1936 meant anything shorter than a meter in wavelength, that is, higher than 300 megacycles in frequency. As early as 1934 and 1935 the Naval Research Laboratory had a group, involving Dr. L. R. Philpott, J. P. Hagen, W. J. Cahill, Dr. C. E. Cleeton, and W. C. Curtis working on wavelengths as low as 7 centimeters, which means on frequencies higher than 4000 megacycles. Both the Army and the Navy were interested in those new activities from the standpoint of communications as well as radio location (radar).

Early in the summer of 1936, as already pointed out, the Naval Research Laboratory had an 80 megacycle pulse radar set which was capable of giving quite spectacular demonstrations. The performance of this equipment was witnessed on the 4th of June 1936 by a delegation brought down by Lieutenant Commander (now Commodore) J. B. Dow of the Bureau of Engineering, Navy Department. This group included Lieutenant Colonel R. B. Colton of the War Department, who was recently retired as a Major General; Lieutenant Colonel W. R. Blair of the Signal Corps Laboratories, Fort Monmouth, retired not long ago as a Colonel; Captain J. D. O'Connell of the Signal Corps Laboratories, now believed to be a Colonel, and Mr. R. I. Cole and Dr. P. E. Watson of the Signal Corps Laboratories. Dr. Watson died not long ago. The present Watson Laboratories of the Signal Corps are named after him, a well deserved compliment for a very competent radio engineer.

In addition to the demonstration of the 80 megacycle radar set we showed the Army delegation the start we had made on 200 megacycle equipment and pointed out that we were having great difficulty in getting the required power with available transmitter tubes. It was necessary for the Navy to go at least this high in frequency, in our opinion in order to get the size of the rotating beam down to reasonable dimensions, suitable for shipboard installation. We had learned this lesson from the YE homing equipment previously described. Since the Army didn't put so strict a limit on the size of the rotating beam we advised that they push their work in the 100 megacycle band where they could get much higher peak pulse power from available tubes than could be had at that time at higher frequencies. This the Army accomplished in their first long range search radar which, although large and clumsy, nevertheless gave excellent performance over very long ranges.

On the 12th of October, 1936, Commodore Dow brought down Colonel Mauborgne who was then Assistant Chief of the Signal Corps, becoming Chief between 1937 and 1941 with a rank of Major General, after which he was retired. Maughborgne was an old friend, having been our first Army Liaison Officer.

In the latter part of 1936 when Vice Admiral A. J. Hepburn, now retired, was Commander-Chief of the United States Fleet, he advised Admiral Bowen, then Chief of the Bureau of Engineering, to arrange for an early demonstration and practical test of radar with the Fleet. The Laboratory was not yet ready to send a search radar to the Fleet, but I felt that we should make some tests on board a ship with what gear we had even if it was only what we called soap-box equipment. We obtained an opportunity early in April of 1937 to put such equipment on the USS LEARY, a destroyer which had docked at the Washington Navy Yard, a very convenient place to make the installation. We had very little advance warning of the availability of this ship but all hands went to work practically night and day to put in readiness three items; first, a 200 megacycle pulse radar with a small antenna; second, a 1200 megacycle equipment modulated at 30 kilocycles and operating not on the pulse principle but on the phase shift principle; third, a communication equipment also operating on 1200 megacycles. The 1200 megacycle equipment had a reasonably sharp beam but the antenna for the 200 megacycle set could not be very large since it had to be strapped on the back of a five inch gun and was aimed at the target by training the gun. Naturally this small antenna did not give a sharp beam. Nevertheless radar ranges on airborne and surface targets at moderate distances were obtained with both equipments.

This trip on the destroyer LEARY was of great interest to me not only on account of the radar tests but because, although I am pretty well qualified as a pilot for the Potomac waters, I had never before had a chance to run the river during a flood.

The stage of water in the Potomac River is subject to some very unusual and rather interesting influences, the commonest of these being the prevailing direction of the wind during a prolonged blow, say of three days' duration or more. If the wind is from the northwest the tidal reaches of the Potomac, which means from Washington to Point Lookout, experience abnormally low water. On the other hand if the wind is from the northeast, or east, the pressure of the water coming in through the Virginia Capes into Chesapeake Bay causes unusually high water. A second influence on the stage of the water in the lower Potomac at Washington, is due to conditions on the upper river including all the branches such as the Shenandoah River. If there is very heavy rainfall in this mountainous area the water comes down very rapidly and the river is capable of very high and very rapid rise in the upper section above the Georgetown district in Washington. If, as happened on the day the LEARY was scheduled to sail, these two conditions happen at the same time, a very high stage of water results in both the upper Potomac and the tidal section.

The final influence is the stage of the tide itself. This means that if the peak flood conditions occur at Washington coincident with the time of high tide the very worst conditions for a flood are obtained. That was almost exactly what happened on this morning in April when the LEARY was scheduled to sail. My wife drove me to the Navy Yard and had to bring the car on to the dock through nearly a foot of water in order that I could jump to the gangway without getting wet.

Of course an immense amount of driftwood and debris had come down from the upper Potomac where pretty heavy damage occurred. The Captain of the LEARY was not too anxious to make the trip but finally agreed to try it. We had to proceed very carefully for the first forty or fifty miles, but after we rounded Maryland Point we found that we had outrun the mass of debris which was tearing down the river and were then able to resume normal speed. It seemed strange to run down on this extremely high water which gave the old river an unnatural aspect. We didn't get too much help from the normal aids to navigation because many of the spar buoys had been torn from their moorings and others were completely under water. The main difficulty was of course avoiding the driftwood as there was little danger of running aground anywhere.

That night in Lynn Haven roads the ship's force cleared out the water intakes on the LEARY, finding in them a surprising amount of strange material, but no damage had been done. By the time we came back up the river a few days later flood conditions had largely subsided and the river was fairly clear of flotsam and jetsam.

When lying in Lynn Haven Roads we were able to pick up planes flying back and forth from the Naval Air Station at Hampton Roads. On these planes the 200 megacycle equipment gave 15 to 18 miles range. We didn't have any very rough water during this trip but did have enough to get our first lesson in what later came to be called "sea-returns", which means radar echo signals from waves. Such water waves were annoying only in the first two or three miles of the radar range. Modern radar has advanced to a point where much of this difficulty can be alleviated, particularly by the use of very high frequencies and very sharp beams, but I think it is fair to say that the wave echo difficulty is still with us and can always be reckoned on to some extent when we are trying to identify surface targets which are within two or three miles of the radar.

In spite of the very calm weather we had on the trip down the Potomac I recall that Dr. Philpott was extremely seasick, this in spite of the fact that the ship was as steady as a rock. Philpott is one of these unfortunate people who can get thoroughly seasick even when standing on the end of a dock and merely looking at the water. I could not help but admire the grit and determination with which Philpott stood up to his work and took part in all the tests in spite of being so ill that he could hardly stand on his feet. Fortunately no other members of the party were similarly affected. In spite of the meager results obtained with these tests I felt very much encouraged since they showed that radar could work under sea-going conditions. I knew of many improvements that could be made in the equipments to tremendously increase their range and reliability. These then were the first radars to go afloat in the United States Navy. One operated on 200 megacycles and one on 1200 megacycles.

Not long after the tests on the LEARY, namely, on the 17th of February, 1937, we received a visit from Mr. Cole and Dr. Watson of the Signal Corps Laboratories, accompanied by Lieutenant (now Colonel) Corput. These gentlemen witnessed the demonstration of the 80 megacycle equipment and discussed our program with the 200 megacycle equipment. In turn they outlined the work being done by the Signal Corps. This was my first meeting with Colonel Corput. I was very much impressed with his forcefulness, knowledge and engineering ability.

Shortly after this visit Mr. Young, then one of the Associate Superintendents of the Radio Division, with Dr. Hulburt, Chief of the Division of Physical Optics, visited Fort Monmouth Signal Corps Laboratory. There they saw the Army's 105 megacycle high power set in operation and were given a beautiful demonstration, including semi-automatic training of guns and search lights synchronized with the training of the radar. The Army's work in tying up the whole system, namely, searchlights, guns and radar, was much ahead of anything we had done at that time. It was pioneer work, paving the way for automatic radar gun pointing and fire control. Of course the Army's system at that time was far from being fully automatic, since it required the services of an operator to dial the radar and radio information over to searchlights and guns. Nevertheless, it was a very encouraging demonstration.

People in both the Army and Navy in those days were more interested in radar as a means of defense against aircraft attack than in any of its other possibilities. The Army Air Corps people were equally interested because if our potential enemies employed radar, they might have to modify their own tactical procedures.

On the 23rd of June, 1937 we received a visit from Colonel C. B. Culver of Wright Field, accompanied by an aide, who appears to have been an officer named Carver, and by another officer whose rank I do not know, the latter being from the War Department. I had known Culver well since Anacostia days so we had a very enjoyable visit. I believe he is now retired.

We were now getting close to the point where we could think of radar production. Production has never been our direct responsibility at the Naval Research Laboratory, but is rather the responsibility of the materiel bureaus. However we had to stand by for advice and technical assistance and were as eager to get radar into production as were our friends in the bureaus. Accordingly we recommended to these bureaus that they call in experts from one of the big corporations after duly cautioning them about the secrecy of the projects. In accordance with this, on the 13th of July 1937, we were visited by Dr. E. L. Nelson, Dr. J. W. Smith and Mr. A. Merquelin of the Bell Telephone Laboratories. Nelson was mentioned in Chapter VIII as having been with me for a short time at the Naval Aircraft Laboratory, temporarily established at the Bureau of Standards in 1919. When we called these gentlemen into conference we told them what we had. They were frankly skeptical. I told them that I didn't expect them to believe that we could locate planes many miles away but that I believed I could convince them with an actual demonstration. So we went out to the building called the Field House, where we had installed the 80 megacycle equipment, and put on a very convincing demonstration. After that we returned to the main Laboratory to the roof of Building 1 and gave them a demonstration on 200 megacycles. This was not quite as effective as the one given on 80 megacycles, because this particular equipment hadn't been worked up to the necessary high power pulses on account of our inability to procure suitable vacuum tubes.

We asked the Bell Laboratory people whether they would consider a development contract to produce a radar along these lines and put it into production. They replied that since we were apparently about five years ahead of them in techniques, they preferred not to take a contract at that time but would agree to go to work on systems studies, paying particular attention to the improvement of tubes and component parts with the needs of radar circuits especially in mind. It wasn't very long before they felt themselves in a position to take on their first contract for Navy fire control equipment, that is, radars specifically designed for very accurate pointing of guns on unseen targets. The first radar equipments designed solely for gun firing were produced by the Bell Telephone Group.

In December of this year, 1937, we called in Dr. J. A. McCullough and Mr. Adolph Schwartz, representing the Eimac Corporation. We had been favorably impressed with the experiments which we had made with the high vacuum Eimac tubes. They showed great promise of meeting the peculiar conditions necessary for operation of radar transmitters. The transmitting tubes for a radar are required to take a momentary but tremendous overload in order to get the very high peak power necessary for the successful operation of the radar. In other words, the tubes have to be worked with momentary voltage many thousands of volts higher than the normal voltage which would be used on such tubes for communication purposes.

This contact with Eimac developed into a close cooperation between their tube laboratories and our radar people. It is due to their enthusiastic support that we were able to have a number of radars in our Fleet some considerable time before we went to war with Japan. The cooperation of this company was not only prompt and effective, frequently involving shipment of tubes from the West Coast to Washington by air, but was carried out without any costly development contracts. We merely paid relatively low prices for such tubes as they were able to produce in accordance with our specifications.

It was on the 17th of June 1936 that I first met Mr. Lawrence Marshall, president of the Raytheon Corporation, and succeeded in interesting him in undertaking certain Navy contracts. This had far reaching consequences a few years later in the field of radar.

Besides the work specifically directed towards the development of radar, many other interesting studies in the field of very high frequency were carried out in the Special Research Section of the Radio Division. Altogether, during the period under discussion, some 20 projects were under way. Only a few of these were actually carried to a final conclusion due to lack of financial support and insufficient personnel. Later on a number of these projects were revived when funds and personnel became available. I will mention only a few of the projects which fall in the second category.

As early as 1934 Mr. W. C. Curtis did important work with magnetrons operating at about 750 megacycles. Curtis, it may be noted, is now employed at the David Taylor Model Basin at Carderock, Maryland. The records show that in the spring of 1936 10 centimeter (3000 megacycle) radar work was being done using as targets passing ships in the Potomac River. The men on this early 10 centimeter work were Mr. Hagen, Dr. Cleeton and Dr. Philpott with a few assistants.

In June of 1936 the first work was done with pulsed magnetrons with the idea of using the equipment for developing a suitable radar type of altimeter. However, the erratic behavior of the magnetrons then available led us to substitute certain new triodes which appeared to be more reliable for this type of work. The pulse radar type of altimeter produced a few years later was the outcome of this work.

The problem of an accurate and reliable recognition system is one that has been of great concern to the armed forces for many years. In the middle 1930's no one in the Army or Navy had a suitable device which would permit certain identification of possible targets either on the ground, the sea, or in the air. The necessity of such positive identification should be obvious. Long range guns are capable of firing on targets so far away that positive identification, to determine whether they are friendly or enemy targets, is often entirely impossible by optical methods. Because of the relatively long ranges desirable for recognition equipment it had long seemed obvious to me that identification should be carried out by the use of some form of radio. In order to prevent the enemy using false signals to confuse the identification it was obvious that the equipment had to be capable of being coded in such a way that even if the enemy succeeded in duplicating the equipment, he would not necessarily have the code for the day and therefore would not be able to correctly duplicate the identifying signals.

A few years later the British introduced the term "IFF" meaning Identification, Friend or Foe, to be used in referring to radio (or radar) identification equipment. Therefore I shall use the term IFF in that sense from now on.

Our first identification equipment developed in the middle 1930's was known as the XAE. It was intended to permit a ship to determine the identity of approaching planes. This equipment, operating in the 500 megacycle band, involved an installation on the plane which permitted the transmission of a 500 megacycle wave modulated at 30 kilocycles. This 30 kilocycles in turn was modulated into dots and dashes by a small rotating device built into the transmitter. This device was provided with a number of discs each giving a different identifying code. These discs could be changed every hour or so, if advisable, in accordance with a prearranged schedule of identifying codes. The pilot, on approaching friendly ships, could turn on this equipment and leave it on as long as he was in the neighborhood of friendly ships or of unknown ships which might be friends.

The ship carried only a receiver, properly designed to interpret these signals. This was connected to a mildly directive antenna array. The whole equipment on the ship was portable and intended for operation from the bridge. When approaching planes were seen the operator on the bridge swung his directive Yagi type antenna (named after a Japanese scientist who first used this simple directive array) and listened for signals. If no signals were received it could be assumed that the plane was an enemy plane.

In the tests of this equipment from the roof of one of our buildings against planes provided by the Naval Air Station, Anacostia, reasonably satisfactory results were obtained, so it was decided to send the equipment to the Fleet for more practical tests. Unfortunately, money for civilian travel was so scarce that we were not allowed to send our engineers out with the equipment and the results obtained in the Fleet were not satisfactory. The equipment, operating in a frequency band where none of our fleet people had had any experience, was naturally difficult for them to handle. Also, it must be admitted that the equipment was far from being a finished product. Tests made in the Fleet somewhat later gave us more promising results.

Like other Government institutions, the Navy Department had to keep its money in different pockets and, especially in times of peace, was not permitted to transfer money from one pocket to another. This frequently resulted in false economy and delays in production of new equipment. The Navy, ever since I have known it, has been very tight with funds for civilian travel, telephone and telegraph communications, and for automobile and truck transportation. During a war these difficulties did not exist, at least not during World War II. In the long run the Navy saves no money by this false economy, but loses money and has to accept unfortunate delays. The Navy Department is not wholly responsible for this policy; it is primarily due to the fact that the appropriations for these services are carried as separate items on the appropriation bills.

The IFF which I am discussing had certain very definite faults. We would never have recommended its final adoption unless these faults had been remedied. In the first place, the evidence of identification was not sufficiently conclusive. The friendly plane might have a faulty transmitter or the pilot might not have turned on the equipment soon enough. In other words, the evidence was negative rather than positive. Nevertheless the experience with this equipment led finally to the development of the Mark IV IFF equipment a few years later. This equipment, operating in the same frequency band, permitted a challenge to be emitted by the ship which was automatically answered by a plane using coded pulses similar to radar pulses. Indeed the equipment was used in connection with ship's radar, which actually made the challenge.

These sets went into a very limited production for the Army, Navy and the British but were never generally adopted, principally because of British opposition, based on the fact that the frequency was somewhat close to that used by German radar. Thus the British were afraid that the Germans would pick up the IFF signals too readily and learn how to jam them. It should be perhaps noted at this point that the British were using, as early as 1939, a radar challenge and pulse reply IFF system and should be given the credit for being the first to use such a system in actual warfare.

Throughout the period covered by this chapter the work was continued on the aircraft altimeter problem, particularly the pulse type of altimeter although this work was not brought to a successful conclusion until 1940. Indeed the altimeter project and recognition project were so closely related in technique that they were largely handled by the same group of engineers.

In the summer of 1934 the combined fleets carried out extensive training exercises off the Atlantic Coast. Since I had not been out to sea with modern naval radio for a number of years I requested orders which permitted me to be with the Fleet during these maneuvers. I was attached to the battleship WEST VIRGINIA which was the flagship of one of the battleship divisions. The Flag Officer in question was Admiral T. M. Craven. Captain Stark, later Admiral and Chief of Naval Operations, was on Admiral Craven's staff.

I had an opportunity to observe the way various radio circuits were performing and to see how radio was intimately connected with the maneuvering of ships in battle formation. It amazed me to see how many radio communications appeared to be necessary in order to carry out even relatively simple maneuvers when a large fleet was operating in formation. The general plan in those days was to have the battleships in a fairly compact formation, flanked by destroyers ahead and on either side and still farther out in the direction of the potential enemy, a submarine screen. The area of the sea surface covered by such maneuvers may amount to many thousands of square miles. Information as to the positions of ships and possible enemy contact is communicated to the ships immediately interested. It takes a surprisingly large number of channels to properly carry out such maneuvers. It did seem to me that a good many messages were sent which didn't serve any very useful purpose, but the radio officers insisted that in any event they were excellent training for those people manning the radio networks.

I have nothing to report in respect to this cruise which is of special technical interest, but I will mention two very curious incidents that happened while the Fleet was coming back into Newport after the conclusion of the maneuvers. Both happened early in the morning while the battleships were strung out in a long column about 500 yards apart and steaming at fifteen knots.

Two sailors on the after end of our ships were painting the jack staff. One of these sailors fell off the stern of the ship. There are two hazards in an accident of this sort; a man may very easily be cut to pieces by the ship's propellers, or he may be swept so rapidly astern in the back wash of the propellers that it is impossible to get a life preserver to him in time. In a good many cases of this sort the next ship behind the column, having been alerted, will pick the man up. In our case the sailor's companion immediately threw a life buoy over the stern of the ship but it was caught by the back wash and rapidly swept astern far out of his reach.

This man was saved by a peculiar bit of good luck. There is a region of water astern of most ships which actually follows the ship. Between this region of water and the stern itself there is a dangerous area wherein the man may be drawn into the propellers. Back of this region of water the back wash takes effect and the man is rapidly swept astern. The man who fell overboard was fortunate enough to get into this region of "following" water. He told me that he didn't have to swim at all; he merely was obliged to tread water to keep afloat. Thus he sailed along at fifteen knots ten feet back of the ship. A rope was thrown to him and he was readily hoisted aboard.

Later I took advantage of knowledge of this phenomenon on board my own small yacht because I found that following water came right up to the stern of my craft on account of the shallow draft at the after end. I used to put my fish in a wooden box with a wire screen across the bottom and lower the box over the stern of the ship close to the stern. That box would follow the ship back to port without any line attached to it although I generally used one so that I wouldn't lose the fish if I unexpectedly stopped or made some unusual maneuver. In towing a row boat behind a small ship, especially in quiet water, advantage can be taken of this same effect so that the boat will tow very easily indeed.

The second incident on this same morning occurred at the other end of the WEST VIRGINIA when we struck a whale which I would estimate to be about seventy feet long. He was buckled so tightly across the bow that he couldn't get loose, the pressure of the water holding him in place. Naturally we were pushing a tremendous bow wave and were being slowed down, so the captain signalled to the Flagship requesting permission "to turn out of line to clear a whale". We had to repeat this message four times since it didn't seem to be a reasonable message. The operators were sure it had been garbled. Finally we received permission. We turned out a little to one side and slowed down so that the whale was able to get away, although evidently pretty seriously crippled. It is not unusual for a naval ship to hit a whale, but it is usually a glancing blow. I never heard of one being pinned across the bow of the ship. A collision with a whale, incidentally, can be a very serious matter to a smaller ship, such as a destroyer.

It will be recalled that during the period covered in this chapter, the Germans were interested in establishing an aircraft transportation service with this country using large dirigibles. Many people will remember seeing the great German dirigible HINDENBURG and remember that it finally crashed at Lakehurst. I was drawn in as an expert witness during the investigations following this crash.

The HINDENBURG arrived over the Navy's Lakehurst field, the only one on the East Coast capable of handling such a ship, during very thick weather. The ship cruised around for a good many hours, waiting for the visibility to improve, finally coming in with the intention of tying up to the mooring mast. Captain Preuss was in charge of the HINDENBURG and Captain Lehman was a passenger. Captain Lehman was on his way to confer with Dr. Eckener, who represented the German Company in negotiating with the United States with respect to the acquisition of a suitable airport and permission to carry on a regular commercial transportation service with this country. Eckener was already in this country.

Captain Lehman was the author of a book covering the operations of the German Zeppelins during peace and war. I have never seen this book translated into English although it should be, because it is an extremely interesting and entertaining document. Captain Lehman himself had a very charming personality and had been with the Zeppelins ever since they started flying.

On arriving over the Lakehurst base, the HINDENBURG dropped her drag rope, which was seized by the Navy ground crew in order to lead the ship to the mast and complete the mooring. Just at this moment there was a burst of flame and in a matter of seconds the whole ship, after a few minor explosions burst into flames and settled rapidly to the earth. The wind was from one side so that some passengers were able to escape through the windows of the cabins on one side of the ship, making a jump of 8 or 9 feet to the ground. Everyone who attempted to get out of the other side of the ship was killed or badly injured as the roaring flames completely blanketed everything on that side. The casualties were very high; many passengers lost their lives as did many of the crew. Captain Preuss, although terribly injured, escaped with his life. Captain Lehman was killed outright.

I drove up to Lakehurst, accompanied by my wife, the day before the investigation opened and got in touch with Lieutenant (now Captain) Watson, the radio officer of the Lakehurst Station, and reported to Captain (now Admiral) Rosendahl, retired. I also met Lieutenant Commander Anton Heinen. Heinen was a strong anti-Nazi and a very good Zeppelin pilot with much knowledge of the construction of the ships. He has been given, through some special dispensation, a commission in the United States Navy. I was well impressed with his ability and judgment. He spoke English very well although we carried on most of our conversation in German. I am sure he was absolutely loyal to the Navy.

It was plain from the start that the Nazi government, which immediately sent representatives over to sit in at the investigations, wanted to claim that the ship was destroyed by sabotage. They plainly hinted in certain quarters that the American Jews must have had something to do with the affair. Of course this was rather ridiculous. It was pointed out to them that if any such thing had happened it must have been accomplished by planting a time bomb in the HINDENBURG before she left Germany, in which case we had nothing to do with the matter. In the second place, it was pointed out that since the Germans were going to operate trans-atlantic aerial transportation service, they couldn't very well shut out Jewish passengers from the United States, since many Jewish business men have financial interests in Europe and probably would be among their best customers. Whether as a result of this or not I do not know, but the Nazis "piped down" on this charge of sabotage. As a matter of fact no one ever discovered what caused that crash. If anyone had planted a bomb and set it to explode a few hours after ship made the mooring mast at Lakehurst, so that the loss of life would not be excessive, it could have happened when it did, since the HINDENBURG was delayed a number of hours behind the scheduled arrival at the mooring.

I never saw a more disagreeable crowd than this Nazi group. It included representatives of the Nazi party and government, also some scientists. One and all they seemed a hard boiled and vindictive lot. Incidentally, they had no use for Lieutenant Commander Heinen nor he for them.

Failing to make the charge of sabotage stick, they next asserted that certain radio stations in the vicinity must have emitted signals that induced sparks in the rigging and set off some hydrogen gas. I was ready for them there, having collected information on the frequencies and powers used by all stations in the vicinity and was able to prove that it was impossible for them to have developed such considerable voltage in the rigging as to cause any such action. It was difficult to get much evidence out of the wreckage. It was a terrible sight. The fire had thoroughly consumed so much that there was little available for examination. The motors were about the only things left which were intact. We did find one electrical contact from a certain indicating instrument which might have created a small spark during operation, but there was no conclusive proof that it had done so.

All such ships as this collect a heavy charge of electricity, so that when they first make contact with the earth a spark might be given off. However, since the first contact was made by the dragging rope, a Manila line, thoroughly moist on account of having been carried around in foggy weather for several days, it was obvious, to me at least, that any charge on the ship would have leaked off slowly and quietly through this drag line without doing any damage.

There were, I believe, three investigations of this crash; one was a Naval court of inquiry; the second, in which I was involved, was carried on by our Government and the third by the Nazis. They all ended in an impasse as far as explaining the disaster was concerned.

I was impressed with the extraordinary skill and ability in the field of aerial navigation and meteorology displayed by the Germans. These great ships flew for many years with very few accidents. I was also glad that our own ships were filled with helium instead of hydrogen. Had the HINDENBURG been filled with helium it wouldn't have been able to carry as high a pay-load, but on the other hand it wouldn't have exploded.

During the latter part of the period covered by this chapter Captain Hollis M. Cooley was appointed Director, June 17, 1935. Commander L. R. Moore served as Assistant Director from 1935 followed by Commander (later Captain) Lyman K. Swenson in 1937. I am sorry to say that Captain Swenson was later killed in action in the Pacific.

Among the officers acting as Technical Assistants was Lieutenant (now Admiral) W. S. Parsons who was Ordnance Officer from July 1933 to May 1934. Admiral Parsons is now right hand man for Admiral Blandy for the Crossroads Project at Bikini, in charge of all technical activities. Lieutenant (now Cpatain) E. H. Pierce was Radio and Sound Officer from July 1933 to 1935. He was followed by Lieutenant Commander Maurice E. Curts (now Rear Admiral Curts). Curts and Pierce were both extraordinarily effective men and of enormous assistance to the Laboratory. I am not surprised that they both distinguished themselves during the late war.

Captain Cooley did more towards selling the Laboratory to the Naval Service than any director who had preceded him, or for that matter, almost as much as all the former directors put together. He comes from an interesting family, his father having been for many years dean of the School of Engineering at the University of Michigan. Dean Cooley was well along in years when I first met him, but of extraordinary ability and acumen, in addition to which he had a very charming personality. One of the Captain Cooley's brothers was long identified with the engineering program undertaken by the Chicago Drainage Commission. Captain Cooley himself doesn't claim to be a distinguished engineer but he has another characteristic which was of the highest possible value to the Laboratory; namely, the ability to make friends in all quarters and to persuade every high ranking officer in the Navy who happened to be in Washington for a few days, to come down to the Laboratory and see what was going on. Of course our work in radar was the big drawing card, but although Captain Cooley would inveigle important people down in order to see radar, he never let them go without giving them a bird's eye view of a lot of the other Laboratory activities.

I very well remember when he brought down Admiral W. H. Leahy (so long attached to the President's staff) with the Secretary of the Navy, Charles Edison. This was for a radar demonstration, although they saw a number of other things. Fortunately for us the demonstration was unusually good that particular day. Mr. Edison was at the Laboratory on a number of other occasions. He is certainly a firm believer in research, both inside and outside the Navy. At the same time he modestly disclaims any technical ability on his own part. When at luncheon one day, I heard him admit that he had an engineering education (MIT) but that it didn't "take" very well. Personally I think he was too modest.

At the end of this period then, we find radar out of the pioneer stage and ready to make a start towards development and installation; we find high frequency communications pushed up to higher and higher frequencies, opening up more and more channels for both Government and commercial use; television was making excellent progress; recognition equipment was getting a fairly good start; homing systems were well on the way to production and practical application within the Fleet, but not yet completed; the solution of the radio altimeter problem was in sight. The Laboratory was beginning to be provided with much larger funds and personnel, particularly in the radio field, was being expanded very rapidly although not as rapidly as should have been the case. The fact is that none of us after being starved for years, could accustom ourselves to thinking in terms of millions rather than in hundreds of dollars. We continually underestimated our immediate and future needs. The work that we were to be called upon to do from now on would have been done more quickly, with less cost and with better end results, if we had had more liberal support during the years of peace.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER XIV - 1938-1941

PREPARATION FOR WAR

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1938-1941 - Preparation for War

Ominous developments in the international political situation, particularly in Europe, brought many thoughtful people to the very disagreeable conclusion that it was entirely possible that a second World War might develop. It seemed to many of us more than likely that the United States would be drawn into this conflict.

I am aware that many people believe officers of the armed services to be "war mongers", not interested in measures for the prevention of war. Having been a Naval officer for over five years myself and for another twenty-five years closely associated with a large number of Naval officers, as well as with a considerable number of Army officers, I believe I am qualified to give an opinion on this point, based on actual experience over a period of thirty years.

It is probably occasionally true that some very young officers in the lower ranks take a very belligerent and unfortunate attitude on this question. It is, of course, true that promotions in the armed services are slow in peace times and very rapid, in fact too rapid, in times of war. Thus eagerness for rapid advancement may cause a few of the younger officers to make statements which would appear to give some basis for misconception by the general public as to the attitude of the armed services. But the younger officers do not control the policies of those services. I have yet to hear any officer of either the Army or the Navy, above the lowest echelons in rank, advocate war. This may seem a little surprising in view of the fact that these very men have been trained for a large part of their lives in the armed forces. There is no doubt that these men are willing enough to fight once war has come, but there is every practical reason why they should personally prefer peace to war.

The life of a Naval officer in particular has its drawbacks even in peace time. Normally an officer serves three years at sea for every two years he serves on shore. Thus the amount of time he can spend with his family is definitely limited. Moreover his shore duty is unlikely to be repeatedly in the same place, so that he is not always able to set up a permanent home. However in times of peace the ship on which he serves generally operates out of some particular base as New York, Pearl Harbor, San Francisco, San Diego, or Bremerton, etc. Thus in between fleet exercises he is able to spend some time with his family during the period of sea duty. This means of course that the Navy wife must always be ready to move to a new port.

In time of war it doesn't generally work out this way. For instance, I have a son-in-law who served on board a battleship in the Pacific for three years and during two years of this time he was not within two thousand miles of Pearl Harbor, to say nothing of California. In over three years' time he was only able to make one flight to southern California to see his wife very briefly. Thus a Naval officer leads a much happier life, a better family life, and enjoys more social amenities by far in peace time than he does in time of war.

Nevertheless it is true that officers of the armed services have the obligation of always looking for the possibility of war and making adequate preparations for it. Most of them are definitely realists, but equally definitely, taken as a class, they are not war mongers. Because they talk about the possibilities of war and what we ought to do to prepare for it, they are probably often misunderstood and thought to be advocating war.

As far as my thirty years' Naval experience goes, I am convinced that not one officer in the armed services out of a hundred has any such unfortunate attitude. The older officers in the services are by the very nature of their profession students of international politics and keenly alert to any developments in their field. Certainly many of my friends in the Navy at the beginning of 1938 took a very gloomy attitude as to the likelihood of our being drawn into a second World War. They realized only too well that we would have to fight on two or more widely separated fronts and that the Navy was not yet large enough to take on such a job with complete confidence.

In my own case perhaps I was particularly interested in the situation developing in Germany, because in the summer of 1937 my son went to Germany, coming home on Christmas day of 1938. Although his head quarters were at Dresden, where he worked under Professor Barkhausen, he spent a great deal of time traveling around Germany, Belgium, Holland, France and Italy.

He spoke the German language fluently and through it became quite friendly with many Germans, and indeed was often mistaken for a German himself. Of course all his letters to me had to pass the German censorship but I could read a great deal between the lines. Furthermore, he saw to it that I got copies of a German newspaper every week so that I was able, due to my own previous experience in Germany, to get an insight into how the Germans themselves were looking at the international situation.

This information directly from Germany increased my determination to do everything we could at the Naval Research Laboratory in the way of preparation for war, in so far as these preparations were affected by the work of the Radio Division. As we saw it early in 1938, our most immediate problems were as follows:

First: Assist the Bureaus in getting radar into production and to the Fleet at the earliest possible moment.

Second: Complete our work on radio homing devices which would get our planes back to their carriers or to their shore bases safely.

Third: Develop radio control devices for pilotless planes, or drones as we called them, which would give the Fleet experience in shooting at targets which were actually maneuvering instead of at towed sleeves which could not be maneuvered and were relatively easy to hit.

Fourth: Develop new channels of communication as needed in the very high frequency field.

Fifth: Develop high frequency direction finders and assist the Bureaus to get into production with them.

Sixth: Complete the pulse altimeter for aircraft and push work on instruments to measure drift. This means a determination of the actual movements of the plane over the ground or sea regardless of the influence of wind. It is very necessary for long range navigation.

Seventh: Develop devices for protection of harbors, particularly portable or semi-portable devices which could be used at advanced bases.

Eighth: Push the development of IFF equipment.

There were many other items of lesser important but, although the Radio Division had expanded considerably by this time, we felt unable to cover all the ground and proposed to concentrate the major portion of our efforts on these eight problems.

Before actually going into production with radar, it was necessary for us to get up a really finished radar equipment at the Laboratory and take that equipment to sea on a first-class ship. The 200 megacycle search radar which was now performing very well on the roof of Building 1 at the Laboratory was in no sense satisfactory to take to sea. It was necessary to make a complete engineering redesign and build the antenna, the radar equipment proper, and the controlling gear in such a way as to meet the Navy standards of ruggedness and durability.

A very important conference, destined to have far reaching consequences as far as the development of radar was concerned, was held on the 24th of February 1938 at the Bureau of Engineering. It would appear from the record that Captain J. M. Irish of that Bureau presided at that meeting, assisted by Commander W. J. Ruble, then head of the Radio Division of the same Bureau.

