

CURRICULA VITAE

N C Gerson

Date of Birth: October 15, 1915

Place of Birth: Boston, Massachusetts

Married: Sareen R. Epstein, August 26, 1945

Children: Donald F., Stanton L., Richard K., Martha B., Stephanie L.

Education:
B.S. Physics, Magna cum Laude, University Puerto Rico, Rio Piedras, PR,
1944
M.S. Physics, New York University., New York, NY, 1948

Positions:
1938 - 46 Technical Investigations Section, U.S. Weather Bureau
1946 - 48 Asst. LF Loran Evaluation, Watson Labs.,USAAF, Ft.
Monmouth, NJ.
1948 - 55 Chief, Ionospheric Physics Lab., AF Cambridge Research
Center, Cambridge, Mass
1965 - 68 Staff Scientist, Mitre Corp, Bedford, Mass.
1968 - 70 Staff Scientist, Syracuse University Research Corp, Syracuse,
NY
1970 - 91 Consulting Physicist

Committees:
1948 - 51 Chairman, Tropospheric Propagation Committee, USAF
1948 - 51 Member, Ionospheric Propagation Committee, USA
1949 - 52 Appointed Civil Service Commission
1952 Member, USAF Committee to Evaluate Alaskan Radar
Defenses
1953 - 57 Secretary, US National Committee for International
Geophysical Year (USNC for IGY)
1953 - 57 Secretary, Executive Committee, USNC for IGY
1953 - 55 Chairman, first two Antarctic Committees, USNC for IGY

- 1955 - 58 Vice Chairman, Arctic Committee, USNC for IGY
- 1954 - 58 Member, Ionospheric Panel; Rocket and Space Panel, USNC for IGY
- 1955 - 58 Chairman, Advertising Committee, American Geophysical Union (AGU)
- 1954 - 55 Assigned by AGU to negotiate with Merle Tuve for transfer of Jour Geophys Research from Carnegie Inst Washington to AGU

Consultantships:

- 1956 Department of Defense, Washington, DC
- 1956 National Academy of Sciences, Washington
- 1959 - 60 Lincoln Laboratory, MIT, Lexington, Mass
- 1960 - 65 Advanced Research Projects Agency, Washington, DC
- 1966 - 69 Syracuse University Research Corp, Syracuse, NY
- 1968 - 70 Mitre Corp, Bedford, Mass

Publications:

See Attachment for partial listing

Field Experience:

Implemented field experiments in Arctica (from Greenland to Alaska) and Antarctica. Worked beyond polar circles a total of over four years, two years of which were within 700 km of North Pole, Period: 1946 -1980.

Overall Career:

Actively engaged in research or the management of research on atmospheric problems (both more applied and more pure) during entire career. Stimulated ionospheric-auroral research in North America through direct sponsorship, initiation of Conferences, and personal investigations. Directly improved operational systems.

Accomplishments:

U.S. Weather Bureau

Responsible for all thermodynamic calculations routinely utilized in meteorology and weather forecasting. Completed first thorough evaluations of pseudo-adiabatic equations. Designed all thermodynamic charts needed to evaluate upper air soundings (used by USWB and Dept. Defense for decades--many still used today). Conceived and designed unique tephigram on 60 degree axes for meteorological purposes. Undertook first upper air wind determinations using wartime radar and resonant dipoles suspended below radiosonde balloons (Puerto Rico).

Watson Laboratory, USAAF

Assisted in evaluations of 180 kHz experimental Loran electronic navigational system in SW Canada; subsequently implemented globally as LF Loran. Responsible for calibration and acceptable operation of all field sites in Canada and Alaska--from 45 degrees north to beyond arctic circle. Completed first analysis of natural LF noise intensities in the subarctic.

AF Cambridge Research Center, USAF

Established and directed Ionospheric Physics Laboratory of AFCRL. Reoriented USAF "radio propagation research" from statistical studies of intensity variations on point-to-point circuits to an understanding of the physics and dynamics of the ionosphere. Initiated a broad coordinated investigation (internal and under sponsorship) of auroral-, geomagnetic-, ionospheric- and particle-physics and their control by the sun. Implemented an airborne observatory to obtain ionospheric soundings over North Pole; "follow the sun" (as the earth rotated); examine arctic ionospheric drifts; obtain midlatitude airglow spectra;, etc. designed infrared laboratory for examining atmospheric effects

USNC for IGY, NAS

Involved with overall planning and specific geophysical studies. Successfully defended establishment of South Pole Station. Suggested under-ice Arctic transit from Atlantic to Pacific using nuclear submarine (hitherto impossible with conventional submarines). Prepared geophysical and national justification for U.S. Antarctic research sites. Prepared U.S. Arctic Basin research program including implementation of ice island station. Active in coordinating U.S. polar, satellite and global programs with representatives from other nations.

Consulting Physicist

Responsible for a number of innovative changes in operational systems. Initiated large scale study of ionospheric dynamics and their impact.

Mitre Corp

Involved with evaluating various DoD systems and concepts. Prepared text on Radio Wave Propagation.

Syracuse University Research Corp

Examined ionospheric conditions within Polar Cap. Prepared reports on inter- and intra-polar hf propagation, physics of transequatorial propagation (hf and vhf), and midlatitude sporadic E

Consulting Physicist

January 1991

Concerned with magnetospheric-ionospheric interactions, future of ionospheric research, influence of ionospheric dynamics on operational systems, improvement of practical systems affected by the ionosphere.

Awards:

Meritorious Civilian Service Award, Dept. of Defense	11 Jun 1982
Antarctic Medal, National Science Foundation	1976
Exceptional Civilian Service Award, Dept. of Defense	Jan 1989
Fifty Year Federal Service Award, President Bush	1989

Letters of Commendation:

Vice Adm Bobby R Inman, USN, D/Dir CIA	23 Mar 1982
Lt Gen Lincoln D Faurer, USAF, Dir NSA	3 Aug 1981
Director, Communications Security Establishment	20 Jul 1981
Howard E Rosenblum, D/Dir, Research and Engineering	2 Sept 1977
Norbert H. Szymonowski, D/Chief, Operations Analysis	16 Aug 1977
Howard E Rosenblum, D/Dir, Research and Engineering	14 Dec 1976
Howard E Rosenblum, D/Dir Research and Engineering	7 Nov 1976
Lt Gen Lew Allen, USAF, Dir NSA	1 Nov 1976
Br Gen J H Jamesmeyer, USAF	15 Sep 1976
Br Gen R N Senior, Dept National Defence	19 Aug 1976
Howard E Rosenblum, D/Dir Research and Engineering	19 May 1976
Howard E Rosenblum, D/Dir Research and Engineering	22 May 1975
Lt Gen Lew Allen, USAF, Dir NSA	31 Jul 1974
Robert J Hermann, Chief, Office of Analysis	22 Jun 1973
A D Braeuninger, Chief Engineering Div	7 May 1973
A P Albrecht, Office Sec Defense, Pentagon	2 Nov 1973
Robert J Hermann, Chief, Operations Research	30 Apr 1973
John R. Harney, Commandant, National Crypt School	21 Apr 1973
Leo Rosen, Exec NSA Advisory Board	7 Dec 1962
Leo Rosen, Exec NSA Advisory Board	5 Jun 1962
S Kullbach, Dir Research	27 Jul 1959
Ma Gen J R Phillips, USA, Chief Army Sec Agency	17 Apr 1958
Lt Gen J R Samford, USAF, Dir NSA	6 Apr 1958

January 1991

Dr. D. W. Bronk, President, National Acad Science

Sept 1957

Some Experiences

I have been sent to both the Arctic and Antarctic, and have worked (on TDY) within 700 km of the North Pole a total of 2 years.

In Antarctica I slipped on an icy hillside and slid 3000 ft. free fall to the bottom. On another occasion my weasel stalled on a very steep icy path. I parked and carried the 75 lb dummy load cross country uphill 200 yds through knee deep ice encrusted snow. The flight "to the ice" from New Zealand was unusual; a forward hatch unexpectedly opened and discharged a liferaft which automatically inflated and slammed against the tail section. The plane was thrown out of control until the liferaft finally blew off. Unfastened passengers were thrown about like corks. I had been sleeping on the cargo and could not reach a bucket seat until grabbed by friendly hands. The pilot recovered less than 1000 feet above the sea. We were three hours from McMurdo Sound.

In the Arctic one December morning I left the mess hall after breakfast for work. The outdoor temperature was -49F and the wind 60 mi/hr. I was blown down onto the snow four times during the 3/4 mile walk to the Operations Building. Time for the "walk" was 45 minutes. During two summers (outdoor temperature +52F) the work was so arduous that perspiration soaked through all clothes to the outside of my parka. On several trips I lost 10 lbs during the first week. During summers while working at isolated outside facilities grazing caribou came within 15 ft. On one occasion far from the station and alone, I came across fresh wolf tracks in the soft tundra.

While working at Arecibo, Puerto Rico, the open cable car taking me from the carriage house (500 ft. above the antenna) to the ground struck the feed structure. The car "caught" and became horizontal as it was haltingly dragged over the structure. The car almost dislodged itself from its support cable; had it done so it would have plunged to the ground. I hugged the only vertical (now horizontal) strut on the flimsy cable car for dear life. After scraping clear, the car swung wildly while its support cable oscillated vertically 15 - 20 ft.--all about 500 ft. in the air. I was glad to reach the ground. (A \$10 override switch in the circuitry would have prevented a mid-air collision course between the cable car and the feed structure. I expressed my peevishness to the management.)

Collaboration in Geophysics — Canada and the United States 1948-1955

by *N.C. Gerson*
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1. Introduction

It may be of interest to outline the actions that led to an era of material advances in geophysics in North America in the 1950's. Some of these arose from a conscious attempt to harness talent and to strive towards advancing the frontiers of knowledge in upper atmospheric physics. To accomplish this end one vital aspect was the establishment of a stimulating environment for research. Only in this fashion could a cadre of knowledgeable, motivated younger individuals, who could supply the needed creativity, be assembled. It was my good fortune to be able to participate and in some small way to influence that effort, especially during the period 1948-1955.

The account which follows traces the development of the overall program and the reasons that brought it about. However, before describing them in detail, some background comments leading to my involvement seem pertinent.

My field was physics, and after graduating, I became involved with meteorological thermodynamics. One day in 1946 as I was happily slaving away, an old professor of mine appeared in the office and told my boss that I should work for him. In the ensuing three hour discussion they both totally ignored me. However, I did learn for the first time that my boss appreciated me. The net result of this debate was my transfer from the U.S. Weather Bureau to the USAF Watson Laboratories at Fort Monmouth, and thenceforth to my preoccupation with electromagnetics.

Five weeks after that meeting, on a Monday morning, I left Washington for New Jersey. I promised my tearful wife that I would return Friday after work. On Wednesday I was given 120-day travel orders (later extended for another 120 days) for locations that for the most part I had never heard of; Ottawa, Winnipeg, Gimli, Portage la Prairie, Hamlin, Edmonton, Churchill, Baker Lake, Yellowknife, Norman Wells and "CIPAP." I never knew the English for this acronym, but it meant "wherever else ordered."

My work involved an evaluation of the experimental LF Loran Chain, a joint RCAF - USAF project in southwestern Canada. It originally had the name Musk Ox, then Musk Calf, and then a variety of derivatives. The transmitters were located at North Battleford, Gimli and Dawson Creek. All other sites were monitors of one type or another. In addition, several B29s were employed to check operational performance from the North Pole to Bermuda. Because of its stability, reliability and accuracies, the concept was adopted and exists today as the Loran C 100 KHz hyperbolic navigational system.

I was one of two men who spent six months criss-crossing Canada (and Alaska), visiting all fixed sites, and checking the reliability and accuracy of the system. I finally saw my wife again a few days before Christmas, 1946, and also for the first time, my three month old son. When my wife learned that I had almost crash landed on the frozen Great Slave Lake only about one week earlier, she decreed that I leave the job.

My boss, Col. George Higginson, countered with another offer; evaluate the LF Loran records and the 180 KHz noise data being obtained. Shortly thereafter, Ralph Cole, Director of the Laboratories, encouraged me to complete a master's degree in physics at New York University. Watson Laboratories split in 1948; most of the Units were moved to Griffiss Air Force Base and became known as the Rome Air Development Center. Four Laboratories including mine, (all mainly on paper) were ordered to Massachusetts where we founded the Geophysics Research Directorate of the USAF Cambridge Research Laboratories.

2. Initial Planning

My involvement in United States - Canadian collaboration in geophysics began in 1948, before our group moved to Massachusetts. By that time, I had already completed my thesis, "Nocturnal Ionization in the Ionosphere," (later published in the Reviews of Modern Physics) under the guidance of Drs. Haurwitz and Korff of NYU. Also, I had been asked to head an Electromagnetic Propagation Laboratory and develop a research program for USAF. To assist me, a secretary and an office were provided.

Development of the overall program required about seven months. I was given very wide latitude in deciding what topics to include or exclude. The long term objectives were improved electromagnetic (mainly radio but also other frequencies) propagation in the terrestrial atmosphere. Operational difficulties arose from the general unpredictability of the received intensity, from the onset of polar blackouts at high latitudes, or from the various types of fading.

These problems arise because the medium, the atmosphere, is a fluid in a constant state of flux. We did not know the energy sources and sinks or their time variability. (We still do not.) We did not understand the dynamics involved, the energy density of incoming particles, or the spectral distribution of the solar radiation. I felt that as a first approach a better understanding of the atmosphere, through which the man made radiation travelled, was needed. I was most reluctant and refused, to adopt the then standard procedure for examining propagation: establish a transmitter at point A and a receiver at point B, operate both for a month or a year at the prescribed frequency, and then obtain diurnal and monthly statistics. I complained that we could repeat this procedure forever whenever A and B "were moved 10 meters" and still never know what caused the differences.

USAF was involved with frequencies from kHz through GHz into infrared. The interacting medium for GHz and IR was principally the troposphere, and for the remaining frequencies, primarily the ionosphere. However, since most emphasis was in the band VLF to VHF, I renamed the laboratory, "Ionospheric Physics Laboratory." It contained four Sections; Ionospheric Physics, Auroral Physics, Infrared, and Radio Meteorology. Geomagnetism, excitational processes and solar terrestrial relationships were implicitly included. Emphasis had been given to the aurora because of the polar blackout problem. At a later time, the Radio Meteorological Section moved elsewhere, and for it I substituted a Geomagnetism Section.

