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

Walter F. Blanchard

The following is the second 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 www.internationalnavigation.org.

KEY WORDS

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

1. AIR-TRANSPORTABLE LORAN. The first few months after my return from Europe were devoted largely to a crash program that did not have my complete sympathy. The Army Air Force had convinced Division 11 of the Radiation Laboratory that it should develop a tactical aid called Air-Transportable Loran (AT Loran) that could be installed quickly in the field and, when necessary, could have its stations moved from time to time. This was wanted primarily for the China-Burma-India region. This effort required three main developments: new and semi-automatic synchronizing devices, lightweight transmitters, and simplified methods for calculating lines of position and plotting them on charts.

The synchronization was established, mostly by Jack Williams and Glenn Musselman, who had come to us after experience helping Major Armstrong in the

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development of wideband FM, by adding automatic control circuits to navigational receivers. These circuits, with some modifications to improve accuracy and stability, made it possible for a slave station to maintain its phase with relatively little operator attention. Al Pote and Dick Lawrance designed and built very pretty transmitters of about one-tenth the size and weight of the standard ones. Unfortunately, the power was reduced to half or a little less, with the result that these transmitters were never used in the field as the Air Force decided to fly in standard transmitters for the sake of the extra power. So far as equipment went, the Radiation Laboratory might as well have saved its efforts because standard transmitter timers could have been used instead of the air-transportable ones with little extra effort on the part of the Air Force.

The real success of the AT program was in the development of simple calculating methods and in the training of dozens of young Air Force officers for the field production of charts. This effort made possible the use of ten or a dozen Loran stations in Asia under circumstances that did not often permit sufficient time for the production of the regular Hydrographic Office charts. As an additional benefit, the Army Air Force could have Loran lines on its normal aeronautical charts that were drawn to a different scale and style from the Navy charts. These achievements, and others, must be credited entirely to Professor Bancroft W. Sitterly, an astronomer who had come to us from Wesleyan University. Professor Sitterly was not only an able astronomer and mathematician but also probably the best teacher I ever knew. I have always felt that he could be trusted to teach anything to anyone. During the time I was in England, 'Banny' had completed a short text called 'The Elements of Loran'. This has never been equalled as an introduction to the theory and practice of the new system. It was published as a Radiation Laboratory report in March 1944, and republished within a month by the US Navy, which distributed thousands of copies.

A difficulty with Loran resulted from the errors of the celestial methods used for determining the geographic positions of transmitters and the general inaccuracy of charts, particularly in the higher latitudes. The combination of these troubles sometimes resulted in errors of a mile or two (or occasionally half a dozen miles in the Arctic). How to 'correct' the Loran lines to agree with charts that were known to be inaccurate remained a nagging problem. The dilemma had to be resolved in favour of forcing agreement with the standard charts because the over-riding requirement was to bring the navigator to the place he was trying to find. These difficulties were increased in AT Loran operation because the baselines were generally very short and so the geometrical contributions to errors were large. Banny made a very thorough study of these effects, with the result that a few observations of discrepancies between the chart and the terrain could be used to deduce approximate corrections for the entire coverage area of a group of stations. These methods became a part of the AT Loran charting program, and Banny taught them to many Air Force officers who used them to great advantage in the last year of the war against Japan.

2. LF LORAN. For some time, I had been wondering whether the Loran technique could be used at a lower radio frequency. This possibility appeared important because of the large distances between some of the island groups in the Pacific Ocean. It seemed likely that an attack on the Japanese homeland, which it was

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generally thought would be necessary, could not be well served by Loran when operated at a frequency of two megahertz. At a lower frequency the measurement of time difference could not be as good, but the experience with SS Loran had taught us the advantages of a longer baseline, which might go far toward making up for decreased accuracy of measurement. Of primary importance was the expectation that we might be able to double the distances served by Loran. My new position gave me a seat on the Steering Committee of the Radiation Laboratory. I could, of course, contribute little to the discussions and decisions about problems and proposals for microwave radar systems. On the other hand, no one else on the Steering Committee had much understanding of Loran. It was therefore relatively easy to get approval for anything in which I believed strongly. I soon secured an allocation of about two million dollars for the work on Low Frequency Loran. I found that my efforts to spend this money wisely, in spite of the need for speed, made this the hardest administrative job I could imagine. It did, in fact, give me a permanent distaste for the 'business' side of research.

By the autumn of 1944, our commitments to AT Loran were largely completed, and I could get more and more people into the LF work. We were able to use somewhat more professional methods than time had permitted in the original Loran design. For example, the standard Loran transmitters were simple high-powered oscillators, turned on or 'modulated' by short driving pulses at the right instants. This caused no trouble because the radio-frequency period was only about half a microsecond while the typical measurement error was about twice that value. The phase of the radio frequency was therefore of no importance. At the low frequency we ultimately decided on, 180 kilohertz, the period was about five and a half microseconds or considerably more than we hoped the normal error would be. It was therefore necessary, or at least advisable, to use a crystal controlled source of the radio frequency that would be coherent with the pulse repetition frequency, so that each radio-frequency cycle could have its standard position within the pulse envelope. This would have been good engineering practice in standard Loran but could be neglected without harm in that case. The use of this technique required the development of new methods for deriving and modulating the carrier frequency.

This 'phase coherence' also made it possible to make more accurate measurements by comparing the timing of individua1 radio-frequency cycles as well as the time of arrival of the pulse envelope. Thus we could have a coarse and a fine measurement because the pulse envelope would, we hoped, be stable enough to identify a particular radio-frequency cycle (for example, the third or fourth visible one, or perhaps one at a certain fraction of the maximum height of the pulse) and take the exact reading on the rf cycle itself. This technique turned out to be difficult to handle with high reliability in a navigator's receiver, but it was used successfully from the first in holding synchronism between the transmitting stations. We believed that we were able to maintain the relative timing to about a tenth of a microsecond.

3. OTHER LF LORAN TECHNIQUES. We also made a number of other improvements in technique, such as elegant attenuators for the transmitting stations, to reduce the over-powering local signal in the receivers. These gave very great attenuation at the right instants without incurring noticeable phase shifts on the distant signals. Another important device for preventing interference from the locally generated carrier was a modulation system that guaranteed the total absence of the

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carrier between pulses. The problem of receivers for the Low Frequency Loran system was temporarily solved by the construction of small converters that would translate the low-frequency signals to one of the Standard Loran frequencies so that existing receiver-indicators could be used. This technique was possible because all of the timing characteristics were the same at the two frequencies except that at the low frequency it would be possible to have one master and two or even more slave stations operate at each recurrence rate. The converters were unfortunately manufactured with too narrow a bandwidth, which turned out to be a limiting factor in the accuracy that could be achieved.

Transmitting antennas for the low frequency constituted a major problem. We designed and ordered for use in the Pacific three 625 foot lattice towers to be used in an 'umbrella' configuration with heavy top-loading cables. These could not be produced quickly so we settled for balloon-supported wires for the experimental stations set up in the United States. The balloons were little VLA (very low altitude) models about 35 feet in length and with a diameter of 114 feet. They had stabilising fins so that the kite effect gave them some additional lift from the prevailing winds. Each balloon supported an antenna of about 1300 feet of copperweld wire with large insulators at the top and bottom. The Army Air Force provided the balloons and helium to fill them, and also assigned crews to fly them for us. We had three stations at East Brewster, Massachusetts; Cape Fear, North Carolina; and Key Largo, Florida. The balloons flew more reliably than we had dared to hope but, of course, were less than perfect. They had a maximum load-carrying capacity (in still air) of 58 pounds while our antenna wire and insulators weighed 48 pounds. It was therefore essential to keep the balloons well inflated. It surprised me that, even with the balloons flying in three quite different weather patterns, all three of them were in the air for more than ninety percent of the time. This would not have been satisfactory for operational use, where absolute reliability of the signal is highly desirable, but it was adequate for the experimental work. Our continuing attempts to keep the balloons flying as much as we could lead us into interesting quandaries. It was impossible to reel in a balloon and land it safely at a wind speed of more than 20 or 25 miles per hour, but the strength of our antenna wire was adequate to permit the balloons to fly in winds up to 40 or 50 miles per hour. Whenever a high wind was predicted we had to guess how extreme it might become and try to get the balloon down, if necessary, at the last possible moment. If we guessed wrong and the balloon was carried away, it became embarrassing because we had to alert the Air Force to send out planes to shoot the balloon down before it became too much of a hazard to aviation. There was also the risk that a trailing piece of antenna wire might short-circuit electric transmission lines in the neighbourhood but, so far as I know, this never happened.

The Brewster low-frequency station operated from late 1944 for trials of developing equipment, but the three-station network was available only from April to September 1945. We made fixed-station observations during this period at various points between Minnesota and Trinidad and the Azores with fair success, although the accuracy of the system was not as good as we had hoped it would be. The Signal Corps also made even more extensive observations at a number of places. By the time the trials ended, we believed that our steel towers would be delivered and all necessary equipment ready to ship within two months, but the end of the war brought the whole project to a quick stop.

