

***EXTRACT FROM***  
**A PERSONAL HISTORY OF**  
**H.M. NAUTICAL ALMANAC OFFICE**

30 October 1930 - 18 February 1972

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## **PART 4: AT HERSTMONCEUX 1949 – 1972**

### **CHAPTER 12**

#### **Early years at Herstmonceux: 1949 - 1951**

##### **Move from Bath to Herstmonceux**

The move from Bath to the Royal Greenwich Observatory at Herstmonceux was made during the weekend of 7-10 October 1949, from the Friday to the Monday, with remarkably little trouble. The actual physical transfer was most efficiently handled, and everyone was unpacked and installed in a very short time. A British Railways transport collected our things in Bath (including all the desks, tables and other furniture that we had man-handled in the move from Greenwich to Bath) on Friday and they were delivered at Herstmonceux the following Monday. Most of the staff travelled by train from Bath on the Monday and we did some work on the Tuesday. Miss J. E. Perry, my secretary (from Bath), as usual did a fine job and played a not inconsiderable part in the planning and execution of the move.

Ten of the Greenwich staff moved to Herstmonceux, although S. G. Daniels had heart trouble and resigned shortly after the move to live at Sandown in the Isle of Wight. He continued, however, to proofread for the Office for many years. His brother, A. J. Daniels, stayed on as a temporary S.E.O.; he lived in Eastbourne and continued to give valuable service until he retired in late 1951; he also continued to proofread for many years afterwards. Porter, Candy, Hulme, Taylor and Misses Fooks, Gibson, Grove, Hawkes, James and Iris Restorick, who were recruited in Bath accompanied us. Miss Histed had died during the war, Miss Reddy had transferred to the Admiralty on promotion, Misses Simm and Mounteney had transferred back to Greenwich during the war, and marriage had eliminated {!} Misses Hitches, Ifield, Pullen and Scadeng and all of the ANTs.

I had arranged, at Clemence's invitation, to visit the U.S. Naval Observatory shortly after the move, and so I, personally, was concerned with the trip to the United States, on which I left only a few days after the move, but before (if I remember correctly) I was able to unpack and sort all my papers. I did not return from the U.S.A. until mid-November, by which time all (or, at least, most) of the difficulties at Herstmonceux had been sorted out.

##### **Early days at Herstmonceux**

The winter of 1949–1950 was terrible — probably the wettest winter on recent record. The initial conditions at Herstmonceux were poor, and the post-war shortages made it impossible to make adequate provision for the staff quickly. The huts on either side of the south courtyard were, however, ideal for our office requirements and we settled in rapidly. The canteen in the Castle was also good, but living and travelling conditions were poor.

Most of the senior married staff and some unmarried staff managed to find permanent or temporary housing in Eastbourne, Hailsham or Bexhill-on-Sea, but the

less-well paid staff had difficulties. The original proposal to accommodate *families* in the huts (which were then around what is now the South Courtyard) was obviously unacceptable. I can remember the A.R. complaining to the Admiralty about conditions and stating his view that they were completely impossible for families as the Admiralty had proposed; he was, I think, reprimanded for this, but the proposal was not carried through. There was at that time some opposition to the idea of a hostel for staff, but the huts were, however, used for single rooms for male staff. The servants' bedrooms in the attic of the North Wing of the Castle were used for unmarried ladies. The Hostel Warden was Mrs E. Ramsey and her assistant was Miss S. C. Chapman. A flat was made in the Castle for Mrs E. M. P. Marples, who was appointed as canteen manageress and supervisor of the hostel in 1950–1951.

Many of the junior staff were, however, recruited locally, but the transport arrangements left much to be desired. Petrol was still rationed, and very few of the staff had cars. The Observatory ran 'transport' to and from the village and Pevensy Bay Halt, but it consisted of a canvas covered lorry with loose wooden bench seats, with no lighting inside and, of course, no heating, and entry and exit were over the tailboard. Nevertheless, the staff accepted the discomfort not only of the transport, but also of the general living conditions in the Hostel. It was perhaps the austerity, and possible hardship, of these conditions that led to a truly remarkable sense of comradeship among the staff of the whole Observatory, who, after all, were comparative strangers. The solar and administrative staff were from Greenwich and Abinger, the Chronometer Dept. staff were from Bradford-on-Avon, and the N.A.O. staff were from Bath.

On my return from the U.S.A. (not having made any definite arrangement for accommodation before I left) I asked for temporary accommodation in the Castle for a few days until I could find rooms. I was told, rather bluntly I thought, that this was not the purpose of the Castle and I was given (for some reason which I did not appreciate) a limited period of a week to stay in the Castle and I must then find alternative accommodation, or stay in the Hostel in a hut. I stayed in the Hostel for only a few nights as I soon found excellent accommodation in Eastbourne, where I stayed until 1954. (My landlady, Mrs. Delaney, remained a good friend of ours.) Fortunately I had a car, which was a rare luxury in 1949, so that I was not compelled to rely on the R.G.O. transport to and from Pevensy Bay Halt.

{ Council houses had been built for the Chronometer Dept. staff on the Fairfield estate in Herstmonceux, but the houses on the Denefield estate were not ready when the N.A.O. moved and some staff had to wait up to 6 months. — Ed. }

### **Administrative arrangements**

In spite of the move, the Office retained its separate identity, except in so far as administration and accommodation were concerned. It continued to have its own Navy Vote, its own complement, and its own secretariat and library. There was little astronomical contact with the other departments at Herstmonceux, which, at first, had few interests in common — the Meridian, Time and Astrometry Departments did not move until later. (But as mentioned below, there was much friendly contact between the staffs.) Spencer Jones did not concern himself much with the work of the Office, or the content of the Almanacs, though I consulted him on all major changes. He did, however, try to persuade me to carry out a discussion of the international latitude observations from the point of view of deriving a better observed value of the constant of nutation. For many years I saw the dust slowly accumulating on the many volumes of

the published results of the International Latitude Service, and from time-to-time sketched out the required programme of analysis, including the frightening task of correcting all the observations for instrumental and personal errors, as well as for changing star places. I am sorry to say that I never did get down to it. It would have been a major undertaking requiring not only much calculation (which could have been delegated), but also much research into the observational techniques of which I was quite ignorant.

The first step was to fill the remaining vacancies in the staff. We had recruited as many as possible before we left Bath. We had been fortunate to obtain a recommendation from Professor Smart (who taught me astronomy in Cambridge) of two girls who had been taking astronomy under his supervision in Glasgow. Miss McBain went to Glasgow from Bath to interview them, and so we recruited two Assistant Experimental Officers, Miss Gibson (now Mrs Wayman) and Miss James (now Mrs Jarrett). In spite of our 'recruitment' programme, the actual staff in 1950 was less than the approved complement, particularly in the grades of S.O., A.E.O. and Assistants (Scientific). The standard of the locally recruited junior staff, the Assistants (Scientific), was exceptionally high, presumably because of the 'glamour' of the Castle and of astronomy, but also because there was no other outlet in the neighbourhood for school-leavers with mathematical and scientific interests. In those days there was not an almost automatic university entrance for those who stayed to take the higher level leaving certificates. Barry, Green, Harragan, Miller, and Misses Barton, Crisford, Crowley, Grogan, Knight and Nevell were recruited during the first two years. We were fortunate in getting later, in October 1951, G. A. Wilkins as an S.O..

N.A.O. was, for a long time, by far the largest department in the R.G.O., and there was no transfer of staff. There was a rather rigid rule on the relative proportions of staff in the various grades, e.g. P.S.O. : S.S.O. : S.O. and S.E.O. : E.O. : A.E.O., and consequently much interest in prospective vacancies in the higher grades, particularly in the S.E.O. posts. But there was a sharp distinction between the classes of S.O., E.O. and S.A. so that promotion from one class to the other was difficult.

I had tried to recruit staff so as to give a reasonable career structure within the N.A.O. itself, though all that one can do is to try to ensure that the organisational structure is such that no blockage of promotion will occur under normal expectations. The Office staff was, however, far too small to attempt to achieve a stable population. R.G.O. was not so well placed, and there were difficulties, but Spencer Jones did not attempt to fill N.A.O. vacancies with R.G.O. staff, even though in some cases they were of greater seniority (though not necessarily of greater suitability) than the N.A.O. staff. It was not until much later, after I had proposed that Harding should be promoted to S.E.O. and transferred to the Astrometry Dept (under Dr. A. Hunter), that interchange of staff (other than junior staff) became possible. My proposal (based on the fact that we could not provide Harding with an S.E.O. post, or with work suitable for his outstanding qualities) was not well received at first, but I persisted and it was accepted. No-one has had any reason to regret that decision.

### **The start of the R.G.O. Club**

The atmosphere at the R.G.O. was remarkably good, considering the conditions; everybody knew everybody else and, in spite of the practical difficulties, a Sports and Social Club was formed. It was centred on one of the huts, in which all people gathered at lunch times, and in the evening for the staff living in the Hostel; it had a billiards

table, darts, table-tennis etc.. The Club was started by members of the Chronometer Department, who came down from Bradford-on-Avon in 1948, and of the Works Dept. from Chatham. Initially it was organised largely by A. Shortland of the Chronometer Dept., but N.A.O. staff played a large part in its subsequent running and development. Joan Perry took over the secretaryship from A. Shortland and held the post for many years. Norman Rhodes (of the Solar Dept., who later married Iris Restorick but died tragically young) was chairman of the Club for many years. N.A.O. staff, particularly Harding and Smith, and later Wilkins, were instrumental in bringing a very high standard of enthusiasm and organisation into the Club. That was the time when there was a children's Christmas party (with tea in the Castle and presents given out in the Staircase Hall by the A.R. as Father Christmas), followed by a pantomime, and by an evening Club party. The Club also entered a float in the Herstmonceux Bonfire Society parade and organised numerous parties and dances.

The annual Club parties, with the traditional pantomimes, were attended by almost all the staff and their families, and the spirit was excellent. Four of the N.A.O. girls, Mavis Gibson (Mrs Wayman), Evelyn Grove (Mrs Green), Angela James (Mrs Jarrett) and Audrey Nevell (Mrs Candy) usually took leading parts in the pantomime; Audrey, who had been recruited locally, was an outstanding singer. All four subsequently married members of the R.G.O. staff! [P. A. Wayman became Director of the Dunsink Observatory and Assistant General Secretary of the I.A.U.; J. S. A. Green became a meteorologist in the University of East Anglia; K. E. Jarrett served in the Chronometer Dept for many years; Mrs Jarrett later became a teacher of mathematics in a girls school in Eastbourne; M. P. Candy (one of the Bath contingent) became Director of the Perth Observatory in Western Australia.]

[This is not part of the history of the N.A.O., but I think that it is relevant in that members of the N.A.O. played a significant part in the organisation of the Club. Others can cover these unofficial, but none the less important, activities far better than I can; I did not personally contribute much to them.]

### **Work of the Office**

I cannot now recall the work of the Office in any detail or in any reasonable order, except that, although there was a seemingly adequate number of staff and all worked hard, little new work was done. Perhaps more should have been done, but as will be seen there was much to do merely to make good the deliberate run-down of 'routine' work during the war. 1950 and 1951 were years of considerable activity, especially in the cooperation between the U.S. and H.M. Nautical Almanac Offices.

### **Visit to the U.S.A. in 1949**

The principal justification for my visit to the U.S.A. in 1949 was to attend the first meeting of Working Party 53 of the Air Standardisation Coordinating Committee (A.S.C.C.), which was held in Washington. Clemence and I were the representatives of the two Nautical Almanac Offices on the Working Party, which included representatives of the R.A.F., the U.S.A.F. and the R.C.A.F.. The R.A.A.F. joined later. We were on to represent astronomical navigation. The papers were confidential, the Admiralty insisted that I travel by sea. This was approved by the Air Ministry and so I and the two R.A.F. representatives travelled on board the Queen Elizabeth; all the papers were deposited with the purser.

The meeting of the Working Party was concerned with the initial plans for H.O. 249, and need not be dealt with here. As on later occasions we only attended on at most 2 days. But I spent most of the time at U.S.N.O. with Clemence, discussing all aspects of the work of the two offices. Much of our discussion centred on two quite distinct problems: the possible revision of the system of astronomical constants, and the provision for astronomical navigation at sea and in the air. We also covered many other matters of mutual interest, with a view towards achieving greater coordination and cooperation. Details of what Clemence and I discussed, and certainly the decisions that we came to, are in the files.

Our discussions on tables for air navigation were largely held in consultation with the Hydrographic Office. The U.S. Naval Observatory calculated values in H.O. 249 and printed them on the card-controlled typewriter; we were all interested in the layout of the pages. The N.A.O. proofread the sheets, in preparation for the British edition A.P. 3270. I think that Clemence and I also selected stars for the *Air Almanac* (certainly prepared for me by Scott) that are used in H.O. 249. The Hydrographic Office was mainly concerned with the choice of the 6 stars that were tabulated in Vol. 1. (7 stars were given in subsequent volumes.) The N.A.O. also prepared the table for the correction for precession and nutation that appears in Vol. 1; we also suggested the table of G.H.A. Aries that appears in A.P. 3270.

We discussed the *Abridged Nautical Almanac*, and its redesign in 1952, and compared it with the *U.S. Nautical Almanac* that was prepared on the card-controlled typewriter. It was slightly different though based on the same principles. A similar comparison was made of the two *Air Almanacs*. A difficulty was that we did not have a card-controlled typewriter; moreover, there was trouble between I.B.M. and B.T.M.C..

Clemence amplified the momentous (I do not think the word is too strong) proposal that we should unify the *Air Almanac* and the *American Air Almanac*. This was a proposal from Clemence that at first rather shocked me, but in the course of one day we worked out the difficulties. There were a number of significant differences between the two almanacs that had to be ironed out. Due to his broadmindedness, and absolute reliability — if he promised anything it would be done — we reached complete agreement on all important matters, and on almost all details. We subsequently published a joint article in the Navigation journals and prepared a draft report for the meeting of Working Party 53 in Montreal, where the final details were agreed.

As I have mentioned before { see chapter 11 }, we had received approval from the U.S. Hydrographer for the photographic reproduction of H.O. 214; but, owing to some changes in the usages between the two countries (and my dislike of the introduction of H.O. 214) we agreed we should have our own explanation, illustrations and tables. We proofread all the originals and made many corrections.

We also discussed the paper on fundamental constants that Brouwer and Clemence had written while not knowing that Danjon would be calling a conference in Paris in April 1950. This was during an informal annual meeting of the eastern group of astronomers, probably at Yale.

Clemence and I visited his family house in Johnstone, Rhode Island, where he was born. His mother was then still teaching in the local village school, and his elderly aunt 'ran' the house.

The important concern of my visit was the computing aids that we should apply for while bearing in mind that we should be cooperating with the U.S.N.O.. There was at this time a reciprocal agreement between I.B.M. and B.T.M.C. (Hollerith) by which each could sell the other's products. The I.B.M. model 602A was in use at the U.S.N.O., and was a great advance over the multiplying punch that B.T.M.C. had to offer. In particular, I was much impressed by the card-controlled typewriter, for forming copy for direct printing by photography. On my return to England we obtained permission from the Admiralty to order from B.T.M.C. an I.B.M. 602A, to supplement the tabulator, reproducer and other auxiliary equipment. A room for the installation was provided in the hut opposite to that occupied by the N.A.O., on the west side of the car-park.

Shortly after we had settled down at Herstmonceux, we discovered that the arrangements between B.T.M.C. and I.B.M. were now terminated and there was bitter competition between the two companies. The B.T.M.C. machines were installed by early 1951, but we had considerable trouble in getting the 602A from B.T.M.C.. It came with only one plugboard and it took much longer to get a second one and the division relays from I.B.M.. The 602A used a separate plugboard on which could be set connections representing multiplication, division, addition, and subtraction; the second board was a necessity. But it was an excellent machine that gave us little trouble.

### **Conference on Astronomical Constants in 1950**

The next important assignment for me was participation in the International Conference on The Fundamental Constants of Astronomy in Paris in April 1950. This was organised by Professor A. Danjon in recollection of the previous conference on fundamental stars, which was held in Paris in 1896, and at which Newcomb's constants were adopted. I must admit that I did not have much knowledge of this subject at the time and I did not contribute a paper. But the discussions I had with Brouwer and Clemence in 1949 prepared me for the discussion on the departure of Universal Time from a uniform time-scale. With Danjon, Jeffreys, Spencer Jones, Brouwer, Clemence, ... taking part, I was relatively junior (in authority if not in age) and was immediately appointed secretary and reporter. I did not play a large part in the discussions, and there was much that I still do not fully understand.

I learnt a lot from that conference, including how to draw up resolutions. On the morning of the final session Clemence approached Spencer Jones (after breakfast, just before leaving the hotel in which we were all staying) to find out what resolutions he was proposing for discussion at the conference. Spencer Jones had been asked by Danjon to consult with other delegates and submit a number of draft resolutions. He had, apparently, not done so; Brouwer and Clemence had not been consulted. In about ten minutes (all the time we had) Clemence pencilled in the six draft resolutions - which were, in principle, finally adopted. They had already been discussed in general terms, but this was a fine effort.

The main recommendations of the conference were that the existing constants (dating essentially from 1896) should continue to be used unchanged, and that Ephemeris Time should be introduced. It was defined in terms of a formula for the difference between Ephemeris Time and Universal Time. There were several aspects of the proposal to introduce Ephemeris Time that might (with hindsight) have been more fully investigated if I had asked for explanations of some of the points that I did not understand. Some were due to my ignorance, but one or two were, and perhaps still are,

significant. It must be remembered, however, that the original definition of Ephemeris Time was essentially an operational one, designed as a practical working convenience, rather than a carefully-planned, precisely-defined, fundamental time-scale. I wrestled with many of the unanswered questions in writing up the proceedings of the conference; the consequences were set out in my article on Ephemeris Time in *Occasional Notes*. The subsequent development of Ephemeris Time into a precisely-defined time-scale is well known, and I will only repeat that, unfortunately, several mistakes (errors of judgement might be a better term) were made which should have been avoided. I do NOT exonerate myself from blame. The most serious are the ambiguity in the definition of the zero point (due to the implied use of  $20''.47$  for the constant of aberration) and the underestimate of the uncertainty of the constancy and value of the secular acceleration of the Moon, though this does not affect the definition, only the realisation. Nevertheless, I still think that the concept and definition of Ephemeris Time are sound. Incidentally, it must be made clear that the name 'Ephemeris Time' is a 'proper' name, chosen by the 1950 conference from among a number of suggestions, such as: Newtonian Time (second in the opinion poll) and Gravitational Time. It was not, and is not, 'descriptive' in the sense that it was chosen because the time-scale would be the independent variable of the ephemerides.

There were two other recommendations of great significance: that the ephemeris of Mars be based, in the future, on Clemence's new theory, and that the ephemerides of the five outer planets be based on the numerical integrations made by Brouwer, Clemence and Eckert. Two other recommendations, of perhaps even greater interest, were essentially a consequence of the adoption of Ephemeris Time, namely that:

the empirical term be omitted from the lunar ephemeris, and be replaced by a correction to mean longitude; and

the second be defined by the formula for Ephemeris Time (rather than by the expression for mean solar time).

The recommendations were addressed to the I.A.U., and the Office was asked to report on the practicability of the introduction of the corrections to the lunar ephemeris, as a guide to a decision by the I.A.U..

### **Follow-up and other activities**

After the Paris conference I was faced with the task of writing a report on the conference, including details of the discussions. Fortunately, Clemence came back to England with me and insisted that I do it immediately. I did so with his help and encouragement. I also had the help of Professor Fayet (director of the office of the *Connaissance des Temps*) who translated my English into French and the report was published in *Bulletin Astronomique*.

These resolutions were to have a marked effect on the work of the Office. I wrote an Amendment to the Lunar Ephemeris detailing the steps required to amend the current ephemeris (which was then completed up to the year 2000) from Brown's Tables for the effect of the resolutions. Fortunately, we were not called upon to make complicated corrections.

The second meeting of W.P. 53 was held in London in the spring of 1950, when approval was given to the layout of H.O. 249. This was the real reason for the visit to England by Clemence.

### Vacation students

Our first ‘vacation student’ at Herstmonceux was Mary Almond and the second was J. M. A. Danby, a student of H. H. Plaskett’s at Oxford, who spent six weeks in the summer of 1950 in the N.A.O.. He was interested in celestial mechanics and worked on a number of problems (comets) with Porter; but his main ‘education’ was certainly in the field of numerical computation. Danby is an accomplished musician who played, for some time, with the London Symphony Orchestra. He chose, however, to make astronomy his career; he has made many contributions to celestial mechanics and has written a book on the subject. He became senior Professor at Raleigh University in North Carolina, U.S.A., where he, his wife and family play a leading part in the community. The R.G.O. itself had a ‘vacation student’ at the same time, namely, V. C. Reddish, who became Astronomer Royal for Scotland!

