Indian Ocean Proposed Drilling

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Tentative plans for the Ocean Drilling Project (ODP) are for the drilling vessel *SEDCO/BP 471* (*Eos*, March 13, 1984, p. 97) to work in the Indian Ocean during all or parts of 1987 and 1988. The Indian Ocean Advisory Panel of ODP solicits letters of intent or proposals for possible scientific ocean drilling during that period. All areas within the Indian Ocean and any important problems, including tectonics, nature of the lithosphere, paleoceanography, and sedimentary processes will be considered.

Please send proposals, with appropriate charts and copies of pertinent data, in triplicate to the Office of Joint Oceanographic Institutions Deep Earth Sampling (JOIDES Office, Rosenstiel School of Marine and Atmospheric Science, University of Miami, 4600 Rickenbacker Causeway, Miami, FL 33149) and, if possible, also send one copy to the chairman or to any other members of the panel. Proposals and letters received before September 1 will be reviewed at the panel meeting scheduled for the first week of September. Indian Ocean panel members are J. R. Curray (Chairman), Scripps Institution of Oceanography, La Jolla, CA 92093; J. R. Cochran, Lamont-Doherty Geological Observatory, Palisades, NY 10964; F. Gradstein, BIO-Geological survey of Canada, Dartmouth, NS B2Y 4A2, Canada; R. Herb, Univeristy of Bern, CH-3001 Bern, Switzerland; W. L. Prell, Brown University, Providence, RI 02912; R. Schlich, Université Louis Pasteur, 67084 Strasbourg Cedex, France; U. von Rad, Bundesanstalt für Geowissenschaften und Rohstoffe, D-3000 Hannover 51, Postfach 510153, West Germany; R. White, Cambridge University, Cambridge, England.

This news item was contributed by Joseph R. Curray, *Chairman of the Indian Ocean Advisory Panel.*

Warm Core Rings

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Gulf stream phenomena have been the focus of numerous studies by U.S. and Canadian oceanographic laboratories. Two years ago, observations of warm core rings associated with the Gulf Stream were reported in *The Oceanography Report*, (November 2, 1982, p. 834). It was noted then that the structure of warm core rings can undergo rapid transformation. Recently, a multidisciplinary group of physical and biological oceanographic institutions has examined the evolution of warm core rings in detail [*Nature, 308*, pp. 837–840, 1984]. The study has involved research vessels *Endeavor*, *Atlantis II*, and *Albatross IV* for surface measurements of temperature, salinity, and for measurement surface pigments to assess the concentration of marine plants. The results are that even though warm core rings are often very stable, undergoing only slow changes, it turns out that major alterations in structure can and do occur in short periods of 2–5 days.

Warm core rings are volumes of Sargasso Sea water that are formed by the separation of north-extending meanders of the Gulf Stream that have broken away from the main continuous velocity gragient. Warm core rings have diameters that can be as large as several hundred kilometers. They are always characterized by having a clockwise rotating rim, which is a vestige of the Gulf Stream itself. Thus, these rings are rotating bodies of Sargasso Sea mass that move in Continental Slope water between the Gulf Stream and the Continental Shelf.

The causes for rapid structural changes are due to interaction with other Gulf Stream meanders. The Gulf Stream absorbs the core and much of its energy in such an interaction. Barring this type of interaction, warm-core rings will not change rapidly.

This study involved observations of warm-core ring 81-D in the period September-October 1981. Ring 81-D was observed by surface ship measurement and by satellite observation; the results on the ring's surface temperature field correlated well. After interaction with the Gulf Stream meander, however, the shipboard measurements were insufficient to describe the changed properties of the ring compared with the satellite imagery.