3. Implementation

It should be noted that from the beginning I had two enormous advantages. Because of the preparation for my thesis, I knew ionospheric and upper atmospheric physics, and already had compiled a list of deficiencies which should be attacked. (Helmut Landsberg later induced me to publish a portion as "Some Unsolved Problems in Atmospheric Physics," in "Advances in Geophysics.") Secondly, I had very strong administrative backing within USAF, and was well aware of their practical problems.

To implement the program, a double pronged approach was chosen — both in-house and contractual efforts which complemented each other. I felt that the internal staff (yet to be hired) must be active researchers in their own right if they were to guide any University effort.

The initial moves from planning to implementation occurred during spring and fall, 1948. In this account, the second action will be

**Memorandum written by F/L J.H. Gidman, Northwest Air Command RCAF,
Edmonton after the flight from Norman Wells to Hay River (December 4, 1946)**

Notes on Special Trip by Air, Norman Wells to Yellowknife, NWT, Beachcraft #1396, leaving Norman Wells 1000 hours 4 Dec./46 (temperature Norman Wells - 48 degrees below zero). Crew Captain — S/L Sanderson, CO#6 Comm. Flt; Co-Pilot — F/O L. Bell #6 Comm. Flt; Aircraft Mechanic — LAC N. Zanusse; Passengers — F/L J.H. Gidman; Mr. S. Gerson (U.S. — Canadian Loran Expert); LAC Melanson (Chef.)

After take-off at Norman Wells, we headed on course according to flight plan for Yellowknife direct, non-stop. After 1½ hours flying, we ran into zero visibility (fog), which increased in density as we proceeded and it was then discovered that there had been some error in navigation calculations and that we were some considerable distance North of our true track.

After 3¼ hours flying, we finally raised what we thought to be the North-West shore of Great Slave Lake, but we could not distinguish or orient ourselves with the shore line. We immediately swung to a Northerly direction and proceeded apparently to a point midway between Ray and Fort Radium, when it was decided to back-track and fly South.

After flying 50 minutes in this direction, the Settlement of Providence loomed up ahead. Incidentally, we were flying at a ceiling of 50 to 200 feet with severe icing conditions creeping in. The port and starboard carburetor heater controls were now iced and unserviceable, as also was our Air Speed Indicator, being completely iced in the pilot tube, the Air Speed Indicator showing zero miles per hour.

We swung again in a direction which we considered was toward Yellowknife, but almost immediately got off track once more. By this time, the gas in three of the four tanks was expended and we were well into the fourth. Again we changed our course and without any radio aid whatsoever (the radio in the Aircraft had in the meantime gone unserviceable), we headed in what seemed to be the right direction for Hay River and after several minutes more flying, an abandoned air strip loomed up which we identified as Providence, and at no height at all, we attempted a landing on this strip in an atmosphere which developed into a very unpleasant snow storm, and bearing in mind we had no indication of air speed whatsoever as the air speed indicator was showing continuous zero reading all the way, we ap-

proached the field on a wing and touched to our surprise, the bottom through snow drift at least 4' in depth. We bounced some 40' and slid down the runway still airborne, much to our amazement, as we had all expected the Aircraft to overturn on its back, and on the instant we ran out of field. S/L Sanderson in control, opened both throttles wide and we cleared the tree tops at the end of the runway by several inches (very nice work). In the knowledge that we now had power in spite of the fact that the Air Speed Indicator was reading zero, we again groped along the South shore of the Lake. The time, incidentally, was a few minutes past two, and our gas supply was running low. We flew through a period of time which seemed as though it would take us into the darkness.

We finally crossed the mouth of the Hay River and the small settlement. Now with less than 10 gallons of gas left in our last tank, we once more groped around in an attempt to locate the air strip (the weather in the meantime was becoming absolutely filthy, definitely zero-zero). With an out-dated chart, our orientation with ground resulted in absolute failure to locate the field, and so tracking back along the shore line once more to the Settlement with no altitude in a last desperate effort, we reached the Air Strip at Hay River. In the meantime, the Aerodrome Attendant had closed the circuit to the revolving beacon, showing great presence of mind and with the aid of the beacon, the Pilot headed the Aircraft low and directly over the area. We had now less than five gallons of gas in the last tank and we literally screwed around on a wing in order to approach the leaside of the field. We slipped in and made a beautiful landing, after four hours, 55 minutes in the air.

The trip to Yellowknife was completed from Hay River the morning of 5 December, without incident.

The Captain of the ship should be commended on his coolness during this whole period of hazardous operation, considering all the circumstances — impossible weather, unserviceable flying instruments and absolutely no radio communication, piloting the Aircraft under his charge to a safe landing in that portion of the NWT over which he had never flown before.

J.H. Gidman F/L

described first. When originally asked to head the Electromagnetic Propagation Laboratory, I had been given an existing contract with Pennsylvania State University (then Pennsylvania State College). I paid little attention to the contract because it was of the radiowave propagation type and was generally concerned with ionospheric reflection coefficients. However, as the time for implementing my program approached, I wondered whether the desired research into ionospheric physics could be initiated there.

During my visit to Penn State, I indicated to A.H. Waynick my intention of scrapping the existing effort and substituting a new one devoted to an examination of the physics, photochemistry and dynamics of the ionosphere. He agreed, but to insure that he understood my views, I repeated them in detail before Eric Walker, then Chairman of the EE Department. I also informed both that in due time I desired a Conference on Ionospheric Physics, and asked whether the College would act as host. If so, I would provide the funding and supply details later. The outline of problems brought to Penn State were those culled from a larger list assembled while engaged in my thesis. We also included an experimental study of the lower ionosphere.

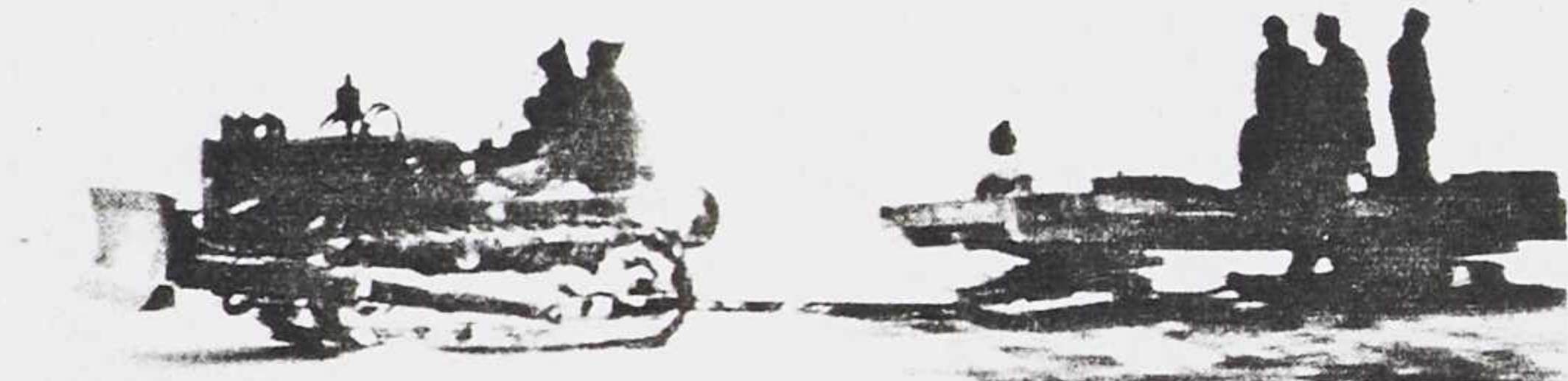
4. Canadian Participation

In retrospect, the earliest steps taken to prosecute the program bound me inextricably into the atmospheric research efforts of Canada. In the spring of 1948 I had contacted Dr. Cullwick, then at the Defense Research Board, about sponsoring auroral research (at the University of Saskatchewan) and radio meteorological investigations (at McGill University). Cullwick arranged a meeting at the old National Defence Headquarters building to include himself, Don McKinley of the National Research Council, Balfour Currie of Saskatoon, Stuart Marshall of Montreal and myself.

We discussed the inauguration of a rather large auroral research effort at the University of Saskatchewan and the augmentation of radio meteorological studies at McGill. More specifically, my proposals contained for Saskatoon: (1) a detailed auroral spectroscopic program involving observations and analyses, and (2) a radio auroral observational and analytical program. AFCRL would support the former outright, and would supply the equipment and seek DRB funding for the latter. For McGill, the proposal was to increase and strengthen their program in precipitation

physics, rain size distribution, and associated topics. AFCRL would supply a radar and furnish funding.

McKinley, Marshall and Cullwick were enthusiastic. McKinley wanted an additional radar for NRC so that he could expand his radar studies of meteors. (I agreed.) Currie thought that the radar observations might (could?) fail and seemed reluctant to agree. On this point we decided to review the matter the next working day after everyone had had a chance to think things over. Marshall was delighted and accepted on the spot. (The radar for McGill was of the nodding type, called the "tipsy nine" — TPS9. AFCRL provided funding for what became the "Stormy Weather" project of McGill.) In the following meeting, all agreed with the proposals. I now had more paper work.



First Class Travel, Baker Lake, November 1946: Ice three feet thick

5. The Auroral Program

Some time elapsed before I arrived at Saskatoon with an outline (essentially a shopping list) of spectroscopic and radar research topics that should be pursued. The major items included accurate identifications, temperature deductions from the nitrogen rotational lines, altitude determinations (using Currie's observations at Chesterfield Inlet during 1932), observations of sunlit aurora, spectra of "auroral primaries," correlation of optical and radar observations, emission altitudes, and electron densities in ionized aurora. Petrie had a few additional items which we incorporated. The three of us, in two half-day sessions, went over the items methodically and in some detail. For each topic we considered the feasibility, value, method of attack and the necessary equipment. It was obvious that for a definitive identification of the auroral and airglow emissions additional flat field Schmidt cameras (designed by A. Meinel of Yerkes Observatory) were needed, I agreed to have him construct these cameras for both Saskatoon and our internal studies at AFCRL.

During the visit, I made the usual inspections; the laboratories of the Physics Department, the spectroscopes, the instrumentation on the roof and in the field. Again I became acquainted with the operation of equipment outdoors during cold winters. On leaving Saskatoon several days later, I had the distinct impression that both Currie and Petrie were overwhelmed by the prospects (Some years later, the Upper Atmospheric Research Institute was established at the University. Currie used to introduce me as its grandfather.)

Back in Massachusetts, I prepared a contract of considerable flexibility to cover not only the paths that we felt were clear but also those that invariably are uncovered by ongoing research. Further, to insure that the studies would not be subject to wild funding gyrations, I obtained a five year contract — either the only one, or one of a very few, of that duration ever issued by AFCRL.



Ice fishing, Baker Lake, November 1946 The fish was for the sled dogs. We ate canned vegetables, canned hot dogs, canned butter, etc.

It may be of interest at this point to note my rationale for concentrating auroral research at the University of Saskatchewan. The answer is relatively simple; competence and location. I had a favorite phrase at that time: "you do not fish in the desert." Thus, I did not become involved with the efforts at Cornell or Colgate. With respect to the University of Alaska, although the location was good, specialists were absent and could not be readily attracted. Indeed, I made a conscious effort on a trial basis and shipped one of the identical 106 MHz radar (that had been dispatched to Saskatoon) to the Geophysical Institute, Fairbanks. In addition, funding was provided. The results were disappointing and I terminated our involvement.

An unexpected development later occurred that caused me some worry. Bill Petrie's wife never liked the winters at Saskatoon, and he finally departed for the warmer climate of Ottawa and DRB. After the move he pressed me to transfer the flat field Schmidts to DRB so that he could use them at Churchill. I felt, however, that they should remain at the University where there was a vibrant and interested staff and a source of active graduate students. I am not certain that Bill ever fully accepted or liked the decision to retain the equipment at Saskatoon.

6. Excitation Mechanisms

A notable lack of information, that obscured the interpretation of atmospheric emission spectra, existed in the excitation transitions involved. There was little ongoing research in this field. It seemed to me that physics had by-passed relatively low energy atomic and molecular reactions and collisional processes and had instead rushed pell mell into examining high energy nuclear interactions. (The reasons were obvious.) Further, past laboratory studies for atmospheric species had been spotty and unsystematic. Possibly the most recent laboratory studies relevant to atmospheric emissions were those of Kaplan at UCLA and Vegard in Oslo (on nitrogen spectra and afterglows). Current work was essentially absent. In effect, there was no concerted effort in this field.

While pondering on this experimental deficiency, I stumbled upon a young enthusiastic investigator who was delivering a paper on this topic at the annual infrared spectroscopy meetings of Ohio State University. I was impressed and, after thinking about the matter over a weekend, telephoned him. He was R.W. Nicholls of the University of Western Ontario.

In this case the objectives were easier to describe. Also, it helped considerably since I knew what I wanted, and he had it. We discussed the possible experiments, goals, and laboratory equipment. The various scientific, contractual and funding details were essentially agreed upon prior to the end of a twenty minute conversation. It remained only to formalize them in various correspondence and actions. Just before concluding, and almost as an afterthought, Nicholls thought of his "chain of command." He asked me to broach the potential contract with his Department Chairman, A.D. Misener. A separate telephone call a few minutes later settled the matter. Misener remained a staunch backer of the project. Nicholls and I owed him a great debt for absorbing or deflecting some of the administrative jetsam that invariably surfaces in university contracts.

7. Comments from DRB

Within a few months after the contract with Nicholls at UWO, "reliable sources" told me that Frank Davies was upset. He felt that I was pouring too much money into Canadian Universities and whetting their appetites into requesting more support than necessary from the Defence Research Board. (As an example, consider the contract with the University of Saskatchewan for auroral research. I provided \$30K annually, guaranteed for five years, for the spectroscopic endeavors. In its wisdom, DRB provided \$3K, reapplied for annually, for the associated radar studies. Note, that the period was 1949.) Frank felt that my funding was too high and covered too many Canadian Universities.