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4. THE FIRST CYCLE MATCHING. We had no hope of getting special 'cycle-matching' receivers into production and use for many months, but Glenn Musselman and I had been working on such a device for some time and had, before the end of the war, built two or three models. These receivers were intended to make very accurate measurements at distances short enough so that the ground-wave pulse was usable in the few cycles that arrived before the stronger and later-arriving sky waves could cause interference. It was uncertain whether the 'correct' cycle could be recognized among its very similar neighbours, a matter that could be settled only by experiment. We took one of these receivers to Bermuda for tests just at the end of the war; in fact, the news of the first atomic bomb reached us as we passed through South Station on our way to New Jersey to catch an Air Force plane to Bermuda. We set up our gear quickly and made observations for a week before the Japanese surrendered, and it became our obvious duty to get home again.

These experiments were fairly successful. The station at Key Largo was about a thousand miles away and yielded very uncertain cycle identification, but the pair connecting Cape Fear with East Brewster was much better, and we could choose the right cycle about eighty percent of the time. The accuracy within the cycles was, as we had expected, about a tenth of a microsecond, which gave us a probable error of about 160 feet in the line of position through our site in Bermuda. The average position, however, disagreed with the chart by about 1200 feet. At the time it was uncertain whether this discrepancy was caused by a calibration error somewhere in our equipment or a mistake in the charted position of Bermuda. A year or two later, the question was settled in our favour, as the Hydrographic Office decided to 'move' Bermuda about a quarter of a mile on its charts. The whole experiment indicated that high-precision measurements were indeed possible but that, as we had expected, the problem of cycle identification would require more work and especially careful instrumentation. A dozen years later, this low-frequency system came back into existence as Loran C, which has now become the normal form of Loran.

Loran C differs from our experiment chiefly in using multiple pulses from each station and up to 40 times as much power as we had. These changes bring the weak ground wave up above the ambient noise to improve the reliability of cycle identification. This is also aided by the use of a lower carrier frequency (100 kilohertz) with longer cycles. I regret that, in our reports at the end of the war, we did not emphasize the low-frequency work, preferring to concentrate on the proven performance of Standard Loran, later known as Loran A. As a result, almost no one today knows that the Radiation Laboratory came very close to bringing cycle-matching to the status of a useful method, and it is often thought that Loran C was 'invented' by the Coast Guard or perhaps by the Bureau of Standards, which tried hard to stimulate interest in that system. Actually, the unique and important distinguishing features of Loran C were devised by Winslow Palmer, a very clever engineer from Sperry, who had worked closely with the Radiation Laboratory people during the war, and who continued for some years to suggest and experiment with ingenious ways to make cycle-matching reliable.

On my return to Cambridge, I found that there were only three duties left: to terminate all work as quickly as possible; to find jobs for everyone and to help write a complete account of the achievements of the Radiation Laboratory. This last effort resulted in the publication of the 'Radiation Laboratory Series' of twenty-eight volumes that recorded the many wartime developments, chiefly in microwave theory

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and practice. Dick Woodward and Alexander McKenzie (whom I had known, at least by radio, since the mountain-top days in Maine, and who had headed our divisional publications office) and I were designated the editors of the Loran book, which appeared as Volume 4 of the series. Until the formal end of the Radiation Laboratory on December 31st, everyone who could write and who had not left for other work was kept busy composing this account. As far as possible, the chapters were written by those most expert in the various subjects, but a few sections were ghost-written by the editors in an effort to give credit where it was due in the case of important contributors to the program who could not or would not write. The editors were kept on the payroll at half rate for the first months of 1946 to carry this work forward.

5. RADUX. A full-time graduate student who did his research in our group was Alan Waterman, Jr., the son of the man who had become the principal scientist in the Office of Naval Research. This coincidence no doubt caused us to be informally inspected more often than would normally have been the case, but it probably did us no harm when the time came for contract renewals. Alan found the right mathematical treatment for a problem that had been nagging at me. It had seemed for some time that using pulses for navigational aid, which required only a very slow rate of transmission of information, was a very inefficient use of the radio spectrum. Alan and I were able to show that, at least at the low radio frequencies, a continuous train of waves with a low modulation frequency could probably have its time of arrival measured with about as much precision as the long pulses of Low Frequency Loran; and would occupy far less of the spectrum.

The most important idea was that the 'relative information bandwidth' for a time-measuring aid to navigation is proportional to the speed of the vehicle divided by the velocity of light. Even at aircraft speed, a low-frequency signal would need a bandwidth of only a few hundredths of a cycle per second. We knew that the receiver bandwidth for a useful pulsed system, such as LF Loran, should be 20000 cycles per second or more; a million times greater than the theoretically needed bandwidth. We could not, in 1946, conceive how to build a receiver with no more than the theoretical bandwidth for our proposed signal, but it was easy to show that one could be built with an effective bandwidth of no more than about five cycles per second. Such a receiver would pass several thousand times less noise energy than would a receiver for Low Frequency Loran. This would mean that we could either work with smaller required transmitter power or use signals at greater distances, or both.

With these ideas in mind, and supported by what I knew or could find out about low-frequency propagation and the ambient noise levels, I proposed a new kind of aid to navigation which I named 'Radux'. This would operate with several stations sharing time at a common frequency; each one would transmit a 'burst' of signal lasting a large fraction of a second. Slight differences in the lengths of the bursts would identify each of the stations. The navigational information in each signal would reside in a train of modulation at 200 Hertz, with coherence maintained from burst to burst. Such a signal could be radiated from a practical, though large, antenna at a frequency as low as 40 or 50 kHz. I calculated that, under normal noise conditions, a few kilowatts of power should provide signals useful up to a distance of 3000 nautical miles, and estimated that the average errors might be no more than three miles, or about the same as the usual errors of celestial navigation on shipboard.

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Because one cycle in a long train of sinusoids is indistinguishable from another, Radux readings would be ambiguous in units of 2500 millionths of a second, or about 400 nautical miles or more in position. It was to be expected that no navigator would ever be 'lost' by as much as 200 miles (especially with a continuously-running aid to navigation) so this ambiguity was judged to be unimportant in an operational sense.

These ideas were disclosed in a report that was published in 1947 in classified form although, in principle, Harvard would not permit research to be done under conditions of military security. How I managed this feat I do not really understand. One of the inconveniences was that Professor Chaffee insisted upon sequestering the manuscript and spare copies of the report in his own safe. The difficulties in disseminating a classified idea from Harvard, together with a wish to get on with our unrestricted research program, resulted in my circulating the Radux report in military circles and then paying no further attention to the idea for about three years. At the end of that interval, the Navy had decided that the idea should be tested, and I came back into the navigation field. To start helping to plan an experimental program for the Radux system, I took my second trip to San Diego and began making the acquaintance of a research and development group there, with which I was to be closely associated for fifteen years or so. For most of this period I made four or five trips to the West Coast each year, for it was far easier to confer in person than to deal with the various problems in writing, especially with the classification difficulty hanging over my head. The Navy Electronics Laboratory (NEL) agreed to build and operate transmitters at San Diego and Hawaii, where reasonably adequate low frequency antennas were available and not in continuous use. I began making a receiver and one or two of different types were to be made under contract by the Sperry Corporation, where Winslow Palmer (who had worked in cooperation with MIT during the war, and who designed the first direct-reading Loran receiver) could guide the development. Win, Dick Woodward, and I, with representatives of NEL, the Naval Research Laboratory, and the Bureau of Ships were formed into a steering committee that really worked remarkably well, chiefly because it met frequently and directly with the people doing much of the design and experimentation.

6. RADUX INSTRUMENTATION AND TRIALS. In this 1950–1952 era, we at Harvard made rather cumbersome receiver-indicators of a radically new design, because the merits of cross-correlation (as opposed to simple filtering) were just becoming recognized, while the elegant techniques that later came into existence had not yet been devised. An almost miraculous property of cross-correlation (or the art of comparing an incoming signal with a noise-free replica of it produced in the receiver) is that it does not have to be done extremely well to produce nearly perfect results. This unexpected factor came to my aid, so that at Cambridge my first crude machine detected and measured the relative phase of a signal from Hawaii, although we never expected the reliable range of the system to be more than 3000 miles. The proof of this feat was somewhat amusing. My first attempt to measure the phase difference between the signals from Hawaii and San Diego resulted in a reading about 2500 microseconds away from the expected value. After tracking the signals for several hours and becoming convinced that the results were real and not a statistical accident, I telephoned San Diego to inquire whether an explanation of this result could be traced to the transmitter synchronizing method. After studying for several hours, NEL called back to say that the modulation signal had accidentally been

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inverted at the slave station with a resulting error of one-half period, or precisely 2500 microseconds. Because I had detected this difficulty in Cambridge before it had been discovered in San Diego, everyone involved in the program had to admit that I had really received the proper signals, unexpected as that might have been.

