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THE *BULLETIN* INTERVIEWS: Professor Marcel Nicolet

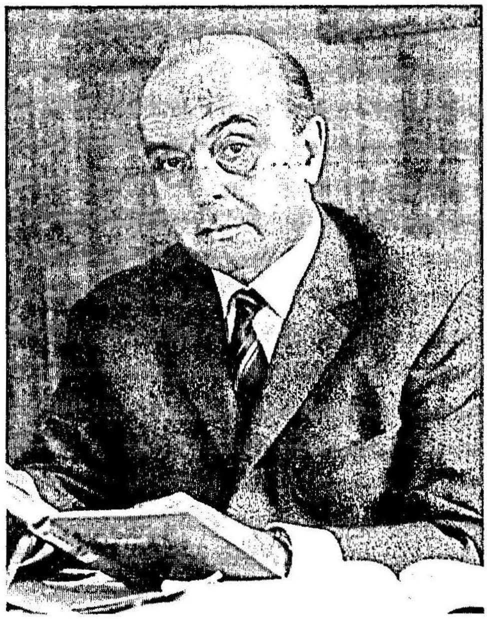
A glance through the list of his publications can leave little doubt that Professor Marcel Nicolet has made a pioneering contribution to our understanding of the chemistry of the Earth's atmosphere. His first works (1934-1938) were in the field of astrophysics when he was able to determine the molecular composition of the sun, and he was the first to show that a rotational structure of CH molecules was needed to understand the composition of comets. His imaginative studies revealed the physical and chemical processes in aurorae and airglow, of which he made the first correct spectral analysis. This work also led to an abiding interest in ozone.

He entered the Belgian Royal Meteorological Institute in 1935 and became a forecaster for aviation whilst still carrying on his investigations into the high atmosphere and beyond at the University of Liège.

During the Second World War when all weather forecasting operations were restricted to the occupying German forces, Nicolet took the opportunity to conduct theoretical research in aeronomy, the study of the physics and chemistry of the high atmosphere. After the war, he became an authority on atmospheric ion chemistry when he released for publication a classic paper entitled 'Contribution à l'étude de la structure de l'ionosphère'. *The same year—1945—he deduced the presence in the upper layers of the atmosphere of nitric oxide, important because this minor species plays a leading role in the ion chemistry and neutral chemistry of the Earth's atmosphere.*

Perhaps his greatest service to the international scientific community was as Secretary-General of the Scientific Committee for the International Geophysical Year (CSAGI) from 1953 to 1959. Here he needed much more than scientific competence as will be evident from his account of that exciting period.

His background in aeronomy was of great service to WMO in the mid-1970s when the depletion of the stratospheric ozone layer became a matter of public concern and the Organization decided to issue a statement.



Professor Marcel Nicolet

In more than five decades, during which we have seen the dawn of the space age, there is hardly any facet of aeronomy to which Professor Nicolet has not made an important contribution. He travelled widely and kept in close touch with his colleagues in other institutions, notably the Pennsylvania State University (he says he has crossed the Atlantic more than 200 times and still tries to go to the USA at least once a year). He has published more than 300 scientific papers, including in 1980 a 530-page review entitled 'Etude des réactions chimiques de l'ozone dans la stratosphère'. *This study presents the fullest available account of how our knowledge of chemical reactions in the atmosphere has advanced and contains a quantitative analysis of the various chemical reactions in the stratosphere and mesosphere as well as a set of equations that will be used in the future for the numerical studies.*

Professor Nicolet has numerous awards and distinctions to his name. These include the Guggenheim Prize of the International Academy of Astronautics in 1963, the Hodgkins Medal *of the Smithsonian Institution (1965), the William Bowie Medal of the American Geophysical Union (1984) and the Tsiolkowsky Medal of the Astronautical Society of the USSR. He is also member of the Royal Academy of Sciences and Arts of Belgium, foreign associate of the French and U.S. National Academies of Science and the Lincei National Academy of Italy, Fellow of the Royal Astronomical Society and the American Geophysical Union, and many more. The AGU has recently instituted the Nicolet Lecture in aeronomy which will be given by invited speakers at meetings of the Union. Such is Nicolet's modesty that Dr Taba had to approach Dr Marcel Ackerman, director of the Belgian Space Aeronomy Institute since 1985, to learn that by letters patent of 19 March 1987, His Majesty King Baudouin of Belgium had awarded Marcel Nicolet the hereditary title of Baron. Dr Ackerman chuckled as he recalled the secrecy with which the preliminary inquiries had been made, and Baron Nicolet's utter astonishment when he learnt officially that he was to be made a peer of the realm. He had to choose his own motto — Haute science, humble constance — (which we do not try to translate) and design his coat of arms with professional guidance.*

We are most grateful to Professor Nicolet for agreeing to give the following interview which took place at his home on 28 March 1989.