Additional observations of the study included those of warm core ring 82-B, April-August 1982. Again, it was observed that ring evolution was strongly influenced by episodic interactions with the Gulf Stream.—*PMB*

Editor's Notes

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Since its debut in the September 1, 1981, issue of *Eos, The Oceanography Report*(TOR) has provided timely dissemination of information of general interest to the oceanographic community. Under Arnold Gordon's able editorship, 32 installments of TOR have been published with monthly regularity. A quick survey shows that 11 issues have dealt mainly with physical oceanography; nine with ocean policy or agency matters; three each with biological, chemical, and geological oceanography; and three issues were mainly concerned with new instrumental techniques. While it is not always practical or desirable to classify TOR's this way, there has clearly been a disparity among articles related to the various sub-fields of oceanography. This reflects the nature of articles submitted and not TOR's editorial policy, which is to provide an outlet for general and timely information of potential interest to all oceanographers. We continue to invite contributions to TOR in any of its five principal parts:

1. Article: A substantive essay on a topic of general current or historical interest, including overviews of multidisciplinary or multiinstitutional scientific projects and programs.
2. Information Report: A shorter description of support services and new technology available to oceanographers.
3. News and Announcements: Brief items of interest to oceanographers, such as agency reports, national political matters affecting ocean policy, new appointments, and editor's notes.
4. Letter of Opinion: Signed responses to articles or other items published in TOR, or concise comments which would be of general interest to oceanographers.
5. Meetings: Schedules and agenda of upcoming meetings and reports of meetings already held.

David A. Brooks
Oceanography Editor

AGU

1984 William Bowie Medal to Marcel Nicolet

Citation

PAGES 428–430

Professor Marcel Nicolet has been awarded the William Bowie Medal for 1984 because of his contribution to the elucidation of the chemistry of the earth's upper atmosphere and for acting as Secretary General of the Special Committee of the International Geophysical Year (1953–1960). Marcel Nicolet has made a greater contribution to our understanding of the chemistry of the earth's upper atmosphere than any other single individual alive or dead. Some of the contributions are (1) determination of photoionization and photodissociation coefficients; (2) showed the importance of diffusion in determining concentration profiles in the thermosphere; (3) elucidated the mechanism for the infrared airglow formation; (4) predicted the presence of the He layer; (5) explained the formation of the *D* region as due to NO ionization by Lyman-α radiation and cosmic rays; (6) predicted the presence of NO, NO_2, HNO_3, HO_2, and H_2O_2 in the upper atmosphere before they were measured; (7) was the first to recognize the importance of NO_x (NO, NO_2), and HO_x (HO, HO_2) in the upper atmosphere; (8) was the first to conclude that N_2O by its reaction with $O(^1D)$ is important in the stratosphere as a source of NO.

Perhaps the most remarkable thing about Nicolet is that he undertook the study of aeronomy to amuse himself during the Second World War, when the German occupation army would not let him practice his profession at the time, which was meteorology. Other aspects of his career have been pointed out by some of his colleagues. Professor David Bates wrote the following:

"I think some considerable stress must be put on his work (1953–1960) as Secretary General of the International Geophysical Year. Without his devotion and skills, his knowledge, and his sense of responsibility toward science, toward fellow scientists, and toward participating countries and organizations the IGY could not have been the outstanding success it was.

"Marcel Nicolet owes his every great achievement as a scientist partly to a remarkably retentive memory in which he has systematically stored a wide knowledge of aeronomy and related sciences; partly to being sensitive to early signs of conflicting evidence in an extremely complex subject. This combination enables him to focus his acute scientific insight on a problem at the rewarding early stage. Moreover, he has the tenacity to return to and pursue a problem if later developments show it is more tangled than originally supposed, as so often happens in aeronomy."

Dr. Alan Grobecker wrote:

