

PREFACE

The first edition of *The Nautical Almanac and Astronomical Ephemeris* was published for the year 1767 and was designed and prepared by the fifth Astronomer Royal, Nevil Maskelyne, with the assistance of unknown persons. He and his successor, John Pond, continued to oversee the production of the Almanac until 1818 when Thomas Young took over the responsibility for the supervision of the work. At that time many of the computers, who carried out the calculations in their own homes, lived far from the Royal Observatory at Greenwich. This rather unsatisfactory system was superseded in 1831 when W. S. Stratford was appointed as Superintendent of the Nautical Almanac. He established the Nautical Almanac Office, which was located and funded separately from the Observatory. His successors were J. R. Hind (1853), A. W. M. Downing (1891), P. H. Cowell (1910), L. J. Comrie (1930) and D. H. Sadler (1936). The initials H. M. were first applied to the name of the Office in 1904 in the preface to the Almanac for 1908.

Donald Sadler was on the staff of H. M. Nautical Almanac Office for over 40 years and he was its Superintendent for about 35 years. During that time there were major changes in the role of the Office and in the facilities that were available for the computation of the data for the almanacs and other purposes. During the Second World War the Office acted as the computing centre for the Admiralty Computing Service and the astronomical activities were reduced to the minimum necessary for the production of the almanacs. After the war Sadler played a major role in the redesign of the almanacs for international use and he also served as the General Secretary of the International Astronomical Union. After the move to the Herstmonceux, and especially after the transfer from the Admiralty to the Science Research Council, the Office became more heavily involved in astronomical research and the introduction of electronic computers led to further changes in the character of the work of the Office.

After his retirement in 1972, Sadler started to write a comprehensive history of the office of Superintendent of the Nautical Almanac, but he could find very little new material for the period before 1930. Consequently, he decided to concentrate his efforts on writing about the period during which he had been on the staff of the Office. His widow, Flora Sadler, gave me the manuscript after his death in 1987. After my own retirement from the Office in 1989 I typed and edited his manuscript. I produced and distributed about a dozen copies of a preliminary version in 1993 in time for a staff reunion. I did not then continue with the editorial work since it appeared unlikely that commercial publication would be possible and since other projects were demanding my attention. The invitation to give a talk about the history of the Office at a conference in Washington in March 1999 to mark the sesquicentenary of the American Nautical Almanac Office led me to show the text to another publisher, but he gave the same negative response. Nevertheless, further enquiries have suggested that there was sufficient interest amongst former members of the staff and others to justify another limited production run. I prepared a revised version in 2003, but I failed to carry through the editing to a final conclusion.

Then in June 2006 I learnt that Catherine Hohenkerk was preparing to expand the notes about the history of the Office that were given on its web-site. This led to the suggestion that Sadler's Personal History be made available as a set of short files that could be published on the web-site and downloaded as required by interested persons.

Consequently, I have been encouraged to complete the editing that I started in 2003 but my other activities have led to the process taking much longer than I had hoped.

The first part of this *Personal History of H. M. Nautical Almanac Office* covers the period from 1930 to 1936, during which time L. J. Comrie was the Superintendent. Sadler wrote two separate accounts of the period from 1936 to 1972, and I can only surmise that between 1978 and 1986 he had mislaid or forgotten the first draft. In producing the account that is given here I have combined these two original versions. In making this combination I have omitted duplicated material and, where appropriate, I have interwoven material that covers the same topic but gives different information. Only rarely do the two versions contradict each other. I have usually been able to resolve discrepancies by reference to other 'sources', but in a few cases I have given both versions. The other sources were sometimes documents and sometimes former members of the staff, including myself!

I have not attempted to eliminate the near repetition of detail and comments that occur from time to time, particularly when the same topic occurs in different chapters. Only rarely have I inserted new material; except in trivial cases, such material is enclosed in { curly braces }. I have, however, inserted chapter and section headings so that readers will be able to find more easily topics of particular interest.

I received very few comments from those who received copies of this History in 1993, but one letter suggested that quite considerable changes should be made before it was made available to a wider readership. In particular it was suggested that technical material should be placed in an appendix. I feel sure, however, that many of these technical details will be found to be of interest to some readers, and so I have left it to each reader to decide when to skip the details that Sadler has given. The draft forewords and explanatory notes that were with the manuscript are given to serve as an introduction to the main text.

In the early stages of the preparation of this document I received invaluable assistance from the late Marion Rodgers, who served in the Office from 1932 to 1969 and who continued to correspond regularly with many past members until her death. She delved into her diaries to help to resolve uncertainties about persons, dates and places and provided much supplementary information. Joan Perry and the late Flora Sadler also read my early typescripts and helped me to eliminate errors in my typing and in Sadler's memory. I am grateful to the late Betty Atkinson for her comments on the typescript for the period when her husband, Comrie, was the Superintendent. Adam Perkins, who is responsible for the archives of the Royal Greenwich Observatory in the Cambridge University Library, also helped me in my attempts to verify or complete Sadler's account.

It was my intention to prepare an index to this account, but eventually I decided that this would delay the completion of the project for too long. I hope that the detailed listing of the contents, or searches on keywords, will be sufficient for most purposes. Similarly, I have decided not to attempt to give a list of the papers that were published by Sadler. Nor have I attempted to incorporate the recollections and views of other members of the staff that were sent in letters and notes to Sadler, at his request, in 1977. It may, however, be possible to transcribe them for the web, or to use them in the preparation of an extension of the history of the Almanac and of the Office. In any event, the letters and notes will be deposited with other documents relating to the Office in the RGO archives at the Cambridge University Library.

Comments on and corrections or brief additions to Sadler's text would be welcomed so that it may come closer to the standards that he would have wished it to achieve. Recollections of the work and staff of the Office both before and after 1972 would also be welcome so that others may gain greater insights into the history of H. M. Nautical Almanac Office. Such material should be sent to the Office so that it may be added to the archives and considered for inclusion on the web-site.

George A. Wilkins

2008 February 6

FOREWORDS BY DONALD H. SADLER

Foreword to the manuscript for the period 1930–1936

At the time of writing this (early 1977) H. M. Nautical Almanac Office has ceased to exist as a separate entity, even though it still retains its name. It has been integrated, and absorbed, into the Royal Greenwich Observatory. The Superintendent can no longer regard the production of *The Nautical Almanac* (and other ephemerides) as his main responsibility, and his historic title of “Superintendent of the Nautical Almanac” no longer gives him the ex officio right of membership of the National Committee on Astronomy. This is thus a convenient time to write a comprehensive history of the Office, since it can be brought up to a current date in the knowledge that few further additions will be necessary. [I have not found sufficient material to raise any hope that such a history will be possible.]

I have been a member of the staff for most of my time since 1930, and know more about the Office during those years than anyone else; it is essential that I should put my recollections on record, preferably in the form of a connected account for incorporation in the comprehensive history of H. M. Nautical Almanac Office from 1766 to the present day. I have, however, been concerned with the affairs of the Office since 1930, and many incidents have personal associations, and so it would be exceedingly difficult to write in the third person. I have accordingly decided to write in the first person, thus emphasising that this is a personal account based on personal recollections and views. I hope that it will be possible to add the views of others, either by incorporation or by later addition.

Because of the personal nature of this narrative it seems desirable to start with a prologue that gives a short personal account of myself.

Foreword to the first version for 1936–1972

This personal account is being written, continuously from memory, to serve as a basis either for a fuller and more accurate history or for a much shorter contribution to a general history for possible publication. Factual records and correspondence exist for the period from 1936 onwards; and they can later be used to amplify, amend or verify the necessarily imperfect record given here. For this purpose the left-hand pages have been left blank, though I have occasionally added there-on material previously overlooked or explanatory notes.

Although the adopted procedure is by far the quickest and easiest for me (especially as I do not have access to the records), I hope it will provide a record of personal views, reasons, prejudices as well as of events not formally recorded elsewhere.

Foreword to the second version for 1936–1972

My writing is now (1986) so poor that I doubt whether anyone will be able to read it. My memory is not as it was, and I cannot remember names or the order in which events occurred. Fortunately I think that the relevant files should be in the R.G.O.’s archives or files, so that anybody who wishes to make a formal History of the Office

can easily fill in the details. [I subsequently borrowed the A.R.'s Annual Reports, from Wilkins, and corrected my memory from them.] The peculiarity is that I can remember more clearly the events that took place 50 years ago than those of 20 or 30 years ago; I understand that I am not alone in this respect.

In 1949 I was 41 years old, and had never done any research in astronomy — apart from the few months I spent on Comet Comas Sola. Since 1936 I was engaged in other work — principally in navigation, but also in computing — with never a time when I could engage in research. I lacked the inspiration that a good researcher in celestial mechanics required, and the energy to analyse observations to add another figure to values that were well-known. This may be a mistaken view, but when I think of the enormous amount of work that Spencer Jones did (in the early years of the war) on the analysis of the Eros observations to obtain the solar parallax — to result in failure — I am not too sure.

Donald H. Sadler

R.G.O. = Royal Greenwich Observatory

A.R. = Astronomer Royal

PROLOGUE

Personal history to 1930

I, Donald Harry Sadler, was born at Dewsbury, Yorkshire, on 22 August 1908, the second son of James Wright Sadler, a master tailor, and Gertrude Jane (née Needham), formerly a schoolteacher. After primary school I attended the Wheelwright Grammar School from 1919-1926, when I went to Trinity College, Cambridge, with an open Entrance Exhibition in mathematics, and various other scholarships.

At Trinity College, Cambridge

At Trinity my supervisor was J. E. Littlewood, perhaps the greatest pure mathematician of the time; but his supervision of undergraduates was rather superficial and I essentially chose the actual courses for myself.

I duly obtained a first class in part 1 of the Mathematics Tripos, and was awarded a Senior Scholarship on the results of the second-year examination (the “Mays”). I did not, however, get the coveted Yeats and Rouse-Ball Essay Prize; I was beaten into second place by Harold Davenport, who romped away with the prize. [My essay, which I now find quite incomprehensible, was titled “Moving Axes and Differential Geometry of Space-curves and Surfaces”; it was highly commended!] My mathematical interests were varied, but as I had done no physics beyond School-Certificate level I, perhaps wisely, took no course in mathematical physics. I attended Stratton's course on spherical astronomy and Smart's courses on orbit calculation and celestial mechanics, but I was not impressed by the latter — possibly because there were too few students (3 reducing to 2 — the other being F. M. Dean, later Sir Maurice). I also thought that the treatment was too theoretical; I suspect that Smart had never himself computed an orbit from three observations. I also attended Smart's course on practical astronomy at the Observatory, and duly determined its position with a sextant, an artificial horizon and 7-figure logarithms.

In Part 2 of the Tripos, I was awarded a first class with a b* (the highest class) in the voluntary Schedule B; but I did not get the Tyson Medal (for distinction in the field of celestial mechanics), which was not awarded that year. The reason (presumably) was that I failed to answer at all adequately the compulsory question on orbit determination. A study of previous papers had convinced me that Leuschner's method (which had appeared in 3 of the last 4 papers) would not be set again. I disliked the method, and therefore I did not revise my understanding of it, and in the examination I could not reproduce the main argument. [That should have taught me a lesson, but it didn't.]

My scholarship allowed me to stay up for a fourth year, but I could not decide in what branch of mathematics to specialise. I had already realised that, compared to my contemporaries at Trinity (Coxeter, DuVal, Todd and above all Davenport — all of whom became Research Fellows and young F.R.S.s) I was well below the standard of original thought for research in pure mathematics. I was also disappointed at not getting the Tyson Medal and, in any case, I knew insufficient physics to work on general astronomy. I had attended Eddington's lectures on relativity, which I did not fully understand, and on the combination of observations, which gave me much pleasure. I accordingly suggested to Littlewood that I would like to study, and research, in the field of mathematical statistics — then a subject that was almost new. He referred me to G. Udny Yule, the most distinguished statistician of the day, who suggested a line of work

to me; but he was primarily an economic statistician. He demonstrated the first calculating machine that I had seen — a recent acquisition on his part — and suggested that I should look at the periodicities of sunspots; I duly did this with negative results. But he then drew my attention to his own work on spurious correlations between time-series, and asked me to analyse mathematically some of his tentative, descriptive, theories. I tried to extend his ideas of serial correlations, but the progress that I made towards the much later concepts of auto-correlation and the power spectrum was small; the amount of calculation required effectively prevented its practical application. I did not allow my interest in celestial mechanics to lapse, but I was the only student to opt for H. F. Baker's lectures (I think he expected no-one). He talked to me on some dynamical problems in astronomy (variation of latitude, precession, etc) assuming that I knew more of the basic theory than I did — I should have read more and learned more.

Looking for a job

During the year I made tentative efforts to find a job, although this was not easy in 1930. I knew that I could certainly get a post as Assistant Lecturer (at a standard rate of £300 p.a.) at a smaller university or a teaching post at a school, but I was not interested in either. Through the Cambridge University Appointments Board I answered an advertisement by Rowntrees (chocolate manufacturers of York) for a mathematical statistician. I went to York and had a most interesting competitive interview, including one of the first intelligence tests. They offered me the post at a salary that I declined; the following day I got a letter offering me a much higher starting salary, but I turned it down!

Towards the end of the summer I was considering two jobs — an Assistant Lectureship at King's College, London, and a teaching post. Then I received an invitation to attend for interview for a post in H. M. Nautical Almanac Office.

It should be made clear that, although I was 22 (just) and had been at Cambridge for four years, I was extremely inexperienced in almost all practical aspects of life. I was incapable of assessing or appreciating the circumstances of my early years in the Office.

PART 1: AT GREENWICH 1930 – 1936

CHAPTER 1

First impressions

Appointment in H. M. Nautical Almanac Office

In late summer 1930, I received an invitation from L. J. Comrie, who had recently been appointed Superintendent of H. M. Nautical Almanac Office, to consider a post in the Office and, if interested, to attend for interview; he had been given my name by W. M. Smart.

I duly arrived at Charing Cross after the journey from Dewsbury, only to find that the train to Greenwich had been cancelled — this was due to the swing-bridge at Deptford having been swung to allow a ship to leave the docks. No-one told me that I could go to New Cross and get a bus, and so I was late for my interview and for lunch. Although Comrie was most considerate, he was clearly not pleased at his arrangements being upset. I was not the only candidate for the post. The other, better qualified and probably more suitable, was another Trinity mathematician, J. C. P. Miller, who was an Isaac Newton Student and who was already writing papers for the *Monthly Notices of the Royal Astronomical Society*. He was two years ahead of me, but after graduation he had had a serious illness which kept him away from Cambridge for nearly a year. Unfortunately for him, but fortunately for me both then and later, his health was such that he was not acceptable for the Civil Service. He could not be considered for a permanent position in Office, but Comrie was looking for a Deputy Superintendent. Comrie explained to me that he himself could only offer me an unestablished temporary post, and that appointment to the vacant post of Assistant, and possibly, Deputy, would have to be subject to open competition under the procedures of the Civil Service Commission. The job was, however, almost exactly what I wanted in that it combined mathematics, dynamical astronomy, numerical computation (in which I had always been interested even though there were then no courses in it, and numerical analysis was in its infancy) and practical application. I accepted his offer, at £6 per week, to start in October.