### Visit to North America in 1951

The I.A.U. General Assembly, scheduled to be held in Leningrad in 1951 was postponed by the Executive Committee at the time of the Korean war on the questionable grounds that the climate of opinion (mainly in the U.S.A.) was against a representative attendance. This allowed Clemence and I to meet again in Washington, Montreal and New York in the autumn of 1951, as the third meeting of W.P. 53 was held in Montreal, where specimens of H.O. 249 were presented for approval. I can recall a flight by Comet 1, with a stopover at St Johns in Newfoundland for refuelling; the return flight was made on a Britannia.

I first went to Washington, where Gerald Clemence and I outlined the contents of the Joint Supplementary report to Commission 4. We then left by car for Montreal to take part in two days of meetings of the Working Party; the meeting lasted a fortnight but we had agreed with the chairman to take all the ‘astro’ on two days.

Clemence and I had prepared a draft report concerning the unification of the *Air Almanac* and the *American Air Almanac* for Working Party 53 and it was, after much discussion, enthusiastically approved. After receiving permission to make the matter public, we called on Wing Commander Branch, who was then in a senior position in the International Civil Aviation Organisation (I.C.A.O.), and sought his advice for ‘selling’ the combined A.A. to others for civil use. He was interested and took us round I.C.A.O., introducing us to several delegates and taking us to a meeting in progress, where it was discussed and approved. During the Montreal meeting (if I remember correctly) we also jointly described the unification to several meetings of astronomers and R.C.A.F. officers. We had a busy few days in Montreal!

‘Viv’ Branch was the original Chief Instructor for the war-time Specialist Navigational Courses (Spec. N.), and accompanied students to the N.A.O. on their annual visits. He was the Secretary of the Steering Committee, of which I was a member, that gave rise to the Institute of Navigation, and was a good friend. He resigned from the R.A.F. to take a top administrative post in I.C.A.O..

On leaving Montreal Clemence drove us down to Eckert’s farm in New Jersey, where Brouwer and Herget (and possibly Schilt) joined us. The ‘farm’ was a recent purchase by Eckert to provide a retreat far from the tentacles of I.B.M. (of which he was then their Director of Pure Science) and the telephone. It had once been a farm, but how anyone could have made a living from the rocky land is a mystery. Eckert had made the house weatherproof, installed a calor-gas cooking-stove, fridge and lighting, and was

rapidly making it into an attractive, but 'rough' retreat. His instructions were: each to bring food and drink, while the essentials (bread, milk, etc.) would be provided. We took whisk(e)y, steak, and cheese of different varieties, and in large quantities, and we had a marvellous bachelor weekend.

During this weekend we planned the *Improved Lunar Ephemeris* (I.L.E.) in full detail, including the technical specification, the sharing of the calculations (N.A.O. was responsible for the conversion from longitude, latitude and H.P. to R.A. and Dec.), the arrangements for publication and financing, and other related matters. The calculation of the improved lunar ephemeris was carried out on the Selective Sequence Electronic Calculator (SSEC). Largely due to Eckert's influence and expertise, this was then the 'show-case' exhibit of I.B.M.. It was demonstrated at the I.B.M. headquarters in New York, in a shop-window. As a part of its program it calculated the Moon's position according to Brown's theory, as distinct from Brown's Tables. Eckert had planned the derivation from the theory, and this demonstrated the SSEC's ability to sum a very large number of trigonometric terms. Woolard (Clemence's assistant) compared the results with those from the Tables, and within the accuracy of the latter, got agreement except for one error in the Tables, which was subsequently corrected. We all persuaded Eckert (who did not require much persuading) to obtain I.B.M.'s permission to produce the longitude, latitude and parallax of the Moon from 1952 to 1959. The SSEC was not in full use for practical problems and this was a perfect example of a useful usage of the SSEC. I hope that I.B.M. benefited from the publicity it gave them. The agreement cost us nothing other than for the conversion and publication and, according to my estimate, saved the Office about £50000. This I duly reported to the Admiralty. It was one of the most enjoyable and productive weekends I have ever spent.

When seeing the SSEC I had an appointment with a senior executive of I.B.M. in relation to the supply of a card-controlled typewriter directly and not through B.T.M.C.. We agreed a specification and the order was duly placed and duly delivered after some delay. I was also pleased with my interview since he responded to my request for another plug-board for the 602A and he arranged it with B.T.M.C..

### **Office appointments and activities in 1950 and 1951**

During the years 1950 and 1951 we gradually built up the staff. In particular, we recruited the A.E.O.s Green, Harragan and Miss Knight; and S.A.s Barry and Miller. But the most important appointment was that of G. A. Wilkins as Scientific Officer at the beginning of October 1951. His interest was primarily in geomagnetism, but he had a wide knowledge of mathematics and he expressed an interest in astronomy. This was a much needed appointment, as it filled the gap below P.S.O.. And, I must say, it was a very happy and successful one.

On looking through the papers we wrote during the interval since 1950, apart from the navigation articles concerning the selection of stars for the *Abridged Nautical Almanac* and the *Air Almanac*, there was: a paper with Porter on stellar aberration; a paper with Atkinson on the proposed modification of mean sidereal time, later to be known as the Atkinson proposal; a paper with Clemence and Porter on the correction of the lunar ephemeris for aberration; and a proposal by me (not intended for publication) for an international publication for the fundamental ephemerides on the general lines of the *Apparent Places of Fundamental Stars*.

### *The Star Almanac for Land Surveyors*

One of the most successful innovations during this period was the publication for the year 1951 of *The Star Almanac for Land Surveyors*, which was first suggested by de Graaf Hunter and discussed in August 1947 at the Conference of Commonwealth Surveyors, which both Richards and I attended. {See chapter 10.} Approval was given for this new publication to be prepared by the Office and published by H.M.S.O. “by Command of the Lords Commissioners of the Admiralty”. It has remained almost unchanged ever since, though some details have necessarily been altered. Remarkably, its annual sale increased every year, but H.M.S.O. ignored my advice to print more copies, and for many years we had to have a reprint: it was a good buy! The emphasis was on economy of presentation, light weight, small bulk and small price. The production of the *Star Almanac* provided contacts with land surveyors as the Office generally was represented at the periodical surveyors’ conferences. These proved to be of some, though not great, interest and benefit. One of the consequences was an organised visit to the Observatory by a group, mainly of surveyors working abroad, from each conference, but these visits did not continue.

Richards, who had had practical experience in Tanganyika before he joined the Office, played the most prominent part in the discussions with the surveyors and was responsible for the preparation of the Almanac. He spent a lot of time over such details as the choice of stars and, especially, on the footnotes to the apparent places of stars regarding doubles and variables. But he seemed reluctant to prepare the examples for the illustration that we had planned and (I recall with some feeling) I had personally to compose the examples as well as preparing the text of the illustration. All this was done in such a short time that the copy was sent to the printer in manuscript. Incidentally, I found it difficult to devise a comprehensive realistic illustration covering all main usages of the Almanac that avoided special cases; the main points are (I think) still in use.

## CHAPTER 13

### End of the Spencer Jones era: 1952 - 1955

#### Installation of punched-card equipment

Before the move to Herstmonceux, approval had already been given for the installation of punched-card equipment in the Office, and after my visit to the U.S.A. in 1949 we obtained approval to add an I.B.M. 602A calculating punch. Owing to the unfortunate delays in the delivery of the second plugboard and the division relays it did not become fully operational until late in 1952. After my second visit in 1951 we obtained approval to order directly from I.B.M. a card-controlled automatic typewriter similar to the successful one at U.S.N.O.. It was delivered in February 1953 and completed, after years of negotiations and delays, the first comprehensive punched-card installation to which the Office had had access, in spite of Comrie's application of punched-card techniques to the work of the N.A.O. in the late 1920s, some 25 years earlier. And it was not an unqualified success, owing to the distance from the nearest service engineers, the relatively low mechanical efficiency (especially of the typewriter) and the mixture of B.T.M.C. and I.B.M. equipment. The regular maintenance engineer, Mr. Arthur Burton, came from Brighton.

The whole punched-card installation was under the general supervision of A. E. Carter, who had had his first experience with the reproducer and multiplying punch 20 years earlier at the Royal Naval College at Greenwich. The installation, on rental, probably paid its way, though (for many reasons which need not be gone into) its full potential was not realised. As in all similar installations of 'new' computing equipment (at least until recently, say post 1960s) many years of efficient operation are required to offset the effort expended in changing methods, in training staff, in programming and in overall control, together with the high capital, rental and running costs. In most cases its useful lifetime is considerably smaller. Personally, I was always very doubtful about the too early replacement of existing methods and equipment; but there were many pressures. In retrospect, I think we would have gained by waiting, especially as our requirements were not large, our prospective usage small and we could not expect to get the optimum equipment, or even that within the price range that we wanted. But I suspect that almost all similar (or larger) organisations had similar experiences.

We were particularly unfortunate with the mechanical performance of our machines. The Nationals were very bad in this respect, and so were the card-controlled typewriter and, much later, our first computer the I.C.T. 1201 {see chapter 14}. The typewriter proved useful, as the *Improved Lunar Ephemeris* (I.L.E.) demonstrated, but it suffered many mechanical or electrical faults: the mechanic became almost a member of the staff. I do not think that the fault lay with us, in our usage of the machines, but the designs were faulty. It did not happen with the 602A.

#### Responsibilities of Miss McBain

Miss McBain, who had been previously editor of *Monthly Notices* since 1947, was elected Secretary of the R.A.S. in 1949, and served until 1954. This pleased me, as

the Office had been interested in the work of the R.A.S.; it was particularly appropriate as Hunter, who was then at Greenwich, was the 'senior' secretary.

Miss McBain was regarded by all occultation observers as the key figure in the reduction of occultations and their analysis. She was in charge of the Office occultation programme, which consisted of the preliminary reading from the occultation machine, the final predictions, and the reductions of the observations collected by the Office. For many years she published an annual analysis (continuing that started by Brouwer) giving the deduced error in the lunar ephemeris. She was assisted by Miss Rodgers and other members of the staff. She was secretary of the I.A.U. Commission 17 on the Moon from 1955 to 1964.

### **The lunar occultation programme**

It is not possible here to describe the work of the lunar occultation programme in any detail. It involved a great deal of very careful recording of observations, from published sources or personal communications; a very considerable computational programme, for both predictions and reductions, together with all the associated recording, publication and distribution; much correspondence; and careful discussions. Little benefit could be gained from the punched-card equipment, and there was a reasonable doubt as to whether the programme was viable, in view of the claims (later shown to be much exaggerated) for the Markowitz Moon-camera. The work did, however, produce an annual mean deviation of the Moon from its ephemeris position to adequate precision for *practical* determinations of ephemeris time.

The observations, coupled with much effort, are of permanent value and have, in fact, been rediscussed using a better lunar ephemeris and high-speed reductions on the computer. The predictions were (and are) necessary to make possible the observations. The scope of the predictions was gradually expanded, and the number of observations received gradually increased. It was not possible to apply limb-corrections owing to the delay in the publication of Watts' charts; but preparations were made for the inclusion of such corrections.

In 1950/51 a major alteration was made in the design of the 'Moon system' on the occultation machine. Modifications, designed to permit easier setting of the parameters of the Moon's orbit, were suggested by Dr Perfect (then at Abinger) and carried out by A. C. S. Westcott (who had built the machine, and who was then in charge of the R.G.O. workshop). I think these modifications worked well. But the most significant innovation during this period was the introduction of predictions of occultations of radio sources; these originally played a considerable part in the determinations of the positions of the sources, and later in their structure.

I was not certain of the value of the occultation programme, but I was proved wrong by the results obtained later by Morrison and others.

### **The I.A.U. General Assembly in Rome in 1952**

The planned General Assembly of the I.A.U. in Leningrad in 1951 was postponed to 1952 in Rome. The General Secretary requested supplementary reports to cover the extra year, and this provided the opportunity for the very comprehensive proposals for the revision of the national ephemerides, as from 1960. Clemence and I produced a 'Supplementary Report', including all the recommendations of the 1950 conference, and all subsequent developments, for approval by Commission 4. It gave me considerable satisfaction to be able to present so many proposals, in a unified and

essentially final form, together with a reasonably adequate explanation and full references. The proposals were approved for submission to the General Assembly. They formed the basis for the (foreseeable) future of the *Nautical Almanac*, which was to be renamed the *Astronomical Ephemeris*, for 1960 onwards.

G. Fayet was President of I.A.U. Commission 4 from 1938 to 1952, but he did very little in preparation for the 1952 meeting. I was Vice-President, and also acted as Secretary, and, I am afraid, I took over most of the work of the Commission. Fayet was then, I think, in his late seventies, and had little interest in the new developments. I was appointed President of Commission 4 for the next two meetings.

There was little of direct interest to the Office, other than that just mentioned, in the meetings. There were many other attractions in Rome, including an audience with the Pope at Castel Gondolfo. In the field of astronomy we made contact with Zverev and Nemiro, who came from the Pulkovo Observatory and whom I had met during the 1950 conference in Paris, and for the first time we met Alla Massevitch, with whom I cooperated later in the I.A.U..

### **Navigational work in 1952 – 1955**

There were some staff reductions, in the light of the increased use of machines, but there was still a shortage of an S.O.. All the staff worked well on the sometimes routine jobs, and there is very little I can add to the account of the work during 1952-1954. In addition to the normal tasks of the Office there was the Decca work, which was extraneous, and several navigation problems that could, and should, have been appropriate to the Office. I had long held the view that a professional approach to the problems of astronomical navigation was the province of the Office, as it was in the days of Maskelyne.

Of the many navigation problems that we investigated, the most important papers published in the *Journal of the Institute of Navigation* (later called the *Journal of Navigation*) in 1953-1955 were:

‘The correction of astrofixes for precession’. This meant that the star volume of A.P. 3270 could, if necessary, be used for many years. A.P.3270 was the designation given to the British edition of the *Sight Reduction Tables* H.O. 249.

‘The genesis of the Experimental Astronomical Navigation Tables’, with E. W. Anderson. This was a wartime experiment, which antedated H.O. 249 and used mean time as argument instead of L.H.A. Aries; specimen tables were issued, but H.O. 249 came along.

‘An improved astrograph’. This was a device to extend the old astrograph (which was limited to two stars) to three or four stars and to the Sun and planets. Experimental films were made and tried out, but by this time the astrograph was little used and its mechanical design inadequate.

‘The precision of the *Air Almanac* and A.P. 3270’. This was an attempt to give a complete statistical treatment of the errors arising through their use, with the object of persuading the Admiralty to use a tabular accuracy of 1' - instead of 0'.1. It set the method of treatment of such tables, and formed the basis of comparison with observations.

‘Continuous plotting of position lines using A.P. 3270’. This was a valid attempt to obviate the use of a chosen longitude; it certainly worked well, but it proved too

complicated for practical use.

We also conducted observations with a sextant and theodolite in order to investigate the dependence of dip of the sea horizon on various factors. The programme was to observe the dip, accurately by theodolite, from fixed locations on the seashore (or cliffs as appropriate) using the height of the tide to give varying heights above sea-level. The cooperation of Trinity House enabled air and water temperatures to be taken by the staff on the Royal Sovereign lightship, which was approximately on the horizon. Unfortunately we had not allowed for the tidal 'wave' which affects the dip near the shore as the sea surface is not an equipotential near the shore. I was never able to find anyone able to provide an adequate theory of the shape (curvature) of the tidal wave and so it did not prove possible to reduce the observations. These were made very largely by Scott, with the assistance of Harragan, Taylor and several of the girls. We got various results, but we did not publish them.

Another project, which took many years to complete, was the analysis of low-altitude observations to determine, observationally, the effect of irradiation on the horizon and on the limbs of the Sun. As we were concerned with differential observations (alternating observations of the altitudes of the upper and lower limbs) the precise value of dip did not enter. Many series of observations were made by many members of the staff in the early morning. The results were not conclusive since observations by officers from H.M.S. Dryad tended to disagree with those by N.A.O. staff, but there was little doubt that the correction of 1'.2 that was then incorporated in the altitude corrections for the Sun's upper limb was not justified: and it was dropped. Much of the detailed information was not published, though some of it was extremely interesting. The state of the horizon (recorded by the observers) had an enormous effect on the precision of the observations. Individual observers had 'patterns' of observational errors, varying with the progress of the series of 20 or 40 observations in each set, which were recognisable as proper to the observer. Although there were very many observations, the amount of material hardly justified a full analysis of this latter effect since the circumstances would not arise in practical navigation.

### **The Institute of Navigation**

I was elected President of the Institute of Navigation in October 1953. For this I sought approval from the A.R. and the Admiralty, though as both the A.R. and the Hydrographer had preceded me, I could not expect any opposition. My first duties (apart from thanking my predecessor, Vice-Admiral Sir Archibald Day) was to act as host to the Duke of Edinburgh at the reception on H.M.S. Wellington. The Institute later became the Royal Institute of Navigation.

I delivered two presidential addresses during my term of office, namely on "The role of the Institute" and on "The place of astronomy in navigation".

In 1953 I became Chairman of an Institute of Navigation Working Party on the Accuracy of Astronomical Observations at Sea. The working party discussed the broad outlines of the problem, and the Office undertook the reduction and analysis of the observations made by navigators, at sea, in accord with the specifications we laid down. It was a major operation, not only in computing but also in organisation; Scott, who was a member of the Committee, did a large part of the work. The final lengthy report (there were many drafts to discuss with the working party) was published in the Journal of the Institute in 1957. I think it gave the most reliable indication yet of the accuracy

(or inaccuracy) of astronomical navigation at sea in practice. Although we had hoped for many more observations, the conclusions of the Report are realistic and valuable.

### **Visits by navigators**

A long series of visits by the navigation courses of the Royal Navy navigators started in 1953. The first came from (if I remember correctly) Royal Naval College, Greenwich, but later courses came from H.M.S. Dryad, the school for navigators of the Royal Navy (and of other nations). This was a whole day visit, in which the officers were shown the work of the R.G.O. (in particular that of the Chronometer Dept and the Office). These visits were originally scheduled as part of the training, and the members were expected to answer questions; but they became less formal. They provided us with a contact with our customers and them a break in their navigation instruction. We tried to organise these visits so as to combine both general and special interests, and I think they have been mutually beneficial. Miss Perry, and later Miss Hanning, efficiently organised the visits, and many members of the R.G.O. staff (in addition to N.A.O. staff) were extremely helpful. In 1977 Miss Rodgers was entertained at the Jubilee Review on board H.M.S. Sheffield as a guest of the Mayor of Sheffield, and found herself sitting next, at lunch, to the navigating officer who recalled with pleasure his visit to the R.G.O.. But I think rather more important benefits have arisen from the visits.

### **A minor annoyance**

I was annoyed (not an unusual thing) by a slip-up in the appointment of Wilkins. The formal Admiralty letter of appointment offered him a salary which he accepted; it was not for several months that Wilkins, looking through conditions of service, realised that he may not be entitled to one allowance. We discussed this and we agreed that the proper thing to do was to refer it to C.E. Branch; he wrote a letter to Barker (the Secretary-Cashier of the Observatory) and I sent it to him. Nothing was heard of the outcome for many months, when C.E. Branch pointed out the error and demanded repayment. We pointed out that we had raised this matter a few months ago, and that it was their mistake in the first place and that it should have been corrected when we wrote to enquire. They replied that they could not find the letter from Barker. In the end Wilkins decided to accept, though I wanted to carry on what I think would have been a lost cause. {The problem arose because my starting salary in a temporary position was based on my qualifications, whereas after my establishment my salary was based on my age. — Ed.}

### **Responsibilities of Dr Porter**

Porter was in charge of the calculation of the fundamental ephemerides for the years 1960-1980, which was a major part of the work of the Office after the move to Herstmonceux. Much of actual desk computations were done by E. Smith, while the punched-card operations were planned and supervised by A. E. Carter and G. A. Harding. Porter was also responsible for the preparation of the volume of *Planetary Coordinates* covering those years.

The Office had full responsibility for the calculation of the ephemerides of the inner planets, but the basic work for the Moon had been done by Eckert, and the heliocentric coordinates for the outer planets by numerical integration by Brouwer, Clemence and Eckert on the SSEC. It was usual to calculate the heliocentric ephemerides for a period of 20 years, and it had to be done well in advance for use in the computation of the orbits of comets and minor planets, as well as for publication in

the almanacs. The punched-card installation of the Office (particularly the 602A) was heavily used, and it became possible to print the result on the card-controlled typewriter. Thus it came about that the third volume of *Planetary Co-ordinates for the equinox of 1950.0*, covering the years 1960–1980 was produced with the data from the ephemerides reproduced from typewriter copy. It was decided that we should give a comprehensive account of the different methods of calculating perturbations, and this project was given to Porter. He undertook, with the help of Wilkins and Candy, a comprehensive investigation into methods of computing special perturbations. We ‘invented’ or ‘discovered’ a special comet, with desirable characteristics for this purpose. We used many different methods to calculate the orbit of this fictitious comet and we then compared the methods for effort and accuracy.

