A BACKGROUND OF TECHNOLOGY AT A HIGH LEVEL IS NECESSARY FOR A COUNTRY TO PROSPER

Professor Henry A. Whale

Bio:

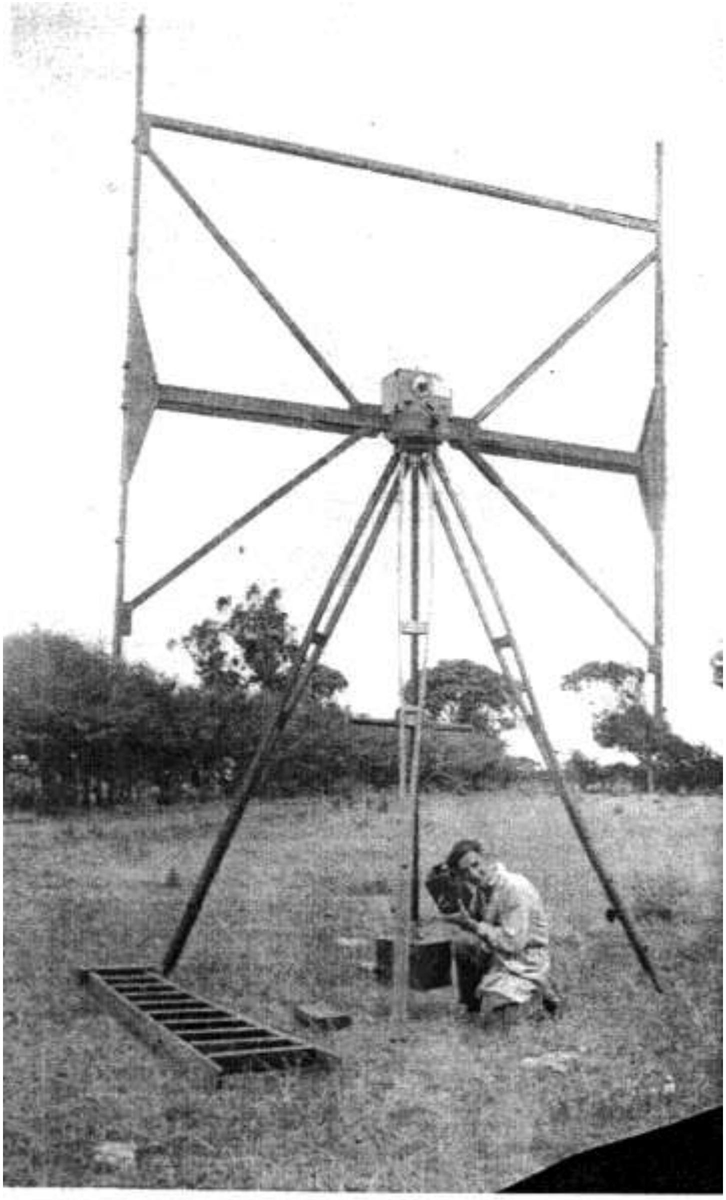
Henry Whale was born in England, and graduated from Auckland University College in 1943. He completed his Ph.D at Cambridge in 1950.He commenced his professional duties with the NZ DSIR in 1944 on electronic development and radio communication studies. From 1947 he worked in the Radio Section of the Cavendish Laboratory on the design, construction and operation of an ionosonde and antenna system for the study of the fine structure of the E Region of the ionosphere. In 1951, he returned to New Zealand to found the Seagrove Radio Research Station where he held the post of Director. In 1959 he took up a research position with NASA at the Wallops Island Launch Site, Virginia. In 1962 he returned to Seagrove, and in 1966 was appointed to a Personal Chair in the Faculty of Science.

From 1969 until his retirement from the University of Auckland in 1982, he held a variety of teaching and research posts in the United States, Germany and New Zealand. He was a foundation member of the Communications Advisory Council and has served on a number of bodies concerned with Communications and Electronics

In 1984, Professor Whale was awarded the IEEE Centennial Medal, has continued his consulting work, and completed and published his research work while at the University.

This is an historical year and accordingly I will try to present a kind of historical perspective on electronics over the last 50 years or so as I have seen it. My own entry into this field was really not by design but by circumstances. It became obvious in the early stages of the war that the outcome would depend largely on the use of techniques such as radar and the content of the last year of the science degree course (in 1942) was largely made up of radio physics, whether one liked it or not. The first approach to Britain's need for technologists had been to send many young scientists to England - unfortunately a number of these were captured on the way and spent the rest of the war in POW camps. It was then decided that the work could be diversified and the DSIR and the Universities set up groups in Wellington, Auckland and Christchurch to supply technological backup to the armed forces. New Zealand actually had a good radio industry in place before the war and supplied most of the Pacific with radio sets. One's career in those days was determined by an office in Wellington called the Manpower Officer and I was directed into the group at Auckland that was then called the Auckland Technical Development Branch (ATDB). The projects were many and varied but a few that come to mind were the radio controlled speed boat that was designed to be filled with explosives and directed from afar to attack enemy warships, massive smoke-generating machines for camouflage purposes, a series of timed underwater explosives that would generate tidal waves to wash the enemy off low-lying Pacific islands and other less exciting ones such as making test equipment for our own forces since it had become impossible to get this from overseas suppliers. Overseas communications was a continuing problem and there was an ongoing programme on direction finding of shortwave signals that had been started some years previously.

