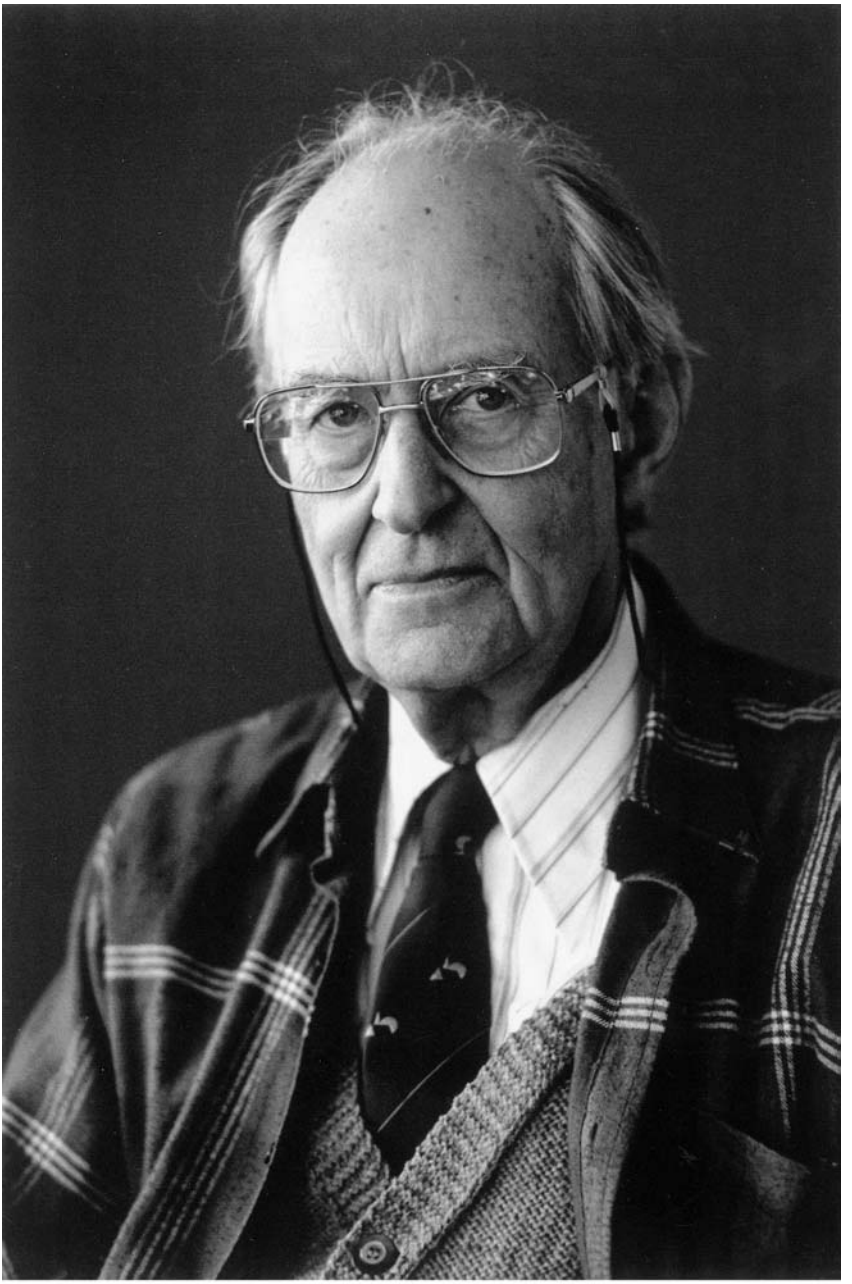

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MY CRUISE THROUGH THE WORLD OF ASTRONOMY

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■ **Abstract** I was born in 1914 in Amsterdam. I grew up there, filling my teenage years with activities as an amateur astronomer. I later studied at Leiden University and volunteered at Leiden Observatory. From 1938 to 1945, I was assistant at the Kapteyn Institute in Groningen, including during the war years 1940–1945, returning to Leiden in October 1945. After prolonged stays at Yerkes Observatory in 1947–1948 and 1952, and participation in Leiden's astrometric Kenya expedition in 1949–1950, I became associate professor at Yerkes Observatory in the fall of 1953. In 1957, I returned to the Kapteyn Institute and soon became involved in the creation of ESO, of which I became scientific director in 1968 and director general from 1970 to 1974. In 1975, I joined Leiden Observatory again, staying until my retirement in 1981, and since then I have enjoyed the hospitality of the Kapteyn Institute. I was president of the IAU from 1976 to 1979. From 1982 to 1989, I was chairman of the Scientific Programs Selection Committee for the European Space Agency's satellite, *Hipparcos*. My principal research interests have been in galactic structure and star formation, with emphasis on stellar associations. In addition to my astronomical interests, I have researched and published on Dutch village history.

1. CHILDHOOD AND COLLEGE YEARS

I well remember the day in the summer of 1932 when I traveled from Amsterdam to Leiden and for the first time faced the stately entrance of Leiden Observatory. I had completed high school in my native city, Amsterdam, and thanks to my mathematics teacher, Leo van Wijk, and a staff member of Utrecht Observatory, Jan van der Bilt, I was about to meet the famous astronomer Willem de Sitter. In my pocket I had my results from the final exam of high school, which were, in Van der Bilt's opinion, outstanding and which had induced him to organize this trip for me. I had received the highest possible grades for the four topics in mathematics: algebra, trigonometry, solid geometry, and descriptive geometry. The antique doorbell resounded loudly in the observatory's entrance hall, and the

custos, Mr. Kasten, soon appeared, who led me to De Sitter's office. Here he was, a kind, tall, frail-looking man who, from behind his big desk, regarded me with his bright, penetrating eyes. After a short conversation, he kindly suggested that I should indeed enter Leiden University. I had been an enthusiastic amateur astronomer; now I would enter the world of the professionals.

Little did I know what this really meant! The academic world was *terra incognita* for me. My parents had no academic background. My father had been raised in a small dairy farmers' community. Physical handicap had forced him to abandon his early wish to become a surveyor and made him pursue a career in auditing. He ultimately became the head of the auditing department of one of the main banks in the Netherlands. Yet all their lives my parents cherished their ties with my father's native village, where his two brothers and their sons continued in the farming tradition. And of course, for us children, what could be more fun than spending holidays on the farm with animals around and free rides in the row boats? Even later in life, we agreed that living on a farm is really the thing to do! My mother's background was quite different. Raised in Amsterdam, she became a governess, and before her marriage she helped educate her employer's children. Six of those years she spent in Java, Indonesia. Her memories of those years and close ties with the children she had helped raise kept us aware of her early career. Indonesia, a colony of the Netherlands, played an important role in the life of many Dutch people. Not only did my mother spend many years there, but so did my wife before she went to high school in Holland. Both her sister and my sister lived in Indonesia when the Second World War broke out, and they barely survived the Japanese concentration camps.

Recalling my earliest childhood, I remember my family spending the evenings in the livingroom, all of us at his or her place at the large table in the center, my mother mending, the children reading or doing homework, my father smoking a cigar and working on some bookkeeping for relatives or friends. On the table stood the tea set; we children never drank coffee or wine—nor beer, a drink my mother associated with drunkards. There were no easy chairs in this room, only some in an adjacent room behind sliding doors that opened on Sunday, when my hospitable parents were at the center of friends and relatives, my mother offering them coffee and brandy. My father's ancestors were Mennonites, my mother's Lutheran. During his life, my father became increasingly involved in organizational matters of the Mennonite community in the Netherlands, but his religious adherence was never imposed on us. I was admitted into the Amsterdam Mennonite community at age 18. In the 70 years since then, I have always felt well at ease among the austere and simple Mennonites. I recently published a study of the seventeenth-century origins of the Mennonite community in my father's native village based on archival documents kept in state and university libraries (Blaauw 2001), a subject I have studied periodically over the years.

What aroused my interest in astronomy? The roots are hard to trace, but prominent in my memory (and still on my bookshelf) are the excellent popular books by Camille Flammarion and an article on "building your own telescope" in a

semipopular magazine. I did build one, using a piece of commercial copper tube, a specially designed two-component objective lens of about one meter focal length, and several microscope eye-pieces. I mounted the telescope on a heavy, solid wooden tripod. We lived in the outskirts of Amsterdam where dark nights were still really dark, and I enjoyed many hours of observing. I learned to find my way among the constellations and became familiar with the Moon's craters, the planets, the Pleiades, Mizar-and-Alcor, some globular clusters, the Andromeda Nebula, and many other objects. I photographed through my telescope the Moon's changing appearance during the lunar eclipse of September 26, 1931. And—this I remember distinctly—I wondered about how far it all reached. Perhaps from this stems my later special interest in the structure of the Milky Way.

Besides Flammarion, there was Sir Arthur Eddington, who had recently published his masterpiece *The Internal Constitution of the Stars* (Eddington 1926), a milestone in the study of the interior of the Sun and the stars. He followed it up by writing a popular version, *Stars and Atoms* (Eddington 1929). I had been bad at foreign languages, but I found *Stars and Atoms* so fascinating that I forced myself to look up every unfamiliar word in the dictionary and note it down for rehearsing to myself—allowing me to pass my high school final exam with an excellent grade for English. Eddington also wrote his fine *The Expanding Universe*. And then there was that other giant of the 1920s, Sir James Jeans, whose magnum opus, *Astronomy and Cosmogony*, appeared in 1928, and who, too, was an excellent writer of popular books, among which I well remember *The Mysterious Universe* and *The Stars in their Courses*. I never met Jeans, except on the cover of the magazine *Punch* of February 27, 1935, where he appeared armed with his slide rule.

The first-year curriculum at Leiden University was dominated by mathematics and physics, with a course on elementary astronomy given by De Sitter that kept my interest in astronomy lively. In my second year, J. Woltjer lectured on celestial mechanics two hours a week. It was one of the toughest but also one of the most beautiful courses I followed. It dealt, successively, with lunar theory, dynamics of the planetary system, and the motion of the Earth as a solid body. Notwithstanding his highly mathematical treatment, Woltjer succeeded in conveying to us a clear understanding of what was behind the formulae, as in the case of the various components of the Moon's perturbations or the latitude variation. Exams for his course were oral, with a minimum of three afternoons, one for each of the subjects. I was proud to pass after three afternoons (and pretty exhausted) with grades ranging from "good" to "sufficient." For more advanced students, Woltjer gave courses on stellar atmospheres and stellar structure. He was one of the first to appreciate fully the importance of Eddington's work (Woltjer 1927). The recommended textbook for astronomy was Russell and colleagues' *Astronomy* (Russell et al. 1926, 1927); I also found Spencer Jones' *General Astronomy* (Jones 1934) most useful, as well as the book by father and son Strömgren (Strömgren & Strömgren 1933). Although my interest in astronomy continued, I also fell in love with mathematics; beautiful, transparent courses by mathematician H.D. Kloosterman left long-lasting impressions.