### **Visit by Professor Herrick**

In 1952 we had Professor Samuel Herrick in the Office for a year on a Guggenheim Fellowship. He brought with him (in addition to his wife, Betulia, and three children) an assistant C. G. (Jeff) Hilton, who also worked in the Office. They worked largely independently of the Office since Herrick was mainly engaged on the preparation of his major textbooks on what was later called ‘astrodynamics’. Quite frankly, I do not think that the arrangement was very fruitful to either side. Herrick had founded the U.S. Institute of Navigation, and was then its secretary, and we were interested in having him visit us and in using the opportunity to organise cooperation with our Institute.

I liked him personally, but he was obstinate (or perhaps I was) about the subjects of perturbations and particularly about the practical co-operation between the two Institutes of Navigation. Herrick had earned the scorn of Brouwer for his claim to compute special perturbations by numerical integration without a double integration, and he had quarrelled with most dynamical astronomers in the U.S.A. by refusing to acknowledge this fundamental error in his paper (in P.A.S.P. from memory). Although he was, I am sure, disappointed that we were doing so little work in celestial mechanics — only numerical work on comets, and little theory — he was extremely difficult to get on with. Many of our weekly ‘discussions’ ended in disagreements (mathematical) and mutual frustration. I was not alone in finding him difficult. Michael Richey, who was secretary of the Institute of Navigation, and others found it almost impossible to get Herrick to cooperate in any way. In spite of this, we remained reasonably good friends, but at a distance. [He and Betulia called to see us just before Christmas 1973, when Sam was clearly seriously ill. He died in 1974.]

### **Activities in 1952 – 1955 continued**

The card-controlled typewriter was delivered in 1953 and (in due course, since we had a period of experimentation with it) was used eventually for the preparation of copy for all suitable publications (such as the A.N.A. and the A.P.F.S.). We insisted on high standards of design and presentation, and this involved a very great deal of painstaking work and examination. This gave us much more work, but was a great saving to H.M.S.O.. Much credit is due to Carter, Scott and all the girls who supervised the operation of this temperamental machine. Large preprinted ruled and headed forms were used, and the initial setting of the paper was crucial. The subsequent photographic reduction sharpened up the print even further. In retrospect it might have been better to have accepted lower standards, particularly in respect of the tolerance between printing

and rules. But I am certain that the whole procedure was worthwhile, and that users were not inconvenienced.

The main astronomical papers were, with Porter, on the accurate calculation of the apparent places of stars, a fairly routine matter of allowing for second-order terms that were previously ignored; and a descriptive article on Ephemeris Time, in the *Quarterly Journal of the R.A.S.*, in which the concept of the ephemeris meridian was introduced.

A leaflet was prepared (mainly by myself) on the 1954 June 30 solar eclipse, and it was put on sale to the public.

Clemence visited the Office in 1953, for a meeting of W.P. 53, and we had long discussions about the implementation of the decisions of Commission 4 in Rome.

The staff of the Office was increased in 1954 by the appointment of W. Nicholson as a Scientific Officer; in addition to holding a degree in astronomy, he had served in the R.A.F. as a navigator.

### **Loss of position in computing**

It is perhaps worth mentioning here that the N.A.O. gradually lost its leading position in the computing field after the war and its rate of loss increased greatly as faster and bigger computers were developed. It was clear to me (though not necessarily correct) that N.A.O. could not possibly compete in the post-war computing business. In the immediate post-war years the emphasis was almost entirely on techniques that were completely outside the N.A.O.'s interest and competence. The computer manufacturers lagged behind the universities (and other organisations such as N.P.L.) which could design and build experimental machines, so that the N.A.O.'s 'traditional' role of exploiting, for scientific computing, the commercial machines designed primarily for the business world was not applicable. Moreover, the demand, inside the N.A.O. and even in the broader R.G.O., could not (it certainly did not!) justify anything other than a relatively inexpensive machine. It was a pity not to use the Office experience to better advantage (I am thinking now mainly of Wilkins and Carter), but there was some little recompense in the success of the former A.C.S. staff.

We were called upon to make contributions to such matters as: a symposium on automatic digital computation; the printing of mathematical tables; and exhibits on various occasions.

### **Visit to the U.S.S.R. in 1954**

In May 1954 I visited the U.S.S.R., at the invitation of the Soviet Academy of Science, to attend the re-opening of the Pulkovo Observatory, which had been completely demolished by the Germans during the siege of Leningrad. My companion on the trip was T. G. Cowling, and there were several others (such as Brouwer, Oosterhoff, Oort, and many Soviet astronomers, whom I knew) with whom I was glad to meet in the lavish hospitality that was heaped upon us. The chief item of interest to the Office was my visit to the Institute for Theoretical Astronomy. I escaped the attendance of my interpreter, and took a taxi to the Institute. There I met Subbotin, the Director, with whom I had frequently corresponded, but never met. He did not speak English, and we spoke a little in French until he produced an English-speaking colleague. I thanked him for his support of A.P.F.S. and for the I.T.A. contributions thereto. He then showed me round the Institute; they had a number of pre-war punched-card machines, and several calculating machines, but nothing modern.

At the formal re-opening ceremony neither the U.K. nor the U.S.A. had come prepared with ceremonial addresses or gifts, as elaborately provided by other countries, such as China, which had a magnificent silk banner suitably inscribed. In desperation (since we came at the end of the tribute) I wrote out a message on the back of a sheet of paper and read it out and Nassau did the same for the U.S.A.. It was a pity that the Royal Society did not have the forethought to provide us with a suitable memento of the occasion. In addition to visits to observatories in Leningrad and Moscow, we were taken to theatres (ballets and operas, and, in particular, a performance of Hamlet in Russian) and parties.

It was a most enjoyable visit, especially as I was able to make personal contact with the Director (Subbotin) and staff of the Institute of Theoretical Astronomy in Leningrad. I also made many lasting friends, such as Alla Massevich, A. A. Mikhailov (then Director of Pulkovo), M. S. Zverev, the Kukarkins and Kulikovsky.

### **Conformity of the almanacs**

My next visit abroad was to Washington in October 1954 and it, on the other hand, was extremely productive. It was probably to take in a meeting of W.P. 53 (but I am not certain of this); the main item of discussion was the ‘conformity’ of the *American Ephemeris* and the *Nautical Almanac*. Clemence and I worked out ways and means (both technical and administrative) of unifying these two astronomical almanacs. This was a considerable breakthrough in our co-operation, and meant much detailed planning. A great deal of the planning took place in Washington (although there was much that could be left until later). I can recall vividly the sense of relief when we realised that, after going through the whole volume, there was nothing on which we could not agree. We, contrary to my opinion of Clemence whom I thought drank very little, proceeded to go out to a bar and drank martinis to celebrate. The unification was not due to come in until 1960, when a number of other changes were being made.

This change was, I think, a logical extension of the unification of the navigational publications, but it involved many compromises. As on previous occasions I found Gerald Clemence a most cooperative collaborator, who was prepared to devote endless time and effort to meet major and minor differences (e.g. the spelling of ‘centre’ and ‘metre’). At the same time, he defended the practices of U.S.N.O. (e.g. the method of calculating the circumstances of eclipses), which could not (in his opinion) be changed without offence to his staff, with considerable firmness. An interesting point was that complete unification seemed unattainable, at least to the extent of the *Air Almanac*. We accordingly spent an interesting hour in the library of U.S.N.O., with Webster and O.E.D. and other reference books, seeking the precisely suitable word, which we agreed should be ‘conformity’. Subsequently, we were able to achieve almost complete uniformity, but the chief difference remained for another 20 years in the titles. I think that Clemence would have been prepared to change (even though this would have required amendment to an Act of Congress), but there was solid opposition from the American Astronomical Society and other bodies.

At the same meeting the possible unification of the *Abridged Nautical Almanac* and the *American Nautical Almanac* was discussed. We had experimental layouts on the card-controlled typewriter in two forms; one of two days to a page, and one of three. Later, in consultation with the R.N., we decided on the three days to the page, as in the American N.A.. The unified publication was to appear in 1958, though the common title of *The Nautical Almanac* was deferred until 1960, when the new title of *The*

*Astronomical Ephemeris* was adopted for our N.A., whose full title was *The Nautical Almanac and Astronomical Ephemeris*.

Clemence had arranged a flight for me on MATS (Military Air Transport Service) and I can recall it well. The flight was delayed and I can recall sitting all day in my hotel room waiting for the phone to ring; but we got away the next day. MATS flew a piston-engined aircraft, and I was shaken to discover that the engines glowed red at full power. The noise was intense and the comfort minimal. We stopped to refuel at the U.S. base in the Azores, and we were made welcome by the U.S. Navy. Our landing was at the Naval Air station at Patuxent near Washington, where Clemence met me. I had all papers ready, including a small-pox certificate, but at 7 am on Sunday morning the medical staff were woken up to give me clearance.

Clemence and I published articles on this agreement in both Journals of Navigation, pointing out the benefits that a completely unified system of almanacs and tables for both sea and air navigation would bring, not only to the English-speaking countries, but to others as well. The U.S.A. has (by statute) no copyright on government publications, so any country can reproduce at will. But we wished to go further than this by making available reproducible material for direct photography. Through the good offices of the Director of Publications at H.M.S.O. we made an arrangement by which specially-pulled pages could be made available to the almanac-producing agency in any country at a nominal charge, to include copyright fees. The pages of the almanacs are suited to the replacement of the headings by other languages and this method has been frequently used for the N.A., in particular. I wish to pay tribute to the outstanding interest of successive Directors of Publications in our methods that led to savings in the costs of composition, but also to their understanding of the need to keep copyright fees so low while still maintaining the principle. McGrath and Cox (who became Deputy Director of H.M.S.O.) were good friends of ours.

A consequence of the agreement was the decision to make the promised (so long promised!) *Explanatory Supplement* a joint supplement. But that will come into the next period.

### **Marriage to Miss F. M. McBain**

I got married to Miss McBain in December 1954. This was a complete surprise to the Office, and in fact the only people in the R.G.O. who knew were the A.R. and Lady Spencer Jones, who were some of the few guests at the wedding. The Office did us the honour of decorating the hutment on our return in January 1955. They placed over one door the emblem of a thistle and on the door a RED rose — a welcome to a Yorkshireman! Sir Harold and Lady Spencer Jones duly carried out their promise to have the Castle floodlit. Mrs Sadler stayed on as a P.S.O. for about a year, and afterwards became part-time.

### **The I.A.U. General Assembly in Dublin in 1955**

The next General Assembly of the I.A.U. was held in Dublin in August 1955. I was the President of Commission 4 and thus had to present a report; this contained two special items (apart from the usual reports of progress):

(a) The Atkinson proposal had been circulated but, though there had been a slight majority in favour of its adoption, I proposed that it should NOT be adopted and this was agreed at the meeting.

(b) In view of the interest displayed by the International Committee of Weights and Measures in adopting a unit of time, it was necessary to review that proposed in Rome; I suggested a definition that was accepted by the Commission and later by the Assembly.

There was a general tidying up of the exchange agreements. I had circulated to the Directors a proposal for consideration of an inter-national ephemeris (I.F.A.E.), but difficulties, largely financial, had arisen and I withdrew the idea. I was re-elected for a second term as President of Commission 4.

Much discussion was concerned, in Commission 31, with the development of atomic-time systems. Brouwer, Clemence and Herget stayed with us before the meeting so giving me an exchange of ideas, especially on the unit of time. Clemence had a full week in the Office. This was the beginning of my interest in 'time' and particularly in U.T..

The General Assembly was rather small, but enjoyable. As far as we were concerned the high spot was the party given by the U.S.S.R. delegates at which Flora was induced to sing while standing on a piano. We went on a visit to Belfast and from there we took the boat to Glasgow, where there was an R.A.S. meeting. We learned (unofficially) who was to be the next A.R.. This was the first meeting of the R.A.S. that Pagel attended and he spoke extremely well; it was a pleasure to see him later at the R.G.O..

### **The change of Astronomer Royal**

Sir Harold Spencer Jones, K.B.E., retired as Astronomer Royal and Director on 31 December 1955, and was succeeded on 1 January 1956 by Dr. R. v. d. R. Woolley, who was the Commonwealth Astronomer at Mount Stromlo in Australia. Woolley had been a Chief Assistant at Greenwich before the war, but he had had little contact with N.A.O.. He had no personal knowledge of the somewhat anomalous relationship between R.G.O. and N.A.O. and it was inevitable that his view should differ considerably from that of Spencer Jones. It was only natural that he should at first regard N.A.O. as an integral part of R.G.O., though it was to be a gradual transition.

## CHAPTER 14

### Woolley becomes Astronomer Royal

#### Woolley becomes Astronomer Royal

On 1956 January 1 Richard van der Riet Woolley became the eleventh Astronomer Royal. At an unfortunate interview on arrival from Australia he delivered himself with his view that “space travel is utter bilge”. It was sometime before he became used to the idea that space research was not necessarily a financial obstacle to optical astronomy. I knew him, very slightly in Cambridge, and well when he was Chief Assistant at the Royal Observatory, where we played hockey together. But I knew his wife during my Cambridge days as a number of Girton mathematicians were elected (illegally I suspect) to the Trinity Mathematical Society and Gwyneth Meyner was a friend of theirs. I can remember her asking me what were the prospects of a post in astronomy when Woolley returned from America, where he had spent two years as Commonwealth Fund Fellow at the Mount Wilson Observatory. There were very few posts then.

His wife, Gwyneth, did not come with him (but she came much later); this was a grievous disappointment to him. Spencer Jones did not move out of the Castle immediately as he had trouble with alterations to his house in Tunbridge Wells and so Woolley had to sleep in the spare room and to eat in the canteen. For some days Woolley came to stay with us in Bexhill. He took up his bachelor existence in the Castle when Spencer Jones moved out.

Perhaps it was this episode (which did little credit to S.J.) that turned Woolley against S.J.; but it may have been his general dissatisfaction with his running of R.G.O. and especially the plans for the Isaac Newton Telescope. In March 1956 Woolley (as A.R.) took the chair at a meeting of the I.N.T. Committee, immediately abandoned the duplex design and set up a sub-committee to design a conventional telescope. The current difficulties were not, by any means, the fault of S.J. alone, but that is another story.

Woolley would not have been my choice for the post of A.R.. He told me that he had grave doubts about whether to accept it, but I think this have may have been due to the fact that Gwyneth would not come with him.

Incidentally, when she did come over to join him, I bought a bunch of flowers to greet her, but she went into ‘purdah’, and for many years was little seen in the Observatory. I won a bet with the A.R. (about a test match) and my prize was a bottle of Clos de Conte 1952. I promised to keep it until Gwyneth and he came to dine with us; they never did.

#### Woolley's administrative style

It is perhaps not out of place in this *personal* history to recall my impressions of the effect on the Office of the administration and policies of the ‘new’ Astronomer Royal. The main impression remains one of bewilderment and frustration in their application, particularly in regard to the element of chance in respect of consultation and decision. Fortunately, such decisions had little direct effect on the Office, though I

think that they might, and possibly should, have done. Woolley undoubtedly had a difficult task in taking over an Observatory severely run-down by war and its aftermath of neglect. He certainly put the emphasis firmly on astronomical research at the expense of other activities. But it was his methods that fell far short of his admirable, but difficult, policies. To take one or two examples, not always particularly relevant to the Office:

‘Chief Assistants’ Meetings’ (at which the Secretary-Cashier was also present) were held regularly, but rarely, if ever, discussed serious policy matters; most of the time was wasted on minor administrative questions and domestic organisation (and not with astronomy). It was not Woolley's procedure to circulate proposals beforehand; his usual practice was to demand one's immediate reactions to an oral suggestion. On many occasions I was called down to the Castle to see him, often with the single word ‘Come’ on the telephone, without any indication of the reason. I might then be shown a letter from the Admiralty, re N.A.O. matters, and asked for my views before I had even had time to read it. His attitude to correspondence was, to say the least, haphazard; how much went astray no-one will ever know. He would (at best!) scribble initials on the bottom (e.g. DHS) and put it, without further annotation, in a transit envelope.

Woolley was not a good administrator. It was said that he was ‘a hit or miss’, and he showed this early. On nearly his first day as A.R. he asked what a button (a fire button) was for and he immediately pressed it in order to find out what would happen. The staff obeyed the instructions, but were a little put out by turning out into the courtyard on a cold day. A fire engine also came from Herstmonceux. At a meeting of senior staff he asked who was in charge of the canteen; I answered ‘Rickett, who approved the accounts’. A little later Woolley (who had misheard me) picked up the telephone and asked for ‘Richards’ to come to see him; and he confirmed him in his position.

His treatment of T. Gold (then Chief Assistant) was intolerable. Gold was allocated jobs that were well below his ability, and were more suitable to the lower ranking scientific staff or to secretarial staff. It was certainly no surprise to anyone that Gold soon resigned. The A.R. brought in Olin Eggen, an American with whom he had worked in Australia, as Chief Assistant at S.P.S.O. level. He was given (or assumed) authority and powers that were resented by some staff. The change may well have been of considerable benefit to the Observatory since Eggen is undoubtedly an extremely competent, though odd, astronomer, but the abrupt difference in the conduct of affairs was not judged to inspire confidence. Others can speak of his effect on the R.G.O. He was generally disliked, but I liked him. Although he was not here the whole time, he finally resigned in 1965, oddly enough when he had been offered promotion to D.C.S.O.. He showed me the letter of congratulation from the Chairman of S.R.C., but he resigned a few days later.

I was given the post of chairman of the library committee although I had no special experience. We had a librarian Preston, who was obviously inefficient. When called upon to write his Annual Report I discovered that he had been recommended for promotion by Atkinson (at Greenwich), supported by Spencer Jones; there was little I could do about it. It took me many years of frustration before I could suggest to him that he should retire; he was a curious man, who had a certain charm, he accepted my invitation and we remained good friends afterwards.

I was also appointed chairman of the canteen committee, probably arising from the fact that Mrs Marples (canteen manageress) was running foul of the Admiralty on expenditure. She was quite capable of meeting criticism without any support from the committee, so my job was to pass on suggestions from the staff and organise the Christmas Dinner.

### **Anecdotes about Woolley**

Although not relevant to the history of the N.A.O., I may record one or two matters concerning the A.R..

The A.R. came to the Observatory with a reputation for tennis; I challenged him to a game and I won. Thereafter we played doubles regularly on Sunday morning, with Wilkins and Carter. Much later I took a team from the Observatory to play against a team from a girls' school in Bexhill; the girls were the public schools champions, and were badly in need of some male opposition. The invitation came from the daughter of the Director of Publications (McGrath) at H.M.S.O., who was a teacher at the school. The headmistress thanked me, but said that it would be appropriate on a subsequent occasion to leave out the A.R. since his language on court was not suitable. {My version of this episode is that on the second occasion, the rest of the school turned out to listen to his language! — Ed.}

Earlier we had organised a bridge four (with Harragan and Nicholson); I had played bridge with him at Greenwich. After the imposition of fuel rationing after the Suez crisis at the end of 1956, he insisted that we always came to him at the Castle, in spite of the fact that none of us had an extra ration of petrol.

He gave occasional luncheon parties for distinguished guests; but he never bothered to introduce us (some senior staff) to them or them to us. H.R.H. The Duke of Edinburgh came down to open the Equatorial Group in 1958; after the opening the A.R. had tea laid on for him in the Drummer's Room. His equerry, Atkinson and I were present. After tea had been served by a maid (and she left) we waited for the A.R. to pass round the cakes, but it was the Duke who passed them round.

These and many other eccentricities do not detract from him as a personality; as I have pointed out he acted very quickly to get the I.N.T. on the move.

### **Role of the Astronomer Royal**

Extra-mural office work seems to be an occupational hazard for Astronomers Royal and Superintendents N.A.O.. Airy did almost everything, and in more recent years Spencer Jones attended so many administrative (I exclude here scientific symposia etc.) meetings and functions that he was more often abroad than at Herstmonceux: I.A.U., I.U.G.G., I.C.S.U., UNESCO, I.G.Y.–C.S.A.G.I. and F.A.G.S. are examples. He was once, at least, gently reprimanded by the Admiralty. Admittedly he was much frustrated by the practical impossibility of doing astronomy during his tenure at R.G.O.. There was little hope, with the frequent change of stop-go policy, of getting the Isaac Newton Telescope built quickly or of starting observational programmes at Herstmonceux. On the other hand Woolley, the last Astronomer Royal–Director, devoted himself completely to furthering astronomy, at the expense, sometimes, of neglecting administrative duties. He did not attend administrative meetings and most of his visits abroad were to observe. He was successful in building

up, at a time of considerable difficulty, the nucleus of an astronomical research team and the foundations of the surge of observational optical astronomy.