The Naval Research Laboratory was represented by the Director, Captain H. M. Cooley, Lieutenant Commander M. Curts, and myself. Rear Admiral Furlong represented the Bureau of Ordnance, assisted by Commander W. A. Kitts; the Bureau of Aeronautics was represented by Captain A. C. Reed, Commander Ralph Davison and Lieutenant Commander DeBaun; the Bureau of Construction and Repair was represented by Captain H. J. Chantry; the office of Fleet Training in Naval Operations was represented by Captain R. C. McFall and Commander W.H.P. Blandy. The Office of Naval Communications was represented by Rear Admiral C. E. Courtney and Captain T. E. VanMeter. The ranks given these officers are those they held at the time of the conference. Most of them are now either retired or promoted to much higher ranks. Practically all of these officers had followed with great interest the development of the radar work at the Naval Research Laboratory.

The deliberations of this conference resulted in the decision to get a sea-going model of radar on board a major ship at the earliest possible date. We at the Laboratory were advised to "freeze" the design of the 200 megacycle equipment as soon as possible, and to start the construction of a unit to be completed at least by the end of the year 1938 and suitable for a vigorous test.

Revolutionary new scientific developments are normally the result of first, basic research; second, applied research; third, engineering development.

If the engineering development is successful, the next step is mass production followed by installation and special training of those who are to operate the equipment.

So far as radar is concerned, it may properly be said that the basic research, involving extensive studies in wave propagation, transmission, absorption and reflection of radio waves at various frequencies, had been accomplished many years before radar became a practical device. But the basic research had to be followed by a long period of applied research which had a definite aim in view, but which was no less difficult, nor of any lower order than the basic research which preceded it.

It is in the nature of research people to desire to continually go on with further improvements. No sooner do they succeed in eliminating difficulties from a particular instrument than they see new ways to make further improvements. Certainly no device ever pioneered by this Laboratory was turned out in such a state that when completed we could see no desirability of further improvement. But if this natural tendency of research workers to go on indefinitely improving is not curbed, it may lead to lengthy postponement of quantity production.

It is necessary for those in authority to step in at some point and say. "This is not perfect, but it is good enough to produce as it is. We will proceed with at least a limited mass production."

This "freeze" order for the 200 megacycle radar equipment was given in February of 1938. The equipment as it stood was turned over to the Special Development Section for putting it into suitable shape for installation on a Navy ship. Thus the original research group which was working in Mr. Young's Special Research Section, under the leadership of Dr. Robert M. Page, was able to go ahead with further improvements without worrying too much about the development and production program.

It was necessary, however, that someone in the research group maintain a very close liaison with Mr. Gebhard's development group in order that nothing should be done by that group, in the interest of standardization, compactness, strength, neatness, etc., which would interfere with the efficient operation of the equipment. Therefore, Mr. Gebhard continually called on men in the Special Research Section, particularly on Mr. R. C. Guthrie, Mr. A. A. Varela, Mr. Irving Page, Mr. E. L. Luke (now Lieutenant Commander), and Mr. Edwin Johnson. These people not only gave advice but actually gave much assistance to the development work. Mr. Gebhard was also assisted by Mr. H. E. Reppert. Mr. Reppert is an interesting combination of radio engineer, mechanical engineer, and expert design draftsman. He has had years of experience with radio equipment from the design side. It may be mentioned in passing that he is no longer attached to the Naval Research Laboratory, but is at the Navy Electronics Laboratory, San Diego, California.

It is of interest to examine the attitude of the ranking officers at the Navy Department with reference to the radar program. The Director of the Naval Research Laboratory requested a small addition to our personnel in order to expedite the work agreed upon at the conference on the 24th of February. This matter was within the province of the Bureau of Engineering under whom we were operating at that time. Although that Bureau was fully in accord with the idea of pushing the radar program, our request for additional personnel was denied in a letter from that Bureau to the Director dated March 28, 1938. The Bureau stated that it desired us to do the work with what personnel we had, which was a pitifully small number, not over a dozen men being engaged in the radar program at that time. The Bureau did offer to reduce the priority of some of their problems which had been assigned to us for solution and to cancel other problems in order that we could divert personnel to the radar program.

At about this same time, a letter signed by the Commander-in-Chief of the U. S. Fleet objected to the proposed size of the radar antenna and stated that the plans for installation seemed not entirely satisfactory. We had originally proposed to put the equipment on the carrier YORKTOWN, but the Commander-in-Chief suggested the small antiquated USS LANGLEY, which was also preferred by Admiral A. B. Cook, then Chief of the Bureau of Aeronautics. Admiral Cook urged great haste in the installation, followed by immediate steps to train specialized radio men to look after the new equipment. This latter advice was sound and was immediately followed out except for the choice of ship which was shifted later to the battleship NEW YORK, partly because Admiral A. W. Johnson, commanding the Atlantic Squadron, had seen our radar equipment and was very eager to give it an opportunity to be tested at sea. It was naturally more convenient for us to make the installation on the East Coast rather than on the West Coast.

The attitude of the Chief of the Bureau of Ordnance, as expressed in a letter of June 1938, indicated that he considered radio detection equipment vital, but that it must be so installed that it would not interfere with armament.

A suitable site on the battleship NEW YORK had been picked out in the spring of 1938 by Mr. Young, who accompanied by Mr. John P. Hagen, Mr. G. E. Pray, and Mr. Frank Isely made a cruise on the NEW YORK between January 1 and the end of March 1938 for the purpose of testing limited range radio "bridge-to-bridge" communication in the 500 megacycle frequency band. Incidentally these experiments paved the way for the opening up of new channels in the very high frequency fields for communication purposes, as well as for IFF equipment. The site selected by Mr. Young on the NEW YORK for the rotating antenna of the 200 megacycle search radar was adopted in December of 1938 when the XAF set was installed. The location of the transmitting and receiving equipment was also substantially the same as had been previously indicated by Mr. Young.

In order to get an adequate radar beam for reasonably long ranges, it was necessary to use a large rotating structure approximately 17 feet square. It was expecting a good deal of a Captain of a battleship. Not many would have even considered such a large equipment so high up in the ship's superstructure. We all felt very grateful to Captain (later Admiral) R. M. Griffin, Commanding Officer of the NEW YORK, and to Rear Admiral (later Vice Admiral) A. W. Johnson, the Flag Officer in control of the Atlantic Squadron. They must have felt that this "flying mattress", as the sailors promptly dubbed it, was not only a disgraceful looking object to put on a ship, but presented some definite hazards. Nevertheless they had faith in the project and gave us the most enthusiastic backing.

In view of the size of this rotating beam structure, we decided that it must be built with materials as light as possible, consistent with strength, since it would have to withstand a great deal of wind pressure at the high elevation at which it was to be installed. We called in the Brewster Aeronautical Corporation, who had much experience in duralumin construction, and turned over to them the building of the rotating directional antenna. The Brewster people were first contacted in July of 1938.

In the meantime the Bureau of Engineering had disclosed the basic principles of our work to RCA and had given them a project to build an experimental radar operating near the 400 megacycle band, with instructions to get it ready in time for this same cruise.

As a matter of fact, even with all of our previous experience, we found it a tremendous task to get the XAF, as this pilot model was called, ready in time to get aboard the NEW YORK in December. The RCA group was really not quite ready but their equipment was nevertheless aboard in time to sail very early in January in the same squadron.

The XAF was installed in December of 1938 on the NEW YORK at Norfolk Navy Yard. Dr. Page, aided by Mr. Guthrie and Mr. Reppert, went with the XAF. Mr. Varela was put on the USS TEXAS to assist the RCA engineers in every possible way. The RCA men were Mr. Charrier, Mr. Thompson and Mr. Evans. The RCA set was not expected to give high grade performance because it was really far from being in ship-shape condition, but it did work well enough to prove that radar was entirely possible at 400 megacycles with the advantage that the rotating beam equipment was decidedly smaller than similar equipment at 200 megacycles.

There was great difficulty in keeping the 400 megacycles equipment in operation long enough to get good measurements, but this had been expected. The XAF operated for nearly three months with an average of nearly twenty hours per day. We had a few breakdowns, but none was serious. They were usually repaired in a few minutes. Most of the faults were due to burning out of tubes which were perhaps the weakest link in the radar system.

These tests were made on the high seas in the course of military maneuvers. This was, therefore, a real test of the ability of the equipment to stand up under seagoing conditions and under the shock of gunfire from our own ship.

Almost as soon as the NEW YORK put to sea, it became obvious to the interested officers that radar was destined to be a tremendous aid to navigation. It must be remembered that this equipment did not have the fine discrimination of later equipment either in range or in bearing. Nevertheless fairly accurate ranges and reasonably accurate bearings on land and ship targets were continually reported to the navigating officer. Many of these ranges and bearings could be checked by optical methods. Indeed, at one time while operating in the vicinity of the Virgin Islands, the ships had been executing intricate maneuvers during the better part of the night and even the navigator on the NEW YORK didn't know his position exactly. Dr. Page, while taking bearings on mountain tops on distant islands was able to fix the ship's position quite satisfactorily.

This first radar equipment was shrouded with a good deal of secrecy. Only the senior officers on the ship and such few enlisted men as had been assisting in its installation and were in the process of training in its use, knew what the equipment was supposed to do. Naturally there was much speculation about the "flying mattress" but the sailors were generally given to understand that it was just a new type of direction finder. Since all of them were more or less familiar with radio compass installations, this explanation satisfied them.

During one of the night maneuvers a group of destroyers, coming in from a considerable distance, were supposed to locate and surprise the battleships. While these destroyers were still eight miles away they were all picked up accurately in range and bearing by the XAF and their positions reported to the Admiral. He allowed them to come in to 9000 yards and then a searchlight having been previously trained exactly in accordance with the radar bearing was suddenly flashed on. The Captain of the nearest destroyer said that this searchlight practically "hit him on the nose". Of course this meant that the battleships knew where the destroyers were before they had started their torpedo attack. In actual warfare a condition like this would have meant practically certain destruction to the destroyers.

The Commanding Officer of the destroyer squadron came aboard a few days later and demanded to know how this was accomplished. He was taken in on the secret and shown the apparatus. Since he was still rather skeptical, the Admiral said he would have another exercise that night. The destroyer Captain now had the advantage of knowing that we had the radar and so he would be able to see whether he could evade it. That night the Commander of the destroyers split his attack force into three groups, coming in from widely separated directions. Nevertheless Dr. Page and his assistants picked them all up. They were theoretically immediately put under fire by the battleship. The next time the destroyer Commander came on board he was entirely convinced, especially when he was told that these things could be done in a dense fog.

Commander Parsons, now Rear Admiral, suggested very early in our radar studies that it should be possible to track shells by radar during their flight from the guns to the target. At the time when he made this suggestion none of us were sure that this could be done with the frequencies available. We did believe that it could be done when radar was operating on much higher frequencies. Theoretically the reflections from shells should be much better at higher frequencies than 200 megacycles.

We were, therefore, very agreeably surprised when we found that the XAF radar on the NEW YORK could not only pick up and follow the NEW YORK shells in their flight to the target, but could frequently follow the shells of another battle ship when that ship was not too far out of line. The ships were firing on targets on Viequez Island, at a range of about 13,000 yards as I remember. The radar not only followed the shells but was able to indicate whether they were falling short of the target or going over it. If a shell fell short, it struck the water, sending up a tremendous column of spray, which gave an excellent (although temporary) echo, thus marking the range at which the shell had come down. If the shell struck on land, it raised a column of sand and debris which always gave a reasonably good echo. Since the target itself generally showed as a pip on the radar, it was easy to immediately announce to the fire control officer how far over or under the shells had struck. The bearing of the shells could not be obtained so readily, due to the width of the radar beam and the fact that the antenna could not be moved rapidly enough to take a bearing measurement during the short time of the flight of the shell. It was not until later when scanning radar was developed that this could be done.

This method of spotting shell splashes by radar found many useful applications in the Pacific War and elsewhere. Many a Japanese ship lies on the bottom of the ocean due to the intelligent application of radar spotting by our fire control officers. Such spotting is of course particularly valuable at night when firing, entirely on radar information, at an unseen enemy.

From a continuous observation of the range of the target and the range of the shell splashes, information is immediately obtained permitting exact adjustment of the elevation of the gun. During the NEW YORK cruise, Dr. Page 's assistants took photographs of a number of these radar displays during firing. On one of these photographs, the position of the target, the position of the shell in flight, the position of the shell's splash, and finally, the position of the spotting plane over the target can all be observed.

Page and his associates were probably the first to observe radar echoes from large birds. He tracked some moving targets, traveling faster than a ship could travel, but not as fast as a plane could travel. They turned out to be large birds. Our ships had considerable trouble in the Pacific at times with radar echoes of this nature, some of these echoes coming in from relatively long ranges. One region was said to be infested with what became known as "the galloping ghost of Nansei Shoto". Under certain conditions, probably due to ionization, clouds will occasionally give "false" radar echoes. On a few occasions our ships have fired on radar targets which had no reality.

These tests in the Atlantic Squadron on board the NEW YORK and TEXAS succeeded in determining practical ranges for radio echoes from battleships, destroyers, submarines wholly surfaced, submarines awash, and on small surface craft. Many observations were also made on airborne targets wherever possible.

Comparison of ranges by radar and by optical methods were obtained. It must be remembered that all optical range finders show increasing errors in range measurements as the distances increase. This is due to the fact that the measurement of range is accomplished by measurement of a very small angle subtended at the target by lines of sight from the opposite ends of the range finder. Since for distant targets this angle becomes extremely small, the errors introduced into the range measurements become very large.

Radar, on the other hand, measures range just as accurately on the most distant target which it can detect as it does for nearby targets. In other words, if it can place the range of a target within an error of fifty yards at a five-mile range, it can also place it within fifty yards at 100 miles of range, provided the target gives any echo which can be perceived on the radar screen. This means that for nearby objects the optical range finder is at least as accurate, and sometimes more accurate, then the radar, but for ranges in excess of a few miles radar distance measurements or range are very much more accurate than those obtained by optical methods.

It was of great interest to both us and the ship's officers to check range measurements by optical and radar methods in the daytime when the targets were plainly visible. The results of these comparisons were very encouraging and created a definite feeling of confidence on the part of the officers.

It is interesting to note that Dr. Page recommended, in his report written on this cruise, that radar beacons be established on prominent headlands and on various buoys and lighthouses as an aid to navigation. This was probably the first suggestion, in this country at least, for such use of radar beacons. Dr. Page proposed certain specific reflectors which would give easily identifiable echoes.

When the NEW YORK was enroute to Fort de France in Martinique, there was a time when the ship's position was known to within only fifteen miles, based on the navigator's data, but the XAF was able to fix it within three miles.

A number of experiments were made on what is known as "station keeping". This means keeping the formation of the ships with respect to each other, and knowing at all times the position of all ships in company. Here the radar was invaluable, since it worked during the night hours when all ships were blacked out, as in battle, just as it did in the daytime. The XAF was not satisfactory for this purpose when the ranges were smaller than 1200 yards because the pulse length was too long and the frequency too low to give a highly concentrated beam which would permit reduction of echoes from water waves. Needless to say this condition was rapidly remedied in the succeeding years when radars became available on much higher frequencies with much tighter beams and very much shorter pulse length. Position keeping, or station keeping as the Navy calls it, became a routine matter as soon as our ships were largely equipped with suitable radars.

We were interested in determining whether the operation of radar would interfere with the ship's radio communication facilities. We had been seriously concerned about the possible interference of radar with such facilities but to our great relief we did not find this difficulty to be as serious as had been anticipated. It was, nevertheless, of sufficient magnitude to start immediate investigations on devices to protect some of our receivers on certain frequency bands from the peculiar type of interference created by the operation of radars on the same ship. This naturally took considerable time but the delay did not act as a serious handicap to the program of production and installation of radar equipment for our ships.

The engineers' reports on the operation of the equipments on the TEXAS and NEW YORK were forwarded by their respective commanding officers, together with their own letters of recommendation, to Admiral Johnson, Commander of the Atlantic Squadron. He in turn added his comments, and all of this valuable material was put in the hands of the interested people at the Naval Research Laboratory and in the Navy Department.

The report of Captain R. M. Emmet, Commanding Officer of the TEXAS, emphasized the fact that the CXZ 400 megacycle equipment furnished by RCA was not in such condition to show up well in the test at sea, but "the RCA personnel learned enough on this cruise to insure that the next RCA model would be much more practical and valuable". As explained earlier, we were all aware that the RCA had not had sufficient time to do a well rounded out job; nevertheless, not only did they learn valuable lessons from their experience on the TEXAS, but our engineers also benefited from those same lessons. These tests on the RCA model were, therefore, well justified and well worth while.

Captain R. M. Griffin of the NEW YORK, referring to the XAF said, "The device looks big but really causes very little inconvenience ..... it is well worth the space it occupies .... should be put on all cruisers .... developments should be continued but do not delay installation awaiting a perfect instrument .... include it in all new construction larger than destroyers and reserve space on destroyers for equipment of reduced size. The development of this device enhances the value of indirect fire which will soon be independent of aircraft spotting. The device should have the most far reaching results on tactics".

Admiral Johnson stated in his forwarding letter, "XAF equipment was markedly superior to CXZ in all respects except size. CXZ was not ready for shipboard test. The XAF equipment is one of the most important military developments since the advent of radio itself. The development of the equipment is such as to make it <u>now</u> a permanent installation in cruisers and carriers".

The endorsements in the forwarding letters of Captain Griffin and Admiral Johnson were indeed prophetic. As soon as our engineers returned to Washington a conference was held, in the Office of Naval Operations, presided over by Captain Crenshaw of the War Plans Division. This conference listened to Dr. Page's report and recommended immediate procurement of twenty radar sets to go on the battleships PENNSYLVANIA, CALIFORNIA, NEW YORK; on the cruisers INDIANAPOLIS, CHICAGO and two other light cruisers, and on five carriers including the YORKTOWN and ENTERPRISE.

The matter of procurement was, of course, in the province of the Bureau of Engineering with whom the Laboratory acted in an advisory capacity.

We went to work immediately on specifications which did not need to be too detailed since the XAF brought home from the NEW YORK was to be turned over to whatever company got the contract for the first production. A few improvements were to be incorporated by our engineers, based on the results of the NEW YORK tests.

In the meantime, full details of the equipment were disclosed on the 18th of May 1939 to Messrs. Merquelin, Wilson and Tinus of the Bell Laboratories, and on the 19th of May to Messrs. M. Brunet, W. O. Osborne, J. E. Love, C. M. Charrier, and N. E. Leroy of RCA. By the 15th of June 1939, complete specifications on "Radio Range Equipment" were ready for the two prospective bidders, RCA and the Western Electric Company. The contract was not for twenty models, since the Bureau of Engineering had decided that it would be better to purchase six models only on the first contract and then to increase the number later on.

The contract for these six units was awarded to RCA in October of 1939, it being understood that the Naval Research Laboratory was to assist in every possible way, and that frequent conferences would be arranged between the engineers of the two laboratories. These new equipments were given the name of CXAM, and the first preliminary model was delivered in November 1939. The first finished, or production model, was delivered in May of 1940.

These first six sets were destined for the cruisers CHICAGO, CHESTER, PENSACOLA and NORTHAMPTON, the carrier YORKTOWN, and the battleship CALIFORNIA. The first two of these ships were at that time at Mare Island; the next three at Pearl Harbor, and the CALIFORNIA was at Puget Sound. Shortly after these first six sets were delivered, a few minor changes were discovered to be desirable, and fourteen additional sets were ordered, being known as the CXAM-1 sets. These then were the first radars used when the United States Navy went to war.

Since the battleship NEW YORK was the first American ship to make full scale tests of radar at sea (XAF tests previously described) it will be appropriate to give a very abbreviated history of her war time activities. For this information I am indebted to the Ships Section, Office of Public Information, Navy Department.

The NEW YORK, one of our oldest battleships, being commissioned in April 1914, was the first, with the TEXAS, to be armored with 14" guns. She was used as a flagship by Admiral Frank Fletcher at the time of the Mexican Incident off Vera Cruz. In November of 1917 (World War I) she became the flagship of Admiral Hugh Rodman, joining the British Grand Fleet in the North Sea.

During World War II she made two convoy trips to Iceland, two to Scotland, and two to Casa Blanca. She made a distinguished record while participating in the North African landing (Safi), being credited with neutralizing the formidable Batterie Railleuse, the strongest coastal defense unit in the region.

Between July 1943 and June 1944, she was used for training purposes, operating in the Chesapeake Bay area. Following this she made three training cruises between Annapolis and Trinidad, British West Indies. On November 21, 1944 she started for the West Coast where she underwent an intensive program of gunnery training in preparation for further support of amphibious landings.

In January 1945 she proceeded to Pearl Harbor and then was sent to join other heavy units of the Pacific Fleet at Ulithi, which units were then forming up for the assault on Iwo Jima. On this trip she lost one blade from her port screw, reducing her speed to 13 knots. In spite of this she went to Saipan, joined Task Force 54, and participated in the rehearsal of the Iwo Jima operation.

In February of 1945 she participated in the pre-invasion bombardment of Iwo Jima. After three days of this she was withdrawn because of her damaged screw. She underwent repair at Manus and again joined Task Force 54 late in March. For the seventy-six consecutive days following, she remained in action off Okinawa, a longer period than that of any other capital ship. She participated in the pre-invasion bombardment of Okinawa, covering the landings on the beaches in front of Yontan Airfield and furnished close support for the advance of the Tenth Army and Marines from the beginning of the campaign to the very end. She expended five and one half million pounds of ammunition during this period of action.

After this she went back to Pearl Harbor by way of Ulithi and Eniwetok and was completely regunned. This was the first time that a major ship in the United States Navy had ever been regunned at an advance base. She was nearly ready for battle again when the war ended.

All in all the NEW YORK was a very lucky ship. She had fought against the Germans, the French and the Japanese, but was hit only once and then sustained only superficial damage and two minor personnel casualties. At the time of writing she is one of the target ships in the Bikini Lagoon for Operation "Crossroads".

It will be interesting to see what happened to the first six United States ships to go into battle equipped with radar.

The heavy cruiser CHESTER received news of the attack on Pearl Harbor while she was at sea. Her Commanding Officer, Captain Thomas M. Shock, immediately conducted a scouting operation and the next day took his ship into Pearl Harbor. She sortied again to conduct another scouting operation on the 9th of December 1941. This kept her occupied during the first month of the war. In January of 1942 she got under way with a task force bound for the South Pacific and played an important part in the Navy's first offensive against the Japanese in World War II.

First, under Captain Shock's command, she led two destroyers in the bombardment of Taroa Island in the Maloelap Atoll of the Marshall Islands on the 1st of February 1942. With her accompanying destroyers she was subjected to dive bombing attack and shore shelling. Although hit by one small bomb, the enemy planes were eventually driven off. The CHESTER was only slightly damaged and in spite of some fatalities and injuries among the crew, she later acted as part of the screening force for a carrier operating nearby.

The CHESTER then returned to Pearl Harbor for minor repairs after which she proceeded to the South Pacific to participate in the battle of the Coral Sea, May 7 and 8, 1942, acting as part of the escort for the carriers YORKTOWN and LEXINGTON. The CHESTER, while protecting the YORKTOWN shot down three plans. She was not hit although one torpedo missed her by a narrow margin. The YORKTOWN was slightly damaged in the operation and the LEXINGTON was damaged so badly that she was sunk later by our own gun fire. Two Japanese ships were sunk and others damaged. The enemy lost twice as many aircraft as we did.

This action had the decisive effect of freeing Australia from any immediate threat of enemy invasion.

In October 1942, the CHESTER operated in the Southwest Pacific with the task force under Rear Admiral Carleton H. Wright, USN. She carried the flag of Rear Admiral Norman Scott when she was shaken by a violent underwater explosion. Although she immediately lost all power throughout the ship no fires resulted. Before the CHESTER was able to get underway again it was necessary that she secure number 1 shaft. This was accomplished by Boatswain's Mate, First Class (then Coxswain) J. T. Coons who volunteered to undertake the task without diving equipment. Since the hub of the propeller was many feet under water and the surface covered with a thick film of oil, this was a remarkable job to be carried out on a pitch black night. After this the CHESTER was able to get under way.

Following repairs the CHESTER participated in the assault on Tarawa Atoll in the Gilbert Islands. Being now under the command of Captain Francis T. Spellman, she was part of the fire support unit which bombarded Betio Island, Tarawa Atoll, prior to the landing of the Marines November 19, 1943. Several shells hit close to the CHESTER and one passed between her stacks but she was not hit.

The next day, just before sunset, about fifteen Japanese torpedo planes came in low for an attack. Altho she had a close call, the CHESTER was undamaged.

The next operation of the CHESTER was in the Marshall Islands in January and February, 1944. She was then the flagship of the task group commanded by Rear Admiral Ernest G. Small USN. Day after day this force bombarded the island of Wotje and Taroa Island of the Maloelap Atoll, the same island that the CHESTER had bombarded two years earlier.

During this period the CHESTER conducted thirteen bombardments, ten of which were against the airfield and installations of Taroa. At the present time, I have no record of her subsequent history, but know that she is still in commission.

The battleship CALIFORNIA sank three days after the Japanese attack on Pearl Harbor due to damage sustained during the attack. She rested on the bottom with her main decks awash. The day after the attack her CXAM radar was removed, having suffered no serious damage, and was set up again on a hill eleven hundred feet high and manned by Naval personnel for use in backing up the Army radar in case further attacks were made by the Japanese.

It will be recalled that the Army radar had actually picked up the approaching Japanese planes on the fateful Sunday morning, while these planes were more than 100 miles away. Had that warning been heeded, it is certain that our losses at Pearl Harbor would have been very greatly reduced. Indeed, there are some who believed that the action would have been more disastrous to the Japanese than to us.

The CALIFORNIA was refloated on the 25th of March 1942 and on the 9th of April 1942 was placed in drydock. Her underwater repairs were accomplished. On the 7th of June 1942 she came out of drydock and departed under her own power for the Navy Yard Puget Sound, Washington. Here she was overhauled, reconditioned and completely modernized. After a period of operation off the coast of California she was ready by early 1944 to participate in the amphibious operations then going on in the Pacific. In these she played a distinguished part.

The CALIFORNIA's search radar (CXAM) was installed at the Navy Yard, Puget Sound. Radio Electrician (now Lieutenant) Clifton Shumaker was sent by the Naval Research Laboratory to assist with this job.

After this installation was made, and while the CALIFORNIA was proceeding down the West Coast towards San Pedro, she encountered extremely foggy weather and reduced speed for fear of collision with some of the coast wise shipping. However, the radio man operating her radar kept reporting the positions of all headlands and of all ships. The navigator soon noticed that every ship he overhauled or passed had been previously reported by radar. This was called to the attention of the Commanding Officer who, after observation of matters for a short time, ordered full speed, relying on radar to avoid collision with other ships and to fix his position by bearings on distant headlands. The captain on the CALIFORNIA estimated that he saved several days' time by this procedure.

It was the first instance, so far as I know, of the practical application of radar to the navigation of a battleship, with of course the exception of the experiments carried out with the preliminary model on the NEW YORK.

On December 7, 1941, the heavy cruiser PENSACOLA, under the command of the late Rear Admiral (then Captain) Norman Scott, USN was in mid-Pacific enroute from Pearl Harbor to Manila. When notified of the outbreak of the war, she changed her course for Suva, Fiji Islands, and after a brief stop there she proceeded to Brisbane Australia.

Early in February 1942, under the command of Rear Admiral (then Captain) Frank L. Lowe, USN she joined a task force under the aircraft carrier LEXINGTON. While this group was steaming northeast of Rabaul on 20 February 1942, it was attacked by eighteen enemy bombers. The PENSACOLA and other ships of the formation threw up a thick curtain of anti-aircraft fire and assisted in shooting down sixteen Japanese planes.

Her next action occurred in March of 1942 when she helped to protect the carriers LEXINGTON and YORKTOWN while their planes struck successfully at Japanese shipping off the New Guinea ports of Lae and Salamaua. She participated in the battle of Midway remaining close to the ENTERPRISE. On the morning of June 4, 1942 when the carriers launched the attacks which eventually decided that engagement, the YORKTOWN was damaged and the PENSACOLA was sent to protect her against further attack. Early that afternoon an air attack materialized but the PENSACOLA and other vessels raised such a successful curtain of fire that only a handful of planes came through. Nevertheless two torpedoes from enemy aircraft struck the YORKTOWN.

That evening the PENSACOLA was ordered to leave the damaged YORKTOWN and join other carriers for the pursuit of the Japanese force. She was with the ENTERPRISE on the 6th of June 1942 when our planes made the final attack on the enemy.

Early in October of 1942 the PENSACOLA was with a task group led by the carrier HORNET for air strikes at the Buin-Faisi and Solomons areas. She had completed this duty when the Battle of Santa Cruz Islands developed. The HORNET group at this time was in company with another group built around the carrier ENTERPRISE and the new battleship SOUTH DAKOTA. Early on the morning of the 26th of October, the carriers launched their planes to strike the enemy carriers, which had been located. These attack groups seriously damaged the enemy carrier SHOKAKU and hit a KONGO class battleship and at least three cruisers.

One enemy torpedo bomber attacking the HORNET found the PENSACOLA in the way and made a run on her. The PENSACOLA's anti-aircraft guns set this plane on fire at a distance of 1000 yards. The enemy pilot attempted a "suicide crash" but missed the bow of the ship by a few feet and fell into the water. The PENSACOLA and her group shot down 23 out of 49 attacking planes.

Toward the end of November the PENSACOLA joined four other cruisers and six destroyers to thwart a Japanese attempt to land reenforcements on Guadalcanal - an attempt which resulted in the battle of Tassafaronga.

Late on the night of 30 November 1942, the enemy was intercepted and attacked by torpedoes and gunfire. The PENSACOLA fired on what was believed to be a light cruiser. The enemy vessel sank after the fifth salvo struck it. A few minutes later the PENSACOLA swung her guns on another enemy cruiser, setting her afire with the first salvo and blowing her up with the second. She then immediately located another target and fired several salvos before losing contact.

Shortly thereafter a torpedo struck the PENSACOLA on the port side, starting fires and blowing flaming oil over the ship which started to list. Extensive flooding took place in the engine room and in some magazines. Nevertheless she was able to move slowly and made for port. After four hours the fires were brought under control. She arrived at Pearl Harbor for extensive repairs in January of 1943 and went out on her next assignment under the command of Captain Randal E. Dees, USN. In mid-November of 1943, as a unit of a carrier task group, she participated in the Gilbert Islands operation, bombarding the island of Betio in Tarawa Atoll. In the Gilberts the PENSACOLA covered the landings on Apamama and patrolled nearby waters against enemy interference.

For her next task she was assigned to the amphibious operation which was to secure the Marshall Islands. On January 29, 1944 she bombarded Taroa Island in Maloelap Atoll, blasting runways and installations. On the following day she repeated the performance at Wotje Atoll. Further bombardments of Taroa, Wotje and other points continued through mid-February 1944.

At this time I am not informed of her later actions, except that she is a target ship at Bikini and was badly damaged by the second (under water) atomic bomb.

At the time of the Japanese assault on Pearl Harbor, the carrier YORKTOWN was operating off the Atlantic Coast; later she was moved to the Pacific. In January of 1942, Mr. Guthrie, of the Naval Research Laboratory, assisted in the installation of IFF equipment on the YORKTOWN at San Diego, California. Her radar was installed at Pearl Harbor in January of 1942. The YORKTOWN served as Admiral F. J. Fletcher's flagship during the raid on the Gilbert and Marshall Islands.

In March of 1942, in company with the LEXINGTON and supporting cruisers and destroyers, she participated in the bombing of Salamaua and Lae on the coast of New Guinea.

On the 7th of May 1942 she participated in the Battle of the Coral Sea, where she was slightly damaged. She also took part in the Battle of Midway where her planes, with those of the carrier ENTERPRISE, largely decided that engagement in our favor. The YORKTOWN was so heavily damaged by enemy torpedoes that she was sunk by our own ships.

In 1940, shortly after the YORKTOWN received her radar at Pearl Harbor, she made a run to the California Coast and was the first ship to observe by radar extremely long range echoes, up to 450 miles, on distant mountains. Such echoes only occur during certain atmospheric conditions. Her observations created a great deal of interest when first reported.

The cruiser CHICAGO was the flagship of a force under the command of Rear Admiral J. H. Newton. I am not in possession of enough information to give a complete history, but do know that she sank in the Battle of Tassafaronga, one of the numerous battles that finally decided our control of the Island of Guadalcanal.

The heavy cruiser NORTHAMPTON participated in the bombardment of Wake Island as the flagship of Vice Admiral Spruance. She participated in the raid of the Marshall and Gilbert Islands and took part in the Battle of Midway, bombarding enemy shore installations, and experiencing numerous brushes with enemy aircraft. She took a good toll of Japanese shipping before she was sunk on November 30, 1942, off Savo Island in the Solomons.

On the 8th of December 1939, Rear Admiral H. G. Bowen, then Director of the Naval Research Laboratory, but now Vice Admiral and Chief of the Office of Naval Research, wrote to the Acting Secretary of the Navy, emphasizing the fact that new work on detection (radar) with higher frequencies would, as soon as suitable tubes were available, permit great reduction in size of the rotating antenna. "It is hoped that the Navy will shortly be in a position to use radar on planes for bombing. The 500 megacycle equipment now under development functions well at 20,000 feet .... development is proceeding along two lines, one for long range detection and the other for higher frequencies for altimeter work and fire control, including bombing".

This letter is of interest because it anticipates quite exactly the trend of radar development during the early years of the war.

Under date of February 26, 1940 a report of progress to the Bureau of Engineering again emphasizes the developments on higher frequencies and the importance of recognition (IFF) equipment being integrated with ranging equipment (radar). The report emphasizes the necessity of expediting the application of radar to fire control and the development of the repeater unit, particularly the polar chart or Plan Position Indicator type (PPI).

It is plain from other letters that the Navy Department was certainly not thinking in terms of very large sums of money. A letter from the Bureau of Engineering to the Director of the Laboratory, dated May 7, 1940, states, "The Bureau considers that the inherent possibility of the radio echo methods are such as to offer compelling reasons for pressing the development of all phases of this problem (in so far as consistent with reasonable economy)". Admiral Bowen replied to this requesting two and one-half times as much money as he had the preceding year.

On the 28th of May 1940, a letter from the Bureau of Engineering to the Chief of Naval Operations states, "CNO has urged high pressure on ranging equipment .... unlimited funds could be spent .... since the program is rapid in this new field, new equipment will soon be obsolete .... reasonable economy better in the long run."

