Hunting the Northern Lights
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Abstract
When a tragic accident occurred in November 1962 in the coalmine of Kings Bay Company in Ny-Ålesund, Svalbard, (78°55′N, 11°56′E) it led to the fall of the labour government that had been ruling Norway since the Second World War. A year later the mine was closed, the infrastructure left unattended, and the community of Ny-Ålesund evacuated. The Norwegian government was then facing a challenge as it had to establish a new activity in the village in order to keep the sovereignty over the territory. This was in the middle of the Cold War. The Russian population on Svalbard was about twice as large as the Norwegian, while the production of the Russian mines was approximately half the outcome of the Norwegians. An empty village with well-developed infrastructure on Svalbard was therefore an enticement for the Soviet Union.

1. Beginning in the High Arctic
When a tragic accident occurred in November 1962 in the coalmine of Kings Bay Company in Ny-Ålesund, Svalbard, (78°55′N, 11°56′E), it led to the fall of the labor government that had been ruling Norway since the Second World War. A year later, the mine was closed, the infrastructure left unattended, and the community of Ny-Ålesund evacuated. The Norwegian government was then facing a challenge as it had to establish a new activity in the village in order to keep the sovereignty over the territory. This was in the middle of the Cold War. The Russian population on Svalbard was about twice as large as the Norwegian, while the production of the Russian mines was approximately half the outcome of the Norwegians. An empty village with well-developed infrastructure on Svalbard was therefore an enticement for the Soviet Union.

Luckily for Norway, the European Space Research Organisation was planning to launch two polar-orbiting satellites for auroral research and was looking for a place to install a satellite tracking station at high latitudes. Ny-Ålesund was chosen as the preferred site and the Norwegian government signed, in Paris in 1965, an agreement with European Space Research Organisation to establish such a facility there.

The Auroral Observatory in Tromsø, which had since the Second International Polar Year in 1931/32 been operating field stations on and off at different places on Svalbard, decided in 1966 to move its station from Isfjord Radio to Ny-Ålesund in order to take advantage of the research community that was under development there. The director of the observatory Professor Anders Omholt advertised for two positions to be employed as an engineer and a research assistant, respectively, at the new station to be established.

I was a master student in physics at the University of Oslo at the time and was offered the research assistant position.

2. Installing Ourselves in Ny-Ålesund
We stayed in Tromsø for a month in the summer of 1966 and were introduced to the many instruments we were engaged to take care of during the coming winter. Leaving Tromsø on a cargo boat on 10 July, we enjoyed the birdlife at far distances from the coast. We were watching the petrels wagging their wings just above the water ripples gliding along for distances behind the ship, and we saw whales that were playing among the waves, blowing small fountains to our admiration. When arriving in Ny-Ålesund, all the research equipment had already arrived at the quay. There were magnetometers, ozone meter, All-Sky camera, photometers, riometers, VLF receivers, Omega equipment, and a pile of antennas that we had to install. Since I was going to write my thesis on pulsating aurora, I was especially responsible for keeping the photometers, the All-Sky camera, and the magnetometers operating Figure 1.
We installed ourselves in the abandoned school house and filled one of the rooms with a lot of electronic equipment and erected a number of antennas on the outside (Figure 2). On the tundra behind the village, we placed five small huts to the All-Sky camera, the photometers, and the magnetometers respectively (Figure 3). The magnetometer station that we established has been operating ever since the original instruments, however, have been replaced by new electronic magnetometers. This leaves Ny-Ålesund with one of the longest series of magnetic recording available in the high Arctic.

Ny-Ålesund is today a world center for arctic research as about 20 countries have established their own stations there.

3. Left in the Ice

On 30 October, the last boat left Ny-Ålesund for Norway, and seven lonely men were standing on the quay watching the boat passing the corner. A mechanic, an electrician, a telegraph operator, a chef, and a technician were given the duty to keep watch of the infrastructure in town. We were left by ourselves and forced to endure the company of each other as there would be no chance to escape before the coming spring that appeared to be so far ahead.

The electrician was given the order to provide electricity to three houses only; but as soon as the boat was out of sight, a gleam was observed in a forth house. The well-experienced crew leader who had stayed in Ny-Ålesund more than 15 winters had retreated to his favorite dwelling in the popular part of town called New London. No negotiation would help, so the electrician felt forced to cut the cable. After a while, the electrician had to give in, and the light was shining in the old house again.

The life in Ny-Ålesund was otherwise rather peaceful as I was occupied with my observations when the winter darkness became like a heavy carpet around us. Being a “polar hero” such as me, I was walking in deep darkness without a gun trying to ignore that I was nervously spouting into the black wall in fear of meeting a polar bear. The philosophy was that the bear would see and hear me long before I could fire a shot against him in a haphazard way. We encouraged each other by the idea that the polar bears were more afraid of the human being than we were of them. This was years before we had heard about any polar bear attacking and killing a human, as has happened so many times on Svalbard during the last 40 years.

Every morning, I had to go to the magnetometer hut to change recording paper and to carry the photopapers in a metal box to the photo laboratory. This was the test of my duties. If I had forgotten to wind up the weights in the clock the day before, a black thick stripe would show up across one of the ends of the paper. If I had done my work properly, three curves would wind themselves beautifully along the paper bringing to light whether the aurora had been playing during the previous night or not.

Each day with clear weather, I had to take off the roof of my two huts for optical observations. When it turned on with snow and wind, the huts had to be closed, and I could walk back to my room and work on my study or read some of the more than 50 books I had loaded up in a special trunk. There were titles like

- *A Winter of Our Discontent* and *the Moon is Down* by John Steinbeck,
- *Darkness at Midday* by Arthur Köstler, *No one knows the Night* by H.C. Branner, *The Island* by Aldous Huxley, *The Process* by Frank Kafka, *The Young Dead Ones* by Nordahl Grieg, *To Myself* by Marcus Aurelius, *Rodion Raskolnikov* and *The Brothers of Karamazov* by Fyodor Dostoyevsky, and the complete collection of Henrik Ibsen’s plays and poems.