A book could be written on the wisdom and foresight, enunciated in a Welsh accent, by Frank T. Davies. Here, however, let me note in passing that this forceful and philosophical physicist did much for Canada. We first met in 1946 at the great metropolis of Portage la Prairie, where I was then working. Frank arrived at the RCAF station and installed an ionosonde in the same room that contained the LF Loran equipment of Watson Laboratories. I later operated his sounder continuously during the Perseids meteor shower period, seeking echoes from the ionized meteor trails — to no avail.

Our meeting sparked a long, warm, simpatico relationship that ended with his untimely death. He and Joos, the great German physicist, taught me all I ever knew about the administration of research. Through the years we met irregularly but frequently. My wife and I were in Ottawa during the formative meeting of the Canadian Association of Physicists (May 1948, Standish Hotel, sponsored by NRC). Frank brought us there as his guests, and along the way explained why the meeting had to be held in Hull. "You see Nat," he said, "Ottawa is dry, and no one would come unless there was booze." (He was right.) I always regretted not having signed up as a charter member.

After he had formed the Defence Research Telecommunications Establishment at Shirley Bay, he invariably sought me out during my various visits. In addition to discussions on purely scientific or technical problems (e.g., the ionosphere or communications) he brought in other matters that affected us both: inspiring and motivating researchers, the life cycle of a research organization, and even the value of a lounge. He had an infectious humour and always provided me with interesting tidbits, that I have often quoted through the years. I owe much to him and his wise counsel.

8. The Conferences

During the very earliest program planning stages, when my only staff was my secretary, I was very much aware of the "dearth

of knowledge" in the field. It seemed to me that the sponsorship of scientific Conferences on various aspects of our interest was vitally needed. My argument, which USAF bought, was that non-supported researchers would be drawn into expanding their efforts into upper atmospheric physics without direct support. Also, the stimulation and competitiveness engendered by a Conference would more actively motivate researchers under our direct support.

On this basis I then initiated a number of seminar-type Conferences constrained to my fields of interest. The first two, the Conference on Ionospheric Propagation, and the Conference on Ionospheric Physics, were held at Penn State. The third in this series was the Conference on Auroral Physics, held at UWO. The fourth, a Conference on Microwave Meteorology, was convened at McGill, and the fifth, on Electromagnetic Propagation, at NYU. Proceedings of the first four were published as Geophysical Research Papers by AFCRL, and of the fifth in "Pure and Applied Mathematics."

(After the first Conference, Lloyd Berkner wanted me to host future Conferences under the auspices of the Institute of Radio Engineers or the International Scientific Radio Union. I resisted, feeling that the distinctive nature and thrust of our Conferences would be lost. My objective was the stimulation of research on upper atmospheric physics. I felt that the Committee structure of the IRE and URSI could dilute the effect. My goal was a concentrated, in depth review of the state of knowledge, the better to define the deficiencies and chart the next steps to be followed. I also remembered Frank Davies, "No Committee ever won a Nobel Prize.")

The Conference on Auroral Physics at UWO was a resounding success, and probably the most outstanding of all that I was ever associated with. The location was influenced by non-scientific considerations. I really wanted the Conference held at Saskatoon, but many of the proposed invitees were European. The extra travel costs loomed as a factor. Also, more scientific talent would have been drained away from the research team at US than at UWO. Further, Misener was good at inveigling others to assist in administration. It was upon these considerations that the Conference was held in London rather than in Saskatoon.

Every major researcher in the field, except Vegard, was present: Barbier, imposing and dignified; Chapman, imperious and haughty; Alfven, cheerful and imperturbable; Bates, witty and sharp; Massey, quiet and reserved; Stormer, venerable and warm; Ta Yu Wu, formal and correct; E. Vassy, professorial and effective; Herman, careful and weighty; Herzberg, impeccable and precise. There were the youngsters to whom the torch would pass; Forsyth, Hunten, Nicholls, Meinel, Vallance Jones, Montalbetti, Donahue, Dalgarno, Bowles, Johnson and others. (Chapman always called me Major Gerson; he never realized that after six years in the National Guard I had only attained the rank of corporal.)

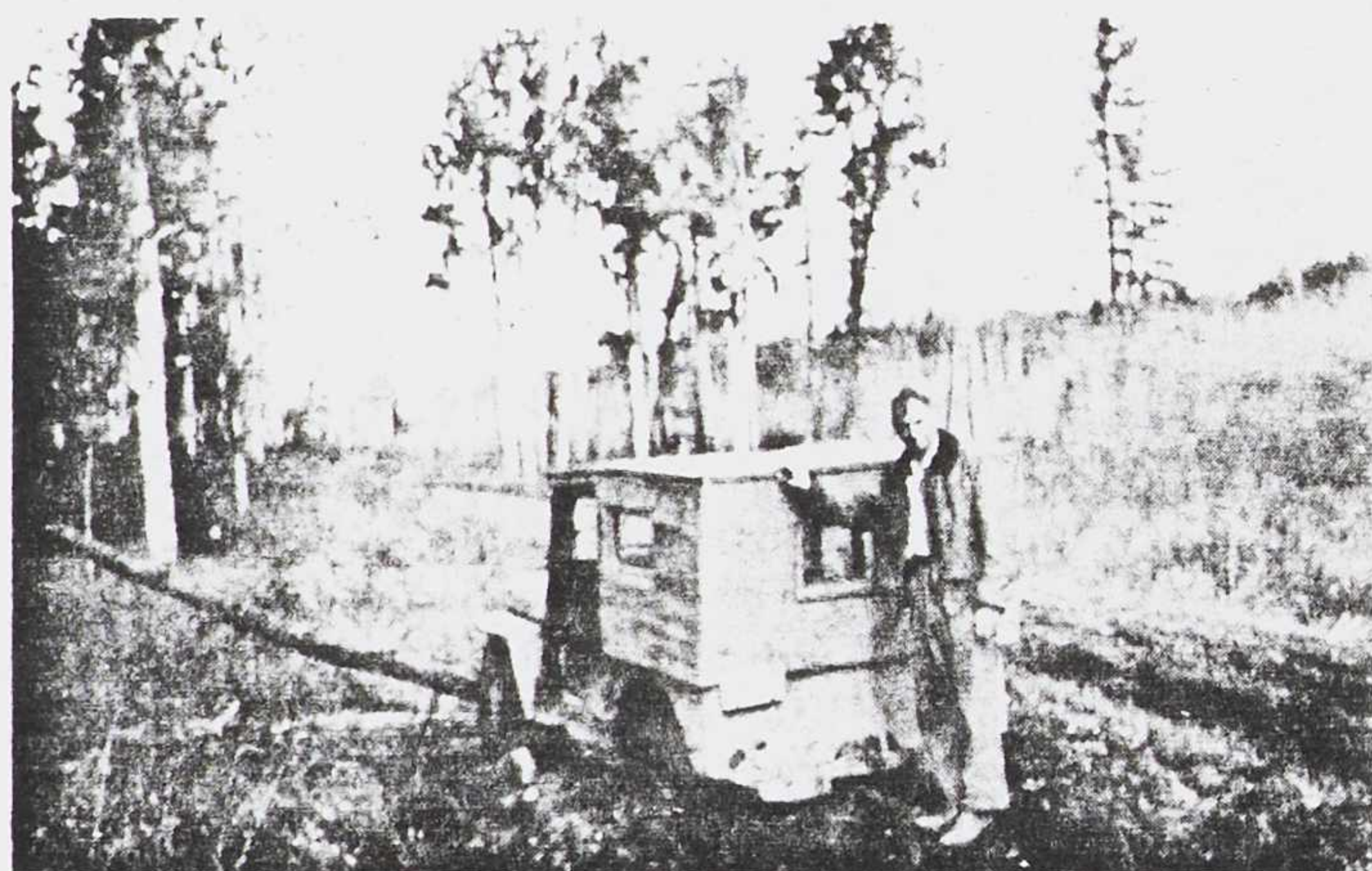
Stormer, the oldest, was deemed the most charming. Not only was he viewed as a symbol of the dedicated researcher foregoing the comforts of the University for the hazardousness of the Arctic of the 1920s, but also as the link to Birkeland and the auroral pioneers of the past. It was Stormer who had obtained the photographs for triangulating altitudes of the lower borders of the aurora. It was Stormer, too, who had laboriously calculated, by hand, the trajectories of charged particles approaching a magnetized sphere (or terrella); results later utilized in characterizing allowable and forbidden paths for cosmic radiation reaching the earth. Stormer's "mouse" trick had to be repeated until all had seen it. He folded a handkerchief in the palm of his hand; when he opened his fist the folded cloth literally leaped forward.

The Conference included all aspects of the auroral problem as I knew them at that time. Alfven and Chapman presented different views on the formation of the aurora, with Alfven introducing the presence of electrical fields. Chapman strongly disagreed. To

insure that all present clearly appreciated the sharp divergence in these approaches, I changed the agenda and arranged a special "debate" that more clearly expressed their differences. Misener cajoled the city into hosting a dinner for us.

The formal Conference was followed by an informal Colloquium attended only by experts and active researchers in the field. (I had instituted this practice following the second Penn State Conference, and the Colloquium on Mesospheric Physics was held at AFCRL in Cambridge. The same procedure was followed at UWO.) Its purpose was to allow frank discussions, to surface private views which could differ from public ones, and to stimulate thought by the clash of ideas. Thus, after most Conference attendees had left, we held a two-day round table forum of four sessions in a basement room of the newly constructed University library. Misener arranged for a photograph of this group. A summary of the Colloquium was published in the Journal of the Franklin Institute.

The excitement and exhilaration provided by the Conference on Auroral Physics was truly remarkable. The increased enthusiasm among the active researchers could never have been purchased with dollars.



Field intensity measurements. Dawson Creek, Sept. 1947.

9. Extraterrestrial Particles and Geomagnetism

My main thrusts to this point had been in aero- and ionospheric physics, involving the ionosphere, aurora, and troposphere; the practical goal was improved radio wave propagation. Polar communications problems at High Frequencies (3–30 MHz) were well known, and I had experienced them myself when stranded at Baker Lake, Norman Wells and Churchill. They were generally unpredictable in onset, location and duration, but it was known that at times their presence could be associated with the presence of auroras. A question that gnawed at me was whether a precursor could be found which would provide warning of an impending HF blackout.

My attention turned more fully to geomagnetism and extraterrestrial particles after the Radio Meteorological Section had moved to another Laboratory within AFCRL. With respect to the latter, I had noticed that some polar absorption events correlated with abrupt cosmic radiation increases. I wondered whether an examination of incoming solar corpuscles might provide a precursor to an absorption period. John Simpson at the University of Chicago has been using neutron monitors to measure increases in the solar component of cosmic radiation. It seemed logical to explore this technique further, particularly since neutron monitors measured the low energy component of cosmic radiation; i.e., that component emanating from the sun. The contract subsequently let with him at UC had this objective in mind. It also included a determination of the cosmic radiation equator obtained by utilising an aircraft borne monitor to determine the latitudinal minima (in neutron intensities) determined at a number of different longitudes.

The problem relative to geomagnetism was more complicated: practically no research in geomagnetism was underway in North America, with the exception of the small effort by Harry Vestine at the Carnegie Institution of Washington. The forefront of geomagnetic research was elsewhere; under Bartells in Germany and Nagata in Japan. I visited several U.S. Universities "sizing up" interest and competence. Rice University had a magnetometer, examined sporadically, on campus but that summarized their interest.

At this time, the National Committees for the International Geophysical Year of 1957-58 were organizing. I was appointed to the U.S.N.C. for the IGY and became Secretary and Secretary of its Executive Committee. During several meetings at the international level, I discussed this deficiency with Bartells of the University of Göttingen at some length. I then asked whether he could recommend one of his thoroughly competent students who would be willing to work in a Canadian or U.S. University. I would insure funding. This student must be outstanding in geomagnetism and, by osmosis, should attempt to interest his associates. Bartells sounded out his proteges, and advised me that one named Schmidt was both suitable and willing to make the move. In the meantime, I approached Tuzo Wilson, head of the Geophysical Institute of the University of Toronto, to determine whether he would be willing to become involved with geomagnetism. He was and a contract was written.

A problem arose when Schmidt changed his mind and decided not to work in North America. Bartells was embarrassed but could provide no replacement. I then pursued the matter with Nagata of the University of Tokyo — during one IGY meeting in Rome, Italy, and over a Chinese meal that he always loved. He was interested and promised to think it over. Time passed and I wrote him on several occasions. Finally he chose T. Obayashi. The Japanese Government paid for his transportation to Vancouver but not one yen more. (Their economy was too strapped.) He arrived there essentially penniless. From the contract, Tuzo advanced him travel expenses to Toronto. When I later asked Nagata why his selection process had taken so long, he replied that Japanese honor would be at stake, and he had to insure himself that only the most outstanding student had been selected. He could hardly have made a better choice.

Since Obayashi knew no English, I waited a period of months before attempting to visit UT. There was no doubt that Obayashi knew geomagnetism. However, during his conversations with me I could never decide whether he was speaking Japanese with an English accent, or English with a Japanese accent. He was one of the most outstanding researchers at UT. He typically arrived at work at the usual time, but frequently worked until 3AM before quitting. The faculty individual assigned by Tuzo to oversee Obayashi was Jacobs. Once when I was chatting with him, Jacobs told me that he was a hard rock geophysicist, did not want to become involved with geomagnetism, and was not going to. Despite the fact that these comments left me with great misgivings, Jacobs did become seriously interested. He and Obayashi attained international reputations in this field.

10. Postlogue

The results of all these efforts were impressive. At McGill the Stormy Weather group became famous for its outstanding accomplishments in precipitation physics. At Saskatoon, the investigations on the optical and radio auroras made Canada the leading nation in this field; the work had shifted the center of gravity of auroral physics from Scandinavia to North America. The careful work experimentally clarifying transitional probabilities brought recognition and acclaim to Nicholls, his associates and UWO. And, at Toronto, Jacobs finally did interest himself in geomagnetism and became an international expert.