To put this into perspective, a telegraph cable was laid to Australia in 1876, the Pacific Telegraph Cable that connected Vancouver, Fanning Island, Fiji, Norfolk Island, Australia and New Zealand was laid in 1902 and enhanced in 1912 and, in fact, operated continuously until the Compac Telephone Cable was laid in 1962-63. The first voice links (telephone) were by radio and were to Australia (1930), Great Britain (1931) and to US in 1945. There were some International Aeronautical Radio teleprinter links established in 1940. Thus, the links were quite few and extremely dependent on the vagaries of the ionosphere. In fact, the Europe - New Zealand link was probably the most difficult to operate that there was, since it was essentially the least predictable. Even this work was hazardous. The direction finder consisted of two vertical antennas rotatable on a beam about 10 feet long mounted on a tripod about 8 feet high. Our site, which had to be flat and clear, was in a paddock near Point England. The operator stood on a ladder by the tripod and this was, of course, an irresistible draw to a curious bull who just happened to be in the paddock on one occasion. Radio technicians, especially those from England, are not known for their familiarity with livestock and this one responded by throwing the apple in his pocket at the bull which promptly ate it and came back for more. Eventually, his calls for help were heard and he was rescued.



1943: The direction-finder at Point England, complete with ladder.

Part of the work at ATDB was on ground wave communications, some on the adaptation of the ZC 1 (a NZ made portable transceiver) to FM and some on the problems of communicating through heavy rain forest i.e. jungle. It was in this connection that four of us were seconded to the Operational Research Section of the Australian Army in order to make some definitive measurements of how attenuation in jungle varied with frequency. This work was carried out in New Guinea. At the end of the war, I completed the MSc degree and was able to obtain an 1851 Exhibition Research Studentship that enabled me to study at the University of Cambridge for a PhD. This award (^300 per year) was enough to cover tuition fees and living in Cambridge but there was the problem of actually getting there and by this time I had a wife and family. So began my first venture into the commercial field. Like most people in electronics at that time I had an interest in hi-fi and since there was a shortage of good equipment, I moonlighted in the hi-fi field by producing a few very good units. The systems were all based on record players and the problem was, as always, that it was extremely difficult to obtain players of adequate performance. However, by sticking to the niche market of quality rather than quantity, I accumulated enough to cover the boat fares to England in 1947. My research at the Cavendish Laboratory in Cambridge was concerned with the behaviour of the low-level ionosphere. There were, at that time, two main groups in the Radio Section, one working on radio astronomy and the other on ionospheric/upper atmosphere problems. Radio astronomy was, of course, the glamour subject but the available student spaces were taken by the time that I arrived in Cambridge. Now I would like to back-track a little since this talk is largely about Research and Development (R and D) and what I have learnt about it. Although, as a scientist, I have a loathing of dogmas and unprovable hypotheses as a basis for argument, there is one statement that I hold as being self-evident and that is that the future well-being of this country as of most others depends on our being able to produce and sell special technologically based products on the world market. One particular reason for this is the cost penalty of transport on unprocessed raw materials unless they happen to be particularly rare. If we lump these desirable industries together as being knowledge based we have a reasonable idea of where we should be heading. There are other industries that are important, of course, including the service industry that caters for tourism and agriculture, forestry etc but my main point is that we must add value to the greatest extent possible. Technologically, it is not enough for us to be up with the competition, we have to be ahead of it in as many fields as possible. This of course immediately implies that we have to make a substantial investment in R and D. Although this outlook has become fairly well accepted in recent years, the question still remains as to what kind of R and D is best and who should carry it out.There are many different types of R and D projects, some carried out by companies, some by government organizations, some by educational institutions, some by individuals.

Some are undertaken in order to overcome problems that have arisen with a product, some to attempt to produce a product superior to the competition, some to attempt to develop a new type of product and some just to take advantage of funding that may have arisen in a particular field. This sounds like the chaos that it is but some broad distinctions can be made.

Probably the greatest overall effort goes into improving the sales potential of an existing product - motorcars, computers, soap powders, radios and television sets etc. These are fields where the boundaries are fairly well defined so that specialists can be trained and, provided enough people put in enough effort, advances can be made. The countries that do well in this are those that are good at organizing and good at working together in largish groups. The Japanese have been particularly successful since they work so very well together. But eventually a situation of diminishing returns develops. In all these cases, the general form of the required end product is known in advance.

The incentive for doing any of this work is often to improve corporate profits but there are, on occasions, the much greater incentive of national survival as occurred during the last war. Probably the most intense R and D of this nature was the development of atomic weapons. This achieved its objective but really because of the great difficulty of the operation, was not typical of R and D directed to a specific objective in that it spawned a whole range of related products, many of which have formed the basis of whole new industries. Again, to quote a war-time example, substantial R and D groups were set up both in US, UK and in Germany to enhance the capability of radar systems. This turned out to be another war within a war since each side was faced with the continuing necessity of producing either some method of nullifying the advances in performance achieved by the opposition or introducing improvements that outstripped theirs. The scope of the efforts widened to include navigation systems so that, when the objective of obtaining superior systems was achieved there were again a wide range of products that were able to be developed from what had been accomplished along the way. This was different from the atomic bomb project in that the end product was only vaguely recognized at the beginning of the operation. In a similar way the general objective of code breaking was recognized but the fact that this would contribute substantially to the development of electronic digital computers was certainly not recognized.

We thus have examples of two different types of R and D - some were directed to a specific end and the others were ongoing, in that the ends changed with time - but all had the side effect of producing the foundations for new products. The common factors were that they were of substantial size (and, therefore needed to be supported by substantial funding - public money in these cases) and that the problems were very difficult. The latter seems to be to be the important point - if the problem is difficult enough then people who can cope with such difficulties are required and then there is a high probability that innovative (and therefore valuable) techniques will be forthcoming. But where did these people come from? The first few were obtained from universities and similar institutions and from the appropriate industries, where they existed. These people started things going but, at the same time, organized their own training groups - it was found that, in the right environment, it is being able to learn rather than previous training that made it possible to produce the essential technicians etc that were needed. But this still took time.