### **Work of the N.A.O. in 1956 – 1959**

I cannot now recall the specific astronomical work done in the N.A.O. in the years after Woolley became Astronomer Royal; all the standard programmes were continued. In addition much time and effort was expended on the design of the *Astronomical Ephemeris* for 1960 and on the preparation of the *Explanatory Supplement*, as well as on the third volume of *Planetary Co-ordinates* for 1960-1980. We were, I think, kept pretty busy before 1960. There is little information in the R.G.O. Annual Reports about meetings, visits, etc; all were struck out by the A.R., who desired the Report drastically cut. This was fine when the report was read to the meeting of the Board of Visitors, but it removed an easy source of reference.

The work on occultations, with which I was not actively concerned, had now been increased by the inclusion of the prediction of occultations of radio sources by the Moon. All observations for stars in the years 1948-1953 had been collated and copy prepared for publication in the *Greenwich Observations*. We were still waiting for the charts of the limb of the Moon that Watts (of the U.S.N.O.) was preparing so that we could apply corrections to the times of the occultations to allow for non-circular shape of the limb. We did, however, analyse a sample of 250 occultations using limb corrections supplied by Watts, with the result that the probable error was halved to 0".3. It seemed necessary to wait until the whole lot could be reduced with limb corrections. Nevertheless the annual discussions were continued, and published in the *Astronomical Journal*. Calculations of the topocentric librations were introduced for the subsequent application of limb corrections.

Navigationally, it was a very busy period. Much of the work of the Office at this time (1956) was devoted to the detailed design of the *Air Almanac* and of the *Abridged Nautical Almanac*. We published the page layout of the latter in the *Journal of Navigation*, giving the unified form that is still used. We (actually Scott) continued with the analysis of marine observations in conjunction with the working party of the R.I.N.. The results, which were published in 1957, constituted a major advance in the accuracy of astronomical observation at sea from skilled R.N. officers to 'tramp steamers'. The *Abridged Nautical Almanac* was unified as from 1958, and the title was changed to *The Nautical Almanac* as from 1960. Apart from the binding there was almost complete identity with the U.S. edition. The *Sight Reduction Tables for Air Navigation* were being produced, with U.K. editions; changes were being made to the *Air Almanac*.

The Decca work was still in progress and, from time to time, we were called upon to compute, or recompute, new chains. Similarly there was a requirement for new star curves for the Astrograph as the existing ones became outdated because of precession; we designed a new form of Astrograph, using graduated intercept lines instead of curves of constant altitude. By this means it was possible to use more stars and to extend the application to the Sun, Moon and planets. Curves were prepared for one latitude band as an experiment, and I think they proved successful in use. But the Astrograph, and to a considerable extent 'astro', had by this time been relegated to a secondary role. The new navigational methods, such as Doppler satellite navigation, required that the azimuth reference be monitored, or checked, from astronomical observations. Before the days of airborne computers the azimuths had to be calculated by hand or taken from tables. Initially a precision of 0°.1 was specified and so we designed a graphical method (using

an old principle) to give about this accuracy. Later the requirement was reduced to a precision of  $0^{\circ}.3$ , and I produced a single small sheet to give the required answers. It necessarily involved a number of rules to cover the sixteen possible combinations of signs and quadrants. I was rather proud of it, but the R.A.F. judged it to be too complex to use in an aircraft. Scott then increased the scale, and with his usual energy and competence produced the Scott Azimuth Diagrams in a booklet of some 24 pages, deliberately designed to avoid all rules at the expense of a large number of sheets. These were produced in small numbers (hundreds) and used by the R.A.F., but shortly afterwards the whole requirement was withdrawn.

### **Visit to Washington and Montreal in 1956**

The next event, for me and Mrs Sadler, was a visit to Washington and Montreal for a meeting of W.P. 53. We went by sea (in the Queen Elizabeth) and were met in New York by a Captain Lee, with whom I had been in correspondence. He took us on a magnificent tour of the city, and gave us a fine lunch, before we took the train to Washington.

Clemence and I had some discussions on the proposed Supplement, and Mrs Sadler spoke to Watts about limb corrections, and I think that Clemence and I visited U.S. Hydrographic Office. In discussing the best way to get to Montreal, Mrs S. said that we had not visited Niagara Falls (neither had the Clemences) and so we went considerably out of our way to Harvard and Yale, and to visit Clemence's home. During our short visit the news came on television that we had bombed Suez. We tried very hard to get some firm information, but it was Halloween and the news was interrupted by children demanding 'trick or treat'. We bought a paper the next day, but we had the greatest difficulty in finding any reference to it. It was not until we reached Montreal that we got the full story from the television.

After some little business in Montreal (and Ottawa) we had a very pleasant trip to the Laurentians where Mrs Clemence's sister had a cottage. During a snowstorm we set sail on R.M.S. Carinthia on the way home.

### **Committee on the definition of the second**

In October 1956 the International Committee on Weights and Measures (C.I.P.M.) adopted the I.A.U. definition of the (ephemeris) second as the fundamental unit of time. At the same meeting it was agreed to set up a consultative committee for the definition of the second (C.C.D.S.). The chairman of this committee was Danjon, President of the I.A.U. and Director of the Paris Observatory. He arranged the first meeting in Paris in June 1957; I was there as an astronomer, in contrast to the other members who were experts on time and atomic transitions. I, as President of Commission 4, gave a rough estimate of the accuracy with which the second could be determined from the Moon; I was well out! The experts could not agree on the atomic transition and so the only action was to hope that something better would come out of the Moon.

### **Preparation of *Interpolation and Allied Tables***

Discussions had taken place at various times since the end of the war on the possibility of a much extended and revised edition of *Interpolation and Allied Tables*. The first idea was a joint effort with the Mathematics Division of N.P.L., most of the staff of which had been members of A.C.S., for publication in their new series of tables.

It subsequently transpired that our ideas of layout and pagination would not fit in the proposed N.P.L. set of tables, and it was published independently of N.P.L. by H.M.S.O. in 1956. Most of the work, as regards both contents and presentation, was done by Wilkins, who was by this time a member of the Royal Society Mathematical Tables Committee. He certainly did a fine job of presentation and the result was an all-time success. Although there was not a great deal that was original, the content was well chosen and excellently displayed. With the cooperation of H.M. Stationery Office, it was published at an absurdly low price for its size and, more importantly, for its high-density content. There have been many printings, and only H.M.S.O. can state how many copies have been sold — my estimate is about a quarter of a million copies.

### **Preparation of *Subtabulation***

For many years I had planned to publish details of the special methods of subtabulation used in the N.A.O.. Many are quite unique; but it was wisely decided, at a fairly early stage, not to include them in I.A.T.. Instead, a subsequent companion booklet, called *Subtabulation*, was prepared. It sold many fewer copies, as by that time such matters were outdated. But there was in it a new method of subtabulation without machines and, for the first time, a general theory of the method of bridging differences. The end-figure methods, used by Comrie mainly to prepare mathematical tables, had been described in supplements to the N.A., and reprinted, but I was of the opinion that they could be systematised and improved for general use by hand. It took me, however, many years to find time and ‘inspiration’ (if that is the correct word) to develop the method of precalculated second-differences, but by which time the demand for a method suitable for purely mental calculation was zero. Still it was satisfying to me. W. Nicholson calculated the tables, checked the examples, etc. The section on bridging differences, applicable at the time to punched-card machines and elementary computers, was a systematic account (by Wilkins) of the methods used in the Office on the National machines. {These methods had been introduced by Comrie, but had been developed over the years by other members of the N.A.O. staff. At that time A. E. Carter was in charge of the use of the National machines and had produced a collection of about 100 ‘set ups’. — Ed.}

But *Subtabulation* was almost too late (for which I must take and accept the responsibility) for practical application since high speed computers made subtabulation unnecessary as such. Repeated interpolation using Chebyshev polynomials could, however, still be quicker than individual calculation for, say, the position of the Moon. But even this ceases to apply with the very high-speed machines of today (1977). It took me a very long time (in the late 60s and early 70s) to appreciate that it was quicker to calculate a long series of trigonometric functions, once programmed, than, say, to do a simple interpolation from stored data. The booklet *Subtabulation* was published in 1958, but, as far as I know, no reprints have been necessary!

### **Visit by Wilkins to the U.S.A. in 1957-58**

Wilkins spent 6 months at U.S.N.O. at the invitation of Clemence, from February to September 1957, followed by 6 months at Yale University Observatory at the invitation of Professor Brouwer, until February 1958. It was not a satisfactory arrangement because it was financially very difficult for him with a wife and young son on a single man’s foreign service allowance (F.S.A.). At that time there was no established pattern of short-term overseas service with adequate allowances, and I do not recall getting much (or any) support from the A.R.. The normal term of duty

overseas was 3 years, and Wilkins went for only one year. All such staff matters (including approval to attend scientific meetings) had to be submitted to the Admiralty through the A.R., and then through Hydrographer to C.E. Branch. It was not easy to get a particular, non-standard, case through and it was a great pity that we were not able to do better for Wilkins. But I think that he did derive some benefit from his visit, particularly his friendship with Duncombe, who followed Woolard as Director, U.S.N.A.O..

{I believe that my visit to the U.S.A. was of considerable benefit to my work in the R.G.O. as first of all it gave me experience in programming and in the operational procedures for the use of the electronic computers. Clemence set me the task of improving the orbital parameters for the satellites of Mars and the results proved to be of wide interest. Then at Yale I attended lectures by Brouwer on celestial mechanics that proved invaluable when I was later given the task of producing a new ephemeris of the Moon from an updated version of Brown's theory of its motion. I also attended other lectures and conferences that widened my knowledge and introduced me to many astronomers with whom I would work later. — Ed.}

### **Prediction service for artificial satellites**

The West Building was completed during 1957, and the N.A.O. moved into its spur in October 1957 during the weekend in which the first U.S.S.R. satellite, Sputnik I, was launched. Woolley had (by accident and sheer bad luck) hit the headlines, on arriving to take up his appointment as A.R., by his remark "Space travel is utter bilge" and he was opposed to any form of space research. The successful launch of Sputnik I put him on the spot! On the same day the A.R. received a call for help from Ryle in Cambridge regarding the interpretation of radio signals from Sputnik 1, which he was tracking by using Doppler techniques. He appealed to Woolley for help in orbit calculation and prediction; Woolley rang up and instructed me (in the middle of the move and at a few hours notice) to go up to Cambridge and help the radio astronomers. I took Candy (who was working on comets) with me and we left the same day. We got to Cambridge at about 6 p.m., spent a few hours in the out-station listening to the 'transits' each 90 minutes. We found that Ryle's team had already sorted out their observations, at least for the time being. It was interesting to see the team at work, timing the maximum radio transmission on each passage. We made a few elementary deductions about the orbit, but we were not able to make any significant contribution in the one day we spent there. It was a long time before King-Hele (at the Royal Aircraft Establishment at Farnborough) and others established an adequate theory. Quite frankly, I had forgotten most of my theoretical mechanics.

It was from this that we set up in the N.A.O. an elementary satellite prediction service and acted as a coordinator for observations made in the U.K.. It was not elaborate but I think it was satisfactory; but we lacked a computer (and we had to do the computing by hand using graphs) and we had need for a signal organisation. It was crude and empirical, but nevertheless it worked, and it provided observers with the data required for their observations. In turn, these enabled King-Hele to derive the principal coefficients in the expansion of the expression for the Earth's gravity field. After only three months, however, the prediction service was transferred to the R.A.E.. If my memory serves me correctly it was some time before they were as efficient as us.

I took an initial interest in planning the operation, but the real hard (and it was demanding) work was done by Scott, Candy and Taylor, with assistance from others.

The actual work of prediction was done mainly by G. E. Taylor, although Scott (as usual) supervised the work, and organised the circulation of the predictions. Scott and Taylor made nightly observations. Taylor was also the organiser of the B.A.A. observation team, which supplied results leading to the first determination by King-Hele of the Earth's oblateness by this technique. Over the years Taylor was the most prolific observer of artificial satellites, and Scott nearly the same.

The A.R.'s attitude to this work was unpredictable. It was tolerated, but not encouraged. Later, he (perhaps justly) criticised me and the N.A.O. for not jumping on the bandwagon and analysing the observations as R.A.E. so successfully did. We did not have an adequate computer, we (or should I say 'I') were not interested in the mainly geophysical results and we did not have, immediately at hand as it were (though undoubtedly we should have), the theoretical 'know-how'. Looking back, I think that we were right not to attempt to devote an all-out effort (anything less would have been pointless) to 'space research' of this kind. Certainly I could not have predicted what, if any, support we would have got from the A.R..

### **N.A.O. accommodation in the West Building**

The West Building provided the first 'custom-built' new offices that the N.A.O. had occupied, other than the hutments at Ensleigh in Bath. Although encouraged by Spencer Jones and the architect to plan for adequate expansion, we *reduced* the amount of space originally allocated, as being too large for the N.A.O.. In fact, if I remember correctly, we cut out a whole section, although by repositioning the staircase we did not lose the whole of the space. We had allowed ample room for storage, so that when additional space was required for the ICT 1201, and later for the ICT 1909, it could be provided by transferring the stores (particularly publications) elsewhere. The N.A.O., as such, has never required more space and now (1978) the spur provides machine and office accommodation for other departments as well. The West Building provides good accommodation, but its architectural design and construction is far from good; in particular, the windows (and/or the walls) were not rainproof. The first really violent south-westerly gale brought rain that flooded most of the rooms on the west side, including the lower-ground-floor computing room, to a depth of several inches. Elaborate (and noisy) repairs to the windows and surrounding brickwork were done on at least two occasions at considerable expense and discomfort. Double-glazing has now (1978) been fitted.

### **Staffing matters**

This might be a convenient time to pay tribute to the number of voluntary observers in the Office. For many years we tried to observe all occultations visible at Herstmonceux, using the solar telescope; even I tried my hand! In connection with the investigations into dip, refraction and irradiation, many of the staff made many observations in daylight. None of these was considered as official observing duties and did not qualify for an allowance. The staff must have been very keen! Later there were 'expeditions' to observe grazing occultations; members of other departments took part in these.

Under the terms (unwritten) of his appointment, Porter was encouraged to carry on his work on comets; he was in fact given the job of writing the explanation of *Planetary Co-ordinates 1960-1980* with the full comparison of different methods of calculating perturbations. He carried on his work of forming a complete list of comets,

as well as the usual report on comets for the R.A.S.; but he also got Candy and a vacation student, B. G. Marsden, interested in comets. Marsden was a student at Oxford, with a poor degree; after some time I formed the opinion that he was lazy and I told him to get a move on if he was to get a Ph.D.. What I did not appreciate was that he did not do the work I gave him because he was spending most of his time working for Porter! He became an authority on comets, and also on minor planets. My error of judgment.

About this time Harding and Candy were transferred to the Astrometry Department; we were sad to lose the services of Harding, but we did not have work suitable for his ability. The A.R. assumed that I was getting rid of a dud, but Hunter quickly disproved this.

### **The I.A.U. General Assembly in Moscow in 1958**

My next visit was to the General Assembly of the I.A.U. in Moscow in August 1958. Not only was I the Assistant General Secretary, but I was also still President of Commission 4. Much of the discussion at its two meetings was devoted to the impact of artificial satellites on astronomy and to preliminary values of the flattening of the Earth; and to the definition of Ephemeris Time. It was agreed to adopt the revised definition that Clemence and I had submitted. We also discussed the exchange of computations, with the result that *Apparent Places of Fundamental Stars* was to be taken over by the Astronomisches Rechen-Institut in Heidelberg, from 1960; this was in recognition of their intention to cease publication of the *Berliner Jahrbuch* as from 1960.

### **The acquisition of the ICT 1201 computer**

One serious effect on the N.A.O. of Woolley's administrative style was in connection with the ICT 1201 computer that was installed in the Office in August 1959. The history of the acquisition of the computer is bizarre, though whether this had any effect on the choice is doubtful. We had, after consultation with Woolley, made approaches to the Hydrographer for the replacement of the punched-card machines (the IBM 602A etc.) by an electronic computer. We then put in a claim, which was sent from the A.R. through the Hydrographer, to the Admiralty for a computer, stating our requirement.

I cannot now remember the details of that approach (I do not think it was well handled by C.C.A. to Hydrographer), but some time later the Hydrographer wrote to the Astronomer Royal requesting that full cases, supported by appropriate arguments, be submitted for all items of equipment in certain categories for presentation at the annual allocation of grants at the meeting chaired by the Deputy Controller. The letter did not specifically refer to computers, but they were on the agenda. Apparently Woolley glanced at it, saw that it referred (among other things) to cranes and other dockyard equipment and lost it! C.C.A. telephoned me a day or two before the meeting to enquire who was to represent the R.G.O. at the meeting and why we had not put in our bid for a computer. I checked with the A.R. and he said that he had a vague feeling that he had received such a letter, but he did not think that the R.G.O. was concerned. I duly went up to London (Bath?) and was very glad to have guidance from the C.C.A.. The meeting was held in a large room with the chairman seated on a platform, like an auctioneer. He started by saying that the appropriate Vote was two or three times lower than the total of the claims, and he would have to be drastic. There were some hundreds of items in the lists, and every one had a case carefully prepared. I did my best, but I was on a very

sticky wicket and the computer was something beyond his understanding. I did succeed in preventing the item 'computer for N.A.O.' being struck out completely (as so many items were), and he referred it to a committee. But we got no allocation from the Vote, and the money had to be found from other sources. This (but possibly there were other reasons as well) was the main reason why we got the wholly unsatisfactory 1201 instead of the more expensive English Electric DEUCE for which we had bid.

The introduction of the ICT 1201 (which was first known as the HEC 4, where HEC stood for Hollerith Electronic Computer) was a serious error of judgement. I should certainly have taken a firmer stand against it as it was the first product of a line of relatively inefficient machines by B.T.M.C. (later I.C.T). There was some pressure for the introduction of an electronic computer, but the small chance of getting an adequate model was lost, although whether we could have succeeded is questionable, as our case was weak. I think, using hindsight, that we should have waited for several years, continuing to use the 602A. It is my normal habit to wait, and not to rush in for every new development, and I should have insisted that we did so then. The greatest loss was in the considerable wasted effort by Wilkins and Carter in organising the installation, operation and programming of the 1201. It provided them with many problems in teaching staff to program and operate it. The programming was elaborate, so much so that I gave up my attendance at lectures, and never learnt to program it (or any other computer). We had to write all the basic software that would have been available on the much more powerful DEUCE. It is my opinion that it is only in relatively recent years that the cost and effort of running a computer has been justified by the work it makes possible. Woolley's attitude to the computer (and to later ones) was ambivalent: R.G.O. departments could make use of the computing facilities of the N.A.O. provided N.A.O. staff essentially did the programming. It took a long time before a reasonable solution could be reached, and full use made of the computer facilities. The machine was finally installed in August 1959.

It is hoped that A. E. Carter, just retired from the post of 'Computer Manager', will write a connected account of the computing equipment in the N.A.O. from the early 1930s to the present (1978). {He did not do so. Ed.} I will make no further comments here, except to say that I personally played little part in the development of the computing facilities in the past 20 years; all the work was done by Wilkins and Carter, who deserve all the credit for considerable achievements in the face of great difficulties (e.g. not being able to obtain the equipment best suited to N.A.O. needs).

### **Staff changes**

The need for computer operators for the ICT 1201 meant some changes in the junior establishment, in addition to the frequent changes in staff due to wastage (usually marriage). The number of internal marriages continued at its earlier high level — with some considerable advantages for the N.A.O., which (because of its relatively large staff of Scientific Assistants) provided many of the brides. They were able to continue proofreading at home after they had started their families; their work was much appreciated by all.

