THE EARLY HISTORY OF VERY LOW FREQUENCY (VLF) RADIO RESEARCH AT STANFORD

YEARS OF DISCOVERY, INNOVATION, AND ANALYSIS, SUPPORTED BY FIELD WORK EXTENDING FROM ANTARCTICA TO ALASKA

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Contents

Part 1

Ground Observations

1.1 Introduction

In the late 1940s and early 1950s a group under young Stanford professor Bob Helliwell began to use dispersed ∼1-20 kHz signals from lightning for remote sensing of the then unknown envelope of charged particles surrounding the Earth.

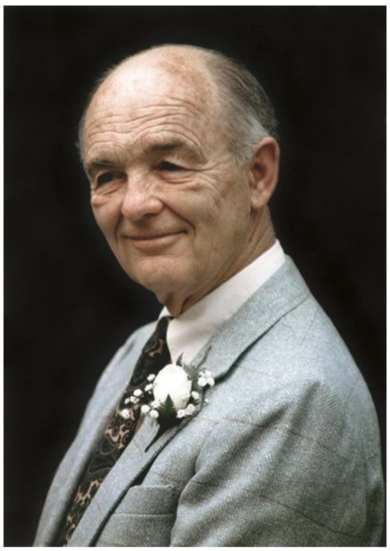


Figure 1.1: Bob Helliwell, circa 2008.

In collaboration with other radio pioneers they were investigating the distribution of low energy charged particles up to altitudes approaching 30,000 km. It was a heady time, years before the first satellites were launched. Now, over 60 years later, Stanford VLF radio work continues, and with great vigor.

Between 1951 and about 1985 the group was directed by Helliwell (Fig. 1.1), and from 1985 onward by Umran Inan (Fig. 1.2), who obtained the PhD under Helliwell in 1978. In the many years since 1951, over 90 PhD theses have been written on VLF topics, 44 under Helliwell and more than 52 under Inan. Under Inan the number of students increased to a peak of 20 or more, reaching a point at which he could walk into a group meeting, see only 16 students, and ask "where is everybody?"

How could a single research group survive so long in a rapidly changing Electrical Engineering Department? What did the group find in the VLF area that was so full of challenges? Our hope in these pages is to answer these questions.

There are already reviews of preceding whistler research by *Alpert* (1980) (Fig. 1.3) and by *Helliwell* (1993), both entitled: "Forty years of whistlers." However, the differences between the present work and these two papers are substantial. The latter are compact, scholarly works that emphasize highlights, while much of the present work is unpublished background material. Alpert's paper was written in 1978 by a plasma wave physicist looking at the whistler-mode propagation field and marveling at the challenges it had been presenting to scientists. The paper was prepared in response to an invitation by R. Gendrin, chair of Commission H of the International Radio Science Union (URSI), and was to be presented at the XIXth General Assembly of URSI in 1978 to mark the 40th anniversary of the adoption of 'whistlers' as a subject of research within the organization. Alpert was not permitted to leave the USSR to attend the Assembly, so his paper was read there by Dr. Gendrin. In 1980 it was published in the *Journal of Atmospheric and Terrestrial Physics.*

Figure 1.2: Umran Inan, 2004.

At the 1978 URSI Assembly, Owen Storey, who had first identified the properties of whistler paths in space, (*Storey*, 1953), asked delegates for help in getting permission for Alpert to leave the USSR. Fig. 1.4 shows a group of the Assembly delegates in the hall where Gendrin read the paper and Storey made his appeal. Alpert received an exit visa only a decade later, during the time of Gorbachev.

Helliwell's paper exists in the form of a preprint prepared at Stanford in 1993. It briefly reviews work on both "ducted" and "non-ducted" whistler mode wave propagation in space plasmas, pointing to the important influence that such propagation may have on the radiation belts. It then describes in broad terms a panorama of non-linear wave-particle interaction effects observed in data from the experimental VLF transmitter at Siple Station, Antarctica. The paper was presented as a special lecture at the XXIVth URSI Assembly in Lille, France in 1993, but to our knowledge was not published in a journal. In 1993 and 1994 Helliwell was talking to Cambridge Press about writing a sequel to his 1965 classic work on "Whistlers and Related Ionospheric Phenomena"(*Helliwell*, 1965). In that sequel it was clearly his intention to draw upon 25 years of observational and interpretive work on wave injection into space from Siple Station. Sadly, that sequel was never completed.

Figure 1.3: Jakov Alpert, 1978.

In contrast to both the Alpert and Helliwell papers, this history uses small brush strokes to focus on specific situations and follow year-to-year developments within the Stanford group. We will be considering the kinds of challenges and opportunities that have been encountered during field work in Antarctica and at high Northern latitudes. What personal stories can we tell about life at the field stations?

Figure 1.4: Dyfrig Jones, Don Carpenter, and Millett Morgan, URSI Assembly, 1978.

On the campus, how have the grad students found subjects for their PhD thesis work? What special contributions have certain individuals made to the overall progress of the group? By taking a more or less chronological approach, we are able to see how successive projects have built upon those that have gone before.

We confess to being fascinated by the rivalries that have developed between Stanford VLF and other groups pursuing similar topics, and will do our best to discuss them. Also fascinating, when not dispiriting, is the ongoing problem of obtaining funding for research. This too, will be discussed.

1.2 The beginnings of whistler research at Stanford

During World War II young Bob Helliwell was operating a C-3 radio sounder in a spider infested basement of the old Ryan High Voltage lab along Stanford Avenue. As a grad student in the EE Department (Fig. 1.5), he was working on problems of density structure in the lower layers of the ionosphere.

That structure might confuse pilots over the Atlantic by causing anomalies in the apparent arrival bearings of HF signals from distant sources such as the BBC. He soon concluded that in order to study the lowest-density, lowest-altitude parts of the D and E ionospheric regions, probing frequencies below 1 MHz were required, 1 MHz being the low-frequency limit of the C-3 sounder. What to do? He decided to try assembling a spark transmitter that would operate at 100 kHz, well below the 1 MHz limit of the C-3.

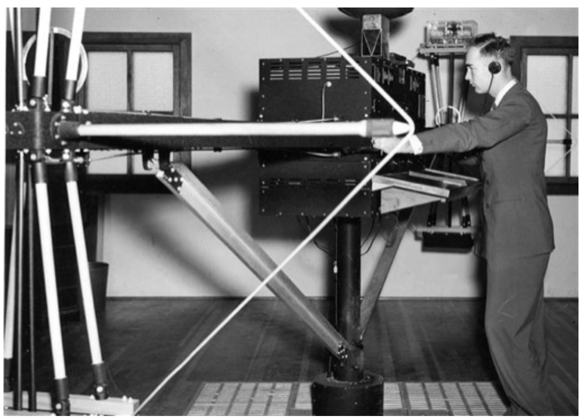


Figure 1.5: Grad student Bob Helliwell operating a direction finder, circa 1944.

The new transmitter turned out to be a frightening device. It was situated at the Ryan lab and powered by a 200-kV charged capacitor. The antenna was a leftover section of a cable 2 inches in diameter being used to convey power from Hoover Dam to locations in Southern California. Transmitter pulses were to be generated in a spark gap roughly every 1.5 s. The FCC had been understandably reluctant to grant a license for operation of this type of transmitter, having published rules on permissible transmission bandwidths. Undaunted, Helliwell found ways to get a license to operate, calling his system an "experimental impulse transmitter" and using his own definition of bandwidth in a way that brought the spark system within the FCC rules (according to Helliwell, the FCC had not clearly defined what it meant by bandwidth). Whatever permissible limits the FCC had specified were of course well inside the actual - 3dB points of the transmitter spectra. According to Helliwell [1997] "the spectrum was so broad that a 30 kHz-wide receiver could detect ionospheric echoes on virtually any clear channel up to several MHz."

The system began operation in 1947, and thanks to the low pulse repetition frequency ($< 1 - s^{-1}$) and the late-night time of the operations, "there were no serious complaints." A receiving station was operated three miles away near Searsville Lake, and the results, including those from a higher-order mode at 356 kHz, became major ingredients in Helliwell's Ph.D. thesis [1948], entitled "Virtual height measurements of the ionosphere at 100 kilocycles" and signed by professor Hugh Skilling.

At some point Helliwell decided that still lower probing frequencies were needed and that impulsive signals near 10 kHz from distant lightning should be tried as sources. At such very low frequencies the signals were known to propagate efficiently in the spherical waveguide formed by the Earth and the overlying ionosphere, and because of their sensitivity to reflection conditions at the lower edge of the ionosphere near 90km, could potentially help to reveal density structure at those poorly explored altitudes. The idea would be to record the impulsive signal wave forms, which were called 'tweeks' when the signals had a quasi-musical quality. A tweek involved a train of pulses at rapidly descending frequencies just above the 2 kHz half-wave cutoff frequency of the nighttime earth-ionosphere waveguide (arrow below in (Fig. 1.6), Measurements of the arrival times of successive pulses could be used to estimate the distance to the lightning source, while the asymptotic frequency of the pulse train, which sometimes lasted for several hundred ms, provided a measure of the ionosphere reflection height.

1.2.1 The first whistlers heard at Stanford

Helliwell loved to tell about the night the first whistlers were heard at the Searsville site. His student Jack Mallinkrodt was sitting up listening to the snapping, cracking, popping sounds of impulses from distant lightning, among them tweeks from sources not too distant. Along with this activity he was surprised to hear something very different, a gliding, whistling tone that lasted one or more seconds and descended slowly in frequency with time (at right in Fig. 1.6). After hearing about this, Helliwell claimed to have told Mallinkrodt to take some time off, presumably to allow recovery from whatever tricks his auditory imagination were playing on him. The gliding tones continued to be received, however, and Helliwell, in attendance one night at the site, heard his first whistlers.

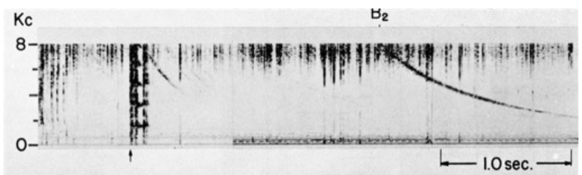


Figure 1.6: Tweek followed by 2-hop (long) whistler.

Mallinkrodt was dispatched to the library to look for literature on the subject and found several items, including the early results of *Barkhausen* (1919) from eavesdropping on Allied telephone conversations during World War I. Later, *Eckersley* (1935) had employed the recently developed magneto-ionic propagation theory (e.g., Appleton, [1932]) to explain the low frequency falling-tone behavior in terms of propagation of a lightning impulse along an unspecified magnetized plasma path in the ionosphere.

1.2.2 Word of Storey's discovery of whistler paths

In 1950 Helliwell heard of Storey's pioneering observations in England; so called "short" and "long" whistlers were identified according to whether they had originated in lightning in the opposite hemisphere or in the local hemisphere (*Storey*, 1953). Using Eckersley's approximation and ray tracing, Storey had concluded that propagation occurred along dipole-like geomagnetic field lines that reached ground level near the conjugate observing stations and extended from two to three Earth radii altitude at their peak. To account for the observed propagation delays, Storey's model required that there be an electron density of $\simeq 400el - cm^{-3}$ at an altitude of order 12,000 km, a concentration orders of magnitude higher than conventional ionospheric theory would predict from upward extrapolation using the scale heights of the heavy ionospheric ions oxygen or nitrogen. Among those who accepted Storey's density estimate, there was a tendency to believe that it represented conditions in the solar corona at the orbit of the earth and not in a region coupled to the regular ionosphere (Storey himself believed this for a time).

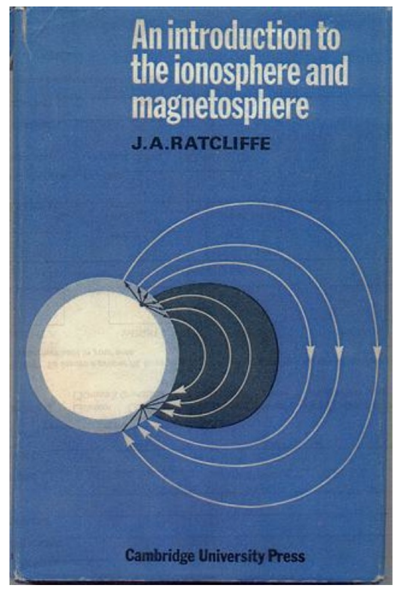


Figure 1.7: Dust-cover tribute to Storey in 1969 by his mentor, J. Ratcliffe.

In 1952 at the URSI Assembly in Sydney, Australia, J. A. Ratcliffe, Storey's supervisor, reported on his student's work, saying that the conclusions were probably wrong but were worthy of serious attention and further investigation.

Over the next few years it became progressively clearer that the plasma Storey had reported consisted primarily of protons and was in fact an upward extension of the ionosphere (*Dungey*, 1955), (*Storey*, 1958), (*Allcock*, 1959).

Although the existence of what was at times called the protonosphere gradually became gospel in the wider science community during the 1950s, Storey's critical role in discovering it (years before the first satellites detected the Earth's radiation belts) did not become widely known outside the ranks of plasma wave specialists. Even today, in a 478 page work entitled "Opening Space Research" that was published in 2011 by the American Geophysical Union and written by George Ludwig (*Ludwig*, 2011), a key participant in the early satellite work, there is no mention of the thermal space plasma. Storey's name is not mentioned, while Helliwell's appears once in connection with an experiment on Explorer VI (see below) involving whistler-mode penetration of the ionosphere by upgoing signals from VLF transmitters.

A contrasting measure of the importance of Storey's work to some wave-propagation specialists may be seen on the cover of a book jacket published by *Ratcliffe* (1969) (Fig.1.7), Storey's thesis advisor. Having once been skeptical of Storey's work, Ratcliffe's appreciation of its importance as a means of probing the magnetosphere is expressed in a sketch showing propagation of whistlers along multiple field-line paths threading plasma regions on both the low and high density sides of the plasmapause.

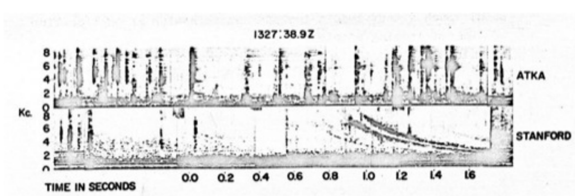


Figure 1.8: Simultaneous 1954 VLF recordings in the South Pacific and at Stanford.

1.2.3 A program of whistler observations at Stanford

After a program of whistler observations began in 1951, It revealed a diurnal curve in activity that depended on whistler intensity, thus suggesting partial control of the activity by thunderstorms. Whistler activity was found to correlate positively with the year-to-year variation in sunspot activity.

In 1954, discussions of whistlers were held at the URSI Assembly in The Hague. Participating were G. McK. Allcock of the D.S.I.R. in New Zealand, R. Rivault of the University of Poitiers, Millett Morgan of Dartmouth College, and Helliwell. It was agreed that a key test of Storey's conclusions about long and short whistlers would be simultaneous measurements at opposite ends of the whistler path. The first conjugate-pair test to produce positive results was made in December 1954 at Stanford and on board the U.S.S. Atka, an ice breaker en route to Antarctica in the southern Pacific Ocean. Excessive ship noise prevented observation of whistlers on the Atka, but on copies of 0-8 kHz spectrograms vs time, the broadband tapes from the ship and from Stanford revealed an excellent correlation between the time of detection of a strong tweek on the ship and the arrival at Stanford of a whistler roughly 1 second later (Fig. 1.8). Later, in August, 1955, a convincing conjugate pair test involving Wellington, New Zealand and Unalaska, Alaska was successfully conducted by *Morgan and Allcock* (1956).

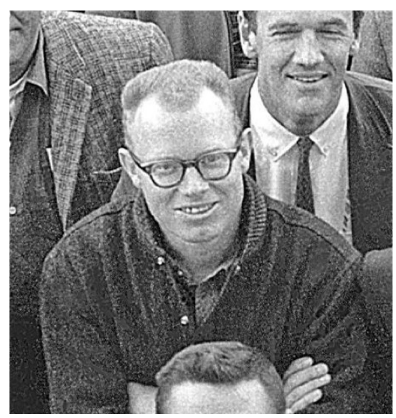


Figure 1.9: Tim Bell at Stanford in 1960

1.2.4 Discovery of the nose whistler

In August, 1956, the growing Helliwell group submitted a report to the Air Force Cambridge Research Laboratories on its whistler related research during a three year period from 1953 to 1956. The work had been a joint effort involving research assistants J. H.

Crary, R. L. Smith, L. H. Rorden, U. Groneman, D. A. George, and T. F. Bell.

Given the focus by Storey on observations at the latitude of Cambridge, Helliwell had been wondering what would be found at higher latitudes, say at College Alaska, near the endpoints of much longer field lines than those of Cambridge and Stanford. In 1954, cooperative arrangements were made with Chris Elvey, then director of the Geophysical Institute of Alaska at College. Stanford supplied a VLF receiver and the Institute provided a quiet site and personnel for operations.

What would be the effects on the whistler frequency-time curve imposed by assuming, as had Eckersley (and Storey), that the ratio of wave frequency to gyro-frequency f/f_H was much less than unity? Under that assumption the travel time of a whistler undergoing longitudinal propagation along a field line would be expressed as:

$$t = \frac{1}{2c} \int_s \frac{f_p}{f^{1/2} f_H^{1/2}} ds \tag{1.1}$$

The variation of whistler travel time with frequency, or "dispersion," could then be conveniently expressed in terms of a constant $tf^{1/2} = D_o$, the constant being obtained from a path integral involving the plasma parameters f_p and f_H only, i.e.:

$$tf^{1/2} = D_o = \frac{1}{2c} \int_s \frac{f_p}{f_H^{1/2}} ds \tag{1.2}$$

There were indications in Storey's data plots at the higher whistler frequencies and in similar plots at Stanford of an increase in whistler travel time over the predictions of the low-frequency Eckersley dispersion D_o . It seemed clear that the approximation $tf^{1/2} = D \approx D_o$ would fail along field lines extending to high latitudes,

In 1955, Tim Bell, an undergraduate physics major, was asked to run some dispersion calculations based on the full expression for propagation in a cold magnetized plasma and using the newly available IBM 650 card-programmed computer.

Around the time that Bell was completing his work, a tape arrived from College, sent by Institute scientist Joe Pope, It contained signals, which when displayed in a frequency-time format of the kind then available from the Kay Electric Sonagraph, exhibited a diffuse, curved behavior, eventually to be described as a multi-component nose whistler (*Helliwell et al.*, 1956). Fig. 1.10 shows spectrograms of a whistler recorded at College after midnight on July 10, 1955. These data agreed nicely with Bell's plots, showing that a lightning whistler propagating along a dipole field-line path would exhibits a nose or frequency of minimum delay.

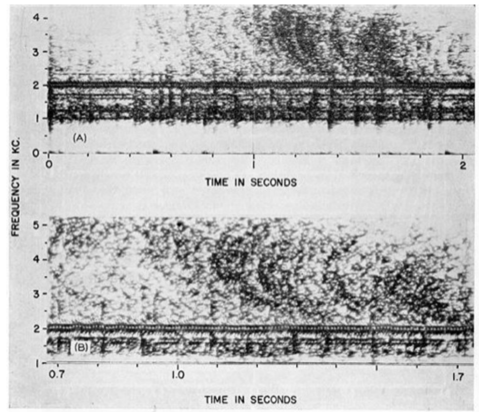


Figure 1.10: Multi-component nose whistler at College, AK 1955. From *Helliwell et al.* (1956).

In their discovery letter *Helliwell et al.* (1956) remarked on the multiplicity of closely spaced traces on the College records and the falloff in their nose frequencies with time, suggesting that propagation had occurred on multiple discrete paths distributed in equatorial radius or L value, and that all the paths had been excited by a single lightning flash.

In this critical discovery paper, the authors showed a new approximation to the expression for group velocity in the longitudinal extraordinary mode, one that should prevail under the commonly observed plasma condition of $f_p \gg f_H$:

$$v_g = 2c \frac{f^{1/2}(f_H - f)^{3/2}}{f_H f_p} \tag{1.3}$$

It was now clear that in principle, the observed travel time of a whistler component would not only increase asymptotically at zero frequency but would

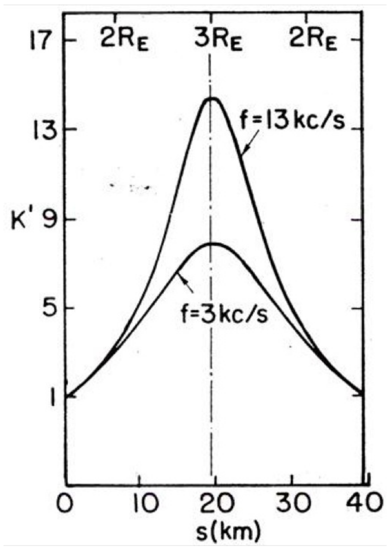


Figure 1.11: Travel time weighting function versus distance along L=3 field line. From *Carpenter and Smith* (1964).

also do so at a high frequency limit, thus forming a 'nose.' Integrating the group refractive index c/v_g along a field line path:

$$t = \int_s \mu_g ds = \int_s \frac{c}{v_g} ds = \int_s \frac{f_H f_p}{2f^{1/2}(f_H - f)^{3/2}} \quad (1.4)$$

revealed that a minimum in total travel time, or nose, would occur within a narrow range around $\approx .0.35 f_{Hmin}$, regardless of the plasma density model $f_p(s)$ used in the calculations. This can be understood in terms of the kernel multiplying f_p in the integrand of (4):

$$\text{ker} = \frac{f_H}{f^{1/2}(f_H - f)^{3/2}} \quad (1.5)$$

which shows why a whistler spends a large part of its travel time in the relatively homogeneous plasma near the equator (Fig. 1.11). If one were to consider only the effects of propagation along a short path at the equator, where $f_H = f_{Hmin}$, the frequency of minimum v_g and hence of an observed whistler nose would be expected at 0.25 f_{Hmin}. Higher values of f_n/f_{Hmin}, between 0.35 and 0.39 were of course observed due to the less heavily weighted but cumulative effects of propagation in lower altitude regions of stronger f_H.

Recognition that f_n should fall within a limited range between 0.3 and 0.4 f_{Hmin} implied that the College whistlers had followed field lines passing within a few hundred km of the College receiver. Having found this strong dependence of a whistler's nose frequency upon the magnetic field, Helliwell et al. [1956] could make a stunning suggestion, namely that "it should be possible to separate the effects of gyrofrequency (the path L value) and plasma frequency $f_p(s)$ on the dispersion and thus obtain more reliable estimates of the ionization density in the outer ionosphere."

In their paper, Helliwell et al. noted that other investigators were becoming aware of this new direction in whistler research. Harold E. Dinger of the Naval Research Laboratory had observed nose effects in whistlers. In studies of the whistler mode refractive index near the gyro-frequency, R. M. Gallet of the Boulder Laboratories of the National Bureau of Standards had found that rising tones could be produced by an impulsive source.

In the 1956 research report by Helliwell's group to the Air Force Cambridge Research Lab, it was noted that G. R. Ellis had published a paper in the June 1956 issue of *The Journal of Atmospheric and Terrestrial Physics* and had arrived at almost the same expression for whistler dispersion shown as equation (1) by Helliwell et al. However, Ellis' equation for the group refractive index needed to show a minus sign before the second term.

1.2.5 The 'undiscovered' 1955 nose whistler at Stanford: a footnote to the College, Alaska whistlers

In a recent (2012) review of the papers of Bob Helliwell, a spectrogram was found of a whistler recorded at Stanford on February 26, 1955. The original tape had apparently been processed at some point in the 1990s by Jerry Yarbrough, long-time data aide for the VLF group, in response to a request from Hel-

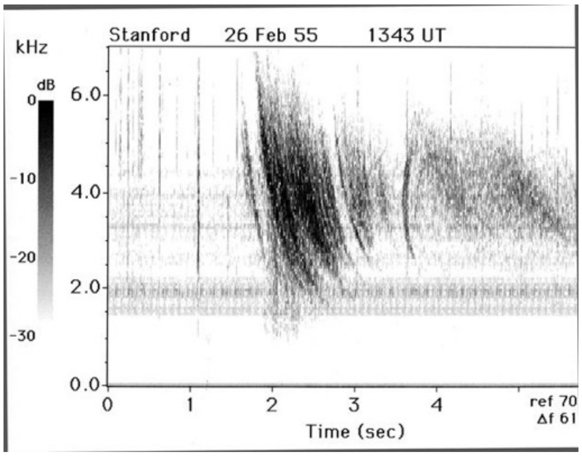


Figure 1.12: "Mystery" nose whistler at Stanford in 1955.

liwell. The whistler shows nose-like departures from predominantly falling-tone behavior similar to those revealed by the College recordings.

We have not been able to find the magnetic tape on which this whistler was recorded or any notes about it in Helliwell's files. At the time of recording in 1955, most of the Stanford VLF work was focused on designing and building equipment, as well as propagation theory. The event may or may not have been heard by aural playback and certainly was not displayed on a spectrogram at the time. The recording itself was probably ad hoc in nature and not part of a regular synoptic program like the one begun two years later in 1957.

The 1955 whistler is unusual in that it shows not only a group of falling-tone components that are typical of middle-latitude paths, but also a strong trace with a well defined nose just below 4 kHz, as would be expected for a path exit point well north of Stanford at $L \approx 4$. An unusually large number of discrete paths were excited, paths distributed in endpoint from the vicinity of Stanford to a region well north of Seattle. The similarities in spectra of the College whistler and this unusual Stanford whistler were consistent with the expectation, derived from later experience, that the latitudinal "fields of view" of the two receivers could occasionally overlap.

1.2.6 First Stanford recordings in Antarctica

Around this time, Alan Shapley of the National Bureau of Standards at Boulder invited Helliwell to visit Antarctica and consider its potential as a site for high latitude VLF measurements (see map below). Encountering local station interference that swamped a portable receiver, but realizing the great potential of Antarctica for VLF studies, Helliwell soon sent improved equipment that began producing excellent results at Byrd Station ($L \approx 7$) as early as 1959.

1.2.7 VLF recordings near Cape Horn confirm Storey's findings

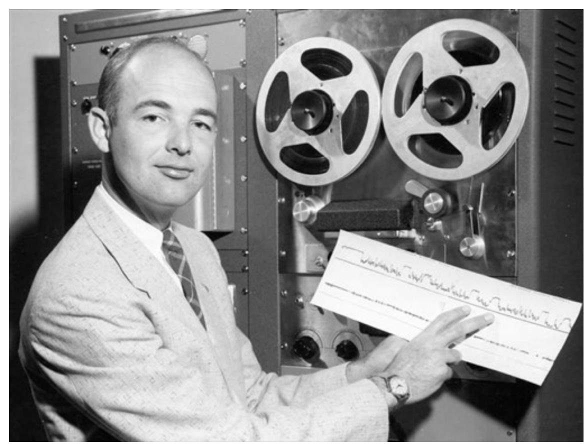


Figure 1.13: Helliwell points to a pair of NSS whistler mode echoes.

In the Spring of 1957, one of my tasks as a data aide was to listen to magnetic tapes recorded by Ernst Gehrels, a venturesome grad student who had been working at Stanford under Prof. Mike Villard. Helliwell wanted to use fixed frequency signals from a known VLF source as an additional means of testing Owen Storey's findings on whistler mode propagation, Ernst agreed to carry a portable receiver, tape recorder and wind powered generator to the magnetic conjugate point of the powerful US Navy NSS transmitter operating at 15.5 kHz at Annapolis Maryland. There were obstacles along the way, such as a customs agent in Santiago whose attitude toward Ernst and his gear immediately changed upon the appearance of a five dollar bill. Foul weather prevented Ernst from being put ashore on a remote island southwest of Cape Horn, and when he was finally able to set up his gear at a location 100 miles northwest of the Cape, 60 mph winds rendered his Sears wind generator useless (it had been designed for less extreme conditions in the U.S. mid-west). Someone told me that at some point Ernst was bitten by a bat, but that may be apocryphal.

Our hero finally managed to install himself at a lighthouse where there was access to local power and where he could begin recording during nighttime periods when NSS was transmitting sequences of 0.25-s pulses with 2-s spacing. In addition to Ernst' sleepy voice trying to announce the date and time on each tape, I could sometimes hear a dog barking in the background.

The expedition was a grand success. First came the direct signal, propagating in the waveguide between the Earth and the ionosphere and arriving at an amplitude of order 150 $\mu V - m^{-1}$ after a delay of only $\approx 30ms$. Then, after a much longer delay between 0.3 s and 0.9 s, came the magnetospherically propagated signal, its relative amplitude only 10 to 30 dB below that of the direct wave (*Helliwell and Gehrels*, 1958). It was speculated that the observed echoes had been received on the margins of the 1000 km "effective area" of down-coming signal illumination (because of the original logistical problems involving access to the conjugate point of NSS), and could well have exceeded the direct echo amplitude in the center of that area. On occasion there was spreading in time of echo delays, consistent with the possibility of propagation on two or more paths. Deep fading of echo amplitude was also seen, suggesting the beating of two components of equal amplitude. In Fig. 1.13 Helliwell is shown pointing to a strip chart displaying the amplitude of an NSS ground pulse followed in $\approx 0.6s$ by a pair of closely spaced whistler mode echoes.

Whistlers were detected from time to time on a broadband receiver, but not in clear correlation with the NSS echoes or with comparable regularity. This suggested that propagation in so-called "magneto ionic ducts" could occur whether or not there was lightning activity capable of exciting whistlers.

This experiment was offered as further confirmation of Storey's work and a clear indication that controlled sources could be used in future tests of magnetospheric propagation. Remarkably, whistler-mode propagation in space from a controlled source was being observed years before being detected by satellite borne receivers.

1.2.8 Early whistler recordings and their visual analysis

When I joined the Helliwell group as a data aide, there was a great deal of activity associated with operating a network of stations that included Unalaska, Alaska, Kotzebue, Alaska, College, Alaska, Seattle, Washington, Boulder, Colorado, Wellington, N. Z., Dunedin, N. Z., and Stanford, California. Among those participating in work either on propagation theory or on hardware development and deployment were Bob Smith, Herb Crary, and Bill Kreiss.

In the lab people were busy building and shipping equipment, while one or two data aides sat next to Ampex tape recorders and listened to two minute synoptic recordings made at various locations 50 minutes after each hour (an internationally agreed IGY schedule. Their job was to record on a data sheet the number of whistlers heard in each two-minute interval as well as the estimated whistler amplitude on a scale of 1 to 5 or 1 to 4.

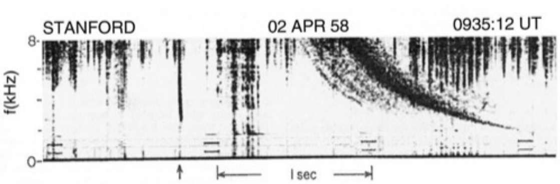


Figure 1.14: Stanford one hop whistler with clear isolated spheric.

There were a number of important questions about whistlers that had not yet been answered, among them that of identifying the time of the causative radio impulse (spheric) on the record. We soon found that this could be done rather easily, thanks to sheer serendipity and the fact that the Whistlers West network was primarily on the west, and not the east, coast of North America. Lightning activity in the vicinity of stations such as Stanford and Seattle was rare; aside from whistlers (the wanted signals), the spectrogram backgrounds tended to be relatively quiet, exhibiting only a distribution of relatively weak impulses that had propagated from very distant sources, An excellent example of a quiet background is presented in Fig. 1.14 from a recording at Stanford in 1958. Meanwhile, on the records of the Whistlers East network, say at Dartmouth College in New Hampshire, the many whistlers observed tended to be accompanied by strong impulsive noise from lightning situated nearby or in storms ranging from Canada to the Gulf of Mexico.

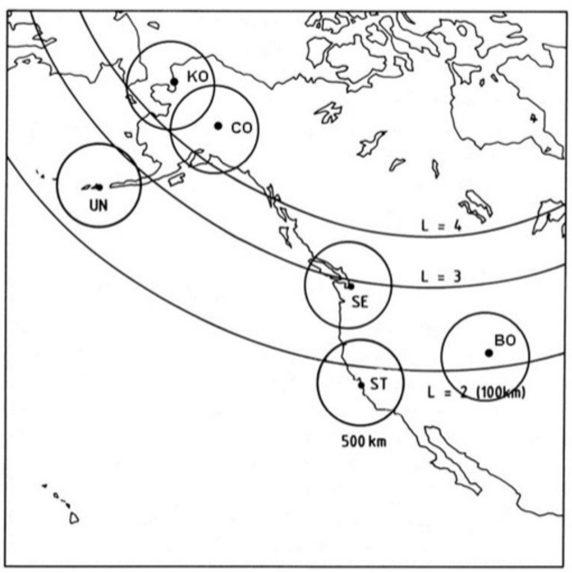


Figure 1.15: Stations in the Whistlers West network.

This difference in background noise was compounded by a dramatic difference in the whistlers themselves. At Stanford and Seattle, most of those detected were of the one-hop variety, as illustrated in Fig. 1.14. They presumably originated in a then poorly known concentration of lightning in the southern Pacific Ocean, now reported to be characterized by exceptionally strong, temporally isolated, events (e.g. *Füllekrug*, 2002, et al.). The one hop whistlers often appeared to have been excited by a single strong discharge whose spheric had propagated efficiently over seawater to the northern hemisphere receiver. The spheric regularly appeared on the records as a single impulse that was relatively well defined above $\approx 2.5kHz$, as illustrated by an arrow in the lower margin of the Stanford figure.

At the Whistlers East stations, on the other hand, a large fraction of the whistlers were of the two-hop variety, originating in local lightning. They were often quite diffuse, as the result of complexities introduced as part of reflection in the southern hemisphere (e.g. *Laaspere et al.*, 1963), and were not easily traced to a single strong causative impulse on the spectrograms.

The West network included a station at Boulder, Colorado, a location much more active than Stanford and Seattle in terms of proximity to lightning and reception of two-hop whistlers. However, when studied in conjunction with data from the quieter West coast stations, the Boulder data were very useful, for example in terms of information on variations in activity with longitude and the effective ground area illuminated by a typical whistler.

1.2.9 The first routine measurements of whistler dispersion

The whistlers that had been studied by Storey, being received at middle to low middle latitudes, had been measured essentially in terms of their low frequency or Eckersley dispersion D_o. At Stanford we took a similar approach at first, making travel time measurements at 5.0 kHz, and for each whistler reporting the quantity $D_5 = t(5000)^{1/2} = t(70.7)$, where t was some kind of a weighted measure in s of the travel times of the whistler's multiple components at 5 kHz. The quantity D_5 proved very useful as a long term measure of activity at a given station and as a way of comparing activity at various IGY stations.

Assuming that the field line paths represented in a station's records did not change systematically in latitude over time, D_5 would remain an approximate measure of the day-to-day and month-to-month val-

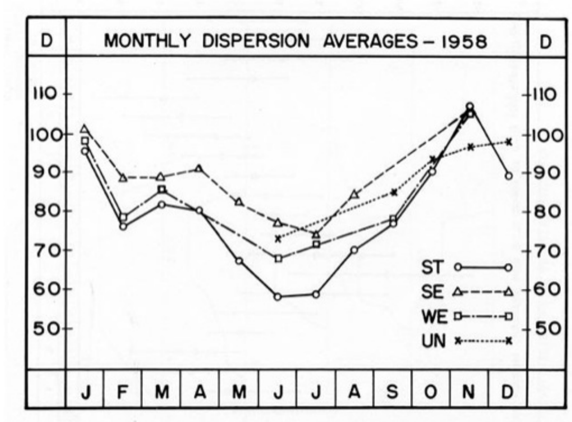


Figure 1.16: Annual D_5 variation in 1958. From *Helliwell and Carpenter* (1961).

ues of integrated electron content along those paths. This would still be true for D_5 at the slightly higher middle latitudes of Seattle and Unalaska (Fig. 1.15).

A remarkable annual variation in magnetospheric electron density began to appear as we generated monthly averages of D_5 for the IGY stations in 1958 (Fig. 1.16). There was a clear high density maximum in December and a minimum in June, implying a magnetospheric electron density variation by a factor of ≈ 2, the square of the Dec/Jun D_5 ratio. As noted below, the annual variation remains little known in the space physics community and is not well understood theoretically.

1.2.10 My farewell to the VLF group in 1959 and quick return

An incident In the Spring of 1959 was decisive in my career. I had earlier met a young professor George Bahrs at the men's swimming pool, a bastion eventually desegregated by defiant women who entered one day and jumped into the pool stark naked. In the fall of 1958 George had offered me a research assistantship in his lab, and I had agreed to take it as soon as my class requirements for the PhD were met. When beginning at Stanford back in 1956 I had not intended to go beyond the Master's degree, but by 1959 the idea of staying on and doing more research had begun to take hold. I realized that there were thesis possibilities in the VLF group, but now felt committed to a different specialty, non-linear transistor circuits (whatever that meant). Thus one morning in the Spring of 1959 I said goodbye to VLF and walked down the hall to George's lab, where I was told that another student had been given the position. George had apparently forgotten what we had said many months before. I then walked back down the hall and rejoined the VLF group. Had I really left?

A few years later I heard that George Bahrs had died, apparently by his own hand. Fate seemed to have steered me back to VLF, and ordained that I continue looking at data.

1.3 Key developments in the use of whistlers to probe magnetospheric density structure

My work in mapping magnetospheric thermal plasma structure beginning in about 1958 was made possible by the earlier work of Owen Storey and Bob Helliwell, and by 'the recent contributions of Bob Smith (Fig. 1.17).

Figure 1.17: Bob Smith

1.3.1 Whistler propagation in ducts

In studying the anisotropic nature of whistler propagation, *Storey* (1953) had recognized that the energy flow at the lower whistler frequencies was constrained to be within 19° of the direction of the magnetic field, thus apparently undergoing the guiding necessary for hemisphere-to-hemisphere propagation. *Smith et al.* (1960) pointed out that Storey had observed long duration whistler echo trains with low decrement from hop to hop and had speculated that the trains were propagating in columns of auroral ionization that might prevent the spreading of energy and thus explain the low inter-hop decrement.

Smith (1961) then argued persuasively that propagation of whistlers received at ground points must occur along discrete geomagnetic-field-aligned paths. Preliminary evidence for such paths had appeared in the form of the multi-component nose whistler reported by *Helliwell et al.* (1956) and in the occasional detection in 1957 of two closely spaced but separate time delays of whistler-mode signals propagating in space to South America from the NSS transmitter in Annapolis, Maryland (*Helliwell and Gehrels*, 1958) (see Fig. 1.13). Smith noted that a single whistler could contain multiple components, apparently the result of propagation on separate paths; the $f - vs - t$ properties of successive multi-component events were similar, irrespective of the location of the lightning source, and the time intervals between successive hops of multi-hop whistlers were identical within experimental error.

Smith noted further that, as *Yabroff* (1961) had demonstrated, the wave-normal angles of whistlers with respect to the magnetic field should increase rapidly during initial upward propagation, thus ruling out the possibility of later ionospheric penetration from above. It was postulated that the discrete paths were in fact tubes of enhanced ionization or 'ducts', in which the wave energy was trapped and guided. As shown in a previous paper by *Smith et al.* (1960), waves with frequency below half the local gyro-frequency would remain within a limiting cone of angles, the size of which depended upon the density enhancement factor of the duct. As the trapped whistler-mode energy moved between duct endpoints in the conjugate hemispheres, it would follow a snake-like path back and forth across intra-duct peaks in the refractive index. The total travel time would be approximately the same as that of a wave with propagation vector oriented strictly along the magnetic field. The density enhancement factor for trapping was found to decrease markedly with increasing latitude, such that while trapping near the equator should be rare, as had been observed, it should be a common occurrence at geomagnetic latitudes near 40° and above, as various investigations had revealed.

The duct hypothesis greatly increased the diagnostic potential of the whistler method, since it essentially freed the observed dispersive properties of the whistler from dependence upon a highly variable and poorly known parameter, the lightning source location.

1.3.2 Smith's work on a universal whistler dispersion curve

Smith had good reason to believe in a universal dispersion curve for whistlers, one that would permit upward extrapolation from those parts of the low frequency whistler traces that were clearly displayed on the records. First of all, the evidence for ducting indicated that the plasma distribution along any duct was dictated only by forces acting along the field line, and not by cross-field diffusion from nearby regions. Second, it was known that based upon propagation theory, a nose would form at a particular value of $f_n/f_{Hmin} \approx 0.38$, more or less regardless of the density distribution along the field lines (this being in large part due to the substantial fraction of its total delay that a whistler would spend in the near-equatorial region). Third, he knew that if you integrated the value of $D(f)$, normalized to its zero frequency value, versus f/f_n) along any particular dipole field line at latitudes of interest, you would get a curve that varied only by a few percent over a wide range of latitudes (Fig. 1.18).

Smith then suggested that this curve be approximated by $D(f)/D_o = S_1(f/f_n)$, where S_1, a function of f/f_n only, was based om a complete Elliptic integral of the second kind. Since this expression had two unknowns D_o and f_n, they could be

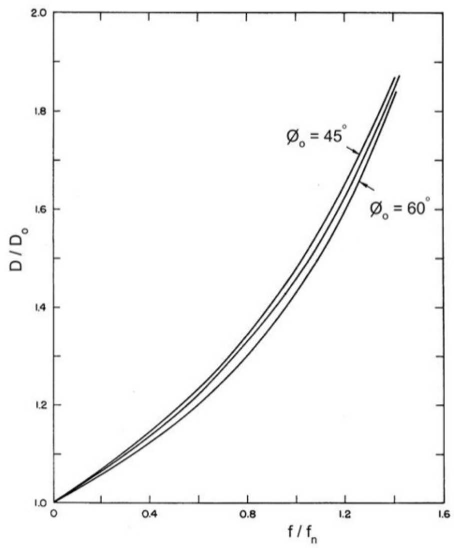


Figure 1.18: Smith's universal dispersion function along **B**. From *Helliwell* (1965).

obtained by two independent measurements of D on any whistler. This was accomplished by graphical methods, as noted below.

Smith's theoretical work was dramatically supported by a multi-component nose whistler event recorded at Seattle on June 7, 1959 (upper part of Fig. 1.19). In this clear case there were two closely spaced lightning sources (arrows in lower margin), each launching a whistler along multiple field line paths with L values ranging from ~3 to 5. The shapes of the various whistler traces, when normalized to the nose frequency and nose delay were remarkably similar, as one might have predicted from the fact that such multiple waves propagated along a spatially distributed set of dipole-like field line path segments, each of which was centered on the magnetic equator.

1.3.3 The need for curve fitting on non-nose whistlers

Smith's work provided a critical tool for remotely mapping the magnetospheric thermal plasma distribution. Most whistlers recorded at the middle latitude IGY stations were eventually found to have propagated along field lines at L values in the range $L = 2 - 4$, and a large fraction of these, for example most events received at Stanford, were limited to $L = 2 - 3$. Their spectra tended to be well defined only in some range below $\approx 13kHz$, in large part due to a falloff above $\approx 10kHz$ in the efficiency with which most middle latitude lightning sources could excite whistler paths. The energy spectra of such lightning sources had been found by Helliwell et al. [1958] to peak near 5 kHz.

The universal dispersion function provided a basis for estimating the values of f_n and t_n associated with any whistler component, thus enabling us to obtain an altitude distribution of equatorial density values from a compilation of individual events that propagated over a range of L values or from multi-component whistlers such as the one shown in Fig. 1.14. A convenient graphical method of estimating f_n and t_n from travel time measurements at two widely separated frequencies along the whistler trace was explained by *Carpenter and Smith* (1961).

1.4 Mapping the cold plasma in space

Beginning with Storey's measurement of high altitude plasma density at $L \approx 4$, and continuing with Allcock's profile estimates, it was only a matter of time until whistler workers could begin to provide maps of plasma density versus equatorial radii as well as variations with time at given distances.

Smith and Helliwell (1960) took some of the first steps, extending the measurements to lower latitudes by curve fitting at lower L. It was shown that the entire density profile between $L = 3$ and 5 exhibited the December maximum and June minimum that had earlier been found in the D_5 data at Stanford and other stations near $L = 3$.

1.4.1 Alex Dessler's 1959 visit and a look at magnetic storm effects

An even more remarkable effect was found in the aftermath of a visit to Stanford by Lockheed Space

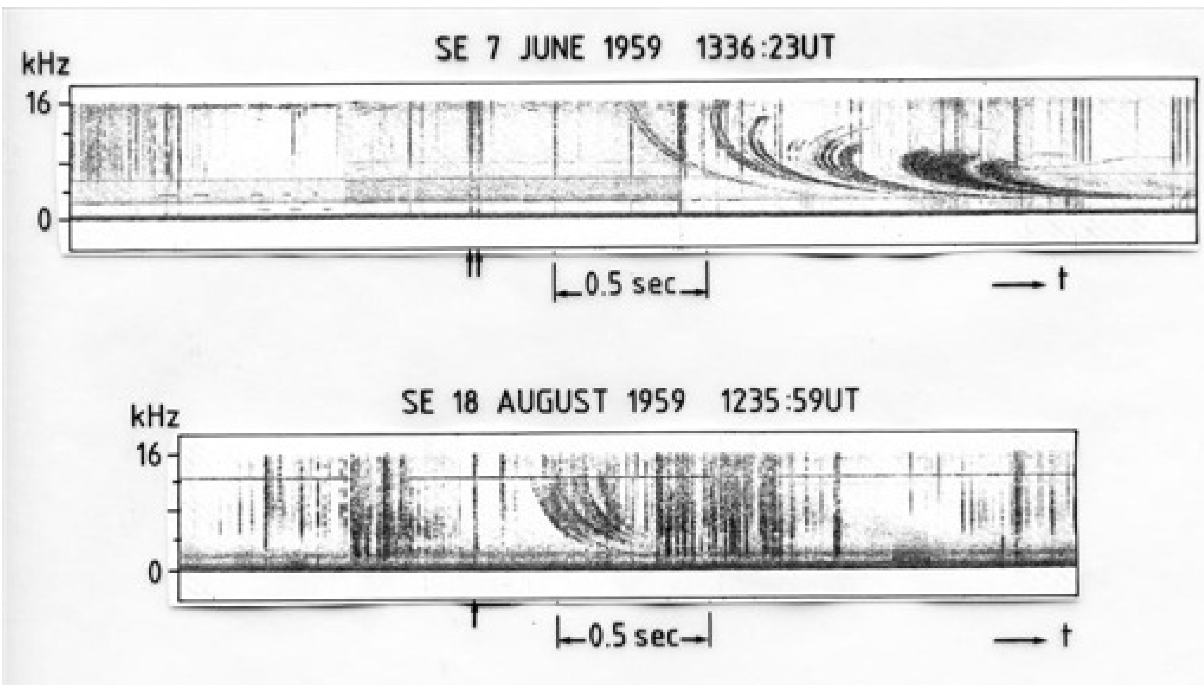


Figure 1.19: Magnetic storm effect on whistler dispersion; quiet time nose traces above, storm-time event below. From *Carpenter* (1962).

physicist Alex Dessler, who suggested that we look at our data on electron densities for effects associated with disturbed geomagnetic conditions that had begun on 16 August, 1959 during the operation of a satellite. The K_p index had reached 8+ twice on 18 August.

This was a multi-month-long period of high sunspot activity, higher than during any solar maximum in the previous 200 or so years. Without realizing it, we neophyte students of "space weather" had begun work under the most extreme weather conditions possible. There was an upside, however, in that effects appeared in our Stanford and Seattle data that might not have been so prevalent during the quieter parts of the 11 year sunspot cycle. The problem, if any, was that we were slow to appreciate the meaning of these effects.

The wide separation of Seattle and Stanford ($\approx 1500km$) exceeded the 500 km radius of their nominal effective collecting areas, so that the same whistler trace was not usually seen at both locations. However, there were tantalizing and confusing variations on this. On 13 January, 1958, weak but differently dispersed whistler traces were observed to have similar time delays at 5 kHz at both SE and ST. However, the SE trace was more curved and nose-like, suggesting propagation on a higher-latitude path than that followed by the ST whistler and in a more tenuous plasma. Recognition of a possible density anomaly came thanks to our ability to identify the spheric that had caused both events.

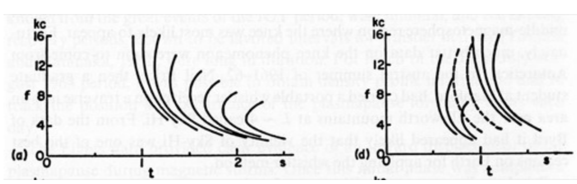


Figure 1.20: Sketches pf multi-component whistler confined to high density region inside the knee (left) and whistler following paths on both sides of the knee as well as in the region of steep density gradients (right). From *Carpenter* (1963).

The presence of what later came to be known as the "knee" effect, illustrated in Fig. 1.20, was more clearly indicated on later SE and ST records, for example on November 7, 1959 when a group of relatively strong, similarly dispersed components appeared with the same delay at both stations while several weaker traces with shorter delays appeared only on the SE record, These weaker, more dispersed traces, were later interpreted as having propagated to Seattle through a low density region outside the knee.

In 1959-1960 the concept of a persistent density knee was not yet well established, so Carpenter began placing in a special category of "crossing traces" those whistlers with common origin that did not nest together in a descending group, as in Fig. 1.20 (left), but fell on top of one another. There were other tantalizing observations, such as comparison of the elegant multi-trace nose whistler observed at Seattle during a quiet period in June, 1959 with one recorded during the disturbed period that Alex asked me about in August of that same year (Fig. 1.19, bottom panel). In the disturbed case, there were multiple traces, but their nested distribution at low travel times suggested that densities had dropped sharply over the entire L-shell range (outside the knee) that the traces had penetrated. For a given nose frequency, say near 15 kHz,, the travel time was less than in the quiet case by a factor of about 3, implying density lower by an order of magnitude.

1.4.2 Clarifying data from Byrd, Antarctica

The clarifying data came from Byrd Station, Antarctica, near $L = 7$. Two successive knee whistlers in Fig. 1.21 illustrate the large density differences between the paths outside the knee (group near $t = 1s$) and those inside. Fig. 1.22 shows a remarkably well defined knee whistler in frequency ranges 0-10 kHz and 0-5 kHz, its multiple components with nose frequencies in the range $\simeq 4 - 6kHz$ having followed paths in a region of steep density falloff near $L = 4$. Other components propagated well beyond this region, out to $L \simeq 7$, where noise at 1-2 kHz was triggered and echoed several times.

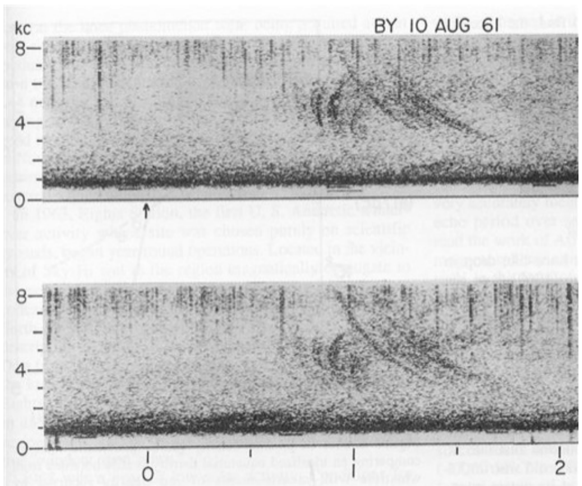


Figure 1.21: Two successive knee whistlers at Byrd in 1961. From *Carpenter* (1963).

1.4.3 The Axford and Hines article on magnetospheric convection

In casting about for understanding of this density effect, I fell nicely upon a 1960 article in the *Canadian Journal of Physics* by Ian Axford and Colin Hines (*Axford and Hines*, 1960). Those two fine gentlemen were contemplating the bulk motions of the plasma enveloping the earth, and finding that there was an inner region in which the plasma encircled the Earth and should remain high in density through contact with the ionosphere, and an outer region that did not encircle the Earth, was set in motion by the passing solar wind, and was only weakly coupled to the ionosphere (Fig. 1.23).

I was delighted to find this work. Axford and Hines were describing some kind of physical basis for outer and inner regions in space, only the inner of which encircled the Earth.

1.4.4 Konstantin Gringauz and an ion dropoff seen on Lunik 2

In 1960 and 1961 we began to see Russian journals in our libraries, most often in translation. From time to time there were articles by K. Gringauz, (*Gringauz et al.*, 1960), who had been involved in the particle measurements on the various Soviet lunar probes. Gringauz was writing in several cases about results from Lunik 2, which showed a faster than expected dropoff in the ion density profile with altitude as the rocket lifted off from near 60° degrees latitude and then moved generally equator-ward. The drop-off occurred between 2 and 3 R_E altitude as Lunik, having moved to lower latitude field lines, crossed toward higher latitudes. I showed an article describing the retarding potential ion traps to Bob Mlodnosky (see below), who indicated that the density claims by Gringauz could probably be believed. Eventually I realized that Lunik 2 had only briefly penetrated the plasmasphere, whose boundary the plasmapause had probably been close to 60° invariant latitude during its flight. Had the rocket been launched under more disturbed conditions, the plasmapause might have been well inside 60° and been missed altogether. I prepared a figure comparing the Lunik results as best I could interpret them with the general properties of an equatorial density profile based upon whistler data thus far measured (Fig. 1.24). The two were only loosely comparable since the rocket was measuring along an off-equatorial track.

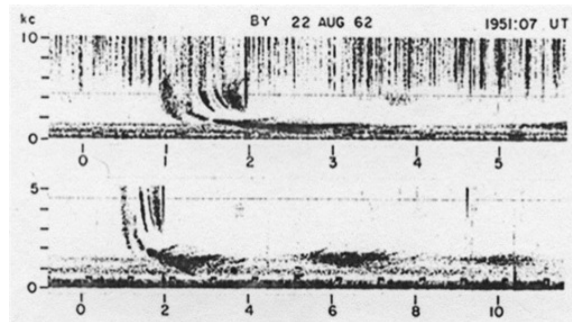


Figure 1.22: Spectacular knee whistler at Byrd in 1962.

My thesis in 1962 was focused on the density changes during magnetic storms, which by this time were being well documented. I didn't have any theoretical ideas that would explain the low densities, and simply suggested that they were attributable to some type of latitude dependent heating of the ionosphere.

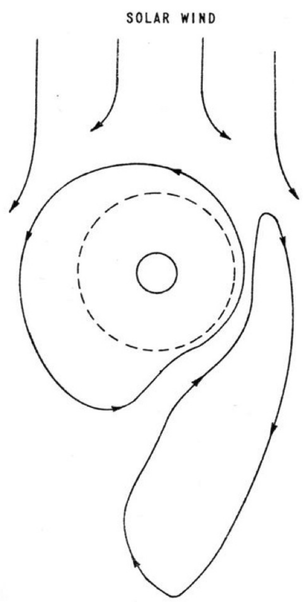


Figure 1.23: Theoretical convection pattern induced in the magnetosphere by the solar wind. Adapted from *Axford and Hines* (1960).

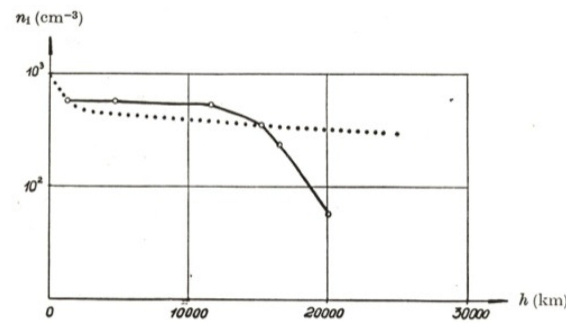


Figure 1.24: Ion dropoff with altitude detected on Lunik 2. Adapted from *Gringauz* (1963).

That the knee was a permanent feature of the plasma profile became clear as more data were accumulated and in particular as more cases from Antarctica became available, such as those in Figs. 1.21 and 1.22 above.. Announcement of the knee effect was made in 1963 (*Carpenter*, 1963).

Meanwhile, strong evidence of the at least occasional steepness of the density profile at the knee had emerged in a case of three whistler components received in 1958 near $L = 4$ at Unalaska. The three components, all from the same source, were spread out in time delay, but our curve fitting technique showed that all three had propagated at roughly the same L value (Fig. 1.26). We could not know how spatial details of the plasma density were distributed in longitude, but had found new evidence suggesting that the knee region itself was a particularly favorable one for ducted propagation between hemispheres.

Note that the words "plasmapause" and "plasmasphere" were not yet in use in the early 1960s. They were introduced in the major articles by *Carpenter* (1966) and by *Angerami and Carpenter* (1966) and were soon widely employed by the community.

1.4.5 The URSI Assembly in Tokyo, 1963

In 1963 Bob Helliwell was chair of the International URSI commission on Radio Noise of Terrestrial Origin, which in later years became the Commission on Waves in Plasmas. In those days one had to become an official national delegate to an URSI Assembly, and because of Helliwell's position, I easily became a delegate and was invited to give a talk on our new findings about the plasmapause.

That was my first international meeting and an overwhelming experience. I got to meet Yvonne Corcuff of Poitiers, France (Fig. 1.27), who became a lifelong friend. She was at the meeting with Professor Rivault, who ran a wonderful little lab in a wooded area outside Poitiers. In whistler data she had seen some of the same effects that were appearing in our data, but had not been able to develop a full picture of the knee effect.

In Tokyo I was awed by the presence of prominent physicists such as Jack Ratcliffe and Tatsuzo Obayashi. Fortunately, I had my own prominent guys with me, Ron Bracewell, a force in the radio astronomy world as well as Helliwell.

A special highlight of the Assembly for me occurred when Bob led me through a labyrinth of hotel corridors to find the office of the URSI secretariat.and to

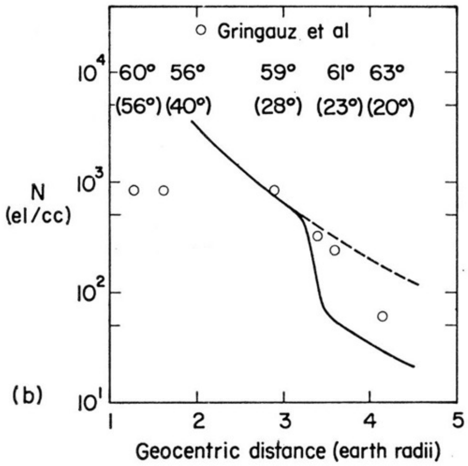


Figure 1.25: Lunik 2 Data. Invariant latitude (upper numbers), magnetic latitude (lower numbers) compared to idealized equatorial altitude profile from whistlers.

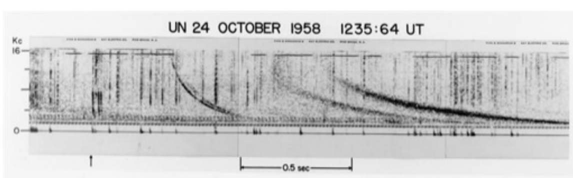


Figure 1.26: Three whistler traces that propagated at roughly the same L value. From *Carpenter* (1963).

meet there Jela Bogić, who was at that time and for many years afterward the URSI secretary (Fig. 1.28). I could hardly believe it. Here I was in Tokyo Japan, speaking with a wonderful Serbian woman, who also became a lifelong friend! It was almost too much. I don't recall how Bob knew that I spoke Serbian (from graduate work in political studies before Stanford), but somehow he did know it and made a point of seeing that Jela and I got together.

Figure 1.27: Yvonne Corcuff at the wedding of her niece Marina Galand in 2005

Figure 1.28: Jela Bogić at home in Brussels in 2002

1.4.6 Gringauz's Lunik 2 data supported by whistlers

There were at least two other highlights of the Tokyo Assembly. I purchased a Canon 8 mm film camera, which then served our family for a decade, and I met Konstatin Gringauz, of the Russian URSI delegation. I heard that Gringauz was staying at the Imperial Hotel in Tokyo, a building that had been designed by Frank Lloyd Wright and had survived a huge earthquake. It was the height of the cold war, not long after the Cuban Missile Crisis, and it was not a time when the Russians would mingle freely with others at international meetings.

At the hotel I asked to see Gringauz and he came down to meet me. I had no idea how important this moment was for him, as I showed him the graph I had made comparing the Lunik 2 and whistler results (Fig. 1.25). I only later realized that Gringauz' claim of a dropoff in thermal ions on Lunik 2 had not been widely believed in the USSR when first put forward for publication in 1960 (*Lemaire and Gringauz*, 1998). This was in part because of a fear in the USSR Academy of Sciences of the embarrassment that would attend publications with which the global science community would strongly disagree. The opponents of Gringauz were apparently unprepared for plasma distributions that were highly organized by the **B** field but which could vary widely in the trans-**B** direction. Lunik 2 had apparently been moving transverse to **B** enough to sense the trans-**B** density gradient.

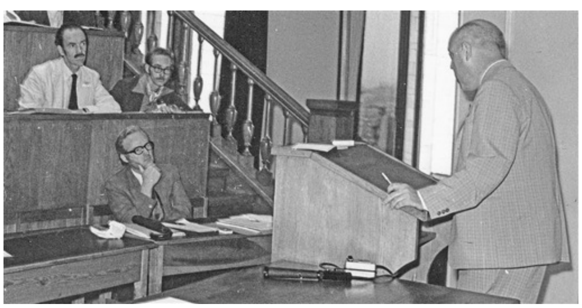


Figure 1.29: Gringauz speaking at the 1972 URSI assembly in Warsaw

I did not realize at that time how much the whistler-Lunik comparisons meant to Gringauz' career. From then on his standing at home must have improved, because he appeared at later URSI Assemblies and continued to report on results of Soviet density probes in space (e.g. Fig. 1.29).

1.5 U.S. Navy VLF transmitters as controlled whistler mode sources

Once it was known that whistler mode signals at 15.5 kHz from the NSS transmitter in Annapolis, MD could be readily observed in South America (*Helliwell and Gehrels*, 1958), the signals from NSS, NPG, NPM etc. became fair game. Pulse sequences lasting several minutes at a time were transmitted hourly as early as 1959, with recordings at Byrd Station, Antarctica and at Ushuaia, Argentina (a temporary location) as well as at receivers in the U.S. In this section we focus on issues of signal travel time in the magnetosphere, later paying attention to phenomena of wave growth and triggering.

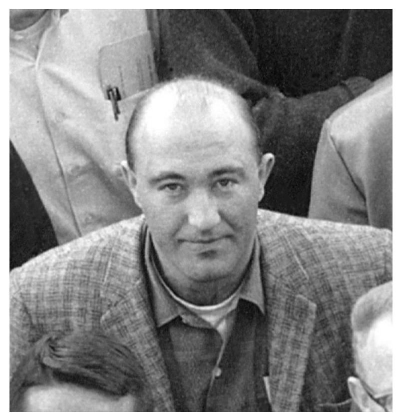


Figure 1.30: George Carpenter, 1960

George Carpenter (not my relation) joined the VLF group in 1960 (Fig. 1.30). His first research assignment under Bob Helliwell was to process magnetospheric whistler-mode signals associated with US Navy VLF transmissions. To first had to upgrade the existing processing equipment, which consisted of an oscilloscope operated in an A-scan mode and a 35-mm motion camera to record the scope display. The input to this processing system was the intermediate-frequency (IF) output of a narrowband VLF receiver recorded on analog magnetic tape. The scope sweep was triggered each time a pulse was received at the

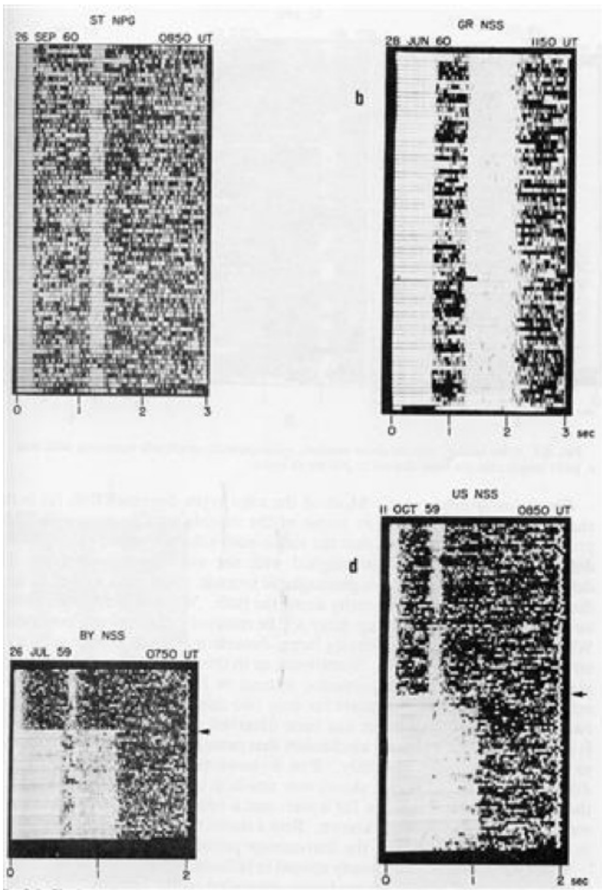


Figure 1.31: US Navy Transmitter signal delays. Adapted from *Helliwell* (1965).

remote location over the subioonospheric path from the transmitter. A film speed of about 1 cm per minute produced a waterfall display with the direct transmitter pulses aligned at the left and the whistler mode signals at a delayed time appropriate to the additional length of the magnetospheric propagation path.

Signals from two sources, NPG (18.6 kHz, Jim Creek, Washington, 200 kW) and NSS (80 kW) are illustrated in Fig. 1.31, which shows time delay on scales of 0-3 s or 0-2 s from the leading edge of a ground pulse. The data were recorded from top to bottom for up to 4 minutes, At upper lift in the figure are signals from NPG received at Stanford. Two hop pulses of $\approx 200ms$ duration arrived with a delay of about 1.2 s. At upper right are receptions at Greenbank, W. VA. of 600 ms pulses from NSS. The delay is $\approx 1.2s$, with some indication of spreading in time

Below from 1959 are one hop NSS signals detected at Byrd Station, Antarctica, and at Ushuaia, Argentina. At the points marked by arrows the pulse length was changed from about 100 ms to 600 ms. The time delays are 0.6 to 0.7 s.

An FM technique was devised by Helliwell and Bud Rorden and applied by George Carpenter for use with NPG. The transmitter frequency was swept linearly from 18.65 kHz to 18.55 kHz over successive 2-s periods. The echo signals and direct signal were envelope detected to produce difference frequencies, the lower of which at $\approx 40Hz$ indicated a travel time of 0.8 s. The analyzed results became the basis of George's Engineer's thesis in 1964.

The transmitter studies provided information on a multitude of whistler mode propagation topics, including echo fading, discrete echoes on separate paths, echoes spread in time, and irregular echoes with complex temporal variations. The two hop echoes of NPG at Stanford were received after roughly 10 percent of the transmitted pulses. Absorption in the ionospheric D region at one or both of the path endpoints was found to be a controlling factor in limiting the stronger signals to periods of darkness.

There were marked day to day changes in NPG two hop echo activity at Stanford, explained in large part by a startling result obtained by *Carpenter* (1963), who compared NPG echoes at Stanford with HF scattering from meter scale irregularities north of Stanford in the ionospheric F-region through which the whistler mode echoes were thought to pass. The correlation between NPG echoes and the F region irregularities was almost unbelievably high, suggesting a close physical connection between the irregularities and propagation in whistler mode ducts.

1.6 Whistlers excited by nuclear explosions

Between 1953 and 1962 there were five above-ground U. S. nuclear tests, two in Nevada and three near Johnston Island in the Pacific. Each of them acted as a source of an observed whistler. The story here is based on data analysis by D. Carpenter and discussion of the topic in Helliwell's 1965 monograph.

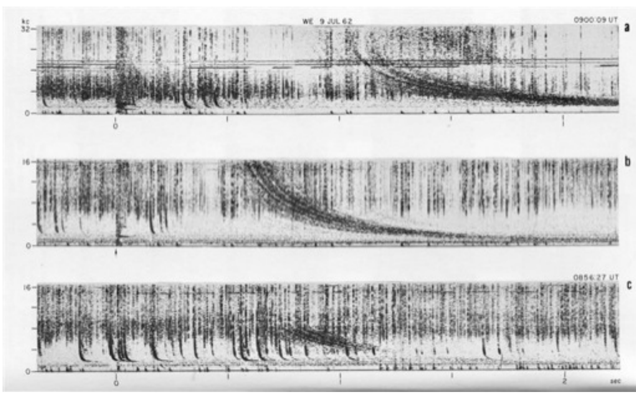


Figure 1.32: Starfish Prime spectra (upper two) recorded at Wellington, N.Z on July 9, 1962. Below is a natural whistler recorded 4 minutes earlier. From *Helliwell and Carpenter* (1963).

1.6.1 Starfish Prime, 1962

Whatever the altitude of the explosion, the whistlers produced looked as if they had originated in a radio atmospheric in the Earth-ionosphere wave guide. Thus Starfish Prime, with 1400 kT yield at 400 km altitude, did not show the incremental dispersion that would be expected if it had originated above the peak of the F layer. Fig. 1.32 shows two spectrograms from the Starfish Prime whistler recorded at Wellington, N. Z.,and a third, 0-16 kHz record, from a natural whistler recorded a few minutes prior to the test. The test whistler appeared to follow the ducts available for natural events but was well defined up to frequencies near 30 kHz, well above the $\approx 10kHz$ upper limit of the natural event.