*The Polar Aurora* by Carl Störmer was also part of my readings. The first half was well readable, but the second one was rather soporific, full of
differential equations as it was describing the motion of a large variety of charged particles in magnetic fields. Students of today must be happy that they can leave the struggle of solving these equations to a dedicated computer program. *Cosmical Electrodynamics: Fundamental Principles* by Hannes Alfvén and Carl Gunne Fälthammar was also part of my syllabus that introduced me to modern plasma physics. I did not know then that I would meet both authors further ahead in my carrier.

Sometimes we played ping-pong in the basement or billiard in the library. The chef was an expert in both plays and especially in ping-pong. Sometimes I happened to beat him once or twice, and I felt very proud. After a few days, it was impossible to match him again. When I was able to beat him the next time, I realized that even the best ping-pong player will have problems with the over-screw after “pulling himself up” by too many screw drivers.

### 4. Waiting for the Christmas Plane

Onboard the last boat that left for Tromsø, there was a pile of letters I had written to my family, friends, and girlfriends in a lonely hope that I would not be forgotten so far out of sight. Just before Christmas, an airplane from Tromsø had landed in Longyearbyen bringing our Christmas mail. Engineer Alfred Tiefenthal had his own airplane situated in Longyearbyen, and according to traditions, he would bring the mail over to us. We activated the snow scooter Evinrude to prepare the airstrip that we did our best to smooth out with our skis. For landing lights, we filled some empty oil drums with wood shaving and oil that we put on fire.

We were watching with excitement in moonlight the plane coming in across the glacier Kongsbreen, the large glacier at the end of Kongsfjord. The landing was excellent, and we received our mailbags safely, but Tiefenthal had to go back to Longyearbyen to catch a second pile. While we were waiting for his return, we went inside and emptied the load. It was mostly old newspapers, but one sack was full of my letters and packages. My writings in the fall had not been in vain, but the six other guys had to stand there crestfallen stretching their expectations for the next flight.

### 5. Russian Visitors

One day in March, two Russian helicopters from Barentsburg landed on the tundra. As we had not seen people since Tiefenthal visited us at Christmas, we politely invited our neighbors to coffee, cakes, and cognac. We soon realized that our treatment was too petty minded as the Russians turned back to their helicopters for more supplies like bread, gherkin, butter, sausage, and not to forget, plenty of vodka.

We had a problem with one of our generators at that time, and the missing spare part was sitting in Longyearbyen. We had no way to transport it to Ny-Ålesund because the Norwegians had no helicopter on Svalbard; the only transport was dog sledges in the wintertime. We brought up our problem to our Russian friends. The only way to communicate was in broken German, but that was enough to get our guests to promise that they would fly to Longyearbyen the next morning and fetch the missing part. The Russians stayed overnight as none of them were in condition to fly the helicopters back to Barentsburg. When they had sobered up in the morning, they went to Longyearbyen and solved our problem. This incident unfortunately came out in the media with the headline “Russians are transporting NATO equipment on Svalbard.” Military equipment was and is strongly prohibited on Svalbard. It was a well-known fact that Soviet Union believed that Norway and NATO were in the process of establishing a center for espionage operation in the satellite tracking station in Ny-Ålesund. In 1965, Prime Minister Aleksej Kosygin brought up this matter with the Norwegian prime minister Einar Gerhardsen and gave him a hard time. The station, however, was completed in 1968 and in operation until 1973.

I had my own experience with the Russian spies on 23 August in 1968, after having acted as stand-in during 3 months in Ny-Ålesund in the summer. My fiancé, that later became my wife, came to Ny-Ålesund on a passenger ship to join me on my journey back to Tromsø. As soon as the ship came alongside the quay
and the gangway was in place, two Russians came strutting ashore and took off toward the satellite tracking station. After entering the ship and when Ny-Ålesund was disappearing in the rear, we sat down in the saloon to enjoy a drink. Then, a well-dressed man came up to us and asked my fiancé whether she would join him to the captain's cabin for a talk. It turned out that he had spotted her on the deck as she was talking to the Russians when they were listening to the radio. Clearly enough, the Russian spies were shadowed by a Norwegian security officer. This was also the time when the Cold War was closest to freezing as Soviet Union had invaded Czechoslovakia on 20 August.

I experienced another event of Russian helpfulness when I visited Ny-Ålesund again in the summer of 1968, and we went across Kongsfjord by boat to a camp in Krossfjord where the Russian geologist Nikitin was carrying out his research. When we were going to return to Ny-Ålesund, our boat would not start. Then, Nikitin offered to tug us back with his small dinghy. It took us more than 2 hr in a beautiful summer night, and we were extremely grateful for the friendly help.

6. An Accident

On a sunny Sunday in April, the telegraphist set out on the snow scooter to look for his spring gun mounted for killing a polar bear. He brought with him a brush and some seal blubber to fresh up the bait. Although he had made the device himself, he was caught by his own trap as the bullet penetrated his forearm during the smearing. As he did not return to town at the agreed time, we went out looking for him and found him driving in a circle out on the tundra, hanging fainted over the scooter. We immediately brought him back to town and called Longyearbyen for help. Tiefenthal came with his plane and was able to bring him safely to the hospital over there.