Of the astronomy courses for advanced students, those by Oort impressed me very much. The main topics were galactic structure and dynamics and interstellar matter; his interest in comets had not yet developed. It was clear that he had been deeply involved in research on most of the topics he presented and that, in a way, his pondering about the subject matter continued during the presentation. This was why, despite his low, sometimes barely audible voice, he did keep his grip on the audience. Remember, this was before the audio/visual era, the lecturer equipped with only a blackboard, chalk, and an occasional lantern slide. However, relatively little attention was given to historical development and research outside Oort's own work. I learned about Bertil Lindblad's extensive efforts toward understanding spiral structure only after having met it in the literature; the names of Charlier, Seeliger, and Malmquist were seldom or never mentioned. Besides Oort's courses, I benefited from finding my way in Smart's (1938) *Stellar Dynamics* and von der Pahlen's (1937) *Lehrbuch der Stellarstatistik*, and from reading the fine historical reviews in De Sitter's (1932) *Kosmos*.

I also well remember the courses on theoretical physics by H.A. Kramers, based on his German textbook (Kramers 1938). I admired his textbook for how it conveyed to the student not only the mathematical formulation but also the subtle reasoning and intuition that had shaped this still young and fascinating discipline. The turbulent years in theoretical physics were hardly behind us—the "Physik der Zwanzigjährigen" (the "physics of the twenty-year-olds"). The youngsters Paul Dirac, Pascual Jordan, John von Neumann, Werner Heisenberg, and Wolfgang Pauli, the somewhat older Max Born and Erwin Schrödinger, and the veterans Bohr and Einstein had revolutionized physics. In Leiden, students had also been guided in this domain by Paul Ehrenfest, whose untimely death in my second year as a student shocked us deeply. These developments had been very much a German-language affair. It is a pity that Kramers's book had not yet been translated into English when, in the postwar years, the center of gravity of theoretical physics shifted to the English-speaking part of the world. Kramers gave me much latitude in the choice of the four subjects in theoretical physics compulsory for my final exam. When I could not attend his courses anymore, after I had left for Groningen, he suggested an interesting substitute: He had just received Sommerfeld & Waldmann's (1938) new book on quantum statistics and told me, "You study it and tell me by way of exam what it is about."

Normally the doors of the observatory opened for the student only after he or she passed the "candidaats" (Bachelor's) exam. I was fortunate in 1935, before passing the exam, to participate in the observatory's activities and live on its premises, and I soon became a member of the small community of assistants, some paid and others, like me, unpaid. Among the assistants were Adriaan Wesselink, Aernout de Sitter (son of Willem de Sitter), Willem Chr. Martin, Jan Uitterdijk, and Ko Ferwerda. Leiden Observatory, the "Old Observatory" as it is referred to since the department moved to its present quarters, was a cozy enclave, located on one of the remnants of the sixteenth century fortifications of the small, compact city of Leiden. The four bachelors among us, including me, lived in the assistants'

quarters within this enclave. Life was not expensive this way, but I did have to economize. The few assistantships for astronomy students were far beyond my horizon. Fortunately, my parents, although definitely not wealthy, could support me financially. "Un père, c'est un banquier naturel" is a wisdom I once picked up from my Mennonite teacher.

Of our small group, Wesselink became well known as coauthor of the Baade-Wesselink method for determining the size of Cepheids from the combination of radial velocity variation and photometric properties. Ferwerda became a high school teacher. Little could we surmise that within ten years World War II would deprive us of the other three. In 1939, Aernout de Sitter was appointed director of Bosscha Observatory in Indonesia, and Martin, who had written his monumental thesis on the variables in the globular cluster Omega Centauri, joined his staff in 1941. Both died from exhaustion in a Japanese labor camp in Sumatra, Indonesia, in September 1944 and June 1945, respectively. Uitterdijk, also in Indonesia, perished in September 1944 when a Japanese ship transporting war prisoners was sunk by Allied forces on its way from Indonesia to Malaysia.

2. LEIDEN OBSERVATORY

Graduate students normally first landed in the division headed by Ejnar Hertzsprung. Hertzsprung emphasized photometric and astrometric astronomy, much of it based on photographic plates obtained with the Leiden telescopes or elsewhere. I participated in Hertzsprung's systematic survey of faint variable stars based on plates collected with the Franklin Adams telescope of Johannesburg Observatory, South Africa. I searched for the variables through a blink comparator, estimated the variable's brightness relative to a set of comparison stars on some 300 plates, and then determined the nature of the variation. Would it be a Cepheid? An RR Lyrae type? A Mira type? An eclipsing binary? Determining the correct period could be quite a challenge, and hitting on an eclipsing binary with eccentric orbit (the majority are circular) would be a fine discovery to please Hertzsprung. Normally the student compiled a "bouquet" of 25 variables for publication, but I quit half way because I shifted to galactic research under Jan Oort. But the work had not been entirely in vain. When in later years the editor of the *Bulletin of the Astronomical Institutes of the Netherlands* (henceforth the *BAN*) faced a blank page in a forthcoming issue, I could help fill it with one of my spare variables.

My first substantial observational program concerned the study of the eclipsing binary YY Gem (Castor C) consisting of two M dwarfs, components of the multiple star Alpha Gem. For that purpose I collected many photographic plates with the largest of the telescopes on the observatory's premises, the excellent 32-cm double astrograph (dating from 1896), and measured them with the observatory's Schilt photometer. The choice of the object was typical of Hertzsprung's approach: For a variable member of a close double star, the companion by its proximity provides an ideal comparison star, allowing highest possible photometric accuracy. I did

not finish the project; after I had left Leiden in 1938, it was completed by a fellow student (Binnendijk 1950). Curious as it may seem to the Leiden astronomer of today, I remember with nostalgia these long hours at the telescope, the night silently falling over Leiden, the fresh night air filling the dome.

Hertzsprung had succeeded De Sitter as director of the observatory, but in practice much of the personnel and financial administration was left to Oort, who enjoyed increasing international recognition because he had revealed galactic rotation in the late 1920s. De Sitter had suddenly died of pneumonia in November 1934, at the relatively young age of 62. I therefore did not witness his scientific activities. Although he is remembered now mostly for his pioneering work on the astronomical consequences of the theory of relativity, his early career had been mainly in astrometry and celestial mechanics, and he had done an outstanding job by reorganizing Leiden Observatory (Blaauw 1973a). An example of his astrometric efforts was the preparation of the first of two Kenya expeditions, in 1931–1933. I return to these below. According to Oort (1981), De Sitter's cosmological activities hardly played a role in scientific discussion among the staff. He met more challenging audiences among theoretical physicists like Ehrenfest and Einstein (the latter in those years a regular guest at Leiden) and in international contexts. In the aftermath of the 1928 International Astronomical Union (IAU) General Assembly, held at Leiden under De Sitter's presidency, his strong efforts were remembered in the restoration of international collaboration in astronomy. At the occasion of the 300th anniversary of the observatory—I attended the ceremonies in the university's auditorium—he published an admirable history of the observatory (De Sitter 1933).

When it comes to absolute devotion to science, Danish-born Hertzsprung has impressed me more than anyone else—perhaps with the exception of Otto Struve. Hertzsprung could be found at the observatory at any time of day, spending many an evening at his broad desk where we observed him—curtains open—measuring photographic plates of double stars or variables, or just leaning back. He was keenly interested in our scientific work, but not in the other aspects of our lives. His marriage to Kapteyn's daughter Henriëtte had become a sad failure, and this has caused him to be regarded among Kapteyn's descendants as "the black sheep in the family" (private communication by the late Maria Noordenbos, Kapteyn's granddaughter). But he dearly loved the daughter from this marriage, who, unfortunately, was born with a slight handicap. She occasionally stayed with her father at the observatory. He had christened her Rigel after the luminous star Beta Orionis (B8Ia), possibly inspired by his great discovery of the occurrence of giants and dwarfs among stars of similar spectral class. He impressed upon us the importance of the exact measurement of phenomena variable in time, for which we may not have immediate use but that may in the distant future prove its value. Thus, he collected photographic plates of double stars from various observatories equipped with long-focus refractors, some of which he observed himself according to carefully thought out schemes. He measured these himself for angular distance and position angle of the companions and had one of his trusted technical staff

members do the same as a check. Having obtained this unique documentation, he was reluctant to have it published. He even went so far as to have his highest precision double star measures printed on little metal plates, thus ensuring their preservation for the small circle of experts in the field. As far as I know, he entrusted this collection in later years to his Danish pupil, the late Kai Strand. The citation index, a deplorable invention for "evaluating" scientific merit, would have been a horror to Hertzsprung.

Guest astronomers at Leiden, always welcome of course, were supposed to render a special service to Hertzsprung: the measurement of one particular plate containing exposures of the double star Antares (Alpha Sco). After collecting measures by 44 invitees and himself during 1932–1940, Hertzsprung determined for each of them his or her personal measuring accuracy and deviation of the average angular separation from the overall mean, their "personal systematic error" (Hertzsprung 1942). The moral was that an observer's measures are always affected by a systematic error, however keen he or she may be to make the best of it.

Hertzsprung had, as he used to say, entered astronomy through the back door, as a chemist and amateur astronomer who worked on photographic plates, applied these to stellar photometry, codiscovered the Hertzsprung-Russell diagram, was discovered himself by Karl Schwarzschild, and thus entered astronomy. Of course, he was the subject of numerous stories, passed on by senior staff to the juniors. He used to say "mathematics makes thinking superfluous," and Karl Schwarzschild allegedly should have said "Hertzsprung denkt immer, ich denke nur gelegentlich."

As a first assignment suggested to me by Oort, I critically reviewed a series of papers (Strömberg 1930a–d, 1936) dealing with the question of a possible "subgiant" branch in the G- and K-type domain of the color-magnitude diagram. Its occurrence had been suggested by the author on the basis of an analysis of proper motions and radial velocities. I found that these observational data could be equally well represented without the subgiant branch. Oort agreed and approved my manuscript, but he wished to consult with Hertzsprung about publication. Hertzsprung also supported my conclusions but advised against publication because "it would not be wise for a student to start his career with a negative judgment of work by an established astronomer." And so my carefully composed manuscript joined my supply of unpublished variables.

My next project with Oort was more substantial. It dealt with the vertex deviation in the distribution of the space velocities of stars in the solar neighborhood. This distribution, according to simple dynamical theory, should fit a velocity ellipsoid with its axis of largest velocity dispersion directed toward the galactic center and anticenter. However, for the brightest stars it does lie in the galactic plane but points to galactic longitude around 20°. Would this also be the case for more distant stars? The appearance in 1934 of the *Radcliffe Catalogue of Proper Motions* (Knox-Shaw & Scott Barrett 1934) of faint stars in Kapteyn's northern selected areas offered a possibility to reinvestigate the problem. I found that, except for the highest galactic latitudes, these stars—at distances beyond a few hundred parsecs—did not show the vertex deviation and thus confirmed its local nature (Blaauw 1939). However,

the method I had invented differed from that applied in most of the work on the bright stars. Whereas the latter had used primarily the directions of the proper motions, I used their size, and this difference called for more detailed study of the structure of the local velocity distribution.