H.T. — **Professor Nicolet, thank you for inviting me here to conduct this interview. To begin with, please would you say something about the family into which you were born.**

M.N. — I was born on 26 February 1912 in the small village of Basse-Bodeux that lies about about 40 km SSE of Liège in the Belgian Ardennes. It was a pleasant mild day with the sky about half covered by stratocumulus and a moderate south-westerly wind. Solar activity was particularly subdued; there was no sunspot (indeed, none were observed in February 1912). In spite of these circumstances, my mother said that I was a nervous baby, a temperamental trait that has remained with me to this day. My father had a brewery and also made effervescent lemonade using the perfectly pure water from a local spring. There is a family tree that traces our ancestry in this region back to 1600, a line of master carpenters with the coat of arms showing a griffin and woodworker's tools such as mallet, chisel, saw, ladder and dividers. The addition of the name Bréda indicated that the first Nicolet had come from the region of Grenoble in the sixteenth century when there had been a substantial emigration of Nicolets to Geneva, Fribourg, Neuchâtel, Liège, Paris and Normandy. A Nicolet served as geographer with the explorer Jacques Cartier (1491-1551) who discovered the St Lawrence River, and a Jean Nicolet founded the town on the right bank between Montreal and Quebec that carries his name. Later he put Wisconsin on the map when he landed near Green Bay on Lake Michigan which was then a large Indian settlement.

H.T. — **Where did you have your early education?**

M.N. — When I was two-and-a-half years old I started at a nursery school where in due course I learnt to read, write and do simple arithmetic. It was during the First World War, and I have never forgotten the first words I wrote: *le roi, la loi, la liberté* (king, law and freedom). In October 1918 I went on to primary school. I was quite excited by history—to learn how Julius Caesar had come here more than 2000 years ago and made the remark *Fortissimi sunt Belgae* (the Belgae are the bravest—Belgae being the original Celtic inhabitants), to learn about the Franks and Emperor Clovis and later Charlemagne. But I was also interested in arithmetic and grammatical problems which my father helped me with in the evening. At the end of primary school I took the cantonal examination and got very good results so that it was suggested to my parents that I go straight to high school and study the humanities. Thus I spent the next six years learning ancient Greek, Latin, French literature, history and philosophy. I drew the conclusion that 'things are not what they are but what they seem', a reasoning that lives on today. At the age of 17 I had the culture of someone from a rural village outside the ambit of the intelligentsia. I now had to choose what field of study to pursue, and, contrary to everyone's expectation, I opted for mathematics because it was an excursion into the unknown.

H.T. — **So that was the subject you took at university?**

M.N. — I did two years of mathematics at the University of Liège and passed the *candidat* examination in 1932, but this had given me time to discover that my true vocation lay rather in physics. Therefore, by special dispensation I was permitted to sit for the *candidat* physics examination in 1933, and obtained my *license* (first degree) in physics the following year. I developed a particular interest in spectroscopy and photochemistry which I had studied under Professor Pol Swings and Professor Victor Henri respectively, and that was my reason for choosing astrophysics as subject for my research. A joint paper on B-stars with Swings in 1934 was published in the *Astrophysical Journal* of the USA and in the German *Zeitschrift für Astrophysik*. The classification of stars in decreasing value of effective temperature (ranging from above 10 000 K to about 3000 K) has the mnemonic: **O**, **Be A F**ine **G**irl, **K**iss **M**e **R**ight **N**ow, **S**ally. I was the first to detect argon in B stars and wrote a short note on this for the *Astrophysical Journal* in 1936. Even before the end of 1934 it had been agreed that I should study the molecular composition of cold stars for my Ph. D., and my father agreed to provide the necessary financial support. The sun is a G (yellow) star, and since its visible spectrum had been studied in detail I used it as the basis for my analysis. I found how the photosphere, chromosphere, corona, spots, faculae, flares and so forth make up a complex system that influences the atmospheres of the various planets.

H.T. — **What were the circumstances that led you to join the Royal Meteorological Institute?**

M.N. — Whilst I was working on stellar atmospheres, and solar-terrestrial interactions in particular, I decided to take a course in meteorology given at the University of Liège by Jules Jaumotte, then director of the Meteorological Institute at Uccle. Jaumotte was well known for his pioneering work in aerology and for having introduced to Belgium the concepts of the Norwegian school. He was gratified by the interest I showed by my questions during the course, and the last evening suggested that I come to work at the Meteorological Institute. I would have to learn the ropes as a weather forecaster at Brussels airport for three or four years, but after that he would get me to study solar radiation in all its various aspects.
The prospect of a fixed salary was very attractive because I was anxious to get married.
In September 1935 I contrived to combine working by night as an astrophysicist at Liège, as a weather forecaster at Brussels airport 160 km away by day, and meeting my fiancée at dusk. At the Meteorological Institute I made the acquaintance of Jacques Van Mieghem and Lucien Poncelet who later became respectively head of the aerology and climatology departments.