An early experiment on Radux resulted in my going to Hawaii to support (or perhaps just to watch) some trials with a new receiver made by the Naval Research Laboratory. These observations were made at Wahiawa, a village surrounded by pineapple plantations near the centre of Oahu, where the Navy had a small installation whose nature I do not recall. The experiments were neither very successful nor failures and left no great impression on my memory. Of course, I visited the Radux transmitting station at Haiku. This was established in the remains of a volcanic crater, surrounded by a horseshoe of mountains about 3000 feet high except where the wall had collapsed on the side toward the sea. The antennas were stretched across the mile and a half width of the horseshoe, with downleads from the centres of the various spans that dropped 1500 feet to the station on the floor of the crater. There was a large antenna, consisting of four long spans, that had served for Navy very-low-frequency communications, and a shorter span that had been used for communications in the 100 kilohertz region. This antenna had been loaned to the Radux project for which it was admirably suited. The whole site was the most beautiful place I saw on Oahu, as the surrounding mountains were steep and green and folded in complex patterns. The view out to the sea a mile or so away included the only islands that decorate the coast of Oahu. The only defect from a radio point of view was that the semitropical climate and heavy rainfall encouraged a luxurious growth of vines which climbed over the counterpoise network of wires that had to be used instead of a normal ground system. Chopping away this growth required frequent and unusual maintenance efforts to keep the efficiency of the antenna system at a reasonable level. The Radux work was carried on until at least 1958 but then subsided in favour of a variant system that will presently be described.

I should expose the other side of my small career as a navigator. This episode came near the end of the Radux program, before that effort evolved into the more interesting and fruitful work at the very low radio frequencies. Radux had developed and been tested to the point at which the Navy wanted independent trials of the system. These were to be conducted by operating forces and were not even to be observed by those who had developed the system. At this date there were three transmitters at Hawaii, San Diego, and near Seattle, so that good crossings were available between the lines from a rather long pair across the eastern Pacific and those from a somewhat short pair along the coast. One of the proposed exercises was to take advantage of the fact that the Radux signals could be measured a few feet below the surface of the ocean. It was planned that an aircraft and a submarine, both navigated by Radux, should rendezvous off the California coast, when the plane was supposed to fly over the submarine just as the latter came to the surface.

I happened to be in San Diego shortly before these trials were to begin, and had the opportunity to direct an aircraft in a make-believe meeting with a fictitious submarine at a given time, I decided that the most elegant way to do this was to fly out to sea along a Radux line of position that would cross the line of the other pair of stations which passed through the meeting point, and then to follow that second line to arrive at the correct point at the agreed time. It happened that we flew west crossing a strong south wind, but I had plenty of time to measure the wind speed and

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calculate how long it would take to go from our turning point to the rendezvous. Having started in good time, I found that we reached the place where we should turn south with ten minutes to spare. I then thoughtlessly ordered the pilot to circle for a few minutes to use up our surplus time and then relaxed, which I should never have done. When I returned to paying attention to our position, I found that the wind had carried us so far north during the circling that we could not fly fast enough to reach the rendezvous point in time. When we duly arrived at the right place three or four minutes late, I was promptly and none too courteously advised that I should have asked the pilot to drop a smoke float so that we could circle a fixed point. This suggestion was so simple and immediately obvious that I still feel the flush of shame as I write about it. I assume, without proof, that the professionals who carried out all the official trials made no such foolish errors. At least their conclusions were moderately complimentary. The truth was, however, that no one really wanted an aid to navigation that was less accurate than Loran, even if it did have greater range. This simple fact had much to do with the success of later developments.

7. VERY LOW FREQUENCIES. In 1953, a happy circumstance began to turn my attention toward the low and very low radio frequencies. This spectral region had seemed a never-never land for an aid to navigation because, at these frequencies, it is not possible to radiate a short pulse or even a tone of ordinary audible frequency without an antenna a large fraction of a mile high and an extent of at least several miles. It turned out that I was seriously undervaluing the measurement of the phase of a steady radio-frequency signal, even though I had become familiar with this idea in the work on Low Frequency Loran nearly a decade earlier. The fortunate event was a visit of Dr. Louis Essen to Harvard to give a Physics Colloquium, very probably about his recent work in measuring the velocity of light. Dr. Essen was a Principal Scientific Officer at the National Physical Laboratory in England. At the time, I am sure I did not know of all three of his recent successes, any one of which would have established a first-class reputation. He had controlled the size and shape of a quartz cavity, a few inches in dimensions, so accurately that in that small space he made what was at the time the most accurate measurement of the velocity of light. He had also made what remained for years the world's most constant crystal clock, using a large and very accurately machined toroid of quartz (the 'Essen Ring'). And he was at the time developing the first clock controlled by an atomic resonance in cesium; a technique that has now reached a remarkably advanced stage with clocks that approach a precision of a millionth of a second per year.

After his lecture, Dr. Essen came to see our work in the long-distance measurement of propagational factors, which depended upon reasonably good frequency control at both ends of the radio circuit. He promptly saw that this technique might easily be applied to the measurement of a standard-frequency transmission that had begun from the great Post Office radio station at Rugby, England. As a tool for research and commercial calibration within the British Isles, the frequency of a small transmitter at 60 kilohertz had been derived from an Essen Ring oscillator. This frequency was monitored and corrected through observation at the National Physical Laboratory as well as at the Greenwich Observatory and at the Post Office headquarters in London. This signal was transmitted for only an hour each day and at rather low power, but Dr. Essen thought our techniques might be sensitive enough to permit good measurement of the frequency at Harvard.

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8. 60 KHZ FREQUENCY COMPARISONS. Almost as soon as Dr. Essen left us, we began the construction of a special receiver for this 60 kHz signal. This became the first of a long line of receivers for phase-stable transmissions. We gradually perfected our technique for limiting the output level of a signal, rather than using automatic gain control, and we learned to separate the two functions of amplification and establishment of frequency selectivity, so that the output phase became independent of the strength of the signal or of the noise interference. Even this first carrier-frequency receiver, however, performed what seemed to us to be a miracle, considering the weakness of the input signal. The new instrument produced an output so free of contamination that its phase could be measured with a standard deviation of no more than four or five degrees of arc, or a quarter of a microsecond in the apparent time of arrival. These observations opened a surprising set of possibilities. The most immediately important was that during the one-hour transmission of the MSF signal (as it was identified by its call letters, the SF representing standard frequency) we could measure its frequency to one part in ten billion, or to the equivalent of about ten millionths of a second per day in the rate of our clock with respect to Rugby.

To appreciate the importance of this observation, it is necessary to review the possibilities that existed in 1953 for long-distance inter-comparison of clocks. The standard of time was then, as it had been throughout recorded history, the rotation of the earth. This standard is not as constant as one might wish, because the rate is slowed gradually by tidal friction and other factors, and because the rotation is made irregular by the varied push or drag exerted by the action of the winds and the currents in the ocean and in all probability by similar currents in the molten interior of the earth. There is also an annual variation caused by the accumulation of snow on the mountains in the northern winter, which slows the rotation until melting returns some weight toward sea level in the springtime. The total of these effects, and doubtless others, is to make the time as established by earth rotation vary through a tenth of a second or so during the year. There are also occasional sporadic variations, possibly related to earthquakes that help to make the prediction of the rate of rotation of the earth difficult and inaccurate.

Another limitation arises from the fact that time, defined in this way, must ultimately be measured by the passage of stars across the meridian and this, because of the slowness of the passage and of ordinary errors of observation, cannot be determined with an error less than about five milliseconds. At this level of error, the rate of rotation of the earth can only be measured to about one part in ten million from one day (or night) to the next. It is also unfortunate that the sending of time signals from one part of the world to another was, before the age of satellites, subject to errors of at least the same magnitude. For transmission at low frequencies, as said above, one cannot produce a short pulse that could be measured with precision. The same limitation applies for transmission by telephone or telegraph wire, except in the case of the coaxial cable, which is not yet available except between a limited number of terminals. For high frequency propagation, except at very short distances, irregularities are caused by fluctuations in the height of the ionized layers that reflect the signals. In short, errors in frequency measurement (which is not the same as time measurement but is at least closely related to it) were not often, at long distances, smaller than a few parts in one hundred million. Thus, surprisingly, we found ourselves with an apparently reliable radio method of inter-comparing frequencies

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from distant places with an accuracy between a hundred and a thousand times better than that which was known before. It was immediately obvious that there would be a great increase in the average accuracy of frequency measurement if the signal from MSF could be tracked continuously from one day to the next.

9. PROBLEMS IN INTER-COMPARISON OF TIME. It is an inconvenient fact that the phase of a simple sinusoidal signal is highly ambiguous, as has been noted earlier. With a signal at 60 kHz, these ambiguities were separated by only $16\frac{2}{3}$ microseconds, while the apparent precision of a part in ten billion corresponded to a probable drift of more than half that amount in a period of twenty-four hours. Thus the straight line in frequency, determined in one hour, could be projected forward to the succeeding day with a chance, but no certainty, of identifying the correct cycle on the second day. The reliability of such an estimate would depend almost entirely upon the question of whether the oscillators (or clocks) at both ends of the radio circuit could be relied upon to maintain constant rates during the many hours when they were not being compared. With the limited knowledge of precision oscillators available in 1953, a long silent period seemed to make the measurement too uncertain to be worth considering.