"During the period 1971–1976, while I was director of the U.S. Department of Transportation's Climatic Impact Assessment Program (CIAP), Dr. Nicolet's work provided a principal basis for the realization of possible threats to the atmospheric ozone from injections of H_2O, NO_x, and ClO_x. He explained the complicated interrelationships of the chemistry, transport, and radiation characteristic of the stratosphere. His was a leading contribution to the coordination of the many contributions of more than a thousand scientists drawn from over nine nations. The coordinated contributions represented effort leading to the scientific understanding of the environmental consequences of pollution of the stratosphere by aircraft emission and by chlorofluoromethanes used in aerosol sprays and refrigerators. He was influential in organizing the international scientific community in independent but cooperative studies. These studies included those of the United Kingdom's Committee on the Measurement of the Effects of Stratospheric Aircraft (COMESA) and of the French Committee for Stratospheric Research (COVOS-Consequences des Vols Stratospheriques). Reports of these committees corroborated and augmented that of the U.S. National Academy of Sciences Climatic Impact Committee and were the first internationally recognized assessments of the consequences of stratospheric pollution by aircraft. Perhaps even more important than these reports was the development of stratospheric science by the accelerated attentions of the international community of atmospheric scientists, which included nationals of Australia, Canada, England, France, Germany, Japan, Norway, Switzerland, Sweden, United States, and USSR. I regard Dr. Marcel Niclet as the greatest single contributor to the study of the stratosphere and mesosphere."

Dr. Herbert Friedman wrote:

"The extent to which Professor Nicolet's work has led the field of the chemistry of the upper atmosphere is truly remarkable. Furthermore, in all his voluminous work, I cannot recall a single instance where his theory has failed to come extremely close to subsequent observations."

Because of his work, Dr. Nicolet has received the following scientific distinctions:

1. 1st Prize (95%), The University Examination of Belgium 1935–1937 for the group of the physical sciences.
2. Triannual prize of the foundation Agathon de Potter (1940–1942) of the Belgian Royal Academy of Sciences for the investigations in Solar Astrophysics.
3. Daniel and Florence Guggenheim prize of the International Academy of Astronautics for the discoveries in the field of Astronautics in the last 5 years (1963).
4. Hodgkins Medal Citation of the Smithsonian Institution (1965) for achievements in Aeronomy.
5. Member of the Royal Academy of Sciences and Arts of Belgium.
6. Foreign Associate of the U.S. Academy of Sciences.
7. Foreign Associate of the French Academy of Sciences.
8. Honarary member of the Royal Irish Academy.
9. Doctor honoris causa, Queen's University of Belfast.
10. Fellow of the Royal Astronomy Society.
11. Past Member and Chairman of the Science Group of the International Academy of Astronautics.
12. Corresponding Member of the Royal Society of Sciences of Liège.
13. Fellow of the American Geophysical Union.
14. Member of Chapter-at-large of Sigma Xi.

An objective indication of the scientific work of Marcel Nicolet is provided by the list of his publications indicating the wide range of reactions of molecules and atoms that have been understood as a result of his research.

In his first publications in astrophysics (1934–1938) for which he received the Belgian Academy Award in 1940, and also became a foreign member of the Royal Astronomical Society, M. Nicolet examined quantitatively and determined the molecular composition of the sun. He carried out a detailed analysis of the cometary molecular composition and was the first to show in 1938 that the rotational structure of the CH molecules was needed to understand the composition of comets. During the same period (1937–1940), Nicolet pioneered the study of atmospheric chemistry by investigating the physical and chemical processes involved in the atmospheric airglow and aurora. He made the first correct analysis of the identification of spectral lines in the aurora and airglow, and he correctly contended (with Bates) that the luminous layers of the atmosphere could not be at such great altitudes as was claimed by observers. His survey published in 1939 on the problem of atomic species in the upper atmosphere provided the fullest account available of the atmospheric structure resulting from chemical dissociation processes.

After World War II (1945–1950), M. Nicolet became an authority in atmospheric ion-chemistry after the publication of his famous work on the constitution of the ionosphere. A set of papers was published that has formed the basis for many subsequent studies of the ion-chemistry of the upper atmosphere of the earth. His proposal in 1945 of the existence of nitric oxide in the upper layers of the atmosphere was an important theoretical discovery of a minor constituent which plays a leading role in the ion-chemistry of the terrestrial atmosphere and also in the neutral chemistry of the stratosphere. This early discovery of nitric oxide was invaluable in connection with the development of atmospheric chemistry. Nicolet's work in atmospheric chemistry led him (with Bates, 1950) to carry through pioneering studies on the photochemistry of hydrogen-oxygen, as distinct from a pure oxygen atmosphere. The various reactions introduced in this first study and resulting from the specific actions of OH and HO_2 radicals are still, after 30 years, an essential element for the explanation of stratospheric and mesospheric chemistry. In addition, the explanation of the infrared emission in the atmospheric night glow by the reaction of atomic hydrogen with ozone leading to excited OH radicals was also a result of these pioneering studies.