H.T. — **Was your work in meteorology limited to aviation forecasting?**

M.N. — Most of my time was spent in preparing forecasts for local flights in Belgium and for flights by Fokker, Junker and Savoia-Marchetti aircraft to places in the British Isles, continental Europe and North Africa. Their ceilings were below 4000 m. This gave me an opportunity to analyse changes in weather conditions at ground level (touching on such phenomena as fog and icy roads), in the boundary layer (clouds, precipitation, thunderstorms), and in the troposphere (synoptic features such as pressure patterns and fronts). At the same time I was trying to learn something about climatology in order to find out how it might be used for making medium-range forecasts. In spite of the demands this work made on my time, I managed to finish my main thesis entitled 'Study of the spectra and composition of stellar atmospheres' and a subsidiary one called 'Discussion on the thermal inversion observed in the stratosphere' and gained a D.Sc. in 1937.

H.T. — **I believe you did some work at Arosa in Switzerland at about this time?**

M.N. — I entered a national competition in physics—the *Concours universitaire*—with my work on stellar atmospheres, but also studying in depth the spectra of comets and discovering the presence of hydrogen in addition to carbon compounds in their atmospheres. On 19 January 1938 I learnt that I had won first prize which was a fellowship for nine months' study abroad, and, with the blessing of Swings and Jaumotte, decided to go to the *Lichtklimatisches Observatorium* at Arosa to study ozone problems as well as radiation climatology and the airglow spectrum with Paul Götz. I had been on a preliminary visit in March 1937, and the idea was that I should return with my bride after we had been married. However, in March 1938 there was the so-called union (*Anschluss*) of Germany with Austria, and the situation was such that as a member of the Meteorological Institute I had to stay in Belgium. With the signing of the Munich agreement in September, the crisis was believed to have lessened, and so the following month we travelled to Arosa and stayed for about eight months.

Dr G. W. B. Dobson was there, and with his and Götz's guidance I learnt how to make ozone measurements of all kinds. At the time of the new moon I made airglow observations using a small uv spectrograph. In addition, Götz, R. Penndorf and myself made synchronous measurements of uv-B at Chur (600 m above sea-level), Arosa (1850 m) and Weisshorn (2650 m); we found the maximum to be at Arosa, but the results were never published because the Second World War broke out. Before we left Arosa in May 1939 I went over to Davos to discuss solar radiation climatology with Dr Walter Mörikofer. Back at Uccle I made my first pyrheliometer and actinometer measurements of solar radiation.

H.T. — **You remained with the Royal Meteorological Institute during the war and undertook a great deal of research in solar physics, astrophysics and aeronomy. What were your main findings?**

M.N. — When the German forces overran Belgium in May 1940, the whole of the Institute was evacuated as the army's weather forecast service to a place on the coast near Dunkirk where we still had direct contact with the British and French Meteorological Services, but the situation soon became impossible. Jaumotte, who had been a pilot during the First World War and invented aerial photogrammetry for the Belgian army, was wounded and did not return to the Institute. With most of France quickly occupied by the German army, there was nothing to do but return to Uccle. There were less than a dozen staff members altogether, and only two scientists: Alphonse Van den Broeck (who had been head of the forecast office at the airport) and myself. Belgians were forbidden to do any synoptic meteorology or weather forecasting, so it was necessary to find something for everyone to do that was mostly based on research. At first we were not allowed to enter the Meteorological Institute headquarters, so, since I was the only one who knew the exact distribution of offices in the building, I was able to suggest a division of our contingent into four groups, namely (*a*) atmospheric electricity and magnetism (under Edmond Lahaye), (*b*) climatology (under Lucien Poncelet), (*c*) aerology (under Jacques Van Mieghem) and (*d*) radiation (under myself). We all worked under the direction of Van den Broeck, but of course a German official with a French-speaking deputy had been appointed to keep us under supervision. We decided to develop theoretical research in all fields of geophysics and plan new projects for after the war. I wrote a 138-page paper called *Introduction à l'étude des relations entre les phénomènes solaires et terrestres: Le Soleil* (Introduction to the study of terrestrial and solar relationships: The Sun) published in 1943. This naturally led me to want to study the effect of solar uv radiation on the ionosphere, and although we were in the dark as concerns work being done in other countries, with the help of some 40 pre-war publications I found in the library I elaborated a general theory of the ionosphere. However, I did not put it up for publication since the subject was top secret because of its relevance to short-wave radio propagation. It eventually appeared in 1945 under the title *Contribution à l'étude de la structure de l'ionosphère.*

H.T. — **What happened when you were liberated from the Axis domination?**

M.N. — The Allied forces reached Brussels in September 1944, and a meteorological station with radiosonde ascents was established in the radiation tower. The next six months were chaotic, but I managed to finish and publish my ionosphere paper as well as two others, one on ozone and its relationship with the atmospheric situation and the other on solar radiation and its biological action. On VE-day—signifying Victory in Europe (8 May 1945)— I was made *agrégé* at the University of Brussels.