The lesson that it was valuable to a country to have a supply of technologically trained people available and that such people made innovative contributions that led to new industries amongst other things was brought home by wartime experiences and there was a strong move toward setting up organizations for encouraging the development of science and technology in the immediate post war period. The survivors of this "enlightened" period in New Zealand are, for example, DSE (Defence Science Establishment) and many of the branches of DSIR. The general approach was that these groups should undertake some long-term difficult problems while at the same time offering solutions to the short-term problems that arose in industry and defence.

However, the system did not really produce the benefits in the way of generating new industries that had been anticipated. One problem in New Zealand is that we were doing too well out of our primary produce for us to have to be vitally concerned with what we called secondary industries. The US was more concerned with industry and they really had an advantage in not having much of a system of government research organizations. The fact that they had developed a system of fairly large state universities and quite well endowed private universities and a tradition of co-operation between universities and industry meant that the necessary research centres were catered for by the universities. The wartime projects were, for example, closely associated with the appropriate universities. These universities themselves had long standing associations with industry so that the necessary link between research and manufacturing was in place. Although some groups that were set up in NZ during the war (ATDB in Auckland and a similar one in Christchurch) were located with the universities, this union was severed in the post-war period. There were, in fact, long and often heated discussions about the roles of the university and DSIR in research - the conclusion that universities should do pure research and the government organizations should do applied research (or useless and useful if you like) did not really help much. A better distinction between the two types of research, although the boundary is fuzzy, can probably be made on the basis of the length of time that is required. A reasonable crossover point is three years actual time although there is the inherent rule that any non-trivial research or development project takes 3 times as long as anticipated.

The situation in New Zealand in the 30s and 40s was that our universities were small and the tradition of research other than theoretical work was almost non-existent. One reason was that a university department usually consisted of 2 or 3 people and there was no funding in the system for anything but teaching and laboratory materials. As I have already pointed out, for research to be effective or productive, it must be adequately difficult and this really means that it must be part of an ongoing advanced programme.

There were no PhD degrees offered by the University of New Zealand (we were all colleges of the UNZ) and the topics for MSc degrees seemed to arise mostly from what the appropriate professor had jotted down in his little black book as "likely research topics" during the previous year (come to think of it, this point may not have changed much!). On the other hand, many of the senior staff had worked in very reputable overseas research organizations. In fact, it was these people who were able to create the groups that worked so well during the war. The group that I was in at Auckland was concerned with radio communications and this was an area of considerable concern since as I have said NZ happens to be very unfavourably situated when it comes to radio links with Europe. The organizations concerned with long-distance (as opposed to local) radio were the NZPO (for telephones), NZBS (for overseas broadcasting) and Civil Aviation (for links to air terminals etc).

Professor P W Burbidge had been a director of the wartime technical group at Auckland and he decided that it could be to NZ's advantage if a similar group could be formed to at least attempt to solve the problems associated with our communications situation. With great persistence and dedication he persuaded NZPO, New Zealand Broadcasting Service (later to become BCNZ) and DSIR to support such an undertaking. This took him until 1950 to achieve.

To return now to my time in Cambridge. In order to carry out investigations on the lower ionosphere I built a pulse transmitter that operated in the 0.5 to 3.0 MHz region with a tracking receiver and an antenna that was mounted on two masts that had originally been used for the original British coastal radar system. In fact, almost a11 of the equipment and supplies were war surplus. In those days there was a standard price for war surplus equipment whether it was valves, cables or almost anything else. The price was 15s per cwt. As an aside, the driving mechanism for the large radio telescope that was constructed at Jodrell Bank near Manchester at that time started life as a gun turret drive on a battleship. It is also interesting that some of the best small radio telescope dishes were radar dishes liberated from the Germans. I spent a great deal of time in designing a suitable vertical looking antenna carrying out all of the calculations with a slide rule and it was only later that I found that there was a mathematical laboratory that had some very interesting machines available. In the meantime, I made my first lasting impression on the Cavendish Laboratory - once my equipment was operating it was set to run continuously unattended but, like all valve equipment with high voltages around, one night it burnt itself up. Fortunately the building itself survived but since that time all the Cavendish Laboratory field huts have been insured against fire. To return to the Mathematics Laboratory - I found that there was a course on numerical calculating using hand operated calculating machines and this seemed well worth attending.

In doing so I discovered that this lab also had a mechanical machine called a differential analyser that would be useful in analysing some of the results that I was obtaining in my own research. It was seldom used because the major effort was on completing a digital electronic machine (2000 valves + innumerable valve diodes) called EDSAC (stands for Electronic Digital Storage Automatic Calculator).

I also discovered that, although the EDSAC was not fully operational, it could be used more or less satisfactorily at night and found a Canadian member of the group (J P Stanley) who was interested in using it and in instructing me in its use. Programming was in machine language.

EDSAC was a direct descendant of the code-breaking machines that were developed at Bletchley during the war and it was one of the people working with EDSAC that went from Cambridge to work for Lyons chain of restaurants and produced LEO (Lyons Electronic Office). This was probably the first electronic digital machine specifically designed for commercial applications. Most of the people concerned with EDSAC were mainly interested in its technical performance but there were some who were more interested in using it for obtaining results.

Stanley and I organized a system so that one of us was preparing the next input tape (5 hole punched paper tape) while the other was running the machine but we had to let other users have their turn. The result was a batch queuing system that consisted of a row of paper clips to carry the tapes and hung on a wire in the computer room.