My first step toward better day-to-day precision was, of course, to communicate with Dr. Essen and presently with Mr H. T. Mitchell, the Post Office executive most concerned with control of frequency, to see whether the MSF transmissions could be extended in time. It was ultimately possible to get occasional 24-hour transmissions, but so rarely were they available that determination of the reliability of a continuous measurement was not attainable. When I was sure that this effort was not becoming very productive, I began a more serious campaign. Rugby was famous (aside from football) not for the weak MSF transmission but for the 16 kHz signal whose call letters were GBR. This transmitter was maintained and operated by the Post Office but used to a great extent for Naval communications on a nearly worldwide basis. The power radiated was many times greater than that used by MSF, and the frequency was better adapted to long-range communication. This station, incidentally, was so well known that it was one of the few places I had visited during my first stop in England in 1936.

My first attempts to induce the Post Office to derive the frequency of GBR from the same source used for MSF met with no success. It did not appear to be difficult to provide the necessary equipment for this change, but the Post Office realized quite clearly that, for their limited purpose of supplying a standard frequency useful in the British Isles, the MSF signal was better than that of GBR would be, because for a given accuracy the higher frequency could allow faster measurements. When these attempts showed no indication that they would ever be successful, I attacked the problem through Mr J. A. Ratcliffe, whom I had met as far back as my 1936 visit and who had become the director of the Radio Research Board's experimental station at Slough. My chief argument was that there was to be a total eclipse of the sun in 1954 whose track would cross the path of the signal from England to New England. I made the suggestion, with or without serious belief, that the behaviour of the phase of the GBR signal at such a low frequency might cast new light upon the structure of the ionosphere. To my complete surprise and pleasure, Mr Ratcliffe's resulting pressure on the Post Office was completely successful, as the necessary frequency-synthesizing

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equipment was soon prepared and installed. I am sorry to have to admit that the eclipse effect, although I duly observed it, was so minor that it proved nothing in particular. The GBR frequency-deriving equipment, however, since it had been officially made and installed by the Post Office, remained in existence and provided a very useful service until the present day, I believe, or certainly at least until my retirement.

10. THE FIRST VLF PHASE MEASUREMENTS. At this lower frequency and with transmission that was continuous except for an hour or two each day, there was no longer a question of cycle-slipping or ambiguity. It quickly became clear that, although there was a change of more than 30 microseconds in the time taken by the propagation between day and night, at any chosen hour of the day the phase of the received signal repeated from day to day with excellent accuracy. This was the first of many observations that showed a propagational stability greater than that usually attained by our oscillators. This aspect of the newly discovered precision of frequency measurement made many people doubtful that I could be attaining the results I claimed. As I have suggested above, extremely careful control of the temperature of our oscillators and of the voltages applied to them were an important part of the program. Another was the fact that I was not required to judge the stability of propagation from continuous data. I could select periods of a few days, or occasionally a fortnight or so, during which there was no evidence of anomalies in the operation of our oscillators. At these selected times, the day-to-day repeatability of the transmission time seemed to be in the neighbourhood of two microseconds in time. This estimate turned out to be approximately correct, but it was to be several years before I could demonstrate this fact by an unambiguous experiment.

In the interim, my carefully selected data seemed to show that there was no characteristic average variation of the time of arrival of the GBR signal throughout either the daytime or the nighttime hours. The distinctly different transmission times at these periods were connected by nearly straight lines during the sunrise and sunset periods, which naturally lasted for five hours or so; the length of time it takes a sunrise or sunset to cross the Atlantic Ocean. The result was a somewhat trapezoidal pattern with sunrise and sunset slopes that varied during the year, as the angle changed between the sunrise or sunset line and the great circle representing the transmission path. It later turned out that the nearly uniform time of transmission during the hours of full daylight was an unusual condition peculiar to the GBR frequency and the length and location of the transmission path. It is much more characteristic of low-frequency propagation in general that the daytime level (of the time of arrival) is depressed to some degree as the sun gradually moves higher in the sky, so that there is a broad minimum near the time when it is noon at the middle of the transmission path.

It did not take long to discover that the sharpest corner of the GBR diurnal trapezoid when sunrise impinged upon the eastern end of the transmission path, occurred about 30 minutes before ground-level sunrise at the point below the nominal 'first reflection point' 500 miles or so away from the eastern end of the path. The other three corners were less sharp, especially the one at the end of the sunset period when the effect of recombination made the return to a full nighttime level somewhat slow. The variation of these observations as the date changed led to some interesting conclusions about the detachment and re-attachment of free electrons from and to

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neutral ions, but these theoretical deductions were perhaps less important than the fact that, with a little experience, the diurnal variations in the time of transmission across the Atlantic could be predicted to within a few microseconds.

11. THE BEGINNING OF RADUX-OMEGA. These observations and conclusions led me in 1955 to write two papers that were to be of future importance. One was a general announcement of the new technique. It surveyed the background and the results of observation, and gave something of a list of uses to which this mechanism might become helpful. Emphasis was placed upon the new possibilities for inter-comparison of clocks at widely-separated locations. Applications to radio aids to navigation were reasonably obvious, at least to me, but were only casually mentioned in the paper because the whole subject of navigation aids was still held under military security. I did not choose to violate the spirit of this decision (with which I violently disagreed) even though such a newly recognized mechanism could not, of course, be classified in advance of discovery.

The second paper took the form of a letter report, which could be classified by the recipient, to the Chief of Naval Research. This study, called 'An Improved Radio Aid to Navigation', reviewed the work on Radux to that date and called attention to the weaknesses of that system: especially the limited range that could be served and the magnitude of the typical errors that were already being recognized as rather larger than were likely to be accepted by maritime navigators. The report proposed the addition of very-low-frequency signals to be radiated by the same antennas. These signals at, say 10 kHz would be compared in a receiver that also accepted the Radux signals at 40 or 50 kHz. This composite system would exhibit the sensitivity and reliability of the 10 kHz signals, while the increased ambiguity (because the proposed carrier-frequency period was fifty times smaller than at the Radux measurement frequency of 200 hertz) would, it was hoped, be resolved by the Radux signals. The net result should, I believed, be an increase of perhaps five times in the accuracy of navigational measurements.

These suggestions were quickly endorsed by the Navy and some efforts to this end were soon added to the Radux program at the Navy Electronics Laboratory and elsewhere. These developments envisaged a composite system called Radux-Omega. The Omega part was a name I chose by way of implying that it occupied the furthest end of the radio wavelength spectrum. More recent naval attempts to improve communication with submerged submarines may have made this connotation invalid, although the communication studies have been concealed by security classification and bedevilled by political considerations (including, of all things, concern about the health of earthworms) until it is hard to tell how successful they may be. Now, if I am asked for a reason for the name, I am more likely to suggest that it indicates 'Pierce's absolutely last effort in radio aids to navigation'. I don't suppose I have ever protested much at the idea that the name connotes 'the last word', although I realize as well as anyone that a permanent solution to the navigation problem, if there ever is one, will probably come from artificial satellites, if and when costs decrease to make that kind of solution generally available.

12. PRE-OMEGA STUDIES. One of the early pre-Omega attempts at Harvard was the reception of a signal from San Diego at a frequency of ten thousand cycles per second. This frequency is clearly audible to the human ear, as was shown

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by an early cartoon sketched by one of the men at San Diego, labelled 'Omega Receiver, Mark I, Mod I', which showed a circuit diagram consisting of a pair of head-phones connected between an antenna and ground. My reception was done satisfactorily, using the new cross-correlation technique, although it was possible to radiate only thirty watts from the relatively small antenna supported by three 625-foot antenna towers at Chollas Heights. In fact, I was delighted to find that, with lengthy integration, I could measure the field strength to a decibel and the relative time of arrival to a microsecond after the radiated power had been reduced to one watt. Of course no one seriously proposed a useful system that did not have thousands of times this power level, but the success of the experiment showed clearly the great advantages of the very low frequency in range and reliability.

At the time of these studies, we were still recording the time of arrival of the pulse transmitted once per second from the standard-frequency station WWV, as part of our continuing study of high frequency propagation. WWV had not then moved from the Washington, DC area to its later home at Fort Collins, Colorado. Because of the short path from the transmitter to Cambridge, we were able to record a good first-hop E layer signal at five MHz in the daytime hours. This signal, like the Loran ones of the preceding decade, was far more stable when reflected from the E layer than from the higher F layer. As a result, we could measure the relative time of arrival to about five microseconds a much greater error than that at 60 kHz. Even so, measurement of the drift over 24 hours gave us a standard deviation of the day's average frequency of about one part in ten billion. We thus suddenly found ourselves in the position of being able to inter-compare the British and American versions of standard frequency with an accuracy about a thousand times greater than had been possible before. Unfortunately, explanation of this fact and its importance will require a little digression into the methods of determination of time and frequency that were available in 1955.

