

# GENESIS OF THE INTERNATIONAL GEOPHYSICAL YEAR

## INTRODUCTION

The plan for a third International Polar Year, later broadened in scope and renamed the International Geophysical Year 1957-1958, originated on April 5, 1950, at a small dinner party of geophysicists at my home at 1105 Meurilee Lane, Silver Spring, Maryland. The basic concept was put forward by Lloyd V. Berkner. He and Sydney Chapman were principally responsible for developing and enlarging the concept to a persuasive level of detail and potential implementation, with the help of suggestions by others present: Ernest H. Vestine, J. Wallace Joyce, S. Fred Singer, my wife, Abigail, and myself. I will give a brief account of the context within which this meeting occurred and of the evening's discussion.

First, I will make a few remarks on the circumstances that led to this occasion, beginning with a quotation from an article by Sydney Chapman in *Nature*, August 22, 1953:

In 1882-83, many nations joined in a great international scientific enterprise, the International Polar Year, in which the geophysics of the polar regions—mainly the Arctic—was intensively studied, and expeditions set up polar meteorological, magnetic, and auroral stations and operated them for twelve or thirteen months. In 1932-33 the jubilee of this First Polar Year was celebrated by a repetition and extension of the enterprise, and ionospheric observations were included in the programme.

I am among the few persons here today who participated in the second International Polar Year (IPY) or, indeed, have any personal knowledge of it. My participation was minuscule but it made a deep and durable impression on me.

Following my freshman year at Iowa Wesleyan College, I spent the summer and autumn of 1932 helping Thomas C. Poulter, the professor of physics there, prepare geophysical equipment for the second Byrd Antarctic Expedition. This expedition was to have been

a part of IPY II, though it actually occurred about 2 years later, in 1934-1935. My jobs were testing reticles for the observation of meteor trails, a seismograph and a portable magnetometer of the Department of Terrestrial Magnetism (DTM) of the Carnegie Institution of Washington. This magnetometer was the most beautiful instrument that I have ever used. With the help of Daniel L. Hazard's *Directions of Magnetic Measurements*, a 1930 publication of the U.S. Coast and Geodetic Survey, I learned how to use the magnetometer and its associated theodolite and made a magnetic field survey of Henry County, Iowa. I then sent my observations to John A. Fleming, director of DTM, for inclusion in the national grid of 1932.

Seven years later, I went to DTM as a post-doctoral research associate. My own work there was in Merle Tuve's nuclear physics laboratory, but I also got acquainted with a wide spectrum of geophysicists on the staff: John Fleming, Scott Forbush, Alvin McNish, Harry Vestine, Bill Rooney, and others. In addition, the European geophysicists Sydney Chapman and Julius Bartels were occasional visitors to DTM during the course of the preparation of their great, two-volume treatise on *Geomagnetism*.

After World War II, I organized a research group at the Applied Physics Laboratory (APL) of Johns Hopkins University, using V-2 and Aerobee rockets for high-altitude scientific measurements. Harry Vestine, who had been a Ph.D. student of Chapman's at Oxford University, urged us to undertake a search for the equatorial electrojet that Chapman and he had inferred from ground-based magnetometer records. The basic idea was to fly a magnetometer directly through the electrojet, thought to be in the *E* layer of the ionosphere at an altitude of about 100 km, and to determine the altitude distribution of the density of electrical current from its magnetic signature.

The most promising instrument for this purpose was a three-axis fluxgate magnetometer of the type that was in use at the Naval Ordnance Laboratory (NOL). By arrangement with L. R. Maxwell and Leroy Alldredge, Elwood Maple and W. A. Bowen of NOL undertook the development of a suitable small unit and S. Fred Singer, L. W. Fraser, and I, of APL, adapted it for flight as part of the payload of an Aerobee rocket. The first proof flight of the system was made at the White Sands Proving Ground in New Mexico on April 13, 1948, to an altitude of 113 km above sea level. Good measurements were obtained throughout the flight but, as expected, the principal finding was that the field strength obeyed the inverse cube law.