It is a tribute to all individuals involved (including many not named), and a matter of some awe to me, that these four contracts

trained a generation of atmospheric physicists, advanced the frontiers of knowledge, brought fame to their Universities and brought renown to Canada. Looking back, I tend to ascribe this remarkable record to three causes, the first two of which are minor. Firstly, I had just completed my thesis and I saw certain obvious deficiencies in the field. Secondly, I was asked to establish a research Laboratory in this very field and was given very strong backing and extreme latitude in implementing my program. No one questioned my decisions. Thirdly, I was able to find bright, competent and vigorous physicists. They performed the work and they provided the accomplishments. Perhaps I supplied the seed and the watering, but they deserve the credit.

Again I remember Frank Davies. "Nat, in physics if you want something done well, find a bright young lad and back him. You can always find plenty of mediocre chaps, but they won't get you anywhere." I had only followed his advice; all of us reaped the benefits.

11. Associated Comments

The USNC was formed in 1953, and the Canadian Committee for the IGY somewhat later. In addition to being on the parent Committee itself, I was active in the Panels and lesser Committees. Thus, I was designated Chairman for the first two Antarctic Committees and also vice Chairman of the Arctic Committee. Considerable coordination was needed, particularly among the national polar Committees to insure that a globally comprehensive investigation eventuated. These meetings again brought me into contact with Balfour Currie, Don Rose and Tuzo Wilson at the more formal international gatherings in Europe. Currie was extraordinarily helpful in assisting me as I prepared the Arctic Ocean Basin program for the UNSC.

Women in Physics

by *W.J. Megaw*

Department of Physics, York University

I have the privilege of being Chairman of a Department of Physics in which there are 25 faculty members, of whom one is a woman, 40 graduate students including two women, and we produce about a dozen physics graduates every year including, if we are lucky, one or two women. There seems to be a big imbalance somewhere. From discussions with my colleagues in other Ontario Universities I believe that this situation is reproduced, with minor variations, right across the Province. The pattern seems to be rather different in other departments of the Faculty of Science at York. Three out of twelve chemistry graduate students are women and half of the graduating bachelor's class will typically be women. In biology 27 out of 54 graduate students are women. Of all entrants to York 51% are women and out of all graduate students at York, 36% are women. It seems therefore that physics as a discipline is failing to attract its share of half the population of university students.

I have thought a lot about this problem in the last year or so. I believe that women in general make first class physicists and I am anxious to do something to redress the balance as far as my own department is concerned. It seemed sensible to go to the high school physics teachers and seek an explanation there.

I found an explanation, but not a reason. The same situation exists in the high schools. I am told that a typical grade 11 biology class will be split more or less equally between the sexes, that a chemistry class will perhaps be 40% girls and a physics class 10%. Thus biology is apparently considered (among women) to be an appropriate subject for study, chemistry moderately respectable and physics quite beyond the pale. I cannot really see why this should be. A superficial look at the subjects would suggest that a quite different priority might prevail — after all to the grade

Several isolated items may be mentioned. Don Rose's specialty at NRC was cosmic radiation. I visited him in February 1956 (to coordinate various activities) and found his laboratory in some disarray. "The equipment was not working," and the electronics was spread out on the bench as the individuals meticulously checked each circuit to find and fix the trouble. About noon the Secretary brought in an "urgent" message stating that the most extensive cosmic radiation event in history was now in progress, having begun several hours ago — at the time the technicians concluded that the equipment was not working.

A similar "first thought" later occurred to Van Allen, when he examined the telemetry from his cosmic radiation experiment aboard the Explorer. After first believing that his equipment was malfunctioning, he later realized that the sensors were swamped and that the cause must lie in the presence of the high energy radiation belts. Luckily he could not tamper with his equipment.

Another item concerns Frank Davies. After the initiation of the IGY, he confided to me that he would like very much to revisit the Antarctic. It became apparent that he did not want to make a formal request himself, but would relish an invitation. I spoke to Lloyd Berkner, who enthusiastically agreed, and Frank was subsequently invited. However, because of the pressure of other matters, he did not make the trip.

Finally, I should mention that after acquiring a suitable staff, I assigned various individuals to monitor the contracts; D. Atlas for McGill, W. Pfister for Penn State, N. Oliver for the University of Saskatchewan, F. Innes for UWO and L. Katz for the University of Toronto.

7 or 8 student biology is mainly concerned with cutting up worms or frogs and chemistry with evil smelling gases and noxious liquids while physics is the only one of the three with any pretensions to hygiene or civilised society, so the phenomenon cannot be ascribed to more delicate sensibilities. It has been suggested to me that there is a widespread belief among school counsellors that physics is 'too difficult for women' but I cannot believe that such a statement could ever be taken seriously in this day and age.

At York we have a system, of which I approve but the first year students certainly do not, that all science students have to take at least one social science and one humanities course. Conversely all non-science students have to take a Natural Science course, typically at first year level. These courses are non trivial and it is a source of satisfaction to the science professors teaching them that while the students, without exception, take them against their will and only because they are forced to, by the end of the course a high proportion of them are kind enough to say that they are glad they had been made to take the course. I happen to teach one of these large courses and I find it rather depressing to be able to identify perhaps 50 students per year (equally split between the sexes) who, in my view, would have made good scientists if only something had not turned them off earlier in their school careers. Even more depressing is the fact that there are probably 20 each year who have been turned on to the extent that they would now like to become scientists except that it is too late, unless they are prepared to spend at least one year, and probably two, doing remedial work to cover not only the physics they have missed, but, even more so, all the math. I think that perhaps the most important reason for women not entering

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Military Physics — The Balloons

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Dr. Gerson worked for many years in collaborative U.S. — Canadian projects. He described many of these in his article in the January 1984 issue of *Physics in Canada*, "Collaboration in Geophysics — Canada and the U.S. 1948-1955".

INTRODUCTION

While balloons of childhood are decorous, delightful and even joyous, those of adulthood can be awkward, onerous and ornery. By the time that I joined the project, Headquarters had committed itself. Three balloon-borne antennas already had been deployed, one at each of the three transmitter sites: the old RCAF aerodromes at Gimli, Man., North Battleford, Sask., and Dawson Creek, B.C. All balloons followed the example of the young girl in Grimm's *Fairy Tales*. When they were good they were very very good, but when they were bad, they were horrid.

The transmitters operated in the Low Frequency portion of the radio wave spectrum. At these frequencies the usual difficulty of "radiating the energy from the wires" is overcome by utilizing large antennas on high towers with properly coupled circuits. High towers are costly; we used the rule of thumb that costs increase with the cube of the height. It was decided that at least in the early stages of the experiment towers were unwarranted. The primary objective still was a determination of whether LF Loran had utility as a long range electronic navigational system.

Although he had never launched one, Dr. K. had seen many meteorological balloons released. Also, he once had inveigled me (while I was a student) into undertaking the first tests for determining upper air winds using ground based radar and balloon borne reflectors. (The technique was so successful that it became standard practice.) On the basis of this remembrance he jokingly suggested to Col. H., "Well, we could always use balloons to support the antenna wire." The Colonel took the suggestion seriously.

According to theory a vertical radiator about 1300 ft high was desirable. The cost of a steel tower of this height would be about one Megadollar. In contrast a tethered balloon, with the tether being the antenna wire, could be about as effective (despite a few difficulties) and much cheaper. The problems did not seem insurmountable. Large balloons and a good supply of helium for inflation were needed. Projections for losses of balloons and helium during inclement weather were made on the back of an envelope and deemed acceptable. The wire type and size were determined from estimates of the stresses expected and the electrical properties required.

From these quickie calculations the number of balloons, amount of helium and length of wire were determined. As a safety factor the quantities were doubled. All supplies could be stored in the cavernous hangars at each transmitter site. "Simple," said the Colonel, "Good idea. I'll have someone track it down."

The type of balloon adopted was chosen using statistical mechanics and invoking Maxwell's Demon. The sergeant examining the availability and costs of suitable balloons blundered into the wrong Hail and Farewell ceremony. He knew no one but the beer was free and the food good. The affair was long and convivial and ultimately extended into the wee hours of the morning. While wandering about he overheard an Army Warrant Officer complaining about his Commanding Officer.

It seemed that the CO had inherited two warehouses full of WW2 barrage balloons and was badgering the WO to get rid of them. He had been a tank commander during the war and now wanted to store surplus WW2 tanks there instead. Emboldened by his fourth beer, our sergeant disregarded channels and interservice distinctions, and broke into the conversation. While both were still able, they swapped organizational addresses. They agreed to shift accountability to our Project in two months. "Not to soon," said our sergeant, "otherwise they'll give me more work."

The balloons arrived and were transported to our storage area. They appeared ideal for our purpose and provided several features not originally anticipated. Their fins allowed additional free lift (produced by kiting in the wind), and a smaller horizontal displacement from the tether point during windy periods. Both factors brought stability and reduced strains on the wire. They were larger than necessary (requiring more helium per filling) and while they placed a greater constant strain upon the wire they decreased the strain jitter. On the whole, however, they wandered less and so provided a more consistent antenna pattern and navigational coverage area. Because of their kiting action they were sometimes called kitoons.

The plan was implemented and the wire, helium bottles and balloons were stored in the hangars. Considerable helium was used for each filling. All supplies were freighted northwards from New Jersey by our C-47s. When needed the bulky material was unfolded on the hangar floor, inflated, and carefully brought outside to the antenna shack. There it was tethered to the antenna wire and slowly winched upwards.

Both ends of the wire were attached to large "Johnson" insulators. During darkness we were all reluctant to stroll near the tether point. Two GIs had been severely jolted one night when, on a dare, they laughingly touched the high voltage antenna. News of this incident spread throughout the system, and proved far more effective in catching attention than all the "High Voltage" posters and repeated verbal warnings combined.

We relearned Benjamin Franklin's dictum, "Experience keeps a dear school but fools will learn in no other." First, current knowledge of the climate of southwestern Canada proved inadequate. Statistics on gustiness (which we needed) were not readily available. To us, winds and turbulence could be disastrous. Many tethered balloons simply broke loose and disappeared into the cloudy grey yonder. The transmitter shut down when the helium supply was exhausted. During one period of continued losses, the possibility of shipping tank cars of helium to the sites was considered.

Our experiences exposed deficiencies in physics and synergistics. We discovered a curious inversion of statistics unknown to theorists and meteorologists; e.g., the probability of malfunction increases with the dire necessity to function. Thus, in our case balloon and transmitter problems maximized under two conditions: (a) when navigational areas and accuracies were being measured aboard our B-29s flying over the arctic ocean, or (b) when I was measuring field intensities in the wilderness around Fairbanks, Norman Wells or Baker Lake. With no communications, we in the field spent hours dismantling and checking equipment or moving to new areas.

The balloons posed challenges of ingenuity and difficulty never appreciated by Headquarters. During high winds transfer of an inflated balloon demanded considerable agility and a disregard of personal injury on the part of the crew. Movement of the balloon to the antenna was virtually impossible during these conditions even with ten men hanging on. It pounced, flipped, banged the side of the hangar, attempted to stretch, and ultimately struck some unnoticed sharp object that caused a tear and deflation. A sudden gust of wind could strain muscles and tendons.

The original guidelines stated that when high winds were anticipated balloons should be lowered and secured. At first these procedures were initiated on the basis of the weather forecasts. However, the number of false alarms was too great so that later the reports were disregarded. Attempts to lower balloons during high winds invariably failed. They could not be handled. We lost the balloon and its helium.

We learned the hard way. The constant tugs on the wire ultimately caused breakage by metal fatigue, almost always at the ground end. The balloon then roamed the countryside with its dangling 1300 ft trailing wire. If this happened during high winds or in winter when temperatures were low, the vertical movement of the balloon was excruciatingly slow. The wire could drag on the ground or over housetops for considerable distances. When we knew that a balloon had broken loose no one wanted to answer the telephone.

The balloons also rose to the top of the Government. About a year after their introduction, a small civilian aircraft flying during daylight at a much lower altitude than allowed clipped the wire with its wing. The plane landed safely with no damage, but the pilot was furious. He rushed first to the media and then to the authorities claiming that had it struck the wire the propeller would have shattered or the fuselage would have been shorn in two. In either case the plane would have crashed and lives would have been lost.

The issue quickly became a cause celebre and escalated into a confrontation between Defence and Commerce. We at the technical level did not believe the dire comments of the pilot but were rather reluctant to suggest an experiment. Both RCAF and USAF planes had been flying near and around the balloons for months without incident. Further, balloon locations, altitudes and the fact that they supported antenna wires had been widely publicised in the official Notices to Pilots.

The dilemma was resolved at the ministerial level. The fact that the plane should not have been flying so low or so near the balloon, that the mishap occurred during daylight when the balloon was fully visible, or that the pilot should have known better, was disregarded. A lighting system was impractical because the free lift of the balloon, even though oversized, was not large enough. It could not accommodate lights and the antenna. The Conferees decreed that henceforth the balloons must be illuminated during the night and dusk.

The Canadian Army was ordered to implement the decision. It instinctively dusted off its Table of Manning and discovered the following. Three searchlight sets were necessary at each site; the first for operation, the second as a spare, and the

third as a backup. Each set included a searchlight and diesel-electric generator for power. Each shift required two operators and one diesel mechanic; three shifts required four times that number to allow for training and leave. This additional complement required billeting and meals. Two more cooks and the appropriate rations were added. Rooms were no problem.

The Army contingent duly arrived. They were neither engineers nor technicians. They were lusty farmboys; outgoing, earthy and rough. They introduced a new vocabulary into the mess hall and thence into the corridors. They frequented the local bars and quickly became their major source of revenue. Homesteaders began to place curfews on half their children. As one local wag stated, "For want of a tower the town was lost."

A minor problem occurred one day at Gimli during a high wind. The crew shut down the transmitter and tried vainly to lower the balloon. It tugged, bounced and lashed wildly. During some gusts the winch let loose and the balloon soared in a frenzy. Finally the wire at the base snapped. The free balloon galloped at the mercy of the elements across the countryside of Manitoba. It gained little or no altitude. The wire dragged across fields, barns, roads, and telephone wires.

Unfortunately, there was a high tension line only several kilometers from the transmitter. The suspended wire draped over this 23,000 volt line while its free end still straddled the local electrical supply system located on the ubiquitous poles. Havoc broke loose. The wire vaporized as phases and voltages mixed. Throughout every house and establishment large blue arcs immediately flared out of each duplex outlet. The internal contacts fused and an area on the wall four inches around each outlet became charred. Pandemonium erupted in the area. Incredibly there were no personal injuries or fires.