In August 1938, I attended my first assembly of the IAU in Stockholm, Sweden. Oort and Hertzsprung had arranged for us students to be unofficial guests and had found some travel money. Total attendance numbered some 300 astronomers plus guests, the size of a modern symposium, and an adequate number for the whole assembly to fit on the moderate-size ship that took us for a sightseeing trip in Stockholm's beautiful harbor. I have only vague—and mostly irrelevant—recollections of the sessions. I remember the host, Bertil Lindblad, opening the meetings and addressing the audience from the balcony of a stately building. I watched famous astronomers Adriaan van Maanen and Fritz Zwicky quarreling about the accuracy of some photometry; Oort presenting his latest results on the large-scale stellar distribution in the Galaxy; and a member of the Swedish royal family addressing the audience in boy scout apparel, leaving some of the participants pretty stunned, like astronomer Jackson who for this occasion had brought his tuxedo all the way from South Africa. Quite vivid in my memory is the formal closing dinner held in the beautiful bluish reception hall of Stockholm's City Hall, where I found myself immersed in a group of lively Harvard students. They clustered around famous Annie Cannon, the author of the impressive *Henry Draper Catalogue* containing her hundreds of thousands of spectral classifications, the catalog I had frequently used. And I remember Sir Arthur Eddington, the newly elected president of the IAU, giving his closing address and referring to the dark clouds on the political sky. Within little more than a year, World War II broke out.

After one of the sessions at the assembly, I was approached by Pieter van Rhijn, director of the Kapteyn Astronomical Laboratory at Groningen, who offered me an assistantship at his institute. I knew very little about the Groningen Institute yet needed little persuasion; here was my first paid job. On October 1, 1938, I met the institute's staff. Several well-known astronomers had preceded me in this job, among them Peter van de Kamp, Jan Schilt, Jan H. Oort, Jean Jaques Raimond, and Bart J. Bok. Bart's name and that of his wife Priscilla Fairfield undoubtedly ring a bell among the older generation of astronomers, especially in the United States and Australia. Bart and Priscilla became dear friends of my wife and me. It was at one of our gatherings, with Bart rich in stories, that Priscilla confided to me: "My husband has an excellent memory for the irrelevant." Perhaps, in this respect, the reader of this chapter may note a certain affinity between the late Bart and myself.

3. GRONINGEN 1938–1945

Early in the twentieth century, the Kapteyn Laboratory had become famous for Kapteyn's work on galactic structure. Van Rhijn, his successor in 1921, saw it as his task to continue Kapteyn's broad schemes for collecting observational data. And

so, during my first years at the institute, I spent a good deal of my time supervising the photographic photometry of stars in Kapteyn's Selected Areas, a joint project with Bergedorf Observatory in Hamburg (Schwassmann & Van Rhijn 1935–1953). For this project, we received multi-exposure photographic plates from Harvard Observatory. I soon found that a modification of the reduction procedure produced much higher photometric accuracy than had been obtained so far (Blaauw 1940).

On May 10, 1940, German troops crossed the Dutch border. Five years later, in April 1945, the city of Groningen was liberated by Canadian and Polish troops. At the Kapteyn Laboratory, as in most scientific institutes, initially research continued along the prewar course. However, with Pieter van Rhijn suffering from tuberculosis, staff members who had served in the Dutch army forced into hiding, and the activity of the remaining staff increasingly restricted by Nazi raids and worsening living conditions, work at the institute gradually slowed down. When, in December 1941, the United States entered the war, the shipments of photographic plates from Harvard stopped, and the institute's main project gradually came to a standstill. What time was available for research I could now spend on what became my thesis; I return to it below. With the exception of the incident reported below, I refrain from describing the ups and downs of my family's life during the war. For every word spent on my story, hundreds should be spent on the much more miserable fate that struck so many others, among them my Jewish classmates whose faces I see in my school photographs from the 1920s. I can only surmise the horrible fate that must have struck most of them. Among them also was my friend and associate, German-born Lukas Plaut, a Jewish student who was expelled by the Nazis from Babelsberg Observatory and took refuge first in Leiden and later in Groningen, where he was again victimized by the Nazis. In postwar years he became a highly valued staff member of the Kapteyn Institute but eventually suffered terribly from a stress syndrome resulting from his internment in a concentration camp (Blaauw 1985, 2004).

A curious incident occurred toward the end of the war. One day in February 1945, my doorbell rang and two lads of high school age, amateur astronomers, asked to see me. With all the bad things the war brought us, the forced blackouts at least provided amateur astronomers with a unique opportunity to watch the sky. These lads now also wanted to watch and speak to a real astronomer, and of course they were welcome to enter. The lads were Maarten Schmidt and Jan Borgman; both later had excellent careers in astronomy. As one of them reported years later, my wife met them with the words, "You are lucky, he has just been released from prison!" Why had I been in prison? I had been accused of listening to and spreading news broadcast from London—something strictly forbidden. Luckily, the war was approaching its conclusion, and although nobody could tell how long it would continue, some lower-ranking German officials were ripe for some gentle bribery. My relatives and friends collected a sufficiently persuasive sum and succeeded in getting the prison door open for me. Two things I recall of that memorable release on February 8, 1945—it was my little son's second birthday, and I could at last satisfy my hunger, as far as circumstances allowed.

4. LEIDEN AND KENYA

One day in 1944, when Jan Oort stayed with us at Groningen, he offered me a position at Leiden Observatory beginning after the war. I began at Leiden in October 1945. At Groningen, Pieter van Rhijn gradually recovered from his illness and Lukas Plaut was appointed to the staff.

Leiden Observatory entered a new era. Hertzsprung had retired, Jan Oort had become director, and Pieter Oosterhoff was associate director. My office was opposite Jan Oort's in the observatory's main building, separated from the rest by the meridian room. This allowed me to share in a natural way in his plans and some of his worries. In these years began a growing friendship that lasted until Jan's death in 1992. At the occasion of his 80th birthday, in a *liber amicorum*, I had the opportunity to express my deep admiration for his many accomplishments (Blaauw 1980a), and I was deeply moved by the remark he addressed to me in his letter to the authors, "[M]y nearly life-long friend who shared with me the same interest and the same manner of research: nobody could have better reviewed my work," words that echoed in his contribution to my 70th birthday symposium (Oort 1985).

From one of Jan's worries stemmed my mission to Africa in 1949–1950. There had been a long-standing problem in positional astronomy: the systematic errors in declinations determined with meridian telescopes as a consequence of the atmospheric refraction and the flexure of the telescope tube. The existence of such errors was apparent from the differences in the declinations and the proper motions in declination given in two main catalogs: the GC (Boss 1937) and the FK3 (Kopff 1934). Which catalog was the best? Oort and I had independently investigated this problem without arriving at a solid conclusion (Oort 1943, Blaauw 1944). The Kenya expedition of 1931–1933, organized by De Sitter and led by C.H. Hins, as well as this new expedition to Kenya applied an alternative method. For an observer located on the Earth's equator, the instant a star rises or sets its angular distance from the pole measured along the horizon is a measure of its declination, supposedly free of the troublesome atmospheric refraction and flexure of the telescope tube, both working in vertical direction. During the war, Oort developed plans for a new expedition to be supervised by Bert van Herk, Hins's assistant in 1931–1933. Photographic cameras now recorded the horizontal readings instead of the visual observations of the first expedition. Unfortunately, results obtained during the first months of the expedition were not satisfactory, the photographic readings needed improvement, and at this stage Jan sought my collaboration. I had new cameras designed, and with these and some tools in my suitcase I boarded a plane for Entebbe, Uganda, on November 15, 1949. From there, a train ride brought me to the tiny railway station Eldoret in Kenya where I was awaited by Bert van Herk. A U.S. army surplus vehicle took care of the last stretch, and at an elevation of 3000 meters on Timboroa Hill I arrived at the temporary settlement—in the middle of nowhere, but exactly on the equator. Five months later, in April 1950, I was back home. The cameras performed well and the expedition continued for several years.

The results were published five years later (Van Herk 1957). The proper test of the method is to compare the results with *Hipparcos*—the latter reduced to the epoch 1951—but this comparison has not been made yet. We have, however, good reasons to believe that the method worked quite well. The complete story of the Kenya expeditions has been written by Jet Katgert-Merkelijn (1991).

In recalling my five months in Kenya, I realize that, like so many others who have spent time in central Africa, I have had a nostalgia for it ever after. Wildlife in Kenya at that time was still abundant and relatively untouched by tourism. Friendly members of the Kikuyu tribe, serving the observatory in a variety of jobs, formed part of the large family of our small settlement. The Masai tribe in the distant steppe lived relatively untouched by Western influence. I have nostalgia, indeed, in spite of the fact that during nightly observing hours, at an altitude of 9000 feet, strong, extremely humid winds sometimes made me shiver miserably from the cold and soaked our notebooks!

After completing my task on Timboroa Hill, I left on April 4, 1950, for a 12-day journey to Khartoum in northern Sudan, first by train, then by boat, then by some four-wheel vehicle via Uganda to Butiaba on the shore of Lake Albert. Next, for a week, I traveled by paddle-steamer on the White Nile through the endless swamps of the southern Sudan, with scheduled stops at Packwach, Rhino Camp, Nimule, Juba, and Malakal, and improvised ones at native settlements hidden behind the high papyrus along the border of the river. I was watched by still-abundant but suspicious crocodile, hippo, rhino, and elephant. Finally, I traveled from sunny Khartoum by plane to rainy Holland. For Oort, the effort did not imply further commitment to the observatory's tradition in meridian circle astrometry. He judged this henceforth to be the task of specialized institutes such as, for instance, Paris Observatory. And so it happened that its director, André Danjon, at one of his visits to Leiden in the 1950s, was stunned to learn that his *bon ami* Oort had turned the meridian room into a recreation area and replaced the meridian circle with a ping pong table.

5. YERKES OBSERVATORY 1947–1948, 1952, 1953–1957

In March 1946, a small group of leading astronomers met at Copenhagen to discuss resuming IAU-context international projects. Among the participants from the United States was Otto Struve, director of Yerkes Observatory of the University of Chicago, located in Williams Bay, Wisconsin. Struve was greatly admired for the way he had brought Yerkes Observatory to the forefront of research. He was a descendant of several generations of famous astronomers in eastern Europe and had been born there himself. He would as few others be able to contribute to the meetings. Later that year he visited Leiden to discuss research on cluster-type variables. As a result, he collected hundreds of spectrograms of RR Lyrae at McDonald Observatory in June and July 1947, and I planned to collaborate with him at Yerkes Observatory in the measurement and analysis. Yerkes Observatory

became my *pied à terre* in the United States, first for two prolonged visits in 1947–1948 and 1952, and next with my family in 1953–1957.