From dispersion measurements it was found that the whistler had propagated to Wellington on paths near 50° latitude, with endpoints well poleward of the 14° latitude of Johnston Island.

1.6.2 Hybrid whistlers

Teak, a Megaton test in 1958 at 77 km near Johnston Island, produced a weak whistler at Stanford with the characteristics of a "hybrid" event, for which the spheric travels in the Earth Ionosphere waveguide to the opposite hemisphere before exciting a one-hop path leading back to the hemisphere of spheric origin.

1.6.3 George Carpenter's work on EMPs

In the years of early whistler studies, our research group did not pay close attention to the physics of the electromagnetic pulses, or EMPs, that are associated with terrestrial lightning. Later we gave indirect attention to that topic through the acquisition of Sanborn chart records of VLF transmitter signal amplitudes as they were affected by nuclear detonations.in the 1950s and 1960s.. Dramatic departures from normal solar cycle effects as well as the effects of solar flares were observed during the US and USSR high-altitude nuclear tests in the summer and fall of 1962. George was encouraged to carry out extensive processing of all the data recorded on analog magnetic tape at the remote field sites during the six 1958 US high-altitude tests and during the five 1962 US and the three 1962 USSR high-altitude tests.

The results of this work were instrumental in confirming a theory by Cullen Crain at the Rand Corporation that high-altitude nuclear tests fill the visible magnetosphere with neutrons that decay and deposit beta electron on magnetic field lines. Some portion of the beta electrons that spiral up and spiral down the magnetic field lines are deposited in the D region of the ionosphere, almost instantaneously affecting VLF propagation over a large fraction of the world. The results were documented in a Stanford University report, but funds to publish it were not available. The report was eventually published by Stanford Research Institute with a Stanford University cover and was widely distributed (*Carpenter*, 1964).

At SRI after 1964, George further analyzed these data and developed computer codes to predict the effects of high-altitude nuclear tests on VLF propagation. Magnetic tapes were processed at SRI to display the electromagnetic pulse (EMP) waveform observed at each of the remote locations for each of the high-altitude tests. In almost all cases, the EMP saturated the highly sensitive whistler measurement and recording systems. (As an interesting historical note, the EMP from the three Argus tests was only recently (2007) identified in the broadband VLF signals recorded by Stanford University in the Azores and Spain in 1958). Those results are contained in a two volume history of EMP measurements that George completed in 2008 (*Carpenter*, 2008).

1.7 A photo tour of the VLF lab in 1961

One of the pleasures of historical work is the renewed contact it provides with colleagues whose memories and in a few cases, long held photographs, become unique resources for the work in hand. Thanks to George Carpenter, we can offer here a set of casual photos depicting the VLF lab at Stanford in 1961

Bob Smith is shown in Fig. 1.33a, sitting on the floor in his office. I cannot recall having seen him thus, and am pleased to enrigh my good memories of him with this shot.

Nick Dunckel (Fig. 1.33b) joined the VLF group in 1960 and as discussed in Section 2.7 assisted for some time in the preparation of spectrogaphic illustrations for Helliwell's 1965 monograph.

Charlie Carpenter (Fig. 1.33c), Don's younger brother, spent a year doing whistler analysis and secretarial work in the lab in 1960-1. He had previously been recovering from leg injuries suffered in a rock climbiing accident in Southern Oregon.

Jerry Yarbrough, featured in Fig.1.33d, deserves special mention. Jerry spent his entire working career at Stanford, generating data records of one or another kind. He began with the Sonagraph, the machine by which he is standing in this shot, but later moved on to operate devices such as the Rayspan and Ubiquitous Analyzers, which were capable of generating real time 35 mm f-t records. Jerry became broadly and deeply familiar with VLF noise and whistler phenomena as they appeared in the data and helped many of the graduate students find good examples for illustration as well as new effects of which they had been unaware. His work with the voluminous Siple transmitter data was exemplary, providing data examples for both theses and published articles.

Photo Fig. 1.34a is of Herb Crary, whose thesis in 1961 was entitled "The effect of the Earth-ionosphere waveguide on whistlers." My memories of Herb are few, and tend to focus on the friendly way in which he would occupy the doorway of your office, listen sympatherically to your statements about problems, and then urge you to recall that "we're in this for the long pull."

Lloyd Provan (Fig. 1.34b) was a familiar figure in the lab, especially in the 1960s and 1970s when we produced lots of illustrations that depended on the work of draftspeople and film photography.

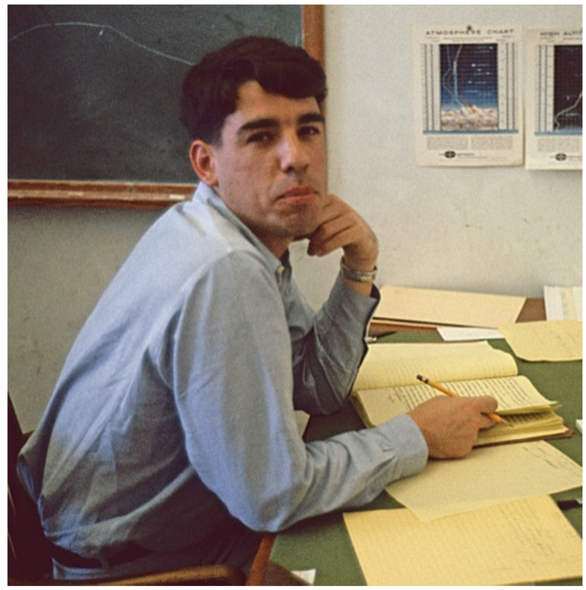
The rare photo of Don Carpenter with Bob Helliwell (Fig. 1.34d) shows us in the lab in the Electronics Research Laborotry (ERL) in 1961.

1.8 Eights Station, Antarctica

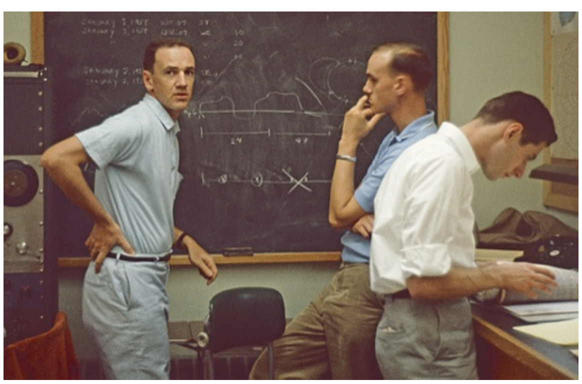
In the austral summer of 1961-2 an enterprising graduate student from Australia, Neil Brice (Fig. 1.35), carried a portable whistler on a traverse into an Antarctic area called Sky-Hi at $L \approx 4$ near the Ellsworth mountains. Based on what had been found at Byrd ($L \approx 7$), it was clear that a receiver near Sky-Hi (along the 75° west meridian) would be ideally located for receiving whistlers in the winter. To further justify establishing a year-round operation there, it was hoped that there would be whistler activity in the summer as well.

Neil came back with some astonishing examples of December whistlers. Some had well defined components covering wide ranges in L value, with delays as long as 6 s for paths near $L = 6$. It was wonderful, additionally supportive of the previously identified December peak in annual plasmasphere density.

(a) Bob Smith, on the floor in his office.



(b) Nick Dunckel, at his desk in 1962.



(c) Don Carpenter, his brother Charlie, and Mike Trimpi at right.

(d) Jerry Yarbrough by the Sonagraph in 1961.

Figure 1.33: VLF Group members.

(a) Herb Crary at his desk.

(b) Lloyd Provan, photographer for the Electrical Engineering Dept.

(c) Ulla Lundquist, who worked extensively measuring details of whistler records.

(d) Bob Helliwell talking with Don Carpenter in the ERL data lab.

Figure 1.34: More VLF Group members.

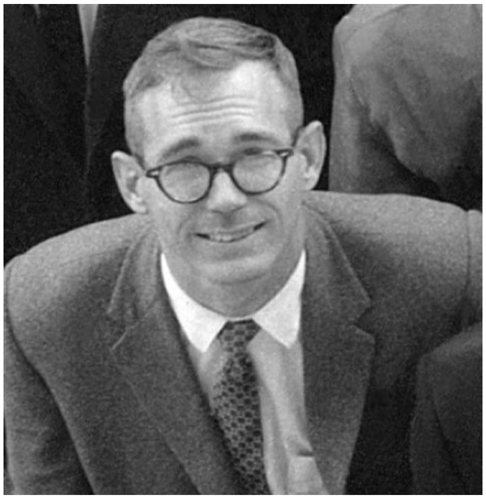


Figure 1.35: Neil Brice at Stanford, 1960

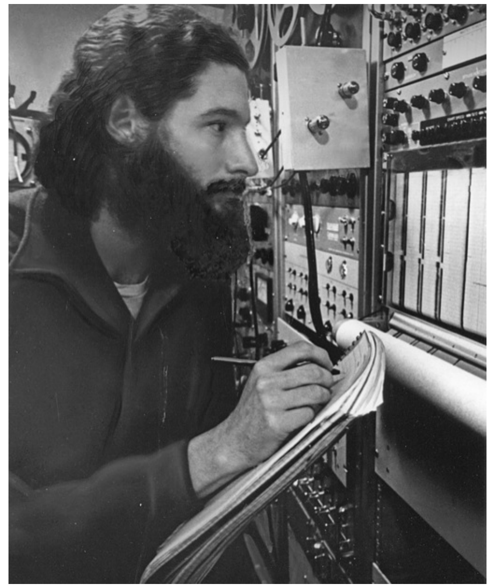


Figure 1.36: Bearded Mike Trimpi, taking notes at Eights in 1963. New York Times photo by Allyn Baum.

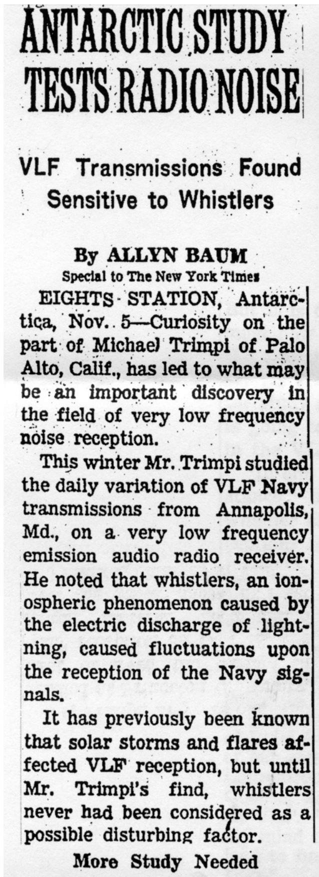
Lin Martin, visiting scientist from New Zealand, began preparations for Stanford's participation in work at Eights Station, to be built in the vicinity of Sky-Hi and occupied for winter-over science in 1963. The final stages of preparation and management of the field activities were undertaken by John Katsufrakis.

1.8.1 Mike Trimpi, legendary field engineer, discoverer of the "Trimpi Effect"

In my view Eights Station was one of the wonders of the world. First of all, there was Mike Trimpi, our field engineer (Fig. 1.36), who not only ran the $2 - min - hr^{-1}$ synoptic whistler program but also managed to make daily 3-hour continuous recordings (at the times of similar recordings at Byrd) whose period of coverage changed every week or so. It was a moderate year of solar activity, so we were regularly seeing what we now call "space weather". but only to the extent that there were weak magnetic storms two or three times a month. Thanks to their high occurrence rate, whistlers continued to appear throughout the storms and tell us what was going on. And thanks to a year or so of previous work back at Stanford, Mike was attuned to the varieties of VLF noises that one could hear at a ground station.

In late November, 1963, a science writer from the New York Times named Allyn Baum came to Eights and interviewed Mike, An article by Baum in the Times on November 26, 1963 (Fig. 1.37), described a remarkable new discovery that Mike had made in the preceding weeks (nly reported in a peer-reviewed journal ten years later in 1973, this discovery eventually came to be known as the "Trimpi Effect".) During recordings of the amplitudes of fixed frequency Navy transmitters NSS (Annnapolis, MD), NAA (Cutler, ME), and NBA (Canal zone), Mike had noticed fast (1 s) changes in level followed by slower (10-20 s) recoveries. Such changes occurred several times an hour, and at each change a whistler could be heard to arrive from a lightning source in the north.

Baum's article in the Times was most perceptive, lauding Mike for his work and quoting him at some length. For example, as follows: "in one of my charts ...I have one whistler affecting the VLF stations in



ANTARCTIC STUDY TESTS RADIO NOISE

VLF Transmissions Found Sensitive to Whistlers

By ALLYN BAUM
Special to The New York Times

EIGHTS STATION, Antarctica, Nov. 5—Curiosity on the part of Michael Trimpi of Palo Alto, Calif., has led to what may be an important discovery in the field of very low frequency noise reception.

This winter Mr. Trimpi studied the daily variation of VLF Navy transmissions from Annapolis, Md., on a very low frequency emission audio radio receiver. He noted that whistlers, an ionospheric phenomenon caused by the electric discharge of lightning, caused fluctuations upon the reception of the Navy signals.

It has previously been known that solar storms and flares affected VLF reception, but until Mr. Trimpi's find, whistlers never had been considered as a possible disturbing factor.

More Study Needed

Figure 1.37: Article in New York Times, Nov. 26, 1963.

completely opposite manners. On one frequency, the VLF reception increased, simultaneously, the other frequency decreased....I think it is going to be necessary for us to go over all the VLF records of Byrd Station as well as those of our conjugate point, Parc de Laurentides, 100 miles north of Quebec, as to whistler effects on VLF. There is no doubt in my mind that this finding must lead to further studies of whistlers."

Through curiosity and diligence, Mike had found his way to the problem of wave induced bursts of energetic electrons from the radiation belts into the Earth's neutral atmosphere. Years were to pass before the full dimensions of this problem began to be revealed.

Figure 1.38: Angerami at Stanford, 1960

1.8.2 Jacyntho Angerami

Thanks to the 1963 Eights data, it became possible to extend our description of the plasmasphere both in space and in time. Jacyntho Angerami, one of our students from Brazil (Fig. 1.38), was the master of the radial profile of plasma density as well as of estimates of the variation with latitude in tube electron content above a square centimeter at 1000 km altitude and of the plasma density profile along the field lines. Exquisitely careful in making and interpreting measurements, he paid attention to the accumulating reports from Alouette of plasma density at 1000 km and then compared these with downward extrapolations of whistler data on equatorial density or with total flux tube content along field lines both inside and outside the plasmasphere. He concluded that the field line profile inside the plasmasphere was consistent with a diffusive equilibrium distribution, as was being discussed at the time (*Angerami and Thomas*, 1964), and that the profile in the tenuous region outside the knee fell off much faster, approaching R^{-4} as predicted for an ion exosphere by *Eviatar et al.* (1964).

Angerami's work showed that he had paid close

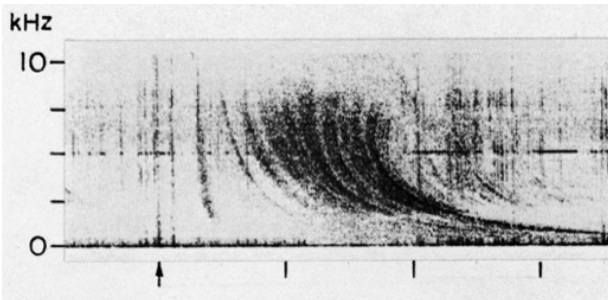


Figure 1.39: Eights night-time knee whistler from July 8, 1963, indicating large density falloff and irregularity at the plasmapause. Adapted from *Angerami and Carpenter* (1966).

attention to the comprehensive data set acquired in 1963 at Eights that covered the full diurnal cycle and provided a basis for detailed study of the way in which the plasmasphere was eroded and then recovered during the course of magnetic storms. Taking advantage of the high levels of whistler activity during most phases of disturbance, he explored the density dropoff at the knee (Fig. 1.40), which sometimes reached a factor of 30 to 100 on the nightside within an equatorial distance of less than 0.15 R_E. One of his most remarkable (and largely unrecognized) contributions was demonstration of irregularity in plasmapause radius, on occasion as great as 0.4 R_E, as seen within the longitudinal wedge of $\pm 15°$ estimated to be the whistler "view" from Eights Station (this finding was later supported by sweep frequency receiver (SFR) data from the eccentric orbiting ISEE and from CRRES as those spacecraft moved longitudinally near 6 R_E (see below)).

Jacyntho used his skills to clarify questions being raised by a Stanford research group who were looking at radar echoes from the moon and who had at first suggested that there were about $100 el - cm^{-3}$ all along the echo path from the Earth to the lunar surface. This really worried those of us who had some experience with whistler-mode density probing. We gratefully turned to Jacyntho who, having worked with whistler data and being in no rush to uniformly weight integrals, showed that most of the electrons along the cis-lunar path were actually crowded comfortably into its first ten or twenty thousand km.

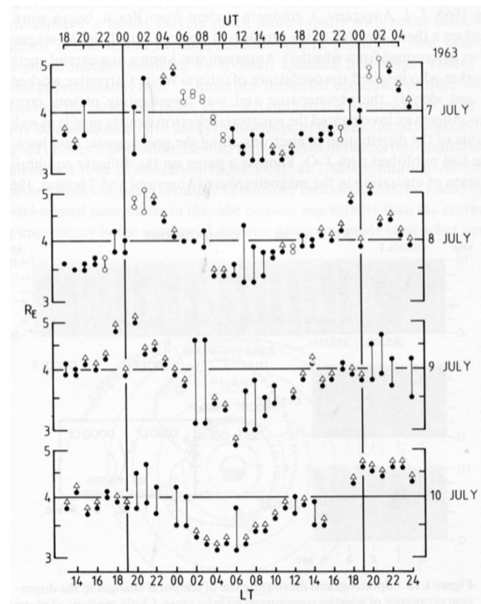


Figure 1.40: Diurnal variation of plasmasphere radius on four days in 1963. From *Carpenter* (1966).

1.8.3 The late afternoon bulge in the plasmasphere

It was hard to believe the richness of the Eights data. Hour after hour throughout the Austral Winter days and through the course of weak magnetic storms, whistlers continued to appear. They provided clear evidence of an inward displacement of the plasmapause during magnetic storms, primarily in the night-morning sector. Once this initial phase was complete, a distinctive diurnal variation in plasmapause radius was observed for several days as relatively steady substorm activity continued into the recovery period (Fig. 1.40). We would see outward drift in the morning sector to a noon peak in plasmasphere radius (very likely part of the solar-driven SQ current system), and then the late-afternoon or evening changes associated with what we were beginning to call the "bulge". Fig. 1.41 was used to summarize the principal elements of the global model.

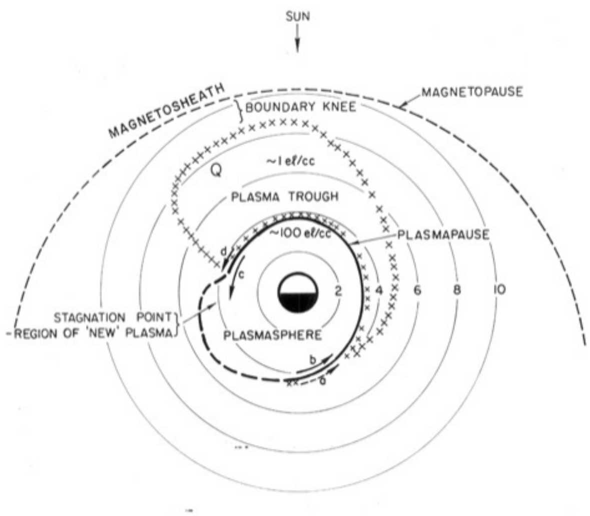


Figure 1.41: Summary of plasmasphere morphology, 1966. From *Carpenter* (1966).

A key detail was found as we tracked the plasmapause across the afternoon sector. Each day during a several-day-long recovery period, we would detect a sudden outward shift in plasmapause radius, indicating a localized sector of larger radius rather than a region of fast outward flow. Furthermore, the local time at which the sudden outward shift in radius would appear varied with the K_p index. If conditions were becoming more disturbed during local afternoon, the shift tended to appear earlier, while on average it appeared near dusk. If on the other hand the magnetic activity in late afternoon suddenly became deeply quiet, the extended plasmasphere region or "bulge" was not seen at all, and we continued to view whistler propagation on the same distribution of paths observed during the previous afternoon.

The effort to describe the plasmasphere as a global phenomenon culminated in a 1965 joint AGU-URSI symposium on Solar-Terrestrial relations held in the State Department auditorium in Washington D.C. It was the perfect opportunity. Ross Jewell (see below) assisted me in preparing a set of slides showing, among several items, 24 Eights whistlers from August 5, 1963, one from each hourly recording.

Meanwhile, a cartoon of a mouse drawn by B. J. O'Brien of Rice University and labeled "Anthropomorphic Magnetosphere" began circulating in the community (Fig. 1.42). It featured a number of mice body parts that included "nose whistlers" and "Carpenter's Knee", thus attracting to our VLF group an unaccustomed level of attention.

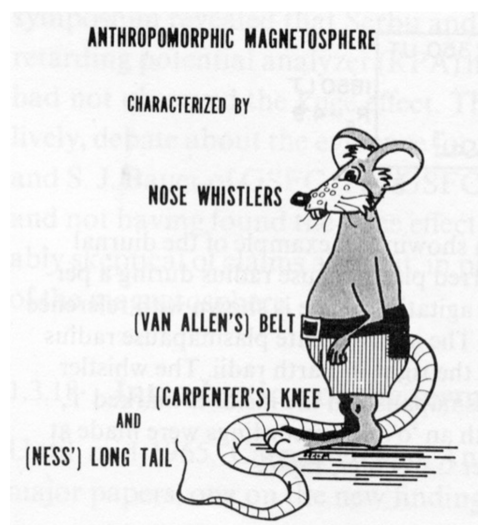


Figure 1.42: B.J. O'Brien's 1965 cartoon of the "Anthropomorphic Magnetosphere".

At the symposium, we heard about the work of H.A. Taylor, Jr., who had acquired data from an ion mass spectrometer on OGO 1 (launched in 1964, see below). Taylor and his colleagues at NASA Goddard Flight Center had observed pronounced light ion density decreases that appeared to be related to the whistler knee (Fig. 1.43), and were preparing an article for publication. On the other hand, questions from the audience revealed that G. Serbu and H. Maier, who worked in a Goddard group doing electron retarding potential measurements (RPA) on the IMP 1 and 3 satellites, had not observed the knee effect. In 1977 they wrote that "Our measurements of particle density do not indicate a whistler "KNEE" phenomenon (*Serbu and Maier*, 1967). In fact, we observe 10 to 50 times as large a density beyond the

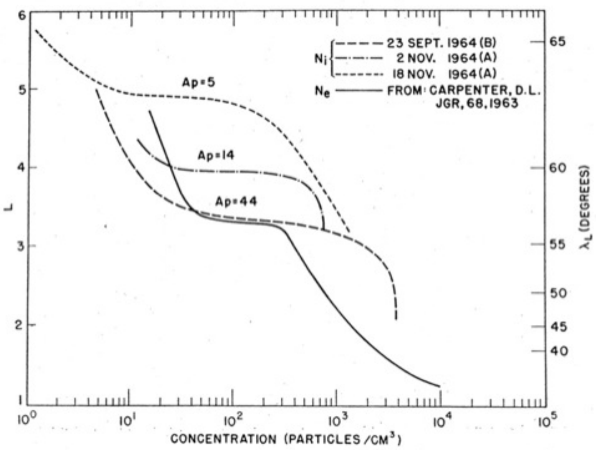


Figure 1.43: High altitude ion density profiles from OGO 1 in 1964. From *Taylor et al.* (1965).

"KNEE" as has been reported from whistler results." Thus was laid the groundwork for an exciting (to us) debate at the 1966 URSI Assembly about the reality of the knee effect.

Prior to the URSI Assembly in Munich, Carpenter had attended an Inter-Union Symposium on Solar Terrestrial Physics in Belgrade, where J. W. Dungey discussed the whistler results and in particular the dusk-side bulge. He interpreted the bulge as evidence of interaction between a sunward convecting plasma driven by the solar wind and the corotational motion in the plasma induced by the Earth. Similar views were soon published by *Nishida* (1966), by *Dungey* (1967), and by *Brice* (1967).

Only 13 years before, Carpenter had been a student of public affairs in Belgrade, speaking Serbian and drinking plum brandy with close friend Vitomir Blagojević, an older relative of the notorious Illinois governor.

The URSI debate about the knee effect between Carpenter and S J. Bauer of GSFC was a well attended Commission IV session on "new results." D. Gurnett and F. L. Scarf acted as reporters as Carpenter summarized the evidence that had been obtained from Eights, which now included data from an additional year of recording in 1965. He also provided a brief review of non-VLF sources of evidence, including ion density from satellites, slant columnar electron content to the ground at middle latitudes, and abrupt spatial changes in satellite observations of whistlers and of VLF noises such as triggered chorus. Bauer then defended the GSFC electron trap data from IMPs 1 and 3, pointing out that there was some evidence for a fairly rapid decrease in density but not for one as rapid as the one reported from whistlers or from Taylor's OGO 1 instrument. It was noted, however, that uncertainty in the effective collecting area of the GSFC instruments could modify the inferred densities downward by a factor of 2-3. In the ensuing discussion R. L. Smith and F. L. Scarf questioned the effective collecting area of the GSFC traps, since the whistler results would indicate a discontinuous increase in the sheath size by almost an order of magnitude as the spacecraft crossed the region of steep density gradients. Questions about the possible effects of geomagnetic field distortions and magnetospheric inflation were raised by Bauer and N. F. Ness. At the end of the debate there was an unofficial show of hands, for or against the knee. The outcome was not recorded, but Alex Dessler, who had started us on the road to studies of magnetic storm effects seven years before, stood up and said "I vote for Carpenter." It was one of those moments that a young researcher does not forget.

1.8.4 Ross Jewell

Ross Jewell began work with me in 1963-1964 on analysis of data from Eights and Byrd Stations. He had served as a field engineer for our group on the Eltanin research ship as it operated in the Southern Pacific Ocean, probably in 1962. I don't recall just how we got started on the Antarctic data except to say that I had certain ideas about what we should measure and, at least early on, Ross seemed in agreement on what we should do. It soon developed, however, that he wanted to emulate the statistical practices of the particle people and put together isodensity contours in the equatorial plane derived from averages of whistler data. For my part, I preferred to look on each day's data as a case study that had happened and needed to be understood in its own right.

The broader statistical setting to which it belonged would become clear in due time, as more cases were accumulated. In those days we were just starting to use the BOSCAR, a hulking machine that allowed an observer to review a 35 mm film of whistlers, select points of interest along the salient features of the data, and record their frequency-vs-time coordinates on punch cards. Ross was a whiz at using devices such as the BOSCAR, and he was clearly way ahead of me and others in our group on issues of computing in general.

In time Ross and I realized that we could not continue working together and he left the group. I was genuinely surprised at this, having previously thought that I could work with anyone. Had we been able to collaborate well, as was fortunately the case with others, he would have been a co-author of the 1966 discovery paper on the plasmasphere and its dynamics. As things were, I never heard from him again.

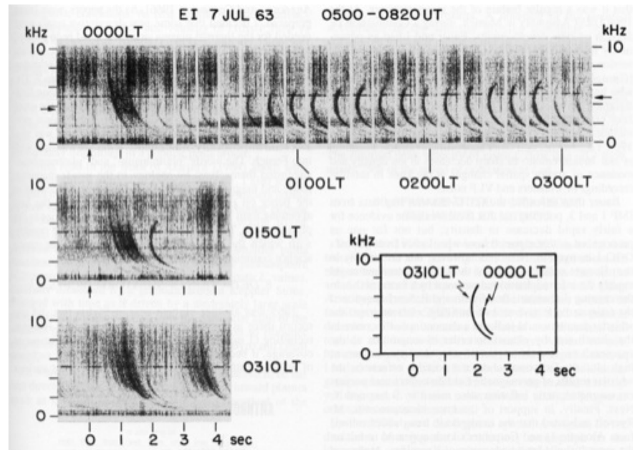


Figure 1.44: Eights spectra showing a 3 hour night-side inward drift in 1963. From *Carpenter* (1966).

1.8.5 The first tracking of cross-L plasma drift motions

In 1962 - 1963 I read in JGR about bulk plasma flow in the magnetosphere, and realized that it should be magnetic-flux preserving, such that thermal plasma existing within a given cluster of geomagnetic field lines, would tend to stay within that same cluster, even as the latter were displaced, say for example by the Earth's rotation. It occurred to me that the whistler ducts we had been studying could be physical evidence of this hydro-magnetic theorem. As a test, I found a period when long trains of multi-hop echoes were recorded at Eights and then pasted a 40-to-50 *s* succession of spectrograms on the wall in a long hallway. Just below this set I mounted a similar series from a recording roughly one hour after the first one. Both displays showed single path events that bounced back and forth between hemispheres. I was delighted to see that in the second event, the travel time at a given frequency to the tenth hop was less than in the first event by several tenths of a second. You could just stand in the ERL hallway and do the experiment.

If in fact ducts were undergoing bulk motions in the magnetosphere, there must be temporal changes in the nose frequency of a whistler that repeatedly follows a moving duct. Thanks to Mike Trimpi's inspired 3 hour continuous recordings in 1963, one such case (Fig. 1.44) served as an excellent illustration. We could display (at left) the overall form of the whistler at three times during a 3-hour night-time period, as well as (extending to the right) a succession of spectral samples showing only the highest-L-value whistler component as it drifted inward and showed a steadily increasing nose frequency..

I soon read in JGR an article that discussed the flow of a liquid past a rotating cylinder. Although the streamlines of the liquid flow were affected by the action of the cylinder, in a steady state there was a region close to the cylinder which the impinging liquid did not penetrate. It seemed clear from this and from the work of Axford and Hines that both the existence of the plasmapause and the markedly lower densities present in what we now called the plasma trough region were the result of unsteady interaction of the magnetosphere with the solar wind and hence little time for refilling of the outer, low density, region from below.

1.8.6 Plasma drift motions during substorms

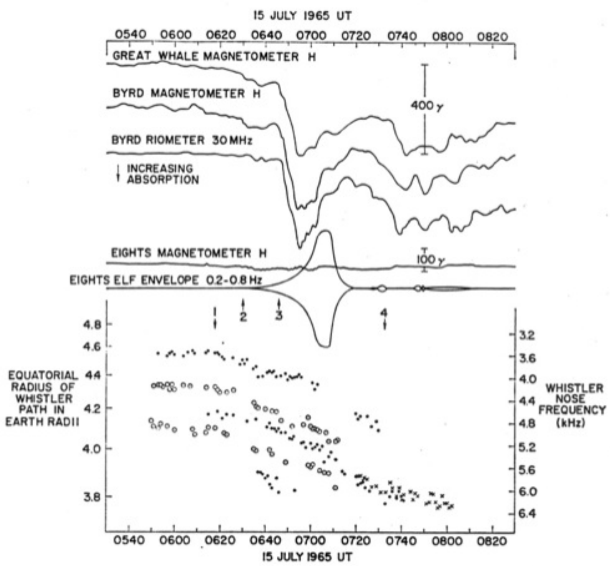


Figure 1.45: Inward nightside plasma drifts during a polar substorm in 1965. From *Carpenter and Stone* (1967).

Once we could measure the inward and outward (cross-L) movements of whistler ducts, it was only a matter of time until we could explore bulk motions associated with geomagnetic disturbances. In the early 1960s people were already trying to measure the magnetospheric electric field by flying high altitude balloons (Mozer) or seeding field line areas with barium clouds (Haerendel). These were not easy methods to apply, and as far as I know did not yield unambiguous results. However, at Stanford we were hearing about the work of people like Syun Akasofu, a student of Sydney Chapman's, who had been pulling together information on visual aurora and had defined what was called a "polar substorm."

Our field engineers such as Henry Morozumi at Byrd Station in 1962 and Rob Flint at Plateau Station in 1966 had paid attention to the high-latitude sequences of geophysical events involving visual aurorae and VLF noise activity. Thus I was on alert when in 1964 Akasofu presented the concept of a polar sub-storm as a 1 to 2 hour event focused on the night-morningside of the Earth (Akasofu, 1964). A polar substorm had some of the elements of a global magnetic storm but occurred on a local and temporally limited scale.

Mike Trimpi's wonderful Eights data from 1963 had revealed much about plasma motions, whetting our appetites for synoptic recordings every 5 *min* and longer continuous ones. These we got at Eights in 1965, including the famous multi-hour July 15 event illustrated in Fig. 1.45 (*Carpenter and Stone*, 1967). The whistler nose frequencies, reported on the bottom panel, were scaled by a talented student named Kepler Stone, who had helped with other papers in the 1960s but did not remain at Stanford. The event covered a several-hour time period after local midnight, showing that collective inward drifts began at $\approx 0620UT$ and reached speeds of a few tenths of an $R_E - hr^{-1}$. Above in the figure are plotted various ground magnetometer records and below them the envelope of a $P_i 2$ event, an object of study by various people in connection with the onset of sub-storms. This remarkable event was, as far as we know, the first clear indication of a sunward surge of convection in the nightside magnetosphere during a substorm. It was followed by a decade of whistler studies that gave further details on plasma drifts vs local time and geomagnetic conditions.

Whistler-based material remained a dominant source on the geo-electric field until well into the 1970s, when the first results from incoherent and coherent scatter radars became available.

1.8.7 Rivalries with other research groups

From the 1950s onward there was whistler mode research activity in several laboratories around the globe. As a member of a group, you pretty well knew which labs were more or less on your side and which were really hoping or trying to outdo you. In these latter cases there always remained layers of gentility,

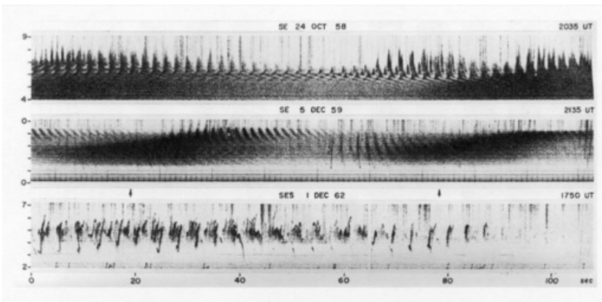


Figure 1.46: Long enduring periodic emission spectra recorded at Seattle and at Suffield experimental station during and after the IGY

such that intergroup relations could be nuanced, with the individual members of each group experiencing the rivalry to various extents, if at all.

PhD student Neil Brice made a number of studies of periodic VLF emissions in the early 1960s. Such emissions had been known to researchers for some time, and had been well defined during the IGY, as illustrated in Fig. 1.46. During his work on the emissions, Brice encountered a suggestion by Richard Dowden of New Zealand that so-called "dispersive" periodic emissions are generated by bunches of electrons spiraling back and forth along field lines (*Dowden*, 1962). *Brice* (1962) then showed that since such generation depended upon both whistler mode propagation delay from the electron source to receiver and the changing location of the source, the observed emissions could not be due to this electron bunch mechanism. Instead the non dispersive emissions could be explained by an emission launching event, followed by passive propagation back and forth along the field lines, thus leading to changes in emission $f - t$ shape from one echo to the next, as illustrated in Fig. 1.47 for SE 1 Oct 61.

With the Dowden group there was ongoing "competitive" activity: in the early 1960s on VLF emissions (as already noted), in the 1960s on identification of whistler sources, in the 1970s on VLF wave injection from experimental transmitters, and in the later 1970s and early 1980s on various aspects of wave-induced particle precipitation. This rivalry posed challenges to defend what we were doing and to further improve its quality.

1.8.8 Brice on multi-phase periodic emissions

Using excellent Antarctic recordings at Eights and Byrd as well as simultaneous observations in the North at Great Whale river and Quebec City, Canada, *Brice* (1965) was able to study multi-phase periodic emissions, emission trains with individual elements arriving at intervals less than 1/2 or 1/3 the two hop whistler mode bounce period. Brice noted that multi-phase emissions would sometimes continue for minutes at a time and that the two phase variety were substantially less stable or enduring than the three phase type,in which successive wave bursts tended to be separated by 1/3 the whistler mode two-hop bounce period. Brice suggested that the generation of each wave burst involved a relaxation process, during which energy for generating a new emission was pumped into the system.

COMMENT: if only two phases are present, successive wave bursts may be propagating in opposite directions such that they arrive simultaneously in the region of regeneration as candidate triggers for the next wave bursts. One phase may then tend to be weakened in comparison to the other. On the other hand, with three phases present, spaced by 1/3 the whistler mode two hop period, each arriving emission would be free to obtain regeneration by whatever energy sources are available.

1.8.9 Bob Smith's early memories of the 1950s and 1960s

Bob Smith, one of only a few grad students in the mid and late 1950s, was tasked with various field projects as well as later theoretical work both in the VLF group and at the computer center. Bob writes that his first trip was to Bermuda, where he got a whistler station working again. He made a few trips to Alaska, one to Unalaska way out on the Aleutian chain (a location of importance in mapping the plasmapause (see Fig. 1.15), then another to College, where the first nose whistlers were recorded (Fig. 1.10).

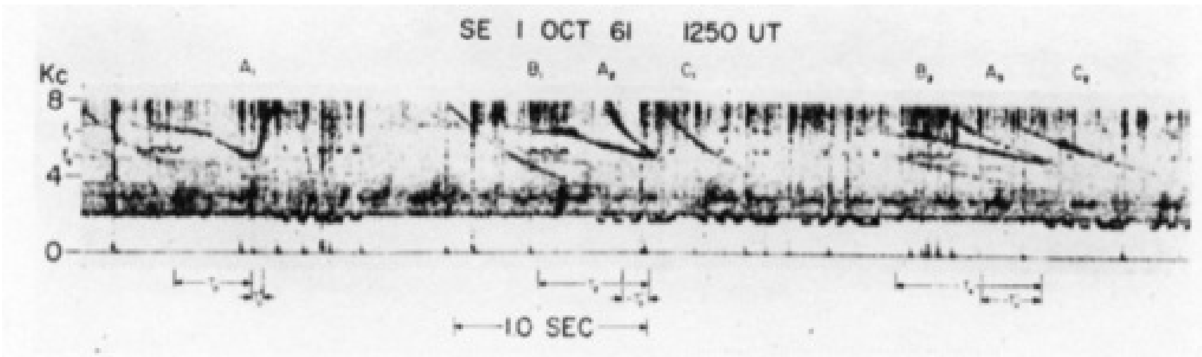


Figure 1.47: Dispersive periodic emissions recorded at Seattle in 1961. From *Brice* (1962).

Bob reports that he went to Kauai to observe the Teak and Orange high altitude nuclear tests (1955-1956), taking measurements outside and observing a very scary "rainbow" effect, essentially an aurora following the magnetic field lines.

To the long list of Bob's accomplishments while at Stanford (see also section on space observations) must be added his work at the computer center, where, inter alia, he developed a better algorithm for division of two complex numbers.

1.9 Helliwell's classic monograph

Bob Helliwell's 1965 monograph "Whistlers and Related Ionospheric Phenomena"was a masterwork. Today, some 50 years later, it remains the best reference on many aspects of VLF, especially the spectral forms of whistler-mode phenomena detected at ground stations. When the illustrations were being assembled in the early 1960s, many by Nick Dunckel (see section on Space Observations), Helliwell actively sought our comments on whistlers and VLF emissions, relying on our considerable expertise to identify features such as causative sferics, periodicities, numbers of traversals of field line paths (hops) being represented, etc.

In this VLF history we hope to illustrate the monograph's richness in terms of the "unpublished knowledge" that is present in the spectrographic figures, knowledge often mentioned in the figure captions but rarely touched upon in the main text. This hope can only be partially realized; the original material is just too rich and extensive. In what follows we present just a few gems from "Whistlers and Related Ionospheric Phenomena."

1.9.1 A third hop anomaly

The caption of Figure 4-10 begins with: "Anomalous echoes." As Fig. 1.48 in this history, It shows spectra of two events that were recorded simultaneously at Stanford and Seattle on March 23, 1958. The first event is from 1135 UT and the second from one hour later. The caption says that the first event is a three hop echo at Stanford with no discernible first hop parent. How can this be? How could there be a third hop without a first hop?

On the noisy Settle record a weak first hop echo is seen as well as the third hop. In the second event, an hour later, both the first and third hop echoes appear at Stanford, while at Seattle the third hop is not seen.

A tentative answer to these questions came in the 1980s in a JGR paper by Desanka Šulić, Andy Smith, and myself (*Carpenter et al.*, 1986). We found examples of whistlers with third hops that were more intense at the lower frequencies than those in the first hop. There had long been evidence that lightning whistlers underwent wave growth, such as the triggering of emissions by low frequency whistler tails and high ducted whistler intensity just below the

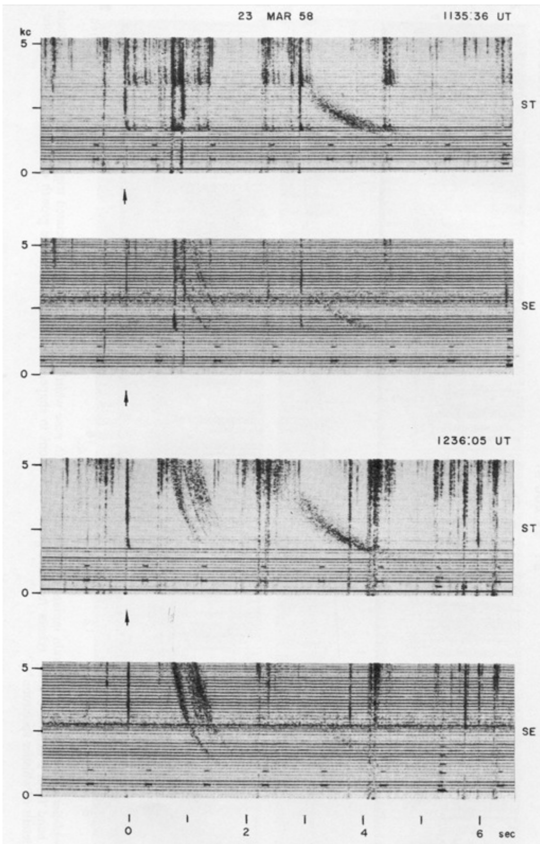


Figure 1.48: At top, only third hop visible at Stanford. From *Helliwell* (1965).

half-gyro-frequency cutoff. In our cases it appeared that from hop to hop, as the whistler propagated farther and became more highly dispersed at its lower frequencies, it was also becoming more coherent as "seen" by resonant electrons. At the time of the third hop, its amplitude was apparently increasing faster than it was decaying, through propagation losses.

1.9.2 Diffuse two hop nose whistlers

The caption of Figure 4.30 in *Helliwell* (1965) (Fig. 1.49 here) says, without much elaboration, that these are variations in whistler amplitude with frequency. What was going on? The data were acquired at

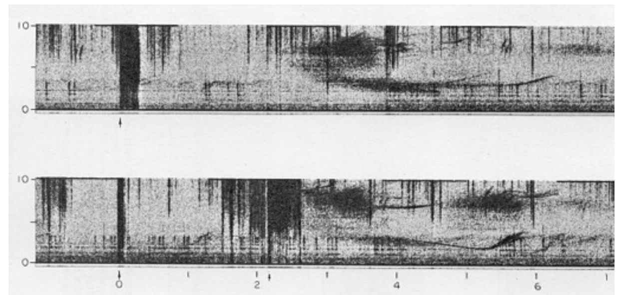


Figure 1.49: Diffuse two hop echoes. From *Helliwell* (1965).

Suffield Experimental Station in Alberta, Canada, ($L \simeq 3.8$) on July 25, 1962 during the post IGY IGC period. These are two hop whistlers (the time origins of two are marked below the lower record), followed by weaker fourth hops. The diffuseness of the whistlers may be attributed to the excitation of variously spaced return paths at the time of reflection of one hop signals, the same effect that must have plagued Millett Morgan and others at Dartmouth in those early years when all of us beginners were looking at whistler data and trying to make sense of it.

The low frequency tails of the whistlers trigger rising emissions, perhaps due to the same mechanism that causes the third hop anomaly shown in Fig. 1.48. The strength and echoing of the whistler energy above $\simeq 7$ kHz are believed to have a counterpart in the upper frequencies of one hop whistlers that appear, for example at $t \simeq 5.5s$ on the upper panel and are not well defined otherwise. High intensity above the whistler nose was often seen in Eights or Byrd records of propagation at $L \simeq 4$. For example, a periodic 12 phase periodic emission event was recorded at Byrd on August 23, 1961. The primary echo consisted of the upper part of a nose whistler that was tracked over 18 hops. A total of 12 nose traces were distributed along the echoing path.

1.9.3 Whistler-triggered chorus outside the plasmasphere

RAH Figure 7-62 features chorus triggered by whistlers (here Fig. 1.49). The recording was at

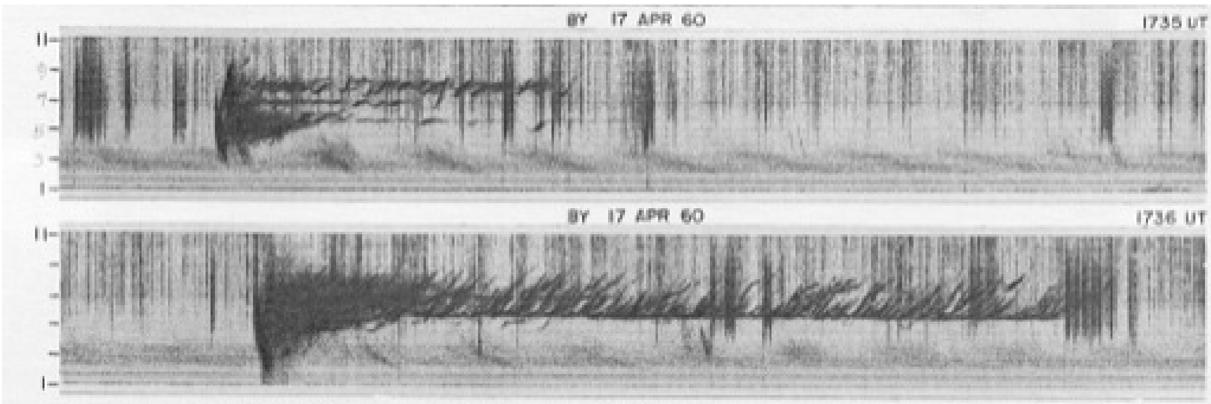


Figure 1.50: Chorus triggered by whistlers outside the plasmasphere. From *Helliwell* (1965).

Byrd, Antarctica on April 17, 1960, within two minutes after 1735 UT. Each panel covers 54s, showing effects clarified by later work with whistlers and with the Siple transmitter. Whistlers propagating outside the plasmapause are seen to trigger long enduring emissions, bursts of chorus that favor narrow frequency bands. In contrast, noises triggered within the plasmasphere by Siple or whistlers have been regularly observed, but tend to be brief, usually less than 1 s in duration (*Carpenter and Šulić*, 1988).

The triggered chorus in these cases is presumed to have propagated on the path or paths of the triggering whistler components. The mechanism of chorus generation must have operated quasi-continuously, not depending on the echo period along the path(s).

Fortunately, the monograph was reproduced in 2007 by Dover Publications and is now available in paperback.

Figure 1.51: Chung Park 1n 1966 at Byrd Station

1.10 Chung Park, pioneering student of ionosphere-plasmasphere coupling

In the fall of 1966 I made my first trip to Antarctica and spent several days at Byrd Station, where I met Chung Park. Photographed wearing a well seasoned Anarack, he had just spent a year at Byrd as a Stanford field engineer. Little did I know that he would soon begin a decade of contributions on multiple VLF topics and become identified as a pioneer on the topic of ionosphere-plasmasphere coupling.

Beginning in 1967 as a graduate student, Park participated in a number of studies based upon whistler data from Eights and Byrd. In one case involving collaboration with satellite experimenters Don Williams and John Ahrens, it was shown that a localized maximum in freshly injected outer belt electrons appeared in the plasmapause region (or PBL) at $L \approx 3.9$ in the immediate aftermath of a magnetic storm, and that with time during the recovery period, the electrons spread to higher and lower L values (*Carpenter et al.*, 1971). In another case (Fig. 1.51) Park took the lead in demonstrating that the plasmasphere could be highly structured in longitude, with density variations by as much as a factor of 3 within a range of about ±10° of the Eights longitude (*Park and Carpenter*, 1970).

Storey had once used whistlers to demonstrate the existence of the protonosphere, a cloud of light ions extending thousands of kilometers upward from the Earth's oxygen-dominated ionosphere. Almost 20 years later, Chung was using data from Eights to show for the first time that the ionosphere and the region Storey had discovered were strongly coupled in terms of plasma interchange. By 1970 he was able to produce a landmark Ph.D. thesis, entitled "A whistler study of the interchange of ionization between the ionosphere and the protonosphere" (*Park*, 1970a).

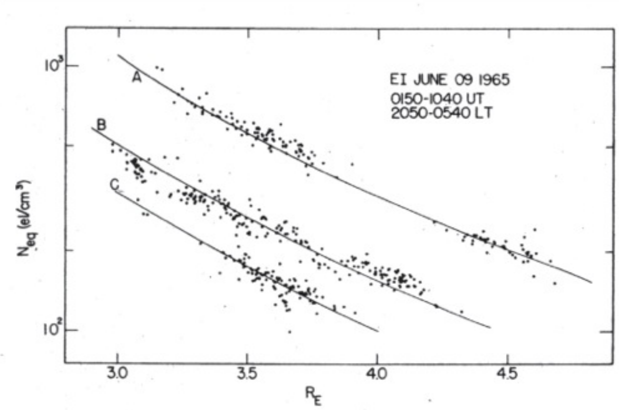


Figure 1.52: Equatorial densities near the Eights meridian showing 3 distinct levels within the plasmasphere. From *Park and Carpenter* (1970).

Prior to Park's work there was widespread agreement that downward fluxes ranging from $5x10^7$ $el-cm^{-2}-s^{-1}$ to $3x10^8$ $el-cm^{-2}-s^{-1}$ were needed in order to sustain the decaying ionosphere at night. However, the maximum daytime upward flux attainable had been found by *Hanson and Ortenburger* (1961) and by *Geisler* (1967) to be $\approx 1.5x10^7$, an order of magnitude less than the needed night time drainage flux. This low limit was attributed to the action of the so called diffusive barrier, a section of the upper ionosphere (Fig. 1.52) through which protons must pass after originating at lower altitudes through charge exchange of H with O+ in the chemical equilibrium region. Within the diffusive barrier, defined as extending to an altitude at which protons become the dominant ion species, the upward progress of the protons would be inhibited by Coulomb collisions with the more numerous heavy ions. The predicted upward flux would not only fail to allow the protonosphere to serve as a reservoir for the nighttime ionosphere, but would also be insufficient to refill depleted regions outside a newly eroded plasmasphere within the few days during which substantial replenishment had been found to occur (from anecdotal whistler data).

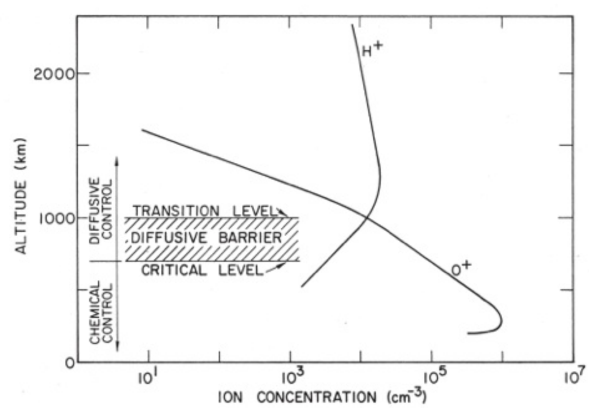


Figure 1.53: The diffusive barrier at ionospheric heights. From *Park* (1970a).

The limiting flux calculations did involve several parameters that were not well known, and investigators *Geisler and Bowhill* (1965) and Hanson(private communication to Park) had stated that the actual flux might exceed their calculated values by an order of magnitude. Furthermore, *Banks and Holzer*

(1969) had obtained limiting fluxes of $2 - 7x10^8$ $el - cm^{-2} - s^{-1}$ by using atmospheric models different from those used by the earlier investigators. The time was certainly ripe for the definitive measurements that Park was prepared to make.

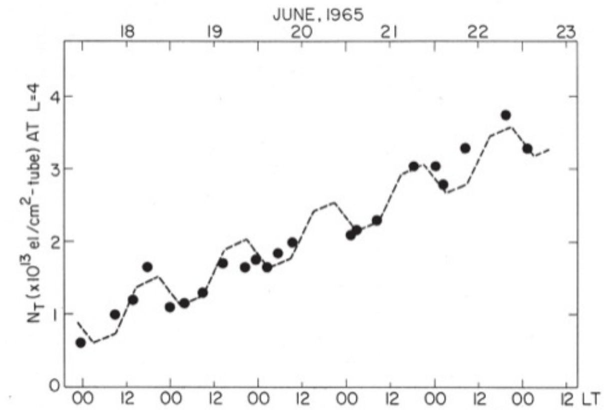


Figure 1.54: Multi-day recovery of tube electron content at L=4. From *Park* (1970b).

Working with Eights data, he was able to measure the changes from day to day in electron tube contentat L=4 during a five day period of deep quieting following the magnetic storm of June 15-16, 1965. Fig. 1.53 shows a sequence of the measurements for both day and night from June 18 to June 22, 1965. The dashed line is an empirical model based upon his estimate of an upward flux of $3x10^8$ $el - cm^{-2} - s^{-1}$ when the 100-km level at both ends of the L=4 path had been in sunlight, and a drainage flux of $1.8x10^8$ when in darkness.

During one four-hour afternoon period, Park was able to show increases in tube electron content along four discrete whistler paths between L = 3.7 and 3.9 (Fig. 1.54). The quantity displayed is travel time at the whistler nose frequency for each of the paths. The nose frequencies varied only slightly with time, implying that the magnetic shells of their paths were roughly constant and that the increases in travel time represented increases in tube electron content.

Park began his summary of findings with the following statements, remarkable for their time: “The

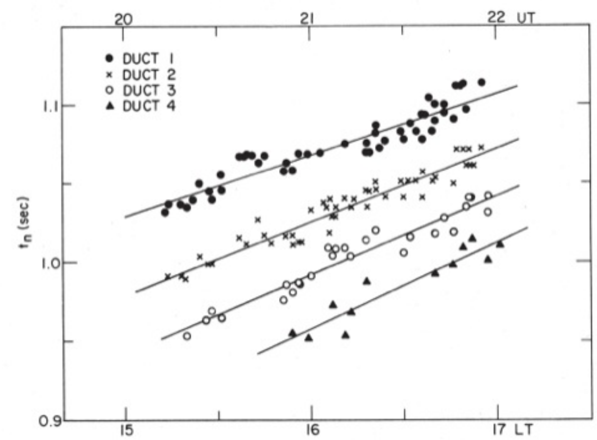


Figure 1.55: Increases in tube electron content on several field aligned paths during a four hour period. From *Park* (1970b).

observed daytime electron flux from the ionosphere into the protonosphere under quiet geomagnetic conditions is $2 - 4x10^8$ $el - cm^{-2} - s^{-1}$. This flux is larger than the downward flux necessary to maintain the nocturnal ionosphere.

The observed downward electron flux at night under quiet geomagnetic conditions is $\approx 2 - 4x10^8$ $el - cm^{-2} - s^{-1}$, an amount considered sufficient to maintain the nocturnal ionosphere.” Among Park’s other conclusions was the following:

“The post-storm recovery of the plasmasphere takes place primarily by filling from the iononsphere.”

Given that this statement has become almost an article of faith in space physics, it may seem surprising to realize that until Park’s work the refilling process had not been demonstrated by measurements and, as noted above, important theorists had not found it plausible.

Park was among the first to confront the differences between the ionosphere and the overlying region in terms of recovery following a magnetic storm. He found that while the ionosphere recovered from the June 15-16, 1965 storm in about 3 days, the protonosphere required about 5 days to reach the June, 1965 monthly median level (Fig 1.56). Furthermore, the protonosphere continued to fill during the 8 exceptionally quiet days prior to a new disturbance on June 25, 1965, and "did not reach any saturation level." This was an early point of view that eventually became widely accepted, namely that the plasmasphere is a dynamic region that "is strongly affected during geomagnetic disturbances" and is "most of the time recovering from previous disturbances."

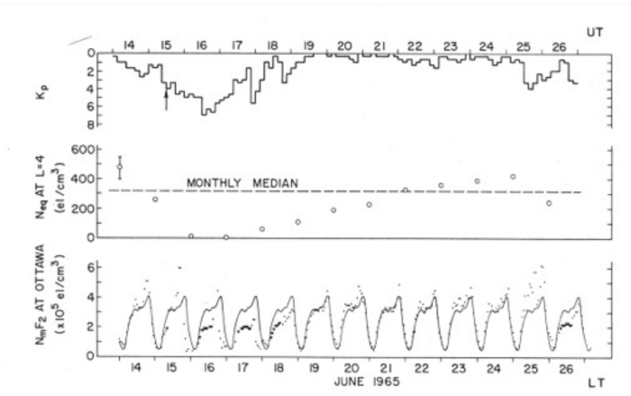


Figure 1.56: Compared recoveries of the ionosphere and overlying region following the June, 1965 magnetic storm. From *Park* (1970b).

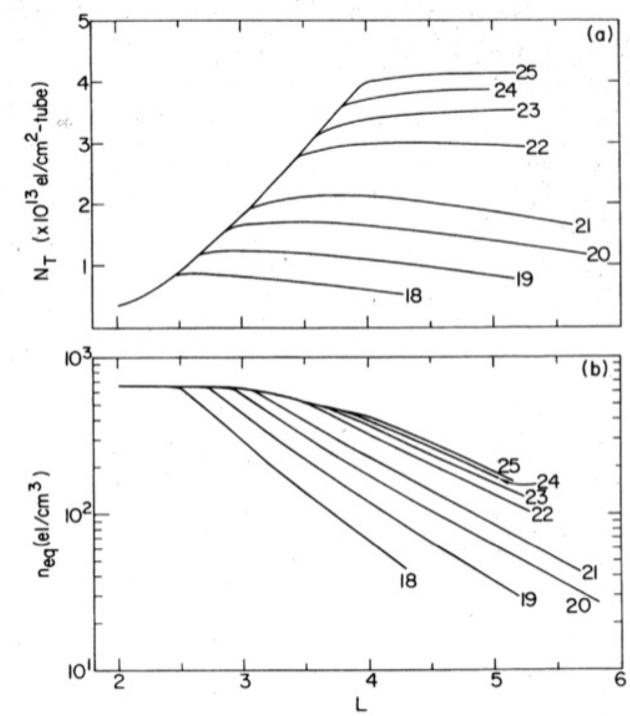


Figure 1.57: Multi-day quiet period recovery of averages of equatorial density and tube content. From *Park* (1974a).

The extent to which Park's results have become cornerstones of our understanding today is illustrated on p. 336 of the monograph "Ionospheres" by *Schunk and Nagy* (2000), where the authors state that "the downflowing H^+ ions charge exchange with O to produce O^+, and this process helps to maintain the nighttime F region." Furthermore, in agreement with Park's (1974a) finding about the time-dependent spatial division between an inner plasmasphere that is in equilibrium with the underlying ionosphere over a 24-h period and an outer plasmasphere that is still recovering from disturbance (see Fig. 1.57), they note that "the flux tubes at low latitudes refill fairly quickly because their volumes are small...", whereas "the flux tubes in the outer plasmasphere can take many days to refill. . .."

In his thesis, *Park* (1970a) estimated an overall loss of !$3x10^{31}$ electrons from the plasmasphere during the 15 June 1965 magnetic storm. Later, he pointed out that "the fate of such large amounts of plasma has important implications for the stormtime behavior of the ionosphere and the circulation of light neutral gases" (*Park*, 1973). In saying this, he recognized a broad area of work that would eventually receive much attention at conferences and in the literature (e.g., ISSI, 1997; Chen and Moore, 2006).

As a graduate student, Park identified for the first time a class of electron density decreases that appear within the eroded plasmasphere during storm periods. In a case study he showed that the plasmasphere could be highly structured in longitude,with density varying from near quiet-time levels down to as much as factor of three below them within a range of about 10° of the Eights longitude (*Park and Carpenter*, 1970) (Fig. 1.52). While the global distribution of such structured decreases was not known, Park estimated that the number of electrons lost from the plasmasphere to the ionosphere through them was a significant fraction of the overall losses from the plasmasphere during a magnetic storms (*Park*, 1973).

In a related pair of studies involving both plasmasphere whistler data and multi-station ionosonde data, *Park* (1973, 1974a) found evidence that portions of the plasmasphere had been rapidly drained of plasma within a few hours during a substorm period, and that this drainage had been confined to a particular longitude sector. Furthermore, he found an increase in ionospheric density within the same sector and was able to infer downward fluxes into the affected low-altitude region of order $10^9 el - cm^2 - s$ (*Park*, 1974b). Physical mechanisms for reducing the plasmasphere density by downward field-aligned flow had been advanced by other authors (*Hanson*, 1964). In his 1973 paper and at the inaugural Yosemite meeting in 1974, Park suggested that as a result of longitudinally structured cross-L inward drifts during substorms, the ionosphere at middle latitudes was lowered in a limited region and that the vertical pressure gradient of the H^+ distribution in the region of charge exchange with O was thereby enhanced so as to stimulate downflow. Whatever the merits of Parks explanation, his work remains the only empirical study of which I am aware that demonstrates this first-order effect on the plasmasphere from the perspective of magnetosphereionosphere coupling.

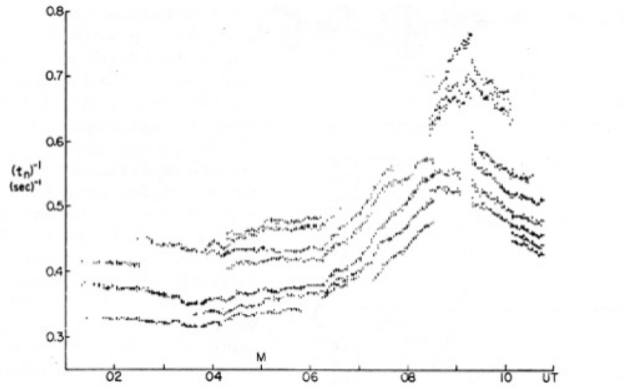


Figure 1.58: Plot of time variations in whistler path measurements during the Jul.15, 1965 substorm See text for details. From *Carpenter et al.* (1972).

1.10.1 How do whistler ducts work?

The atmosphere, ionosphere, and magnetosphere are coupled through the interplay of thunderstorm electricity, lightning, ground-based transmitter signals, power grids, field-aligned density irregularities, and magnetospheric wave-particle interactions. Park helped to identify the places of these diverse phenomena in magnetospheric physics.

In several papers he and colleagues tackled the problem of "ducts," elusive field aligned structures that somehow allow whistlers recorded on the ground to serve as diagnostics of magnetospheric plasma structure and dynamics. Two aspects of ducts were of special interest to Park: the process of their formation and their effectiveness in controlling the waves trapped within them. At the time of his work, indirect evidence for the existence of ducts was well established. Ducts had been postulated to involve field-aligned density irregularities, several-percent enhancements of ionization that internally trap wave energy and guide it between conjugate hemispheres (e.g., *Smith*, 1961; *Helliwell*, 1965; *Angerami*, 1970). However, the mechanisms of their formation had not yet been well established, in part because of the elusiveness of ducts as objects of measurement in situ.

With this background, Park considered several questions, among them: to what distance from a lightning source can magnetospheric ducts be excited? In collaboration with Phil Krider and using data from an early version of the present countrywide lightning-detection network, he showed that lightning could excite ducts with ionosphere endpoints as much as 2,500 km poleward of the lightning location.

Another question was the possible role of thunderstorm electric fields in the formation of ducts. He suggested that if the horizontal electric field extending radially from a thundercloud charge center could be mapped upward to the ionosphere, that field would set the overlying plasma into circular motions (*Park and Helliwell*, 1971). These motions would give rise to fine structure, irrespective of the smoothness of the initial ionospheric density distribution. The fine structure could then diffuse upward to form field-aligned irregularities. Fig. 1.59 (TO BE ADDED) shows the calculated distortions in an originally smooth horizontal density profile roughly 2 hours after the imposition of a model thunderstorm electric field.

But was the upward mapping of thunderstorm fields in fact efficient enough to affect the ionosphere? Here Park was joined by a gifted student member of the Helliwell group from Thailand, Mongkol Dejnakarintra. The two pushed aside traditions according to which thunderstorm electricity was confined to the electrosphere, a spherical shell below the ionosphere with a conducting upper limit that precluded meaningful electrostatic or dynamic coupling to the ionosphere. They developed numerical models of the penetration to the ionosphere of both the electrostatic field associated with a thundercloud as well as the time varying fields associated with lightning discharges (*Park and Dejnakarintra*, 1973; *Dejnakarintra and Park*, 1974). Significant field amplitudes at ionospheric heights were deduced, including fields large enough to support Park and Helliwell's duct formation hypothesis. The authors recognized that there was substantial uncertainty in their findings, based in large part upon uncertainty in their assumed conductivity models. However, in retrospect, they must be credited with taking pioneering steps along the road toward the rich research ground that thunderstorm coupling to the ionosphere has proven to be.

How do ducts behave so as to permit the spreading of wave energy from an initially limited field line region to a much larger one? Questions about the interplay between a magnetospheric field line path and the ionospheric layers at its ends had been earlier suggested through evidence that as energy trapped in a duct approached the ionosphere from above, an important fraction of it is reflected from the ionosphere and propagated back upward within nearby ducts, within the original duct, and also in a non-ducted mode. In a collaboration with Paul Bernhardt, *Bernhardt and Park* (1977) the authors undertook to show numerically how this could occur. They identified the various geophysical conditions and altitudes at which the ducting action of modeled ducts effectively ceases as the stronger ionospheric gradients take over from the weaker transverse density gradients of the ducts. Thus they provided a quantitative basis for what had only been reasonable conjecture, namely that downcoming wave energy can illuminate additional magnetospheric regions as it becomes untrapped, undergoes reflection, and then returns upward.

1.10.2 Park measures the vertical electric field at Vostok

Chung Park was the first member of our group to pay attention to what the Atmospheric Science Section people were saying at their AGU meetings. He folowed their work on thunderstorm electric fields, and as noted, took some of our first steps toward understanding the ionospheric effects of thunderstorm electricity.

Chung became interested in looking at connections between the Earth's electric field and interplanetary phenomena that could be of importance to the Earth's weather conditions. He acquired a field mill for measuring the vertical field at Vostok Station, Antarctica and reported on the data for March-November 1974, finding that the electric fieldat Vostok was depressed by about 15% for 1-3 days following the passage of a solar-sector magnetic boundary.

1.10.3 Diem Ho and L. C. Bernard extend whistler diagnostics

As the years passed in the 1960s, various research groups worked on refining and extending the ground based whistler probing method. Here at Stanford we were accustomed to identifying whistler-causing spherics directly on the spectrogram records. *Ho and Bernard* (1973), realizing that the spheric was not always known, proposed and tested a simple method for scaling the key whistler parameters f_n and t_n on the basis of three independent measurements of f and t that did not include knowledge of the whistler time of origin.

1.10.4 Extraodinary results from use of the buried long wire antenna at Byrd for recording

The 21 km long wire antenna buried in the ice at Byrd in 1963 (see later sections), while essentially a failure as a source of interhemispheric magnetospheric transmissions, provided excellent results in terms of passive receptions. Two extraordinary whistlers recorded in the late afternoon local time in 1967 are shown in Fig. 1.59. The spectrograms cover frequencies below 2.5 kHz and are free of local power line interference. We see multiple components with nose frequencies as low as $\simeq 800Hz$ and nose travel times extending to about 6 s. This figure was eventually published as part of a study of the observed outer limits of ground to ground whistler propagation *Carpenter* (1981a), limits that were found to extend as far as 8 Earth radii on the dayside of the Earth in quiet times.

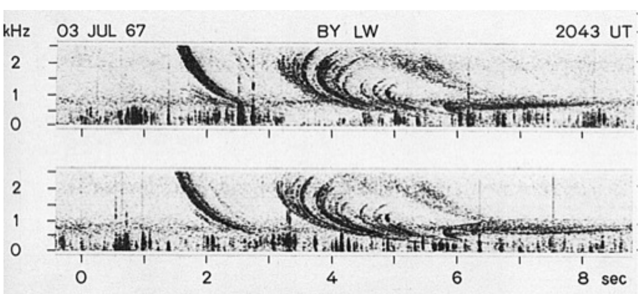


Figure 1.59: Two whistlers recorded within minutes using the antenna at Byrd Long Wire. From *Carpenter* (1981a).

1.11 More work on plasma drifts in the 1970s

In the late 1960s and well into the 1970s we continued to have a field day with the continuous or near continuous whistler data from Eights. For example, Jan Siren made a stop-action film of the whistler components recorded over a 10 hour period on July 15, 1965. Every film frame included a single whistler, and there was a new frame every minute or two, positioned with respect to the time of the causative sferic.