### ***The Explanatory Supplement***

During my visit to Washington for the meetings of the International Council of Scientific Unions (on which I represented the I.A.U.) I stayed with the Clemences. Clemence had just been promoted to Scientific Director of U.S.N.O. and his successor

as Director of the N.A.O. was E. W. Woolard. Ever since 1954 when agreement had been reached on the 'conformity' of the *American Ephemeris* and the *Nautical Almanac*, we had been discussing the *Explanatory Supplement to the Astronomical Ephemeris and to the American Ephemeris and Nautical Almanac*. It will be recalled that it was agreed, between U.S.N.O. and N.A.O., that it should be a joint publication with contributions from both offices. The whole was to be edited and produced by the N.A.O. — a sizeable task. We had drawn up a synopsis, with indications of the chapters for which each of us would be responsible. We discussed these with Clemence when he visited us on the way to Moscow, and while I was in Washington I had the opportunity of revising these with Woolard. I, quite frankly, did not get on as well with Woolard as I did with Clemence; he was a great theoretical expert on spherical astronomy and his view was that the Supplement should be absolutely correct. He was, in his own way, a perfectionist so that in the material he personally contributed (e.g. to the chapter on Time) every tiny detail was elaborated. We had many differences of opinion, mainly in respect of details of presentation, but occasionally on matters of substance and of fact. I found that some of his explanations were so involved that I could not follow them and so I added bits to his chapters on time and on eclipses, but I am sure he was right. On the whole, however, I think the collaboration worked well.

The origin of the *Explanatory Supplement* is described therein, together with a list of contributors. It took a long time to prepare, and I am certain that it was well under way by 1957. Copy was completed by about the end of 1959, but it was not published until 1961. There was a lot of checking to do on the proofs, and the revisions were quite substantial in some cases. It is a tribute to a collaborative effort, and I can now express my thanks to all who contributed to it. In particular Wilkins edited the volume, and one glance at it will indicate how expert he was. He was assisted by a young Scientific Assistant, Miss A. Springett, who had a natural aptitude for the often dull, uninteresting (to the general understanding) and painstaking sub-editorial work required to maintain a high standard of presentation and consistency. She retained her interest and, when she left the Office, she took up a position in a London publishing company.

The publication of the *Explanatory Supplement* was a great relief to me as I had promised it in 1942. It was partly this which led to my disinclination to seek the directorship (or not to be disappointed at not being offered it!) of the Mathematics Division of N.P.L.. I felt it was a promise I must keep, but I could not have done it without the help I received from the staff.

On the other hand, the *Explanatory Supplement*, as a record of the derivation of the data in the A.E., was either ten years too late or ten years too early! {The abbreviation A.E. serves for the *Astronomical Ephemeris* and for the *American Ephemeris*. Ed.} It fitted in well with the unification of the two almanacs, and so enabled the one book to serve for both. It described, however, methods and techniques that (with one or two exceptions) could have been so described ten years earlier. On the other hand, it was too early for the age of the computer, with the enormous simplification of methods that it has made possible. Direct calculation of the effects, for example, of precession and nutation, using exact formulae now replace the necessarily approximate expansions previously used. Its purpose, however, was to show how every quantity in the A.E. was derived, and much of this is undoubtedly useful to the programmer, but quite different methods are now used. Many sections are of permanent utility.

One such section is that on the authorities used for the solar, lunar and planetary ephemerides in former editions of the A.E. (compiled by Richards); another is on the calendar (written by Woolard); while the section on Computation and Interpolation (mainly by Wilkins) indicates the interest the N.A.O. has in such matters.

### **Other activities in 1958 – 1959**

I was pretty busy during the second part of 1958 and during 1959. There were only three papers by me on navigational matters. The rest of the Office were not idle; I.A.U. Commission 40 drew up a list of 37 radio sources, which formed the basis for enlarged occultation predictions. We got some relief in that 1959 was the last year in which we were responsible for the preparation of A.P.F.S.. There was, however, a lot of extra work to do because of the changeover from the *Nautical Almanac* to the *Astronomical Ephemeris*.

With the A.R.'s approval I had invited the Executive Committee of the I.A.U. to meet at the R.G.O. in 1959. It was a glorious summer, and I can recall our first informal meeting in the sea at Bexhill. This was the year that the R.G.O. Clubhouse was built, with few, if any, interruptions for rain.

Later in the year I had a meeting of the Executive Board of I.C.S.U. in the Hague. Nothing of relevance to the Office transpired, but we had a good time, including a marvellous Government dinner!

### **Promotion to D.C.S.O.**

On a more personal note, Woolley had, very kindly, put me forward for a Special Merit Promotion to Deputy Chief Scientific Officer, in early 1959. He had got Atkinson so promoted, and later would do so for Eggen. He was also successful in getting many P.S.O.s (including Bernard Pagel) promoted to S.P.S.O.. He was a member of the Board and took much pride in his achievements in this field. In my case I cannot help feeling that the Special Merit Promotion was not suitable; it was awarded for outstanding scientific achievement and was conditional on being relieved of administrative work and given freedom for research. Neither applied to me, especially as I was then General Secretary of the I.A.U. and deeply involved (personally) in the mainly non-research activities of the Office (*Explanatory Supplement*, etc.). My limitations in the field of original research were well appreciated by myself and could hardly be removed at the age of over 50.

I attended the interview board, which seemed to have an enormous number of members, only one of whom was an astronomer, namely the A.R., who kept quiet. The questions asked were largely on radio and space research and I (apparently) answered them reasonably well. Many months passed without me hearing the result, until Eggen told me that I had been promoted. I was rather annoyed that I should have heard from Eggen that I had been successful in my interview before the Special Merit Promotions Board. Woolley quite rightly made no comment after the Board meeting, but Eggen had known for a long time before he told me and was surprised that I did not know. Shortly afterwards I received the official letter from the Admiralty. Promotion to D.C.S.O. was certainly most agreeable, but I would much have preferred to have received it in a less devious manner. With my commitment for the next 6 years to the I.A.U., and no suitable replacement as Superintendent (even if there had been a post) I just carried on as before. This was agreed by lack of protest by the Admiralty and the A.R., but it gave rise to comment later from the S.R.C..

I should mention here that the A.R. proposed me, together with others, for Fellowship of the Royal Society. We did not get elected. I think that he did not appreciate the standard required; I certainly did not expect to be elected.

## CHAPTER 15

### Period as General Secretary of the I.A.U.

#### Relationships of N.A.O. and R. O. Cape to R.G.O.

There was only a gradual change of relationship between R.G.O. and N.A.O., starting with a freer exchange of junior staff and, with the transfer of Harding in 1956, rather greater flexibility of senior staff. There was still a separate complement for the N.A.O., but overall 'ratios' of the various grades applied to the R.G.O. as a whole. During this period there was perhaps rather less arbitrary restriction on numbers. There was, however, a major change in 1959 in the relationship between the R.G.O. and the Royal Observatory at the Cape of Good Hope. This is shown by the following extract from A.R.'s Report of 1960 June 11.

"On 1959 May 21 the Lords Commissioners of the Admiralty approved in principle a joint recommendation by the Astronomer Royal and H.M. Astronomer at the Cape that the Royal Observatory at the Cape of Good Hope should be connected with the Royal Observatory at Herstmonceux, in the same manner as H.M. Nautical Almanac Office is connected."

I was highly amused when this was announced as no-one knew *precisely* how the N.A.O. was connected to the R.G.O.. If I remember correctly, the original arrangement, in 1936, was that, in future, the N.A.O. should be under the direction of the Astronomer Royal; I have tried, in this personal account, to say how this was interpreted *in practice* (which is all that matters). It was obvious that there would be an increasing tendency towards full integration; which would be enhanced by each change in the A.R.. The change to S.R.C. could not then be foreseen. There were many practical reasons (mainly difficulty of recruiting staff) why H.M. Astronomer should seek more formal assistance from R.G.O.. I foresaw, however, that the agreement meant the end of the independence of H.M. Astronomer and possibly of the Cape Observatory, but not in the way in which it came about.

There was an increase in the exchange of staff between N.A.O. and R.G.O., as has already been mentioned, and I am quite certain that this proved beneficial to both. Without attempting to give dates (or to mention junior staff), the principal changes were: Richards to take charge of the R.G.O. publications as he was much experienced in editorial work; Dickens (taken on as an A.E.O., but clearly primarily interested in astrophysics) to work for the A.R. in his 'department', where he has done extremely well; and L. V. Morrison from the Meridian Department to the N.A.O., where he found his interest in occultations and the secular accelerations of the Sun and Moon. Grimwood, who had transferred to N.A.O. before the war, was transferred to R.G.O. (and I think back again to N.A.O.) before opting to go to South Africa. Grimwood was put in charge of a small group (including Norman Rhodes and Arthur Cordwell) to make observations of artificial Earth satellites using a kinetheodolite from R.A.E.; they did a good job at Herstmonceux. The group was given a room in the N.A.O. spur. It was decided to send the instrument to the Cape, where the observations would be greater in number and in value; Grimwood did a good job there too.

### **Appointment as A.G.S. of the I.A.U.**

During the meeting of the Consultative Committee for the Definition of the Second (C.C.D.S.) in 1957, at a 'party' at the Paris Observatory, Danjon, the President of the I.A.U., invited me to take on the post of Assistant General Secretary and to accept nomination as General Secretary to succeed Pieter Oosterhoff, who had succeeded Bengt Stromgren in 1952. I obviously could not accept on the spot; I said I would discuss it with the A.R. and let Danjon know. The A.R. was far from enthusiastic; he raised no objection to my accepting, but he did not offer me the support that I must have if I became General Secretary. The A.R. was at this time a Vice-President, but he was not then, nor later, very keen on the I.A.U., except as means of getting something from it. (I can recall a demand from him for a catalogue of stars on which he was working, and when I pointed out that this was a matter for the commission of which he was a member, and not of the G.S., he was furious.) His lack of enthusiasm, and my feeling that my responsibilities to the N.A.O. should not allow me to accept, overcame my desire to take on a job that I thought that I might be able to do reasonably well; to follow men such as Fowler, Stratton, Oort, Stromgren and Oosterhoff was a great temptation. I accordingly wrote to Danjon declining his invitation. Danjon wrote back, saying it was the wish of Oosterhoff and other members of the Executive Committee that I should accept, and that they would discuss it at the next meeting. In the meantime, Danjon and others persuaded Woolley to put pressure on me to accept — he was to convey the result to the Executive Committee at its meeting in Liège in July 1957. He was, I think, impressed by the 'demand' for my appointment and he agreed to make available to me, if I accepted, some assistance from the R.G.O.; and so he duly persuaded me to change my mind. The position was left to him, though there was little doubt that, if both the Executive Committee and he agreed, I would formally be nominated at the meeting. On the day before the meeting, Woolley broke his toe (apparently he stubbed it on a chair in the dark when walking around in bare feet) and it appeared that he would not be able to attend the meeting. But the local doctor patched him up and he went a day late. The Executive Secretary appointed me Assistant General Secretary (A.G.S.). In view of the past history of Superintendents I had made it an absolute condition of my acceptance of the post that I received Admiralty approval to do so. Woolley was not keen on making the submission to the Board, through Hydrographer, but he eventually did so. We received a reasonably guarded approval, as much as I think the Admiralty could have expected to give.

My duties as A.G.S. were light, and consisted mainly in correlating the symposia that were being held before the General Assembly (G.A.) in Moscow in August 1958. There was little for me to do in preparation for the G.A. itself, but I did a lot of editorial work on the draft reports.

### **Appointment as General Secretary of the I.A.U.**

From my own point of view the most important and far-reaching event of this period was my election (appointment is perhaps the more correct term) to the post of General Secretary of the International Astronomical Union. I had taken an interest in I.A.U. affairs (in addition to matters arising in the Commissions with which I was concerned) since 1948 when I was acting-Chairman (in the absence of the Canadian who had to leave the General Assembly early) of the Finance Committee. The finances were complicated and I learnt a great deal about the I.A.U. from Jan Oort, who was then General Secretary and who had carried the whole burden of the I.A.U. in extremely

difficult circumstances from 1938. I was appointed Assistant General Secretary in 1957 and I was General Secretary from 1958 to 1964, being responsible for the organisation of the General Assemblies in Berkeley, California, in 1961 and in Hamburg in 1964. These duties are not as such relevant to the Office, and will not be mentioned again except when they impinge directly on the Office, or are particularly interesting.

Early in 1958 I was invited over to Leiden to discuss with Oosterhoff the handing over of the I.A.U. work. I was much impressed by Miss Nel Splinter, and I later persuaded her to come and work for me in Herstmonceux. She agreed and made my life very much easier for the next four years. She spoke, in addition to her native Dutch, English, German and French; and she was (and is) a delightful person. Nel, who was 'Miss IAU I', gave me invaluable assistance and was, and is, one of the most hard-working, cheerful and effective persons I have ever known. A separate room was provided in the Office for Nel, but otherwise there was very little support for her; there was mutual help, of course, but no postage or telephone costs, except for local calls. One event that annoyed me was when Woolley (without asking me) sent Nel a long paper in French to be translated into English. Nel was very busy, but I felt that we should manage it if possible; I think that she did it on a Sunday. He then complained to me "that Nel did not know French well" as she had translated 'actuellement' as 'actually', instead of 'presently'. After all French is her fourth language!

I should remark here that I did my best to separate the N.A.O. work from that of the I.A.U.. My first duties in the Office were to go through the Office post and dictate replies, when necessary, to Miss Perry or to Miss Celia Hewerdine from the Typing Pool; I then called in Nel for a similar function for the I.A.U.. The great amount of I.A.U. work was done on a Sunday, when we could work peacefully, but I am afraid the work did spill over at other times, when there were matters that Nel could not deal with herself.

In October 1958 I had a meeting of the I.C.S.U. Executive Board and the General Assembly in Washington; I was one of two representatives of the I.A.U. (the other was the President Oort). I was not impressed with the bureaucracy of the organisation; one of the things that gave me, and Nel, much trouble was the annual report, which demanded full accounts of expenditure on all matters on which the I.C.S.U. grant had been spent. It got better later when I became a member of the finance committee!

One thing of interest was the (official) threat of the U.S.A. to withdraw all support from the planned General Assembly in the U.S.A. in 1961 if the Union did not admit Taiwan (which they called China) to membership. It was an unfortunate beginning to my term of office as General Secretary. We did manage to overcome this, with the loss of China, which did not rejoin the Union until 1982. The I.A.U. statutes did not allow Taiwan to be excluded from membership though the motive of the U.S.A. was, undoubtedly, to remove China.

In the spring of 1963 Nel Splinter decided to return to the Netherlands in order to further her career; the next General Secretary of the I.A.U. was to be J.-C. Pecker, and she did not wish to move to Nice. Fortunately, Pecker knew of an American girl in Paris with both the requisite secretarial skills and an interest in astronomy. So Dorothy Bell (Miss IAU II), from Mobile, Alabama, joined me; she was another extremely competent (in a rather different way) person. Dorothy Bell had her first taste of an Executive Committee meeting in Erevan, and she did remarkably well. She therefore had a good background for the preparation of the General Assembly in Hamburg in August 1964.

I was exceedingly fortunate to have had two such helpers; they much reduced the time that I had to devote to I.A.U. affairs. Both were agreeable to working on Sundays, and I certainly spent almost every Sunday in the office during those years — often taking 2 hours off in the morning to play tennis, usually with Woolley, Carter and Wilkins. In spite of that, and doing much work at home in the evenings, it was inevitable that I should have taken a considerable amount of time away from the official duties of the Office of Superintendent. Being General Secretary involved (apart from the administrative and editorial work) attendance at two meetings a year, one of the Executive Committee of the I.A.U. (usually 3-4 days) and one of the Executive Board of I.C.S.U. (usually 4 days). I tried to avoid using expensive facilities and calling on the N.A.O. staff for assistance, but the staff (particularly Miss Perry) were, at all times, exceedingly helpful, especially when Miss Splinter was on leave.

The six years 1958 – 1964 were very full ones for me, but I had much ‘job satisfaction’ with the I.A.U., made a great number of friends in all countries and had many enjoyable visits abroad. I doubt very much whether I should have been able, if I had not taken on the office of General Secretary, to make a significantly greater contribution to the N.A.O.. I was then 50 and had done essentially *no* research since I left Cambridge after one year of rather abortive research in statistics; until 1948, when I was 40, there was no opportunity for research in astronomy as such. From then onwards until 1958, I was engaged in work on ephemerides and navigation, together with the details of unification, which, however useful they might be, could not be regarded as research. The only field open for research in the N.A.O. should be that of celestial mechanics and I had hoped to be able to make some contribution to the theories, and ephemerides, of bodies in the Solar System. Even with all the time available, and unlimited access to the rapidly-developing electronic computers, I am pretty certain that my ability and experience would have been quite inadequate to make any significant contribution.

### **Overseas visits in 1960 – 1963**

In 1960, I had three visits abroad. The first was a meeting of the Executive Committee of the I.A.U. in Prague, in which we made many friends. The second was to Lisbon, where the Executive Board of I.C.S.U. was meeting; I was there put on the Finance Committee to try to sort out the desperate state of the finances. I then worked every evening with the Treasurer (Laclavère) in our extra room at the hotel with a bottle of whisky. I am afraid that this was the pattern for me for all subsequent I.C.S.U. meetings. The third visit was to California to plan the General Assembly of the I.A.U. in 1961. I went via Washington where I stayed with Clemence. I do not recall what Clemence (and Woolard) and I talked about, but in spite of all our agreements about publications, there was still plenty to discuss, such as refraction and above all the definition of the second. We realized that the second of ephemeris time would not long satisfy the need of the International Committee on Weights and Measures (C.I.P.M.), but we were quite ignorant of atomic time. There was also a meeting of W.P. 53 in Washington, which I also attended for a single day! There was nothing more that we could discuss about astronomical navigation. This was the last meeting that I attended. Later meetings (including some in Australia, which had now joined) were attended by Scott and Taylor.

The main event of 1961 was the I.A.U. General Assembly on the campus of the University of California at Berkeley, near San Francisco. It was preceded by a

symposium on 'Space Age Astronomy' in Pasadena, which was also attended by Wilkins.

In August 1962 I attended a meeting of the Executive Committee of the I.A.U. in Erevan in Armenia. We were the guests of the President, Ambartsumian, and really had a fabulous time. This was followed by a scientific meeting in the Crimea. Considering the fact that Ambartsumian very rarely wrote to me, and in answer to my letters said that he agreed with all I said unless he sent me a telegram, he conducted the business of the Executive Committee admirably in English.

In October 1962 I went to a meeting of the Executive Board of I.C.S.U. in Prague. Nothing of interest to the N.A.O. except the opportunity to meet old friends. We were delayed at London airport because of fog in Prague, but all the English people going to the meeting elected to go on a small Czechoslovakian aircraft that could land in fog. We all stayed in the International Hotel, comfortable enough but inferior to the Yalta (!) where we stayed before in 1960.

In May 1963 I (accompanied by Stoy and Wilkins) attended a meeting on the system of astronomical constants in Paris (I.A.U. Symposium No. 21). I cannot recall whether Clemence and I went to Heidelberg on this occasion or not; if so we had discussions with Fricke on relevant matters, and he drove us to Paris. But it was Wilkins who was appointed secretary of a Working Party to draw up the 1964 System for presentation to the General Assembly. We had much discussion on the system of constants before we left for Paris; I had very little to do with what followed. Wilkins arranged a meeting of the Working Group in the N.A.O. and wrote the report for the General Assembly with great efficiency.

In June, Stoy and I attended the Executive Committee of the I.A.U. in Liège. The main topic was the organisation of the 1964 General Assembly in Hamburg, but we did refer to the outcome of Symposium No 21.

Scott went to Washington in October 1963 to attend a meeting of W.P. 53 and to discuss sight reduction tables and almanacs with U.S.N.O. and U.S.H.O.. I cannot remember whether we had yet formulated the outline of the sight reduction tables for surface navigation.

In November I attended a meeting of the Executive Board and the General Assembly of I.C.S.U. in Vienna. The meeting was considerably upset by the news of the assassination of Kennedy. We had organised a dinner party (four British, two Swiss, one Frenchman and perhaps others) and when the waiter dashed in to tell us the news, we could not at first believe him. Then one of the Swiss (who was an uninvited guest) said "He deserved it". We had the greatest difficulty in keeping the peace, and the party broke up.

At that meeting a vote was taken on whether the General Assembly should be held every two years instead of three. The I.A.U. voted for three years, and this was carried in direct opposition to the officers of I.C.S.U.. By dubious tactics, and much lobbying, the vote was retaken on the following day and the decision was reversed. Earlier I had been appointed a member of the committee to revise the bye-laws; we had several meetings, the final one starting at five and going on until nearly midnight. No wonder Flora said that I did not have time to see even the Danube! Ambartsumian, Pecker (who was representing France) and I visited the Observatory on a courtesy call.

In early December there was a meeting of C.C.D.S.. I do not recall what happened, but I judge that there was so much discussion as to the accuracy of caesium or the hydrogen maser that it was decided to continue with the E.T. second. It was not until October 1967 that the definition was replaced by the present one based on a caesium transition.

