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# Technical Extracts from the Memoirs of Dr. J. A. Pierce

Edited by Walter F. Blanchard

The following is the first part of an extract from the memoirs of Dr. J. A. Pierce who was deeply involved in the development of radionavigation aids in the USA between 1941 and 1973. These are, of course, personal reminiscences, and some issues might be challenged by others working in the same field at the time. However, they give a remarkable insight into the problems faced by the scientists of the day and how they were overcome. The editing principle used here has been to rigorously preserve Dr. Pierce's own wording while eliminating those parts of a purely personal nature. The entire original document without cuts may be found on the Institute's website.

KEY WORDS

1. History. 2. Radio Navaids. 3. Design.

1. MY WARTIME WORK BEGINS. It was probably in April, 1941, when my life began a major turn from Physics toward Engineering. Professor J. C. Street, who had studied cosmic rays for years, began to sound me out about joining the secret work being done at the Massachusetts Institute of Technology. I was somewhat reluctant, partly because I felt that I owed Harvard a great deal and partly from serious doubts about my practical ability. It was, of course, impossible for Professor Street to tell me just what the work would be, and I finally suggested that

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he should proceed to get my security clearance so that he could tell me all about the new project. To my surprise, MIT yielded to this request and, after the military intelligence forces had spent a few weeks deciding that I did not favour the Fascists (or perhaps the Communists), I was able to attend a meeting of a few people to have the project explained. This group, which included Dr. Street, on leave from Harvard, had been organized to test equipment that was being built to form a new kind of radio aid to navigation. This new system, then known only as 'Project C', was intended to operate in the lower part of the VHF (very high frequency) radio spectrum, between 30 and 40 megacycles per second. It would, for propagational reasons, serve only a very short range near the surface of the earth and to a distance of 300 miles or more for high-flying aircraft. It was obvious that the first war-time need for better navigation for the United States' forces would be in the convoy routes in the North Atlantic, where a system having such short range would be next to useless, I thus found myself being asked to join the group to explore any possibilities for providing additional radio circuits at lower frequencies, to take advantage of the greater sky-wave transmission ranges available in the short wave (or high frequency) band. The proposed transmissions would be not unlike those used for ionospheric sounding, but the requirement for the precise measurement of time differences would be from ten to a hundred times greater than had been used in ionospheric work. As the precision of ionospheric soundings had always been severely limited by the narrow bandwidths enforced for experimental work by the Federal Communications Commission, there was no experience to indicate whether the required accuracy could be achieved; but, equally, there were no data that proved it to be impossible. Since this proposed work was obviously important and fitted well with my experience at Harvard, my doubts were somewhat relieved and I agreed to take a year's leave of absence beginning July 1, 1941 and see what I could contribute. This decision was made easier by the discovery that I already had three friends in the group of about ten that existed in the spring of 1941. In addition to Dr. Street, I also knew one of his former students, Dick Woodward, who had taken his doctorate under Professor Street after I had come to Harvard. I was especially pleased to find that the head of the group was Mr. Melville Eastham, the President of the General Radio Company in Cambridge, who had been a regular attendant at Harvard Physics Colloquia and whose abilities I greatly admired. There were also Donald Fink, whom I knew slightly as the Editor of 'Electronics' magazine, and two brilliant young recent college graduates, David Davidson from Trinity College and Dick Lawrance from MIT.

2. THE OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT. Long before the American entry into World War II, Dr. Vannevar Bush, a famous scientist and engineer who had formerly been Vice-President of MIT convinced President Roosevelt of the importance of mobilizing the scientific talent of the country. Dr. Bush was made the head of the Office of Scientific Research and Development (ORSD), which was supported by ample funds and made entirely independent of the military forces. As seen from the viewpoint of one far below him in the organization, Dr. Bush's essential contribution was the provision of a climate in which the scientists could work freely while OSRD and its subordinate entities kept financial problems and military demands from delaying or preventing the scientific work. In retrospect, it is clear that Hitler's stopping of scientific research and trying to fight the war with what he had in 1938 was one of his serious errors. It was exactly

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this mistake that Dr. Bush prevented in this country, in spite of determined military opposition to devoting manpower to anything except production and fighting. It has been claimed that microwave radar gave the allied forces the margin that won the war while the atomic bomb ended it. Neither of these developments would have been possible without Dr. Bush and his organization.

The National Defense Research Committee (NDRC) was organized under the OSRD. This committee dealt directly with the scientists. It was headed by James Bryant Conant, the President of Harvard and a distinguished scientist himself. The NDRC had the respect and support of the scientific community. It was organized in a number of divisions dealing with various scientific disciplines, of which the one of importance to me was Division 14 devoted almost exclusively to microwave radar.

At the beginning of the war, radar had been developed in several countries. These early devices operated mostly at wavelengths of one or two metres because vacuum-tube transmitters could not develop the required high power at shorter wavelengths. This in turn meant that the needed directional antennas had to be large. Effective radar was therefore available only for large ships or fixed installations ashore. This was the type of radar that detected the Japanese attack on Pearl Harbour, although administrative laxity prevented an effective warning. The British CH (Chain Home) radar stations around the coasts of the United Kingdom had a far better record, as they made it possible for the Royal Air Force to win the Battle of Britain in 1940 by giving early warning so that the famous 'few to whom so many owed so much' could be concentrated to meet each German attack wherever it came.

In the summer of 1940, Sir Henry Tizard brought a famous mission to the United States. One of his duties was to induce America to produce the cavity magnetron and incorporate it into a useful airborne radar system. This magnetron had been developed, if not invented, by a group headed by Professor Oliphant at the University of Birmingham. It could generate large amounts of power at wavelengths of only a few centimetres. With the war at a very critical stage, Great Britain could not support the effort required to put the magnetron into production and develop the necessary modulators, antennas, receivers, and other elements needed for an effective radar system. Doctors Bush, Conant, and others welcomed this opportunity to contribute and assigned the responsibility to Division 14, whose executive body was called the Microwave Committee. President Karl Compton of MIT agreed to support this effort under NDRC and established the famous Radiation Laboratory, which eventually grew to about 4000 people, nearly half of them of professional calibre. The effort was a great success, but its story is only a tangential part of these memoirs, important here only because it established at MIT a flourishing defence organization under the leadership of Dr. Lee A. DuBridge.

3. PROJECT C. I have always felt, without any proof, that some small amount of information about a developing British aid to navigation, called Gee or G, leaked from the Tizard Mission although I understand that the mission was not authorized to discuss this subject. Gee had been proposed by and developed under the direction of a brilliant and aggressive engineer named Robert J. Dippy. From 1937 onward he had fought for this idea and had the happiness of seeing it find a very useful place in the war effort, beginning in early 1942.

Wherever the idea came from, a proposal to develop a similar system was made in November 1940, by the Chairman of the Microwave Committee, Mr. Alfred Loomis.

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Mr. Loomis was a rich amateur experimenter who maintained a private electronics laboratory in Tuxedo Park, New York. The other members of the Committee were, in general, vice-presidents or directors of research of major electrical or radio companies, and included Melville Eastham. The acceptance of this proposal established 'Project C', one of the first three efforts approved by the Committee.