In 1947, access to the United States still was difficult. It took Struve many months to convince the U.S. immigration authorities that I was a safe bet. Taking my family along was out of the question for many reasons—including financial. On June 20, 1947, I boarded the steamer *Westerdam* that landed in New York on June 30. Nine months later, in March 1948, I was back home. Our joint paper appeared soon after (Struve & Blaauw 1948). RR Lyrae has a principal period of 0.567 day, but we found the velocity curve to oscillate in time with a period of approximately 75 cycles, in agreement with photometric observations by Walraven (1949).

Don Osterbrock's admirable description of Yerkes's turbulent history and the role of Struve (Osterbrock 1999) refers to the period 1945–1950 as its golden period, followed by gradual decline. I was fortunate to belong to it in these great days. That they did not last long after Struve's departure in 1950 is not surprising; an opera with many prima donnas requires a director of extraordinary authority. Struve belonged, with Hertzsprung, to those few people who impressed me by their almost exclusive devotion to astronomy, and he was a man of extraordinary energy. While I spent a considerable part of my time during the first half year of my stay on the measurement of about two thirds of the 390 spectrograms, Struve, in addition to measuring his share, was fully occupied with administrative matters and moreover found time for writing popular articles. Among my later recollections is the moment when, at a dinner party, he spoke about the next IAU Assembly to be held in Moscow in 1958. Having fought on the side of the White Russian army, Struve had been forced to flee Russia in the turbulent years of the revolution and had not set foot there since. He was eager to see Russia again. Yet, he told us, he would not attend. His American citizenship, according to American law, protected him everywhere in the world except in his native country. He had no reason to fear that he still was *persona non grata* in the USSR, yet, tragically, he did not want to risk a situation that might have been embarrassing for U.S. authorities.

Among Struve's prima donnas at Yerkes were Subrahmanyan Chandrasekhar (Chandra), Gerard Kuiper, William (Bill) Morgan, Albert (Al) Hiltner, and prominent visitors Bengt Strömgren and Gerhard Herzberg. Many of the students became leading astronomers in the United States. Chandra organized the weekly colloquia, and students realized that they had better be present, for at their exams they might be questioned on subjects discussed at the colloquia. Lodging for some of the students was provided by the Van Biesbroeck family, the "Van Bies." George Van Biesbroeck, a retired staff member but still an ardent observer of comets and double stars, and his wife Julia had adapted their roomy house to offer board and lodging. I enjoyed the meals, in the lively company of students and some staff, but made use of the lodging only by the end of my stay. The observatory offered free lodging in the building's attic, nicknamed "the battleship" because of its circular windows. Up there, it could be suffocatingly hot and terribly humid during the summer months, but it saved me money that I could spend on clothes and other items I sent home, where in postwar years goods were still extremely scarce.

Four years later, on April 16, 1952, I again crossed the Atlantic to spend six months at Yerkes at the invitation of its new director, Bengt Strömgren. Bill Morgan's work on the spectral classifications in the Morgan-Keenan system suggested a fruitful collaboration on the calibration of Bill's luminosity classes and on stellar associations in general. It resulted in several joint papers (Blaauw & Morgan 1953a, 1953b, 1954). However, again I was away from home for half a year, and in between this visit and my previous one had been my five months with the Kenya expedition. This naturally had been a heavy burden on my wife, a burden she always courageously had borne. In this respect, pursuing an international career in astronomy at that time was incomparably heavier than nowadays. (Our third child was born during my first stay at Yerkes; I saw the baby only months later.) It was during this 1952 stay that Bengt Strömgren approached me about joining the staff of Yerkes. After having declined an approach by Gerard Kuiper a few years earlier, this time we decided to take the bold step. Naturally, to leave Leiden was a hard decision. Early in October 1953, I flew with my son via Reykjavik, Iceland (cheapest route!) to the United States to take up my job and for my son to begin the school year; my wife and two daughters followed by steamer. I drove with my newly acquired Plymouth (my first car) from Williams Bay to New York and met them at the quay on November 10.

We stayed at the Van Bies's until the big chest containing our household goods arrived. Our belongings still needed to be screened by U.S. Customs, however. Customs officials told us by telephone that we could unpack our belongings and put them into our new home and that they would come by the next morning to inspect them. That morning we overslept and found a note pinned to the front door:

> November 25, 1953
>
> Sir,
>
> U.S. Customs was here this morning—your effects were observed through your windows. Everything appears to be in order. Your effects have therefore been released.
>
> Donald J. Barth, Customs Ex. Aid, Milwaukee

Could we have wished for a more generous welcome?

Scientifically, my expectations were fully met. I could pursue research and give classes in the stimulating atmosphere I had learned to appreciate so much in earlier years. This, by the way, included courses I gave on the Chicago campus, in that classroom where the frame of the blackboard carried a student's counsel, "If you can't convince them, confuse them," and where the duster I took from home always came in handy because the University obviously preferred to spend their money on research and teaching than on cleaning services. I have often realized in later years, back in Europe, that these years at Yerkes were crucial for my future work and career. My acquaintance with the American astronomical community and some of its leading scientists was of great help in the years of my directorate

of the European Southern Observatory (ESO) and later during my IAU presidency. My editorship (jointly with Maarten Schmidt) of the volume *Galactic Structure* (Blaauw & Schmidt 1965) of the series *Stars and Stellar Systems* and my chapters on the calibration of luminosity criteria and on stellar populations in this series stem from the time I was at Yerkes. Among my collection of slides, some show a happy family enjoying life, either in Williams Bay, the children playing in or near Lake Geneva, or in a Texas setting, climbing rocks on the mountain slopes near McDonald Observatory. Observatory staff and their families, along with students and visitors often housed on or near the observatory premises, formed a close community within the rural village of Williams Bay. There was always much musical talent in this community. We enjoyed playing chamber music for violin, viola, and piano; I played the flute.

Yet in August 1957, we returned to the Netherlands, albeit after considerable hesitation. Van Rhijn had retired from the directorship of the Kapteyn Laboratory; I had been asked in 1955 to succeed him but declined. The offer was discussed again in detail during my stay in Holland in the fall of 1956. The University of Groningen clearly intended to breathe new life into the astronomy department. Could the Kapteyn Laboratory, that in Kapteyn's years had been so successful as a center of research even without possessing its own observational equipment, restore this position? Optimism about my chances for collaboration with observational institutes elsewhere in the world, and for me the very appealing trend toward scientific collaboration on the European scale ("il faut faire l'Europe," as my friend and colleague Charles Fehrenbach used to say), combined with the powerful potential of radio astronomy in the Netherlands, made me decide to accept the challenge. At Yerkes, Chandra's interests shifted toward the physics department at Chicago, Gerard Kuiper drifted toward Arizona, Bengt Strömgren was lured away to Princeton, and Al Hiltner went to Michigan. Nevertheless, the decision was a very difficult one.

In the years 1967–1969, at Larry Fredrick's invitation, I enjoyed a visiting professorship at McCormick Observatory in Charlottesville, Virginia, with participation in its astrometric programs. Always, when a plane has landed in the United States, I have had that "at home" feeling. It added an extra dimension to the honors I had the pleasure to receive from the American side, like the Bruce medal, an honorary membership in the American Astronomical Society, and the foreign membership in the American Academy of Arts and Sciences. Walter Baade used to tell European astronomers who spent some years in the United States: "The time may arrive for you to return home, but not until you have come on first-name terms with your principal American colleagues." I believe I had met the Baade criterion.

6. STELLAR ASSOCIATIONS

Throughout the years, a recurrent topic in my research has been the youngest component of the stellar population. In a way, it stemmed from research by Kapteyn in 1910–1918 after he had discovered the phenomenon of the "star streams"—the

preferential direction in stellar motions that later was described as the ellipsoidal distribution. The B-type stars did not fit Kapteyn's model. He then focused on these, leading to two exhaustive papers (Kapteyn 1914, 1918) with emphasis on the individual secular parallaxes. The appearance in 1937 of a new catalog of proper motions (Boss 1937) suggested a new analysis, and this resulted in my thesis, defended at Groningen a year after the end of World War II (Blaauw 1946). It was written under the very unusual circumstances of the last war years and immediately after the liberation, with many interruptions. It was a solitary adventure with little opportunity for exchange of views with colleagues. Van Rhijn was hospitalized with tuberculosis, and communication with astronomers elsewhere was complicated. However, there was ample opportunity for studying the literature, and I guess few students have struggled through Bertil Lindblad's lengthy papers on stellar dynamics as thoroughly as I did. I was awarded a doctoral degree *cum laude*, and I venture to think that this boosted my confidence in being able to do worthwhile research even when left on my own.

I obtained new, very accurate secular parallaxes for the B-type stars of Sco-Cen, but I found the structure of the Sco-Cen clustering and its kinematics the most intriguing aspects. I realized that the mass density of this aggregate was far below the critical density for it to remain internally bound, and, inspired by Lindblad's treatment of the kinematics of individual orbits in rotating stellar systems (Lindblad 1940a,b, 1941, 1942), I tried to find an explanation for the peculiar elongated structure of the cluster and its orientation with respect to the direction of galactic rotation. I succeeded up to a point by assuming the stars to have been members of a small clustering, subject to a disturbing force at an epoch between 60 and 150 million years ago. A few years later, Walter Baade, commenting on my thesis, suggested that I had "missed the boat" because the Sco-Cen structure bears a certain resemblance to the wisps observed in spiral systems, and Baade thought I might have considered my wisp-like feature as an indication of the occurrence of spiral structure—a feature of the Galaxy totally unknown at that time, but anxiously looked for. To me, this seemed a bit too far-fetched. But in another respect, I do think I missed the boat in that I did not go as far as Viktor Ambartsumian (1948, 1949, 1954) did. He had emphasized that, because loose clusters like these cannot have existed for time spans more than a hundred million years, they must have been born much more recently than the majority of the galactic population. It was, in fact, only in those years that stellar birth became recognized as a process still going on in the Galaxy (and elsewhere), and that corresponding evidence emerged from the understanding of nuclear burning and the energy output of massive stars (see, for instance, Unsold 1944). Years later, after I had published the *Annual Review of Astronomy and Astrophysics* review on associations mentioned below (Blaauw 1964), Ambartsumian's associate Ludwig Mirzoyan expressed his gratitude for my having done justice to the fine contributions to this field by Soviet, notably Armenian, astronomers. Astronomical activity in the English-speaking part of the world came so much more easily to the forefront of research than that achieved in the rest of the world, a well-nigh unavoidable bias that was, however,

painfully felt in those countries—and that, unfortunately, to some degree persists today.