Nicolet carried out the first quantitative investigation on atmospheric diffusion in photochemical and chemical equilibrium systems. He explained the upward transport of a heavy molecule, namely, molecular oxygen and the downward transport of atomic oxygen before its recombination at low altitudes. He predicted an abnormally large concentration of O_2 molecules toward high altitudes which was later confirmed in detail by in situ measurements using rockets. The work he and his associates did on the dissociation of various atmospheric molecules and the transport of their products was of the utmost importance in connection with the constitution of the terrestrial atmosphere at various altitudes.

Nicolet and several students under his immediate supervision have continually provided information essential for the analysis before or in parallel with the rocket and satellite observations. In fact, his theoretical work has led to numerous suggestions for balloon, rocket, and satellite observations, as, for example, he successfully predicted in 1961 the existence of an extended belt of helium in the high atmosphere which was later observed by rockets and satellites. In the 1960's, Nicolet's conclusions were not only confirmed as a result of the studies made with rockets and satellites, but also stimulated the study of the composition and constitution of the upper atmosphere as a function of the phase of the solar cycle. The observed structure of the upper atmosphere can now be understood with helium and hydrogen atoms surrounding the earth above the nitrogen and oxygen layers.

Nicolet was also involved in much of the early work on the photoionization processes in the atmosphere. He provided the first explanations (1945) of the various origins of the atmospheric ionized layers in relation to the ultraviolet solar radiation, which had not been observed at that time. He also provided (in a classic paper with Aikin, 1960) the explanation for the ionization in the mesosphere below 85 km. He showed that the simultaneous action of solar ultraviolet radiation (Lyma-alpha) on nitric oxide of solar X rays and of cosmic rays on molecular nitrogen and oxygen were involved in ion production. This work was selected as the most cited paper by the citations index for ionospheric research in the terrestrial atmosphere.

During the last 10 years, Nicolet has made important contributions to atmospheric chemistry by drawing attention on the various aspects of the interactions between the photodissociative action of solar radiation and the nature of chemical reactions. Particularly important was his work clarifying the problem of nitrogen oxides and hydrogen compounds in an oxygen atmosphere. He discovered in 1970 that the stratospheric production of NO is not caused by the photodissociation of nitrous oxide into nitric oxide and atomic nitrogen but by the reaction of the excited oxygen atom (resulting from ozone photodissociation) with nitrous oxide. During the course of his work he has developed and applied general methods by which he can determine the photodissociation parameters to be used in the quantitative analysis of the atmospheric chemistry.

A general survey (1980) of 530 pages entitled "Etude des rèactions chimiques de l'ozone dans la stratosphère," starts with the fullest historical account available on the growth of our knowledge on atmospheric chemical reactions, then contains a quantitative analysis of the various chemical reactions in the stratosphere and mesosphere and provides the full set of general equations that will be used in the future for the numerical study of the stratospheric and mesospheric chemistry.

Julian Heicklen

Acceptance

A few weeks ago, a telegram was delivered by hand to my home in Brussels. When I opened it, it was with genuine emotion that I read the message and also the congratulations addressed to me by our President, James Van Allen. For me, the award of the William Bowie Medal could only be regarded as a present arising out of the generosity of my friends. As I read the text of the telegram, my thoughts were carried 35 years backward in time to my first journey to the United States in January 1950. One of my lasting memories of that occasion was my first visit to Washington, D.C., and to Cambridge, Mass., and the kindness shown to me by Lloyd Berkner, Merle Tuve, and Harry Vestine at the Department of Terrestrial Magnetism, by Edward Hulburt and Richard Tousey at the Naval Research Laboratory, by Grote Reber at the National Bureau of Standards, by Harlow Shapley, Donald Menzel, and Fred Whipple at the Harvard Observatory,. . . .