When news of this disaster clobbered Headquarters, Col. H. was overheard muttering about retirement. RCAF agreed to reimburse all damage costs. Every household in the vicinity, even those with no electric power whatsoever, submitted bills for repairs. All were paid. For some farms this payment allowed electricity to be introduced. The duplex outlets and fixtures in our buildings at Gimli were among the last to be replaced: The charred area around each outlet was not repainted for a year.

Balloon operations in cold regions caused us to relearn applied physics. One night at North Battleford, try as I might, the antenna meter indicated no radiated power. We exhausted all conceivable tests on the equipment and circuitry inside. I finally concluded that the balloon with its antenna must have broken loose. We turned off the power and I bundled up for the -32F temperature outdoors. Walking slowly over the crunchy snow I sought the outline of the balloon against the stars.

It could not be seen. Uncertain as to where it might be I cautiously approached the antenna shack. It was always possible that someone inside had thrown the switch; in that case I did not want to find out by touching the antenna. Walking with my eyes riveted on the heavens I still could not find the balloon. The night was clear and cold with a slight breeze. How could the balloon have broken loose?

When my chin touched a damp cold rubberized fabric I jumped back in fright. The balloon was lying on the ground like a large cow, the wire looped hither and yon. I had collided with it while peering at the heavens. The answer came quickly; the outside temperature was too low. The volume of helium within the balloon had decreased to such an extent that it no longer could support the weight of the antenna and the fabric. We quit for the night.

Despite their deficiencies and in spite of their problems, the

balloon borne antennas served their purpose. Even with the losses of helium and balloons, and lapses caused by resupply delays, they allowed the feasibility and practicality of the LF Loran system to be demonstrated at reasonable cost. Based on the results obtained from these tests, the 100 kHz Loran C navigational system subsequently was widely implemented in the Northern Hemisphere. Despite satellite positional systems, and the Omega hyperbolic system, it still serves as a mainstay for air and marine navigation.

* * * * *

Nuclei Far from Stability: Observations from Rosseau Lake

I.S. Towner
Chalk River Nuclear Laboratories

The production and examination of nuclides far from stability are the consuming passions of many nuclear physics laboratories around the world. Last September key players in the field gathered in Canada for an international conference at Rosseau Lake in the delightful Muskoka region of Ontario. This was the fifth in a series of conferences on "Nuclei Far From Stability" that began in 1966 at Lysekil, Sweden and has proceeded at about five-year intervals since: at Leysin, Switzerland (1970); Cargèse, France (1976); and Helsingor, Denmark (1981). This year's gathering, the first to be held outside Europe, brought together 180 physicists from 20 countries (90 from Europe; 50 from USA; 20 from Canada; 7 from Japan; 4 each from USSR and Israel; 2 from India and one each from China and Kuwait). There were 36 hours of lectures spread over six days interrupted by a boating and hiking excursion to Beausoleil Island in Georgian Bay Islands National Park. The meeting was organised by scientists from the Chalk River Nuclear Laboratories under the chairmanship of John Hardy.

There are three directions in which an explorer may venture in climbing out of the valley of stable nuclei. In light-mass systems he may ask: how many neutrons can be added to an isotope of given atomic number before the formed nucleus emits neutrons spontaneously. This limit is known as the neutron drip line. Likewise he might ask how many protons can be added to an isotone of given neutron number before the formed nucleus emits protons spontaneously — the proton drip line. In the standard chart of the nuclides in which the neutron number is the abscissa and the proton number the ordinate, these explorations take the venturer in easterly and northerly directions respectively. And if that alone were not enough the third and more glamorous direction of exploration is east-north-eastwards to the heaviest elements ever synthesized.

It is estimated there are 8,000 nuclides within the limit of the drip lines of which 2,000 have been identified so far. There is much uncharted territory. Conference summary speaker Peter Armbruster from the GSI laboratory in Darmstadt, West Germany noted that in recent times about 40 new nuclides have been identified every year. But he also envisaged a saturation point. He doubted whether more than 3,000 nuclides would ever be identified. The cross-sections for production become so small and the lifetimes so short to decay by α -emission, β -emission or spontaneous fission that their observation would be technically not feasible.

At the conference Dominique Guillemaud-Mueller of the GANIL laboratory in France reported that the relatively high beam intensities of heavy ions available and the performance of a doubly achromatic spectrometer have enabled a breakthrough to be achieved in reaching the neutron and proton

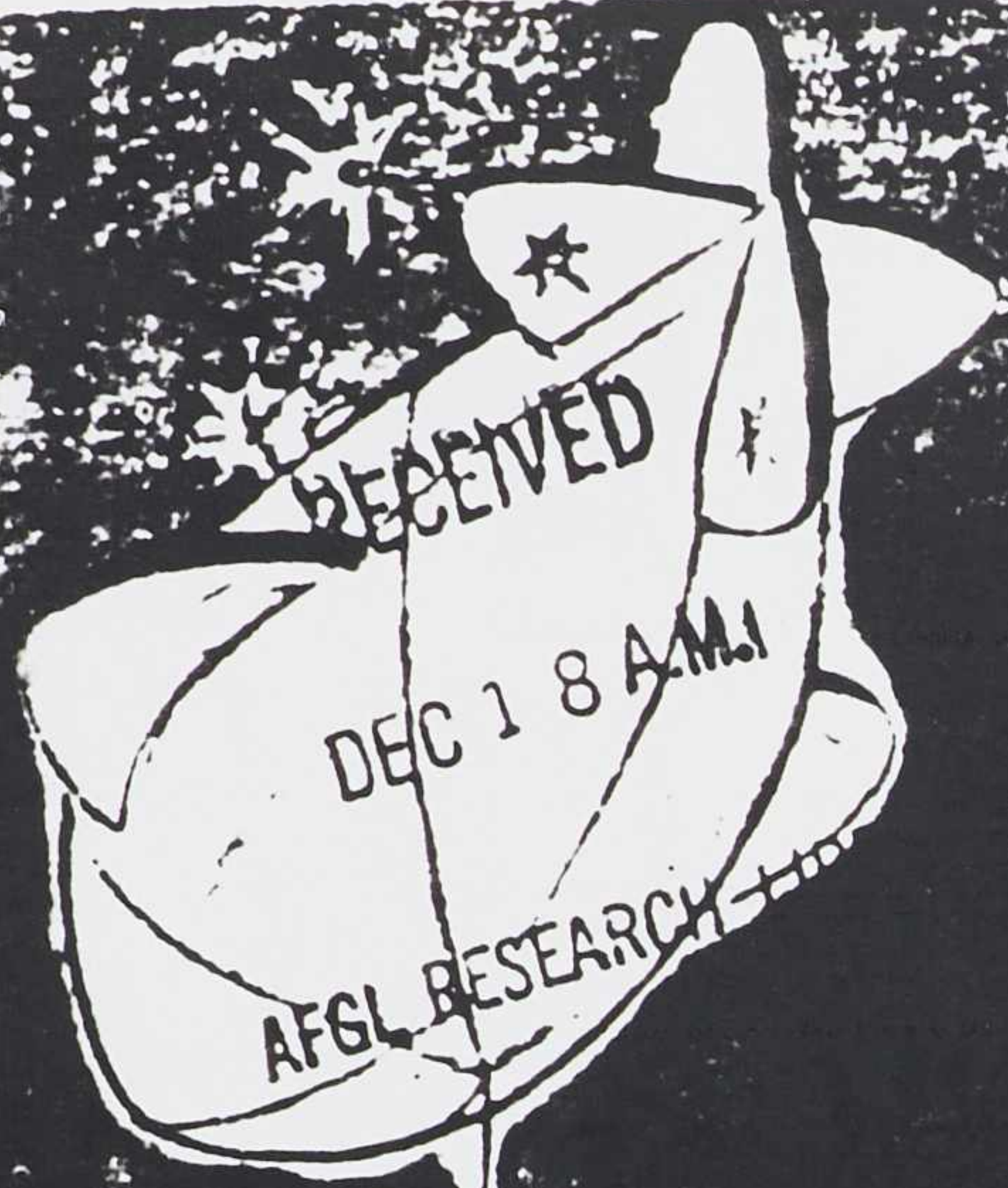
drip lines. On the neutron-rich side she showed data proving the existence of the new nuclei ^{22}C , ^{23}N , ^{29}Ne and ^{30}Ne and confirmed the theoretical expectation that ^{18}B , ^{21}C and ^{25}O would all be unstable. On the proton-rich side, three experiments with projectile beams of ^{40}Ca , ^{36}Ar and ^{58}Ni on natural nickel targets found that: (1) ^{23}Si , ^{27}S , ^{31}Ar and ^{35}Ca with $N-Z = -5$ are all "stable" to nucleon decay; (2) ^{22}Si , the first-ever identified nucleus with $N-Z = -6$, is also "stable" contrary to some theories while ^{19}Na and ^{21}Al are not; and (3) twelve new proton-rich nuclei ^{43}V , ^{44}Cr , $^{46,47}\text{Mn}$, ^{48}Fe , $^{50, 51, 52}\text{Co}$, $^{51,52}\text{Ni}$ and $^{55,56}\text{Cu}$ are "stable" too. The GANIL work is a veritable tour de force.

Earlier this year, the Russian group from Dubna at an international school on physics in Varenna, Italy reported on experiments aimed at synthesizing the element with proton number $Z = 110$, which would be the heaviest element yet found. It was particularly surprising, therefore, when a few days before the conference was due to begin a telex was received by the organisers indicating the Dubna spokesman for the experiment would be unable to attend. After a further flurry of messages it was agreed that a preprint of the Russian work would be available to the conference. In it, Y.T. Oganessian et al. state: "By comparing experimental cross sections for producing the spontaneously fissioning nuclide, obtained in the $^{235,236}\text{U} + ^{40}\text{Ar}$ reactions, with the calculated ones and with those measured in the control experiments using the $^{232}\text{Th} + ^{40}\text{Ar}$ reaction we are inclined to assign the observed activity to the decay of a nucleus with $Z = 110$ ". There was considerable scepticism of this result, however, especially from the competing GSI laboratory. Spokesman Fritz Hessberger said that in two experiments, $^{208}\text{Pb} + ^{64}\text{Ni}$ and $^{235}\text{U} + ^{40}\text{Ar}$, they had seen no α -decay or spontaneous fission that could be attributed to the element 110. Their upper limit for production of element 110 in $^{235}\text{U} + ^{40}\text{Ar}$ is quoted as being less than eight picobarns. The Russians state "the nuclide has been produced with a cross section of about ten picobarns".

The GSI group announced two positive results. In 13 days of irradiating ^{209}Bi with ^{58}Fe one decay sequence was observed that could be attributed to the heavy nucleus, $^{266}109$, with a production cross section of 15 picobarn. And in a 14-day experiment, $^{208}\text{Pb} + ^{58}\text{Fe}$, three α -decay chains were seen that could be attributed to $^{265}108$, with a formation cross section of 19 picobarn.

Production of nuclei far from stability in itself is not the only interest of the conference delegates. Equally important and occupying much more of the conference time are measurements of their masses and decay properties. For any mass measurement, two main problems have to be solved: adequate production cross section and adequate mass resolution. W. Mittig reported that at the GANIL facility with a high resolution energy-loss spectrometer a mass resolution of 8×10^{-4} is obtained in measuring masses of neutron-rich isotopes of boron to phosphorus. From Los Alamos National Laboratory, Dave Vieira reported on experimental results from the time-of-flight isochronous spectrometer that had been built expressly for direct, systematic mass measurements of light- Z neutron-rich nuclei. Results were presented for nuclei from carbon to phosphorous and with the exception of $^{27,28}\text{Ne}$ there is generally good agreement between the two groups.

In heavier isotopes mass measurements far away from stability are generally based on the determination of decay Q values, as described for example by Erik Hagberg (Chalk River) and Fritz Münnich (Braunschweig). Even at their best, though, these methods suffer from uncertainties introduced by summing up the individual uncertainties of the many mass differences which link the mass of the isotope under investigation to that of a well known mass. Hence it is very desirable to determine the masses directly, independently of the knowl-



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Planning for the U.S. Polar Programs
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It may be of interest to note for the record some of the factors which influenced planning of U.S. Polar Programs for the International Geophysical Year (IGY) 1957-58.

The author had been appointed to the U.S. National Committee (USNC) for the IGY, a committee of the National Academy of Sciences, and was made Secretary by Joseph Kaplan, Chairman. He was later designated chairman of the first two Antarctic Committees and Vice-Chairman, under William Reed, of the Arctic Committee.

The first Antarctic Committee was charged with the policy question: "Should the U.S. undertake an antarctic expedition?" Some preliminary discussions about a possible Antarctic Program were first raised in the USNC Executive committee by Lloyd V. Berkner, who proposed that geophysical investigations there were consistent within the framework of the IGY. He suggested one station at McMurdo Sound, the base of previous U.S. antarctic expeditions. He had been there during the Second Polar Year of 1932 under the leadership of Admiral Richard E. Byrd. (Other members of the full USNC also had been at McMurdo at that time: Paul Siple as the foremost U.S. boy scout, Laurence Gould, etc.)

During subsequent discussions within the Executive Committee, two Antarctic sites were proposed for scientific reasons. Harry Wexler noted that almost all explorers had reported that

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coastal winds blew towards the sea. He suggested that they arose because radiational cooling formed a dense continental air mass which then flowed down the slopes towards the sea. To test this hypothesis he proposed location of a station well in the interior of Antarctica. Ultimately, the Byrd Station was established for this purpose.

The author strongly urged locating a station at the South Pole. He noted that the geographic poles constitute two unique points in earth-sun geometry where even at ionospheric altitudes sunlight is completely absent for months on end. According to the conventional theory of the time, the ionosphere should totally disappear because of the recombination of electrons and positive ions during the long polar night. He had previously examined the mid-latitude ionosphere and could not understand why ionization in the F layer there persisted throughout the night. One possible cause was erroneous electron-ion recombination coefficients; another was transported ionization from other regions. To obtain additional information he had instrumented (while chief of the USAF Ionospheric Physics Laboratory) an ionosonde aboard an aircraft. This aircraft was sent to Thule, Greenland, where it was to fly to the North Pole, circle to the extent allowed by its fuel and then return to Thule. Ionospheric observations were made from takeoff to touchdown. Unfortunately, the aircraft spent an undue amount of time in Oklahoma City "because of oil leaks." When he learned that the pilots' wives lived in Oklahoma City, he realized that the logistical problem was unsolvable.