Staff and accommodation in 1930

At the time of my appointment, the Office was an independent entity responsible to the Board of Admiralty through the Hydrographer of the Navy and the Vice-Chief of Naval Staff (V.C.N.S., who was a member of the Board and to whom the Hydrographer was himself responsible). Except in so far as the Hydrographer might consult the Astronomer Royal, the Office had no administrative connection with the Royal Observatory, even though it was housed in the Royal Naval College within a few minutes' walk. Essentially, the Hydrographer's Department was the Headquarters establishment through which communications from and to the Board had to be made — through Civil Establishments Branch as regards finance and staff, and through the Director of Navigation and V.C.N.S. on matters concerning navigation and,

presumably, astronomy. The Office had a separate vote in the Navy Estimates, approved by Parliament each year; this gave details of the number of staff allowed, with salary scales, and of the “Lump Sum”, which the Superintendent could use to employ temporary staff, full-time or part-time at time-rates or piece-work rates, or to make small purchases within rather loose control (except in respect of rates of pay). The policy of few established staff supported by temporary assistance paid for from a Lump Sum had been encouraged by Comrie’s predecessor, P. H. Cowell; Comrie was engaged in reversing this trend, though with difficulty due to the economic situation.

In addition to that of the Superintendent, the permanent posts consisted of two Junior Assistants, Higher Grade, filled by the two brothers A. J. and S. G. Daniels, who had only just become established after many years of unestablished service paid out of the Lump Sum, and one Junior Assistant, Lower Grade, filled by W. A. Scott, who also had served for several years in an unestablished capacity. There was also the vacancy for the post of Assistant (or Deputy Superintendent), which I was filling. It should, perhaps be explained here that unestablished staff, paid from the Lump Sum, had no pension rights and very little security; establishment was extremely difficult to achieve as, even if a post were available, it was by open examination under the strict rules of the Civil Service Commission; candidates had to pay their own expenses to attend examinations or interviews, and successful candidates had to produce (at their own expense) evidence of medical suitability and to accept (except in very few cases) the minimum of the salary scale. Previously unestablished staff had to accept a reduction in pay on establishment, and, in most cases, to mark time (that is to stay at that pay level) until they had served long enough to reach the appropriate time point on the salary scale. As competition was severe, a would-be Civil Servant had a hard time!

In addition, there were a number of temporary, unestablished staff recruited locally by the Superintendent and paid out of the Lump Sum. Most of these were women, but one, E. Smith, had recently joined from the Royal Observatory, where he had been a computer; among them was a shorthand-typist (Mrs W. Rayson) and a punched-card machine operator (Mrs N. Frayne).

At that time the accommodation, on the first floor of King Charles’ block at the Royal Naval College at Greenwich, consisted of: one long narrow room used by the Superintendent and, when necessary, by his shorthand-typist; a large light room, communicating with the Superintendent’s room, often occupied by 6 or 7 staff (these were supervised by W. A. Scott, who sat on a high chair at a sloping-top desk originally designed for a standing clerk); and a largish, square, dark room on the opposite side of the stairway, later occupied by E. Smith and myself. In addition there were two large store rooms on the mezzanine floor (the ground-floor rooms were very high) and a small machine room, which housed the Burroughs Class II two-register printing/adding machine. Heating in the main rooms was by open fires attended to by the messenger, who was (I think) on the pay-roll of the College. There were toilet facilities for men, but the women had to use those in another building.

Early work in the N.A.O.

I joined the Office on 13 October 1930 and, if my memory serves me correctly, I found Miller, with Comrie, being instructed on that day in the numerical integration of Emden’s equation, which describes the internal structure of a gaseous star. Comrie was then the Secretary of the British Association Mathematical Tables Committee (B.A.M.T.C.), which, at the suggestion of Eddington, had undertaken to produce tables

of solutions for use in astrophysical research. Miller was already working, in Cambridge, on the theory of Cepheid variables. I then had my second lesson on a desk calculating machine (Brunsviga 4a), and an introduction to the practical solution of a second-order non-linear differential equation. Comrie later asked me to take charge of the project, with Miller's assistance, and we later published the completed solutions, with the necessary auxiliary quantities, in *B.A. Mathematical Tables* Vol. II, 1932, *Emden Functions*.

On my second day I was introduced to the brothers A. J. and S. G. Daniels, placed in their care and given desk space in their room (together with the other new recruit E. Smith). My first job for the *Nautical Almanac* (N.A.) was the preparation of copy for the 'Moon's Hourly' for 1935; this was the name given in the Office to the ephemeris of the Moon, giving its apparent right ascension to $0^s.01$ and its apparent declination to $0''.1$ for each hour of G.M.T. It involved subtabulating to twelfths the values for 0^h and 12^h of each day that had been previously calculated (by logarithms from values of the longitude and latitude calculated from Brown's *Tables of the Motion of the Moon*) and then checked by differencing. The method was extremely ingenious, but I was shown what to do and not why; the 'what' was difficult enough! It involved calculating, by means of tables with arguments derived from the differences of the values at the intervals of 12^h , the end-figures of the first differences of the hourly values; the second differences could then be deduced in full, since the maximum third difference is only 2 ($0^s.02$ or $0''.2$), and added (or subtracted) successively to give the first differences, in units of $0^s.01$ or $0'.01$, in full. These were written, as neatly as possible, in the spaces allocated on the large sheets that would eventually be the printers' copy; and then the first differences were converted, mentally, to minutes and seconds and added (or subtracted if necessary for the declination) to provide the right ascension and declination for each hour. Exact agreement with the original values at 0^h and 12^h was an essential check, but was not a sufficient guarantee against compensating errors of addition or the erroneous use of higher differences. The standard of arithmetic was high, and the penalty for error was (since this was printers' copy) the lengthy process of eradication of erroneous figures and the substitution of the correct ones (itself a source of errors) or the scrapping of a whole sheet; initially I had much practice in error correction, but I rapidly improved.

The Daniels and Richards

I was greatly impressed by the skill and conscientiousness of the brothers Daniels; they worked according to rules and instructions given to them by the Superintendent (primarily P. H. Cowell, but for newer work by L. J. Comrie), and applied them rigorously. They rarely made arithmetical errors, but, like every good computer, they always checked their work. (A. J. Daniels claimed, to my knowledge not quite accurately, that he never made a mistake.) They worked extremely quickly and continuously throughout their short day of 6 hours, apart from a break for a sandwich lunch. (They claimed, successfully, a shorter working week owing to some peculiarity in their terms of appointment.) They were excellent proofreaders, and completely dependable in everything they did, which included the preparation and despatch of copy and proofs to the printers. But their extensive practical abilities and experience were counterbalanced by their lack of theoretical knowledge; and I had to do my best to deduce the theory from the practice. The second job that I was given, on Comrie's direction, was the calculation of the ephemeris of the Moon at transit at Greenwich; this was regarded as a 'heavy' task, presumably because it involved inverse interpolation to

obtain the time of transit, followed by direct interpolation and checking by differencing at an interval of half a day. The Daniels clearly thought that I was inexperienced, which was true; but, after making a large number of systematic and accidental errors, I did finish my allotted task.

At about this time Comrie recruited a temporary Assistant, H. W. P. Richards, although there was no prospect of subsequent establishment. Richards had been a computer with the Colonial Survey in Tanganyika, but he had returned to the United Kingdom for domestic reasons; he was recommended to Comrie by the Director of the Colonial Survey, and he had had considerable experience of astronomical and miscellaneous computing. Another room, with only a small window, was made available and was occupied by Richards and myself. At first Richards was concerned with the programmes under Comrie's direction: prediction of occultations, star positions and apparent places, and the satellites of Saturn.

Emden's equation and other tasks

In addition to my 'routine' work on the Moon I was continuing with the solutions of Emden's equation, the computations of the auxiliary functions and the preparation of copy. Most of it was done in the Office (it needed a calculating machine) and some of the routine calculations were done by other members of the staff: but most of the copy, the explanation and proof-reading were done at home. Miller supplied some of the solutions, including the extension for the polytropic index $n = 3$ (the one most likely to be used), and much of the theory. The method of solution, used on a large scale for the first time, was suggested by J. R. Airey, who also supplied the coefficients of the ascending series. Essentially, the method is based on the fact that the formulae for the higher derivatives f, f', \dots , in terms of the function and its first derivative f' , can be derived from the differential equation itself by repeated differentiation. Having thus obtained numerical values of f, f' and higher derivatives at argument x , values of f, f' can be obtained from Taylor's Theorem for arguments $x \pm h$; agreement with the earlier value at $x - h$ gives confidence in those at $x + h$, before the next step is started. For a linear equation, recurrence can be used to calculate the higher derivatives, but in Emden's equation the formulae are lengthy. I was invited to present a paper on the method and the solutions at the British Association meeting in London in October 1931, and was allocated 30 minutes to do so; but earlier speakers overran their allocations and the chairman told me, as he called on me to speak, that I had 10 minutes only — and this was the first time that I had spoken in public.

I took great care in drafting the introduction, and I made somewhat extravagant claims in regard to the accuracy of the solutions and of the printing. (I had spent many hours proofreading!) Comrie was kind but adamant; possibly half of my draft survived his criticisms — that they were fully justified did not lessen their impact. But electronic computer solutions have since verified our manual solutions.

Richards and I were given the considerable, and responsible, task of computing from the appropriate tables the heliocentric ephemerides of the Sun and planets for the twenty-year period 1940-1960 for the N.A.. The period up to 1940 had been covered many years earlier, but the availability of calculating machines made possible many minor changes of procedure; we did try, however, to introduce some improvements by replacing tables (such as those for the equation of centre) by direct calculation. Scope for innovations was limited by the requirement that the ephemerides should represent the tables that were quoted as the authorities for them; we corrected actual errors (of

which there were quite a number) in the tables, but we did not feel justified in, for example, replacing the series for lunar perturbations of the Earth by direct calculation.

Apart from our inexperience, and ambiguities and errors in the tables, there was one recurring difficulty — namely the change in the beginning of the day in 1925. All the tables were based on 0^h G.M.T. being noon, whereas we were constantly using in other applications 0^h G.M.T. as midnight; also Brown's *Tables* used a half-day count. It is easy to say that such a difficulty should have been contained, but errors (usually in applying the 12^h difference with the wrong sign) did occur, especially when other staff were allocated tasks. In spite of the greatest care, and elaborate warnings, occasional errors continued to occur from this cause; the moral is clear: never introduce a discontinuity into a time-scale!

The work of the junior staff

The junior staff did 'routine' work: mainly operating the Burroughs and, later, the National machines; tearing and pasting the printed results to make printers' copy; and systematic simple arithmetical jobs, with or without a calculating machine. Most systematic computations were planned to be done on numbered forms, with one date (or other argument) to the column; Comrie had designed, and had obtained, foolscap computing paper with 56 numbered ruled horizontal lines and 8, 10, 12 or 6 x 2 (the most used) columns. The 'precepts' were set out on the cover sheet and, in addition to indicating the quantity to be entered (e.g., the first-difference contribution to an interpolation), would give how it was to be obtained (e.g., line 7 x line 10); an illustrative numerical example would also be given, if not already available from an earlier year. The packet, usually of 61 sheets covering the whole year, would be farmed out to whichever girl was free; usually only one line was done at a time. This procedure was far from ideal and meant boredom for any girl who had reasonable ability, and some frustration for the person in charge of the work: a girl would come in, dump a completed packet on the desk and ask "What do I do next?"; or no one would be available to do a routine stage required urgently. Using hindsight I think that most of them could have been taught, for example, to do interpolation using second differences without writing other than the final result.

CHAPTER 2

Mainly about the work of the Office

The method of cyclic packs

Calculations of the fundamental ephemerides of the Moon had been in progress for several years, and certain sections had been completed to the year 2000. The calculation of the lunar ephemeris from Brown's *Tables* was a major undertaking, not only in the amount of arithmetic involved, but also in the complexity of some of the instructions (devised by Brown to minimise the amount of calculation). Brown had devised a method of tabulating the periodic terms so that for each term there was a table of values arranged in columns. The number of rows in each column and the number of columns varied from term to term. Successive entries in a column corresponded to successive half-daily values; at the end of the column a switch had to be made to another column, not the next, according to a precalculated scheme. For each half-day the (human) computer had to add the appropriate value from each of the tables. Comrie brilliantly applied punched-card accounting machines, using this principle, to make the required summations. Each table was represented by a pack of punched cards; a change of column was then achieved by a discontinuity (by cutting the pack appropriately) in the order. Starting from a specified date, at which the serial numbers of the cards in the several packs (arranged in order of serial number) were given, cards could be taken (by hand) from each pack in turn to form groups corresponding to successive dates. This process continued until the operating instructions indicated that a discontinuity was required in one or more packs. At such a discontinuity, the packs concerned would be cut appropriately and the process restarted. Checks were made from time to time to verify that no error had been made in mixing the cards. Then the cards were fed into a tabulator in order to form and print the totals for each group. Usually the same packs and cycles could be used for the terms in longitude, latitude and horizontal parallax so that the three values for each date could be obtained in one run. As the cards came out of the tabulator they had to be sorted back into packs ready for reuse. Care had to be taken in planning the work to ensure that discontinuities did not occur frequently nor require the immediate reuse of cards that had only just been used.

The use of punched-card machines

The practical application was made more difficult by having to use punched-card machines at other establishments — at this time H. M. Stationery Office — at their convenience. As many cards as possible would be pre-mixed and sent in advance, and on the agreed day two members of the staff would take up the detailed programme, operating instructions and checks. On arrival the sorter and tabulator would be set and plugged for the particular layout of the cards. After checking, the tabulator run would be started. One operator would feed the tabulator with the mixed cards and, after their passage through the machine, would feed them into the sorter; the other operator would mix the cards into groups by hand and make the appropriate discontinuities as required. The process would be continuous, with the pre-mixed cards providing the necessary 'start' for the mixer, who had to keep pace with the feeding speed of the tabulator. But

the 45-column, round-hole cards went through the machines many times, and their leading edges were sometimes too rough for the finely-adjusted jaws of the feeds. The resulting jams and tear-ups required remaking the cards (sometimes from small scraps picked out from the machines) and restarting the process (of tabulating or sorting) after careful checking.

On one occasion I assisted Mrs Frayne, who was *the* expert operator, at a morning's session at H. M. Stationery Office when we had several tear-ups (possibly because I was not squaring up the cards as precisely as an expert). She was magnificent in handling the crises, as well as in operating all the machines and mixing the cards with incredible dexterity. We achieved our target with a few minutes to spare, with me being quite exhausted.

The great majority of this punched-card work had been done before I joined the Office, much of it under the direct supervision of Scott, though Comrie himself shared in the actual operations. It involved not only the tabulations of the lunar ephemeris, but also a very large amount of subtabulation (for various projects) by the end-figure process, using pre-punched cards selected by hand according to the differences of the functions to be subtabulated. These end-figures were differenced by hand on forms printed for the purpose, and the second differences, which could then be deduced in full, used for numerical integration on the Burroughs machine.

All those concerned with these processes deserve great credit. Scott had the responsibility for the organisation and supervision of the routine work by the temporary staff and deserves the largest share. Mrs Frayne was a most efficient punched-card operator and Miss L. H. Burr, among others, was a fast and accurate Burroughs operator. Miss Burr later operated the National machine, and was taken (with the machine) by Comrie to demonstrate when he gave lectures. The methods devised and, rather more importantly, implemented in the face of great difficulties by Comrie revolutionised systematic computing. They required the willing cooperation of staff to do repetitive tasks at high speeds with a very high standard of accuracy; and this he obtained. Using spare machine-time whenever possible the unit-cost of the work was incredibly small. Man- and, particularly, woman-power was extremely cheap, and Comrie, as a matter of principle, insisted on making optimum use of staff and machines. Some of the machines were at Imperial College, where Sidney Chapman was using punched cards for his major investigation into atmospheric tides.