7. Some Scientific Outcome from the High Arctic

After a year in isolation, I could return to Tromsø and write my theses on the basis of the data I collected in Ny-Ålesund. From photometer recordings of the auroral green line 5,577 Å and the 4,278 Å emissions, I coauthored with Omholt in 1968 my first scientific paper on a study of the relationship between the spatial distribution of these two auroral emissions. It was believed that since the 5577 Å emission was due to excited oxygen atoms in the metastable state O1S with a certain lifetime against radiation and that this state probably was partly excited by a delayed collision process between O2 molecules and O atoms, 5,577 Å would be more smeared out in space than the spontaneous emission at 4,278 Å. In contrast, Romick and Belon in Alaska, a few years earlier, had found that the 4,278 Å emission had a broader profile. In the paper, we found that we were in support of the Alaskan scientists. In retrospect, I am a little doubtful, since the printer that we used to record the intensities was logarithmic and that made it very hard to define the zero levels for the curves.

Another paper that came out of my efforts in Ny-Ålesund was based on some of the very first observations ever made of pulsating aurora in the high Arctic, demonstrating that this phenomenon also occurred at such high latitudes where the field lines were expected to be open. The idea concerning pulsating aurora was that they occurred due to some resonance transport of electrons along closed lines of force and that it was rather unlikely that such oscillations could occur at such high latitudes as Ny-Ålesund.

I stayed at the Auroral Observatory a few years studying pulsating aurora together with Kjell Henriksen and Helge Pettersen. We were correlating observations of the delayed emission at 5,577 Å with the spontaneous emission of 4,278 Å in order to calculate the lifetime of the O1S state in atomic oxygen. These studies followed up on earlier work by Omholt and Harang. As the theoretical lifetime of the O1S state is ~0.70 s and we found values larger than that, we were looking for delayed excitation processes in the O1S state.

8. Preparing for Chatanika

At 26 March in 1968, the Norwegian Parliament (Stortinget) decided to establish a University in Tromsø. The Auroral Observatory had been in activity since 1928 and was an internationally well-known institution with scientists well qualified in mathematics and physics that would form part of the department of natural sciences in the new university. I was brought into the planning process of the department and appointed secretary of a comity headed by Professor Olav Holt.
In the fall of 1971, I received a letter from Egil Leer, who was at the time working as a PhD student under supervision by Professor Juel Fejer at the Institute of Applied Physics and Information Science (APIS) at The University of California in San Diego (UCSD). Leer told me that Professor Peter Banks was looking for a person to fill a research scientist position at UCSD who would be going to work on the Chatanika Radar newly installed close to Fairbanks, Alaska.

I applied for the job, and just before Christmas, I got a letter from Peter offering me the position. It was a big surprise to me, but I understood later from Peter that the fact that I had survived 1 full year in isolation in Ny-Ålesund, 5 years earlier, proved that I was able to stand the rough environment he expected in Alaska. Being a Norwegian, Alaska was a place that attracted my interest, but admittedly, it appeared a long detour to go through San Diego.

I had heard about the plans for an incoherent scatter radar (ISR) in Northern Scandinavia and knew that some of the senior scientists at the observatory were involved in the planning process. I got hold of a copy of the famous Green Book: A European Incoherent Scatter Facility in the Auroral Zone. A Feasibility Study. Proposal for A European Incoherent Scatter Radar in the Auroral Zone (EISCAT), coauthored in June 1971 by persons like F. du Castel from France, O. Holt from Norway, B. Hultqvist from Sweden, H. Kohl from Germany, and M. Tiuri from Finland, who are all well-respected scientists in the European science community at the time. The Green Book had a list of references that lead me to more literature about incoherent scatter theory and radars. Papers that I remember reading to prepare myself for meeting with the experts in the United States were such classical work as follows: A theory of radio scattering in the troposphere by H. G. Booker and W.E. Gordon from 1955, Incoherent scattering of radio waves by free electrons with applications to space exploration by radar by W.E. Gordon from 1958, Scattering of radio waves by an ionized gas in thermal equilibrium in the presence of a uniform magnetic field by J. A. Fejer from 1961, Density fluctuations in a plasma in a magnetic field with application to the ionosphere by T. Hagfors from 1961, and a string of papers by J.P. Dougherty and D. T. Farley with the common title: A Theory of Incoherent Scattering of Radio Waves by a Plasma, published between 1961 and 1966. I had learned about Thomson scattering from my basic physics courses but soon understood that the incoherent scattering we were looking for by the radars was far more difficult to understand as it was not incoherent after all.

9. Working at UCSD

I arrived at APIS at 14 April 1972. Peter was not there, but I met Dr. Joe R. Doupnik, whom I should learn to know pretty well, before my engagement at UCSD was over 2 years later. Just after a week in San Diego, I was asked to join Joe for a trip to Stanford Research Institute (SRI) in Palo Alto to participate in an ISR school. I did not notice any other participant from outside SRI, so I enjoyed a very special course in an extremely friendly atmosphere. I was given the report DNA Project 617 Radar: First Auroral Zone Results written by Murray Baron, Ted Watt, Chuck Rino, and Juris Petriceks. That was my first written introduction to the Chatanika Radar that I was going to work on for the next 2 years (Figure 4).

Here I was listening to Murray Baron giving excellent lectures about the radar technology: klystrons and clutter, range gates and time delays, system temperatures and power profiles, autocorrelation functions, and power spectra. Chuck Rino and Odile de la Beaujardiere explained the online and offline analysis programs and introduced me to the difference between velocities obtained by first moment and matched filters and demonstrated how they could print out all the data information and housekeeping on the Calcomp plotter, a device I first came across at SRI and that I would spend many nights together with in the months to come. Juris Petriceks talked about system temperatures and wave guides, output power and gain, and crowbars and power failure. They all were extremely patient and polite in answering my many silly questions, and in spite of my ignorance, I felt very well accepted as a member of the Chatanika science team.
10. Impressions from Chatanika

SRI had disassembled a mobile ISR that was standing idle in the hills nearby Stanford and installed it in Chatanika close to Fairbanks, Alaska. This radar was originally built to be mounted on a ship to go to the Pacific for observing the ionospheric disturbances that were expected to be formed by the planned atmospheric atomic bomb tests to be carried out. Since these tests became banned, Banks and colleagues figured out that the radar should be moved to the auroral zone where it was believed that the ionospheric disturbances formed there by nature most closely resembled the ionospheric effects of atmospheric atomic bomb tests in the atmosphere.