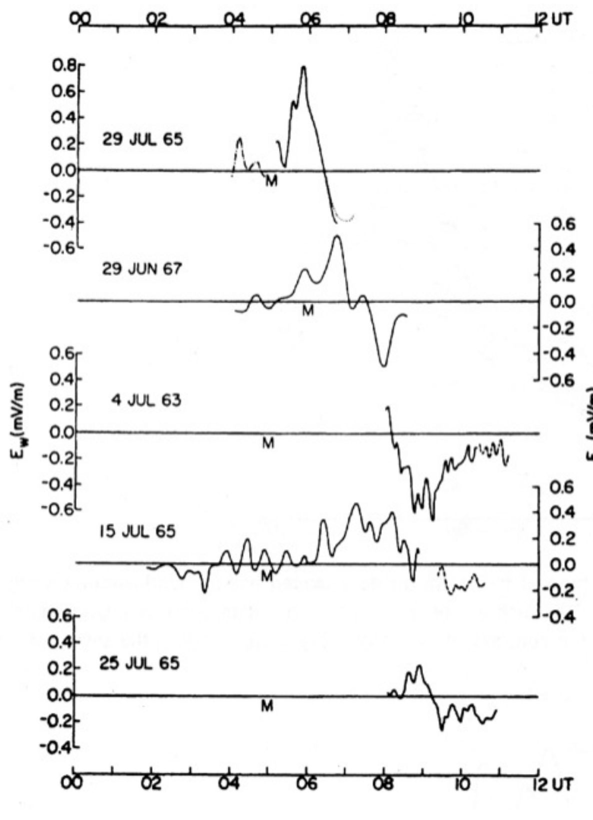


Figure 1.60: Examples of the inferred westward electric field E_w at $L \simeq 4$ during and following temporally isolated substorms. From *Carpenter and Seely* (1976).

Fig. 1.59 is a plot of the time variation of ${t_n}^{-1}s^{-1}$ over an extended period around the time of the substorm event illustrated in Jan's film, There is a high degree of coherence among the motions on nearby paths. Inward cross-L motions are associated with rising values of ${t_n}^{-1}s^{-1}$. The slope of any data curve is approimately proportional to the local westward electric field.

The cross-L plasma flows associated with temporally isolated substorms showed a consistent reversal following the initial fast inward drift. The reversal can be seen after 09 UT in the data of Fig. 1.60.

In such cases the outward drift could be comparable in speed and duration to the preceding inward flow. Fig. 1.60 shows examples of post sub-storm outward drifts that began at various night-side local times.

1.11.1 Nate Seely's contributions

Nate Seely, a grad student in the 1970s, was most helpful in addressing questions about the interpretation of whistler data in the presence of solar-wind induced distortions of the Earth's magnetic field.. We had always believed that such effects were relatively small, but Nate nicely confirmed and extended our ideas by his research on the Earth's magnetic field.

In 1976 he worked with me on a paper about the cross-L whistler path drifts in magnetically quiet times (*Carpenter and Seely*, 1976). Under such conditions we could put aside concerns about substorm effects and pay attention to residual plasma motions connected to the Earth's quiet day current system, a system driven by dynamo effects in the ionosphere. That system had occupied the attention of ionospheric workers for many years.

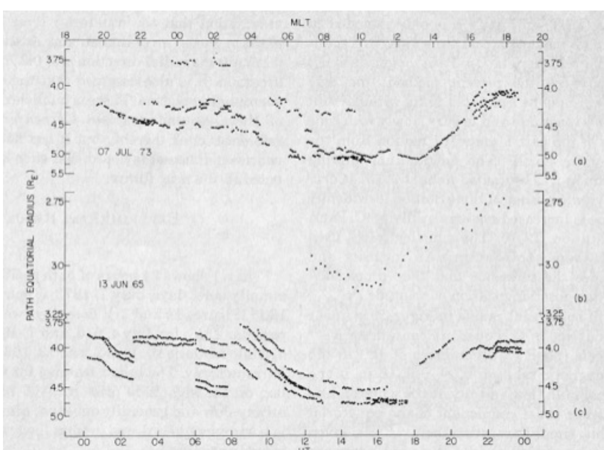


Figure 1.61: Example of whistler drifts on two exceptionally quiet days. From *Carpenter and Seely* (1976).

Fig. 1.61 shows the cross-L drifts of whistler paths versus both UT and MLT on two exceptionally quiet days, July 7,, 1973 (Siple Station, Antarctica) and

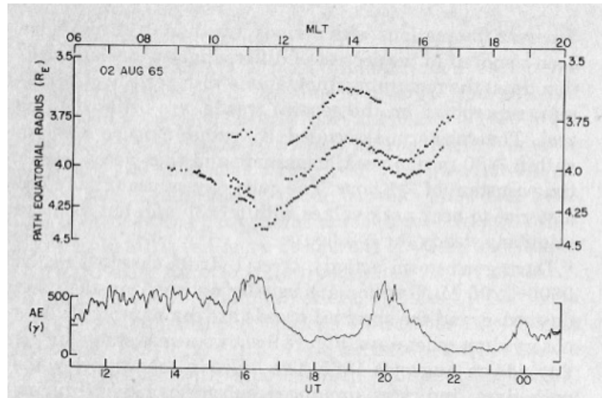


Figure 1.62: Brief interruption of afternoon in ward drifts by penetrating substorm electric fields. From *Carpenter and Seely* (1976).

June 13, 1965 (Eights Station). The night=side drifts were small and slightly outward, while the dayside activity included fast outward drifts in the morning sector and comparably fast inward motions in the afternoon. The activity on multiple paths was highly coherent, with similar behavior evident at some times over an equatorial range of $\simeq 2R_e$.

In studying whistler path drifts, Nate was successful in finding the patterns shown in Fig. 1.62 , which describes the penetration of the afternoon sector by temporally isolated substorm electric fields. Sunward flow driven by the substorm fields briefly interrupted an ongoing period of inward flow associated with otherwise quiet magnetic conditions.

1.11.2 A visit to Stanford by Lars Block of Sweden

In 1974 Lars Block visited our group and succeeded in clarifying and extending our methods of using whistlers to measure the high altitude westward component of the geomagnetic field. We had been tracking the radial drifts of whistler paths for years and had talked about challenges of the method in debates such as the one held at the URSI Assembly in 1966 at Munich (see Section above)

In a paper discussing whistler diagnostics of mag-

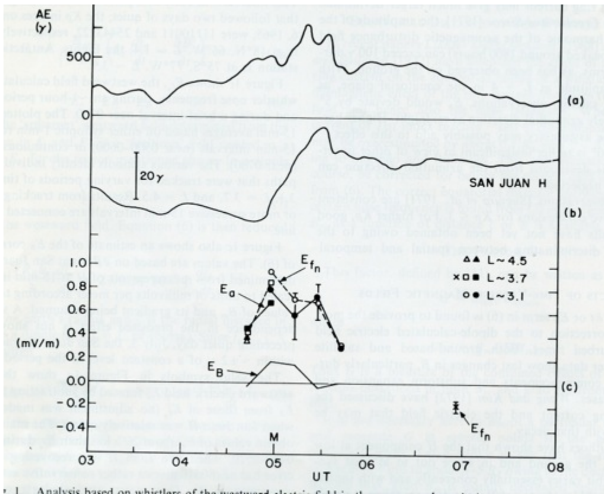


Figure 1.63: Separate effects of dfn/dt and dB/dt in a substorm in 1965. From *Block and Carpenter* (1974).

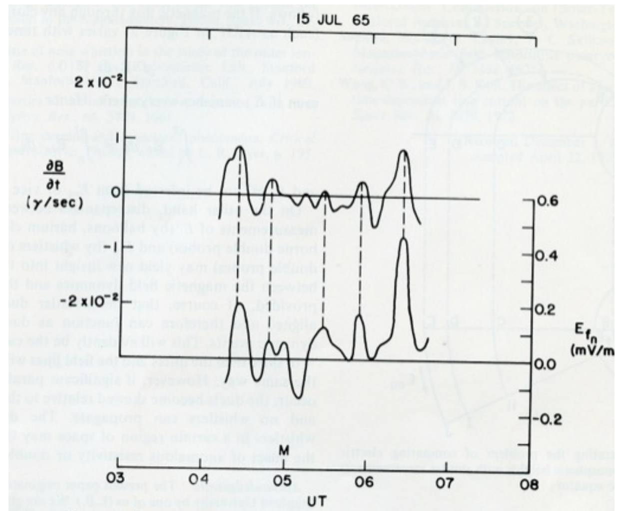


Figure 1.64: Presubstorm pulsations in E field in phase with fluctuations in **B**. From *Block and Carpenter* (1974).

netospheric electric fields, Lars considered the effects both of departures of the Earth's **B** field from a dipole and changes with time in the **B** field, such as those associated with magnetic storms and substorms. Departures from a dipole can be easily taken into account, but temporal changes are a more difficult issue. Measurements of whistler nose frequency with time have two aspects, one representing radial movement in the inhomogeneous **B** field and the other changes in **B** with time. If the latter are known, the corrected value of the potential E field can be estimated.

As it turns out, much of our E field work had been done under geophysical conditions such that substorm activity was temporally isolated and not at a high global level. In a sample case from July 6, 1965, it was found that the changing **B** field (on a time scale of 15 min) added about 20% to the apparent total field. A correction could then be made based upon inferring the high altitude **B** field from fluctuations in the H magnetic component at low latitude. The case is shown in Fig. 1.63, where the estimated induced E field is shown as E_B and has been subtracted from the measured changes in f_n above to get a corrected E field E_o.

Lars discussed a quite different situation that developed on July 15, 1965 in a quiet period before a temporally isolated substorm. In this case there were small but relatively fast changes in **B** that coincided with apparent whistler path drifts (see Fig. 1.64). When we had first reported this case, it had been suggested that large scale oscillations in the geotail were involved.

1.12 Reminiscences of Mark Leavitt, a VLF graduate studen, developer of a frequency tracking direction finder and later Doctor of Medicine

Viewed in retrospect, my career trajectory is a multihop whistler, bouncing alternately between the hemispheres of engineering and medicine (propagation time 10-15 y). Although each traversal is exciting, it is sad to lose touch with some wonderful friends and colleagues. So I was delighted to hear from Don Carpenter last year as he gathered a history of VLF research at Stanford, and I'm honored to be able to share some reminiscences about my relatively brief time and modest contribution in that long-running enterprise.

How did I come to be a graduate student at Stanford? In my earliest memories, when asked what I wanted to be, I'd answer "a doctor" – the most parent-pleasing response. But at age 10, we moved into a Tucson house with a mysterious wire dangling from the ceiling, connected to a rooftop short-wave radio antenna left behind by the previous resident. My first "career hop" was thus launched, by way of an amateur radio license, then part time broadcast engineering jobs in high school, and finally an EE degree at the University of Arizona.

At graduation, two job opportunities beckoned: one at MIT Lincoln Laboratory and another at Electromagnetic Systems Laboratories (ESL) in Sunnyvale. Given the Lincoln Lab's historic reputation and the possibility of partial graduate tuition support at MIT, I was leaning strongly eastward. But my father fatefully intervened by disobeying my request for confidentiality; when the ESL recruiter called my home hoping to clinch the deal, Dad leaked that I was "in Boston talking to some laboratory named after a president." Apparently in response, ESL doubled down on their offer, adding 100 percent tuition support and fully paid time off for graduate studies at Stanford. Sold!

ESL was a fast-growing startup founded by William J. Perry, a highly distinguished Stanford alumnus who later served as Secretary of Defense in the 90's. Arriving in 1971, I was surrounded by classified, cutting edge work in electronic reconnaissance, with my own efforts focusing on precision airborne direction-finding (DF) systems across the HF to UHF spectrum. My part-time graduate studies at Stanford provided a welcome counterpoint: an open, non-secretive environment and a diverse international community. Upon completing the MS degree work in 1973, I cherished the Stanford experience too much to leave, so I applied for the doctoral program. Ron Bracewell, my advisor, thought my DF experience might be of interest to Bob Helliwell's VLF group, and my thesis topic soon emerged: to develop an instrument to accomplish DF on received whistlers and other VLF emissions. Although I had experience in higher-frequency radio wave propagation, the physics of VLF wave-particle interactions drove my brain nonlinear. Fortunately, on the Stanford VLF team, an instrument-builder such as I could be patiently guided by expert scientists such Bob Helliwell and some of the more experienced graduate students and field engineers.

At the time, broadband received VLF was being recorded on magnetic tape, then processed through a spectrum analyzer using a rotating drum of narrowband filters. The resulting continuous spectrogram displayed the frequency-varying behavior of whistlers and other VLF emissions beautifully, but gave no indication of the spatial origin of the signals (other than by calculating likely duct latitudes from the whistler nose frequency). One attempt to apply DF techniques used a goniometer (another mechanically rotating device) to combine the signals from crossed loop antennas and a vertical monopole, creating a periodic null in the signal. The timing of the nulls corresponded to the direction of arrival, but signals of brief duration could not be analyzed, and polarizations other than vertical could cause large errors. I hoped to do more sophisticated phase and amplitude analysis of the three antenna signals, but it was challenging to do this while the signal was sweeping widely in frequency, and with noise and other emissions often interfering.

During my teenage years as a broadcast engineer I had been intrigued by the ability of wide-band frequency modulation to overcome noise and interference. Now I wondered if the frequency excursion of whistlers might be turned to advantage rather than being a hindrance. From my ham radio years I was familiar with superheterodyne receiver design, converting a range of received frequencies to a fixed intermediate frequency (IF) to make sharp filtering and detection easier. Combining these ideas led to the VLF Tracking Receiver concept. Like an FM radio, the receiver would have closed-loop automatic frequency control, optimized to dynamically track the frequency of whistlers while translating them to a fixed IF band. Because the 3-channel superheterodyne receiver used a shared local oscillator, the phase and amplitude relationships between the antenna signals were preserved. These signals were fed to the DF Processor, the other half of the instrument. Together they made up the TR/DF.

Today, I suspect an EE student might solve this challenge with a 5 dolLar microprocessor or DSP chip as a summer project. But in 1974, Intel's newest 8080 microprocessor could only do 8 bit arithmetic at 2 MHz and was not up to the task. Luckily, analog integrated circuits were about a decade ahead of digital processors, so I used over fifty low cost op-amps in my creation. After 18 months of design and construction, the TR/DF emerged as a cross between a radio receiver and a specialized analog computer. Results were output as analog voltages to be recorded on a strip chart, and also as FM audio signals that could be superimposed on a VLF spectrogram.

The TR/DF performed satisfactorily on simulated signals in the lab and did respectable direction-finding on the Navy VLF beacons, but there wasn't much whistler activity at the Stanford ground site, so both I and the equipment were shipped to Roberval, Quebec, in the summer of '75. I believe I spent a week there, but John Billey, the site operator, continued to collect data, and other Stanford VLF people pitched in to analyze the results.

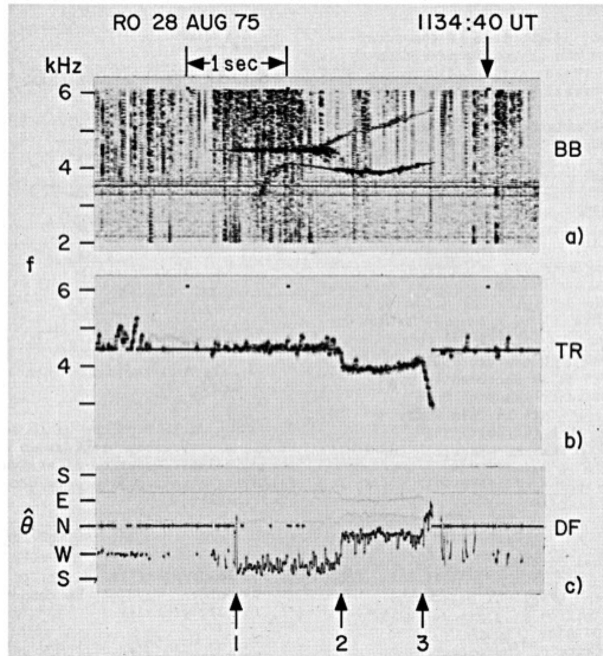


Figure 1.65: Spectrographic output of the TR/DF system during test operation at Roberval in 1975. From *Leavitt et al.* (1978).

To my relief and as illustrated in a figure from Aug. 28, 1975 at Roberval, the instrument proved capable of capturing and tracking many whistlers as well as stimulated emissions from the Siple signals. The top panel is a 2-6 kHz boradband spectrogram showing a Siple pulse near 4.5 kHz. Below the Siple pulse is a noise event triggered by the upper part of a whistler. The middle panel shows a replica of the passband center frequency of the tracking receiver, while the bottom panel provides signal arrival beaaring information. The bearing was automatically set to north when the receiver was not in the tracking mode.

Sometimes the DF result was reasonably clear, while for other signals the indicated bearing was very noisy, presumably due to polarization error, high angles of incidence, or multipath effects. (See a later study of DF on Siple signals by *Carpenter* (1980).)

With my dissertation complete in 1976, it was time to return to ESL, but storm clouds were gathering in my personal life. My wife developed severe complications from juvenile-onset diabetes, threatening her vision and sometimes her life. In contrast to my growing electronics expertise, I felt helpless in the face of her health challenges. I felt an increasing pull to redirect my problem-solving efforts toward the medical sciences. I learned of a medical school program at the University of Miami that annually admitted a dozen engineers with PhD degrees and propelled them through medical school in two years instead of four (former students covertly dubbed it the Quickie Quack program). One year later I was headed to Miami, and back toward my first childhood career aspirations.

I completed the MD training followed by a three-year residency and set up practice in Portland in 1982, fully expecting that my career-hopping days were over. But I couldn't resist experimenting with the newly available technology of personal computers. I wrote software to automate first the billing and then the clinical record-keeping in my office. Other doctors wanted the software, and what started as a tiny side business grew and grew.

By 1992 I was forced to choose between practicing medicine and running an emerging electronic health record (EHR) software company. Since every previous bounce had led to new adventures, I opted to be swept along again, and what a ride it was. The company went public in 1999, was battered by the technology stock crash in 2000, and ultimately sold to General Electric in 2002. A few years later, the Federal government made EHR adoption a policy goal, and I ended up chairing a non-profit organization that tested and certified EHR systems. Government incentives for adoption of EHRs have now driven health IT to be a 30 Billion dollar industry. I retired in 2010, freeing me to tinker once again, this time in personal health technologies. I am hoping this whistler's hopping is finally complete.

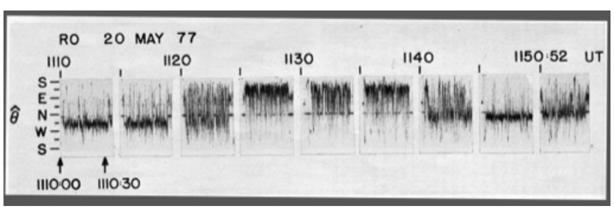


Figure 1.66: Sequence of TR/DF arrival bearing data covering 40 minutes

1.12.1 Dynamics of arrival bearings in a 1977 case study

In 1980 I wrote a paper about arrival bearing data from operation of Leavitt's TR/DF at RO in 1977. There were findings about what appeared to be the dynamics of ducted VLF propagation and also the effects of eastward drifting low energy particles that could be in resonance with VLF waves along variously distributed ducts.

Some of the observed effects are illustrated in Figs. 1.66 and Fig. 1.67. which show arrival bearings during a 40 min period near dawn on July 20, 1977. 30 s segments of bearing data are presented every 5 min. The main effect is a shift from west of north at the beginning to east of south near mid point and then back to west of north. In the segments beginning at 1120 and 1140 there is evidence of at least two directions of arrival. The activity is complicated by the stepping power level of the Siple transmitter, which was lowered by 5 dB every 30 s. At about 1138 there began a line radiation event that grew in intensity over a period of minutes. This event appears to have been concentrated in the south and east direction.

In Fig. 1.66 we see the details from 1135 to 1140 UT, with the spectrum above and the arrival bearing below. Already at 1136 there was evidence of a second bearing south and east, one that soon became dominant. It was suggested that the shifts from west to east and later back again were due to the presence of temporal or spatial structure in 1-2 keV electron clouds that were gradient drifting eastward in the magnetosphere.

It was suggested that the TR/DF data could be the basis for deeper insight into duct activity than had thus far been possible. Unfortunately, this did not happen except, for example, through cases of use of the TR/DF by John Billey at Palmer Station in 1979 and during the Siple rocket/balloon campaign in 1980-1, as well as through deployment by Tsuruda and Ikeda of spaced receivers in the RO area in 1975.

1.12.2 Whistler mode waves along the plasmapause surface

From the early 1960s, particularly after 1963, we knew that the high frequency extent of whistler components propagating just outside the knee could differ spectrally from the well defined cutoffs at $f_H/2$ that we often saw elsewhere, particularly inside the plasmasphere. An example of this anomalous extent is provided by the leading whistler components in Fig. 1.68, which appeared in our 1966 discovery paper on the global plasmasphere. There are two main "anomalous" effects in these two examples recorded about 1 min apart on July 7, 1963, an extension of the leading trace to $\simeq$13 kHz, well above the usual cutoff at $f_H/2 \simeq$7 kHz for a whistler at that location, and trailing echoes that rose in frequency with time near 13 or 17 kHz.

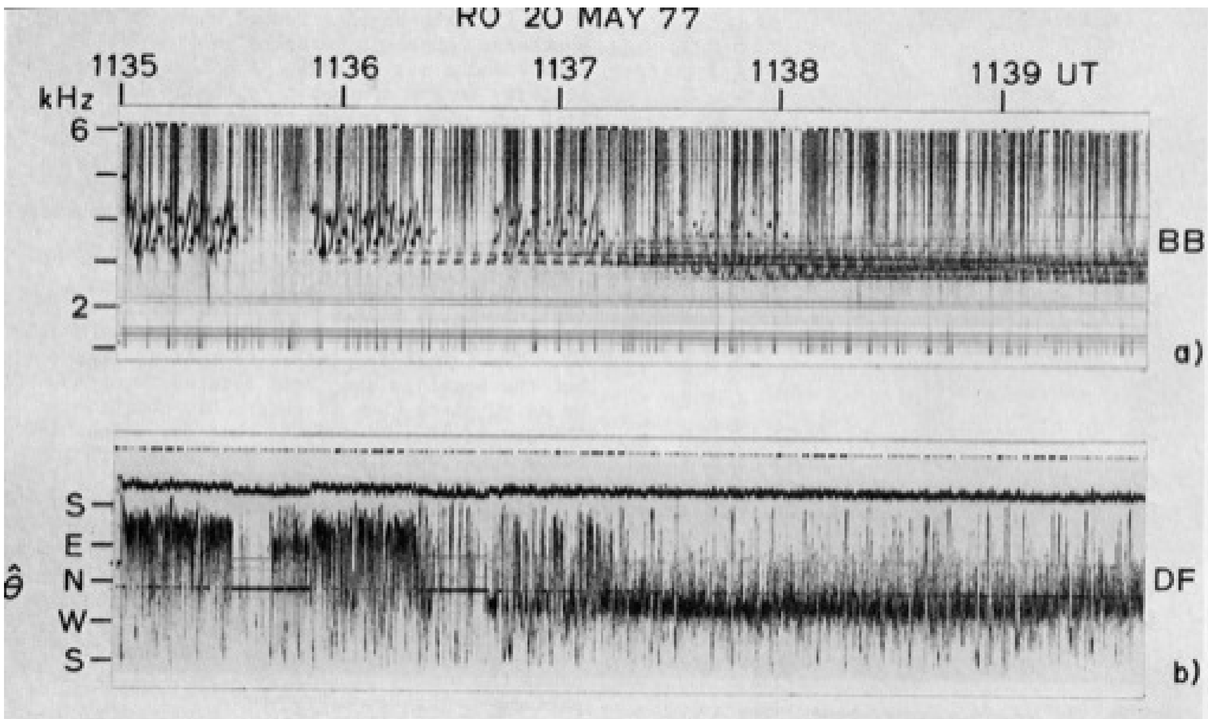


Figure 1.67: Continuous TR/DF data showing transition in bearing

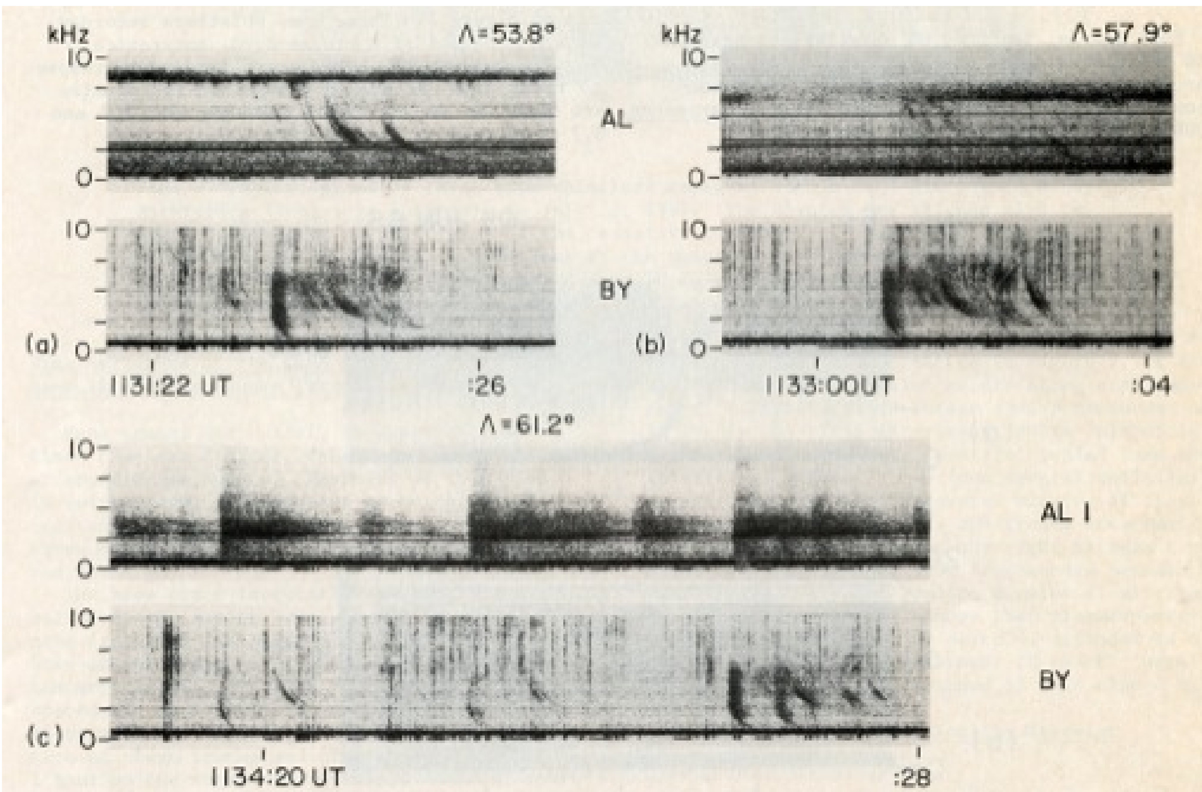


Figure 1.69: Three spectral segments as Alouette 1 crossed the plasmapause near Byrd, From *Carpenter* (1978).

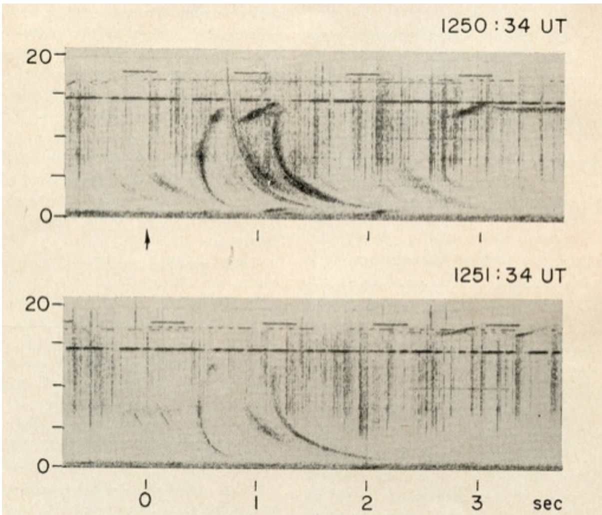


Figure 1.68: Knee whistlers recorded at Eights in 1963.From *Carpenter* (1966).

During the late 1960s and early 1970s we became increasingly familiar with the complexity of whistler mode propagation along paths located near to and just outside the plasmapause. A great asset was our ability to record at both Byrd and Eights the broadband VLF received by the Alouette satellites, observations that culminated in a JGR article in January, 1978 (*Carpenter*, 1978). Fig. 1.69 shows simultaneous data from Alouette and Byrd during 3 several-second intervals as the satellite moved poleward from about 54° to 58° and then 61° N. The wave activity was shown spread out in time so as to display details of whistler spectra as the satellite crossed the plasmapause. In the upper two Alouette 1 records the LHR noise band is well defined, first near 8.5 kHz and then at ≃ 7 kHz. The plasmapause was clearly penetrated before the next Alouette recording, since by then the LHR band, was being triggered by diffuse whistlers and had dropped to around 2.5 kHz.

Consider first of all the three panels of ground data at Byrd. The whistlers were recorded within an interval lasting about 3 min and are similar to one another. The first component was diffuse, strong, and showed little if any dispersion above ≃ 2 kHz. It did not extend much above ≃ 6 kHz. In the ground records there were also later components, not well defined but indicative of propagation in the outer plasmasphere. As we had come to expect, there was essentially no evidence of the LHR noise band in the ground data.

The three Alouette records tell a story of change in time and space. Both the top records showed evidence of some of the plasmasphere whistler components seen on the ground at Byrd. Such components disappeared poleward of the plasmapause, which must have been passed near 60°. Remaining was the strong knee trace, which appeared also on the ground records but on Alouette clearly contained components at large angles to **B** that interacted strongly with the LHR noise in the manner discussed by Smith and Brice in the 1960s.

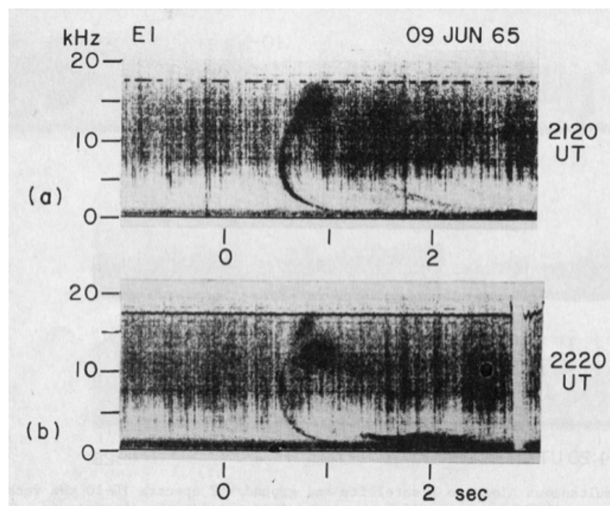


Figure 1.70: Knee traces at Eights. From *Carpenter* (1978).

Another example of knee whistler extension to high frequencies is in Fig. 1.70, which represents recordings 1 hour apart at Eights in 1965. There appeared dispersive effects well above the whistler nose, ones that may have developed into trailing diffuse noise.

Three recordings at Byrd long wire in 1967, all within 3 min (Fig. 1.71), showed a multicomponent, somewhat diffuse whistler that was not well defined above nose frequencies near 6 kHz but then reap-

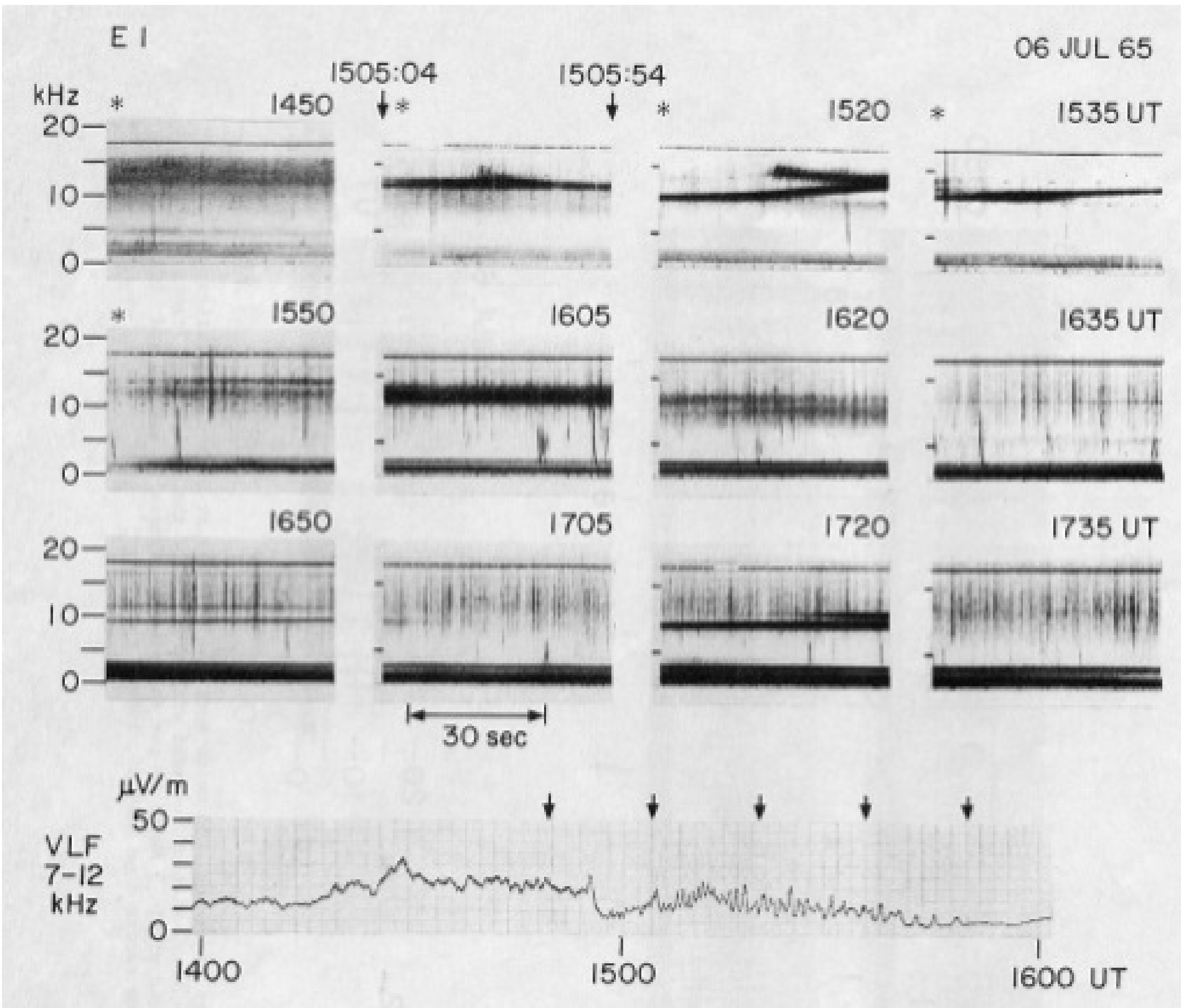


Figure 1.72: Noise bands at the plasmapause. From *Carpenter* (1978).

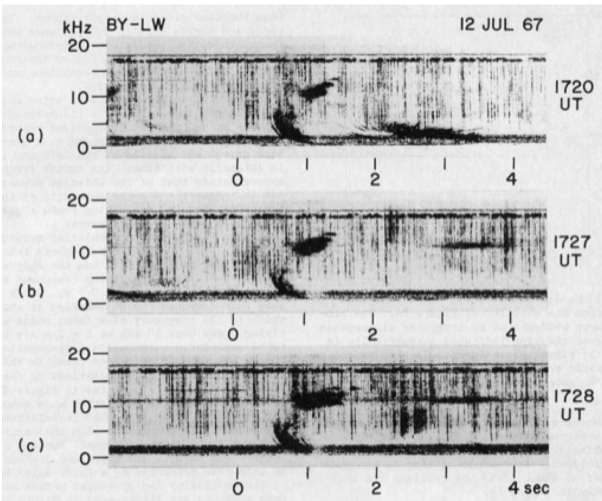


Figure 1.71: Knee whistlers from Byrd long wire 1967 recording. From *Carpenter* (1978).

peared as a dispersed echo near 11 kHz and was seen once again $\simeq$2 s later as a 3d hop echo. Meanwhile, after the second example a noise band at 11 kHz developed.

The occurrence of noise bands at the plasmapause is not uncommon. Figs. 1.72 and 5.24-25 in Section 5 show noise activity near 6 kHz on EX 45 at the equator as it exited the plasmasphere on September 11, 1973.

Eights once provided about 3 hr of noise bands that clearly involved repeated echoing along whistler mode paths that varied in output from time to time but were probably just outside the plasmasphere.

1.12.3 A summarizing paper on electron density from whistlers

A paper bringing together much of the work on the equatorial profile was published by Park et al in 1978. Chung was joined by me and by Don Wiggin, who did a lot of work scaling the data. The paper was intended to encompass results over the period from the IGY in 1959 to the 1970s. As such it focused on Antarctic data from Eights, from Byrd, and from Siple, while also including some historic results from

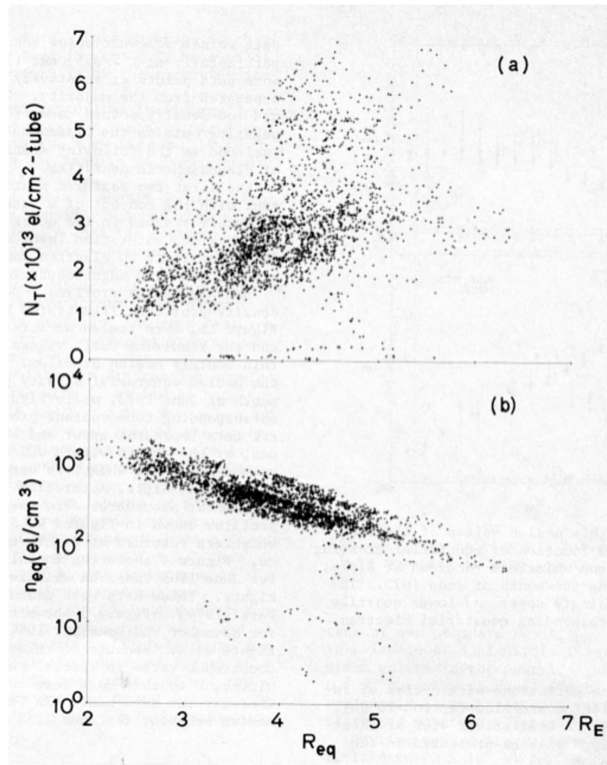


Figure 1.73: (a) Total electron content versus L value; (b) Equatorial electron density. From *Park et al.* (1978).

the Stanford receiver, located on a meridian some 30 degrees to the west of the Eights/Byrd longitude.

Fig. 1.73 shows a mass plot of tube electron content (electrons above 1 cm^2 at 1000 km extending to the equator) measured from 3000 whistlers recorded at Siple in June, 1973. Below is the corresponding plot of equatorial electron density. It was noted that the plasmaspheric profile beyond L=3 tends to be dominated by variations associated with magnetic disturbance and subsequent recovery. "In the aftermath of disturbance, the plasmasphere tends to be divided into an inner 'saturated' region which is in equilibrium with the underlying ionosphere in a diurnal average sense and an outer 'unsaturated' one which is still filling with plasma from below."

A special result was the annual variation in electron

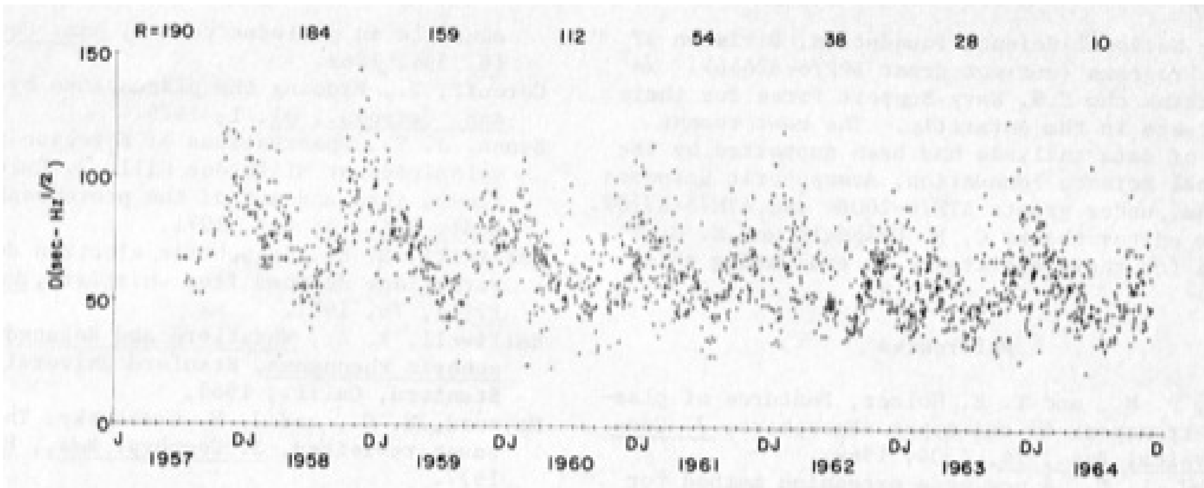


Figure 1.75: Stanford whistler dispersion at 5 kHz for seven years. From *Park et al.* (1978).

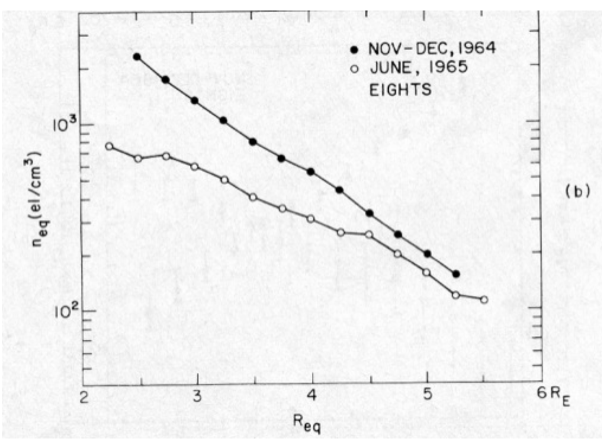


Figure 1.74: Equatorial electron density profiles from Siple whistlers showing a large annual variation. From *Park et al.* (1978).

density detected near L=3, with densities larger in December than in June by factors of as much as 2 or 2.5. Fig. 1.74 is a plot of monthly averaged equatorial electron density measured at Eights in 1964 and 1965. To explain this effect, models of coupling between the ionosphere and plasmasphere have been advanced by Smith and Smith, but the question has not been pursued experimentally or theoretically in ways that would clarify its longitudinal extent.

Fig. 1.75 shows measurements of Stanford whistler time delay at 5 kHz made over a series of years from 1959 to 1964. An annual density variation with peak near December is clear, as is a decline in the yearly average electron density that accompanied the changing annual sunspot numbers indicated above. The path L values of the Stanford whistlers were estimated to be on average near L=2.5.

Part 2

Space Observations

2.1 Introduction

By 1960-61 our group had a good observational grasp of ground-observed whistlers. Then in the early 1960s we were introduced to wave observations in space. There were two major questions about this new class of data: what propagation effects were to be seen, and what diagnostics of plasma structure and dynamics could be developed?

2.2 First observations in space

Figure 2.1: Rorden, Beattie, and Helliwell with EXPLORER VI receiver

2.2.1 EXPLORER VI

Helliwell and his associates were quick to take advantage of early research opportunities in space. Accumulating studies had made them aware of a huge class of whistler-mode waves that could only be detected in situ. Back in 1958 and 1959, they conducted a pioneering measurement of the altitude profile of a 15.5 khz transmitter signal as the EXPLOER VI satellite ascended from liftoff through and well beyond the Earth's regular ionosphere. At Cambridge in England there had already been substantial experimental and theoretical work on the ionospheric reflection of transmitted VLF waves (e.g. Bracewell, 1959). There were also predictions of the effects of the ionosphere on a wave penetrating from below.

Fig. 2.1, probably displayed in 1959, shows a copy of the VLF receiver flown on EXPLORER VI, a harbinger of future satellite instruments. Grouped around are the principal creators of the instrument Bud Rorden, of Stanford Research Institute, Bob Beatie, of Develco corporation and Helliwell At this time, both Rorden and Beatie were studying for graduate degrees at Stanford.

The satellite was launched from Cape Canaveral on August 7, 1959. Signals at 15.5 kHz from NSS (Annapolis, MD), roughly 1000 km distant in the earth-ionosphere waveguide, were received over the altitude range from zero to 160 km, in spite of the unintended confinement of the antenna between the third stage rocket case and the folded solar paddles and also within an expoxy shroud covering the satellite and the third stage rocket. The measurements were reported in a 1963 article by R. F. Mlodnosky, R. A. Helliwell, and L.H. Rorden [1963]. They estimated that measured attenuation through the daytime lower ionosphere was 43 ± 4 dB, consistent with predicted values. Taking more data from the later LOFTI satellite, launched in 1961, they concluded that "the launching of the whistler mode, at least in daytime, can be described to a first order by utilizing a sharply bounded model of the ionosphere to account for the reflection-transmission phenomenon, and a slowly varying model to account for the absorption experienced by the transmitted wave."

A footnote to the article advises that it was submitted for publication, but due to the untimely death of co-author Bob Mlodnosky (see below), did not undergo final revision.

2.2.2 LOFTI

The LOFTI-1 satellite, sponsored by the Navy Research Laboratory, operated during February and March of 1961.Brginning thereafter, unmanned satellite research activity became largely the province of NASA.

A short article entitled "An interpretation of LOFTI-1 VLF observations" was published in 1963 under the names of Rorden, at SRI, and Smith and Helliwell, at Stanford. Special signals from Navy transmitters NBA in the Canal Zone and from NPG, Jim Creek, Washington, were detected using a small loop antenna (*Rorden et al.*, 1963). Both direct up-going signals and indirect down-coming signals were received, neither of which showed evidence of being trapped (ducted) in field aligned columns of ionization Differences in the attenuation of the up-going NBA and NPG signals were consistent with the predictions of magnetoionic theory. They were attributed to pronounced differences in the tilt of the earth's magnetic field, the weaker NBA having entered the ionosphere with a less favorable wave normal angle than that of the south-going NPG signal.

2.2.3 The ionospheric transmission cone

Around this time Bob Helliwell was carefully looking at the conditions on down-coming wave normals that had to be fulfilled if a whistler were to be detected at ground stations. On the assumption of a horizontally stratified ionosphere, he could define a cone of angles within which a wave normal must lie if Snell's law were to be satisfied and the wave were allowed to penetrate (*Helliwell*, 1963). Wave normals outside the cone were beyond the Brewster angle and thus subject to complete reflection. Much depended, of course on the angular spectrum of waves incident on the lower ionospheric boundary, and in general there were many unanswered questions about the form and excitation of ducts and about the untrapping of ducted waves as a function of altitude in the real ionosphere, which was not expected to be free of horizontal gradients.

2.3 The glorious 1960s; Alouettes and OGOs

2.3.1 The Alouettes

In 1961 the Canadians launched the Alouette I satellite into a 1000 km circular orbit, the first member of an inspired series of topside sounders called ISIS or International Series of Ionospheric Sounders. We couldn't have been happier, since the Canadians included in the ISIS series broadband receivers covering VLF up to at least 10 kHz. The data were telemetered to ground stations from time to time and were available at Stanford once a week during a ten minute run. The broadband records, run off on 35 mm paper or film, were full of signals that had propagated to the satellite on a short path (0^+ whistlers) or had crossed the equator after injection into space in the conjugate hemisphere (1^- whistlers). In large part these were all non-ducted events that called for new efforts at interpretation.

In 1960 Australian student Neil Brice had joined our group (as noted above) and had quickly become an important player in many projects. Neil and Bob Smith got particularly excited about the VLF data coming in from the Alouette satellite, launched during that glorious interval when the Canadisns were doing solidly productive work in Space Science. (An earlier period of high productivity had been 1947 when they had unaccountably assembled a truly world class group of scientists, among them Owen Storey, Ian Axford, and Jules Fejer. True to form, they tightened up the budget in the next year, forcing people like Storey to go elsewhere. Which he did, going to France to establish the French low altitude satellite program, which drew excellent people such as Gordon James (from Canada.)

2.3.2 The lower hybrid resonance noise band

The Canadian Alouette VLF principals were Ron Barrington of the Canadian Defense Research Telecommunications Establishment and a colleague Jack Belrose, who observed an unusual noise band with a sharp lower frequency cutoff that moved downward in frequency with increasing latitude (*Barrington and Belrose*, 1963).

Brice and Smith, attuned to the subtleties of changes in the whistler-mode refractive index in space, concluded that the noise was due to local bending of down-coming rays in the direction transverse to **B** at the local lower hybrid resonance (*Brice and Smith*, 1965). In their view a measurement of the noise meant a local measurement of the lower hybrid resonance, In turn the LHR was the result of a resonant interaction involving the local ion constituents,

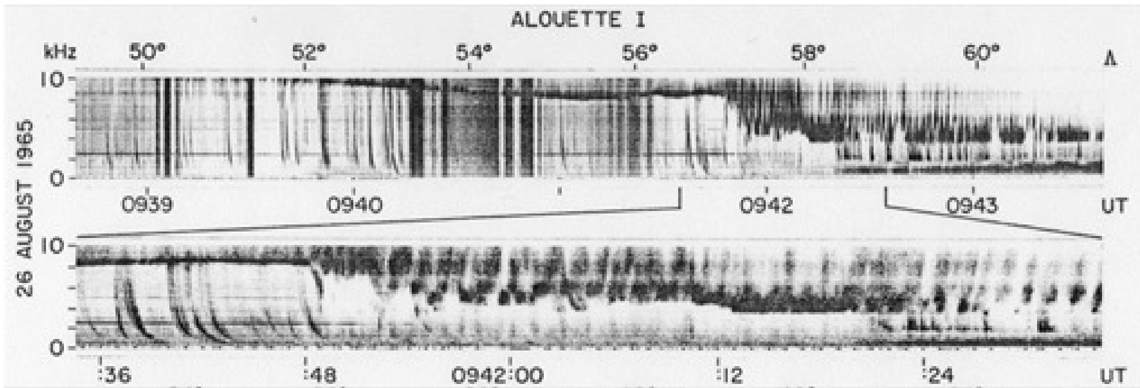


Figure 2.2: Changes in the Lower Hybrid Resonance noise band during a plasmapause crossing by Alouette 1. From *Carpenter et al.* (1968).

O^+, He^+, and H^+. The so-called LHR frequency was defined by an equation involving the effective mass m_{eff} of the plasma:

$$1/f_{LHR}^2 = (1836m_{eff})(1/f_{pe}^2 + 1/f_{ce}^2) \qquad (2.6)$$

with the fractional abundance of the constituents appearing as:

$$1/m_{eff} = \alpha H^+/1 + \alpha H_e{}^+/4 + \alpha O^+/16 \qquad (2.7)$$

Our interest in the LHR was stimulated by the possibility of measuring it as a function of latitude. At the lower latitudes, it tended to be near 6 or 7 kHz, but at the plasmapause there tended to be a sudden drop in f_{LHR} by several kHz to a level dominated by the presence of O^+. In addition we were finding that in the ionosphere at 1000 km, the drop in the LHR could appear abruptly as the satellite moved at about 7 km/second, indicating that the plasmapause, as a cross-L variation, could be very narrow. Fig. 2.2 shows how beautiful that Alouette data could be on this and related points. At left the measurements are within the plasmasphere and changing only slowly with time. Then, within as interval of order one second, there was a major change toward a lower level and the noise began to break up.

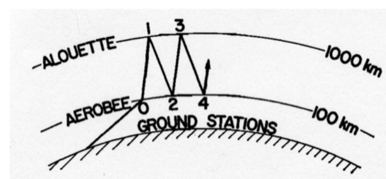


Figure 2.3: Idealized trajctory of a aub-protonospheric whistler, from *Carpenter et al.* (1964).

2.3.3 The sub-protonoospheric whistler

Among Alouette whistlers we were suprised to find events that penetrated the ionosphere but did not continue above $\approx 1000km$, instead bouncing back and forth between $\approx 1000km$ and $\approx 100km$. This so-called SP whistler consistend of a series of low dispersion components that were received at intervals of $\approx 150ms$.

Our observations of SP events were diverse, including at one time or another Alouette at 1000 km, OGO-4 at 670 km, an Aerobee rocket at $\approx 100km$, and even a ground receiver. We could therefore find odd multiples of SP components near the upper reflection altitude, and even multiples near 100 km or on the ground.

Several Stanford VLF people took up the chal-

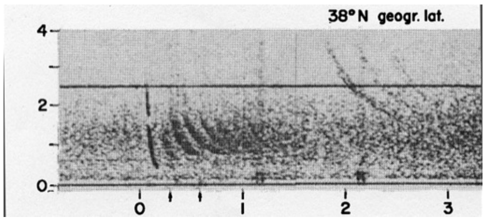


Figure 2.4: SP whistler with 7 or 8 components. Later traces involved propagation along magnetospheric field-aligned paths.

Figure 2.5: Iwane Kimura, circa 1979

lenge of explaining the SP whistler propagation. Bob Smith was first, pointing out that the SP wave normal must be refracted through the direction transverse to the geomagnetic field (*Smith*, 1964). The possibility of transverse propagation had been identified by *Hines* (1957) as occurring when the presence of an ion is taken into account and the wave frequency is below the local lower hybrid resonance. Smith pointed out that the condition of wave normal reversal can occur below the protonosphere at levels where the refractive index diminishes with altitude along the ray path.

Visiting scientist from Japan Iwane Kimura (Fig. 2.5) then confirmed Smith's ideas using a sinusoidal variation with latitude in electron density such that a wave could propagate back and forth along the same path and thus be seen by a vehicle in space as a series of echoes lasting less than a second. (This material was presented at some length in the pioneering paper by *Kimura* (1966) in which the MR reflection process in the plasmasphere was first described).

Years later, in 1975, grad student Raj Raghuram took up the problem of the SP whistler path, having access to the full range of experimental observations made in the 1960s, including those made near 670 km altitude on polar orbiting OGO 4 (*Raghuram*, 1975). He paid particular attention to variations in amplitude of the SP whistler elements and suggested that the paths to 1000 km of the successive elements were different, corresponding to different ionospheric entry points of the causative flash energy and to variations with latitude in the irregularities giving rise to signal refraction.

2.3.4 The transverse whistler

In 1963 we were receiving weekly telemetry from Alouette and paying special attention to departures from the whistler forms familiar in ground-observed events. One such departure was at first called the "walking trace" whistler (by John Katsufrakis) because successive examples seemed to "walk" steadily across the frequency-time spectrogram as the satellite moved from $\approx 44°N$ to $\approx 30°N$. During this interval each example could be described as the combination of a whistler with a particular value of dispersion plus a time delay that was constant for all observed frequencies but which changed (decreased) steadily from event to event over the latitude range identified (*Carpenter and Dunckel*, 1965).

With Iwane Kimura as a science visitor and Brice and Smith paying attention to propagation effects associated with the LHR, an explanation of the walking trace whistler quickly emerged and was published in JGR (*Kimura et al.*, 1965). As in the case of the SP whistler and the LHR noise band, propagation transverse to **B** was found to be involved, so the walking trace whistler was now called the "transverse whistler". The explanation was summarized in a sketch (Fig. 2.6) showing that the part of the event obeying the $D \approx constant$ law was unchanged from event to event, and that energy reflected at the bottom of the ionosphere propagated in the quasi-transverse or QT mode across **B** . As various field lines were crossed, there was scattering from small irregularities into QL propagation along the field lines to the satellite. The principal time delay after the reflection was attributed to the QT mode, since the delay down the field lines to Alouette from the scattering centers would be relatively small.

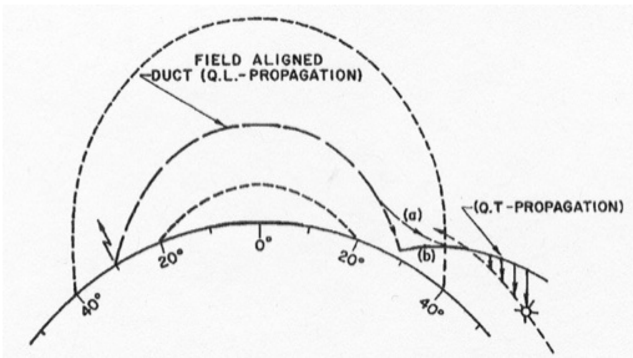


Figure 2.6: Diagram of transverse whistler propagation trajectory. From *Kimura et al.* (1965).

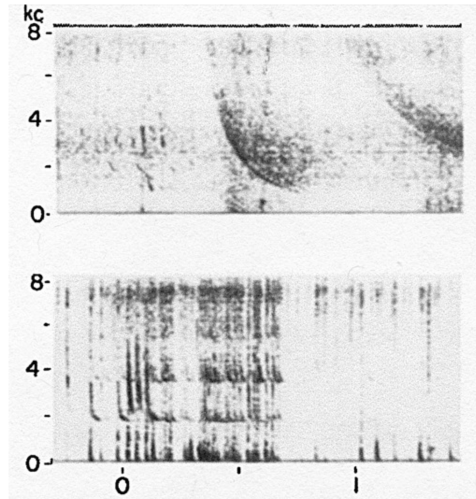


Figure 2.7: Simultaneous Alouette (above) and ground spectra of a transverse whistler. From *Carpenter and Dunckel* (1965).

Our confidence in the original discovery paper depended heavily on comparisons of spheric activity at Stanford with the transverse whistlers seen on Alouette. Fig. 2.7 shows for a case in 1963 the initial impulse on Alouette as well as the same spheric at Stanford (or possibly two very closely spaced ones) roughly 500 miles to the east (the spheric received at Stanford is just to the right of the zero on the time scale). Its spectra are typical of the causative impulses with which we at Stanford had been blessed for years (see Fig. 1.14), well defined above about 2.5 kHz and attenuated or strongly dispersed near the $\approx 1800 Hz$ wavefuide cutoff frequency. It helped that the surrounding spheric activity was less dispersed at that frequency. As discussed below, we ran into opposition about our spheric identification from Stan Shawhan of the U. of Iowa, opposition that disappeared once Stan visited us and became convinced of the correctness of our identification.

2.3.5 A short-lived argument with Stan Shawhan of the U. of Iowa

One of the nicest guys in our field was Stan Shawhan, who got his degree under Don Gurnett at U. of Iowa.in 1966 (*Shawhan*, 1966). His thesis turned out to be flawed, but Stan went on to accomplish notable things, including work on the concept of a wave distribution function and providing excellent leadership in space physics at NASA headquarters before he was stricken with complications that led to his early death in 1990.

I knew Stan in several ways over the years, beginning with his thesis work in the 1960s on a ray tracing code for VLF propagation in space plasmas. In his thesis, Stan claimed that Nick Dunckel and I had misinterpreted the supporting data in a paper about a whistler seen on the Alouette satellite. The whistler had been detected in the low latitude northern hemisphere near the meridian of Stanford, and we had used Stanford ground whistler data to help identify the times of origin of this particular event as well as a series of similar Alouette whistler(s), At the time of our paper I was deeply familiar with the visual properties of whistler spectra recorded on the ground at Stanford, and was fairly well acquainted with broadband records from Alouette. Upon hearing of the U. of Iowa challenge, I tried to explain to them over the phone that we were very confident of our results, being able to see the causative atmospherics very clearly on multiple ground records. To demonstrate my confidence, I laughingly said that If the data had been incorrectly interpreted, I was prepared to come back to Iowa City and work for nothing for the rest of my life (just an expression of bravado; my wife Betty would have killed me).

I managed to persuade Don and Stan to visit us and see the data for themselves. They did visit, looking at rolls of 35 mm paper spectrograms laid out on tables. As I expected, they were quickly persuaded that the causative spherics of the Alouette whistlers were where we had claimed they were.

Unfortunately for Stan, a flaw in his code involving the sign of the electron charge prevented prediction of the magnetospheric reflection, or MR phenomenon, in which the ray direction of a signal with respect to the geomagnetic field **B** is reversed near a location where the frequency of the wave becomes equal to the lower hybrid resonance frequency. (Not surprisingly, Bob Smith was the one who pointed out this error). Fig. 2.8 shows Kimura's remarkable prediction in 1966 (the year of Stan's thesis) of how the MR spectra of a whistler could appear as 1 kHz wave packets move back and forth across the geomagnetic equator from conjugate reflection points. Fig. 2.42 shows the spectra of an MR whistler that crossed the magnetic equator at least seven times. The event was received on OGO 1 (see below) in 1965 and was featured in a paper by Bruce Edgar (*Edgar*, 1976).

2.4 Bob Smith in the 1960s

Bob reports that his happiest times at Stanford were during the 1960-1965 period when (as already noted) he and Neil Brice were wriring papers together. Neil would "generally come over to my house. We would often work until the wee hours. Every so often we would take a break and treat ourselves to Neil's favorite liquor: Drambouie. I still often keep a bottle, and every so often take a small amount and think back to those wonderful days with Neil. I still miss him." (Beginning in 1965, Neil was at Carlton University in Ottawa. He was later at Cornell and at the time of his tragic death in a plane accident in 1974, was working at the National Science Foundation in Washington, DC).

Bob remembers when he and Neil discovered the "crossover" frequency, related to the "hydrogen" whistler. They had just taken a course from Ron Bracewell showing how to derive the electromagnetic equations in a plasma by using rotating instead of the usual Cartesian coordinates. While at a conference, Neil and Bob worked it all out at their hotel "quite simply at night".

As one might expect, there was rivalry between Stanford VLF and the U. of Iowa Physics and Astronomy group, who were represented most strongly in the 1960s by Don Gurnett. Smith recalls a "main disagreement" concerning who was the first to explain the proton whistler and the crossover frequency phenomenon (for the latter see below). Bob firmly believes that he, with help from Neil, should be credited with the explanation, but says there is no way to prove that.

Another area of interest to Bob was the OGO-5 satellite experiment. Bud Rorden did much of the actual receiver design, while someone at Lockheed designed and tested the three axis antenna system. Bob was the one who built the actual input transformer, with double electrostatic shielding.

2.4.1 An example of the Smith-Brice collaboration

A triumph of the Smith-Brice collaboration was a JGR paper entitled "Propagation in Multicomponent Plasmas" (*Smith and Brice*, 1964). The paper seems to have started with the Alouette data and study of a noise band in the few-kHz range whose lower frequency cutoff was suggested by *Brice and Smith* (1965) to be the local Lower Hybrid Resonance in the ionosphere. The LHR had been identified by *Hines* (1957) as an upper frequency limit for propagation transverse to the static magnetic field. It was known that in the case of a moderately dense plasma containing a single ion the local LHR value depended on the mass to charge ratio of the ion relative to that of the electrons. However, since the ionosphere contained multiple ionic species, it became of interest to study how the LHR developed under such conditions.

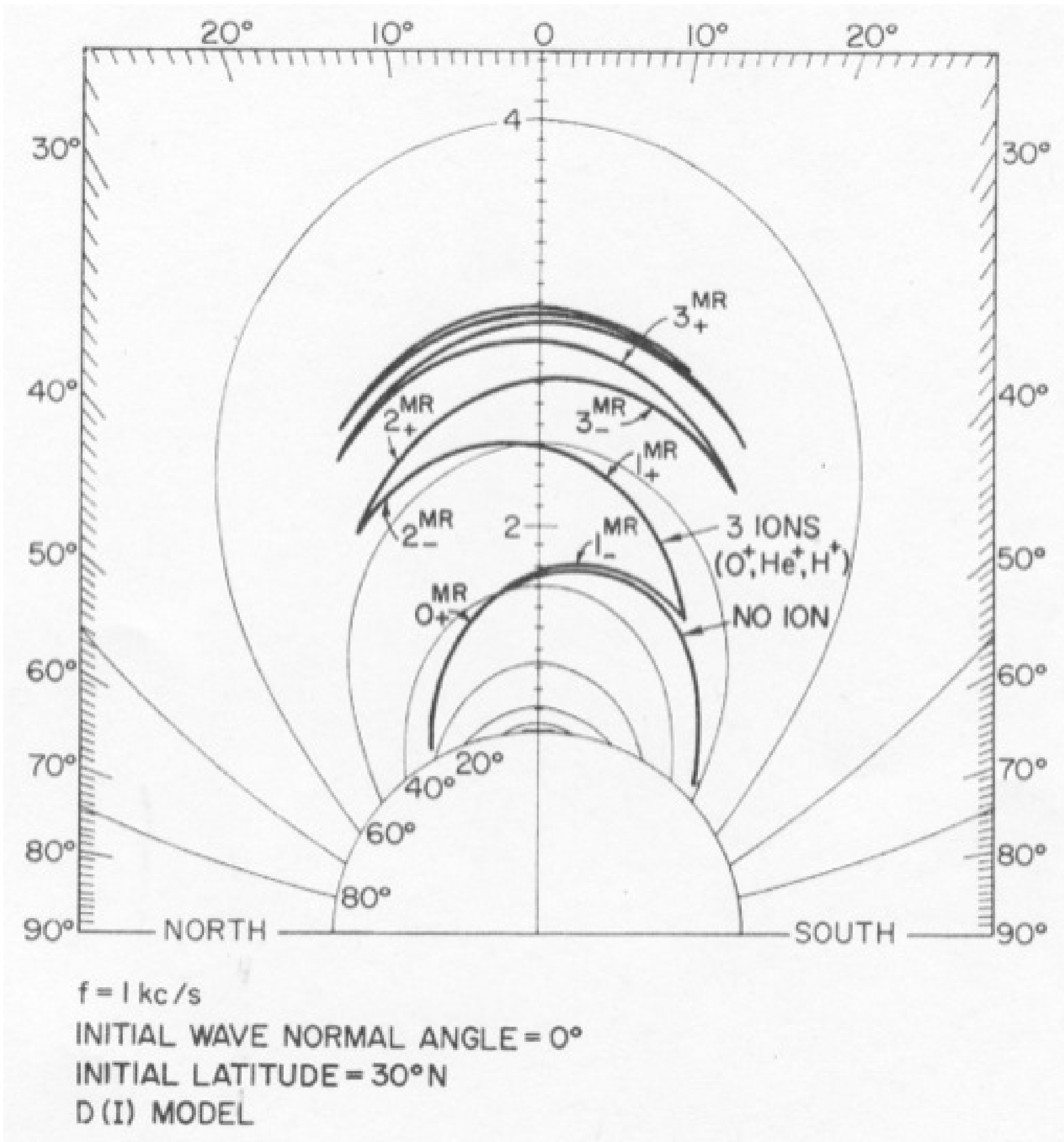


Figure 2.8: Predicted ray path at 1 kHz of a wave packet injected into the ionosphere at 30°Λ. From *Kimura* (1966).

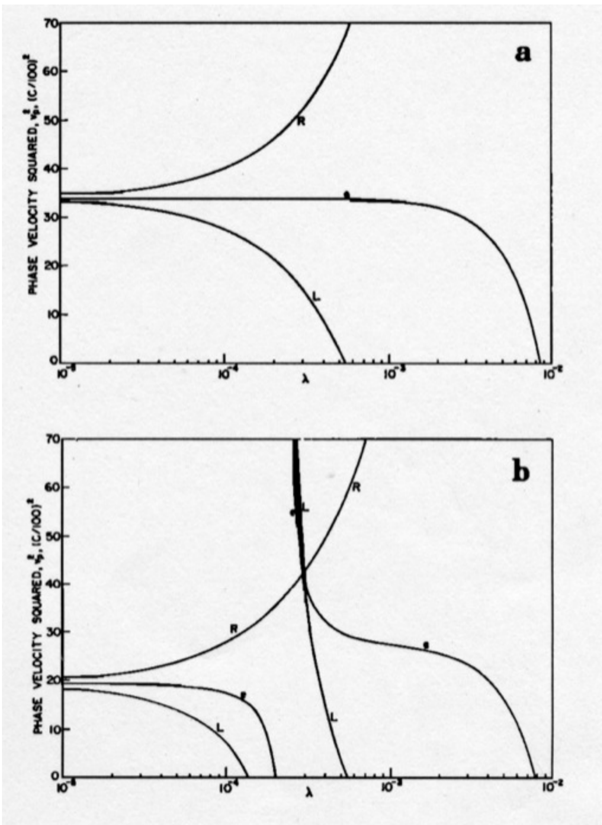


Figure 2.9: The square of phase velocity versus normalized frequency for plasmas consisting of (a) electrons and protons and (b) of 75% protons and 25% Helium ions. From *Smith and Brice* (1964).

Beyond that, there loomed the fascinating question of wave propagation in multicomponent plasmas, a question that had not previously commanded the attention of whistler workers.

In tackling that question, S-B began with Maxwell's equations and proceeded to use the polarized coordinates $(x+iy)/\sqrt{2}$, $(x-iy)/\sqrt{2}$, z of Buneman (1951) as well as Buneman's subscript notation, so that plasma motions may be described as:

$$v_1 = (v_x + iv_y)/\sqrt{2} \tag{2.8}$$

$$v_{-1} = (v_x - iv_y)/\sqrt{2} \tag{2.9}$$

$$v_o = v_z \tag{2.10}$$

As a result, the refractive indices for the three principal modes of propagation in the plasma, the right (+1), and the left (-1) circularly polarized longitudinal modes and the transverse plasma mode are simply expressed as:

$${\mu_p}^2 = 1 - \sum_r \frac{X_r}{pY_r + 1} \tag{2.11}$$

where p may be chosen as +1, -1, or 0.