It was one of Comrie's principles that it was wasteful to punch (and check) a quantity on a card in order that it be used once only. He prided himself on using both cards and punch-operators with the maximum efficiency. He disliked in principle the use of the verifying punch for checking punching, and insisted on listing followed by proofreading. {This attitude persisted in the N.A.O. for many years as is shown by the reluctance to punch from proofs in order to check the printing.} Cards would be designed so that they were fully used — some were even turned upside down so that they were fully used — and all time allocated on the machines was fully used. Routine work, such as end-figure differencing, was done at piece-work rates. I was responsible for paying for the work and I recollect that the rate was 100 figures for 1 penny (d)! Logs of output were kept for the Burroughs (and later the National) machine.

More on the use of cyclic packs

Reverting to the calculation of the heliocentric ephemerides for 1940-1960, we used the method of cyclic packs to calculate, among other smaller jobs, the nutation in longitude and latitude, and the lunar perturbations of the Sun. The conversion to geocentric apparent ephemerides was done by the 'new' method (made possible by the use of calculating machines) of combining, for each day, the heliocentric equatorial rectangular coordinates of a planet with those of the Earth. The whole process was planned to make optimum use of the first punched-card 'multiplying punch', which could read A , B , C from a card and punch $+A+B \times C$. The 0.25 – 0.5 million trigonometrical functions involved were interpolated from tables (using mental calculation), mainly by outside workers at piece-work rates. A 'reproducing punch' was used to copy data (such as the Sun's coordinates and the sine and cosine of the obliquity of the ecliptic) so that the multiplications could be done most efficiently — that is with the minimum rental of the relatively expensive multiplier. This was certainly the largest scientific use made so far of punched-card machines. The remaining stages (such as checking by differencing and the preparation of printers' copy) were done on the National machines, or by hand. The opportunity was taken of including the calculations and copy for the second volume of *Planetary Co-ordinates*, covering the years 1940–1960. This was my first punched-card job and, thanks to much help from the more experienced members of the staff, it went very well — in spite of the fact that the College labour force dropped the multiplying punch when unloading it and broke one of its legs, so that it had to be propped up on wooden blocks. A. E. Carter, who had recently joined as a Junior Assistant, did much of the operation.

The use of Brown's Tables

The complexities of the calculation of the lunar ephemeris were not reduced by the systematic calculation of some parts, often with modifications (such as added constants). The final stages of summing the many contributions, which had had been calculated at different times by different people, seemed to me to be peculiarly liable to error. Comrie agreed that there should be a series of completely independent checks, and he invited A. C. D. Crommelin (retired from the Royal Observatory) to calculate positions for a series of dates at 100-day intervals. I had the task of reconciling the N.A.O. calculations and the independent checks, very few of which agreed within the wide range of uncertainty that must be allowed in such a comparison. It was generally regarded that a reasonably experienced computer would require 6 hours to calculate the Moon's longitude, latitude and horizontal parallax from Brown's *Tables* for a single given date; it would be less for a series at a uniform interval, and, of course, much less for a systematic ephemeris. Crommelin was not familiar with Brown's *Tables*, and not as accurate as he was when he collaborated with Cowell in their epoch-making work on Halley's Comet. The calculations were full of errors of all kinds; misinterpretations of Brown's instructions; systematic errors in entering the tables, and in interpolation; and many arithmetical errors. The agreements proved to be accidental. After much labour, and many sessions with Crommelin at his home in Blackheath, we eventually reached the conclusion that the N.A.O. values did represent Brown's *Tables* — with only very minor queries outstanding. It was amusing to discover that Crommelin had used the backs of cancelled cheques for his intermediary calculations, and had retained them. I certainly learned more about the use of Brown's *Tables* in this way than I could have done by almost any other, and I got to know, admire and like Crommelin.

Status of N.A.O. work in 1935

By 1935 most of the fundamental calculations for the *Nautical Almanac* were complete up to 1960, though the final stages of the lunar ephemeris were outstanding, and the actual printers' copy was prepared more or less as required. There remained the ephemerides at transit, the apparent places of stars and several miscellaneous sections, such as lunar occultations, satellites of Saturn, etc. The data on eclipses, satellites of Jupiter and other planets, and on the ephemerides for physical observations were supplied by other national ephemeris offices, as were most of the apparent places of stars. The Daniels brothers supplied or prepared this material for the printer. Scott did much of the work on apparent places of stars, though only a small proportion was actually calculated in the Office.

Various incidents involving Comrie

Comrie was an enthusiastic proponent of the value of observing 'phenomena' and played a major role in encouraging amateurs so to contribute to astronomy, especially through the British Astronomical Association. He was responsible for the formation of the Computing Section of the B.A.A., and for most of its projects. These included: the introduction of the *Handbook of the B.A.A.*; the prediction and reduction of occultations of stars by the Moon and planets; the prediction of the mutual eclipses and occultations of Jupiter's satellites and the phenomena of Saturn's satellites. (These predictions were not given in the national ephemerides). Comrie's doctoral thesis had been on the prediction of planetary occultations, and he was keen to put his computing methods into practice. He wrote excellent papers giving detailed instructions, with worked examples, for these predictions. These instructions were followed by B.A.A. computers for Jupiter. For Saturn, he thought that the ephemerides of the satellites supplied by the U.S. Nautical Almanac Office were not the best possible, and he developed new expressions. Richards was made responsible for calculating the (simple) expressions to be given in the *Nautical Almanac* for use as the basis of the predictions of the phenomena.

Comrie had a habit of mixing official and unofficial duties and he persuaded Richards, as a member of the B.A.A., to undertake the actual predictions. This was a considerable task, especially as the timescale was so short. (The phenomena of Saturn's satellites are only visible at intervals of about 9 years.) Richards failed to produce the copy on time, although he claimed that he had sent it by post to A. E. Levin (then the director of the B.A.A. Computing Section and the editor of the *Handbook of the B.A.A.*) and that it must have been lost in the post. There was a row, and Richards was severely admonished. I never learnt or understood the full story, but I can recall that I was deeply sorry for Richards. I, together with several others including Comrie himself, repeated the calculations as quickly as we could.

An earlier incident had given rise to the same feeling. The B.A.A. had a requirement for storage space for its publications, and Comrie offered the use of the lighter of the two store rooms. Much of the material then in the store rooms was scrapped in order to compress the remainder into one room. Although I can recall the occasion well (Smith and I were given the task of moving the material and I was wearing a new shirt which suffered), I cannot remember what was destroyed. Certainly, many records and, possibly, some correspondence was lost. Specimen volumes were retained, but old attendance books, an old letter-press, old computations, devices for the calculation of the apparent places of stars, stocks of quill pens, etc, disappeared. The B.A.A. store room was used to store surplus stocks of publications as they came in from

the printer. Comrie gave the job of looking after the store, and of supplying the back numbers in response to requests, to a Miss M. E. Williams. She was an expert operator of the Monroe calculating machine — the only electric machine in the Office at that time. On one occasion she made an error (what and how I do not know) and sent out the wrong material to the wrong person; she was immediately sacked by Comrie. Other members of the staff came to me, on her behalf, to ask me to intercede with Comrie. With some trepidation, I did this successfully.

The occultation programme

The occultation programme of the Office was greatly expanded to include the predictions of occultations of bright stars by the Moon that could be observed from most habitable parts of the Earth instead of, as earlier, from only the British Isles and the British Empire. This systematic extension was made possible by the use of the 'occultation machine' that was designed and built by J. D. McNeile. This machine, which was made in 1928 in wood, simulated the relative movements of the Earth and the cylindrical 'shadow' formed by the Moon and star. It enabled preliminary times of disappearance or reappearance of a star to be estimated with an accuracy of about 2 or 3 minutes for a large number of stations at one setting. It also, perhaps more valuably, enabled conjunctions that did not lead to occultations in suitable conditions to be eliminated without calculation. Comrie purchased the machine for the Office, and arranged for an improved version to be made professionally in metal by the workshop staff (under A. C. S. Westcott) at the Royal Observatory. With his usual energy and efficiency Comrie planned every detail of the programme, from the predictions themselves (using the machine times as first approximations) to the arrangements for publication of the predictions in many different countries. The Occultation Supplement to N.A. 1938 gives a photograph of the new machine as well as details of the methods of computation.

Richards was in charge of the operation of the occultation machine and of the recording of preliminary times, but many others were involved. Many of the predictions were done by outside workers at piece-work rates. [If my memory serves me correctly, the rate was £30 a station, starting from the preliminary times taken from the occultation machine. An average number of conjunctions would be about 80, of which about 2/3 (perhaps rather more) would result in predictions. Two 3-figure calculations, each involving about 45 written quantities, were required, so that the average cost per step was between 1d and 1.5d; but copy was also required. I still consider that the rate was high in comparison with other rates then paid.] It was an enormous additional work-load for the Office, and any return could only be in the distant future. At that time the Office did not make any attempt to compare the ephemerides with observations, this being mainly left in the U.K. to the Royal Observatory. For the Moon, E. W. Brown personally compared the lunar ephemeris with observations of occultations, and after his death in 1938 this work was taken over by Dirk Brouwer. Towards the end of his life Brown had to spend much of the time in bed, where he continued to reduce the observations of occultations and to combine the results.]

In fact it was with Brown's requirements in mind that Comrie devised a method of reduction of the observations that used 'reduction elements', which were included in N.A. 1937. These elements were pre-computed for each observable conjunction of Moon and star, with the expectation that they would be used sufficiently often to justify computation and printing. Comrie also designed a series of printed cards on which the

reductions were entered; the final line at the bottom of the card gave the contributions to the equations of condition; and the cards had a series of holes on the left-hand side so that they could be placed on a peg-board to overlap and to show only the final lines. It was a grand project, but one for which I was never able to raise great enthusiasm, especially in view of the very limited complement of the Office. [This statement is made in anticipation of the decision, in 1940, to replace the calculation and publication of the occultation elements by a promise to reduce, after the war, all observed occultations of predicted conjunctions. I am not sure that this was a wise decision, but shortage of paper made publication difficult and the average number of observed occultations per conjunction was not high.]

Eclipse and comet work

Comrie had earlier re-expressed Chauvenet's classic methods of eclipse calculation in a more direct form suitable for desk calculating machines, and had given an impeccable illustration of their use. Nevertheless, the Office continued to take the data published in the *Nautical Almanac* from the U. S. Nautical Almanac Office, according to international agreement. His most lasting contribution to astronomy was, however, probably his introduction in 1926 of the use of a standard equinox for dynamical calculations. Typically, he not only formulated the idea but implemented it by making appropriate provision in the *Nautical Almanac* and in the separate volume of *Planetary Co-ordinates for the Standard Equinox of 1950.0*, covering the years 1920–1940, with an extension backwards to 1800. I was lucky enough to be given the job of using these tabulations for the calculation of the orbit of a comet, to be used as an illustration in the volume. Comrie made an arrangement with Miss Julie Vinter-Hanson (of Copenhagen) to use “her” comet — Comet Comas Sola (1926f) — for this purpose. We each used two methods (Cowell's and Enke's) of integration and made frequent comparisons to ensure that not only was no serious error made, but also that we agreed on the comparative merits of the two methods. We used the calculations to illustrate the published volume, and she used them to publish elements and a search ephemeris. (The comet, much fainter than predicted, was duly picked up not far from its ephemeris position.) I greatly valued the opportunity of working with and meeting Miss Vinter-Hanson, who remained a staunch and gracious friend until her death.

Navigational work

The navigational work of the Office in the early 1930s was mainly restricted to the routine preparation of *The Nautical Almanac, Abridged for the use of Seamen* (A.N.A.). Introduced for 1914, this was revised in 1925 with the change in the measurement of G.M.T. from midnight instead of from noon. Comrie introduced, for 1929, the concepts of *E* and *R* and so greatly reduced the chance of introducing error through applying the equation of time with the wrong sign. To what extent this change had been considered and approved by the navigational interests concerned is not clear. There was certainly a lot of (uninformed and unjustified) criticism in the Merchant Navy and in the Royal Astronomical Society, which was, for the first time, not consulted by the Admiralty! With the exception of the ephemeris of the Sun (given at an interval of 2^h), almost all the data in the A.N.A. were copied from the N.A.. Comrie, with his insatiable appetite for designing and providing tabulations to minimise the user's work, had introduced two innovations: the concepts of civil and nautical twilight (corresponding to depressions of the Sun of 6° and 12°), which with the established astronomical twilight (18°), would provide information on illumination throughout the

twilight period; and the tabulation of the times of moonrise and moonset for southern latitudes. Both of these were introduced into the N.A. and A.N.A. for 1937.

If I remember correctly, Comrie asked many people for suggestions for the names for the 6° and 12° twilights. The 12° depression is not necessarily the most suitable for judging the visibility of the horizon and stars; 6° and 12° were chosen to provide roughly equal intervals between $0^\circ 50'$ (sunrise or sunset) and the established astronomical twilight of 18° . It is impossible to say whether another value would have been better for surface navigation, but it was certainly a great advance. The decision to tabulate times of moonrise and moonset for southern latitudes had an interesting consequence: the Chief Examiner of Masters and Mates at the Board of Trade complained that the N.A.O. had robbed him of a standard question in the examinations!

I do not recall (but I may not have been consulted or informed) that Comrie actually consulted the Admiralty (Hydrographer or Director of Navigation) over these additions to the N.A.. He certainly discussed them, in my presence, with the Dean of the Royal Naval College, who was usually considered to be the Professor of Navigation. It is unlikely that he would have sought approval for changes in the N.A. itself, but the A.N.A. was a different matter on which the Admiralty should have been consulted. I think that he must have done so.

The whole of the preparation and production of the A.N.A. was in the hands of Scott, who also was responsible for the calculation and presentation of the rising, setting and twilight data. At this time (1936) the proofreading load was not as excessive as it subsequently became, partly because the Daniels did so much so well and because former members of the staff helped; but most of us did our 10-20 pages a week.

The use of National machines

The introduction of National accounting machines in 1933 transformed the methods of checking and calculation, and eventually the whole planning of the work of the Office. Comrie had, for many years, been looking for a commercial accounting machine that could be applied as a "Babbage machine" for mechanical integration and differencing. Several multi-register machines existed, but all suffered from the practical objection that transfer from register to register could only be achieved through a single "cross-footer". The Burroughs Class II machine, with one register and one cross-footer, was, however, in almost continuous use for integration from pre-calculated second differences, which were entered manually on the keyboard.

The original single-register Burroughs printing-adding machine had been introduced into the Office at a much earlier date, probably by T. C. Hudson, who retired in 1923. I met Hudson on two occasions; he was eccentric (some would have said he was a little mad), but he had quite brilliant ideas, particularly on interpolation and subtabulation. I only met Cowell twice; once when he visited the Office, in about 1931 or 1932, for a few minutes only, and once after a Commemoration Feast at Trinity (as Harold Davenport's guest) when I played bridge with him for one short rubber.

The National machine had, originally, 4 registers to any or all of which numbers could be added, either from the keyboard or by transfer from one of them, according to the positions of small levers operated by "stops" that were encountered in turn by the moving carriage. The register of which the contents were to be transferred or merely printed was selected by the operator. Two more registers could be added, but with some restriction; and direct subtraction was limited to two of the original four registers.

Originally it was not possible to transfer from 2 to 5 or 4 to 6, because registers 5 and 6 were added to the racks of the existing registers 2 and 4. This restriction, which was removed later, reduced the flexibility considerably, as it meant that the orders of operation could not be chosen freely to achieve the natural order according to the positions of the operating keys. Although not ideal, the National machine did fulfil both roles: it could integrate from sixth differences set on the keyboard; and it could difference to the fifth difference.

The main difficulty was obtaining approval to purchase. Comrie initially overcame this difficulty by persuading the British Association for the Advancement of Science (he was Secretary of the B.A. Mathematical Tables Committee, which was very active in calculating Bessel functions) to buy a machine and locate it in the Office. In return for processing B.A. calculations on the machine and for providing accommodation for it, the Office was able to use any spare time that was available. This was a decimal machine as required by the B.A.. But some time later the Admiralty and the Treasury gave approval to the purchase of a sexagesimal machine, designed to add in degrees (or hours), minutes and seconds; this was made possible by the commercial need to provide for calculations in pence. Both machines were much used.