The Microwave Committee then proceeded in a most curious way. The representatives of the various companies divided up the problem and agreed to build the components that would be required for the proposed system. If memory serves, the General Electric Company and Westinghouse each was to build a two-megawatt pulse transmitter; the Radio Corporation of America and Sperry were to make two different kinds of receiver-indicators; and the Bell Telephone Laboratory was to make two transmitter timers, the most critical components. The only thing obviously missing was a good idea about how the timing of the received signals was to be measured and presented to the navigator. The Committee made a number of suggestions, none of them very workable. The best, to my eye, was the idea that one station of a pair might transmit 19 pulses in the same elapsed time in which the second station would transmit 21 pulses. One of these series could then be read against the other in the manner of a vernier scale. My sympathies, after the fact, were with the unfortunate Bell Laboratories people, who had to build transmitter timers that would respond to any of the several proposals of the committee, thus requiring an almost impossibly complex mechanism. After authorizing contracts for all this equipment, the Microwave Committee passed on to other duties and as a whole was not heard from again, although Mr. Loomis kept in touch with the project from time to time.

4. THE IDEA OF HYPERBOLIC NAVIGATION. During the winter of 1940–1941, Mr. Eastham had recognized the fact that a technical group would have to be formed to receive and test the Project C equipment when it should be delivered. As he lived in Cambridge and the Radiation Laboratory had been formed at MIT, Mr. Eastham arranged to have this group housed there and included in the Radiation Laboratory staff. The navigational work was distinct from that on radar and used many different techniques, but this affiliation persisted throughout the war and the LRN (Long Range Navigation) group became Division 11 of the Radiation Laboratory, the only division that was not working on the many implementations of microwave radar.

The new idea behind all this activity was simple and powerful. In 1940, the only radio aids to navigation were various kinds of direction finding that had acceptable accuracy only at relatively short ranges. The new methods were to be based upon the time of transit rather than the direction of radio signals. Because the velocity of light, or of most radio waves, is approximately a thousand feet in each microsecond, measurements to that accuracy should yield position to a fraction of a mile if the velocity of propagation could be predicted accurately. The proposed technique was the exact inverse of the acoustic ranging that was so successful in the first World War, when enemy cannon were located by measuring the relative time of arrival of the sound waves from their firings at three or more known locations. In the radio implementation it was proposed that two separated transmitters should transmit short pulses, or some other easily timed signals, from known locations with an accurately known time separation. The measurement of the time difference between the two received signals would establish the position of the receiver on one of the

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infinite family of hyperbolae having the two stations as foci. To take a case of maximum simplicity, suppose two pulses are sent out simultaneously. If a navigator received them at exactly the same instant, he would be equidistant from the two stations or on the perpendicular bisector of the line between them. If he received the pulse from station A, say, before that from station B, he would be somewhere on a curved line somewhat nearer station A and concave toward that station; and the exact amount of the time difference would tell precisely on which of the many curves (hyperbola) he must be.

Of course the navigator wants to know more than his position 'somewhere' along a line. This problem is solved by making a second measurement on a second pair of stations (which may or may not include one of the stations of the first pair). The position then is at the intersection of the two lines of position. Various modifications of this fundamental idea are required in practice. For example, it would be very inconvenient to try to receive two pulses from different stations at the same instant. This problem and others are resolved by transmitting the pulses alternately so that, for example, the pulse from station A is always received before that from station B no matter where the navigator may be. This technique makes it possible to identify which pulse is which; an important factor as it is obviously better to have the two pulses absolutely similar.

This simple description sounds as though only a single pulse were to be transmitted from each station. Actually the pulses are endlessly repeated many times per second in an accurately spaced sequence. This makes it possible to show the pulses steadily on an oscilloscope and permits the navigator to determine a position at any time he may wish.

5. HYPERBOLIC THEORY AND EXPERIMENT. Nothing has yet been said about the process of getting two separated transmitters to emit trains of pulses in exact synchronism. This needs to be done with almost infinite precision because two practical 'clocks' or oscillators at the two stations will gradually drift apart without limit as time passes. A solution to this problem is to install something much like a navigator's receiver at a known place within range of both stations, and to have an observer then transmit to one of the stations information enough so that adjustments to the time of transmission can be made so as to keep the time difference exactly what it should be at the monitoring point. With the oscillators available in 1941, it turned out to be necessary to make small corrections every few seconds, or at least once or twice a minute.

Even this bare outline probably includes more than was thoroughly understood in 1941 about the theory and practice of hyperbolic navigation, as the method was to be called later. Much needed to be learned about the construction of satisfactory equipment, about the velocity of propagation (at least in the sky-wave mode), about the mathematical methods that would allow the computation of a fix from the measured time differences, and about presenting this information to a navigator in a sufficiently simple and rapid way.

My first step along the road was to arrange for the construction of a pair of relatively low power pulse transmitters, suitable for a wide range of frequencies. I had these made at Harvard because the Cruft shop had an excellent machinist and radio technician, Victor Ghigli, who had built much of my South African equipment and who was so used to my ways that he could make what was wanted without needing

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detailed plans and specifications. This work began some weeks before I enrolled at MIT, and the cost was borne by the Radiation Laboratory. The transmitters were ready in August and were promptly installed in two discontinued Coast Guard stations that had been taken over for the Project C high-powered transmitters. These stations were at Fenwick Island, Delaware, and near Montauk Point on Long Island.

I was much impressed by the local electric fields from the first two-megawatt transmitter being tested at that time at Fenwick. I found that I needed rubber gloves to handle even small tools in the yard of the station. When I passed a piece of antenna wire through an insulator and bent it back to form a loop, sparks would leap the gap as soon as it was reduced to three or four inches. I was enthralled to realize that this was exactly the technique used by Heinrich Hertz in his first detection of electromagnetic waves in the 1880s, although Hertz never had such an abundance of energy to work with.

There was at this early date no mechanism for synchronizing the pulses from the two stations. My experiment, therefore, was to time the pulses independently from two crystal oscillators that were as stable as possible and to observe them on an oscilloscope that was timed by a similar oscillator. I hoped to be able to judge, without precise measurements, whether the pulses seemed sufficiently stable at long distances and whether they passed through the ionospheric reflection process without serious distortion of their shapes. A schedule was set up for transmitting at four or five frequencies, between three and ten or twelve megacycles per second, at appropriate times of the day or night. With these arrangements made, Mason Garfield, who had followed me from Cruft to the Radiation Laboratory, and I set out on a junket in a rented station wagon full of experimental receiving and test gear. We would stop every few hundred miles and observe the pulses wherever we could stay with friends or find a motel that would let us, for a price, use enough electric power to run our gear.

6. CIRCULAR SWEEPS. Mason and I were trying to observe our signals on a system of three oscilloscopes that had circular sweeps operating at speed ratios of one, ten, and one hundred. The idea was to read the angular positions of the pulses as in taking the reading from the dials of an electric watt-hour meter. This was impossibly cumbersome, and somewhere on our travels I conceived the idea of showing the two pulses to be compared superimposed on relatively short horizontal sweeps whose timing could be adjusted to agree with that of the pulses. I must admit that my idea of an electronic mechanism to do this feat was too awkward and inaccurate to be of much use. Fortunately, while Mason and I were travelling, some design information about the British Gee system had been received at the Radiation Laboratory and it was found that the same superposition idea was used there. Dr. Street and Donald Kerr promptly set to work devising circuits for such a receiver, using their experience derived from the construction of cosmic ray counters and other new devices.