### **The I.A.U. General Assembly in Hamburg in 1964**

The I.A.U. General Assembly in Hamburg in 1964 was my second Assembly and so we avoided the mistakes of the first, as far as I could. I paid an advance visit to Hamburg, where I had discussions with Heckmann, and much more profitably with Haffner. He drove badly, with guidance from the stars, as compared with Heckmann, who had a large Mercedes which he drove very carefully. But Haffner was the man who organised the Assembly precisely, in distinction from Berkeley in 1961! [The one fault was that at the closing dinner the formal speeches could not be heard, but I am sure he was not responsible for this.] Heckmann met me at the airport and took me to my hotel where I checked in, leaving my case to be taken to my room. We then went to the University where he introduced me to the Principal and to Haffner, who showed me round the facilities prepared for our use, and then showed me around Hamburg. I just had time to meet Heckmann for dinner, where he was most courteous, but not about the things that I wanted to know. We arrived back at the hotel well after midnight, with Haffner calling for me at 8 a.m.. My chief regret was that I did not have time to appreciate the magnificent room in which I stayed.

The main item of interest to the Office was the adoption of the I.A.U. 1964 System of Astronomical Constants, for which we can thank Wilkins. The rest of the General Assembly was dominated by a first showing of pictures from Ranger of the craters on the Moon. After the General Assembly we went on a day trip to Berlin to see the wall and to look around. With my responsibilities at an end, we had a few days holiday in Copenhagen, where we had the flat in the centre of the city belonging to Captain and Mrs Schmidt with whom we had kept in touch since his visit to the Office in 1946. Dorothy Bell also came with us.

### **The Federation of Astronomical and Geophysical Services**

In 1964 I was appointed the I.A.U. representative on F.A.G.S. (the Federation of Astronomical and Geophysical Services), which was responsible for the 12 (it may be more or less today) services that had been set up on a permanent basis for the collection and publication of observed data in certain fields. Our annual meeting, under a good secretary and president, lasted half a day! But we usually had to go to Paris to meet. I was elected Vice-President in 1967; but there were yearly meetings until I was elected President in October 1968. I attended only one meeting, in 1969, after that. There was a meeting planned for November 1970, but on the day before I felt unwell and I did not attend. Garland took over the meeting and either then or later I resigned the Presidency. Fortunately (for F.A.G.S.) the I.A.U. appointed Wilkins as its next representative.

### **N.A.O. activities in 1960 – 1964**

I can remember little of what I did for the Office in 1960-1964. Certainly observations were continued into the effects of irradiation of the Sun's upper limb, and I analysed them. These results were published only in N.A.O. Technical Note No. 12 since they were negative.

The work on occultations continued, with increasing emphasis on the predictions for radio sources and assistance with their identification with optical objects. The transfer of Morrison from the Meridian department was of great benefit to the Occultation Section, both then and in later years. Taylor continued with his predictions of the occultations by planets and minor planets; he was an enthusiast, but he could not write a paper. I spent some time putting his drafts into shape. At this time he was assistant to Scott in the Navigation Section, and I had to tell him he was neglecting his duties.

### **Retirement of Dr. Porter**

The main event in 1961 in the Office was the retirement of J. G. Porter in June after 12 years service. For some considerable time he had been troubled by a heart condition and he felt that he had plenty to occupy himself at home with his editorial and broadcasting activities. He had supervised the calculation and preparation of the fundamental ephemerides since he joined the Office, had done a great deal of work on *Planetary Co-ordinates 1960-1980*, including the collection of formulae for, and examples of, various methods of special perturbations. In particular, he had encouraged Candy to take an interest in cometary work, as well as B. G. Marsden, a vacation student from Oxford, who originally seemed to lack both application and ability, but who later (as at present) became the established authority on the subject. Porter had concentrated on cometary work, and did not appreciate the necessity of exercising supervision over the work of his section of the Office. There were several mistakes in the *Nautical Almanac* (or *Astronomical Ephemeris*) that could reasonably be due to him or I suppose to me! His place was taken by Wilkins. His heart condition has now (1977) become considerably worse and he has difficulty getting about. {He died in 1981.}

### **N.A.O. Reunion in 1963**

On 27 April 1963 there was the first reunion of the N.A.O. staff. It was organised by Miss Perry, who formed a link between the staff at Bath and Herstmonceux. It was a remarkable occasion with many people travelling long distances to come. There was a large attendance of pre-war staff, most of whom had kept up with Miss Rodgers by correspondence. Mrs Betty Atkinson, who had been Comrie's second wife, was there, as was E. T. Silk, who had been my secretarial assistant, from the Hydrographic Department, before Miss Howard; he was living in Battle. I think that all the "Ants", who compiled the pre-war *Astronomical Navigation Tables*, were there. They were joined by a group of us who were still serving. It brought home memories of pre-war Bank Holidays when the staff organised staff outings to such places as Whipsnade and Windsor. After the war the staff in Bath organised trips to Weston-super-Mare and Lyme Regis. The only one that I can remember at Herstmonceux was the visit in 1967 to Greenwich and London in celebration of the bicentenary of the *Nautical Almanac*. [There were, after my retirement, further reunions in 1974, 1982 and 1987.]

### **Replacement of the ICT 1201 computer**

In 1961 or 1962 we started proposals for a replacement computer for the ICT 1201. In the Annual Report for 1962/63 it was stated: "The need for its replacement has become urgent; its slow speed, small capacity, and difficult programming characteristics seriously limit its usefulness so that it acts as a deterrent, rather than as a stimulant, to research investigations". And in 1963/64: "The ICT 1201 is probably the least efficient computer still used in any research establishment, and its early replacement is

essential". Reference has already been made to the frustration, and additional work, imposed by the 1201 on the computing staff, particularly Wilkins and Carter. Some work (on the lunar ephemeris) had been done with hired time on an IBM 7090 in London and more was to follow. The Treasury O & M decided that there would have to be a full specification of requirements followed by 'bids' to fulfil the requirements by *all* the machine companies operating in the U.K.. We wanted a machine that would (apart from more technical characteristics) allow a measure of continuity with the N.A.O.'s 'investment' in punched-cards, and compatibility with U.S.N.O. machines and procedures, which were based on I.B.M. equipment. Relative simplicity of programming (at that time specifying adequate software services) was also a requirement. There were initially 11 or 12 companies in the 'exercise', but fortunately some dropped out before the separate visits to the Office of the sales teams. The discussions, involving individual meetings with teams of salesmen, and technical experts, with some 8 or 9 machine companies, took a lot of effort and time, much of which was wasted since most 'bids' could have been rejected out-of-hand. This involved a long and arduous task for the staff. We did not have the expertise to match their sales talk, but Wilkins and Carter put across our requirements and in some cases the sales staff were impressed and were friendly. One I.B.M. man (who lived locally) played tennis with us.

In due course we analysed their reports, and submitted a report to Treasury O. & M. Division. The Treasury had clearly made up their minds that the contract was going to a British company and we, whatever we said in favour of I.B.M. or other company, finished up with an ICT 1909. The exercise was undoubtedly inspired by the Treasury policy of appearing to be completely neutral and objective, while making it quite clear internally that I.C.T. would get the contract *unless* it failed to meet the specification significantly. The inevitable decision was most disappointing, especially for Wilkins who had put so much effort into writing the specification, analysing the 'bids' and making out the case for an IBM 360. I did not have the technical knowledge or the 'clout' to push our case sufficiently; I feel that in this case, and the previous one, I let the Office down. Whether I personally could have done more I do not know; N.A.O. had little 'muscle' and the political situation was weighted strongly against the installation of U.S. machines in government departments.

[But, when the Institute of Theoretical Astronomy was set up in Cambridge, Hoyle insisted on having an I.B.M. computer, and threatened to resign if he was not allowed to get what he wanted. I doubt, however, if my threat to resign would have had much effect! I.B.M. machines were also acquired for atomic energy/weapons research!]

In June 1965 a firm order was placed for an ICT 1909 for delivery in March 1966. The ICT 1201 was removed in September 1965 for extensive alterations to be made to the computer room to make it suitable for the 1909. Until it was installed in May 1966 we used an IBM 7090 computer in London on a service basis.

### **Transfer of R.G.O. from the Ministry of Defence to the S.R.C.**

The most important, and far-reaching, event in the recent history of the R.G.O. was undoubtedly the transfer, as from 1 April 1965, of control from the Ministry of Defence (Navy), which was the logical successor of the Admiralty, to the newly formed Science Research Council (S.R.C.). It had little immediate effect on the N.A.O., but it was clear that the 'special position' and separate identity held by the Office would eventually be lost, especially after Woolley and I retired.

Spencer Jones had discussed with me, on many occasions whether there would be any advantage to the R.G.O. (and N.A.O.) in seeking to transfer to the aegis of the Department of Scientific and Industrial Research (D.S.I.R.). It was one of the very few such matters he ever discussed with me! My view, then (and now), was that it would be disadvantageous. Spencer Jones' relations with the Admiralty were good and with successive Hydrographers (particularly Edgell and Day) excellent. Apart from administrative difficulties (the Admiralty was notoriously bad as regards staff matters) the R.G.O. had been treated well in major matters, with some (at least) helpful decisions. D.S.I.R. was in considerable difficulties (from my N.P.L. contacts) and I thought that the possible advantages of informed scientific control were only comparable to the positive advantages of being the effective scientific advisor to a non-scientific board of control. The R.G.O. Vote would always have to be squeezed out of a reluctant Treasury: it was really a choice of whether Admiralty (later M.o.D.) or D.S.I.R. had the greater influence. I do not think that, at any time, there was any question of 'telescopes or guns'.

I was consulted, in 1964, as to whether N.A.O. should transfer with the R.G.O. and be under the control of S.R.C. or whether it should stay with the Ministry of Defence. It was, I think, discussed by the senior members of the staff, but there was no real question of separation from the R.G.O., even if M.o.D. would have been willing to take N.A.O.. M.o.D. could only have justified having a unit for providing the astronomical ephemerides and tables required for navigation. Such a unit would necessarily be small, and would probably have to be assimilated in Hydrographic Department as part of a computing unit including Tidal Branch and the special branch dealing with astrographic projections, Decca lattices and similar numerical tables. I proposed that after the transfer to S.R.C. the N.A.O. should, with the minimum of administrative and financial control, continue to provide the Navy and R.A.F. with the astronomical data and associated tables, etc. that they required. It soon became clear that, although the above proposal had apparently been accepted in principle, S.R.C. was not prepared to forego the interdepartmental payment for services rendered. No question was, however, raised as to 'control' over the nature of the work done for M.o.D..

## CHAPTER 16

### Transfer to the S.R.C.

#### Transfer of the R.G.O. to the S.R.C.

The most important event in 1965 was the transfer of the R.G.O. and the N.A.O. from the Admiralty to the newly-formed Science Research Council (S.R.C.). The A.R. was consulted, but I do not think that he could oppose a government decision, even had he wished to do so. There was much more doubt about the N.A.O.; after careful study I gave as my opinion that we should stay with the R.G.O., though whether my views had any weight I do not know. The A.R. gave a formal luncheon party to mark the end of a long tradition and, hopefully, the start of a new one.

We had much difficulty in agreeing terms of service with S.R.C., but the appointments in the Civil Service remained almost unchanged. After this the main problem concerning the N.A.O. was the amount of subsidy the Ministry of Defence should pay S.R.C. for the work that N.A.O. did on navigational subjects. For many years the Air Ministry had been paying the Admiralty an annual sum for the work N.A.O. did in respect of the *Air Almanac* and other air navigational work. The sum, originally £5000 a year, was agreed at my estimate; it was varied (increased by reason of additional work on sight reduction tables, etc., and by inflation) from time-to-time without anything more than a telephone consultation with me. On more than one occasion some one from the Admiralty telephoned Miss Perry to ask if any change was necessary: she told them to hang on, came in to see me and I said “no change” or “increase to ..”, and everyone was happy. But this attitude towards internal government book-keeping was not acceptable to S.R.C., whose funds, admittedly, came from a direct Treasury grant.

I went to London to an S.R.C./M.o.D. meeting and I explained the method I had used, for nearly 30 years, in deciding the amount to be paid by the Air Ministry to the Admiralty for the work we did on the *Air Almanac* and *Sight Reduction Tables*. I recall, vividly, that I was annoyed when S.R.C. refused to accept an estimated lump sum (which M.o.D. would have preferred) and said that the amount must be fixed by a thorough cost-accounting exercise. I then lost my temper, as I frequently did, and I expressed myself rather forcibly. I proceeded there and then to estimate the cost (mainly by staff time, doubled to allow for overheads) and came up with the figure (I think) of £15000. They noted it, certain that it would be proved wrong.

In due course three accountants arrived at R.G.O. for three weeks to cost the whole of the work of the N.A.O. and the part appropriate to M.o.D.. They also costed the central administrative services of the R.G.O. and added an appropriate fraction to the N.A.O. costs. They interviewed each member of the staff and they finished up with a mass of paper. Eventually they produced answers that, of course, were no more accurate than the extremely inaccurate data on which they were based. But this did not prevent such entries as ‘cleaning the windows in the computer room’ being included in order to fix an hourly rental for the use of the ICT 1909 by the University of Sussex and occasionally other users. In exasperation, I asked them whether, under common services, they had taken into account the cost of feeding the ducks and geese on the

moat! The figure they came up with for the work done for M.o.D. was within a few hundred pounds of mine! That for computer rental had to be substantially increased to match the 'market price'.

The exercise was a complete waste of time and effort, typical perhaps of a completely new organisation. It was paralleled by administrative and committee procedures that were both time-consuming, paper-productive and not particularly efficient. Procedures have undoubtedly greatly improved with time, and the enormous load of decision-making processes (both administratively, financially and scientifically) is now handled very well, but still demands enormous quantities of paper and the time of many scientists. But very little, at this high level, affected the N.A.O..

We did not, however, have any trouble with our estimates while I was in the Office; all such matters were handled by the A.R. and the cashier.

### **Activities in 1965 – 1966**

The work of the Office during 1965 and 1966 was (as far as I was concerned) devoted to three things: namely, the preparation of the *Supplement to the A.E. 1968*, the celebration in 1967 of the bicentenary of the publication of the *Nautical Almanac and Astronomical Ephemeris*, and the publication of the *Sight Reduction Tables for Marine Navigation* being prepared in U.S.A. by U.S.N.O. and U.S. Oceanographic (formerly Hydrographic) Office. The latter involved much correspondence and detailed design, and I personally spent a great deal of time on them.

The *Supplement to the A.E. 1968* arose from the recommendations of the I.A.U. General Assembly in 1964 in Hamburg; it was a joint publication with the U.S.N.O., but I think we did most of the work. I personally spent a considerable time on it, particularly in the differential corrections to the ephemerides of the Moon required to allow for the changes to the fundamental constants. There are errors (not many) and ambiguities (in the text and in the mind of the reader!) in the explanations of Brown's theory, and I wrestled with these for a long time. There is some suggestion that I made an error, and, if so, this would not surprise me, though my formulae were supposed to be checked not only in the Office but also, independently, at U.S.N.O.. Differential corrections are unsatisfactory, and it is hoped that there will shortly be a new ephemeris based on a more coherent theory and expansion. The Supplement was published in January 1966.

I notice that during 1965 I wrote forewords to *The Principles of Navigation* by E. W. Anderson and to *The Mathematical Practitioners of Hanoverian England, 1714-1840* by E. G. R. Taylor; this gave me great pleasure, as I greatly admire them both.

There was a meeting of the Executive Committee of the I.A.U. in Nice in 1965. Before then (in 1964 or 1965) Dorothy Bell and I (and Flora) visited Nice to hand over the duties of General Secretary to J.-C. Pecker and to discuss the arrangement for Dorothy to join him. We had a wonderful time on each occasion.

### **Celestial mechanics and astronomical constants**

The major advances in astronomical ephemerides during the decade 1960-1970 were the solution of the main problem of the lunar theory (in which the N.A.O. essentially played no part) and the introduction of the I.A.U. System of Astronomical Constants in 1964, in which Wilkins played a major part. He was a member of the I.A.U. Working Group, and was primarily responsible for ensuring that the system was

adequately presented, explained and publicised. It was introduced, in part, into the *Astronomical Ephemeris for 1968* by means of a Supplement, in which the effects of the change of system were fully set out. Since then Wilkins has been particularly active in the field of astronomical constants, and has been chairman (or, if not, effective leader) of many committees and working parties concerned with integrating 'modern' observational data and requirements into the system. {I feel that DHS has exaggerated my post-1964 role. Ed.} The lunar theory received its most effective 'boost' with the development of computer techniques to solve the fundamental differential equations that specify the main problem (of the motion of the Moon, perturbed by the Sun) in a series of algebraic and trigonometrical expressions with exact numerical coefficients. (Contact transformations similar in principle to the Delaunay solution.) The lunar ephemeris known as  $j=2$ , which was based on Brown's original theory but amended to accord with the revised astronomical constants, continued to be used until it was eventually replaced in 1984 by an ephemeris derived from a numerical integration.

During this period we had an S.S.O., J. S. Griffith, in the Office for a year or two. I suggested to him that he should look into the possibility of recalculating, or checking, the planetary perturbations of the Moon; but I was unable, personally, to give him much assistance. He did quite a lot of work before he decided to take a university post in Canada, where he hoped to continue with the research. He had not, however, made much progress before he left and, although we corresponded for some time, he did not proceed with it.

No reference has previously been made to the work of Wilkins on the satellites of Mars, a problem posed to him by Clemence during his sojourn at U.S.N.O.. He made the investigation into the supposed secular acceleration a piece of major research, at first with inadequate computing facilities and shortage of accurate observations. He succeeded in showing that, at the very least, there was no necessity to introduce assumptions of a non-natural origin for the satellites! He was later successful in encouraging A. T. Sinclair to take an interest in the satellites and more generally in the motions of the satellites in the solar system. {Sinclair joined as an S.O. in 1968 after completing his Ph.D. thesis at the University of Liverpool on the motions of minor planets.} The observation of position (of both planets and satellites) through the techniques of radar and laser ranging, as well as by direct photography from space-craft, made such studies of much greater challenge and interest. I am very glad indeed to pay this tribute to their work in this difficult field.

### **The occultation programme**

During 1965 and 1966 the new computer made a lot of difference to the occultation programme, as well as to the preparation of ephemerides. Mrs Sadler and Morrison were chiefly concerned; but Nicholson made a preliminary discussion of the observations of the occultations of stars by the Moon to give the relation between the rates of ephemeris and atomic time.

L. V. Morrison gradually took over the organisation, on the ICT 1909 computer, of the prediction and reduction of occultations, under the direction of Mrs Sadler, who continued to handle the observational material. He used the enhanced computing facilities to revolutionise the procedures. One of the major tasks was to convert Watts' charts of limb corrections to numerical form in such a way that the corrections could be calculated for each observed occultation. Previously the Scientific Assistants had read off the corrections from the charts, using visual interpolation between the plotted

contours. Through S.R.C. we were able to use a D-Mac rectangular coordinate plotter at the Royal Armament Research and Development Establishment (R.A.R.D.E.) at Fort Halstead, near Sevenoaks, to do the conversion. The equipment was installed later at R.G.O. for other purposes. There was a remarkable coincidence; the Chief Scientist at R.A.R.D.E. was Maccoll, whom I had introduced to doing anti-aircraft trajectories on the National machine in 1937; I do not think we had met since then.

The enormous increase in speed, and decrease in man-power, required for all stages of the occultation programme (other, possibly, than for handling the actual observations sent in) has enabled Morrison to extend the analysis of the reductions and to make many important contributions to the study of the rotation of the Earth and the secular acceleration of the Moon. He has recently re-reduced and re-discussed all the observations since 1943 when the N.A.O. assumed responsibility for their reduction and he plans to extend this research back to cover all recorded observations.

The Office became increasingly involved in investigations requiring occultation techniques. It had earlier played a considerable part in determining the positions of 'discrete radio sources', by predicting and analysing the observed occultations. It was now called upon to predict, with considerable precision, the times of occultations of 'interesting objects' (for example, X-ray sources) as seen from rockets. Later, the discovery of pulsars led to similar work, and to the provision of reduction tables to allow for the varying position of the observer on the Earth relative to the barycentre of the Solar System. {B. Emerson contributed significantly to this work.} I think the efficiency of the Office in meeting all these various requests was much appreciated by those concerned.

My personal contribution to the above work was very, very small. Although I would like to think that it was because I was engaged on other matters, I suspect that the real reason was that I was unable to make any effective contribution.

### **Bicentenary of the *Nautical Almanac***

The celebrations of the Bicentenary of the *Nautical Almanac* went on a long time, well into 1966 and 1967. They consisted of many items.

(a) A special article was included in the *Nautical Almanac* for 1967. This was written largely by W. A. Scott, who was the most appropriate author. It contained: extracts from contemporary publications; an account of the contents of the first edition of 1767, with illustrations of their usage; and a brief account of subsequent developments. It was reprinted in *Man is not Lost* {see below}.