When I visited SRI a few years later, Walter Chestnut took me into an especially secret room where I was shown some of the photographs from the atomic bomb tests at the Bikini atoll. A few months ago, I read in the news that these more than 50‐year‐old film rolls were now for the first time opened for the public.

The first test measurements at Chatanika had been obtained in July 1971, and a couple of campaigns had taken place in the fall the same year. In February 1972, just before I arrived at UCSD, a very successful campaign had taken place at Chatanika. For the first time, an experiment was carried out with the antenna pointing sequentially into three different positions observing the line of sight ion velocities at different range gates along each antenna beam pointing direction. The observations of these ion velocities opened up a new way of studying the electric fields associated with auroral substorms, and the very first measurements of the E‐region neutral wind were obtained by ISR observations in the auroral zone. Joe had made a program on the Calcomp plotter displaying the velocities in so-called Clock Dial Plots and demonstrated how the velocities turned from westward to eastward around magnetic midnight (Figure 5).

In June 1972, I went to Chatanika for the first time with Joe, and we carried out a campaign together with Chuck Rino and Murray Baron lasting for about 2 weeks. When we drove out from Fairbanks to Chatanika, we passed a huge storage of enormously large pipes at Fox, pipes that later should be welded together as the Alaskan Pipeline.

Chatanika is situated in Tanana Valley where Felix Pedro in 1902 discovered the gold. The gold dredge situated close to Chatanika Lodge still reminded us about the gold rush about 70 years earlier (Figure 6).

The summer of 1972 was extremely hot in Alaska, and it was said that 150 forest fires were burning every day throughout the state. My wife and baby son came over from Norway to Alaska, and we stayed in Town House Motel downtown Fairbanks. The room was at street level and not very well suited for keeping doors and window open to let fresh air enter. It turned out to be an extremely hot place for a little baby.

The campaign came about due to the solar eclipse on the morning of 10 July and gave us several days of very good velocity measurements. At this time, there was a symposium about ISRs in Fairbanks, and here I for the first time met some of the senior people in the field like W. Gordon, H. Carlson, R. Leadabrand, and R. Behnke. Neil Brice, who later so sadly perished in an airplane accident on Pago Pago in 1974, was also there.

During the summer of 1972, Peter stayed with Shin‐Ichi Akasofu at the University of Alaska, and I had little contact with him. I was uncertain about my job and what science problems I should focus on. When finally, Peter returned to UCSD, we discussed about my feature research, and he suggested for me to study the neutral wind in the E‐region. I did not know...
much about the neutral wind at the time, but I grasped the idea at once and started to work on the E-region neutral wind.

The idea was that since we were able to observe the ion drift velocities at different altitudes, we could see the rotation of the drift velocities primarily due to increasing collision frequencies between ions and neutrals by decreasing heights. The ion velocities observed at the uppermost altitudes were reflecting the $E \times B$ drift and could therefore be used to deduce the electric field. In the simplified ion mobility equation, the only unknown was the neutral wind as long as we had reasonable models of the collision frequencies and the magnetic field. By this technique, it would be possible to deduce the neutral wind, although with a crude altitude resolution (~50 km) in the E-region. Based on this idea, I set out to study the neutral wind. The interesting question was rather whether the neutral wind could be influenced in the E-region by geomagnetic and auroral disturbances. Some studies based on rocket experiments by David Rees and Willy Stoffregen had indicated that this really happened. I therefore tried to look for differences between the neutral wind patterns during quiet and disturbed conditions, but I must admit that it was hard to find such a difference. One difficult question was actually what the derived neutral wind data actually represented.

11. Hitting a Very Strong Geomagnetic Storm

In early August 1972, a big solar flare was observed, and fortunately, people from SRI were present at the radar. I was ordered by Joe to go to Alaska to assist in the observations, and we obtained several days of extremely good data also covering a couple of storm sudden commencements and a beautiful Pe5 event following the first SCC on 4 August. It became extremely important to get the data processed as soon as possible because it was clear that the storm was one of the biggest recorded in modern time, and data from the Chatanika radar were very essential to the science community. Therefore, I returned through SRI and spent a week assisting Chuck Rino in analyzing and printing out sheet after sheet of data on the Calcomp plotter.

During this strong magnetic disturbance, we were measuring very large ion velocities or electric fields of the order of 60–70 mV/m that we hardly could believe were true. It was as if the whole ionosphere was in turmoil.

12. My First International Science Meeting

Peter and Joe decided that I could participate in the AGU meeting in San Francisco in December of 1972, to present my work on the E-region neutral winds. This was the first time in my life I was going to present a talk to an international audience in English. I went to the laboratory and printed out on the Calcomp plotter a number of clock dial plots showing the changes in direction of the neutral wind for every half hour, the time it took to complete a full three positions measurement with the radar. I then had to go to the photoshop to get my beautiful drawings photographed and mounted in slide frames.

We worked closely with the SRI group and especially with Chuck and Odile, and a question that was not really solved was the actual direction of the Doppler shifts we measured, whether they were away from or toward the radar. Anyhow, Joe and I believed we had the correct signs in the data we were using and left for the AGU meeting where we met Chuck in the hall, who could tell us about an unfortunate mistake that was recently discovered; in the data set we had available, the signs on the velocities were wrong. I got a terrible feeling and was on the edge of canceling my debut as a science speaker. We got, however, hold of a felt tip pen and tried to indicate the correct directions on the slides. It was an awkward task to explain to the audience that an arrow that pointed toward the center in the clock dial plot was directed southward and not northward as was the natural convention. I was afraid of giving an impression of being more original than would fit a shy Norwegian and was making it through the talk on shivering legs and an overheated head. To my great surprise, the audience was kind with me and asked questions that I felt I was able to answer with satisfaction and I suddenly found myself as an expert in a science field among people far more senior and experienced than myself.