7. MR. LOOMIS TALKS. I have always been pleased that my report of this first field trip happened to be dated October 1, 1941, because, after a year of frantic activity, the first four Loran stations went into operation on October 1, 1942. This speed record is especially notable because my report said, in effect, 'I think it will work, but we must abandon all our present ideas about instrumentation and start

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again'. I had at that time no idea how many changes and additions would be required in the future. My next step was to retire for a couple of months to my old office at Harvard, primarily because it was quiet and closer to our apartment, and try to compute the time of transit for a simple sky-wave as a function of frequency, distance, and time of day. This was an impossible task, considering how little was known about ionospheric variation at that time, but I hoped that I could at least deduce the probable shapes of the curves relating these various functions so that the results of a very few experiments would fit easily into place. This turned out to be mostly wasted effort, but it at least kept me from interfering with the construction of the new receiving equipment, for it must be admitted that I was by no means well acquainted with the new techniques Dr. Street was developing. During this time and for months thereafter, we did not realize that the lower frequency work would be more than an adjunct to the VHF pulse system, so we kept waiting for the Project C equipment to provide the synchronism that was required. The transmitters and timers for Project C were delivered and partially tested, but the receivers never did work very well. In fact, I do not think that satisfactory synchronizing signals were ever received at Manahawkin, New Jersey, a Bell Telephone Laboratories field station approximately midway between the two transmitting sites.

In December, a crude navigator's receiver for the lower frequencies was in existence in breadboard construction and a second one had been built in more transportable form. Dick Lawrance, who had been working entirely on Project C, took the breadboard to Manahawkin and found that be could receive excellent signals from both stations and, by relaying a continuous stream of instructions over a telephone line to Fenwick, could maintain a sufficiently constant time difference between our two transmissions. We therefore were able to set up a synchronized pair without waiting longer for the Project C equipment to be put into successful service. This was fortunate as that project was encountering longer delays and greater disappointments.

8. PRE-LORAN OBSERVATIONS. At the end of December, Mr. Eastham sent me to Bermuda to make the first pre-Loran observations. I took with me Ed Stevens, a young technician, and went by ship, landing in Hamilton on December 31st. On this trip Ed is, I am sorry to say, chiefly memorable for being fearfully seasick. After a miserably lonesome New Year's Eve in a cheap hotel, we established relations with the officers at the Daniel's Head Z station, a naval installation at approximately the point in Bermuda nearest New York. After the holiday our equipment was delivered there and set up most conveniently. We found more than adequate quarters in a beautiful cottage colony at which the British Ministry of Supply had pulled strings to get us accommodated at a nominal rate. I am sure the present management of Cambridge Beaches would be shocked to hear that this rate was eight dollars a day for the two of us. As no cars were then permitted in Bermuda, we rented bicycles to commute between the cottage and the station.

The Royal Navy accepted us in their social doings so far as our irregular operating schedule permitted. An especially memorable evening party was one in which I was introduced to a charming old gentleman, Admiral Kennedy-Purvis, who had been called out of retirement to take a senior position at the Bermuda Dockyard. In about 1906, Lieutenant Kennedy-Purvis had apparently been the Royal Navy's chief expert on wireless. He was, of course, intensely interested in our activities. I was in great doubt about how much I was allowed to tell him, but this turned out not to be crucial

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as I was forced to suspect that the Admiral had not learned any more about radio since the days of Edward VII, and I found it impossible to give him the least idea of what we were trying to do. The officers we met were most helpful and, with the exception of the Admiral, were very discreet and asked almost no questions.

The experimental work went very well. My transmitters were of such low power, a few hundred watts, that we could not detect a ground wave at the distance of about seven hundred and twenty miles, but this did not trouble me as I still believed that my mission concerned only sky waves. At all times of day we received useful signals at one or more of the frequencies we tried. We quickly found, as expected, that the signals reflected from the higher F-layer were far too unstable to be useful for our purposes, but the earlier arriving E-layer pulses were quite satisfactory. Averaged over all our frequencies and times, the measurements showed a mean error of about two and a half miles, and the centre of the distribution agreed well with our calculations.

A computation I made at the time of this trip is of historical interest. We used two of the extremely complicated Bell timers, two transmitters, synchronizing equipment in New Jersey, and receiving equipment in Bermuda. I counted up a total of 768 vacuum tubes that had to be operating correctly before a navigational measurement could be made. Because vacuum tubes were not as constant in their characteristics as are today's transistors and chips, it was a frequent accident for some circuit to 'jump in phase' and require readjustment before the measurements could be continued. Fortunately those corrections could usually be made in a few minutes. The two-trace oscilloscope indicator gave us easy and accurate measurements but had two serious deficiencies. In order to make the readings the pair of pulses to be compared had to be positioned in special spots on the receiving oscilloscope time base. The only accurate way we had for making this setting was to mis-adjust the crystal oscillator frequency and allow the pulses to drift slowly around the time pattern until they reached the right places. This was an impossibly slow process as the duty of the oscillator was to be as exact as possible. The effect was much the same as trying to set a clock by speeding it up and waiting until the hands read the right time. This unhappy situation could be somewhat ameliorated by inducing random phase jumps in the timing circuits, until the pulses happened to come to rest reasonably near the desired positions, obviously something better had to be arranged. We also found that it was often necessary to compare a strong pulse with a weak one. This meant a severe loss of accuracy because a weak pulse could not be seen well without increasing the receiver gain until the strong pulse overloaded the linear circuits and became distorted, The most important discovery that we made in Bermuda was that at our lowest frequency of 2·8 megacycles per second, we received stable and useful E-layer pulses all night except for a 'fuzzy' period of an hour or two just before dawn. This was a great surprise as the E-layer was then generally supposed to disappear at sunset. We later found that, by lowering the frequency somewhat further, a reliable E-layer signal could be received all night at all distances beyond 200 to 250 miles. The lack of skywave reliability at such short distances was not important as that distance region was served by strong groundwave signals.

9. RECEIVER MODIFICATIONS. Back at the lab I found that Dick Woodward had already corrected the first of the deficiencies in the receiver. The timing circuits divided the 100,000 cycle-per-second frequency of the crystal oscillator

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successively by ratios of 2, 5, or 10 to obtain the pulse repetition frequency of 25 per second. Between the second 'counter' and the last, the division ratio was 400 to one. Dick had discovered that the output pulse of the last counter could be fed back to the second to reduce the count by one unit each 25th of a second. This effect, when switched on and off, could alter the overall division ratio from 400 to 399, thus yielding an output frequency that allowed our received pulses to slide around the oscilloscope pattern at a controlled rate that could be stopped when the pulses arrived at the correct position.

I did not understand how Dick's invention worked so I set up a model circuit to study. There was a capacitor that coupled the output pulses back to the earlier divider. Because I could not see how to decide what size capacitor to use, I put a large variable capacitor into the circuit. To my surprise, I found that the capacitance made no difference in the output frequency until it reached a particular size when the frequency suddenly changed. A further increase in capacitance made no difference until a second critical value was reached, when the frequency changed again. It soon developed that, if the 'feedback' pulse were applied to a counter that divided by ten, at least eight different frequencies could be obtained as the count was momentarily reduced to less than ten. The obtainable overall division ratios were thus 400, 399, 398, … 393. We quickly made the transmitter timers and the receiver-indicators operate at any chosen one of these eight frequencies (between 25 and about 25 7/16 pulses per second) by incorporating in each a switch that inserted one of seven adjustable capacitors. This meant that we could now have eight pairs of stations operating at a single radio frequency. By selecting the correct 'recurrence rate', the desired pair of pulses could be made to stand still on the oscilloscope so that the time difference could be measured, but at the same time the other seven pairs drifted around the 'picture' at different rates such that they did not interfere seriously with the measurement.