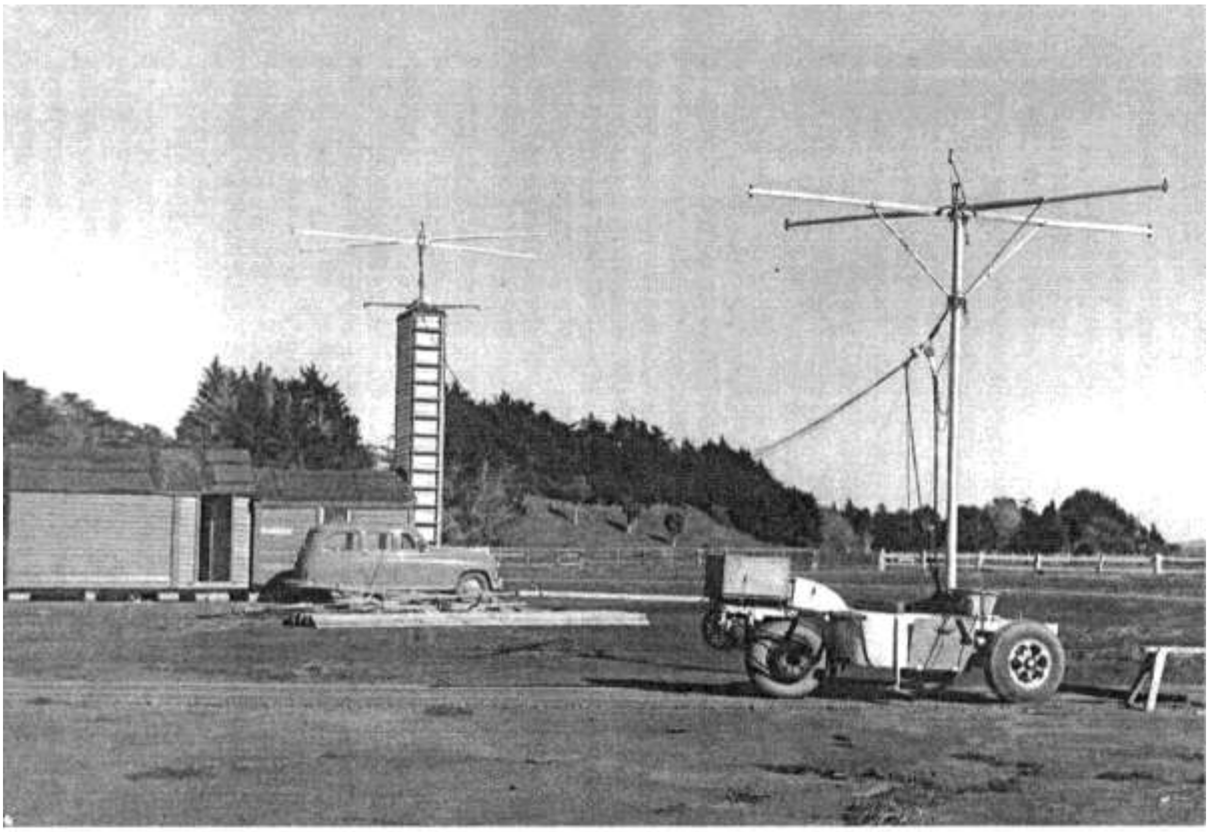
I completed my PhD work towards the end of 1950 but there were certainly no worries about finding employment in those days. None of my contemporaries in the Cavendish Lab who, incidentally included some of the group who were captured by the Germans on their way to England in 1942, intended to return to New Zealand although there were a few Australians who went back there. I was aware of the project that Prof Burbidge had initiated and although I was, in fact, offered a position in Cambridge, I felt that it deserved support. Although funding for the scheme was not secure in the long run, largely because government departments were not able to make long-term commitments, it seemed to me to be the basis of a very desirable change in the NZ tradition. I had found that the Cavendish Laboratory, which is one of the world's most prestigious organizations for carrying out basic or pure research had very good links with industry and although I realized that an important ingredient of such an establishment was the tradition that had been built up over many years, I felt that the same standards could be achieved elsewhere. Certainly, the project had one of the criteria that I have mentioned previously i.e. the problems were adequately difficult so that there was a high probability that other side benefits would evolve during their solving.

When it became known that I was returning to New Zealand it was revealed to me that, underneath the stair in the Mathematical Laboratory was an early model of the differential analyser and since, apparently, I was the only one who had used this machine recently, the model was offered to me. We paid ^50 and I spent some time in packing it for shipment. This is the machine that eventually found its way into the