CHAPTER 3

Mainly about L. J. Comrie and his work

Comrie and mathematical tables

Comrie had been Deputy Superintendent to P. H. Cowell from 1926 and had already in the four years before I joined the staff revolutionized the methods of calculation and the contents and form of presentation of the publications. Comrie had himself also done a great deal of table-making. His edition of *Barlow's Tables* and the *Standard Four-Figure Mathematical Tables* (prepared with L. M. Milne-Thomson) are still the finest such tables in existence in respect of content, presentation and accuracy. I have no personal knowledge of the computation of these tables, but I did see the page proofs of the four-figure tables, by which time Comrie and Milne-Thomson had quarrelled. Milne-Thomson was Professor of Mathematics at the Royal Naval College and he lunched in the mess each day, as did Comrie and myself. One day Comrie took me to task for entering into conversation with Milne-Thomson at lunch, warned me against having anything to do with him and showed me some of the proofs, without however telling me the full story. The basis of the quarrel was undoubtedly the quite different attitudes each took to accuracy. Comrie, the experienced computer, was prepared to devote much time to finding and correcting the few errors that would almost certainly occur; Milne-Thomson, the theorist and far less experienced computer, was not willing to spend time doing so, regarding it as unnecessary. [It might be added, as a pure speculation that I believe corresponds to M-T's philosophy, that M-T probably considered that the existence of a few undetected errors was of little consequence.] The known accuracy of both tables (neither is quite perfect) is a tribute to Comrie's enormous energy and care. I know of one error in each book, but there are (I think) a very few more, mainly extreme rounding-off errors.

Comrie had planned, in cooperation with Dr J. Peters of the Astronomisches Rechen-Institut at Berlin, the calculation of two major tables, one of 7 figures and one of 8 figures, of natural trigonometric functions for every second of arc. Each would have 162000 x 4 entries (sin, tan, cot, cos), or 900 pages with tabulations for 3' on each page. Peters supplied most of the many-figure basic data (taken from Andoyer's 15-figure tables in the main), and Comrie organized the calculations (built up on the Burroughs machine from hand-set second differences, and rounded off for copy) and preparation of copy (made by tearing the Burroughs sheets into strips and pasting them on specially-printed forms). Although most of this work had been done before 1930, it had not been completed (especially for the final stages of copy) by 1936. Again, Scott supervised the enormous, but routine, work of calculation, pasting and preparation of copy. Special "tearers" and pasting machines were used.

At the same time, or perhaps a little later, Comrie also prepared copy for the seven-figure trigonometrical tables with argument in time for every second; although essentially complete in 1932, when approval for publication by H.M.S.O. was obtained, and greatly used in manuscript form, they were not published until 1939.

Comrie and calculating machines

In addition to the truly enormous amount of work that Comrie did, his main interest was undoubtedly in computing and the utilisation of commercial calculating machines since purpose-built machines could not be financially viable. He made himself thoroughly familiar with the principles of design, technical construction and practical operation of all the available machines; and he analyzed and assessed their suitability for scientific computing, often bringing out applications not envisaged by the manufacturers. He wrote many papers describing various machines and their use, gave a course of lectures at Imperial College (as well as many invited lectures), and was inundated by requests from scientists for advice on the choice of machines. He did not always agree with the technical assessments made by the Treasury Investigation Department, which was the forerunner of the Organisation and Methods Department. He was much consulted by the leading manufacturers, particularly Hollerith (as the British Tabulating Machine Co. Ltd., now International Computers Ltd.) and Block and Anderson (the British agent for Brunsviga). He was able to obtain machines on loan for trial, so that the Office had the opportunity to try out almost all models of desk calculating machines.

Comrie's approach to computing

As regards computing Comrie was pragmatic, preferring self-checking methods (often based on iteration or the zeroization of a discriminant calculated for a number of values of the argument) to direct evaluation using an analytical solution. As a simple example, he would much prefer to find the real root of a quadratic equation by inverse interpolation from the values for three arguments, rather than use the standard formulae. He devised machine methods for iterative calculations (such as inverse interpolation); these had the great advantage that most or all of the data were visible. He planned the layout of lengthy computations (such as eclipses) with consummate care to minimise the chance of error as well as the amount of calculation. His approach to the solution of differential equations (and similar non-direct calculations) was based on the same principles and was coupled with his interpolation methods. Until Comrie clarified and codified interpolation, there was no recognised notation, few tables and very little available literature. The various *Supplements to the Nautical Almanac* (1931, 1935 and 1937) were reprinted under the title *Interpolation and Allied Tables* and transformed the whole subject. Although some of the basic ideas, such as the throw-back and bridging differences, were not new, they were now incorporated in a highly practical form for universal use.

Comrie as a 'consultant'

Many individual scientists (Hartree, Jeffreys, Watson-Watt, for example) and organisations (Colonial and Military Survey, Armament Research, etc.) sought his advice on their computing problems; in many cases he devised detailed lay-outs, and in others he undertook the actual calculations. The tables of travel times for the P and S phases of earthquakes are an example of his planning, while he persuaded me to assist Hartree with his work on self-consistent atomic fields. I spent many hours in my spare time at home (I was living in Comrie's house at the time) with these horrible non-linear differential equations, which had to be iterated with differing initial values until a "consistent" solution emerged. Most of the calculations for the self-consistent fields were 2 or 3 figures only, and could be done without a machine. But, on another occasion, he referred to me a system of differential equations which a visitor had

suggested might be suitable for solution on the National machine. After an evening's analysis I found an analytical solution and was able to derive numerical values using a dartboard, tape-measure and a square-pattern linoleum. On the following day I discovered that the visitor was J. R. Womersley (later the first Superintendent of the Mathematics Division at the National Physical Laboratory), who was then at the Shirley Institute of Cotton Research in Manchester, and I realized that I had reproduced the original cotton-weaving problem, and its simple solution!

[These illustrations are given primarily because I remember them well, but also to indicate Comrie's wide involvement with many scientists and others who were seeking numerical solutions to their problems. He was always willing to discuss and advise, both on methods and on choice of calculating machines — usually the Brunsviga for a desk machine; he was often tempted to take on some of the actual calculations.]

More on Comrie and mathematical tables

Another major interest was in mathematical tables; the National machine provided him with the ideal means for checking them and it was used a great deal for this purpose. I can recall a set of tables of higher mathematical functions (mainly Bessel functions) by a Japanese, Hayashi, in which Comrie found several thousand errors. A German, Brandenburg, optimistically offered a sum of money for each error in his 7-figure tables, and Comrie collected quite a number. Comrie wrote many articles on mathematical tables and he published lists of recommendations; later, he was responsible for the publication of the comprehensive *Index of Mathematical Tables* by Fletcher, Miller and Rosenhead.

Comrie had associations with the printing trade in his native New Zealand, and was familiar with printing techniques and terminology. He prided himself, with justification, on his ability to design and lay out tabular material, and he certainly set new standards in this respect, for both mathematical tables and the Office publications. His attitude towards computing errors (as distinct from proof-reading errors — he refused to accept that there could be printers' errors if the author took the precaution of using plates instead of loose type) was flexible: "absolute" accuracy implied errors less than $1 \times 10^{-(f+4)}$ for an f-figure table of trigonometrical functions; "workable" accuracy of $2 \times 10^{-(f+2)}$ for more difficult mathematical functions (e.g. Bessel functions calculated to 2 figures more than required, followed by subtabulation and rounding); but he would allow errors of 1, or possibly 2, in the end-figures for the apparent coordinates of the Sun, Moon and planets, controlled by differencing. Miller and I sometimes tried to do better. But, when it comes to balancing the amount of effort required to reduce the maximum error from $0.5 + x$ to, say, $0.5 + 0.1x$, there is little logical reason for it. I can recall Comrie's determination of the number of errors in Gifford's factor table: the first proofreading produced, say, 300 errors; the second independent reading a further 20; and a third, 5. What was the chance that there was more than one remaining? I don't think that he put it to the test.

Comrie and the International Astronomical Union, etc

As far as I know, Comrie had no leanings towards celestial mechanics or the improvement of the theories of the motions of the Sun, Moon and planets. His work on dynamical astronomy was limited to the calculation of cometary orbits, using special perturbations; and he was content (as was almost everyone else) with the theories and tables of Newcomb, Hill and Brown. Although adequate for most of his work,

mathematics was not his strong subject (he took his first degree in chemistry) and he did little theoretical work: apart from other considerations there was no time! As President of Commission 4 in 1932-1938 he was responsible for proposing, and getting agreement to, the adoption of a fixed numerical value for the Gaussian constant of gravitation. He attended the General Assembly in 1932 in Cambridge, Mass., leaving me in charge of the Office. In 1935 he created a so-far unique precedent in persuading the Admiralty to allow him to take his secretary (Miss M. M. Roberts) to Paris with him, at Admiralty expense, on the grounds that he knew little French! Neither the Astronomer Royal nor F. J. M. Stratton, then General Secretary of the Union, had any assistance! I attended the General Assembly at my own expense, but with half the time counting as duty. At both the 1932 and 1935 meetings (and, of course, later) much discussion took place on the nomenclature for G.M.T.. I think that Comrie rather enjoyed being able to rely on his repeated assertions that, by Admiralty orders, no change could be made in the use of G.M.T. (measured from midnight) in the *Nautical Almanac*. I am fairly sure that by this time he was convinced that G.M.T. was by far the best notation and that the danger of confusion was long since over; I do not know how often the Admiralty was formally consulted on the continued use of G.M.T. in the light of I.A.U. recommendations to use "universal time".

Comrie's personal relations

It seems necessary to say something about Comrie himself. Almost the first thing that he said to me (I think it was during my first interview) was that he did not suffer fools gladly. He was impatient with, and rather intolerant of, those who did not attain his own high standards. He did not make allowances for the frailties of others, and was far from tactful in pointing them out. Although he was almost certainly right, he was often rebuffed by those whom he criticised. He lost his expensive court-case against a firm which supplied him with a water-softener that did not fulfil the claims made for it. He was unsuccessful in his attempt to force the governors of the Roan School (within sight of his home) not to allow its clock to show G.M.T. instead of B.S.T. (or vice versa) on the Sunday following the change. Moreover, he quarrelled with numerous collaborators (in addition to Milne-Thomson), including R. A. Fisher the statistician, because they did not, in his view, take sufficient care to obtain accuracy. Fisher, who later became Sir Ronald Fisher, F.R.S., President of Caius College, Cambridge, was notoriously careless and it was said of him — not by Comrie — that anything that he did was worth doing again! Fisher was a member of B.A.M.T.C. when Fisher and Comrie had their difference of opinion — Fisher resigned.

On one occasion he caused an inter-departmental conflict by his criticisms of the *Manual of Field Astronomy*. It was the custom for a service department to allocate the task of revising, rewriting and editing service manuals to officers who (however efficient they may have been) had no special qualifications for such work; the manuals were then printed by the firm that presented the lowest tender to H.M.S.O.. The results were almost always deficient in presentation, clarity and accuracy, and often in fundamental concept as well. Comrie wrote a scathing criticism of the 1935 edition of the *Manual of Field Astronomy* prepared by the Directorate of Military Survey, listing the several hundred errors he had found during an evening's work; and he sent it direct to the Head of the Department. The General was furious and asked the Permanent Secretary to the War Office to demand an apology, which he duly received, from his opposite number at the Admiralty. One consequence of this was that when the

Hydrographer wanted a new edition of the *Manual of Hydrographic Surveying* he asked my advice on presentation and on some aspects of the content.

Comrie and bureaucracy

Comrie was equally assertive and tactless in lower levels. For example, when an order for stores was queried by some junior clerk in H.M.S.O. who asked why such a large supply of ink eradicator was required, he overwrote "to eradicate ink" and returned it to the Head of Stores. He was furious when H.M.S.O. changed the colour coding for tags, and he said so. He insisted on having a supply of punches remade because the register (determining the position of the punched holes) was about 1/10th inch different from his specification. This was at a time of great national economy, but Comrie specified and obtained the best available quality of stationery stores, including coloured carbon paper so that the order of numerous carbon copies was known. Although there was such a tight control over staff establishment and the lump sum, which could not be overspent by as much as a penny, there was essentially no restriction on the ordering of supplies (including books) from H.M.S.O.. I later discovered that the ordering code used was the Admiralty general code and that presumably no one queried the relatively small contribution by the N.A.O..

The most outrageous example of Comrie's behaviour arose in connection with an attempt by the Hydrographer (later Vice-Admiral Sir John Edgell, F.R.S.) in 1936 to smooth the relations with the Admiralty. After a personal discussion (most contacts were through Hydrographer's civil staff), Edgell reminded Comrie that he should submit periodical reports direct to him about the progress of the work of the Office and the plans for the future, but excluding reference to establishment matters. Typically, Comrie sat down and wrote a report immediately; Edgell returned it a few days later pointing out, with what must have been his disappointment, that most of the report was devoted to a restatement of Comrie's demands for additional staff which he had asked should be excluded. Comrie's reply was to the effect that Edgell's letter had requested the exclusion of establishment matters "which were to be dealt with through other channels"; the matters he had included were not therefore excluded since they were not being dealt with (that being the basis of Comrie's complaint), and that if Edgell had wished to exclude all establishment matters he would doubtless have used "that" instead of "which" to introduce the defining relative clause. Comrie probably had Fowler's authority for his view, but it is not surprising that Comrie was not popular within the Admiralty.

Comrie's marriages

Shortly after I joined the Office in 1930, Comrie's wife Noeline left him for T. Whitwell (a business-machine company executive and an amateur astronomer), whom Comrie had referred to as his best friend. I recall, with some distaste, the low-voiced remarks made by Mrs Comrie and Whitwell when I was playing bridge with the three of them; Comrie was almost deaf in one ear and could not hear them. After she left, Comrie installed a housekeeper and invited me to share his house. I did this with considerable comfort since he spent almost all his time, other than mealtimes, in his study working. In 1933 he married Miss P. B. Kitto, who had been on the Office staff as his secretary shorthand-typist, replacing Mrs Rayson. She was the younger sister of Grace, the wife of W. M. H. Greaves, then Chief Assistant at the Royal Observatory; she was a Fellow of the RAS and she used to be a frequent attender at meetings. [After Comrie's death she married again, becoming Mrs Betty Atkinson, but she was shortly

afterwards widowed for a second time.] Throughout these years he was a most generous host, entertaining most foreign astronomers during their visits to this country.

Comrie's relations with N.A.O. staff

Comrie's relations with his staff were mixed. He could be generous with his praise for a job well done, with bonuses for output (the hourly output of the Burroughs machine nearly doubled when a bonus was offered and paid) and with time off for special occasions (such as the Business Efficiency Exhibition and the Boat Race). Although he accepted computing errors as inevitable, he was intolerant of careless checking and unpunctuality, and at one time he caused a minor revolution by objecting to women members of the staff going to wash (they had to cross to another building) in office time shortly before finishing work for the day.

Comrie and the N.A.O. complement

There was a constant battle (no other word adequately describes it!) with the Admiralty about the complement of the Office, that is the approved number for each grade of staff. The procedure was that Comrie would send proposals, accompanied by long memoranda, including comparisons with the other national ephemeris offices, to the Hydrographer. These would then be received by the Registry and referred to the Chief Civil Assistant, (C.C.A., then Ewart Llewellyn, a most competent administrator, but who spent a large proportion of his time on the staff side of the various Whitley Committees), through a Higher Clerical Officer, whose job was to write a precis of the submission with a suggestion to the C.C.A. as to how it should be treated. Eventually, C.C.A. (with or without the specific concurrence of the Hydrographer) would send a docket containing Hydrographer's version of Comrie's proposal to the Civil Establishments Branch, which would handle it in a similar manner. The eventual decision would be transmitted back through the same channels and would reach the Office "By Command of their Lordships" without any reasons being given. Comrie refused to accept anything less than that for which he had asked, and submitted repeated proposals.