The distinction between time and frequency can be made clear by thinking of an ordinary tall clock. If it is so regulated that its pendulum beats exactly 86,400 times in a mean solar day, its frequency is correct. In horological terms it is usual to say that its rate is right. This exact adjustment of frequency, or period, has however nothing to do with the time indicated by the hands of the clock, except that this indication will drift slowly if the rate is only a little in error. The hands must be set, separately from adjustment of the rate, so that the clock indicates substantial agreement with some external standard such as a time signal. Alternatively, it is possible to leave the hands alone and to account for the clock error by saying, for example, that it has been gaining 1·3 seconds a day since it was correct on the 23rd of September. In either case, a means of establishing the correct time is needed. This inter-comparison must be repeated at intervals that depend upon the stability of the clock and, of course, upon the required precision of the time measurement. It will be remembered, for example, that in the early days of Loran it was necessary for the slave station to readjust the 'hands' of its 'clock' as often as once or twice a minute in order to maintain the required precision to a millionth of a second of the 'clock' time with respect to that at the master station.

13. MEASUREMENT OF TIME. It is thus seen that frequency can be measured relative to some standard without requiring the measurement of the error in the indication of time. In many cases that need only frequency or rate, an

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instrument may have no mechanism for indicating time. A clock, however, must have such a mechanism. It is therefore technically incorrect to call an oscillator a clock, although it is often done. For the thousands of years when the rotation of the earth was the standard, an instant of time had to be determined by the apparent motion of some heavenly body, usually the sun or a star. Until a few centuries ago, the sundial was the most usual measuring device. As clocks developed and became more accurate, a more precise measurement was required to set them correctly. This soon took the form of a transit instrument, eventually using a telescope with illuminated cross-hairs, to determine the instant at which any one of a number of standard 'time stars' crossed the meridian of the observer. When corrected for the known positions of the stars in the sky and the observer on the earth, this method gave sidereal time, which the astronomers define as 'the hour angle of the vernal equinox'. This time can be converted into the ordinary mean solar time by noting that, because the earth completes an orbit around the sun in exactly one year, the solar or sun time that we customarily use loses two hours in the average month, or a little less than four minutes per day as compared with sidereal time.

This digression is important because, even with today's best instruments, it is impossible to measure the transits of stars in a single night with a standard deviation less than about five milliseconds. This permits a daily measurement of the rate of a clock with a standard deviation of about one or two parts in ten million. It is only a coincidence that the error in the transmission of a telegraphic time signal, or one sent by radio, is of about the same order. These considerations make it easy to see that for generations the whole concept of time implied that it could not be standardized with an accuracy greater than about one part in ten million. This usually did not worry scientists much because the other fundamental physical properties of mass, length, and electrical charge could scarcely be measured with even that relative accuracy. There was thus a strong feeling that nothing could be measured more accurately than a part in a few million. This made it hard for the specialists in time to accept the idea of a large increase in the precision with which relative frequency could be distributed. In fact it was two years or more before I could convince the director of the time service at the Naval Observatory that my measurement of phase was not something having no relation to his measurement of time. As soon as the first people in one or two time-standard laboratories were convinced, however, I found myself operating a frequency comparison service. This took the form of daily comparisons of standard-frequency stations (beginning with GBR and MSF in England and WWV in the United States) that were reported weekly or monthly to the National Physical Laboratory in England and to the Bureau of Standards in the United States. As time went on and the method became more widely appreciated, others were added to the list of stations and more organizations to the list of recipients. It was 1964 before there were enough others using my method, or other new ones, so that I could discontinue this routine service with a very noticeable sense of relief.

14. TRANSATLANTIC CESIUM CLOCK COMPARISONS. The earliest and most responsive customer for my observations of frequency was, naturally, Dr. Essen. The third of his recent triumphs, that I mentioned above, was the development of a cesium beam resonator in terms of which he could calibrate the frequency of one of his ring oscillators. Dr. Essen used this device for a passive

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resonator. So far as I know, Dr. Zacharius at MIT was the first to use such a resonator with a servomechanism to control the frequency of a crystal oscillator. This device was, for a time, put on the market under the trade name 'Atomichron'. It must be regarded as the first of the modern atomic clocks. By late 1956, one of these was in operation, at a station in central New York State that was operated by the Air Force for testing a proposed radio aid to navigation called Navaglobe that operated at a carrier frequency of 95 kHz.

The existence of this station gave me an opportunity to collaborate with Dr. Essen in the first international inter-comparison of cesium-controlled oscillators of two kinds of construction. The measurement network was a little complicated. The MSF signal from Rugby was received at Teddington and had its frequency measured by Dr. Essen. The same signal reached Cambridge, Massachusetts, and was measured against my crystal oscillator. At more or less the same time, my crystal was rated against the signal from Camden, New York, which was also measured there in terms of the Atomichron. By inter-comparison of all these measurements it was possible, in principle, to subtract out all factors except the difference in frequency between the two cesium devices. Comparisons of this kind were made on a half a dozen or a dozen days a month on which all the necessary measurements had been successfully made. The average of all of these measurements in six months indicated a difference of 4 parts in 10^{10} between the Atomichron and Dr. Essen's resonator, with a standard deviation also equal to 4 parts in 10^{10}. Dr. Essen was quite pleased with this demonstration of the good agreement between the frequencies of the two devices because it was about as close as his estimate of their absolute accuracy. I was, however, very disappointed in the magnitude of the standard deviation as I had published an estimate of 2 in 10^{11} for the available precision of the daily transatlantic inter-comparison, and I hated to announce a standard deviation 20 times larger than I felt it should be. It was reasonably obvious that the source of the trouble, if trouble there were, was because of the unfortunate necessity for making many of the inter-comparisons at different hours of the day. Because this could not be avoided there was every possibility that the crystal oscillators were varying diurnally, and that these variations, with the normal errors of the many measurements, were dominant over the propagational errors that I had believed I had estimated. I was, of course, unable to prove this assumption and was left with the unhappy possibility that I had made an unjustifiable guess.

In spite of my disappointment with our results, I joined Dr. Essen and his assistant in writing a short paper for Nature to report our results. I then began to try to find a way to perform the same comparison under circumstances that would clearly prove the true stability of the propagation. As I should have expected, the British Post Office showed no interest in the idea of acquiring an Atomichron (necessary because there was as yet no British equivalent) and using such an untried device to control the frequencies of their transmissions.

15. THE THREE-ATOMICHRON EXPERIMENT. By 1959, the situation had changed considerably. The Signal Corps of the US Army had kindly provided us with an Atomichron on loan and was ready to take another pair to England. With the cooperation of the Post Office, these were installed at Banbury, within easy groundwave range of Rugby. Recording equipment that I had provided then made records using one of the Atomichrons at Banbury that were identical to

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mine at Cambridge except for the use of a different Atomichron and for the long propagation path across the Atlantic. The results were delightful. Daily values of phase were analysed for frequency differences. Studied for about a month in September and October, these showed an average difference of -4 in 10^{11} with a standard deviation of 2·6 in 10^{11}. Simultaneous inter-comparisons between the 'operating' and 'standby' Atomichrons at Banbury showed a difference of $+6{\cdot}0$ in 10^{11} with a standard deviation of 1·7 in 10^{11}. It thus seemed that not even all of the transatlantic errors ought to be charged to the propagation link. I was greatly relieved, as the propagational stability that I had been reporting for four or five years could no longer be doubted.

My studies of frequency comparisons later had a most curious effect, possibly at the suggestion of Dr. Essen. At about the time that my routine reports ended, I was surprised to be notified that I had been appointed by the International Bureau of Weights and Measures to a membership on the Consultative Committee for the Definition of the Second. This appointment was certainly inappropriate. In the previous years I had learned some useful things about the distribution of accurate standard frequency, and perhaps even time, but I had no qualifications that fitted me to judge how the standard of time should be defined. On the other hand, membership in the Committee provided such a pretty feather for my cap that I could not bear to decline the appointment. The other members were senior officers of standards laboratories of a dozen nations; about the same number of representatives of astronomical observatories with a responsibility for time, such as Greenwich and Neuchatel and three gentlemen from Innsbruck, Zagreb, and Milan who, like me, were known as Membres Nominativement Designes. I had already learned that the most recent revision of the standard of time represented a total change of philosophy as compared with the old standard that had, in effect, obtained ever since the clock had become a more useful timekeeper than a sundial. This change, in 1956, certified final official recognition of the fact that the rotation of the earth is a poor standard. As noted above, it varies by a tenth of a second or so with seasonal variables and it also has sporadic changes of unknown origin. It is an inconvenience, although one that can be compensated, that the elliptical form of the earth's orbit makes noon, or the moment when the sun is highest in the sky, vary as much as plus or minus 16 minutes with respect to the imaginary 'mean Sun' that is supposed to move uniformly in average agreement with the true sun throughout a year. The old definition of a second was 1/86,400th of a mean solar day. This became awkward as soon as new kinds of clocks began to have a consistency exceeding that of the earth. The ability to measure a frequency had become more exact than the stability of the standard.