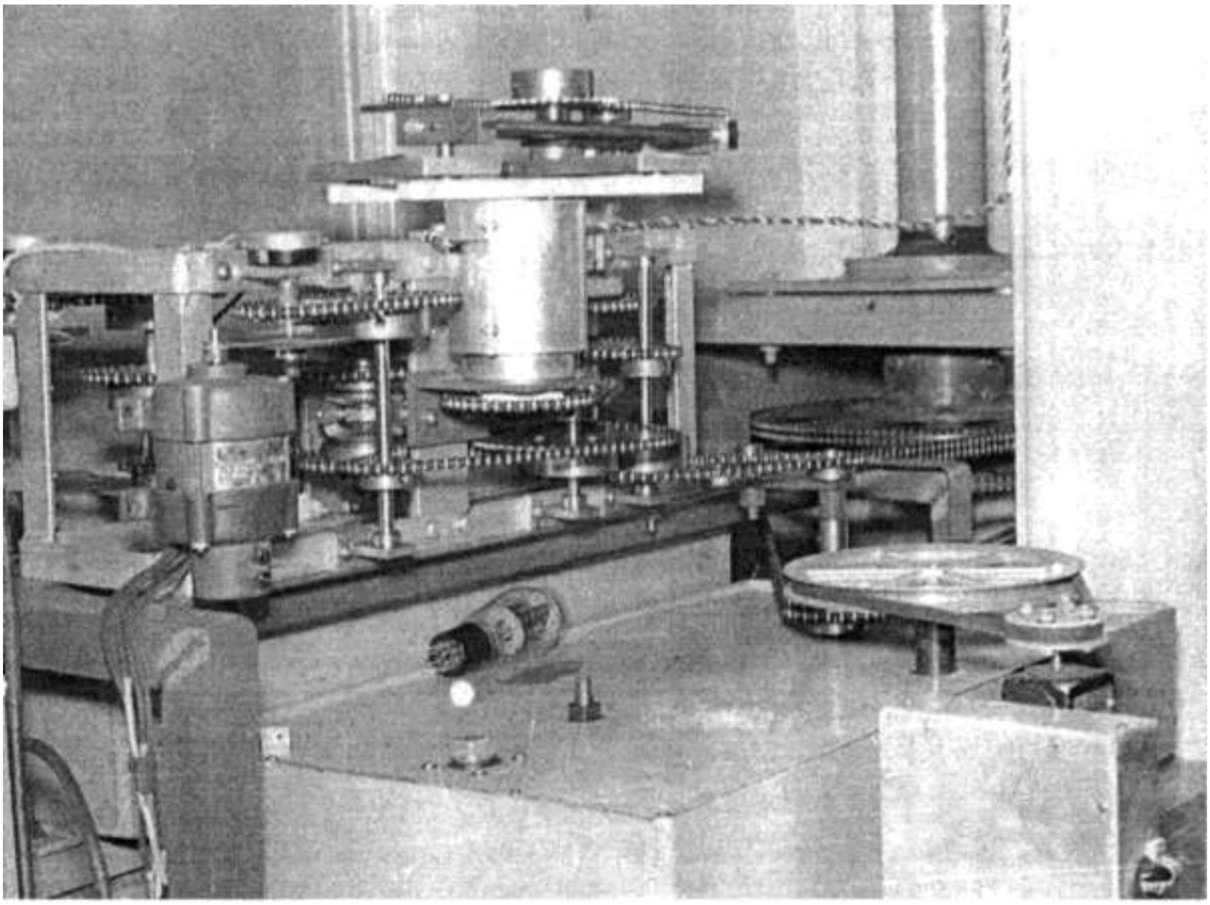
Museum of Transport and Technology here. As far as I can ascertain, this machine was built about 1935 by D R Hartree, a very famous mathematician. His other claim to fame, related to me by a friend of his family in Cambridge is his reply, when very young, on being taken to see the trains "it is not a puff-puff, it's a locomotive". This machine was made mostly of Meccano components which lends some credence to the statement that scientists are merely little boys playing with larger and more expensive Meccano sets.

Also, before returning here, I obtained a supply of valves and cables and a few other things that looked as though they might be useful from the war surplus stores. These kept us going for many years. The 1950s were years of just hard slog. For example, to get anywhere at all in this type of investigation, it was necessary to find the bearings of signals that had arrived by way of the ionosphere - there was no way of doing this at that time. To also measure the vertical angle at which these signals arrived (especially since there were usually many different signals from different directions arriving together) just added further complications. Remember, this was in 1951, before there were any satellites or wide band cables in the offing. However, the problems were solved and the main factors that were important were determined.

One of the important reasons for associating research organizations solving adequately difficult problems with universities is that the necessarily longish-term programme provides an excellent range of topics suitable for postgraduate research students (usually MSc and PhD). This has two effects - it provides good training in investigative methods in that it is "hands-on" similar to an apprenticeship and, eventually, communicates the techniques, rather than just the final scientific results, to the outside world. As a comment on this, most of the techniques of radio astronomy used in the early post-war years were based on the theory and practice of detecting and analysing very weak radar reflections. The site of the Seagrove Radio Research Station had been chosen as a good direction-finding location i.e. flat, electrically quiet and available - it was, in fact, an airfield that had been build for US forces. There is no doubt that it was a good site but it was too remote (100 km or so from the city) for sufficiently close liaison with the University. For this reason, in the late 1950s, it was decided to abandon the Seagrove site in favour of one that was close to Ardmore Aerodrome where the School of Engineering was located at that time.



1953: MUCH WINDING – the first rotating interferometer for measuring bearing and elevation angles at Seagrove.