The terms longitudinal and transverse refer to propagation along or transverse to **B**.

Smith and Brice proceeded to expound upon the refractive index for arbitrary directions of propagation, using material provided in class notes by Buneman (a member of the English Mafia who stormed Stanford back in the 1950s, as did Ron Bracewell, radio astronomer who arrived from Australia). After this they produced sections on polarization, on group refractive index, and on resonance and cutoff frequencies. The fascination of Brice and Smith with these topics is not surprising, given that for much higher frequencies, of the order of or greater than the electron gyrofrequency, "ion effects on propagation are extremely small."

To clarify some of the key features of multicomponent plasma propagation, the authors presented figures illustrating group velocity and phase velocity as a function of frequency (normalized to the proton cyclotron frequency) in a plasma consisting of 80% hydrogen and 20% oxygen. They noted the two-ion resonance, the two ion cutoff frequency, and the crossover frequency. At the latter, the phase velocities of several modes are the same and for a propagating wave there can be a change in polarization.

Neil had a vintage year in 1965. Among other works, he and Bob Smith took part in a major study of the ion cyclotron whistler phenomenon, focusing on interpretation of the data already acquired on satellites such as the U. of I.'s Injun 3 (*Gurnett et al.*, 1965). The first two authors of the paper, from the U. of Iowa, were people with whom there had been differences of opinion in the past, but with whom our group always seemed to be on friendly terms. The paper utilized a number of the ideas that had recently been developed on propagation in multi-component plasmas (by both groups), emphasizing the role of the local crossover frequency, at which (as noted above) an up-going VLF impulse can undergo a switch from right to left hand polarization, allowing energy between that frequency and the next highest ion cyclotron frequency (usually that of protons) to propagate in a distinctive, highly dispersed form called an" ion cyclotron" or proton whistler (Fig. ?).

Figure 2.11: Bob Mlodnosky, circa 1960

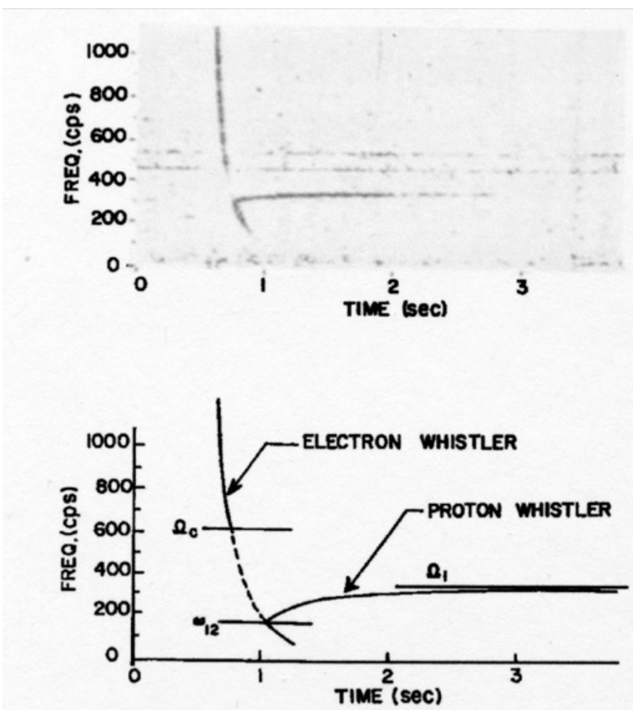


Figure 2.10: The conversion of an upward propagating whistler from the electron mode to the proton mode. From *Gurnett et al.* (1965).

2.5 Bob Mlodnosky, an expected leader of Stanford programs on the OGOs

Bob Mlodnosky, whom I met for the first time around 1960 or 1961, joined the Helliwell group as a Research Associate in 1959 after completing a PhD at Stanford on the topic of radio scattering from meteor trails. He was expecting to be a major player for Stanford in the forthcoming OGO program, talking to me with enthusiasm about measurements of radio noise in space.

For reasons that I never well understood, he asked me to be a coauthor of a short paper he was preparing on the subject of non-thermal noise measurements near planets (*Mlodnosky et al.*, 1962). The paper, presented at a colloquium in Liège, France in 1962, discussed in introductory terms the use of vehicles in either flyby or gravitationally bound orbits to study plasma density as a function of distance from a planet. One would measure both the spectrum of natural noise activity intrinsic to interplanetary plasmas as well as waves emanating from electrical activity in the planetary environment of interest. My connection with Bob was enhanced during a meeting in Washington DC when we were at a table with Tim Bell at a restaurant. There was live music in the form of guitar playing by the great Charley Byrd, which Tim Bell, a veteran banjo player, particularly appreciated. I do not remember specifics of the conversation, except that the enthusiasm of Bob for his work on upcoming OGO projects was manifest.

2.5.1 The tragic loss of Mlodnosky

A few weeks later, on June 24th, as I was walking down a hall in the Electronics Research Lab, a secretary spotted me and called out for help. Bob had apparently been standing by her desk, and I saw him beginning to slump against a bookcase by the far window. I rushed in and grabbed him, lowering his now unconscious body to the floor. The draining of color from his face, as if by gravity, could be clearly seen. I soon learned that he had had an aortal aneurysm, wherein blood was no longer being supplied to his brain. I called to a passing student Ron Todd, who grabbed a CPR breathing tube from a first aid kit and tried to blow air into Bob's lungs. There were no signs of life. I ran outside the building to guide the expected ambulance and spotted a student who I knew was an MD. He rushed in and attempted some type of injection into Bob's chest, but it was clearly of no avail. I soon went to the Stanford Hospital and managed to tell Bob's wife Lucille what had happened. The shock of losing Bob must have been overwhelming; within a few years Lucille took her own life.

The loss was hard on Mike Villard, then director of the Radio Science Laboratory. Not long thereafter he lost one of his key young researchers, Phil Gallagher, through electrocution at a field site. Mike must have wondered how it was that Fate had brought so much woe in his direction.

2.6 The orbiting geophysical observatories (OGOs)

The OGOs were a phenomenon of the 1960s, an age of exploration and discovery in polar orbit and on eccentric orbits extending through the near-equatorial magnetosphere and into the solar wind. Stanford VLF was an active experimenter on OGOs 1 and 3, in eccentric orbit, and on OGOs 2, 4, and 6 in polar orbit at ionospheric heights. In a sense we were also participants in OGO 5, in eccentric orbit, through our collaboration with and mentoring of the OGO 5 investigators, although we did not provide a receiver for that mission.

2.6.1 The launch of OGO 1 in 1964

In July, 1964, I had the privilege of watching OGO 1 launched from Cape Canaveral, Florida. Bud Rorden of SRI, in charge of the VLF receiver, had been there for some time. He invited John Katsufrakis and me over for oysters and dinner one night after I had unaccountably had some ice cream elsewhere. From that day onward I had minimal craving for oysters.

In any case it was thrilling to watch the launch and to realize that our VLF group was on the way to acquiring data from unexplored territory at great altitudes. I did not expect to participate in studying the new data, since I knew next to nothing about the receiver's capabilities and was myself immersed in study of the anomalous ground events we were beginning to call "knee whistlers." However, I did take part in meetings held in the fall of 1964 as we talked excitedly about all the papers that were going to be written.

Around this time OGO 2 was launched from Vandenberg Air Force Base in California into a nearly circular polar orbit at about 800 km altitude. It had thus joined Canadian topside sounding satellites Alouette 1 and 2, the former in 1000 km circular orbit and latter in polar orbit varying in altitude from 3000 to 600 km. I was personally excited about the so-called fractional hop whistlers, impulses propagating upward from lightning and dispersed only to the extent of effects imposed during their first passage through the ionosphere. With OGO 2 our group could give special attention to topics such as the latitudinal variations in strength of up-going signals from transmitters, variations in whistler form with latitude, etc. On the latitudinal records we became familiar with sharp changes in whistler-mode waves such as signals propagating from whistler sources in the opposite hemisphere and VLF noise bands, many of which were not present on simultaneous ground recordings.

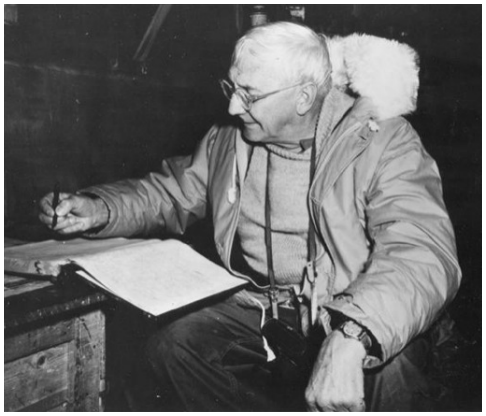


Figure 2.12: Sir Charles Wright, circa 1965

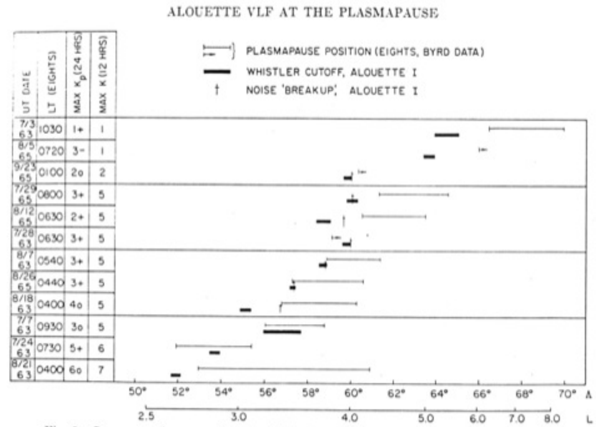


Figure 2.13: Correlation of plasmapause positions at 1000 km inferred from Alouette 1 in 1963 and 1965 with the knee position found simultaneously from whistlers at Byrd and Eights Stations. From *Carpenter et al.* (1968).

2.6.2 Coupled high and low-altitude effects at the plasmapause

During the 1960s the ionosphere was being sounded both from above and below. Much had been learned, but there were burning unanswered questions at conferences about what was going on above the ionosphere. What are the electric fields and plasma fluxes there that might affect the structure and dynamics of the region below? Not yet asked were specific questions about effects at the ionospheric projection of the plasmapause.

A fortunate aspect of our work was that the knee was essentially a middle-latitude phenomenon and was probably aligned with the Earth's magnetic field (Angerami had provided strong evidence for such alignment). Some ionospheric workers therefore began to seek an equivalent ionospheric variation. An example was a strong interest on the part of 80+ year old Sir Charles Wright (Fig. 2.12), a veteran Antarctic explorer and long time student of geomagnetic variations. He visited us several times at Stanford, later reciprocating by greeting Bob Smith and me at the Vancouver B.C. airport and then (with improbable success) driving us into town through a rainstorm.

Sir Charles was seeking some feature in ground based magnetometer records that might be related to the knee, as were one or two people looking at latitudinal variations in density observed on satellites or in radio scintillations detected on the ground. Prominent in studying density variations was Michael Rycroft, visitor at nearby NASA Ames research laboratory. A "mid latitude trough" had been found in density at 1000 km (e.g. REF), a feature that Michael considered promising (see note above) in terms of its regular occurrence near L=4 and its tendency to be displaced equator-ward at time of increased disturbance.

Discussions of coupled high and low altitude effects were carried on at Stanford and in the wider community for several years beginning in $\simeq$ 1965. We used both Alouettes and various OGOs, gradually obtaining a more nuanced and focused picture. That picture was not going to be simple: cross- $\mathbf{B}$ profiles at high and low altitudes showed evidence of close coupling during the relatively fast onsets of disturbances but during quieting seemed to recover at different rates and therefore be influenced by different physical processes.

On Alouette we often identified the 1000 km projection of the knee as a latitudinal "breakup" or sudden change in the frequency or spectral form of LHR noise. At this projection there also tended to be changes in the spectra of whistlers propagating from the conjugate hemisphere, including whistler triggering of LHR noise at and just above a rapidly decreasing lower hybrid resonance frequency (see Fig. 2.2) as well as a general decrease in whistler activity. (These changes were not surprising: on ground records whistler propagation in or beyond the region of steep knee gradients had always been less common than in the nearby plasmasphere. Multiple plasmaspheric paths had often been excited but had also shown high-latitude limits, an example being the 1959 SE event on the upper panel of Fig. 1.19. The lowest nose frequency at $\simeq 7kHz$ corresponds to an outermost plasmaspheric path at $L \simeq 3.6$.

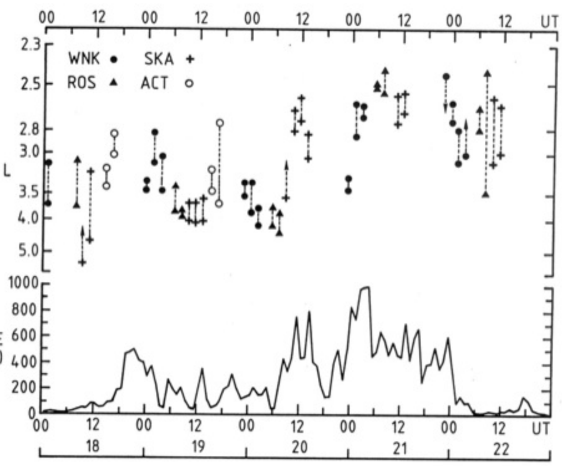


Figure 2.14: Plasmapause crossings by OGO4 during five days in September, 1967. The AE index is shown below. From *Carpenter and Park* (1973).

Over time we found that ionospheric changes in whistlers and LHR noise on Alouette were well correlated with, but often slightly equatorial of, locations known from conventional whistler analysis to represent the equatorial region of steep density gradients. As shown in Fig. 2.13, this relationship prevailed over a wide range of magnetic conditions and hence latitudes. A dipole geomagnetic field was used in both analyses, and apparent positional differences between low and high altitude were attributed to distortions in the geomagnetic field (*Carpenter et al.*, 1968).

The polar orbiting OGOs used loop antennas, which might have been expected to make them less sensitive to the noise band than were the Alouettes with their electric dipoles. However, they showed strong LHR noise changes along the many orbits that could be seen from OGO ground stations. Fig. 2.14 shows a five day series of plasmapause locations identified from OGO 4 in September, 1967. The position of the knee is either bracketed or marked as lying beyond an outermost detectable plasmaspheric path.

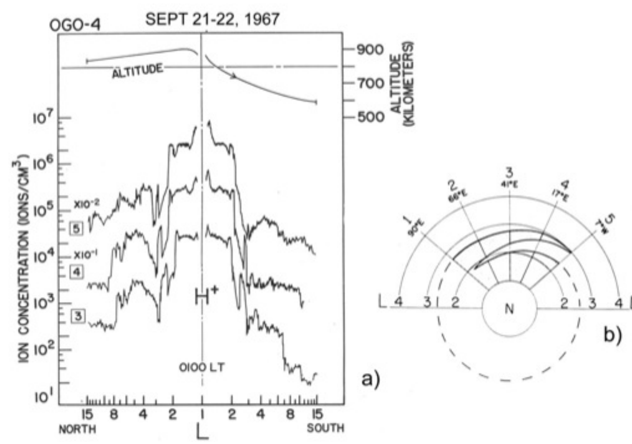


Figure 2.15: (left) OGO 4 ion mass spectrometer profiles of plasma density near 800 km on three successive orbits. (right) Inferred equatorial form of outlier indicated in 800 km data. From *Taylor et al.* (1969).

From studies of OGO 2 and 4 data it was found that upgoing signals from powerful communication transmitters dropped sharply in intensity (with increasing latitude) at the plasmapause (*Heyborne et al.*, 1969). This was a finding of Bob Heyborne, the only grad student who began every one of his talks with an after dinner joke. Such a drop in intensity was not surprising, since we had found from the 1963 and 1965 Eights data that ground-to-ground whistler activity was relatively low beyond the plasmapause, and when observed, tended to concentrate in certain local time regions either close to the plasmapause or on L shells some distance beyond it.

In the 1960s I had the pleasure of working with Harry Taylor of GSFC and his colleagues, who (as noted above) operated ion mass spectrometers on several of the OGOs. Their data obtained from OGO 2 in 1965 between 400 and 1200 km showed clear projections to the ionosphere of what we believed to be the plasma structure near the plasmapause in the overlying medium (Fig. 2.15). The summary of an article published in 1969 (*Taylor et al.*, 1969) states that "the high-latitude ionospheric distributions of H^+ and $H_e{}^+$ are rapidly depleted, forming a light ion trough near 60°Λ. This trough is often quite steep, as n(H^+) and n($H_e{}^+$) decrease by as much as an order of magnitude within 3°Λ. Coincident with the light ion trough the rate of reception of whistlers and the ground-to-satellite propagation of VLF signals (*Heyborne et al.*, 1969) also decrease abruptly within 3°Λ, the transmitted VLF signal dropping by as much as 20dB within this interval.

Taylor's figure shows the ionospheric projection of an outlying dense structure that has come in recent years to be called the "plume." Data from successive orbits were combined to produce the sketch at the right, indicating an inferred extension of the main plasmasphere. Such a connected form was later reported from whistler data (*Ho and Carpenter*, 1976).

A truly astounding combination of measurements was achieved on September 29, 1967 as Alouette 2 moved poleward at 800 km and sounded the ionosphere (*Clark et al.*, 1969). The satellite experimenters noticed not only echoes from the ionosphere below, but also from what appeared to be a "wall" of ionization located near Alouette in the equator-ward direction and apparently aligned with the Earth's magnetic field. The authors were evidently unaware that they had sounded the plasmapause surface. Fig. 2.16, from their report, shows iso-density contours deduced from three successive soundings around 0908 UT, while Fig. 2.17 shows their simplified model of the reflection at a density wall of a transversely directed sounder ray at a particular frequency.

Another element of this remarkable combination of observations was an extensive sub-auroral red (SAR) arc that was under observation in the ionosphere as Alouette passed over. There were also ionospheric measurements of wave activity from successive orbits of OGO 2. Fig. 2.18 shows the orbital projections of Alouette 2 and OGO 2, as well as the location of Fritz peak, labeled F, from which the SAR arc was observed. Evidence of a plasmapause crossing collocated with SAR arc observations appears in Fig. 2.19, with sudden changes in LHR noise and whistlers from conjugate sources. A bar indicating the estimated location of the SAR arc appears below the OGO 2 record.

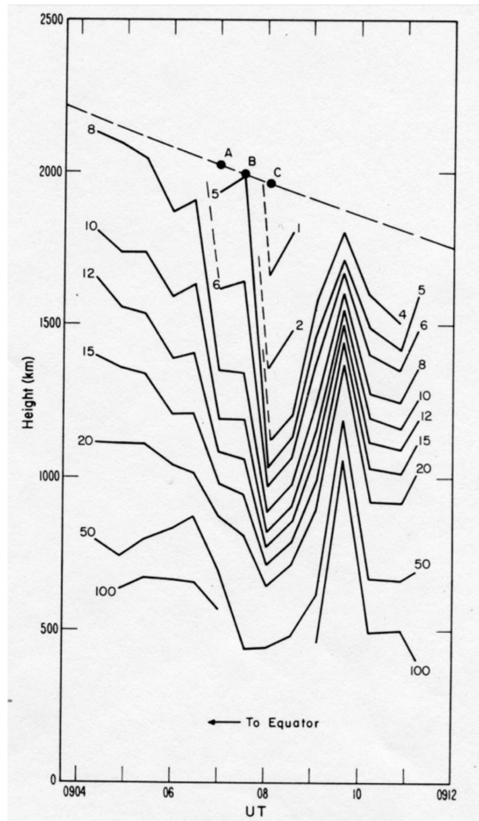


Figure 2.16: Isodensity contours deducedfrom three successive Alouette 2 soundings of the plasmapause at 800 km. From *Clark et al.* (1969).

Thus by 1970 our view was that during periods of enhanced disturbance, the plasmapause was physically coupled to SAR arcs and was characterized by light ion upflows and a region of enhanced absorption of locally injected VLF waves.

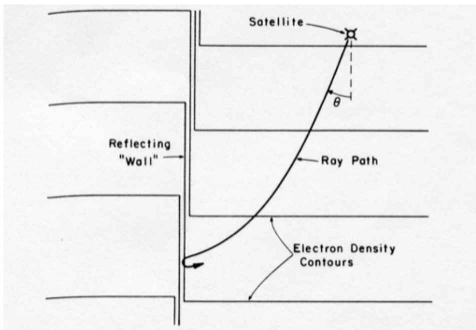


Figure 2.17: Simplified propagation model of a sounder ray at a particular frequency reflecting from a nearby density wall. From *Clark et al.* (1969).

2.6.3 Grad students from Brazil: Fernando Walter

Looking back over the OGOs and the 1960s one sees the emergence of ray tracing as a primary tool for investigating whistler-mode propagation in space. Iwane Kimura had already taken big steps in that direction (*Kimura*, 1966), and now Brazil gave us new explorers, among them Fernando Walter. Fernando modified the ray tracing procedure developed by Kimura and with the help of Jacyntho Angerami (*Walter and Angerami*, 1969), used it to study a new type of non-ducted whistler that was often observed on the polar OGOs 2 and 4 between 47 and 56 degrees invariant latitude in the hemisphere opposite to that of the lightning source.

To appreciate the discovery context in which this occurred, consider the spectra in Figs 2.19 and 2.20 from the Walter/Angerami (W/A) paper. In Fig. 2.21 there are what appear to be a pair of falling tone whistlers, labeled 1_{-}, across which two equally spaced rising tones, labeled WT, are extended. The times of origin of these overlapping whistler components are visible as two vertical lines at the left (the first one is marked 0_{+}). How can this overlap occur? Then in Fig. 2.22, we see fixed frequency whistler-mode pulses from Omega transmitters in the $\sim 10 - 12kHz$ range. One of these pulses, from Aldra, Norway, was transmitted at 11 1/3 kHz at the time marked by a line at the top of the figure. This

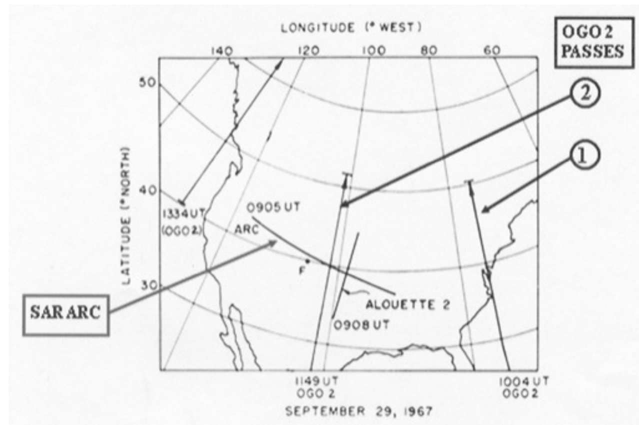


Figure 2.18: Three OGO 2 suborbital tracks near the time of Alouette 2 sounding of a density wall and correlated SAR observations from point F. From *Carpenter* (1971).

pulse was received twice in the southern hemisphere, once at 11 1/3 kHz and again about 1.2 sec later at a higher amplitude and downward shifted from 11 1/3 kHz by about 100 Hz.

The W/A paper explaining these remarkable effects was a milestone in the emergence of ray tracing as an investigative tool. For example, it brought to light those physical factors that most influence an up-going whistler mode wave when the wave is not subject to guidance (ducting) by field aligned density irregularities. In the discussion of ray paths starting near 45° degrees invariant, the behavior of the refractive index and hence the ray were found to depend at lowest altitude on the geomagnetic field, at higher altitude on the plasma density model, and then as the ray reached its maximum L value and began to point along **B**, on the high altitude curvature of the **B** field and associated increases in the refractive index and reductions in wave phase velocity Fernando was a good teacher, on this occasion and on others making sure that the reader was aware of the major factors controlling the behavior of ray paths.

The W/A work shows how quickly the Magnetospheric Reflection (MR), first reported by *Kimura* (1966), had become important in the study of non-

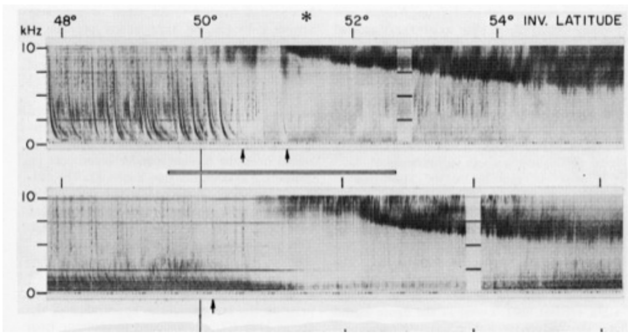


Figure 2.19: VLF records from OGO 2 on two passes from Fig. 2.18. SAR arc location during second pass shown by a horizontal bar. From *Carpenter* (1971).

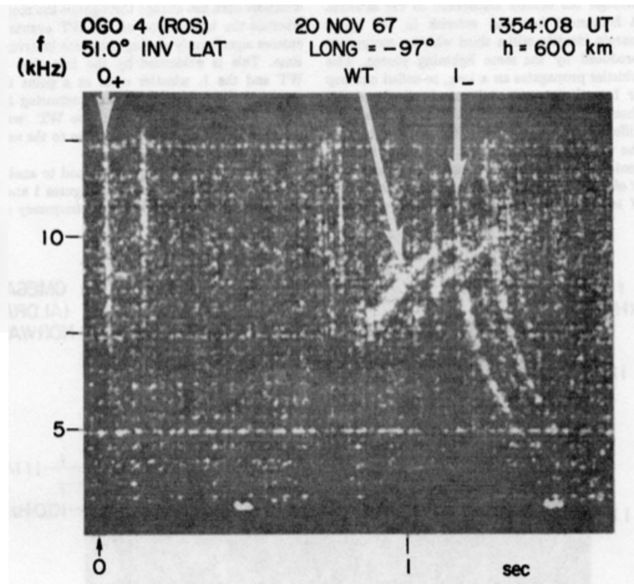


Figure 2.20: OGO 4 whistlers observed at about 50° invariant. From *Walter and Angerami* (1969).

ducted propagation in space. Fig. 2.23 is a plot of the LHR as a function of altitude according to a particular magnetospheric plasma density model. There are two peaks, near 6 kHz and 7 kHz, the lower one because of the density increase in the F layer, the upper because of the transition with altitude from an $O+$ dominated plasma to an H+ dominated one. When whistler mode waves propagate downward with high wave normal angles after crossing the equatorial region, those at frequencies below the value of the upper maximum LHR are reflected upward from points along the LHR profile. Hence in Fig. 2.20 those reflected waves are not seen on OGO 4, which operated below the $\sim 7kHz$ kHz peak.

Walter and Angerami succeeded in using ray tracing to explain virtually all of the effects noted in Figs. 2.21 and 2.22, including the way in which, within a couple of degrees observing latitude, the rising trace in Fig. 2.21 appeared to move or "walk" across the relatively fixed falling tone. They were able to predict the dispersion properties of the multiple WT whistlers observed during a particular OGO 4 pass. They could predict focusing and Doppler shift in transmitter signals, due variously to the convergence at a given reception point of signals injected at different latitudes and to the tendency for wave vectors to become large and perpendicular to **B** in the hemisphere opposite the transmitter. They regarded the WT mode as a delicate effect, quite sensitive to details of magnetospheric structure in both plasma density and **B**,. Taken together, their advances in understanding of non-ducted propagation, offered within only a few years of Kimura's landmark paper, were breathtaking. Not until the operation of polar orbiting Demeter in the mid 2000s were the effects they so carefully studied "rediscovered."

2.6.4 Additional comments on the W/A work

How did W/A explain the whistler marked 1_- in Fig. 2.21, which did not exhibit substantial changes in dispersion during a sequence of WT events? A communication to W/A from another Brazilian student, R. Scarabucci, suggested that those whistlers were non-ducted but propagated at relatively small wave normal angles as a result of horizontal density gradients in portions of the magnetosphere (hence their frequencies below 7 kHz would not reflect at the LHR above OGO 4). W/A comment that the presence of those density gradients would also limit the occurrence of WT whistlers in latitude.

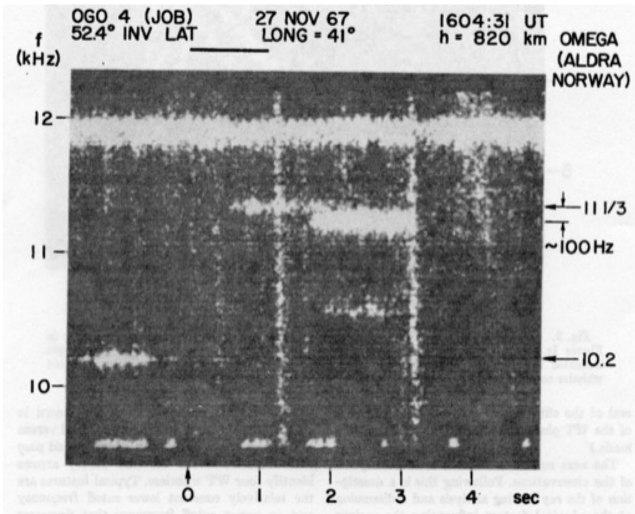


Figure 2.21: OGO 4 signals from Aldra, Norway Omega transmitter showing WT propagation effects. From *Walter and Angerami* (1969).

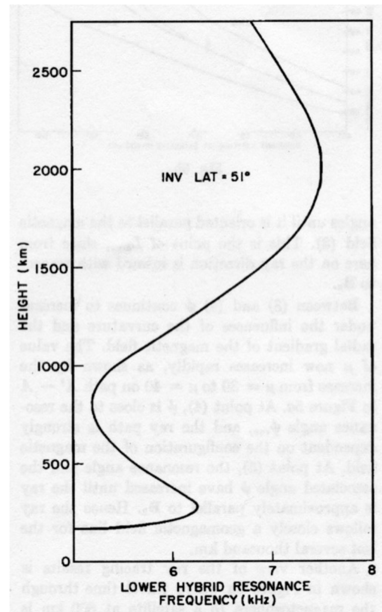


Figure 2.22: Altitude profile of the Lower Hyrbrid Resonance based on a plasma density model. From *Walter and Angerami* (1969).

John Katsufrakis (Fig. 1.31), who regularly looked at 35 mm film or paper records of incoming data, was the first to spot what he called the "Walking Trace" whistler and call it to Walter's attention. John had previously seen anomalously changing components in Alouette whistlers recorded within a lower range of latitudes (30 to 44°). (These had come to be called "Transverse Whistlers" and as described above had been interpreted as having first propagated quasi-longitudinally along an essentially fixed field line path and then, after reflecting from the conjugate ionosphere, propagated pole-ward across geomagnetic field lines. As the waves crossed field lines they were scattered from irregularities and thus spread downward to the satellite. The slowness of their pole-ward transverse propagation led to substantial variations with reception latitude in total propagation delay.)

2.6.5 Evidence of whistler ducting found on OGOs 1 and 3

The two high altitude satellites OGO 1 and OGO 3 provided our first opportunities to observe in situ the field aligned ducts that years of ground observations and interpretive work had led us to believe in, but for which we had mostly indirect evidence. Satellites could hope to penetrate ducts and record whistlers propagating therein. The first such observations came with OGO 1 in October 1964 and were described by *Smith and Angerami* (1968) in a discovery paper that also covered a variety of previously unknown non-ducted whistler events.

Figure 2.24, from that paper, shows tracings of 6 whistlers received near L= 3 on October 24, 1964. Each is referred to the time of its causative spheric recorded at a ground station. The ducts were penetrated successively on an inbound pass, and within each, multiple whistlers exhibited the same travel time. The equatorial separations of the ducts ranged from 50 km to 500 km and their equatorial thicknesses were typically about 400 km. As the satellite moved inward to lower L values, the event travel times decreased and nose frequencies increased ac-

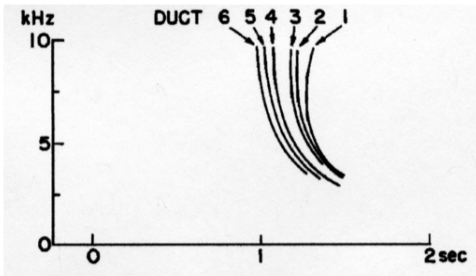


Figure 2.23: Tracings from 35 mm rrecords of a succession of ducted whistlers received on OGO 1. From *Smith and Angerami* (1968).

cording to expectations. S and A used the travel times of the whistlers to test models of the distribution of ionization along the field line paths, thus finding new support for a diffusive-equilibrium type model as opposed to one that varied as R^{-4}. There remained issues possibly associated with magnetic field distortions or small uncertainties in satellite position, but the principle findings about ducts appeared to be on solid ground.

2.6.6 Angerami finds striking evidence of ducting on OGO 3

Then *Angerami* (1970) published a meticulous study of ducted whistlers observed on an inbound pass of OGO 3. More than 40 years later we still depend upon and are enormously grateful for this rare work, which corroborated and extended ground based observational studies and in so doing put the ducting phenomenon on a foundation that could easily be defended.

In the past twenty years I have twice had occasion to argue with space physics people about the reality of field aligned whistler ducts. In confronting these skeptics I have had excellent ammunition, in particular the 1970 Angerami paper. In both cases the skeptics became believers.

The propagation concepts involved in the OGO 3 case are illustrated by the sketches of ray paths in Figure 2.25. In part (a) there are two ducts, and all wave frequencies are assumed to be below half the

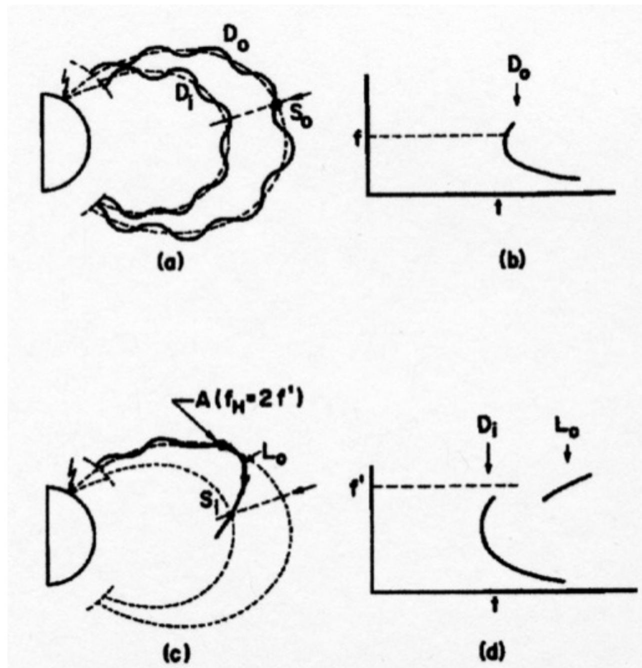


Figure 2.24: Idealized sketches of two ducts, including leakage from the outer to the inner duct in (c) and (d). From *Angerami* (1970).

gyro-frequency everywhere along the duct. A satellite S_o crossing the outer duct would be expected to receive a whistler as shown in the idealized f-t spectra in (b). Part (c) shows the ray path of L_o, an inward propagating signal at frequency F that leaks inward from duct D_o. The leakage begins at a point A along D_o where F is equal to half the local gyro-frequency. The signal above F may then be received as a rising tone by a satellite inside duct 1, where a ducted signal D_1 may also be received.

For some time our group had argued that the relatively sharp upper cutoff of ground-observed whistlers was a cold plasma cutoff effect, attributed to the untrapping of whistler waves above half the equatorial gyro-frequency, the lowest such frequency along a given ducted path. and was not due to the thermal]damping that other investigators had proposed (Scarf (), Liemohn ()). Jacyntho was the perfect man to find direct evidence of this leakage phenomenon. He observed leaked signals in the form of rising tones, and then connected a given signal with the duct at higher L value from which it had escaped. Using ray tracing, he calculated propagation paths for rays with a small range of wave normal angles that could escape from a duct at a particular altitude. The leaked waves would then propagate onward, variously remaining close to the duct at first and then turning inward so as to be detected between and within ducts at lower L values.

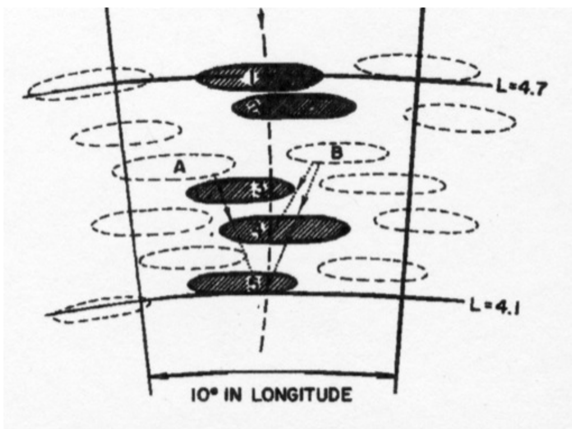


Figure 2.25: Equatorial cross sections of ducts intercepted by OGO 3 (shaded). From *Angerami* (1970).

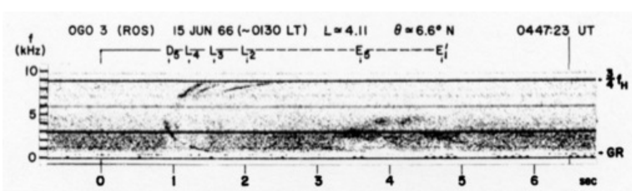


Figure 2.26: Spectra from inside duct no. 5, the innermost. From *Angerami* (1970).

The inferred equatorial cross sections of the whistler ducts encountered by OGO 3 on June 15, 1966 are marked by shading in Figure 2.26. Five discrete paths were intercepted between L=4.7 and L=4.1, each well inside a magnetically quiet nighttime plasmasphere. The observations were on magnetic meridians that followed the east coast of the US and Canada, where substantial summertime lightning activity could be observed and could be expected to launch whistlers over a wide range of latitudes. The ducts were intercepted at near-equatorial latitudes in the northern hemisphere and within each duct there were discrete whistlers, each with the same dispersion. Figure 2.27 shows a spectrogram from inside duct no. 5 at L=4.1. A line near 9 kHz indicates the local value of f_H. The time scale is referred to the time of the spheric that launched the whistler marked D_5 and also the leakages from ducts 2, 3, and 4 that appear in the upper part of the record as rising tones L_2, L_3, and L_4. This spheric time was determined from VLF on the ground recorded simultaneously with satellite data being acquired at the NASA tracking station at Rosman, NC.

On the basis of the radial extent of the intercepted ducts, Angerami estimated that they were elongated longitudinally by a factor of 4 or 5, a principal consideration being the probable blocking by such shapes of inward leakage from nearby ducts (sketched as A and B) that were not directly penetrated by OGO 3. Jacyntho was also able to use wave duct trapping theory to infer that the density enhancement factors were in the range 4-8 percent. His work was capped by ray tracings that were based on a diffusive equilibrium density model of the plasma along the field lines and which agreed well with the observed ducted whistler delays as well as those of the leakage signals.

This was a true tour de force. It was based upon Angerami's intimate knowledge of both ground and satellite data, his analytical expertise, the meticulous approach that he took to every problem, and the thoroughness of his work. He found help within the group, but also provided much help to others. The VLF field was deprived of one of its best in 1972, when Jacyntho returned to Escola Politécnica in Brazil

2.6.7 Jose Muzzio explains the "ion cutoff whistler"

Jose Muzzio was another of our group of grad students from Brazil back in the days of OGO 2 and OGO 4. Among his lesser known but quite fascinating achievements was identification of the "The Ion

Cutoff Whistler", a downward propagating event that became subject to reflection at frequencies below the local proton gyro-frequency f_H, not because of steep density gradients but because of the pathology of the whistler mode refractive index at frequencies below f_H in the presence of multiple ion species. Jose's discovery helps to explain why the OGO data were so much fun to look at.

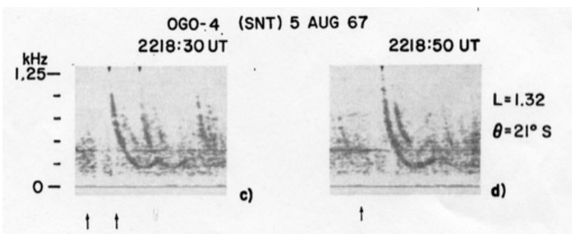


Figure 2.27: Spectra of ion cutoff whistlers received on OGO 4 at low southern latitudes. From *Muzzio* (1968).

Fig. 2.28, from his paper (*Muzzio*, 1968), shows several ion cutoff whistlers received on OGO 4 at low southern latitude on Aug. 5, 1967. The whistlers exhibited Eckersley type constant dispersion down to about 500 Hz, but then flattened out at about 450 Hz and turned upward for about 200 Hz. They were called"cutoff" because of an apparent reflection at what had recently been identified as the two ion cutoff frequency.

It is not easy to visualize the development of an ion cutoff whistler. Figs. 2.29 and 2.30, included in Muzzio's article and slightly modified for clarity, can help with this. In Fig. 2.29 we see once again, as in the work of *Smith and Brice* (1964) and *Gurnett et al.* (1965), a sketch of wave phase velocity versus frequency in a range strongly affected by the relative H^+ concentration, extending from the proton gyro-frequency f_H down past the first crossover frequency f_x and the first cutoff frequency f_c (see Fig. 2.29). Fig. 2.30 shows how f_c and f_x, being dependent upon ion composition, vary with altitude, approaching f_H at low altitudes where the relative concentration of H^+ is low. In contrast, as altitude increases and the relative H^+ concentration goes up, the two frequencies become increasingly separated from f_H, as shown on Fig.2.30.

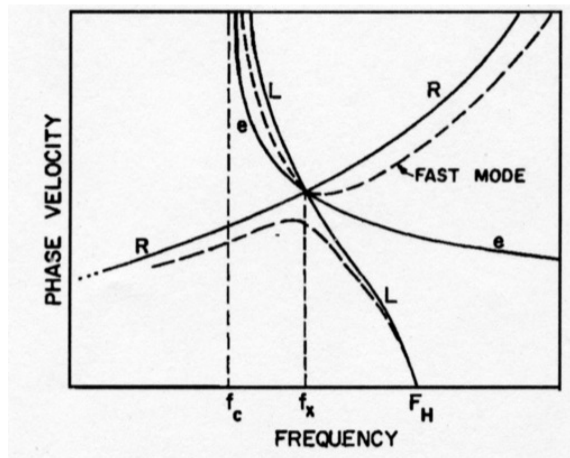


Figure 2.28: Phase velcity versus frequency for the principal modes in the frequency range most affected by the H^+ concentration. From *Muzzio* (1968).

Consider a downward propagating whistler, sketched in the inset of Fig. 2.30, as it would be observed on a satellite at altitude h_s. With time, wave frequency f decreases in falling-tone fashion (from right to left along the horizontal axis of the figure). As f falls below local f_H, a wave at frequency f_1 may propagate down past the satellite, be reflected at the cutoff frequency f_c for f_1, and begin propagating upward. Reflections and upward propagation then continue at $f < f_1$, occurring at progressively higher altitudes as shown by the plot of f_c in Fig; 2.30. Reflection finally occurs at f_s, where $f_s = f_c$ at the altitude of the satellite. Frequencies below f_s are not seen by the satellite, being reflected above its altitude. Meanwhile, reflected waves at frequencies from $\simeq f_1$ down to f_s, traveling upward to the satellite on paths of decreasing length, have caused the received whistler to bend upward on a time-frequency spectrogram. The time delays from the initial receptions of f_1 to f_s on the satellite to their receptions after reflection (at the right side of the whistler dip) provide measures of the group velocity at the altitudes in question.

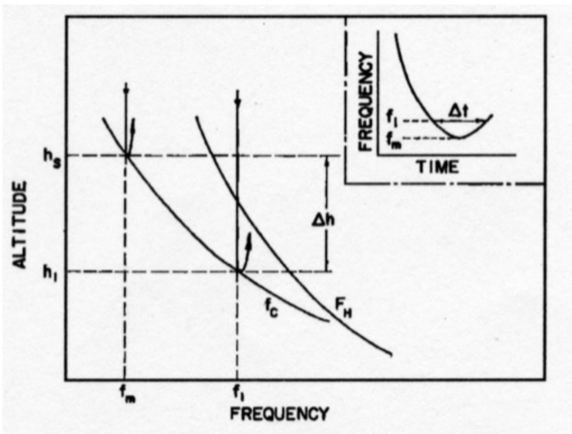


Figure 2.29: Sketch of the variations of f_c and f_H with altitude and a corresponding ion cutoff whistler diagram. From *Muzzio* (1968).

2.6.8 Muzzio and Angerami, a final tour de force with OGO 4 data

In 1972 Jose Muzzio, aided by Angerami as co-author, published a paper entitled "Ogo 4 Observations of Extremely Low Frequency Hiss" (*Muzzio and Angerami*, 1972). At the time of publication in JGR, March 1, 1972, Muzzio had obtained his degree and both authors had returned to Brazil to work in technical universities or institutes, Muzzio at I.T.A., Divisão de Electrônica, Sao José dos Campos, S.P., Brazil. Angerami at Esc. Politécnica Univ. S. Paulo, Caixa Postal 8174, São Paulo, Brazil.

Their paper was of a discovery nature, in that it provided tha most complete description to that time of what later came to be called "plasmaspheric hiss". Evidence of this Extremely Low Frequency (ELF) hiss, which they called band limited hiss, or BLH, had been reported from other satellite observations such as those of Dunckel and Helliwell [1969], who had found evidence on OGO 1 that ELF hiss was generated close to the equatorial plane; Thorne and Kennel [1967], who had investigated the dependence of ray paths on the initial wave normal angle; Russell et al. [1969], who had observed hiss in certain regions outside the plasmasphere; Mosier et al. who had reported on the Poynting flux of hiss observed at low altitudes. However, Muzzio and Angerami were able to give new and more comprehensive evidence of ELF hiss along the polar OGO 4 orbit.

The hiss was well defined between $\sim 10°$ dipole latitude and $\sim 55°$. It exhibited a sharp low frequency cutoff at around 400 Hz, which was identified as the two ion cutoff for downward propagating signals (see fig. 2.29). The cutoff exhibited equatorial erosion on both sides of the equator at $\sim 10°$ and was spatially variable because of its dependence on the geomagnetic field strength. The upper cutoff of BLH was near 600 Hz, and was nearly independent of satellite altitude between $\sim$ 430 and 900 km.

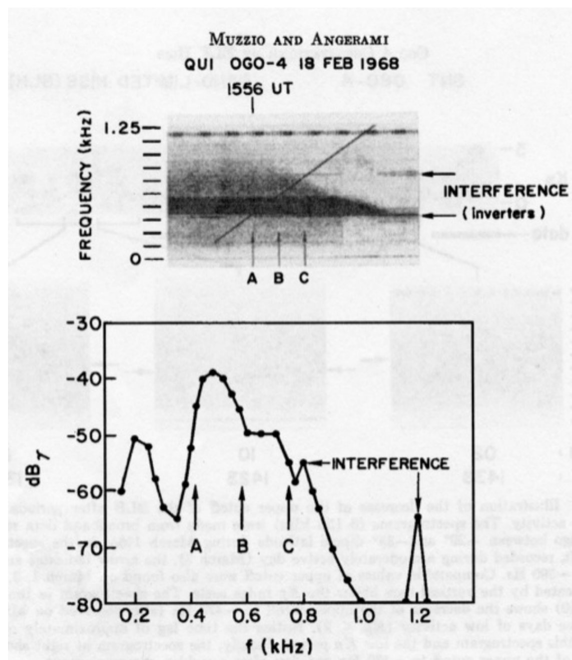


Figure 2.30: Measurement of hiss amplitude from the Band 1 sweeping receiver on Ogo 4. From *Muzzio and Angerami* (1972).

Figure 2.31, from the paper, shows a segment of the near-equatorial erosion and the amplitude data provided by an on board sweeping receiver. The track of the narrow band receiver through the low latitude data is shown as a slanting line in the upper part of the figure, while the lower plot shows a profile of the measured amplitude, Well defined is the intensity peak at -40 dBγ achieved in the middle of the BLH band, which the authors reported to be the strongest signal in the 0 to 3 kHz range up to $\sim 55°$.

The authors worked hard with ray tracing to explain their results (something to which we had become accustomed over time through the works of Walter and Angerami). They launched 600 Hz rays at L=3 at the equator, where the LHR was 750 Hz and the refractive index was therefore closed, By varying the initial wave normal angle through a wide range they found consistency with several of the observations in other papers on the location of hiss events and on Poynting flux directions, as well as their own records of equatorial erosion. Their calculated ray paths included a variety of conditions under which ray directions would be reversed to go from downward to upward or from outward to inward so as to cross the equatorial plane (this is the magnetospheric reflection phenomenon, originally predicted by Kimura and later discussed more fully by *Edgar* (1976), among others. Within a range of initial angles some rays could exit the plasmasphere, while those with initial wave normal angles between 85.5° and 89° could reach low altitudes near the equator but be split in arrival latitude so as to explain the equatorial erosion. The upper cutoff of the BLH at 600 Hz was interpreted as an issue of accessibility, one that would affect an equatorial hiss source at larger L, near 4,

The authors looked at both Cerenkov and Doppler Shifted Cyclotron radiation of noise from electrons as possible mechanisms of BLH generation, but concluded that some degree of amplification was needed in order to explain the observed power fluxes of the waves.

2.6.9 Rege Scarabucci and Nelson Dantas

As a historian of our VLF group I have enjoyed recalling the 1960s and the extraordinary accomplishments of a series of students from Brazil, each of whom worked with the OGO satellite data. They were Angerami, Walter, and Muzzio, and then Scarabucci and Dantas, Each made a substantial contribution to knowledge of VLF wave propagation in space.

The last two paid particular attention to results from OGO-4, a stabilized spacecraft operating in polar orbit at $\approx 700km$ altitude. Both, Scarabucci in 1969 and then Dantas in 1972, studied strong spatial changes in the VLF waves from ground sources that were detected in 1967-68 by the satellite as it moved at equatorial latitudes, one of the measurements being the amplitude of the 17.8 kHz signal from NAA in Cutler, Maine. However, they managed to interpret those spatial changes quite differently in terms of a physical cause. Scarabucci paid attention both to (a) daytime losses from up-going waves in the absorbing regions of the ionosphere as determined from full-wave calculations, and to (b) nighttime defocusing of VLF rays at OGO-4 altitudes, determined from ray tracing in a model of the topside ionosphere. Meanwhile, Dantas found that if he took account of a density anomaly in the equatorial ionosphere reported from topside sounders, the spatial changes in the VLF data could be explained by the effects on ray paths of caustics in space, regions from which rays of a given frequency could be excluded. We find this disagreement to be fascinating and yet disappointing, in that it has (until now) been largely lost to history; Scarabucci's work appeared both in his thesis (*Scarabucci*, 1969a) and in an article in JGR (*Scarabucci*, 1970), while Dantas' writing was limited to his thesis (*Dantas*, 1972). In the following pages we briefly describe the work of the two men, hoping our summary will lead to clarifying reviews by future students of VLF propagation.

2.6.10 The OGO-4 data

Both Scarabucci and Dantas studied OGO-4 data on the amplitude of the NAA signal as it was detected by a stepping receiver tuned to 17.8 kHz. Fig. 2.31, from *Scarabucci* (1969a), shows NAA on Jan. 29, 1968 on two successive northgoing nighttime passes in which the amplitude fell close to or below the equipment threshold of $-100dB_\gamma$ at $\approx -10°$ magnetic latitude. The next figure, from *Dantas* (1972), represents a nighttime pass on Sept 30, 1967 and indicates how sharp this cutoff could be. The NAA amplitude is dis-

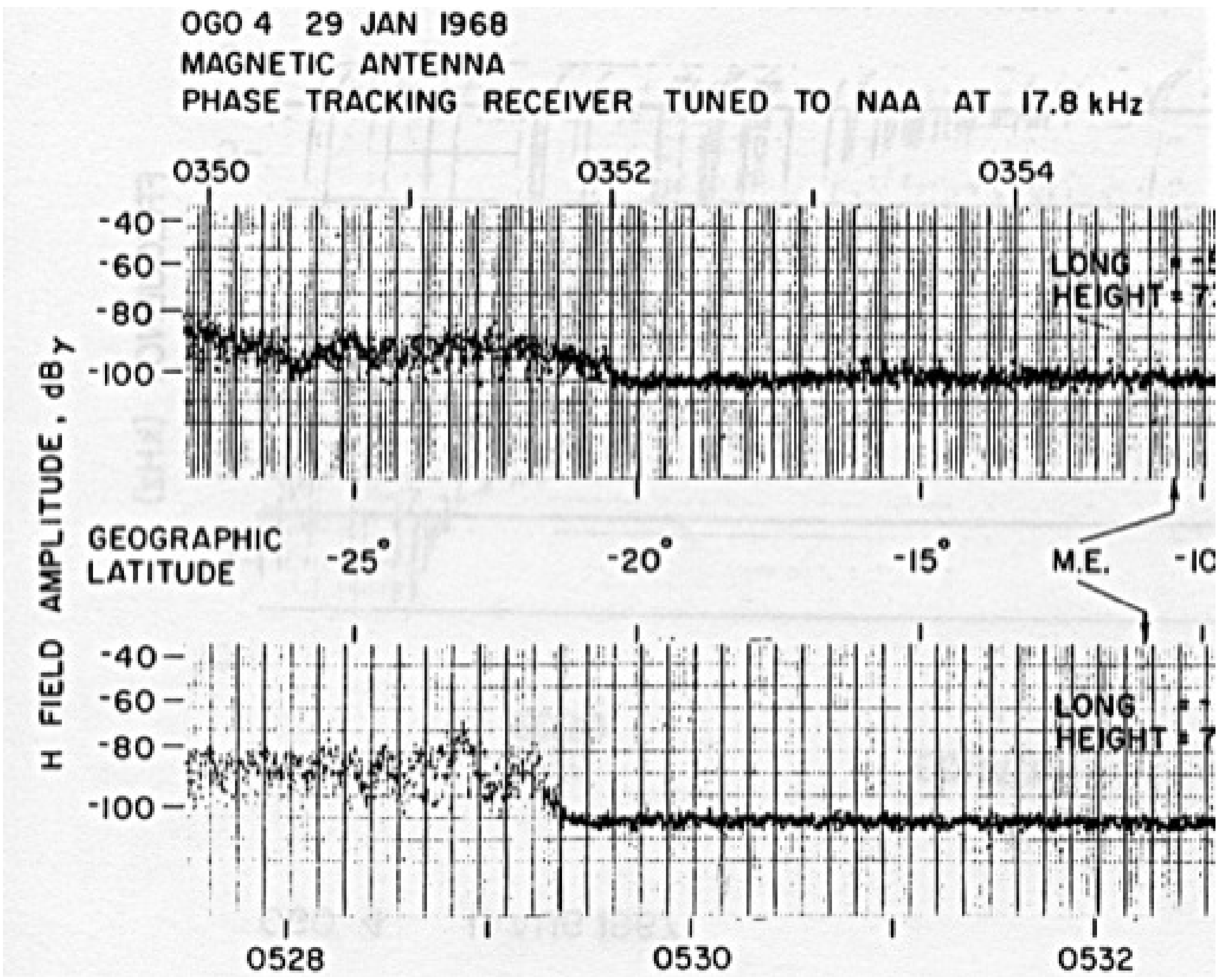


Figure 2.31: NAA amplitude versus geographic latitude on successive nighttime OGO-4 orbits as detected by a stepping receiver tuned to 17.8 kHz. From *Scarabucci* (1969a).

played by a VCO inserted in the 15 -20 kHz frequency range of the Stanford Rayspan Analyzer. The amplitude dropped by $\approx 20dB_\gamma$ within less than 1 s. Note that at the time of the cutoff, the separate dropouts of the two FSK NAA frequencies 17.85 and 17.8 kHz can be identified, implying a spatial frequency-dependent separation in the cutoff mechanism of $\approx 70m$ along the OGO-4 orbit.

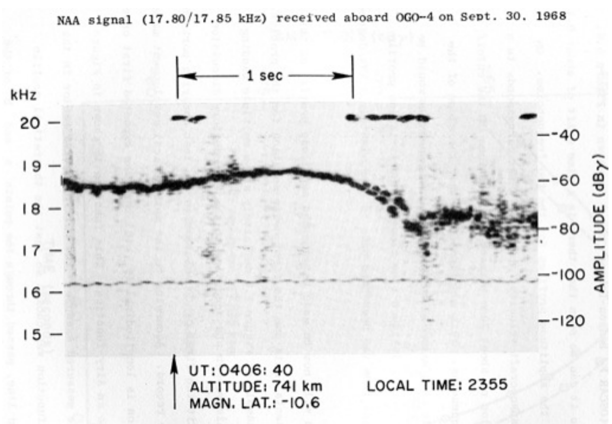


Figure 2.32: Expanded view of VCO displaying NAA amplitude at 17.8 kHz as OGO 4 reportedly encountered caustics at low southern latitudes. From *Dantas* (1972).

The changes in OGO-4 broadband VLF data displayed by Scarabucci included what was called “equatorial erosion”. An example from daytime is shown in Fig. 2.34 by a series of down-coming whistlers detected as OGO-4 approached the magnetic equator. They are sharply cut off at a rapidly decreasing frequency, essentially disappearing as the cutoff dropped below $\approx 1kHz$.

A similar broadband event, shown by *Dantas* (1972), occurred at nighttime and differs from that of Fig. 2.32 because of what were called “striations”, irregular bands of enhanced intensity that followed the sloping trend of the upper frequency cutoff. Also mentioned were enhancements of fixed frequency signals around the times when their respective frequencies were cut off. This occurred at $\approx 0406:42$ for the 17.8 kHz signal being displayed as a VCO output and at $\approx 0406:45$ for the $\approx 12kHz$ Omega signals

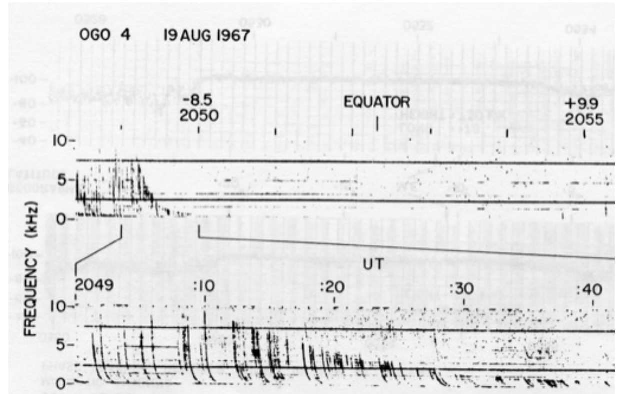


Figure 2.33: Daytime erosion event with down-coming whistlers exhibiting a decreasing upper cutoff frequency as OGO-4 approached the magnetic equator. From *Scarabucci* (1970).

near the top of the 0-12.5 kHz broadband spectrum.

During his tenure in the VLF group, Scarabucci did special work on full wave treatments of propagation in the lower ionosphere, producing or contributing to two special reports on that topic, (*Scarabucci*, 1969b) and (*Scarabucci and Smith*, 1970). In his thesis he attributed much of the daytime equatorial erosion illustrated in Fig. 2.34 to absorption in the lower ionosphere. Dantas agreed that such daytime losses were important, noting that they were in rough agreement with calculations that Helliwell had shown in his 1965 monograph on whistlers and would also be consistent with an upper frequency cutoff that fell sharply along a satellite orbit at low latitudes. However, Scarabucci had not discussed or explained the above-mentioned striation bands or the fixed frequency signal enhancements near the cutoff.

Both Dantas and Scarabucci used ray tracing as an analysis tool, it having been developed at Stanford some years earlier by *Kimura* (1966) and later modified by Walter [ref]. The differences in their ray tracing results were dramatic, however, because of the two-ion electron density models used. Both men paid attention to results from the Alouette topside sounders of the era, such as provided to Scarabucci by Larry Colin of NASA Ames Laboratory. Dan-

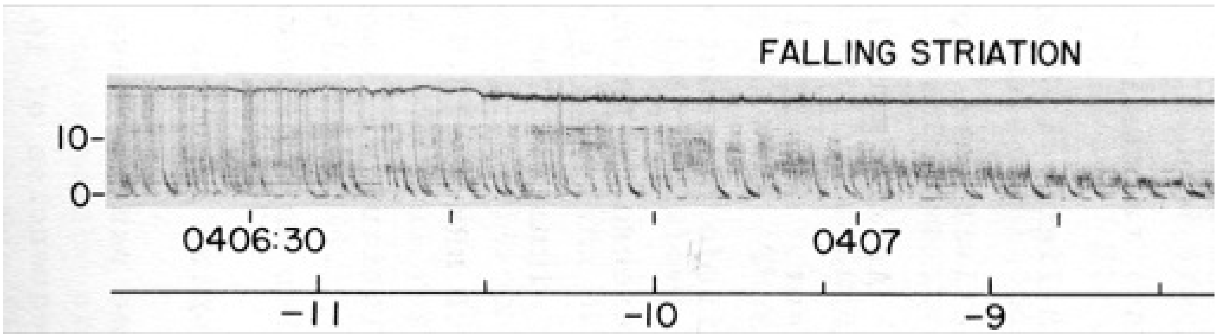


Figure 2.34: Nighttime example of "falling striations," bands at frequencies near an upper whistler cutoff frequency that decreased as OGO-4 approached the magnetic equator. From *Dantas* (1972).

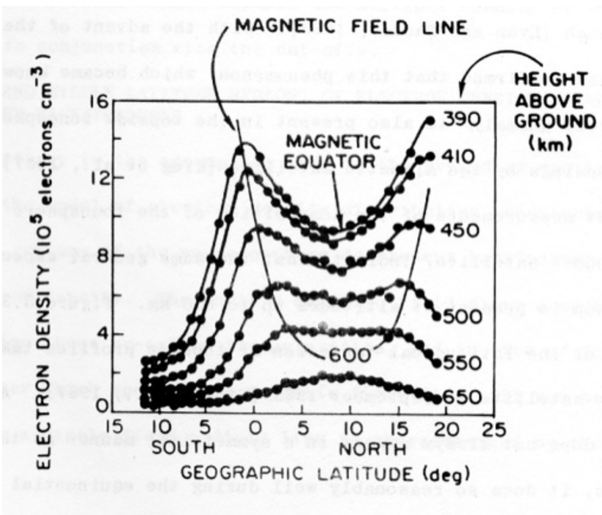


Figure 2.35: Eqautorial electron density anomaly used by Dantas in ray tracing. From *Dantas* (1972).

tas referred to a study of the equatorial density anomaly made by *King et al.* (1967) (Fig. 2.36), whose findings were later quoted by *Rishbeth and Garriot* (1969) in a treatise on the ionosphere that was beginning to be widely known. Fig. 2.37 shows Scarabucci's ray paths for 10 kHz based on a model ionosphere with no latitudinal variation, while Fig. 2.38 shows Dantas'results based on a model with an equatorial anomaly.

Scarabucci found the equatorial dropout in signals

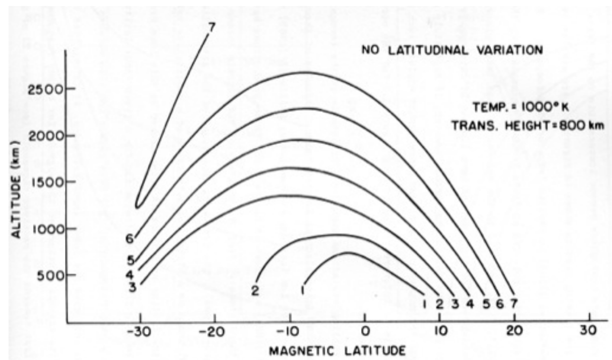


Figure 2.36: Ten kHz ray paths originating at a range of low latitudes. From *Scarabucci* (1969a).

such as illustrated in Fig. 2.34 to be a defocusing effect attributable to the drastic increase in scale height in the O+-H+ transition region near 800 km. Meanwhile, Dantas found the principal factor in signal behavior to be caustics, frequency dependent regions in space whose locations could be identified through analysis of focal points along signal rays. Both students did calculations that showed consistency with their observations. However, Dantas had a distinct advantage in terms of additional time to review the data and consider what had been learned about the equatorial ionosphere.

Overall, Dantas' arguments would seem to be the more persuasive. Like his predecessor he was aware

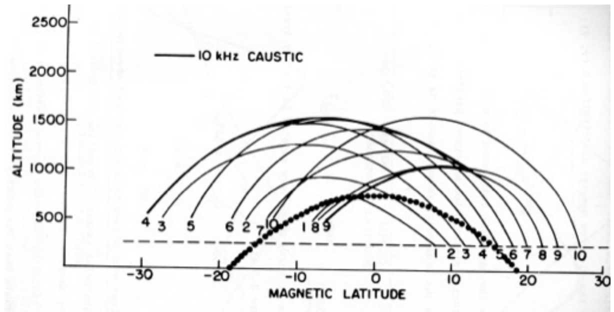


Figure 2.37: Ten kHz ray paths entering the ionosphere at a range of latitudes and remaining confined within the two heavy curves. From *Dantas* (1972).

of issues of absorption, which strongly affected the daytime OGO-4 activity. However, his special focus on intensity variations of signals in the vicinity of caustics and on the phenomenon of striations would seem to have given him superior insight into physical mechanisms.

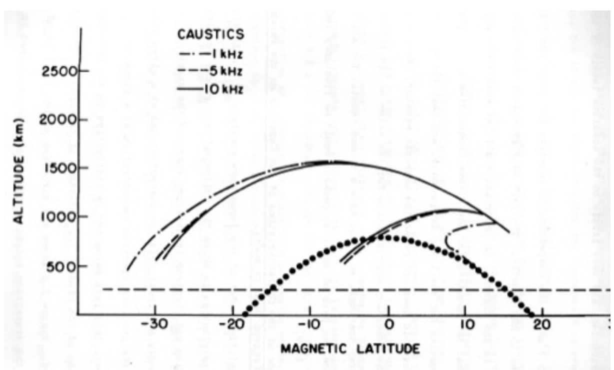


Figure 2.38: Calculated caustics for 10, 5, and 1 kHz without showing the distributions of defining ray paths. The dotted curve below represents the field line along which the density model of Fig. 2.36 was located. From *Dantas* (1972).

Dantas pointed out that as OGO-4 moved northward at southern latitudes, it first encountered rising striations near $\approx -30°$, followed by an intermediate region of broadband wave activity and then falling striations near $\approx -10°$. A figure shows how numbered 10 kHz rays that were up-going in the northern hemisphere at a range of latitudes would become down-coming in the southern hemisphere while also ranged between two conjoined caustics, shown as heavy curves. Another figure shows the calculated caustics (but not the rays) for 10, 5, and 1 kHz. The three, while close to one another (except in the north), were spatially separated, so that a north-going receiver at a fixed altitude would at first see a rapid rise in the topmost frequency of whistlers and at the end a rapid fall. In his calculations, Dantas found that rays should reach a peak in intensity at the cutoff, as he had found for the Omega signals and NAA.

As to the cause of the irregular striations, Dantas had little to say, other than to attribute them at least partly to the effects of electron density irregularities in the limited field line region in which the striations occurred. Both Scarabucci and Dantas considered how variations in the O+-H+ transition height could affect the results of their ray tracings.

Overall, the works of rach man were key parts of a pioneering,polar satellite program. We therefore regret that Dantas' thesis was never published, and are in hopes that our brief discussion here will lead to awareness (and appreciation) of his work, along with that of the other Brazilian students, by many of our readers.

2.6.11 Bill Burtis studies OGO 1 and 3 data on chorus

Bill Burtis worked in the late 1960s and early 1970s to become our group expert on the properties of VLF chorus as seen on OGOs 1 and 3. Chorus on OGO 1 had been reported by *Dunckel and Helliwell* (1969) but *Burtis and Helliwell* (1969) described its properties in some detail. (At the time of this report, Burtis worked at NASA Ames lab, but later took up full time graduate studies at Stanford.)

The spatial variations in chorus frequency suggested generation at the magnetic equator between 0.2 and 0.5 f_H and non-ducted propagation therefrom to the satellite. Fig. , from the *Burtis and Helliwell* (1969) report shows an elegant example of the variation of chorus along an OGO 1 orbit in 1966.

Plotted above the spectrogram are the approximaate variations of gyrofrequency at the satellite f_H and at the magnetic equator f_{Ho} along the field line passing through the satellite. A rough dependence on the equatorial value of f_H along the field line is clear.

As a regular grad student, Burtis continued to work with chorus, eventually focusing on the OGO 3 data for insight into wave growth and occurrence details (*Burtis and Helliwell*, 1975). His thesis was completed in 1974 (*Burtis*, 1974), and detailed papers on chorus properties were published in 1975 and 1976. Again there were extensions of observational work by others as well as new findings in 1975 (*Burtis and Helliwell*, 1975) that (1) growth rates were often exponential and in the range 200 to 2000 dB_s and (2) that the slope of rising chorus elements did not vary with emission intensity. This latter was considered supportive of the cyclotron resonance interaction theory previously advanced by *Helliwell* (1967)

The chorus paper in 1976 provided occurrence data from 400 hours of OGO 3 observtions in 1966-67 (*Burtis and Helliwell*, 1976). The most frequent detections were in the morning sector, from 0300 AM to 1500 PM. Occurrence was found to be moderate near the equator, weaker at about 15°. and maximum at higher latitudes, well down the field lines from presumed equatorial sources. The normalized frequency distribution of chorus within 10◦ of the equator showed two peaks, at $f_{f_H} \simeq 0.53$ and $f_{f_H} \simeq 0.34$, independently of L, LT, or Kp. The majority of the emissions rose in frequency at a rate between 0.2 and 2.0 kHz_s that increased with Kp and decreased with L.