In the early 1950s, by still more precise proper motions, I provided evidence in favor of Ambartsumian's inference that some of these young groups, owing to lack of gravitational binding, must dissolve among the galactic field population on a timescale of the order of 100 million years or less, and I concluded that, early in the process, their structures may temporarily assume elliptical shapes and an orientation that bears similarity to that of the Sco-Cen complex (Blaauw 1952a). I later showed that this process can be recognized even more convincingly, and on larger scale, in the structure and kinematics of the Gould Belt (Blaauw 1965a). In 1953, upon arrival at Yerkes, I undertook a systematic study of the northern O-B5 stars, the northern complement of my work on the southern Sco-Cen region (Blaauw 1956). Bill Morgan had kindly made his classifications in the MK system available, and H.R. Morgan of U.S. Naval Observatory provided me with newly computed proper motions in his N30 system (Morgan 1952). Among the results was the discovery of an extended, loose group with common motion with respect to the sun and very small internal velocity dispersion. I christened it the Cas-Tau OB association and estimated its age to be about 50 million years. The origin of the Gould Belt may well be intimately connected with the evolution of Cas-Tau that is located at the center of the Belt.

I furthermore considered that because the association members represent an early stage of stellar evolution, it would be of great interest to address the fundamental problem of the frequency of duplicity and multiplicity. I therefore collected during the years of my association with Yerkes, 1952–1957, some 1000 spectrograms with the McDonald 82-inch telescope to arrive at unbiased statistics of OB spectroscopic binaries. (I remember traveling by train from Williams Bay to Pecos, Texas, where superintendent Marlin Krebs picked me up and drove with airplane speed over the corrugated road to McDonald Observatory.) After my move to Groningen in 1957, the measurement and analysis of these spectrograms, together with additional ones obtained in Europe and South Africa, became a joint project with my student Tjeerd van Albada (Blaauw & Van Albada 1963). We found strong evidence for a bimodal distribution of the separations of the components of B-type binaries (Blaauw & Van Albada 1967), and in a series of papers Van Albada (1968a–c) followed up by investigating whether the wide doubles (separations exceeding 100 AU) might have originated from dynamical interaction in small, young OB clusters shortly after their formation; this appeared to be the case indeed. These were the years when electronic computers entered and changed the character of our research institutes. Van Albada's work constituted one of the first steps in the numerical approach to stellar dynamics that soon revolutionized this branch of astronomy. For the close binaries we obtained the much-wanted unbiased statistics of periods, semiamplitudes, and eccentricities, including, for instance, the revealing velocity amplitude versus log period diagrams (Van Albada 1985).

At an early stage, I wrote the review paper "The O Associations in the Solar Neighborhood" (Blaauw 1964). I pointed out that in the larger ones of the nearby

associations, spatially separated subsystems can be distinguished whose color-magnitude diagrams indicate different degrees of evolution, and whose relative sizes suggest an increase with age. I also noted that within an association, the proportion of earliest type stars was largest in the most concentrated subsystem and stressed that the degree of association with (optically visible) interstellar matter decreases with increasing size and evolutionary age. I concluded that formation of the massive stars in the associations was not the sort of continuous process we had been inclined to assume, but instead proceeds sequentially in adjacent domains. I noted that the separations of the subsystems were of the same order of magnitude as the sizes of the subsystems themselves, that the numbers of stars in the subsystems were remarkably similar, and that the directions of mutual orientation of the subsystems tended to be parallel to the galactic plane. Students of star formation did not quite seem to know what to do with these findings and left the article slumbering. But then, in September 1976, the unexpected occurred. Having completed my term as director general of ESO, and while trying to catch up with progress in my favorite research areas, I attended IAU Symposium Number 75, *Star Formation*, in Geneva (De Jong & Maeder 1977). During one of the sessions a statement by one of the participants caught my ear: my 1964 review paper had been "dusted off" and found useful for the occasion. What had happened? Spectacular progress had been made in the field of molecular observations, providing data on the physics of the interstellar clouds and revealing their size and structure. Observations presented by Patrick Thaddeus now placed my findings in a new context. Sequential star formation was to be seen as the result of a physical process proceeding through a massive cloud by a kind of sequential ignition, and an attractive model was proposed by Elmegreen & Lada (1977). The most conspicuous examples were the series of subgroups in the nearby OB associations Ori I and Sco-Cen discussed in my review paper, but, of course, this whole sample of nearby associations now was to be studied from a different angle.

In 1991, at a NATO conference, I presented a new review paper, now in proper molecular cloud context (Blaauw 1991a). I emphasized the Gould-Belt context in which the study of the nearer associations must be placed, and I introduced the notion of the Evaporation Function E(t) for OB associations, E(t) being the number of stars evolving away (supposedly through the supernovae process) per million years for the standard association subgroup, as a function of time. Introduction of E(t) is necessary for estimating how many supernovae may have contributed to the disruption of the original molecular cloud in the immediate surroundings of an association. I pointed out in this paper that the rich Cas-Tau association located in the immediate vicinity of the Sun during its existence over the last 25 million years must have produced some 15 supernovae; these must have dominated the local changes in the structure of the ISM.

Proposals for observing the OB association stars figured prominently in the applications for *Hipparcos* in the early 1980s, and the impressive census paper by de Zeeuw et al. (1999) is a result. That my name is the last among its authors reflects that I looked with much pleasure over their shoulders, watched in fascination

their treatment of *Hipparcos*'s massive amount of observational data, and, once in a while, injected a reminder of things not to be overlooked. Our census article promises to remain for many years the principal reference when it comes to questions of membership and kinematics of the nearest OB associations. Meanwhile, little progress has been made regarding the statistics of duplicity and multiplicity. A wholesale attack, extending the radial velocity observations to a much larger, unbiased sample of B stars, remains in order. As an example of what to look for, I mention one of the results—also shown in my 1991 NATO conference paper—that we found from our radial velocity surveys. When plotting the semiamplitude of the velocity curve against log period, the old, rich Cas-Tau association shows a deficiency of high-amplitude, short-period binaries compared with the ensemble of four much younger associations in the sample. Is this a consequence of the age difference, or does it point to different initial conditions in the rich Cas-Tau association?

One of the items in my program that did come to a provisional rounding off were the runaway OB stars. These high-velocity objects represent a minute fraction only of the galactic OB population, but I was struck by their peculiar behavior and, upon my return to Groningen in the late 1950s, I wanted to first settle with these. It led to my hypothesis that the runaways are the released secondary components of binaries of which the more massive primary has suffered major mass loss in the supernova explosion (Blaauw 1961). By now, this explanation has found general acceptance. Researchers also recognize, however, that the origin may be in dynamical interaction between massive binaries in a compact young cluster (Poveda et al. 1967). We may even be able to distinguish between the two mechanisms: This takes into account the rapid rotation and the occurrence of high metal abundance in the atmospheres of the high-velocity objects released by the supernova process as well as the expected absence of these characteristics in the case of dynamical interaction—relations for which I found evidence in a later stage (Blaauw 1993).

In a delightful essay, *The Psychology of Invention in the Mathematical Field*, mathematician Jacques Hadamard (1945) discusses the seemingly irrelevant, relaxed circumstances under which certain discoveries in his field were made. Perhaps he might have been interested to learn that the idea of the supernova release for the OB runaways occurred to me while I was shaving, and that the discovery of the correlation between high velocity, high rotational velocity, and chemical abundance came to me while I was browsing through astronomical data at a summer resort.

7. BACK AT THE KAPTEYN LABORATORY

The years in the United States had enriched us with an American-born daughter, new ties of friendship, and an appreciation of "the American way of life." The University of Groningen loyally fulfilled their promises: A chair for astrophysics was created and filled by Stuart Pottasch, and two lecturers were appointed, Hugo van Woerden and Jan Borgman (both in later years promoted to professorships),

as well as additional technical and computing staff. Outside Groningen, remote from its city lights, a new optical observing station was erected with an excellent mechanical workshop and a 61-cm optical telescope. Under the supervision of Borgman, it served for the design and testing of new apparatus to be used in combination with large optical telescopes elsewhere in the world. In later years it became the cradle of the space research program of the Kapteyn Institute. Through Van Woerden's efforts, the Kapteyn Laboratory became fully integrated into the national program of radio astronomy, with emphasis on the observation and analysis of the 21-cm line of neutral hydrogen through the Dwingeloo radio telescope. Principal objects of research in these first years were the cloud structure of the HI medium and the intermediate- to high-velocity currents outside the thin gas layer in the galactic plane. For the study of the cloud structure, we developed the method of resolving the HI 21-cm profiles into Gaussian components. Results of these analyses were published by Takakubo & Van Woerden (1966) and Takakubo (1968a,b).

In a way, this was a sequel to my earlier work on the interstellar medium based on optical data (Blaauw 1952b). I had analyzed measures of the radial velocities of the interstellar CaII H and K lines published by Adams (1949) on the assumption that the interstellar gas is distributed over separate clouds with random individual motions. This, of course, is an extreme simplification of the true situation, but at least it offered the possibility to assign some numerical values to such quantities as "the number of clouds traversed by the line-of-sight per kpc" and "the mean random velocity of the clouds." This analysis was complicated because of the limited resolving power of Adams's spectroscopic equipment and hence blending of components. For the introduction of models of the random motions of the clouds, I had resorted to large numbers of paper slips on which I wrote a number so that the frequency distribution of these numbers simulated either a Gaussian distribution or an exponential one, borrowing some of these samples from statistically minded biologists. Selecting randomly from these samples and taking into account the spectroscopic blending did the trick fairly well. It earned me the reputation of having introduced Monte Carlo techniques *avant la lettre*. I found that Adams's observations could be represented well by assuming the exponential distribution and a mean speed in the radial component of 5 km/s, and 8 to 12 clouds traversed per kpc.

Lukas Plaut, for whom a special chair for variable star research was created, conducted the extensive Groningen-Palomar Variable Star Survey. It originated from discussions at the first IAU Symposium (Blaauw 1955), where Baade had suggested deep surveys of RR Lyrae variables in the direction of the galactic central regions to explore the surroundings of the galactic nucleus. Plaut collected many Palomar Schmidt plates of four regions carefully tested by Baade for low interstellar absorption. For the detection of the variables, Borgman designed a new type of blink comparator: It allowed the observer to scan two plates taken at different epochs for the differences between the plates by eliminating all the images of nonvariable stars. Plaut succeeded in completing this impressive project

by 1971, but I had already left Groningen to assume the directorate of ESO. It became the basis for his paper with Oort on the distance of the galactic center (Oort & Plaut 1975).

My own work, apart from the program on stellar associations and my growing involvement in the ESO project, initially dealt with two subjects that had long drawn my interest: calibration problems and stellar populations. Kaj Strand, editor of Volume III, *Observational Data* of Gerard Kuiper's compendium *Stars and Stellar Systems*, had invited me to contribute the chapter "Calibration of Luminosity Criteria." From earlier work by Russell & Moore (1938, 1940) and by Van Rhijn (1939), I knew it was a tricky subject with nasty pitfalls. I tried to explain these and compiled a table for the relation between the luminosity classes in the Morgan-Keenan system and visual absolute magnitude, which became the standard reference for many years (Blaauw 1963, 1973b).