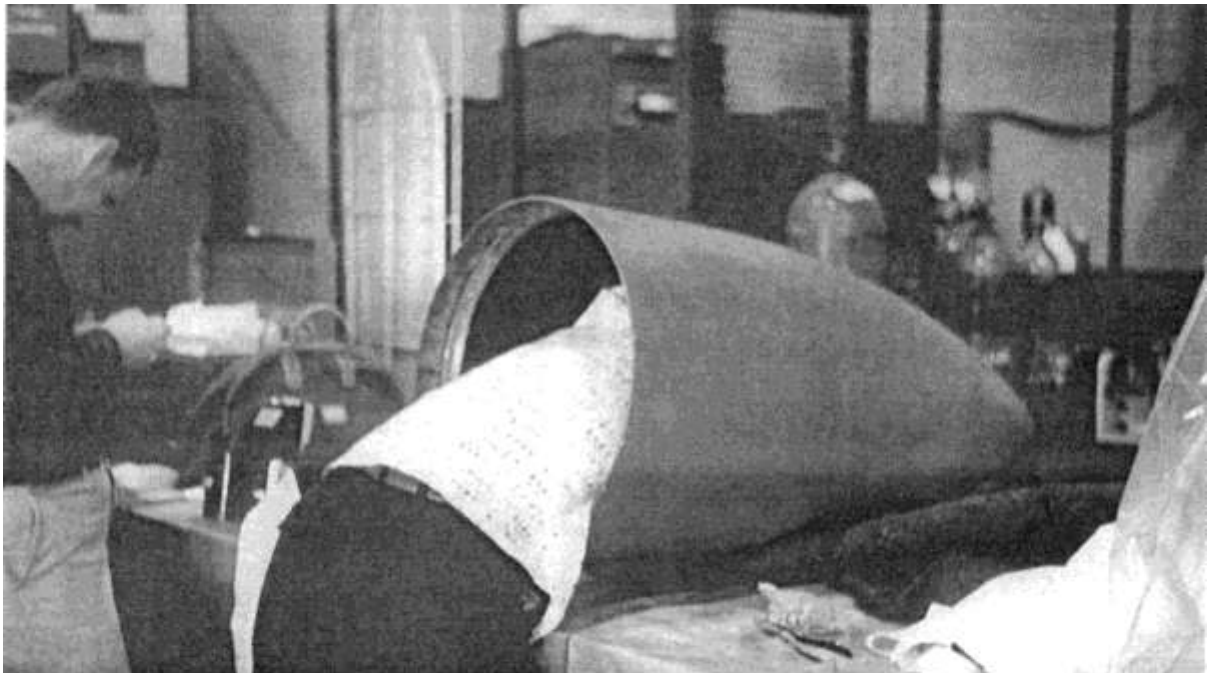
1957: Seagrove: the mechanical computer part of an automatic direction-finder.

By this time we had developed several efficient methods of measuring and analysing the angles of arrival of long-haul short wave signals and had essentially solved most of the outstanding practical problems. Also, the initial funding system had been revised so that an allowance for the expenses of the Radio Research Station was "built into" the funds allocated to Auckland University College by the University of New Zealand. Since the operation was by then reasonably well established I decided to accept an offer of a Senior Post-doctoral Resident Research Associateship from NASA. An ex-student, John Titheridge, who had gained a PhD in the Cavendish Laboratory at the University of Cambridge returned to manage the project while I was away. His interest in the use of transmissions from satellites to investigate the ionosphere and the upper atmosphere fitted in quite well with our general programme. I worked at the NASA Goddard Space Flight Center in Washington, DC, from 1960 till 1962 before returning here. The group that I was associated with was called the Planetary Atmospheres Division, the planet in question at that time being the earth. Their operations then were concerned with using small and medium sized unmanned rockets and satellites for investigating the earth's upper atmosphere.

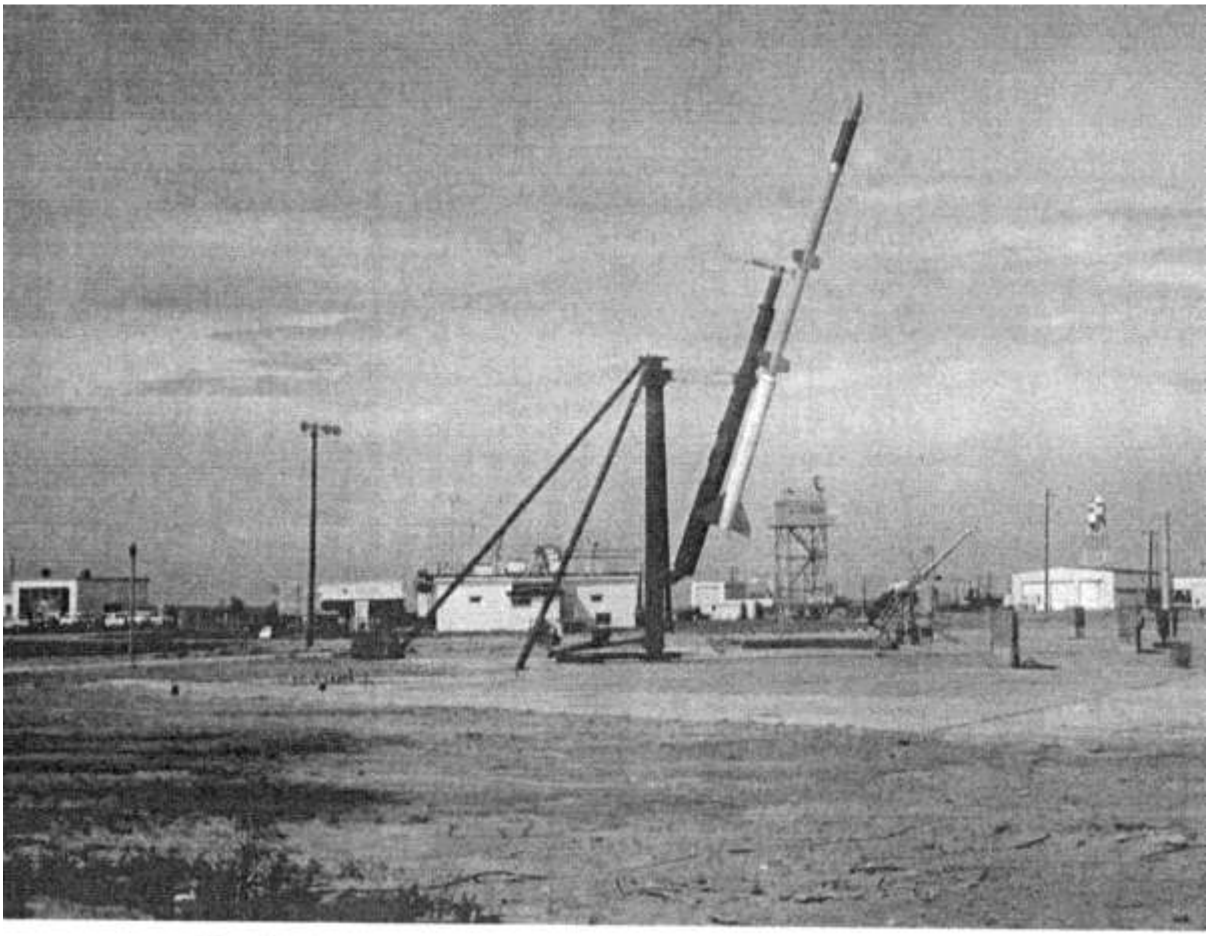
There were the in-house programmes but there were also a very large number of programmes that were carried out in conjunction with universities and other research groups from the US, Canada and England. For the first year, the Goddard Space Flight Center was situated in one building within the US Naval Research lab. The first US satellite was, in fact, built and launched by the US Navy. Security, of course, was extremely tight in such an organization and I, as a foreign national was required to have an escort at all times. This rule was relaxed within the NASA building but my escort had to carry the papers that allowed me to get in and out of the Navy establishment. This caused some embarrassment one day when my current official escort left for a two-day conference in Canada taking the papers with him so that officially I could not leave till he returned. The situation was solved by calling the head of NASA security who arranged with the head of Naval security that it was allowable for me to go home. Some of my work was concerned with the behaviour of antennas in the ionosphere where there are some problems because they are actually immersed in a conducting medium.