During the 35 years that have passed by since that visit, I have crossed the Atlantic about 200 times and have made use of all kinds of transport: small slow ships rocked by the waves, and then the great ocean liners; later on, the early propellor-driven aircraft, buffetted by turbulence in the low troposphere, and finally Concorde, flying above Mach 2 in the calm of the stratosphere.

Every year since 1950, my wife, Alice, has accompained me on these transatlantic crossings, especially when my stay was expected to last for more than 1 week. Unfortunately, she was unable to come with me on this occasion, and she very much regrets that she can not be present here tonight.

It would take too long, alas, to recount all the enjoyable events associated with my many short visits and prolonged stays in the United States. On these occasions, I have traveled from east to west, from south to north, and I have even crossed the frontier into Canada several times. Nevertheless, I would like at least to say publicly here that the kindness shown to me and my wife by our hosts has been limitless and has been greatly appreciated.

I must recall especially that, in 1950, it was my good fortune to have been invited by Franklin Roach to come to Pasadena to work with David Bates on the airglow emissions and to have been there for 6 months with Sydney and Katherine Chapman. In Pasadena, I had the extraordinary opportunity of being able to talk to Millikan, Epstein, Gutenberg, Bowen, Minkowski, and many others at Caltech and at the Mount Wilson Observatory, and also to have discussions often with Joe Kaplan and Jack Bjerknes at UCLA. From Sydney Chapman and David Bates I learned how to make use of certain ingredients essential to scientific research, and in particular how to remain patient for more than a few minutes each day. In addition, Katherine Chapman taught me how to avoid being much of a skeptic.

It was at Pasadena, too, that, after long discussions with David Bates and myself, Sydney Chapman proposed the new nomenclature for the various layers of the atmosphere, which was designed to cover heights from the troposphere up to the exosphere, and which has since become classical and has been adopted for everyday use. Since I had expressed the reasonable opinion that the upper limit of the stratosphere should be 50 km, the temperature maximum corresponding to the stratopause, it became necessary to find a term suitable for application to the region between the stratosphere and the thermosphere. It was at this point that Katherine and Marcel recalled that they had both studied Greek at school and that the word *mesos* meant middle. It was in this way that the intermediate region between the stratosphere limited at 50 km and the thermosphere received the appelation "mesosphere."

While I was still in California in May 1950, it was in the context of my research work at Pasadena that I received an invitation to participate in a meeting of a few days in a "isolated camp" in the desert at Inyokern, near China Lake, Calif. The aim of this meeting was to discuss the problems of the upper atmosphere in the light of the then dawning space age. The Naval Research Laboratory had already modified various laboratory instruments for use in V-2 rockets, in order to make high-altitude measurements of atmospheric pressure, solar UV and X radiation, oxygen, and ozone absorption, and various ionospheric parameters. Those present at this meeting were J. A. Van Allen, W. N. Arnquist, E. V. Ashburn, D. R. Bates, L. V. Berkner, S. Chapman, C. T. Elvey, J. L. Greenstein, B. Gutenberg, J. Kaplan, A. B. Meinel, M. H. Nichols, M. Nicolet, M. O'Day, R. Penndorf, F. L. Roach, F. Rogers, M. A. Tuve, G. I. Weissler, and O. Wulf. It was at this meeting that I had the opportunity of meeting Jim, our President, for the first time. This was long before the appearance in 1958 of the well-known photograph of the triumphant trio associated with Explorer I: von Braun, Pickering, and Van Allen; but in April 1950, only a month earlier, it was at Jim's home in Silver Spring, Md., that the idea of the "future" International Geophysical Year (IGY) was launched.