The difference in amplitude between the chosen pulses was compensated by a circuit that reduced the gain of the receiver at the time when the stronger pulse was expected. The technique was simply to position the pulses and adjust the main gain control until the weaker pulse had the chosen size, and then to reduce the size of the stronger pulse by turning the 'differential gain' control. By this means, and by adjustment of the time separation between the oscilloscope sweeps, the two chosen pulses could be precisely superimposed (visually) and the time difference measurement could be made with maximum, accuracy. I was surprised, after the war, to find that I had been granted a patent on this device, which was apparently one of the first implementations of a time-variant gain for a receiver.

With these modifications, and a few minor changes, the circuits of the new measuring instrument had reached a useful form. Don Fink, who acted as Mr. Eastham's administrative assistant, arranged a contract with a minor division of the Radio Corporation of America to package our circuits in 'commercial' form and find and supervise a manufacturer who would produce a trial quantity of receiver-indicators. This work, with a good deal of assistance from Radiation Laboratory people, went with amazing speed. The first discussions about the new contract were held, I remember, on St. Patrick's Day in 1942.

10. RECEIVER-INDICATOR PRODUCTION. The first receiver-indicators, built by FADA on Long Island, were received at the Radiation Laboratory

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before the end of August. This speed was possible, in part, because FADA never waited for commercial delivery of components. When a part threatened not to be ready in time, an emissary would be dispatched to Cortland Street in New York with a pair of large suitcases. He would buy the necessary number of any parts that were reasonably similar to the desired ones. As a result, probably the entire production run never contained more than ten units that were absolutely identical. This made no real difference, as all the receivers were sent to the Radiation Laboratory for checking and any necessary modifications.

I have always felt that the rapid success of Loran, as it came to be called, was due largely to Mr. Eastham's wisdom and courage in ordering an initial supply or 250 receiver-indicators. We soon found that it took lots of units to fill the Naval supply pipeline. Another negative factor was that, although the Navy could order the installation of receivers on its ships, each captain was the absolute authority on board and could not actually be forced to use the new equipment. The value of a large number of trial receivers was that, whatever delays and reluctances there were in the naval system, a few units fell into the hands of officers who would use them. It was then only a few months before favourable opinions began to circulate in the officers' clubs and the system was able to expand as fast as equipment could be produced.

When we were preparing for the Bermuda experiment I had happened to visit Montauk Point and had seen that an excellent groundwave pulse from Fenwick Island was received, even at our highest frequency of 8·5 megacycles per second. This stimulated the idea, which we should have had months before, that a Loran 'slave' station could synchronize itself with the signals from the 'master' station received at the slave station site, and that there was no need for an intermediate monitor station to perform this function. This realization led to the conclusion that we need wait no longer for the Project C signals.

Work was immediately started on higher power transmitters (100 kilowatts) under the direction of Al Poté, an MIT trained engineer who was the part owner and manager of a local broadcast station. At the same time, Dick Woodward began the construction of transmitter timers that were less complex and therefore far more reliable than the Bell Laboratory devices we had been using. The idea of maintaining synchronism within the slave station meant that we had to devise a way to prevent the extremely strong local transmission from saturating the sensitive receiver that was needed to receive the signals from the master station. In Dick's first model this took the simple form of relays that closed at the right time to short-circuit the transmission line from the receiving antenna. Division 11 was growing rapidly throughout this period and a larger and larger fraction of the people were working on the lower-frequency project. It gradually became clear that this modification was a complete system by itself and that there was no real need for the VHF system. I do not remember when the work on the original Project C was finally dropped, but I believe that some of the equipment ordered for that project was never removed from the shipping cases.

11. THE SPRING AND SUMMER OF 1942. By June of 1942, we had installed high power transmitters in our two stations, and construction of two more stations, at opposite ends of Nova Scotia, had begun. In this connection, the Canadian Navy sent an officer, Commander J. R. Foster, to act as liaison with the Radiation Laboratory. Commander Foster, or John, promptly endeared himself to us

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by taking off his uniform jacket to go to work with us in the lab, and generally doing everything he could to educate himself and advance our cause. He was chiefly responsible for the fact that the Canadian Navy accepted Loran even more quickly than did the American Navy, and he of course saw to it that the Canadians received some of the very first receiver-indicators to be distributed. To the American Naval Liaison Officer, Lt. Cdr. Larry Harding, goes credit for coining the name 'Loran'. He observed that a shipment of equipment to Canada, addressed to the 'LRN Project', had arrived with the words 'Long Range Navigation' pencilled on the boxes. This was too transparent, so he expanded it to 'Loran'. Curiously, this term was never deciphered, so far as I know. Most people apparently assumed this to be the name of the inventor of the device.

Near our two American stations, the work had attracted a good deal of local interest and wild rumours began circulating. Walter Tierney, our highly important construction man borrowed from the Bell Telephone Laboratory, turned out to be an excellent practical psychologist. He had someone paint very large signs, 'Fenwick Island Radio Direction Finding Station' and 'Hither Plains Radio Direction Finding Station' and had them attached to the faces of the old Coast Guard stations. Apparently everyone knew that direction finding was not exciting and, to my surprise, the local interest subsided immediately to zero.

In June 1942, we had also finished one or two quite complete receiver-indicators, incorporating all the modifications suggested after the Bermuda trip, and electrically nearly identical to those being produced by FADA. Mr. Eastham again selected me to give the new system its first mobile trial. I was surprised to find that this would take the form of a trip in a Navy blimp, flying from Lakehurst, New Jersey. This was actually a very well chosen vehicle, because the blimp could fly slowly at low altitude directly to any number of check points; something that could not be done nearly as easily in a car, aircraft, or boat.

I must explain that the calculation of the Loran readings to be expected at a given point is direct and relatively easy, while deduction of the position from the readings is not simple, fundamentally because the Earth is not quite spherical. The oblateness of the Earth is enough to produce serious errors (if the calculation is done for a sphere, which is the only shape for which the formulae are easy) at distances of more than a hundred miles or so. Loran therefore was planned to work with pre-plotted charts or tables that had to be based upon many weeks of calculations. Fortunately these computations would need to be done only once for a given pair of stations; the results could be stored, in charts, for example, and used forever after by any number of navigators. At the time of the blimp trip no such computations had been made. The idea, therefore, was to fly along the coast passing over lighthouses, bridges, or any other identifiable points that could be located on navigational charts. After the readings had been recorded at these places, I could come home and spend several days calculating what the readings should have been at each of the points where an observation had been made.

12. THE BLIMP TRIP. I drove to New Jersey with my equipment on Sunday, June 7th, and spent the night in a small hotel in Tom's River. In the morning I was greeted by the news that during the night a pair of blimps had collided and that one of them had fallen into the ocean with the loss of its crew and a number of scientists who were on board. This disturbed me, not because I thought that such an accident

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was likely to happen again but because my wife was, at this time, nearly six months pregnant, and I could visualize her reaction when she heard the news on the radio. My first duty, therefore, was to telephone her to assure her that the accident had nothing to do with me, and that blimps were (in spite of the evidence) perfectly safe to travel on.