In this context it turned out that the knowledge of acoustics that I had acquired while designing loud speaker enclosures many years before stood me in good stead since electrical pressure waves generated in the ionosphere could explain many of the features that were observed. I designed and built a probe for directly measuring this effect to be launched, with other experiments, on a Scout rocket - this was a largish, four-stage solid fuelled rocket. The experience of constructing equipment that would survive the shaking experienced during the launch of such a rocket was extremely educational. It is rather leveling to watch your carefully constructed unit fall to pieces when it is tested on a shaking table. In the event, the alkaline batteries for this payload failed because they had internal leads that resonated at a frequency that only occurred when the payload was mounted on a half-spent fourth stage rocket. After my year at NASA had become two and a half years, I had to decide whether to stay permanently or return to New Zealand. Washington is an interesting city but it is too hot in the summer and too cold in the winter so we came back here. As an aside, until quite recently, British diplomats stationed in Washington were given a special hardship allowance because of the climate there.

1961: Goddard Space Flight Center: payload for detection of electro-acoustic waves in the ionosphere.



1961: Goddard Space Flight Center: installing a parasitic antenna inside a heat-resistant nose cone.



1961: Wallops Island, Virginia: SCOUT rocket with the nose cone in place.

By this time the importance of radiotelephony for New Zealand had diminished considerably since the Compac cable was then installed. However, since there were many features associated with long-distance propagation that were still unexplained and since we had developed some quite powerful tools for investigating these features, I decided that the direct advantages to students and the possible indirect advantages in the future arising from experience in applying electronic techniques to the solution of adequately difficult problems merited the continuing of the organization.

This was the period when the first electronic computers were appearing in New Zealand. Since my experiences with EDSAC I had had no other contact until I went to NASA. There was no hands-on contact there since they had a computing installation complete with programmers etc who would take the mathematical problem given to them, code it, run it and return the results. It was a very good service and I obtained some very good results from it.

The main drawback, I suppose, was that instead of quietly throwing your garbage results in the waste-paper basket you had to explain that it had to be done again because of your own idiocy. However this only confirmed what the mathematicians already knew, that they were much brighter than the physicists. While with NASA I had considerable contact with US university groups and in particular with the sub-contractors who built a lot of the mechanical components. I was most impressed with the standards of their business dealings except maybe on one occasion. We had a deadline for a particular project and needed some extra technicians for a while. These were hired from an agency, a very common practice in the US, but I was slightly surprised when I was informed that the rate for one of them would have to be increased. The reason, I was told, was that he had been untrained in our particular field but now that he had been with us for three months he was trained in that field and accordingly they were entitled to a higher rate. However, since my colleagues informed me that that was normal practice, I had to let it go through.

To return to the 1962-1970 period, we were becoming so good at producing data, especially from some satellite transmission experiments and the long-term runs on overseas transmissions, that it became essential to have our own computer for analysis. This, tome, was justified for the useful experience that it provided for students as well as for the immediate benefit in producing research results. During all of this period I had the conviction that NZ's future depended on the level of our technology but it seemed extraordinarily difficult to get this message across.

In 1969 I spent a year at the University of California, San Diego, where the research interest was in a radio astronomy field - specifically the scattering effect on the signals from cosmic radio sources of the material being continually ejected from the sun and called the solar wind. The probability theory associated with this was very similar to what we had developed in our work on ionospheric propagation and they had a much larger computer than Auckland that until then was being under-utilised.

In the 1970s I overcame my reluctance to serve on committees etc - in the past I had considered that most of this type of work could be better carried out by people with some spare time and running an active research project had never left much time to spare - and accepted a position on the Communications Commission since I considered that it might provide an opportunity to promulgate my views on the value of technology (in particular, electronics) to New Zealand. This Commission's report "Telecommunications in New Zealand" appeared in March 1977 which was the year that I spent in Germany at the Max Planck Institute for Aeronomy.

There are a number of Max Planck institutes in Germany, each built around a particular person or group of persons rather than being dedicated to a subject. They essentially take the place of DSIR or equivalent groups in the British tradition. This particular group had its origins in a German wartime establishment of radio physicists who, by the end of the war, were located in Austria.