In the course of various discussions with Lloyd Berkner and Sydney Chapman at Inyokern, I became "intoxicated" about the concept of the IGY. On the other hand, it was not until a year later, in November 1951, that I had my first introduction to the world of cosmic radiation. This was during a symposium in San Antonio, Tex., on the "Physics and Medicine of the Upper Atmosphere," the proceedings of which were published by the University of New Mexico Press in 1952. Just after I had spoken on "Solar Physics and the Atmosphere of the Earth," James Van Allen explained to us "The Nature and Intensity of the Cosmic Radiation," which he had actually been observing at high altitudes with the aid of rockets. It was in this way that I conceived the idea that information immediately available on cosmic radiation might provide a clue to the origin of the normal ionization in the lower *D* region.

But between May 1950 and November 1951, other events were taking place. First of all, in June 1950, I was invited to Columbus, Ohio (Cincinnati was to come later) to attend the annual Conference on Spectroscopy and to present some unpublished results on the photochemistry of water vapor which I had obtained with David Bates. It was a question of explaining the mechanism responsible for the prominent features of the night airglow spectrum in the infrared, which are due to the rotation-vibration spectrum of the OH molecule. The unknown origin of these excited molecules had just been discovered by David Bates and Marcel Nicolet (more by David than by Marcel); they resulted from a process, taking place in the mesosphere, in which two minor atmospheric constituents, atomic hydrogen and ozone, reacted together to produce an OH radical excited up to the actually observed vibrational level.

Just afterward, on leaving California, David Bates, Sydney Chapman, and Marcel Nicolet received an invitation from Art Waynick who, in 1949, was the founder and the first Director of the Ionospheric Research Laboratory at State College, Pennsylvania State University. We were to attend an international Conference on Ionospheric Physics in the last week of July 1950, at which about 250 participants were expected. Most of the papers were to be presented by visitors from other countries: D. R. Bates, H. G. Booker, S. Chapman, W. Dieminger, L. Harang, L. G. H. Huxley, D. F. Martyn, P. M. Millman, S. K. Mitra, M. Nicolet, J. Sayers, K. Weekes, and R. de V. R. Woolley. It was here that my second "intoxication" began; my wife and I were overwhelmed by the particularly friendly welcome we received from Art Waynick, whose idealism and desinterested attitude to research were very striking. It was following this meeting that Art Waynick invited me to become a permanent "Pennstate Ionosphere Lab. Resident," and I was honored when his successor, John Nisbet, renewed the invitation. More than 30 years of collaboration with the Ionosphere Research Laboratory can not be summarized in a few words, nor can they be forgotten. I would like to say simply that, notwithstanding the various surprises of crossing the Atlantic, these have been years of fruitful scientific research, thanks to the collaboration of my graduate students, also my research associates. I shall mention only the first of these, *ab uno disce omnes*: Phil Mange, who was and still remains a true scientist, and who is nevertheless willing, perhaps too often, to devote much of his time to the development of the science of others.

After returning to Belgium, full of enthusiasm following my travels and meetings, I was confronted in Brussels by the limitations of a small country. The plans that I had in mind for the future were far from receiving general approval, in spite of the support given by a few highly placed personalities. In fact, my various proposals were admittedly not yet orthodox, but the reasons for this require, perhaps, some further explanations.

Before preparing (1934) my doctorate on stellar spectroscopy and, in particular, on the spectrum of the sun, I had also been engaged (1935) on meteorological forecasting during 3 years before World War II. Moreover, I had spent some time (1938–1939) at the Lichtklimatisches Observatorium at Arosa, Switzerland; there, at an altitude of 1800 m, I had made observations of the UV spectrum of the night sky and had studied the problems of atmospheric ozone with Paul Götz and with G. M. W. Dobson, who was on a visit. It should be remembered that during the military occupation of Belgium from 1940 to 1944, active research in meteorology was forbidden. It was this fact that led me, during the War, toward a study of the relations between the sun and the earth, and first of all to an investigation of the solar radiation itself in all its aspects: the emission and the spectral distribution of the radiation from the solar disk, its transformation under the influence of the atmosphere at various levels, and, finally, its effects at ground level. These considerations resulted in the creation of a Department of Radiation at the Royal Meteorological Institute in Belgium. In the Library of the Institute, the discovery of papers published before 1940 aroused my interest in the ionosphere. In the light of all this very varied experience I gradually adopted the concept that there was a need for a common approach to the many different phenomena: an operation of synthesis which is very characteristic of geophysical research. This was my first real "intoxication." Indeed, for me, such a concept began to take concrete form just after the war in 1946–1948 when scientists in Europe were concerned about the reactivation of international activities which had been dormant since 1939. There was general agreement that a coordinated approach to problems was necessary after 6 years of closed frontiers.