Captain Rosendahl and the staff of the Naval Air Station were much too occupied with the aftermath of the accident, and the very impressive memorial service that followed, to pay much attention to me and my problems. I was installed in a 'balcony' room in the large hangar and allowed to amuse myself with the receiver all week. Finally on Saturday, the 13th, the time came for our experiment. My gear had been lashed down on the navigator's chart table so that I could operate it from a standing or crouching position in the catwalk. I was disturbed at first by the magnitude of the noise and vibration; it appeared that in a blimp there was no lift to waste on sound absorbing material, and the propellers were whirling just outside the thin walls of the gondola.

We flew down the coasts of New Jersey, Delaware, and Maryland until we passed well beyond the station at Fenwick Island, while I made measurements timed to coincide with shouts by a crewmember whenever we passed over an identifiable point. This was not quite as easy as I had hoped because the aircraft's power supply was not very steady and induced phase jumps that had to be corrected every few minutes. These did no real harm, however, and I found myself thrilled to see one pulse move with respect to the other as we progressed. I had, of course, known that this would be so but to see it actually happen was very exciting.

My original plan had been to fly inland from this most southern point and see how far we could receive our pulses over land. We had known that this range over land would be (for the groundwave pulses we were using) far less than over water, and we wanted to see just how bad this condition was. Unfortunately it was a windy day, and I soon became airsick. I found that a blimp at low altitude over land can perform all the gyrations of a small boat in a choppy sea, plus a curious motion of its own as the gondola swings like a pendulum beneath the gas bag. After half or three quarters of an hour of misery, I was relieved when the Captain came back to inquire how things were working. With some apparent hesitation he asked how important this overland part of the trip was to the experiment. By this time I was ready to admit that it was not at all necessary and was happy to have him say, 'I guess if you don't mind we will go back and return by sea. The ship can take it but the crew can't'. Although we had never thought about the possibility before, I was inspired at this point to 'invent' homing along a Loran line of position.

13. THE MIDDLE OF THE HANGAR. While awaiting my chance to fly, I had played with the receiver until I thoroughly knew the reading to be expected at the Lakehurst hangar. I realized that if we flew along the line having that reading in the right direction we would be sure to get there sooner or later. I therefore suggested to the Captain that I could shut down the equipment for a while (thus getting some much needed rest) and that he might fly well out to sea and 'lose' me. I would then start making measurements and see if I could guide him back to Lakehurst. This sounded like fun to him, so he flew sixty or eighty miles out to sea. I then set up the reading for Lakehurst on the indicator and found, as expected, that the pulses were far apart on the screen. I soon discovered which way to go to bring the pulses together

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and, with some trial and error, found a heading that kept the pulses nearly in coincidence. From then on it was only necessary to shout 'a little left' or 'a little right' to the pilot from time to time as we strayed from the line.

After an hour or more, I saw that we were approaching the coast and that the hangar was easily visible, although not straight ahead because we were, naturally, heading somewhat into the wind to keep from drifting off the line. I paid no attention to our heading, as I was satisfied to find that we were clearly approaching the hangar. Finally the pilot broke away from my instruction to make his final approach to the landing area in an upwind direction. Then there was a long delay because a small engine that was supposed to trim the blimp's fore-and-aft balance refused to start. We floated for some time over the landing field, our nose pointing steeply downward, with occasional bursts of engine power to bring us to a lower altitude and keep us from drifting away from the field. After this delay, the ship was brought into a horizontal altitude so that the landing crew could catch our mooring ropes and bring us down.

When we landed I waited a few minutes until the Captain alighted. He approached me in a state of some excitement and gave me Loran's first official accolade, saying, 'You know, boy, we weren't headed for the hangar; we were headed for the middle of the hangar!' He was certainly correct, as the hangar was about a thousand feet long and the precision of our groundwave pulses was such that we were probably within a hundred yards of our line of position most of the time, but it was a delight to have our system receive such an endorsement.

After I had returned to Cambridge and had had time to calculate the 'expected' readings for the various places we had pinpointed, I found that most of our measurements on this flight had been within 200 yards and that the average of all of them was in exact agreement with the calculations. I found out later that there were better methods for computing the values on an oblate Earth, but at such short distances the difference would probably not have been great. In any case, I was so happy with the agreement between calculation and observation that I was careful never to re-compute the readings to be expected.

14. OTHER EARLY TESTS OF LORAN. Within a couple of weeks we had a chance to use the same receiving equipment in a B24 aircraft that belonged to the Royal Air Force and happened to be in Boston to have some Radiation Laboratory radar equipment installed. I do not remember which of the boys in our group had the fun of this trip over the Gulf of Maine and Nova Scotia. I should have explained that all of this work with the more powerful transmitters and improved receivers was done at a frequency of 1·95 MHz. After the Bermuda trip, we had reduced the operating frequency to 2·2 MHz in an effort to improve the night-time reliability of E-layer transmission. This was a successful change, but unfortunately our pulses at that frequency had begun triggering automatic distress alarm systems on ships in the Great Lakes. Because the war had made it necessary to forbid amateur radio operation, we found it convenient to make a further move into the former amateur 160-metre band where Loran remained for many years.

Shortly after the blimp trip, Dave Davidson took another receiver-indicator for a trip of several weeks' duration on a Coast Guard weather ship in the North Atlantic. This trip was principally to check out the skywave accuracy at night, but it also gave us the first real proof of the distances we could serve: about 700 nautical miles for the

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ground wave in the daytime and about twice that distance for the skywave at night, Dave's measurements thus did much more than prove that Loran could provide useful service in the Atlantic Ocean quite far from land.

The possibility remained that, by extra transmissions at higher frequencies, we could extend the daytime range of usefulness to equal that at night. This never would be done (although occasional trials were carried out both by the Bad Lab and the Coast Guard) primarily because it always seemed more important to use new transmitters for new areas to be served by Loran than for extension of a useful daytime service area.

After these successful trials, Mr. Eastham called a large conference of military officials. He had no difficulty in convincing them that we had a new and useful device, and soon had an agreement that the armed services (primarily the Navy) would pay for the receivers we had ordered and would reimburse the Radiation Laboratory for all the transmitters and timers we could construct while the military purchasing people were setting up their own sources. Al Poté continued to make excellent transmitters, and Dick Woodward began construction of a second generation of transmitter timers in the lab. Orders were placed for more of each with the Research Construction Company, a model-shop manufacturing facility set up by the Radiation Laboratory. This effort continued through even a third generation of timers before these models were supplanted by Naval supplies. We also arranged a contract with the General Electric Company for a prototype airborne receiver. The model they produced was probably the best receiver I saw during the war. Unfortunately, the War Production Board decreed that Bridgeport, Connecticut, where GE proposed to build the receivers in quantity, was an overloaded area and that the receivers would have to be built by Philco in Philadelphia. This led to an unfortunate delay as Philco had to redesign the receiver because they could not, naturally, use the GE designs.

15. CHARTING. In the middle of 1942, we were joined by Professor Fletcher Watson, an astronomer from Harvard, and Henrietta Swope. These two took over the calculation and plotting of the first charts and tables. They brought professional expertise to this important part of the program, which was very fortunate because none of us in the electronic area knew enough to do the job well. This phase lasted only a few months before these experts accepted commissions in the Navy and moved the problems to New York, where they ultimately collected a large force of computers, and sent the results to the Hydrographic Office for the plotting and publication of Loran charts. By 'computers' I do not mean today's instruments but people who thundered away by the hour on mechanical desktop comptometers, aided by eight-place tables of the trigonometrical functions. I believe that this force of about a hundred people was barely able to keep up with the demand for new charts and tables as the system expanded.