H.T. — **Can you please explain a bit more about your discoveries relating to solar radiation and the ionosphere?**

M.N. — Before the first scientific rockets were launched in 1946, the solar ultraviolet emission was considered equivalent in the visible region of the spectrum to a black body at about 6000 K. However, such a radiation temperature was far too low to explain the presence of ionized layers reflecting radio waves at levels between 70 km and 300 km, with electron densities ranging from 10^3 to 10^6 electrons per cubic centimetre. But far-uv radiation emissions from the solar chromosphere and corona were equivalent to black bodies from 7000 K at 75 nm to 100 000 K at 5 nm in the x-ray region. I had considerable difficulty with the absorption cross-sections of atmospheric constituents like molecular nitrogen and molecular and atomic oxygen, concluding that nitrogen was not dissociated since emissions of the ionized molecule N_2^+ had been observed by Störmer in sunlit aurorae up to altitudes of more than 800 km.
A more presumptive hypothesis concerned the origin of the lowest ionospheric layer—the D-layer that reflects long waves— below 100 km; I was convinced that the uv radiation that ionized oxygen and nitrogen was completely absorbed above the 100 km level and so attributed the effect to a trace constituent absorbing radiation at a longer wavelength, more than 100 nm, in some atmospheric window. I suggested therefore that nitric oxide was ionized by hydrogen Lyman-α radiation at 121.6 nm going through the absorption bands of molecular oxygen. Ten years later this hypothesis was validated by rocket observations of the sun, laboratory measurements and *in situ* observations from rockets. It was D. R. Bates and M. J. Seaton who brought these results to international attention in 1950.*

H.T. — **In 1946 you were appointed head of the radiation division of the Royal Meteorological Institute and you became an authority on atmospheric ion chemistry. Would you care to say a few words about this period?**

M.N. — When the war finally came to an end, the four divisions we had created on an *ad hoc* basis in 1940 were formally established, but of course the synoptic meteorology and weather forecasting functions were also rehabilitated. With the help of about ten people — mathematicians, physicists and engineers in mechanics and electronics — we developed a solar radiation climatology with respect to extra-terrestrial irradiation, spectral radiation under a cloudless sky, direct and diffuse radiation at the ground, on vertical and inclined planes, problems associated with pyrheliometers and actinometers, parameterization methods and so forth. Thanks to the enthusiasm and diligence of Lucien Bossy, the Radiation Division made monthly forecasts of ionospheric conditions for short-wave broadcasting to and from the Belgian Congo (later to become the Republic of Zaire), and this, of course, entailed making observations of the ionosphere. I was present at the general assembly of the International Union of Radio Science (URSI) at Paris in 1946 where I made the acquaintance of Edward Appleton, Sydney Chapman, Jack Ratcliffe and many others who had been leaders in ionospheric research during the Second World War. The following year, I gave a course at the Meteorological Research Centre in Paris on basic actinometric problems in solar radiation. David Bates and Harrie Massey invited me to speak to the Gassiot Committee in London about my spectral analyses of the aurora and airglow and my deductions on conditions in the upper atmosphere, and I was also invited to a CNRS colloquium in Lyons to speak on solar-

* Bates & Seaton (1950): Theoretical considerations regarding the formation of the ionized layers. *Proc. of the Physical Society* B **63**, 2, 362B pp.129-140, 148.

terrestrial relationships. The year 1948 was even busier for me; I gave a course on problems in solar physics at the Astrophysical Institute and several lectures on atmospheric ozone at the Meteorological Research Centre in Paris.
The International Astronomical Union met in Zurich in August and IUGG held an assembly in Oslo the same month. It was notable that there was an overlapping of interests between the Association of Meteorology and the Association of Terrestrial Magnetism and Atmospheric Electricity. Sydney Chapman was the retiring president of the Association of Meteorology and president-elect of the Association of Terrestrial Magnetism and Atmospheric Electricity, and it was agreed with him to (temporarily) constitute a mixed commission, which eventually led to the creation of the International Association of Meteorology and Atmospheric Physics (IAMAP) and the International Association of Geomagnetism and Aeronomy (IAGA). But that only came about at the IUGG assembly in Rome in 1954 when Professor Ramanathan became president.
My activities corresponding to WMO's sphere of interest centred mostly on problems of measuring radiation — such things as observational procedures, standard scales, filters, turbidity and aperture conditions for actinometers. I served on the CIMO Working Group on Radiation and also on the Radiation Commission under IUGG.

H.T. — **In 1950 you went to work for six months in California. What were the circumstances of this visit?**