16. DRACO. Having followed the fortunes of frequency measurement for about a decade, I must go back in time to describe my first efforts to devise a practical aid to navigation using the very low radio frequencies. It may have been as early as my days in the Radiation Laboratory when I began to talk about what I then called the 'phaseless phase' navigation method. Discussion of this topic was purely as a tutorial device, because it seemed clearly impossible that such a method could exist in the real world. The idea was a development from a possibility that later became known as 'rho-rho' or distance measurement. This earlier 'impossible' concept suggested that a receiver should carry a perfect oscillator against which it could observe a signal derived from an equally perfect oscillator and transmitted from a known position.

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Changes in distance from the transmitter would then be measured by changes in the observed phase of the signal, and integration of these phase changes would (if the velocity of propagation were accurately known) always indicate the distance of the receiver from the transmitter, provided that an initial phase had been measured at a known starting point. Without this calibration at a known place, the method would be of little use, and it was obviously necessary that the signal should be tracked continuously after this definition of the starting point. It was clearly impossible in the 1940s that such a rho-rho system could be successful, as the best oscillators, even when operated under carefully controlled conditions in the laboratory, would surely have errors corresponding to a fictitious rate of at least a mile an hour, while it would obviously be prohibitively expensive, even if possible, to attain such accuracy in a moving vehicle.

The 'phaseless phase' concept suggested a way of eliminating oscillator drift as a problem. It proposed that two transmitters (controlled by good oscillators that were kept coherent by frequent or continuous monitoring) should radiate two different frequencies whose only limitation was that it should not be too complex a problem accurately to derive both frequencies from the same oscillator. A vehicle would then carry separate receivers and phase trackers for these two frequencies. Each of these phase-change summations could then be multiplied by an appropriate factor to reduce the measurement to that which would have been observed at some convenient reference frequency. The measurement would then be one of the total change in the difference of the distances from the two transmitters. This line of position could, like that in any other hyperbolic system, be crossed by one or more other lines produced by observation of other stations. Like the rho-rho idea, the phaseless-phase mechanism would not yield absolute position, but only the distance and direction travelled since the navigator had been at a known place.

17. DRACO PHILOSOPHY. A little thought reveals the fact that this kind of mechanism is nothing but a form of dead-reckoning tracer, with the peculiar and important property that the errors with which position is carried forward are limited to those of measurement and propagation. In conventional dead reckoning, on the other hand, the errors can grow without limit, more or less in proportion to the length of time since the position was determined. This difference is of philosophical interest, and it justified some discussion of this method even when there was no known mechanism for reducing it to practice. After the unexpected discovery of the great predictability of the very-low-frequency propagation velocity, the phaseless-phase idea suddenly seemed capable of implementation. One of the powerful arguments for this system stemmed from the fact that there were a number of wireless telegraph stations operating in the region from 15 to 25 or 30 kilohertz. Some of these had quite high power and all of them, in those days, used on-and-off keying in Morse code. It was clear that if some of these stations could be 'taught' to keep their signals flowing even if they had nothing to say (and if their operating frequencies could be made coherent that is, in exact mathematical ratio) one or more ready-made navigational pairs could be set up without great expense. This we set out to do.

In promoting such an idea, it was obviously necessary to have a name for the system. For this I chose 'Draco' because, as I have always believed, if a name has as yet no denotation it should at least have some connotation. In my mind, at least, the two first letters symbolized the dead-reckoning tracer that I recognized the system to

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be, while the word draco (Latin for dragon or serpent) suggested the train of sine waves that was the output waveform of each station. Now that I come to write about it, I realize that it is possible that I never before explained this origin.

The circumstances for this experiment were auspicious. We knew that GBR operated about 23 hours per day and, except for a maintenance period, seldom fell silent for more than a few seconds at a time. It would certainly provide a useful signal over long distances. With this as a master station, it only remained to build the machinery that would use the Rugby signal to lock a good oscillator from which the operating frequency of some other station could be derived. Thus an experiment could be performed using Rugby as a master station. Its operators would need to take no action, although it would certainly be a courtesy to advise them of what would be going on. By the time we had decided to test this idea, the discouraging news was received that the Rugby station, or at least the 16 kilohertz transmitter, would fall silent for several months or more to permit reconstruction of its antenna. This was a complex structure of wire cables and insulators supported by a dozen or more 820-foot towers. During the long silent interval, the important communication function would be transferred to a stand-by station at Criggion, Wales. This station would transmit at much the same schedule but at a slightly less favourable frequency and with only one-sixth of the radiated power of Rugby.

I did not feel that this change could be allowed to delay our experiment, so construction of equipment to receive and use the new frequency was begun. The most convenient station to make a 'slave' was at a place called Jim Creek in the town of Arlington, not the one near Washington DC, but somewhat north of Seattle, Washington, in the foothills of the Cascade Mountains. This was, at the time, the Navy's most powerful VLF transmitter. Its acceptance trials had shown a radiated power of 330 kilowatts, but it had been found more satisfactory to run the station at the 200-kilowatt level, as this allowed less frequent interruptions and replacement of components. The great circle path from Criggion to Jim Creek is a little less than 5000 miles in length, but it arcs northward to cross the Greenland ice-cap and then traverse a thousand miles or more of the permafrost region of Canada, north and west of Hudson's Bay. Both kinds of frozen terrain were believed (and later proved) to cause great attenuation of low-frequency signals. We had, however, confidence in our improving techniques for signal integration and we felt that we could deal with extremely weak radio signals.

18. PROBLEMS WITH THE EXPERIMENT. There were two major problems to be faced in the design of our equipment. One was to assure maximum stability in the many necessary selective circuits, so that changes in temperature or in the characteristics of vacuum tubes would not cause phase shifts that would vary with time. This was required as there was no definable reference phase. It was therefore necessary to arrange things so that, whatever relation an output phase had with respect to an incoming signal, it could be expected to remain constant, at least over the length of time it would take a navigator to go from one known place to another. This requirement was met, as far as possible, by designing frequency-adding or frequency-dividing circuits that required a minimum of filtering to produce the necessary purity of tone. This work had three parts: planning so that a needed frequency was removed as far as possible from all unwanted by-products; development of circuits that added or subtracted to produce the desired sum or

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difference frequency at a power level much higher than that of any other frequency present; and use of lattice filters with bandwidths as broad as circumstances permitted rather than sharply-tuned selective circuits that would be more sensitive to changes in the temperature of components. The final equipment, which filled a large relay rack, was certainly the most carefully designed and built that we ever made.

The second major problem was really a series of problems, all of which stemmed from the tremendous 'local' signal that would be received on the station premises from the Jim Creek transmitter itself, as compared with an extremely weak signal to be expected from Criggion. Before going to the Seattle area, we had no idea how weak this signal might be, so we prepared for the worst and made the equipment as sensitive as we possibly could. It was well that we did so, for the signal turned out to be only about a microvolt per metre, which may be compared with the hundred or more microvolts per metre commonly required for a useful Morse code signal. The Jim Creek signal was at least as big an obstacle as expected. It was found to be about 4000 volts per metre at the best location we could find on the station property. There was thus a voltage difference of about four billion to one in favour of the unwanted signal in our receiver. The ratio of the power inputs was even more fantastic, being over a quintillion times greater for the unwanted signal. In technical terms, it would be necessary to provide enough frequency selectivity to reduce the unwanted signal by more than 200 decibels, because that signal would have to be reduced to negligibility with respect to the wanted one. Some idea of the meaning of this ratio can be gained by realizing that it is more than twice the logarithmic difference between the loudest noise the ear can tolerate and the most delicate sound that can be heard at all.

The problem of attaining adequate selectivity was made somewhat more difficult for the signal from Criggion, as that frequency was closer to that of Jim Creek than was the Rugby signal's. Criggion operated at 19·6 kHz and Jim Creek at 18·6 kHz. There was thus a difference of only 1000 cycles per second between the wanted and unwanted signals in the receiver. It is perhaps more appropriate to say that the ratio of the two frequencies was almost the equivalent of one semitone in the musical scale. To attain the great degree of selectivity that was required, it would be necessary to use, at least in the first tuned circuits, coils having a figure of merit (called Q) of at least 250 or 300. In 1956, the best available Q in physically small inductors was of the order of 200 or more, but was attained by the use of windings on toroidal iron cores. It would be impossible to use such inductors in the earlier tuned circuits in our receiver because they became non-linear when the iron was magnetically saturated by too great a current in the coil. This would lead to a change in inductance and a phase shift in the tuned circuit. This possibility was not tolerable because it would mean that the receiver output would have a different phase when the Jim Creek 'key' was down than when it was up. In other words, the phase of the observed Criggion signal would vary, depending upon the 'duty-cycle' of Jim Creek; or, crudely, upon whether it was transmitting mostly dots or mostly dashes at a given time. The solution for this problem was to construct air-cored inductors that had adequate Q without iron. By using Litz (or Litzendraht) wire composed of dozens or hundreds of fine wires, the radio-frequency resistance could be kept low, while making the coils larger increased the figure of merit. After some experimentation, we learned to make multi-layer solenoids that were six or more inches long and about four inches in diameter. To protect these inductors from outside, possibly non-linear, influences, each of the coils was installed in the middle of a copper box containing something more than a cubic

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foot of air. These shielding boxes were provided with electric heaters and thermostats and were thermally insulated from the environment, so that temperature changes and consequent phase shifts could be minimized. By making the first two tuned circuits in the receiver with these cumbersome components, it was possible to reduce the 'local' signal to the point where it could then be dealt with by more conventional methods.