On the 30th of April, Admiral Bowen wrote to the Bureau of Engineering listing seven important phases of radio detection work of which only three were very active, due to lack of personnel and facilities at the Naval Research Laboratory. Those three were: (1), surface and aircraft detection work; (2), basic developments; (3), detection of aircraft by submarines.

On the 1st of June 1940, the Chief of Naval Operations (CNO) wrote to the Bureau of Engineering as follows: "The policy with regard to the development (of radar) was satisfactory at the time. In view of the present international situation, it is desired that every effort be made to expedite the completion of the development of the project in all its phases and to commence procurement at the earliest possible date that is justified by the success obtained in every subdivision of the project. Accordingly, it is suggested that expansion of facilities and personnel be undertaken as soon as funds are in hand".

It is evident from these letters that the chief of Naval Operations was seriously concerned and was continually urging more rapid progress. Since CNO had to depend on the funds of the material bureaus, that office was not in a position to do anything further. The bureaus, on the other hand, were heavily burdened with new construction which was, of course, equally important, so these bureaus were inclined to proceed with more caution.

The Bureau of Ships did take an important step on the 23rd of July 1940, decided on at a conference in the Office of Commander Spriggs, then Chief of the Radio Division, and attended by Dr. Lack, Dr. Kelley, Mr. Merquelin and Mr. Tinus of Bell Laboratories. It was decided to give the

Bell Laboratory people a contract which would result in delivery of a fire control equipment operating on 500 megacycles and suitable for the control of fire on surface targets. The first delivery was to be made in October of 1940.

A memorandum on file at the Naval Research Laboratory, but evidently prepared in the Bureau of Engineering (which by this time, due to consolidation with the Bureau of Construction and Repair, had become the Bureau of Ships), stated that the radio range equipment procurement program being initiated by the Office of Naval Operations "will require ten million dollars for 1941 and twenty million dollars for 1942". It is evident that when the Bureau of Ships did decide to go ahead rapidly with radar procurement, they went ahead in a big way. A few days before this memorandum was written, a conference at the Bureau of Ships, attended by representatives from the Naval Research Laboratory decided to list funds for NRL to cover personnel and additional space. The Bureau also decided to finance a laboratory on Chesapeake Bay which was finally known as CBA (Chesapeake Bay Annex).

For a number of years, Mr. Young and I had been advocating the construction of a branch laboratory for the prosecution of certain activities, particularly radar test activities, at a point sufficiently distant from the main laboratory to completely remove such work from the inevitable mutual interferences experienced at the main laboratory.

The main laboratory, located on the Potomac River, is only able to obtain very limited radar ranges over water, since this whole region is surrounded by hills between 100 and 250 feet in height, giving an enormous number of land targets which clutter up the radar screen in the first ten miles of its range. Of course this does not prevent much longer ranges, up to 100 miles being obtained on airplanes which are flying at considerable elevation. It is a handicap on moderate range work on water borne or surface targets, as well as on airplanes within short range at low altitudes as would be the case in a torpedo bomber attack on a ship of the Fleet.

It was evident that if we did not have extensive operational evaluation, as well as engineering evaluation tests performed on the new models of radar which would soon be coming out in quantity, the Fleet might get a lot of equipment not satisfactory for their purposes. It was becoming increasingly difficult to get ships assigned for test installations of this sort.

It was clear then that we needed a laboratory in such a location that radars could be installed at various elevations, up to 140 feet above the water simulating the elevations which radars would occupy when installed on ships. It was also necessary to have an unobstructed range of 15 or 20 miles over the water.

Since the work of the new laboratory would have to be very closely integrated with that of the main laboratory at Washington, there would be much travel of personnel between the main laboratory and its branch. Therefore it was obvious that the site must be in a general area reasonably nearby.

When the Bureau of Ships agreed to consider supporting such a laboratory, Commander W. B. Goulett, then Radio Officer at the Naval Research Laboratory, with Mr. Young and myself, explored the entire Chesapeake Bay area south of Annapolis for a suitable site. We finally recommended a site two and one-half miles south of the little summer resort town North Beach, Maryland, at a distance of about forty miles from the main laboratory. This site is at the top of an almost vertical cliff, a little over 100 feet above the water. There are certain low spots and ravines where installations can be made at lower levels right down to the level of the beach itself. Such low level installations simulate installations on small Naval ships carrying radar, or on beachheads seized in landing operations.

This site stretches approximately one-half mile along these bluffs in a north and south direction. It permits unobstructed over water ranges to the southeast and to the northeast of between twenty and thirty miles. Straight across the bay the Eastern Shore, as it is known, is very low and flat, so that with the elevation at our command we can get fairly long ranges on airplanes even when they are flying at fairly low altitude. This site is, in fact, about the most desirable site for this type of work on the whole Atlantic coast.

After the site had been approved and money obtained from the Bureau of Ships, plans were drawn up for the first three buildings. In the spring of 1941 construction work was started. By November of 1941 the first laboratory building was completed, as well as a small building which served as combination of guard house, residence for Mr. Butler, the caretaker and his family, and sleeping quarters for eight or ten scientific or technical employees. There was also a small cottage put up at the same time which served as temporary quarters for visiting engineers, either from our own Laboratory or from the corporations who were doing important radar work and whose apparatus was under test at the Annex.

From this humble beginning, the Laboratory expanded rapidly. There are now five sizeable laboratory buildings, two of which have elaborate equipment, permitting the installation of fire control gear and even guns up to forty millimeters. Rights were obtained for the use of a small harbor two miles from the station where a sheltered inlet was available for small craft not over 100 feet in length and 7 feet draft. When heavier ships of the Navy came to work with us, as a very large number of them did during the war, they operated well out in the Chesapeake Bay in deep water, several miles from the Annex.

In addition to the main buildings, there are a number of small field laboratory buildings, a substantial office building, a sizeable building for use as barracks, and a building housing the cafeteria. On the eastern shore there are located five 100 ft. wooden towers, very substantially built, upon which radar targets, countermeasures equipment, or optical equipment can be mounted. These sites are very useful for the work of this Laboratory, permitting observations over ranges varying from 7,600 yards to 38,000 yards. Two of these towers are on Tilghman's Island, which is practically part of the eastern shore. Under one of the towers is a small field house with sufficient accommodations for small parties of scientists so that they can settle down to work over a considerable period of time without commuting back and forth to either the main Laboratory or the Annex.

The operational test setup, primarily for radar and radar countermeasures, with strong emphasis on fire control radar, was put in operation just in the nick of time. During the War period, became one of our most important facilities, I have no hesitancy in stating that it contributed a great deal to the success of the Navy's radar program. The work was by no means limited to the testing of equipment developed by the main Laboratory. A great deal of the equipment developed by commercial companies, by the Army, by the National Defense Research Committee, the Bureau of Standards and other Government agencies was put through operational and engineering evaluation tests at this site. This Laboratory will continue to be an important adjunct of the Naval Research Laboratory in peace time. It has always carried a certain number of problem from other than the Radio Divisions. It is likely that from now on a considerable number of Laboratory divisions will continue to use the Annex, although the Radio Divisions will probably, for a long time at least, have the largest interests.

Four Naval officers deserve much credit for their part in building up this Laboratory. The part played by Vice Admiral H. G. Bowen, now Chief of the Office of Naval Research, but then Director of the Naval Research Laboratory, has already been mentioned. He was, of course, largely instrumental in persuading the Bureau of Ships to provide the initial financing. Vice Admiral H. P. Blandy who was at the time of which I write Chief of the Bureau of Ordnance, gave us extensive support and ample funds for two buildings to be financed by that Bureau. Captain Martin Lawrence, then Commander Lawrence, and now Assistant Chief under Admiral Bowen, was in charge of all construction work at the Annex, as well as that at the main Laboratory. He deserves much credit for his interest in the work and his skill in laying out buildings and grounds and overseeing all construction work. Our first Commanding Officer, Captain Keith R. Belch, now retired, deserves much credit, not only for his enthusiastic support of the project, but for the outstanding manner in which he cooperated with the civilian scientists at the Naval Research Laboratory and other laboratories. Dr. Merril Distad who was first in residence as Senior Scientist, also deserves a great deal of credit. It is doubtful if we could have found a better man for this important position.

There is, however, one man who stood back of this project from its very inception and had more to do with it than any other one individual. That man is Mr. L. C. Young. The Annex Laboratory has been his particular pet project from the moment of its inception. He has kept a watchful eye over it during the entire war period. Since the work of the Laboratory at the beginning was all for the Radio Division, it was to Mr. Young as Associate Superintendent of that Division to whom Dr. Distad reported. It was Mr. Young who, with the collaboration of Captain Lawrence, did the major part of the planning for expanding, improvements, etc. It was Mr. Young who allocated and followed up with great interest all of the problems sent to the Annex for solution. He will always be thought of by the old timers of the Naval Research Laboratory as the godfather of the Chesapeake Bay Annex.

There were two developments in the latter half of 1940 that were destined to be of tremendous influence not only on the work under way at the Naval Research Laboratory but on the whole conduct of the War. The first of these was the formation of the National Defense Research Committee.

Early in 1940 President Roosevelt formed a Council of National Defense which consisted of the following members: Louis Johnson, Acting Secretary of War; Lewis Compton, Acting Secretary of the Navy; Harold Ickes, Secretary of the Interior; Henry A. Wallace, Secretary of Agriculture; Harry L. Hopkins, Secretary of Commerce; and Frances V. Perkins, Secretary of Labor.

This Council, acting under authority of Section 2, Act of August 29, 1916 (39 Stat. 649) established the National Defense Research Committee with the approval of the President. This formal approval by the President occurred on June 27, 1940.

According to the record at my disposal, the first formal meeting of the National Defense Research Committee, hereafter referred to as NDRC, occurred on the 2nd of July 1940 with Dr. Vannevar Bush as Chairman and Dr. Irvin Stewart as Secretary. After about a year of operating under the Council for National Defense and apparently reporting to this Council, the Office of Scientific Research and Development, reporting only to the President, was established by executive order of 28 June 1941 with Dr. Vannevar Bush as Head. Dr. J. B. Conant, President of Harvard University was then appointed as Chairman of NDRC, which in turn reported to OSRD. Dr. R. C. Tolman of the California Institute of Technology was appointed Vice-Chairman of NDRC. Other members were Dr. Roger Adams, Conway P. Coe, Commissioner of Patents, Dr. K. T. Compton, President of the Massachusetts Institute of Technology, Dr. Frank B. Jewett, Chairman of the National Research Council, Major General R. C. Moore of the Army, Admiral H. G. Bowen of the Navy, and Dr. Irvin Stewart, Secretary. Admiral Bowen was succeeded by Captain Lybrand P. Smith.

Various divisions of the NDRC which covered various fields of scientific and engineering activities usually had Army and Navy members. Commander R. P. Briscoe, now Admiral Briscoe, then Assistant Director of NRL, was the first liaison officer for the Navy Department but was superseded by Commander E. W. Sylvester in October 1941. Commander Briscoe remained liaison officer for NRL.

In the Office of Scientific Research and Development (OSRD), there was an Advisory Council wherein the Navy was represented by Rear Admiral J. A. Furer, then Coordinator of Research and Development for the Navy. This organization was destined to make tremendous contributions to the application of research in the interest of national defense. The research and development problems confronting the nation at this time were entirely beyond the capacity of the Government laboratories. The OSRD and NDRC, with ample finances at their disposal and far less red tape, were able to step immediately into the picture.

In the early days there were small frictions and small difficulties which required adjustments and the exercise of good will and tolerance on both sides. The OSRD was able to bring into its fold some of the very best engineering and scientific minds of the country. It was obvious that many of these people had had little or no contact with military and Naval research and no understanding of the difficulties of designing equipment which would meet the exacting conditions of ruggedness and light weight necessary for the armed services. It was natural that some of the people in the military service should have some doubt as to whether such people could produce practical results. Nevertheless the record of accomplishment of OSRD is so outstanding and so wide in scope that it will forever stand as a tribute, not only to the scientific ability of the workers of that organization, but as proof of their ingenuity and flexibility in adapting themselves quickly to the needs and standards of the armed services. The country was fortunate indeed in obtaining the services of Dr. Vannevar Bush at the head of this organization.

The Contacts of NRL with this organization were frequent and complete. The exchange of ideas and information was made with resulting benefits, I hope, to both parties; certainly we were greatly benefited at NRL. We were able to concentrate our efforts on those projects which we knew we could complete immediately for the Fleet, leaving some of the longer range work on radars completely to the organization operating under the NDRC, with complete confidence in their ability to work out a practical solution. Subsequent events in later years, when their products came into application in the armed forces, proved that our confidence was by no means misplaced.

Our first contacts with NDRC were naturally with Section D which dealt with detection, controls and instruments. This was the section that developed the famous Radiation Laboratory at the Massachusetts Institute of Technology. Dr. Karl T. Compton, President of MIT was Chairman of Section D. Section D-1, which specialized on detection (radar), was under the Chairmanship of Dr. A. L. Loomis. The Naval Research Laboratory had many contacts with Section C which covered communication and transportation under the chairmanship of Dr. Frank Jewett, formerly of the Bell Systems and Chairman of the National Research Council. He was aided by Dr. C. B. Jolliffe as vice-chairman and by Dr. R. W. King as technical aide. Dr. Joliffe was chairman of the subsection on communications.

Later on the radar work of NDRC was done by Division 14, with Dr. A. L. Loomis as Chief. At the Radiation Laboratory at MIT, which operated under Division 14, Dr. L. A. DuBridge was Director, aided by Dr. Loomis and Dr. I. I. Rabi. Dr. Compton and Dr. Loomis visited the Naval Research Laboratory in July of 1940 and for the first time saw Naval radar in operation. This conference laid down the foundation for the frequent visits between scientists of NRL and the Radiation Laboratory.

The second important development which very greatly affected our work and the outcome of the war was the decision to pool the scientific and engineering efforts of Great Britain and the United States. This resulted in the establishment of the British Scientific Mission in this country, headed by Sir Henry Tizard. NRL received its first visit from Sir Henry about the middle of September 1940. He was accompanied by a number of distinguished officers and scientists many of whom from that time on almost continuously visited the Laboratory and participated in the exchange of information which was unquestionably of great benefit to both countries.

Unknown to either, both countries had been working on radar for some years but along somewhat different lines. The British were certainly the first to use radar in actual warfare, both on the ground and in the air. Their system of warning stations is credited by all competent British and American observers with saving England during the aerial blitzkrieg when England's air force was still very small compared to the Luftwaffe. Without radar the British fighter pilots and planes would have been completely worn out since they would have had to be in the air almost at all times to fend off the enemy bombers. With the aid of British warning networks they were able to keep their fighters on the ground until such time as they were actually needed.

In another respect the British were definitely ahead of this country. They had developed a very useful airborne radar for heavy patrol planes which was of great assistance to them in running down German submarines. In other words they had the first airborne search radar to be used in warfare. They also had under development equipment for fighter planes to assist them in making contact, especially at night, with the German bombers. This was known as the "AI" equipment meaning "aircraft intercept". They had some equipment on a few ships, but it was my own feeling that we were definitely ahead of them in the field of shipborne equipment. I believe our equipment was a little better than theirs, but at the same time much smaller and easier to handle. We were starting to design radar for submarines and soon to put it into production and start installation. They had not even considered this matter.

Neither country had much in the way of fire control equipment which could be said to have a high order of accuracy. One thing the British did have which was of great importance, and that was a newly developed vacuum tube of the magnetron type which was capable of operating in the 3000 megacycle band with very short pulses with peak power of the order of 10 kilowatts. This came as a complete surprise to us and we could hardly believe that these little tubes could do so much. Centimeter wave development in this country was centered on these British tubes which were soon improved upon and manufactured in great quantities in the United States for supplying the armed forces of both countries.

It will be seen then that we probably gained somewhat more scientific information from the British than they did from us. We had developed a method of sending and transmitting with the same antenna-duplexing, it was called. This was of great interest to the British. On the whole it seemed to me then, and it still seems to me now, that they were definitely ahead of us in their development. This is not surprising, since under the imminent pressure of war, they had put hundreds of scientists and engineers to work in the field of radar. On the other hand, they badly needed our help in the manufacture and production of technical equipment. So all in all the advantages of the exchange were quite evenly balanced. Arrangements were promptly made to furnish us with samples of new British vacuum tubes and some complete radar sets, particularly airborne equipment, while we arranged to give them all information on component parts and our duplexing system, as well as a continuous flow of information concerning our further developments in the frequency bands from 200 to 1000 megacycles.

I think this pooling of scientific interests between the two countries was one of the finest examples of international cooperation that I have ever seen. The ability and caliber of the officers and scientists that the British sent over in connection with this Mission was of the highest. It was a pleasure indeed to be so closely associated with them.

Scientific missions from Australia, New Zealand and Canada participated in the British-American Exchange. Before long some of our scientists made visits to England, while others from our establishment visited a Canadian establishment, being especially interested in their radio and radar work. Most of the contacts with the Radio Division were with representatives from the British Air Commission and from the British Admiralty.

One of the most frequent visitors was Dr. Barton of the British Air Commission, a very distinguished son of a distinguished father. He was, and still is, one of the most competent of the leaders of the British missions in this country.

Among the early visitors, we particularly remember with pleasure the visits of Dr. E. G. Bowen, Mr. Davies, and Professor Cockroft. Dr. Bowen, a wiry young Welshman, seemed to me one of the keenest and most alert men attached to the British Mission. We had the pleasure of welcoming him in many discussions at the Naval Research Laboratory and we invariably benefited by his comments and advice. Mr. Davies, the British expert on Recognition (IFF) Equipment was also a very frequent visitor and was especially well informed in all fields of radar but particularly in the field of recognition equipment. Professor Oliphant, who visited us a little later, gave us valuable information on the design of new tubes, particularly the design of very high power magnetrons.

Mr. C. H. Monroe and Dr. J. W. Tawsey of the Australian Legation were frequent visitors, requesting information concerning our CXAM equipment. Subsequently, their coastal defense service long range warning radars incorporated many of the features of our CXAM design, and were built to use American tubes.

Dr. Touch, British vacuum tube expert, was always a welcome visitor and extremely accommodating in procuring for us samples of new British vacuum tubes. Among other early visitors we recall with pleasure were Commander Crossman, Mr. Drabble, Flight Lieutenant Baillie, Squadron Leader G. W. Hignett and Wing Commander C. A. Bell of the RAF.

A little later we had many visits from Captain G. F. Burghard, RN, Lieutenant Commander W. R. Westhead, RNVR, Lieutenant Rowley, RNVR, and Commander E. G. LeMesurier, of the British Admiralty. Among the Canadian visitors were Dr. John T. Henderson of the Canadian National Research Council, and Dr. F. H. Sanders. A great many others, some of them very distinguished men indeed, should be mentioned but the list would be too long to include every one.

It was natural that after the successful outcome of our tests of the XAF on the NEW YORK, the results of which became widely known among officers of high standing of the Navy, that we should have many distinguished Naval officers as visitors. It is recalled that during the month of May 1939, demonstrations of the XAF were given to Admiral Hooper, Admiral Stark, Admiral Richardson, Admiral Pratt, Captain Lybrand Smith, and Captain (later Admiral) Allan P. Kirk, who was Naval Attache in London. I remember well that when Captain Kirk returned for a month's leave, he talked about the situation in England at a conference of Admirals and Captains at which I happened to be present. He advised us to conserve at all costs the scientific and technical personnel needed to fight the war which we all saw coming. This problem of conserving personnel against the inroads of the draft was to prove a bitter headache in the years to follow. I cannot help remarking at this point how much more sensible the British and Russians were in handling this problem. The British gave indefinite draft deferments to any scientists and technicians needed in the prosecution of war research. According to a recent report in the Scientific Monthly, written by Dr. Langmuir, following a visit to Russia as guest of the Russian Physical Society, the Russians followed exactly the same policy and no research work in Russia was in danger of losing its young assistants to the Russian army.

In the early days of World War II, Hitler considered it would be a blitzkrieg which would be over very shortly. Consequently, the Germans made no particular effort to keep their brilliant young scientists out of the Wehrmacht. Some of their surviving high ranking officers now openly state that this was the reason Germany lost the war. In the last year and a half of the war some of the German leaders convinced Hitler that he was making a mistake. He attempted to revise this policy, especially in connection with the flying missiles V-1 and V-2 being developed at Peenemunde, but it was too late. Had this effort been made a year earlier, the outcome of the war might have been far different. A good many American officers share this opinion. If this country ever faces another war I hope that we will properly safeguard the work of brilliant young scientists. In the modern war a young scientist can serve his country in no better way than by applying his talents, temporarily at least, to military research.

Among other distinguished Naval visitors we find that in July of 1939, we were visited by Admiral Sexton, Admiral Greenslade and Captain Badger of the Navy General Board. Many other distinguished American officers, too numerous to mention, visited the Laboratory to see what progress was being made with radar. There was one young officer attached to the Bureau of Ships to whom the Navy owes a definite debt of gratitude. To us at the Laboratory Lieutenant (now Captain) Sam Tucker was the Bureau of Ships' "spark plug" for radar. The encouragement, and occasionally the scoldings which he gave to the personnel of the Naval Research Laboratory for not thinking in larger terms, were of enormous benefit to us. It was this brilliant officer who coined the word "radar" which was adopted officially November 29, 1940.

During the period of which I write we had so many distinguished civilian visitors that only a few can be mentioned. In June of 1939 we were visited by Dr. F. B. Jewett, Dr. R. A. Millikan, Dr. John Johnson, Dr. Max Mason, all to be associated with the National Defense Research Committee. In July of 1940, we were visited by Dr. Richard Tolman and Dr. Conant, Vice Chairman of NDRC and President of Harvard University respectively. Secretary of the Navy Knox visited us in the same month. Charles Kettering of General Motors, an old friend of the Laboratory, who became Chairman of the National Inventor's Council, visited us repeatedly during this, and subsequent periods. Mr. Kettering's wide interests and keen analytical mind makes him an extremely interesting man with whom to discuss scientific and engineering programs. It is extraordinary how many fields of scientific work claim his interest and, very often, his active support.

Members of the House Appropriations Committee of Congress visited us several times, notably in March 1939 and again in February 1941.

During this same period we had been trying to get the General Electric Company and the Westinghouse Electric and Manufacturing Company interested in going aggressively into the field of radar. We found their engineer willing enough but the leaders of those two companies didn't know exactly wha it was all about. Admiral Bowen finally succeeded in getting President Charles E. Wilson of the General Electric Company, and a little later, President A. W. Robinson, Chairman of the Board of the Westinghouse Electric and Manufacturing Company, to come down and witness demonstrations of radar so that he could personally urge them to greatly expand their radar program and give their engineers unlimited support. The subsequent efforts of these two corporations show that Admiral Bowen succeeded very well in his purpose. The Navy owes much to these two companies.

In order to get the first installations of CXAM search radar equipment off to a good start, it was necessary for us at the Laboratory to train a small nucleus of service men, headed by Chief Radio Electrician (now Lieutenant) Shumaker, in the maintenance and operation of this equipment.

Six petty officers with Chief Radio Electrician Shumaker later supervised the first installations; and each member of the group, which had been picked very carefully from men of exceptional intelligence, was put to work to train another group. These in turn were distributed among the ships of the Navy which needed their services.

The CALIFORNIA received her installation at Puget Sound; the CHICAGO and the CHESTER received theirs at Mare Island Navy Yard, while the YORKTOWN, the PENSACOLA and the NORTHAMPTON had their radars installed at Pearl Harbor. Much credit should be given to this group of petty officers for installation and training of radio maintenance men and operators. Plans for great expansion of radar school activities were soon under way. It was foreseen that many hundreds of such specialists in uniform would soon be needed in this work. The Radio School at the Naval Research Laboratory took the lead and set the pattern.

Mr. R. C. Guthrie, present head of the Search Radar Section of the Ship-Shore Radio Division, personally checked up on the installations at Pearl Harbor. It was due to the effective coordination of research, development, production and training that the Navy was to have at least a few radars afloat which were destined to be of great value to us in the early campaigns of the Pacific.

After we were able to procure suitable tubes for the purpose, vigorous work was carried on at the Laboratory in connection with radar experiments on higher frequencies, up to 600 megacycles. Work was also underway on the development of Plan Position Indicators which displayed a large number of targets simultaneously more or less after the fashion of a map.

As early as May 1941 the records show that Dr. I. I. Rabi of the Radiation Laboratory, accompanied by Dr. Bacher, visited NRL to become informed of its progress, particularly with respect to PPI development.

Plans were also well underway for a 200-megacycle search radar of much higher power than the CXAM, but using a much smaller antenna so that it could be used on destroyers. The Laboratory's model of this equipment was known as the XAR. The first contract for the construction of such equipment was awarded to the General Electric Company. Several hundred equipments were desired as soon as they could be produced. At least three production models of this set came out before the production models were able to equal the performance of the pilot model built by NRL. These were called SC radar equipments. Similar equipments made a little later by RCA bore the name SA. These equipments became very widely used - even their earlier production models, which we at the Laboratory though very poor indeed, were so useful, especially in convoy work in the Atlantic, that the skippers of ships on such duty were eagerly demanding their installation. One such captain told me that while shepherding a convoy to England via Newfoundland, he was able to save three days' sailing time in getting his convoy to England because he was able to proceed in fog bound Newfoundland harbors at normal speed, since the SC radar outlined for him every headland, most of the lighthouses, and all other ships in that area, including ships of his own convoy. He was able to keep stragglers in the convoy in position in any weather without being able to see them. This was in spite of the set not giving much more than one third, or at best one half of the performance that it should have been giving had it not been constructed in such haste.

The General Electric Company was not to be blamed for this condition of affairs, since the conditions of the contract were such that the times of delivery were extremely short. It was practically impossible to make necessary changes and improvements and still meet the date of delivery. In due time, these early issues of the SC equipment were taken off the ships and replaced by more satisfactory models. In spite of their faults they were widely used in both the Atlantic and Pacific areas.

In this same period a considerable amount of work had been accomplished in connection with recognition equipment to be integrated with radar. At the time we did not know that the British were also developing such equipment. Due to their immediate need, they had put a lot of effort and energy into this problem. The extent to which we needed recognition equipment was perhaps not fully realized in all quarters, but it was brought home to us with the attack on Pearl Harbor.

After the attack of the Japanese on Pearl Harbor had ceased, the ENTERPRISE, approaching Pearl Harbor, sent a number of our own planes into the air to land them in the Pearl Harbor area. They were not aware of the Japanese raid. Three of these planes or more were shot down by our own gunners, due to lack of effective recognition equipment. This matter of recognition became a hot problem from that time on. It is still a hot problem although much progress, some of which will be described in a later chapter, has been made.

Another interesting development was the so-called aircraft warning radar for submarines. The idea of attempting such an equipment was first suggested by Admiral Bowen, who called me into his office one day to ask me if I thought something couldn't be done about the matter. The submarine, of course, fears the airplane more than any other enemy. In cloudy weather, when it is cruising on the surface or awash, planes can appear very suddenly and deliver a bomb attack before the submarine can be submerged far enough under water so that she cannot be seen.

I told Admiral Bowen that the problem was a difficult one because we couldn't hope to put a large directive antenna on submarines since it would offer too much water resistance when submerged and would be too bulky to be housed in any way against the action of the water. At the same time, I said that an omni-directional radar might be developed, using a submarine periscope antenna, which would give warning of the approach of a plane perhaps as far as fifteen miles away. This would just about give a submarine a chance to get under water. Such a radar, of course, would not give the angular position of the plane but only the range. But after all, the submarine ducks under water when a plane approaches no matter from what direction the plane is coming. I may also add that it seldom stops to try to determine whether the plane is friend or enemy. Certainly in the early days of the war, our submarines feared our own planes almost as much as they did the enemy planes. Of course our ships had deck markings which were supposed to definitely identify them to all at war, but visibility being often far from perfect, especially under the conditions favorable to close and sudden approach of a plane, mistakes could often be made.

The Laboratory was requested to immediately go ahead with this problem and Mr. Guthrie was put on this work, during the months of March, April and May of 1940. It was decided to do the job in the 114 megacycle band, using a special antenna which protruded out of the periscope. Mr. Varela, aided by Mr. Herring, finally took over and completed this job, the first production units being built by RCA. This set became known as the SD equipment, and for sometime was very popular with the submarines. Later in the war, the Japanese developed receivers covering this frequency of the SD and placed them in scouting planes so that they could home on our signals, thus somewhat turning the tables on us. In fact we lost one or two submarines due to these tactics. By that time very much higher frequency equipment, much more appropriate for installation on the submarine, was available so that before the end of the war the SD equipments were taken off. Nevertheless they saved a good many ships before the Japanese found out a way to render their operation hazardous. As an interim procedure, the Laboratory recommended a special method of keying and operating these sets which rendered detection by the Japanese much more difficult.

In the fall of 1940 the British gave us complete information on their airborne search radar, operating on 175 megacycles. The Bureau of Ships immediately made arrangements to obtain a considerable number of these sets and try them out on our large patrol planes, especially in the Aleutian Islands. The weather in this region is such as to render the use of some form of airborne radar practically indispensable, if for no other purpose than that of acting as an aid to navigation. Commander Malcolm P. Hanson, mentioned in an earlier chapter, was killed in the Aleutians while flying with one of these sets in an attempt to correct its faulty operation. The average visibility in this region is extremely low so that any properly operating radar is not only of tremendous aid to aircraft, but it is also invaluable for ships. Later, when our armed forces made landings resulting in the capture of Attu and Kiska, expert use was made of radar to locate exactly the coves and landing beaches when the visibility was almost nil. I have been assured by a cruiser officer, who participated in this operation, that it would not have been possible without the use of radar.

The British airborne search radar (ASV), on account of its low frequency, used a number of very large antennae. Moreover, not being equipped with arrangements for duplexing, it required a larger number of antennae than our own duplexing arrangements. One of our first jobs in the Aviation Section of the Radio Division, in connection with airborne radar, was to modify the British equipment for duplexing and a reduction in the size of the antenna. Our pilots very much disliked to fly with the British equipment because it offered enough head resistance to cut down the speed of the plane very materially.

The modification of the ASV was carried out successfully and applied to many installations of this equipment. In the meantime, I insisted that this job should be done on a much higher frequency which would very greatly reduce the size of the antenna. Our British friends were doubtful whether this could be done and still maintain a useful range for the radar. Nevertheless, in January of 1941, we were hard at work converting our NRL altimeter, operating at 500 megacycles, into an airborne search radar. By October of 1941, we were able to give sufficient information to Westinghouse Electric and Manufacturing Company for them to start preproduction models. By November RCA had the same information, and Bendix Radio was drawn into the production work in February of 1942. This equipment was known as the ASB, and the first production was by Westinghouse, coming out in May of 1942. This equipment, especially useful for navigational purposes and for running down ships - especially submarines at sea, tremendously widened the area which could be covered by patrol plane, and was extremely effective in implementing our anti-submarine program. Over twenty-six thousand of these sets were produced. A considerable number of them were ordered by the Army and some by the British. Although they were ultimately replaced by sets of still higher frequency, there were a good many of them still in existence and in operating condition when the war ended. It is very fortunate for the Navy that this equipment was put into production so promptly and to such an extent, because the sets on much higher frequencies were absolutely unobtainable at the time. The illustration serves to point out how very rapidly new equipment, even very good equipment, can become obsolescent.

So much of this chapter has been given over to the story of radar that it may appear that NRL accomplished nothing else in preparation for war. The radar development was certainly the most important and the most interesting, but it should not be forgotten that the high frequency homing equipment, previously mentioned, was put into production and installed on most carriers in 1940 and 1941. It will be recalled that this was the equipment that aided the pilots in getting back to their carriers when operating at sea. The importance of such a device, when operating under actual battle conditions, is obviously not to be under estimated. At the end of an engagement, the pilot's carrier may be a hundred miles or more away from the point where he left it, and unforeseen incidents develop during the battle making it impossible to say just where the carrier will be when the pilot, having accomplished his mission, starts back for his floating home. The value of this equipment was most effectively brought out in one of the battles in the Marianas, where in the waning hours of daylight, our planes followed up a stricken enemy practically to the limit of what their fuel would permit. Most of them came back after dark and homed in on the YE and YG equipment. Many planes were saved and a great many pilots are alive today because of the excellence of this equipment. The production agency for most of these units was RCA, and they did an excellent job indeed.

In the period covered by this chapter, Captain Safford had very frequently visited the Laboratory, and it was at his suggestion that I set aside a small group of five people, headed by Dr. Claud Cleeton, for the work on problems having to do with identification and secret communications. We called this group the Communications Security Section. Later this section was tremendously expanded, as will appear in the following chapter. As early as June 1940, this section recommended the study of the advantages of communications using pulses instead of the types of modulation hitherto used. This study had important bearings later on the work on identification systems.

Another very important development was the sono-radio buoy problem. The sono-radio buoy is a buoy which has suspended beneath it a form of microphone which will pick up noises in the water. It also contains a battery as a source of power, a radio transmitter, and an antenna protruding out of the top of the buoy. This radio transmitter picks up under-water noises, and particularly noises, say, of submarine propellers when they are running submerged and invisible, and repeats these noises by way of radio transmission. They can be received at moderate distances, either on shore, on board ship, or in an airplane which has been provided with suitable receivers.

Our work on the sono-radio buoys was an instance of close coordination between the Radio and Sound Divisions. The principal responsibility of the problem lay with the Radio Division, while the Sound Division was responsible primarily for the sound pickup suspended from the buoy. This problem was assigned in March of 1940, and was the principal responsibility of Mr. Haulman, Mr. Hollweck, and Mr. Hilferty (now deceased). Our principal concern was with buoys intended for harbor protection and for advanced bases. We had developed a buoy small enough to be carried by a plane, so that it could be dropped in an area suspected of harboring underwater a submarine, and then cruise around and listen for the underwater sounds picked up by the buoy. This phase of the problem, namely, the airborne model, was turned over to NDRC who did an excellent job in getting it ready for production. The first production models of our harbor protection buoy, known as the JM, were brought out by the Airplane-Marine Direction Finder Company of Clearfield, Pennsylvania. This was an exact copy of the NRL pilot model. These buoys were not only very widely used during the war, but a great many of them have recently been used in operation Crossroads at Bikini for registering close-in underwater effects, also sending the results by radio to recording equipment and observers at a safe distance.