M.N. — In the Belgian Congo, several radiation stations identical to that at Uccle had been established, there was an ozone station and three ionospheric stations. I served on the three commissions (for astronomy, geophysics and meteorology) of the Institute for Scientific Research in Central Africa. It was planned to set up a new observatory to study radio-astronomy and provide a time service.
Dr George Van Biesbroeck, a former Belgian who had emigrated to the USA and was then working at the Yerkes Observatory of the University of Chicago, came to Belgium to discuss this. It was while he was with us that I received an invitation from Frank Roach to go to Pasadena to work on the interpretation of airglow observations that he and Chris Elvey had made at the McDonald Observatory in Texas. David Bates in Northern Ireland had a similar invitation, and so had Daniel Barbier in Paris. With the enthusiastic support of Van Biesbroeck, my wife and I decided to accept. There was a long and stormy January crossing of the Atlantic in a small boat, a week's break in Washington and then a three-day crossing of America by train.
I met many famous physicists and astrophysicists at Caltech, UCLA and the Mount Wilson Observatory: S. T. Epstein (molecular and turbulent diffusion), O. Wulf (atmospheric ozone), R. A. Millikan (cosmic rays), Joe Kaplan (auroral and airglow spectra) and Jack Bjerknes (ozone and atmospheric dynamics). Sydney Chapman and his wife Katherine had come from Oxford, and with David Bates we spontaneously made up a close-knit quintet. From David and Sydney I learnt to be patient, and Katherine, with her perfect knowledge of Greek and Latin, taught me to be less of a sceptic. After long discussions with David and me, Sydney put forward his proposed division of the atmosphere in layers from the troposphere to the exosphere that has now been generally adopted. When he sought a name for the intermediate zone between the stratosphere and the thermosphere, Katherine and I recalled our studies in Greek and suggested 'mesosphere'.

H.T. — **Were you all working on auroral and airglow spectra?**

M.N. — It was David and I who mainly concentrated on this. We worked on a theoretical determination of the altitude of the airglow green line of atomic oxygen and the yellow doublet of sodium. Thanks to help from the observations made by A. B. Meinel and R. Tousey, we were able to explain the presence of the strong OH radical red lines in airglow by the reaction of ozone with atomic hydrogen: $O_3 + H \rightarrow O_2 + OH^*$. Before we left the USA, Bates, Chapman and I received an invitation to attend an international conference on ionospheric physics at Pennsylvania State University. Our host was Art Waynick, head of the Ionospheric Research Laboratory, and we shall never forget the particularly friendly welcome he and his wife Lilian gave us. Before the meeting had ended Art had invited me to become a permanent 'Penn State Ionosphere Lab resident', a title I hold to this day through the goodness of his successor, John Nisbet.

H.T. — **In the WMO *Bulletin* for July 1982, you were kind enough to contribute an article on preparations for the International Geophysical Year (IGY), so I know that you were involved in this from the very beginning. Perhaps you would remind readers of the course of events?**

M.N. — In May 1950, whilst I was still working at Pasadena, we attended a meeting at an isolated spot called Inyokern, in the Californian desert near China Lake, to discuss problems of the upper atmosphere in the context of the dawning space age. Five years after the end of the Second World War, geophysicists working in this field had become very keen to expand their knowledge and test their theories against observations, and Lloyd Berkner, whom I had met when passing through Washington earlier in the year, voiced their wishes by proposing a similar campaign just 25 years after the Second International Polar Year (1932/33). This came up at the meeting at Penn State and then at the URSI/IAU/IUGG Joint Commission on the Ionosphere which met in Brussels under the chairmanship of Sir Edward Appleton in September 1950. The Commission noted that 1957/58 would correspond to a period of maximum solar activity and unanimously supported the proposal. The three ICSU bodies endorsed it, and WMO was invited to take part in developing the programmes. In May 1952, ICSU set up a Special Committee for an International Geophysical Year (CSAGI) (because, as Sydney Chapman pointed out in his capacity of president of IUGG, this time it would not be confined to the polar regions). The members of the Committee were L. V. Berkner and W. J. G. Beynon (representing URSI), J. Coulomb and V. Laursen (IUGG), N. E. Nørlund and myself (IAU), J. M. Wordie (IGU) and Jacques Van Mieghem who was subsequently co-opted to represent WMO. Berkner, Coulomb and I met in Brussels on 13 October to draft invitations to academies and similar scientific institutions, and the first full meeting of the CSAGI took place in Brussels in July 1953. Julius Bartels suggested using the initials of the French title since this could be punned to *comité qui agit* (committee that acts).

H.T. — **I have been told that you consider your role in the IGY as one of your most rewarding scientific activities. Is this true?**

M.N. — Thanks to the support I received from my country and from individuals too numerous to mention, I think I can justifiably look back on this period with considerable satisfaction. As is usually the case with such large undertakings, the real difficulties often lay behind the scenes. Since whoever was chairman of the CSAGI held the keys to the success of the IGY, there was strong pressure on Sydney Chapman to stand for election to this office, but because of an unfortunate remark made by someone, he was unwilling to do so. However, after a few months the joint arguments of Bartels, Coulomb and myself persuaded him to change his mind.
On 30 June 1953, Chapman was elected President of the CSAGI and I was elected Secretary-General. The ten years I worked with Sydney Chapman were richly rewarding; he was a truly great scientist and an utterly honest man.
Among those who helped me prepare for that important first session of the CSAGI was Phil Mange, who had come from Penn State after gaining his Ph.D. We had had positive responses from 30 academies, and 22 national committees had been created by the time the CSAGI met. Political sensitivities were still acute, but following the death of Stalin in March 1953 and the subsequent rise to power of Nikita Khruschev, a door silently opened to improved scientific communication and co-operation with the USSR. It was at its fourth session in October 1953 that the WMO Executive Committee formally decided to participate in the IGY, and the CSAGI Bureau met in Washington the following month to explore the new possibilities for collaboration and to discuss practical and technical aspects of planning.