16. SERVING TIME AT BUSHIPS. In August, 1942, as I have said, we began to receive the first commercially built receiver-indicators. This kept us busy with testing and minor modifications for a long time. The first Naval installations were on the battleships *New York* and *Massachusetts*. I had almost no connection with the installation problems. They were handled by Dave Davidson until they were taken over by a Naval group established for the purpose. Whether in Washington or Cambridge, I worked on a self-imposed project for producing operating and

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maintenance instructions for the new receiver-indicators now being installed. These had twenty or thirty adjusting devices, all of which had to be correctly set. In fact, if two or three were wrong at the same time, it might be very difficult to tell what to do to correct the situation. I worked out a logical order for the adjustments (which were necessary to compensate for changes in voltages, temperature, or the age of vacuum tubes) so that each one to be tested and corrected would remain undisturbed by later adjustments. I then tried to explain in writing where the innumerable switches should be placed for each test, and what visible effect should be produced on the oscilloscope screen as each adjustment was made. I set down each of the many steps in simple language and then tested the whole set of instructions with volunteers who had never seen the instrument before. I watched carefully and silently while each one tried to follow the written instructions. Whenever one of my 'victims' hesitated or made an error, I noted just what happened and revised the rules to make them less ambiguous. After several weeks and a hundred or more volunteers, the instructions, having reached a point where errors were seldom or never made, were reproduced and distributed. They were, in fact, reproduced again and again, with minor modifications to match various receiver models, and many of the paragraphs remained the same for years. This enterprise kept me from becoming totally disheartened during my time with the Navy.

After about six months, which seemed a long time as there was a war to be fought and the Loran sets were needed, my specifications were given what I took to be final approval and I was allowed to return to the Radiation Laboratory. By the time a contract for Navy receivers was arranged, however, someone had re-introduced many of the cumbersome requirements into my specifications. As a result, the General Electric Company, which won the contract despite the War Production Board's earlier action, used many of the techniques developed under the Radiation Laboratory contract for the airborne receiver and built an excellent but massive receiver that might have served well as an anchor for a small ship. It weighed about 230 pounds and was, I was told, too heavy to install on the bridge of a destroyer that was already top-heavy with radar antennas. The small production run of this DAS model was ultimately given to the Coast Guard and used entirely in shore-based monitor stations that continuously checked the performance of the Loran stations. These receivers were certainly the best, from an electrical point of view, produced before the end of the War. In the meantime, the Radiation Laboratory had ordered more of the FADA receivers. The same units were later reordered by the Navy and several thousand were produced with no change except for a military designation, DAS-1. I do not think that the first true Navy production sets were delivered in time to be of service during the war.

In the winter of 1942–1943, besides working on my specifications and instruction books, I collected what data I could about the delay of our skywave pulses behind their corresponding ground waves, and tried to find a rule for predicting this delay more accurately so that we could improve the corrections to be applied to navigational readings when skywaves were used. I should explain that one of the great merits of the pulse system was that the ground wave and the various modes of skywave reflected pulses were all seen separately on the oscilloscope. A navigator would always use the ground waves (for maximum accuracy) when they could be seen, and could make a deliberate change to the skywave mode when it was available at night and at medium and long distances. We found that the skywave was always

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delayed at least 65 microseconds, which would shift the apparent position by twenty miles or more except that the sky wave from the other station was also delayed, usually by a different amount. These corrections were very important and attempts to improve our rules for their prediction continued for a year or two, although little further improvement was made after the first few months.

17. EAST BREWSTER REORGANIZATION. On January 1, 1943, operation of the U.S. stations was turned over to the Coast Guard, although Radiation Laboratory supervisors remained in them and were effectively in charge for another six months. I suppose that at the same time the two stations in Nova Scotia were turned over to the Canadian Navy, although my memory is not distinct on this point. At this time operation was maintained for only sixteen hours a day, which gave us some time for experimentation with various things. Realising that we would soon lose this facility, Walter Tierney found a convenient site at East Brewster on Cape Cod and built us a fine transmitting station for experimental purposes. An admirable feature of this station was the Consodine House in Brewster where the owner, Mrs. Consodine, who was then in her eighties, accommodated some of the permanent staff of the station as well as the very variable number of experimenters from Cambridge who worked there from time to time. Mrs. Consodine served meals that were truly worthy of a New England inn, and it became quite customary, when returning late after a hard evening's work, to find a large piece of apple pie and a glass of milk waiting on the table by one's bed. I have always been sure that Mrs Consodine kept the inn open throughout the war for our benefit, as she sold it as soon as the war was over. If so, her kindness was a valuable but unsung contribution to the war effort.

Soon after the Coast Guard and the Navy had effectively taken control of the operating system, Mr. Eastham retired to his proper responsibilities as the head of the General Radio Company, along with many valuable committee and advisory contributions of his time. Don Fink was made Division Head, but for some reason I never understood he apparently did not get on well with the higher administration at the Radiation Laboratory. After about six months he found it convenient to join the Office of the Secretary of War, where he performed many useful services, some of them in the interest of Loran. Dr. Street then became the Division Head, while Al Poté became the Group Leader in charge of the Laboratory, and I turned into the leader of a small group with uncertain, but primarily propagational, duties called 'Loran Operational Research', which I don't think it ever really was. Walter Tierney remained the head of what we called the Field Service Group.

A disadvantage of the hyperbolic systems is that, except near the baseline between a pair of stations, lines of position diverge as one goes farther from the baseline and farther from the centreline. At large distances near the centreline the increase in errors is nearly proportional to the distance divided by the length of the baseline. For example, in our first Bermuda experiment the errors (for a given accuracy of time measurement) at the range of about 720 miles were about three times those along the coast of New Jersey near the 240-mile baseline. For the ground wave, the relative precision is somewhat worse than this factor because at the longer distances the signals are weaker and the effects of random noise reduce the accuracy of measurement. In Loran the skywave timing errors are large at short distances and are somewhat reduced as distance increases because fluctuations of the height of the E layer are less important for relatively flat angles of reflection. This modifies the

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theoretical 'geometrical accuracy', but it is still not satisfying to have to operate at long distances. It was good Loran practice to keep the baseline lengths at about 300 miles, although occasional pairs were set up with twice that separation. This usually required the use of directional receiving antennas at the stations to improve the signal-to-noise ratio and permit satisfactory synchronization of the signals.

18. THE BIRTH OF SS LORAN. In April 1943, soon after returning from Washington, I came to realise that the greatly superior properties of a long baseline might go far toward offsetting the inferior accuracy of our skywaves. Because a new station had been built at Bonavista, Newfoundland, but was not yet in service, I found it simple to make a trial of this idea. This was, by long odds, the easiest and most private experiment of my life. Without telling anyone what I had in mind, I telephoned to ask Bonavista to be on the air during the eight-hour silent period Loran observed at the time, and asked Fenwick Island to synchronize itself with the skywave from Newfoundland, 1150 nautical miles away. This, of course, was possible only during the night, which I spent making measurements in Cambridge. To my delight, I found that a long series of observations had an average error of only one-half mile in the line of position.

The next day I explained my experiment at the lab. It was obvious that we had a new function for Loran that would work nearly as well over land as over seawater, but with the great disadvantage that it could only be used at night. Quick discussions with the Navy, Army, and Army Air Force confirmed our opinion that a system with these advantages and limitations would not be of value to the American forces. The obvious area of interest was over Europe, where long-range bombing sorties were a major activity. The American Army Air Force did only daylight bombing, but the Royal Air Force made most of its attacks at night and therefore seemed a potential customer for our new scheme. This technique was soon coined Skywave Synchronized Loran, or, ordinarily, SS Loran.