With this experience, the author insisted that a South Pole station was essential to provide ionospheric observations during the long polar night. It would also allow continuous rather than a spotty record of ionospheric changes, and thus better serve in explaining electron production. The executive committee in principle accepted both stations and established the first committee

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to report on the value of an Antarctic IGY program.

The meeting of the first Antarctic Committee was held one Saturday morning in the board room of the National Academy of Sciences around the heavy, ponderous wooden table. This meeting became a lesson in frustration. Paul Siple and Admiral Elliot Roberts (U.S. Coast and Geodetic Survey) squared off as gladiators for their favorite choices as leader of the expedition: Admiral Byrd or Capt. Finn Ronne, USNR, respectively. Comments about their respective deficiencies darkened the air but little discussion ensued about the need for a geophysical program. However, the charge to the committee was to define the need for an antarctic program first; the choice of a leader could come later.

Since the protagonists could not forget their choices the meeting adjourned about noon. Nothing had been accomplished.

A second meeting of the committee at the Academy resulted in similar partisanship, but no progress. The author felt discouraged and ultimately wrote the entire report himself and mailed copies to all committee members with a brief note: "If I do not have your comments within two weeks, I will assume that you concur." Siple was the only person who responded. He indicated typographical changes and suggestions on glaciological aspects of the program. All were incorporated. His most profound comment appeared in the margin of the paragraph that stated the value of the program to the U.S. was worth at least \$5 million dollars. Siple wrote, in pencil, "What is the value of God?"

The report, as submitted, was approved by the Executive Committee, endorsed by the full USNC, and finally submitted to Congress where it was funded.

The initial report of the first Antarctic Committee recommended the three stations noted above. Some concern was voiced in the USNC about the inclusion of three stations, but before the report was finalized and submitted to Congress, the author

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received a telephone call from Berkner: "The Soviets have proposed five Antarctic stations. Increase the number of U.S. sites." The final report proposed seven stations (McMurdo, Byrd, South Pole, Weddell, Knox, Adare, Williams Air Facility.)

DeepFreeze-1 constituted an exploratory visit to Antarctica by the U.S. Coast Guard icebreaker, ATKA. It was charged with determining the status of the old site at McMurdo and the feasibility of establishing the other proposed sites. Throughout the trip, it undertook cosmic ray, meteorological, and other geophysical observations. On the basis of its report DeepFreeze-2 was organized, with Admiral Byrd in command, to implement the sites. The author was asked to join it as Admiral Byrd's Chief Scientist. However, his wife, uncomfortable because of the 6 month assignment, stated: "Tell them that if I can go, you can go." Wexler joined as Chief Scientist.

Prior to the expeditions, the Navy had been approached relative to supporting the proposed stations. It responded with a formal letter detailing every expense involved, with a total price of about \$40 million dollars. In the Executive Committee, Merle Tove exploded: "They are charging for every day of every seaman, every shoelace, every meal, fuel, etc. These men would be on duty anyway." He finally finished with: "We will show them. We can hire a Norwegian freighter for about 30 thousand dollars and accomplish the same end." No one responded. Later Berkner spoke to President Eisenhower, whom he knew personally. Miraculously, all problems disappeared and the Navy cooperated fully.

The second Antarctic committee considered lists of supplies and detailed plans to implement DeepFreeze-2. The list had been prepared by the Navy and followed its routine procedures. On the whole, the second Antarctic committee rubber stamped these plans: no one wanted to examine communications, building sup-

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plies, fuel, rations, etc. Further, few had the time or expertise. One question was asked by the author and Wexler: "Why must the floors be tiled and waxed?" In prior visits to Arctic sites, the author lived in buildings with oiled, wooden floors. Gerson later visited both Antarctic and Arctic stations to conduct experiments. At McMurdo he was assigned the top bunk in a tier of four which rested on tiled, waxed floors. In the Arctic (Baker Lake, Hay River, Yellowknife) he slept in military sleeping bags lying on oiled, wooden floors.

A humorous incident arose when the subject of fuel was raised. The Navy agreed to tow two fuel barges to McMurdo and knew that on the high seas they would pitch and roll continuously. No one volunteered to man them. Finally, a bright staff commander suggested to the Air Force that it assign officers and men to man the barges as they were towed. The Air Force thought this offer was an honor and quickly filled the billets. The men were seasick until they left the barges at McMurdo.

The Third Antarctic Committee was chaired by Gould, with Admiral Byrd as Honorary Chairman, was concerned with overseeing implementation of the Antarctic program. At a subsequent meeting, Gould reported that Paul Siple was unhappy. He wanted to be placed in charge of the South Pole Station. The USNC approved, and he was named Station Chief. After his untimely death, the South Pole Station was named the Siple Station in his honor.

Planning for the Arctic geophysical program proceeded differently. After his experience with the first Antarctic Committee, the author wrote the entire Program himself, then critiqued it with oceanographers and glaciologists. Insofar as meteorologic, ionospheric, auroral, and geomagnetic physics were concerned, observations and objectives were similar to those for Antarctica.

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The original arctic report proposed three floating stations: one on the ice island and two on the pack ice. The Executive Committee severely truncated the program. During the lunch break the author complained to Berkner that the curtailed program of \$100 thousand dollars was below viability. After lunch, Berkner cleared his throat and announced: "I have been thinking...." Funding was increased threefold but the two ice floe stations were eliminated. The Air Force was given responsibility for implementing and supporting the program. In private lamenting about the lack of U.S. Arctic interest, a phrase was coined: "When the Antarctic is mentioned, Congress opens its check-book. When the Arctic, it yawns." Justly or unjustly, this cause was attributed to Senator Byrd, who stoutly defended his brother's expeditions to Antarctica.

Berkner lectured the Executive Committee on many occasions about the Antarctic. He heaped considerable praise on Admiral Bellingshausen of the Russian Navy, whom he felt history had ignored.

Berkner was a remarkable man. He discussed his thoughts about Antarctica becoming a "military free" zone and later pushed this law through the various international committees until it was accepted by most nations. He was also instrumental at the international level in fostering the IGY from a thought to reality. He cajoled many national delegates into expanding their Geophysical programs, and becoming more involved in IGY panels and details. Detlev Bonk, President of the National Academy, was asked to recommend that Berkner be nominated for the Nobel Peace Prize.

The author later inspected various Arctic observatories, including Trailer City on the ice island. At that time one end of the island had grounded on the ocean floor about 100 miles north

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of Barrow, and the other end moved up and down (like a hinge) with the tide. Before departing the island he went to the restroom (in its own unheated, waterless trailer). On opening the door to leave he saw a large sign, unobservable on entering: "If your penis freezes to the can do not get up. Yell!"

It may be worthwhile to mention several items which were raised but never incorporated into the U.S. IGY Program. The author had been asked by Helmut Landsberg to prepare a history of the International polar years for inclusion in Vol. 5 of "Advances in Geophysics". In examining the past material he was struck by the fact that a number of attempted submarine journeys beneath the Arctic Ocean had failed because the pack ice prevented surfacing to recharge the batteries. He noted this fact before the Executive Committee and suggested that with the advent of nuclear powered submarines a complete under-ice transit of the Arctic was possible. Hugh Odishaw, Executive Director of the USNC, brought this possibility to the attention of higher levels of government. He announced at a later meeting that when this possibility had been placed before the Navy, it was enthusiastically endorsed. This transit thus became a Navy, not an IGY, project.

In preparation for an international working group meeting in Stockholm seeking to integrate the various Arctic National programs, the author prepared several position papers for the U.S. delegation. None were adopted because of U.S.-USSR antagonisms.

One concerned freezing-in an icebreaker to serve as an Arctic scientific station. It could be strengthened and outfitted for experiments in a mid-latitude port, then steam to the Bering Straits where it would freeze in. It would be safer than the hazardous ice floe sites and would remain as a long-term scientific station. Rotation of personnel and equipment could be made peri-

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odically. Ultimately it would be discharged (after several rotations around the ocean) through the Greenland Sea. Coast Guard admirals, on hearing of the proposal, badgered the Executive constantly for support. However, Odishaw was adamant and would not raise it at either national or international levels.

A second proposal called for trans-arctic flights from such locations as Fairbanks to Murmansk, and Thule to the New Siberian Islands, respectively, to survey the pack ice status. Flights would be made alternately by Soviet and U.S. aircraft. This proposal also never surfaced.

DoD Symposium and Workshop on Arctic and Arctic-Related Environmental Sciences, 1987, Laurel, Md.

Arctic and Arctic-related environmental sciences...
proceedings and workshop report, ed. by Edward J. Harrison, Jr. *Read 7/15/88*
and Richard B. Gomez. Hampton, Va., A. Deepak Pub., 1988. *from Dr. Gerson*

RELUCTANT COMPANIONS — THE ARCTIC AND THE U.S. OR A NATIONAL ARCTIC EMPHASIS

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ABSTRACT

The objective of this note is to highlight a U.S. deficiency — lack of sustained interest in a vital geographic area. Past arctic investigations have been undertaken independently by various groups almost on an ad hoc short-term basis. The U.S. should implement an Arctic Institute responsive to national needs in fundamental and applied research. The region is too close, too important, and too vulnerable to be ignored by an adjacent great power.

1. Introduction

The objective of this note is to stimulate formulation of a strong sustained national program devoted to basic and applied studies of the Arctic. Past U.S. efforts have been relatively short lived and more designed to meet relatively local problems.

2 Background

Many reports from the 1950s to the present (e.g., from those issued by the U.S. National Committee for the IGY to those from the National Academy of Science in the 1980s) have bemoaned the relative inattention given to the Arctic by the U.S.

To a large extent the U.S. viewed arctic problems as Alaskan problems and thus of local concern. This narrow focus infused a limited approach which ignored the distinct peculiarities of the arctic continental, oceanic and insular regimes and their national implications— e.g., oceanic sovereignty, mineral exploration, or military significance. However, the Arctic remains a region of strategic, geopolitical, and scientific importance.

Even if environmental and exploration potentials were absent, the military factors alone warrant a more alert national awareness. They also suggest that DoD leadership is essential to the success of any integrated research program: many logistic irritations can only be overcome by direct DoD involvement. No other governmental department— Commerce, Science, Interior, Environment or

Transportation—has the comprehensive viewpoint, the need, or the facilities.

Unfortunately, the U.S. has no strong tradition of continued interaction with the Arctic. The successes are few and isolated: Peary's march to the Pole and the submerged transarctic transit of the Nautilus. There were failures (the ill-fated Lady Franklin Bay Expedition where many perished) and bursts of interest (relatively short-lived investigations in time, geographical area, and scientific discipline). Most were driven by immediate problems during and after WW2.

ONR established the Arctic Research Laboratory (now in limbo) at Pt. Barrow with primary emphasis on oceanography, biology and oil exploration. DOD and NSF stimulated studies at several universities in auroral-ionospheric-magnetic-particle physics, in meteorology, oceanography, and glacial conditions. Satellite and rocket measurements were undertaken in Alaska where some still continue. USA, concerned with cold weather effects on personnel, equipment and operations, established centers in the northern tier of states.

USAF manned the ice island for several years undertaking meteorological, oceanographic, sea ice, auroral, magnetic, and ionospheric observations. AFGL haltingly conducted limited ionospheric and propagation measurements at Thule, Greenland. DOD, NSF and NASA sponsored upper atmospheric investigations in Alaska. DOD examined the performance of communications and navigational systems. A geophysical aircraft sporadically probed the upper atmosphere. Isolated meteorological, oceanographic, and ionospheric tests were made from the pack ice or below. Probably the most integrated national effort occurred during and after the International Geophysical Year of 1957 - 58.

Military concerns helped open the Arctic. Dew Line stations, BMEWS sites and air bases were constructed in Alaska, Canada and Greenland. Scattered communications systems evolved to link these sites and the U.S. and Canada. The USNC for the IGY suggested a submarine passage from the Atlantic to the Pacific; it was implemented by USN and followed by increased oceanic exercises. Oil brought the first modern quest for a North American northwest passage, and an arctic pipeline of large size.

3. Soviet Activity

In contrast to the hesitant U.S. efforts, the Soviet Union consistently investigated and exploited the Arctic for decades. They established observational sites annually on the floating pack ice from the fall through winter and well into the summer. They discovered the Lomonosov range which divided the ocean floor into two basins. They established the farthest north geophysical observatory (82N, in Franz Josef Land) over ten years ago. Their Northern Sea Route Administration convoys over 100 ships each summer from the Pacific to the Atlantic along the Eurasian arctic coast. They detonated the only atmospheric nuclear device in the Arctic (on Novaya Zemlya). Their Polar Institutes (in Moscow and Murmansk, founded two decades ago) constantly conduct extensive investigations and experiments. It may be noted that Soviet activity in the Arctic and Antarctic exceeds that of all other nations combined.

The Soviet icebreaker fleet of about a dozen ships is the most modern and largest of any nation. Several vessels are nuclear powered and much more powerful than any in the U.S. inventory. They have sailed close to the North Pole in summer, and operate in both hemispheres. The U.S. is far behind and cannot match this performance. It possesses four icebreakers about 40 years old, usually assigned to antarctic waters.

This perseverance provides the Soviet Union with live hands-on experience (retooled annually) in arctic logistics, communications, navigation, and transportation. It provides a wealth of information from complementary scientific studies in most if not all areas of geophysics. Further, actual operations imposed an integrated practical command and communications structure among all military services, and between military and civilian groups. The U.S. has no comparable legacy, experience, or history.

Finally we should not forget that the bitter cold of winter has been a wartime ally of the Soviet Union. It defeated Napoleon on the Russian steppes, freezing men and horses alike. It treated Hitler's armor ingloriously. Batteries died, oil congealed, and grease solidified. Vehicles could not move, artillery could not fire, and men could not fight. It was at this time that the cold-trained Soviet Army began an offensive that initiated the German retreat.