H.T. — **I believe that you were responsible for designing the IGY emblem?**

M.N. — After several ideas had been studied and rejected, Berkner and Chapman readily agreed to my design of an emblem that highlighted an orbiting artificial satellite, the terminator (between solar radiation and the night sky) and the Antarctic. Of course, no spacecraft were as yet in orbit, but the President of the Academy of Sciences of the USSR had hinted in December 1953 that such an achievement was within reach.

H.T. — **Please enlarge upon the preparations for the IGY.**

M.N. — I have already mentioned the IUGG general assembly in Rome in September 1954 when Professor Ramanathan succeeded Chapman as president. A delegation from the USSR came; the first time they had been present at an ICSU gathering. An unofficial meeting was arranged on 25 September between Chapman, Berkner and me with a delegation of Soviet physicists: V. V. Beloussov,

Rome, September 1954 — The second plenary meeting of the CSAGI. *Left to right*: M. Nicolet, S. Chapman, L. Berkner and J. Van Mieghem

A. G. Kalashnikov, A. S. Monin and M. B. Cormoung (who was acting as interpreter from and into Russian). The difficult task of interpreting for Cormoung from and into English (because he only knew French) fell to me. It was difficult for Lloyd Berkner too because his countrymen did not unreservedly welcome participation by the Soviet Union. However, Chapman could always be relied upon to find a way of smoothing over difficulties. The second plenary meeting of the CSAGI had been arranged in conjunction with the IUGG assembly, and the end result was that the USSR Academy of Sciences was represented at the final session of the CSAGI assembly. A working group under the chairmanship of Jean Coulomb was able to identify the major gaps in station networks for various disciplines and draft detailed recommendations. As predicted at the closing ceremony by the meteorologist, T. E. W. Schumann, the meeting was indeed of great historical significance.

H.T. — **What was the situation concerning satellite observations for the IGY?**

M.N. — On 25 July 1955 a special messenger brought me a letter from Joe Kaplan, chairman of the USA's national IGY committee, announcing his country's intention to launch a scientific satellite as part of the IGY programme.
I released this important piece of news in the Brussels *Palais des académies* at 18.40, a few minutes before the official announcement was made from the White House. As I already knew, at the inaugural session of the fourth assembly of the CSAGI at Barcelona in September 1956, Academician Bardin, president of the USSR's national IGY committee, had announced his country's intention to launch a satellite to make observations of atmospheric pressure and temperature, cosmic rays, micrometeorites, the Earth's magnetic field and solar radiation.
I am sure your readers will have read that the Soviet Union put *Sputnik–1* in orbit on 4 October 1957, about three months ahead of the USA's *Explorer–1*.

H.T. — **I understand there were some problems before the IGY programme finally got under way?**

M.N. — There were indeed problems on all sides, and even a crisis within the Bureau of the CSAGI. At my request, Serge Korff, a cosmic ray physicist at New York University, visited many Latin American countries to assess the situation and analyse the possibilities of scientific participation. Then there were the problems of overlapping claims for parts of the Antarctic by Argentina, Chile and the United Kingdom. Also the People's Republic of China had shown interest in participating in the CSAGI, and a complicated arrangement was made for the delegation to the fourth assembly at Barcelona to have entry visas to Spain (virtually no western European country at that time had diplomatic relations with the People's Republic). Naturally the scientists were delighted at the prospect of such a vast country taking part. Unfortunately, however, the attitude of Taiwan, which had political backing from the major powers, resulted in the People's Republic of China formally withdrawing, in spite of Sydney Chapman's valiant efforts at mediation. All was not quite lost because the Chinese scientists kept in touch with the USSR's national IGY committee and programmes were carried out. As for the CSAGI, Lloyd Berkner had now become president of ICSU and tended to take decisions over the heads of the other members of the CSAGI Bureau. This was apparent to the membership in general, and voices were raised at Barcelona demanding Berkner's replacement as vice-chairman and member of the Bureau. I was opposed to such a move and tendered my resignation. The outcome was that the IGY Advisory Committee (which comprised members of each of the national committees) held a special session at which it unanimously adopted resolutions expressing confidence in the Bureau and in myself as Secretary-General, a position later endorsed by the CSAGI. So, at the beginning of 1957 the membership of the Bureau was Chapman (chairman), Berkner (vice-chairman), Nicolet (Secretary-General), Coulomb and Beloussov, and we were able to pick up the reins again just in time for the start of the IGY.