Another difficulty lay in the necessity, or at least the advisability, for compensating the diurnal phase shifts on the long path from Wales to the State of Washington. It was obviously more desirable to let the Jim Creek signal have a constant timing in agreement with Criggion in an absolute sense, rather than to allow it to follow the diurnal fluctuations of the signal from Criggion as received at Jim Creek. This meant that corrections should be applied to the observed Criggion phase to make it simulate a signal that would have been observed if there were no diurnal variations in the time of propagation. This problem had two parts: computation of the phase changes to be expected; and design of a device that would correct the observed phase by the expected amount. The second part was achieved by building a mechanism that would drive a phase shifter at one of two adjustable rates. This was actuated by two 'alarm clocks' that could be set to the minute, and there was an adjustable limit to the rotation of the phase shifter. At the predicted time of sunrise in Wales, the phase shifter would start rotating at a selected rate, so that at the time of sunrise at Jim Creek the phase of the local comparison signal would have decreased by the expected amount, which was intended to exactly balance the phase change in the incoming signal during the sunrise interval. At the time of sunset in Wales, the reverse process would begin, the only difference being in the rate of change established by a second adjustable speed changer. This sunset variation would continue until the original nighttime phase level had been restored. It was, of course, necessary to arrange for conditions, such as midsummer on the long east-west path, when there was no complete night over the whole transmission path. Under these circumstances, the diurnal correction pattern would be triangular, rather than trapezoidal, and one of the stable levels would never be reached. It would be necessary to change the starting times, the rates, and the total day-to-night variation each two or three days because of the changing length of daylight. I did my best to pre-compute all of these variables and give Jim Creek a table of what all the settings should be for various dates. Fortunately, with care and study these calculations could be corrected, because a continuous record of the observed phase track at Jim Creek would show the result of errors in the assumptions by its departure from linearity. In principle, therefore, the tables could gradually be made more exact.

19. CONCEALMENT OF THE DRACO SYSTEM. I felt that one other complication ought to be built into the equipment we made. It was clear that any navigation aid that concentrated its service near the United States would have its best accuracy in or near that area. It could therefore be more useful to an enemy than would an equivalent system operated from his part of the world. With a passive system, like Draco, any number of navigators could use its service at the simple cost of building appropriate receivers. The best defence I could think of to counter this possible threat was to conceal the fact that such a system was in operation. To do this, at least for a time, we made a phase shifter that was driven by a synchronous motor through change gears that permitted selection of any of a large number of rates. These

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were adjusted so that they could be varied in tiny steps up to a frequency difference of about 3 parts in 10^8, or enough to guarantee an error of fifteen or twenty miles an hour. One of these was used at the slave transmitter and a similar device was used to provide a compensating offset in the frequency of the oscillator used in each navigator's receiver.

The total effect was as follows. After much effort a signal from Criggion would be used to stabilize the frequency from which the Jim Creek transmitting frequency would be derived. The transmitter frequency would then be set off from the nominal frequency by an amount that in ordinary terms was very small, but that corresponded to a large error in the coherence of the two signals when considered as an aid to navigation. At the navigator's receiver an opposite offset would permit operation exactly as if the offsets did not exist, because the frequency differences inserted were mathematically exact. Any, desired pre-planned code could be used to schedule the frequency offsets. These might be changed a number of times a day so that an untutored observer who inter-compared the signals would only discover that they seemed to be derived from crystal oscillators that aged at independent rates, as all oscillators did. An 'error', for example, could be arranged to increase gradually and then be brought back suddenly, to simulate the occasional correction of a frequency in terms of some standard. For our experiment it was not necessary to use complex coding tables. As I have described above, in this era there was a considerable difference between the standard frequency signals of Great Britain, and America, as they were adjusted according to differing philosophies. It was easy therefore to operate the oscillator at Jim Creek in coherence with the Criggion signal, and then set off the Jim Creek frequency by an amount that brought it into approximate agreement with the American standard. It was not necessary to do this with frequent changes, as the two standards differed chiefly with the season of the year, and because no station kept to its assigned frequency very exactly in those days.

20. JIM CREEK. When the equipment for all these things had been completed, it was the summer of 1956. All of the gear, with measuring instruments, spare oscillators and small parts, was loaded into a panel truck and driven to Seattle by a pair of technicians, while I flew to San Diego for a more or less routine meeting and thence to Seattle. On arrival at Jim Creek we found that the Navy's permission for us to work there must have been unusually wholehearted, for the first activity was to show us the coffee mess and invite us to use it as our own. The man in technical charge was the civilian chief engineer, Lou Riley, who had been a Chief Petty Officer in the Navy when sent there to help with the installation of the transmitter. Lou loved the Cascades and presently retired from the Navy to come back as a civilian so that he could not be ordered to go anywhere else. One of his favourite pastimes was taking groups of Boy Scouts on camping trips in the mountains. He always kept fishing gear in his car and usually stopped at every stream he passed to see if he could quickly catch a trout. Lou was the heart and soul of the station, and I used to wonder whether it could be kept in operation without him. Later I heard that he was offered a corresponding position at the really super-powered station at Cutler, Maine, but he declined this plum so that he could stay in the Cascades.

The size of the station was a revelation to me. The antenna consisted of ten copperweld cables 8000 feet long strung across a narrow valley between two ridges 3000 feet high. The centres of these strands were connected to downleads that were

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brought together into a sort of transmission line that carried them back to the transmitter building. The antenna was actually separated into two halves, each excited by its own transmitter, so that in case of accident or the need for maintenance the station could operate at half-power for a time. The transmitter building was a concrete box a hundred feet or so square without windows and with access to the area of the transmitter itself only by elevator from below.

As befitted a station with a transmitter whose component sections were mostly of the order of cubes ten feet on a side, the elevator was so big that we simply drove our truck into it for the ride up to the operating level. We spent two or three days setting up our equipment and erecting a whip antenna for receiving the signal from Criggion. As the transmitter building was the only possible site for our gear in the immediate vicinity, the whip was installed on the roof about fifty feet from the 'lead-in' which carried about 700 amperes of radio-frequency current. It was in setting up this antenna that we discovered the falsity of the common statement that 'rf doesn't shock; it simply produces surface burns'. This may be the truth for small quantities as high-frequency currents tend to flow only on the surface of a conductor, but it fails by a wide margin to explain the behaviour of large currents at such a low frequency as Jim Creek's. Our rough calibration of the field strength near the transmitter lead-in was as follows: a bit of metal up to five or six inches long (such as a screwdriver or a pair of pliers) stings like a nettle; rubber gloves are a necessity for handling metal objects a foot or two long; and touching a conductor five or six feet long can knock one down.

21. TRYING TO FIND THE CRIGGION SIGNAL. The minor pain we encountered in setting up this antenna was wasted, as we never detected a signal from Criggion at that site. Two or three days passed while we searched for the signal. This was a slow process as the only indication of its presence would be the tracking behaviour of our servos over a half-hour or more. The search was complicated by the fact that our oscillators had completely lost calibration in the trip across the country, while the signal from WWV, which we had expected to use to find the correct frequency, was received so poorly as to be essentially useless. At that date, the only real access to precise frequency was through the signals transmitted for the purpose by WWV from near Washington DC, and also from WWVH in Hawaii. Neither of these signals was received well enough for the very accurate calibration we had to make. It therefore was a painful and erratic search, moving our oscillator in small steps through what we hoped was an adequate frequency range, and watching the servo record for symptoms of proper tracking. Frequently random behaviour would look real for a few minutes and lead us to erroneous corrections because our patience was under such strain. In the end we gave up trying to operate at the transmitter site.

In the search for an alternate we found that there was a little microwave hut at the top of one of the mountains that supported the large antenna. This hut received signals, from Seattle I suppose, and relayed them down to the station in a telephone cable. The hut was not much more than a mile in a direct line from the transmitter, but was reached by seven miles of mountain road. The hut was near the southwestern-most stub tower that supported one of the strands of the big antenna. This tower was about 200 feet high and made an ideal point to which to tie a fairly long wire receiving antenna. At this site we set up our gear in an odd corner and, without too much

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difficulty, detected the Criggion signal. Without any proof, I still believe that our failure down below was due to the weakness of the Criggion signal at the bottom of the narrow valley – the signal was none too strong at the top of the mountain. We estimated the one microvolt per metre that I mentioned above from the degree of sluggishness of our servos. In other words, if the signal had been stronger the tracking would have been faster or better.