In June of 1938 the Aviation Section of the Radio Division tested voice communications equipment operating in the 300 to 360 megacycle band for communication from plane to ground, or plane to ship, obtaining satisfactory ranges of over 100 miles. This was a very early use of rather high frequencies for communication purposes on board airplanes.

During this period, we also made a number of interesting tests on board various ships of which I will only mention one as an example. It had to do with tests on a variety of equipment on board the new battleship NORTH CAROLINA, then on her shakedown cruise from New York to the coast of Nova Scotia, where she went through firing practice with her main battery. This was in the summer of 1941. Dr. Norgorden, Mr. Burgess and Mr. Powell made this trip. Unfortunately for Powell, he left Washington with the thinnest of summer clothes, and since his baggage didn't catch up with him in time to get on board the ship, he had a pretty chilly time off the coast of Nova Scotia. It will be recalled that we were in an undeclared war against German submarines at this period. Not long after leaving New York, a suspicious object was sighted in the distance and two escorting destroyers were sent by the NORTH CAROLINA to look into the matter. Claude Mahoney of the Star and Walter Winchell were on board the NORTH CAROLINA. They immediately questioned the Executive Officer as to whether this suspicious object could be a submarine. That officer shrugged his shoulders and said, "Well, it could be a whale." Upon being asked whether the region a few miles outside of New York wouldn't be a very likely region in which to encounter German submarines, the Executive Officer replied that that was indeed his opinion. The next question, "Do we not have to return by this same route", was also answered in the affirmative. At any rate, after two hours' search, the destroyers made no contact and the expedition continued to Nova Scotia where firing tests gave our experts an excellent opportunity to see how our equipment stood up under the shooting of 16-inch guns. One salvo fired from this ship was the largest one that had ever been fired. It was fired by nine 16-inch guns plus ten 5-inch guns in one instantaneous explosion, and no deleterious effects were noted to any of the radio equipments. On returning from this cruise, the NORTH CAROLINA entered the Chesapeake Bay and anchored off the site of the Chesapeake Bay Annex of NRL, where some studies were made of the radiation patterns of her various antenna systems. We didn't have any suitable buildings up yet at CBA but did the work from portable equipment brought to the shore line south of the Laboratory. The NORTH CAROLINA was the first of the larger ships, and among the first of all of the Naval vessels which visited CBA from then on.

Finally, there was one very important development carried on to completion in a large way during this period. This was the drone program. The drone is a pilotless plane to be used as a target. As indicated earlier, we had been interested in such work since the very early 1920's. This work was revived again in January of 1938, and in April of 1939 the overall coordination of the problem was put under Captain D. S. Fahrney. NRL had the radio control aspect of this problem, and the Naval Aircraft Factory had the problem of modifying the planes and building the equipment into them. The problem was supported by both the Bureau of Aeronautics and Bureau of Engineering. One reason that progress for this problem in earlier days had not been more rapid was because no strong high level coordination had been applied to it. Captain Fahrney supplied this in an admirable fashion. He is one of the most competent officers with whom the Laboratory has ever had any dealings. It was not long before a certain number of squadrons of drones were available for target practice in the Fleet. It was not long, either, before the gunnery officers of the Fleet found how much they had to learn by shooting, not at a sluggish towed sleeve, but at an actual airplane target which could be maneuvered by the controlling planes operating by radio a few miles behind. Thus, the controlling pilot from several miles away could cause the target plane to change elevation or course, or dive at will. It was quite a while before one of these targets was brought down by a Naval gunner. I believe that these drones taught our Naval gunners a great deal that was to be valuable in the ensuing years.

RADIO REMINISCENCES

A HALF CENTURY

CHAPTER XV

SCIENCE AT WAR

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Science at War

This history of warfare shows that all new weapons are sooner or later followed by the invention of new methods of defense. The invention and military or naval application of a new weapon are offensive operations. These in turn are invariably followed by the development of suitable countermeasures on the part of the enemy. It is a correct military principle to follow up with an offensive as rapidly as possible; this we at the Naval Research Laboratory did in developing and advocating early installations of radar.

From the beginning we were well aware that the time would come when the enemy would develop countermeasures. Up until 1942 we felt that we were safe in putting the main emphasis on the offensive side, postponing serious consideration of countermeasures until more personnel were available. At the time of the outbreak of the European war, the British had their radar chain stations so located that they would give adequate warning of the approach of German planes. The Germans were not entirely ignorant of the principles upon which radar is founded, but had developed nothing in the way of effective and practical radar.

This situation did not last long. The Germans began to push the development of radars both for long range search and for short range use for the accurate control of searchlights. Ultimately these searchlight control sets were extended to actual control of the anti-aircraft guns. Examination of their captured equipment after the war, however, does not disclose any fire control radar comparable in performance with our Army and Navy radars used for similar purposes.

The Germans were early with countermeasures, as were also the British, so that after the war was well under way a never ending battle of wits went on wherein each side tried to out guess the other and vary their techniques to evade countermeasures.

Due to their close contact with the Germans, the Japanese were aware of the existence of radar quite early, but they had not started making their first sets which were search radars until early in 1942. These sets were very crude, using vacuum tubes which were very ill-adapted indeed to the use of radar; nevertheless, they did get some useful service out of them at rather limited ranges, especially on airborne targets. Our troops captured the first of these sets on Guadacanal before it was set up in fully operating condition. Our people managed to put it together and make use of it themselves before it was supplanted sometime later by one of our own design. The second and decidedly better set, made much later in 1942, was captured when we took the Islands of Attu and Kiska in the Aleutians. Both of these sets are now at the Naval Research Laboratory.

The play and counterplay of scientific methods in warfare may easily be illustrated in fields other than radar. For instance, the mines used first in World War II were contact mines not very dissimilar from those used in World War I. These could be swept and disposed of by mine sweepers in the old way. Soon, however, the Germans began using a mine which could not be disposed of by the old method for the simple reason that this mine responded to the magnetic influence of a ship gliding over it. The mine rested on the bottom with no awkward projections protruding from it so that the ordinary sweep wire or cable failed to disturb the mine. Until the British captured one of these mines the difficulties they had been having in the mine fields were not understood. The countermeasures for this magnetic mine were two-fold. First, the so-called degaussing coils were applied to ships in such a way as to neutralize their magnetism, and, second, a magnetic method of sweeping was employed which would bring the mines to the surface and explode them. Following this still other mines of an entirely different character were used, so that it is evident that the battle of wits in modern warfare goes on and on.

In ancient days every war had to be fought pretty much with the weapons that were on hand when the war started. I have heard some fairly capable officers argue that the same thing held during the last war. However, a critical analysis of the play and interplay between measures and countermeasures shows that neither side could afford to sit tight with the weapons that they had developed before the war. It is certainly true that the basic research on the development of new weapons should be done in periods of peace, but it is equally true that the work on measures, as well as countermeasures, must continue on at an even accelerated pace in time of war.

No new weapon remains secure and unknown to the enemy indefinitely. If he does not ferret out knowledge concerning a new weapon through espionage, he will at least sooner or later capture one of these weapons and his scientific experts will tell him what it is.

When the Japanese took the Philippines they gained access to some very good research radars. They captured another one of an excellent British design at Singapore, but it had been well broken up and they could not make much of it. Unfortunately, however, they discovered somewhere a copy of the instruction book and so were able to rebuild it, although they never did succeed in making a copy of it as good as the original.

I have spoken earlier of our airborne search radars on the 500 megacycle band which went in to very large scale production. During some of the fighting in the southwest Pacific, two Japanese planes were shot down, each of which was equipped with an ASB radar. These were not copies of our radar but were actually originals obviously taken from two American Naval planes which had crashed on some Japanese Island. It must be inferred that they captured more than two because certainly at least one would have been sent back to Japan for analysis and study.

I will give one more illustration of the way measures and countermeasures follow each other. Early in the European war the Germans conceived the idea of sending a relatively narrow high frequency beam of signals from the Norwegian coast, then in their possession, so as to pass over a certain town in England that they wanted to bomb. Another similar high frequency beam was sent from some point on the French or Belgium coast and also pointed toward the same town. If these beams were properly directed they would cross over the town in question. The German bombers then could follow out along one beam, not necessarily starting at the source, but picking it up as soon as they intercepted it in flight, and follow that beam out until they heard the signals from the second, which would be the signal for letting loose the bombs on that particular town. Now these signals were fairly strong high in the air, but rather weak at the ground level. Nevertheless, the British picked them up and soon became aware of what the Germans were doing. Of course, the simplest thing to have done would have been to jam the signals with a powerful local signal so as to try to prevent their reception, but the British did it in a much more clever way. They introduced a false signal into the German system that the German bombers were led away from their beam. Then when this extra signal was suddenly removed, the German planes were left without any guidance at all. Many of them jettisoned their bombs in localities where they did little harm; others were shot down, and at least one that I heard of crashed in the sea, having run out of gas before it could get back to German held territory.

It seems that the Germans had for sometime been aware that something was wrong with their beacon system and that the bomb loads were not going to the proper destinations. I have been told by a reliable witness that Field Marshal Goering called in the German scientist who had devised this system and told him that something was wrong with his instruments. The scientist insisted that there was nothing wrong with the instruments either on shore or as installed in the planes, but Goering told him something was wrong anyway and ordered him to make the next flight over England and check up on things. It didn't do the scientist any good to object and he made the flight, as a result of which he wound up in an internment camp in Canada. He was so incensed at the way he had been treated that he spilled the whole story.

With the transition of the status of our Government from an undeclared war on German submarines to a declared war against Japan, Germany, and Italy, research, development, procurement and training were all stepped up rapidly. If we had not all realized what a battle of wits World War II was going to be, we certainly did after talking things over with the members of the British Scientific Mission to this country. I well remember a statement made in my presence by Dr. Fowler of that Mission, "You know," he said, "the Germans aren't any smarter than we are. They don't think up any more ingenious things than we do, but they are exceedingly clever with mass production. With their system of totalitarian government, they can compel mass production in a fashion which is difficult to equal in a democratic country."

After September of 1940 the British scientific reports in large numbers came to us regularly as part of the general exchange which had been authorized. Many hundreds of these reports passed over my own desk, and I would like to say here that I was tremendously impressed with the flexibility and ingenuity of the British scientists, both on offensive measures and countermeasures.

When it came to recruitment of new personnel in large numbers for the laboratories of the armed services, it was at once evident that the United States Civil Service Commission couldn't possibly supply more than a small fraction of the demand. We were, therefore, forced to do much of our own recruiting, principally through the colleges and universities of the country, and take on promising young men with their salaries fixed at so much per annum on a contract basis instead of putting them through Civil Service. I do not mean that there was anything wrong with the Civil Service employees - on the contrary they were the backbone of our organization, but the ratio of supply and demand was such as to render it literally impossible to fill vacancies with sufficient rapidity and in sufficient numbers to meet the demands of the military services. I wanted to put these young men in a technical reserve as officers and petty officers according to their ability, because had this been done there would have been no danger of our losing them later to Selective Service, but the draft itself was not popular and it was impossible to get such a technical group into uniform without running the risk of having them all called "draft dodgers".

As far as our relations with the commercial laboratories were concerned, we early had an unwritten but well understood agreement that we would not go after the other's civilian personnel. It wouldn't have helped matters any if the Government services and the production laboratories and shops were openly competing in the personnel market. We had a similar understanding with other Government Laboratories engaged in war work. We accepted applications from no members of the staffs of such laboratories without the consent of the respective heads of the laboratories.

In 1942 the radio program at NRL put its principal emphasis on five fields of activity which were of almost equal importance for the prosecution of the war. Greatly increased emphasis was placed on the development of fire control radar, particularly with reference to the integration of such radars with the computers, the predictors and the guns.

It should be kept in mind that ships of the Navy generally shoot at moving targets. If a ship's radar indicates an enemy target at a certain range and bearing, the surest way to miss that target, if it be moving rapidly, would be to point the guns on the same bearing that the radar indicates and to give the guns an elevation corresponding to the indicated range. Such a shot would almost certainly miss because by the time the shell arrived the moving target would no longer be in that position. Hence it is necessary to have automatic or semi-automatic computers and predictors which when used in connection with either optical range finders or radar will place a shell at a given point of space at the exact instant that the target also arrives at that same point in space. The problem for surface targets is not so complicated because the target remains at a fixed altitude of zero, or in case it is a land target at some other fixed figure greater than zero. However, if there is any relative motion between the firing ship and the target, both the relative bearing of the target and the range will change from moment to moment. This is usually true even when firing on land targets because ships are very commonly under way while carrying out shore bombardments in order that they may be in a better position to avoid enemy fire from the shore, or attack from enemy planes, submarines, or other ships. Considering that the roll and pitch of the ship which acts as a platform for the guns also has to be taken into consideration, one cannot help but marvel at the extreme accuracy with which modern Naval gunfire can be delivered.

When shooting at an aerial target the problem is still more difficult because the target relative to the ship, may change not only in range and bearing but also in elevation.

Radar arrived after a good deal of development had already been accomplished on fire control systems using the optical range finder. It was the Navy's particular problem to integrate radar into the picture so as either to replace completely or supplement the functions of the optical range finder.

As far as the control of searchlights is concerned, the problem is relatively simple, since the searchlights can be trained directly on the bearing indicated by the radar. The velocity of light is so great compared to the velocity of even the fastest airplane that the radar pulses arrive at the target after such a short time in transit that the position of the target does not have time to change materially but a gun must point well ahead of the targets depending on its speed and course, and also be elevated to point well above it since the shell follows a curved or parabolic flight path. In September of 1942 the activities already under way and in sight on fire control had assumed such proportions that it was deemed advisable to form a Fire Control Section of the Radio Division under the leadership of Mr. R. M. Page (later Dr. Page), whose principal assistants at that time were Mr. Schooley and Dr. Bernet.

This Fire Control Section was concerned not only with the problem of integrating fire control radars with existing systems, but as a corollary to the problem it was equally concerned with very exacting tests on new gun director systems which were then under way under the guidance of the Navy Bureau of Ordnance. As might be expected the first work undertaken was the assimilation of radar with existing fire control systems. In the meantime everyone was of course looking forward to the development of a new system which would utilize to the fullest advantage new fire control radar. The Naval Research Laboratory was the first to make evaluations of the different sources of error in fire control systems. In the past, systems produced according to Navy specifications had been put on a few ships and put in action against towed targets. If a reasonable percentage of targets were hit, a certain amount of quantity production was started. Naturally the systematic evaluation of errors turned out to be a much more satisfactory guide on the road to production than the old method.

The next field of activity which was vigorously stepped up concerned the development of rapidly scanning radar. This means radar which provides for a rapid movement of the beam so as to cover a considerable section of territory, sometimes involving a 360 degree sweep around the horizon and a vertical sweep from zero elevation to 90 degrees or even somewhat beyond the zenith. Such radars came into wide use in our armed forces, not only for long range warning purposes where they were able to display a large number of targets in different directions all on one screen, but for use in fighter direction. Perhaps the term "fighter direction" needs a bit of explaining. As far as I am aware, the British were the first to use fighter direction by the aid of radar. Having located a flight of German bombers, they would send up a squadron of British fighters to intercept them. Now the British fighter planes also appeared as pips on the radar screen so that the fighter pilots could be told where to go and when they were getting close to the Germans. The great value of these tactics in night fighter operations is obvious. Our Navy did the same thing in the Pacific, particularly in dealing with Japanese "snoopers". The "snooper" is a scout plane which is trying to locate, say, one of our fast carrier task forces. Our night fighters, operating under fighter direction by the aid of radar, were able to shoot down many of these snoopers before they had made contact with elements of our Fleet. This had a military value out of all proportion to the value of the destruction of one Japanese plane because it greatly handicapped the Japanese in counter offensive movements. Fighter direction is also of great value in the daytime because the radar sees enemy planes many times as far away as can the human eye.

Another item to receive extremely serious consideration was the problem of producing a satisfactory radio-radar recognition system suitable for general use by the Allies. Partically, of course, this meant suitable for use by ourselves and the British. Although by this time both countries had such recognition systems, neither of them was fully satisfactory. At any rate when final requirements of a satisfactory system were drawn up in such a way that they satisfied both our needs and those of the British, it was evident that a very high degree of scientific effort would be required to produce such a system. It was, therefore, proposed that a group be set up at the Naval Research Laboratory, and under the direction of that Laboratory, consisting of personnel from our Army and our Navy, from NDRC, and from the British laboratories. This group, set up in 1942, became known as the Combined Research Group. The British sent over an extraordinarily competent group of scientists who played a leading part in this work. Among them may be particularly mentioned Dr. B. K. Bowden and Mr. R. H. Brown.

Before the end of the war this group had expanded to over 300 people and had become a Subordinate Command of NRL, although the Technical Director of the group, Dr. Claude E. Cleeton, was still a member of the Radio Division. Captain G. B. Hall was the first Commanding Officer of this Subordinate Command. Naturally the security regulations on this project were extremely strict and even now I am not at liberty to say anything further about this work except to remark that it was another beautiful example of international scientific cooperation. I should like to quote at this point from an article by Squadron Leader Vernon Noble, published in "Britain" in October of 1945. In the last paragraph he states, "The Battle of Britain was fought and decisively won with equipment which was British in conception and construction but the succeeding radio battles have been just as decisively won by the combined scientific and constructive resources of Britain and America. The great English speaking allies merged their radar activities, as everything else, into one gigantic combined effort."

The necessity for high powered longer range search radar was obviously connected with the increasing speeds of both bombing and fighting planes. A longer and longer period of warning was necessary in order to make suitable preparations for defense and counterattack. Moreover we had to get such radar on some rather small ships, particularly the destroyers, as has already been mentioned. A very considerable amount of the energy of our search radar group, therefor, went into an attempt to get search radar sets developed whose power would be reckoned in hundreds of kilowatts instead of tens. By this time the various commercial companies engaged in the development and manufacture of vacuum tubes had made notable strides and were all very ready to cooperate with us.

The Radiation Laboratory, under NDRC, concentrated on high power in the 3000 megacycle band, whereas we at NRL put our efforts on high power in two lower bands, namely at 200 megacycles and 400 megacycles. Our efforts resulted finally in the development of the SR equipment which got out to the Fleet in plenty of time to give a good account of itself in battle. The same remark may be made in connection with the high power sets developed by Radiation Laboratory.

As indicated in the first part of this chapter the Naval Research Laboratory became more and more involved in radio and radar countermeasures as the war went on. From a scientific point of view, countermeasures may be divided into two general classes - namely, defensive countermeasures and offensive countermeasures. The development of techniques and production of apparatus designed to protect our radio and radar systems from enemy interference constitutes defensive countermeasures. On the other hand, the development of special apparatus designed to interfere with the enemy signals of radio and radar must be considered as offensive countermeasures. Furthermore, these definitions do not need to be restricted to the field of radio, but can be broadly applied to any technical military or Naval development.

It was not until late in the war that the Japanese succeeded in getting any radar on board their fighting ships. They did have a number of rather crude shore establishments which greatly increased in numbers, and considerably in performance, towards the end of the war. They even attempted a fire control radar which went into some limited production, and is known to have been used in the home islands of Japan against our Army and Navy planes. Nevertheless, on the whole we were definitely in an offensive position as far as the use of radar was concerned. Therefore, our earliest efforts in the countermeasures field were directed towards the improvement of our own radars so as to render them less susceptible, to possible interference or "jamming" by the Japanese. Work was begun as early as the later part of 1941 on anti-jam circuits for our own radars. This work was greatly accelerated in the following years, so that many of our radars which went into battle with the Japanese in the last two years of the war ware protected by anti-jamming circuits. I do not mean to infer that such circuits invariably give one hundred percent protection but they do make it much more difficult for the enemy to conduct jamming operations successfully.

Being closer to the scene of operations, the British scientists took the lead in countermeasures directed against the Germans. The British were certainly first with one very interesting device which constitutes a countermeasure and has the purpose of deceiving the enemy, but I think this still can be classified as an offensive countermeasure because it partially denies the enemy complete use of his own radar. This British device consisted of very thin sheets of metal foil, cut to suitable dimensions and dropped in great numbers by large planes which preceded the main group of bombers which might be on its way to bomb a German city. These clouds of light thin metal foil dropped from a great height settled down slowly, and during this process they created myriads of false responses on the screen of the German radars because these strips of foil were capable of reflecting the radar energy. In other words, they made a very excellent target and appeared, indeed, on the enemy radar, according to reports of prisoners of war, as though a very large number of bomber planes were in the vicinity. Of course, the German artillery promptly turned loose and the German fighters took to the air. It is true they sometimes brought down the path finder plane that had dropped the foil, but more often than not this plane escaped because it turned off to one side of its previous flight path and the Germans didn't bother shooting at it or chasing with fighters when they apparently had a whole flock of bombers almost immediately overhead. In the meantime the main raid arrived over the target by another route and often managed to drop their bombs without much opposition. The first and one of the most devastating bombings of Hamburg, a heavily defended place, was carried out by this means.

The Radio Research Laboratory, operating under Division 15 of NDRC, immediately took up this problem and carried it into a very large development. In order to use this material effectively in large operations, literally tons of the material were required. Therefore, American production facilities went to work to produce this material both for us and for the British. The Japanese made attempts to use a variation of this method of deception but without too much success, largely due to the fact, I believe, that our radar operators were of a higher order of intelligence and ability, and it did not take them long to learn how to distinguish these false targets from the genuine ones. It must also be admitted that the British did not have so much success with this method once the Germans had discovered what was going on, which they were bound to do because tens of thousands of sheets of this foil fell on German targets.

When it comes to offensive countermeasures looking forward to the jamming of the enemy radio or radar, the first step is to determine what he has on the air and where it is. The radio direction finder can, therefore, be looked upon as a countermeasures device, but it must be a special type of direction finder which will operate with great rapidity and will scan a wide band of frequencies and display the results on some kind of a screen. The development of rapid scanning receivers, covering a wide band of frequencies had actually been started in October of 1929 by Mr. C. B. Mirick of the Naval Research Laboratory, who applied for a patent on a panoramic receiver. A working model of this device was built and successfully tested. However, this work, due to pressure of other things, was not followed up, so the whole subject was reopened when countermeasures became highly important. Modern military communications are sometimes made with such extreme brevity and at such high speed that an old fashioned type of direction finder receiver does not have time to get any results whatever on such signals. The development of suitable receivers for various bands of frequency was pursued vigorously by the NDRC laboratories, by the Army, and by the Naval Research Laboratory. Of course, some of these receivers had to go on board scouting planes, involving weight and sise limitations; others were placed on shore, particularly at advanced bases, and many went on board the ships.

The Japanese themselves had some very good direction finders installed in the islands, particularly in the southwest Pacific, and they took immediate advantage of any careless use of communications by our Naval personnel, and generally knew altogether too much of the whereabouts of our ships. It was necessary for Army and Navy bombers systematically to bomb out these Japanese installations, a job that had to be repeated frequently because such stations are very small, not very complicated, and can be rebuilt in a few days. I have already explained in an earlier chapter how the Japanese put a receiver on board their scouting planes which enabled them to track down some of our submarines using the SD radar.

When it comes to picking up and identifying radar signals, it is necessary that the equipment used for this purpose show, not only the frequency of the enemy signal, but its principal characteristics, such as its pulse length and the number of pulses occurring in each second. This required the development of pulse analyzing equipment to go along with these search receivers.

The next step in the offensive countermeasures program after enemy stations have been identified, their characteristics studied and locations determined, is the development of equipment which will effectively jam their operation. Furthermore, in jamming, operations must, if possible, be carried out in such a way as not to jam all of our own equipment. This means, therefore, that jamming operations have to be applied with considerable discretion or they may do more harm then they do good.

One of the most interesting countermeasures in the development of which the Naval Research Laboratory participated has to do with one of the German flying missiles.

In August of 1943 the Germans started using radio controlled bombs which threatened to take a heavy toll of British and American ships. The first of these was a jet propelled flying bomb which was released from a large "mother" plane which remained at a distance of from three to ten miles from the target. The launching plane was therefore generally out of the range of anti-aircraft fire. This flying missile acquired a start and high initial velocity from a jet engine which generally used up all its fuel before the plane reached its target. These bombs were released at heights of 1800 to 9000 feet and came down in a long glide. The mother plane maneuvered into a position behind the flying bomb so that it could exercise control over its movements by radio. To aid the control plane in keeping track of the missile the latter was provided with a bright light in the tail. The missile was practically always used during the daylight hours when visibility was sufficient to pick out good targets.

In general this bomb was used in making attacks on shipping in the Mediterranean and the Bay of Biscay. It came into use at a time when allied anti-submarine warfare had been developed to such a state of perfection that the Germans were losing almost as many submarines as the Allies were losing in the way of convoyed ships. The system of using combinations of destroyer escorts with escort carriers which could stay at sea for long periods of time had definitely put a crimp in the German submarine warfare program and German submarines were being sunk much faster than ships and crews could be replaced.

The German flying bombs were a bold attempt on the part of the enemy to regain the initiative in their attack on allied ships and convoys. The advantage of the new missile was two-fold. In the first place the mother ship or control plane could remain out of range of anti-aircraft fire and in the second place, since the flight of the missile was under control, it was extremely difficult for the ship under attack to so maneuver as to avoid the missile. It should be made clear that this missile could not fly indefinitely and it tended to gradually lose altitude. It was really a glide bomb which got its initial start and high speed from the small jet engine which operated only for a very short time.

The second type of German missile, coming into use a little after the first one, was the FX, which was a dive bomb without wings but with control surfaces in the tail which could be operated by radio signals from the plane dropping the bomb. This missile was dropped from altitudes as high as 27,000 feet and depended entirely upon gravity for propulsion. However it was possible for the plane dropping the missile to exercise a limited amount of control to right or left and to make small corrections in the range. In other words, this missile was an attempt to get a dive bomb which would have a very high degree of accuracy. If the pilot dropping the bomb saw that it was going to be a little off the target he could make small corrections during the flight of the bomb, thus insuring a hit.

These two types of flying bombs threatened to become a major hazard. They succeeded in sinking a number of ships, including the British WARSPITE, the Italian battleship ROMA, which was being delivered to the Allies under the terms of the Italian surrender, the American cruiser SAVANNAH, which was taking part in the landings at Salerno, and a good many others. It was apparently one of the dive bombs which hit the SAVANNAH directly on the top of a turret. It apparently had a delayed action fuse as it didn't explode until it got below decks. Nearly 200 people were killed and a hole was blown in the side of the ship, just above the water line, big enough to drive a jeep through. It speaks well for the morale of the officers and crew of the SAVANNAH that this hit did not prevent her from continuing her support of the landings. She finally managed to limp into port to get a temporary patch on her side after which she came back to the United States for full repairs.

From the beginning it had been suspected that the Germans had been using radio control but no one knew exactly what frequencies were being used or how they were modulated. The British had some evidence that the control frequency was 17 megacycles but this turned out to be an error. In the late fall of 1943 Dr. Krause and Lieutenant Commander Luke went to England and later to Italy to assist in the examination of bomb fragments. It was their conclusion that the frequency was considerably higher than 17 megacycles.

In the summer of 1943 the Chief of Naval Operations started a highly secret program for the purpose of preparing countermeasures against possible German missiles. Our program at the Naval Research Laboratory started with the determination of suitable countermeasures against our own radio drones which could, of course, have been used against an enemy as well as for the purpose of practice in anti-aircraft fire.

Toward the end of July 1943 we set up tests at the Chesapeake Bay Annex which showed that it was possible to jam the control signals used in connection with our drones controlled by either one of two systems. It was shown at that time that the ordinary transmitters with which the Allies' navies were equipped had no frequencies high enough to interfere with control frequencies well above 20 megacycles. We built a special jammer with which we were able to demonstrate that after interception of the control signals we could put this jammer on appropriate frequencies and take control of the missile away from the party operating it.

Dr. Lutz, aided by N. R. Best and other members of the Communication Security Section of which Dr. E. H. Krause was Chief, had general charge of this program.

In order to find our definitely what the Germans were using in the way of control, it was necessary first to build intercept receivers which would scan a considerable band of frequencies and instantaneously record any signals found in the band. Since these bombs only required the operation of control signals over a period of about one minute, there wasn't much time to do this job. The signals were only on the air intermittenly, and very briefly, even in this one minute period. It was also necessary to put modulation analyzers with this intercept equipment in such a way as to find just how the signals were modulated in order to affect the controls. The control of such missiles involved four controls; up, down, right and left.

The Naval Research Laboratory started a "crash" program to provide, with the utmost speed, equipment which could be put on two "guinea pig" destroyer escorts, the DAVIS and the JONES, which were sent to the Mediterrnean area where most of these missiles were being used, to offer themselves as targets in the hope of getting the necessary information on the method of control before the ships could be sunk. It is interesting to note that at least 10 or 12 of the missiles were turned loose at these two destroyer escorts but neither was sunk at that time, although later one of them was lost by submarine attack in the Atlantic. The Laboratory had to train people to handle and use this equipment. Lieutenant W. Leonard was in charge of the equipment on one of the ships and Lieutenant H. C. Dowling on the other. The Laboratory also sent Ensign Edward Koontz on one of the ships.

The DAVIS and JONES were ordered to report to the Washington Navy Yard where the large quantity of experimental equipment necessary to go on the mission was to be installed under the direction of Dr. E. J. Krause and Dr. S. G. Lutz, Naval Research Laboratory engineers. Not knowing why they were ordered to report at the Navy Yard and realizing that few combatant ships ever put into Washington, the most popular scuttlebut among the enlisted men of the two ships was that they were to receive 16-inch guns which were being built at Washington, or that they were to be converted to helicopter carriers. The enlisted men on the two ships were never told for what the equipment aboard was intended or to what task the ships had been assigned.

Under the direction of Mr. N. R. Best at the Naval Research Laboratory, the two countermeasures teams had been trained in two weeks to operate and maintain all of the special equipment put aboard the ships. One team was assigned to each ship, and extensive operational tests and exercises were held in the Chesapeake Bay, with the engineers participating, before the two ships proceeded to the Mediterranean.

These ships were not only equipped with the intercept and modulation analyzer equipment but were equipped with jammers with which it was possible to interfere with the German controlling signals. They were fortunate in several respects to have this equipment: first, the Germans loosed a good many bombs, giving them an opportunity to operate their equipment frequently and get the desired information as to the exact frequencies and method of modulation employed in controlling the missile. Second, they were fortunate in not being hit. The photographic records taken by these ships were rushed back by air to the Laboratory and analyzed by Dr. Lutz and his aides, with the assistance of Mr. Koening of the Bell Laboratories who had a special analyzer which was extremely useful in unravelling the method of modulation used by the Germans. During all these attacks the guinea pig ships radioed back information as to the progress of their experiments. Being fearful as to whether they would survive the attacks, they were anxious that the information should get into the proper hands as soon as it was obtained.

Rush work was immediately started on the production of a number of intercept and jamming equipments. In this work Dr. Lutz was aided particularly by Mr. E. F. Kulikowski and Mr. J. E. Gall of the Special Development Section of the Radio Division. Much of this equipment was shipped by air to the North African and Mediterranean area. Two sets were installed at the Brooklyn Navy Yard on a mine sweeper and destroyer respectively.

It should be pointed out that these jamming sets required special broad band antennae for the development of which Mr. Martin Katzin, consultant for the Radio Division of the Naval Research Laboratory, was responsible. He was very efficiently aided by Mr. Carter of RCA.

This equipment went into rapid production and Mr. N. R. Best, aided by Mr. J. T. Bolljahn and Mr. H. E. LaGow was sent to England to install complete sets on transports, battleships, cruisers, destroyers, escorts and British "beach accommodation" ships. These later were to be sunk off the Normandy beaches in shallow water.

In the meantime the British had started a vigorous countermeasures program and NDRC's ABL (American British Laboratory) in England had gotten into the program. At the time of the landings in southern France, some sixty ships had been equipped in that area. Fifteen ships were equipped by NRL engineers for the Normandy invasion, ten of them in England and five in the United States. There were probably at least as many more equipped by the British and by the ABL. According to the information at my disposal not one of these ships was hit by a flying bomb. There is no doubt that this rush program which involved day and night work, not only by our engineers but also by our shop people for a considerable period of time, saved a number of ships and many lives.

In the meantime Dr. Lutz and Dr. Perlow had suggested another system, desiring to facilitate more precise placing of the jamming frequencies on the intercepted control frequencies. When one realizes what a short time is available for carrying out these operations the difficulties can be appreciated. It is evident that a great deal of training is necessary to perfect the technique of the operators in getting the most out of the equipment. The two destroyer escorts which were equipped as guinea pigs practiced jamming each other alternately all the way across the Atlantic.

A second system suggested by Dr. Lutz and Dr. Perlow was made up into a working model and developed later by the Airborne Instruments Laboratory into what became the MAS. Mr. Bolljahn of the Naval Research Laboratory was sent to Italy and Africa with the MAS Installations. The Navy was ready to make intensive installations in the Pacific when the war with Japan ended. It had been anticipated that, since the Japanese obviously knew about the German flying missiles, they might try to use them extensively in the last stand defense of their homeland. Instead of doing this however, the Japanese developed the famous suicide attack "Kamakazi" which in a way was far more dangerous because it couldn't be jammed. They too had a glide bomb which could be launched from a mother plane but it had a human occupant lying in a cramped space flat on his face. He was part of the bomb and died with the explosion. He took the place of radio control. This bomb was known as the "BAKA".

Of course in connection with this intercept and jamming program our engineers were steadily at work to extend the ranges of intercept equipment and jammers to higher and higher frequencies, anticipating that the enemy might change frequencies. It turned out however, as we found out after the war when we had opportunity to examine huge masses of German equipment, that the Germans had committed themselves to a definite frequency and definite control on both their bombs and, having gone into large scale mass production, were not in a position to switch frequencies. In this I think they made a serious mistake. Had they had means for quickly changing their frequencies, the Allies would have had a lot more trouble in using effective countermeasures against them.

While on the subject of flying bombs it may be well to mention at this point that neither of the famous large bombs, the V-1 and V-2 used by the Germans against England, were radio controlled and therefore they could not be jammed. The V-1 or Buzz bomb was a winged bomb flying at speeds of between 400 and 500 miles an hour, propulsion being by a jet engine. It was fired froma ramp on a definitely predetermined course. The equipment within the bomb held it on that original course except for such deviations might be produced by wind. This bomb did have a small radio transmitter in its nose which had nothing to do with controlling the bomb. This transmitter was turned on just before the bomb dove down on its target at the end of a flight, so that the Germans could take radio compass bearings on the signals and find out where the bomb hit. This equipment was not on every bomb, but one one out of every ten or fifteen so that wind deviations which had not been allowed for in the original settings of the bomb courses, could be corrected from time to time.