In the summer of 1946, the International Union of Radio Science (URSI) held its General Assembly in Paris, and there I met Edward Appleton and others. In 1947, the Gassiot Committee of the Royal Society organized an international meeting in London on airglow and the aurora. Scientists who had worked in this field were invited to participate, and it was there that I met David Bates for the first time. About the same time, the Centre National de la Recherche Scientifique de France invited research workers to attend a conference in Lyon to discuss the relations between solar and terrestrial phenomena. In July 1948 the Scientific Unions convened a meeting of the Mixed Commission on the Ionosphere in Brussels in order to review the results of the past 6 years. For the first time in 10 years, the International Astronomical Union met in August 1948 in Zurich, Switzerland, just a week after the Assembly of the International Union of Geodesy and Geophysics (IUGG) in Oslo, Norway.

The series of international occasions, and especially the IUGG Assembly, enabled me to meet the leading scientists of the time, and to take part in discussions on many different topics. On the other hand, I had become convinced that numerous meetings and isolated discussions could not alone lead to a clear understanding of geophysical phenomena or, in particular, of those relating to the upper atmosphere. Solar physics had its own objectives, but the knowledge to be acquired must be applied also to the study of geophysical phenomena. Routine meteorological observations were essential, but meteorology must become part of the broader field of atmospheric research. The physics of the earth's interior was concerned with specialized investigations, but these needed to be linked to studies of the upper atmosphere.

On August 26, 1948, in Oslo, Sydney Chapman was chairman of an animated discussion in which I participated (in the role of "head of the rebels" to quote Chapman himself); others present were Arlette Vassy, Jean Coulomb, Edward Hulburt, Joe Kaplan, Frantizek Link, David Martyn, Merle Tuve, and Fred Whipple: all of us under the friendly regard of Carl Störmer and Leiv Vegard. The aim was to consider setting up a permanent organization within the Union for the study of upper atmospheric phenomena. What had started off the discussion was the fact that Edward Hulburt was to give a talk on "The Brightness and Polarization of the Daylight Sky" to the Association of Terrestrial Magnetism and Electricity, while, at the same time, Fred Whipple was talking about "Meteors" elsewhere in the Association of Meteorology. This discussion was the detonator that led in the end to the creation of the Association of Geomagnetism and Aeronomy at the IUGG Assembly in Rome in 1954.

The events described earlier provide the background to my arrival in the United States in 1950. Although I could not forget either my interest in astrophysics and the sun, or my responsibilities as a meteorologist, nevertheless my "intoxication" with the concept of the need for synthesis attracted me irresistibly toward the aeronomy of the future. But, although a European by upbringing, I was about to become intoxicated by American friendship.