(b) A separate note on *The Nautical Almanac and Astronomical Ephemeris 1767 to 1967*, included in the A.E. for 1967. This included the Preface to the original edition, a list of its contents and a brief account of its history.

(c) A booklet with the title *Man is not Lost* as a record of two-hundred years of navigation with the *Nautical Almanac*. The title was the rubric that was adopted for all the air publications of the Office on the suggestion of Wing Commander L. K. Barnes in 1941. This booklet was written, at the request of the National Maritime Museum, as a joint publication of the R.G.O. and the N.M.M.; it was published by H.M.S.O. and sold mainly by N.M.M.. I wrote this in great haste and I actually dictated it from a rough draft to Miss Hanning, who had recently succeeded Miss Perry as secretary. She typed it out (in one or two days) and I then went through it carefully to edit it for the printer, indicating, as usual, precisely how I wished the material to be set, before it was sent to

N.M.M.. The copy was prepared in an incredibly short time; but it took a very long time to reach the proof stage and publication.

It has never been my misfortune, before or since, to have to correct such systematic and accidental errors. Waters of N.M.M. had seen fit to rewrite part of the introductory section and when the typescript came back the first few pages had been retyped badly, with a number of errors that had not been corrected. Some general comments, most of which I rejected, had been written on the remainder. It took a few more months to receive proofs, and when they came they were the worst proofs I had ever seen. All of my notes to the printer had been ignored and there were erroneous changes to my text. The Museum 'editor' had left the styling to H.M.S.O., who had ignored, or overruled, most of my indications on the copy. For example, there was no indenting of the first line of a paragraph and there were no leads between paragraphs so that, with the small measure they used, it was often impossible to say whether there were paragraphs or not. Eventually in 1968, after the celebrations were over, the booklet came out not too badly. I wrote to the Director of N.M.M. several times about this; it was plain that the person dealing with publications was inexperienced. The booklet sold well, with many reprints since. I think that I received only two or three copies of the first printing.

(d) An exhibition at the Old Royal Observatory, organised in conjunction with the National Maritime Museum. It was to be prepared jointly, but Howse said he was too busy, and left it to others to help us. That help was minimal. We supplied all the material, with legends, apart from one or two instruments from the Museum. (Incidentally I think that some of our material has not been returned to us; after the first showing N.M.M. said that the exhibition was to be continued in a different form and we forgot about it.)

We had a similar unfortunate experience in the setting up of the exhibition. The worst thing was the treatment of an exhibit to show the method of lunar distances for which we had designed a working model of the observation of a lunar distance. It was a complicated set-up, requiring gearing to move the Moon and a 'sextant' to measure the distance from Moon to star. N.M.M. were too busy to do the complete construction and assembly, but had undertaken to construct the simple wooden structure. We got the R.G.O. workshops to design and construct the mechanical gearing and linkages required to move the Moon as the Earth rotated. We also mapped out the positions of the stars (correctly) on the projection and we also undertook to place the stars in their correct places and sizes on the blue-painted hemisphere. After the usual delays, we were told that the structure (wood and canvas) was ready for the installation of the mechanical 'drive' through a handle which viewers could turn.

We arranged to be at the Observatory at a certain time (early, say, 9.30 or 10 a.m. from memory) on a certain day suitable to N.M.M.. Scott (of course!) had designed and planned everything in detail; we had Ticehurst (a skilled mechanic) with the beautifully made parts and Miss Tidmas with her stars, coordinates and instructions for sticking them on. Having a full day before us, we left home early and arrived at the Old Royal Observatory at the proper time to find no one there other than a rather uncooperative warder. After waiting for some time, I did persuade the warder to telephone the Museum, at the bottom of the hill, to say that we were waiting. It was well over an hour, after several more calls, that Cdr W. E. May (the Deputy Director) turned up with his people. During that time we had been standing in a cold empty gallery, without seats of

any kind. For some reason he was in full naval uniform; I lost my temper (not unknown) and proceeded to tell him what I thought of him for keeping us waiting! He turned around, without a word, and left us to carry on. I formed the impression that the staff were pleased. Neither then, nor on previous visits, were we invited to lunch.

On the day of the opening of the exhibition, I was invited to lunch with the Director. Previously, I had asked that my name should not be given, rather that of the N.A.O. should be used, but it was so given to the press earlier that day. I mentioned this fact to the Director! Unfortunately the exhibit { which was referred to as the 'Sadlerium' } was not too successful since the old sextant, on a universal joint set up by N.M.M., failed to be exhibition proof as it did not meet the machinations of the children! It was first modified and later withdrawn from permanent exhibition. Otherwise the exhibition of instruments, almanacs and tables proved quite interesting.

(e) A paper with the title "A Modern View of Lunar Distances", which I wrote with other staff and which was published in the *Journal of Navigation*. We not only gave a 'simple' (not so simple in absolute terms, but relatively so) method for the reduction of an observed distance, but also tabulated lunar distances, so that those who so wished could try out the method in practice. It was a new attempt to simplify the calculation, using the computing methods then available in 1767, but with modern methods of true accuracy. We gave sample tabulations of lunar distances for parts of February and August, and a comprehensive illustration of their use. I used a differential method of reduction that involved using parallel columns of logarithmic functions; this method, to the appropriate accuracy, is very quick. In the illustration I used accurate 'observations', deduced from the ephemeris, with assumed errors, for a known position. I then calculated the position by lunar distances and explained the discrepancies in relation to the errors assumed. I was rather pleased with this as it demonstrated the large effect of relatively small errors of observation. It conformed with my principle of making illustrations as realistic as possible. There was a common fault in most navigation books of the time: the authors would (unnecessarily) give examples that were unreal — stars only observable in daylight, Moon unobservable, and positions that were unreal, some on dry land. It was a pleasure, which I could not resist, to point out these faults in reviews and to some members of the staff of the N.A.O.! We had a large number of reprints, and offered them free of charge to purchasers of the N.A. and A.E.; none are left. This was published in 1966, with tabulations for 1967.

(f) Several articles of general interest, which I wrote for various publications.

(g) A staff visit to Greenwich to see the exhibition which we had mounted and to let the staff who had joined us after the war see the Old Royal Observatory and the Royal Naval College. Afterwards we went on to London to the Royal Festival Hall for a concert by the National Youth Orchestra.

We could perhaps have made more of the bicentenary, but it is difficult to judge how much effort is worth putting into such a celebration. Unfortunately, (note how unfortunate we were!) the bicentenary came 7 or 8 years too soon; with modern hand-held calculators, the reduction of lunar distances offers few problems.

### **Relations with the A.R.**

Wilkins and Sinclair continued their work on the satellites of Mars. And I, under some pressure from the A.R. to do some research, started to work on the orbit of Mercury, with some idea of checking the theory of relativity. I did not get very far

because of pressure of other work and, let me face it, my inability to do it! I did not know whether the impetus for the research came from the S.R.C. or from the A.R..

I might as well mention here an example of how the A.R. took considerable pleasure in getting people's instant reactions, instead of giving them time to consider the matter beforehand. On one occasion I was ill in bed, and he rang me to discuss my disestablishment; he read me a letter from S.R.C. and asked for my instant response; I did know about this in general terms, but I would have liked to study the letter carefully. He would send me letters labelled 'D.H.S.' and I would have to judge whether I had to reply directly, to return it with comments, or to draft a reply for his signature. What effect it had on the filing system I did not know.

My criticism of the A.R. is very personal, as he was the very antithesis of my view of what an administrator should be. But some liked his methods, such as apparently acting as the Devil's advocate; and everybody forgave him when he smiled! He did have a difficult job as A.R., in the middle of an astronomical revival, following Spencer Jones.

### **Notes on the staff**

At this point it is convenient to say something about the staff. Earlier {in 1957} we recruited (along with several Scientific Assistants and Clerical Assistants) two A.E.O.s; one of them was Emerson, who was still in the Office when I retired, and the other was Dickens. I think that Emerson had better qualifications than Dickens, but neither was good. Woolley turned both down and said the N.A.O. could have them. (The A.R. had an interview panel consisting of himself, Hunter, myself and the cashier, J. H. Whale.) I think his objection to Dickens was that he did not play cricket or perhaps it was his manner. His interest in astronomy was far wider than we could cater for and we recommended that he be transferred to Astrophysics; he did remarkably well and made quite a name for himself. {My recollection is that Dickens used a program written by Harragan to determine the periods of some variable stars and that this led Woolley to take Dickens into his research team on a part-time basis in the first instance. Ed.}.

We had an S.S.A., John H. Barry, who had been a long-term soldier and who was recommended to me by the Director of the Ordnance Survey. He had entered the army without any qualifications, and had proved himself so competent on survey calculations that he was lecturing at the Royal Military College of Science at Shrivenham. He was a most careful and conscientious worker. His army manner did not go down well with the young S.A.s! One other recruit (in 1969), G. G. C. Raymond-Barker, was an ex-R.A.F. officer, who had been invalided out because of multiple sclerosis. He was a man who loved his work (primarily on the A.A.) and was so competent; he took on the work of Miss Rodgers on the publications when she retired. He rapidly improved in health, and was popular with all.

There were retirements and transfers. Miss Joan Perry, who had been a truly efficient secretary since 1942, was made librarian {in 1965} when Preston left; her place was taken by Miss Pat Hanning, an equally efficient secretary, who had been in the R.G.O. Typing Pool. She was helped by a Clerical Assistant, Miss Alison Gaydon, who looked after the files, the library and Miss Hanning. In 1967 Alison married W. L. Martin, who had worked in the Office, but who had transferred to Astrophysics.

Other retirements were (not in order of date) Richards, Scott and Miss Rodgers; all had been in the Office before the war. Before he retired (in 1967) Richards had been

transferred to the R.G.O. to help Dr Hunter; he cannot have been happy about his past, but he did some valuable work for the *Explanatory Supplement* and, much earlier, in completing the punched-card ephemeris for the Moon. I cannot forget my share in his downfall. Miss Rodgers took over Mr. Scott's editorial duties for about two years before she retired, on her 60th birthday in 1969, to live in London. She was admired by all of us, and loved by all the girls whom she had trained, or had worked under her. She still keeps in touch with them and many of the staff. {She died in 2003, aged 93.}

### **Appreciation of work of Mr. Scott**

Frequent reference has been made throughout this 'personal history' to W. A. Scott, who made such great contributions to so many projects and to so much of the work of the Office. Scott had been a 'tower of strength' to the Office since well before I came to the Office in 1930. As those who read this will know, he was involved in all the navigational activities and in many others besides. He was in charge of the Navigation Section from the beginning, though being an S.E.O. he was nominally under the head (a P.S.O.) of the division of the Office responsible. In effect he worked directly for me for most of the time since I took a major interest in all navigational matters. Scott was, however, called upon to do far more than his share of the 'routine' navigational work of the Office, including, for example, the painstaking touching up of the copy for the *Nautical Almanac* prepared on the card-controlled typewriter. Such is the reward for conscientious devotion to high standards of presentation, accuracy and, above all, reliability. The number of 'jobs' that Scott saw through during his service in the Office is very large. In this personal account it will be noticed that he was supervising intricate punched-card calculations before 1930 and he continued with similar responsibilities until his retirement at the end of 1966. It was therefore a difficult decision for me to choose Harding to go to sea, on H.M.S. Dalrymple, in 1949 (see chapter 10); Scott was understandably disappointed. He would certainly have carried out his duties (including observations and any practical tasks) extremely well — he was, and still is, extremely good with his hands and a most competent workman. But his modest manner, withdrawing personality, and his strong teetotalism (to the extent that, on several occasions, he refused to join a group after a meeting at the Institute of Navigation wishing to continue the discussion over a glass of beer) made me think that difficulties might arise. I may have been wrong, but Harding was certainly a success. [This is clearly an attempt to justify a decision that I have long had on my conscience. Scott never complained, but he clearly felt he was not getting the rewards, or opportunities, that his long service, experience and competence deserved.]

It was difficult to recognise his many contributions adequately: there appeared, at this time, no possibility of promotion to P.S.O., since the complement was inflexible and the two P.S.O.s were rather more, than less, than the N.A.O. was entitled to. I cannot now recall the precise dates, but I twice put him up for promotion to C.E.O. (Chief Experimental Officer, a new grade) with what I thought to be an overwhelming case. But both the Admiralty and the S.R.C. rejected the bids, largely, or entirely, on the grounds that he was not supervising other S.E.O.s and a C.E.O. post could not be justified by the number of S.E.O and E.O. posts in the N.A.O.!

I often wonder whether I could have had him promoted to P.S.O.. He was a very passive individual, with an extraordinary inability to get out when the business was finished. Many members of the staff called attention to this and the only way was to say "you may leave me now" or the equivalent. He was not at his best at grammar [not that I

am much better, judging by my typewriting] and most of the things that he wrote (though excellent in themselves) I had to alter myself. [Oddly enough, his bright daughter who worked in the Office until she got married, failed her O-level in English grammar.]

I had much earlier, in 1957, proposed him for election to the Fellowship of the Institute of Navigation; but he appeared so diffident that, for example, he made few contributions to its work or the discussions on its Technical Committee. I had, for many years, attended meetings of the Air Standardisation Coordinating Committee's Working Party 53 — though, frankly, I used them largely as an excuse to go the U.S.A. or Canada and spent most of the time at the U.S. Naval Observatory. Scott attended, with me, the meetings in London, and as from 1961, or 1962, he attended the meetings instead of me. He did extremely well at these meetings, much better than I had expected; and he earned the respect of the other participants, mainly R.A.F., R.C.A.F., U.S.A.F., and R.A.A.F. officers. On the whole, he was unlucky and I am certain (though he never said or hinted anything) that he felt that his abilities had not been fully utilised; he spent a great deal of his time in doing, meticulously, routine jobs such as the examination of sheets off the card-controlled typewriter.

I did not quite realize the value of his services until he left. J. H. Blythe, who was his next 'boss' at the U.S. Oceanographic Office, could not compare with him. We did manage to get him an M.B.E. (grade of order determined by civil service grade) before or on his retirement.

### **Retirement of Mr. Scott**

Scott retired on 31 December 1966, after more than 40 years service in the N.A.O., but in 1968 he took up a year's appointment in the U.S. Oceanographic Office, to help with the preparation of the new *Sight Reduction Tables for Marine Navigation*, which were being produced in the U.S.A. to my design. This appointment had been arranged for him by the Director of the U.S.N.A.O., who had no vacancies on his staff, and it was an admirable move, not only for Scott, but also for the new tables. A curious point arose in 1974; a paper in *Navigation* said the interpolation tables had been calculated in a certain way, which I thought was wrong. But on checking I found the table had been recalculated; we had prepared the table here, but it had been recalculated to prepare copy. The table was erroneous in the extract we gave in our publication in 1966 in both journals. Still I never thought of checking it then, or afterwards! [The error arises in the fact that a mean value of the group of ten (say 36.0 - 36.9) was used as 36.5 instead of 36.45; thus the value in table 3'.7 should be 2'.2 instead of 2'.3. It is not serious.]

### **Overseas visits in 1966 – 67**

There was a meeting of the Executive Committee of the I.A.U. in Prague in 1966; my impression was that the Czechs wanted to get my views on the organisation of the General Assembly in 1967. Nothing much of interest to the Office arose during this visit.

I think that there must have been a meeting of the C.C.D.S. in Paris in 1966. The chair was taken by a scientist from the N.P.L.; there was a discussion on whether a second based on an atomic transition could now be adopted, and the chairman had two draft resolutions. He asked for a 'straw vote', which resulted in a clear majority for the one he did not want. He made clear that the meeting would finish before noon on a

certain day so that we could make transport reservations. There was a great deal of lobbying and he appointed a committee to make a recommendation. He then announced that the definitive vote would be taken on the afternoon of the certain day. The vote was in his favour as most of the opponents had left. I wrote at once to the President of C.I.P.M. (to which body the C.C.D.S. reports), pointing out the faulty conduct of the meeting. He ruled that the vote was invalid. Neither the chairman nor I were included in the following committee, which formulated the draft definition of the atomic second for use in the international system (SI) of units.

In March 1967, I was invited to a Symposium on Continental Drift, in Stresa; presumably because of F.A.G.S.. I made little contribution, though I did chair one session and was on the resolutions committee. I also visited the U.S.A. in 1967 for a meeting of the U.S. Institute of Navigation and stayed with the Duncombes. Richey, who was getting an award, and with whom I discussed further cooperation with the U.S. Institute, also went and he flew over to Washington in a private jet. I had other business at Yale in New Haven, where I stayed in Clemence's flat; it may have been a regional meeting or it may have been a discussion on time and on the efforts to change the definition of the second.

In 1967 the C.I.P.M. issued a draft definition, which was discussed at the I.A.U. in Prague later in the year. In October 1967 the C.G.P.M. {the General Conference ..., to which the International Committee ... reports} adopted the current definition of the SI second. They specifically rejected the I.A.U. view that the ephemeris second should be recognised for use in astronomy. I should explain that I sent copies of my letter to the chairman and to the Director of N.P.L.; I think that I asked not to be included again, as the astronomical aspect was clearly dead.

### **The future of the *Astronomical Ephemeris***

During this period (i.e. from 1965 onwards, not after I ceased to be Superintendent), the future of the *Astronomical Ephemeris* (A.E.) was raised on several occasions. The main criticisms directed against it were that it failed to provide the observational requirements of the practical astronomer. These are valid criticisms, but they are not ones that can be easily met without a complete reappraisal of the traditional function of the A.E.. Way back in 1955 I had proposed to I.A.U. Commission 4 the introduction of an International Fundamental Ephemeris that would uniquely provide the basic data, thus leaving each national ephemeris freedom to give the ephemerides to such lower precision as observers required. But (and there are difficulties) the proposal was not accepted. Woolley was one of the main critics and he demanded that an *Observer's Handbook* be designed and produced, under threat of withdrawing support from the A.E. if it were not done. By coincidence there was, at about the same time, a similar threat at U.S.N.O. against the U.S. edition of the A.E., but for different reasons. It is perhaps worth noting here that U.S.N.A.O. was (and perhaps still is) much more reluctant to consider drastic changes in the A.E. (to make it more acceptable to observers) than was the N.A.O.. The sales of the A.E. in U.S.A. are much higher as many are bought by astrologers! Moreover, there seems to be a built-in resistance to change.

We had for many years provided for the R.G.O. and the Cape Observatory topocentric ephemerides and long computer printouts of data required for the meridian observations at the two observatories. There was little positive response to our circulated request for suggestions as to content of the *Observer's Handbook*, but (if my

memory serves me correctly) we did (for myself reluctantly) draw up a specification and lay-out for such a publication. I submitted to the A.R. for comments, but he had by then lost his direct interest and the project hung fire; it was certainly not pursued by me. Wilkins, with more enthusiasm, did later produce a publication (I have no copy here) which seemed to me to provide as much usable data as is technically possible. It was reasonably well received; but whether this is the optimum method of providing the data is another matter. The availability of on-line computers and of hand-held calculators clearly brings into question the whole subject of the relative advantages of centralised calculation and publication on the one hand and on the spot calculation of data actually required on the other.

### **Proposal for a department of celestial mechanics**

Woolley was not happy with the failure of the N.A.O. to do active research, and he had some justification. He was also, I assume, under pressure from S.R.C. to regularize my position as a Special Merit D.C.S.O.. Although a bitter critic of the space research programme, resenting even the small contribution (mainly by predictions of satellite transits) that we were able to make, he was much impressed by and jealous of the work and success of King-Hele at R.A.E.. At one time, he mentioned (I use this word deliberately as contrasted with 'informed' or 'discussed') rather casually to me that he was considering the possibility of setting up a department of the R.G.O., to include N.A.O., to do research in the fields of celestial mechanics, geopotential and similar subjects. He had in mind the introduction of someone outside the R.G.O. to head the department, with King-Hele as the first choice. I do not know whether, or to what extent, he had discussed this project with S.R.C., but he certainly approached King-Hele. He (King-Hele) discussed the proposition with me, thus giving me more information than I had previously had about the possible effect on N.A.O. staff. I made it clear that I was not personally concerned, since I planned to retire before any such scheme could come into operation; but it would, of course, affect Wilkins' prospects. I think King-Hele turned down the invitation, though it is possible (I just do not know) that S.R.C. withdrew its support; in any case the proposal was quickly dropped. I do not think that it played any part in my retirement or in the delay in appointing Wilkins to succeed me. The proposal was certainly one that merited consideration, provided that the right person was available, since it opened up the possibility of an 'institute for the practical application of celestial mechanics' in the U.K., something which had been conspicuously missing, in spite of Cowell's tentative proposals in 1910. But, objectively since I was not involved personally, it seemed to me unsound as being in between the known successful arrangements of the massive organisations, (such as the Jet Propulsion Laboratory), with large staffs and elaborate equipment, and the lone-worker who made progress through theoretical developments and personal application. I could not see even an enlarged department of the R.G.O. providing the large organisation or necessarily acquiring the services of outstanding theorists.