On details, such as the starting points on salary scales and the arrangements for advertising posts and for interviewing candidates, the Office could deal directly with C. E. Branch; but Comrie went directly on other matters when he could. The system, coupled with his poor relations with the Admiralty, posed an extreme difficulty for Comrie as his proposals were, apparently, never considered by anyone competent to judge them on their technical merits. Comrie dictated to his personal secretary — then a luxury which very few civil servants had — long, complicated letters in reply to most communications received from the Admiralty (there were many outstanding at any one time). After several weeks without a reply he would occasionally send a reply-paid telegram demanding a decision! There was, however, a gradual move towards an established staff at a reasonable level, a considerable advance over that in 1930. Other arrangements were also changed: up to 1932 [?], I personally acted as accounting officer and cashier (with no experience and no knowledge of Admiralty procedures). I paid all the temporary staff in cash, which I personally collected from the bank in which the Office had an account, kept petty cash and stamp accounts, sent monthly statements to the Admiralty, and maintained records of expenditure from the lump sum. Unfortunately, I paid a Miss Tickner (an unestablished typist) for the time when she was on sick leave, and months later this brought forth Admiralty displeasure and a decision, against which Comrie fought in vain, to transfer such cash duties to the

Secretary and Cashier at the Royal Naval College (C. E. Borrie), together with a demand for repayment of the money by Miss Tickner.

A note on my establishment and appointment

Although there was a vacancy for the established post of Senior Assistant (with the possibility of the holder being regraded as Deputy Superintendent — without additional pay!), no steps were taken to fill the post until 1933. I still do not know whether this was a consequence of Comrie's repeated refusals to accept C.E.'s pronouncements, or a deliberate delay on his part, possibly with the hope that Miller might be sufficiently recovered to pass the medical examination. Eventually, agreement was obtained for the post to be advertised. I duly applied and, after interview, was offered the appointment, subject to the standard Civil Service Commission conditions: a medical examination, for which I had to pay, of course; various undertakings to be signed; and the preparation of a Civil Service Certificate, for which I again had to pay. My starting salary was, I think, about £273 12s 0d. This was less than I was getting in an unestablished capacity, but I was entitled to six weeks leave a year! My years in the unestablished position have only counted as half-years in regard to pension establishment.

Very much later, I discovered among Admiralty papers the names of those on the short list of applicants for the post who were interviewed by the C.S.C.. As far as I recall, the Board consisted of a C.S.C. chairman, Comrie, the Astronomer Royal, and the Hydrographer (or his representative). The candidates included no less than five wranglers, most with the b* qualification. Miller withdrew because his doctor told him that he would not make the grade; all attained high positions in various mathematical fields. Competition was extremely high in the early 1930s and I doubt if I should have got the job if I had not been able to demonstrate at the interview my experience and my ability actually to do the work. Comrie, quite correctly, did not disclose who the other candidates were.

Other appointments

I have not included references to the recruitment and establishment of other staff for two reasons: full details are on record and I cannot remember dates and circumstances. Carter recalls in his note, however, that he and K. C. Blackwell from the Royal Observatory were successful in an internal competition for establishment in 1935, when Comrie had planned that the posts should be filled by E. Smith and N. H. Harrild, both of whom were in unestablished posts in the N.A.O.. I can recall Comrie's disgust that his plans had been disrupted and that the R.O. computers (of whom he had no high opinion) had been placed before the N.A.O. staff! Blackwell returned to the Royal Observatory after a short stay as a J. A. (L.G.); Harrild took his place, but he left after an even shorter period to join the Customs and Excise Service, where he prospered and reached a high position.

In 1935 the Office had its first vacation student as a result of a private arrangement between Comrie and J. A. Carroll, Professor of Natural Philosophy at the University of Aberdeen. Miss F. M. McBain spent several weeks in the Office during the summer familiarising herself with Comrie's computing methods. Afterwards she participated in an eclipse expedition, organised by Carroll under the auspices of the Joint Permanent Eclipse Committee, to Omsk in Siberia. In 1937 she joined the permanent staff of the Office.

“Winds” — a job for the War Office

“Winds”, which later became an official Office wartime job, was a project for the correction of the sound-ranging methods then used for the location of enemy artillery by allowing for the effects of wind and temperature. It was a classified War Office Project, but in 1936 it seemed to be the personal province of a Major Husskinson who, both then and later, was rather inflexible in his requirements. It was both computationally and operationally a horrible job. The basic requirement was to provide information to field personnel to enable them to correct travel times of sound waves for departures from the conditions of no wind, a standard temperature and a standard temperature gradient. Apart from the need to present the information, for any specified conditions, as a two-dimensional plot, there were two main difficulties. The first was that, in many cases, the shortest time-path is not the ground wave and has to be calculated by minimising an integral. The second is that a very large number of conditions (at least three parameters) are required; moreover the user has difficulty in choosing the nearest approximation to the observed conditions at his station. With official approval (since the job was classified), I “farmed out” some of the massive calculations to the mathematics staff of University College London and Bangor University College. H. Davenport soon came up with a three-parameter representation of conditions, simulating actual conditions at least as well as Husskinson’s rather arbitrary selection, that would allow the minimum travel times to be expressed in direct analytical form. This reduced the time taken for a plot, corresponding to each set of conditions, by a factor of at least three and probably much more. It also made possible a systematic representation of conditions, with the possibility of interpolation between them if required. But it took a long time to persuade Husskinson to adopt the representation.

Comrie’s final bid for more staff

During 1936 the progress of establishment was still too slow for Comrie, especially as he wished to extend the scope of the work of the Office. His services were in constant demand, and he decided to engage staff to be paid from his own pocket to do the computing for various jobs that he had undertaken. There was already the unofficial agreement with the B.A. Tables Committee, by which the Office staff could be used for non-office work in return for the use of the decimal National accounting machine. [I don’t think that this agreement was known to the Admiralty, but it may have been.] Comrie had no hesitation in “balancing” work done by “his” staff for the Office against work done by the Office staff for him. I have no doubt that Comrie considered that he operated this arrangement fairly and with mutual advantage, but it created difficulties of priority within the Office. One job, known colloquially among the staff as “Winds” (see previous paragraph), took an increasingly large part of the work of the junior staff. Comrie was still demanding additional staff and he wrote to the Admiralty saying that with the present staff it was impossible to find time to prepare copy for *The Nautical Almanac, Abridged for the use of Seamen* for 1937 — and that if the Civil Establishment Branch did not understand the seriousness of that, the First Sea Lord would!

On 19 August 1936 a small investigating team from the Admiralty (led by an Assistant Secretary, J. Lang, later Sir John Lang, who became the Permanent Secretary) descended on the Office without warning. After a couple of hours, they suspended Comrie from duty forthwith, they impounded all work on Winds and much other material, and they instructed me to take charge of the Office and not to communicate

with Comrie nor to allow him access to the Office. They found that the copy for the A.N.A. 1937 was complete (as it would be since Scott was in charge) and there could hardly be any doubt that Comrie was using the Office and its facilities for his private business. There was a formal enquiry, as a result of which Comrie's appointment as Superintendent was terminated; he never visited the Office again after he was suspended on 19 August 1936. Acting on specific Admiralty instructions, I had the unpleasant task of returning to Comrie his personal possessions and papers, and collecting from him, the books, papers, machine, etc., belonging to the Office that he had at home.

PART 2: AT GREENWICH 1936 – 1939

CHAPTER 4

Change and expansion

Changes in administrative responsibility for the N.A.O.

It is not possible to recall, either in order or in detail, the events immediately following the termination of Comrie's appointment. I was appointed Acting Superintendent and then there were consultations with the Astronomer Royal (H. Spencer Jones), with the Hydrographer of the Navy (Rear-Admiral J. A. Edgell), and with various people in Civil Establishments Branch of the Admiralty. Edgell asked for reports on various aspects of the Office and its staff, which I duly prepared and sent to him with excessive formality (as I recall with humility!). There was much to do.

All the staff paid by Comrie were dismissed, with the possible exception of Miss S. M. Burrough, who was partially paid as a temporary member of the staff; she soon accepted an offer by Comrie of a post in the Scientific Computing Service. The rest of the staff, relieved of their temporary work on Comrie's special jobs, such as "Winds", were brought back to normal work, and continued with the minimum direction from myself. A. J. and S. G. Daniels, together with Eric Smith, worked on the preparation of the *Nautical Almanac*, and were largely undisturbed. Richards and I were engaged on the preparation of the 1940-1960 heliocentric ephemerides. The person who had the main responsibility for the 'routine work' was W. A. Scott, and his experience was invaluable. He was assisted by Miss M. R. Rodgers, a recently entered Junior Assistant. They were magnificent, though I certainly did not give them adequate credit at the time; they (with assistance from Smith and Carter) saw to it that the junior staff were fully employed and supervised.

There appeared to be little surprise among the staff at the Admiralty's investigation and its outcome. Certainly there was no criticism of the termination of Comrie's appointment, and I received much sympathy and complete support in the difficult position in which I was placed.

The first decision, taken immediately after the termination of Comrie's appointment, was to make the constitutional change by which the Office became responsible to the Admiralty through the Astronomer Royal and the Hydrographer, instead of directly through the Hydrographer. The Admiralty decreed that the Office should be part of the Royal Observatory. The particular phrase used was something like "The Nautical Almanac Office will in future be under the direction of the Astronomer Royal". Although there was a gradual change towards full integration into the Observatory, the original arrangement (which lasted until much later than 1949 when the Office became physically part of the Royal Greenwich Observatory) was that the Office was a separate establishment under the direction of the Astronomer Royal. As such it continued to have a separate vote in the Navy Estimates, and there was little contact of any kind with the Observatory other than administrative — that is formal communications to and from the Admiralty went via the Astronomer Royal, and the

Secretary and Cashier took over the very minor accounting from his opposite number (C. E. Borrie) in the Royal Naval College. The only other contacts, apart from a few personal requests for advice on computing techniques from the Chief Assistants (W. H. M. Greaves and R. v. d. R. Woolley), were on the hockey field (Candler, Carter and I played for the Royal Observatory — quite a good team under Woolley's enthusiastic captaincy) and at the annual visitations, when the staff were "instructed" to volunteer to show visitors around.

I subsequently discovered that Spencer Jones had been extremely keen to have direct responsibility for the Office but had refused to accept it while Comrie was Superintendent. He took the view that its work should return (though it was, as such, never part of the Observatory) to the Astronomer Royal. He must have proposed the change as soon as he heard (from me) of the termination of Comrie's tenure of office; it was a change that had an obvious appeal to the Hydrographer and the Admiralty, in view of the appalling relations and difficulties with both Cowell and Comrie — while Spencer Jones' relations with both were excellent — and of my youth and inexperience. I was not consulted in any way and, although I put forward the strongest protest that I could muster in my weak position as Acting Superintendent (still on my old salary) when I was informed by the Hydrographer of the new arrangement, I could hardly expect my views to have any weight.

When my appointment as Superintendent came through in 1937 I noticed that the maximum of my salary scale was about £150 (I forget the exact figure) less than that of the Chief Assistants (and the professors at the Royal Naval College) to which it was supposed to be linked. It was even later when I discovered that the Astronomer Royal's salary had been increased, by at least the same amount, because of his additional responsibility. The Hydrographer (Edgell) promised that he would ensure that the maximum was revised before I reached the relevant scale-point; in fact, it was so raised.

In the early days, Spencer Jones' "direction" was — I venture to think — rather unimaginative. I was completely unused to official correspondence, and my draft report and my draft letter to the Hydrographer — signed "Your obedient servant" — were remarkably crude. But they, no doubt, created a welcome relief to Hydrographer, who had been called to task by Comrie for mixing up 'which' and 'that'. Initially, I tended to consult Spencer Jones on most establishment and administrative questions, as well as on the principles (but not technical details) of the various additional projects that the Office undertook. The consultations became less frequent as I became more experienced in dealing with the Admiralty. This trend was accentuated, owing to distance, after the Office was evacuated with the Admiralty to Bath in 1939, and owing to the Office taking on war-time projects that had little, or no, connection with the Observatory or with astronomy. Although I kept him (I hope reasonably fully) informed, it would have been unrealistic to have consulted him, or worked through him on, say, the organisation and work of the Admiralty Computing Service. He made occasional visits to the Office in Bath as an extension of his periodical visits to the Chronometer Department at Bradford-on-Avon. It was, however, often impossible to get him to give opinions or make decisions. Both then, and until he retired in 1955, I often had to assume that his silence meant consent to the proposals I made in personal discussions. (This same taciturnity was apparent when we were both officers of the R.A.S., the meetings of which provided opportunities for less formal exchanges of views on Office matters). On the whole, the arrangement worked well; I knew that I had his moral support, and (in general) his considerable influence behind my submissions to the Admiralty. On my

side, I tried to consult him as well as inform him — he may have been very wise in refraining from “interference”, though I occasionally thought that some encouragement might not have been out of place. We got on very much better together after he retired!

The work and staffing of the Office

The Office was in a most unstable state, both as regards work-load and staff — mainly due to the combination of Comrie’s initiative in undertaking new and additional (astronomical) tasks and his conflict with the Admiralty as regards the complement. The main work of the Office — the calculations for, the preparation of copy for, and the proofreading of the N.A. and A.N.A. — proceeded under the supervision of the Daniels and Scott; the fundamental ephemerides of the Sun, Moon and planets for 1940 onwards were by then available. The four main new undertakings, arising shortly before or shortly after August 1936, were:

the greatly increased occultation programme, involving the preparation and publication of an *Occultation Supplement*, as well as the calculation and publication of occultation reduction elements;

the design, calculation and publication of the *Air Almanac*;

the design, calculation and publication of tables for air navigation (the *Astronomical Navigation Tables* — known as A.N.T.s); and

the redistribution of responsibility for the calculation and publication of the apparent places of stars.

There were also a number of enquiries arising from Comrie’s activities. We kept being asked for advice on particular calculating machines — in particular by the Treasury Investigation Department. The Treasury disliked Comrie, but in 1936 it had little knowledge of ‘modern’ machines, and our advice on calculating machines was requested on several occasions. We were asked to recommend particular machines for various jobs and to report on new models. I was far from happy about this, as it seemed quite out of our orbit and it involved a great deal of work.

Another such approach in 1937, or 1938, was from the Ordnance Board, which had an armament research team at the Woolwich Arsenal, concerned with the ballistics of anti-aircraft (A.A.) gun trajectories. Hence my meeting with Dr J. W. Maccoll, who was interested in the application of the National machine to the calculation. They were using the “manual” produced in the 1914-18 war by a team of first-class mathematicians. But techniques had changed a lot, and I found a method of integrating the two simultaneous second-order differential equations at the same time on the National machine. All the multiplications and entries into the air-density and resistance tables had, of course, to be done on an auxiliary machine, but the National machine did all the numerical integration and differencing. To the accuracy required, the 11-figure accuracy of the (decimal) National could conveniently be divided into 2 x 6 figures as the first figure could be supplied by hand. With the availability of a Brunsviga 20 to do the calculating, it was possible to do a trajectory without writing, or with the least possible writing. Maccoll was very impressed and immediately ordered a machine.