2.6.12 Burtis' observations of an LHR noise band with the OGO 3 electric antenna

Burtis noted that when the OGO 3 antenna was connected through a matching transfomer to an electric sensor rather than a magnetic loop, there would often be clear measurements of VLF noise at the LHR (*Burtis*, 1973), a phenomenon familiar to us from Alouette data in the ionosphere (*Brice and Smith*, 1965).

Burtis dealt with two major variations in satellite measurements of the LHR frequency, one involving the geometric mean of the proton and electron gyrofrequencies and the other when the ion plasma density is lower than the gyrogrequency, as can happen at moderate to high latitudes poleward of the plasmapause. This was illustrated by a case in which the plasmapause was crossed at mid latitudes and the density profile beyond the PBL was measured (see Fig).

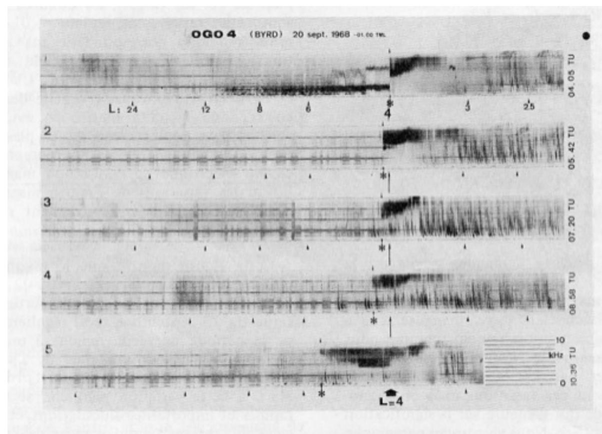


Figure 2.40: OGO 4 0-10kHz spectra from passes over Byrd on 20 September, 1968. From *Corcuff et al.* (1972).

2.6.13 Visitor Pierre Corcuff analyzes recovery in the plasmasphere boundary layer

Around 1970 we were visited by Pierre Corcuff, brother to our French collaboator Yvonne Corcuff. Pierre wanted to gather material for a research degree from the Universit of Poitiers. He succeeded in doing this by assembling a global model of the plasmasphere variations during a magnetically disturbed period in 1968. Working carefully with several data sets, including those from OGO 4 and 5 as well as ground stations Kerguelen and Byrd, he was able to presemt the best picture then available of the changing density profile in the plasmasphere boundary layer as

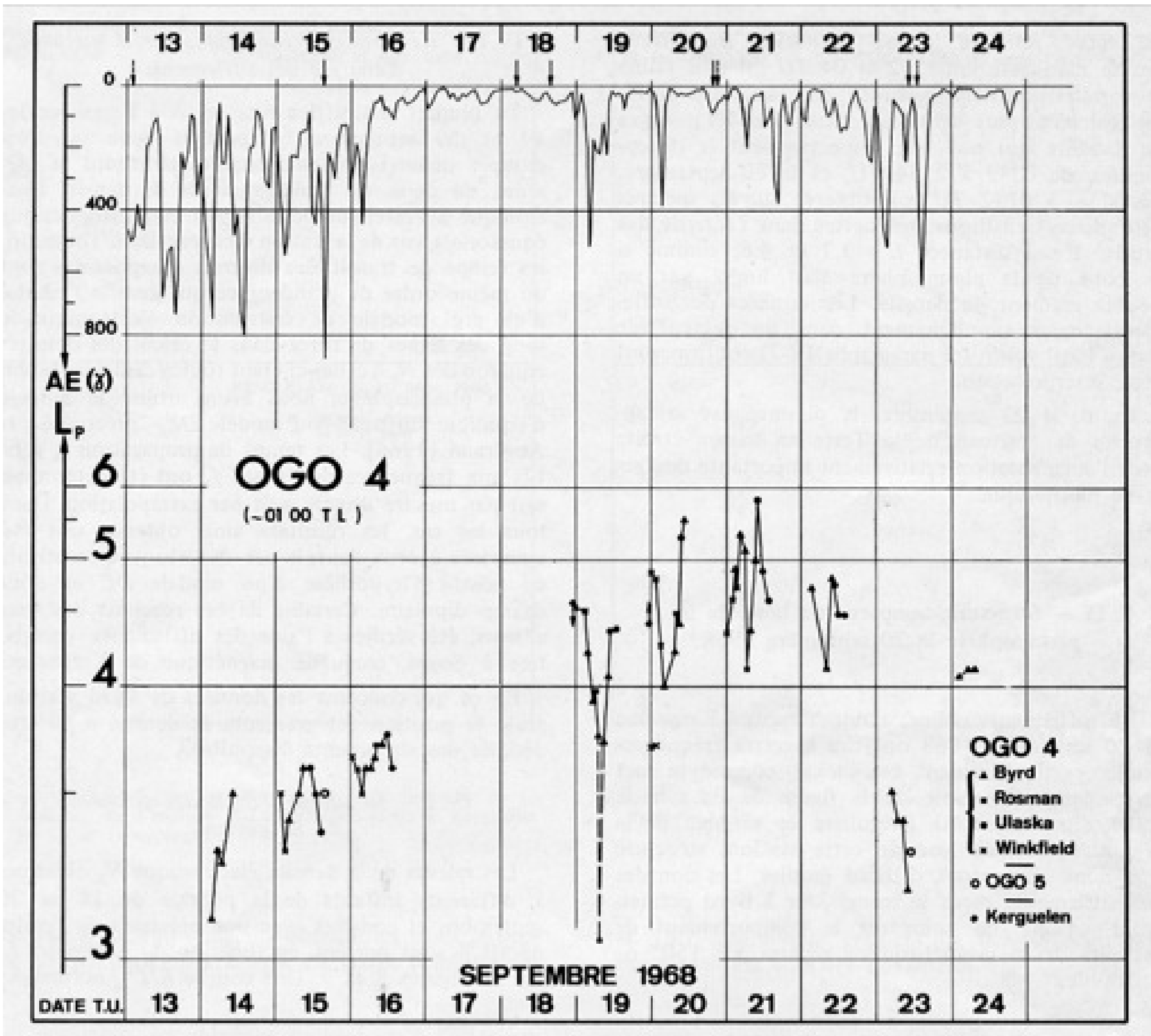


Figure 2.39: (above) AE index in September, 1968; (below) Plasmapause radii measurements. From *Corcuff et al.* (1972).

recovery from a storm proceded.

A partial view of this over a 12 day period in September 1968 is provided by Fig. 2.39, where the AE index (above) increases downward, and plasmapause radii measurements from the satellites (mostly OGO 4) and ground stations are below.

Another figure, no. 2.40, provided 0-10 kHz OGO 4 dynamic spectra from five successive passes on September 20 near Byrd station. Plasmapause crossings (asterisls) near L=4 were marked by noise bursts registered by the satellite's magnetic loop antenna. Note the cutoff or change in whistler activity at the boundary.

Pierre's observations extended and underscored our general belief that plasmasphere erosion was compratively short lived in comparison to the much slower recpvery process and that recovery was the most probable state of the plasmapause density profile. His work became a prized addition, and we used it with enthusiasm (see Fig. 2.41) in an article written by myself and Park in 1973 for the benefit of ionospheric workers who needed to know what had been learned about the overlying region.

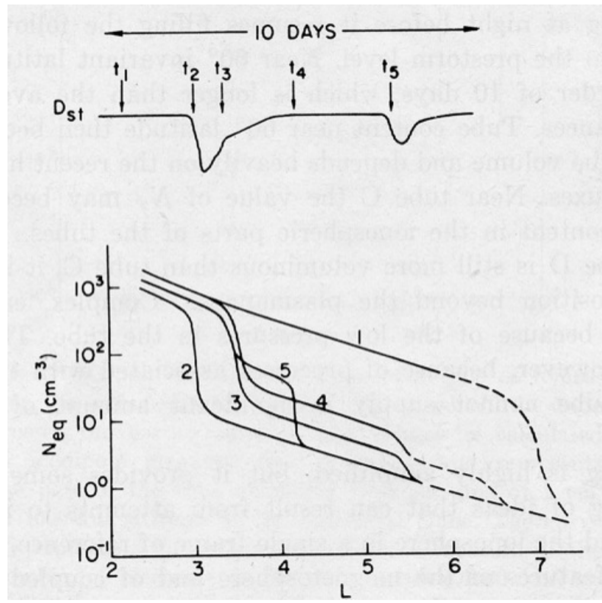


Figure 2.41: Model of density recovery in plasmapause region, based on work by *Corcuff et al.* (1972). From *Carpenter and Park* (1973).

2.6.14 Bruce Edgar uses the MR whistler to study VLF propagation in the presence of changing magnetospheric densiy structure

Bruce Edgar joined the VLF group in 1965 and tells of "being given the job of explaining the 'Anomalous nose whistlers' observed on OGO-1, which soon, in the light of Kimura's raytracing (Fig. 2.8), became known as Magnetospherically Reflected (MR) whistlers." Such whistlers, when recorded between L=2 and L=3 (see example from L≈2.4 in Fig. 2.39), exhbited nose frequencies far lower than the 15-30 kHz one would expect in ducted events propagating along field lines in that same region. By applying the available ray tracing program as most recently developed by *Walter and Angerami* (1969) and using a diffusive equilibrium model of a smooth magnetophere medium, Edgar was able to calculate spectral MR whistler forms similar to those of Fig. 2.39.

To better understand how this happens, recall the difference between the ducted whistler, wherein all the frequency commponents follow essentially the same field aligned path, and the non ducted whistler, in which the frequency components follow ray paths whose start points in the ionosphere and later trajectories are distributed in space. Equally important are the differences between ducted and non ducted whistlers in terms of variations along their ray paths in wave normal angle and ray direction with respect to the direction of earth's magnetic field. In contrast to the ducted whistler, the components of an MR whistler propagate for extended parts ot their paths with wave normals that are nearly transverse to the direction of B.

In a 1976 paper, Edgar presented two diagrams: the first one, Fig. 2.40, showing how MR raypaths to a chosen point at a single frequency vary with input latitude. There are three ray paths, each for a wave at frequency 1.5 kHz. As their starting latitude decreases, the ray paths increase in length, undergoing

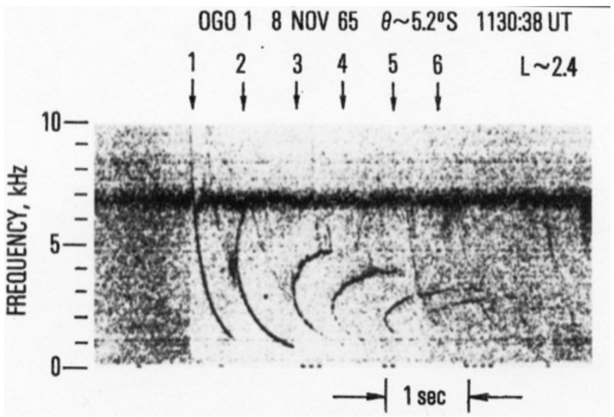


Figure 2.42: MR wistler received on OGO 1 near the magnetic equator in 1965. From *Edgar* (1976).

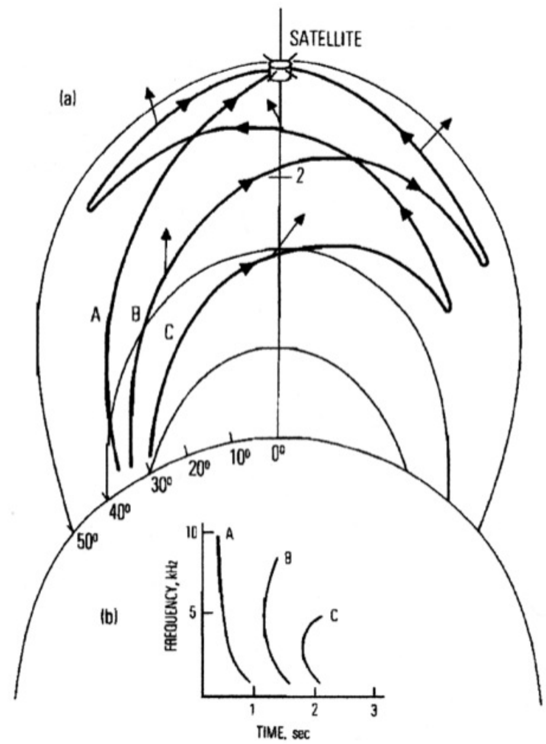


Figure 2.43: Ray oaths at 1.5kHz to the equator at L=2.4. From *Edgar* (1976).

an increasing number of magnetospheric reflections. The plot below of three MR components is for all the frequencies in each component (in addition to 1.5 kHz) that will reach the receiver. As the next figure shows, the frequencies above 1.5 kHz begin over a continuum of latitudes extending poleward from the start points of each of the three 1.5 kHz cases.

A second diagram, Fig. 2.41, looks at a single MR component and shows how the ray paths of three of the frequencies in that component vary in starting latitude and trajectory. A key point for the component shown is the development of a nose frequency, the result of a transition from the lower frequency part of the component, where the group ray refractive index varies in 'Eckersley fashion' as $f^{-1/2}$, to an upper frequency part that is dominated by increased path length and magnetospheric reflections that occur as the various upper frequencies fall below the local lower hybrid resonance frequency.

If in summary one asks why the MR whistler is divided into discrete components, the answer is that for any frequency received at a given point in space, only a discrete number of ray paths can be traced back to starting points in the upper ionosphere under conditions of vertical wave normal.

Bruce became an expert on the occurrence and morphology of the MR spectra, reaching a point where he could tell you the recent history of storm conditions in space just by looking at the OGO-1 or 3 spectrograms. "In a quiet period before a magnetic storm, MR whistlers could be observed from L $\approx$ 1.8 out to L>3. The spectra would exhibit very wide bandwidths with very few small scale "wiggles." Immediately after a storm sudden commencement, MR observations would cease above L$\approx$ 2. Then on passes several days after a storm commencement, they would extend out to L=2.5. The whistler traces typically exhibited severe bandwidth limitations with extra traces and odd wiggles not predicted by the earlier ray tracing calculations. Passes occurring after the magnetosphere had fully recovered usually exhibited MR whistlers from L=1.8 to beyond L=3 and with very few spectral irregularities."

In his early ray tracings, Bruce found that observed MR components often exhibited cutoffs, not extend-

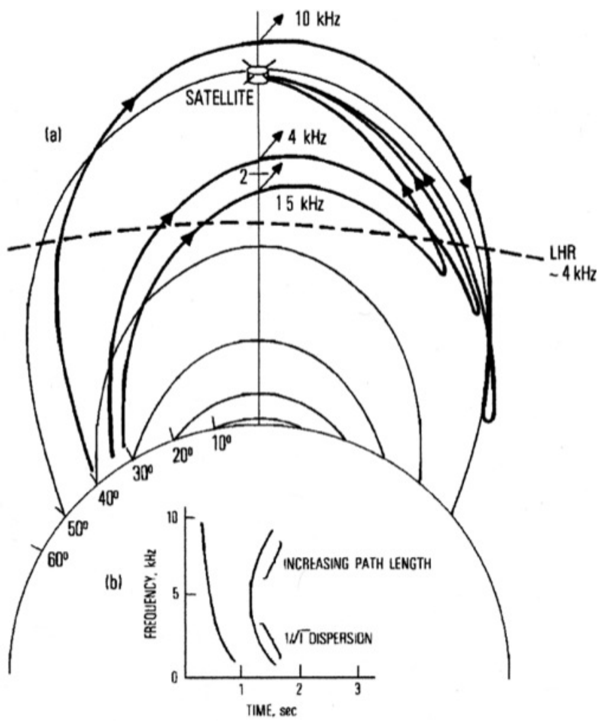


Figure 2.44: Ray oaths at three frequencies to the equator at L=2.4. From *Edgar* (1976).

ing as high or low in frequency as the ray tracings in a smooth magnetosphere predicted (*Edgar*, 1976). It was noticed that "a typical MR whistler trace was produced by raypaths that started over a wide section of initial latitudes. I found that by restricting the starting latitudes of the raypaths, I could reproduce the observed upper and lower frequency cutoffs." But what propagation mechanisms could explain the starting latitude restriction?

Answers came thanks in part to Angerami, who "had added a field aligned density irregularity feature to the ray tracing program. It used Gaussian shapes that could describe peaks, troughs, step ups and step downs in the density profile." On Fig. 2.42, for example, are shown some predictions and observed examples of the effects of a strong (30%) duct at L=2 on MR waves that originate near 33°. Bruce further found that "a sharp density dropoff of more than 50% could trap non ducted raypaths (along the field line of the trap), rays that had begun over a range of starting latitudes. A series of dropoffs from L=1.8 to L=3 could trap non ducted ray paths and prevent any observation of MR whistlers by a satellite receiver."

Fortunately, examples of such ionospheric profiles were appearing in the OGO 4 H+ data of Harry Taylor (see Fig. 2.15). Bruce adds that "for dropoffs of 30%, partial trapping of the non ducted rays could occur which explained the double MR traces observed on many passes. For large field aligned density peaks or "ducts", the predicted MR spectrograms reproduced many of the wiggles observed in the data."

2.7 The puzzling Hissler phenomenon

2.7.1 A study by visitor Inge-Marie Ungstrup

Auroral Hiss, diffuse broadband noise at frequencies extending down into the kHz range, had been widely detected by our group and by others who recorded VLF on satellites and ground stations at mid to high latitudes. We did not give our main attention to the origin and occurrence of high latitude hiss, except in what might be considered special cases, such as Nick Dunckel;'s work with data from OGOs 1 and 3 and the investigations of T. B. Jorgensen of Denmark, who visited us in the 1960s.

One special case was the "hissler", which appeared as a succession of falling tones either embedded or partially embedded in a relatively unstructured background hiss. Hisslers were seen regularly on records from the loop antenna systems of OGOs 2 and 4 and were also detected at high latitude ground stations such as Byrd and Vostok, as illustrated in Fig. 2.43. These events were detected in 1968 at Byrd Station (above) and in 1966 at Vostok Station, on the southern polar cap. At Vostok the events occurred during a period of deep global quieting, lasted for many minutes and consisted primarily of non-overlapping falling tones with time varying extent in frequency. Although in some respects whistler-like in $f - vs - t$ form, their dispersion properties were

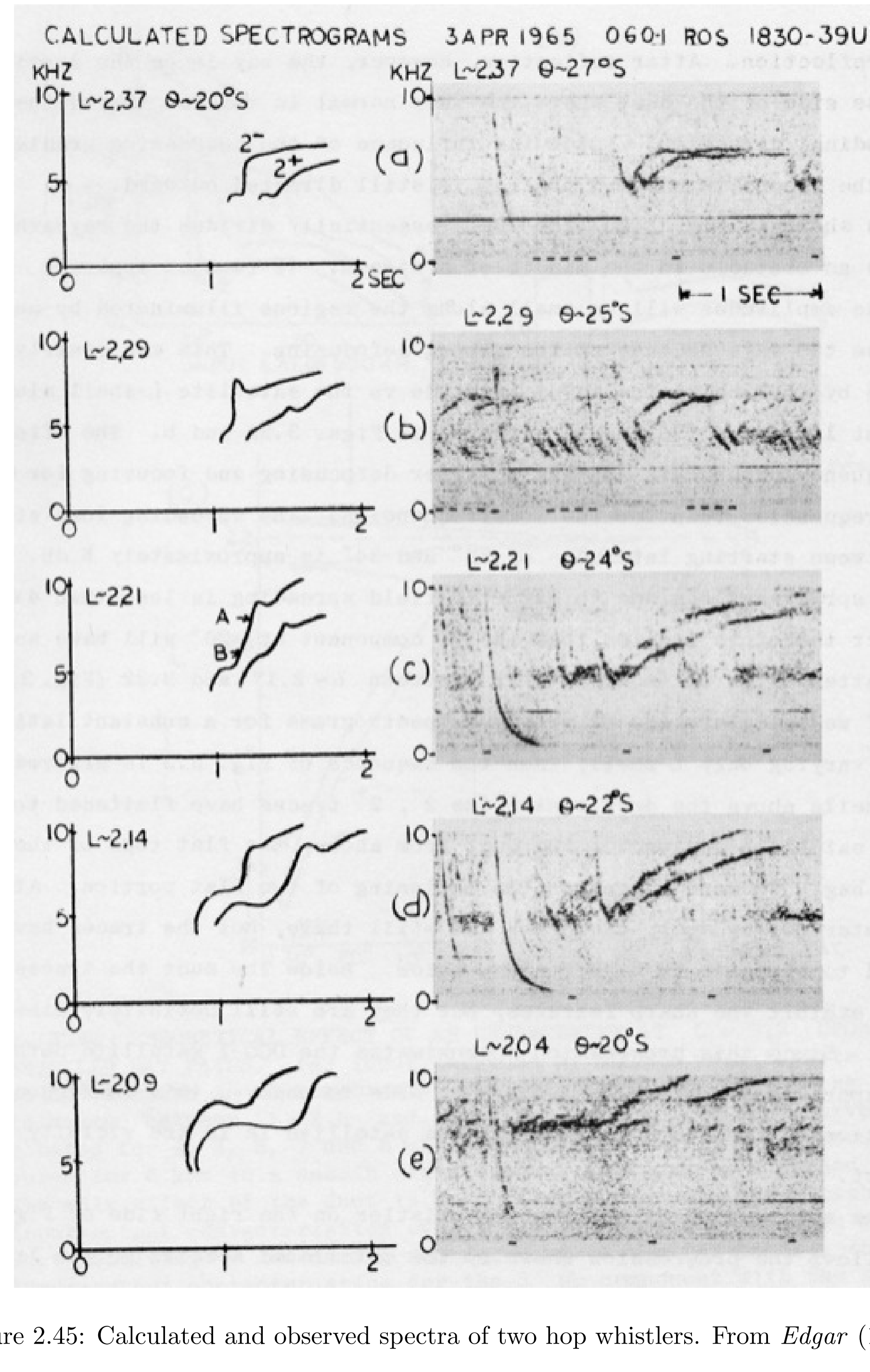


Figure 2.45: Calculated and observed spectra of two hop whistlers. From *Edgar* (1972).

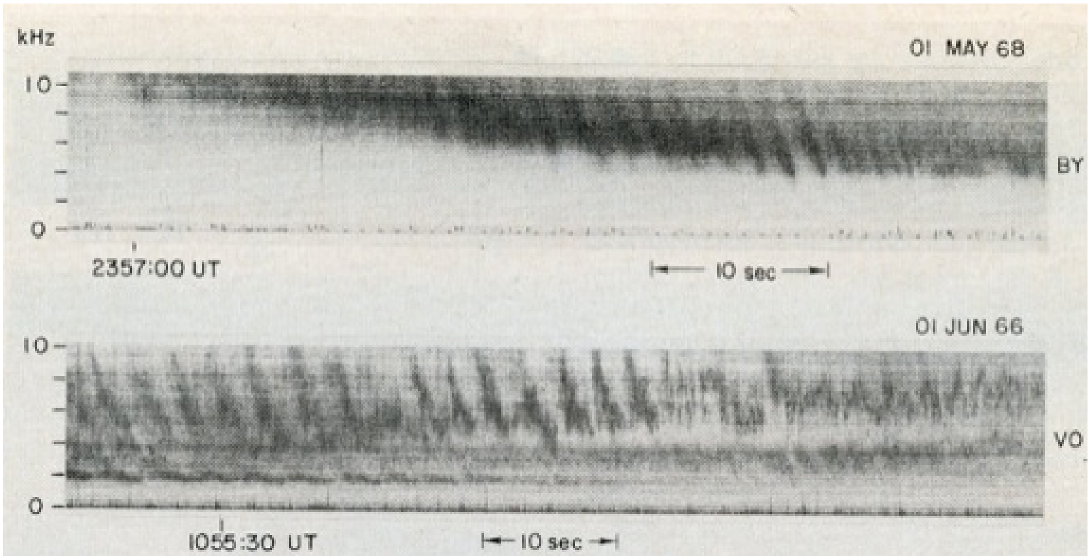


Figure 2.46: Spectrograms of hissler activity in 1968 at Byrd Station and in 1966 at Vostok Station from *Ungstrup and Carpenter* (1974).

clearly not consistent with repeated propagation between hemispheres along field line paths that reached high latitudes.

A review of the many OGO 2 and 4 records and those from ground stations was published by *Ungstrup and Carpenter* (1974) following a working visit to Stanford in 1972 (at times under the harrowing conditions imposed by student protests against the Vietnam War). Occurrence of hisslers was found over a wide range of latitudes poleward of $\simeq 58°$, with some indications of a concentration near 75°. In any single observation, the activity was interpreted as originating in a localized ionospheric region.

The hissler phenomenon has not, to our knowledge, been widely studied in recent years and may well be an unexploited source of information on the physics of whistler mode propagation and particle precipitation at mid to high latitudes. Hisslers tend to occur during substorm periods and at preferential high latitude locations. Pehaps they propagate with wave normal angles such that they are basically unducted and thus unlikely to be seen at both satellite altitudes and on the ground.

2.7.2 Jan Siren and the "fast hissler"

In looking at OGO 2 and 4 records from near Byrd Station, grad student Jan Siren found a type of event that he called a "fast hissler" and interpreted as the dispersed form of a noise burst occurring along an auroral field line terminating near the ground station (*Siren*, 1974). In such cases there was not the multiplicity of falling tones shown in hissler events such as those of Fig. 2.43, Jan found it possible to deduce the altitude of the noise burst occurrence from measurement of the dispersive effects of propagation from burst origin to the lower ionosphere, inferring values ranging from 10,000 to 30,000 km (*Siren*, 1972).

2.8 Nick Dunckel

2.8.1 Early activities in the VLF group

Nick began work for Helliwell in 1961, a few years before beginning his own graduate research on the OGO-1 satellite data.

He learned to program the large Burroughs central computer in the "Balgol" (Burroughs Algol) language to generate the ionospheric absorption expected for a typical electron density distribution in the ionosphere. " (Each line of instruction required punching an IBM card on a "card punch"; programmers would carry around a box of cards containing their programs. The great fear was that of dropping them on the ground, which happened occasionally.)"

Nick was helping with Bob Helliwell's now classic monograph. His contributions "included applying non-parametric statistics to the occurrence of vlf phenomena and writing the index." He was pleased when Helliwell pointed out a favorable comment on the index by one reviewer.

"An editor from the Stanford University Press carefully edited the book. This was the best training in writing I ever had! I could see what I had carefully reviewed and in some cases written then being improved by a professional editor."

"People in the group were generally very kind to me." Nick mentions being included as a coauthor on a paper of mine on whistlers, yet claims to have been "minimally involved" in the research. In fact he was a helpful coauthor of *two* papers dealing with new, non-ducted whistler phenomena seen for the first time on the Alouette satellite records (*Carpenter and Dunckel*, 1965), (*Carpenter et al.*, 1964).

Nick mentions "another time when I was struggling (again) with Maxwell's equations, I asked the group's expert on wave theory..Bob Smith if he really thought he understood Maxwell's equations. I felt much better when he modestly said that he didn't, even though he understood them far better than I."

The weekly RadioScience Seminars were held over a period of many years. One visiting speaker was concerned that a future moon lander might sink into a layer of soft dust covering the moon. As Nick says, "fortunately for our space program, the moon's surface turned out to be more firm than his (the speaker's) studies indicated."

2.8.2 A focus on OGO-1 and VLF noise events originating in the heliosphere

Nick got to attend a development meeting at TRW with representatives from all 20 experiments on OGO-1. "It was an eye-opening experience to realize that despite the obvious ability of the project manager, the satellite and its 20 experiments were too complicated for any one person to completely understand, which led to issues of physical and electrical interference between the experiments."

Nick also writes about being in the control room at Goddard Space Flight Center during the launch of OGO-1 and witnessing "first hand the fear of the NASA employees that the expensive spacecraft might be lost because it failed to stabilize. Fortunately the spacecraft was able to function despite its rotation; our vlf experiment on board worked well."

In the OGO-1 program, Nick concentrated on the spectral data and "saw two new types of phenomena. A strong wideband burst-like signal with components up to 100 kHz was dubbed "high-pass noise." Another was a weaker band-limited noise that descended slowly from 100 kHz to around 30 kHz." NIck came to realize that "this latter signal was the low-frequency component of a "Type III" solar radio burst previously observed at higher frequencies and associated with particles from a disturbance on the sun sweeping outwards along the spiral magnetic field lines in the solar wind and generating signals at one or two times the local plasma frequency."

"We published initial papers on both of these phennomena. The Type III burst data and the environment between the sun and the earth were refreshingly different from what I had been working on, so I wanted to focus on that data for my thesis. Professor Helliwell preferred that I focus on the highpass noise. I didn't recognize at the time that his preference probably also involved issues of funding, laboratory focus, and personal experience. I argued for focusing on Type III bursts. Eventually, Professor Helliwell kindly agreed."

I learned of a modest-sized (i.e. it fit in a small room) computer on campus that could process the digital tapes containing the spectral data sent to us by NASA. I programmed that computer to process the data, to display it, and to control a 16 mm camera that would photograph it to produce "ampligrams" that provided both the benefits of intensity vs frequency and time and true amplitude. (It is of historical interest that the memory in this computer amounted to only 16K words, about 100,000 times smaller than the memories of today's personal computers)."

In time NASA reduced its tracking of the data from the OGO satellites. We requested and received funding to use the "Big Dish" to track OGO-1 (or OGO-3?). "NASA provided a printed ephemeris that I transcribed onto punched cards and then onto paper tape to feed into the PDP computer that drove the antenna in azimuth and elevation. It was exciting to rotate with the huge antenna...." We also used small antennas to track the low-orbit OGO-2 and OGO-4 satellites.

"After receiving the PhD, I began a career in the defense industry that lasted 25+ years. TRW, the company I worked for, had a program to provide stipends for graduate students, and after realizing I was in a position to influence the selection, I managed to steer a small stipend toward the VLF group at Stanford. It was a pleasure to interact once more with the people I knew there."

2.9 Cold plasma diagnostics in space using signals from the Siple transmitter

2.9.1 Trapping of injected waves by the plasmapause

In 1977 there were several publications involving satellite reception of injected Siple signals near the plasmapause. The calculations in one paper involved the trapping and guiding of injected whistler mode waves by the density gradients present along a plasmapause surface (*Inan and Bell*, 1977b). It was found that the signals trapped in this way can be received in the conjugate region much as are regular ducted whistlers. Input waves from ground sources trapped in this mode could come from a latitude range of $\simeq 4°$ and involve frequencies 1-4 kHz. Under some condtiions there could be focusing of the wave energy by 3 dB at the equatorial plane, but also poleward departures of downcoming signals by several hundred km from the field lines of the plasmapause surface.

2.9.2 Siple signals received on Explorer 45 and Imp 6

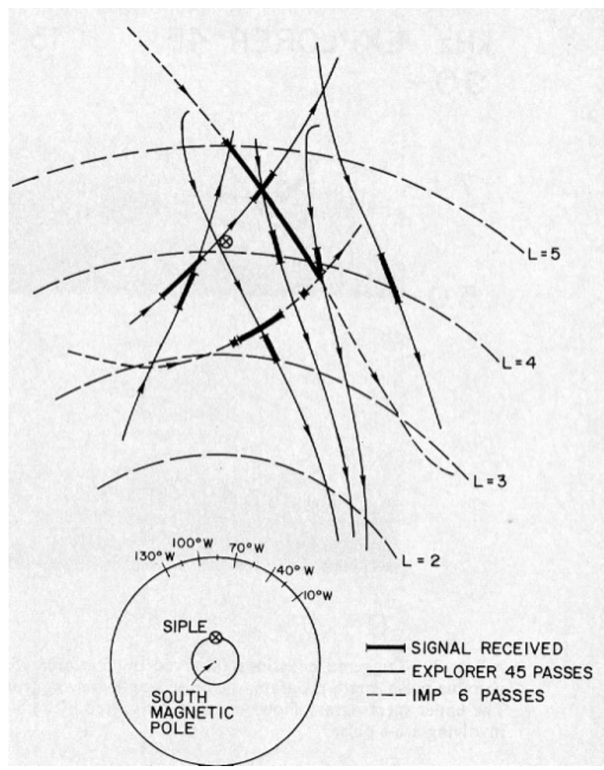


Figure 2.47: Equatorial projections of EX 45 and Imp 6 orbits in 1973 and 1974 when SI signals were received. From *Inan et al.* (1977).

In 1973 and 1974 there was a three month campaign to observe Siple signals on EX 35 and Imp 6. EX 45, also mentioned in section 4 of this work, was essentially in equatorial orbit with apogee near L=5. At the time of this work Imp 6 was crossing the equator near L=5 at an angle of 25°. On a total of 25 passes there were 10 periods of reception on one or another of the spacecraft. Fig. shows a plot of the locations where signals were detected within a longitude range from 20° East to 15° W of the SI-RO field lines. Emissions stimulated by the transmitter were seen on only 3 of the 10 passes

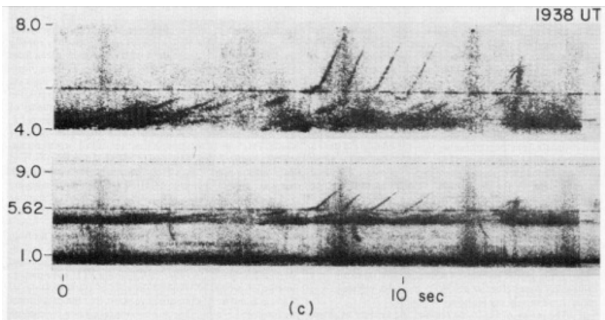


Figure 2.48: SI signal triggering on Imp 6 using two frequency scales. From *Inan et al.* (1977).

An example of the apparent triggering of emissions, or possibly their entrainment, as observed on Imp 6 as it moved between L=4 and L=5, is shown in Fig. . This case suggested the occcurrence of some generation or entrainment process well away from the equatorial region. Amplitude measurements near the equator seemed consistent with the multi-milligamma values discussed in earlier work by Helliwell rather than the tens of milligamma levels discussed by Nonn.

2.9.3 Siple signal propagation to IMP 6 near the plasmapause

In another related paper Inan and colleagues discussed the use of an iterative ray tracing method to infor the high altitude magnetospheric densiy profile (*Inan and Bell*, 1977a). An initial density model was assumed, and then modified according to SI signal travel time results and corrected ray tracings that better accorded with those results. The 1973 test case used involved continuous reception of Siple signals on Imp 6 as it moved through a plasmapause boundary layer. The diagnostic method was found to be in good general agreement with a ground whistler probing of

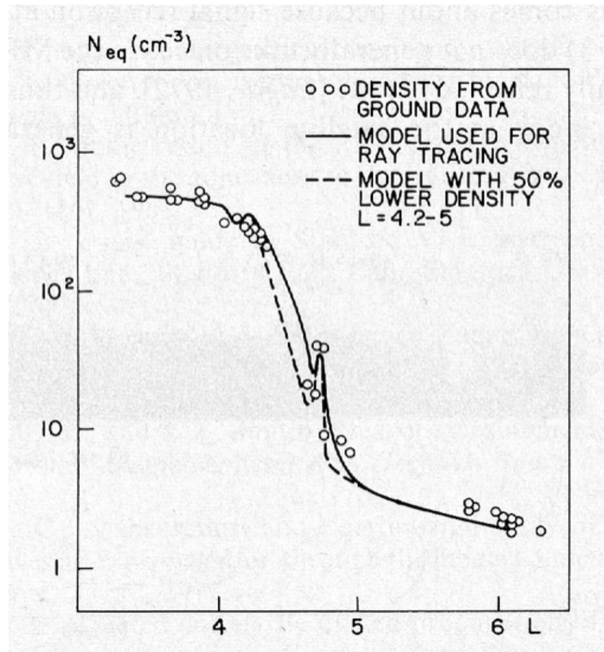


Figure 2.49: Comparison of an equaatorial density profile derived from SI transmissions to Imp 6 with a corresponding profile from ground observed whistlers. From *Inan and Bell* (1977a).

the same region, given allowance for the non ducted nature of the SI ray paths to the satellite as opposed to the guided nature of the whistler observations.

Part 3

Field Operations

Figure 3.1: John Katsufrakis, circa 1960

3.1 Introduction

3.1.1 John Katsufrakis, supervisor of Antarctic field programs

I met John in 1956 when we shared a freshman calculaus course at Stanford. We were both years older than the other people in the class and knew less calculaus than they did, but over time we managed well. In late 1956 I was working as a part time data aide in the FLF grooup and before long was authorized to recruit John to join us.

John soon demonsrated a flair for communicating with our government sponsors, among them Arnold Shostak of the Navy Research Lab and Ray Heer of the National Science Foundation. Then in the early 1960s I remember him nervously preparing for a first field trip to South America. It must have gone well, because in the mid 1960s he became supervisor of our growing Antarctic Field activities. He turned out to be good at picking people who could deal well with the isolation and limited social dimensions of winter-over life.

There is much more to be said about John, especially in the context of Antarctic Field operations, where he was the unquestioned leader. These additional words as well as related photos are persented at various points in the following section on field operations.

3.1.2 A program of VLF fixed frequency measurements along the west coast, remembered by George Carpenter

"In the early 1960s Helliwell decided that it would be a good idea to establish a mobile capability to measure the temporal and spatial character of whistlers and narrowband VLF signals. A Navy communications truck, shown in fig. 3.1, was obtained and instrumented to perform these measurements. The truck was dispatched north in the late summer 1962 with a crew of three - Brad Helliwell, Charlie Weigle, and myself. The first stop was in the vicinity of Ashland, Oregon. The purpose of this stop was to find a very remote location away from power lines and record whistlers and other VLF signals in a very-low-noise environment for a week or so.

The NPG US Navy transmitter at Oso, WA was the center of attention during the second phase of measurements. The truck was first deployed about 100 km south of the transmitter where the signal was dominated by the ground wave. Measurements were carried out for 24 hours. The truck was deployed about 25 km further south every 24 hours for about two weeks. This schedule was ideal for the crew as it allowed us to attend the World fair in Seattle while measurements continued unattended.

The need for an even lower noise level than achieved at Ashland apparently led to us being instructed to deploy to Great Falls, MT for further whistler measurements. We found a site far from power lines and carried out measurements for a week. I do not know what happened to all the data collected during this month long trip. Even after I left Stanford I continued to monitor the chart recording of VLF transmitters at the new field site building of Page Mill Road west of Highway 280. At least one paper was published containing specific results of these measurements (*Kimura*, 1968)."

Figure 3.2: Radio truck stationed in the State of Washington. Brad Helliwell and Charley Weigle stand nearby.

3.1.3 A group picnic in 1961

In the late summer of 1961 there was a highly successful picnic, as illustrated in Figs. 3.2 and 3.3. There was a keg of beer and a touch football game that is remembered even today for the bruises acquired when one got in the path of George Carpenter. Illustrated in the picnic figures, among others, are Lyn Martin of New Zealand, our grooup's first in a series of science visitors. Also shown is Patsy Flanagan, who served as Bob Helliwell's secretary at the time.

3.2 Plateau Station, a Reminiscence by Rob Flint

Like many first time Antarcticans, when I boarded the C-130 for Christchurch on Valentines Day in 1965, I assumed that I had seen the last of the continent, and it was time to get on with "real" life. I had been at Byrd Station for fifteen months, maintaining equipment, collecting data, and building antennas for Stanford's VLF (Very Low Frequency) program. It was a good experience: it was my first job after grad school, a relatively harmonious winter, and the third winter group at comfortable and spacious New Byrd. I gained a lot of practical experience and new friends. After a "decompression" tour of New Zealand, Australia, Singapore, Thailand, and Japan, I returned to Stanford to debrief. Almost immediately, my boss, John Katsufrakis of Helliwell's VLF group at Stanford put me to work on helping to design equipment for a proposed new Antarctic Station to be known as Polar Plateau Station.

This station, I learned, was to be air transportable so that it could be relocated after a few years. It was therefore necessarily very small - it would accommodate just four scientific personnel and four support from the US Navy. The first location of the station was to be on the high polar plateau of east Antarctica. Being slightly higher than Vostok, this location was expected to have the coldest temperatures in the world. Stanford would oversee the upper atmosphere geophysical programs. Other programs were to be meteorology - especially micrometeorology -, aurora, and geomagnetism. John asked me to be a consultant for the design of this new station from the perspective of one who had wintered in Antarctica. So I was sent to ATCO (Alberta Trailer Company) in Calgary to discuss the design of the new station. I am not sure if I contributed much, but I did persuade them to make all the bunks extra-length - and I heard later that my shorter winter-over colleagues would have preferred to have more closet space instead! I still had not committed to wintering at this new station, and my diary does not reflect any specific decision date, but after a couple of months of working on equipment for the station and design of the station, I came to realize that I would like to see how this station would really work, and agreed to return to Antarctica as the first Station Scientific Leader of Polar Plateau Station.

So the summer and fall of 1965 were filled with all the preparations for another year on the ice: equipment design, construction, and testing, physicals, seeing lots of friends to make up for lost time, training on equipment at NBS Boulder, the Skyland orientation meeting, another trip to Calgary to see a test assembly of the station modules, and a lot of dating to compensate for another celibate year to come.

The plan was that the station would be constructed during the warmest part of the austral summer. Therefore the initial fly-in was scheduled for mid December. There were conferences and planning sessions at McMurdo, and more at South Pole where those of us on the initial flight were sent to acclimate for six days. On December 13, 1965 we took off for for where? ..for a somewhat indeterminate point on the high plateau of East Antarctica. The intention was to find the high point along longitude 40° E. Since the winds on the high plateau are largely katabatic or downhill-flowing, this point should be a point of minimum wind. The C-130 seemed to take forever to get off the runway at Pole, even with eight JATO (Jet - Assisted Take Off rockets). But finally we were airborne for three hours over the completely featureless plateau. The initial party consisted of Lt. Jim (Doc) Gowan, who was to be the first winter-over Navy leader, Charlie Roberts - a meteorologist who had extensive Antarctic experience, Ed - a radioman, Art Weber - the Navy architect who had designed the station, a photographer/reporter, and myself as Station Scientific Leader. There was a mail

(a) At the keg, from left Charley Weigle, John Katsufrakis, Peter Katsufrakis, and Lloyd Provan.

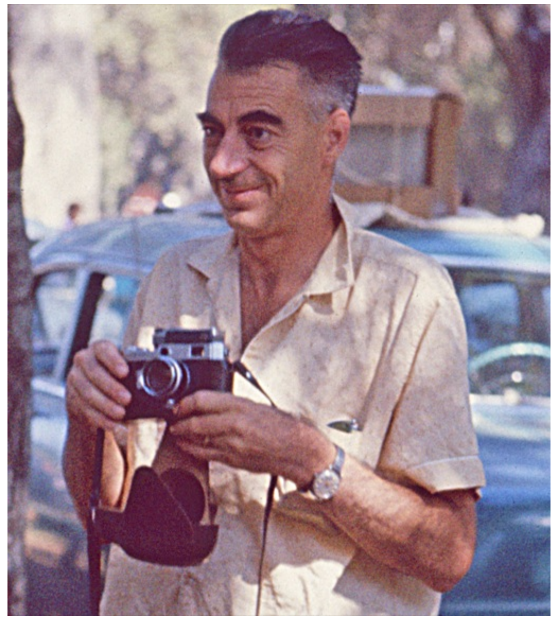
(b) At the table, George Carpenter, Jerry Yarbrough, and Charley Weigle, with others.

(c) Charley Weigle.

(d) Bob Mlodnosky.

Figure 3.3: The 1961 VLF Group Picnic.

(a) John Katsufrakis at the picnic.



(b) Lyn Martin, science visitor from New Zealand.

(c) Lloyd Provan, expounding.

(d) Helen Katsufrakis and Patsy Flanagan.

Figure 3.4: More of the 1961 picnic.

bag for Plateau on the plane, and Jim and I decided to have a look. The loadmaster saw us and started to chew us out: he said that he had to deliver this mail bag TO THE STATION. We explained that if there was any station, it was probably embodied in the two of us! (Included in the mail was a box of slides for me, which I had mailed for processing from Byrd Station the previous year, had been lost in the mail, and somehow made its way back to the Ice for the Plateau flight!)

At about $79°S$, the radar altimeter indicated that the elevation was beginning to decrease; so we flew a grid pattern to try to find the local high point in the area. There was very little elevation change, but we finally agreed that we had done the best we could to find the local elevation maximum. We descended to a few feet above the snow, then made a “feel”” of the snow, dragging the skis - ready to add power and take off again, and finally settled down for a landing in the soft snow. The doors were opened, and there we were, at the site of Plateau Station - $79°15'S$, $40°30'E$.

Figure 3.5: The first plane into what became Plateau Station

The first impression was the soft snow - it was unlike any snow that I had seen at Byrd or Plateau Stations - it was almost powdery. A second and surprising impression was the warmth: it was -15 degrees Fahrenheit, making it warmer than Pole when we had left. We had timed the landing to be at the warmest part of the day - about 1 pm local time. It was windless and sunny, and shortly we were stripping off parkas as we unloaded the plane. Another first impression was the utter lack of features: the only disturbance of the snow surface or horizon in any direction were our tracks where the plane had landed. The Navy quickly planted the flag and had the photographer take pictures of the plane and the Lieutenant and a representative of the admiral. Meanwhile the radioman was trying to set up the portable radio and make contact with South Pole. Charlie Roberts had his thermometers and barometer out to measure temperature and attempt to estimate elevation. The elevation was later established as 3625m (11,894 feet) above sea level. The plane crew were attaching fresh JATO rockets to the side of the plane. The rest of us were unloading fragile equipment so that the rest of the cargo could be dumped out the back of the plane. It took about two trips from my pile of belongings to the plane to remind myself that we were at high altitude, and I needed to move more slowly. We quickly erected a couple of Scott tents to serve as emergency bivouacs in case anyone came down with severe altitude problems. Then we went to work on our 16’ x 16’ Jamesway which would be our first home.

Figure 3.6: Jet assisted takeoff at Plateau Station

While we were working on the Jamesway, the plane was attempting to take off. With the soft snow and no wind, they had to keep taxiing back and forth until they had burned off enough fuel to lighten the load. Finally, after an hour they were able to get off in a roar of JATO, and we were left in our new home.

We had all been warned about the typical problems of high altitude acclimatization - frequently people are afflicted with nausea and headaches after a few hours. The doctor himself was the first to succumb and retired to a tent with an oxygen bottle. The rest of us continued to work on the Jamesway. At this point, we discovered that the stove pipe for our heater had been forgotten; so even if we DID finish the hut right away, we could not heat it. With this news, progress became less enthusiastic. And we realized that we were quite hungry: on Pole Station time, it was seven am, and we had been up and working throughout the night (on our body time). A small gasoline stove was located along with some cans of soup and chicken. We had gotten the ribs of the Jamesway up, but not all the blankets over them; so the first meal at Plateau was served under the bare ribs of an incomplete Jamesway. Revived by food, we finished the Jamesway, and at least we were able to get out of the slight wind that had sprung up for a cold, but welcome, sleep.

Breakfast consisted of more chicken and soup. I had entirely recovered from headache and exhaustion, brought on, I think, as much from hard work and long hours as from altitude. Someone forgot to tell Charlie that we were at high altitude, as he never slowed his pace. Art, the architect, likewise bounced back quickly. Ed the radioman just couldn't seem to get warm: I think he was simply exhausted. The doctor and photographer stayed mostly in their tent - they were slow to recover. I think that if they had gotten out and moved around a bit they would have been happier, but I could not tell them that. The little generator quite objected to the altitude and was very slow in starting. It finally ran just long enough to make a contact with Pole Station, and Ed repeated "stove pipe" several times before the generator died for good. Later we heard that this transmission had been garbled and was interpreted as "please help please" - this must have really upset the receivers as Pole Station. At this point, Charlie decided what the Plateau really needed was a decent outhouse. He went to work with a few packing crates and nails and soon the Plateau had its second and welcome building.

Believing that our requirement for stovepipe had been understood, when we heard a plane come on the second day, we assumed that we would soon be warm and snug. But alas! There was a brand new shiny stove for us, but no stovepipe! Our radio problem had, however, been correctly diagnosed, and there were several new generators in case any of them reacted better to the altitude than our original one. Later in the day, another plane arrived with stove pipe (hurrah!), but also, amusingly enough, crates and crates of movies - enough so that there would be one for every night of the winter-over. But at that point, of course, we had no electricity, no building, no projector, and no inclination to watch movies.

With the camp straightened up and organized, the outhouse built, there was suddenly not much for me to do. It was the last time for a year that life would be so leisurely. I read some Christmas present books, and did a local survey by walking two or three miles away from the Jamesway, then made a quarter circle around it to see if it would stay exactly on the horizon, which it did - indicating that the area was very flat. I also dug a thirteen foot glaciology pit to look at the layers and take a snow temperature reading as an estimate of the annual temperature average. I thought that this pit was well out of the way, but a few days later, the camp tractor backed into the hole, which was thereafter known as "Flint's tractor trap". Our estimates for temperature averages turned out to be correct within five degrees.

On Christmas Eve, the flag had been flying at Plateau for ten full days. We had been short of cots; so had shared beds in the Jamesway, taking turns sleeping. There was always someone stirring about, running into your cot, or coveting it. I decided to movc out for Christmas Eve, and slept in a tent which was cold, but peaceful. Art, the architect, had volunteered to be our camp cook, and did an excellent job. Wanting to reward him, I played S.Claus and sneaked a stocking full of goodies over his bunk. "What the hell is this?!" were Art's first words on Christmas morning - but I could tell that he was pleased. We hung Christmas cards from the ceiling and Art did an excellent job of Christmas dinner, including an eggnog made with frozen eggs, frozen milk,

Figure 3.7: First yeat winter-over crew:Jerry Damschroeder, mechanic; Rob Flint (kneeling), station Sientific leader and geophysics/VLF observer; Bill Lulow, cook; Hugh Muir, aurora observer; Marty Sponholz, meteorologist; Jim Gowan, doctor; Bob Geisel, magnetician; Ed Horton, radio operator and electronics technicion

Figure 3.8: The completed station

and medicinal brandy. We agreed that we may have had more pleasant Christmases, but none so memorable.

Figure 3.9: A fuel flight

On December 30, the Seabee crew who were to build the station arrived with their larger Jamesway. I saw in New Years 1966 repairing my motor toboggan - I worked on it all night and turned in at 7:30 am. The Seabees were forbidden to drink on New Years Eve, but Art ran a speakeasy for the rest of us in the Jamesway. I don't think that all the Seabees were deprived, however, as a bottle of brandy that I had hidden away disappeared during the night! The camp became a hive of activity as the station was laid out, and starting January 2, the vans arrived one after another. The station was quickly assembled and by the 16th of January, I was moving electronic equipment into our laboratory. I enjoyed my first shower in weeks, when the bathroom van was activated. The rest of the month was taken up with stringing antennas, digging pits for various detectors, installing equipment, and generally moving in. As soon as the buildings were complete, fuel flights started to fill the fuel bladders for the winter. In all it took about 45 flights to establish the station and about half of these were fuel flights.

Figure 3.10: Queen Maude traverse

The Queen Maud Land traverse arrived on January 29. In the days before GPS, they had a little trouble in locating the station, but were able to find us due to "looming" - they could see us even when we were nominally below the horizon. Needless to say, they were very happy to be in the relative "civilization" of Plateau Station, and we enjoyed the infusion of eleven visitors. They went right to work in disassembling the three snowcats for shipment back to McMurdo.

The last plane left on February 10. We did not know what was in store for us, but I, for one, had no worries: we had redundant systems and many months stretching before us for scientific and self-discovery. Despite the New Zealand newspaper which described our project as "Eight will Dice with Cold Deatiny", I was looking forward to the year.

Postscript - the station was occupied for three

Figure 3.11: Snow chunks being hauled to the snow melter

years, and was never moved again. It was abandoned in 1969, and the first visitors since then arrived on Christmas Eve 2007, when a Norwegian traverse made a brief stop. They found the snow level just about to the top of the vans, but the station was intact and undisturbed.

Figure 3.12: The Aurora Australis

3.2.1 My brief visit to Byrd Station in 1966

In the fall of 1966 John Katsufrakis sent me to visit Antarctica and Byrd Station in particular. On the

Figure 3.13: Don Carpenter at the Christ Church airport

way, in Christ Church NZ, I was impressed by two brief intervals, at 5 PM when groves of gray clad men would appear on the streets and immediately disappear into pubs, and at 6 PM when the pubs would close and the men would appear once again and board their bicycles for the ride home.

A photo (Fig. 3.13) shows me waiting patiently at the Christ Church airport for the Constellation aircraft that would take a group of press people to McMurdo Station. From McMurdo I traveled via C-130 to Byrd, where I met our two field engineers who had spent the preceding austral winter at the station. One, Chung Park, already shown in Fig. 1.51), was to become a highly successful grad student and researcher at Stanford (see Secion 1.10). The other, Ron Sefton (Fig. 3.14), opened a succcessful sea spa in the Caribbean after leaving the ice.

Ron was kind enough to drive me out to see Byrd Long Wire Station. which had been in operation for a couple of years at that time and is discussed in Section 3.6. I made use of an old 16 mm Bolex movie

Figure 3.14: Ron Sefton in an office at Byrd

Figure 3.15: Ev Paschal near the dome at South Pole in 2003

camera from Stanford as a way of gethering impressions here and there during the visit to Byrd. Sadly, the whereabouts of the film is not presently known. As I left Byrd it was the eve of Thanksgiving, and when I arrived at McMurdo, the special dinner had already been served. Thus I became an innocent victim of the International Date Line.

3.3 Ev Paschal, key contributor to the Stanford Antarctic program

Ev Paschal occupies a hallowed place among Stanford VLF field engineers, designers, researchers, and degree candidates, having arrived at Stanford in 1968 as a first year graduate student and having put the last word to his PhD thesis some 20 years later, albeit only after the money for his support ran out. There now follow his early recollections, as told to (and redacted by) Don Carpenter.

Ev attended the notorious Reed College in Portland, about which people in Portland have always spoken with respect but which no one I ever met seems to have attended. After doing physics at Reed Ev considered applying to Stanford and MIT for graduate work, possibly in radio astronomy. They told him he should apply to a third institution as well, which he did, namely Case Western. Case readily accepted him, while Stanford and MIT waited to see if he would receive a graduate fellowship so that he wouldn't cost them anything. He did get a fellowship, and decided on Stanford, where Radio Astronomy was in the EE department.

Figure 3.16: A Lockheed Constellation, used for transport NZ to McMurdo until 1969

While getting a Masters degree in his first year, Ev spotted a poster in the hallway of the Stanford Electronics Research Lab inviting passersby to consider visiting Antarctica, a great place to "keep your cool." The poster was the brainchild of John Katsufrakis, who seemed noncommittal when approached by Ev, who recalls being a skinny and somewhat scruffy guy who wore lederhosen, which had the advantage of never requiring washing.

By the beginning of summer in 1969 John K had only two candidates for Antarctica, Ev and Rob Bly, both of whom were slated to winter over at Byrd Station in 1970. Meanwhile, Ev spent the summer working on equipment to be installed at the new Siple Station by Jan Siren, Mike Cousins, John Kelly, and Steve Andrews, who would establish a summer camp and make tests on the site. The equipment Ev was working on included a programmer designed by Mike Trimpi that would turn off and on automatically. For Ev this was a learning experience in digital circuit techniques; an important aspect was soldering technique. They were using perf boards. All the wires to an integrated circuit socket had to be separately touch soldered onto pins, a process that seemed to drive John Kelly crazy. When an additional wire had to be added, the entire connection had to be heated up again and a wire already in place would frequently pop out.

3.3.1 A slow flight to McMurdo from Christ Church, NZ

The plane from Christ Church, NZ to McMurdo was a venerable Constellation, as shown in a photo. In those days the radio comms were not reliable, and, lacking word from McMurdo, the Connoie was three times forced to return to ChiChi before reaching the half way point to the ice.

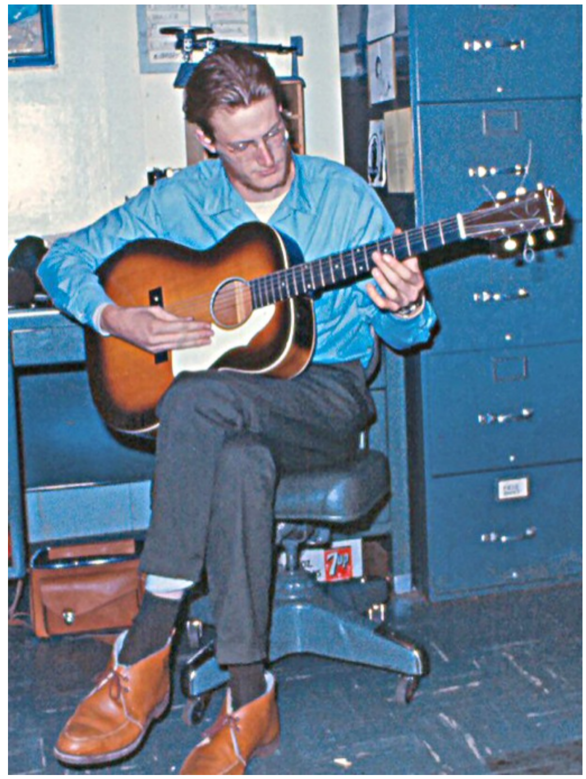


Figure 3.17: Ev Paschal at Byrd in 1970

3.3.2 Ev's winter at Byrd Station and Long Wire and preparations to move the Zeus LW transmitter to Siple

At the end of the summer, Ev flew with John K and others into New Byrd, a 1963 station replacing Old Byrd, a relic of the IGY. New Byrd was set in trenches 30 feet deep that were covered by arches so as to minimize the impact of the constantly drifting snow. Upon arrival a new person would see an opening that led down into the station and a prominent sign warning him to "abandon hope, all ye who enter here."

The VLF lab was in a trench mile from the main station and the IGY antenna and vacuum tube preamp were another 3000 feet away. It turned out that Bly did not stay and winter over, but in his absence Ev was not overloaded with work and.had a good winter. A musical guy, Ev looks quite content in a photo with his guitar.

At the end of Ev's year at Byrd there was still some drilling to base rock going on at Byrd. Ice cores were brought up and were offered to visitors to spice up their drinks. The visitors loved the bubbling as the air trapped in the snow was released. The strata from the time of Christ were particularly popular and were described as being espcially thick.

Figure 3.18: Irene Peden (left) and Julie Vickers at LW in Nov. 1970

In November 1970 the first woman was permitted to visit the interior. This was Irene Peden, from the U of W. A "delightful" KIWI woman, Julie Vickers was broght in to accompnay her the two are shown in a photo from Nov. 1970.

As noted in Section 4.6, U of W had a 30 kW system at LW and Stanford a 100 kW switching transmitter. The UW operations included several receivers within 13 miles of the transmitter. They were able to detect scattering from ionospheric irregularities as well as ionospheric waves. At Stanford we were hoping to detect the LW VLF signals, but succeeded only on satellites passing overhead. Our receiver was placed near Great Whale river in Northern Quebec at Port de Baleine. A major problem was the sliding of the ice near Byrd over irregular terrain below, causing the antenna to be stretched and broken. Furthermore, at VLF frequencies the ice is quite lossy, such that losses in the near field of the antenna reduced its efficiency to fractions of a percent. It was therefore not a surprise that in spite of a coherent integration coding scheme devised by a U of W graduate student, no whistler mode signals were detected in the northern hemisphere conjugate region.

Figure 3.19: John Billey with Irene Peden preparing ice cream at LW in 1970

Ev described the Stanford LW transmitter as being designed in modular sections so as to be passed through the hatch of a submarine. Ninety six SCR bridges were used to handle pulses of current through resonant tuned circuits that were connected to a center tapped output transformer. You could operate at a frequency up to or below the value for which the circuit was tuned, but above that would have a short circuit with current trying to flow on both sides of the transformer at once. You would send a pulse of current through one turned-on bridge going one way and get a half sine wave at the tuned frequency and then a pulse through the other side to complete the sine wave form. Fairly efficient, the system could be operated below the tuning frequency but as the pulses became further apart the output power would drop rapidly. There were four tuned tank circuits using bridges of four arms of 6 SCRs,. All told there were 96 high power 600 V 150 amp SCRs, each on a metal block connected by plastic pipe. Power was supplied by two 80 kW generators, which could bog down and groan painfully when first turned on.

One of Ev's jobs at Byrd was to learn how to operate this system. He would go out every couple of weeks, and at these times would see the need for various modifications. Frequency could be adjusted by punching buttons on the frequency synthesizer but at Siple we would need much more flexibility in doing transmissions. His job was clearly going to be design of equipment to accomplish this. Ev's last operation at LW was in September 1970. The station was going to be packed up and shipped to McMurdo. Meanwhile, Byrd station would run for one more year.

LW had been built like a mobile home with a semicircular arch over it. They were going to dig down, go in one end, pull out a big plywood bulkhead and haul out the equipment and the building. The LW buildings were a product of Pepsico in Canada, with aluminum outside skin, freezer doors, and fairly good insulation. There were two bedrooms, a darkroom, lab space for recording, a kitchen, generator room, and a transmitter room transverse to a hallway, The arch would be abandoned. Someone at McMurdo would refurbish the building for use at Siple. Bedrooms at Siple would be added by the addition of a T5 building stuck on the end. Such structures, brought down during the IGY, were made of panels with foam between them held together by clips. Ev took the transmitter apart and had most of the packing done by Xmas. The new crew for Byrd included John Billey, who helped Ev with the final packing.

A photo shows John Billey and Irene Peden at LW working on a much appreciated task, making ice cream.

3.3.3 Digression for a memory of Rossman Smith, Stanford field engineer at Eights and at Byrd

The LW VLF antenna had an RG8 copper core that was surrounded by a one half inch dielectric. In 1963 or 1964 it had been laid out on the ice by a crew that included Rossman Smith, who had wintered over for Stanford. John K reportedly told Ev that after the antenna deployment there had been a pyramid of wooden spools, which Smith proposed to burn as a bonfire. Gasoline was poured over the spools, which began to burn nicely, but in the interiors of the spools there was gasoline vapor that soon led to a rather sensational explosion. Rossman, who had been standing close to the fire, came running up with a splinter sticking out of his forehead yelling "Hey, did anybody get a picture of that?" Later, in further illustration of Smith's daredevil approach to problems, he and another guy decided to drive to Eights station in an RT2 vehicle. Upon arrival Rossman suggested that to gain access to the station, they discharge a C3 plastic explosive. A discharge occurred and the snow rose grandly in the air and then fell grandly back into place.

When Rossman returned to the Bay Area, his daredevil approach to problems became manifest in a company whose name included the word "Hazards,"

3.3.4 The summer of 1970-1 at Siple: the stunning X-ray correlation event captured by balloon

At Xmas JK and Ev, Mike Trimpi, and Rob Flint were waiting at McMurdo to get into Siple. The Siple crew would include a U. of Maryland group led by Ted Rosenberg, Ernie Grossenbacher, Klaus Cristoff, and Jan Holtet. Jan had a VLF receiver to go with the U. of M. high altitude balloon and operate in conjunction with Ted's low energy X ray detector. Hans Lee, an excellent field man, was in charge of the Bell Labs magnetometer.

On the morning of Jan. 2, 1971 (as shown in Fig. 4.5) a balloon was launched and after gaining altitude, detected busts of X rays that, as Ted Rosenberg

Figure 3.20: Rob Flint on radio to McMurdo

Figure 3.21: At Siple celebrating the X-ray-VLF correlation event

excitedly reported, preceded bursts of VLF chorus by a fraction of a second. They were using a mylar superpressure balloon that was overcharged with helium for a fast rise. After overshooting the desired altitude the balloon could operate for several days as it dropped toward the desired altitude. To mitigate overshooting and unaware at the time of the "ozone hole," the balloon crew used ballast in the form of a bottle of freon, whose contents would evaporate during the balloon's ascent.

As a diversion, Ernie Grossenbacher cut some plywood footprints, and one morning left some tracks around summer camp, as if Bigfoot had been there.

3.3.5 Contructing the Siple transmitter antenna

Figure 3.22: Ev working on a section of the Siple 21 km antenna

The plan at Siple for EV, Mike Trimpi, Rob Flint and John K was to put the antenna on masts of aluminum channel 6 inches across, with the wide part driven into the snow and guyed with ropes. An insulator would be put at the top. The masts would be about 200' apart forming a dipole extending over a distance of 7 1/2 miles. Fig. 3.22 shows Ev at work on the eventual antenna construction The poles and wire had arrived, and in Ev's opinion the entire job could have been done if there had been enough help.

Everything was mounted on a Nansen sled. They would go out magnetically east west and sight back along the line in order to put in another pole. There was fog, but they kept a remarkably straight line over the 7.5 miles involved.

The poles were 200 feet apart, separated by lengths of polypropylene cord.The team would dig a hole down a couple of feet, put in a foot square anchor board, lift the mast and fill the hole with snow. Three anchor (guy cables or ropes?) were attached to the pole, leading to anchor boards 2 by 1 ft to which they were connected by eye bolts. These boards would be driven in with a sledge hammer. They then used wire clips to tighten up the guy cables. The antenna wire was designated for mounting the next year.

3.3.6 Back to Stanford after the 1970-71 summer season

Ev got back to Stanford thinking he would at first go for grad studies, but there was an obvious ongoing need for peripheral equipment at Siple. Bill Trabucco had been hired on as a winter-over Siple field operator. He had an undergrad engineering degree from Berkeley, and he and Ev became lifelong friends. They started building power supplies and keying equipment for the new Siple station.

Bill was working on new recording equipment for Siple and Roberval. Ev had worked on gear.at Byrd sent down during the IGY. One of the advantages of the vacuum tube design had been its self heating-it worked in the cold. Ev started designing transistorized pre amps; which became his major contribution: common base input design amplifiers, still being used more or less in some form by Stanford and U. of Florida.

3.3.7 Siple develops gradually; Ev is "recruited" for another year

John had found someone to operate at Siple during the coming year 1971-2. Bill and Ev were getting ready to go down to complete the station. When they got there they found that the buildings weren't ready to go in. The guy at McMurdo who had been assigned to refurbish them had been overwhelmed by the size of the job, It was clear that Stanford couldn't operate at Siple for another year, but wire could be put on the antenna and the properties of the antenna could be measured and tuning networks could be built.

There were four 71 foot antennas to carry transmitter lines out to the antenna and allow for moving around underneath. Part of the arch was constructed that summer by Navy Seabees. It was similar to the construction at Long Wire.

At the end of season 1971-2 Ev came back and worked on finishing up the receiving systems at Siple and installing the station at Roberval. John had chosen a winter over guy for Siple, but toward the end of 1972 the guy failed his physical. John then asked Ev if he knew anyone who knew the transmitter, could pass the physical and therefore spend the winter. So Ev volunteered to go down with Bill for the forthcoming season.

3.3.8 Ev encounters the "promity" effect

At Siple there occurred a major problem in the design of the transmitter system. Back at Stanford Ev had sent the requirements for the special Siple inductance coils to an East Bay company called Elma Engineering, people who were known for their coil building. They produced were things of beuty, rectangular wire air spaced with fiberglass supports. Ev, JK and others went over to see the result and found the inductance to be correct. However, Ev brought an lCR bridge and found the Q of the coil to be surprisingly low. As he said, he didn't suspect a problem, so ignored it. When it came time to test the system at Siple, however, an acrid smoke rose and spread from the tuning room throughout the station. The polyethelene sheath of the coils was being burned by the intense heat generated due to what turned out to be the proximity effect, by which the current was being pushed out to the edge of the coils by the magnetic field in the multiple coil elements. Instead of going into the antenna, the power was being dissipated in the coils. When Ev later got back to Stanford he consulted Terman's book on radio engineering and found that a good article on the proximity effect had been published in a British journal of radio engineering in 1928, a journal which was thankfully available at Stanford.

They were then forced to scrifice some of their targets in terms of coil inductance and low frequency tuning limits by rebuilding the coils on site with available cable, this time with larger separations of elements so that the proximity effect was not be serious.

3.3.9 Siple underway at last

At Siple Seabees were busy building the arch. The original summer camp Jamesways, still in use, were now well buried, down as much as 15 or 20 feet. They learned to build a hatch at each end and an entrance ladder, so that when you went down it was very comfortable.

This was the smallest station the US had run, There were only 4 men, with Ev and Bill, Jay Clink the mechanic, and the medical guy Thrallkeld. Jay became a good friend of Ev and Bill, and in later years would come around and see them. Sadly, he died not long ago. Thrallkeld spent most of the time in his room trying to write the great American novel.

In the 1960s Byrd had had an auroral substation in the winter, only 3 guys about 70 miles away. It provided an interesting psychological study; there would typically be one person against two, but a switch would occur every month or so.