H.T. — **Which disciplines were programmed for the IGY?**

M.N. — Fourteen disciplines were formally adopted by the CSAGI, a rapporteur being designated for each. The subjects were: (*i*) World days (A. H. Shapley); (*ii*) Meteorology (J. Van Mieghem); (*iii*) Geomagnetism (V. Laursen); (*iv*) Aurora and airglow (S. Chapman, F. Roach and C. Elvey); (*v*) Ionosphere (W. J. G. Beynon); (*vi*) Solar activity (Y. Ohman); (*vii*) Cosmic rays (J. A. Simpson); (*viii*) Longitudes and latitudes (A. Danjon); (*ix*) Glaciology (J. M. Wordie); (*x*) Oceanography (G. Laclavère); (*xi*) Rockets and satellites (L. V. Berkner); (*xii*) Seismology (V. V. Beloussov); (*xiii*) Gravimetry (P. Lejay); (*xiv*) Nuclear radiation (M. Nicolet). The *Annals of the International Geophysical Year* have been published by Pergamon Press in 48 volumes (actually 55 separate books because some volumes are subdivided). Volume 48 appeared in 1970.

H.T. — **You relinquished your post of Secretary-General of the CSAGI in 1959.**

M.N. — The last assembly of the CSAGI was hosted by the Soviet Union in Moscow during the summer of 1958. More than 500 people were present, WMO being represented by its Secretary-General, D. A. Davies. But Lloyd Berkner was not there; he had resigned from the CSAGI, a victim of the conflict between politics and science that still characterized the world situation at that time. I decided I had neglected my scientific work for too long and, as you say, resigned as from 31 October 1959. The IGY had taught me how to remain a simple scientist in spite of the constant politicizing of

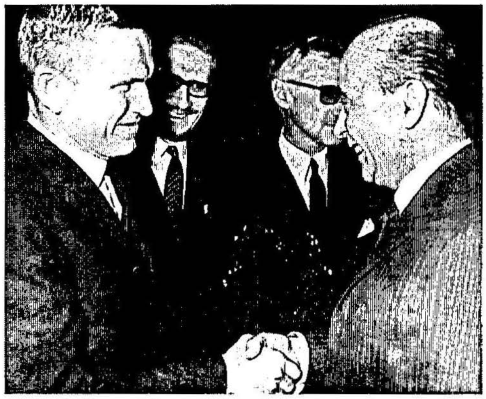
Brussels, April 1987 — The former members of the IGY secretariat were reunited in the Palais des Congrès on the occasion of a symposium on comets held in Professor Nicolet's honour. *Left to right*: 'Mike' Baker, Paulette Doyen, Professor Nicolet, Mrs Nicolet, Maurice Hautfenne, Delphine Jehoulet, Jean Palange and Phil Mange scientific problems, and I cannot have been alone in that because thousands of people from 67 nations had also succeeded in avoiding or softening the rigid administrative strictures that too often accompany international scientific relationships.

H.T. — **Everyone who has visited the Belgian Space Aeronomy Institute agrees that it was planned by a perfectionist. What can you tell us about it?**

M.N. — Before Jacques Van Mieghem became Director of the Royal Meteorological Institute in 1962, it had been decided to develop meteorology up to stratospheric levels and simultaneously to pursue space research. From this had stemmed the Belgian National Space Research Centre. I was head of the Space Aeronomy Study and Research Group in the Meteorological Institute. When I came back from a visit to Pennsylvania State University late in 1964, I learnt that there was now going to be a Belgian Institute of Space Aeronomy and that I was its director. I already had experience in building additional laboratories and the radiation tower for the Meteorological Institute, so it was not too difficult to plan the new buildings and infrastructure: offices, library, printing unit, computer, electronic and spectroscopic laboratories, workshop and so forth. There are four departments: mathematical, theoretical, experimental and applied aeronomy, and sections to deal with numerical analysis, fundamental dynamics, atmospheric and interplanetary physics and chemistry, ionospheric physics, photochemistry, optics and instruments. I was given a staff of 60 instead of the 75 I had proposed.

H.T. — **Notwithstanding your heavy responsibilities with the Institute, you also lectured in geophysics and space physics at the Universities of Brussels and Liège.**

M.N. — These courses were for graduate students and developed according to their common interests, and lecturing kept me up to date in a broad field of research without being too demanding. For instance, at Brussels students might have graduated in mathematics, physics, geology or geography so that I needed to adapt my lectures suitably and



Professor Nicolet meets the American astronaut Frank Borman who was on the first manned space mission to circumnavigate the moon

follow all recent developments related to atmospheric research even when they were outside my own field.

H.T. — **You are widely known as an authority on atmospheric ozone. When did you develop that particular interest?**

M.N. — When I went to Arosa just before the Second World War, I was lucky enough to be with Paul Götz and G. M. B. Dobson who were at the forefront of ozone research. You will remember that at the time my profession was meteorology; through my astrophysical research I was interested in spectroscopy, and from my university studies I was interested in photochemistry, thus with so many unsolved problems in the domain of atmospheric ozone and the expert guidance of Götz it was not surprising that for me this became a challenging field. To mark the centenary of Dobson's birth I wrote an article entitled 'The atmospheric chemistry of ozone' that was published in *Planetary Space Science* in December 1989. It was a tremendous pleasure for me when the International Ozone Commission cited me as 'one of the few scientists who had continuously made major contributions to the study of ozone during the last three decades' on the occasion of the thirtieth anniversary of the IGY.