As the Russians were advancing in that area, it was considered by Sir Edward Appleton and his assistant Roy Piggott that these people were too valuable to the west to be left to disappear into Russia. Piggott obtained a requisition for 7 trucks (which he amended to 70) and rescued them and their families and transported them to the British area in the north of Germany. There were three directors when I was there, one of them being Ian Aoford, a New Zealander. As well as their interest in the upper atmosphere they were also involved in space research in conjunction with NASA. I found an interesting project in the design of a large moderately steerable vertically looking antenna for a programme concerned with the ionosphere in the arctic auroral regions. I discovered that the accepted theory of beam steering by moving the feed of a parabolic reflector was wrong and in fact spent several years in getting it right. When I returned in 1978 the Communications Advisory Council was formed - this was larger than the Commission and was ongoing and provided a much better forum for pushing the view that technology was a good thing. However, the general result tended to be that just as we seemed to be getting the message across, either the relevant minister or the government or both would change. This was the period when the strange views on level playing fields etc were being pushed, mainly by Treasury I believe, and our views were most unpopular. This was most frustrating and it was somewhat of a relief when the Council, along with some other quangos, was disbanded in 1986.

One theory of what happened at that time was that economics had become too hard so that the reaction was to throw it all away and hope that everything would magically sort itself out. This was based on the belief, ascribed to Adam Smith that free markets led automatically to the most efficient final situation. What Adam Smith actually said was that universal wealth and freedom should be achievable through the new technology combined with freedom of trade. In this case the new technology was the invention of the efficient steam engine. But he later qualified part of this to say "to expect, indeed, that the freedom of trade should ever be entirely restored is as absurd as to expect that Utopia should ever be established ... It has become dangerous to attempt to diminish the monopoly which our manufacturers have obtained against us." As an aside to this, although he was obviously referring to Utopia as being a perfect world, Thomas More who wrote Utopia said "the king should keep his subjects impoverished ... lest they become insolent with wealth and freedom".

Although almost every other country has accepted for many years that it is essential to nurture small manufacturing enterprises, this lesson seems only now being learned here.

Funding the type of research that we were doing in the late 70s and early 80s became very difficult and I decided to retire in 1982. I still carried on with my studies of parabolic reflectors and stayed on the Communications Advisory Council. At that time I was a member of NEDA which was a member of the Asian Electronics Union based in Japan. Through AEU I was able to study the development methods of the other member countries which include Taiwan, Korea, Malaysia, Singapore, India etc.

AEU is financed mainly by the Japanese government although indirectly. For a while I was puzzled by the Japanese interest in promoting electronics development in what are becoming rival economies. The AEU answer to this was that enhancing the standard of living of these countries was beneficial to Japan in that this provided better markets for Japanese products. One extra benefit, to my mind, was that each country produced detailed reports on its electronic developments for each AEU meeting and I am sure that these were of tremendous value for MITI, the Japanese development ministry.

One of the research projects that the Radio Research Centre undertook in the early 1980s with Geoff Dingley who was a PhD student and in collaboration with the Department of Civil Engineering was the development of a radio system for tracking the drifting buoys that are used in oceanographic research to study currents. The system that we employed was, in fact, descended from a system that had been used at Goddard Space Flight Center to continuously measure the exact distance from the ground of a rocket carrying a small transmitter. I mention this because in 1987 I received a query as to whether it was possible to track racing cars accurately. The system that I devised was a descendant of the oceanographic system and I entered into a partnership to develop and market the system with the backing of a venture capital company. It turned out that this project was really their only successful one but it also turned out to be their last in that they ran out of money so that the project was sold to a Canadian company who have now, as far as I am concerned, disappeared back into the wilds of Ontario. I undertook this project mainly because it offered me an opportunity to obtain some practical experience of the value and difficulties in exploiting a niche market. However, an offshoot of the tracking system was the knowledge that there was a need in the motorcar industry for an instrument that could measure the angle at which a tyre was travelling with respect to the road - this is called the slip angle. Now light reflected from a road has much in common with radio waves reflected from the ionosphere and it was possible to use this analogy to make a suitable instrument. Quite a number of these were sold but although they worked very well there were some aspects of their behaviour that I did not fully understand. From 1992 until the present time I have spent a great deal of effort on this and now have all the problems solved. My reason for mentioning this is that the background knowledge necessary for the effective development of any product can come from a wide variety of sources and it is my strong conviction that success in R and D is much more likely to come with people with wide backgrounds rather than with those that are highly trained in very narrow fields - in practice we need both types, of course.

It was in connection with this project that I resumed my contact with the Japanese and was able to study their methods. They are very practical people. For example it is said that 80% of Japanese follow the Shinto religion and 70% follow Buddhism. The reason is obvious since Shintoism ensures that good things happen in this world and Buddhism takes care of the next one. They are not overworked on the factory floor, they just do the job quietly but do not make mistakes. The main problem in dealing with the Japanese is that it is rare to find an individual who will make decisions. Most decisions are made by a group and, in many ways this is understandable since then the responsibility is shared.

The feeling that it is dangerous to stick your head up is even stronger in Japan than it is here. I met a Swiss who was selling shop fittings at one stage. He had spent the day with Japanese companies and his comments roughly were "when my boss sent me here he said `study these people, they are the most efficient in the world and you can learn a lot from them.' What do I find? They are the most inefficient - whenever you ask a question they go into a huddle and they are not even talking about the matter in question!"

Actually I believe that it is all part of a game that they play and just needs understanding. It was a bit different with the top executives possibly because in Japan, as in continental Europe, about 60% of them have engineering qualifications even in non-engineering companies. But, from the top to the bottom, there is one attitude in common and that is that what they do must be good for Japan. It has worked there and I believe it will work here too.

I have made a number of comments as I have reported this potted history but the overall message is that since large industries grow from small industries, we need to initiate and look after our small industries if we are ever to regain our rightful place in relation to other economies. At the same time we have to face the realities of the extra difficulties we face because of our remoteness even from our nearest offshore market in Australia.