4. Desiderata for an Arctic Program

A variety of past U.S. reports already indicate desirable studies (scientific and applied) to be pursued in the Arctic. Their findings will not be repeated here in detail;

Example, in oceanography objectives include models of sea ice, ocean temperature, salinity, compressional wave propagation, and sea bottom profiles, structure and mineral content. In meteorology, goals include ocean- or surface-air energy transport, wind systems, genesis and dissipation of arctic storms, upper air temperature and pressure profiles into the ionosphere, stratospheric warming events, ozonospheric changes, energy transfer upwards from the sea and downwards from higher strata, and global diffusion of pollutants.

With respect to the arctic ionosphere, more information is needed on its overall climatology, relation to magnetic storms, character of marked spatial changes, scintillation effects, total electron content, plasma convection in the polar cap, magnetotail and magnetosphere, and global electric current systems-- all as a function of space and time. In magnetism and particle physics, knowledge is required on the frequency and character of storminess, solar particle precipitation, solar plasma influences, solar wind - magnetospheric - ionospheric coupling, plasma flows in the magnetotail, and the detailed chemical and hydromagnetic interactions involved.

Fragmentary information is currently available on many of these topics. Too commonly, data taken at different sites and periods are merged in attempts to extrapolate future events in time and space. Even if this procedure is the only logical choice, the paucity of data precludes satisfactory results. In too many instances data required for constructing physical theories and models are lacking. Only a better observational base can provide the climatology needed for planning practical systems, or clarifying the important reactions involved.

5. An Arctic Institute

Many of the present deficiencies would be ameliorated by establishing a U.S. Arctic Institute charged with prosecuting and fostering a dedicated national arctic program. It would be open to visiting scientists, and would include a Polar Observatory located within the polar cap. It would study both basic and applied problems.

In today's age an automated representative network of advanced sensors would report on command or schedule to the Observatory. Further dissemination to active researchers elsewhere poses no problem. Power supplies (alkalal batteries, propane thermo electric generators, or nuclear packs) capable of one year operation with low drain sensors, computers, and transmitters are available.

The Observatory would serve as a local base for field expeditions elsewhere in the Arctic. It would house technicians servicing

experiments, and theoreticians analyzing the data. It would incorporate sufficient communications channels to allow on-line control of sensors from U.S. or Canadian laboratories. Thus comparisons among different data sets (auroral, magnetic, ionospheric, and particle) obtained from different locations could be rapidly conducted from midlatitude centers.

An optimum Observatory site would be on the northern islands of the Canadian archipelago. With other locations, logistic, administrative, and communications problems could proliferate unduly. A joint endeavor with Canada would optimize the total effort and reduce costs.

6. An Ocean Laboratory

Augmenting but not duplicating the fixed Observatory would be a Polar Laboratory aboard an icebreaker frozen-in in the Arctic Ocean. It would provide a base for year round oceanographic, sea ice, meteorological, auroral, ionospheric, and magnetic observations within the Polar Basin itself. Once frozen-in in the Chuckchi Sea north of Alaska it could make several counterclockwise transits of the Ocean, under the influence of wind and sea currents, prior to discharge between Greenland and Spitzbergen.

The advantage of an icebreaker over an ice floe station are considerable. The former could be outfitted in a warm water port. It would provide comfortable and less hazardous facilities for workers. Personnel could be rotated annually or semi-annually. Resupply would pose no serious problem. (A deliberately frozen-in research vessel has not occurred for about a century. One could be implemented today with far less trepidation and discomfort than it was then.)

7. Arctic Communications

Radio wave communications in polar regions are markedly influenced by the auroral oval and its temporal and spatial variability. The term oval implicitly defines four subregions; the trough, auroral D, the oval and the polar cap. Propagation from ULF to UHF is affected by these regions but in different fashions. UHF satellite links can be degraded by scintillation effects particularly during the peak of solar activity. ELF and ULF may suffer increased attenuation during magnetic storminess whose occurrence increases with solar activity.

HF circuits whose rays do not intersect active auroral regions may remain unaffected. Otherwise, as in long radio paths tangential to the oval, serious disruption by absorption usually occurs. VLF propagation into polar

the more intense D layer. In general, a long litany of abnormal problems characterize circuits encountering the oval, the trough, or auroral D. HF links within the polar cap can expect conditions somewhat worse than those for midlatitude paths, although long outages can occur during the peak years of the solar cycle.

Because the earth's magnetic and geomagnetic poles are located in the North American Arctic, problems on HF circuits arise more frequently on North American - European paths than elsewhere. During magnetic storminess the auroral oval expands equatorwards and engulfs the propagation path. Corresponding problems are much less frequent in the Asian sector which lies at lower geomagnetic latitudes. Both communications and surveillance systems are affected.

While the U.S. places more reliance on the newer technology of satellite communications, the Soviet Union straddles the new and the old. They employ HF to a greater extent than does the U.S. even in the Arctic. It cannot be convincingly argued that one approach is far superior to the other. Indeed, operational philosophies can be devised which permit comparable efficiencies for either method.

On the whole it may be presumed that the Soviet Union has better studied the effects of polar disturbances on frequency usage, ray deflection out of the great circle plane, absorption, scintillation, and avoidance of auroral penetrations. Further, considering their long experience with platforms in, under, and over the Arctic it also may be presumed that the Soviets have resolved propagation alternatives as well as the important practical matter of interoperable equipments among all services. The U.S. has yet to demonstrate that it has satisfactorily attacked these problems.

While the preceding comments apply to electromagnetic propagation, an associated topic includes compressional wave propagation within the Arctic Ocean. In this case the unusual (reversed compared to midlatitudes) temperature versus depth profile can produce subnormal propagation. More study is required of dispersion and attenuation within, and from external waters into, the Arctic Ocean.

8. Ionospheric - Magnetospheric Physics

Additional ionospheric observations over long time periods are necessary within auroral and polar cap regions. The data would allow development of first order physical models characterizing ionic transport among the magnetotail, polar cap, aurora, and solar wind as a function of time of day, year, cycle and magnetic index. Such a model (based upon the energy deposited on the planet during a particle influx) would assist in describing communications behavior from ELF through HF to

Since data obtained at any one location is insufficient, observations from various polar observatories would be combined when proscribing a suitable model.

9. HF Predictions

Ionospheric predictions are the basis for planning the establishment of HF circuits anywhere on the earth. Although current schemes of the U.S. and Western Europe are inadequate for polar regions they are employed as "better than nothing." They can be improved considerably by simple procedures: incorporating known factors on (a) the auroral oval and its associated regimes, and (b) past results from propagation experiments. Application of usual methods--now underway by some groups--may not allow advances within the foreseeable future.

The pro forma inclusion of magnetometers at each HF system site would provide an indication of the existing state of magnetic storminess, and allow inferences on the extent and duration of the accompanying propagation disturbance and circuit difficulties. Although not a predictive feature, it does allow local interpretation of the sources of the observed problems.

10. A Fiber Optics Cable

The attainment of reliable polar communications may be resolved into two portions: (a) advances in the practice and (b) advances in the science. The first represents the straightforward "plumber's approach; make it work irrespective of the difficulties. The second represents the academician's approach: seek generalized knowledge and, if possible, adaptively react with the variable ionosphere.

The latter requires a better understanding of the physics governing the polar ionosphere and its vagaries. It attempts to evolve a model which describes the observed events. Hopefully, the result may be suitable for practical purposes. This approach is fundamental to any area of physics, and must be pursued. However, it is beyond the scope of the present note.

The practical approach seeks to satisfy the objective; reliable large bandwidth circuits for high latitudes, whether polar cap, transauroral, or tangential-auroral. It involves technology (not necessarily science) and considers what is possible. It introduces the possibility of laying a fiber optics cable from Thule through Hudson's Bay to Canada and the U.S. This nonmetallic, wide bandwidth link completely bypasses almost all auroral and earth current effects on communications. It provides a highly reliable circuit with

The circuit offers the potential of extension to Europe, (i.e., Thule to the U.K.), to western North America (Thule to Pt. Barrow) or Asia (Pt. Barrow onwards). In this fashion it provides cable redundancy to a number of high density circuits and relieves the orbital crowding of satellite circuits. Costs incidentally of satellite or cable circuits on a per channel basis are comparable.

Implementation of these routes would knit further all coasts and the interior of North America, as well as North America-Europe. In addition Thule could serve as a node for "local HF or other service" within the polar cap. HF propagation is much more dependable within the polar cap than via the auroral zone. Present HF circuits from the polar cap to midlatitudes are notoriously unreliable as are all paths intersecting the active oval and its regimes.

The greatest difficulty inherent in implementing a fiber optics cable centers around its safe emplacement. The arctic coast contains many grounded ice blocks (nominal dimensions 10m x 10m x 5m). They scour the shore when forced landwards by wind or sea-ice pressure. This bulldozing action requires that the cable be trenched to depths of 5m - 10m for distances of 100m - 300 m landwards and seawards of the shoreline. With today's technology trenches of this type can be constructed.

11. Conclusions

Since its birth over two centuries ago, the U.S. has given scant attention to a region at its back door, a region of strategic, geopolitical and scientific importance. It is time to reverse the neglect, recognize U.S. interests, and initiate a dedicated long-term program which meets national needs. The suggestions which follow are given in this vein.

One approach would be the designation of DOD as the executive agent in charge. No other agency of government can adequately supply the logistics, assess the deficiencies, or apply the resources. Even today fitful approaches in no priority order creep into and out of the funding system. To enhance national efforts it is suggested that DOD:

- a. formulate a coordinated, dedicated arctic research program designed to meet DOD and national needs,

- b. establish an Arctic Institute devoted to polar problems and having under its control fixed and floating sites,

- c. establish an Arctic Observatory located within the Canadian archipelago, and

studies (oceanographic, meteorological, communications) within the polar basin.

In view of the great importance of communications for all human endeavors, the general unreliability of HF within the Arctic, and the overriding imperative of redundancy for critical circuits, a number of specific suggestions seem in order; viz:

a. institute annual joint exercises to ensure interoperability of all participating communications and surveillance equipment,

b. install magnetometers at all communications facilities to provide on line indications or magnetically stormy (and thus ionospherically difficult) periods,

c. emplace a fibre optics wideband cable from the U.S. to Thule via Hudson's Bay, with consideration for extensions to the U. K. and Alaska,

d. seek simple correlation indices between the degree of magnetic storminess and the degradation of HF communications on polar cap, transauroral, and tangential-auroral paths, and

e. implement a test link to an equatorial synchronous satellite from Thule. (Any slow tropospheric fading can be minimized by spatial diversity.)

Although they are inadequate, present HF prediction schemes available in the U.S. may be improved now by including information on when reflection points intersect the auroral oval and its associated regimes. The present headlong and hasty efforts seeking a "quick fix" for polar ionospheric predictions merely re churn barren soil. It is suggested that instead, efforts be made to:

a. incorporate the climatology of ionic transport as a function of time (diurnal, annual, and solar cycle) into an HF polar cap prediction technique. Some of the needed data is already available.

b. provide indications of when ray paths intersect the auroral oval, trough, auroral D and the polar cap

c. devise a climatological mode of expected scintillation as a function of time (diurnal, annual, and solar cycle) for auroral regions (i.e., the oval, trough, auroral D and polar cap).

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Military Physics — The Balloons

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Dr. Gerson worked for many years in collaborative U.S. — Canadian projects. He described many of these in his article in the January 1984 issue of *Physics in Canada*, "Collaboration in Geophysics — Canada and the U.S. 1948-1955".

INTRODUCTION

While balloons of childhood are decorous, delightful and even joyous, those of adulthood can be awkward, onerous and ornery. By the time that I joined the project, Headquarters had committed itself. Three balloon-borne antennas already had been deployed, one at each of the three transmitter sites: the old RCAF aerodromes at Gimli, Man., North Battleford, Sask., and Dawson Creek, B.C. All balloons followed the example of the young girl in Grimm's *Fairy Tales*. When they were good they were very very good, but when they were bad, they were horrid.

The transmitters operated in the Low Frequency portion of the radio wave spectrum. At these frequencies the usual difficulty of "radiating the energy from the wires" is overcome by utilizing large antennas on high towers with properly coupled circuits. High towers are costly; we used the rule of thumb that costs increase with the cube of the height. It was decided that at least in the early stages of the experiment towers were unwarranted. The primary objective still was a determination of whether LF Loran had utility as a long range electronic navigational system.

Although he had never launched one, Dr. K. had seen many meteorological balloons released. Also, he once had inveigled me (while I was a student) into undertaking the first tests for determining upper air winds using ground based radar and balloon borne reflectors. (The technique was so successful that it became standard practice.) On the basis of this remembrance he jokingly suggested to Col. H., "Well, we could always use balloons to support the antenna wire." The Colonel took the suggestion seriously.

According to theory a vertical radiator about 1300 ft high was desirable. The cost of a steel tower of this height would be about one Megadollar. In contrast a tethered balloon, with the tether being the antenna wire, could be about as effective (despite a few difficulties) and much cheaper. The problems did not seem insurmountable. Large balloons and a good supply of helium for inflation were needed. Projections for losses of balloons and helium during inclement weather were made on the back of an envelope and deemed acceptable. The wire type and size were determined from estimates of the stresses expected and the electrical properties required.

From these quickie calculations the number of balloons, amount of helium and length of wire were determined. As a safety factor the quantities were doubled. All supplies could be stored in the cavernous hangars at each transmitter site. "Simple," said the Colonel, "Good idea. I'll have someone track it down."

The type of balloon adopted was chosen using statistical mechanics and invoking Maxwell's Demon. The sergeant examining the availability and costs of suitable balloons blundered into the wrong Hail and Farewell ceremony. He knew no one but the beer was free and the food good. The affair was long and convivial and ultimately extended into the wee hours of the morning. While wandering about he overheard an Army Warrant Officer complaining about his Commanding Officer.

It seemed that the CO had inherited two warehouses full of WW2 barrage balloons and was badgering the WO to get rid of them. He had been a tank commander during the war and now wanted to store surplus WW2 tanks there instead. Emboldened by his fourth beer, our sergeant disregarded channels and interservice distinctions, and broke into the conversation. While both were still able, they swapped organizational addresses. They agreed to shift accountability to our Project in two months. "Not to soon," said our sergeant, "otherwise they'll give me more work."