The second bomb, the V-2 was not a winged missile at all but really a huge shell driven some fifty miles up into the upper atmosphere by a form of jet propulsion which expired at the time the shell reached this high altitude. However, by this time the missile had a velocity of considerably over 3000 miles an hour and by internal mechanism was tilted over and automatically put on a pre-set course. From that point it went on by its initial momentum and gradually came down in accordance with the pull of gravitation. It was not possible to interfere with either of these bombs by jamming methods.

It may be pertinent at this point to make a comment on the Japanese Kamikazi attacks and explain why it was so difficult to deal with them, especially off Okinawa where we lost so many ships to these attacks.

It is difficult to say just when the Japs first adopted the suicide method of airplane attack since there were a number of incidents early in the war that might have been of this character. It was not until we began to approach the Japanese homeland that these attacks increased very greatly in number and were pressed home with great determination. The results were costly to say the least.

Very early in the war the Japs became aware of our use of radar and adopted two well known methods of radar evasion. It is very difficult for a search radar to identify an enemy target which is moving very close to fixed objects on land. In other words, discrimination between the echo from the moving enemy target and the strong land echoes from objects immediately behind it becomes very difficult. The Japs took advantage of this both with respect to operations of their surface craft and their airplanes. It was frequently possible by flying low and close to the shores of islands to escape detection until fairly close to the target. This procedure could not be used for shipping unless there were deep channels close into the shore.

The second method of evasion was for the Japs to make an approach to the target by flying low behind an island so that the radar beam was intercepted by the hills and the attacking plane or planes could not be discovered until they had come over the top of the island and were by that time quite close to the target. The Kamikazi attacks frequently followed these tactics in the last days of the war. It is interesting that on the other side of the world the Germans followed exactly the same tactics when their bombers were attacking our ships during the Sicilian landing operations. Due to the high mountainous character of the island of Sicily these tactics were very successful and the German bombers often got very close to our ships before they were detected. It must be remembered too that the operators in those days had had little experience in target discrimination.

In the campaign for Okinawa both the Army and the Navy set up long range search radars ashore as soon as we had captured high points suitable for such installations. This assisted greatly in giving warning of approaching attacks. Nevertheless the Navy was forced to use destroyers and detroyer escorts as picket boats out and beyond those of our ships which were engaged in shore bombardments, these pickets being provided with search radars and, of course, suitable radio communications to convey warning of approaching enemy planes. The picket boats took a severe drubbing as a result of this and many of them were sunk. Their use was justified because it permitted the operation of shore bombardment and support of landings to go on without interruption. Furthermore, it was obviously much better to lose a few destroyers than a carrier.

In some cases the Kamikazi attack came from very high altitude in the form of a plunging dive on the target. This required as countermeasures, the development of special radars to give more adequate information at very high angles. The picture was further complicated in those days by the fact that the Japs had captured and copied some of our identification equipment so that it was not always possible to distinguish friend from enemy. The Jap Baka bomb, previously mentioned, with its human pilot who took the place of radio control, was not a very serious menace because our fighters were frequently able to shoot down the mother planes before they got near enough to turn loose the Baka on its target.

The Naval Research Laboratory was called upon to continue highly specialized work as a result of the Kamikazi attack but the war ended before many of these countermeasures could be tried out against the enemy.

A large amount of testing of new countermeasure devices was carried on at the Chesapeake Bay Annex. This was an ideal place for such an activity because we could draw on the Naval bases at the mouth of the Patuxent, thirty miles down the Bay from our location, for ships and planes to take part in the testing operations.

This operational testing of countermeasures involved not only our own devices but many of those worked out by RRL, the Radio Research Laboratory at Harvard operating under NDRC, and under the immediate technical leadership of Dr. Terman. I do not mean to say that all RRL countermeasure devices were tested at CBA; many of them were independently tested in England, others in this country by the Army and still others at other Naval stations. We maintained very close contact with the work of RRL and for some time had a regular exchange of visitors between the two laboratories. An RRL man would come down to NRL, stay a week or so and then return and an NRL man would then go to RRL at Harvard.

In the latter part of August 1943, Vice Admiral McCain, Deputy Chief of Naval Operations for Air, requested us to get up a very complete demonstration of both offensive and defensive countermeasures. This demonstration was prepared at the Chesapeake Bay Annex and involved rather elaborate operations with some forty airplanes and five or six ships, as well as a considerable number of shore station installations. There were although thirteen scheduled events which had to be run off exactly on time so that the movements of the vessels and planes and the operations from shore stations could be properly correlated.

The day before the main demonstration was to have been given, a dozen things went haywire and our people worked through the better part of the night to be sure that everything would be in good order the following day. We all felt a bit nervous about it because, in addition to Vice Admiral McCain, who later commanded a Fast Carrier Task Force in the Pacific, there were a large number of high ranking Naval Officers present, including Admiral Ernest J. King, Commander in Chief and Chief of Naval Operations; Admiral Ramsey, Chief of the Bureau of Aeronautics; Vice Admiral DeLaney of Naval Operations; Vice Admiral A. J. Hepburn of the General Board; Rear Admiral Redman, Director of Naval Communications; Rear Admiral A. H. Van Keuren, Director of the Naval Research Laboratory at that time, and several others including a large number of captains and commanders from the Office of Naval Operations and the three materiel bureaus, Ordnance, Ships and Aeronautics.

The demonstration came off like clock work on September 16th, partly due to our strenuous efforts of the preceeding day and night and partly, I will admit, due to good luck. The demonstrations of what could be done with various jamming equipment, both airborne and shipborne, went off very well as did the work with deceptive devices, such as "window" and "chicken". "Window" was the code name given to the packets of aluminum foil which, when dropped from a plane, would give false echoes. "Chicken" was a light corner reflector made of chicken wire which was carried aloft by a small balloon and towed behind a small ship. I do not know that there is any particular significance to "window" but the name "chicken" was given to the second device because it was made principally of chicken wire. The action of "window" has been described earlier; the action of "chicken" was to make the radar signal from a very small ship appear as large as the signal from a big ship, like a cruiser or a battleship. I have been told that "chicken" was used quite successfully during the dark hours preceeding the Normandy landings. A group of very small vessels went up the English Channel, not far from the German occupied coast, in the early hours of the morning. The Germans, thinking that a fleet of heavy ships was bound their way, judging by the indications on their radar, let loose with their heaviest coast defense guns. The idea was to convey the impression that the landing was going to be considerably farther up the coast than was actually the case. There were, of course, various other reasons why the Germans were sure the landing would be near the Pas de Calais. Perhaps the use of "chicken" helped to support this delusion on the part of the enemy. It was rather surprising that these little ships were not thoroughly shot up but the fact is they all got back to British ports. Perhaps they were too small to hit. Since the main radar echoes came from the "chicken" suspended from the small ballons towed behind the ships, the shore batteries relying on radar were probably trained considerably behind the ships themselves. This may partly account for the inaccuracy of the German fire.

This demonstration at the Chesapeake Bay Annex was surrounded with secrecy and only a few of us knew the full implications. I do know that when he was out in the Pacific with a Carrier Task Force, Admiral McCain made considerable use of the information he gained that day. Both Admiral McCain and Admiral King were greatly interested in these domenstrations and very pleased with their outcome. Admiral McCain wrote a very commendatory letter to the Director of the Naval Research Laboratory saying it was seldom that he had seen such a complicated schedule of events carried out with such accuracy and precision. He didn't realize what a tough time we had had to bring about just the desired result. I would like to say in passing that the Navy lost one its most brilliant leaders when Admiral McCain died not so long after he returned from the Pacific.

At about this same time, in response to urgings from the office of Naval Operations, a special countermeasures building, known as Building 5, was hurriedly put up at CBA and equipped with rooms on the second floor which could be used as barracks and class rooms, the main laboratory being on the first floor. Special classes in countermeasures were organized as part of the crash program to get competent people out in the Fleet who knew something about this work. In a short while, however, the Navy had regular schools going in other places which were able to take up this burden in a big way. The countermeasures building remained the center for our counter-measure work and for setting up and examining captured equipment as fast as such equipment came in.

The examination of captured equipment is, of course, naturally carried out as well as can be in the field by special teams qualified to make such examinations. For detailed examination, however, it is necessary for highly trained technicists to set up and operate the equipment as a laboratory where suitable measuring equipment and other accessories are available. As the war drew to a close, especially after the surrender of Germany, huge quantities of captured equipment began to arrive at NRL; some of it is set up and in use at CBA but great masses of it still await examination. It may be said here that the best Japanese equipment clearly shows the influence of German engineering and German design. Their electronic equipment in general, is not only inferior to ours but is also inferior to German equipment. There are still some valuable lessons to be learned from this mass of captured equipment, if the staff of the Laboratory can find the time to study it. The present Director of the Laboratory, Commodore H. A. Schade, was at the head of the American Technical Naval Commission to Europe and is responsible for much of this German equipment getting back to this country.

I have already pointed out the contributions of the Naval Research Laboratory in connection withthe development of homing equipment for the purpose of bringing aircraft back to the carriers. Early in the war it became apparent that equipment which would guide aircraft to a given destination at a considerable distance from its base was of great importance. Equally important was the problem of getting the aircraft safely home, especially in foul weather.

The equipments developed before the war were designed primarily for homing back to base from distances not in excess of 150 miles. It was obviously highly desirable to develop equipment with far greater range and a very high degree of precision so that it could be used both for blind bombing and for homing back to base from great distances.

The British developed a system which bore the code name "Gee", designed primarily to get their big bombers back at night after raids over Germany and guide them to the General neighborhood of their individual landing fields. As the number of planes used in the raids increased, an astonishingly large number of landing fields had to be developed in the British Isles for handling these huge numbers of planes. Since many of these fields were frequently obscured by fog, there was a time, so I have been told, when the number of planes lost in landings on the return to England was as large as the number shot down by the Germans during the raid.

The "Gee" system consisted of three transmitters, all on the same frequency, which was somewhere between 40 and 60 megacycles, and synchronized so as to simultaneously emit fairly high power and very short pulses, like radar pulses. One of these stations would act as a master station with an automatic device controlling the other two. These simultaneously emitted pulses would be received for several hundred miles, especially under favorable conditions, by a receiver in the aircraft which could indicate the exact position of the plane. This position determination was possible by the use of certain timing circuits because the pulses from the transmitting stations, having to traverse different distances to arrive at the plane, arrived with certain definite time differences depending on the plane's position. This, as in the case of radar, required very accurate measurement of times even shorter than a microsecond. This system was very successful in getting every bomber to the general location of its particular landing field, from where short range blind landing devices could take over and bring the plane down in safety.

Although this system was designed primarily for getting the planes home to England, it was used for a time for blind bombing of German industrial cities known to be making implements of war. However, this condition did not last long because the Germans soon found out what was going on and erected large numbers of low power jammers in various parts of the countries under their control. These effectively prevented the bombers from getting accurate fixes over important targets. At the same time this jamming, which is an interesting example of measures and countermeasures, did not in any way prevent the English planes from using the "Gee" system to get home because, after they approached the English Coast, the signals from the jammers rapidly grew weaker and were entirely insufficient in strength to interfere with the proper location of the British air fields.

The long distance range and navigation ("Loran") system developed in this country on a much lower frequency, in order to have very long ranges, was evidently a follow-up on the British "Gee" system. "Loran" works on the same general principle but covers very much greater distance, sometimes up to over 1500 miles and is usable by either ships or planes. In the Pacific areas the American forces set up many "Loran" stations in captured islands, especially in the later stages of the war as we approached Japan proper. These contributed greatly to night bombing operations. By far the largest part of the "Loran" program in this country was carried out by the Radio Research Laboratory operating at Harvard under the NDRC. The Naval Research Laboratory also had a small group of six or eight people continuously involved in this program during the last two years of the war. There are many "Loran" stations in active operation today. They mark a definite advance in the art of navigating blind, either by ship or plane.

The system has its limitations, particularly in the tropics where rather heavy static prevails on the lower frequencies which are necessary for very long distance navigation. The equipment on shore is definitely complicated and, I would say, a bit tricky in operation. The equipment carried by the plane is not very heavy and is relatively easy to operate if it has frequent service check-up before and after flight.

The development of special beacons to work in conjunction with radar occupied a good deal of our time during the period covered by this chapter. The radar beacon is a device which permits a target which carries such a beacon to be picked up and identified when illuminated, so to speak, by radar. This identification can be made by the aid of a beacon over far greater ranges than can be obtained from the simple radar reflection or echo. This type of beacon is, then, a device which is capable of being triggered off by the incidence of a radar pulse, or series of pulses. In turn it gives out a reply signal which is much stronger than the normal echo would be if received from the same target not equipped with the beacon.

The idea of using such beacons was proposed at a rather early stage in the war both in this country and in England but the British, I believe, were the first to successfully put it to work in actual warfare. Their first airborne recognition system was of this type, Before the war was over beacons were used for a good many purposes. The Naval Research Laboratory had a hand in the development of a number of them, the work being under the direction of Dr. Claud E. Cleeton and Dr. E. H. Krause assisted by Mr. T. R. Burnight and a number of others.

At the time our ships first started to use radar they experienced difficulty in properly adjusting transmitters and receivers when they were cruising in a region where there were no suitable targets. The echoes from water waves came from too short ranges to be properly used as a means of adjusting a radar to maximum performance. When ships were operating in company they could use each other for targets at respectable distances and thus tune the radars so as to get maximum response. Unfortunately, some ships actually got into engagements with the enemy with their radars not functioning properly, although no one was aware of it.

To remedy this situation, Mr. Frank Isely of the Receiver Section, under Mr. T. McL. Davis, developed what finally received the name "phantom target". This consisted of a small "black box" which could be connected in such a way to the radar equipment that it gave a response just as useful for tuning up the set as would an actual distant target. This device is very simple and very clever. Mr. Isely deserves much credit for the work he did on this "phantom target". It went into very large scale production and became a regular part of many of the radars.

I have said something about how American and British airborne radars assisted the anti-submarine warfare campaign against the Germans. Another device is worth mentioning in connection with the battle of the Atlantic. When the Germans developed their "wolf pack" attacks on convoys they found it necessary to use high frequency communications in making rendezvous for the pack. After a German submarine located a convoy it would not immediately attack, but waited until it could summon a number of other submarines from the same vicinity so that a joint attack could be made with the hope of inflicting much more serious damage. The frequencies used by the Germans were known but they were not only difficult to intercept because of their extreme brevity, but their frequencies were high enough so that they were beyond the range of any direction finders with which our ships were then equipped.

The British developed a type of high frequency direction finder which, while not capable of giving accurate bearings at any great range, would give an approximate bearing at a considerable range, and a fairly good bearing inside of thirty miles. At the Naval Research Laboratory Mr. W. B. Burgess and Dr. M. K. Goldstein of the Direction Finder Section modified a number of these British direction finders and perhaps improved on them somewhat. We then assisted the Bureau of Ships to go into a considerable production program and trained people to install and operate these new direction finders. They were put on destroyers and destroyer escorts. There is no question but what these devices, suggested originally by the British and jointly developed and produced by both countries, helped a great deal in the anti-submarine operations in the battle of the Atlantic. Our escort ships were often able, by the use of these special direction finders, to locate and break up the German submarine "wolf packs".

For a number of years the Naval Research Laboratory had been developing special equipment for testing radio gear for resistance to shock, vibration, humidity and large temperature variations. We had not been fighting long in the Pacific before we found a new factor which came to be called "tropical deterioration". This came about due to the effect of high humidity, high temperatures and fungi. Much of our equipment was ruined before it was ever installed in planes and ships especially if it had been shipped to advance bases and had been lying in a jungle somewhere before installation. The Naval Research Laboratory started the first program on tropicalization with the cooperation of the Department of Agriculture and other Government laboratories. We succeeded in developing some special lacquers or "dope" with which apparatus could be painted without destroying its electrical insulation. This would retard the growth of these fungi. The situation was often so bad, in the Pacific at least, that aircraft equipment had to be taken out of the planes every night and put back in the morning after a thorough drying out. This program of tropicalization was utimately turned over to an NDRC group and to various commercial people. Much was learned about the way to prepare electronic equipment for shipment and use in the tropics. This research will have important commercial aspects in peace time.

Toward the end of the year 1943 our most serious concerns, as I have perhaps previously indicated, were the development of both offensive and defensive countermeasures and the development of a better radar operated identification system than any of the systems which the Allies had been using so far.

It seemed reasonable to assume in late 1943, that the war might drag into 1947 or even into 1948. The rapid pace of production assured us that it would not be long before the Allies would have very definite superiority in the field of aviation. This meant that the need for improved identification systems, especially for airplanes, would be even greater than earlier in the war.

Earlier in the war, when the Axis Powers had superiority in aviation, our problems of identification were easier because we did not have many planes in the air at one time whereas the enemy had a great many. Later in the war when we were to have more planes in the air than the enemy, the problem of properly identifying our own planes would become more important if we were to successfully avoid shooting down our own planes. Both British and Americans had encountered, to their cost, several incidents of this nature.

At this time the Allies had an improved identification system worked out jointly by their scientists but based principally on what was known as the British Mark III system. The simplier Mark II system had already been discarded because the Germans could too easily imitate it and too easily intefere with it. At the Naval Research Laboratory we had developed another system known as the Mark IV which was being held in reserve for an emergency in case the system then in use should be jeopardized by enemy countermeasures. It will be clear that the scientists of both countries were desperately eager to produce a new system which would be in every way better than existing systems, less susceptible to interference and more fool proof in operation. There is probably nothing more discouraging in the whole field of warfare than to shoot down a friend who has been mistaken for an enemy.

Nevertheless this problem was so difficult that the combined research group, located at NRL and made up of British scientists, NRL scientists, Army scientists and NDRC scientists, was not willing to guarantee a solution which would meet the demands of all the allied services and at the same time could be put into production at an early date. It was perhaps natural that the military people should be irritated over this state of affairs, but the truth of the matter is that the earlier systems were pretty good. It was quite impossible to produce a new and better system meeting the military requirements specified by all the branches of the Allied Services and expect to get it into production at an early date. Vice Air Marshall Tate, R.A.F., accompanied by Sir Robert Renwick came over from England to look into this matter and to urge us to spare no pains to speed up this work. However, the opinion of both the British and American scientists was unanimous that we could not advance the dates which had already been specified. Both the Air Marshall and Sir Robert were very much upset. The appealed to the combined Chiefs of Staff and the Secretary of the Navy to put pressure on us to move faster. But this wasn't a case of putting on pressure or more money and, although we were still adding men to this group as rapidly as possible, it wasn't wholly a question of more men. We were simply up against a problem that couldn't be solved in a short period of time.

In December of 1943 the British sent over Sir Robert Watson-Watt to look into a number of things having to do with radio and radar production and, in particular, into the IFF or recognition program. I was particularly interested in meeting Sir Robert, because in a way he was my "opposite number" in England. At the same time, he was connected with the British Administration at high levels and had very wide influence in their entire scientific effort. Naturally I had read many of his publications over a period of years but neither of us, up to the beginning of the British disclosures in 1940, knew that the other was interested in the field of radar, although we may both have suspected that such was the case. Sir Robert Watson-Watt impressed me as a man not only of extraordinary scientific ability and wide range of interest, but with other characteristics indicating very unusual administrative and executive ability.

I attended a long and somewhat acrimonious conference on the recognition problem. Army, Navy, NDRC and British representatives were present. This conference was held at NRL. It was for the purpose of reviewing and settling once and for all, what we were to do about that problem. In other words, if the war could be expected to end soon, this large group should not try to solve the problem at all. These people could of course be used in other fields. Although the British seemed reasonably sure that the war in Europe would be over in 1944, our people were not so certain and we refused to commit ourselves to an early date for the end of the war in the Pacific.

After two hours of arguing about this matter, the Chairman of this conference called on Sir Robert, who had remained silent throughout the discussion. He had taken no notes on the various remarks, although his secretary had done so. Without even referring to his secretary's notes, he gave the most succinct and clear summary of all the pros and cons; apparently he had perfectly remembered every detail of every argument that had been presented at that long conference. It was a remarkable example of concentration, good memory and unfaltering judgment. He came out with the opinion that the work should go on, since the war might well last longer than the military people estimated. His summary was so conclusive and so well put that there was absolutely no argument on the matter. The work was ordered continued.

On the 13th of December 1943 Sir Robert visited the Chesapeake Bay Annex, accompanied by Air Commander C. P. Brown, R.A.F.; Captain Burghard and Captain Walthers of the Royal Navy; Group Captain D. H. Johnson, R.A.F.; Dr. Horton and Dr. Lewis. This group witnessed a number of interesting demonstrations concerning radar, recognition and countermeasures.

Extensive demonstrations and an important conference on countermeasures were held at the Chesapeake Bay Annex on the 23rd of December. The conference was attended by Admiral McCain, of Naval Operations; Admiral J. R. Redman, Director of Naval Communications; Admiral Lybrand P. Smith, Executive Office of the Secretary; Admiral F. D. Wagner, Commodore Oscar Smith and Captain S. Teller of Operations Research Group; Major General McClelland, Lieutenant Colonel McCrea, Major Sheetz and Major Wilson of the U. S. Army; Dr. Vannevar Bush of OSRD; Dr. Frederick E. Terman and Dr. Howard Chinn of the Radio Research Laboratory, Harvard (OSRD). The British had a considerable delegation at this conference headed by Captain R. Tollenmarche of the Royal Navy and Lieutenant Colonel (Lord) Sysonby. Other British observers were: Captain Willett, Captain Glover and Lieutenant Bird of the Royal Navy; Major Baron and Major David of the British Army; Squadron Leader Bailie, Group Captain Bell, Flight Leader Furness, Flight Leader Kelsey and Wing Commander Reddrop of the Royal Air Force. Also present were Commander Bowen of the Royal Australian Air Force; Commander Giles of New Zealand, Squadron Leader Stockwell of New Zealand Air Force and Lieutenant Colonel Jarvis of Canada.

I mention all these names to illustrate how wide were our contacts and how close our cooperation with various branches of the Allied Services.

During the period covered by this chapter we had so many military and civilian technical visitors from England and other parts of the British Empire that it would be impossible to list them all.

The visit I received in September of 1943 from Sir Edward Appleton, Director of the British National Physical Laboratory, and in a way, one of the principal scientific advisers of the British Government, deserves special mention.

Aside from the pleasure of renewing an old friendship, I found it very interesting to talk to a man who was in such close thouch with British work in various fields of science. In the course of our conversation Sir Edward remarked that during the period when they were rushing preparation of their radar chain stations, which had so much to do with saving England during the German aerial Blitzkrieg, one of the British officers asked him if he thought that the Americans had radar. (The British called it "RDF"). His reply was that he was certain that we had it an equally certain that the Naval Research Laboratory had much to do with it. When pressed for a reason as to why he had come to this quick conclusion, he stated that any man who had as much experience in ionosphere research as Dr. Taylor would be sure to have some of his people working on radar. In other words, he felt sure that anyone who had been aware for years that radio echoes could be obtained from the ionosphere would be seeking methods to put such technique to work for military or Naval purposes.

Except for the British, Canadians, New Zealanders and Australians we had few visitors from military or technical delegations of other Allies. We did have visits from Admiral Renneft of the Royal Netherlands Navy and his principal Technical Aid, Captain Houtsmuller. Admiral Renneft, the dean of the Naval attaches in Washington, made a good many visits both to the main Laboratory and to the Chesapeake Bay Annex. He was always a welcome visitor and one with whom we were very glad to cooperate. I would often draw him into discussions of the international political and military situation on which subject he was extraordinarily well informed. Captain Houtsmuller was a very keen and alert man in all radio and radar technical matters. He had a very quick mind and keen appreciation of new ideas.

As far as I know we had no visitors from France. That country's ability to give technical aid in the war was destroyed very early, which probably accounts for the fact that we had practically no technical contacts with the French. We did have two young Chinese officers with us for a day or two, and one Russian officer, Commodore Orlavsky, who came down with Mr. Kachaloza of the Russian Embassy as interpreter. Since, at that time, I spoke no Russian, I was not in a position to judge the Commodore's ability. He was interested principally in radio direction finders for ships. Through the interpreter he asked a number of questions and seemed satisfied and interested in the answers which we gave him. It is my impression that he himself was not technically trained in the field of radio and radar.

The use of radar and other very high frequency devices at sea and in the field soon brought out the necessity for further research on the subject of radio wave propagation. Early in the war anomalous propagation of radar signals were observed. It was of the utmost importance that systematic studies of wave propagation at extremely high frequencies should be prosecuted in order to provide suitable guides to the use of such equipment in battle.

Normally the propagation of very high frequency waves, whether they be used for radar or radio communications, follows fairly simple laws. These waves normally reach but little beyond the optical horizon. This fact was well known long before the war commenced, but as early as 1941 evidence began to come in, particularly from British sources, that under certain meteorological conditions very high frequency radar waves were capable of reaching very far beyond the horizon. On the other hand there were unexplained instances where radars failed to detect targets at moderate distances at considerable elevations above the horizon.

Before radar ever went out to the Fleet it was known that the majority of these "fade out" zones were due to interference between a direct reflection from the target and another which returned by way of some additional reflections from the surface of ground or sea, but there were still other instances of failures to pick up targets for which there was no explanation. The discovery of radio "ducts" furnished the key to the understanding of these effects which turned out not to be so unusual after all.

Reports reaching us from the British, written by Mr. Pearcey of the Radio Research and Development Establishment of the British Army and by Dr. Booker of the Telecommunication Research Establishment of the Royal Air Force, recounted measurements made on British radars at relatively low elevations. Nevertheless these radars, under certain meteorological conditions, obtained phenomenally long ranges, detecting both airplanes and ships far beyond horizon distances. To these two British investigators must go the credit for the discovery of what later came to be known as the "duct". They attributed the unusual behavior of radars under these conditions to the presence of a very low-lying stratum of air which, due principally to its unusual moisture content, was able to bend these radar waves so as to follow more or less the curvature of the earth, thus enabling them to reach beyond the optical horizon. Measurements made in the Irish Sea by the British in the latter part of 1943 showed that one persistent form of this effect occurred over the ocean when there was considerable wind blowing and a good deal of spray flying from turbulent seas.

Between February and July of 1944 the Naval Research Laboratory organized an expedition to Panama in collaboration with Dr. Stephenson, Dr. Anderson and Mr. Fitzsimmons of the National Defense Research Committee. This expedition worked with the cooperation of the Navy and the Army. It succeeded in definitely confirming the existence of such radar ducts in that region. The NDRC representatives then proceeded to the Pacific and confirmed the existence of such ducts in the New Guinea area and also in the area near Saipan. The Naval Research Laboratory then decided to sponsor an expedition to make an extended study in the Caribbean area, selecting the island of Antigua for the principal base of operations. The study was carried out at that point because it seemed likely that ducts would be found there, since they were associated with winds, and the trade winds of the Caribbean were practically continuous over a long period of time. Also the U. S. Army maintained a base on the island which could furnish facilities and supplies needed. The expedition to Antigua remained there between February and May of 1945. Dr. Stephenson and Dr. Anderson assisted Mr. Katzin with the preliminary planning and Dr. Stephenson went to Antigua and remained there for a short time. Mr. Katzin was in general charge of the test. The Navy furnished ships and personnel, the Army supplying many important facilities and services which were of great assistance.

The next step in this project was to prove that similar persistent duct effects occurred in some other part of the world where persistent winds were a frequent occurrence.

By this time we were interested mainly in the Pacific area, since Germany had surrendered. Plans for the Pacific tests were started in July of 1945. These provided for tests at Pearl Harbor, in the Marshalls and in the neighborhood of the Philippines. This expedition was entirely organized by the Naval Research Laboratory under the direction of Mr. Katzin, assisted by other members of his wave propagation group and by two men from the Airborne Coordinating Group at NRL. These men were Lieutenant Marvin Bannon and Ensign Gordon Raisbeck. ACG had also sent Joseph Suhi, Technican, to Antigua during the experiments there. This expedition, which didn't leave for the Pacific until the last week of February 1946, had the use of an especially modified PBM-5 plane equipped with very high frequency radar equipment and associated test equipment, as well as aerological equipment. It was planned to have the plane fly at various levels and distances, transmitting the signals to a receiving station on the ground. This was first set up on the Island of Oahu. The project was known as "Project Ducks".

By April 1 the first ground station was set up and fully equipped but unfortunately the big tidal wave which started down from Alaska wrecked nearly all of this equipment and washed out the installations on the northeast shore of Oahu. Other equipment, and such as could be salvaged, was then taken directly to Okinawa where very satisfactory measurements were made. Proceeding then to the Philippines further measurements were carried out. Finally, on returning to Pearl Harbor, where damage to the shore equipment had been repaired, the experiments were repeated and finished in June of 1946. The tests at the Marshalls were cancelled because of the project "Crossroads" activities in which the Kwajelein base was heavily involved.

The result of these Project Ducks activities was to show that a low lying duct, sometimes not many feet thick, lay over the surface of the sea when it was whipped by steady winds. The lower part of this duct was heavily impregnated with water vapor which rapidly tapered off towards the top. It was this variation in water vapor content from bottom to top of the duct which gave it the electrical properties necessary to bend the wave in such a way as to get well over the horizon. However, in order to shoot the radio waves into the duct they had to be shot from quite low levels indeed. If the radar was operated from the mast head of the ship, well above the duct level, deviations and irregularities of the signals occurred which fully accounted for the fact that such radars could occasionally miss nearby targets. They could miss targets on the surface of the sea, such as ships, since the radar signals did not penetrate too well into the duct region unless they were started from a low level actually inside the duct.

These researches show that intelligent use of extremely high frequency radar requires close cooperation with meteorological observers. I may say that the effect is not often observed on the lower frequencies unless the duct is of very great thickness, which does not often occur over the open ocean. It is the very high frequency radars which are subjected to certain limitations. On the other hand, if such radars are mounted at suitably low levels they can operate, and have often operated, over extraordinarily long ranges. The record shows that several Japanese ships have been contacted by radar with the help of ducts when they were a great deal too far away to shoot at successfully. Approaches were made by radar and in due time these ships went to the bottom. In other words, the duct can be of great help at times, but under other conditions, it can be somewhat of a nuisance. Part of this research was finished in time to give assistance and advice to the Fleet in the Pacific operations, but the final answer was not obtained until after the Japanese surrendered.

This is an excellent example of the way research fits into a military program. First comes basic research on the device, then development, then production and then a follow-up of research in order to give the best possible advice to the people in the service who are to operate the equipment. Needless to say this duct business is of equal importance to merchant shipping which is already beginning to be equipped with radar.

During the administration of Admiral Van Keuren as Director, two very important radio activities were set up at NRL as subordinate commands, not directly connected with the Radio Division, but very frequently working in close cooperation with the Division. The first of these activities was set up in 1943 at the request of the Bureau of Aeronautics and was known as the Airborne Coordinating Group under the able leadership of Commander A. C. Packard. This group succeeded in setting up a large pool of trained officers, enlisted men and civilians with headquarters at NRL but with their actual operations carried out at numerous forward bases in many parts of the world. This group served to indoctrinate the people on the fighting fronts with the proper use and adequate maintenance of airborne radio and radar equipment.

From time to time these groups would report back to the Naval Research Laboratory headquarters, on being relieved by a new group sent out from headquarters to carry on their work. Naturally they brought back a tremendous amount of information concerning the performance of such equipment in the field, often under actual combat conditions. Many of these men went out with combat missions to observe the performance of this equipment. The information they brought back was carefully edited, summarized and promptly forwarded in the form of special bulletins to the interested bureaus and to the Naval service in general. This group did a wonderful job and has received many commendations for its work.

The second group, known as the Electronic Field Service Group, was set up in 1944, at the request of the Bureau of Ships, to perform similar services for equipment on ships and shore stations. This group, for a short time, was under the leadership of Captain John Emerson Williams, followed by Commander D. C. Good. It is now headed by Commander T. C. Hyers. This group has also done a good job. It could have done a much better job had it been set up two years earlier.

In 1944 the Aircraft Electrical Division, now called the Electrical Division, was set up, largely at the instigation of the Bureau of Aeronautics, to deal with research, development and engineering evaluation tests of electrical equipment carried on aircraft. At the present time the scope of this division is much wider, since its activities are not restricted to airborne equipment.

The arrival of VE day on the 8th of May 1945 brought little change in the activities of the Radio Division at NRL. For many months our faces had been turned towards the Pacific, where the war against the Japanese was, from the very nature of its setting, spear-headed by the Navy. There was no celebration and no cessation of work on VE day. On the contrary, everyone increased his effort in the hope of rendering every possible assistance to bring about VJ day and the surrender of Japan in the same year. Our maximum effort was still in the field of countermeasures.

With the approval of Admiral A. H. Van Keuren, then Director of the Laboratory, a basic reorganization of Radio Division activities was put into effect on the 1st of July 1945. During twenty two years of existence the Radio Division had grown from a mere handful of approximately twenty three scientists, engineers and technicians to nearly a thousand members of the Division. In the course of the years the burden of directing the work in the Division had very greatly increased, being especially heavy during the war years. From time to time assistant superintendents, first Mr. Gebhard and then Mr. Young, had been added to my office. Up to the first of July I was assisted, as superintendent of the Radio Division, by three associate superintendents, namely, Dr. J. H. Miller, Mr. L. A. Gebhard and Mr. L. C. Young and by two assistant superintendents, Mr. Edwin A. Speakman and Mr. Matthew H. Schrenk. I cannot speak too highly of the work of these men in building up the division. They all carried very heavy responsibilities, especially during the war period.

In addition to these competent leaders I had attached to my office two administrative assistants, Mr. Warren H. Andrew and Mr. John M. Clayton. Since the office also carried, in the person of Mr. L. C. Young, the secretariat of the Radio Problem Priorities Board, made up of Navy Department Officers mostly of the rank of Captain, this was an additional burden, although without the services of this Board we would never have gotten through the war period as well as we did. These men, representing Naval Operations and the three materiel bureaus, Ordnance, Ships and Aeronautics, were in a position to define the relative priorities of war work.