Later, before my return to Europe, I was to be intoxicated for the third time, at Inyokern by Lloyd Berkner and Sydney Chapman (see above). The formal proposal for a third Polar Year was presented at a meeting of the Mixed Commission on the Ionosphere, at the Palace of the Academies in Brussels, held from September 4–6, 1950. The chairman of the Commission was Edward Appleton, who was also president of URSI at the time. After the acceptance of the proposal by the Scientific Unions, the International Council of Scientific Unions formed a small special committee of seven members. Three members of the Committee, Lloyd Berkner, Jean Coulomb, and Marcel Nicolet met in Brussels on October 15, 1952, and wrote to the Academies of Sciences and similar bodies inviting them to form their own national committees which would be responsible for the local organization of an International Geophysical Year. In the end, the Special Committee for the IGY was created with Sydney Chapman and Lloyd Berkner as president and vice-president, respectively; Jean Coulomb and Vladimir Beloussov were members of the Bureau and consequently Marcel Nicolet the secretary general. As a result, I became totally immersed in the organization of the IGY over an interval of 7 years which I have always regarded as the climacteric period of my scientific career. My responsibilities were to encourage participation in the IGY on a worldwide scale, even though the Korean War has been in progress since June 1950, and would only be halted by an armistice in July 1953. It was necessary also to ensure the collaboration of the leading scientists and specialists in the various disciplines. My contacts with the active scientific communities were made through the dozen or more discipline reporters; these were eminent scientists who had the task of planning the detailed aspects of the program in their respective fields. In spite of the enthusiasm associated with the international cooperation of scientists from 65 countries, it was not always possible to avoid difficulties and obstacles. Problems relating to the organizational structure engendered differences of opinion, for I had always advocated the avoidance of rigid administrative arrangements and the need to concentrate rather on the effective management of the scientific program. In general, the risks to be avoided and the problems encountered had political undertones. To quote one example, the question of the participation of the Peoples' Republic of China occupied the attention of Chapman and myself for several years, and severely restricted the time available for dealing with scientific matters. Although this problem may now seem unimportant for young scientists, its political aspects 25–30 years ago took up a considerable portion of the 10,000 hours or so that I devoted to the IGY. Perhaps this is why I have always felt somewhat dissatisfied with my activities as secretary general of the IGY Committee. In fact, I often regret the energy dissipated without the least advantage to science and the damage done on some occasions to the relations between individuals working for a common cause. However, in spite of everything I like to recall that, in the end, the IGY allowed me to make friends the world over: in all the continents and in all the countries that participated in the enterprise. If I still retain a certain feeling of bitterness about having devoted too much time to the organization of the IGY, especially during the years of my life when I should have been more active scientifically, this feeling has been wiped out by the distinction conferred on my by the American Geophysical Union through the award of the William Bowie Medal. Since the word "unselfish" in the citation is intended "to mean willingness on the part of the recipient to step out of his organized field of competence and work with men in other sciences to depend more or less on their knowledge, to sacrifice time and energy in meetings, and to correspond with fellow scientists, although this may not bring tangible rewards in the form of papers from his own pen" (*Eos*, p. 315, 1967), then I welcome this interpretation not only with great pleasure, something I often feel, but also with great satisfaction: a sentiment which is exceptional for me.

However, it would not be appropriate for me to end these reminiscences on a too personal a note. The legacy of the IGY has taken many different forms, and this international scientific enterprise became the origin of extraordinary developments in geophysics, such as can be seen in the AGU, for example. The present text has been written partly in my study at home, and partly in my two offices in the Royal Meteorological Institute and in the Institute for Space Aeronomy (a post-IGY creation). All three locations lie within a radius of 1 mile and, inside the circle, I am aware of the international group, created originally for the IGY, which have since become permanent national institutions.

Before concluding, may I refer to another very recent legacy of the IGY, namely, the idea of an international decade which is to be devoted to the coordinated study of all aspects of both the geosphere and the biosphere. In my opinion, it will be of the greatest importance to ensure that the data acquired during this new enterprise shall be as accurate and as reliable as possible. The accumulation of large volumes of observational data must not tempt research workers to publish statistical results that contain hidden systematic errors. At the persent time, although there is an abundance of observational data relating to space research, there is also evidence for changes in the sensitivity of some sensors. In consequence, unfortunately, it is often far from easy to draw reliable conclusions from the measurements available. It is my firm opinion that, in a mature branch of science such as geophysics, the need for cross calibrations ought to be accepted as a basic requirement designed to ensure that the theoretician and the observer can at least become close allies.

With this word of advice from an old friend, I must conclude; but let me express my profound gratitude to the members of the American Geophysical Union, to Julian Heicklen, Eugene Shoemaker, and the members of the William Bowie Award Committee, and to James Van Allen and all the other council members.

Marcel Nicolet