13. A Visit to Chatanika

At this time, Bob Schunk and Ray Park joined the group at UCSD, Bob to work on his theoretical models and Ray to study the field aligned currents by the Chatanika radar. He made some excellent work combing
Chatanika measurements with TRIAD data, but unfortunately, he left the field, and new knowledge about the field aligned currents had to wait. Jim Horowitz (Figure 7) came into the group in the beginning of 1973 as a PhD student under supervision by Joe, and he and I were sent to Chatanika for carrying out experiments, me for the first time being “in command.” Before that, I could always lean on Joe. At this time, a new scheme was introduced by Chuck and Joe, the so-called azimuth scans that improved the time resolution of the complete ion velocity vector by at least a factor of 3.

Jim and I had to run a couple of 36 hr runs, and during one night, the young student disappeared. While surging for him, I found him in deep sleep at the toilet. I promised not to report this incidence back to Joe. During this trip, we were staying at the Chatanika Lodge (Figure 8) by Bud and Mary, a water hole for the men from the bushes. I remember a Saturday night arriving very tired at the crowded lodge after 24 hr watching the displays on the radar. I got my food and prepared myself for the bed by taking a shower. There was only one unlocked shower room with a water closet on the side. While I was standing in the shower, the door went up, and a man came into the bathroom. When he realized that I was in the shower he asked, “How are you?” I answered, “I am naked.” “Fine” he said and sat down on the toilet bowl. I was so chocked that I remained quiet behind the curtain until he had eased his burden. After finishing the shower, I entered the bed room and prepared for sleeping, but that was difficult due to the noise from the bar. Even worse was the couple next door that certainly was having an hour of love to the extent that I could hear their excited breathing.

14. Studies of E-field and Electric Currents in the Ionosphere

Jim and I worked close together and discussed a lot about the relationship between the electric fields, currents, and magnetic fluctuations. I had, based on the electron density profiles, calculated Hall and Pedersen conductivities and also the conductance ratios for all the available Chatanika experiments. We were then discussing how to compare this to the magnetic disturbances.

I was inspired by Chuck and the others at SRI by presenting the Hall and Pedersen currents separately, while Jim suggested rather to derive the currents in a geomagnetic coordinate frame. I followed his idea and got extremely good correspondence between the eastward currents derived from the Chatanika measurements and the magnetic H-component at College. From the observations on 13–14 March 1972, we derived a result showing that the ratio between the height-integrated Hall and Pedersen conductivities was very similar to the ratio $E_x/E_y$ between the northward and eastward electric field components, respectively. This supported very strongly the theory by Rolf Bostrom that the auroral electrojet was a polarized current (Figure 9). I was so impressed by this result that I contacted Yoshuke Kamide, whom I knew was studying ionospheric currents from magnetograms with Akasofu. When we compared our data, we felt excited like we had discovered a deep secret. Together we were able to publish a couple of papers in JGR based on a combination of radar and magnetometer data.

15. Meeting with Scientists

At the Geophysical Institute of Alaska in Fairbanks, I also met many well-known physicists in the field of auroral science. It was Akasofu of course, but there was Neil Davis, the well-known auroral physicist and director of the institute at the time. The white-haired Eugen Westcott with his impressive and well-trimmed beard was well-known from his many rocket measurements hunting for the electric field in the ionosphere. There was Al Belon and Jerry Romick, my heroes from auroral
triangulation who had demonstrated the intriguing difference between the volume emission rate profiles of 5,577 Å and 4,278 Å. There was Fred Rees, who had calculated excitation profiles of the different auroral emissions based on incoming electron beams with a variety of energy spectra. Charles Wilson had impressed me by demonstrating a relationship between infrasound waves observed by microphones set out in an array when an auroral arc passed supersonically overhead. I had the feeling that this was a result obtained on the ground of first principles in physics that are hard to get in our times when most is hidden in advanced and obscure computer codes. I wish we could see more of this kind of classical fundamental physical reasoning. Howard Bates was there, and so was Robert Hunsucker, both involved in the Chatanika radar experiment.

Charles Deehr was there, and so were Hans Stenbeck Nielsen and Thomas Hallinan who were working on auroral imaging with high-resolution TVs. Most outstanding were their observations of conjugate aurora from airplanes and their extremely thin pulsating layers at unexpected low altitudes. There was a lot to learn from these guys at the Geophysical Institute, but so it was from the NOAA station close by where we went to pick up the magnetometers when we had made an experiment at Chatanika. The observatory impressed me by the variety of magnetometers that was in use there, and I enjoyed especially seeing some of the huge induction coils I had read about in H. H. Campbells papers.

At APIS at the time, there was a good group of extraordinarily skilled scientists like Hannes Alfvén, Ian Axford, Henry Booker, William Coles, Jules Fejer, Carl McIlwain, Asoka Mendis, Barny Ricket, Victor Rumsey, and Harold Urey to be mentioned in alphabetical order. Kenneth Bowles was also there, the first scientist to test out the ISR technique. I shared the computer room and the Calcomp plotter with some of them and was able to sit in on their seminars and go to lunch with them. It was an environment that was extremely inspiring for a young scientist.