[It gave me great pleasure to meet Maccoll again in 1975 at the inauguration of the new computer at R.A.R.D.E. at Fort Halstead; he had been retired for many years, but he recalled how the anti-aircraft ballistic tables had been calculated by National machines! (R.A.R.D.E. = Royal Armament Research and Development Establishment)]

These new activities will be described in appropriate detail later; their effect on the increase in the complement and in the requirements for additional equipment and accommodation will be dealt with first. Only the main outlines, and principal changes or appointments, will be included as full details of all appointments, and the appropriate complement, are on record.

The post of N.A.O. Secretary

Illogically in the circumstances, as I then and still do consider, the Hydrographer obtained Admiralty approval to second a senior Clerical Officer to act as secretary to the Office (and perhaps as my nursemaid!). It seemed unnecessary for its stated purpose of ensuring that I was familiar with Admiralty and Hydrographic Office procedures as the Astronomer Royal and his secretary were almost on the spot for consultation. The Clerical Officer (C.O.) received an allowance of £50, which could hardly have been adequate compensation for (in almost all cases) the additional journey from Central London to Greenwich; it was, however, accepted by the current senior C.O.. But since any vacancy in the Higher Clerical Officer grade was filled by the current senior C.O., changes of incumbent were very frequent as successive holders were promoted and left. What was I to do with them? Some of them became 'members' of the staff; for others it was just a step to promotion. Until August 1939 when Miss H. A. Howard was recalled to act as Billeting Officer in Bath the C.O.s were: E. T. Silk, R. Gornall, L. V. Granger, W. R. J. Brockwell, Mr Gibbs, H. A. Carrick, Miss H. A. Howard. The obvious difficulties were that there was far too little work and their training and outlook were so different.

On the whole, however, they were helpful and certainly tried to interest themselves (some more than others) in the work of the Office — particularly Silk, Granger and Miss Howard. It was a recurrent chore for me to find jobs which they could usefully do; without exception none had any mathematical or scientific background, so that they could not help with the actual work of the Office, though they did some proofreading occasionally. I had a shorthand-typist to whom I dictated letters; and it took far longer for me to discuss minor administrative matters with the C.O., to approve the draft letter of minute, and for the shorthand typist to type it, than it would have taken to dictate it directly. Eventually I started an elementary abstracting system with the intention of building up a card-catalogue of articles in certain well-defined fields. The articles to be included, and notes thereon, were made by the staff, and the C.O. did the rest. But the scheme was too ambitious and too poorly planned to be of real use — I was (and still am) ignorant of bibliographical practices and I was also too busy to devote much time to it. Although the Office certainly gained by the secondment of these C.O.s, there can be little doubt that the arrangement was not the most efficient means of achieving its object.

Silk rapidly became a valued person and was popular with the staff; after promotion he kept in touch, and turned up at the 200th birthday celebration of the *Nautical Almanac*; he died in 1976. The last person was Miss Howard, who was in the Office when we moved to Devonport House, but she did not stay very long. She was most efficient and played a large part in the organisation of the move — although she objected to the view of the hospital's mortuary from her window! She was called back to the Admiralty to act as Billeting Officer for the move to Bath. She has continued to keep in touch with some of the members of the staff then in the Office.

Initial recruitment of junior staff

At a fairly early stage a complement was agreed by C. E. Branch for the Office. The Lump Sum, from which temporary staff were paid, was much reduced but was retained, primarily to allow payments to be for “outside work” at piece-work rates. A number of staff had (by 1936) been established, including A. J. and S. G. Daniels, and some established staff had been recruited. Comrie had, however, refused to accept any grade lower than Junior Assistant, Lower Grade (J.A.(L.G.)), then roughly equivalent to Assistant Experimental Officer (A.E.O.) in the post-war scientific Civil Service. The agreed complement did allow for some Clerical Assistants, for which grade the qualifications appeared adequate for much of the more routine work of the Office (for example, operating the National machines). At, or about, this time A. E. Carter, G. A. Harding, Miss M. R. Rodgers, E. Smith were established as J.A.(L.G.). Before the expansion in 1937, however, Greaves (who was, incidentally, Comrie’s brother-in-law through Comrie’s second marriage with Miss P. B. Kitto, sister to Grace Greaves) remarked that, in the actual staff, Comrie had been replaced by a C.A. (Clerical Assistant)!

It was not my intention to attempt to refer to all staff appointments, but it is perhaps worthy of special mention that Comrie’s secretary, Miss M. M. Roberts, left a month or two after him for a post in London. She had much higher qualifications than were necessary for the shorthand-typist post that was the only one available in the Office. She was replaced by Miss V. M. Hooper who, although quite efficient at her job, had very odd religious views (she would, for example, refuse to type an excuse for not accepting an invitation if she knew that it was not precisely true). We only got her services because (as I later discovered) the Superintendent of the Admiralty pool insisted on getting rid of her. But, in spite of occasional difficulties, she served the Office pretty well until her behaviour during the war became too extreme for us to cope with.

The Clerical Assistants (not by any means the Junior Assistants, who were in a higher grade) were recruited from local schools — and by any standards they did a magnificent job. The first recruit in that grade was Miss D. J. Ifield, who with training at evening classes and inside the Office learnt to type and became assistant to my Secretary, Miss Hooper. She was one of the successes of the whole staff and became a great asset to the Office; now Mrs Barrett, she still keeps in touch. Five ‘ANTs’ were recruited to work on the *Air Navigation Tables*: Miss M. C. Scadeng (later Mrs Cooling) in charge as J.A.(L.G.); and as Clerical Assistants, Miss E. N. Histed, Miss V. H. Hitches (Mrs Rogers), Miss M. B. Simm (Mrs Goodfellow), Miss R. E. West (Mrs Hinkin) and Miss J. E. Pullen (Mrs Boas). They did a truly magnificent job and, although they worked extremely hard in very difficult conditions, seemed to enjoy it. [Miss Histed died from tuberculosis during the war; she was transferred to London from the hospital in Bath after the air raid.]

It must be said that Marion Rodgers, who supervised their initial training in computation, was the key member of the staff; apart from being extremely competent in her work, she was firm and tolerant with — let us face it — a bunch of 16-year olds straight from school. There were many more in later years — Kathleen Restorick (Mrs Hewitt) and her sister Iris Restorick (Mrs Rhodes), Miss Vera Peasgood, Miss Joyce Mounteney, Miss Jackson, ... — but Marion has kept up with them all this time.

The staff required for the third project (the A.N.T.s) were only needed for long enough to complete the tables — about three years. The printing of the 17 volumes of A.N.T.s took several years, and it was not finished until after the war, when a final volume covering latitudes to the pole was published. With the completion, I sent the Treasury an account of the expenditure — it came to less than the £5000 previously estimated. But the girls who did the work were paid miserly wages.

Expansion for air navigation

The most senior posts in the Office — as Assistants — were recruited later as a considerable expansion of the work and of the staff came in 1937, with the requirements for the *Air Almanac* (A.A.) and the *Astronomical Navigation Tables* (A.N.T.s). Apart from additional staff, it was possible to establish Richards as an Assistant and to promote Scott to J.A.(H.G.). We had a vacancy for an Assistant (in my position) for a permanent post, and I got approval for a temporary post for an Assistant for the A.A. and A.N.T.. Both were open to general competition, and were advertised through the Civil Service Commission. Although I saw the applications, and suggested 6 or 7 names (out of some 40 or 50) to appear on the short list, I was not invited to serve on the interview board, the Office being represented by the Astronomer Royal. The successful applicant was Miss F. M. McBain, who had been a vacation student. If I remember correctly, the list of other candidates was quite impressive — an Assistant post (salary maximum for a man was about £278 a year) was amply good enough to attract a first-class degree. She duly took up her appointment and served, in a part-time capacity in later years after her marriage, until she retired in 1973.

A few days after the closing date for the permanent post, an application was received for the temporary post — the applicant had been away on a skiing holiday, so missing a chance for a permanent post. We appointed W. E. Candler, a first-class honours graduate of Trinity College, who had (although I did not know this until much later) been recommended by Sir Arthur Eddington to the Astronomer Royal as a Chief Assistant. His research work was on the theory of stellar interiors, and he thought that a mixture of two (or more) values of the index number n of Emden's equation would meet the physical conditions. He served us well, supervising the early stages of the A.N.T.s and doing the exploratory investigation that was necessary. He did other jobs as well and was also encouraged to continue his research on polytropic gas spheres. Later he took a more general interest in the work of the Office. He served until 1941 when he had to be released for more urgent war-time duties — first at Orfordness and later at Helensburgh.

We had a number of Junior Assistants in the period before the war. A. E. Carter was recruited by Comrie from the Royal Observatory — the Temporary Computers at R.O. had only a limited outlet, and many took the opportunity of transferring to the N.A.O., where establishment was somewhat easier. We subsequently took on W. G. Grimwood by transfer from the R.O. where he had failed to become established and would thus be redundant; he became, still later, a J.A.(H.G.). He was transferred back in 1953 and to Cape Observatory in 1967. Direct entry from examination by the Civil Service Commission resulted in the appointment of G. A. Harding.

We then had to find accommodation for all the extra people. The Royal Naval College put at our disposal two or three rooms on the upper floor and actually provided a ladies toilet that was (almost) inside the building.

Acquisition of new calculating machines

I do not recall the details, but approval was also given for several additions to the equipment: a new decimal National machine and several desk machines, including a fast semi-automatic electric Marchant with nine multiplier keys to be used in succession for the multiplier. The new National machine was to replace the one on loan from the British Association; this was installed at University College and was used, under my distant supervision, by a computer paid by the British Association Mathematical Tables Committee — a most unsatisfactory arrangement! [The computer was then Miss S. M. Burrough, who had previously worked in the Office and in the Scientific Computing Service. She continued to have firm friendships with members of the staff of the Office; after retirement she won several first and a second prizes at the Royal Horticultural Show.]

Complete logs were kept on the National machines, mainly under the supervision of Carter. They were so prone to mechanical errors that the National mechanic became a member of the tea-club and almost a full-time member of the staff! I recall with some pride how I tracked down a series of apparently unconnected and unexplainable errors that completely defeated all the mechanics. The girls worked in two-hour shifts and I first discovered that all occurred while Miss W. D. White (later Mrs Carter) was the operator. By watching her operate, I noticed that she had a habit of resting her hand, very lightly, on the keyboard. The very slight key depression during a transfer operation would cause the number, corresponding to the depressed key, to be transferred instead of the correct one — provided it was the larger. It was no wonder that analysis of the errors failed to indicate the cause!

CHAPTER 5

New developments

The Occultation Supplement

Comrie's plans for the 1938 *Nautical Almanac* included an appendix on the Prediction and Reduction of Occultations. In August 1936 copy for this should have been ready; unfortunately, Comrie had taken all his notes on the subject, but there still remained in the Office his detailed lists of precepts, and appropriate forms, for the predictions. It was decided to defer the appendix to a separate publication. I started to draft this Occultation Supplement from scratch, conditioned to use Comrie's methods in every respect; I had expected to receive every assistance from Richards, particularly in the provision of complete illustrations of all stages of prediction and reduction. I recall my disappointment at his apparent lack of cooperation — or was it merely delay? — and my need to do most of it myself; it was quite a big job. It also involved the first publication by the N.A.O. of details of the Occultation Machine, which was designed and constructed by A. C. S. Westcott on the model of the original constructed by J. D. McNeile. There are a number of errors in the Supplement due to my lack of appreciation of the methods, and I think that I should have avoided them; but there were few numerical errors in the illustrations and no serious "bloomers". It was issued in 1937 under the title *The prediction and reduction of occultations as a Supplement to the Nautical Almanac for 1938*.

The additional work for the occultation programme involved a lot of time operating the occultation machine and much outside work on actual predictions, together with all the organisation and supervision required. Although Richards was nominally responsible, Scott was the "expert" on the machine. Miss McBain later worked on the programme and eventually took over completely. Comrie had planned it in great detail, with special tables prepared for each station, and with printed forms for the 3-figure calculations which had to be done twice — first with the approximate time from the occultation machine and then with the improved time. I, personally, had little or no connection with the programme, though I had (like everyone else) done some routine predictions. Frankly, I was not in favour of the Office devoting such a large proportion of its potential on the project, which had previously been in the hands of amateur astronomers. [The question is arguable on both sides, but the arguments are not relevant here.]

Air Almanac and Astronomical Navigation Tables

Shortly after Comrie left, two R.A.F. officers (Wing Commander W. Underhill and Squadron Leader P. H. Mackworth) from the Operational Requirements (O.R.) Division at the Air Ministry, visited the Office to continue an earlier discussion they had had with Comrie concerning the provision for astronomical navigation in the air. They were terribly enthusiastic, but were unused to numerical calculation. I recall their proposal (in writing!) for sight reduction tables giving direct solutions of the PZS triangle for every minute of arc of the three arguments; I was soon able to persuade them that this was not the answer. I had no experience of navigation by 'astro' — except a course at Cambridge in which the highlights were the determination of the

position of the Cambridge Observatory, using a sextant and reflector [artificial horizon] — and Chambers' 7-figure logarithm tables. We produced the *Nautical Almanac – Abridged for the Use of Seamen*, but it gave no indication of the means of reduction of sights. Moreover, the Office, as such, did not — and had not since the time of Maskelyne — produced sight reduction tables. This was due to the Admiralty insistence on the cosine-haversine method, and, as I later found, to an arrangement with a commercial firm for the provision of the relevant tables. The *Abridged Nautical Almanac* (A.N.A.), as it is usually called, was completely standardized and its routine preparation (mainly copying) was handled by Scott.

The demands for the R.A.F. were urgent; a form of *Air Almanac* (A.A.) for 1937, and the first volume of the *Astronomical Navigation Tables* (A.N.T.s) by the end of 1938. This did not leave very much time for planning. After a general agreement on the contents and arrangement of the *Air Almanac* and on the A.N.T.s, it was arranged that the Air Ministry should formally request the Admiralty to allow the Office to undertake the projects — with an inter-departmental payment. The Admiralty (probably the Hydrographer, but I cannot remember) arranged two meetings to discuss the projects, *technically*. There was an open meeting at the Royal Geographical Society, and an internal one, at which the representation of the Royal and Merchant Navies was much in excess of the representation of the R.A.F. and the one representative of civil aviation. The Astronomer Royal was the principal speaker at the R.G.S. meeting, leaving me to describe our, then rather tentative, proposals. My recollection is that the discussion was not very informed nor helpful — the only points raised were: the relative merit of tabulating G.H.A. stars directly or G.H.A. Aries and, what later came to be called, S.H.A. Stars; A. R. Hinks (Director of the R.G.S.) strongly objected to our invented names (e.g. Avior) for the bright southern stars for which no recognised names existed. At the internal meeting, almost all the discussion centred on the extent to which the *Air Almanac* could be used at sea, and whether its availability would affect the use of the A.N.A.. The meeting recommended that the two projects be undertaken — and appropriate approval was given without delay.

Introduction of the *Air Almanac*

The first approach to the *Air Almanac* was in the form of a cover, with constant information such as interpolation, into which an 'ephemeris' could be inserted. This was an attempt to make the actual almanac as light as possible, but the experimental edition for the last quarter of 1937 (which was produced rather quickly) was far from ideal, particularly in respect of the need to transfer the daily pages to the separate cover. This attempt failed, and it was speedily changed to a 'tear-out' Almanac, in which unwanted sheets would be torn out. There were several changes of format before the present form was adopted — itself to be considerably modified when unified with the *American Air Almanac*. With hindsight I consider that we were by no means as far-seeing as we could have been expected to be, even though neither of the two technical meetings were adequately critical.