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This article is taken from a speech prepared at the invitation of David Stern, chairman of the AGU Committee on the History of Geophysics, and delivered at the 1982 AGU Fall Meeting in San Francisco.

Dr. Van Allen, professor of physics and head of the Department of Physics and Astronomy at the University of Iowa, is the recipient of numerous research awards and honors. His interests include nuclear physics, ballistics, cosmic radiation, terrestrial magnetism, magnetospheric physics, solar-energetic particles, solar X rays, interplanetary medium, and planetary magnetospheres.

The next stage of the investigation consisted of three Aerobee flights near the magnetic equator off the coast of Peru from the USS *Norton Sound* in March 1949. This expedition provided the first U.S. flights of high-altitude sounding rockets at any location other than White Sands. Two of these flights, carrying NOL magnetometers and APL cosmic ray equipment, were successful, reaching altitudes of 105 km. One of the magnetometer records, the one taken at the proper local time, yielded a convincing signature of at least a partial penetration of the electrojet in the altitude range 93 to 105 km. The report of the results by Singer, Maple, and Bowen may be found in *Journal of Geophysical Research*, 56, 265 1951. We made further Aerobee flights of cosmic ray equipment in the Gulf of Alaska in January 1950.

## THE DINNER

Vestine was delighted with our equatorial electrojet results, as was Chapman who was visiting the United States in early April 1950. On April 5 they visited APL in order to learn about the results at first hand. Chapman expressed an interest in getting together with us and with Lloyd Berkner and Wally Joyce for further discussions. I immediately called my wife to confirm a previously tentative plan that she would have the group for dinner at our home. During the day she cleaned the house, prepared a splendid dinner, and managed to feed our two young daughters and tuck them into bed as the guests arrived.

The occasion turned out to be one of the most felicitous and inspiring that I have ever experienced. Berkner was one of the leading experts on ionospheric physics and telecommunications at that time, had been a member of the scientific staff of the first Byrd Antarctic Expedition in 1928-1930, and had extensive experience in international cooperation in science while a member of the U.S. State Department. Joyce was a distinguished geomagnetician who had published the well-known *Manual of Geophysical Prospecting with the Magnetometer* in 1937 and was, as I recall, on the staff of the National Research Council at this time.

The dinner conversation ranged widely over geophysics and especially geomagnetism and ionospheric physics. Following dinner, as we were all sipping brandy in the living room, Berkner turned to Chapman and said, "Sydney, don't you think that it is about time for another international polar year?" Chapman immediately embraced the suggestion, remarking that he had been thinking along the same lines himself. The conversation was then directed to the scope of the enterprise and to practical considerations of how to contact leading individuals in a wide range of international organizations in order to enlist their support. The year 1957-58, the 25th anniversary of the second polar year and one of anticipated maximum solar activity, was selected. By the close of the evening Chapman, Berkner, and Joyce had agreed on the strategy for proceeding.

The occasion is described in Chapman's words as follows, also from his 1953 *Nature* article:

On April 5, 1950, Dr. L. V. Berkner, at a small gathering of geophysicists at the then home of Prof. J. A. Van Allen, Silver Spring, Maryland, U.S.A., proposed a second repetition after twenty-five years instead of fifty years, in view of the rapid advances made since 1933 in scientific, especially ionospheric, techniques. Being favourably regarded, this proposal was formally brought before three international scientific bodies in the summer of 1950—the first to meet was the Mixed Commission on the Ionosphere (MCI), formed by the International Council of Scientific Unions (ICSU) under the sponsorship of the International Union for Scientific Radio (URSI) with the cooperation of the International Astronomical Union (IAU) and the International Union for Geodesy and Geophysics (UGGI). The Mixed Commission on the Ionosphere presented the proposal in some detail to the International Council of Scientific Unions, and recommended the formation of a committee to organize a third polar year in 1957-58.

The remainder of Chapman's paper gives eloquent testimony to the vigor and sagacity with which the matter was pursued.