I should like here to say a few words about the invaluable work of Mr. Andrew and Mr. Clayton. Mr. Andrew came to us as a volunteer who wanted to get into the war effort but couldn't get into active military service in either the Army or Navy. He had not only a good technical background, having been an upper class radio amateur experimenter for a great many years, but he was, and still is, a very capable business man. I felt that such a man would be of great value to our organization but I had to start him with a moderate salary. Mr. Andrew promptly closed up his business in Colorado and came to Washington. It wasn't long before he proved himself, first as a member of the Aviation Section of the Radio Division and later as a technical administrative assistant in my office. We were very fortunate to have his services. He is now doing similar work in Dr. Miller's division.

The other man, John M. Clayton, is probably known to most of the members of the Institute of Radio Engineers, having been secretary of that organization for a long time. He also is a "died-in-the-wool" amateur and has been with the General Radio Company for a good many years.

In 1942 we began to have great difficulty in getting supplies and materials to carry on our work. Many people thought that the Government laboratories could get anything they asked for but this was not true, since all of our production activities came under the Office of Production Management. I am not making any argument against this set up; nevertheless we were frequently in a jam on a very hot problem, which required producing something in a few weeks' time and getting it at once to the Fleet, because we couldn't get the necessary electronic and electric components with which to build the equipment.

About this time John M. Clayton came to see me. He said that his work as sales engineer at General Radio, in his opinion, wasn't very important and he wished he could do something to get into the war. He had for many years been an officer in the Naval Reserve, but he was no longer able to qualify physically. I told him I would be glad to give him a job as special expeditor for electronic equipment if Mr. H. B. Richmond of General Radio would loan him to us. The result was that he was with us between the middle of June 1942 and the middle of February 1944. He established the necessary contacts and relations with OPM and other organizations having to do with the production material. The result was a magnificent job which tided us over many difficulties. I have no hesitancy in saying that this was of very great benefit to our war effort. Even when Mr. Richmond requested him back in 1944, we were loathe to lose him although by that time he had indoctrinated certain assistants in his office, particularly Mr. Andrew and Mr. Leighton, and had so systemized the work that we could carry it on in his absence. Certainly I am very grateful to Mr. Richmond for permitting this arrangement to last as long as he did. Johnny Clayton received a letter of commendation from Admiral Van Keuren when he resigned on the 15th of February 1944.

In addition to these people working in the office of the superintendent, I had several consultants and coordinators who gave general advice and did "pinch hit" contact work throughout the Division. Among these I must specifically mention Dr. Andrew H. Haeff, Dr. Bernard Salzberg and Mr. Martin Katzin. Their work was of the highest order.

The change made on the 1st of July 1945 resulted in the replacement of the old Radio Division by four divisions, then called the Special Electronic Research Division; the Airborne Radio Division; the Ship and Shore Radio Division and the Fire Control Division, each under a superintendent who reported only to the Director of the Laboratory. In addition to this my office became the Office of Chief Consultant and Chief Coordinator. Dr. Miller and Mr. Gebhard respectively headed up two of the new divisions and Mr. Young remained with me as Assistant Coordinator and Secretary of the Radio Problem Priorities Board. The principal consultants remained attached to the Coordinator's Office which was further supplemented by Mr. C. B. Mirick and Mr. Warren M. Andrew as Technical Administrative Assistants. The routing of all reports and mail, as well as the assignment of problems to the different divisions remained the responsibility of the Coordinator's Office. The office also made its consultants available to any division. They spent practically all of their time within the divisions rather than in the office.

Coordinators were appointed from time to time to take care of particularly knotty problems which were being simultaneously worked on by more than one division. Nevertheless the Chief Coordinator and Consultant interfered in no way with the direct control of the individual superintendents over their work, and interposed no obstacle in the way of direct contact between the superintendents and the Director. The Coordinator's Office operated as part of the staff of the Director rather than as a divisional organization. This relieved me of a great deal of burdensome detail and permitted me and my staff to concentrate on the broader aspects of radio problems.

Two officers were also attached to the office; one as a consultant in aviation matters, Commander W. C. Clay, and the other Lieutenant Commander R. R. Lewis, who had been for some time acting as coordinator for the testing of the gun directors used for fire control (director systems). Lewis has retired to civilian life some time age. I want to say here that he did a splendid job under very difficult conditions. Commander Clay is on terminalleave at the present time and will soon be lost to us. He will be greatly missed.

In November of 1945 Admiral Van Keuren, already past the legal age of retirement, was detached from the Laboratory, being relieved by Commodore H. A. Schade. Before this, namely on May 19, 1945, the Office of Research and Inventions had been established as part of "EXOS" (Executive Office of the Secretary) and NRL became a division of this office. On the 1st of January, 1946 the new Director, Commodore Schade, decided to still further simplify the electronics set-up. The Steering Committee, consisting of the superintendents of all divisions in the Laboratory with the Deputy Director as Chairman, was charged with the functions of coordination and with the details of problem assignment. My consultants and coordinators were, therefore, distributed within the divisions. I was retained on the staff of the Director as Chief Consultant for Electronics. (I may add that this further lightening of my duties is responsible for my having sufficient leisure to write these reminiscences.) Later, on August 30, 1946, the Office of Research and Inventions became the Office of Naval Research with Vice Admiral H. G. Bowen as the present Chief. He will retire in November of this year, 1946, and be superseded by Rear Admiral Paul F. Lee.

With the advent of VJ day on September 2nd, 1945, we were able to cut out all overtime work, restore the Laboratory to normal working hours, and sit back and consider what our post-war program ought to be.

There is no doubt but what the advent of the atomic bomb has had a profound influence on plans for future Naval research. It should be remembered however, as one commentator very aptly put it, that while the atomic bomb finished the war, radar fought the war. At the same time it is inconceivable that we can dispense with further advances in that part of the electronics field known as radio, if we are either to use atomic weapons in the future or are to find a defense against them. I see more unsolved problems in the radio field today than at any time in its history, but at the same time a better promise of satisfactory solutions.

Naturally everyone hopes that these terrible new weapons will be outlawed but we cannot overlook the fact that down through the years, weapon after weapon has been invented, some of them looking like very terrible weapons at the time they were produced. But terror of a new weapon has never in the history of the world prevented people from using it in warfare. It has only stimulated the search for countermeasures. Although we all hope and pray that the old rule will be broken, we cannot afford to neglect these countermeasures, human nature and national aspirations being what they are today.

As I look back on thirty years of Naval experience I am profoundly impressed with the great change in the official attitude with respect to scientific research. Even thirty years ago there were officers here and there who had respect and appreciation for research in the field of radio and, no doubt, in many other fields as well. But these officers were in the minority. It could be said that the official attitude was either a mild tolerance or indifference. Today that is all changed. The present official attitude has been aptly expressed by Fleet Admiral Ernest J. King in "The Navy at War". On page 228 he says: "Scientific research can not only expedite the invention and production of weapons but can also assist in securing their correct use". Again on page 231 he says: "Only by continuing vigorous research and development can this country hope to be protected from any potential enemies and maintain the position which it now enjoys in possessing the greatest effective Naval fighting force in history".

Aside from taking a natural pride and satisfaction in the work of the members of the organization at NRL which I directed for so many years, I feel an equal satisfaction in the fact that so many of these achievements have had important peace time applications which have benefited this country as a whole.

The field of radio is still fertile. There are more opportunities for advanced research and development today than ever before. If I could shuffle off fifty years and begin over again I would ask nothing better than to start another fifty years of radio.

RADIO REMINISCENCES: A HALF CENTURY

CHAPTER XII - 1929-1933 - New Developments and Super-High Frequencies.

CHAPTER XIII - 1933-1937 - The March to Still Higher Frequencies Radio and Radar.

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CHAPTER XII

1929-1933 - New Developments and Super-High Frequencies.

During this period radio investigators were getting into more and more desperate straits for lack of suitable insulators which would stand up under the influence of strong high frequency fields. At this time we had Pyrex which even at reasonably high frequencies stands up well, but it had to be cast or molded. It was very difficult to produce it to the exact dimensions which might be required. Moreover, it could not be machined or worked up into small parts for special purposes. We also had Micalex, previously mentioned. To some extent this could be machined but was not suitable for making up into small parts. Neither was it as good an insulator for high frequencies as we needed. We also had quartz, both of the crystalline and fused form, but although this substance is one of the best of insulators, even at very high frequencies, it is so hard that it is impossible to machine it at all except by long drawn out grinding operations. In the desperate search for a new high frequency material, we were ready to try almost anything.

An interesting incident occurred in connection with this matter. It led to the development of a substance which, temporarily, at least, enjoyed great popularity not only with the Naval Research Laboratory but with the commercial organizations who were equally concerned with its use. Later this substance was supplanted by still better insulating substances so that it is no longer widely used. The possession of it in the early 1930's permitted us to go ahead in the high frequency field so that its development, although not leading to a permanently useful product, was of a very great importance to the Navy and to the industry. This substance was finally named Victron.

The discovery of the properties of Victron came about in this way. Dr. Bichowsky, then head of the Chemistry Division at the Naval Research Laboratory, visited the Naugatuck Laboratories, a subsidiary of the U. S. Rubber Company. One of their chemists showed him a translucent substance which could be produced in a great variety of colors. It was a bi-product of the manufacture of alcohol and ought to be useful to somebody except that it was a little too expensive to use for hair brushes and tooth brush handles and such gear. Also it had a rather low melting point. Bichowsky asked if they had tested the properties of this substance as an insulator. They replied that they had no facilities for making such tests. Bichowsky then asked for a few samples to turn over to the Radio Division at the Naval Research Laboratory, since it was known to be on the lookout for anything new in the way of insulators.

Our tests on this product showed it to have lower high frequency losses, that is less heating effect, than any insulator that we had so far tested except quartz. However, its low melting point was a decided objection to its use in high frequency transmitters which were bound to get pretty warm. Moreover it wasn't as strong as such an insulator should be.

We immediately contacted the company's engineers asking for a conference to see what we could do about remedying these difficulties. The result was a long series of joint experiments. The company impregnated this material with various substances such as powdered quartz, mica and other things to see if they could raise the melting point and give it more mechanical strength without destroying its excellence as an insulator. Curiously enough, the most satisfactory substance for impregnation turned out to be lithopone, although this substance of itself is a very poor insulator indeed. This gave the material a somewhat higher melting point which, although not fully satisfactory, made the substance useful. At the same time the impregnation improved the mechanical strength. So Victron was born and in the process changed its color from a pale translucent amber to white. This substance was easily machined and the company engineers were able to supply us with sheets of varying thickness as well as with rods which could be made up into whatever forms we desired. While it didn't have either the resistance to high temperatures or the mechanical strength ultimately desirable, it was a very important and valuable stop-gap, permitting progress in the developments in high and superfrequency fields which might have been stalled without it. Of course further experiments with non-machinable insulators such as isolantite, steatite and other ceramics were vigorously continued, again with the close cooperation of the industrial agencies.

This continual pushing into new fields required constant effort in the development of new instruments. A radio engineer needs for test purposes an instrument called a signal generator. This is really a low power but very precisely calibrated transmitter which can be used to test and calibrate receivers and other gear. There were no such instruments available for the higher frequency fields in which the Laboratory was already working.

We were fortunate in being able to push some of the instrument makers into this field. We were obliged to build some instruments ourselves to prove that such instruments were actually feasible. Melville Eastham of General Radio and the members of his staff cooperated very closely with us after I made a special visit to Cambridge, Massachusetts and applied all the persuasion of which I was capable. Ferris was already in the field of signal generators so that company also began to push into the high and superfrequency fields. Many special contracts were let with such people for the sole purpose of getting them interested. I know that this helped not only the Navy, but the whole radio industry. Mr. Eastham used to say it didn't seem that he could ever satisfy us because, by the time he got one signal generator designed, we already had on the board another one of much higher frequency. I am sure today he would be the first to admit that we were the first to see the tremendous demand that would come from all directions for such instruments.

I have mentioned in a previous chapter the investigations which we carried out in connection with high frequency hazards which might come from sparks, induced by high frequency transmitters, which could in turn set off explosives or gasoline. By this time this particular ghost was pretty well chained. Proper procedures and countermeasures had been developed so as to render this hazard far less than other hazards which are daily faced by Naval ships.

There was another type of hazard which created some worry at this time. It had been noticed by all of us who worked in close proximity to high power high frequency circuits that our hands and arms, reaching out to make adjustments in the transmitter, would get very hot although not in contact with any circuit. Further than this, we noticed that those engineers who worked for hours in the neighborhood of such high power equipment, often complained of slight headaches and a feeling of lassitude. Dr. Whitney, of the General Electric Company, about this same time pointed out that exposure of the human body to high frequency electro-magnetic fields could produce an artificial fever and recommended this for the treatment of certain diseases. The medical profession was definitely interested. It wasn't clear whether these induced currents in the human body, which were not a result of contact with the circuit but were transferred to the body by the process of induction or coupling, were solely heat effects or whether there was some abstruse electrical effect in addition.

The Navy has always been anxious to minimize the exposure of its personnel to unnecessary hazards. We therefore asked the Navy Bureau of Medicine and Surgery to cooperate with us in some practical tests.

At this time we had a new type of water cooled transmitting tube, made by the General Electric Company, which was capable of putting out approximately 30 kilowatts of high frequency energy at 50 megacycles. If this energy was fed into a vertical rod a quarter wavelength long (approximately 5 feet) it was capable of creating a very strong field of electrical force anywhere within a range of 10 or 20 feet. Indeed, if the top of this antenna were touched with an insulated metal rod which was subsequently withdrawn, it would burst into a brilliant flame which streamed off into the air. Unless this discharge was started in this manner there was no visible effect. Engineers working in the vicinity of this equipment experienced headache and lassitude within an hour. The measurements made by the doctors from the Bureau of Medicine and Surgery showed that their body temperatures went up sometimes to as high as 103°. The Laboratory furnished a number of engineers, including Dr. L. P. Wheeler, then assistant Superintendent of Radio, Mr. L. C. Young, Mr. O. C. Dresser and several others to act as human guinea pigs. Some real guinea pigs and rabbits were also provided. The latter were exposed to very strong fields and were pretty thoroughly ruined. The outcome of the investigation showed that these effects were not permanently disabling to a man of normal health. Indeed, curiously enough, some of the volunteers stated that although they felt miserable the day of the test, they felt very much better than normal the following day. Local temperatures in the arms, if the hand held a short metal rod, could be raised to as high as 107°, although this rod was not closer than 10 feet from the antenna. It was finally concluded that with properly shielded transmitters there was no appreciable hazard to Naval personnel. I am glad to say that none of our human guinea pigs showed any unpleasant after effects as a result of these experiments. Today machines to deliberately produce this effect on a regulated scale are commonly used by both doctors and chiropractors.

As these machines have come to be widely used they have developed a real problem for the Federal Radio Commission (later Federal Communication Commission) because they are in effect transmitters which violate the law, since they operate without licenses and without keeping to assigned frequencies.

Mr. Young and I made exhaustive experiments with one of these machines using it as a transmitter to communicate satisfactorily from Washington to Seattle. I have without great difficulty picked up by radio receivers in Washington machines which were operating in Europe. The European machines used 50 cycles supply current, whereas all American machines at that time used 60 cycles except a few on our West Coast which used 50 cycles. A directional receiver would readily differentiate between these West Coast machines and those of Europe.

It was definitely proved that these machines could operate without interference with world wide communications if they were operated in a shielded room The shield could be made of copper screening and still have good ventilation in the room.. Suitable filters had to be put in the alternating current supply leads to the machine in order to prevent the high frequency currents of the machine from backing up into the service lines for electrical supply and finally radiating out into space.

Harvard University cooperated with us in a test which proved that this could happen. We heard in Washington a shielded machine at Harvard without difficulty, using a not particularly sensitive receiver. With suitable filters in the supply line the signals could not be heard. The circuit of the machine used in these tests was provided with a key by which identifying dots and dashes could be transmitted.

For machines used in hospitals this Laboratory had always recommended complete shielding and filtering of supply leads. Unfortunately, however, this doesn't solve the entire problem since many of these machines are portable and the doctors like to bring them to the homes of bed-ridden patients where treatment is required. The legal question arises as to whether or not it is in the public interest to improve communication by suppressing portable machines. There is a way out of this. By international agreement a set of precisely fixed frequencies could be allotted to these machines so that they could be operated without shielding, but this in turn means a more expensive machine. Naturally this is vigorously opposed by the manufacturers of these devices as well as by the customers. Nevertheless I think this is the final answer to this problem.

Communication at this time advanced tremendously, both in the Navy and in commercial circles. The point had been reached where simultaneous transmission and reception was a necessity rather than a convenience. The commercial centers, like RCA and Bell Telephone Laboratories, solved the problem by physically separating the transmitter and receiver sites by a good many miles, but the Navy was very slow to follow this procedure because, it must be remembered, we were in a depression period. Personnel in the Navy was at a low level and money was scarce; it would require extra personnel and more money to operate a separate receiver site. I had steadily kept pressure on the Navy since 1919 to adopt the separate receiving center with remote control of transmitters but it was a good many years before I succeeded in getting the backing necessary to put it over.

On board ships the condition was difficult indeed, since adequate separation simply could not be obtained. However, we did accomplish a great deal by localizing the receiving antennae which, thanks to amplifiers, could be small in dimensions, in one end of the ship and the transmitting antennae in the other end. Suitable filtering circuits and the right type of receivers soon permitted us to do a great deal of "duplexing". This means the operation of transmitters and receivers simultaneously although not necessarily on exactly the same frequency. The speeding up of communications resulting from the Navy's adoption of duplexing operations on large ships was a notable achievement.

Mr. L. A. Gebhard made an extended trip on the ARIZONA in March, 1931 when she cruised to Puerto Rico and the Virgin Islands. He checked up on the performance of her radio equipment and arranged for duplexing. President Hoover, who was on board during this cruise, inquired whether he could be put in contact with his son in Asheville, N.C. where he had an amateur radio station with radio telephone attachment. Thanks to the cooperation of Lieutenant Commander (later Captain) C. F. Holden, the Communication Officer, and Lieutenant (later Captain) H. A. Tellman, Radio Officer, Mr. Gebhard was able to put a telephone attachment on the XAF standard high frequency transmitter with which very satisfactory exchange of conversation was obtained with young Hoover at Asheville. In the President's cabin, the loud speaker on his broadcast receiver was connected to a receiver for amateur frequencies and a microphone was installed for the President's use, so that the conversation was carried on from his own quarters.

The commercial stations took the lead in the development of highly directive equipment on high frequencies for their long haul point to point work. The trans-atlantic telegraph and telephone circuits operated by RCA and the

Bell System were able to put up large directive installations for both receiving and transmitting. This was possible because they worked over fixed circuits from this country to cities in other countries. Therefore a reasonable number of these directive systems could cover all necessary circuits. The Navy lagged in this matter, probably because there was no way of applying these cumbersome antennae to our ships. The Laboratory had directive equipments which were used as mentioned in a preceeding chapter in the communications with the Byrd Expedition to the Antarctic and on many other tests. Finally our shore stations began to put in directive equipment over their principal circuits.

General adoption of what was called the Cornet System, which the Naval Research Laboratory originally installed at Arlington, soon followed. The Cornet System permitted simultaneous broadcasting of four frequencies in the 4, 8, 12 and 16 megacycle bands, all these frequencies being in harmonic relation. This greatly improved our intercept schedules sent out to all ships. A report on the Shanghai incident of 1932 refers to the efficiency of this system. At that time we had a number of very small shallow draught gun boats going up and down the Yangtze River as far as the Gorges. These ships, originally equipped with low frequencies, were never able to maintain decent communication with our Asiatic Fleet or with the Legation at Peking. We advised the Bureau of Engineering of the type of high frequency sets the ships could carry. After they received this equipment they never had any serious difficulty in maintaining communications. The PANAY, later deliberately sunk by the Japanese long before the opening of the War, was one of these ships.

Concerning the directive antenna used by the Navy Shore Stations, particular mention should be made of those developed for RCA by H. H. Beveridge and his assistants at Rocky Point, and to the Bruce antennae with variations thereon, developed by the Bell System, and adopted by the Navy.

The matter of fading of high frequency signals due to recurring destructive interference between the different rays arriving at a receiver continued to give us a good deal of worry. Here too, the commercial interests had much at stake. To them must go the greatest credit for the "diversity" systems devised to greatly ameliorate this nuisance.

A diversity system consists of a combination of the outputs of several receivers connected to fairly well separated antennae, in such a way that the system automatically selects the signal which is stronger. When receiving antennae are separated only a few hundred feet, the signals do not fade at the same instant on the different antennae. The Bell System accomplished this thing through a very ingenious arrangement for picking up the rays at varying angles of approach from the ionosphere. The first system may be called "space" diversity and the second "angle" diversity. Some of my readers may remember the extremely poor quality of the early high frequency broadcasts from Europe, which were rebroadcast on ordinary frequencies in this country. A great deal of distortion and fading was present and, except at rare intervals when things remained steady for a few minutes, it was very difficult to understand the speaker. If we contrast this with the extraordinarily good quality of rebroadcast European transmissions as received in this country today, we can realize the tremendous improvements brought about in high frequency long distance reception by the use of diversity systems. The Navy had adopted diversity in a number of its shore bases but it is not feasible to put it on board ship since it requires a very large area in which to install it.

The rapid increase in the use of high and superfrequencies introduced new and difficult problems in the field of radio direction finders. At low and intermediate frequencies the electric forces in the arriving radio wave are usually nearly vertical. A simple wire frame or loop of one or more turns may be so connected to a radio receiver as to permit the rotation of the loop to disclose the direction from which the signal comes.

In military and naval work the radio direction finders are useful not only to keep track of our own ships and airplanes but to permit, by observation of the direction of arrival of a wave from various shore stations, the accurate location of the position of our own craft with respect to the nearest shore. Furthermore, direction finding by radio is absolutely essential if we are to learn something of the enemy's activities and whereabouts. The extensive sweep of the early Japanese operations in the Pacific Islands during the late war permitted them to establish large numbers of direction finding stations on islands here and there so that they were able to give our units operating in those waters a great deal of trouble. They had an excellent network giving much advanced information as to the whereabouts of our various units. Finally these stations were systematically bombed out by our Army and Navy planes. This was none too easy a job. It had to be preceeded by elaborate photo reconnaissance and many stations had to be bombed out more than once, because the Japanese had a lot of spare parts. Unless the installations were pretty completely demolished, they would rebuild them in a few days.

High frequency direction finding required the development of new and special devices, since the high frequency waves arrived at the direction finder site with their electrical forces in all sorts of angular positions. Moreover the angular position of the electric force varied constantly from time to time. This sort of thing completely ruins the action of a simple loop direction finder, our main reliance on low and intermediate frequencies. It was necessary to devise an antenna system for the direction finding receiver which would respond only to vertical components which were sure to be present from time to time and of sufficient duration to permit the taking of bearings. The British scientists, particularly Dr. Smith-Rose, must be credited with the earliest and most successful attack on the problem of developing suitable direction finders for high frequency. The Adcock System of direction finder antennae is named after the British scientist who developed it. This system uses two vertical sets of rods which collect only the vertical components of the arriving wave. The connections between these two sets of rods, spaced some distance apart, are carefully shielded against radio influence.

Most of our high frequency direction finders were developed as variations or improvements on the original Adcock system although some of the arrangements worked out in this country, particularly the system devised by Dr. DeFriis of the Bell Telephone Laboratories, are sufficiently original to be cc sidered new systems. Our early work on high frequency direction finders was carried on by Warren B. Burgess, aided by Mr. A. H. Moore and Mr. R. H. Worrall. A little later the problem was very aggressively attacked, under my immediate direction, by Mr. Harris Hastings and Mr. Ray Gordon with several assistants. Our particular effort at the time was to get a model that was readily transportable and could be set up on a suitable site in a half hour's time. We also hoped to use such an equipment on board ship, and did take several of such equipments to sea. However, the accuracy obtained on board ship with any high frequency direction finder devices, other than radar, which will be mentioned later, leaves much to be desired. At shore bases on the other hand, our semi-portable equipments were widely used with great effectiveness.

Since we had always gotten into trouble if we ran long leads from the vertical collecting rods to a receiver housed in some shelter, we decided, for this equipment, to put the receiver into the rotated and elevated equipment, with the operator on a little platform at about the same level.

This was, of course, emergency equipment and not intended for final permanent installation.

While we were working on the equipment, we often set these direction finders, mounted on their portable wooden tripods, out in the fields near the Laboratory while they were under test. This put the vertical collecting rods about ten or fifteen feet above the surface of the ground and gave reasonably good results when used on flat terrain. One day, while Harris Hastings was on the upper platform rotating the antennae assembly in order to take bearings on some European stations, Admiral Courtney and Captain Van Meter came down from the Navy Department to visit the Laboratory. Admiral Courtney, then Director of Naval Communications, was very keenly interested in the problem of high frequency direction finding. In those days, Naval officers in the Washington area were not wearing uniforms. When the Admiral saw this direction finder out in the field he recognized it for what it was, stopped his car, got out, walked over and called out to Hastings "Well young man, what are you doing here?" Hastings, who hadn't met the Admiral and knew that the work was considered pretty confidential replied, "Well, who the hell are you"? Admiral Courtney replied "I happen to be Admiral Courtney, the Director of Naval Communications". Hastings, of course, was very much embarrassed and started to make profuse apologies but the Admiral said "Never mind my boy, you made exactly the proper answer". After that they had a good get together and the Admiral mounted the platform and took a lot of bearings himself. From then on he was a frequent visitor whenever high frequency direction finding was in progress.

All of our earlier Navy receivers had been supplied with storage batteries for lighting filaments of the tubes and dry batteries for the higher voltages used on the plates and grids of the tubes. This was, of course, a terrible nuisance as batteries had to be continually replaced or recharged. The advent of an alternating current supply system for receivers required a general redesign of all Navy receiving equipment. Most of our earlier receivers required the simultaneous adjustment of at least two controls, generally the tuning control for the antenna system and the tuning control for the receiver circuit, which was loosely coupled to the antenna circuit. The adjustment of a third control was necessary if we wished to receive unmodulated signals with a local heterodyne oscillator. If there was any adjustment required while the operator was making copy, he was obliged to use both hands to make these adjustments and, therefore, lost some of the incoming messages while the adjustment was being made. Single dial control was badly needed and indeed, had arrived in broadcast receivers used by the general public.

The Naval Research Laboratory was called upon to develop the basic designs of the first Navy receivers to use alternating current supply and single dial tuning. Mr. T. McL. Davis and Mr. Edwin L. Powell deserve a great deal of credit for the thorough way in which this was done, resulting in a line of receivers, finally produced for us by the industry, which stood up a great many years. The people in the radio industry cooperated with us heartily in this program.

In order to hold our transmitters precisely on frequencies we had adopted the use of quartz crystal control to a very large extent. However, as the demands of the service for more and more frequencies and rapid changes in frequencies continued to increase, it was apparent that an appalling number of crystals would have to be issued to the Fleet in order to have all frequencies available which they might need. Furthermore, it was obvious that if we operated on exactly fixed frequencies, an enemy would know just where to look for our signals and have comparatively little difficulty in "jamming" them. This means rendering the signals unintelligible by superimposing on them enemy emissions of exactly the same frequencies.

Commodore Jennings B. Dow, Chief of Electronics Division, Bureau of Ships (then a Lieutenant) insisted that we try to get a high precision of frequency control with the possibility of continuous variation which could not possibly be had with the crystal control system. With continuously adjustable frequencies it would be much easier to avert deliberate enemy interference by prearranged and rapid shifts of frequency.

Dow himself proposed what came later to be known as the Dow circuit, which he worked out at the Laboratory with the assistance of Raymond Owens, L. A. Gebhard and R. B. Meyer. Our engineers went to work along the same lines. It meant producing mechanical arrangements for extremely accurate adjustment of capacities and inductances such as had hitherto never been attempted anywhere in the field of radio. Mr. Meyer was largely responsible for the development in 1931 of the TAD transmitters which had a continuously variable frequency from 2000 to 4525 kilocycles. He used the best ideas contributed by Lieutenant Dow and the Laboratory engineers. Commodore Dow should be given much credit for being the principal initiator of this program.

The Laboratory called in representatives of three large commercial radio interests and explained to them what we wanted and what we had worked out in the way of circuits. They all said the thing was impossible; they couldn't undertake it. So we built the first transmitter ourselves and then had these corporations send down their engineers and draftsmen in order to convince themselves that the system was feasible and workable. By the use of frequency multipliers we could step up the frequency to much higher bands than 4000 kilocycles once we had a stable master oscillator. Westinghouse got the first contract for this type of equipment, resulting in the TBF transmitter, a transmitter that has come through many successful battles and is recognized by all radio officers in the Fleet as one of unusual excellence.

Due to the changing structure of our ships it was becoming increasingly difficult to get enough transmitting and receiving antennae to meet the needs of radio communications. We were able to get along with very few receiving antennae after we learned how to put a lot of receivers on one antenna without interfering with each other. This Laboratory undertook to see if anything of a parallel nature could be done with transmitting antennae. Here the situation was very different. It really doesn't make much difference if you have a rather inefficient receiving antenna, because you can make up the difference with a good amplifier. This cannot be the case with transmitters, because high gain amplifiers in transmitters mean a very great expenditure of power. The transmitter with its associated antenna must be an efficient combination. Nevertheless the Laboratory did develop apparatus which permitted two and sometimes three frequencies to be delivered simultaneously from three different transmitters into one antenna, provided these frequencies were separated in different bands, differing rather widely in frequency. These "diplex" units as they were called, were installed on a great many ships.

Up to this point we did not have any too clear an idea of the efficiency of our high frequency transmitters; that is, we did not know exactly how much of the electrical energy put into the transmitter finally arrived in the antenna. With the assistance of Mr. Hastings I worked out a method of accurately determining this matter at least as far as water cooled transmitting tubes were concerned. Some success was obtained in extending these results to lower powered air cooled tubes. These results were reported in a joint paper by Hastings and myself, published by the Institute of Radio Engineers.

Equipment for submarines had been much in our minds. Some of the experiments we made with them have already been recounted. We continued to push this work for all frequencies, even superfrequencies, under the leadership of Lieutenant Harry Hill who was then at the Laboratory, assisted by Mr. Ray Gordon. It is perhaps appropriate to mention at this point that Mr. L. A. Gebhard made an extended trip to Europe in 1930 and came back with a lot of valuable information as to what was going on, particularly in France.

The use of automatic and teletype transmission and reception was steadily increasing. In teletype reception the message is printed out as though coming out of a typewriter instead of appearing on the tape in dots and dashes.

During this same period the Bureau of Standards had taken over very efficiently the systematic study of ionosphere heights, using variations of the pulse method. They also had started their broadcast of extremely accurate frequencies so that laboratories far and near could accurately calibrate their precision frequency measuring equipment. Both of these services nowadays are literally indispensable.

In the meantime our aircraft people had become definitely sold on the use of high frequency and superfrequency. They were using high frequencies up to 13,000 kilocycles and were using fixed antenna on fighter aircraft. In 1930, L. A. Hyland, now executive engineer for Bendix Radio, built the first 50 megacycle superfrequency set (XT) for an airplane, for code transmission only, and successfully tested it. Mr. Matthew Schrenk, aided by Harris Hastings built in 1931 the GL set for voice communication, operating between 50 and 60 megacycles. This set was the first set to derive all necessary power from an engine driven generator, the power being supplied through a filter to cut out ignition interferences and other disturbances from electrical equipment on the plane.

It was while Hyland was testing a high frequency direction finder on board a plane that he first observed the radio echoes from airplanes in flight. The Laboratory was emitting signals on 32.8 megacycles while Hyland had his direction finding equipment installed on an airplane on the ground at Anacostia Naval Air Station, two miles away from the transmitter. The antenna emitting the signals was horizontally polarized. Hyland employed on the plane a 15 foot horizontal wire with a connection to the receiver leading in from a point a little off from the center of this wire. By swinging the tail of the plane around he was able to get a very sharp minimum on the signals from the Laboratory. While this was going on, he noticed that whenever an airplane appeared in the air anywhere in the vicinity, the minimum was disturbed, indicating that additional signals reflected from the airplane were coming down to the receiver installed in the plane on the ground. These were distinctly fluctuating signals such as we had used for radio echo work in 1922. Theoretically we knew that all objects reflected radio signals to some extent at least, but this was the first positive proof that the magnitude of the reflections of an airplane in flight was sufficient to readily betray its presence and, to some extent, its position in space. When Hyland reported these results we felt that we were now in a position to get support for future work on the detection of enemy vessels and aircraft, which had been rather neglected for the past eight years. The Navy was very keenly alert to the significance of the rapid development of air power. We felt sure that they would be interested in any device which could betray the presence, and eventually locate, aircraft as well as ships. Incidentally, Young, Hyland and myself filed a joint patent at this time (which was long held in the secret status) for the location of moving craft either on the ground, on the water or in the air.

Further experiments on these airplane echoes were vigorously prosecuted, using principally the personnel of the Aircraft Section of the Radio Division, then under C. B. Mirick. L. A. Hyland, J. J. MacGregor and J. D. Wallace were active in this work. Experiments were continued with the installation on the plane used as receiver. The plane was parked on a compass rose belonging to the Army side of the Anacostia Field. One day, in the fall of 1930, Commander E. B. Almy, then Assistant Director of the Naval Research Laboratory, accompanied by some other officers, visited this set-up to witness the experiments. Two Army officers from the Army's side of the field came over to see what was happening on their compass rose and were much interested in the observations. I am sorry I cannot recall their names or ranks, but this is probably the first knowledge obtained by any one in the Army of our work on the location of airplanes at a considerable distance beyond the range of vision. I do not think they made any official report on the matter.

The experiments were expanded to cover a range of frequencies up to about 100 megacycles, using portable equipment which was operated in various locations within 30 miles of the center of Washington. Photographic records of these pulsating signals were made so that we could correlate the position and speed of the plane with the signal observations.

By 1932 we had worked out a complete system for the protection of an area. This involved a set of directional transmitters of moderate power, operating on superfrequencies. These transmitters were to be arranged around the periphery of a circle whose center was the center of the area to be protected; transmitters were to be located about 15 miles from the center of the area of protection. Each transmitter was to send out a fan beam so that a few miles further out these beams gradually overlapped each other, which was possible since the beams were not too narrow. Fifteen miles further out in the direction of these beams, another circle of receiving huts was to be erected. The output of the receivers was to be connected by wire with a central recording station for the entire system. We were able to demonstrate the efficiency of such a system of area protection by building only a few of the component parts and subjecting them to actual tests. Thus it was proved that we could detect the presence and the approximate location of many planes within fifty miles of the center of the area to be protected.