My recollection is vague regarding the details. I knew very little about surface navigation, and nothing at all about air navigation. The only publication for comparison was the French *Ephemerides Aeronautiques*, which was based on the concept of 'vers-R.A.'; its bulk rendered it quite unsuitable, and I doubted its value. [It was later dropped and replaced by the equivalent of the *Air Almanac*.] Many of the suggestions for revision, and improvements, came from the active and fertile brain of (then Squadron

Leader) “Kelly” Barnes, who wrote the admirable *Manual of Air Navigation* and, later, inaugurated the Specialist Navigation School at Cranage (near Byley, Cheshire). It was he {or was it Mackworth?} who, requiring notations and symbols for use in the manual, demanded more-or-less instant decisions from me. Firstly, for $360^\circ - \text{R.A.}$ (in arc), for which, admittedly without large-scale consultation, we introduced S.H.A. (sidereal hour angle), now generally accepted. Secondly and less controversially, for the correction to the observed altitude of Polaris to give latitude for which we adopted Q , also now in general use. As far as I can remember we had the copy ready before the R.A.F. had made up their minds. The S.H.A. was criticised in that it was *not* an hour angle, but the critics were not faced with an instant decision; moreover, the various alternatives [e.g. left ascension] were cumbersome and unsuitable. A minor point was that of star names; the R.A.F. insisted that the stars must be named, and there were two or three bright stars in the southern hemisphere that had no classical names. Scott made a hurried search of the literature, and we adopted the names Avior and Peacock for the two stars without names.

At a somewhat later date, Scott designed a series of ‘posters’ showing the 22 stars used in the *Astronomical Navigation Tables* in the field of view of neighbouring stars. These were printed by the R.A.F. and exhibited widely.

The responsibility for the preparation of the A.A. later came under Miss McBain, but the actual routine work of preparation of copy was done under Scott’s supervision. The division of responsibilities and duties varied from time to time, according to varying demands and to the staff available.

Astronomical Navigation Tables for use in the air

The design of the A.N.T.s was rather less hurried: it was a R.A.F. requirement that provision for both stars and Sun and planets be in the same volume in a similar format. The emphasis was on single sights and single position lines, rather than on position fixing; this largely determined the arrangement. Although the main details of the A.N.T.s were sound in principle, there is a major defect in the tabulation of the stars. Mackworth did, as I recall, at one time suggest using L.H.A. Aries as argument for the stars instead of L.H.A. Star, but (with the single-star arrangement) this would have doubled the amount of tabulation required. It affords, however, the user an automatic selection of stars and a much simpler form of calculation. The device had been used for certain stars in Weems’ Star Altitude Curves, and later in the astrograph, but was not introduced into formal sight reduction tables until after the war. We did not consider the ‘Hutchings’ arrangement of tabulating the altitudes and azimuths of the optimum selection of stars for each latitude and value of L.H.A. Aries — a great pity and a regrettable oversight. I have since regretted that I did not use this opportunity for introducing the argument L.H.A. Star in the A.N.T.s. Otherwise, the A.N.T.s were a ‘model’ tabulation, with printing of a high standard and impeccably proofread. I spent much effort on typographical design of the tables and, in retrospect, I am reasonably satisfied with the result — however inappropriate for its purpose it may have been!

[The ‘Hutchings’ arrangement was used in the first edition of *Sight Reduction Tables for Air Navigation* (H.O. 249) published in 1947; but the idea had been earlier used by Hoehne.]

I was required to give the Admiralty an estimate of the total cost of producing the tables, excluding printing and binding, but including proofreading, etc.. This depended

on the method of computation, and there was a difficult choice between the use of punched-card machines (the newly developed multiplying punch could be used to interpolate trigonometric tables) and the combination of hand-methods with the “adding” National machines. We had no punched-card machines — although we had used the Multiplying Punch for a period of 6 months — and the only practical method then available for production was letter-press printing, which involved preparation of suitable copy and proofreading. So we decided to use the National machine to produce what is *sin* altitude (H). A special table in critical form to give H , which is the altitude affected with refraction at 5000 ft, was used to allow computers to enter the appropriate altitudes directly into the copy — thus avoiding rounding-off and copying errors. For the azimuths we interpolated, using the method of bridging differences, at wide intervals on the National. I am proud of the fact that that my extremely careful estimate (in non-inflationary days!) of the total cost was within 1% of the actual cost. It worked out in total for £5000, including 5 extra staff. The ‘package’ was approved by C.E. Branch and the Treasury, and staff were recruited in 1937. The large staff of 5 provided flexibility to keep a smooth flow of copy, proofreading, etc, to match the printer’s promised (and attained) output.

On this point, I should mention that in late 1936 the Hydrographer brought down to see me the U.S. Hydrographer, who had with him the first copy of the first volume of H.O. 214 *Tables of Computed Altitude and Azimuth*. We discussed the publication, and there was general agreement that it was too large (and too accurate) for air navigation. He left me a copy to go through in detail. That same evening, while idly turning the pages, I proofread a page and found 3 errors; on the next page I found 5 errors. Further on, I found the whole calculation of the azimuth was wrong. It was a terrible example of table-making. Working at the explanation, I discovered a systematic error; the difference for declination was given for the actual value of the declination but the precepts and the interpolation table itself were given for the excess of the declination. After some further checks, by Scott, the next day, I gave a summary of my findings to the Hydrographer, and asked him to convey my comments to U.S. Hydrographer by cable. The first edition of H.O. 214 was formally withdrawn, but, being wary of Comrie’s position in criticising the *Manual of Field Survey*, I did not publish my comments.

I.A.U. General Assembly in Stockholm in 1938

I attended the General Assembly of the I.A.U. in Stockholm in 1938 — for the first time as a full member. Comrie was still President of Commission 4, and this could have been awkward. I certainly tried to give him appropriate credit for the massive work he had done for the Office, and also in inaugurating the cooperation that led to the publication of the annual volume of *Apparent Places of Fundamental Stars* (A.P.F.S.). We had much discussion of the proposal — made by Comrie — for the combination of the apparent places of stars in one volume, instead of the duplication involved in the separate publication in various almanacs. I drew up a programme by which I proposed a limit of 200 stars in the individual almanacs while the apparent places of all the stars in the FK3 (1535) would be given in a separate volume to be prepared by the N.A.O.. Since all the apparent places of stars were interchanged freely between the offices of the five nautical almanacs, this meant a marked diminution in the work of each. I had previously made proposals with the Astronomer Royal and with H.M. Stationery Office in respect of the new publication. Admittedly, I had only carried out Comrie’s concept in practical terms.

One snag threatened to force a revision of the programme. Professor G. Fayet of the *Connaissance des Temps* said that his office could not take on the work of calculating apparent places of stars. This was countered by an offer of the U.S.S.R. Institute for Theoretical Astronomy to supply them. I accepted the offer — and undertook to provide an explanation in Russian, in addition to those in English, French, German and Spanish. I ventured to think that my ready acceptance of their offer contributed much to the subsequent good relations between the Offices.

As President of Commission 4, Comrie arranged discussions on astronomical navigation and mathematical tables — subjects not usually mentioned at I.A.U. meetings — primarily to mention the work being done by his Scientific Computing Services Ltd. I reported on the ephemerides for air navigation with a sample page of the *Air Almanac*; and mentioned the A.N.T.s. Comrie poured scorn on the A.N.T.s, and gave particulars of his own *Sea and Air Tables*, which I subsequently described in a review as one of the finest navigation tables that I had seen. It was a masterpiece of table-making, and, unlike the majority of such tables, it was completely accurate. But like the A.N.T.s it was soon overtaken by other — sometimes more crude — tabulations.

Comrie proposed a resolution recommending publication of the 7- and 8-figure trigonometrical tables, which he, in association with J. T. Peters of the Astronomisches Rechen-Institut, had prepared, and for which copy was in the Office. The 8-figure table was almost complete, but the 7-figure table required some detailed work. They had been impounded by the Admiralty as having been done by N.A.O. staff paid by the Admiralty. Peters wanted publication, and I tried hard to get Admiralty to authorise publication by H.M.S.O. or to allow some other organisation (such as the Ordnance Survey) to do so. I failed — it was a big task to print a 900-page book which neither side was prepared to pay for. Peters replied to me that he could provide the finance for the 8-figure table. So it was eventually agreed to allow Peters to publish the 8-figure table in Germany, leaving us with complete freedom to publish the 7-figure table as and when we could. It was also agreed that I should take the 8-figure table to him, and keep the 7-figure table in the Office, in the hope of subsequent financial help. I took the whole copy with me to Stockholm, but returned via Berlin, where I handed over the copy to the Director of the Astronomisches Rechen-Institut, Professor A. Kopff. The *Achtstellige Tafeln* were published shortly before the war, and there was great demand for it from the War Office. It was then photographically reproduced. I cannot remember, but it may have been my copy that was used!

Once the 8-figure table was available, there was no demand for the 7-figure table; I tried at various times to have it published - and eventually we wrapped it up, with a note concerning its origin, and deposited it in the Royal Society Archive of Unpublished Tables. [The Royal Society inaugurated the storage of unpublished tables at the request of the R.S. Mathematical Tables Committee, which took over from the B.A. Mathematical Tables Committee.].

A threatened dispute on copyright

I had been invited to give a paper on the applications of the National machine at the meeting of the British Association to be held shortly after the I.A.U. meeting. Two days before I left for Stockholm I received a letter from Comrie's solicitors threatening me with an injunction if I claimed credit or priority for discovering, and applying, the potentialities of the machine. Naturally, I had no intention of taking away any of

Comrie's well-deserved credit, and I ignored the letter. But I did introduce two applications of which I think that Comrie was unaware. Firstly, Carter and I had discovered a very much improved 'set-up' for differencing. [The 'set-up' gave the operating instructions together with the sequence of the 'stops' that determined the operations on the numbers in the six registers of the machine.] Secondly, we had demonstrated how we could use the machine for the solution of differential equations, such as those for anti-aircraft gun trajectories. Comrie was present, but he made no comment and took no action.

Apparent Places of Fundamental Stars

The detailed arrangements for the *Apparent Places of Fundamental Stars* (A.P.F.S.) were made by correspondence with the directors of the other national ephemeris offices (France - Fayet; Germany - Kopff; Spain - de la Puente; U.S.A. - Robertson; U.S.S.R. - Subbotin). It was a considerable organisational task for which we designed two forms. The first on which the cooperating offices were to enter the apparent places and the second on which they were to be pasted up for copy, thus obviating the making of duplicate copy. We found, however, that we had to recopy in many cases as the original copy was not of sufficient quality, and so the amount of work was greater than we had expected. The proofreading was also 'heavy', in the sense that the best check against accidental setting error was the consistency of functions and differences. We initially proposed that the first volume should be for 1940; but we soon realized that this could not be achieved — copy would have to be sent to the printer in early 1939 — and so it was deferred until 1941. The volume for 1941 was published approximately on schedule. All the arrangements for the volume, within the Office, were supervised by Richards, who was also responsible for the auxiliary data, lists, indexes, etc.; he did an exceedingly good job with great attention to 'awkward' detail, such as double stars, proper motions, etc.

Other publications

One other project, albeit a very minor one, was the preparation, duplication and circulation of an Eclipse Circular giving data for the total solar eclipse of 1940 October 1. This was undertaken at the suggestion, and under the guidance, of J. A. Carroll (who was then Professor of Natural Philosophy in Aberdeen). It contained (if I remember correctly!) central-line data for coelostat settings, and for bearings of sunrise and sunset points (for some weeks ahead of the eclipse) for ensuring that the apparatus was correctly orientated. Similar data have not been given, as far as I am aware, either before or since! Eclipse Circulars are traditionally the province of U.S.N.O., and are usually both comprehensive and elaborate.

Two other publications were prepared in this period, though they may not actually have been published until later in 1939. The *Seven-figure Trigonometrical Tables for every Second of Time* had been essentially prepared by Comrie, but required completion and editing. The volume *Planetary Co-ordinates for the years 1940-1960 referred to the Equinox of 1950.0*, which was a continuation of the earlier 1800-1940 volume, was prepared *ab initio*. The ephemerides were prepared as part of the major operation of converting heliocentric to geocentric ephemerides, and were supervised by H. W. P. Richards. The example, illustrating the use of the ephemerides in the calculation of the orbit of Comet 1933f, was calculated and prepared by Miss F. M. McBain. The proof-reading added to the already large load borne by all members of the staff.

Towards the end of the period (i.e. 1939) the amount of proof-reading each year was enormous: the N.A. of some 800 pages; the A.N.A. of 300; the A.A., eventually about 1200; A.P.F.S., about 600; 3 or 4 volumes of A.N.T., each about 300; predictions of occultations for some 40-50 stations sent out in manuscript for publication elsewhere. Each member of the staff spent about 2 hours each day on proofreading in some form, and much use was made of those outside workers (some former members of staff, such as Doak, Sprigge) whose ability was known. It would have been an impossible task forty years later, since the staff would not do such 'unproductive' work, and, in any case, it would cost too much.

Of course, we still continued to receive much data for the N.A. from other ephemeris offices, and, in return, we sent them stereo proofs of the first part of the N.A. containing the fundamental ephemerides. A. J. and S. G. Daniels undertook the responsibility for ensuring that copy for the N.A. was prepared and ready to be sent to the printer according to schedule. They could be relied on absolutely. Richards, and later Miss McBain, was responsible for dealing with the printer and for ensuring that the proofs were read and corrected, and that the routine exchanges were sent and received.

We were fortunate with our printers, C. T. Tinling and Co. Ltd. of Prescott, near Liverpool; they took over from Truscotts in 1936 or 1937 and over the years we developed a good relationship with them. Their work was good, although it was not their usual line (which was some newspapers, novels, commercial colour printing). H.M.S.O. were helpful sometimes, but kept changing the staff who handled our work as soon as we had 'trained' them in our ways.

CHAPTER 6

Procedures and moves

A comment on official procedures

It was during the period of expansion that I was first really shocked by official procedures; my strict non-conformist conscience did not readily accept the need for anything less than complete frankness and true estimates of requirements. The A.R. (Spencer Jones) strongly advised me to ask C.E. Branch for a larger establishment than I first proposed, and much against my inclinations, we did so. I seem to recall (it will be on record) that we got just about what I thought was really required. After my return from Stockholm in 1938 it was desirable that I should place the whole staffing position for approval by the Admiralty and C.E. Branch. I drew up detailed specifications for all the jobs required: preparation of *The Air Almanac* (A.A.) and proofreading; preparation of the *Astronomical Navigation Tables* (A.N.T.s) and proofreading; preparation of *The Apparent Places of Fundamental Stars* (A.P.F.S.) and proofreading, with an allowance for the loss of the preparation of some apparent places. I duly prepared detailed estimates of the numbers and grades of staff required for the man-hours involved, together with submissions to the Admiralty for approval to undertake the projects. The case for A.A. was straightforward — it was an R.A.F. military requirement — but the case for A.P.F.S. was by no means watertight; it involved considerable additional work, with the main compensating savings being made by the other four co-operating offices of the national ephemerides (France, Germany, Spain, U.S.A.). Although justified nationally (we received other benefits in return) and a considerable advance internationally, the direct benefit to the Admiralty was negligible. My submission to the Hydrographer (through the A.R.) was criticised by his Chief Civilian Assistant. He took the view (supported by the Hydrographer and the Astronomer Royal) that A.P.F.S. should not be mentioned; and that all the staff should be supplied for air navigation. He considered that a list of staff for the supply of air navigation material for the R.A.F. — without giving details of their work — would be approved without question by Civil Establishments Branch. It was! But I resented that the considerable work in preparing and proofreading A.P.F.S. went without full recognition. I learnt a lot, but I still consider that it was wrong. The submissions in detailed form that Comrie made for additional staff merely added to delay and to the number of queries made, and they led to the general air of distrust.