The balloons arrived and were transported to our storage area. They appeared ideal for our purpose and provided several features not originally anticipated. Their fins allowed additional free lift (produced by kiting in the wind), and a smaller horizontal displacement from the tether point during windy periods. Both factors brought stability and reduced strains on the wire. They were larger than necessary (requiring more helium per filling) and while they placed a greater constant strain upon the wire they decreased the strain jitter. On the whole, however, they wandered less and so provided a more consistent antenna pattern and navigational coverage area. Because of their kiting action they were sometimes called kitoons.

The plan was implemented and the wire, helium bottles and balloons were stored in the hangars. Considerable helium was used for each filling. All supplies were freighted northwards from New Jersey by our C-47s. When needed the bulky material was unfolded on the hangar floor, inflated, and carefully brought outside to the antenna shack. There it was tethered to the antenna wire and slowly winched upwards.

Both ends of the wire were attached to large "Johnson" insulators. During darkness we were all reluctant to stroll near the tether point. Two GIs had been severely jolted one night when, on a dare, they laughingly touched the high voltage antenna. News of this incident spread throughout the system, and proved far more effective in catching attention than all the "High Voltage" posters and repeated verbal warnings combined.

We relearned Benjamin Franklin's dictum, "Experience keeps a dear school but fools will learn in no other." First, current knowledge of the climate of southwestern Canada proved inadequate. Statistics on gustiness (which we needed) were not readily available. To us, winds and turbulence could be disastrous. Many tethered balloons simply broke loose and disappeared into the cloudy grey yonder. The transmitter shut down when the helium supply was exhausted. During one period of continued losses, the possibility of shipping tank cars of helium to the sites was considered.

Our experiences exposed deficiencies in physics and syn-ergetics. We discovered a curious inversion of statistics unknown to theorists and meteorologists; e.g., the probability of malfunction increases with the dire necessity to function. Thus, in our case balloon and transmitter problems maximized under two conditions: (a) when navigational areas and accuracies were being measured aboard our B-29s flying over the arctic ocean, or (b) when I was measuring field intensities in the wilderness around Fairbanks, Norman Wells or Baker Lake. With no communications, we in the field spent hours dismantling and checking equipment or moving to new areas.

The balloons posed challenges of ingenuity and difficulty never appreciated by Headquarters. During high winds transfer of an inflated balloon demanded considerable agility and a disregard of personal injury on the part of the crew. Movement of the balloon to the antenna was virtually impossible during these conditions even with ten men hanging on. It pounced, flipped, banged the side of the hangar, attempted to stretch, and ultimately struck some unnoticed sharp object that caused a tear and deflation. A sudden gust of wind could strain muscles and tendons.

The original guidelines stated that when high winds were anticipated balloons should be lowered and secured. At first these procedures were initiated on the basis of the weather forecasts. However, the number of false alarms was too great so that later the reports were disregarded. Attempts to lower balloons during high winds invariably failed. They could not be handled. We lost the balloon and its helium.

We learned the hard way. The constant tugs on the wire ultimately caused breakage by metal fatigue, almost always at the ground end. The balloon then roamed the countryside with its dangling 1300 ft trailing wire. If this happened during high winds or in winter when temperatures were low, the vertical movement of the balloon was excruciatingly slow. The wire could drag on the ground or over housetops for considerable distances. When we knew that a balloon had broken loose no one wanted to answer the telephone.

The balloons also rose to the top of the Government. About a year after their introduction, a small civilian aircraft flying during daylight at a much lower altitude than allowed clipped the wire with its wing. The plane landed safely with no damage, but the pilot was furious. He rushed first to the media and then to the authorities claiming that had it struck the wire the propeller would have shattered or the fuselage would have been shorn in two. In either case the plane would have crashed and lives would have been lost.

The issue quickly became a cause celebre and escalated into a confrontation between Defence and Commerce. We at the technical level did not believe the dire comments of the pilot but were rather reluctant to suggest an experiment. Both RCAF and USAF planes had been flying near and around the balloons for months without incident. Further, balloon locations, altitudes and the fact that they supported antenna wires had been widely publicised in the official Notices to Pilots.

The dilemma was resolved at the ministerial level. The fact that the plane should not have been flying so low or so near the balloon, that the mishap occurred during daylight when the balloon was fully visible, or that the pilot should have known better, was disregarded. A lighting system was impractical because the free lift of the balloon, even though oversized, was not large enough. It could not accommodate lights and the antenna. The Conferees decreed that henceforth the balloons must be illuminated during the night and dusk.

The Canadian Army was ordered to implement the decision. It instinctively dusted off its Table of Manning and discovered the following. Three searchlight sets were necessary at each site; the first for operation, the second as a spare, and the

third as a backup. Each set included a searchlight and diesel-electric generator for power. Each shift required two operators and one diesel mechanic; three shifts required four times that number to allow for training and leave. This additional complement required billeting and meals. Two more cooks and the appropriate rations were added. Rooms were no problem.

The Army contingent duly arrived. They were neither engineers nor technicians. They were lusty farmboys; outgoing, earthy and rough. They introduced a new vocabulary into the mess hall and thence into the corridors. They frequented the local bars and quickly became their major source of revenue. Homesteaders began to place curfews on half their children. As one local wag stated, "For want of a tower the town was lost."

A minor problem occurred one day at Gimli during a high wind. The crew shut down the transmitter and tried vainly to lower the balloon. It tugged, bounced and lashed wildly. During some gusts the winch let loose and the balloon soared in a frenzy. Finally the wire at the base snapped. The free balloon galloped at the mercy of the elements across the countryside of Manitoba. It gained little or no altitude. The wire dragged across fields, barns, roads, and telephone wires.

Unfortunately, there was a high tension line only several kilometers from the transmitter. The suspended wire draped over this 23,000 volt line while its free end still straddled the local electrical supply system located on the ubiquitous poles. Havoc broke loose. The wire vaporized as phases and voltages mixed. Throughout every house and establishment large blue arcs immediately flared out of each duplex outlet. The internal contacts fused and an area on the wall four inches around each outlet became charred. Pandemonium erupted in the area. Incredibly there were no personal injuries or fires.

When news of this disaster clobbered Headquarters, Col. H. was overheard muttering about retirement. RCAF agreed to reimburse all damage costs. Every household in the vicinity, even those with no electric power whatsoever, submitted bills for repairs. All were paid. For some farms this payment allowed electricity to be introduced. The duplex outlets and fixtures in our buildings at Gimli were among the last to be replaced. The charred area around each outlet was not repainted for a year.

Balloon operations in cold regions caused us to relearn applied physics. One night at North Battleford, try as I might, the antenna meter indicated no radiated power. We exhausted all conceivable tests on the equipment and circuitry inside. I finally concluded that the balloon with its antenna must have broken loose. We turned off the power and I bundled up for the -32F temperature outdoors. Walking slowly over the crunchy snow I sought the outline of the balloon against the stars.

It could not be seen. Uncertain as to where it might be I cautiously approached the antenna shack. It was always possible that someone inside had thrown the switch; in that case I did not want to find out by touching the antenna. Walking with my eyes riveted on the heavens I still could not find the balloon. The night was clear and cold with a slight breeze. How could the balloon have broken loose?

When my chin touched a damp cold rubberized fabric I jumped back in fright. The balloon was lying on the ground like a large cow, the wire looped hither and yon. I had collided with it while peering at the heavens. The answer came quickly; the outside temperature was too low. The volume of helium within the balloon had decreased to such an extent that it no longer could support the weight of the antenna and the fabric. We quit for the night.

Despite their deficiencies and in spite of their problems, the

balloon borne antennas served their purpose. Even with the losses of helium and balloons, and lapses caused by resupply delays, they allowed the feasibility and practicality of the LF Loran system to be demonstrated at reasonable cost. Based on the results obtained from these tests, the 100 kHz Loran C navigational system subsequently was widely implemented in the Northern Hemisphere. Despite satellite positional systems, and the Omega hyperbolic system, it still serves as a mainstay for air and marine navigation.

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Nuclei Far from Stability: Observations from Rosseau Lake

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The production and examination of nuclides far from stability are the consuming passions of many nuclear physics laboratories around the world. Last September key players in the field gathered in Canada for an international conference at Rosseau Lake in the delightful Muskoka region of Ontario. This was the fifth in a series of conferences on "Nuclei Far From Stability" that began in 1966 at Lysekil, Sweden and has proceeded at about five-year intervals since: at Leysin, Switzerland (1970); Cargèse, France (1976); and Helsingør, Denmark (1981). This year's gathering, the first to be held outside Europe, brought together 180 physicists from 20 countries (90 from Europe; 50 from USA; 20 from Canada; 7 from Japan; 4 each from USSR and Israel; 2 from India and one each from China and Kuwait). There were 36 hours of lectures spread over six days interrupted by a boating and hiking excursion to Beausoleil Island in Georgian Bay Islands National Park. The meeting was organised by scientists from the Chalk River Nuclear Laboratories under the chairmanship of John Hardy.

There are three directions in which an explorer may venture in climbing out of the valley of stable nuclei. In light-mass systems he may ask: how many neutrons can be added to an isotope of given atomic number before the formed nucleus emits neutrons spontaneously. This limit is known as the neutron drip line. Likewise he might ask how many protons can be added to an isotone of given neutron number before the formed nucleus emits protons spontaneously — the proton drip line. In the standard chart of the nuclides in which the neutron number is the abscissa and the proton number the ordinate, these explorations take the venturer in easterly and northerly directions respectively. And if that alone were not enough the third and more glamorous direction of exploration is east-north-eastwards to the heaviest elements ever synthesized.

It is estimated there are 8,000 nuclides within the limit of the drip lines of which 2,000 have been identified so far. There is much uncharted territory. Conference summary speaker Peter Armbruster from the GSI laboratory in Darmstadt, West Germany noted that in recent times about 40 new nuclides have been identified every year. But he also envisaged a saturation point. He doubted whether more than 3,000 nuclides would ever be identified. The cross-sections for production become so small and the lifetimes so short to decay by α -emission, β -emission or spontaneous fission that their observation would be technically not feasible.

At the conference Dominique Guillemaud-Mueller of the GANIL laboratory in France reported that the relatively high beam intensities of heavy ions available and the performance of a doubly achromatic spectrometer have enabled a breakthrough to be achieved in reaching the neutron and proton

drip lines. On the neutron-rich side she showed data proving the existence of the new nuclei ^{22}C , ^{23}N , ^{29}Ne and ^{30}Ne and confirmed the theoretical expectation that ^{18}B , ^{21}C and ^{25}O would all be unstable. On the proton-rich side, three experiments with projectile beams of ^{40}Ca , ^{36}Ar and ^{58}Ni on natural nickel targets found that: (1) ^{23}Si , ^{27}S , ^{31}Ar and ^{35}Ca with $N-Z = -5$ are all "stable" to nucleon decay; (2) ^{22}Si , the first-ever identified nucleus with $N-Z = -6$, is also "stable" contrary to some theories while ^{19}Na and ^{21}Al are not; and (3) twelve new proton-rich nuclei ^{43}V , ^{44}Cr , $^{46,47}\text{Mn}$, ^{48}Fe , 50 , 51 , ^{52}Co , $^{51,52}\text{Ni}$ and $^{55,56}\text{Cu}$ are "stable" too. The GANIL work is a veritable tour de force.

Earlier this year, the Russian group from Dubna at an international school on physics in Varenna, Italy reported on experiments aimed at synthesizing the element with proton number $Z = 110$, which would be the heaviest element yet found. It was particularly surprising, therefore, when a few days before the conference was due to begin a telex was received by the organisers indicating the Dubna spokesman for the experiment would be unable to attend. After a further flurry of messages it was agreed that a preprint of the Russian work would be available to the conference. In it, Y.T. Oganessian et al. state: "By comparing experimental cross sections for producing the spontaneously fissioning nuclide, obtained in the $^{235,236}\text{U} + ^{40}\text{Ar}$ reactions, with the calculated ones and with those measured in the control experiments using the $^{232}\text{Th} + ^{40}\text{Ar}$ reaction we are inclined to assign the observed activity to the decay of a nucleus with $Z = 110$ ". There was considerable scepticism of this result, however, especially from the competing GSI laboratory. Spokesman Fritz Hessberger said that in two experiments, $^{208}\text{Pb} + ^{64}\text{Ni}$ and $^{235}\text{U} + ^{40}\text{Ar}$, they had seen no α -decay or spontaneous fission that could be attributed to the element 110. Their upper limit for production of element 110 in $^{235}\text{U} + ^{40}\text{Ar}$ is quoted as being less than eight picobarns. The Russians state "the nuclide has been produced with a cross section of about ten picobarns".

The GSI group announced two positive results. In 13 days of irradiating ^{209}Bi with ^{58}Fe one decay sequence was observed that could be attributed to the heavy nucleus, $^{266}109$, with a production cross section of 15 picobarn. And in a 14-day experiment, $^{208}\text{Pb} + ^{58}\text{Fe}$, three α -decay chains were seen that could be attributed to $^{265}108$, with a formation cross section of 19 picobarn.

Production of nuclei far from stability in itself is not the only interest of the conference delegates. Equally important and occupying much more of the conference time are measurements of their masses and decay properties. For any mass measurement, two main problems have to be solved: adequate production cross section and adequate mass resolution. W. Mittig reported that at the GANIL facility with a high resolution energy-loss spectrometer a mass resolution of 8×10^{-4} is obtained in measuring masses of neutron-rich isotopes of boron to phosphorus. From Los Alamos National Laboratory, Dave Vieira reported on experimental results from the time-of-flight isochronous spectrometer that had been built expressly for direct, systematic mass measurements of light-Z neutron-rich nuclei. Results were presented for nuclei from carbon to phosphorous and with the exception of $^{27,28}\text{Ne}$ there is generally good agreement between the two groups.

In heavier isotopes mass measurements far away from stability are generally based on the determination of decay Q values, as described for example by Erik Hagberg (Chalk River) and Fritz Münnich (Braunschweig). Even at their best, though, these methods suffer from uncertainties introduced by summing up the individual uncertainties of the many mass differences which link the mass of the isotope under investigation to that of a well known mass. Hence it is very desirable to determine the masses directly, independently of the knowl-

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