It is not my purpose to go into the mathematics of this system here. I will only state that a knowledge of the general direction of the beam, plus a continuous record on the recorders of the signal fluctuations, permitted a fairly good estimate to be made of the position of the plane, although in no way comparing with the great accuracy of subsequent radar equipment.

The difficulty with this system was that it was not applicable to Naval ships since it required fixed location of transmitters and receivers. Of course it was applicable to protection of Naval bases, but up to that time and for many years later, the protection of the shore bases of the Navy was largely the Army's job, as at Pearl Harbor on December 7, 1941. Therefore, I wrote a letter in 1932 to the Secretary of the War, via the Secretary of the Navy, calling the attention of the Army to the fact that we had developed this area protection system (it wasn't called radar yet) and that the system seemed far more adaptable to the needs of the Army than to those of the Navy. The Army was invited to send representatives to the Laboratory to witness a demonstration of the system in the field. This letter was signed by the Director of the Laboratory and resulted in a delegation of four Army officers visiting the Laboratory and witnessing the demonstration. For purposes of convenience, the transmitter for this demonstration was located at the Laboratory and the receiving equipment near Fort Foote, Maryland. Some time after this the Army asked permission to disclose this matter to the General Electric Company because they were considering giving them a development contract. The result was a conference at the Laboratory on June 20, 1935 between our engineers, Major Jackson, Captain Harding and Lieutenant Bell of the Army and Mr. Kenney of the General Electric Company, who has been mentioned in Chapter VII.

This contract was never completed for the simple reason that more effective developments were already under way, giving a more direct, more effective answer to the problem. It is interesting to note that this early type of radar, which can be called the FM (frequency modulation) type, was revived during the late War because it had the power of definitely discriminating between moving and fixed targets. It is still a live issue. With the much higher frequencies we now have available, and the very much sharper beams, there are possibilities that did not exist in the early 1930's.

It was during this period that we formed what we called the Special Research Section, organized principally to push forward in the field of what was, in a few years, known as Radar. We called it then "The Location of Enemy Ships and Planes". Mr. L. C. Young was made head of this section and in 1933 Mr. Robert M. Page was put on this work, followed a little later by LaVerne R. Philpott and R. C. Guthrie.

In 1933 Mr. Young proposed that we abandon, for the time being, the frequency variation or beat method and attempt to do the job by the use of high power pulses such as we had used in 1925 in getting range measurements on the ionosphere in collaboration with Drs. Tuve and Breit of the Carnegie Institution. I told Mr. Young that I would be glad to see this tried but warned him that it would be a much more difficult job than getting reflections from the ionosphere. Although the ionosphere was a long distance away, it was a very large and very perfect target, giving strong echoes, whereas the location of an airplane at a similar distance, say 100 miles, would require shorter pulses of very much higher power and new types of receivers. With this understanding we went ahead with the pulse method which was the basis of modern radar, having with us our background of having built high power pulse transmitters for the ionosphere work in 1925.

During the period covered by this chapter there had been several changes in the organization and leadership of the Naval Research Laboratory. The Laboratory remained under the Office of the Assistant Secretary of the Navy until 1931. At that time it was transferred to the cognizance of the Bureau of Engineering because that Bureau had most largely supported the Laboratory by the assignment of problems and the allocation of funds. Captain D. E. Theleen Director from July 1926 to July 1930, had been succeeded by Captain (now Admiral) E. J. Marquart who, in turn, was succeeded by Captain E. G. Oberlin, former Assistant Director, in February 1931. He, in turn, in March of 1932, was succeeded by Commander (now Captain, Retired) E. B. Almy. Commander M. A. Libby who had been Assistant Director between December of 1929 and January 1930, died very suddenly and was succeeded as Assistant Director by Commander (later Captain) E. B. Almy, who served until March 1932 when be became Director. Lieutenant Commander W. J. Ruble (later Captain) served from March 1932 until May of 1933 as Assistant Director. Between 1929 and 1933, Lieutenant Commander (now Captain) A. D. Douglas and Lieutenant Commander (now Captain) B. W. Chippendale had served as executive officers. Lieutenant W. J. Holmes, who was Shop Manager between 1932 and 1933, later, after his retirement, became famous as an author of stories of the sea, particularly about submarines, which were published in the Saturday Evening Post. I remember him as a man of remarkable ability and keenness of mind with a completely absorbing interest in everything that had to do with submarines. He was called back to active duty after Pearl Harbor but I suppose he is now again on the retired list. I certainly hope that he will continue his stories in the Saturday Evening Post which he writes under the pseudonum of Alex Hudson.

Rear Admiral W. S. Parsons (then Lieutenant) and Captain E. H. Pierce (then Lieutenant) were attached to the Laboratory in 1933 as Technical Assistants. We owe much to these two. Commander Parsons was tremendously interested in the Radar program and gave us many practical suggestions and valuable ideas. He did everything he could to get money to support the program. He has been often mentioned in the press of late on account of his connection with the atomic bomb. He made the trip with the first bomb dropped on Hiroshima. Captain Pierce was one of the most effective radio and sound officers we ever had at the Laboratory. A man of unusual ability, wide understanding and excellent training and experience in radio and sound work.

Early in 1932, the Bureau of Engineering suggested that the Radio Division be separated into two divisions, one a small research group and the other an engineering and development group. The Director called me in and asked me what I thought of the idea. I told him that I didn't think much of it because it has always been my theory that to have a well balanced and enthusiastic organization one should permit at least some research work to go on in all divisions. I believed that the close contact between research workers and engineering development workers was good for both of them. The development engineers were sure to get a lot of new suggestions and ideas from the research workers who in return would certainly get better ideas of what the equipment would finally look like when it was sent to the Fleet. Thus they would be better able to direct their research work in such a way as to make it easier to take over when development came into the picture. "Certainly" I said, "There is no objection to having a special research group, but some research ought to go on everywhere".

Since the Bureau of Engineering held the purse strings at this time and they seemed rather insistent on trying out this reorganization, I told the Director that I was willing to try it out. He asked me which group I would prefer to head up. I told him I would prefer to stay with the small research group. Mr. L. A. Gebhard took over the Radio Engineering Division, although he too, was strenuously opposed to the split. I took over the Radio Research Division which consisted of only eight individuals.

The Radio Research Division had a reasonable appropriation for those days, no obligation to make frequent reports and a considerable latitude in choice of problems upon which to work. This split didn't last long. The Bureau of Engineering soon saw that the total output of the Laboratory was not as high under this arrangement and so restored the original divisional organization, under my direction, in January of 1934.

During the time the split was in effect, I think I had one of the happiest periods I have spent in this Laboratory. I had very few administrative headaches and a minimum number of technical reports to bother with so that I was able to spend most of my time on problems which I had wanted to undertake for some little time. Mr. Young and I went to work on a system of secret communication which would be difficult for the enemy to pick up and de-code. This system was based on the combination of two audible frequencies with which the transmitter was modulated, these two audible frequencies being kept in absolutely perfect phase relation to each other. This we could do by a device which is called a locked synchronism oscillator. We could, for instance, produce two frequencies one of which was exactly twice or three times the other, with fixed phase relation, or, we could produce two frequencies which bore the exact relation of two to three or three to four, four to five, etc.

These signals could be received on an ordinary radio receiver. After detection both of these audible tones could be heard, sounding like a major or minor chord as the case might be, but they could also be displayed on the screen of a cathode ray oscilloscope.

The oscilloscope deserves, at this point, a special description because it became so absolutely vital for our radar equipment during the late war, besides which it had many other uses. This instrument is a recording instrument wherein a pencil, so to speak, writes a luminous inscription on the screen of the tube. This record can be read by the eye or photographed by a camera. This pencil is not a mechanical pencil, but it is a narrow stream of electrons which are propelled through the so-called cathode ray "gun" of the tube and create a fluorescent spot wherever they hit a chemically prepared screen. Now if either magnetic or electric forces are caused to influence this beam during the flight of the electrons from the gun to the luminous screen, the beam will move, even at tremendous speeds, exactly in response to these influencing forces.

The cathode ray tube is really very old but the earlier tubes were so difficult to use that they existed only here and there in physics laboratories. Direct visibility of the resulting pictures was not obtained with the earlier tube; on the contrary, this high vacuum tube had to be opened up and a photographic plate inserted every time it was necessary to record observations. Then the tube must be again evacuated.

During the late 20's and early 30's there was a very rapid development of cathode ray tubes in all countries, particularly in the United States. The development in this country was participated in by the General Electric Company, Western Electric Company, Allen DuMont Laboratories, Radio Corporation of America and others. These tubes were soon produced in all sizes from a one inch to twelve inch screen. They soon became so important to the radio engineer that I used to say that our engineers carried them around in their pockets at all times because they were as useful as a jack-knife. Certainly modern radar could not have reached the state that exists today without the cathode ray tube.

This device then has an index finger that will follow and trace the performance of rapidly changing electric currents, magnetic fields or voltages, even if they are changing so fast that time intervals have to be measured in units smaller than one millionth of a second. The development of the cathode ray tube to a practical tube for the everyday radio man marked another epoch in the advance of radio and radar. It is as important to radar as the telephone is to communication.

With this instrument then, we arranged to display the two tones in locked synchronism which we had put on our transmitter and received at a distant point on the receiver. One tone was arranged to cause the indicating cathode ray pencil to oscillate in a vertical plane. The other tone oscillated the pencil in a horizontal plane. The combination of the tones produced a very pretty picture on the screen.

The configuration of this picture depended on the frequencies and their relative phases. Frequencies in the ratio of 2 to 1 would produce a figure eight in one phase combination and a section of a parabola in another phase combination. In between, other phases would produce figures that gradually slipped from one shape to the other. Sometimes the bottom of the parabola would be on the right hand side of the picture and sometimes on the left. With other ratios as 2 to 3 or 3 to 4, etc. the figures were much more intricate but they too changed, if the starting phases of the frequencies were caused to differ by a small amount.

No matter how these phases were shifted, the tones remained exactly the same so that an enemy listening in with a head telephone would not be aware that information was being transmitted. We were able to work out a phase shift arrangement, operated by a set of keys, that would produce different figures which, incidentally, are called Lissajous figures, on the cathode ray tube without the tone quality of the signal being changed in any way. Thus we could make up a code and transmit information. We studied this system for some time. There were several objections to it. One was that the Fleet didn't have enough transmitters which were capable of taking such a very precise form of modulation; second, we finally decided that it would not take the enemy long to analyze the signals with a cathode ray tube and ultimately break the code. In any case, we put it on the shelf for further consideration. I now consider it a dead issue so do not hesitate to mention it here.

This particular study raised a number of interesting questions concerning the physics of the human ear and its relation to music. The question naturally arose as to whether changing the phase of two tones without changing their pitch would change the sound of the chord as heard in the loud speaker or telephone. When music is played by an orchestra, many combinations of tones occur but they are not like the tones that we produced since exact phase relationship and exact locking of frequency ratios is not possible with musical instruments. Two instruments cannot even be adjusted to the same frequency much closer than to within one beat per second which means that the phases would whirl around 360° each second. It is fortunate for our enjoyment of music that the human ear is not bothered by this matter of relative initial phases in complex sounds.

Since in a small way I am a musician myself, I became interested in this problem as a side issue and made a number of attempts to determine whether this change in phase without change of tones or the sound chord, could be detected by the human ear. None of us could detect this change unless it was done so abruptly as to produce momentary transients. We took care not to do this in our method of making the phase shifts. However, I had the pleasure of testing this matter out with a highly trained musician, namely Dr. Leopold Stokowski who spent several hours in the Laboratory with us, making experiments and discussing the physics of music in which he had always been much interested. Dr. Stokowski has a remarkable sense of absolute pitch but in addition to this he was definitely able to detect these phase differences in the component frequencies in a chord, although we arranged the experiments so that he couldn't see when we made the shifts. He was the only man tested who was able to do so. Perhaps other highly trained musicians have the same ability - it raises an interesting question.

In the process of testing out the range of this secrecy equipment, Mr. Young and I equipped a radio truck and started out on a trip of several hundred miles in order to take measurements both by day and by night at various ranges. This disclosed some data which was of very great interest to students of wave propagation.

At short ranges, where the ground wave signal predominates in intensity, we were able to get steady figures and very reliable communication in the daytime, with reasonably steady figures having only a little variation at night. At somewhat longer ranges the daylight communication was still good; after dark our figures on the cathode ray oscilloscope began to execute the most intricate and remarkable gyrations. When modulation is put on a carrier wave the carrier wave splits up into three components: first, the original carrier frequency; second, a side band which is higher in frequency than the carrier wave by an amount equal to the pitch of the modulating tone; third, a lower side band which has a lower frequency than the carrier wave by an amount equal to the pitch of the modulating tone. Since we were transmitting on a frequency of 250 kilocycles and our modulating frequencies were 1 kilocycle and 1 1/2 kilocycles it will be seen that our receiver was getting the carrier wave at 250 kilocycles, and two side bands, one at 251 kilocycles and at 249 kilocycles. In addition the other two side bands came in at 251 1/2 kilocycles and 248 1/2 kilocycles. The fact that these Lissajou figures change shape so rapidly at night means that there were uneasy movements going on in the ionosphere, which at the longer ranges controlled the predominating signal. These changes in the ionosphere did not affect all members of this group of frequencies in the same way but changed both their relative amplitudes and phases. This is the clearest possible proof of the uneasy turbulence which commonly exists in the upper regions responsible for long distance radio transmission. We decided that if the modulating frequencies could be lowered so that the whole family of frequencies would be in a very narrow band, we could get rid of this effect. We therefore modulated the transmitter with 40 cycles and 60 cycles simultaneously. True, this did reduce the rapidity and amplitude of the effects but it by no means cured it. This then was another objection to this system of secrecy transmission. It would be limited in usefulness to the ground wave range.

An interesting byproduct of this investigation was the development of a radio frequency wattmeter which performed satisfactorily up to 30 megacycles and permitted the direct measurement of the radio frequency power in the different component parts of the system or the antenna. This instrument was described in a paper published in the Proceedings of the Institute of Radio Engineers

The increasing interest in aircraft radio problems, not only in the Army and the Navy, but in commercial aviation as well, led, during this period, to the development of numerous direction finding and homing systems. The efforts of the Bureau of Standards, Civil Aeronautics Authority and the commercial interests succeeded in developing a low frequency system which was originally put into effect over large sections of this country by installing a number of ground stations capable of emitting radio signals of such a nature that the pilot was able to keep himself "on the beam", that is, "on course" by so flying his plane that the signals coming in from one side of the beam blended with the signals coming from the other side of the beam into a long dash when the plane was exactly on course.

The principal advantage of this system was that although it required large and rather delicately adjusted systems on the ground, the equipment in the plane was relatively simple. The disadvantage of the system lay in the fact that it set up fixed courses designed to take the plane from one airport to another. If, due to storm conditions, the plane was forced a long ways off the beam, it was difficult, if not impossible, to get on again.

As far as the Navy was concerned this system was not satisfactory because we could not use it for carrier bourne aircraft. It would be impossible to install these large ground systems on board ship and impracticable to keep them in proper position once they were afloat. Therefore the Navy's interest was centered on a homing system using superfrequencies.

During the period discussed in this chapter many experiments were made with frequencies between 30 and 100 megacycles, using beams putting out a relatively narrow radiation pattern. It was proposed to use these superfrequency beams for blind landing as well as general guidance. This sort of system also has the advantage of putting the more complex equipment on the ground or on the ship and the simpler equipment in the plane, where heavy and complex equipment is highly undesirable. The system also has the advantage of being small enough to put on board a ship.

In the meantime much work was done on the improvement of direction finding equipment on board the plane, particularly in the new superfrequency field. While these devices were not perfected in this particular period, the basic research and preliminary development was well under way.

It was at this same time that we became aware of the great influence which the sun spot cycle has on high frequency radio work. The increase and decrease of sun spots and magnetic storms follow approximately an eleven year cycle. It so happened that since the early work in 1923, 1924 and 1925 the passing of the years had brought us more than half way through this sun spot cycle. The Laboratory continued to take an active interest in wave propagation, cooperating with the amateurs and exchanging information with the commercial laboratories, the Bureau of Standards, the Carnegie Institution and others equally interested in this fascinating study. We concentrated our efforts during this period in the region between 20 and 40 megacycles because it is within this region that the sun spot conditions have the most marked effect. At one part of the sun spot cycle, frequencies above 25 megacycles are of very little use for long range communication, while at other parts they sometimes prove valuable, even well past 30 megacycles. During the right phase of the sun spot cycle we occasionally sent signals across the continent with very low power on 40 megacycles.

These questions were of great military as well as scientific interest, because the Navy wanted to know when it w as safe to use high frequencies for a limited range communication without fear of being intercepted by a distant enemy. If we were building equipment to cover a period of more than a few years it became evident that for strictly limited range communication, communication which never was turned down to distant points on the earth by the ionosphere, we should have to push the f requencies well above 60 megacycles.

In determining the skip distances or zones of silence for frequencies between 20 and 30 megacycles we relied largely on picked amateur observers. As a result of their cooperation we were able to prove that these frequencies sometimes had zones of silence or skip distances as great as 1800 miles. This led to curious communication situations; for instance, there were periods when we could put strong signals on 28 or 30 megacycles into Denver, Colorado without having them heard anywhere between Denver and Washington except for a few miles outside of either city. These signals would bounce again from the ionosphere and come down on the West Coast, say in San Francisco, again giving excellent signals which were not observable at intervening points. These conditions were by no means regular and reliable. Neither were they particularly unusual at this time. Later, at a different phase of the sun spot cycle, it was quite impossible to duplicate them. All of these apparently erratic effects are explainable in terms of the varying ionization produced in the upper layer of the earth's atmosphere by the ultra violet light of the sun, which is greatly affected by sun spot bursts.

In looking over the record of visitors who came to the Laboratory during these years, I see the names of many scientists and radio engineers of national and even international reputation. There are also the names of many famous officers of the Navy and of the Army which have appeared in the headlines during the late war. The record clearly shows that we maintained very close contact with the Westinghouse Company, RCA, Bell Laboratories, DeForest Radio, General Electric, Allen DuMont Laboratories, Heintz and Kaufman Ltd., General Radio, Federal Telegraph Company, National Electric Supply Company, DuPont, United States Rubber Company, Isolantite Incorporated, Submarine Signal Company, International Nickel Company and many others too numerous to mention.

The policy of the Laboratory was not to attempt research and development which could be done for us by competent outside organizations. The policy was also to turn over to such organizations all except highly confidential information because in this way, if war came, there would be a large number of organizations familiar with Navy standards and Navy work. Thus they would be able to go into production for us when the emergency came. This policy paid off well when the time came when we needed huge quantities of radio and radar material.

Among the government departments we had very close contacts with the Army, Bureau of Standards, Lighthouse Service, the Department of Justice, the Coast Guard, the Department of Commerce, the Department of Agriculture and the Coast and Geodetic Survey. Within the Navy our contacts were mainly with the

Bureau of Engineering, Bureau of Aeronautics, Bureau of Ordnance, Chief of Naval Operations, Office of Naval Intelligence, Director of Naval Communication, the Washington Navy Yard, the Naval Academy, Naval Experimental Station at Annapolis, the Marine Corps, the various Naval Fleets, the Navy General Board, the Navy Hydrographic Office and the Naval Aircraft Factory. I think that our close relations with the post graduate school at Annapolis, which continue to this date, have had a very beneficial effect. Many young Naval officers in this way come to know about the Laboratory and its work and know where to turn for help later on when they were faced with difficult problems in the field of radio.

One visitor, an amateur who had assisted us greatly in the determination of skip distances on high frequencies, was Arthur Collins of Amateur Station 9CXX, Cedar Rapids, Iowa, who called on us in October of 1930. Collins was then a very young man but I was very much impressed with his keenness and insight. I like to believe that the encouragement which we gave him to make radio his life work had something to do with the formation of the Collins Radio Company which has contributed no small amount of radio equipment for both Army and Navy during the late war.

CHAPTER XIII

1933-1937 - The March to Still Higher Frequencies - Radio and Radar.

Radio started in 1887 with frequencies between 30 and 300 megacycles. It may seem surprising that more spectacular progress in the exploitation of higher and higher frequencies did not occur, once the tremendously useful properties of superfrequencies were discovered.

The reason for this lack of extremely rapid progress lies in the fact that practically every component of radio transmitters and receivers had to be radically improved or changed in order to function at much higher frequencies than those for which these components had been originally designed. I have already explained how this forced the development of new insulators.

Quite early in the development of radio communications it was known that high frequency currents tend to flow through the outer skin rather than through the whole body of a conductor. This means that the surface of the conductor plays the greatest role in carrying the current. This knowledge led to the creation of a conductor made up of a very large number of fine wires or strands, this being first accomplished by the Germans who put it on the market under the name of "Litzendraht". Even with this special conductor, which we used in the early days for winding coils, it was never possible to reduce the losses to the same point as the losses of the same conductor for currents of commercial electric lighting frequencies. Nevertheless the use of stranded conductor, the individual strands being well insulated from each other, was a great help.

Shortly after frequencies began to get higher than 2000 kilocycles it was discovered that the use of this finely stranded conducting cable not only was no longer an advantage but was actually a disadvantage. Radio engineers were then forced to go back to the use of non-stranded or solid conductors, but since only the outer shell of the conductor was of any use, we soon found ourselves making coils and circuits out of thin copper tubing. In other cases we used thin metal tubing electroplated with silver, since that metal is a still better conductor than copper. In other words, the radio engineers had to patiently learn a lot of new tricks. Considering the magnitude of the difficulties involved, the march toward higher and higher frequencies really went on at an astounding pace.

Perhaps the most critical component of the radio circuit, the one which caused us the most worry, was the vacuum tube upon which we depended for the production of very high frequency oscillations and for the detection and amplification of the corresponding high frequency waves in our radio receivers. Since in this country the Navy was the first to make very extensive use of high frequencies as well as superfrequencies (above 30 megacycles) we had to take the lead in bringing pressure to bear upon the radio tube laboratories of the country in order to persuade them to turn out vacuum tubes suitable for higher and higher frequencies. The Bell Telephone Laboratories, the General Electric Company, the Westinghouse Company, RCA, Eimac and others contributed in a highly important way to the program.

While all this work looking to future applications was going on, the Navy as a whole had consolidated its position in the high frequency field under 30 megacycles by providing the Fleet and the shore stations with a splendid line of transmitters and receivers. In the production of this equipment we had the very able cooperation and support of many organizations, notably, the Westinghouse Electric and Manufacturing Company, RCA, General Electric and the Bell Telephone Laboratories.

The Navy had decided, after experiments carried out on board the TEXAS by our engineers in cooperation with the ship's radio officers, that since we were mainly interested in telegraphic communication by the continuous wave method, it was unnecessary to consider standard equipment for even large ships which would put more than one kilowatt of radio frequency power into the antenna. This wise decision allowed us to avoid the use of a large and cumbersome power plant, water cooled tubes, and various accessories that go with higher powers.

In 1934, there occurred a very interesting instance of the use of very moderate high frequency power for very important purposes. In that year Mussolini apparently decided that, since the Japanese had been extremely successful in grabbing what they wanted in China, he might as well start something in Ethiopia. Ethiopia had protested to the League of Nations as early as May 13, 1934 about the pressure Italy was putting upon it. Our State Department was very anxious to have frequent and uncensored reports on the Ethiopian situation from Addis Abbaba. The State Department requested the Navy Department to provide radio communication from Addis Abbaba to Washington.

The Bureau of Engineering took a 100 watt transmitter, [illegible] capable of operating between 4000 and 20,000 kilocycles, procured a gasoline driven motor generator to supply the transmitter power, added the necessary receivers and shipped the equipment hurriedly, before the Italians got there, into Ethiopia. We also had a naval ship so stationed in the Mediterranean area that, if necessary, it could be used to relay the signals to Washington.

The circuit from Washington to the capitol city of Ethiopia is about 6000 miles in length. The direction from Washington is slightly north of east. This may seem strange to those who are more familiar with maps than with globes. but it is a fact that, although the Ethiopian capitol lies near the equator and Washington far north of it, the true bearing from Washington is a little north of east. In fact, the city of Johannesburg, South Africa, is actually, in bearing, almost exactly east of Washington. These things cannot be understood by examination of a flat map but only by recourse to the globe.

This circuit involved a time difference of over six hours so that during a large part of the twenty four hours the circuit was partly in sunlight and partly in darkness. This meant that during these hours frequencies suitable for night operation would fail to pass the daylight sector, because of high attenuation, while frequencies suitable for daylight operation would fail to pass the night sector because of not being turned down by the ionosphere. Direct communication with Washington must, therefore, be limited to a few hours, namely, during the summer months at least, between 11:00 A.M. and half past three in the afternoon using a very high frequency, since the entire path would then be in daylight. During a similar period in the night hours from 11:00 P.M. to somewhere between three or four o'clock in the morning the whole circuit was in darkness, so that suitable night frequencies could be used.

The Naval Research Laboratory assisted the Navy Department not only in the preliminary tests which resulted in the proper choice of a frequency of over 19,000 kilocycles for the daytime circuit and a frequency not far from 9000 kilocycles for the night circuit, but we actually assisted with the reception of the signals, particularly in the daytime. We arranged to put up a simple but fairly directive antenna system at the Laboratory to aid in the reception of these very weak daylight signals.

The story of the struggles which this small Naval unit had in getting this equipment into Ethiopia and set up, is a tale of superhuman efforts in the face of almost insurmountable obstacles. The antenna had to be camouflaged, to some extent at least, and the power plant had to be muffled as much as possible so as not to betray the presence of the equipment. This set continued to operate for some time after the Italians arrived, as they did not immediately discover its presence. Thus the true story of what was going on in Ethiopia was delivered every day to the State Department when no other method of communication would have been possible without long delays and censorship by some foreign country. As I remember the situation, there was little need to relay the signals through the ship in the Mediterranean.

Since this transmitter in Ethiopia had only 100 watts of power, this constituted a rather remarkable achievement. Of course, the operators in Ethiopia didn't have so much difficulty with reception, since we had much more powerful sets with better antennae in Washington, so we could deliver them a much stronger signal. When one stops to consider that successful and important communications could be handled with no more radiated power than it would take to operate an electric heating pad or a 100 watt lamp, it seems, even in these days, a rather miraculous performance.

During this period the Navy became increasingly conscious of the growing importance of Naval air power. Several influential officers interested in Naval air power suggested to Captain H. R. Greenlee, at that time Director of the Naval Research Laboratory, that the Laboratory should recognize the situation by creating a separate aircraft radio division. Although I did not believe this as efficient as the older method of closely integrating airborne radio developments with those for ship and shore, I readily agreed to try out the idea. Accordingly Mr. L. A. Gebhard, who had had a great deal of experience in airborne radio work, was appointed to head the Airborne Radio Division in January of 1934. After a little over a year's trial, it was apparent that there was nothing to be gained by this separation and much to lose; therefore Mr. Gebhard recommended that the arrangement be dropped.

In March of 1934, the Aircraft radio activities again became a section of the Radio Division, under the leadership of Mr. Matthew Schrenk. Mr. Mirick, former leader, had remained with the Research Group, in the earlier 1932 split of radio into Engineering and Research, and after that engaged in specialized work on aircraft electrical power plants, working very closely with the Radio and Electrical Division of the Bureau of Aeronautics. The new leader, Mr. Schrenk, had had broad experience in various sections of the Laboratory and for some years had been very active in aircraft work. However, it should be noted that a great deal of the work, contributing directly or indirectly to progress in aircraft radio, was done by sections of the Radio Division other than the Airborne Radio Section. Notable among these sections for their contributions were the Transmitter Section under Mr. R. B. Meyer; the Receiver Section under Mr. T. McL. Davis, and the Measurements and Direction Finder Section under Mr. Warren B. Burgess. In the Special Research Section which had been formed in 1933, the first work on airborne radar was started with the construction of a pulse or radar type of altimeter operating on a frequency of about 500 megacycles. Mr. LaVerne R. Philpott was largely responsible for this device.

As Naval aviation grew, there were an increasing number of practice evolutions with groups of ships at sea, including the operation of carrier borne aircraft. Some of these exercises took the aircraft to distances of several hundred miles away from their carriers and from the main body of the Fleet. Of course these planes were generally equipped with radio communication. It was at times possible to get bearings on them from the ships and to tell the pilots what courses to steer in order to get home. Even in good weather planes frequently became lost, because flying over the ocean is a very different thing from flying over land, where there is always a chance of picking up some familiar land mark. On the open sea it is very easy to fail to find a target, even one as large as that made by a group of ships. Since the planes are naturally provided with magnetic compasses, this may seem a little strange.

The main difficulty is, of course, the wind drift. A pilot flying a given compass course may, when the action of the wind is taken into consideration, be actually flying a course considerably different from the one he intends to fly. In other words, the aircraft does not travel over the surface of the sea in the direction in which it is pointed because it is so easily affected by the movement of the medium, namely the air, in which the plane travels.

It is true that an experienced pilot, provided the weather is sufficiently clear, can observe the waves beneath him and make a rough estimate of the "drift". Unfortunately, however, changeable weather conditions may arise where the waves do not by any means truly indicate the direction or magnitude of the wind. Perhaps this explanation shows why we became extremely interested in homing devices which would give the aircraft an absolutely sure course back to their respective carriers.

The development of the first of these devices, known as the YE equipment, started in 1934 with a rotating beam transmitter at a frequency of about 120 megacycles. This transmitted, as it rotated, an indicating signal showing in what direction it was pointed. In other words, this device acted exactly as would a rotating lighthouse beam if it changed color or some other

Characteristic according to the sector toward which it pointed. The radio beam had the advantage of having a range of 100 to 150 miles, provided the aircraft was flying at a reasonable height so that the curvature of the earth would not cut off the beam. This transmitter was mounted on the roof of one of the Naval Research Laboratory Buildings. Many tests were made with planes from the Anacostia Air Station. The development of this transmitter was entirely in the hands of the Transmitter Section under Mr. R. B. Meyer, ably assisted by Mr. Oscar Dresser. The development of the receiver, which was to go on the aircraft and permit the reception of these signals, was turned over to the Aircraft Section under Mr. Matthew H. Schrenk, assisted by Mr. H. R. Miller.

In order to avoid a large amount of additional aircraft equipment, a very small and compact adapter, as it is called, was produced by the Aircraft Division. This could be connected in ahead of the usual aircraft receiver by the simple throwing of a switch, permitting simple and easy reception of the beacon signals.

The breadth of the beam of radiation sent out by the YE was considerable, so that as the beam rotated the pilot would usually hear at least three signals for three courses 10° to 15° apart. This was an advantage rather than a disadvantage, because it was very easy to pick the strongest signal and determine thereby exactly what course the pilot should fly in order to arrive at his carrier.

The YE transmitters on these carriers could be so modulated that it would be possible for a lost pilot to distinguish his own carrier or, in an emergency, to pick out another carrier and ask permission to land on it.

On many occasions the flights around Washington demonstrated the ability of this equipment to bring a plane home in a dense fog, but in spite of these successful demonstrations the Bureau of Engineering decided that the rotating beam antenna structure was too large to easily install on shipboard. We were requested to redesign the equipment for a frequency of 246 megacycles which would permit the reduction in the size of the antenna by a factor of nearly 4 to 1. It was this decision which so greatly delayed the development of these devices for, at that time, there were no vacuum tubes in existence which, at the frequency in question, would give even the moderate amount of power required for this job.

We managed to redesign the equipment for about 200 megacycles but stuck there for some time until new tubes, whose development had been hastened by the needs of radar, came into the picture. Fortunately for the Navy this development was finally completed and introduced into the Fleet in 1940 and 1941, in time to save many lives and many planes in the battles of the Pacific. In the long run, the decision of the Bureau of Engineering to double the frequency from 125 to 246 was justified, but that decision cost laboratory engineers many anxious hours. The work on the YE equipment shows how effective the cooperation was between different sections of the division in the development of equipment of the utmost importance to Naval aviation.

The development of suitable transmitting and receiving tubes for aircraft was a long, patient struggle. Radio equipment in aircraft has to withstand a good deal of shock during take off and landing, tremendous variations in temperature due to varying altitude, a great variation in humidity and above all, constant vibration. Many vacuum tubes which do well enough on the surface ship are utterly unfit for use on board aircraft. The persistent demands of the aircraft people for "non-microphonic" vacuum tubes, that is, tubes which would continue to function in spite of vibration and shock, finally resulted in the development of better tubes for both of the armed services and all branches thereof as well as for the radio industry as a whole.

The vacuum tube section of the Radio Division, under the leadership of Joseph T. Fetsch, aided greatly in the solution of these difficult problems. The manufacturers in the country cooperated whole heartedly and were continually producing new designs in line with our suggestions, sending them to us for a check up. New methods of cushioning shock by special supports for radio gear also helped greatly in arriving at a practical solution of our difficulties.

As the frequencies rapidly became higher and higher it became increasingly evident that the problem of getting high frequency energy from the transmitter into the antenna was a very serious one, not to be solved by the expedients used prior to 1930. It was standard practice in Naval ships to put most radio equipment below decks, behind armor. The power from the transmitters was passed up to the antenna through heavy copper conductors. These conductors passed through what was called a "trunk", which was an opening, 8" or 12" in diameter, running straight up through the different decks of the ship to the top side, where it terminated in an insulated support to which the wire leading to the antenna could be attached.

When attempts were made to send very high frequencies to top side antennae, it was found that most of the energy was dissipated and very little ever arrived above decks. These large trunks, moreover, were a great hazard to a fighting ship in case the enemy should decide to use gas.

The radio industry, because of its interest in high frequency for communications and superfrequency for television, took an active interest in the prompt solution of this problem and played a leading role in providing suitable high frequency transmission lines. For some time we had been using what we call open wire lines, consisting of two parallel wires a few inches apart, supported by insulators and running from the transmitter to the antenna. The losses in such lines were not prohibitive until frequencies got above 100 megacycles.

In the meantime the Bell System developed and made wide use of what is called tubular transmission line. This was made of semi-flexible copper tubing through the center of which was strung a solid copper wire with large numbers of insulating beads on it to prevent the wire from coming in contact with the walls of the copper tubing. These beads could be made of glass or ceramic materials of low loss at high frequencies.