At this stage, excitement took over. We had, at the top of the mountain, a frequency equal to that assigned to Jim Creek and locked to the British standard of frequency with a precision of about a part in ten billion. Down in the valley was the station operating at the same frequency but perhaps in error by a part in ten thousand. And between the two was a telephone cable with pairs to spare. With only a quick look with an oscilloscope to see that the 'local' signal accidentally induced in the cable was not too formidable, and not waiting to provide proper balancing and segregation for our circuit, we connected our output to the line and telephoned down to Lou to try our new signal source on the transmitter. To our delight, it took over the frequency control without a quiver, although a terrible oscillation might have ensued with a megawatt of power trying to feed back into our telephone line. The Criggion-Jim Creek Draco pair was instantly in full operation.

Within a day or two the people in Cambridge reported that the stabilized pair was operating about as expected. Little was left to do except to continue operation for a while to assure ourselves that the long-term statistics were reasonably satisfactory. Like all high-powered stations, Jim Creek had elaborate arrangements for shutting down the transmitter quickly in case of accidents of various kinds. A part of these precautions is spoken of as 'dumping', because as the high-voltage power supply is shut off the large capacitors that smooth the rectified voltages are short-circuited by heavy bars as a precaution against melting down of components or possible electrocution of personnel. Such a dump is momentarily violent, as tremendous voltages and currents are suddenly discharged, and there is often a considerable reaction on the power supply for the station. I had the misfortune to be, I suppose, perhaps, the only person who ever dumped a super-power transmitter by tripping over an extension cord and pulling it out of a 120-volt wall outlet. This statement is amusing but not quite accurate. When I stumbled over the power cord for our equipment in the microwave hut, the transmitter simply fell dead for a moment. Without stopping to think I plugged the cord in again and the transient in our own equipment activated the safety mechanisms and produced the dump. I realised what I had done as the lights failed momentarily, but by then it was too late for anything but apologies. Fortunately, no one belonging to the station made any appreciable fuss over this incident, but my face was very red when next we went down to the transmitter building.

At about this time the Navy's frequency monitoring station in San Francisco sent Jim Creek a notice that it was two or three cycles off from its assigned frequency. Actually the station had never since its construction been so accurately controlled. In fact, it must have been, at this date, except for WWV, the American station with the highest frequency stability ever known. We spent a little time in checking that everything was operating correctly and even more time in trying to decide how to explain the situation in a message to San Francisco. Before this problem could be solved, a second message arrived from the monitoring station, admitting that the first one had been sent in error. Lou Riley relieved his feelings and delighted me by sending

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back a message saying, 'The next time you think our frequency is wrong, don't call us; call WWV'.

22. SECURITY. After this trip I had no further written communication with Jim Creek. The commander of the Naval Station at Seattle was obsessed by fear that communists were watching his every move. He instructed me, on my duty call, that I was to have no communication with Jim Creek until he had set up a security system that would make it 'safe'. In my view, nothing was required. I was deeply concerned about security and was never likely to speak carelessly without adequate camouflage. As it happened, 'Leo the Lion', the commander in question, never did anything further in the matter. I therefore kept my communications to a minimum, chiefly in the form of answering phone calls from Lou when he needed a little technical advice. I have therefore very little written evidence of the whole experimental program.

We continued to observe the stabilized signals for a year or more. The results were generally satisfactory with errors not often larger than a mile or two in the line of position through Cambridge. We gradually improved the prediction methods that were required for the production of charts and tables. In the mean time, the Navy had contracted with Pickard and Burns, the company that Dick Woodward had joined, to produce two sets of synchronizing equipment for transmitting stations and a few receivers for testing purposes. These instruments were built with much more professional finish than our experimental gear, but I doubt that they could have worked better. One set of transmitter equipment was used to replace ours at Jim Creek (although that station kept our gear there for several years for standby and checking purposes) and a second set was installed at a naval station at Annapolis, Maryland, after GBR had resumed operation at Rugby, England. This was a convenient but perhaps unfortunate choice because, as seen from Annapolis, the directions to the Rugby-Jim Creek pair of stations subtended less than 120 degrees. It followed that (neglecting a few hundred miles near Annapolis, within which distance propagational behaviour might be uncertain) the triplet of stations could be expected to give magnificent service in Hudson's Bay and the Arctic Ocean. On the other hand, operation south of Annapolis would be characterized, because of unfavourable geometrical factors, by large errors in latitude. The measurements would be good in longitude because in that coordinate the result would depend almost entirely upon the long pair, between Rugby and Jim Creek. The Navy had, of course, no intention of experimenting with Draco in the Arctic. From this time onward, tests were carried out entirely within the Navy, although outlines of most, if not all, of the data were sent to me for my own interpretation.

23. DRACO OBSERVATIONS. At the time of these experiments, the Navy operated a vessel, the *Compass Island*, entirely for the testing of various aids to navigation. She carried many conventional navigation devices, including the inertial, and also had a computer room, which attempted to deduce the most probable position at all times, and to assign apparent errors to the devices under test. The ship made two trips for which the data were of interest to me. On the first, she proceeded south from New York to a point near the Bahamas and then made a series of runs east and west at separations of two or three miles, followed by a similar series north and south. I never heard what the purpose of these runs may have been, but they provided an ideal testing pattern as the ship spent quite a lot of time moving in an area

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about forty miles square. On the passage south, the *Compass Island* passed a point where Annapolis and Jim Creek had the same bearing from the ship. Because of this, and because of the short distance from Annapolis, no useful information could be had except from the long pair. Happily, this part of the course was within a few miles of being exactly south. I therefore assumed the ship's reported latitude to be correct and solved the observed readings for longitude only. These points, for two and a half days, could conveniently be plotted in a single diagram that showed not more than one or two points in a hundred to be more than a mile east or west of the ship's reported position.

Near the Bahamas, it was possible to use both pairs but, as said above, with much larger errors in the north-south direction. As a result the east-west runs looked terrible on my graph, as the random variations made the tracks fluctuate so much that they frequently overlapped for a few miles. Conversely, the north-south runs looked beautiful on my map as the east-west scatter was quite small. When I took the trouble to plot the errors of Draco in their distance and direction from the centre of a diagram there was, of course, no noticeable difference between the diagrams for the two sets of runs. In both cases the probable ellipses that included almost all of the observed points showed maximum errors of a little less than a mile in longitude but errors four times larger in latitude. At least, the shape and orientation of the ellipses agreed with the theory, indicating that with suitably located stations the standard deviation of a fix should be only about a nautical mile.

24. MORE DRACO RESULTS. The other passage that interested me was when the *Compass Island* came through the western end of the Mediterranean Sea and across the Atlantic Ocean. In this case, the position determined by Draco after the ship left the Gates of Hercules diverged more and more from the supposed position for many hours, until suddenly the computer room report moved sideways about fifteen miles in a fifteen minute interval and came to rest within a mile of the Draco position. There was nothing included with the data that gave a clue to explain why this happened. I therefore wrote the ship's navigating officer to ask if he could explain this peculiarity. After searching the records he produced an answer. At the time of the sudden anomaly, the ship had come within sight of Madeira!

In the neighbourhood of Bermuda there was another instance when, to my prejudiced eye, Draco seemed to have performed more consistently than had whatever the *Compass Island* used for its primary navigational information. I cannot cite this so exactly, as the evidence was less clear and, to tell the truth, I have forgotten the precise details. Because all of this effort was under military security classification, I no longer have any of the documents in the case. As a result of these experiments, there was some small Naval interest in Draco. Unfortunately, however, the only serious office was the one planning navigational methods for the Polaris submarines. Their interest was little less than silly because, as explained above, Draco could tell nothing unless the tracking was continuous. Although signals at the Draco frequencies could be received to some small depth under water, it was impossible to hear them at the depths where the submarines would wish to spend most of their time. This meant that when the submarine was not near the surface it would be necessary to switch to artificial signals from a stable oscillator and advance the position by dead reckoning in terms of the ship's heading and speed (both of which were quite accurately known in the Polaris vessels) so that the real signal would be found near

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the estimated phase when the submarine again came near the surface. The Polaris ships were to have good oscillators and computers that could carry out this procedure, but there was (or should have been) grave doubt about how long such a period without signals could be. There was an interval when the new Polaris submarines had space reserved for a Draco receiver, but elementary common sense ultimately prevailed and the idea was finally abandoned.

During the period of uncertainty I was to be found in the curious position of arguing against the use of my own idea but on this subject there is no evidence that the Navy paid any attention to me. The Draco technique came to life again about 1970. By that time, the US Navy had installed cesium-controlled oscillators in all of its very-low-frequency stations, so that they transmitted what were in effect standard frequencies. These are now very exact and are monitored by the Naval Observatory and occasionally corrected when necessary. These stations and a few in other countries now provide the kind of service we had between Criggion and Jim Creek, without the need for any special equipment in the transmitting stations. There is, of course, no attempt at concealment and this service is useful to anyone. This new form of Draco, usually called simply VLF navigation, has been promoted energetically by the Global Navigation Company of California. Receivers are relatively inexpensive and are favoured by the owners of small aircraft as the method gives very fine results for flight durations of only a few hours, during which diurnal variations in propagation need not be compensated. Some manufacturers are now offering this same VLF service as an adjunct to automatic Omega receivers – but this subject brings me too far ahead of my story.

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