H.T. — **Perhaps you would care to comment on the threat to the ozone layer?**

M.N. — There is no doubt that human activities can affect the total amount and vertical distribution of ozone in the atmosphere. Increased CO_2 may lower the temperature near the stratopause through infra-red emission and this would lead to an increase of ozone in its region of photochemical equilibrium, but the presence of various other minor species may bring about different chemical reactions. Methane (CH_4) originates chiefly from marshes, paddy fields and ruminants, and is increasing at a rate of between 400 and 800 Mt per annum, meaning that an atmospheric load of 4 Gt might be reached in less than a decade. Since CH_4 is transformed into H_2O through a chemical reaction associated with solar uv radiation, the stratospheric water vapour content could be strongly disturbed at certain latitudes. Chlorofluorocarbons (CFCs) that reach the stratosphere can be dissociated photolytically by solar uv to liberate chlorine atoms, and a parallel reaction occurs with bromine compounds. Both these atoms are catalytic agents in destroying ozone. The problem of NO_x effectively boils down to nitrous oxide (N_2O), of which between 10 and 15 Mt (one per cent of the current total) is released each year. There are still large gaps in observations and understanding which means that it is impossible to give quantitatively precise values to the various species, sources, sinks and processes and the impact to be expected on human health and the environment. But in spite of these uncertainties, as scientists we know enough about the potential risks to affirm that world-wide action should now be taken to diminish the known sources of contaminants. In addition, more precise observations and more critical theoretical research work are needed which require strong and continuous international support.

H.T. — **Among the many awards and honours you have received, do some have a particular meaning for you?**

M.N. — In a way they are all souvenirs that have accumulated over more than half a century, and so each has a special meaning. One particular item that I could mention is the rocking chair in our sitting room that was specially made for the Pennsylvania State University and given to me to mark thirty years' association with Penn State. It came through official diplomatic channels from Washington, DC in 1980, the one hundred and fiftieth anniversary of the independence of Belgium.

H.T. — **Apart from the IGY, what other contacts have you had with WMO?**

M.N. — My first association with IMO or WMO was in 1951 when I had to organize the joint meeting at Uccle of the CIMO Working Group on Radiation and the Radiation Commission of the then IUGG Association of Meteorology. As Secretary of the Radiation Commission I was in regular contact with WMO, working in particular with O. M. Ashford and K. Langlo. I was invited to WMO in 1963 to discuss various aspects of the use of meteorological satellites. Later, I was present at several meetings on atmospheric chemistry related to the ozone problem. An important statement was drawn up by the CAS Working Group on Stratospheric and Mesospheric Problems in September 1975 entitled 'WMO statement on modification of the ozone layer due to man's activities and some possible geophysical consequences' and released with the approval of the Executive Committee soon after. I have also been to WMO meetings as ICSU representative. My last meeting in Geneva was in February 1990 and again concerned atmospheric ozone.

H.T. — **In the light of your experience, what advice would you give to a young person who is contemplating a career in atmospheric science?**

M.N. — In his or her study of physics, the person should try to understand the causes and consequences of phenomena, to place mathematical problems in their exact context and appraise the difficulties of experimental determinations. Meteorology must be one of the most complex disciplines since all its parameters are governed by astronomical, astrophysical, geophysical or geographical factors. There are the latitudinal zones with their different characteristics and the irregular distribution of land-masses and oceans. The person's aspiration should be to acquire a composite knowledge of the properties and dynamics of the atmosphere in its various layers and on various scales. Anyone using flat maps of large portions of the Earth needs to have a lot of imagination.

H.T. — **How do you spend most of your time these days?**

M.N. — I still work on scientific matters but I am no longer subject to the constraints of administration. I have resigned from all the Belgian Academy of Sciences committees of which I was a member, and I am trying to develop research subjects that I was involved in after 1970 with Ackerman (current director of the Space Aeronomy Institute), Biaumé, Brasseur, Cieslik, Peetermans, Vergison and others. Quite recently six papers that I wrote with S. Cieslik and R. Kennes on problems of molecular oxygen dissociation were published in *Planetary and Space Science*. But I also re-read old publications. For instance,as long ago as 1963 Dobson had observed that 'the south polar stratosphere is cut off from the general world-wide circulation of air by the very intense vortex of strong westerly winds that blow around the Antarctic continent, enclosing very cold air which is rather weak in ozone, . . . This vortex suddenly breaks down in November.' His constatation has now assumed much significance in connection with current theories on the meteorological factors that are associated with the springtime stratospheric ozone depletion over Antarctica. Incidentally, when I was looking back over the total ozone measurements we made at Arosa in 1938 and 1939, I found that these were years of below-normal total ozone, minima being around 260 Dobson units, much the same as the minima observed in the years 1980-1982. And yet I read in the *New Scientist* for December 1988 that 1938/39 corresponded to an ozone *maximum* over Arosa! In fact it was in 1940 that an exceptional maximum occurred.

H.T. — **Professor Nicolet, thank you most sincerely for such a stimulating account of your long and interesting career. I hope you will enjoy many more years of health and strength to pursue your various fruitful activities.**