These copper tubing lines were a great advantage and, for a time at least, although not for very long, we could forget about the difficulties of getting power from a high frequency transmitter to an antenna say 200 feet or more distant from it. These copper lines had one disadvantage; they didn't work after they became well saturated with moisture. Thus they had to be filled with a dry inert gas like nitrogen and kept under a slight pressure. We will see later how these lines had to be replaced because of the difficulty of keeping them gas tight under battle conditions. Nevertheless, they solved our transmission line difficulties for the time being and permitted progress to go on. Other lines of a more flexible nature were soon produced by various organizations. Although some of them are not of as low a loss as the copper line, they are very useful in certain spaces where flexibility of lines is an important factor.

Early in February of 1934 a demonstration of radio echoes from an airplane was given to a group of congressmen from the Subcommittee on Naval Appropriations of the House of Representatives. On this Committee was a man who had a very great influence in the development of Navy radar - a man to whom the Navy owes a great debt of gratitude. This was the Honorable James Scrugham, later Senator Scrugham, from Nevada, who died recently. Mr. Scrugham had been educated as an engineer. He certainly had an engineering type of mind. He took a very keen interest in everything we were doing in the field of what we called microwaves, which then meant waves of any frequency higher than 100 megacycles. The demonstration we gave in 1934 was of the old radar system which used the beat method. It operated on 60 megacycles and successfully demonstrated radio echoes from airplanes at a very considerable distance. Mr. R.A.Gordon of the Aircraft Division was responsible for the equipment with which this demonstration was made.

The work on pulse radar which had been going on for a year in the Special Research Section had been strengthened by the addition of several very competent engineers, notably, Mr. R.C.Guthrie and Mr. Arthur A.Varela. From the outset, Dr. R.M.Page gave indications of possessing extraordinary ability and fertility of invention. These qualities resulted in his contributing more new ideas to the field of radar than any other one man. Still under Mr. Young's direction as head of the section, he was given very complete authority to go ahead with pulse radar as rapidly as possible.

This radar work had been supported by very pitiful funds diverted perhaps illegally, I will admit, from other projects. The time had arrived when we definitely needed more tangible support. With the consent and approval of Captain Greenlee, then Director of the Naval Research Laboratory, and of Admiral S. M. Robinson, then Chief of the Bureau of Engineering, I went to see Mr. Scrugham early in 1935, accompanied by Dr. Hayes of the Sound Division. We knew that Mr. Scrugham was even then the most influential member of the Naval Appropriations Subcommittee, which he headed the next year and for a number of years thereafter.

We put up a strong plea for a substantial addition to the small direct appropriation which the Naval Research Laboratory usually received from Congress, this increment to be earmarked for long time investigations, particularly in the field of microwaves and supersonics. Mr. Scrugham listened in silence, asked a few questions, but promised us nothing. We left his office feeling very much discouraged, but on the following Monday morning, he telephoned to state that the Committee had agreed to give us an extra $100,000.00 to be spent on this work. This looks like a small amount in these days but it looked like ten million dollars to us then.

It was some little time before this appropriation became available. In the meantime the Bureau of Aeronautics had given the Sound Division $15,000.00 to develop a sonic altimeter. The Sound Division believed that the new radio echo work was more promising for the solution of the problem. With the consent of the Bureau of Aeronautics they turned $10,000.00 of this money over to the microwave groups in the Radio Division. The next year the Bureau of Aeronautics added about as much more to this fund, which kept pulse radar alive in a difficult period before adequate funds became available. Perhaps it is pertinent to add at this point that the House Appropriations Committee doubled and trippled our funds in the next two succeeding years.

Our first laboratory models of pulse radar used a circular sweep circuit instead of the A scope as described in the introductory chapter. It is interesting to note that the German searchlight and fire control radar used the same type of sweep throughout the war. In this type of display the scope has a bright line in the form of a circle. The pips produced by outgoing and reflected pulses appear as protuberances or "corona ray" effects on the outer periphery of this circle. Things are generally so arranged that the outgoing pulse appears at the top of the display and the range is measured around the circle instead of along a straight line as in the case of the A scope.

When the time came to make a practical pulse radar on a scale that would permit a realistic test, Mr. Page argued that we ought to wait until we could do the job with a very much higher frequency. We had been doing this first work at frequencies not much higher than 30 megacycles. Mr. Young argued that since we knew from our earlier radar work of the beat type that ranges of 50 miles on airplanes were possible at 30 megacycles, we ought to start our pulse work at that frequency, which we knew would give adequate echoes, and then move gradually to higher frequencies. Furthermore, we already had a very large directive antenna supported by 200 foot towers, which would direct a 30 megacycle beam in a fairly narrow pencil. I backed Mr. Young up in this argument because I felt that the sooner we got an actual demonstration of this character to show to influential people in the Naval Service, the sooner would we get enthusiastic financial backing. I still believe the matter was decided correctly, because we were definitely able to get such satisfactory echoes with the 30 megacycle beam that we at once raised the frequency to 80 megacycles, also with a fixed beam but much smaller than the one on 30 megacycles. I think this step by step method in the long run gave the best results.

In these earlier experiments with fixed beams we had to have the pilots fly the target planes strictly in the course covered by the beam, which could not be rotated. We usually had radio communication with the pilot so that we could advise him when he was off the beam. One of the pilots got lost in the thick weather one day, twenty five miles south of the Laboratory. We told him to fly five minutes in an easterly course. He did so but we saw no pip on the scope indicating that he had crossed the beam. We then told him to fly ten minutes in a westerly direction. Soon the pip appeared, whereupon we told him he was on the beam. He came home safely. Probably this was the first instance of a plane being brought home by radar in thick weather. This was in the summer of 1936.

The contacts between the Naval Research Laboratory and the War Department during this period were fairly frequent and, I believe, very profitable to both Army and Navy. Mr. W. D. Hirshburger, who had formerly been employed in the Sound Division of the Naval Research Laboratory, was in the employ of the Signal Corps Laboratories at Fort Monmouth in 1936. Later he went to RCA. Mr. Hirshburger spent two days with us early in 1936, discussing various phases of microwave problems. Microwaves in 1936 meant anything shorter than a meter in wavelength, that is, higher than 300 megacycles in frequency. As early as 1934 and 1935 the Naval Research Laboratory had a group, involving Dr. L. R. Philpott, J. P. Hagen, W. J. Cahill, Dr. C. E. Cleeton, and W. C. Curtis working on wavelengths as low as 7 centimeters, which means on frequencies higher than 4000 megacycles. Both the Army and the Navy were interested in these new activities from the standpoint of communications as well as radio location (radar).

Early in the summer of 1936, as already pointed out, the Naval Research Laboratory had an 80 megacycle pulse radar set which was capable of giving quite spectacular demonstrations. The performance of this equipment was witnessed on the 4th of June 1936 by a delegation brought down by Lieutenant Commander (now Commodore) J. B. Dow of the Bureau of Engineering, Navy Department. This group included Lieutenant Colonel R. B. Colton of the War Department, who was recently retired as a Major General; Lieutenant Colonel W. R. Blair of the Signal Corps Laboratories, Fort Monmouth, retired not long ago as a Colonel;

Captain J. D. O'Connell of the Signal Corps Laboratories, now believed to be a Colonel, and Mr. R. I. Cole and Dr. P.E.Watson of the Signal Corps Laboratories. Dr. Watson died not long ago. The present Watson Laboratories of the Signal Corps are named after him, a well deserved compliment for a very competent radio engineer.

In addition to the demonstration of the 80 megacycle radar set we showed the Army delegation the start we had made on 200 megacycle equipment and pointed out that we were having great difficulty in getting the required power with available transmitter tubes. It was necessary for the Navy to go at least this high in frequency, in our opinion in order to get the size of the rotating beam down to reasonable dimensions, suitable for shipboard installation. We had learned this lesson from the YE homing equipment previously described. Since the Army didn't put so strict a limit on the size of the rotating beam we advised that they push their work in the 100 megacycle band where they could get much higher peak pulse power from available tubes than could be had at that time at higher frequencies. This the Army accomplished in their first long range search radar which, although large and clumsy, nevertheless gave excellent performance over very long ranges.

On the 12th of October, 1936, Commodore Dow brought down Colonel Mauborgne who was then Assistant Chief of the Signal Corps, becoming Chief between 1937 and 1941 with a rank of Major General, after which he was retired. Maughborgne was an old friend, having been our first Army Liaison Officer.

In the latter part of 1936 when Vice Admiral A. J. Hepburn, now retired, was Commander-Chief of the United States Fleet, he advised Admiral Bowen, then Chief of the Bureau of Engineering, to arrange for an early demonstration and practical test of radar with the Fleet. The Laboratory was not yet ready to send a search radar to the Fleet, but I felt that we should make some tests on board a ship with what gear we had even if it was only what we called soap-box equipment. We obtained an opportunity early in April of 1937 to put such equipment on the USS LEARY, a destroyer which had docked at the Washington Navy Yard, a very convenient place to make the installation. We had very little advance warning of the availability of this ship but all hands went to work practically night and day to put in readiness three items; first, a 200 megacycle pulse radar with a small antenna; second, a 1200 megacycle equipment modulated at 30 kilocycles and operating not on the pulse principle but on the phase shift principle; third, a communication equipment also operating on 1200 megacycles. The 1200 megacycle equipment had a reasonably sharp beam but the antenna for the 200 megacycle set could not be very large since it had to be strapped on the back of a five inch gun and was aimed at the target by training the gun. Naturally this small antenna did not give a sharp beam. Nevertheless radar ranges on airborne and surface targets at moderate distances were obtained with both equipments.

This trip on the destroyer LEARY was of great interest to me not only on account of the radar tests but because, although I am pretty well qualified as a pilot for the Potomac waters, I had never before had a chance to run the river during a flood.

The stage of water in the Potomac River is subject to some very unusual and rather interesting influences, the commonest of these being the prevailing direction of the wind during a prolonged blow, say of three days' duration or more. If the wind is from the northwest the tidal reaches of the Potomac, which means from Washington to Point Lookout, experience abnormally low water. On the other hand if the wind is from the northeast, or east, the pressure of the water coming in through the Virginia Capes into Chesapeake Bay causes unusually high water. A second influence on the stage of the water in the lower Potomac at Washington, is due to conditions on the upper river including all the branches such as the Shenandoah River. If there is very heavy rainfall in this mountainous area the water comes down very rapidly and the river is capable of very high and very rapid rise in the upper section above the Georgetown district in Washington. If, as happened on the day the LEARY was scheduled to sail, these two conditions happen at the same time, a very high stage of water results in both the upper Potomac and the tidal section.

The final influence is the stage of the tide itself. This means that if the peak flood conditions occur at Washington coincident with the time of high tide the very worst conditions for a flood are obtained. That was almost exactly what happened on this morning in April when the LEARY was scheduled to sail. My wife drove me to the Navy Yard and had to bring the car on to the dock through nearly a foot of water in order that I could jump to the gangway without getting wet.

Of course an immense amount of driftwood and debris had come down from the upper Potomac where pretty heavy damage had occurred. The Captain of the LEARY was not too anxious to make the trip but finally agreed to try it. We had to proceed very carefully for the first forty or fifty miles, but after we rounded Maryland Point we found that we had outrun the mass of debris which was tearing down the river and were then able to resume normal speed. It seemed strange to run down on this extremely high water which gave the old river an unnatural aspect. We didn't get too much help from the normal aids to navigation because many of the spar buoys had been torn from their moorings and others were completely under water. The main difficulty was of course avoiding the driftwood as there was little danger of running aground anywhere.

That night in Lynn Haven Roads the ship's force cleared out the water intakes on the LEARY, finding in them a surprising amount of strange material, but no damage had been done. By the time we came back up the river a few days later flood conditions had largely subsided and the river was fairly clear of flotsam and jetsam.

When lying in Lynn Haven Roads we were able to pick up planes flying back and forth from the Naval Air Station at Hampton Roads. On these planes the 200 megacycle equipment gave 15 to 18 miles range. We didn't have any very rough water during this trip but did have enough to get our first lesson in what later came to be called "sea-returns", which means radar echo signals from waves. Such water waves were annoying only in the first two or three miles of the radar range. Modern radar has advanced to a point where much of this difficulty can be alleviated, particularly by the use of very high frequencies and very sharp beams, but I think it is fair to say that the wave echo difficulty is still with us and can always be reckoned on to some extent when we are trying to identify surface targets which are within two or three miles of the radar.

In spite of the very calm weather we had on the trip down the Potomac I recall that Dr. Philpott was extremely seasick, this in spite of the fact that the ship was as steady as a rock. Philpott is one of these unfortunate people who can get thoroughly seasick even when standing on the end of a dock and merely looking at the water. I could not help but admire the grit and determination with which Philpott stood up to his work and took part in all the tests in spite of being so ill that he could hardly stand on his feet. Fortunately no other members of the party were similarly affected. In spite of the meager results obtained with these tests I felt very much encouraged since they showed that radar could work under sea-going conditions. I knew of many improvements that could be made in the equipments to tremendously increase their range and reliability. These then were the first radars to go afloat in the United States Navy. One operated on 200 megacycles and one on 1200 megacycles.

Not long after the tests on the LEARY, namely, on the 17th of February, 1937, we received a visit from Mr. Cole and Dr. Watson of the Signal Corps Laboratories, accompanied by Lieutenant (now Colonel) Corput. These gentlemen witnessed the demonstration of the 80 megacycle equipment and discussed our program with the 200 megacycle equipment. In turn they outlined the work being done by the Signal Corps. This was my first meeting with Colonel Corput. I was very much impressed with his forcefulness, knowledge and engineering ability.

Shortly after this visit Mr. Young, then one of the Associate Superintendents of the Radio Division, with Dr. Hulburt, Chief of the Division of Physical Optics, visited Fort Monmouth Signal Corps Laboratory. There they saw the Army's 105 megacycle high power set in operation and were given a beautiful demonstration, including semi-automatic training of guns and search lights synchronized with the training of the radar. The Army's work in tying up the whole system, namely, searchlights, guns and radar, was much ahead of anything we had done at that time. It was pioneer work, paving the way for automatic radar gun pointing and fire control. Of course the Army's system at that time was far from being fully automatic, since it required the services of an operator to dial the radar and radio information over to searchlights and guns. Nevertheless, it was a very encouraging demonstration.

People in both the Army and Navy in those days were more interested in radar as a means of defense against aircraft attack than in any of its other possibilities. The Army Air Corps people were equally interested because if our potential enemies employed radar, they might have to modify their own tactical procedures.

On the 23rd of June, 1937 we received a visit from Colonel C.B.Culver of Wright Field, accompanied by an aide, who appears to have been an officer named Carver, and by another officer whose rank I do not know, the latter being from the War Department. I had known Culver well since Anacostia days so we had a very enjoyable visit. I believe he is now retired.

We were now getting close to the point where we could think of radar production. Production has never been our direct responsibility at the Naval Research Laboratory, but is rather the responsibility of the materiel bureaus. However we had to stand by for advice and technical assistance and were as eager to get radar into production as were our friends in the bureaus. Accordingly we recommended to these bureaus that they call in experts from one of the big corporations after duly cautioning them about the secrecy of the projects. In accordance with this, on the 13th of July 1937, we were visited by Dr. E.L. Nelson, Dr. J.W. Smith and Mr. A. Merquelin of the Bell Telephone Laboratories. Nelson was mentioned in Chapter VIII as having been with me for a short time at the Naval Aircraft Laboratory, temporarily established at the Bureau of Standards in 1919. When we called these gentlemen into conference we told them what we had. They were frankly skeptical. I told them that I didn't expect them to believe that we could locate planes many miles away but that I believed I could convince them with an actual demonstration. So we went out to the building called the Field House, where we had installed the 80 megacycle equipment, and put on a very convincing demonstration. After that we returned to the main Laboratory to the roof of Building 1 and gave them a demonstration on 200 megacycles. This was not quite as effective as the one given on 80 megacycles, because this particular equipment hadn't been worked up to the necessary high power pulses on account of our inability to procure suitable vacuum tubes.

We asked the Bell Laboratory people whether they would consider a development contract to produce a radar along these lines and put it into production. They replied that since we were apparently about five years ahead of them in techniques, they preferred not to take a contract at that time but would agree to go to work on systems studies, paying particular attention to the improvement of tubes and component parts with the needs of radar circuits especially in mind. It wasn't very long before they felt themselves in a position to take on their first contract for Navy fire control equipment, that is, radars specifically designed for very accurate pointing of guns on unseen targets. The first radar equipments designed solely for gun firing were produced by the Bell Telephone Group.

In December of this year, 1937, we called in Dr. J. A. McCullough and Mr. Adolph Schwartz, representing the Eimac Corporation. We had been favorably impressed with the experiments which we had made with the high vacuum Eimac tubes. They showed great promise of meeting the peculiar conditions necessary for operation of radar transmitters. The transmitting tubes for a radar are required to take a momentary but tremendous overload in order to get the very high peak power necessary for the successful operation of the radar. In other words, the tubes have to be worked with momentary voltage many thousands of volts higher than the normal voltage which would be used on such tubes for communication purposes.

This contact with Eimac developed into a close cooperation between their tube laboratories and our radar people. It is due to their enthusiastic support that we were able to have a number of radars in our Fleet some considerable time before we went to war with Japan. The cooperation of this company was not only prompt and effective, frequently involving shipment of tubes from the West Coast to Washington by air, but was carried out without any costly development contracts. We merely paid relatively low prices for such tubes as they were able to produce in accordance with our specifications.

It was on the 17th of June 1936 that I first met Mr. Lawrence Marshall, president of the Raytheon Corporation, and succeeded in interesting him in undertaking certain Navy contracts. This had far reaching consequences a few years later in the field of radar.

Besides the work specifically directed towards the development of radar, many other innere sting studies in the field of very high frequency were carried out in the Special Research Section of the Radio Division. Altogether, during the period under discussion, some 20 projects were under way. Only a few of these were actually carried to a final conclusion due to lack of financial support and insufficient personnel. Later on a number of these projects were revived when funds and personnel became available. I will mention only a few of the projects which fall in the second category.

As early as 1934 Mr. W. C. Curtis did important work with magnetrons operating at about 750 megacycles. Curtis, it may be noted, is now employed at the David Taylor Model Basin at Carderock, Maryland. The records show that in the spring of 1936 10 centimeter (3000 megacycle) radar work was being done using as targets passing ships in the Potomac River. The men on this early 10 centimeter work were Mr. Hagen, Dr. Cleeton and Dr. Philpott with a few assistants.

In June of 1936 the first work was done with pulsed magnetrons with the idea of using the equipment for developing a suitable radar type of altimeter. However, the erratic behavior of the magnetrons then available led us to substitute certain new triodes which appeared to be more reliable for this type of work. The pulse radar type of altimeter produced a few years later was the outcome of this work.

The problem of an accurate and reliable recognition system is one that has been of great concern to the armed forces for many years. In the middle 1930's no one in the Army or Navy had a suitable device which would permit certain identification of possible targets either on the ground, the sea, or in the air. The necessity of such positive identification should be obvious. Long range guns are capable of firing on targets so far away that positive identification, to determine whether they are friendly or enemy targets, is often entirely impossible by optical methods. Because of the relatively long ranges desirable for recognition equipment it had long seemed obvious to me that identification should be carried out by the use of some form of radio. In order to prevent the enemy using false signals to confuse the identification it was obvious that the equipment had to be capable of being coded in such a way that even if the enemy succeeded in duplicating the equipment, he would not necessarily have the code for the day and therefore would not be able to correctly duplicate the identifying signals.

A few years later the British introduced the term "IFF" meaning Identification, Friend or Foe, to be used in referring to radio (or dadar) identification equipment, Therefore I shall use the term IFF in that sense from now on.

Our first identification equipment developed in the middle 1930's was known as the XAE. It was intended to permit a ship to determine the identity of approaching planes. This equipment, operating in the 500 megacycle band, involved an installation on the plane which permitted the transmission of a 500 megacycle wave modulated at 30 kilocycles. This 30 kilocycles in turn was modulated into dots and dashes by a small rotating device built into the transmitter. This device was provided with a number of discs each giving a different identifying code. These discs could be changed every hour or so, if advisable, in accordance with a prearranged schedule of identifying codes. The pilot, on approaching friendly ships, could turn on this equipment and leave it on as long as he was in the neighborhood of friendly ships or of unknown ships which might be friends.

The ship carried only a receiver, properly designed to interpret these signals. This was connected to a mildly directive antenna array. The whole equipment on the ship was portable and intended for operation from the bridge. When approaching planes were seen the operator on the bridge swung his directive Yagi type antenna (named after a Japanese scientist who first used this simple directive array) and listened for signals. If no signals were received it could be assumed that the plane was an enemy plane.

In the tests of this equipment from the roof of one of our buildings against planes provided by the Naval Air Station, Anacostia, reasonably satisfactory results were obtained, so it was decided to send the equipment to the Fleet for more practical tests. Unfortunately, money for civilian travel was so scarce that we were not allowed to send our engineers out with the equipment and the results obtained in the Fleet were not satisfactory. The equipment, operating in a frequency band where none of our fleet people had had any experience, was naturally difficult for them to handle. Also, it must be admitted that the equipment was far from being a finished product. Tests made in the

Fleet somewhat later gave us more promising results.

Like other Government institutions, the Navy Department had to keep its money in different pockets and, especially in times of peace, was not permitted to transfer money from one pocket to another. This frequently resulted in false economy and delays in production of new equipment. The Navy, ever since I have known it, has been very tight with funds for civilian travel, telephone and telegraph communications, and for automobile and truck transportation. During a war these difficulties did not exist, at least not during World War II. In the long run the Navy saves no money by this false economy, but loses money and has to accept unfortunate delays. The Navy Department is not wholly responsible for this policy; it is primarily due to the fact that the appropriations for these services are carried as separate items on the appropriation bills.

The IFF which I am discussing had certain very definite faults. We would never have recommended its final adoption unless these faults had been remedied. In the first place, the evidence of identification was not sufficiently conclusive. The friendly plane might have a faulty transmitter or the pilot might not have turned on the equipment soon enough. In other words, the evidence was negative rather than positive. Nevertheless the experience with this equipment led finally to the development of the Mark IV IFF equipment a few years later. This equipment, operating in the same frequency band, permitted a challenge to be emitted by the ship which was automatically answered by a plane using coded pulses similar to radar pulses. Indeed the equipment was used in connection with ship's radar, which actually made the challenge.

These sets went into a very limited production for the Army, Navy and the British but were never generally adopted, principally because of British opposition, based on the fact that the frequency was somewhat close to that used by German radar. Thus the British were afraid that the Germans would pick up the IFF signals too readily and learn how to jam them. It should be perhaps noted at this point that the British were using, as early as 1939, a radar challenge and pulse reply IFF system and should be given the credit for being the first to use such a system in actual warfare.

Throughout the period covered by this chapter the work was continued on the aircraft altimeter problem, particularly the pulse type of altimeter although this work was not brought to a successful conclusion until 1940. Indeed the altimeter project and recognition project were so closely related in technique that they were largely handled by the same group of engineers.

In the summer of 1934 the combined fleets carried out extensive training exercises off the Atlantic Coast. Since I had not been out to sea with modern naval radio for a number of years I requested orders which permitted me to be with the Fleet during these maneuvers. I was attached to the battleship WEST VIRGINIA which was the flagship of one of the battleship divisions. The Flag Officer in question was Admiral T. M. Craven. Captain Stark, later Admiral and Chief of Naval Operations, was on Admiral Craven's staff.

I had an opportunity to observe the way various radio circuits were performing and to see how radio was intimately connected with the maneuvering of ships in battle formation. It amazed me to see how many radio communications appeared to be necessary in order to carry out even relatively simple maneuvers when a large fleet was operating in formation. The general plan in those days was to have the battleships in a fairly compact formation, flanked by destroyers ahead and on either side and still farther out in the direction of the potential enemy, a submarine screen. The area of the sea surface covered by such maneuvers may amount to many thousands of square miles. Information as to the positions of ships and possible enemy contact is communicated to the ships immediately interested. It takes a surprisingly large number of channels to properly carry out such maneuvers. It did seem to me that a good many messages were sent which didn't serve any very useful purpose, but the radio officers insisted that in any event they were excellent training fór those people manning the radio networks.

I have nothing to report in respect to this cruise which is of special technical interest, but I will mention two very curious incidents that happened while the Fleet was coming back into Newport after the conclusion of the maneuvers. Both happened early in the morning while the battleships were strung out in a long column about 500 yards apart and steaming at fifteen knots.

Two sailors on the after end of our ship were painting the jack staff. One of these sailors fell off the stern of the ship. There are two hazards in an accident of this sort; a man may very easily be cut to pieces by the ship's propellers, or he may be swept so rapidly astern in the back wash of the propellers that it is impossible to get a life preserver to him in time. In a good many cases of this sort the next ship behind the column, having been alerted, will pick the man up. In our case the sailor's companion immediately threw a life buoy over the stern of the ship but it was caught by the back wash and rapidly swept astern far out of his reach.

This man was saved by a peculiar bit of good luck. There is a region of water astern of most ships which actually follows the ship. Between this region of water and the stern itself there is a dangerous area wherein the man may be drawn into the propellers. Back of this region of water the back wash takes effect and the man is rapidly swept astern. The man who fell overboard was fortunate enough to get into this region of "following" water. He told me that he didn't have to swim at all; hemerely was obliged to tread water to keep afloat. Thus he sailed along comfortably at fifteen knots ten feet back of the ship. A rope was thrown to him and he was readily hoisted aboard.

Later I took advantage of knowledge of this phenomenon on board my own small yacht because I found that following water came right up to the stern of my craft on account of the shallow draft at the after end. I used to put my fish in a wooden box with a wire screen across the bottom and lower the box over the stern of the ship close to the stern. That box would follow the ship back to port without any line attached to it although I generally used one so that I wouldn't lose the fish if I unexpectedly stopped or made some unusual maneuver. In towing a row boat behind a small ship, especially in quiet water, advantage can be taken of this same effect so that the boat will tow very easily indeed.

The second incident on this same morning occurred at the other end of the WEST VIRGINIA when we struck a whale which I would estimate to be about seventy feet long. He was buckled so tightly across the bow that he couldn't get loose, the pressure of the water holding him in place. Naturally we were pushing a tremendous bow wave and were being slowed down, so the captain signalled to the Flagship requesting permission "to turn out of line to clear a whale". We had to repeat this message four times since it didn't seem to be a reasonable message. The operators were sure it had been garbled. Finally we received permission. We turned out a little to one side and slowed down so that the whale was able to get away, although evidently pretty seriously crippled. It is not unusual for a naval ship to hit a whale, but it is usually a glancing blow. I never heard of one being pinned across the bow of the ship. A collision with a whale, incidentally, can be a very serious matter to a smaller ship, such as a destroyer.

It will be recalled that during the period covered in this chapter, the Germans were interested in establishing an aircraft transportation service with this country using large dirigibles. Many people will remember seeing the great German dirigible HINDENBURG and remember that it finally crashed at Lakehurst. I was drawn in as an expert witness during the investigations following this crash.

The HINDENBURG arrived over the Navy's Lakehurst field, the only one on the East Coast capable of handling such a ship, during very thick weather. The ship cruised around for a good many hours, waiting for the visibility to improve, finally coming in with the intention of tying up to the mooring mast. Captain Preuss was in charge of the HINDENBURG and Captain Lehman was a passenger. Captain Lehman was on his way to confer with Dr. Eckener, who represented the German Company in negotiating with the United States with respect to the acquisition of a suitable airport and permission to carry on a regular commercial transportation service with this country. Eckener was already in this country.

Captain Lehman was the author of a book covering the operations of the German Zeppelins during peace and war. I have never seen this book translated into English although it should be, because it is an extremely interesting and entertaining document. Captain Lehman himself had a very charming personality and had been with the Zeppelins ever since they started flying.

On arriving over the Lakehurst base, the HINDENBURG dropped her drag rope, which was siezed by the Navy ground crew in order to lead the ship to the mast and complete the mooring. Just at this moment there was a burst of flame and in a matter of seconds the whole ship, after a few minor explosions burst into flames and settled rapidly to the earth. The wind was from one side so that some passengers were able to escape through the windows of the cabins on one side of the ship, making a jump of 8 or 9 feet to the ground. Everyone who attempted to get out of the other side of the ship was killed or badly injured as the roaring flames completely blanketed everything on that side. The casualties were very high; many passengers lost their lives as did many of the crew. Captain Preuss, although terribly injured, escaped with his life. Captain Lehman was killed outright.

I drove up to Lakehurst, accompanied by my wife, the day before the investigation opened and got in touch with Lieutenant (now Captain) Watson, the radio officer of the Lakehurst Station, and reported to Captain (now Admiral) Rosendahl, retired. I also met Lieutenant Commander Anton Heinen. Heinen was a strong anti-Nazi and a very good Zeppelin pilot with much knowledge of the construction of the ships. He has been given, through some special dispensation, a commission in the United States Navy. I was well impressed with his ability and judgment. He spoke English very well although we carried on most of our conversation in German. I am sure he was absolutely loyal to the Navy.

It was plain from the start that the Nazi government, which immeciately sent representatives over to sit in at the investigations, wanted to claim that the ship was destroyed by sabotage. They plainly hinted in certain quarters that the American Jews must have had something to do with the affair. Of course this was rather ridiculous. It was pointed out to them that if any such thing had happened it must have been accomplished by planting a time bomb in the HINDENBURG before she left Germany, in which case we had nothing to do with the matter. In the second place, it was pointed out that since the Germans were going to operate trans-altantic aerial transportation service, they couldn't very well shut out Jewish passengers from the United States, since many Jewish business men have financial interests in Europe and probably would be among their best customers. Whether as a result of this or not I do not know, but the Nazis "piped down" on this charge of sabotage. As a matter of fact no one ever discovered what caused that crash. If anyone had planted a bomb and set it to explode a few hours after ship made the mooring mast at Lakehurst, so that the loss of life would not be excessive, it could have happened when it did, since the HINDENBURG was delayed a number of hours behind the scheduled arrival at the mooring.

I never saw a more disagreeable crowd than this Nazi group. It included representatives of the Nazi party and government, also some scientists. One and all they seemed a hard boiled and vindictive lot. Incidentally, they had no use for Lieutenant Commander Heinen nor he for them.

Failing to make the charge of sabotage stick, they next asserted that certain radio stations in the vicinity must have emitted signals that induced sparks in the rigging and set off some hydrogen gas. I was ready for them there, having collected information on the frequencies and powers used by all stations in the vicinity and was able to prove that it was impossible for them to have developed such considerable voltage in the rigging as to cause any such action. It was difficult to get much evidence out of the wreckage. It was a terrible sight. The fire had thoroughly consumed so much that there was little available for examination. The motors were about the only things left which were intact. We did find one electrical contact from a certain indicating instrument which might have created a small spark during operation, but there was no conclusive proof that it had done so.

All such ships as this collect a heavy charge of electricity, so that when they first make contact with the earth a spark might be given off. However, since the first contact was made by the dragging rope, a Manila line, thoroughly moist on account of having been carried around in foggy weather for several days, it was obvious, to me at least, that any charge on the ship would have leaked off slowly and quietly through this drag line without doing any damage.

There were, I believe, three investigations of this crash; one was a Naval court of inquiry; the second, in which I was involved, was carried on by our Government and the third by the Nazis. They all ended in an impasse as far as explaining the disaster was concerned.

I was impressed with the extraordinary skill and ability in the field of aerial navigation and meteorology displayed by the Germans. These great ships flew for many years with very few accidents. I was also glad that our own ships were filled with helium instead of hydrogen. Had the HINDENBURG been filled with helium it wouldn't have been able to carry as high a pay-load, but on the other hand it wouldn't have exploded.

During the latter part of the period covered by this chapter Captain Hollis M. Cooley was appointed Director, June 17, 1935. Commander L.R.Moore served as Assistent Director from 1935 followed by Commander (later Captain) Lyman K. Swenson in 1937. I am sorry to say that Captain Swenson was later killed in action in the Pacific.

Among the officers acting as Technical Assistants was Lieutenant (now Admiral) W. S. Parsons who was Ordnance Officer from July 1933 to May 1934. Admiral Parsons is now right hand man for Admiral Blandy for the Crossroads Project at Bikini, in charge of all technical activities. Lieutenant (now Captain) E. H. Pierce was Radio and Sound Officer from July 1933 to 1935. He was followed by Lieutenant Commander Maurice E. Curts (now Rear Admiral Curts). Curts and Pierce were both extraordinarily effective men and of enormous assistance to the Laboratory. I am not surprised that they both distinguished themselves during the late war.

Captain Cooley did more towards selling the Laboratory to the Naval

Service than any director who had preceeded him, or for that matter, almost as much as all the former directors put together. He comes from an interesting family, his father having been for many years dean of the School of Engineering at the University of Michigan. Dean Cooley was well along in years when I first met him, but of extraordinary ability and acumen, in addition to which he had a very charming personality. One of Captain Cooley's brothers was long identified with the engineering program undertaken by the Chicago Drainage Commission. Captain Cooley himself doesn't claim to be a distinguished engineer but he has another characteristic which was of the highest possible value to the Laboratory; namely, the ability to make friends in all quarters and to persuade every high ranking officer in the Navy who happened to be in Washington for a few days, to come down to the Laboratory and see what was going on. Of course our work in radar was the big drawing card, but although Captain Cooley would inveigle important people down in order to see radar, he never let them go without giving them a bird's eye view of a lot of the other Laboratory activities.

I very well remember when he brought down Admiral W. H. Leahy (so long attached to the President's staff) with the Secretary of the Navy, Charles Edison. This was for a radar demonstration, although they saw a number of other things. Fortunately for us the demonstration was unusually good that particular day. Mr. Edison was at the Laboratory on a number of other occasions. He is certainly a firm believer in research, both inside and outside the Navy. At the same time he modestly disclaims any technical ability on his own part. When at luncheon one day, I heard him admit that he had an engineering education (MIT) but that it didn't "take" very well. Personally I think he was too modest.

At the end of this period then, we find radar out of the pioneer stage and ready to make a start towards development and installation; we find high frequency communications pushed up to higher and higher frequencies, opening up more and more channels for both Government and commercial use; television was making excellent progress; recognition equipment was getting a fairly good start; homing systems were well on the way to production and practical application within the Fleet, but not yet completed; the solution of the radio altimeter problem was in sight. The Laboratory was beginning to be provided with much larger funds and personnel, particularly in the radio field, was being expanded very rapidly although not as rapidly as should have been the case. The fact is that none of us after being starved for years, could accustom ourselves to thinking in terms of millions rather than in hundreds of dollars. We continually underestimated our immediate and future needs. The work that we were to be called upon to do from now on would have been done more quickly, with less cost and with better end results, if we had had more liberal support during the years of peace.