### **Aside on the Institute of Theoretical Astronomy**

When the original proposals for setting up an 'Institute of Theoretical Astronomy' were being discussed by the British National Committee for Astronomy, the plan was to set up the Institute at the University of Sussex. I was a member of the sub-committee (with Bondi, Hoyle and Lyttleton) which drew up the outline specification of the staffing and financing of such an Institute. It excluded, specifically, a division concerned with celestial mechanics and allied theoretical studies. But that plan, for

reasons at which I can only guess, did not materialise; then it reappeared, in modified form, as the Institute of Theoretical Astronomy in Cambridge. I was not involved in any way, and celestial mechanics faded into the background.

### **The I.A.U. General Assembly in Prague in 1967**

The 1967 General Assembly of the I.A.U. was held in Prague; it was preceded by a meeting of the Executive Committee. The only thing of interest to the Office was the discussion on the second. I think that the new definition was inevitable, but I did not think that it would lead to a measure of time — and certainly not so quickly. Before I retired, the International Consultative Committee on Radio (C.C.I.R.), which was responsible for the oversight of radio time-signals, announced that Coordinated Universal Time (UTC) was to be based on International Atomic Time (TAI), and that the maximum difference between UTC and Universal Time (UT1) was to be  $0^s.5$ . I wrote at once to say that this was impossible, and suggested, on the basis of a monthly correction and on current occultation results, a safe limit of  $0^s.7$ ; whether this was accepted or not, that figure appeared in the final version. Much later, in December 1972, I found out that a leap-second was to be introduced in UTC, and this would give a difference of much greater than  $0^s.7$ . I wrote to the Director of C.C.I.R. pointing out that this was in conflict with the undertaking he had given. He replied, in a frank letter, saying that this was in accord with an unwritten agreement with the U.S.S.R. not to have a leap-second except twice a year. He added that the C.C.I.R. had instructed the Director of the Bureau International de l'Heure (B.I.H.), who decides when the changes should be made, accordingly. The value was  $0^s.81$ ; I wrote to him to allow me to quote from his letter, but I got a curt refusal. My main concern was for Guinot, Director of B.I.H., who had to accept the criticism for overstepping the limit.

The last chapter came after my retirement in the question of the retention of the name G.M.T.. I was frustrated at my resolution for the 1973 (Montreal) I.A.U. General Assembly being missed, through accident, and was dismayed at the resolution that was passed. But I think that G.M.T. is now as widely used (except in astronomy) as always. {The conduct of the meeting of Commission 31 (Time) also led to a protest from me! Ed.}

There was one other thing of general interest at the General Assembly. The wish of European astronomers to consolidate publications made Graham Smith and me spend many hours in discussing this proposal. The upshot was that the Council of the R.A.S. turned down the merging of *Monthly Notices* with *Astronomy and Astrophysics*, though we gave it serious consideration.

Wilkins was appointed as President of Commission 4 in Prague.

### **As President of the R.A.S. and visit of the Queen**

I was elected President of the Royal Astronomical Society in 1967 — a completely surprise choice! My nomination was almost certainly due to the previous President, T. G. Cowling. We had been to the U.S.S.R. in 1954, and I had the greatest admiration for him as a scientist and as a man. But strictly speaking, I was not really fitted for the post astronomically. Although this did not involve me in a great deal of work, it did require that I attended all the meetings, and many committee meetings, of the Society. There were also several particularly difficult questions of policy (e.g., the proposed unification of the European astronomical journals, accommodation and

re-decoration of the Society's premises in Burlington House, revision of procedures for the election of Council), as well as the usual presidential addresses.

On 1 December 1967 H.M. Queen Elizabeth visited the R.G.O. for the inauguration of the Isaac Newton Telescope (I.N.T.). I was present as the President of the R.A.S.. It was over 21 years since I served as secretary to the committee that drafted the two reports that were submitted to the Royal Society. It was a good function, and Flora and I were presented to the Queen.

I gave my first presidential address to the R.A.S. on 'Astronomical Measures of Time', in which I stressed the fundamental difference between an observed time-scale and an integrated time-scale. The former concept is very much out of date now, when the integrated time-scale is much better determined than the observed time-scale. My second Presidential address to the R.A.S. was on 'Astronomy and Navigation'; it reads very oddly now.

I got into trouble through writing, as President of the R.A.S. and with the Presidents of the R.I.N. and R.I.C.S., to *The Times* about the proposed use of British Standard Time for the permanent use of British Summer Time. Mrs Paton, of S.R.C., telephoned me before publication of the letter — she had clearly been informed, through the Home Office, by *The Times*. She wished me (in fact she ordered me to do so) to withdraw the letter, but I did not do so. It would have been impracticable to get the co-signatories to agree, even if I had wanted to. Fortunately the A.R. was away when the inevitable letter arrived from S.R.C. with dire threats! Hunter wrote a conciliatory letter, but not without pouring a little scorn on the H.M.G. policy-letter to him, and then a subsequent letter that ended the matter. The letter was published in *The Times* on 24 October 1967. I often wonder whether the threat of the Minister's anger was modified by the signature of the Hydrographer, who was the President of the R.I.N.. The Government's proposal to impose B.S.T. on us was defeated.

### **Statutes of the I.A.U.**

In December 1968 I went to Frankfurt for a meeting on the revision of the Statutes of the I.A.U.. After a long argument we reached agreement on the main changes, but we did not, on my insistence, discuss the wording. This was left to Jappel (a Czech lawyer who had succeeded Miss Bell) and me to draw up in a final form of the Statutes and Bye-Laws for presentation at the General Assembly. We corresponded, but it was impossible to do everything by post and so I invited him to visit Bexhill. He came over in July 1969. I learned a lot from him about legal matters and the fact that the domicile of the I.A.U. is Belgium. We got on well together and I think that he did a fine job for the Union.

### **Relations with other ephemeris offices**

In this personal history I have stressed my appreciation of the great help given to me (and the Office) by the U.S. Nautical Almanac Office, particularly by Clemence, Woolard and Duncombe. I have not referred to the considerable help given to the Office by the directors of the other ephemeris offices. We did not have the same contacts (partly by language) as with the U.S. Naval Observatory, but we got on very well with them and they were most co-operative in all (or most) of our joint projects. I remember Fayet, who in spite of his age, ran the office of the *Connaissance des Temps*, but with increasing difficulty as he lived in Nice. The new director, Kovalevsky, was a more up-to-date man. I have referred to de la Puente, the director of the office of the Spanish

*Almanaque Nautico*; he was very co-operative, though we had little contact with him or with his predecessor, Benites. Subbotin was never well enough to attend the I.A.U. (except perhaps in Moscow), but personal communication was difficult for my lack of language. He was, however, always ready to adopt my suggestions and he never let us down in relation to dates. He was succeeded by Chebotarev, who was a much more approachable man, but with the same integrity.

The last (of the five) was the director of the Astronomisches Rechen-Institut (A.R.I.), and was responsible for the publication of the *Berliner Jahrbuch*. (The equivalent of the *Abridged Nautical Almanac* was published by the Deutscher Seewarte.) I met Kopff before the war and actually visited him in Berlin in 1938. He had a bad time during the war; he was evacuated to a town to the east, and was in danger of being overrun by U.S.S.R.. In my trip to Germany in 1945, I tried to find out where he was, but I had to leave, fruitlessly. The A.R. (Spencer Jones) managed to arrange to bring him to Heidelberg with the loss of his library and much else. {At the end of the war the A.R.I. was split and Kahrstedt became director of the part that remained in Berlin.} Kopff was a charming man, and dedicated to the FK3; we saw much of him, in 1948 and 1950. Later, just before he retired, he told me that there was a danger of an outsider being appointed as director instead of Gondalatsch, his deputy, who came to Herstmonceux at this time and who clearly expected to be the next director.

The appointment went, as he feared, to Fricke, but he need not have worried at all! Fricke was a great help to the Office, and to me, and was a great astronomer. {Fricke died in 1988. Ed.} Apart from his work on FK4 and FK5 (which would have pleased Kopff) he played a leading part in unifying astronomy in Germany. He firstly made an arrangement with Kahrstedt in East Germany on the roles that the two institutes should play. In due course, he took over the *Apparent Places of Fundamental Stars* from us and terminated the *Berliner Jahrbuch*. He replaced the *Astronomisches Jahresbericht* by the *Astronomy and Astrophysics Abstracts*, which became the foremost bibliographical publication in astronomy. He was one of those most anxious to have a European journal and he was instrumental in the resolution that Germans should publish their results in English. He was a leading authority on the fundamental constants of astronomy, particularly on the astrometric side.

I was awarded the "ADION" Medal of the Observatory of Nice in 1969, for contributions to international astronomy. In 1970 I was greatly honoured by the University of Heidelberg, which invited me to accept an Honorary Doctorate, for which Fricke had nominated me. We went over to Heidelberg to receive it in May 1970. I gave a short technical address on Time, in addition to my formal expressions of gratitude.

## CHAPTER 17

### From 1 January 1970 to 18 February 1972

#### A change of duties

I formally relinquished my duties as 'Superintendent of the Nautical Almanac' on 31 December 1969. I was seconded 'for other duties', namely the organisation of the I.A.U. General Assembly at Brighton in 1970. I did not have much time for the N.A.O. work; this was taken over by Wilkins, who had been doing it so well for a long time. In the circumstances, I feel it is absurd to list the work done by the Office, with which I had little to do and which is recorded in the annual reports of the R.G.O. in *Q.J.R.A.S.*.

I append copies of notices to the Directors of National Ephemeris Offices dated 31 December 1969, and to members of the staff of the Office dated 1 February 1971 when I formally retired from the post of Superintendent.. There is a minor difference in 'relinquish my duties' and 'formally retire'. {The texts of these letters are in Appendices 2A and 2B. The latter contains a brief commentary on earlier changes of Superintendent.} Wilkins did the job from 1 January 1970. After my retirement, I had expected Wilkins to be promoted to S.P.S.O. and appointed Superintendent of the Nautical Almanac, an office for which he was (and is) admirably qualified by achievement, ability and experience. But, for reasons that I do not understand, it was many months before the Acting appointment (which was immediately necessary) was confirmed. As mentioned earlier, I do not think that it was because of any possibility of a substantial reorganisation.

#### Preparations for the I.A.U. General Assembly in Brighton

Although I retired as General Secretary of the I.A.U. in August 1964, I continued with some residual duties until the end of 1964 and remained a member of the Executive Committee, as an advisor, until 1967. There was a meeting of the Executive Committee in Nice in 1965 and one in Prague in 1966; at the latter meeting I spent a considerable time helping with the main arrangements for the 1967 General Assembly, to be held in Prague. At that G.A., the U.K. national representative, Hermann Bondi, formally invited the I.A.U. to hold its next General Assembly in Brighton (at the University of Sussex) in 1970. Inevitably, I was asked to chair the Local Organising Committee and I was given complete authority (together with considerable material and man-power assistance) to do so by the S.R.C.. Effectively, I was seconded full-time as from the beginning of 1970, together with Miss P. M. Hanning, for the I.A.U. work which had to take priority over my normal duties as Superintendent. Wilkins then became Acting Superintendent with an 'acting promotion' to S.P.S.O.. Apart from Miss Hanning's time we did not, I hope, call too much on the resources of the N.A.O., except for the use of the ICT 1909 computer for handling lists of participants and records. We had the full-time assistance of Mr. Pepperall, seconded as a 'conference organiser' from the Rutherford Laboratory of the S.R.C., but most of the work was done by enthusiastic temporary staff at a total cost that was exceedingly small. The members of the Local Organising Committee and all the staff did a truly tremendous job and I think the result

was satisfactory. There is not much of interest for the Office and I shall content myself with the main things of interest to me.

The preparation was a major operation. In spite of an appeal by the National Organising Committee, with the Duke of Edinburgh as President, we received a depressing amount of money as a start. The S.R.C. was, however, exceedingly generous to the Local Organising Committee. They guaranteed the Committee against loss, thus enabling us to budget for only a very small surplus. Our thanks to Mr. Hosie, who agreed to our suggestions in a ten-minute interview. In addition, S.R.C. made available to us the services of Mr. Pepperall for six months, subject to repayment if finances allowed. This provision was essential for the budget, which depended on such imponderables as the number of participants etc. It made the task of the Finance Committee easier in fixing the registration fee. We fixed it at £10, and, although we had later doubts, this proved sufficient to pay off Pepperall's salary in full to the Rutherford Laboratory and to produce a small surplus.

Pepperall was made administrator to the Local Organising Committee; he made many significant contributions including financial control, insurance, security, and arrangements for the free loan of duplicating services from Xerox. But he did not get on well with the other staff, being inclined to be lazy when most of us were very busy.

There were many helpers at the R.G.O., at University of Sussex, and in the Ladies Committee as well as numerous students (approximately 100, consisting of schoolchildren, undergraduates and graduates). All of them were keen and efficient. I must, however, pay special tribute to my helpers in the organisation of the Assembly.

The key person was Miss Hanning, who was not full-time; she supervised the whole arrangements, was full-time during the Assembly, and helped me for several months afterwards. Mrs Norris was a clerk, brought in to deal with registration and accommodation; she brought enthusiasm and some knowledge of languages; her name was Hansi, being of Dutch origin.

Much later we had need for a typist, and general assistant. Someone called my attention to Mrs M. Gillingham, the wife of a visiting Australian astronomer at the R.G.O., who was said to be a typist and looking for a part-time job. In the interview with her, after agreeing to employ her, I thought it desirable to ask her formally what her typing speeds were; she answered me that she was currently the Australian champion typist! She was truly magnificent in all she did. After only a day or two she approached me and said we had a computer in the office and why not use it. I replied that we could not afford the punching time needed to record all the data on participants on cards; she said that she would do it herself and did so! This was a major contribution to the success, in that it enabled many copies of printout from the computer, under different listings, to be circulated to all who needed them.

We finally recruited Miss Adams, on vacation from her university course, to help us; she took charge of all the arrangements about paying the numerous assistants and controlling what they did. Her skill with arithmetic, and her neatness, coupled with her firmness in dealing with students was remarkable. She was supported by two volunteers: Philip, the son of Hunter, and Alastair, the son of H. M. Smith. Both were interested in her and Philip won; he married her. Both volunteers were extremely useful in the arduous job of organising transport.

Perhaps the greatest voluntary contribution was that of Mrs Smith, who, as an accountant, took over the record of money handed in on reception and kept immaculate accounts. We would have been in trouble if she had not stepped in.

The members of the organising committee did their work well; the only member of the R.G.O. staff was H. M. Smith, who was responsible for transport.

### **Events during the I.A.U. General Assembly in 1970**

During the General Assembly itself there were several events of general interest:

(a) The opening ceremony was performed by Mrs Thatcher (the Secretary of State for Education and Science), who was hostess at a Government lunch. Many astronomers would now remember being introduced to her by me then.

(b) The new Statutes and By-laws, which Jappel and I had prepared, were formally adopted.

(c) A decision by the Executive Committee to hold two General Assemblies in 1973 was agreed after some argument. This was to allow Poland to hold an Assembly in order to commemorate the anniversary of Copernicus; the Poles had put in their application very late, when the Australians had been accepted. The British (and I) were against the procedure.

(d) I cannot remember anything that happened in the Commissions of special interest to the N.A.O.; but I think that I spoke about Ephemeris Time and Atomic Time, and their respective functions.

(e) I held a dinner, which was beautifully prepared and served by the University of Sussex canteen staff, to repay hospitality we had been given on visits abroad. It also happened to be my birthday!

(f) During the meeting I was informed by Sir David Martin, Executive Secretary of the Royal Society, that my name had been proposed as General Secretary of International Council of Scientific Unions. The proposal had been made by Ambartsumian (who was then President) without informing me. I immediately withdrew my candidature. The next day (or thereabouts) I received a telegram from Ambartsumian asking me to serve. It would have been a great honour to have followed in the footsteps of Stratton and Spencer Jones.

(g) I won my last game of billiards as the representative of the U.K. against Hall who represented the U.S.A.. It was to be my last General Assembly, and I asked Sir Bernard Lovell to stand in for me at Sydney; but he did not make contact with his opponent. Thus I think ends the game first introduced by Stratton and Schlesinger.

### **Activities after the I.A.U. General Assembly**

I did not make much contribution to the work of the Office for most of 1970. Wilkins and Mrs Sadler had to bear the extra work and responsibility that this caused. I stayed on, formally as Superintendent until 18 February 1971 when I retired on pension as D.C.S.O., but was immediately re-employed in the basic grade of P.S.O. without any responsibilities, or duties, in respect to the N.A.O.. I finally retired, a year later, on 18 February 1972.

I cannot now recall, with anything other than vagueness, what I did between the end of the I.A.U. General Assembly in September 1970 and my retirement. There was

great deal of clearing up to be done with Local Organising Committee records, particularly in finalising the accounts; it took many months to clarify outstanding accounts and collect unpaid fees from foreign participants. We eventually finished up with a small balance (of about £600) on a total expenditure of some £30 000, which was satisfactory, although perhaps rather lucky. It would have been impossible to have worked to such close estimates without a guarantee against actual deficit. The clearing up operations continued until May 1971; Miss Hanning helped me to prepare all reports to the Royal Society and the longer reports to the I.A.U..

After that I cannot remember what I did until I retired in February 1972. I recall that I examined carefully the final (late) material for H.D. 486. and, certainly, I spent a lot of time in going through a lot of N.A.O. files (from 1930 onwards), destroying some and trimming others.

### **N.A.O. records**

While sorting out the N.A.O. records I put all the old correspondence (what little survived the pre-1936 destruction) in order, with indices of content. And, more destructively, I pruned hundreds of old files. Most were of almost (but there is always a possibility!) no permanent value, though many were of interest, either because of the (usual minor) questions raised or because of the personal associations involved. It seems impossible to lay down rules or guidelines for dealing with masses of miscellaneous correspondence, mainly on general matters not directly concerned with the main work of the Office. By this I mean correspondence on subjects that may be relevant in a minor way, such as typography, presentation of tables, etc.. These may have some intrinsic interest, but are of no importance to the history or present work of the Office. I did try, however, to record what I did and to make a list of everything that I recommended for destruction.

### **Retirement arrangements**

Under the Admiralty, and M.o.D., it was the custom formally to ask a member of staff reaching the age of 60 (actually 6 months before) whether he or she wished to continue to serve 'subject to health and efficiency being satisfactory'. The individual, and the head of the establishment, were given the opportunity of restating their views each year until compulsory retirement at age 65. A healthy and efficient member of staff could be compulsorily retired if the staffing position made it necessary, though this was rarely the case until recent years. The system (which was applied to all N.A.O. staff, such as A. J. and S. G. Daniels and J. G. Porter) enabled staff to know precisely what the position was, and to express their own wishes in ample time. S.R.C. either did not apply this system, or it was not applied to me. I had mentioned to Woolley that I certainly did not wish to continue as Superintendent until I was 65, but that (because part of my service was 'temporary' and counted only half towards my pension) I would not object to being re-employed in the basic grade of P.S.O. after formal retirement, if I could do any useful work. I was conscious of the fact that I was certainly not doing a special merit D.C.S.O. job and that I was not pulling my weight in the N.A.O.. I also suffered from angina that limited my travelling ability. I was, however, never asked, either by Woolley or by S.R.C., what my wishes were. The first definitive approach was made on the telephone by Woolley when I was on sick leave and actually in bed. He said that he had reached agreement with S.R.C. about me — namely that I should retire as Superintendent at the first convenient date (chosen according to normal custom to make up the integral number of years of reckonable service) and be re-employed as a

P.S.O.. *He wanted an immediate reply*, there and then. The proposal (if that is what it was!) was satisfactory, but it would have been far more readily acceptable if there had been some indication beforehand and time to consider it, preferably in writing. Apparently, he had discussed his own impending retirement and mine at the same time with S.R.C., and both proposals were contained in the same letter from the Chairman, Flowers. This was certainly the impression I received during the telephone conversation since he told me that S.R.C. (which had earlier insisted that he should retire at age 65) had agreed to him staying on until the end of the year in which he reached 65. He was then appointed Director of the South African Astronomical Observatory for 5 years.

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Dr. Alan Hunter, the Acting Director of the R.G.O., paid tribute to Dr. Sadler at a retirement presentation that was held in the Long Gallery of the Castle at 4 p.m. on 18 February 1972. Then Mr. P.S. Laurie gave a short talk on the early history of the *Nautical Almanac* and of the Nautical Almanac Office.