While still at Yerkes, at the suggestion of Gerard Kuiper, I had agreed to do the editing of Volume V, *Galactic Structure*. However, once I decided to leave Yerkes, I felt I should share the task with someone residing in the United States, where many of the authors lived. Maarten Schmidt accepted, and a few years of pleasant collaboration lay ahead (Blaauw & Schmidt 1965). I well remember one of my visits to Maarten's home when Maarten and I cut one of the chapters into pieces to rearrange its paragraphs. The house filled with Maarten's excitement about his interpretation of the spectrum of 3C273—his quasar. My contribution was the chapter on stellar populations (Blaauw 1965b). After Baade's first work on the subject in 1944, I had closely followed developments in this field. In 1951, stellar populations had been the subject of a thorough presentation for Dutch astronomers (Blaauw 1951), and six years later, in May 1957, it was the theme of the first astronomical Vatican Semaine d'Etude (O'Connell 1958). Baade's ideas had always appealed to me, perhaps because he had pointed to features in the stellar population I had noted myself without, however, drawing his daring conclusions. I tried to convey these with emphasis on the local population. The chapter incorporated the refined subclassifications proposed at the Semaine and served as useful reference until it was superseded by the IAU symposium on stellar populations (Van der Kruit & Gilmore 1995).

Mentioning the Vatican conference arouses pleasant recollections (see Figure 1). Naturally, the sessions held in the stately Vatican Academy were very much worth attending, but I am thinking also of two fringe benefits. One was a short stay with Jan and Mieke Oort and Walter Baade at the summer resort Amalfi, on the sunny Italian coast near Naples, relaxing and swimming, where one afternoon we enjoyed the company of Abbé Lemaître, Willy Fowler, and Fred and Mrs. Hoyle. The other was a grandiose happening in the St. Peter's Basilica, a beatification. Here we were, seated in this immense building amid the crowds for whom this clearly was an occasion more of exuberant sympathy than of solemn silence. It reached its climax when the pope entered, and even the normally dignified Abbé Lemaître excitedly jumped on his seat to catch a glimpse of his Holy Father!

Figure 1 At the Semaine d'Etude on Stellar Populations of the Vatican Academy, May 1957, from left to right: Daniel Chalonge, the author, Bengt Strömgren, Abbé Lemaître, George Herbig, D.J.K. O'Connell, Otto Heckmann.

Naturally, at this stage in my career, I did not escape administrative duties; these included the secretariat of the Netherlands Foundation for Radio Astronomy (the governing body for the radio observatory Dwingeloo) in 1958–1964, the chairmanship of the Council of the Centre de Données Stellaires at Strasbourg, and a nearly four-year term as dean of the Groningen Faculty of Sciences. These were, on the whole, sunny years for the Dutch universities. But by the end of the 1960s the climate changed. Student rebellions against the traditional structure of the universities, preceded by student revolts in California and France in 1968 ("les événements du mai"), distracted staff from their regular

work and forced them to spend undue amounts of time on futile matters of administration.

8. ESO IN ITS YOUTH

In the spring of 1953, I witnessed Walter Baade and Jan Oort dreaming aloud about European astronomers' creating a powerful joint observatory—the dream that became the European Southern Observatory (ESO). Forty-six years later, in 1999, I attended the dedication ceremonies for ESO's Very Large Telescope (VLT) on Paranal, the world's most powerful optical telescope. I was fortunate to contribute to the early stages of the ESO project and have described developments up to the mid-1970s in my book *ESO's Early History* (Blaauw 1991b). Subsequently, I compiled and published the inventory of the archives of ESO (Blaauw 1992). I therefore make only some cursory remarks on the observatory here.

For several months in early 1953, I shared my office at Leiden with Walter Baade. Lively and inspiring (and rich in stories), he had been invited for the conference on galactic research to be held later that year (Blaauw 1955). He had made most of his successful career at Mt. Wilson Observatory, but, born and raised in Germany, Baade always strongly sympathized with European astronomy. These were still years of restoring Europe after World War II. Baade thought European observational astronomy should now attempt to rise to the level of its American counterpart. There was only one way: pooling manpower and funds. Later that year, I left Holland for Yerkes Observatory, but when I returned in 1957 I found that negotiations between astronomers and governments had been dragging. It took until October 5, 1962, for Belgium, France, Germany, the Netherlands, and Sweden to join forces and sign the ESO Convention, and Denmark soon followed. The United Kingdom, whose interest in the earliest stage had been voiced by Astronomer Royal Harold Spencer Jones, had withdrawn under his successor. I witnessed in these years the immense effort toward realizing the project by those whom we now justly call the founding fathers of ESO. Among the astronomers, the names of André Danjon (France), Otto Heckmann (Germany), Bertil Lindblad (Sweden), and Jan Oort (Holland) are the first to come to my mind, but also Paul Bourgeois and later Pol Swings from Belgium.

During the five years that elapsed before the convention was signed, ESO tested several sites in South Africa. I became heavily involved in these searches as an executive secretary to the ESO Committee, the precursor to the ESO Council; I describe them in detail in my book. Simultaneously with the last stages of these tests, short tests for comparison with Andes Mountains sites were conducted in Chile by André Muller, who had supervised the testing in South Africa. He did so with the strong support of Jürgen Stock, the pioneer for the Association of Universities for Research in Astronomy (AURA) in the Andes. The tests led ESO to choose the Andes in November 1963. By the middle of 1964, ESO's first director, energetic Otto Heckmann, had established ESO's presence in Chile. The agreement

with the Chilean government was at the level of the Ministry of Foreign Affairs of Chile, similar to that between the ESO member states. On some occasions this has proven very useful. By the middle of 1964, the time was ripe for making ESO astronomers acquainted with the sites for the observatory and the Santiago office. Flying on a French Caravelle, still limited to moderate altitude, we made the interesting detour via the Lower Antilles, reached the westside of the Andes via Bogota, and then descended toward Chile. My slides of that flight clearly show Antofagasta's natural harbor; little could I surmise that three and a half decades later, I would land there for ESO's dedications at Paranal.

Heckmann had selected La Silla from many possible sites, all of which were government property. We explored the mountain on horses and mules for areas to build the access road and water supply pipelines, we checked possible locations for drilling water, and we considered the possible layout for the telescope park, dormitories, workshop, etc. My patient mule Picasse carried me safely over the slopes and kindly gave in when I steered it in directions inspired by my curiosity, but it took matters firmly in hand when it came to choosing the safest path between the rocks down the mountain slope. Years later, as the work on La Silla progressed, the ESO Council occasionally held its meetings in Chile. Without these, many among the government delegates would have been unable to appreciate the outstanding pioneering work done there. The November 1972 visit of the council featured a meeting with President Salvador Allende, arranged by Bengt Westerlund, ESO's director for Chile (see Figure 2). During the approximately 20 minutes of our visit, the president, himself an academic, appeared keenly interested in ESO. Alas, within a year, the coup drastically turned our relationship with the new head of state into a strictly formal one. The political developments in Chile and their effect on the work of an international observatory would have been interesting themes to dwell on in these reminiscences, but I rather refer to the account of Victor Blanco, at the time Cerro Tololo's director (Blanco 2001).

For technical matters, Heckmann had acquired the help of astronomer Jöran Ramberg, a former associate of Bertil Lindblad. While in Chile André Muller supervised the logistics of the first construction phase, assisted by his staff of Chilean collaborators. This phase was crowned by dedication ceremonies on La Silla on March 25, 1969. At that time, the 1.52-m spectrographic telescope and the 1.0-m photometric telescope, as well as some smaller ones, were in regular operation; observations by visiting astronomers had started. Chile's President Don Eduardo Frei Montalva descended by helicopter, and Council members and invitees gathered in the dome destined for the Schmidt telescope to listen to the speakers, among them Heckmann, Council President Bannier, and Chile's Minister of Foreign Affairs Don Gabriel Valdés. I attended in my role as scientific director, and in a small plane from Santiago I accompanied Sweden's Minister of Education Olof Palme, who spoke on behalf of the governments of the ESO member states. His violent death, years later, deprived ESO of an ardent advocate of international scientific collaboration.

Yet there was not only admiration for what Heckmann had achieved, there also was uneasiness. The large telescope—initially planned for at least 3.0 m

Figure 2 A delegation from ESO visiting President Salvadore Allende in November 1972, from left to right: A. Alline, president of the ESO Council; the author; C. Zelle, president of the ESO Finance Committee; Bengt Westerlund, ESO director for Chile; and (from behind) Bengt Strömgren.

but ending up at 3.6 m—and the Schmidt, which were the nuclei of the ESO project, were lagging far behind schedule, and some of the supporting governments showed growing impatience and even doubts about ESO's mission. Heckmann had expected to do the engineering with only the help of the ingenious engineer Strewinski and his small staff, who had done an excellent job building the Hamburg Schmidt telescope—he seriously underestimated the staff needs. In retrospect, the scope of the ESO project was far beyond what collective experience of European astronomy had learned to handle.

9. ESO: TOWARD MATURITY

When in 1970 I succeeded Otto Heckmann, there was no mistake about my principal assignment: realizing these two telescopes. At that time, and throughout my directorate, the administrative services were still centered at Bergedorf, near Hamburg Observatory. We decided that, rather than looking for expertise in telescope building, we should call on scientists and engineers used to tackling projects of comparable size in costs and engineering challenge, whatever the nature of the instrument. Jöran Ramberg consulted several industrial firms, but these showed

little interest in an undertaking offering them no further perspectives. I consulted Hermann Bondi, at that time director general of the European Space Research Organization [the precursor of the European Space Agency (ESA)], who showed much interest in the project, but the gist of his advice was, "My engineers are unnecessarily expensive for you, for they are used to making things space proof, unlike your telescope, to which, if needed, your engineers can go with their screw driver." And then we were very fortunate to find CERN's Director General, Bernard Gregory, and his staff interested in collaborating. After preliminaries at council level, the formal contract was signed by Gregory and me on September 16, 1970, and already a few months later work on ESO's Telescope Project Division was underway at CERN under the leadership of Svend Laustsen. By the time I left ESO, the 3.6-m telescope was nearly complete.

As to the Schmidt, after Strewinski and his staff had brought the construction to a provisional conclusion, the finishing touches were applied under the supervision of André Muller—then back in Europe—and here, again, we greatly profited from the collaboration with CERN's engineers. A support telescope to our large reflector, it was first intended to perform for the southern sky the general survey similar to the one done by the Palomar Schmidt for the northern sky. But then, one day early in 1971, we were surprised to learn it would have company. The good news came from across the Channel. The UK Science Research Council (SRC) planned to build a Schmidt to be located in Australia, with similar ambitions. I spent a few moments wondering how we had remained so unaware of the UK plans and reminiscing about the good old days when, even after Great Britain had declined participation in ESO, we had always welcomed observers from the UK at our ESO Committee meetings (among whom I remember Hunter, Redman, and Hoyle). Then I approached the UK project leader, Vincent Reddish, for possible coordination of our programs. I found him most cooperative. It led to the ESO-SRC agreement of January 1974 by which the southern survey became a joint project, with ESO doing the "red survey" and the UK Schmidt the "blue survey" (after ESO had first done the "Quick Blue Survey" for reconnaissance). I have been criticized by colleagues for this breakdown of tasks, for the Brits' soon began showing off their spectacular photographs of southern celestial treasures, whereas ESO was slower with its survey. However, the choice was deliberate. The extremely demanding red survey required exposure times of some four hours per plate, whereas for the blue survey only about half that long sufficed; I was not sure that the Australian climate would allow completion of the red survey within a reasonable lapse of time, but I trusted that La Silla was up to it.