Hugh Chivers and Steffen Mågø were also there; the latter was a Danish engineer who worked with Hugh on a new type of riometer to be deployed in an automatic station at the South Pole. The result of this work was

Figure 9. Electric fields and height-integrated conductivities observed on 13–14 March 1972 demonstrating the polarization of the auroral electrojet (Brekke et al. J. Geophys. Res. 79, No. 25, pp. 3773-3790, 1974).
later to become the company La Jolla Sciences. I often met Mågø in the laboratory where the Calcomp plotter was, and it was a relief for me to be able to speak with someone who understood my mother tongue although his Danish was much corrupted by an American slang.

I remember once I was invited to lunch at Alfvén’s house when we found a note attached to the entrance door:

“You are wrong about the Moon. Urey.”

It was one of the many disputes that the Noble laureates had going between them. I met Alfvén some years later in a plasma conference in Culham, England. Black holes were a hot topic at the time, but Alfvén was not very interested in them and took a break from the lecture room. Meeting me in the corridor he said, “I am not interested in the black holes, but they are nice to have as we can put all the quarreller in them.”

I often had discussions with Alfvén especially about currents in space in relation to mass transfer from a central body to planets. This led me to a model of the current system between the magnetosphere and the ionosphere around the earth where I combined satellite observations of field aligned currents obtained by Armstrong and Zmuda with my own measurements of E-region currents (Figure 10). As I presented this model to Alfvén and he did not shoot it down, I was very happy.

Dick Chappel drove his Chevy Corvette across the United States and spent the summer of 1973 at UCSD. Andy Nagy came also there with his wife, and we went out with Peter on tuna fishing. I remember Peter brought some Coca-Cola boxes with him, and he gave one to me. I was not much experienced with cans of this kind as they had not really caught on in Norway at the time. When I opened it, I made a mistake, and a big splash of coke was shooting out of the box hitting Peter in his face. “That was very kind of you,” he said, and I felt such a shame that I was about jumping over board. First, we went to buy some barracuda as living bates before we went out in the open sea from San Diego. We got a big sea bass and went back to La Jolla where Peter and his wife prepared a gorgeous barbecue meal for us.

In the door of my office stood one day Henry Rishbeth, who was on a visit to UCSD. I had bought in the bookstore the book Introduction to the Ionosphere that he had coauthored with Owen K. Garriot, the only astronaut at the time that had a PhD in aeronomy. Reading such a well-written book gave me a feeling of understanding ionospheric physics, and I especially enjoyed the chapter about the currents and the E-field and also the explanation of how equivalent currents come about and depend on the conductivity on the ground. It filled me with a desire to be able to write something similar one day.

During the AGU fall meeting in San Francisco in 1974, I enjoyed meeting Owen Garriot as he gave an invited talk in a plenary session about his experience on board Skylab 3. I remember that Akasofu was somewhat disappointed as Garriot could not identify time and position of some auroral observations he had made while in space.

Keith Cole came also by, a scientist I respected very much after reading some of his excellent papers on currents and joule heating in the ionosphere. Based on fundamental principles, he made the complicated physics rather easily understandable.
One day in my office at UCSD, I was reading in EOS and found an interesting article by Robert Holtzworth about folklore and the aurora. As he did not mention any connection between old Norse mythology and the northern lights, I decided that when I came back to Norway, I would like to follow up on this matter as I believed I would be so much closer to the original sources. The Auroral Observatory also was preparing for its 50th anniversary in 1980, and it just fit to write a popular book about the role of northern lights in culture and science that I coauthored by Alv Egeland under the title: *The Northern Light from Mythology to Space Research*.

16. Meeting the Cold War at lunch

This was a time with very strong political tensions at the universities worldwide. The Marxist and philosopher Herbert Marcuse came to UCSD in 1965 at a time when the student revolution against the Vietnam war was starting to bloom. In 1968, Governor Ronald Reagan tried to stop UCSD from employing him as a professor, but the powerful chancellor William J. Gill stood up against the authorities so Marcuse could continue his work at UCSD. The students enjoyed the freedom in so many ways. When eating our lunch on the outside in the sun, we could sometimes watch a boy on a bicycle passing by when dragging a naked friend on a rolling board. They called it *Streaking*, I believe. There was a police station on the campus in case the students got too wild.

The conversations around the lunch table often lead toward the Vietnam War, this *Ragnarok* that so badly dominated the World News. I reacted to the use of the word *enemy* that was often used to describe the communists. As a Norwegian having Soviet Union as our nearest neighbor, I very seldom used such a strong and hateful phrase when referring to the Russians. I had a sense that the feeling of enmity strengthened the patriotism among my U.S. colleagues.

Early in the 1970s, European soccer was not much admired in the United States, but Peter, who was then married to an English lady and had four sons, wanted them to learn the noble sport. In the fall of 1973, he asked me if I would be willing to act as a coach for them. European as I was, I could not evade the challenge. We were sponsored by Jack in the Box and promised hamburgers for every match we won. We never came to Jack in the Box (Figure 11).

17. To Yosemite for a Meeting

In the spring of 1974, just before I had to leave for a position at the young university of Tromsø, Peter and others invited to a conference in the Yosemite National Park. Here I for the first time met Michel Blanc. He came along with some observations by the French ISR that happened to be taken the same day as I was in Chatanika doing measurements. I was impressed by seeing that we both observed effects of the same disturbance events, and from the similarity in the variations of the electric fields measured, we realized that the substorm had a widespread global effect as far as the electric field was concerned. Another person I remember from this meeting was Lars Block, who tried to convince us about the electric double layers in the ionosphere. It created a lot of discussions that continued for years in the community. In the lunch breaks, we went to Badger Pass for downhill skiing. The Half Dome and El Capitan were impressive mountains, but my interest in mountain climbing was not strong enough to persuade me to try the challenge.