Bill had done cosmic rays at McMurdo in 1969 for Pomerantz, and Ev and Jay had both wintered over, Jay while in the Navy at Palmer. The navy's role was now declining, although it was still a major player, having called the shots since IGY times. Siple was the first all civilian station..

Each guy had brought things of personal interest to occupy the expected leisure time, but they were unexpectedly busy and content to be so, taking a day off only every couple of weeks. Not until the final month of their stay did they get to use the games they had brought.

Back at Byrd Ev had been busy with station operation only a short time each day, say about hour. While there he had had daily predictions by teletype. Warnings of radio blackout would only reach Byd after the event was over. The teletyped message would have had to pass through and be delayed at several stations.

At Siple, ham phone patches were your means of personal contacts with people. They mostly used John Stagnaro, Bill Trabucco's uncle (Uncle John), a ham in the L.A. area. In conversation with John, Ev was asked if he knew Wayne Paschal Indeed, Wayne was Ev's dad, who had worked for a radio shop in SF that turned out to have been John's shop. Wayne had lodged at the Stagnaro boarding house.and had dated Bill's mom Mary Stagnaro, who later, by another coincidence, married Frank Trabucco, Bill's father.

When the last plane left in Feb. 1973, the guys were still putting together science gear. Ev was just getting started putting the transmitter together and Bill was still assembling the receiving system. The emergency camp hadn't been finished.

The Siple transmitter began operating in April, 1973. Ev had brought down a Data General minicomputer and during the winter was busy writing an operating system. That system was called Xlan and like BASIC was an interpretive language. The computer could set the operation of the frequency synthesizer to key on and key off. There was no way to vary the output amplitude because of the switching nature of the Zeus transmitter system. Xlan was later used with Jupiter, the second transmitter, as well. And it was such that people back at Stanford could write programs.

There was no floppy disc drive, but it was possible to construct fairly complicated programs. Attempts were made to build electronic devices to drive the keyer, but this system was kept instead. There was an ASR33 teletype machine for paper tape that would be read in to the computer. It was necessary to vacuum around the controls first.to be sure the long paper tape would be read cleanly. A typical roll for a format would be 50 feet long, and there was no error correcting machinery for reading in a tape..

One of the first transmissions from Siple was a hand keyed signal. NQU was the station call sign. It was sent out slowly using Morse Code, sec for a dot and 1 and for a dash to allow for dispersion in the magnetosphere. John Billey at Roberval could read Morse code, and John K may have been there. Conditions were just right one day and they saw the signal at RO. Only later was it learned when in the day and what frequencies to use for optimum transmissions..Ev had sent test tones earlier but this reception was unequivocal. It was in fact a test message with an embedded mistake. John Billey corrected the mistake so it was clearly a Siple message. Ev had been doing this sending for a week or so. He had also sent test tones.

3.3.10 Ev finally writes his thesis

Support funds became increasingly tight for the VLF group in the 1980s. Ev was back in school after 1983, thinking about a PhD project. John offered him a research associateship, but the idea didn't work out. Finally, Ev was laid off in 1987 and took the opportunity to do his research on phse analysis of the Siple signals. It had been 19 years since he had first some to Stanford. Ev joked, with unknown effect, about outlasting the deadly math student Stoleski who had been around Stanford for 20 years.

Figure 3.23: Ev at the Stanford field site in November 1972 discussing Siple plans with Don Carpenter, Bob Helliwell, Bill Trabucco, Tim Bell, and Bruce Dingle

3.4 Antarctic veterans' thoughts about John Katsufrakis

Ev Paschal, as one of our true Antarctic veterans knew very well that the success of our VLF field programs was largely due to John's expert efforts. "He was very detail oriented: in the construction of Siple-you knew if you opened a box that everything you needed would be there, as well as spares." He kknew that after the first summer at Siple, all the cargo there would be buried, so the next year he brought a tripod hoister to aid in retrieving it.

Figure 3.24: John Katusfrakis taking a bath at Siple Station

Ev couldn't imagine life in the field without John. In terms of field programs, RAH was in charge of the science, but how manned and supplied the programs were he had no say that overlapped with John. John could communicate with anyone incljuding the Navy brass, the fliers, the seabees and the storekeepers in the field offices or a shipping clerk. He kept nothing hidden; everyone knew who he was. Ev recalls that John got injured in a football game at Byrd and was begging a doctor to shoot him with Novocaine so that he could continue.

John had a fatherly approach to everyone in the field, but was particularly deferential to Bob Helliwell' science ideas. This could be a problem, because at any one time Bob would give you more ideas than you could hope to implement. John would on occasion try to follow a suggestion that was just not going to work.

On only one occasion did John show anger to Ev, back at Stanford in the case of an aargument with another research group about access to a piece of equipment. John became irate with someone from the other group, and there followed a couple of weeks of silent treatment for Ev. John said that the volatility was due to his Greek personality.

John was in the unfortunate position of being the group bookkeeper during the 1980s, when funds were shrinking. He was forced to leave in 1987, at a time when his health was beginning to deteriorate. When Ev and Jack Dooittle were headed into the field in 1994 John asked them to be pall bearers at his funeral, saying he would hang on until they returned. Sadly, he did not succeed.

3.5 The 1980-81 Rocket-Balloon Campaign at Siple Station

Figure 3.25: Carpenter talking to John Green at Palmer during campaign

I spent the austral summer of 1980-1 as a novice field operator at Siple, helping conduct a series of experiments on effects associated with the VLF transmitter. Fig. shows me in the passive lab talking to super-veteran Ev Paschal. Also taking part for Stanford were Bill Trabucco, another super-veteran, as well as the experienced Mike Dermedziew and John Billey. Mike was a keen observer and meticulous operator of VLF equipment, while John was three miles away at what they called "three mile island," a temporary receiving station where he could do direction finding on down-coming VLF signals. John was one of the only people in our group with field experience operating a VLF tracking receiver/direction finder designed at Stanford by grad student Mark Leavitt in 1976 as part of his PhD work.

Siple was bursting with people, among them the high altitude balloon team from the U. of Maryland: Ted Rosenberg, Larry Lutz, Dan Detrick and Jan Siren. David Mathews, also of the U. of Maryland, was there to work with precipitating electron detectors on the Nike Tomahawk rockets. Fig. 3.13 shows David standing by one of the rockets on Dec. 12, 1980, a rare clear day.

Figure 3.26: Nike Tomahawk rocket ready for launch at Siple. David Mathews, investigator for a particle experiment on board, stands nearby

Paul Kintner of Cornell. supported by Richard Brittain, was in charge of the wave receivers on the rockets. From the U. of Houston came five people to launch Super Arcas rockets, Bob Sheldon, Jim Benbrook, Gar Bering, Jim Roeder and E. Stansberry. For a full list of groups involved both at Siple and in the northern conjugate region see an article in the Antarctic Journal by *Mathews* (1981).

The largest group, seven or eight in number, was the rocket launch team supported by NASA through Goddard Space Flight Center. They were accustomed to firing rockets out over the Atlantic Ocean from Wallops Island, Virginia and thus were having a novel work experience on the ice. The team is memorable in part because whenever it was time to take a group picture, their leader would call out "everybody say 'shit'" and everyone would dutifully say 'shit' as the picture was snapped.

3.6 Running in Antarctica, a Reminiscence by Don Carpenter

I had become a dedicated runner in 1968 at the suggestion of a volleyball coach who pointed out that if we casual players wanted to do well in all day tournaments, we better start running on the university track and also climbing the fifty or so rows of seats in the stadium. I started running and climbing the seats and within a few years had given up volleyball altogether (my reflexes were starting to go anyway). Running had become addictive; on every week day at noon you could find me running with the Angell Field Ancients somewhere on or near the campus. I ran mostly at Stanford, but also wherever else I happened to be, such as in Antarctica.

Before I get to the highlight of my running at Siple, namely my search for a lost rocket booster, I need to mention two lowlights, running on the Siple ski-way and the ill-fated Bunny Boot Beer Run. Although the ski-way was just outside the station and thus very convenient, on most days it was white enough outside to prevent me from seeing anything but the large black markers at the edges of the ski-way.. I had to be very tentative about placing each foot because the ice surface had been crisscrossed by taxiing ski-equipped C 130 aircraft that had carved unpredictable variations in the snow or ice level. It wasn't easy to run, but I managed to go out a number of times.

My big disappointment, and that of a few others, was the Bunny Boot Beer Run. The rules of that run were, as you might expect, a combination of beer drinking and jogging in huge, ungainly boots around a small course. Teams of four would compete, and your job as a team member was to drink a designated amount of beer and then run in Bunny boots from where you started to the next team member along the course. The beer was not going to be all that good; it had been frozen at some point in the preceding year. People were excited about the event, however, and a woman on one team even got busy designing uniforms. My plans for the Beer Run were halted by foul weather, so I chose a new date, allowing time for the bad weather to go away. On the new date the weather again turned bad, forcing yet another postponement. This time, however, I was persuaded to cancel the event completely. For one thing, the rocket team feared that scheduling the Beer Run would bring on bad weather that would again last for days. The sooner the weather could clear and the next launch occur, the sooner they could go home for what was left of the holidays.

On December 21st, the day after the second rocket launch, someone mentioned that the rocket team guys had gone out in a snowmobile to find their first-stage booster, regarding it as the kind of souvenir that would not be available from a launch at Wallops Island. How could it happen that they had failed to find the booster? Surely they knew the right direction to go. The weather was fine, so I decided to put on my running shoes and see if I could succeed where they had failed. This was a rare opportunity for a runner: search for and hopefully find a very large missing object.

It was easy to follow the tracks of the snowmobile but I had no idea what I might see that the rocket guys had not seen. After about a mile, however, I came upon a place where they had turned around and headed back toward the station. My only thought was that this was the right direction and that maybe I should just go on for a while. The ice surface was windswept and irregular, and any buried or partially buried object might be hard to see. After a while I was encouraged by spotting a piece of burnt fiber glass in the snow, assuring me that this was indeed the right direction. However, after a few more minutes I started to worry about getting too far from the station, already a tiny row of dark objects in the distance. I reluctantly turned back, with increasing doubts about any chance of success. Then I found it! There was a remarkably clean hole in the ice, roughly five feet across (I think), not more than 50 or 100 yards past the point where the snowmobile had turned around. The hole was at least three feet deep and contained chunks of ice or snow that covered any view of the booster itself. I was tempted to jump down into the hole, but was fearful of somehow not being able to climb back out again. Instead I simply jogged back to the station and told the rocket folks where I had found their booster.

Immediately the guys who had not been in the snowmobile began casing on those who had gone out. The gist of it was: “you guys in a snowmobile came back empty handed and then some old guy in a pair of running shoes finds the booster.” Later they salvaged chunks of the booster, promising me one as a souvenir but somehow failing to deliver it.

There was one other running experience on that trip that deserves special mention. I had met a group of older runners in Christ Church, New Zealand on my way to the ice in November 1980 and hooked up with them again as I was heading back to the States in February. They told me their club had planned a 20 mile race on Saturday, a couple of days prior to my leaving Christ Church. I dutifully showed up, with intentions of going only 10 miles, 20 being well beyond anything I was prepared for. I spoke to the son of one of the runners, telling him to pick me up at the ten mile mark. He agreed, and at ten miles I looked everywhere. Where was he?. Somehow I managed to continue another 10 miles to the end of the race. I asked him: where were you? He said that he had seen me and that I had looked just fine.

Part 4

Wave-Induced Particle Precipitation

4.1 The "Trimpi effect", first evidence of precipitation of $E > 30$ keV electrons through interactions with whistler-mode waves propagating in space

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In the early days of the space age there was talk of perturbations in energetic particle orbits by whistler mode waves, but no clear evidence of individual scattering events was reported until (as noted above) the NYT reporter Allyn Baum came to Eights Station in 1963., Baum was told about observer Mike Trimpi's remarkable discovery of one-to-one correlations between individual whistlers originating in the northern hemisphere and perturbations of VLF transmitter signals propagating to Eights Station in the Earth-ionosphere wave guide. Baum's excellent article describing the observations and including a picture of Mike (Fig. 1.42) with a glorious full beard, appeared in the November 26, 1963 issue of the New York Times.

Figure 4.1 shows an example of the effect observed simultaneously at Eights and Byrd, Antarctica. At the top is a 0-10 kHz spectrum from a 2-min synoptic tape recording at 0050 UT on October 4, 1963. Next is a chart showing the integrated 2-6 kHz amplitude of the synoptic data just above, and at bottom is the amplitude of 200ms NSS pulses transmitted every 2 s at 22.3 kHz from Annapolis, MD. There was a factor of 2 increase in the pulse amplitude at the time of the large whistler group shown above. A similar change was detected in the records of Byrd Station, but was not reported by the station observer.

Figure 4.2 shows continuous charts of NSS 22.3 kHz and NAA 19.6 kHz amplitude at Eights over a 30-min period on October 5, 1963. The abrupt whistler induced amplitude changes and slower recoveries were opposite in sign at the two stations.

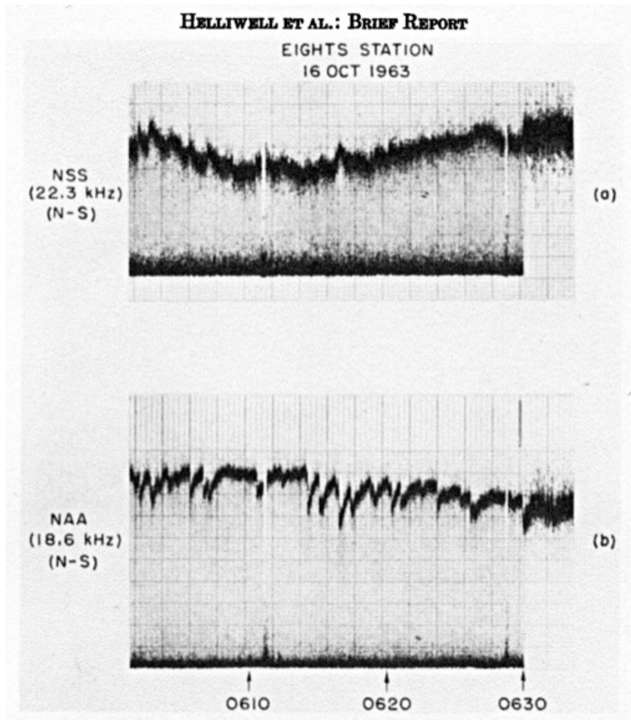

Figure 4.2: Mike's NSS and NAA amplitude charts from Eights. From *Helliwell et al.* (1973).

4.1.1 Why the 10 year interval between the NYT article and the writeup in JGR?

How did it happen that Mike's discovery of what later came to be called the "Trimpi Effect" was not described in a peer-reviewed journal until 1973, ten years after Baum's article appeared? This is one of the great questions that our history must try to answer. My first thought is that in 1960 there was a paper by *Helliwell and Bell* (1960) on the geo-cyclotron mechanism, by which radiation belt particles with energies of order keV could be resonantly accelerated to much higher energies through interactions with VLF waves of variable frequency injected from ground sources. Following the Starfish Prime high altitude nuclear test in 1962, a 1963 research note by *Bell* (1964) discussed the inverse geo-cyclotron mech-

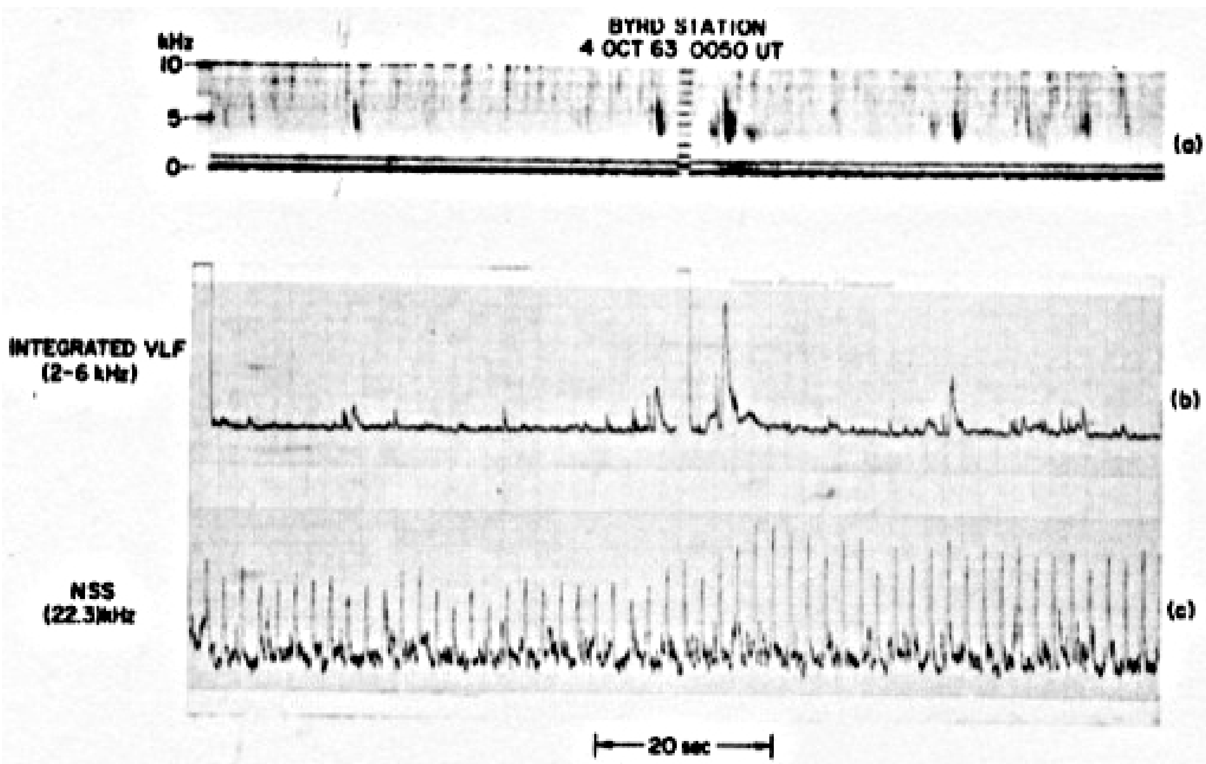

Figure 4.1: Trimpi event at Byrd, Antarctica on October 4, 1963. From *Helliwell et al.* (1973).

anism, by which deceleration of MeV electrons in the radiation belts might be achieved, again through temporal changes in the transmitted VLF frequency. These two research papers involved the perturbations of particle orbits by whistler-mode waves, so that one might wonder why they did not lead to immediate recognition of the significance of Mike's observations.

And then there was Neil Brice, who wrote a 1964 JGR paper on VLF emission mechanisms (*Brice*, 1964), and in doing so discussed the means by which particle energy can be changed through cyclotron resonance with VLF waves. Did Neil hear of Mike's discovery, and if so, how did he react? He was quick to seek an explanation of phenomena uncovered within the VLF group, such as the evening-side bulge in the plasmasphere (*Carpenter*, 1966). Neil's explanation of the bulge was published early in 1967, less than a year after the global plasmasphere and its bulge were first described.

All of this suggests that details of Mike's observations were not spelled out to us in a clear way. Was John Katsufrakis, field program supervisor back at Stanford and recipient of Mike's reports, uncertain about their true meaning? Today, Mike recalls that: "the only comment was made by John at the time I first reported what I had seen. I had sent him a message describing the events. Since this was sent by code it was necessary to (be) short and to the point. A few days later I received from him: "Either you have made a great discovery or you've been there too long". This was in October, I think ... maybe late September. By the time I returned to Stanford the initial excitement had died down." (In those days there was no quick way to transmit data back to Stanford, so proper review of the tape recordings and chart records of VLF transmitter signal amplitudes had to await months-long shipment delays.)

Meanwhile, what was being said to our sponsors in Washington? What did Helliwell know about Mike's observations?. How were they reported to him? What did I know and hear? I may well have been told about a NYT article, but have no memory of actually seeing it. Whatever the case, I must assume some of the group's responsibility for the 10 year delay.

4.1.2 Ongoing studies of transmitter signals in the 1960s

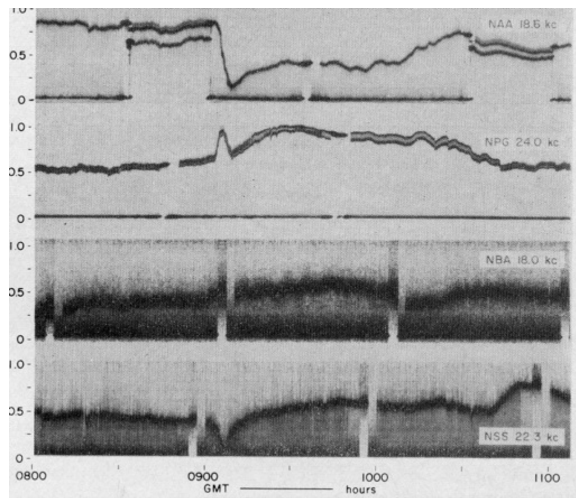

Figure 4.3: Anomalous perturbations on NAA amplitude charts at Stanford. From *Carpenter et al.* (1966).

The value of fixed frequency VLF signal measurements as tools for discovery was illustrated in 1965 by *Carpenter et al.* (1966), who reported on anomalies in the vertical electric fields of signals being observed routinely at Stanford.and at Quebec City in Canada. The changes in signals such as NAA to Stanford on October 14, 1963 appear to have occurred at the time of magnetic perturbations at higher latitudes of the kind that were just beginning to be identified with substorms. Such a case is illustrated in Fig. 4.3, in which the 18.6 kHz NAA signal amplitude (on a linear scale) dropped by of order 30 dB within a few minutes after 0903 UT and remained disturbed for about 2 hours. Meanwhile Fig. 4.4, shows correlated magnetometer perturbations at a wide latitudinal range of stations. The authors of the paper point out that the disturbed VLF signal paths were in total darkness and that there were no solar flares observed at the time. The connections between the magnetometer perturbations at the higher latitudes and the VLF signal changes were not well understood, but in retrospect appear to have been early indications of a large scale substorm-time associated current system and associated particle precipitation.

Observations of spatial and temportal varations of VLF transmitter signals and whistlers in the earth-ionospere waveguide were conducted by our VLF group along the west coast from a mobile platform in the early 1960s. A Navy communications truck was obtained and instrumented to perform the measurements. It was dispatched north in the late summer 1962 with a crew of three - Brad Helliwell, Charlie Weigle, and George Carpenter. The truck was first stopped at a quiet site near Ashland, Oregon. The Navy NPG transmitter at Oso, WA was the center of attention during the second phase of the measurements, which began about 100 km south of the transmitter where the signal was dominated by the ground wave. The truck was then moved about 25 km further south every 24 hours for about two weeks, a schedule that was ideal for the crew, who were allowed to attend the 1962 World Fair in Seattle while measurements continued unattended.

The fate of the data collected on the truck is not known, but George Carpenter, even after leaving Stanford in 1964 with an Engineer's degree, continued to monitor the chart recordings of VLF transmitters at the new field site building off Page Mill Road west of Highway 280. This continued attention led to the remarkable paper by Colin and Carpenter discussed above in Section 1.5.

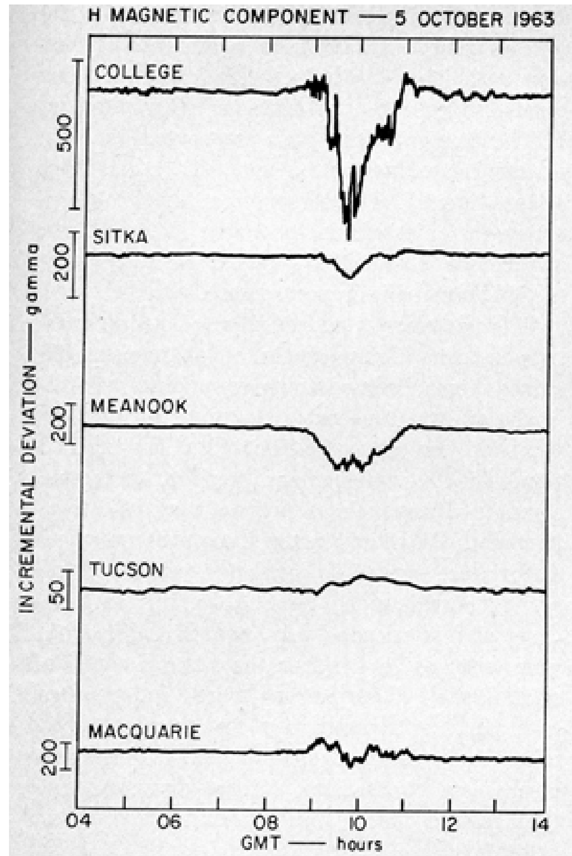

Figure 4.4: Magnetic field perturbations during signal anomalies in Fig. 4.3. From *Carpenter et al.* (1966).

4.1.3 The Jan. 02, 1971 X ray event at Siple

Eight years after Mike's discovery and Baum's NYT article, the occurrence of electron precipitation induced by VLF wave bursts was clearly demonstrated at Siple Station (near Eights) during an X ray balloon campaign involving Stanford and the U. of Maryland (*Rosenberg et al.*, 1971). However, the apparent discovery nature of this finding put Stanford VLF in an awkward position. As collaborating experimenters and coauthors of the X ray paper, Helliwell and Katsufrakis could not very well say "by the way, a similar burst precipitation effect was discovered by our engineer at Eights 8 years ago." Perhaps at Helliwell's suggestion, the paper did note: "It is widely recognized that whistler-mode waves probably play a major role in the precipitation of electrons (Brice, 1964a, b; Kennel and Petschek, 1966)." But it also stated that "*no direct link between discrete VLF emissions and particle precipitation has previously been found*" (emphasis supplied).

It has been traditional among research groups operating in Antarctica for the home-bound principal investigator to be the first author of an article describing a discovery made by a winter-over observer at a field station, thus placing the observer in a supporting role as coauthor of the discovery paper. Such articles were written in the late 1970s, as mentioned later in this section. However, they were published promptly, within less than a year of the key field observations.

So what should have been done in the years after 1963 as our group's position became increasingly awkward vis a vis the Trimpi Effect? How could Mike's observations have been presented as a new discovery when year-by-year they were becoming an old discovery? A mitigating factor may have been the initial report in 1964 on emissions triggered by Navy transmitters, with Helliwell, Katsufrakis, Trimpi and Brice as coauthors (*Helliwell et al.*, 1964).

Figure 4.5: U. of Md team launches balloon at Siple in 1971

With 1971, however, and the exciting Rosenberg et al paper, the occurrence of energetic electron precipitation by wave bursts was newly yet firmly established, and a discovery paper on the Trimpi Effect could be written with minimum embarrassment. Helliwell finally did just that (*Helliwell et al.*, 1973), with John Katsufrakis and Mike Trimpi as coauthors. Their paper had not appeared in 1964, as it should have, but it was nonetheless a great success, heralding future years of observational opportunities across the globe and much new resulting knowledge of particle precipitation. And for at least a while, it brought special attention to the discoverer through use of the term "Trimpi Effect."

The paper featured the 1963 Eights chart records of whistlers that were closely -correlated with changes in the received amplitude of sub-ionospheric 22.3 kHz VLF signals from NSS in Annapolis, MD and 18.6 kHz pulses from NAA in Cuitler, ME. In many cases the charts had been annotated by Mike Trimpi while acting as observer. The 1971 X ray observations at Siple, already reported by Rosenberg et al [1971], were mentioned as supporting evidence that energetic electron burst precipitation into the D region had in the Eights data been induced by whistlers. The Eights whistlers propagated on paths distributed in L value, but the strongest component was found to be ducted at $L \sim 2.5$ within the plasmasphere. Non ducted whistler paths were considered as possible sites of scattering, since transmitter signal paths to both Eights and Byrd could in some cases be affected. The onset times of the TX signal changes were found to be of the order of 2 s, and the decay times of the perturbations of order 30 s.

From whistler measurements of L shell and equatorial electron density it was possible to infer that the scattered electron energies were > 40 keV, numbers comparable to those that had been inferred in the X ray balloon cases. Much was not clear in terms of the scattering mechanism, including the expected role of coherent whistler waves, as opposed to the incoherent waves previously discussed in the literature.

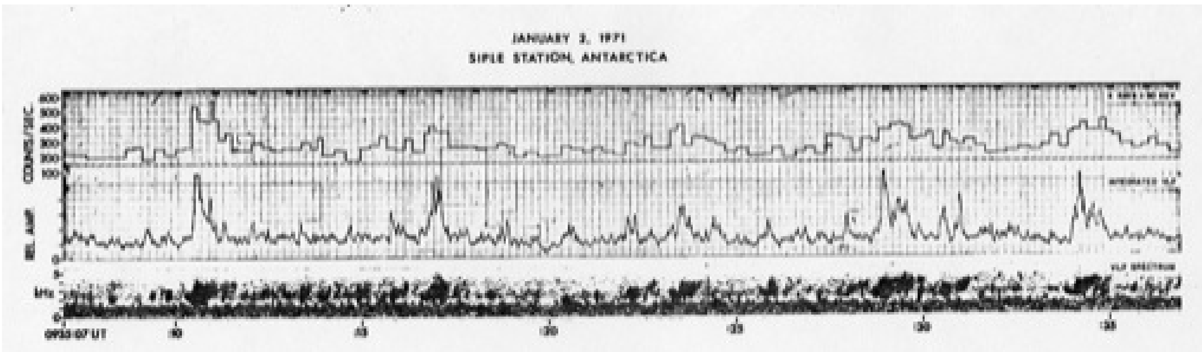

Figure 4.6: Correlation of X ray bursts (above) and VLF wave burst spectra (below). From *Rosenberg et al.* (1971).

4.2 The "Perfect Substorm" and the 1971 Rosenberg et al. paper on correlated VLF wave bursts and X rays measured over Siple

During a substorm period on January 2, 1971, a high altitude balloon launched at Siple Station by a U. of Maryland team led by Ted Rosenberg detected bursts of X rays with average energy estimated at 60 keV. The bursts were repeated quasi-periodically at intervals of $\simeq 6s$ and were time-correlated with bursts of VLF waves in the range $L \simeq 1.5 - 4$ kHz. Fig. 4.5 shows a Siple balloon launch and 4.6 the compared X ray data (top two panels) and the VLF dynamic spectrum in the range 0-5 kHz. Careful examination of simultaneous spectra from the conjugate region indicated that the VLF wave bursts were triggered just outside the plasmapause by whistlers. It was further found that the VLF bursts reached Siple a few seconds prior to the X rays. These observations were found to be consistent with the inferred precipitating particle energies and with a model of the scattering of those particles during transverse resonant interactions with south-going VLF wave bursts, as had been discussed by Helliwell in his 1967 article.

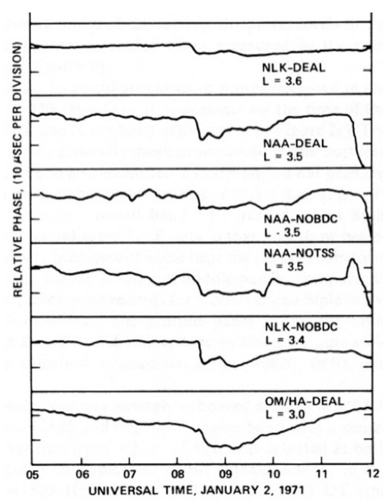

Figure 4.7: Onset of phase perburbations on multiple paths during the "perfect substorm". From *Rosenberg and Saus* (1974).

I became very excited about the Jan. 2, 1971 event for several reasons: it came at a time when we were actively studying substorms and in particular were measuring the cross L plasma drifts associated with them. It was also a time when energetic particle data from synchronous orbit were beginning to be widely reported and were available from ATS 5, located within an hour or two of the Eights meridian. And of course it involved scattering of particles into the ionosphere by whistlers, as had been demonstrated by Ted Rosenberg's X ray measurements.

We knew about Mike's observations of whistler-associated perturbations in VLF transmitter signals, but they had yet to be published.

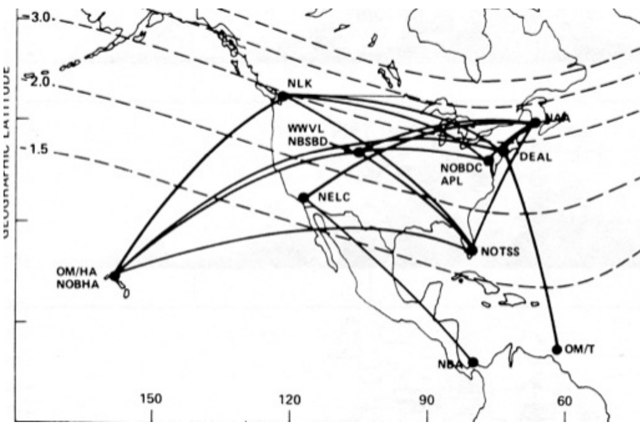

Figure 4.8: Map of VLF signal paths inside the plasmasphere showing phase perburbations during the "perfect substorm". From *Rosenberg and Saus* (1974).

Figure 4.9: Celebration at Siple after the X-ray correlation event.

My excitement led to an informal Stanford lab report that was sent to a number of colleagues in thanks for data and to encourage discussion. The report began with brief hoopla and then posed the question "where were you on Jan. 2, 1971?" I got only one answer, from Fred Scarf, who announced that he had been in his office,

Over time in the next few years we collected a variety of substorm results, one being a change reported by *Potemra and Rosenberg* (1973) and more fully by *Rosenberg and Saus* (1974) in the phase of VLF signals propagating across North America at $L > 2.5$. This change in phase over long paths in the outer plasmasphere is illustrated in Figs. 4.7 and 4.8 by a map of the paths and a plot of the substorm associated phase changes. Even in 1975, when we managed to write a paper discussing the Jan 2, 1971 substorm (*Carpenter et al.*, 1975), it was not known what to make of these perturbations. Tim Bell, aware that we had observed cross-L inward drifts during the substorm period, suggested that those plasma motions, being widespread and persistent, would involve a lowering of particle mirror points that might explain the phase change over a large region.

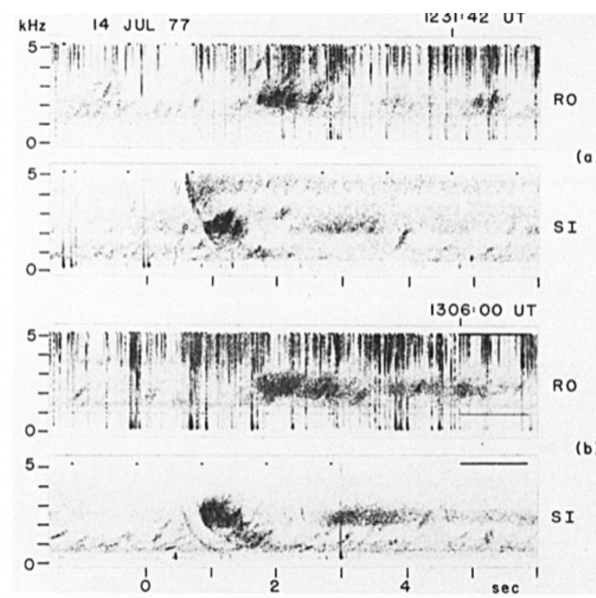

Figure 4.10: Two whistlers ourside the plasmapause that were correlated with optical emissions. From *Helliwell et al.* (1980).

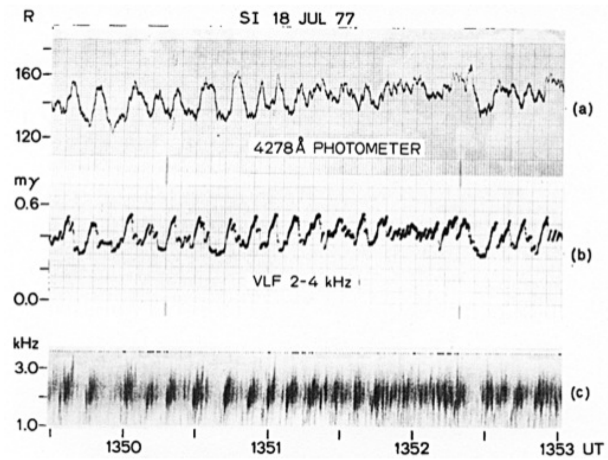

Figure 4.11: Optical emissions (above) and near-simultaneous VLF quasi-periodic emissions. From *Helliwell et al.* (1980).

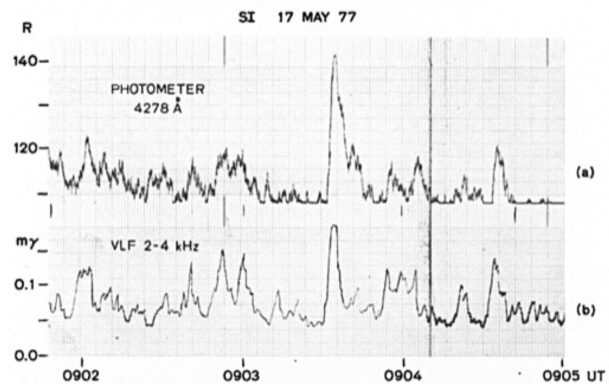

Figure 4.12: Optical emissions (above) and near-simultaneous non-periodic VLF emission. From *Helliwell et al.* (1980).

4.2.1 Stunning wave-induced optical emissions observed in 1977 at Siple

In 1977 an extraordinarily successful winter field operation was held at Siple by Jack Doolittle and Bill Armstrong. (They had been slated to go down in 1976 but were delayed one year by operational issues). In July, 1977 Doolittle and Armstrong made stunning observations of a direct connection between VLF waves propagating outside or within the plasmapause boundary layer and optical emissions at $\lambda 4278$ in the ionosphere over Siple ($L \simeq 4$) (*Helliwell et al.*, 1980). This was an important extension of the electron scattering effects previously observed in the X-ray-VLF burst correlation found by Rosenberg et al. at Siple in 1971 as well as the Trimpi effect, published in 1973 *Helliwell et al.* (1973) but actually discovered in Antarctica in 1963, which involved energetic electron scattering into the ionosphere by whistlers and correlated perturbations of sub-ionospherically propagating VLF waves.

The new Siple observations occurred during 6 of 32 observing sessions near local dawn, usually during or shortly following moderate substorm activity. Two examples of the conjugate recording of whistlers associated with induced precipitation are presented in Fig. 4.10, which shows events recorded some 30 min apart. They originated in the north, triggered noise in the 1-3 kHz range, and echoed back to RO where the wave intensity observed was diminished by daytime ionospheric absorption.

In some of the 1977 correlated observations the VLF waves were quasi-periodic as shown in Fig. 4.11, which displays a close connection between optical emissions and VLF bursts in the 1-4 kHz range. The next figure shows a similarly tight correlation, but this time without quasi-periodicity.

Fig. 4.13 shows a SI whistler and trailing echo that propagated in a region of irregular density in the PBL A closely following photometer event was indicated by a VCO upsurge. This timing relationship appeared repeatedly during the observing session and supported an interpretive model in which a whistler originating in the northern hemisphere reflects back northward after arriving over Siple. The electrons it scatters were predicted to arrive in the time period marked on the record. Note the quieting in background hiss that occurred for about 10 s following the whistler. This was a new observation that remained to be analyzed in detail.

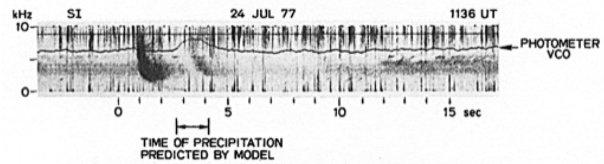

Figure 4.13: Whistler trailed by echo, hiss suppression, and surge in optical emissions. From *Helliwell et al.* (1980).

4.2.2 Inan initiates simulations of electron precipitation near L=4 by coherent waves

In 1978 Umran Inan took steps on a path that led to his later overall leadership of the Stanford VLF group. With Bell and Helliwell he authored a paper based on his PhD thesis entitled "Non-linear pitch angle scattering of energetic electrons by coherent VLF waves in the magnetosphere" (*Inan et al.*, 1978). There had been other work on scattering induced by whistler mode waves, but this case was new in its emphasis on coherent waves as drivers and upon the difference between linear and non linear behavior of the scattered electrons.

The paper focused on conditions along the L=4 field lines of propagation from the Siple VLF transmitter, employing the full nonlinear equations of motion of electrons interacting with longitudinally propagating whistler mode waves. A test particle approach allowed study of the full distribution of particles. Results were shown on pitch angle scattering as a function of particle pitch angle, cold plasma density and wave amplitude. The findings were compared with results of linear theory and a quantitative criterion was developed for the applicability of linear theory in any particular case. For example, it was found that non linear effects become important for driving wave amplitudes as low as 3 mg in the case of a 5 kHz wave near the equatorial plane at L=4.

It was shown that there is significant leverage in the energy of the particle precipitation process. The energy density of the precipitated particle flux can exceed that of the wave by 50-60 dB. Furthernore. the calculations predicted significant enhancements in the density of the D and E regions of the nighttime ionosphere.

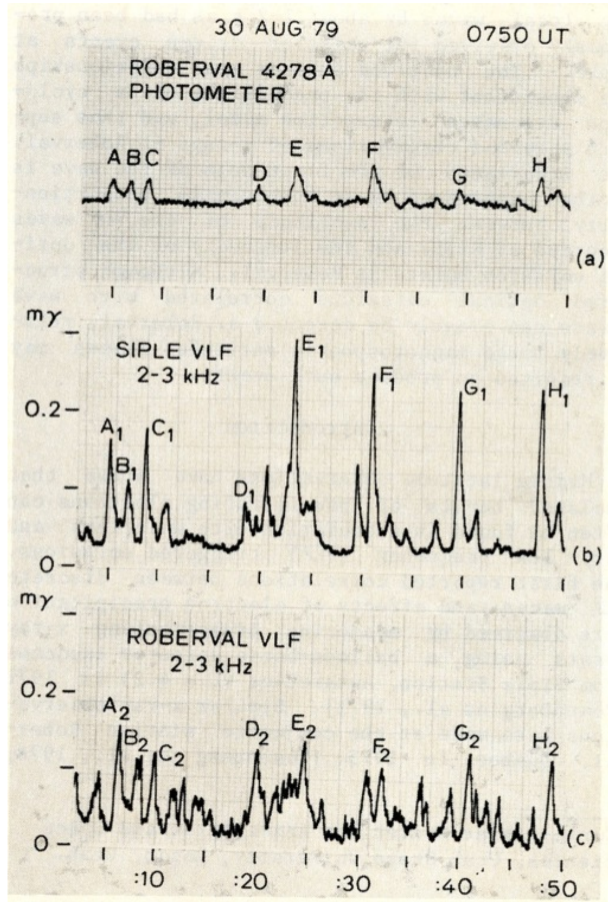

Figure 4.14: Correlated 4278A emissions at RO and 2-3 kHz whistler events at SI and RO. From *Doolittle and Carpenter* (1983).

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4.2.3 Doolittle finds evidence of whistler induced $\lambda 4278$ emissions at RO. From *Doolittle and Carpenter* (1983).

An important extension of VLF wave-optical correlations was achieved at Roberval in 1979 when close temporal relations were observed between λ4278 optical emissions at RO and whistler wave bursts at both RO and Siple (*Doolittle and Carpenter*, 1983). Fig. 4.14 shows the event on simultaneous charts of RO optical emissions (top), a RO chart of 2-3 kHz wave amplitudes and a SI chart of 2-3 kHz waves (bottom). A key element in this event was the high level of preceding geomagnetic disturbance, with Kp = 8. This was apparently sufficient to overcome the difference in particle mirror heights between the northern and southern hemispheres that had previously been found to favor observations of such correlations at Siple. It was found that precipitation into the ionosphere over RO had been "direct" in terms of the timing between the optical flash and the southward progress of a strong locally launched whistler. For example, in Fig. 4.14, optical event E at RO occurred close to the time of wave echo E1 at Siple but well before the arrival at RO of wave echo E2, which under less disturbed conditions might have been expected to induce only an "indirect" precipitation event at RO (see *Helliwell et al.* (1980)).

4.2.4 Correlated effects observed during the 1980-81 rocket balloon campaign at Siple. From unpublished notes by observer D. L. Carpenter

During a high altitude balloon flight on Dec. 30, 1980, a close correlation between VLf chorus bursts and balloon X rays was observed. Very large peak to valley ratios of X rays were noted. The chorus tended to range over several kHz in frequency and showed quasi-periodic rising character at times. Microburst structure was evident in both the VLf and the balloon X rays.

An apparently new phenomenon was found, evidence of triggering of chorus elements near 2 kHz by whistlers propagating beyond the plasmapause and only well-defined below $\simeq$ 2 kHz. The whistlers sometimes occurred at high rates, of order several per second. The effects appeared on a number of days, often near local midnight and thus well away from the noon peak in noise band and Siple transmitter echo activity.

Part 5

Studies of Wave-Particle Interactions in Space

5.1 Early discussion of ways to perturb the "hot" plasma of the magnetosphere

5.1.1 The geocyclotron

In the 1950s there was much interest in the effects of nuclear tests on the Earth's radiation belts (and as noted elsewhere, the VLF group took part in observations of VLF radiation from these tests). Early on, *Helliwell and Bell* (1960) recognized that through gyro-resonance with waves injected from ground based transmitters it should be possible to substantially accelerate electrons trapped in the Earth's radiation belts. But they also realized that in order to achieve the necessary synchronism between the electric field of the injected circularly polarized wave and the spiraling motion of a resonant particle, the conditions for resonance would vary in a complicated manner

Calculations showed that the proper candidates for acceleration were relativistic electrons with mirror points close to the equator. The relativistic mass of an interacting electron could be steadily increased provided the wave frequency were reduced in the proper manner (otherwise the increase would occur only in an oscillatory fashion). In this way it should be possible to "increase the number of high energy electrons in the radiation belts by an order of magnitude!" A comparison of the potential acceleration by a ground transmitter over a six day period and the particle energy injected by the Argus test experiment in 1955 revealed a comparable increase. It was suggested that such experiments could be used to study the efficiency of acceleration, the lifetimes of injected particles, the shape of the earth's magnetic field, the distribution of particle mirror points, and related questions. Further, it offered chances to study whistler propagation, including the then-emerging ideas of ducting and the fact (little exploited since) that the falling-tone nature of whistlers is roughly similar to that proposed for the geo-cyclotron experiments. The authors wrote "it may be that the absorption of whistler energy accounts for a substantial fraction of the observed high energy particles."

In the final paragraph of their letter to JGR, the authors said that "the proposed geo-cyclotron experiment appears to provide a practical means for trapped-particle experimentation without the hazards and difficulties encountered in using nuclear detonations."

5.1.2 The inverse geo-cyclotron

After the publication in 1960 of the geo-cyclotron paper , which suggested a way of adding to the relativistic radiation belt populations, there was a continued period of international tension that in 1962 included the Cuban Missile Crisis and in July of that year the Starfish Prime high altitude nuclear test over the Pacific (see Section 1.6.1). It is therefore not surprising that in 1964 a letter by Tim Bell appeared in JGR suggesting a way to invert the geo-cyclotron acceleration process and to use wave injection from the ground to decelerate electrons of MeV energy (*Bell*, 1964). In this case, the interacting waves would increase in frequency in a prescribed way, rather than decrease as proposed in the 1960 geo-cyclotron letter.

Bell's note repeated the formulas that had been worked out in an Air Force sponsored study to express the rate of particle deceleration in the interaction region, noting that the deceleration occurs when the particles are trapped for a time in the potential well of the wave. In the geo-cyclotron case, the relativistic mass of the particle is increased as the interacting wave frequency decreases to maintain synchronism with the particle spiral motion, while with the inverse the opposite occurs as the wave frequency rises.

Bell discussed various aspects of the interaction region that were not yet well known, as well as the time of operation that would be required to reduce the energy of particles by 1/2 within a band extending originally from 1.7 MeV to 2.2 MeV.

His discussion touched on a familiar theme for the times, one brought up by comments such as those by *Dungey* (1963) that through incidental gyroresonance a whistler mode wave can cause random perturbations in the radiation belts as opposed to the focused deceleration contemplated here. Neither the geo-ccyclotron nor the inverse geo-cyclotron were discussed in later years within the VLF group, but the idea of wave injection to remediate large increases in hot plasma activity in space was not forgotten and as of 2014 is projected to be realized through transmissions from spacecraft in the next few years.

5.2 Early Development of Stanford Wave-Injection Experiments

In the final years of his life, Bob Helliwell worked on a new monograph that would be devoted largely to wave injection from the experimental Zeus and Jupiter transmitters at Siple Station, Antarctica. In discussing the history of this work, he pointed to the 1956 recordings near Cape Horn of magnetospheric signals from the NSS transmitter. As noted in Section 1, those recordings had demonstrated the potential usefulness of VLF transmitter signals for study of magnetospheric propagation.

The evidence that the Navy transmitters were also triggering VLF emissions had come within only a few more years.

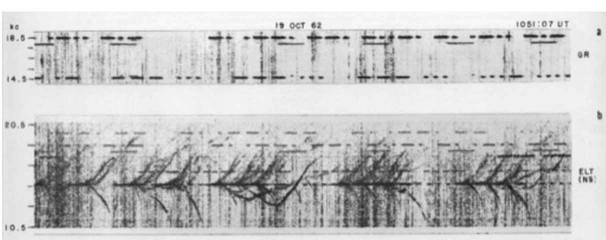

Figure 5.1: Whistler-mode echoes and triggered emissions from the U.S.Navy 14.7 kHz transmitter at Cutler, ME in 1962. (Above) Receptions at Greenbank W.V; (below) receptions on the ship Eltanin in the conjugate region. Adapted from *Helliwell et al.* (1964).

In 1961, when Mike Trmipi was in the Stanford lab monitoring a 1959 tape from Wellington, NZ, he heard high frequency squeaking sounds. On the spectral records the "events" appeared to be triggered emissions from the 18.6 kHz Navy transmitter at Jim Creek, WA. This news excited Bob Helliwell, who asked Mike to further analyze the data. It soon became clear that the $\approx 145ms$-long Morse Code dashes were doing the triggering,

Then on October 19, 1962, Morse code signals at 14.7 kHz from the Navy transmitter NAA at Cutler, Maine were found to trigger strong VLF emissions. The signals were received on board the U. S. Antarctic research ship Eltanin as it was cruising near the Palmer Peninsula (*Helliwell et al.*, 1964). The triggered emissions were in various forms, mostly tones rising in frequency with time, but also falling tones and hooks that fell and then rose (Fig. 5.1).

Details of the emission mechanism were not yet understood, but were assumed to involve a nonlinear interaction between the wave packets and the medium. In earlier work by *Gallet and Helliwell* (1959), Helliwell had sided with Gallet in considering the operative mechanism to be selective traveling wave amplification. Now in 1964 he favored as a mechanism the transverse instability as it had been suggested by *Brice* (1964) and by *Bell and Buneman* (1964).

In their discovery paper, *Helliwell et al.* (1964) pointed out that "because of the reproducible and quantitative nature of these observations, we suggest that they could probably be extended through the planned use of man-made triggering signals operating at substantially lower frequencies (about 5 kHz)."

5.2.1 Kimura's surprising report on emission triggering by a $\simeq$ 100W transmitter

A new dimension was added to the story of artificial triggering when visitor I. Kimura reported examples of spectral broadening and triggering by 1-s 10.2 kHz pulses from the $\simeq$ 100 W Omega transmitter at Forest Port NY (*Kimura*, 1968). Several synoptic recordings of this remarkable effect had been made at Eights, Antarctica in 1963 by Mike Trimpi. Comparison of the data from Forest Port with that from NAA, radiating 1 MW and at roughly the same latitude, suggested that NAA, with its more frequent triggering, was exciting paths with endpoint latitudes distributed more widely in the equator-ward direction. The paths for the Omega signals were found to follow field lines close to those on which 10.2 kHz corresponded to half the equatorial gyro-frequency. Attention was paid to the occurrence of occasional Omega triggering well after sunrise in the hemisphere of the transmitter. This implied loss of as much as 20 dB by the injected input signal and suggested that its amplitude was "not a primary factor" in the triggering mechanism.

Fig. 5.2 shows spectrograms from an Omega triggering event detected at Eights on May 7, 1963 near 1150 UT or ∼ 07 LT. Frequency is displayed from 5.0 to 15.5 kHz, allowing Morse Code from NAA at 14.7 kHz to be seen near the top of the record. Faint rising triggered emissions from the 10.2 kHz Omega signals appear at ∼ 37 and 39.5 s on the bottom record. An elegant multipath whistler, used to identify the path of the Omega triggering signal, appears near 27 s on the upper record. Very weak falling tones from the NAA signals can be seen near 28 s.

5.3 Additional evidence of emissions triggered by VLF transmitters

In 1969 three brief reports on Artificially Stimulated Emissions were published in JGR. Heavily based on the broadband recordings at Eights in 1963 and 1965, they were rich in details of the triggering of emissions by Morse Code signals from NAA, originally operating at 14.7 kHz and later at frequencies several kHz higher. In the first paper, Steve Lasch provided new statistical support for the earlier assertions by *Helliwell et al.* (1964) concerning the restriction of strong emission triggering to the $\simeq 150ms$ Morse Code dashes as opposed to the $\simeq 50ms$ dots (*Lasch*, 1969). Fig. 5.3 shows dynamic spectra from SES in the north above the associated spectrum from Eights. The SES record has been shifted in time (arrow) so as to align the dash-dot patterns at the two locations. The dots consistently launched very weak falling tones, while the dashes grew, broadened in frequency, and then triggered both strong fallers as well as risers. The triggering of falling tones by the dots was itself rare; it was found on only 0.3% of the cases, usually times of very strong ASE activity.

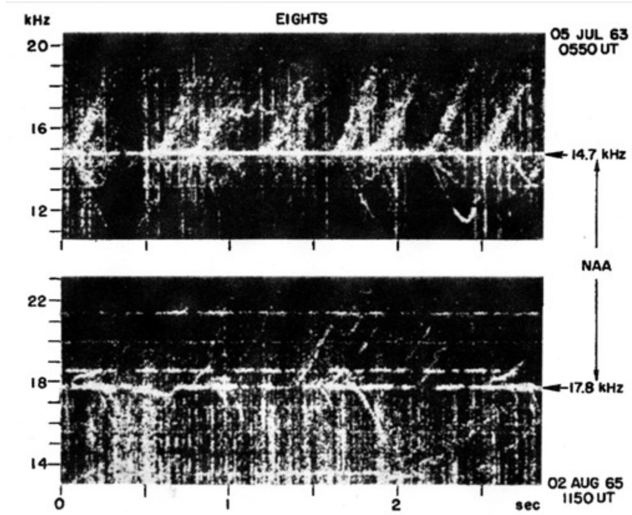

Figure 5.4: Eights 1963 and 1965 records showing triggering by NAA at 14.7 kHz and 17.8 kHz. From *Carpenter and Lasch* (1969).

In those days we were studying the dependence of the production of ASEs on the ratio of the triggering frequency to the equatorial gyrofrequency along the field-line path, f_{tr}/f_{heq}. We were measuring propagation path drifts and asking questions about variations in ASE activity with transmitter frequency. In one paper we described a notable drop in the occurrence and complexity of ASEs when NAA raised its frequency from 14.7 kHz to 17.8 kHz (and later to 18.6 kHz) (*Carpenter and Lasch*, 1969). Our guess was that the preferred path entrance points of the signal had on average moved closer to the earth by $\Delta L \simeq 0.2$ and, more importantly, that the new paths threaded regions of the radiation belts less well supplied by particles capable of resonance with the waves. Another paper described ASEs that occurred along a duct that had been drifting inward, but which only appeared when the fixed transmitter frequency fell to $\simeq f_{heq}/2$ in the drifting duct (*Carpenter et al.*, 1969).

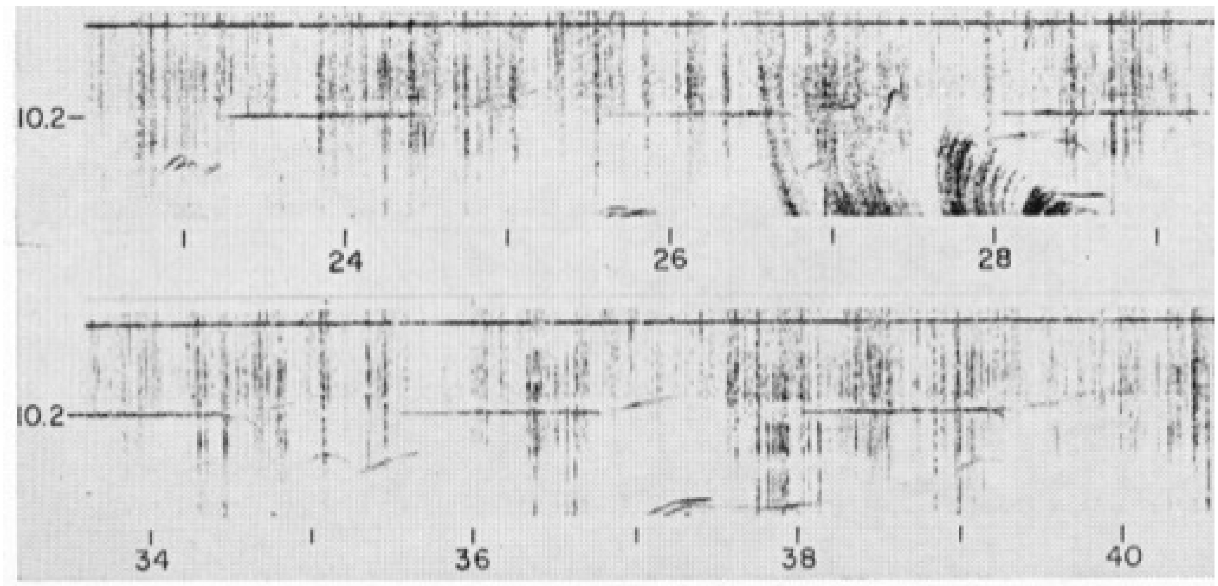

Figure 5.2: Spectrogram displaying 5 kHz to 15.5 kHz at Eights on May 7, 1963, showing an example of emission triggering by the 10.2 kHz Omega signal from Forest Port, NY. From *Kimura* (1968).

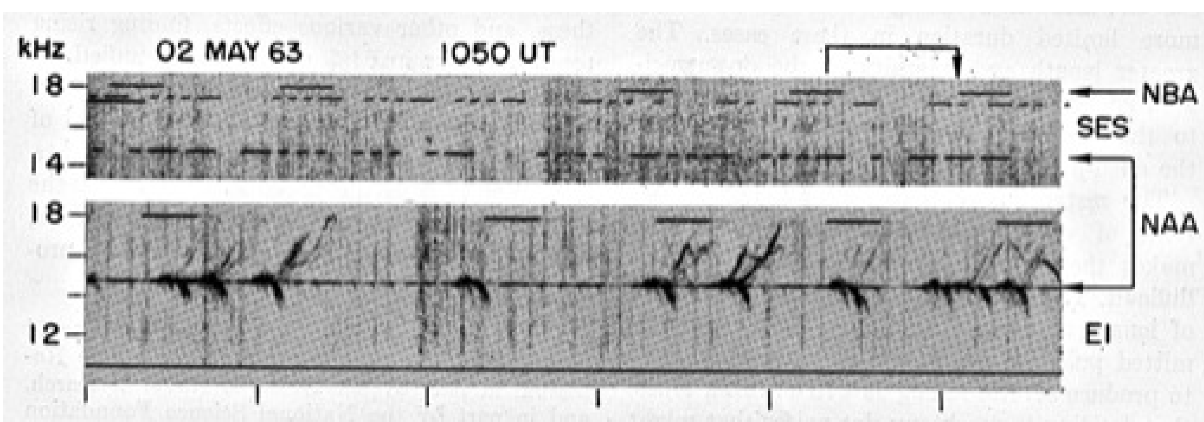

Figure 5.3: Simultaneous records from SES station in Canada and Eights in Antactica, showing triggering by Morse Code dashes but not dots. See text for details. From *Lasch* (1969).

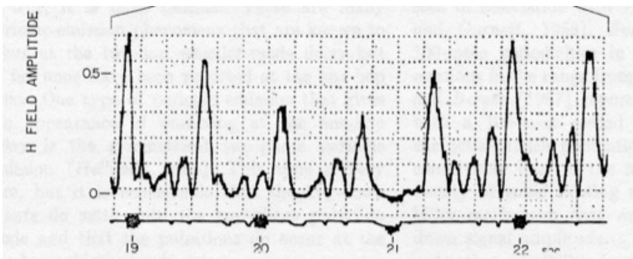

Figure 5.5: Eights recording of the relative amplitude of the NAA keydown signal at 17.8 kHz. From *Bell and Helliwell* (1971).

5.4 Pulsations on keydown NAA signals

After fixed frequency Morse-Code transmitter signals had been found to trigger emissions there was understandable curiosity about what the response would be under key-down conditions. A study was therefore made of seven occasions of such 14.7 and 17.8 kHz signals that were detected at Eights in 1963 and 1965 and lasted up to 2 min (*Bell and Helliwell*, 1971). On three of these occasions only the ground wave was detected, while on four others the magnetospheric whistler mode signal was also observed after a propagation delay of $\simeq 0.5s$. That component was found to pulsate with a period corresponding to the one hop whistler mode delay on the path, or around 0.5 s. Fig. 5.5 provides a relative amplitude record from Sept. 12, 1965 of the best defined of these cases. The pulsations involved increases in amplitude and bandwidth of the signal, but did not trigger emissions. Weak triggered emissions were found between the pulsations, but were evidently not part of the pulsation process itself.

The mechanism controlling the pulsation period was not understood. It seemed related to the one hop whistler mode delay, but other possible factors included oscillations in the energetic particle population, Pc! micro-pulsations, or some intrinsic oscillation in the emission generation mechanism.

5.4.1 Questions about the damping of whistlers by hot plasmas

From our early days we were aware of the effects on whistlers of two regimes, the dense space plasma through which they propagated and the tenuous hot radiation belt plasma with which they seemed to interact in ways that affected their amplitude spectra and might lead to triggering of new emissions.

We were aware that when the whistler nose was inducated on a spectrogram, there tended to be a fairly well defined upper amplitude cutoff. Bob Smith, using ray tracing within a whistler duct, had predicted a cutoff of whistlers above the minimum value of the gyrofrequency f_H along the field line (*Smith*, 1961). Meanwhile, several physicists interested in space plasmas were paying attention to the whistler upper cutoff, and in the early 1960s there appeared a number of papers on hot plasma effects, including *Liemohn and Scarf* (1062), *Scarf* (1962), *Guthart* (1964, 1965a,b). Some of these works were directed toward explanation of the whistler amplitude cutoff as damping effects imposed by hot interacting particle streams. The *Guthart* (1964) work was actually part of a thesis completed under Helliwell's supervision. In a 1965 paper *Guthart* (1965a) studied whistler data from Eights and found that no measurable departures from cold plasma dispersion could be identified above the whistler nose. From the measurement limits he was able to estimate the maximum magnetospheric temperature involved to be $2 \times 10^{4^\circ} K$.

During our early work with IGY whistler data recorded at middle latitudes, travel time measurements had often been made at 5 kHz on records such as that of Fig. 1.14, on which whistler energy near and above the nose was rarely seen. Once in a while, whistlers with well defined nose frequencies above 10 kHz appeared, such as the elegant 1959 example from Seattle in Fig. 1.19. Our occasional data from Byrd had also given us hopes for more high-latitude cases, hopes that were richly fulfilled with the arrival of Mike Trimpi's 1963 data recorded at Eights, Antarctica. Our attention was quickly drawn to the upper cutoff details of Eights whistlers, an example of which is shown on the upper panel of Fig. 5.6. This is the result of a single lightning flash, exciting several

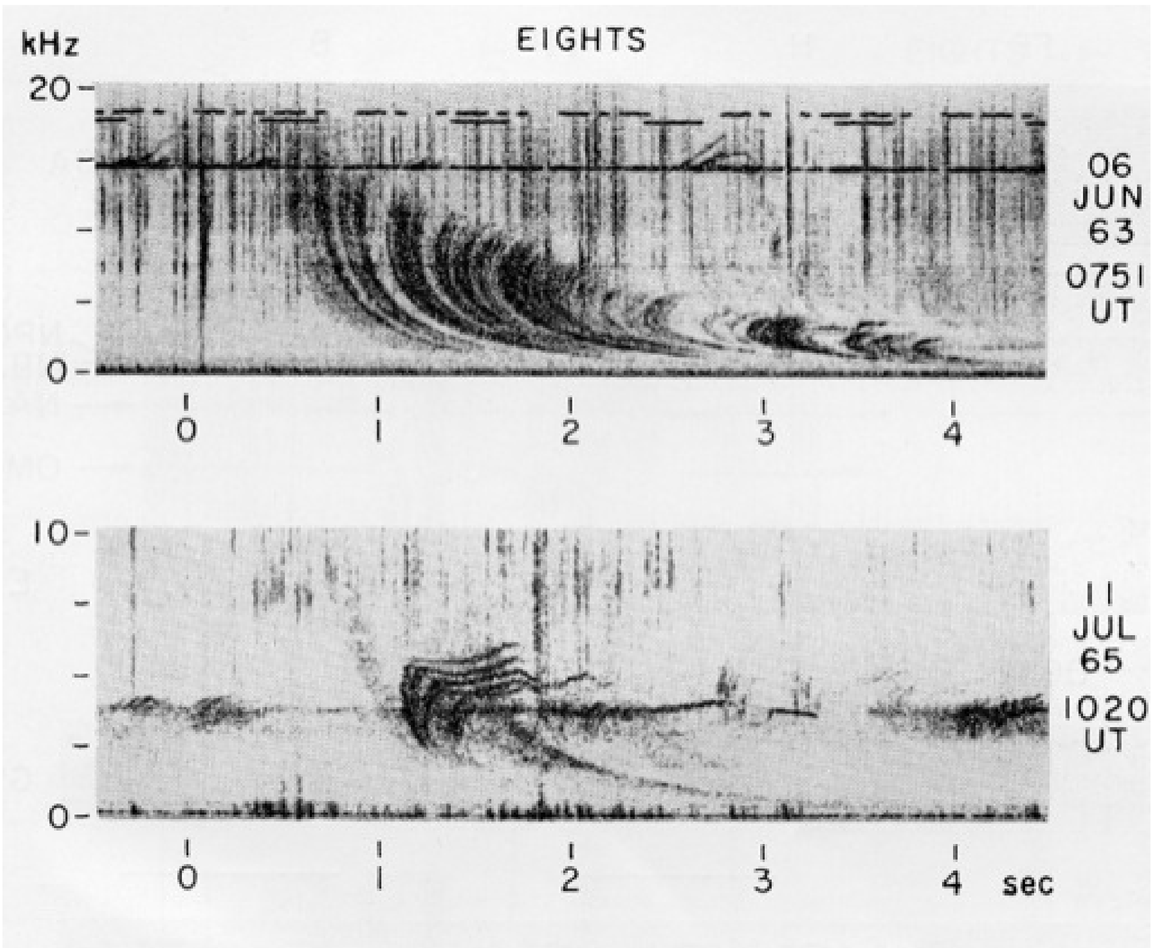

Figure 5.6: (above) A whistler exhibiting upper intensity cutoff at the same normalized frequency on many spatially distributed paths. (below) Whistler with several closely spaced components that trigger emissions. From *Carpenter* (1968).

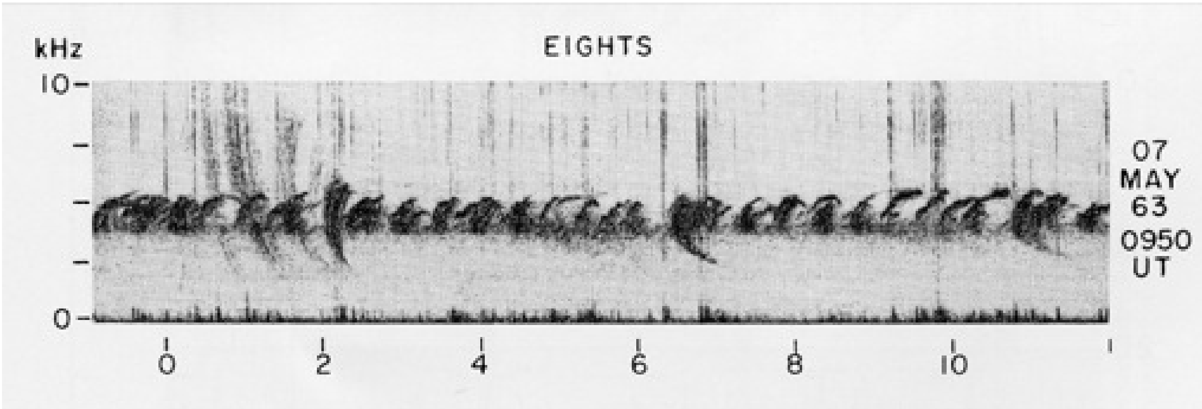

Figure 5.7: Whistler echoing with train of periodic emissions From *Carpenter* (1968).

dozen field aligned paths with upper cutoff frequencies ranging from above 16 kHz down to $\simeq 1.5kHz$.