With the two Schmidts approaching completion, we became increasingly aware of the very demanding conditions required for producing the Sky Atlas. Essential for our work was the creation of our Atlas Laboratory at Geneva—again on the premises of CERN, adjacent to the TP Division, and with much help from CERN—where both the plates from ESO and those from SRC were processed and the Atlas prepared. The establishment and the performance of this laboratory has been an outstanding achievement, the significance of which has not always been fully appreciated. I want to mention in particular Richard West and William (Bill) Miller

of Pasadena. Richard had in 1970 become my assistant in scientific matters soon after I was appointed director general; he supervised the project from the moment of contact with the SRC. Without Bill's active interest, our project could hardly have been realized. At ESO's observational end, on La Silla the work was conducted by Hans Schuster and his Chilean collaborators. Hans had been a staff member devoted to the Schmidt project since he had joined ESO as one of Heckmann's first associates.

The strong support in Pasadena for our Schmidt project was only one of many instances when, over the years, ESO benefited from the interest and advice of our American colleagues. A first and crucial expression of American support for the ESO project had been the $1 million grant from the Ford Foundation which, as early as 1959, sped up the European governments' signing of the ESO Convention (Blaauw 1991b, Edmondson 1998). I well remember how, on my journeys between Chile and Europe, I used to pay visits to AURA's headquarters in Tucson. I enjoyed the counsel from their staff, particularly AURA's directors Nick Mayall and Leo Goldberg and their associates Art Hoag and David Crawford.

Twice or more a year, the ESO director met with ESO's governing body, the ESO Council. The Council consists of two delegates per member state, irrespective of the political or scientific weight of the state. ESO has been fortunate in having among these delegates some eminent nonastronomers, who must be regarded as much founding fathers of ESO as the astronomers already mentioned. Names that come to my mind are J.H. Bannier, M. Deloz, G. Funke, and C. Zelle, from Holland, Belgium, Sweden, and Germany, respectively, who not only paved the way toward government support in the difficult years when ESO was created, but also played leading roles as president of Council or of the ESO Finance Committee. Reminiscing on the spirit in which Council meetings were conducted, I realize how very different the situation was from that in the years after World War I, about which I learned when writing my book on the history of the IAU (Blaauw 1994). In those years, there had been among Allied scientists resentful denial of participation by scientists from the Central Powers in matters of international collaboration. Now, in post–World War II years, there was almost immediate resumption of relations as apparent in both the proceedings within IAU and ESO. Yet politics was not entirely absent, even at ESO Council meetings. Sometimes I sensed that, below the surface, a tiny bit of French-German warfare was still going on. It had to do with the delegates' professional background. Whereas most of them represented their ministries of science or technology, France had chosen its delegate from the Ministry of Foreign Affairs. There could be no doubt about this latter being a strong advocate of international scientific collaboration, and in particular of ESO, yet he was also clearly a representative of his country who would never lose sight of matters of national prestige—such as the language spoken at official occasions. I experienced his true, friendly nature while in his home, within the circle of his family.

The ESO Convention and the related Financial Protocol—the contracts between the member states on which ESO was based—required that the observatory be located in the Southern Hemisphere. They defined the initial instrumentation,

formulated rules for the Council's functioning, and stipulated financial arrangements. However, they left open several important questions, the answers to which ESO would arrive only by trial and error. What kind of observatory was it to be—one in its own right, comparable in status to the largest elsewhere in the world? Or would the observatory be more like the southern stations some of the European institutes had previously operated overseas, not a center of research but rather a place to collect observations to take home? The question became acute as the 3.6-m telescope approached completion. I considered it necessary for ESO to have an in-house scientific staff experienced in working with the telescope and preparing plans for the telescope's auxiliary instrumentation. In a late stage of my directorate, I incorporated a budget item for such a staff in my financial planning, Here, for the first time, the Council did not comply with my wishes; it judged one additional scientific post to be enough. However, the matter was settled for my successor, Lodewijk Woltjer, when the creation of a scientific in-house group became a *conditio sine qua non*.

Of all the problems the ESO directorate confronted, I always found those concerning its personnel the most difficult. The necessity of firing a staff member because of serious misconduct, as I had to do a few times, was an incomparably more miserable experience than any political or financial worry. Another problem regarding personnel management concerned the difference between the systems of rules and regulations for the personnel sent out from Europe and for those recruited in Chile, and the different salary scales for each. The different pay scales resulted from two issues: (*a*) for European personnel to work in Chile, we had to offer a sufficiently attractive salary, and (*b*) for Chilean staff we had to respect a certain equilibrium with respect to salary scales in Chile—which were watched by Chilean labor unions. This discrepancy was considered unacceptable by certain young, democratically minded visiting astronomers who accused me of objectionable discrimination. It gave rise to pretty violent discussions. At such moments another wise counsel from my Mennonite teacher came in handy: "On ne peut pas contenter tout le monde et son père."

An item that gave me much satisfaction during my directorship was ESO's help in creating the journal *Astronomy and Astrophysics*. The first initiatives had been taken by Stuart Pottasch, Anders Reiz, and Jean-Louis Steinberg, and in 1968 I succeeded in establishing a formal relation between ESO and the board of directors of the journal. Thus, ESO could offer its legal services for establishing the contract with the publisher (Blaauw 1975). We welcomed the first issue of *A&A* in January 1969. It was my pleasure to chair the board of directors from 1968 to 1979.

10. LEIDEN AGAIN

My five-year contract as director general of ESO, preceded by two years as scientific director, terminated on January 1, 1975. More than a year before the termination of my contract, the council had suggested that I continue for at least a couple

more years, but for a mixture of reasons I declined. I had given little thought, though, to my post-ESO position. Possible conditions for returning to Groningen had not been fully worked out when Harm Habing called to ask whether I was interested in a chair at Leiden. The prospect of spending my last years in office at Leiden appealed to me and—after some soul searching—I accepted.

Leiden had changed more than I had anticipated. Of course, the dominating leadership of Jan Oort had gone since Jan had retired in 1970, although his presence and scientific inspiration were still evident. New leadership had not yet fully crystallized, although there was no question about the eminence of the scientific but somewhat reticent authority of Henk van de Hulst. Harry van der Laan breathed new scientific life into the institute with numerous graduate students, and he reorganized the radio astronomical set up in the Netherlands. Moreover, the wave of unrest and democratization that had upset university life in the late 1960s had not quite subsided. Furthermore, we were in the aftermath of the department's drastic move from the old observatory premises to a new, plain office building—a move imposed upon the astronomical community by the university administration that had deeply hurt many of the observatory's staff, including, as was well known, Jan Oort. But then, for me, teaching and contacts with a new generation of students eager to pursue research created new challenges and friendships. Also, not surprisingly, administrative and organizational chores were in store. A modest external task was the presidency of the IAU from mid-1976 to mid-1979 (for which, in the ruling spirit of democracy of those years, I was supposed to ask the institute's permission). And then, by the end of the 1970s, there was my growing involvement in the fascinating *Hipparcos* project to which I also return below. A major task became my chairmanship, in 1978–1984, of a newly founded organization called ASTRON that was to function as an intermediary between the Dutch national science foundation and the astronomical community. It played an important role whenever measures involving Dutch astronomy as a whole came up for discussion. Thus, it acted on behalf of Dutch astronomy when, between 1979 and 1981, the UK-NL agreement for common use of telescopes on La Palma was prepared.

11. INTERNATIONAL ASTRONOMICAL UNION

Among the pleasant souvenirs decorating my study is a unique photograph of nine cheerful IAU general secretaries and myself (president 1976–1979) taken at the occasion of the 1994 IAU General Assembly in The Hague (see Figure 3). The secretaries are, in the chronological order of their terms in office: Jean-Claude Pecker (1964–1967), Edith Müller (1976–1979), Patrick Wayman (1979–1982), Richard West (1982–1985), Jean Pierre Swings (1985–1988), Derek McNally (1988–1991), Jacqueline Bergeron (1991–1994), Immo Appenzeller (1994–1997), and Johannes Andersen (1997–2000). [Still among us at the time the photograph was taken, but unfortunately not present for the snapshot, were Lubos Perek (1967–1970), Cornelis de Jager (1970–1973), and George Contopoulos (1973–1976).] The photograph reminds me of my involvement with IAU over many years and

Figure 3 Nine IAU general secretaries at the General Assembly of 1994 at The Hague, from left to right: Immo Appenzeller, Jean-Claude Pecker, Richard West, Edith Müller, Patrick Wayman, Jacqueline Bergeron, Johannes Andersen, Jean-Pierre Swings, Derek McNally. In front, the author. (Photo courtesy of J.-C. Pecker.)

of its hard working general secretaries whose devotion to IAU maintained its authoritative position in astronomy for more than 70 years. Another photograph reminds me of the opportunity I had to convey congratulations, on July 27, 1978, to an audience at the 100th anniversary of the Observatoire du Pic du Midi, at an elevation of 2860 m, surrounded by the snowy peaks of the Pyranees. I attended many of the General Assemblies, my first at Stockholm in 1938, my most recent at Manchester in 2000, and in between 13 others.

At the General Assembly of 1994, IAU celebrated its 75th anniversary. This had induced General Secretary McNally to ask me to write IAU's history, and as a result my *History of the IAU* (Blaauw 1994*)* appeared, just in time for the assembly. For referring to the documentation of the IAU archives I created an inventory and a classification system, resulting in my booklet *Archives of the IAU* issued by IAU (Blaauw 1999). In November 1998, through the intermediary of Jean-Claude Pecker, the IAU archives were considered worthy of incorporation into the archives section of the Académie des Sciences in Paris, where they can now be consulted upon authorization by the secretariat of the IAU. They are of considerable interest for students of worldwide collaboration in science in the twentieth century.

While writing my book, I encountered some critical stages in IAU's history. I witnessed Kapteyn's desperate but futile attempts after World War I to sustain international collaborative projects to which he had contributed so much himself. I came to understand why, in spite of strong efforts toward reconciliation—including the attempts by Willem de Sitter mentioned before—it was only after the Second World War, in 1953, that Germany became a regular member of IAU. I learned about the cautious, diplomatic moves made by IAU President Harold Spencer Jones and General Secretary Jan Oort, aided by leading astronomers like André Danjon, Harlow Shapley, Colonel Stratton, Bengt Strömgren, and Otto Struve, immediately after the Second World War toward restoration of the functioning of IAU. And I came to admire the courage of IAU President Bertil Linblad and General Secretary Bengt Strömgren, who early in 1951, only six months before the General Assembly scheduled for Leningrad, felt that they had to propose to the Executive Committee postponement of this assembly because of the rising tension of the cold war—a decision deeply regretted by Soviet colleagues who had made immense preparations.