18. Leaving UCSD

For me, the stay at UCSD and the close cooperation with the group at SRI were decisive for my future life as a scientist. It opened up doors for me that I else would never have been able to enter. I was especially impressed by the ease with which you could approach senior scientists that you highly respected through the papers you had read. The networks I was able to get in contact with through the many meetings and symposia, and in particular the one in Yosemite, I have enjoyed for years thereafter. I am very grateful for the
opportunity given to me by my colleagues in the United States, and I only regret that I could not stay for a longer time. The Chatanika radar at the time was a gold dredge for ionospheric research far superior to the real one stored close to Chatanika lodge. From whatever data you dug out from the radar, you could write a paper that attracted international interest, and the paper was read by many scientists in the field of ionospheric physics. It was wonderful to be there, and sometimes I think about it as a dream Figure 12.

On my way back to Norway, I stopped by at Ann Arbor to visit Andy Nagy’s group and learn more about the Fabry-Perrot interferometers that John Meriwether was using for measuring neutral winds in the ionosphere. Here I also happened to meet Ralph Cicerone and Richard Stolarski. From Ann Arbor, I went to Urbana Illinois to meet Sidney Bowhill. I was shivering when giving a lecture about my work at UCSD since I had experienced him during conferences where he had tended to interrupt the speaker in the middle of his presentation to give him a rough time with difficult questions. I made it through and got the feeling that Bowhill believed in my neutral winds.

Here I also got the chance to visit the very first operated ISR that Kenneth Bowles so neatly put together.

19. Back to Tromsø and EISCAT

When I came back to Tromsø in April 1974, the staff at the Auroral Observatory was, under the leadership of Professor Olav Holt, preparing for the European Incoherent Scatter Radar (EISCAT) to be installed in northern Scandinavia. I was asked to arrange a summer school in June 1975, and prominent scientists like Tor Hagfors, Bengt Hultqvist, Rolf Boström, Henry Rishbeth, Michel Blanc, Richard Vondrak, Murray Baron, and others were invited and contributed to the success of the school by special lectures. I asked the lecturers to write up their manuscripts for a proceeding that I wanted to edit. This came out in print in 1977 by the title: Radar Probing of the Auroral Plasma. The book became an important introduction for young students wanting to be acquainted with ISRs for use in ionospheric science. I was very impressed when I visited Professor Ma at Wuhan University in China about 30 years later when she handed over a completely disintegrated copy of the book that she had inherited from her predecessor Professor Liu, demonstrating the importance of the book among Chinese students.

In 1975, an agreement was signed between the six European countries, Finland, France, Germany, Norway, Sweden, and the United Kingdom, to install two ISRs in north Scandinavia, a monostatic one operating at 225 MHz and a three static one operating at 930 MHz, respectively. Both radar systems were installed at Ramfjordmoen in Tromsø, and additional receivers in Kiruna, Sweden and Sodankylä, Finland, respectively.

At midsummer of 1977, I was invited to the first Science and Advisory meeting of EISCAT (EISCAT SAC) held in Sodankylä where Henry Rishbeth was the chairman. Here we drew up the plans for the common programs that were going to form the main database from EISCAT to study the long-time trends of important ionospheric parameters. I remember with pleasure the discussions we had with Henry Rishbeth, who had such a profound understanding of ionospheric physics. We agreed strongly about taking advantage of the experience obtained by the experiments at the Chatanika radar. Unfortunately, it turned out to be more complicated to complete the radars than anticipated, and the first data from EISCAT did not emerge before August 1980.

I soon came to work for the EISCAT organization and was elected chairman of EISCAT SAC in 1983. In September of that year, we arranged the very first EISCAT Workshop in Aussois in France with 40 participants.

Already in February 1981, a delegation appointed by EISCAT Council visited USSR Academy of Sciences, Cola Branch in Apatity to discuss the possibilities for setting up a bistatic system for the EISCAT Very High Frequency (VHF) radar. During their trip, they also visited Verkhne-Tulomsk on Cola Peninsula where the Russians were preparing a site for a VHF receiving antenna. It soon turned out that it was
difficult to reach an agreement for how the data should be shared. USSR wanted access to all EISCAT data and not only the data from the VHF radar. This was difficult to accept from the EISCAT site.

The construction work with the antenna system at Verkhne-Tulomsk was rather slow. I visited the site for the first time in 1987 where I could observe the first elements of the antenna. When I came back there in 1992, nothing much had happened with regard to the antenna construction. I noticed a potato field in one corner of the array and commented on that. The answer I was given was: *This coming winter we are not going to starve.* The effects of Perestroika were at full power, and it made it hard for our colleagues in Russia to obtain grants for advanced scientific instruments and even salaries those days.

### 20. Realizing EISCAT Svalbard Radar

Professor Takasi Oguti from Geophysical Research Laboratory at the University of Tokyo, who then was appointed to become director of the Solar Terrestrial Environment Laboratory (STEL) at the University of Nagoya, visited the Auroral Observatory in September 1988. During his visit, he presented the Japanese plans for a bistatic incoherent radar in Svalbard with a transmitter in Ny-Ålesund and an additional receiver in Longyearbyen that he wanted to establish together with Norwegian scientists independent of the EISCAT organization.

In 1989, when Glasnost was a fact in Soviet Union, we could seriously start thinking about a dream we had shared for many years to install an ISR in Svalbard. In September of 1989, U.K. scientists published a report where a radar with three antennas were envisaged in Longyearbyen, three antennas that were supposed to give a large spatial coverage, and hopefully, they also could overlap the field of view of the mainland installation. The proposal was well received by the EISCAT Council that started the discussion for how to make it a reality.