Other high-frequency effects were well defined, such as triggering on multiple paths in the lower part of Fig. 5.6. At each of several upper cutoff frequencies a $\simeq 1s$ coherent tone is triggered. The paths were apparently separate from one another, that is, were in different ducts, but each triggered tone followed a simple slightly wave like frequency pattern and they seemed to refrain from crossing one another in $f - t$ space. The triggering components appear to have been in the region of the plasmasphere boundary layer, a region where strong whistler activity is often observed. In this case they exhibited high intensity near and above their nose frequencies and were part of inter-hemispheric echoing noise activity near 4 kHz.

In Fig. 5.7 a single whistler component echoed strongly and was clearly on an echoing path followed by periodic emissions, which appear to have included as many as 9 major elements or phases. In studying whistler upper cutoff effects, we thus see evidence of propagation in both cold and hot plasma regimes. The upper panel of 5.6 appears to be dominated by the cold plasma issue of untrapping from ducts at 0.5 f_H, while the other panels show both a tendency for wave growth near and above the nose frequency as well as triggering by the sloping wave as it becomes cut off in amplitude.

5.4.2 Harold Liemohn and Fred Scarf

Since the work of Owen Storey in 1952 there had been a fascinating series of discoveries about both whistler mode propagation effects dominated by the cold plasma and those strongly affected by the radiation plasmas. One of my greatest pleasures as a new student in these areas was the opportunity to meet and get to know Harold Liemohn and Fred Scarf, two of the advocates for hot plasma control of the whistler upper cutoff. Both men came to Stanford at some time in the mid-1960s to visit us and hear what we had to say. Sadly, I don't remember the details of our discussions, but believe that in the end, with data such as that of Fig. 5.6, our belief in Smith's 1/2 f_H untrapping cutoff carried the day. I became a good friend of both Harold and Fred and in later years enjoyed very much their company whenever it became possible. Sadly, Fred passed away over 20 years ago, but hankfully magnetospheric physics is now blessed with the active participation of Mike Liemohn, Harold's talented son.

5.4.3 Diem Ho and his contributions

Diem Ho came to us from Viet Nam in the late 1960s. His efforts were concentrated in the area of quasi periodic emissions and the excellent data on them recorded at Eights in the year 1964-65. He was aware of the way that whistlers could suddenly perturb the

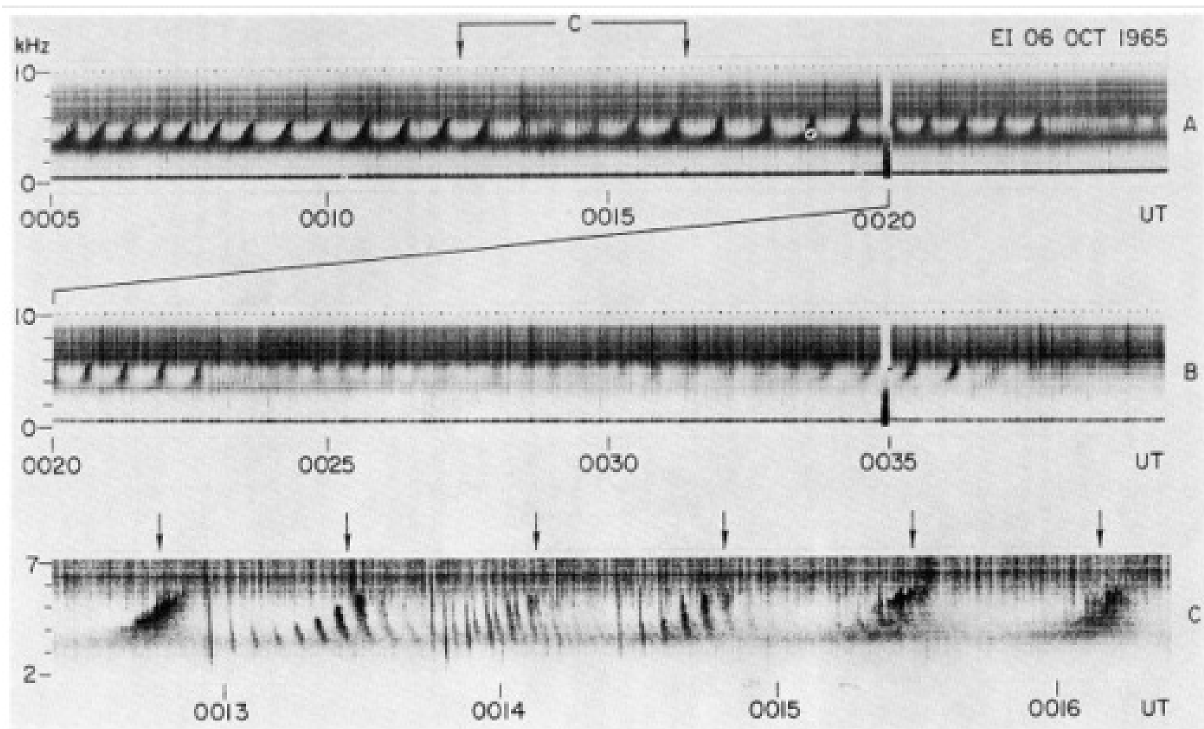

Figure 5.8: Spectrograms showing perturbations of QP activity by whistlers. From *Ho* (1973)
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magnetospheric hot plasma through the particle scattering that led to X ray deposition and to Trimpi events, but now looked at whistler paths where QP emissions had been established for periods of an hour or more, finding evidence of much more gradual perturbations of the hot plasma by whistlers.

Fig. 5.8 is an example of QP activity on a path near L=4, typical of events that could be seen near the equinoxes on magnetically quiet days in the late afternoon. Frequencies were usually in the range 1.5-5 kHz, and quasi-periods were near 30s. Diem noted a variety of ways in which echoing whistlers could modify existing QP activity. Panel B, continuing the panel A data from 0020 UT, shows a whistler ar 0023 UT interrupting QP emsissions for about 10 min. As the QP activity was recovering, a whistler at 0035 UT effectively terminated it. Meanwhile, Panel C, on an expanded time scale, shows a several minute interruption of QPs by echoing whistlers at 0013 UT. Among the effects Diem observed were changing of the QP period, stopping of the QP action altogether, or changing of the fine structure of the QP, which usually consisted of periodic emissions (*Ho*, 1973, 1974).

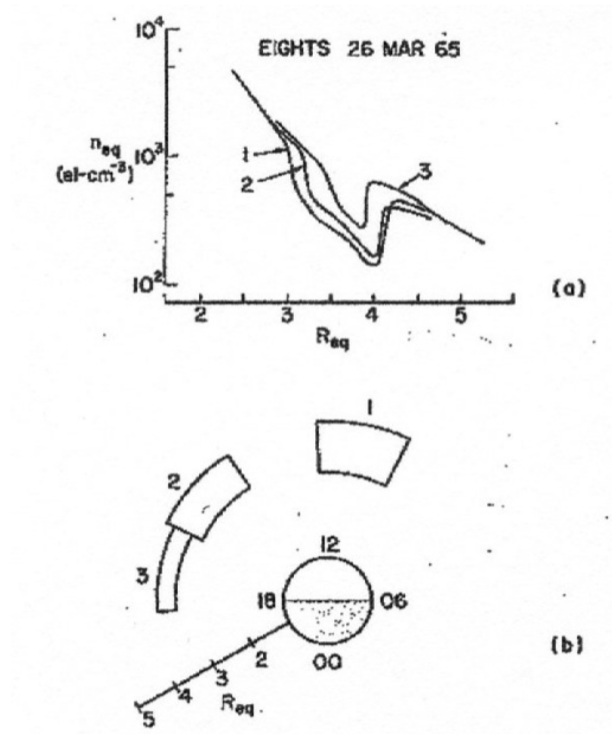

Figure 5.9: Inferred Eights equatorial profiles during a recovery period in 1965. From *Ho and Carpenter* (1976)

5.4.4 Diem Ho on the subject of plume-like irregularities outside the main plasmasphere

There was a special advantage to a whistler as a probe of density irregularities, namely its collection of information from a range of equatorial distances inside of synchronous orbit. Satellites before IMAGE in 2000 could uncover density details along their specific orbits, and by 1974 this had led to important knowledge of the tail-like density plumes that tended to be drawn away from the main plasma-sphere during disturbance and then permitted to co-rotate with the earth during quieting. Diem did some excellent work on the ongoing presence during quieting of an outlying density feature that was rotating or nearly rotating with the earth (*Ho and Carpenter*, 1976). Fig. 5.9 shows three sets of measurements of Eights 1965 whistlers, at local times extending from late morning to early evening. The solid curves are estimates of the density profiles to be inferred from the individual dot measurements. The data represent a quieting period during which the gap narrowed between the main plasmasphere and an outlying plume region.

5.4.5 Diem's difficult life after returning to Viet Nam

In 1974, while Diem was finishing his work at Stanford, there developed some issues about his passport and he was forced to return to Vietnam. This happened only months before the fall of the government in Saigon and the takeover of that city by the North Vietnamese. In the months and years that followed 1974, we were regularly reminded of Diem's plight by Nate Seely, who led an ongoing campaign to help Diem out of what appeared to be a dire situation.

I heard then or later that Diem, whose father had previously been in the Vietnamese banking industry, was being forced to live and work as a questionable political subject.

In any case, but fortunately for Diem, his wife Mai had acquired French citizenship, and both she and he were able to go to France around 1984. It was in Paris that I found him in 1991, roughly 17 years after our last encounter at Stanford. I was delighted to see that he was successfully working for IBM, a company that had briefly employed him in Saigon in 1974. He claimed that his success in launching new projects at IBM came from his graduate experience at Stanford. At first I found this hard to believe, but in the light of several several later visits, have come to accept it.

Sadly, my visit to Diem coincided with word about the killing by a disturbed graduate student of several senior people at the U. of Iowa.

5.5 Beginnings of controlled VLF wave injection experiments from Antarctica

5.5.1 Initial emplacement of a long wire antenna

The next step toward the dream of VLF wave injection from Antaartica, discussed in a paper by *Helliwell and Katsufrakis* (1974). was "construction of a 33.5 km horizontal antenna at Byrd Station (L=7.25) in 1965 (*Guy*, 1966)." Two transmitters were installed at a site called Byrd Longwire, about 3 miles from the main station. A U. of Washington group under Myron Swarm, Don Reynolds and Bill Guy, supplied a system that could be switched to a range of fixed frequencies, while Stanford, through arrangements with the Navy made by John Katsufrakis, supplied a 100 kW high efficiency solid state system. "The dipole was laid on the ice so as to lower its Q and increase its bandwidth. Soundings of the D and E regions were successfujlly carried out over the frequency range 3-30 kHz (*Helms et al.*, 1968). Whistler mode transmitter signals in the 6-9 kHz range were observed by the Stanford receiver on the OGO 4 satellite as it flew over the transmitter in the upper part of the F region but they were too weak to excite detectable whistler mode signals in the conjugate hemisphere."

In a later paper *Helliwell and Katsufrakis* (1978) explained that "as the snow cover increased (loose snow often blows from one horizon to another within a layer extending about a foot above the ice or snow underfoot), differential movement of the ice sheet caused frequent breaks in the wire, making regular operation impossible." Because of the high latitude ($L \approx 7$) and the low efficiency of the wire, which was in direct contact with the ice, no whistler mode echoes were detected on the ground at the northern end of the magnetospheric path, even when coherent integration was used.

In the 1960s it became clear that the VLF transmissions from Byrd Longwire needed to be moved to a lower latitude. This was accomplished after 1967 (see Section 3 on field operations), and in 1973 there began years of successful wave injection work at Siple Station, L$\simeq$ 4.2, $76°S$, $84°W$, conjugate to a receiver at Roberval Canada.

5.5.2 Raj Raghuram and the design of the first transmitting antenna at Siple

The Siple site was occupied for the first time in the summer of 1968-69, when a test dipole was erected over the snow (see Section 3 on Field Operations). Impedance measurements were made and used at Stanford by grad stidemt Raj Raghuram to design the full 21.2 km dipole that was constructed in the summers of 1970-71 and 1971-72, with an initial elevation over the snow of 16 m. The measured resonance frequency was 5.1 kHz, about half the minimum gyrofrequency of 6 kHz along the field lines at Siple. The estimated efficiency at 6 kHz was 4%.

5.5.3 Receptions of Siple signals in Quebec

There was great excitement when the first attempts were made to transmit from Siple. The first field-line

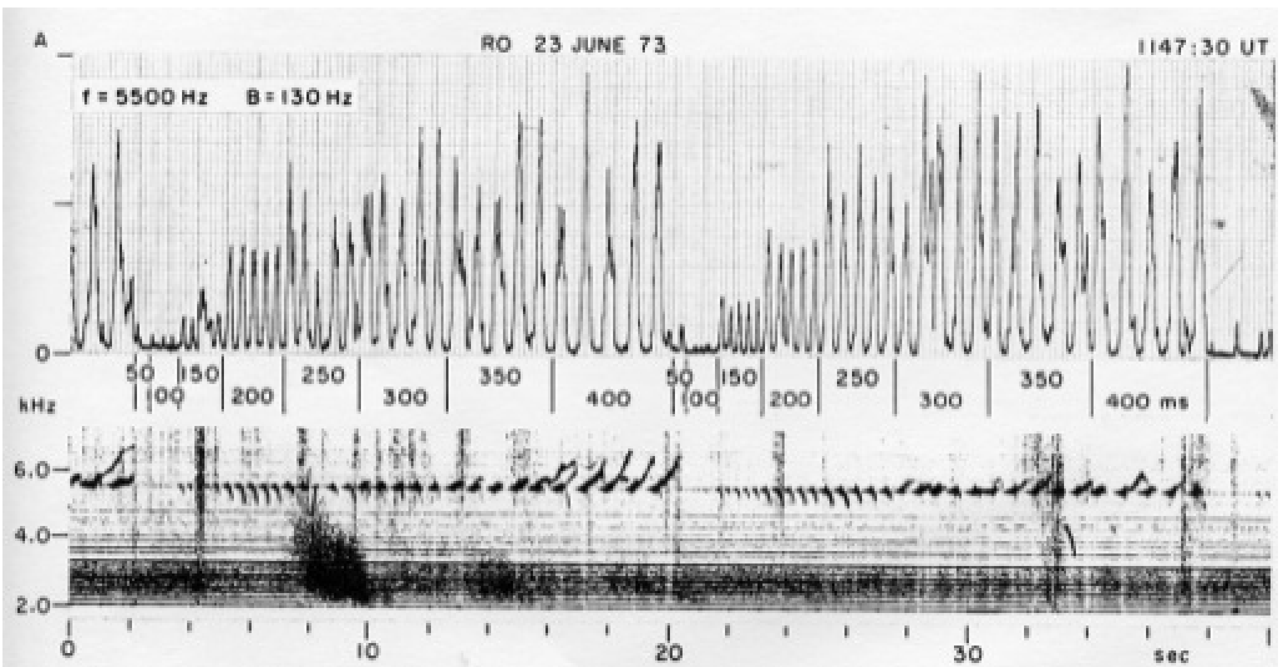

Figure 5.10: First Roberval data showing variations in Siple signal amplitude with pulse length as well as triggered emissions. From *Helliwell and Katsufrakis* (1974).

reception recognized in the northern hemisphere is recalled by Siple field engineer Ev Paschal as follows: one of the first of a series of transmissions was a hand keyed signal. NQU was used as the station call sign and the signal came out slowly using Morse Code, 0.5 s for a dot and 1.5 s for a dash to allow for dispersion in the magnetosphere. John Billey at Roberval could read Morse code, and John Katsufrakis may have been there as well. Conditions were just right one day and they saw the signal. Only later was it learned at what time during the day and at what operating frequencies would they have optimum transmissions. Ev had sent test tones earlier but this reception was unequivocal. It was in fact a test message with an embedded mistake. John Billey corrected the mistake, so it was clearly a Siple message. Ev had been doing this sending for a week or so, having also sent test tones.

5.5.4 An initial published report on Siple transmissions

The Siple transmissions became the great experimental triumph of group leader Bob Helliwell's life and were also a fine example of the work of John Katsufrakis, Stanford's field operations supervisor. Their mutual excitement at the first results was well communicated in a 'brief report' published in JGR in June, 1974 (*Helliwell and Katsufrakis*, 1974).

A notable feature of the first results was the lack of a "warmup effect". When echoes were being received, they started immediately, as soon as a pulse was sent under otherwise quiet conditions.

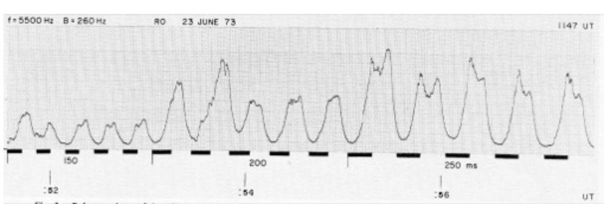

Figure 5.11: Received envelopes of reponses to five 150 ms, 200 ms, and 250 ms transmissions in the second set of Fig.5.10. From *Helliwell and Katsufrakis* (1974).

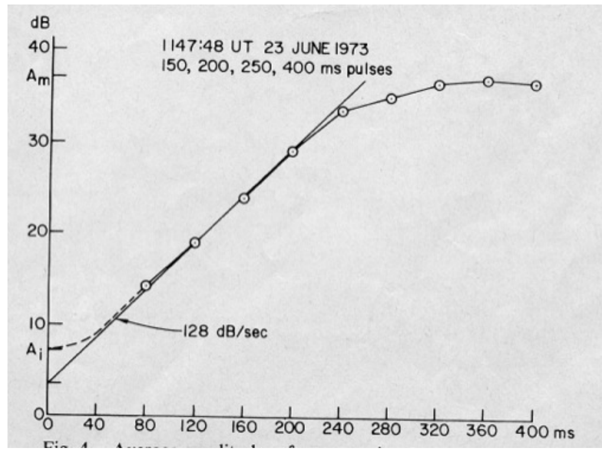

Figure 5.12: Average amplitude of the output pulse as a function of time after the start of the received pulse. From *Helliwell and Katsufrakis* (1974).

Two records from the report are particularly memorable. Fig. 5.10, shows the dot-dash effect, magnetospheric responses to the injection of groups of pulses of length varying from 50 to 400 ms in steps of 50 ms. The measured narrow band amplitudes of groups of five pulses at each length are shown above the broadband spectra. The pulses were formed by switching back and forth between 5.0 and 5.5 kHz, but in this case only the signals at 5.5 kHz were regularly received. The envelopes for the five 150 ms, 200 ms, and 250 ms cases in the second set of Fig. 5.10 are shown on an explanded scale in Fig. 5.11. Solid bars represent the transmitted pulses, corrected in time for a propagation delay at 5.5 kHz of 2.2 s.

The 50 ms pulses were too weak to be observed, but as pulse length increased, peak intensity grew at a rate of 128 dB/s as illustrated in Fig.5.12. There was a saturation effect; for pulse lengths of 350 and 400 ms, peak intensity did not continue to increase, indicating that saturation had been reached at an estimated 30 dB above the input level.

Triggered emissions first appeared at the end of the 150 ms pulses. All emissions were found to first rise in frequency, and then fall or rise depending upon the length of the triggering pulse.

The complexity of the wave growth and triggering

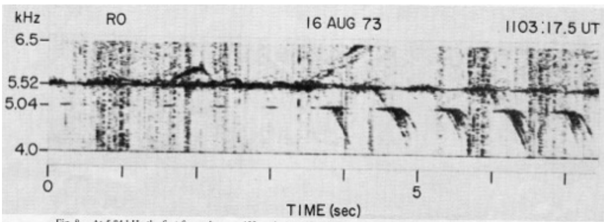

Figure 5.13: Effects of pulse length on triggering. From *Helliwell and Katsufrakis* (1974).

problem, or the dot dash anomaly, was illustrated in Fig. 5.13, which shows pulses at 5.04 and 5.52 kHz. At 5.04 kHz the first five pulses were only 100 ms in length and were separated by 600 ms. They failed to trigger, but the next 5 pulses at 5.04 kHz were 200 ms in length. Again separated by 600 ms, they launched multiple falling tones, probably on at least four separate paths. Note the change in slope of the triggered emissions at 5.52 kHz from rising to falling when the time separation of their originating pulses at 5.52 kHz increased from 100 ms to 200 ms,.

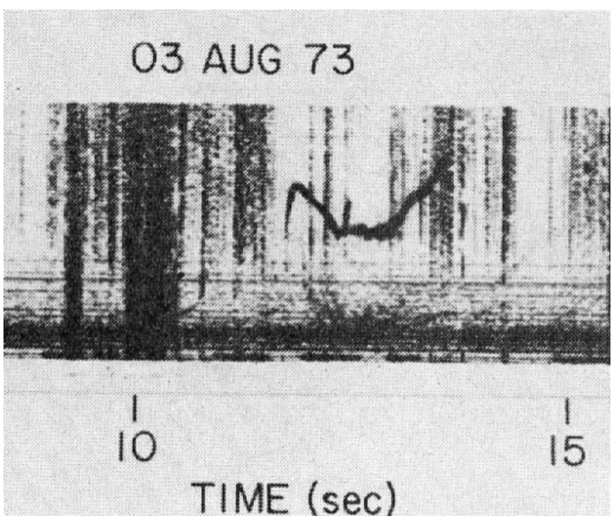

Figure 5.15: Falling-tone emission triggered by a northgoing whistler is entrained by a Siple pulse. Frequency scale is from 2 to 7 kHz. Siple transmissions were at 4.5 kHz. From *Helliwell and Katsufrakis* (1974).

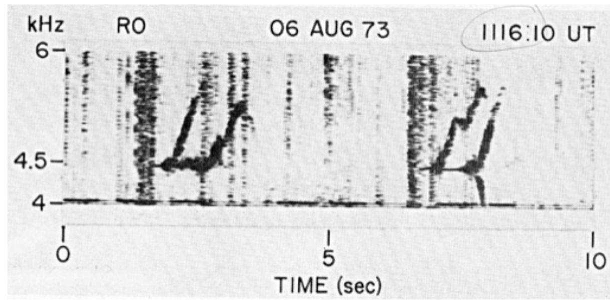

Figure 5.16: Siple pulses trigger rising emissions on two separate paths. From *Helliwell and Katsufrakis* (1974).

Another notable case is in Fig. 5.14, from 19 July 1973, a day of prolonged Siple activity. This record became a favorite of Bob Helliwell's because of his interest in the process of entrainment, by which an apparently weak coherent signal could somehow capture or modify the $f - t$ trajectory of a free running emission. Here the authors reported the behavior of triggered falling tones. In a few cases these tones became hook-like rising tones that "showed inflections and reversals in slope at power line frequencies, defined by the horizontal lines." Looking back to Helliwell's voluminous VLF spectral records in 1965, it was possible to see hints of such effects.

Another example of entrainment of an amplified triggered emission by a much weaker coherent wave is displayed in Fig. 5.15. 1-s Siple pulses were being transmitted every 5 s, and were on a ducted path followed by a whistler that was propagating northward after reflection in the south. The whistler triggered a falling-tone emission that by chance became entrained by a 1-s Siple pulse. The signal then continued at roughly the 4.5 kHz transmitter frequency, being briefly interrupted by the uppter part of another whistler. Once free of entrainment, the signal became a riser for $\simeq 1s$ while maintaining its high amplitude. The weakness of the entraining Siple pulse was strongly suggested by the recordings of weak Siple signals at the pulse frequency before and after this interaction.

Multipath propagation from Siple is illustrated in Fig. 5.16, when again a 1-s pulse was transmitted

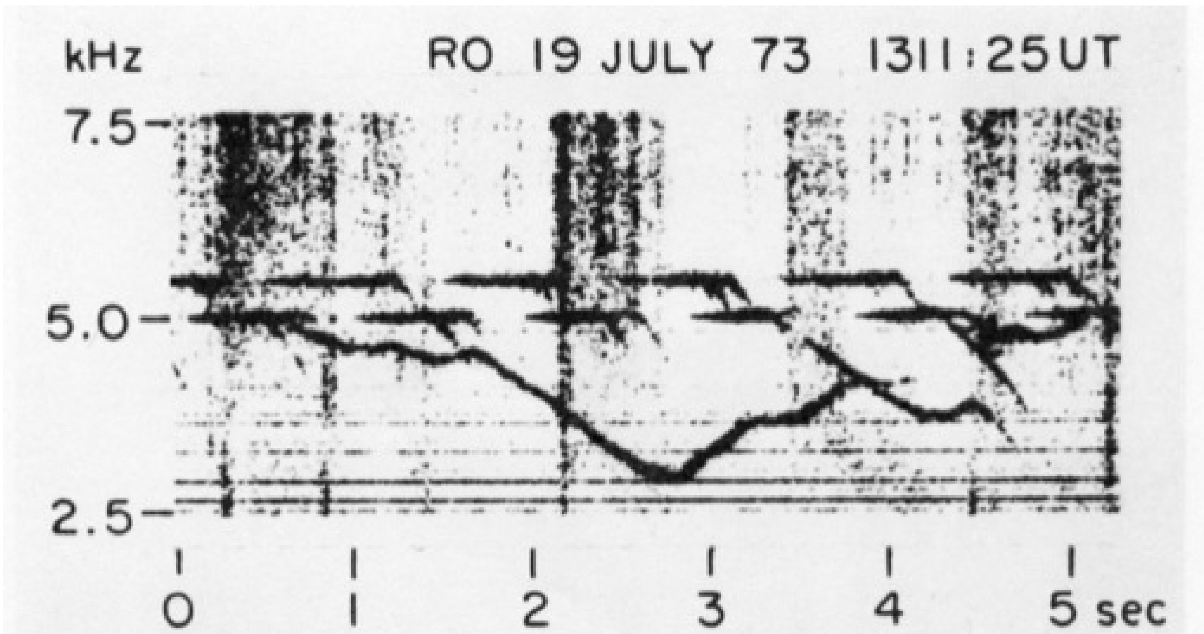

Figure 5.14: Triggering of falling tones with apparent interactions between hooks and power-line harmonics. From *Helliwell and Katsufrakis* (1974).

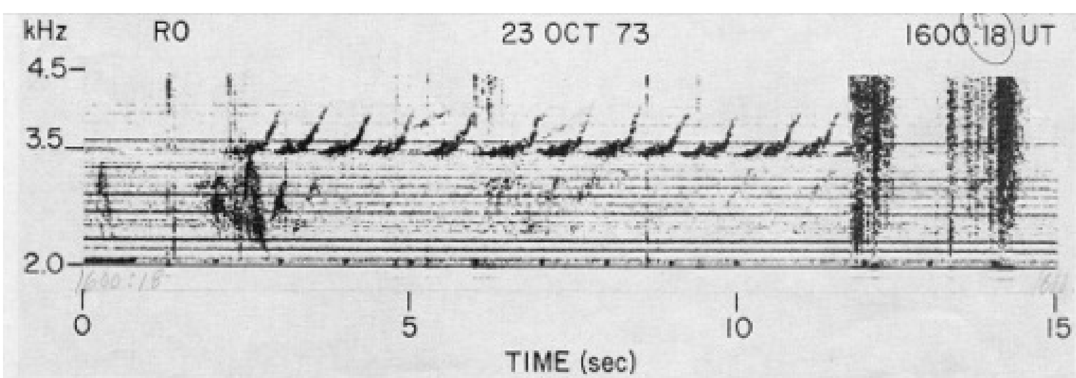

Figure 5.17: Pulsating response to a 10-s keydown transmisstion. From *Helliwell and Katsufrakis* (1974).

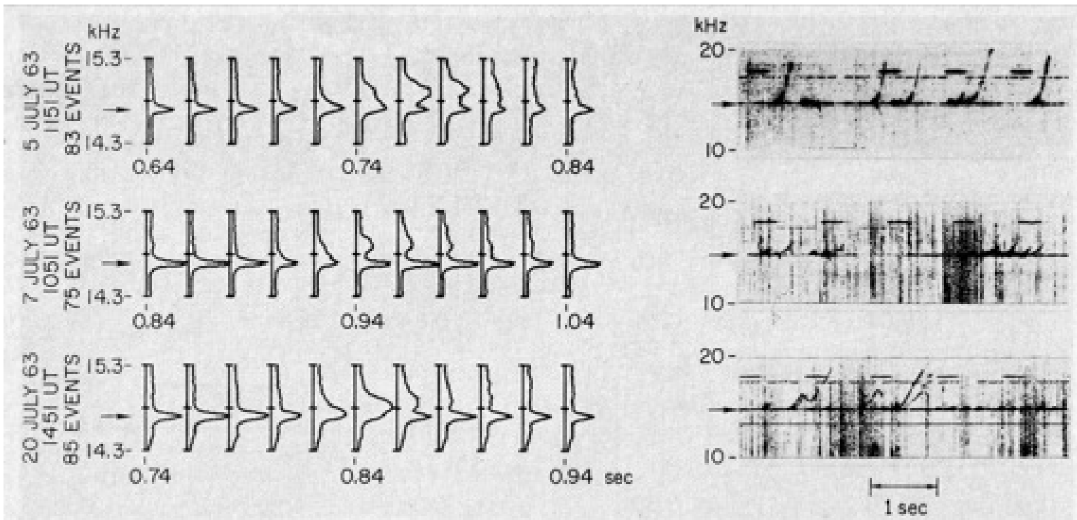

Figure 5.18: (left) Average digital spectra of ASEs triggered by NAA at 14.7 kHz on three days in July 1963. (Right) Analog spectrograms of examples from each of the data sets.. From *Stiles and Helliwell* (1975).

every 5 s. Each pulse triggered two strong risers, with well separated travel times.

An example of pulsations on a keydown signal is shown in Fig. 5.17, in which a 10-s signal at 3,5 kHz is displayed. At intervals of about 1 s, there was a triggered riser and suppression of the transmitter frequency. That frequency of 3.5 kHz then began to grow and launch a new riser.

5.5.5 Siple shares ducted paths with whistlers; its signals received on satellites and at ground stations

A two hop whistler appears at $\approx$ 8s on the spectrogram of Fig. 5.10. The authors suggest that its well defined path with an upper cutoff at 5.5 kHz and two hop delay of 8,2 s was the one followed by the Siple pulses. Signals were received on satellites Explorer 45 and IMP 6 (Roger Anderson and Don Gurnett, personal communication) and on ISIS 2 (Frank Palmer, personal communication). Subionospheric signals from Siple were detected at Halley Bay, Antarctica (K. Bullough, personal communication) and at Dunedin, N.Z. (R. Dowden, personal communication).

5.5.6 The initial application of digital analysis techniques to Siple data

One of the first grad students to study the Siple transmitter results was G. S. (Dyke) Stiles, who looked in some detail at the onset of several hundred emissions triggered by three transmitters, NAA at 14.7 kHz, Omega near 10 kHz and Siple at 5.0 kHz (*Stiles and Helliwell*, 1975). Stiles found that by digitzing the signal of interest in a band around the frequencies of interest, he could apply a Fast Fourier analysis to the data and display the resulting transforms at intervals of 20 ms, as shown in Fig. 5.18 for NAA in 1963. The effective filter bandwidth was estimated to be 45 Hz.

Dyke applied this analysis method to the initail parts of emissions on NAA at 14.7 kHz, Omega at 10.2 kHz, and Siple at 5.5 kHz for the case discussed above. He found that both rising and falling emissions began in the same fashion, starting at the triggering frequency.and not at an offset frequency as had been reported. The fallers reversed slope within a range 24-300 Hz above the triggering frequency, while the slope of the risers often leveled off in a $\simeq 40ms$

plateau and then rose again. Emissions stimulated by all three transmitters contained the same features, suggesting a lack of feature dependence on trnamitter power. The initial growth process was found to be asymmetric in frequency, with no evidence of initial growth below the triggering frequency.

Figure 5.19: Rising and falling ramps with format (above) shifted to agree with receptions. From *Carpenter and Miller* (1976).

5.5.7 Tom Miller and an occurrence study of Siple echoes

Tom Miller worked for us as a data aide through much of the 1970s. He was an avid cyclist, a capable data analyst, and a good friend,

Tom got to know the data well and played a major role in our first effort to describe the occurrence of detectable Siple signals at Roberval (*Carpenter and Miller*, 1976). Fig. 5.19 shows the sample spectrogram that appeared in our report on the activity. As we had seen in earlier work with whistlers and Navy transmitter pulses, the two types of signal tended to propagate along the same magnetospheric paths, their one hop travel times being the same at the transmitter frequency.

We looked at data from multi-month periods in 1973 and 1974, finding that echoes were detected at

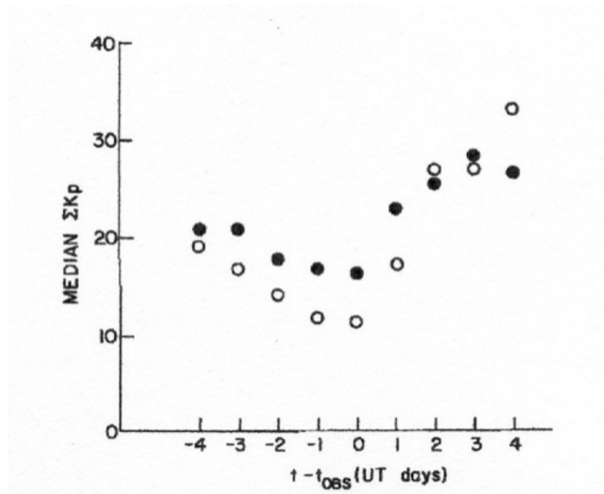

Figure 5.20: Daily Kp sums with respect to day of Siple signal detection at RO. From *Carpenter and Miller* (1976).

RO on 20% of the days of transmission, typically for about 4 out of 8 hours on a given day. Aware of the regular presence of whistler ducts around L=4, we were not surprised to find that the signal paths were concentrated within a range of about 3° of the Siple-RO field lines. There was an activity peak near dawn and moderate levels on the dayside.

The Siple signals seemed to depend upon a prior infusion of energetic electrons during disturbance. Fig. 5.20 is a plot of the average Kp for days before and after a day of echoes. There was clearly a quieting trend in Kp at the echo time, notably deeper when results for a set of five active days from 1974 were separately examined (open circles).

The echo spectra of Fig. 5.19 contained much information. For example, it was clear that a single dominant propagation path had been followed, so that in order to optimally align the records for this day one could simply shift the SI record 3.2 s in time. We found that the rising and falling tones reached high amplitudes, but with delays from their respective beginnings at 2 and 3 kHz. There was no triggering of emissions by the ramps, as was found in the cases of several of the 1 s constant frequency tones that were spaced by 200 Hz over the 2-3 kHz band. The 1-s constant frequency pulse at 2.2 kHz did not show temporal growth, but may have experienced linear growth. Meanwhile, 1-s pulses at 3, 2.8, 2.4, and 2,6 kHz, which initially were at low levels, became rapidly amplified and triggered strong rising emissions.

5.5.8 The phenomenon of line radiation

A paper by *Helliwell et al.* (1975) raised the topic of line radiation and the question of whether systems such as the Canadian power grid were radiating significant power into the magnetosphere at multiples of the 60 Hz line frequency. Such a possibility had previously been discussed in connection with the manner in which triggered emissions recorded in 1973 at Roberval had shown inflections at very weak multiples of 60 Hz, as in Fig. 5.14. Here Fig. 5.21 shows portions of the 4-5 kHz VLF spectrum recorded at RO and SI on June 22, 1973 at a time when magnetospheric lines were present on the records but Siple was not transmitting. Above on the RO record are marked the harmonic numbers of narrow local induction lines, just below wider-banded magnetospheric lines seen at both stations. Most compelling were the vertical enhancements of the lines, indicating in a consistent manner a two hop whistler mode delay as the elements of the lines echoed back and forth.

5.5.9 More on line radiation. Reminiscences of Raj Raghuram

"As we were studying the data from the Siple transmitter received in Canada in the mid seventies, we came across a very interesting phenomenon. The spectrograms showed a series of horizontal lines in the 1 Khz to 6 Khz range. Prof. Helliwell conjectured that these lines could be due to harmonics of power line radiation.

The region around Roberval, Quebec, Canada, generates an enormous amount of hydroelectric power. This is passed along transmission lines not very different from the Siple antenna. One would expect much lower efficiencies because of the proximity of ground. Also, the harmonic content in the kHz range would be lower by at least a couple of orders of magnitude. Compensating for these effects was the fact that these power lines carry thousands of amps of current - far more than the Siple antenna. It did seem plausible that the power lines in Quebec, Canada, were radiating in the same way as was the Siple antenna but in the conjugate hemisphere.

This theory got a lot of publicity and was even written up in the New York Times. I happened, by chance, to be sitting in Prof. Helliwell's office when the NY Times science correspondent (name ? called - one of the key events in my life. Prof. Helliwell explained how this radiation from power lines could be dumping electrons into the ionosphere and perhaps depleting the Van Allen belt. This could reduce the protection the belt offers from UV radiation from the sun.

I and a few others studied the spectrograms carefully and we could never conclusively prove that the lines were due to power line radiation. The frequency was often not quite at harmonics of 60 Hz. One could argue that propagation through the magnetosphere was distorting the signals. Then, neither were the lines always separated by 120 Hz. The theory also was that we were more likely to see odd harmonics of 60 Hz, hence the 120 Hz separation. You either believed in power line radiation or not. Soon the group was divided into believers, agnostics, and atheists. Jan Siren (showing a lot of courage) had a poster on his door which said 'Power line harmonics are the emperor's new clothes'."

5.5.10 Studies of "quiet bands"

"For my thesis I studied something called quiet bands. We found that while there were a lot of emissions in the frequency range immediately above the transmitted signal from Siple Station, the region below was quiet, implying that the transmitted signal was suppressing any activity in the region. This was used as evidence for the presence of power line harmonic radiation. It could be argued that radiation from power lines was enhancing emissions above the harmonic frequency and suppressing emissions below.

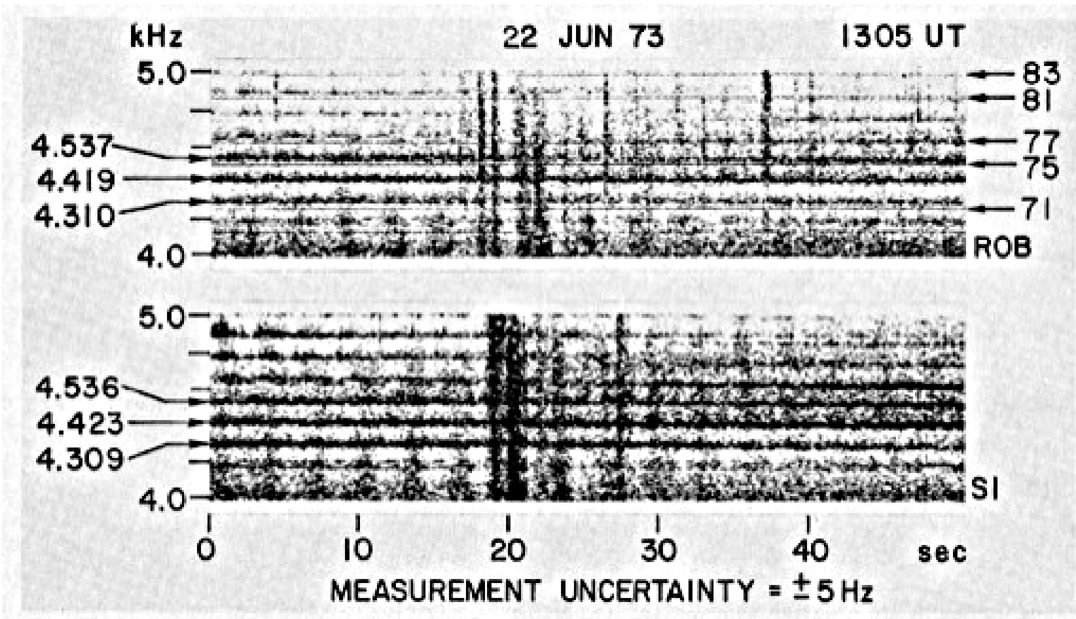

Figure 5.21: Conjugate spectrograms of apparent multi-frequency power line harmonics, showing effects of two hop echoing in the whistler mode. From *Helliwell et al.* (1975).

This led to the presence of clear horizontal lines in the spectrograms.

As part of my thesis, I attempted to explain the quiet bands that formed immediately below the transmitted frequency in the Spectrograms. Based on wave-particle interactions and cyclotron resonance, I reasoned that the electron distribution as a function of parallel velocity could have a dip below the transmitter frequency and then rise again. I had no way to prove it without doing extensive calculations of interactions between whistler mode waves and resonant electrons. Fortunately for me, Umran Inan, who was a fellow graduate student at that time in the group, had just developed a computer program to do this.

A second hurdle was that this required a large amount of computer time on the IBM 360 mainframe and could deplete the computation budget of the group significantly. As luck would have it, Stanford moved to a new IBM 370 just at that time. In order to encourage people to switch over, Stanford said that the usage of the computer would be free for a month. I hurriedly ran as many simulations as I could.

I was able to show that the electron distribution did show a dip below the transmitted frequency. We wrote a paper based on the data for the quiet bands as well as my explanation showing how I expected the electron distribution to show a dip. The reviewers came back and said that the data were fine, but did not buy my explanation. It must be remembered that the Stanford group was the only one which could even come close to having such data and so nobody could ever dispute the data. On the other hand, there were many competing theories about wave-particle interactions - Helliwell's phenomenological explanation, Kennel and Petshcek's linear theory, etc. We decided that it was important to present the data and took out my explanation. However, I had the last laugh. Somewhere in JGR's publishing process they used the figures from the original version of the paper and my theory saw the light of day anyway."

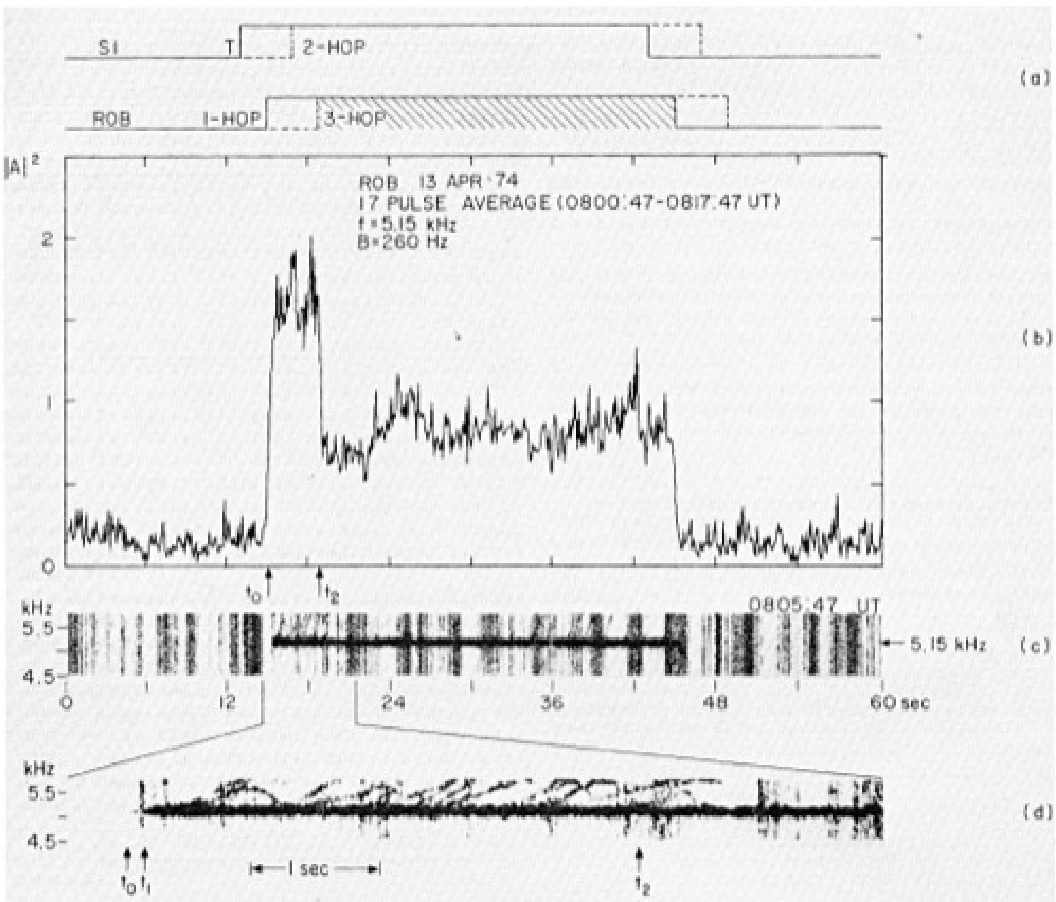

Figure 5.22: Suppression of Siple signals by 2 hop echoes. From *Raghuram et al.* (1977).

5.5.11 Suppression of the transmitter signal by 2-hop echoes

In 1977, *Raghuram et al.* (1977) published a paper showing how echoes of transmitter signals returning as 2d hops from the conjugate region could suppress newly transmitted 5.15 kHz 1-hop signals by as much as 20 dB (*Raghuram et al.*, 1977). This effect is illustrated in Fig. 5.22, where panel (a) shows the time sequences of the various signal hops at Siple and Roberval, and (b) the average amplitude of 17 such signals. (c) is a spectrogram for one of the 30 s signals, with time matched to the scale just above, while (d) is an expanded record of the early part of record (c).

5.5.12 More from the 1978 review paper

The subtle effects of PLR (power line radiation) were thought to be present in natural events such as the whistler echoing illustrated in Fig, 5.23. The expanded lower record from Siple on Apr. 18, 1975, shows the quieting of an echoing whistler and associated emission. As the echoing quiets, narrow frequency elements 120 Hz apart are first excited, but then one frequency element becomes dominant and triggers falling tones that repeat at the whistler-mode bounce period.

Attention was paid to the importance of df/dt in a triggering signal. Fig. 5.27 shows a Siple format (below) adjusted for travel time and including both rising and falling tones transmitted between 2.5 and 3.5 kHz. In this case the notable differences between risers and fallers were not in the growth rate of the signal nor its peak amplitude, but rather the signal development near its end, continuing over hundreds of Herz past the freqency limit of the trigger at roughly the sweep ;rate of the trigger.

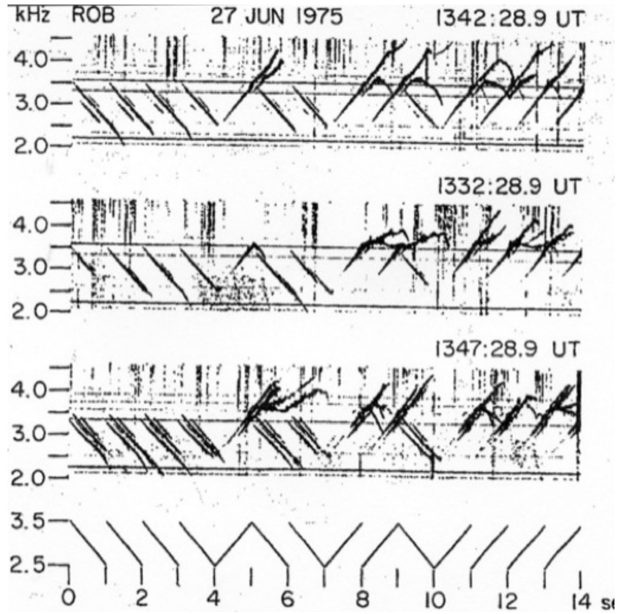

Figure 5.27: Roberval records of Siple risers and fallers. From *Helliwell and Katsufrakis* (1978).

5.5.13 Explorer 45, a near equatorial satellite

In 1973 there was a geomagnetic-field rendezvous between the Explorer 45 near-equatorial satellite, equipped with a broadband VLF receiver, and the Siple-Roberval conjugate pair. Parts of the late night-side period 0810 to 0855 UT are documented by previously unpublished spectrograms for RO (above), EX 45 (middle) and Siple (when available) in Figs. 5 24, 5.25 and 5.26. There were near-simultaneous wave bursts at the two ground stations during the first 20 min of the 45-min period displayed.

Near 0810UT in Fig. 5;24, EX 45 was still wihin the plasmaphere, and abundant dispersed whistler activity was indicated below $\simeq 5kHz$. Near 0819 UT, as EX 45 closely approached the plasmapause, it began to detect a natural noise band near 6 kHz, a type of band that has been observed close to the plasmapause on other satellite records. Around 0823UT a lower band appeared to begin, extending to below 5 kHz and then disappearing after a few minutes. Still another weak band appeared near 4 kHz around 0835 UT for a few minutes as EX 45 moved to slightly larger L value near L=4.

In Fig. 5.24 and 5.25 there were fluctuations in the level of the main noise band observed on EX45 as it moved into the PBL, and it is not clear how these

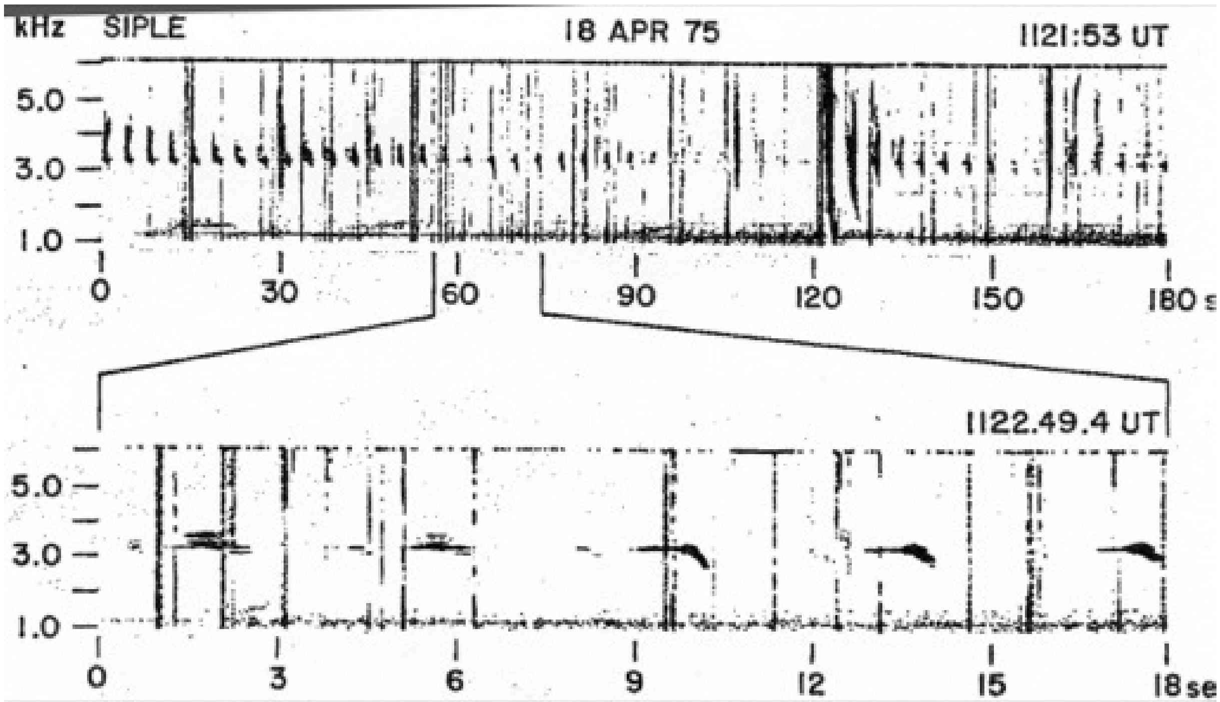

Figure 5.23: Periodic emissions decaying into closely spaced lines with one dominant. From *Helliwell and Katsufrakis* (1978).

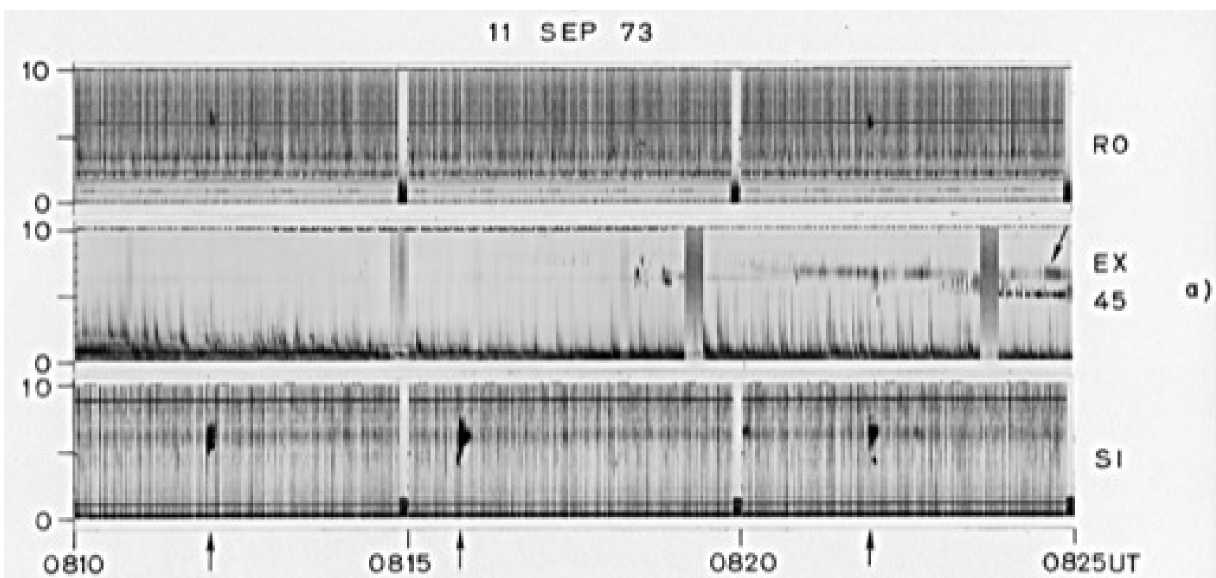

Figure 5.24: Wave bursts at RO and SI and on EX45 as it approached the plasmapause

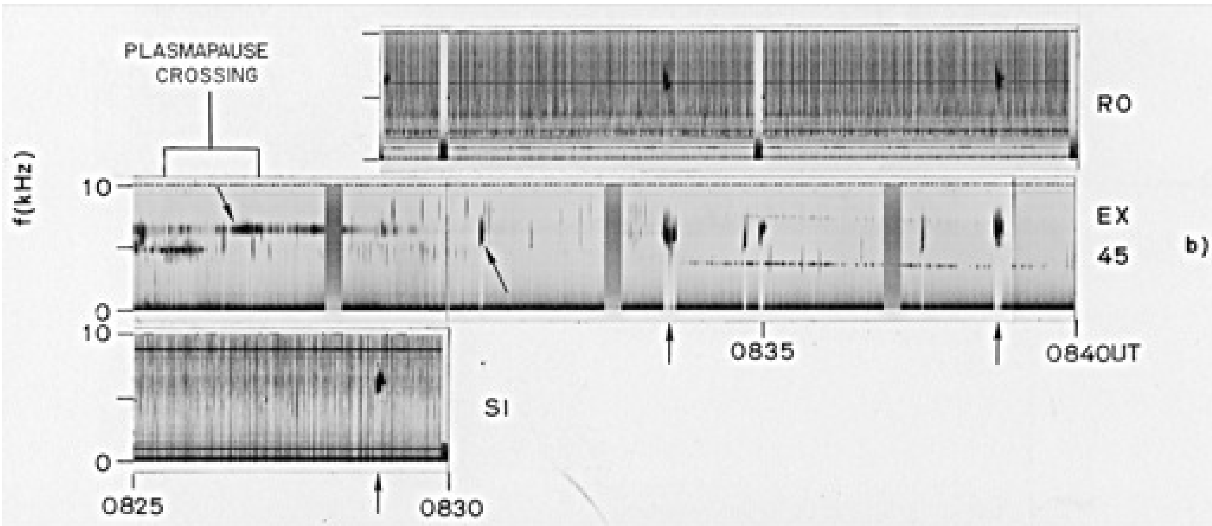

Figure 5.25: Wave bursts at RO and SI and on EX45 as it moved outside and near the plasmapause

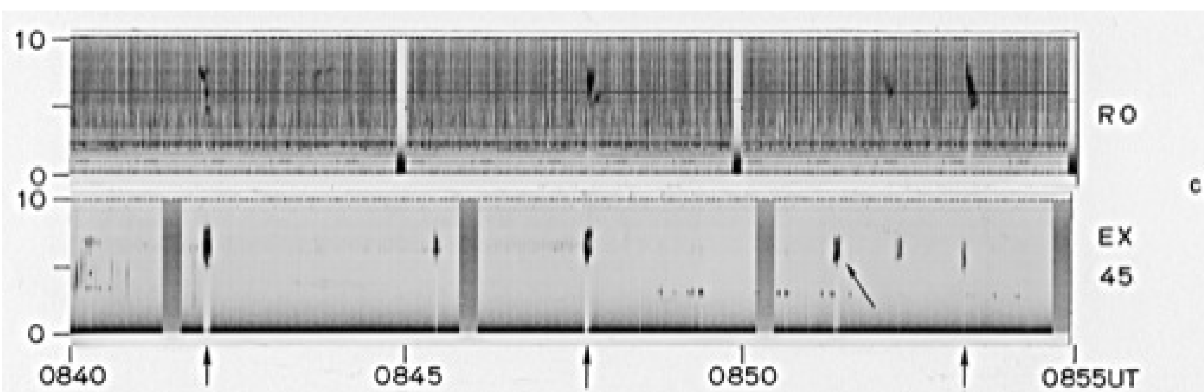

Figure 5.26: Wave bursts at RO and SI and on EX45 as it moved outside and near the plasmapause

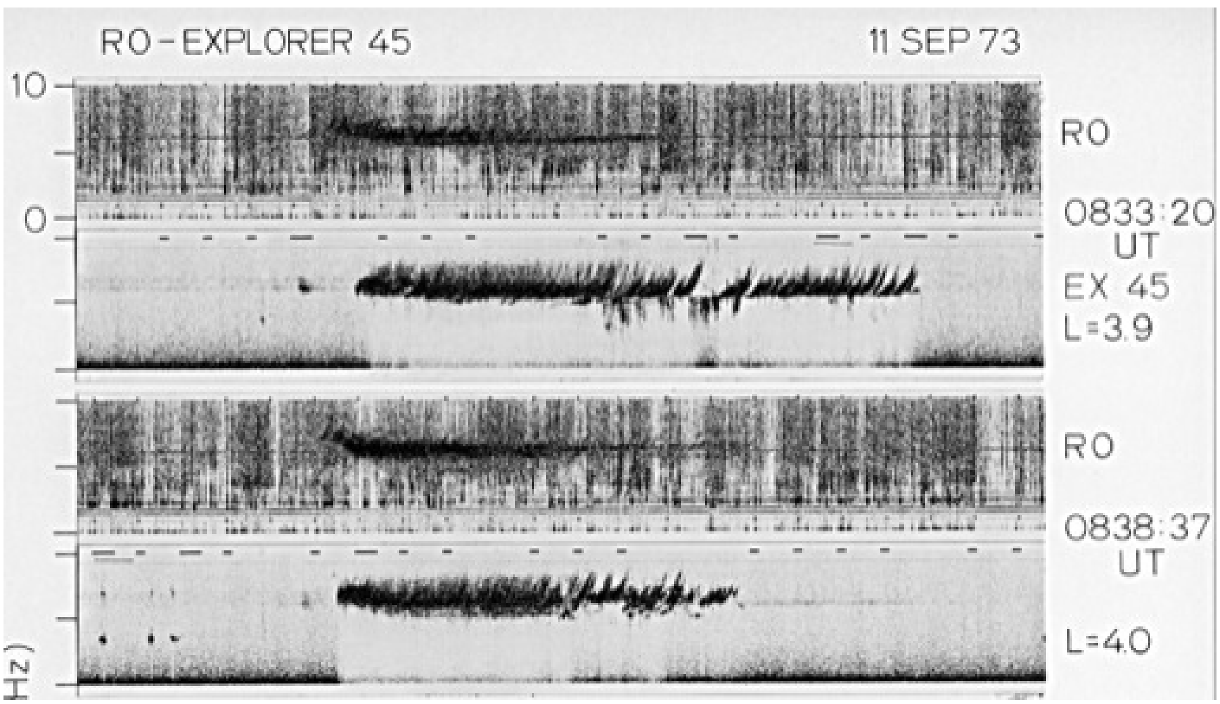

Figure 5.28: Second noise burst at Roberval and on EX45

fluctuations related to the ground observed events. The first of a series of bursts on EX 45 that were clearly connected to the ground events appeared at about 0834 UT and is shown, in addition to a record from around 0838 UT, on an expanded scale with the RO records on Fig. 5.23=. There is a rough similarity between EX 45 and RO in burst frequency and duration. On the upper RO record there are indications of a steady narrow noise band near 6 kHz, and on EX 45 some fairnt indication of a noise enhancement prior to the main burst. There were strong AGC recactions on EX 45 because of the intensity of the main wave bursts.

The perturbations of subionospheric signal paths by precipition induced by these bursts outside the plasmasphere was found to differ significantly from the whistler induced or Trimpi events that had become familiar inside the plasmasphere. There was discussion of this new type of quasi-periodic event in a paper about wave induced particle precipitation onto VLF transmitter paths to both RO and SI *Dingle and Carpenter* (1981). (See section 4).

5.6 Robert Helliwell and his theory of the coherent wave instability, based on comments by Marek Golkowski

The great discovery of Bob Helliwell's career was the coherent wave instability (CWI), identified in the early results from VLF wave injection at Siple Station, Antarctica. As noted above, evidence of the instability, in which non-linear wave growth occurs preferentially within bands of 10 Hz or less, had been seen prior to Siple operations, but it was through Siple transmissions received in Quebec, Canada beginning in 1973 that a relatively complete set of CWI features was exposed. In contrast to this experimental triumph, Bob's eventual failure to develop a widely accepted explanation of the CWI process may have been the great disappointment of his career.

Radio-scientists first became aware of the occurrence in geospace of discrete VLF emissions in the early 1950s, thanks to the pioneering work of *Storey* (1953). Initial speculations in the late 1950s on emission mechanisms favored longitudinal resonance as a key element, but in the early 1960s, with work by *Brice* (1964) and growth calculations by *Bell and Buneman* (1964), attention became focused on the transverse resonance. Brice called attention to the importance of feedback within a region fixed in space, thus invoking a backward wave oscillator that became a starting point for Helliwell's major paper in 1967. And as noted above, there was in 1964 the stunning report by *Helliwell et al.* (1964) of the triggering of rising and falling VLF emissions by U.S. Navy transmitter signals (Fig. 1.45).

A pioneering paper by *Helliwell* (1967) developed the idea of a drifting backward wave oscillator operating in a feedback mode along the earth's B field in an "interaction region" of limited spatial extent. Resonant electrons entering the region were phase bunched by output waves propagating in the opposite direction, thus developing the transverse current required to drive the oscillator. The growth process was self-limiting, such that the interaction region would drift up or down the field lines according to the need to accommodate stronger or weaker particle fluxes entering the region. Thus emissions rising or falling in frequency could be generated according to spatial changes in the conditions for cyclotron resonance.

It was noted that previous attempts to explain the narrow bandwidths of discrete emissions as well as their frequency variations had involved unsupported assumptions. *Dowden* (1962) had used bunched electrons as a source of Doppler shifted backward traveling waves to explain natural echoing emissions, but as both Brice and Helliwell were aware, this was only an extreme limiting case of the drifting source region that Helliwell was proposing. The 1964 report on artificially stimulated emissions (ASEs) excited by wave injection meant that a discrete emission mechanism involving mirroring particle bunches was not tenable.

Helliwell's picture differed in important ways from the widely referenced papers by *Kennel and Petschek* (1966) and by Cornwall [?], who had developed a wave particle interaction model in which only small perturbations of particle orbits were contemplated and the hiss-like waves involved were incoherent.

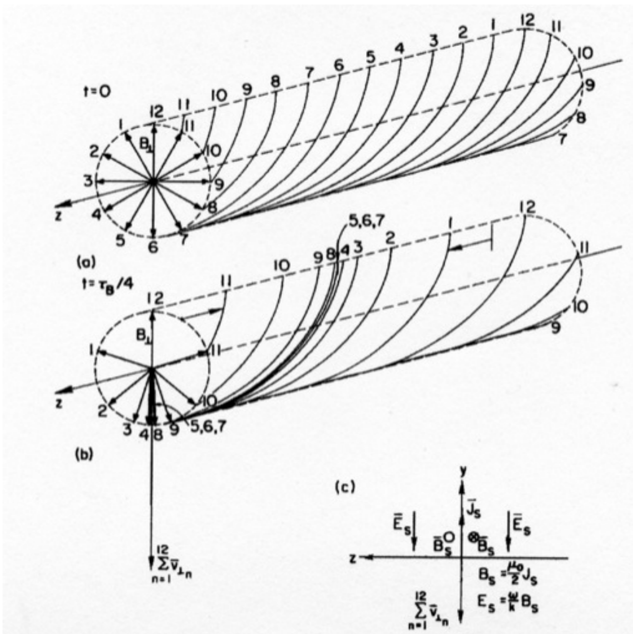

Figure 5.29: The bunching in phase of 12 electrons through interaction with the magnetic field of a monochromatic whistler mode wave. From *Helliwell and Crystal* (1973).

Helliwell was proposing substantial longitudinal displacements by the phase bunching involved in the first order cyclotron resonance. His point was that electrons resonant with the input wave have their trajectories modified and become bunched in phase at the point of minimum Lorentz force. The bunched electrons subsequently radiate a wave at the resonance frequency along the geomagnetic field line with counter-streaming particles acting as a helical phased array. This basic generation mechanism has similarities to the longitudinal resonance amplification process seen in traveling wave tubes. At one level these basic concepts proceeding from Helliwell's engineering intuition cannot be disputed. There was and continues to be general agreement that given a population of perfectly resonant particles in a locally unchanging magnetic field, the process will evolve as described in Helliwell's 1967 paper. However, Helliwell's formulations were criticized by other researchers (e.g.(*Nunn*, 1974)) in that they failed to consider a realistic particle distribution consisting of significant numbers of both resonant and non-resonant particles and also did not properly account for the effects of the locally inhomogeneous geomagnetic field.

A paper by *Helliwell and Crystal* (1973) explored the 1967 ideas with a computer simulation. The numerical model was self-consistent in that it included feedback to the wave fields from the currents created by phase bunched particles. This paper's frequently cited comprehensive illustration of the basic phase bunching process continues to serve as the first stepping stone for students interested in ASEs and triggered emissions. At the same time, the unrealistic physical premises of a mono-energetic electron distribution in a static geomagnetic field has made the paper more controversial among seasoned theorists. Following closely on the heels of the Helliwell and Crystal paper, the first observations from the newly built Siple Station Transmitter were published by *Helliwell and Katsufrakis* (1974). In a succinct paper clearly illustrating the richness of phenomena to which a dedicated research transmitter gives access, the primary features of ASEs, ones confirmed with many subsequent transmissions, were first described. A key feature of the observations was the repeatability of the temporal growth (up to 100 dB/sec) of a signal observed at the ground receiver in the northern hemisphere for a constant power input in the southern hemisphere. These observations of ASEs and triggered emissions left no doubt that they involved significant changes in the magnetospheric distribution on short time scales that could only be explained by nonlinear theory.

The pioneering observational results from Siple Station coincided with significant theoretical efforts on triggered emissions in the United States and around the world. These included the works of *Sudan and Ott* (1971) at Cornell University, *Roux and Pellat* (1978) and *Cornilleau-Wehrlin and Gendrin* (1979) in France, *Karpman et al.* (1974) in the Soviet Union, *Matsumoto and Kimura* (1971) in Japan and *Nunn* (1974) in the United Kingdom. Many of these works emphasized the so-called phase trapping of the resonant electrons in the potential well created by the input wave's magnetic field. In contrast to Helliwell's simpler "phase bunching" concept, the notion of trapping involved a specific amplitude threshold for the input wave and was strongly influenced by the geomagnetic field gradients. Although it is the Lorentz force that drives both phase bunching and phase trapping, the cumulative effects can be very different.

In phase trapping, the trajectory of the resonant counter-streaming electron is not only slightly perturbed as in phase bunching but is forced to remain in resonance with the wave even if the background magnetic field is changing. The phase trapping model thus allows for large changes in the electron distribution, since phase trapped particles are moved to positions in phase space very far from the ones they would have under adiabatic motion. David Nunn's 1974 paper was one of the first to provide analytical expressions and associated computer simulations of how currents resulting from phase trapping in an inhomogeneous geomagnetic field affect both the amplitude and phase of the input wave.

Despite a number of theoretical approaches pointing to phase trapping as a key element of the ASE process, Helliwell remained convinced that simple phase bunching was the main driver. In 1980 the results of a power stepping experiment at Siple Station were published that clearly showed a power threshold at the transmitter that needed to be exceeded in order for nonlinear effects to be observed at the geomagnetic conjugate point. The minimum threshold for radiated power was found to be as low as 1 Watt. Helliwell saw this result as vindication of his approach since the 1 Watt input power could not yield the minimum wave amplitude (trapping threshold) in the magnetosphere that is required for phase trapping. In this context Helliwell explained the observed threshold in terms of a minimum signal to noise ratio for phase bunching to occur in the presence of incoherent waves. For many of the phase trapping advocates, however, the existence of a threshold was very much in line with the trapping threshold that they had identified in their theoretical formulations. The disparity between the occasional 1 Watt input power threshold at the transmitter and the tens of pico-Tesla trapping amplitude believed necessary could be reconciled with growth in the linear regime as described more than a decade earlier by *Kennel and Petschek* (1966).