During my term as president, I collaborated with General Secretary Edith Müller and Assistant General Secretary Patrick Wayman, whose memory I cherish with warmest feelings. As one of our principal assignments, we considered the restoration of the IAU membership of mainland China. This had been a matter of deep concern also for my predecessor in the presidency, Leo Goldberg, but then—with the Cultural Revolution in China not yet over—the time for fruitful approaches was not ripe. China had been a member of the IAU since 1935 but withdrew in 1961 because the IAU granted membership to Taiwan. We wanted both parts of Chinese astronomy among our membership, and we were able to complete this task successfully during the Montreal General Assembly in 1979, after diplomatic work by Patrick Wayman in China. It was one of those rare occasions when I felt so much was at stake that not only the efforts of the general secretary but also the full weight of the presidency had to be flung into the battle—and a battle it was. Thanks to mainland China's accommodating attitude, we were in a position to negotiate with a delegation from Beijing present (but not officially participating) at Montreal during the assembly. Fortunately, the Chinese delegation was headed by my old acquaintance, Professor Chang Yu-Cheh of Purple Mountain Observatory, Nanjing, with whom I had spent pleasant months at Yerkes Observatory in 1947–1948. I was deeply satisfied to submit, at the concluding session of the General Assembly, the terms of our agreement, which opened the door for full membership of China. After the terms were approved by the Chinese authorities and the finishing touches applied by the next IAU team with Vainu Bappu as president and Patrick Wayman as general secretary, mainland China and Taiwan became distinctly present in the membership list of the IAU under the common entry CHINA. The agreement has now withstood the shifting political winds for 24 years.

My battle with Chang had a very pleasant sequel seven years later. At the occasion of the IAU symposium on neutron stars in Nanjing in May 1986, I received a joint invitation from the Chinese astronomical institutes to visit them after the

meetings. It resulted in an unforgettable four-week tour among the observatories of Nanjing, Shanghai, Kunming, Xian, and Beijing, partly by plane, partly by train. I visited many of China's fascinating cultural monuments, including the impressive underground terra cotta army of Emperor Qin Shi Huang near the observatory of Xian. At Nanjing, I had the great pleasure to meet Chang again. He was frail and in poor health; it was a moving encounter. He died not long afterwards and lives on in my memory as the personification of Chinese courtesy and dignity.

12. *HIPPARCOS*

On August 8, 1989, at Kourou, French Guyana, I watched the launch of the Ariane rocket that put ESA's astrometric satellite *Hipparcos* into orbit. The countdown for the launch seemed to run satisfactorily and the *moment suprème* was approaching, but then, close to ignition, it stopped. The crowd feared the worst. We were told—if I remember well—about a technical problem hampering the complete release of the arms that had embraced the satellite for the last-minute supply of superexplosive fuel. The tension rose. Then the incredible happened. The source of the trouble was located—and remedied. Countdown restarted, this time successfully. *Hipparcos* was on its way, and the crowd applauded. It soon turned out, though, that because of trouble with the apogee boost motor, *Hipparcos* did not settle in the intended geostationary orbit. An elliptic orbit was inevitable, and it took the ingenuity of the *Hipparcos* team to keep the satellite reasonably free from the Van Allen Belt. We now know that the handicap of the wrong orbit has been more or less compensated by the lengthening of the time *Hipparcos* was kept operational. At the May 1997 symposium at Venice, some 200 participants testified of the immense effort of those whose vision and perseverance made *Hipparcos* possible, and of the wealth of new results (Bernacca & Perryman 1997).

My own modest role had been the chairmanship of the Scientific Proposals Selection Committee (1982–1989). The observing schedule for *Hipparcos* had to be fixed in advance, with very little room for ad hoc adjustment. The more than 200 proposals contained 210,000 objects, but *Hipparcos* could accommodate no more than 120,000 of them. Moreover, we faced such issues as even distribution over the sky and limitations in apparent magnitude. Our recommendations and these restrictions led to the *Hipparcos Input Catalogue* compiled by the *Hipparcos* INCA Consortium under the leadership of Catharine Turon. The significance of their work can hardly be overestimated. To estimate the approximate positions of the targets at the epoch of observation, the consortium organized new wholesale programs for photographic positions and proper motions, as well as extensive programs for obtaining the accurate apparent magnitudes required for planning the observations. And, on top of all this, it solved a nasty problem. Over the years, astronomers had made quite a mess when identifying and cataloging astronomical objects. The same star occurred under different names or numbers in different proposals. For the Input Catalogue consortium, how could one know that star BD

xxxx listed in proposal X was the same as star HD yyyy listed in proposal Y, and also identical to star GCVS zzzz in proposal Z? Sorting out this mess was one of the first achievements of the INCA Consortium. Having started out with more than 500,000 targets listed in the proposals, INCA ended up with the 210,000 different objects already mentioned.

Hipparcos had met considerable doubt before it gained mission approval from ESA in March 1980. This melted away when it was understood that the project would not serve purely astrometric interests but, in a much wider sense, would provide a new, rigorous foundation for that broad range of topics in astrophysics and galactic structure for which precise luminosities and kinematical constants—and, hence, exact distance measures—are essential. A fine example is the extremely accurate color-magnitude diagram of the Hyades based on the new secular parallaxes of its members (De Bruijne et al. 2001). For modern students of stellar motions, who simply read a star's proper motion from the *Hipparcos* catalog, it is difficult to appreciate the difference *Hipparcos* made for the old guard. In the early days, to study stellar motions properly, I had to acquaint myself with the role of the possible systematic errors in the fundamental catalogs as well as with the adopted corrections to the values of the constants of precession, and with possible magnitude effects. All these worries melted away. Stepping over from wobbling Earth to stable *Hipparcos* was one of the great feats of twentieth century astronomy. Therefore, we owe a great debt to the many names that mark its appearance, including Pierre Lacroute, who came up with the idea (Bacchus & Lacroute 1974, Lacroute 1975), and Michael Perryman, the project leader who supervised its realization.

13. ADVENTURES IN THE DOMAIN OF HISTORIANS

Let me conclude these reminiscences with a topic completely outside astronomy. Forty years ago, my wife and I acquired at a public sale a centuries-old farmhouse near Groningen and decided to make it our home (see Figure 4). The roomy house and its annexes were in a wonderful state of neglect and cried out for restoration. In 1980, I published two lengthy papers on its history (Blaauw 1980b). A few years later, I wrote along the same lines on neighboring ancient farmhouses (Blaauw 1983), and then, feeling well at home in this genre, I tackled the problem of the collective history of the small neighboring village of Westervelde (Blaauw 1987). Recently—*on revient toujours á son premier amour*—I published a note on the antique tiles in my own farmhouse (Blaauw 2003). By raising my hand at the public sale, I had opened the door to fascinating adventures in the domain of historians.

We had the house restored under the supervision of the governmental service for protection of monuments before we moved in. And then, unavoidably, we had questions about who had lived there, how they had lived, and how the farmhouse, over the centuries, had obtained its structure. My curiosity was enhanced by the findings of a carbon-dating colleague of the Physics Department: The most prominent beam structure of the house dated from about 1570. The search for answers led

Figure 4 Jan and Mieke Oort, on a bicycle trip with their grandsons, Marc and Maarten Oort, visiting us at the farmhouse in July 1974. At the center, my wife Atie.

me to the state archives, where I received helpful guidance from their staff when I searched through their rich collection of documents. Most prominent among these were a great variety of tax registers; here were the clues I sought. Professional historians are primarily engaged in the main courses of history and not so much in an individual farm. I had to do the job myself and started it, hesitatingly, during weekends in the late 1960s. Sometimes, during my many travels for ESO, I took along copies from the archives so that, during long hours in hotel rooms or on planes, I could try to decipher seventeenth century handwritings and acquaint myself with the intricacies of farmers' daily lives in former days. I shall refrain from describing the results except to mention that professionals (notwithstanding

an accompanying mathematical formula!) appreciated my insights into the seventeenth century system of taxes pertaining to the farmers' shares in the common property. I experienced all this as a fascinating adventure outside the astronomy field. I had a taste of the—definitely not stuffy!—flavor of the true historian's workshop; I became a mini Le Roy Ladurie, the well-known author of *Montaillou*, and a modest volunteer in the historians' school of the *Annales*.

What made it so fascinating? For one thing, in this kind of research, there is the human element: those farmers of centuries ago, fixing among themselves the rules for the use of the common heath and woods, the tax collectors recording their various possessions, their quarrels, neatly recorded by the local magistrate. But then, it struck me that there is much in common between what incites both scientists and historians: the observational data—whether lists of stellar proper motions, photometry, and radial velocities, or tax collectors' lists, genealogies, and letters—invite one to play with them, to find out what they have to tell, to create an insight, however modest, that did not exist before.

My excursions into the domain of historians had interesting consequences. I was considered by fellow villagers to be the natural chairman of the restoration committee for the old village church, and when the governing board of the province of Drenthe installed an advisory board for its museum of local history, I became the representative of the owners of historical monuments, and soon thereupon its chairman, a task I performed with great pleasure for more than 15 years.

14. LOOKING BACK

I realize now that my visit to De Sitter 70 years ago led to a cruise through the world of astronomy that evokes dear memories. Since my retirement in 1981 at Leiden—where it all began in 1932 and where I also made my in-between stop in 1945–1953—I have enjoyed the fine hospitality of the Kapteyn Institute: a job without obligations and without pay, the most comfortable one I have ever had, and the longest—22 years now. I am beyond the range of citation indices, indifferent to referee reports, but happy once in a while to write one myself, and grateful for the awards and honors I have received. When in 1961 I received an honorary degree from the University of Besançon, and in 1963 was elected a member of the Dutch Academy of Sciences, I felt deeply honored, but I was inclined to conceal these honors—a delicate subject—from colleagues and collaborators. I have come to realize that this was not right because it negates the effort made by those who wanted to see the award granted. It gave me a strong sense of satisfaction when in the last years of my ESO directorate and those following I received foreign academy memberships or honors of a different nature from nearly all of the then–ESO member states and from elsewhere. The Academia Europaea at its creation took an interesting shortcut: it made me an emeritus member right away. Continuing friendships remain from my years in the United States, as do cherished personal ties from those at Bergedorf; I have nostalgia for Chile, Kenya, and South Africa;

Paris reminds me of the IAU, of *Hipparcos*, and of many other pleasant duties. But most rewarding of all is, of course, that as I near my 90th birthday, sitting at home at my PC, faithfully cared for by my wife, with friends helping to keep me within the limits of space (and truth!), I can reminisce on the more than 70 years that have elapsed since De Sitter's encouraging nod.

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