Fortunately, during the time I was in Washington, the British 'Telecommunications Research Establishment', a rough equivalent of the Radiation Laboratory, had sent Bob Dippy to Cambridge to help us. He stayed for eight months and was a powerful force for good as he, together with Jack Williams (our most versatile circuit designer), brought new and far better techniques to the design of our third-generation transmitter timers. This development notwithstanding, Bob's greatest achievement was in forcing the mechanical design of the airborne Loran receivers into exact agreement with the British Gee sets. When the new Loran sets became available, they could be put into the shock-mounted racks of the Gee sets, and would even use identical antenna and power connectors, It was then only a few minutes work to take a Gee set out of an aircraft and substitute Loran.

With knowledge of these two developments in hand, Don Fink took off for England to sell the new idea to the Royal Air Force. His effort was so successful that it was decided to demonstrate SS Loran in the United States as soon as possible, and a team of U.S. Navy, Army Air Force, and Royal Air Force officers was designated to observe the trials and decide whether this use of Loran would justify the diversion of much-needed equipment.

19. SS LORAN TRIALS. During the summer, Walter Tierney performed miracles of organization and built us two new stations at Key West, Florida, and

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Gooseberry Falls, Minnesota. These, with the East Brewster experimental station and Montauk Point (which had been replaced, as an operating Loran station by a new one at Siasconsett, Nantucket Island) gave us a system of two separated pairs that promised good coverage of most of the eastern third of the United States. Somehow all was ready in the early fall, and the three military services prepared to make their own trials in October.

The Radiation Laboratory took no part in these trials, except for operating the ground stations, but I had the pleasure of making one preliminary flight in a B-25 aircraft before the official trials began. It happened that both pairs could not be well observed east of the Hudson River, so the first part of the flight was devoted to getting to the service area. Because the Loran equipment could measure only one line at a time, I was interested to see how quickly I could observe one line and then reset the switches and measure the second line. As we moved across New York State in the darkness I was busy practicing these observations (at about one minute intervals) and recording the times and the readings. One of the British Observers, Flying Officer Ben Halpern, was an unofficial guest on this flight. While I was busy with my numbers, I was conscious that he had taken the aeronautical chart on which we had plotted the hyperbolic lines, and was making little Xs to indicate our successive positions. He presently nudged my elbow and pointed to his latest X, which was in the centre of the yellow patch on the chart that indicated the city of Buffalo. He then pointed to the window, and I looked out to find the lights of the city all around us. This was my second big thrill in the course of the Loran work, nearly comparable to seeing the pulses move during the blimp trip.

Shortly before the official trials began, someone picked up a rumour that the Navy intended to jam our SS Loran transmissions during the trials. This was irritating, as the Navy had exhibited great opposition to the idea of giving up any of its pool of equipment to anyone, perhaps least of all to the Royal Air Force. As soon as I heard the rumour I took the train to Washington and stormed into the office of the Navy's Director of Communications to protest such a dirty trick. Admiral Redman assured me that the Navy would not think of such a thing, and said it so blandly that he fooled me completely. Two days later the trials began and the Navy promptly turned on its jammers. It was our great good fortune that our equipment withstood the jamming better than we, or anyone else, had expected that it could. As three weeks of trials all over the eastern United States showed an average error between one and two miles in spite of the jamming efforts, we had to conclude that the Navy had inadvertently done us a favour.

20. SS LORAN PLANS. As could have been predicted, the Royal Air Force representatives at the trials were in favour of using SS Loran in Europe, while the officers from the American Navy were opposed to it. This left the decision to the Army Air Force which voted in favour of its brother airmen rather than with its 'sister service'. The equipment was promptly taken out of the experimental stations and shipped to Europe and Africa. An original idea had hoped for a station near Leningrad, but this was changed to a more practical plan of a station near Aberdeen in Scotland forming a pair with one in Bizerte, Tunisia, while a second pair extended from Oran, Algeria, to Apollonia in Libya. This network gave good coverage for high-flying aircraft, roughly from central Spain to the border of the U.S.S.R., and from near the English Channel to Sicily and Northern Greece.

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21. A WARTIME WINTER IN ENGLAND. The Radiation Laboratory sent abroad a couple of engineers for each station, to assist in construction and operation, and my secretary and I were detailed to London to coordinate their activities. This turned out to be a misnomer, as my duties in London became almost entirely political, an area in which my abilities were, and are, minimal. My trip to England was on the *Queen Mary*, where fortunately I was given a bunk in the 'first class' area. The ship was crowded with 17,000 American troops, of whom a third had to sleep on the decks. Our first class cabin, that would normally hold two, had fourteen people in double-deck bunks with barely room to move between and around them. There was time to serve only two meals a day and I never learned what they were named. I do however recall that somehow the *Queen Mary* had the best kippers I ever ate, so one of the meals must have been breakfast. Among the saddest sights I ever saw was our old-time steward doing his best to give Cunard service under these trying conditions. I found that from my cabin on the Main Deck it took me three minutes or more to reach a lifeboat. I wondered about the chances of the soldiers on the seventeen decks below us, if we did ever have any trouble. The ship sailed unescorted, as was the habit of the two *Queens* that trusted their speed and routes intended to keep them away from any known concentrations of enemy submarines.

22. CHARTING TROUBLES. Unfortunately I caught the flu in the last day or two on the ship. After shivering all night in a train to London, I had to settle in a small hotel for a couple of weeks before I could attack my new duties. The first of these was difficult and embarrassing. We had cabled to London all the latest information about skywave delay. As the system did not use ground waves, these skywave corrections were included in calculating the lines of position shown on the navigators' charts. The 'standard' delay values for the night-time E layer corresponded to a layer height between 92 and 95 kilometres. Professor Appleton, like all other ionospheric experts, knew little about night-time reflections but insisted that the E layer over England was at a height of 110 kilometres, which was about the value measured anywhere by vertical incidence sounding in daytime. Professor Appleton's influence was so strong that the British charts had been calculated, on this basis and were seriously in error. Almost my first appearance in public was in a large meeting where I had to dispute this matter with Professor Appleton. This was the first of many occasions when I came to respect, and even admire, the English talent for making a foreigner's opinion appear negligible. Professor Appleton and his surrounding assistants insisted that they had experimental data to prove their case. I suspected that these suffered, as had almost all ionospheric measurements, from the 'differentiating' effect of a strong groundwave pulse, but I could not convince the meeting of the soundness of my ideas.

Fortunately, a week or two later, I was able to induce Mr. Naismith, one of Appleton's chief assistants, to make a measurement on a Loran signal from Iceland (which had just begun to operate) where the conditions were reversed, and Naismith had to measure from a weak groundwave to a strong skywave. As his ionospheric equipment lacked the gain adjusting feature of the Loran receiver, he found the differentiating effect reversed and deduced a layer height of 80 kilometres. I was then able to take a Loran receiver, with its amplitude-balance control, and demonstrate our expected value of the delay. This settled the controversy in my favour, but unhappily the beautiful charts from His Majesty's Stationery Office had to be

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scrapped. The new charts, when recomputed and redrawn, turned out to be entirely satisfactory. Because other delays would be encountered, the time lost in this episode was not to become important.