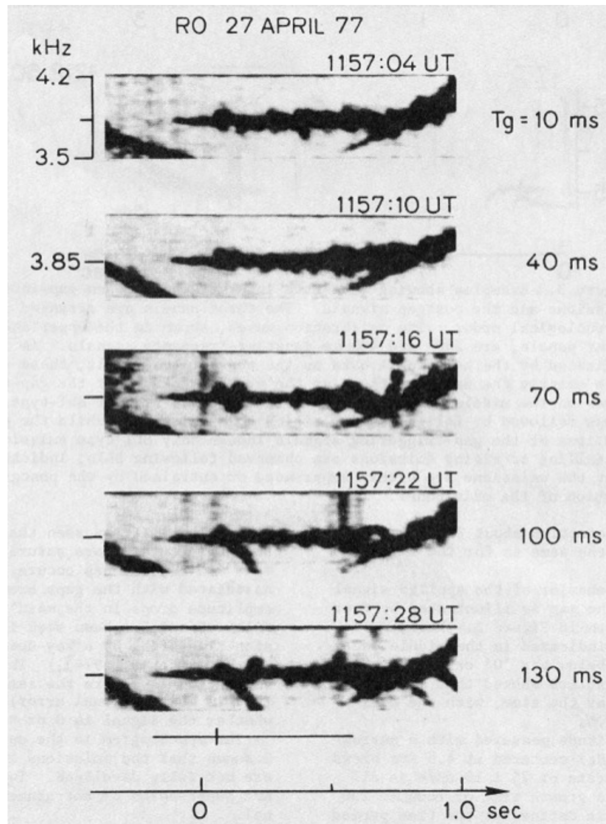

Figure 5.30: Effects of interruptions of varying length inserted 250 ms into 750-ms-long pulses. From *Chang and Helliwell* (1079).

5.6.1 Don Chang reports on the effects of interruptions in transmitted pulses

In 1974, 1975 and 1977 a number of Siple transmissions were used to investigate the effects on a coherent signal of a 10 ms interruption (*Chang and Helliwell*, 1079). Following the interruption, there was either a return to phase or a phase reversal with respect to the initial segment of the pulse. In one experiment, 1-s pulses were interrupted for 10 ms after 400 ms. No change in the growth rate or saturation level of the post gap part of the pulse were found because of the difference in post gap phase.

Rising frequency emissions were often found to be triggered by a gap, and in some cases were suppressed or entrained by the post gap emission activity. The gap induced emissions resembled those emissions found to begin at the ends of coherent pulses, and are thought to represent the previously noted transition from driven frequencies to natural modes once the driving pulse is interrupted.

An experiment was conducted on the relation between gap length and the development of falling tone emissions. The length of the gap was varied; Fig. shows a range of interruptions beginning 250 ms into a 750 ms pulse and lasting from 10 ms to 130 ms. It was confirmed that as noted previously, all falling emissions start with a small rise in frequency. In this case there were two main signal paths, thus complicating the picture, but it was clear that fallers could develop when the gap length exceeded a certain value.

5.7 The 1980-81 Rocket-Balloon Campaign at Siple Station

I spent the austral summer of 1980-1 as a novice field operator at Siple, helping conduct experiments on effects associated with the VLF transmitter. There had been a proposal to launch rockets at Siple in the austral summer of 1980-1 and to measure both generated VLF waves and particle precipitation induced by the waves. Ray Heer of the NSF office of Polar programs, supporter of the campaign, conducted an informational meeting at Scottsdale Arizona. I was impressed by the number of research groups that would be involved but was profoundly skeptical that wave induced particles could be measured through observations that would be both.brief in time and limited in location. However, Siple transmissions were going to be a key element in such a campaign, and as a campaign participant I needed to help make them as productive as possible.

The Nike-Tomahawk rockets could reach a peak altitude of ~200 km and thus be able look for any bursts of precipitated energetic electrons correlated with the Siple pulses. They would also be making the first direct measurements of Siple signals in the overhead ionosphere.

The particle detectors on the rockets did not apparently register effects that were correlated with the Siple transmissions. However, project scientist David Mathews prepared a useful overview of the campaign *Mathews* (1981) in the Antarctic Journal, along with a number of brief preliminary reports by various team members.

5.7.1 Some results of Siple signal propagation along magnetospheric paths during the Rocket-Balloon campaign

The campaign was surprisingly successful in terms of Siple VLF transmissions. Prior to the campaign we had already had several years of experience with the first Siple transmitter, Zeus, and the beginnings of experience with Jupiter, its replacement. However, our data on previous Eiple receptions in December-January were minimal, and as the campaign got underway we were stunned to find that one hop or two hop echoes were being regularly observed, eventually on 16 days of the 45 day observing period from December 1, 1980 through Jan. 14, 1981. This rate rivalled the highest rates previously observed during any month in the austral winter. However, observing conditions were not the same; on many days there were favorable propagation conditions in a narrow time frame around local noon. rather than in a broader period near 07 MLT (from Zeus data in winter months). On such a day a band of diffuse or structured noise near 2-3 kHz, with discrete emissions riding along the top, woould develop.

I remember well a day when walking between the main station and the separate Jamesway used to accommodate summer people such as myself. The distinctive roar of the station's 150 kw generator could suddenly be heard and I knew that Mike had spotted a natural noise band near 2 kHz. As usual, he was launching signals just above the noise bamd at frequencies where discrete natural emissions often appeared.

As the noise band continued and then decayed over a period of order one hour, two hop Siple echoes could be detected for 30 to 90 minutes. As the natural noise band faded, the two hop echoes would continue to be well defined or might actua;;u increase in amplitujde. They would then fade, usually within 10-20 min after the natural noise had dropped to within a few db of the background noise level.

We found ourselves using frequencies between about 2.5 and 3.5 kHz, rather than near 4.5 kHz as work with Zeus by *Carpenter and Miller* (1976) had seemed to suggest.

The high rate of echo occurrence in the austral summer was at least partly due to the presence of multiple observers at Siple and the availability there of a Rayspan to view the spectrum. We were also helped by the fact that there were almost always two hop signals at Siple when the first hop was detected at Roberval. This strong correlation of the first and second hops was well known, having first come to our attention on July 19, 1973, in the early stages of our work with Zeus and now with the emphatic testimony of Mike Dermedziew.

The repeated occurrnce of echoes near local noon is not well understood. We speculated at the time that they developed in a small noon peak in plasmapause radius that arose as part of the SQ current system, a peak that had earlier been documented in our whistler studies of dayside plasma drifts and may have been a factor in the noon-midnight plasmasphere density asymmetry reported from spacecraft by *Gringauz and Bezrukikh* (1976). The azimuthal drift of energetic electrons from sources in the nightside magnetosphere might well have produced local conditions exceptionally favorable to wave growth at VLF.

5.7.2 Siple transmissions to balloons

During the campaign there were approximately 300 Siple operations lasting from minutes to hours, including transmissions to balloons, to satellites, and to Palmer and Halley stations (*Carpenter*, 1981). For transmissions to balloons, a special format was designed by Ev Paschal, involving a series of 10-s long pulses spaced at 1 kHz intervals from 1.5 to 6.5 kHz, each at approximately the same power applied to the Siple antenna. One purpose was to monitor the transmitter field strength at various points as the balloon drifted at 30 km altitude, thus obtaining information on the effects on the transmitter field of the icesheet and underlying rock envirnonment.

5.7.3 Teletype messages to Halley via Jupiter

Ev Paschal modified and extended a program written by Bob Squires to convert ASCII charaters into code suitable for teletype transmission. Several teletype messages were sent to Halley concerning coordinated measurements associated with the R/B campaign. Also, a special message was sent at seven TX power levels separated by 5 dB to determine the accuracy of the decoding as a function of TX power. The experiment was run on two occasions, once when the Halley goniometer modujlation was present and once when it was not. Palmer also recorded both 25-min transmissions.

5.7.4 Direction finding on VLF natural noise, whistlers, and Siple 2-hop echoes

Successful direction finding experiments were conducted at "three mile island" by John Billey. Good reference bearings were obtained on known fixed frequency sources and real time observations were made of whistlers, discrete natural emissions, hiss, and two hop Siple transmitter signals. Results were generally consistent with expectations. Noise activity below about 2 kHz tended to come from the south, while whistlers with dispersion values suggesting paths equatorward of the Siple field lines gave northerly bearings. When X ray-VLF correlations were observed, similar to those from Jan 2, 1971, the wave arrival bearings suggested propagation near the known balloon position, while at times of structured VLF but no correlated X rays, the waves appeared to arrive from a direction away from the balloon.

5.7.5 Rocket measurements of wave activity above the Siple transmitter

In the following we present both unpublished data from the rocket flights as well as results from the excellent papers by Paul Kintner of Cornell (*Kintner et al.*, 1983) and by Richsrd Brittain of Cornell (*Brittain et al.*, 1983).

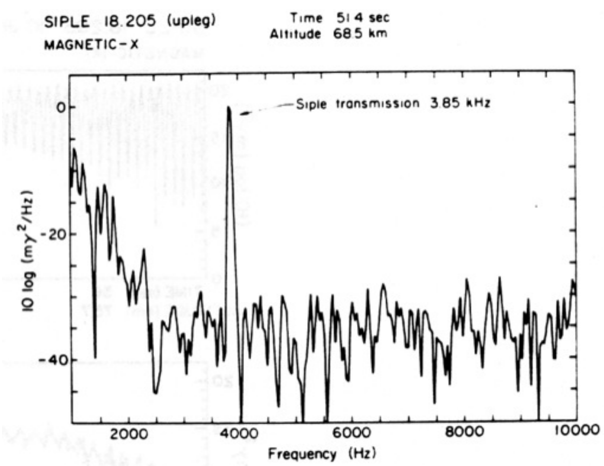

Figure 5.31: Spectral output of the magnetic sensor on rocket 18,205 at 68 km. From *Kintner et al.* (1983).

The transmitter format first used, RBTXA, involved 5-s long ramps or staircases, with a gap of 5 s between them. For rockets 18.204 and 205, however, format RBTXC was used, with spacing between key up and the next key down increased to 15 s so as to avoid suppression of two hop echoes by the transmissions. This allowed for the sometimes long two hop echo delays of up to $\simeq 8s$ and the need to observe higher order echoes. Prior to starting RBTXC on the third rocket, a CW signal at 3.85 kHz was transmitted until the vehicle reached 130 km altitude. Thus the Cornell receiver obtained continuous information on the properties of the received signal as the rocket penetrated the ionosphere.

All three rockets were launched within about an hour of local noon at Siple (1700UT), consistent with ground observations of a noise condition that favored echoing.

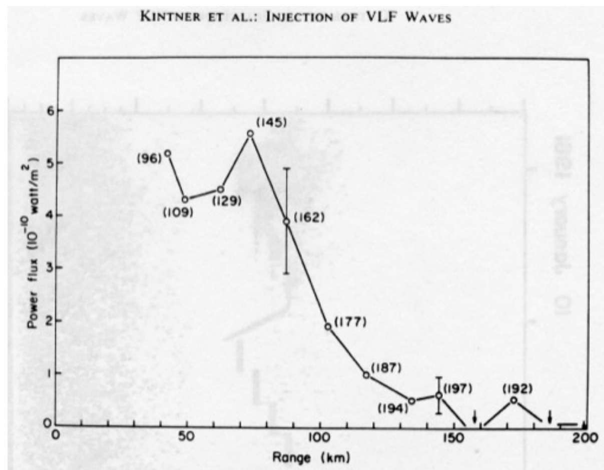

Figure 5.32: Power flux of Siple transmissions versus distance from Siple. Rocket altitudes in parentheses. From *Kintner et al.* (1983).

Fig. 5.31 shows one key to the *Kintner et al.* (1983) peper, the digitized spectrum of the Siple CW transmission, here displayed for an altitude of 68 km. Analysis of the 3.85 kHz signal provided unique confirmation of expectations about VLF ionospheric penetration, indlucing losses due to attenuation, spreading, and refelection. The transition from a linearly polarized subiononspheric signal to a right hand circularly polarized one was illustrated in the data from the magnetic sensors on the rocket. The amplitude data during the 7 min rocket flight were adjusted to show the signal field strength in the ionosphere versus distance from Siple (Fig. 5.32). These results were found to be consistent with *Raghuram et al.* (1974)'s predictions of radiation from the Siple antenna.

Thus while measurements of the upgoing Siple waves were in general agreement with theoretical predictions, the measured spectra reflecting processes of wave growth, magnetospheric propagation, and natural wave activity, Figs. 5.33 to 5.38, were a unique resource.

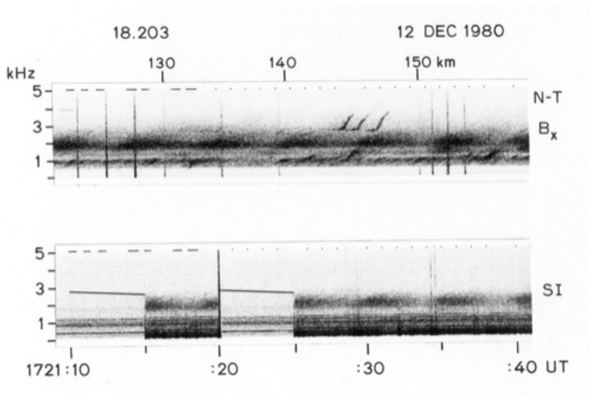

Figure 5.33: Simultandous spectra from N-T rocket 203 and the ground at Siple, Dec. 12, 1980.

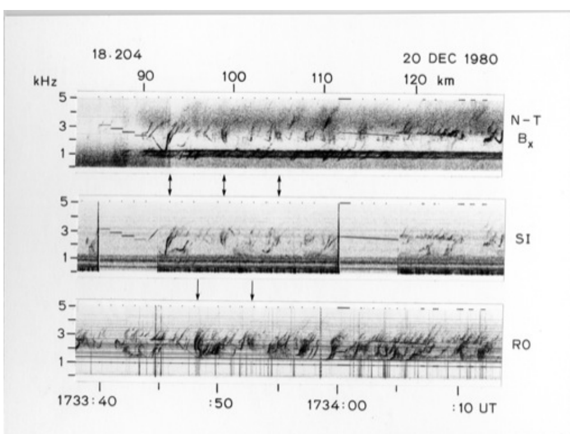

Figure 5.34: Simultaneous spectra from N-T rocket 204 near 110 km altitude and the ground at Siple and at Roberval, Dec. 20, 1980.

On the first rocket, Dec. 12, 1980 (see Fig. 5.33), the transmitted Siple ramps at 1721:10 and 1721:20 were not clearly received in the nearby ionosphere at 140km altitude. However,a weak two hop echo of the first one was detected and the next returning ramp triggered rising emissions. This two hop signal does not appear on the Siple record at this time, although two hops were probably heard earlier as a basis for deciding to launch.

In Fig. 5.33, note the occurrence at both stations of diffuse but pulsating emissions near 2 kHz. They appear to have been simultaneous, separated by the two hop whistler mode delay of about 5 s.

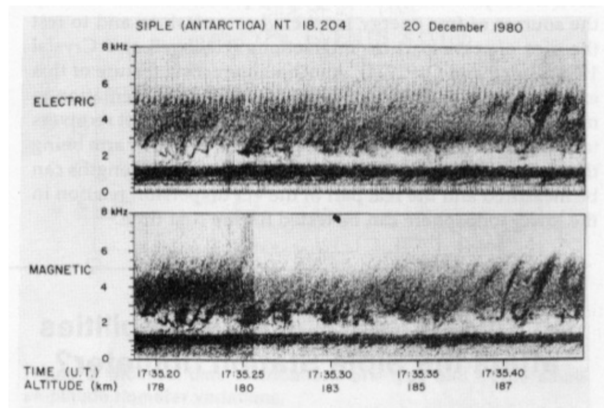

Figure 5.35: Simultandous electric and magnetic field spectra from N-T rocket 204 near 190 km, Dec. 20, 1980. From *Mathews* (1981).

On the second rocket, Dec. 20, 1980 (Figs. 5.34 and 5.35), with different frequency scales), the upgoing Siple ramp at 1734:00 was detected on the rocket in the ionosphere, on the first hop at Roberval and on the second (and possibly fourth) hop on the rocket and at Siple. Note the 2 hop periodicity in the discrete chorus like emissions, marked with vertical arrows between the Siple and rocket records and at intermediate points on the Roberval record. The periodic emissions appear to have propagated on the ducted path follwed by the Siple signals.

Figs. 5.36 and 5.37 show spectra from the third and final NT rocket, launched on Jan. 10, 1981. The figures cover a two minute period as the rocket approached peak altitude and reached a distance of about 150 km north of Siple in the magnetic meridian.

In Fig. 5.36, with rocket spectra only, both upgoing and echoing 2-hop signals are well defined and the echoes appear to trigger slightly structured emissions. There is a weak 4th hop echo after the strong 2d hop. Note the steady noise band below about 2.7

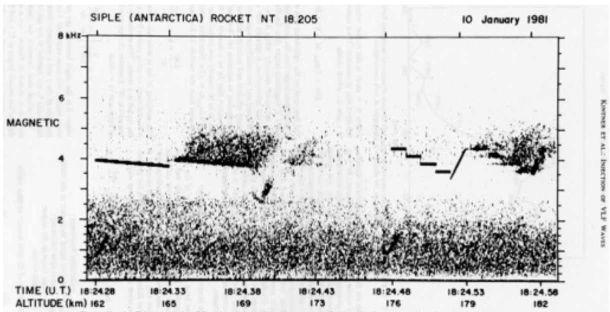

Figure 5.36: Spectra from N-T rocket 18.205 at about 175 km altitude with well defined upgoing and two hop returning signals. From *Kintner et al.* (1983).

kHz. This noise is discussed in the Brittain et al. paper reviewed below.

In Fig. 5.37 the echoes trigger similar patterns of rising emissions, while on the rocket a rare first hop multi-component whistler appears. It is believed to have been launched by a sferic either at t=6.0 sec on the Roberval record or just afterward. The whistler followed multiple paths, as did the transmitter echoes by lengthening beyond their nominal 1 s individual durations. A cluster of one hop whistler components without defined lower frequencies immediately follows the initial whistler cluster and is believed to be associated with propagation in the outer plasmasphere during this local noon-time period.

Fig. 5.37 reveals important differences between the 2 hop signals recorded in situ and the corresponding ground recordings. At Siple, a 5-s gradual ramp exhibits evidence of frequency broadening approximately every 0.5 s (possibly of the kind discussed above) as well as faint triggered emissions. In the fourth hop at Siple, the broadened frequency episodes in the 1-hop appear as a series of enhancements on the spectra. The RO 1-hop record shows faint triggered emissions, but no clear evidence of the frequency broadenings in the echo at Siple. A third hop echo is not clearly present at RO. Meanwhile, on the rocket there is a band of noise above the echo, one that is not present in the ground records. However, there are hints of the enhancements seen in the ground data, and the fourth hop on the rocket resembles the one recorded at Siple.

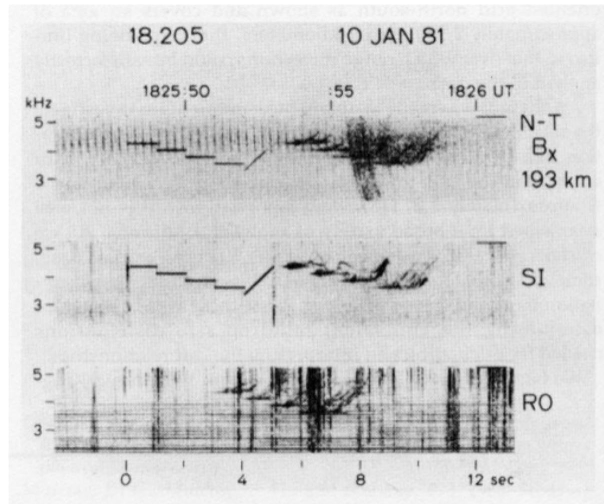

Figure 5.37: Magnetic spectra from N-T rocket 18.205 at about 200 km altitude and magnetically north of Siple by about 150 km, Jan. 10, 1981. From *Mathews* (1981).

In *Kintner et al.* (1983) there were two independent determinations of the local refractive index, one by reporting the value of cB/E and the other by interferometric wavelength measurements using the magnetic field signal phase on the satellite and at the ground as the two ends of an interferometer. Fig. 5.39 shows the height variation of the refractive index determined from the interferometer as well as individual cB/E measurements.

5.7.6 Some surprising reflection effects in VLF emissions

On all three rocket flights there were apparent interference fringes in VLF noise detected from about 90 km to about 140 km altitude (*Brittain et al.*, 1983). Fig. 5.40 shows spectrograms of the magneric channel as rocket 18.205 moved upward from about 90 to 130 km. The upper panel shows the spectra from 1 to 5 kHz, including the steady Siple transmitter signal at 3.85 kHz and a strong whistler that was similar in form to the early part of the whistler recorded a few

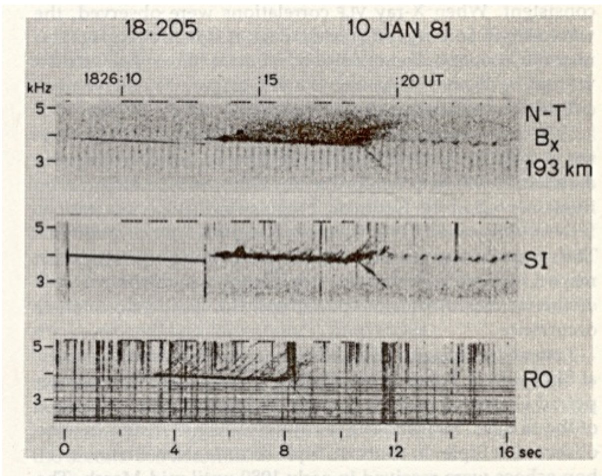

Figure 5.38: Simultaneous spectra from N-T rocket 205 at about 200 km altitude and the ground at Siple and at Roberval, Jan. 10, 1981. From *Kintner et al.* (1983).

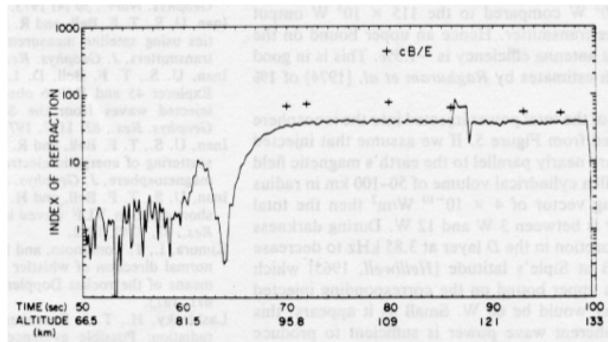

Figure 5.39: Refractive index versus altitude measured interferometrically and by individual measurements of cB/N. From *Kintner et al.* (1983).

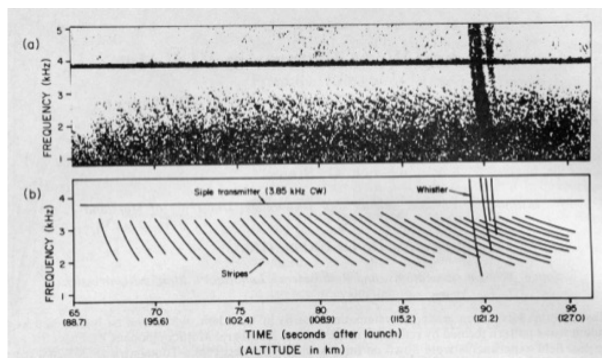

Figure 5.40: Spectra from upleg of 18.205 showing the Siple CW signal, a whistler, and interference fringes in VLF hiss. From *Brittain et al.* (1983).

minutes later near 190 km altitude (Fig. 5.37). The stripes appear in the VLF noise between about 2 and 3 kHz and are traced on the panel below.

These stripes or fringes were believed to represent the reflection of downcoming VLF hiss from a sharp lower ionospheric boundary. They were observed on the upleg and downleg of two rocket flights and on the upleg of the thrrd flight. Assumjing the fringe spacings were related to the wave length of the incident waves, plasusible estimates of the local electron density were obtained. It was concluded that from the appearance of fringes to large orders, the distribution of wave normal directions in the reflecting hiss must have been quite narrow.

5.7.7 Park's manuscript on whistler-induced periodic emissions; Paschal's digital processing of a periodic emission event triggered by the Siple transmitter.

In the early 1980s Chung Park was preparing a paper on the triggering of periodic emission trains by whistlers and by the Siple transmitter. He also paid attention to the way in which magnetospheric line radiation developed as part of periodic emission events. I was invited to be co-author of the paper, which was submitted to JGR but never published, in large part because of Chung's departure from the VLF group and our difficulties in responding to the comments of the referees in his absence. In the following we summarize the results of the paper, which in several cases show a transition from gradual hop-by-hop linear growth to a fast temporal increase in emission bandwidth and intensity.

Figure 5.41 is a recordinig from the Long Wire an-

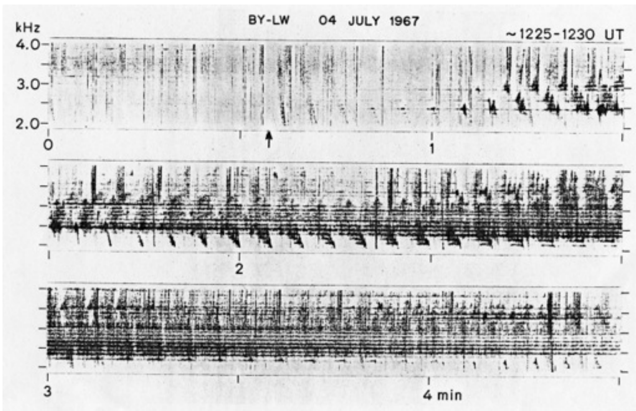

Figure 5.41: Spectrogram from Byrd Longwire showing development of a whistler triggered periodic emission event exhibiting strong line radiation activity and lasting $\simeq$ 5 min.

tenna embedded in the ice about 3 miles from Byrd station. Although not formatted with a timing signal or frequency reference as were the main stations, this recording and others like it were useful in terms of response at frequencies in the frequency range from $\simeq$500 Hz to several kHz.

The figure shows a one hop whistler that arrived at t=$\simeq$ 1:10 s and appeared to be composed of narrowband constant frequency elements separated by of order 100 Hz. It was followed by a series of echoes that selectively emphasized and amplified certain of those elements between $\simeq$ 2.4 and 2.8 kHz. The amplitudes of the elements nearest 2.4 kHz increased on successive hops until on the 13th hop the strongest element, having already exhibited some spectral broadening, widened much more as it increased sharply in intensity. Echoing emissions then spread in frequency and continued for at least 4 minutes while maintaining line separations of order 100 Hz over the 2-4 kHz frequency range (except for some falling tones that appeared between about 2 and 2.5 kHz). One might call this a pre-Siple example of the coherence bandwidth effect.

In 1988, some years after Park's work, Ev Paschal published an example of the launching of a periodic emission by the Siple transmitter, much as whistlers

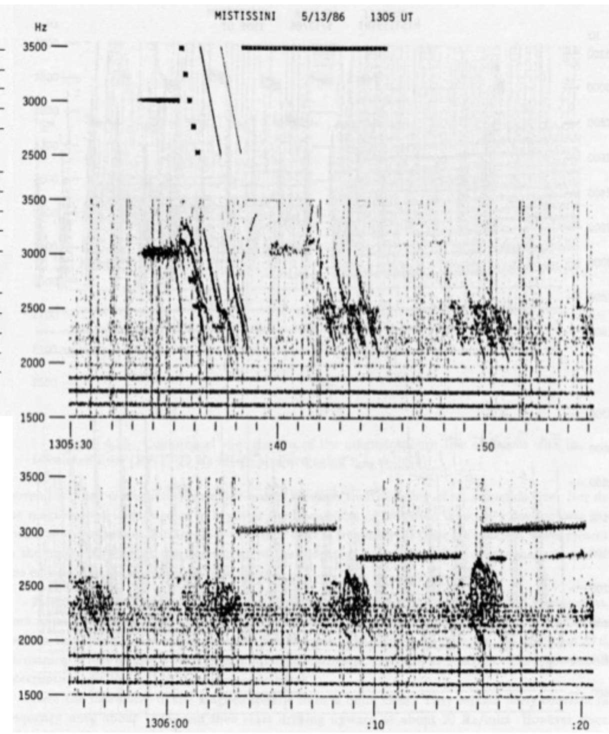

Figure 5.42: Digital spectrogram from Mistissini, Canada on May 11, 1986. From *Paschal* (1988).

had done in examples shown by Park (*Paschal*, 1988). Fig. 5.42 displays frequency-time spectra recorded at Lake Mistissini, Canada during a 50-s period on May 11, 1986. The top record is a digitally processed $f-t$ diagram of the 5-s-long transmitter "diagnostic" format, beginning with a 2-s pulse at f=3010 Hz, followed by a descending staircase, two frequency ramps, and then a 7-s doublet at frequencies fixed 30 Hertz apart.

The received records at Mistissini on the middle and bottom panels illustrate once again the importance of frequency selectivity in the early stages of triggered periodic emissions. The transmitted format echoed back and forth while spreading downward in frequency and exciting a cluster of traces permeated by lines. The most active new frequencies were between $\simeq$ 2150 and 2500 Hertz, just below the lower $\simeq$ 2500 Hz limit of the transmission format. As

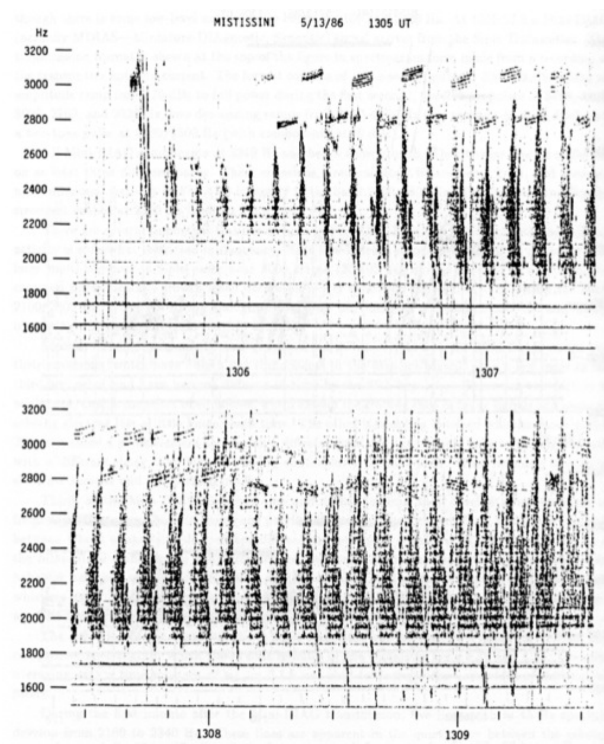

Figure 5.43: Compressed digital spectrogram from Mistissini, Canada on May 11, 1986. From *Paschal* (1988).

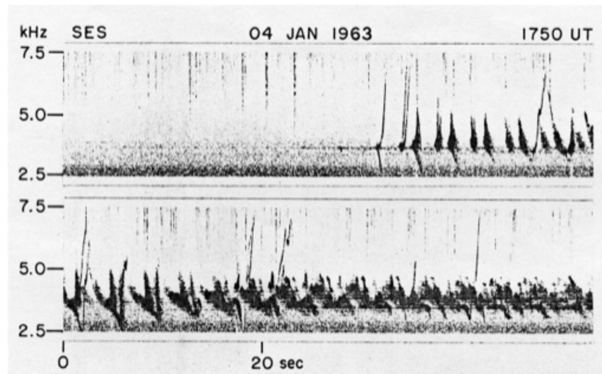

Figure 5.44: Spectrum from Suffield station in Canada 2.5-7.5 khz on 04 January, 1963, showing developing periodic emission event.

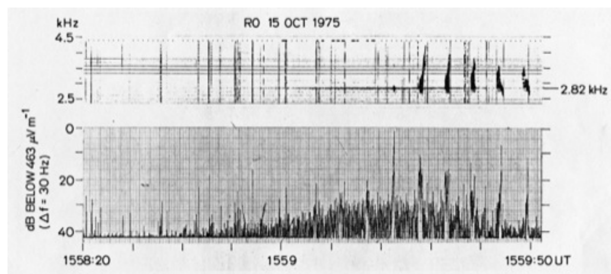

Figure 5.45: Roberval spectrum 2.5-4.5 khz on 15 October, 1975 showing developing periodic emission event. Amplitude in a 30 Hz band centered at 2.82 kHz is shown on the bottom record.

whistler mode echoing of the trace cluster continued, lines separated by of order 50 Hz appeared to become enhanced below $\simeq$ 2500 Hz.

The next figure, no. 5.43, shows on compressed spectrograms the evolution of the periodic emission over several minutes as it widened in frequency and became line-like over the range $\simeq$ 1900 to $\simeq$ 3000 Hz. Beginning near 1306:10 the lines began to drift upward in frequency with time at a rate of about 25 Hz/min. During the event, successions of falling tones were occasionally launched from its lower parts near 1900 Hz.

It is rare that Nature repeats herself on visual records as clearly as she did in the periodic emission events of figs. 5.44 and 5.45. Here is periodicity growing out of the echoing enhancements of a single magnetospheric line near 3.0 kHz, without indications of an initiating whistler or emission. The data were recorded in opposite hemispheres near local noon and some 15 years apart. In both cases the echoing element at the enhanced line frequency suddenly broadened and on the next hop became the first strong member of a periodic emission. The lower panel of Fig. 5.45 shows amplitude in a 30 Hz band centered at the line frequency, with a between-hop increase of order 20 dB at the time of the first spectral broadening.

Nature never seems to run out of ideas when it comes to periodic emissions. Fig. 5.46 shows the transformation of a diffuse noise band into a multilined periodic emission that continued to develop for

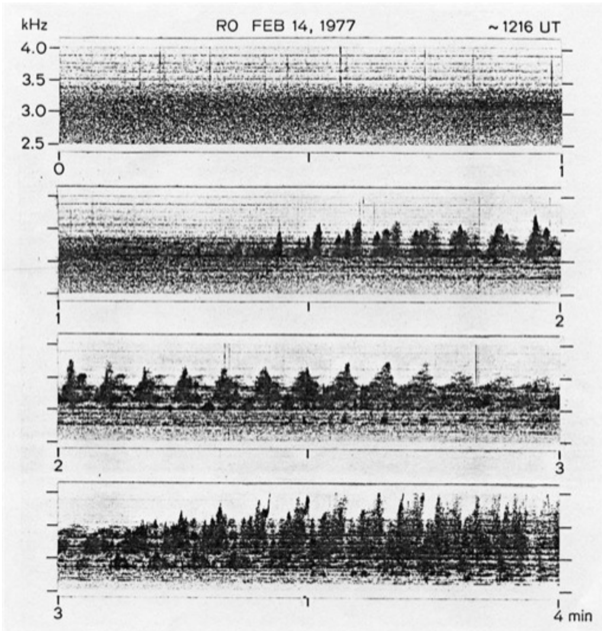

Figure 5.46: Roberval spectrum 2.5-4.0 kHz on 14 February, 1977 showing development of periodic emissions from a diffuse noise band.

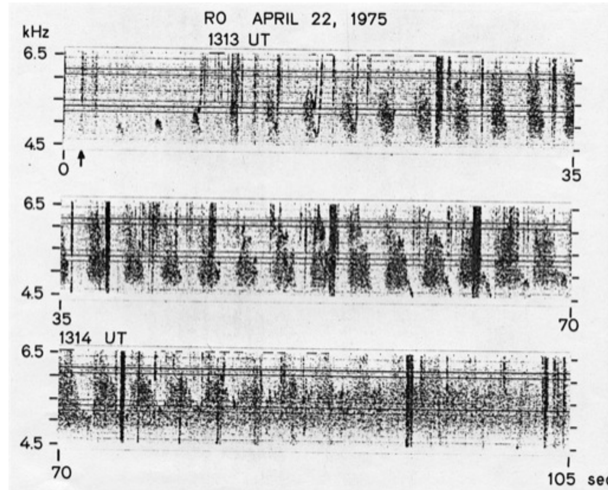

Figure 5.47: Roberval spectrum 4.5-6.5 khz on 22 April, 1975 showing development of a periodic emission event lasting about 2 min.

at least 4 minutes. The periodics began near t=1:20 after a portion of the noise just above 3 kHz darkened into a band after about t=25 sec. No clear initiating trigger is seen for this case.

The periodic emission in Fig. 5.46 began with a series of echoes that rose in center frequency on successive hops, each emission covering more than one closely spaced line. The emissions broadened into diffuse noise forms that spread over 2 kHz in frequency and began merging in time at their lower frequencies.so as to lose their identities as separate echoing emissions. We do not have information on an initiating source,

5.7.8 Unanswered qeustions about the periodic emission examples

How can we relate the cases described by Park and by Paschal to the Siple experiments? There is a clear point in a succession of echoes when the wideband field strength of the echo jumps by of order 20 dB, after previously increasing seadily by about 3 dB per hop. After this sudden increase, successive echoes appear to remain at about the same high intensity level on the records, at least for periods of oreer minutes. We note that there may be some additional triggering and spectral broadening in the first few echoes that reach the “saturation” level.

The abrupt non linear increase in intensity and complexity of a periodic emission after 10 or 15 echo hops appears similar to the onset of fast temporal growth observed in individual Siple transmitter signals. A difficulty with Siple is the lack of information about the linear growth that may occur prior to the fast transition point. As noted above, Helliwell believed in a small signal theory, such that the Siple waves entering the interaction region were below the intensity level necessary for phase trapping.

It would seem that in the Siple and PE regimes, the processes of sudden growth to saturation are similar, starting and ending at roughly the same intensity levels, but that any preceding linear growth regimes are different. Certainly for Siple the magnetosphere seems to move into non linear response immediately, without waiting 40 or 50 seconds for the slow buildup of an echoing event. What is the difference?

5.7.9 More discussion of Siple and periodic emissions

Our impression is that, in spite of the broadening of the periodic emission echoes in frequency and time, they continue to propagate on a single magnetospheric path, as do Siple signals.

We need to study more fully how Siple signal activity dies away. In one of Park's cases, the echoing activity seemms to reverse, dwindling in a way that leaves only a narrow band echoing signal.

A remarkable feature of the intense periodic emission echoes is their tendency to striate and become line-like, in the way shown so nicely be Paschal.

There must be value in studying more closely the retriggering of periodic emissions. The replication of successive echoes, with mostly only minor changes, takes place beginning at the leading edge of the original embryonic echoing emission, and after that at times corresponing to the whistler mode two hop delay. This point and others like it deserve more careful attention.

Bibliography

Allcock, G. M. (1959), The electron density distribution in the outer ionosphere derived from whistler data, *J. Atmos. Terr. Phys.*, *14*, 185–99.

Alpert, Y. (1980), 40 years of whistlers, *J. Atmos. Terr. Phys.*, *42*, 1–20.

Angerami, J. J. (1970), Whistler duct properties deduced from vlf observations made with the ogo 3 satellite near the magnetic equator, *J. Geophys. Res.*, *75*, 6115–35.

Angerami, J. J., and D. L. Carpenter (1966), Whistler studies of the plasmapause in the magnetosphere, 2, electron density and total tube rlectron content near the knee in magnetospheric ionization, *J. Geophys. Res.*, *71(3)*, 711–35.

Angerami, J. J., and J. O. Thomas (1964), Studies of planetary atmospheres, 1, the distribution of electrons and ions in the earth's exosphere, *J. Geophys. Res.*, *69*, 4537–60.

Axford, W., and C. W. Hines (1960), A unifying theory of high latitude geophysical phenomena and geomagnetic storms, *Canad.. J. Phys.*, *39*, 1433–64.

Banks, P. M., and T. E. Holzer (1969), High-latitude plasma transport: the polar wind, *J. Geophys. Res.*, *74*, 6317–?

Barkhausen, H. (1919), Zwei mit hilfe der neuen verstärker entdeckte erscheinungen, *Physic. Z.., 20 401-3*, *20*, 401–3.

Barrington, R. E., and J. S. Belrose (1963), Preliminary results from the very-low-frequency receiver aboard canada's alouette satellite, *Nature*, *198*, 651–56.

Bell, T. F. (1964), Time reversal of the geocyclotron mechanism, *J. Geophys. Res.*, *69*, 177–79.

Bell, T. F., and O. Buneman (1964), Plasma instability in the whistler mode caused by a gyrating electron stream, *Phys. Rev.*, *133*, 1300–02.

Bell, T. F., and R. Helliwell (1971), Pulsation phenomena observed in long-duration vlf whistler-mode signals, *J. Geophys. Res.*, *76*, 8414–19.

Bernhardt, P. A., and C. G. Park (1977), Protonospheric-ionospheric modeling of vlf ducts, *J. Geophys. Res.*, *82*, 5222–?

Block, L. P., and D. L. Carpenter (1974), Derivation of magnetospheric electric fields from whistler data in a dynamic geomagnetic field., *J. Geophys. Res.*, *79*, 2783–89.

Brice, N. M. (1962), Discussion of paper by r. l. dowden, 'doppler shifted cyclotron radiation from electrrons: a theory of very low frequency emissions from the exosphere', *J. Geophys. Res.*, *67(12)*, 4897–99.

Brice, N. M. (1964), Fundamentals of very low frequency generation mechanisms, *J. Geophys. Res.*, *69*, 4515–22.

Brice, N. M. (1965), Multiphase periodic very-low-frequency emissions, *Radio Science*, *69D*, 257–65.

Brice, N. M. (1967), Bulk motion of the magnetosphere, *J. Geophys. Res.*, *72*, 5193–211.

Brice, N. M., and R. L. Smith (1965), Lower hybrid resonance emissions, *J. Geophys. Res.*, *70*, 71–80.

Brittain, R., P. M. Kintner, M. C. Kelley, J. C. Siren, and D. L. Carpenter (1983), Standing wave patterns in vlf hiss, *J. Geophys. Res.*, *88*, 7059–64.

Burtis, W. J. (1973), Electron concentrations calculated from the lower hybrid resonance noise band observed by ogo 3, *J. Geophys. Res.*, *78*, 5515–23.

Burtis, W. J. (1974), Magnetospheric chorus, Ph.D. thesis, Stanford University.

Burtis, W. J., and R. A. Helliwell (1969), Banded chrous–a new tpe of vlf radiation observed in the magnetosphere by ogo 1 and ogo 3, *J. Geophys. Res.*, *74*, 3002–10.

Burtis, W. J., and R. A. Helliwell (1975), Magnetospheric chorus - amplitude and growth rate, *J. Geophys. Res.*, *80*, 3265–70.

Burtis, W. J., and R. A. Helliwell (1976), Magnetospheric chorus - occurrence patterns and normalized frequency, *Plan. Space Sci.*, *24*, 1007–24.

Carpenter, D. L. (1962), New experimental evidence of the effect of magnetic storms on the magnetosphere, *J. Geophys. Res.*, *67(1)*, 135–45.

Carpenter, D. L. (1963), Whistler evidence of a 'knee' in the magnetospheric ionization density profile, *J. Geophys. Res.*, *68(6)*, 1675–82.

Carpenter, D. L. (1966), Whistler tudies of the plasmapause in the magnetosphere, 1, temporal variations in the position of the knee and some evidence on plasma motions near the knee, *J. Geophys. Res.*, *71(3)*, 693–709.

Carpenter, D. L. (1968), Ducted whistler-mode propagation in the magnetosphere a half-ggyrofrequency upper intensity cutoff and some associated wave growth phenomena, *J. Geophys. Res.*, *73*, 2919–?

Carpenter, D. L. (1971), Ogo 2 and 4 vlf observations of the asymmetric plasmapause near the time of sar arc events, *J. Geophys. Res.*, *76*, 3644–50.

Carpenter, D. L. (1978), Whistlers and vlf noises propagating just outside the plasmapause, *J. Geophys. Res.*, *83*, 45–57.

Carpenter, D. L. (1980), Fast fluctuations in the arrival bearing of magnetospherically propagating signals from the siple, antarctica, vlf transmitter, *J. Geophys. Res.*, *85*, 4157–66.

Carpenter, D. L. (1981), Active and passive vlf experiments at siple station, 1980-1981, *Antarctic Journal*, *XVI*, 211–12.

Carpenter, D. L. (1981a), A study of the outer limits of ducted whistler propagation in the magnetosphere, *J. Geophys. Res.*, *86*, 839–45.

Carpenter, D. L., and N. Dunckel (1965), A dispersion anomaly in whistlers received on alouette 1, *J. Geophys. Res.*, *70*, 3781–86.

Carpenter, D. L., and S. Lasch (1969), An effect of a transmitter frequency increase on the occurrence of vlf noise triggered near l = 3 in the magnetosphere, *J. Geophys. Res.*, *74*, 1859–62.

Carpenter, D. L., and T. M. Miller (1976), Ducted magnetospheric propagation of signals from the siple, antarctica, vlf transmitte, *J. Geophys. Res.*, *81*, 2692–2700.

Carpenter, D. L., and C. G. Park (1973), On what ionospheric workers should know about the plasmapause-plasmasphere, *Rev. Geophys. Space Phys.*, *11*, 133–54.

Carpenter, D. L., and N. T. Seely (1976), Cross-l plasma drifts in the outer plasmasphere: quiet-time patterns and some substorm effects, *J. Geophys. Res.*, *81*, 2728–36.

Carpenter, D. L., and R. L. Smith (1961), Extension of nose whistler analysis, *J. Geophys. Res.*, *66(8)*, 2582–86.

Carpenter, D. L., and R. L. Smith (1964), Whistler measurements of electron density in the magnetosphere, *Rev. Geophys..*, *2(3)*, 415–41.

Carpenter, D. L., and K. Stone (1967), Direct detection by a whistler method of the magnetospheric electric field associated with a polar substorm, *Plan. Space Sci.*, *15*, 395–7.

Carpenter, D. L., and D. M. Šulić (1988), Ducted whistler propagation outside the plasmapause, *J. Geophys. Res.*, *93*, 9731–42.

Carpenter, D. L., N. Dunckel, and J. F. Walkup (1964), A new very low frequency phenomenon: Whistlers trapped below the protonosphere, *J. Geophys. Res.*, *69*, 5014–19.

Carpenter, D. L., F. Walter, R. E. Barrington, and D. J. McEwen (1968), Alouette 1 and 2 observations of abrupt changes in whistler rate and of vlf noise variations at the plasmapause–a satellite-ground study, *J. Geophys. Res.*, *73*, 2929–40.

Carpenter, D. L., K. Stone, and S. Lasch (1969), A case of artificial triggering of vlf magnetospheric noise during the drift of a whistler duct across magnetic shells, *J. Geophys. Res.*, *74*, 1848–55.

Carpenter, D. L., C. G. Park, J. F. Arens, and D. W. Williams (1971), Position of the plasmapause during a stormtime increase in trapped (e> 280 kev) electrons, *J. Geophys. Res.*, *76*, 4669–73.

Carpenter, D. L., K. Stone, J. C. Siren, and T. L. Crystal (1972), Magnetospheric electric fields deduced from drifting whistler paths, *J. Geophys. Res.*, *77*, 2819–34.

Carpenter, D. L., J. C. Foster, T. J. Rosenberg, and L. J. Lanzerotti (1975), A subauroral and midlatitude view of substorm activity, *J. Geophys. Res.*, *80*, 4279–86.

Carpenter, D. L., D. M. Šulić, and A. J. Smith (1986), The third hop whistler anomaly: possible evidence of df/dt-dependent wave amplification, *J. Atmos. Terr. Phys.*, *48*, 171–79.

Carpenter, G. B. (1964), Vlf amplitude perturbations associated with high-altitude nuclear explosions, *Tech. rep.*, Stanford University, CA, published by SRI International, Menlo Park, CA.

Carpenter, G. B. (2008), *A brief history of the measurement of EMP during atmospheric nuclear tests, Vol. 1 US tests, Vol. 2 Foreign tests*, SRI International, Menlo Park, CA.

Carpenter, G. B., J. P. Katsufrakis, and I. Kimura (1966), Midlatitude observations of nighttime vlf signal-amplitude anomalies associated with magnetic bays, *J. Geophys. Res.*, *71*, 1928–37.

Chang, D. C. D., and R. A. Helliwell (1079), Emission triggering in the magnetosphere by controlled interruption of coherent vlf signals, *J. Geophys. Res.*, *84*, 7170–76.

Clark, W. L., J. R. McAfee, R. Norton, and J. M. Warnock (1969), Radio wave reflections from large horizontal gradients in the topside ion-exosphere, *Proc IEEE.*, *57*, 493–96.

Corcuff, P., Y. Corcuff, D. L. Carpenter, C. R. Chappell, J. Vigneron, and K. Kleimenova (1972), La plasmasphere en periode de recouvrement magnetique. etude combinee des donnees des satellites ogo 4 and ogo 5 at des sifflements recus au sol, *Annales de Géophysique*, *28*, 679–95.

Cornilleau-Wehrlin, N., and R. Gendrin (1979), Vlf transmitter-induced quiet bands: a qualitative interpretation, *J. Geophys. Res.*, *84*, 882–?

Dantas, N. H. (1972), Ogo-4 satellite observations of whistler-mode propagation effects associated with caustics in the magnetosphere, Ph.D. thesis, Stanford University.

Dejnakarintra, M., and C. G. Park (1974), Lightning-induced electric fields in the ionosphere, *J. Geophys. Res.*, *79*, 1903–10.

Dingle, B., and D. L. Carpenter (1981), Electron precipitation induced by vlf noise bursts at the plasmapause and detected at conjugate ground stations, *J. Geophys. Res.*, *86*, 4597–606.

Doolittle, J. R., and D. L. Carpenter (1983), Photometric evidence of electron precipitation induced by first hop whistlers, *Geophys Res. Lett.*, *10*, 611–14.

Dowden, R. L. (1962), Doppler-shifted cyclotron radiation from electrons: A theory of very low frequency emissions from the exosphere, *J. Geophys. Res.*, *67(5)*, 1745–50.

Dunckel, N., and R. A. Helliwell (1969), Whistler-mode emissions on the ogo 1 satellite, *J. Geophys. Res.*, *74*, 6371–85.

Dungey, J. W. (1955), Electrodynamics of the outer atmosphere, in *Report of the Physical Society Conference on the Physics of the Ionosphere, Physical Society, London*.

Dungey, J. W. (1963), Loss of van allen electrons due to whistlers, *Plan. Space Sci.*, *11*, 591–?

Dungey, J. W. (1967), The theory of the quiet magnetosphere, in *Proceedings of the 1966 Symposium on Solar-Terrestrial Physics, Belgrade, eds King, J.W. and Newman, W.S.*

Eckersley, T. J. (1935), Musical atmospherics, *Nature*, *135*, 104–5.

Edgar, B. C. (1972), The structure of the magnetosphere as deduced from magnetospherically reflected whistlers, Ph.D. thesis, Stanford University.

Edgar, B. C. (1976), The upper- and lower-frequency cutoffs of magnetospherically reflected whistlers, *J. Geopjys. Res.*, *81*, 205–11.

Eviatar, A., A. M. Lenchek, and S. F. Singer (1964), Distribution of density in an ion-exosphere of a nonrotating planet, *Phys. Fluids*, *7*, 1775–79.

Füllekrug, M. (2002), Intense oceanic lightning, *Ann. Geophysicae*, *20*, 133–37.

Gallet, R. M., and R. A. Helliwell (1959), Oregin of "very low frequency" emissions, *J. Res. NBS*, *63(D)*, 21–7.

Geisler, J. E. (1967), On the limiting flux of daytime ionization into the plotonosphere, *J. Geophys. Res.*, *72*, 81–5.

Geisler, J. E., and S. A. Bowhill (1965), An investigation of ionosphere-protonosphere coupling, *Tech. rep.*, University of Illinois.

Gringauz, K. I. (1963), The structure of the ionized gas envelope of earth from direct measurements in the ussr of local charged particle concentrations, *Plan. Space Sci.*, *11*, 281–96.

Gringauz, K. I., and V. V. Bezrukikh (1976), Asymmetry of the earth's plasmasphere in the direction noon-midnight from prognoz-1 and prognoz-2 data, *J. Atmos. Terr. Phys.*, *38*, 1071–6.

Gringauz, K. I., V. G. Kurt, V. I. Moroz, and I. S. Shklovsky (1960), Ionized gas and fast electrons near the earth and in the interplanetary space, *Doklady Akademiya Nauk. SSSR*, *132*, 1062–65.

Gurnett, D. A., S. D. Shawhan, N. M. Brice, and R. L. Smith (1965), Ion cyclotron whistlers, *J. Geophys. Res.*, *70*, 1665–88.

Guthart, H. (1964), Whistlers in a thermal magnetosphere, Ph.D. thesis, Stanford University.

Guthart, H. (1965a), Nose whistler dispersion as a measure of magntospheric elctron temperature, *Radio Science*, *69D*, 1417–?

Guthart, H. (1965b), An anisotropic electron velocity distribution for the cyclotron absorption of whistlers and vlf emissions, *Radio Science*, *69D*, 1403–?

Guy, A. W. (1966), The characterisstics of long antennas buried in polar ice, *Tech. rep.*, U. of Washington.

Hanson, W. B. (1964), Dynamic diffusion process in the exosphere, in *Electron Density Distribution in the Ionosphere and Exosphere*.

Hanson, W. B., and I. B. Ortenburger (1961), The coupling between the protonosphere and the normal f region, *J. Geophys. Res.*, *66*, 1425–35.

Helliwell, R. (1963), Coupling between the ionosphere and the earth-ionosphere waveguide at very low frequencies, in *Proceedings of the International Conference on the Ionosphere, London, July 1962*.

Helliwell, R., J. H. Pope, J. H. Crary, and R. L. Smith (1956), The "nose" whistler–a new high latitude phenomenon, *J. Geophys. Res.*, *61(1)*, 139–43.

Helliwell, R., J. P. Katsufrakis, M. L. Trimpi, and B. N. M. (1964), Artificially stimulated rery low frequency radiation from the ionosphere, *J. Geophys. Res.*, *69*, 2391–94.

Helliwell, R. A. (1965), *Whistlers and related ionospheric phenomena*, Stanford Press.

Helliwell, R. A. (1967), A theory of discrete vlf emissions from the magnetosphere, *J. Geophys. Res.*, *72*, 4273–?

Helliwell, R. A. (1993), Forty years of whistlers, *xx*, *xx*, xx.

Helliwell, R. A., and T. F. Bell (1960), A new mechanism for accelerating electrons in the outer ionosphere, *J. Geophys. Res.*, *65*, 1839–?

Helliwell, R. A., and D. L. Carpenter (1961), Whistlers west–results from the igy/igc-59 synoptic program, *IGY Bull., NAS*, *57*, 1–9.

Helliwell, R. A., and D. L. Carpenter (1963), Whistlers excited by nuclear explosions, *J. Geophys. Res.*, *68(15)*, 4409–20.

Helliwell, R. A., and T. L. Crystal (1973), A feedback model of cyclotron interaction between whistler-mode waves and energetic electrons in the magnetosphere, *J. Geophys. Res.*, *78*, 7357–71.

Helliwell, R. A., and E. Gehrels (1958), Observations of magneto-ionic duct propagation using man-made signals of very low frequency, *Proc. IRE.*, *46(4)*, 785–87.

Helliwell, R. A., and J. P. Katsufrakis (1974), Vlf wave injection into the magnetosphere from siple station, antarctica, *J. Geophys. Res.*, *79*, 2511–18.

Helliwell, R. A., and J. P. Katsufrakis (1978), *Upper Atmosphere Research in Antarctica*, chap. 5. Controlled wave-particle interaction experiments, pp. 100–129, AGU.

Helliwell, R. A., J. P. Katsufrakis, and M. L. Trimpi (1973), Whistler-induced amplitude perturbation in vlf propagation, *J. Geophys. Res.*, *78*, 4679–88.

Helliwell, R. A., J. P. Katsufrakis, and R. Bell, T. F.and Raghuram (1975), Vlf line radiation in the earth's magnetosphere and its association with power system radiation, *J. Geophys. Res.*, *80*, 4249–58.

Helliwell, R. A., J. H. Mende, S. B. Doolitle, W. C. Armstrong, and D. L. Caarpenter (1980), Correlation between λ4278 optical emissions and vlf wave events observed at l$\simeq$ 4 in the antarctic, *J. Geophys. Res.*, *85*, 3376–86.

Helms, w. J., H. M. Swarm, and D. K. Reynolds (1968), An experimental study of the polar lower ionosphere using very low frequency waves, *Tech. rep.*, U. of Washington.

Heyborne, R. L., R. L. Smith, and R. A. Helliwell (1969), Latitudinal cutoff of vlf signals in the ionosphere, *J. Geophys. Res.*, *74*, 2493–97.

Hines, C. O. (1957), Heavy-ion effects in audio-frequency radio propagation, *J. Atmos. Terr. Phys.*, *11*, 36–42.

Ho, D. (1973), Interaction between whistlers and quasi-periodic vlf emissions, *J. Geophys. Res.*, *78*, 7347–7356.

Ho, D. (1974), Quasi-periodic (qp) vlf emissions in the magnetosphere, Ph.D. thesis, Stanford University.

Ho, D., and L. C. Bernard (1973), A fast method to determine the nose frequency and minimum group delay of a whistler when the causative spheric is unknown, *J. Atmos. Terr. Phys.*, *35*, 881–?

Ho, D., and D. L. Carpenter (1976), Outlying plasmasphere structure detected by whistlers, *Plan. Space Sci.*, *24*, 987–94.

Inan, T. F., U. S.and Bell, D. L. Caarpenter, and R. R. Anderson (1977), Explorer 45 and imp 6 observations in the magnetosphere of injected waves from the siple station vlf transmitter, *J. Geophys. Res.*, *82*, 1177–87.

Inan, U. S., and R. R. Bell, T. F.and Anderson (1977a), Cold plasma diagnostics using satellite measurements of vlf signals from ground transmitters, *J. Geophys. Res.*, *82*, 1167–76.

Inan, U. S., and T. F. Bell (1977b), The plasmapause as a vlf wave guide, *J. Geophys. Res.*, *82*, 2819–27.

Inan, U. S., T. F. Bell, and R. A. Helliwell (1978), Nonlinear pitch angle scattering of energetic electrons by coherent vlf waves in the magnetosphere, *J. Geo*, *83*, 3235–53.

Karpman, V. I., J. N. Istomin, and D. R. Shklyar (1974), Nonlinear theory of a quasimonochromatic whistler mode packet in inhomogeneous plasma, *Plasma Phys.*, *16*, 685–?

Kennel, C. F., and H. E. Petschek (1966), Limit on stably trapped particle fluxes, *J. Geophys. Res.*, *71*, 1.

Kimura, I. (1966), Effects of ions on whistler-mode ray tracing, *Radio Science*, *1(3)*, 269–83.

Kimura, I. (1968), Triggering of vlf magnetospheric noise by a low power (∼ 100 watt) transmitter, *J. Geophys. Res.*, *73*, 445–7.

Kimura, I., R. L. Smith, and B. N. M. (1965), An interpretation of transverse whistlers, *J. Ge*, *70*, 5961–66.

King, J. W., K. C. Reed, E. O. Obatunji, and A. J. Legg (1967), The behavior of the topside ionosphere during storm conditions, *J. Atmos. Terr. Phys.*, *29*, 1355–?

Kintner, P. M., R. Brittain, M. C. Kelley, D. L. Carpenter, and M. J. Rycroft (1983), In-situ measurements of.transionospheric vlf wave injection.., *J. Geophys. Res.*, *88*(X), 7065–73.

Laaspere, T., M. Morgan, and W. Johnson (1963), Some results of five years of whistler observations from labrador to antarctica, *Proc IEEE.*, *51(4)*, 554–68.

Lasch, S. (1969), Unique features of vlf noise triggered in the magnetosphere by morse-code dashes from naa, *J. Geophys. Res.*, *74*, 1856–58.

Leavitt, M. L., D. L. Carpenter, N. T. Seely, R. R. Padden, and J. R. Doolittle (1978), Initial results from a tracking receiver direction finder for whistler-mode signals, *J. Geophys. Res.*, *83*, 1601–10.

Lemaire, J. E., and K. I. Gringauz (1998), *The Earth's Plasmasphere*, Cambridge University Press.

Liemohn, H. B., and F. L. Scarf (1062), Exospheric electron temperatures from nose whistler attenuation, *J. Geophys. Res.*, *67*, 2785–?

Ludwig, G. (2011), *Opening Space Research*, American Geophysical Union.

Mathews, D. L. (1981), Siple station magnetospheric physics campaign, *Antarctic Journal*, *XVI*, 202–3.

Matsumoto, H., and I. Kimura (1971), Linear and nonlinear instabilities and vlf emissions in the magnetosphere, *Plan. Space Sci.*, *19*, 567–?

Mlodnosky, R. F., D. L. Carpenter, and R. A. Helliwell (1962), Non-thermal noise measurements near planets, in *La Physique des Planètes. L'Université de Liège*.

Morgan, M. G., and G. M. Allcock (1956), Observations of whistling atmospherics at geomagnetically conjugate points, *Nature*, *177(4497)*, 30–1.

Muzzio, J. L. R. (1968), Ion cutoff whistlers, *J. Geophys. Res.*, *73*, 7526–29.

Muzzio, J. L. R., and J. J. Angerami (1972), Ogo 4 observations of extremely low frequency hiss, *J. Geophys. Res.*, *77*, 1157–73.

Nishida, A. (1966), Formation of plasmapause, or magnetospheric plasma knee, by the combined action of magnetospheric convection and plasma escape from the tail, *J. Geophys. Res.*, *71*, 5669–79.

Nunn, D. (1974), A self-consistent theory of triggered vlf emissions, *J. Geophys. Res.*, *22*, 349–?

Park, C. G. (1970a), A whistler study of the interchange of ionization betweeen the ionosphere and the protonosphere, Ph.D. thesis, Stanford University.

Park, C. G. (1970b), Whistler observations of the exchange of ionization betweeen the ionosphere and protonosphere, *J. Geophys. Res.*, *75*, 4249–60.

Park, C. G. (1973), Whistler observations of the depletion of the protonosphere during a magnetospheric substorm, *J. Geophys. Res.*, *78*, 672–83.

Park, C. G. (1974a), Some features of plasma distribution in the plasmasphere deduced from antarctic whistlers, *J. Geophys. Res.*, *79*, 169–73.

Park, C. G. (1974b), A morphological study of substorm associated disturbances in the ionosphere, *J. Geophys. Res.*, *79*, 2821–26.

Park, C. G., and D. L. Carpenter (1970), Whistler evidence of large-scale electron-density irregularities in the plasmasphere, *J. Geophys. Res.*, *75*(Park, C. G. and Carpenter, D. L.), 3825–36.

Park, C. G., and M. Dejnakarintra (1973), Penetration of thundercloud electric fields into the ionosphere and magnetosphere, 1. middle and subauroral latitudes, *J. Geophys. Res.*, *78*, 6623–33.

Park, C. G., and R. A. Helliwell (1971), The formation by electric fields of field-aligned irregularities in the magnetosphere, *Radio Science*, *6*, 299–?

Park, C. G., D. L. Carpenter, and D. B. Wiggin (1978), Electron density in the plasmasphere: whistler data on solaar cycle, annual, and diurnal variations, *J. Geophys. Res.*, *83*, 3137–44.

Paschal, E. (1988), Phase measurements of very low frequency signals from the magnetosphere, Ph.D. thesis, Stanford University.

Potemra, T. A., and T. J. Rosenberg (1973), Vlf propagation disturbances and electron precipitation at mid-latitudes, *J. Geophys. Res.*, *78*, 1572–?

Raghuram, R. (1975), A new interpretation of subprotonospheric whistler characteristics, *J. Geophys. Res.*, *80*, 4729–31.

Raghuram, R., R. L. Smith, and T. F. Bell (1974), Vlf antarctic antenna impedance and efficiency, *IEEE Trans. Antennas Propag.*, *AP-22*, 334–?

Raghuram, R., T. F. Bell, R. Helliwell, and J. P. Katsufrakis (1977), Echo-induced suppression of coherent vlf transmitter signals in the magnetosphere, *J. Geophys. Res.*, *82*, 2787–?

Ratcliffe, J. A. (1969), *An Introduction to the ionosphere and magnetosphere*, Cambridge University Press.

Rishbeth, H., and O. K. Garriot (1969), *Introduction to ionospheric physics*, Academic Press.

Rorden, L. H., R. A. Helliwell, and R. L. Smith (1963), An interpretation of lofti-1 vlf observations, in *AGARDOGRAPH 74*.

Rosenberg, T. J., and F. A. Saus (1974), Vlf phase perturbations associated with geomagnetic activity, in *ELF-VLF Radio Wave Propagation, ed. by J.A. Heltet*.

Rosenberg, T. J., R. A. Helliwell, and J. P. Katsufrakis (1971), Electron precipitation associated with discrete very-low-frequency emissions, *J. Geophys. Res.*, *76*, 8445–52.

Roux, A., and R. Pellat (1978), A throry of triggered emissions, *J. Geophys. Res.*, *83*, 1433–?

Scarabucci, R. R. (1969a), Interpretation of vlf signals observed on the ogo-4 satellite, Ph.D. thesis, Stanford University.

Scarabucci, R. R. (1969b), Analytical and numerical treatment of wave propagation in the lower ionosphere, *Tech. rep.*, Stanford University, S.E.L.-69-046.

Scarabucci, R. R. (1970), Satellite observations of equatorial phenomena and defocusing of vlf electromagnetic waves, *J. Geophys. Res.*, *79*, 69–84.

Scarabucci, R. R., and R. L. Smith (1970), Full wave treatment of wave propagation through planar irregularities in a magnetoionic medium, *Tech. rep.*, Stanford University, SEL-70-046.

Scarf, F. L. (1962), Landau damping and the attenuation of whistlers, *Phys. Fluids*, *5*, 6–?

Schunk, R. W., and A. F. Nagy (2000), *Ionospheres*, Cambridge University Press.

Serbu, G. P., and E. J. R. Maier (1967), Thermal plasma measurements within the magnetosphere, *Space Research VII*, *1*, 527–34.

Shawhan, S. D. (1966), Vlf ray tracing in a model magnetosphere, Ph.D. thesis, U. of Iowa.

Siren, J. C. (1972), Dispersive aurorral hiss, *Nature Phys, Sci*, *238*, 118–?

Siren, J. C. (1974), Fast hisslers: a form of magnetospheric radio emissions, Ph.D. thesis, Stanford University.

Smith, R. L. (1961), Propagation characteristics of whistlers trapped in field-aligned columns of enhanced ionization, *J. Geophys. Res.*, *66(11)*, 3699–3707.

Smith, R. L. (1964), An explanation of subprotonospheric whistlers, *J. G*, *69*, 5019–21.

Smith, R. L., and J. J. Angerami (1968), Magnetospheric properties deduced from ogo 1 observations of ducted and non ducted whistlers, *J. Geophys. Res.*, *73*, 1–20.

Smith, R. L., and N. M. Brice (1964), Propagation in multi-component plasmas, *J. Geophys. Res.*, *69*, 5029–40.

Smith, R. L., and R. Helliwell (1960), Electron densities to 5 earth radii deduced from nose whistlers, *J. Geophys. Res.*, *65(9)*, 2583.

Smith, R. L., R. A. Helliwell, and I. Yabroff (1960), A theory of trapping of whistlers in field-aligned columns of enhanced ionization, *J. Geophys. Res.*, *65(3)*, 815–23.

Stiles, G. S., and R. A. Helliwell (1975), Frequency-time behavior of artificially stimulated vlf emissions, *J. Geophys. Res.*, *80*, 608–18.

Storey, L. (1958), Protons outside the earth's atmosphere, *Annales de Géophysique*, *14*, 144–53.

Storey, L. R. O. (1953), An investigation of whistling atmospherics, *Phil. Trans. Royal Soc. (London) A*, *246*, 113–41.

Sudan, R. N., and E. Ott (1971), Theory of triggered vlf emissions, *J. Geophys. Res.*, *76*, 4465–?

Taylor, H., H. C. Brinton, and C. B. Smith (1965), Positive ion composition in the magnetosphere obtained from the ogo-a satellite, *J. Geophys. Res.*, *70*, 5769–81.

Taylor, H. A., H. C. Brinton, D. L. Carpenter, F. M. Bonner, and R. L. Heyborne (1969), Ion depletion in the high-latitude exosphere; simultaneous ogo 2 observations of the light ion trough and the vlf cutoff, *J. Geophys. Res.*, *74*, 3517–28.

Ungstrup, I. M., and D. L. Carpenter (1974), 'hisslers": quasi-periodic(t *simeq*2*s* vlf noise forms at auroral latitudes, *J. Geophys. Res.*, *79*, 5196–201.

Walter, F., and J. J. Angerami (1969), Noncucted mode of vlf propagation between conjugate hemispheres: observations on ogo's 2 and 4 of the 'walking-trace' whistler and of doppler shifts of fixed fequency transmissions, *J. Geophys. Res.*, *74*, 6352–70.

Yabroff, I. (1961), Computation of whistler ray paths, *J. Res. NBS*, *65D(5)*, 485–505.