I had available a grant from the Norwegian Research Council (NAVF) that I used to invite the EISCAT Council after finishing its meeting in Tromsø in May 1990 to visit Longyearbyen for surveying alternative sites and evaluating the realism behind the fantastic idea of a radar on Svalbard. I remember that the council members, when I met them at the Tromsø airport before taking off to Svalbard, were very skeptical, but they said they would enjoy the exotic trip. When we split at the airport after returning to the mainland, everyone was enthusiastic about the idea of an ISR at Svalbard. During the meeting at Svalbard, the Polar Cap Working Group was formed headed by Professor Noralv Bjørnå, and in August 1991, the group presented its work, *The EISCAT Svalbard Radar (ESR)* that came up with a budget between 115 and 150 MSEK. Formally, the ESR was accepted by EISCAT Council on its meeting in Uppsala, Sweden on 13 November in 1992.

The Japanese proposal was still pending. Skepticism could be sensed especially among the British in the EISCAT community for expanding EISCAT membership outside Europe. I was strongly in favor as a broader international engagement would benefit everyone, and I had good experience working with Japanese scientists.

Norway was in a very unusual situation as we were part of two proposals for ISR on Svalbard. It was rather impossible to imagine that Norway could participate in both of them. My strong wish was to find a way to merge the two. During the summer of 1989, I visited several research institutions in Japan in order to learn more about their interest in the radar project on Svalbard and invited the Japanese scientists to cooperate with EISCAT on the project. The EISCAT community was, however, skeptical to the Japanese proposal as it was believed that the phased array system, as Nobuo Matuura and Takasi Oguti proposed, was not enough developed for an ISR.

From 1992, I was elected vice chairman of EISCAT Council and was encouraged to continue the discussions with the Japanese colleagues and authorities. I spent 3 months in 1992 at STEL in Toyokawa to learn more about the status of the Japanese proposal and searched for possible solutions toward cooperation with EISCAT.

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Figure 13. From the right Takasi Oguti, Ryoichi Fujii, and Truls Hansen in Ny-Ålesund in August 1992 (photo by Asgeir Brekke).
In August 1992, Professor Takasi Oguti and Professor Ryoichi Fujii, the latter had returned from a year in Antarctica, came to Tromsø and Truls Hansen, and I accompanied them to Svalbard (Figure 13). We visited Ny-Ålesund and learned about the concern related to the plan of installing an active transmitter there where they planned for a noise-free environment. At this time, the science activity had increased strongly in Ny-Ålesund compared to the time when I stayed there. From this visit, Oguti realized that an ISR in Ny-Ålesund would be a rather costly and challenging project to establish. Fujii was rather pragmatic and favored cooperation with EISCAT. During a meeting, we had with the dean at the University of Tromsø after returning from Svalbard, Professor Oguti said that they would like to incorporate their application to Japanese authorities for an ISR at Svalbard within the EISCAT proposal.

On 9 November 1992, I got a letter from Matuura saying: Now, I will like to inform you that we are thinking of changing our future proposal for Spitsbergen plan such that Japan will primarily participate in Longyearbyen radar plan by contributing to the second dish (32 m diameter) with appropriate cost 32 MSEK.

In my response to Matuura on 17 November, I wrote: When I informed EISCAT Council about your change of plan and new strategy, it created much enthusiasm in the meeting in Uppsala November 12 and 13.

As time went by, I traveled at least once a year to strengthen the contacts between Japanese authorities and colleagues to encourage cooperation between STEL and the EISCAT organization, and as I got elected chairman of EISCAT in 1993, I felt that I was given a mandate for leading on in the process of establishing EISCAT Svalbard Radra (ESR). On 22 May in 1993, the Norwegian minister of education, science and church Professor Gudmund Hernes declared the EISCAT Svalbard Radar (ESR) site for open on Mine-7 mountain (Figure 14).

EISCAT Council decided to send a delegation to Japan in September 1993 consisting of Director Jürgen Röttger of EISCAT, Jorma Kangas from the University of Oulu in Finland and me. Eivind Thrane from the Norwegian Defence Research Establishment also joined. We were going to have meetings with the president Dr. Nobuo Kato at Nagoya University and Mr. Masayuki Inoue, director of the division for international research at the ministry of education, science, and culture (Monbushu). The EISCAT delegation encouraged strongly the president of Nagoya University to apply for membership in EISCAT on behalf of Japan.

During our meeting at Monbushu, we again encouraged Mr. Inoue to support a Japanese application to become a member of EISCAT, and we said that we were ready to invite Japan to participate in EISCAT Council with a delegation. During this time, Mr. Amamia at Monbushu came to Tromsø, and I accompanied him to Svalbard. On our flight back to the main land, the pilot announced that we soon could be watching two F-16 fighters on the outside, one close to each wing. Sure enough, the plains were there, and the pilots were waving to us. I could not resist telling Amamia, “You are being brought back in style.”

On 27 June 1994, I received a letter from Director General Masahiro Nishio Nagoya University after he had visited Tromsø and Svalbard where he states: After getting back to Nagoya, we immediately started to negotiate with the Ministry of Education, Science and Culture for the study of feasibility of our participation in the EISCAT project. I strongly felt that this was a turning point in the struggle for merging the two projects and for Japan to become a member of EISCAT.

During our visit in September of 1993, it was agreed that an EISCAT workshop should be arranged in Japan in September 1994. About 60 scientists...
met on the beautiful island of Toba during the first week of September, and the workshop was very successful as it strengthened the personal ties between European and Japanese scientists.

In May 1995, funds were released by Monbushu and on its meeting in Paris in November; EISCAT Council approved the signatures of understanding. From then on, the time was come to realize the second antenna at ESR (Figure 15).

This was in my mind one of the most positive and impressive periods of EISCAT when Director Jürgen Röttger was able to direct the development of the project in time and to the budget. The project also gave me the opportunity to work closely with the Japanese science community and to learn about international science diplomacy by negotiating with governmental officers. I later took advantage of my experience in my efforts to bring Russia, China, Ukraine, and South Korea into the EISCAT organization. It was indeed an exciting time.

5 November 2019