My worst political troubles soon appeared. The Royal Navy was determined that the SS Loran system should not be installed. The pretext was that the pulses would interfere with the reception of signals from small enemy ships in the North Sea; apparently an important part of the naval intelligence network, I have always felt that the British Navy was simply doing a favour for the American Navy, which was attempting to maintain control of the entire Loran program. In any case, I suffered intensely through innumerable meetings, enjoying (in a way) seeing how the naval officers could somehow pass me a cup of tea with one hand and a plate of cakes with the other and manage simultaneously to drive a knife in between my kidneys. In all of this wrangle, the Royal Air Force was of only moderate help, mostly because Group Captain Foss, who had the primary responsibility for the defence, was apparently intimidated by the forces the Navy could array against him. One of my few cheering moments came when a friend, who was a senior officer at T. R. E., said to me in disgust, 'I have just spent the entire morning trying to stiffen Group Captain Foss's weak knees!' I am sorry to speak ill of the Group Captain, because he was a very charming man who was later killed in an aircraft accident in India, but I am sure that many of our troubles were seriously compounded by his indecisive ways.

It was finally decided that Loran would stand or fall as a result of communication trials conducted in the North Sea. These were finally organized in February, 1944, and took the form of trying to copy nonsense Morse signals between a survey ship and a pair of trawlers. These vessels spent several days at various distances from the Loran station near Aberdeen and from each other, and interchanged signals with and without Loran interference. If my mood had been a little better, I would have recognized the trials as being a comedy. They consisted in exchanging groups of letters in Morse code between, say, a vessel twenty miles from the Loran station and one a hundred and fifty miles away, alternately with and without the Loran station transmitting. The ships would then move to other locations and try again. Fortunately for Loran, the receivers they used (although new) were of the old 'single circuit' design that I had not seen since my amateur days in about 1925. Their selectivity was very poor, which meant that they did not 'ring' under the impact of the Loran pulses as better receivers might have done. The result was that the pulses were heard as an annoying series of loud sharp clicks that failed to interfere seriously with the sustained tones of the Morse code signals. The radio operators also seemed very poor and did, as a matter of fact, succeed in reading correctly only about two-thirds of the nonsense groups. This ratio was so poor that it was not detectably affected by the presence or absence of the Loran pulses, even when the vessel I was on was within a mile of the 'interfering' station.

At the conclusion of the trials, the captain of our ship, perhaps not maliciously, led me into my worst political error by asking for help in composing his report. He claimed to have no clear understanding of what his orders required, and I was glad to help him, I was not totally naive, so between us we concocted a report that really drew no conclusions but exhibited the statistics of the messages received in a way that clearly showed that the Loran interference was not serious. I should, of course, have refused to have anything to do with the report because the Admiralty promptly seized upon my participation as an excuse for denouncing the entire test program. The issue

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was not resolved until after I had left England, when the British Branch of the Radiation Laboratory, a group that cooperated with the T.R.E. at Malvern in the introduction and support of the new radar devices, helped the RAF in demanding and carrying out a second series of trials. These, I am happy to say, also proved that the claim of Loran interference was not well founded, and authorization to proceed with the Loran program was finally received.

23. DELAYS AND, FINALLY, OPERATION. All of this shilly-shallying was not, as it happened, what actually delayed the introduction of SS Loran in Europe. Our most crushing blow was the discovery that the production run of airborne receivers was hopelessly deficient, as the high-voltage power supplies and leads burst out with coronal discharges (which created intolerable radio interference) at altitudes above 4000 feet, By the time better-insulated transformers and high-voltage leads were procured and fitted into the sets, the Spring season was so far advanced that it was decided to wait until the long nights came again in the fall before introducing the system.

As it turned out, SS Loran was then helpful because the Germans had at last developed airborne direction-finding equipment that permitted fighter planes to find and attack the bombers that were using the H2S microwave radars. Soon after Loran was introduced, the situation became so bad that the RAF aircraft were only allowed to turn on their radars for the ten minutes before they were due to reach their targets. The radars were thus useless as navigation aids, and many confused aircraft bombed areas that, on radar, superficially resembled the proper targets. As a final result, SS Loran was used to some extent for area bombing as well as for en-route navigation. We were pleased when we found that the accuracy of the system was good enough so that Berlin, as a major example, could be resolved into nearly a hundred 'districts'. This was by no means pinpoint bombing, but it resulted in fewer lost aircraft and aborted missions, We were convinced that the system was useful when, a month or two before the end of the European part of the war, we received copies of intercepted messages ordering various German broadcast stations to move to the Loran frequency and broadcast noise from electric fans in an attempt to jam our signals.

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Extract from Sir Arthur T. Harris's 'Despatch on War Operations' October 1945. This was Harris's official post-war report on his conduct of Bomber Command. It was not declassified until 1985. It is appended in order to throw more light on the SS Loran system.

Appendix A – Radio and Radar Navigation and Bombing Systems used in Bomber Command.

LORAN

Introduction of Loran

87. All aircraft of this Command were to have been fitted with Loran, but trials showed the Loran system to possess considerable limitations in comparison with GEE – its value as a homing aid was low, and the working frequency was very susceptible to interference. Consequently, in November 1944, the decision was amended, and the retrospective fitting of Loran was restricted to all aircraft of Nos. 5 and 8 Groups, together with the heavy aircraft of No. 100 Group.

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Operation of Loran

88. In the Loran system, the working frequency of which (2 mc/s) is much lower than the GEE frequencies (above 20 mc/s), ground stations transmit pulse signals which are received by the aircraft equipment. The pulse signals from the master and slave ground stations are synchronised, and the time difference in reception of the pulse signals are measured by the aircraft apparatus. By this means the position of the aircraft can be determined in a similar manner to that employed with the GEE system. With the Loran system, only running fixes can be obtained as against simultaneous fixes with the GEE system.

89. By virtue of the frequency, Loran gives much greater range and wider coverage than the GEE system; at night the range of reception can be increased to approximately 1400 miles by making use of skywave signals reflected from the ionosphere. In Europe, it was found that the normal ground-wave signals were of very limited operational use, but that the skywave (SS) signals from the European SS Loran chain could be received over Western and Central Europe between 6E and 14E. SS Loran was first used on operations on 11/12th November 1944, against Hamburg, and results suggested that its accuracy as a navigational aid was approximately equal to that of GEE in that area.

Enemy Interference with SS Loran

90. Loran was, however, very easy to jam; enemy interference commenced in February 1945, and was reasonably effective. The area of jamming was widespread over Northern and Western Germany. The jamming effects were overcome to some extent by modifications to the aircraft equipment, and were never as severe in heavy aircraft as in Mosquitoes. Nevertheless, the practical value of the system was seriously affected.

Results with Loran.

91. The value of SS Loran to Bomber Command can be summarised as follows:

(a) In blind bombing: its theoretical accuracy of approximately 2 miles was insufficient except for the area bombing of very large targets. Analysis of bombing results by SS Loran supported by night photographs showed that the errors achieved under operational conditions were far greater than the theoretical error and were quite unacceptable for a blind bombing device.
(b) As a navigational aid: as a long range night navigational aid it was of value, but owing to the frequency employed it was very susceptible to enemy jamming and interference, which rendered it difficult to use except by experienced crews.
(c) In homing and landing bombers: the coverage and accuracy obtained over this country was so poor that Loran was of little use for homing or landing aircraft.

92. On the other hand, Bomber Command experience with Loran was not extensive, and it is not improbable that many of its disadvantages might have been overcome. This would certainly have been tried had the war lasted longer, as its potential value as a long-range navigational aid was of the highest importance.

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