Move to Devonport House

The additional staff needed additional accommodation, and Royal Naval College had little to spare. There had earlier been a proposal that the Office should leave R.N.C., but no suitable alternative accommodation was offered. Comrie turned down, indignantly, the Admiralty's suggestion of a redundant warehouse in the East India Docks! The pressure eased, however, as the College was generous in making available several quite good rooms on the second floor of King Charles' block; although not directly communicating with the older rooms, they proved adequate.

At the end of 1938, or beginning of 1939, the authorities of the R.N.C. were under pressure of accommodation difficulties; they were introducing new courses and

the number of students was greatly increased. (I think that the War College course was introduced then.) They approached the Admiralty for alternative accommodation to be found for the N.A.O.. During 1939 we got offers of accommodation from a most unlikely set of sites; many possible locations, mainly in Greenwich, were inspected, but only three were seriously considered: the Trafalgar Quarters on the river bank (later turned into luxury flats, and later into a luxury restaurant); some old buildings (I think an old school) immediately east of R.N.C., now destroyed, with the site a car-park; and, finally, one floor of Devonport House, a separate part of the Nurse's Home attached to the Seamen's Hospital. We inspected it and it proved suitable, with the minimum of alteration. It provided admirable, modern office accommodation, but it took much effort to persuade the Admiralty to lease it.

I think that the original date for our moving was June/July, but the financial arrangements — which were of no concern to us — delayed the project. I cannot now recall the precise reasons for the situation in which I found myself on the day arranged for the move to Devonport House, which was only a short distance, about 300 yards, from R.N.C.. The removal vans (actually supplied by the R.N.C.) were packed, all the staff had been transferred, and I was alone in my old office with a packing case and a telephone trying to get the final Admiralty Board approval that had been promised each day for the past ten days. I do not like moves at the best of times, even with the optimum arrangements, and I must have been in a terrible temper. I telephoned in succession to all those, in ascending order of seniority, who were directly concerned, and I received only promises. In desperation, I then rang the Principal Under Secretary, who was very annoyed — he was impolite, saying that he could not be troubled with such a matter — but he listened to what I had to say and promised action. I got the approval within the hour!

We were only in Devonport House for a few weeks (with much material still unpacked, or unsorted) before we were evacuated early in September 1939 to Bath.

Evacuation to Bath

During 1939 everyone realised that war was a probability and that large-scale evacuation of London and other large cities would be necessary. Many of the staff took courses in Passive Defence at Woolwich. From confidential reports it appeared that the Admiralty had made no plans for the evacuation of the N.A.O.. Discussions with the Hydrographer were indecisive, so I asked permission to try to make my own arrangements. I therefore got in touch with Sir Arthur Eddington, the Director of the Cambridge Observatory, to enquire about the prospects for taking on the Office in the Observatory. He invited us to visit him, and accordingly Miss McBain, Dr. H. R. Hulme (a Chief Assistant at the Royal Observatory) and I drove to Cambridge to inspect the site. It was rather cramped, but we provisionally made arrangements with him to occupy rooms at the Cambridge Observatory that would otherwise not be required during the war. We also looked into the question of staff accommodation (billeting), and concluded that, in spite of the difficulties, arrangements could be made reasonably. We returned to find a telegram telling us not to go to Cambridge; we duly reported this to the A.R. and the Hydrographer. The A.R. had, I learnt later, made alternative arrangements for himself and his staff in Abinger, where the Time Department moved to.

Within a few days, and only a few days before war was declared, the Admiralty instructed us to report to "town AA9" to join the Hydrographic Department at the

Admiralty's evacuation headquarters. I was informed that AA9 was Bath, but was not permitted to tell the staff; they were instructed to report to Paddington Station, with railway warrants made out to AA9 and with a minimum of luggage. Fortunately, the staff found out about AA9, for at Paddington — where the vouchers for AA9 were to be exchanged — no one knew what AA9 represented! But there was a special train to Bath.

We had not been in Devonport House for very long, only a matter of weeks, before we received our orders to move. I had asked for transport to carry files and papers — and particularly the National and desktop calculating machines. Our instructions, which were common to all Admiralty departments, were that we should find tables and chairs etc. in the new offices at AA9. Stringent conditions were set on what could be taken: no desks or chairs, no bookcases, no cupboards — in fact only the papers upon which the staff was working were to be taken. On the morning we left, a large van drew up at the entrance, and all the staff agreed to fill it with furniture in addition to the machines and papers. We worked very hard, as we had to catch the train at Paddington, and the van driver wanted to get away. Some of the material was loaded before the staff left for AA9; all helped in a hurried scramble to parcel, and label, papers and to load them, using the tea trolley to move them along the corridor to the lift. But the 'strong men' — Carter and Smith certainly, possibly Harding and Grimwood — stayed behind and loaded all the furniture, which proved to be an enormous help. They certainly did a fine job! They were well helped by our messenger (who was really classed as a labourer), Farrer, who chose to come with us; although he liked his beer and was not the most politely-spoken of men, he was genuinely fond of the Office and of the staff. We were able to take almost all the furniture (desks, tables, chairs, bookcases), but it was not possible (and it had been specifically forbidden) to take other than essential, current files, records, etc.. As a consequence, a considerable mass of old calculations, files, records, pictures, etc. was left at Devonport House for subsequent removal and storage at the Royal Observatory. The arrangement was that the R.O. would remove everything that we left behind, since we would evacuate the whole office. The items left behind were stored in the old "New Library" building, which had long been used as a general store; it was damaged during the war and some material was destroyed. (None, as far as is known, was of great importance as it was mainly old calculations, proofs, etc.) Circumstances were not conducive to the making and keeping of records of what went where.

I travelled by train with the rest of the staff. They were all pretty cheerful — possibly in that we had a good friend (Helen Howard) as Billeting Officer in Bath. Fortunately, Miss Howard had been recalled to Hydrographic Department a few days before to act as Billeting Officer for the Department at Bath. She had a most difficult task, but coped extremely well and was able to ensure that the N.A.O. staff had reasonably satisfactory billets. Each member of the staff has some story to tell about his or her experience in billets in Bath. The comradeship among the girls, who were in their teens, was tremendous — as exemplified by the fact that over 40 years afterwards they are still excellent friends.

Appointment as Secretary of the R.A.S.

In the second half of 1938, I was surprised to be approached by the Chief Assistant at the Royal Observatory, W. M. H. Greaves, who had just been appointed Astronomer Royal for Scotland, with a request to act as deputy for him as Secretary of

the Royal Astronomical Society. In 1938 I was a member of the Council, and Greaves and I often discussed R.A.S. matters when we met (we played bridge). When he accepted the post of Astronomer Royal for Scotland in 1938, he proposed to Council that I should be acting secretary, and later that I should be nominated as Secretary of the R.A.S.. Greaves as he realized that he could not travel every month to London. He would retire in February 1939, and he assured me that it was the wish of the Council that I should take his place. I consulted the A.R. about taking the appointment, and he agreed that I should. Greaves was Comrie's brother-in-law as they had married sisters. His excuse in asking me was that I had found reading *Monthly Notices* very heavy since it was becoming all astrophysics and little classical astronomy; he said that as Secretary I would *have* to read the papers!

I was duly elected in February 1939, and thus had my share of responsibility during the war for the actions necessary to safeguard the property of the Society and to ensure, as far as practicable, the continuance of its activities. There was little to do for some months, since Greaves handled the papers in cooperation with H. H. Plaskett, the other Secretary. All I had to do was represent Greaves at Council and at Ordinary Meetings. However, the prospects of war grew worse as 1939 progressed, Plaskett was called up as reservist and sent to an artillery battery, and I was left to deal with all the normal secretarial work and to cooperate with the Treasurer in dealing with the security of the R.A.S. premises. The President, H. C. Plummer, was ill in Cambridge. The full story is told in my section of the history of the Society, covering 1939-1952. {Sadler's account of "The decade 1940-50" is in *History of the Royal Astronomical Society, volume 2, 1920-1980*, pp. 98-147, and his photograph faces page 98. Ed. }

It was always my intention to separate my work for the R.A.S. from that for the Office, except on the full day, once a month, for the Council and Ordinary Meetings. But this did not work out. I had to make several visits to London in the summer of 1939, and thereafter when in Bath I had of necessity to take all Friday and Saturday, once a month, for R.A.S. business. I managed to do some other work (such as that involved in the Admiralty Computing Service), but this was the pattern. I had kept on my bedroom in London and, due to the cooperation of the staff of the R.A.S., I was able to cover all the 'post-agenda business' from the Council meeting before catching the 6.50 from Paddington to Bath on Saturday evening. I resigned the secretaryship of the RAS in 1947 in order to give me more time in the Office. By then, I had a co-secretary (W. H. McCrea) who could take on my work for the Society. During my time in Bath, being a bachelor in a 'billet', I devoted every Sunday, apart from tennis in the morning, to R.A.S. work. I shall not mention again the work for the R.A.S..

PART 3: AT BATH 1939-1949

CHAPTER 7

Early days at Bath (before move to Ensleigh)

Early days in Bath

Towards the end of September 1939 most of the staff travelled by train to Bath, and the others followed a day or so later. {The female staff travelled on 27 September.} On arrival in Bath we were told where our billets were, and the location of the Office. We were very fortunate in that the Office was to be located in a boarding house 'Laggan', in College Road, halfway up Lansdown. It was (I think) a staff residence for the Royal School for Officers' Daughters. It was a fine, large house, standing in extensive grounds. The Royal School (some 100 yards away on the main Lansdown Road) was taken over by the Hydrographic Department, who had there the Chart Department and the Sales and Distribution Sections. In Laggan there were, in addition to the N.A.O., the Assistant Hydrographer, the Tides Department, and the Superintendent of Sailing Directions and his small staff. Sailing Directions was largely staffed by retired officers, who I think would normally work at home. The senior officer was Vice-Admiral Sir ("Daddy") Nares, brought back from retirement after serving as head of the International Hydrographic Bureau (in Monaco); he was the cousin of Owen Nares, an actor who was familiar to all of us. There were several Captains, and Commanders, including Farquharson (Superintendent of Tides) and Shearme, who was one of the two joint authors of the 1922 edition of the *Admiralty Manual of Navigation* — the other being W. M. Smart.

We were given adequate accommodation, though it was rather cramped, especially in the larger general computing room. Miss McBain, Candler and the Daniels shared the conservatory. We were, however, treated very fairly. The only furniture provided consisted of large wooden trestle tables and folding chairs, with open shelving for records. But this was changed when our van arrived! I was placed in a rather delicate position. We, thanks to all the staff who had worked so hard in loading, had all (or almost all) of our desks, chairs, and bookcases with us — and we were faced with a number of senior naval officers who had no better equipment than trestle tables and uncomfortable chairs! I put the problem to the staff — with my recommendation that we should 'loan' a certain number of desks to the senior officers, including the Admiral, until their furniture could be rescued from the Admiralty in London. There was a little demur, but we negotiated with the Admiral an agreement that was excellently received. I think some of the junior staff resented giving up their really good quality pre-war desks and using trestle tables, but it was much better freely to offer them (for the use of senior officers) than to have them commandeered, as I am pretty sure they would have been.

Our 'messenger' had come down with us, though he was not present on the moving day since he had no family in Greenwich, and he helped us to unpack. He was totally devoted to the Office staff — although he did drink (but not in Office hours). He served us all with bets on the Derby and Grand National!

On the whole, I think we were very fortunate to have accommodation in Laggan. For some time the resident housekeeper supplied me (and other senior staff) with coffee and sandwiches for lunch. But then we all had to go half a mile up Lansdown Hill to Kingswood School (also taken over by the Admiralty), where there was a canteen, or a mile or more into the centre of Bath. The magnificent Assembly Rooms had been (or were later, I cannot remember) turned into “a canteen for over-paid Civil Servants”, as the local papers put it! But there were many restaurants, cafés and pubs.

My billets in Bath

The staff were billeted, and all had interesting experiences, some good, and some not so good. Later many, or most, found their own accommodation. Others can tell you of their experiences with billets, and how they overcame them in finding flats and houses.

I had a variety of billets, and this suited me, as I had little time for social life. My first billet was in Somerset Place, where I was with a largish group of supply officers from the Hydrographic Department. The first night, I walked down to Bath to get something to eat and after dinner I came out of the restaurant to find complete blackness! My torch did not work, and I had only a vague idea where Somerset Place was in relation to the centre of Bath. I must have had a sound sense of direction because I discovered that I had taken the shortest route back! I did not stay in Somerset Place long, but (thanks to Miss Howard) I was then billeted with Col. Barryman, who had a large house. They were keeping the billet for a senior officer, who did not turn up; I was surprised at their standard of living. They had four servants (for two people), including two housemaids; they had coal fires — Barryman told me that they normally ordered a truck of coal (equal to 10 tons) at a time — and they rang a bell for a housemaid to put more coal on the fire! My standard of life went up with a bang! I was valeted, with all my clothes neatly folded and laid out! And my car (an old Austin 7) could just occupy a space in his garage next to his Rolls-Royce. He was a keen croquet player, who played in the Croquet Club in College Road.

I did not stay very long (I think that the senior naval officer turned up at last), but I then went to stay with an Anglo-French couple, in College Road; they were very kind to me, though I could not understand their politics or their ideas. Eventually, I was introduced to Mrs Thornton, a recently widowed lady who badly needed a lodger to share her large house. By sheer accident her late husband was a dilettante in that, being rich and unoccupied, he took up odd things (such as landscape gardening, chess and astronomy). He was a great friend of the one-time President of the B.A.A., Dr. A. N. Carr, whom I had known reasonably well. Mrs Thornton was a daughter of a member of the Hobhouse family, and had numerous relatives in Wiltshire. Her husband had been in business in Russia (U.S.S.R.) and had lost much of his money, through the planned ‘trials’ of business people in the U.S.S.R.. He had to sell his magnificent house in Wisley, and bought a house in Bath, ‘Villa Julia’ in Weston Road. I spent several years living, nominally as a billetee but with a private arrangement, in Villa Julia with the elderly Mrs Thornton and her elderly cook and housemaid until it was largely destroyed by a bomb during the ‘Baedeker Raids’ on Bath.

I got on very well with Mrs Thornton, who was rather a cantankerous person. She had a daughter, married to a diplomat, who was a delightful person. She and Solomon shared the same music teacher, and on the occasion that Solomon came to give a recital in Bath, she invited Solomon to stay at Villa Julia. Perhaps it was fortunate that he had

to go back to London, because Mrs Thornton had proposed to tell him what was wrong with his playing. She did not go to his concert. Actually she was very critical of her daughter's husband (later he became Sir Ian Henderson) on account of his policies.

Throughout the war and, in fact, until 1949, I maintained an arrangement with my pre-war landlady in Lee by which my furniture, many books, etc., were stored, and I was able to use my old bedroom whenever I visited London. Such an arrangement was invaluable to me, especially for R.A.S. meetings; but there were many other meetings, and I probably spent an average of one night a week there.

Initial work in Bath

We settled down rapidly in Bath and continued to work on the *Nautical Almanac*, the *Abridged Nautical Almanac*, the enlarged *Air Almanac*, the *Astronomical Navigation Tables*, and *Apparent Places of Fundamental Stars*, a new publication. This work involved an immense amount of proofreading. I cannot remember the order in which different events occurred, but that can be found from the Office records and publications.

The necessity for reducing the size of the *Nautical Almanac* (primarily because of paper rationing, but also from considerations of bulk, convenience to users, and the amount of work involved — especially of proofreading) led to the restriction of the permanent tables and explanations to the minimum and to the omission of the Occultation Reduction Elements. The latter were, however, published for two years (1942 & 1943 ?) by Professor Dirk Brouwer in the U.S.A.. The occultation prediction programme was, however, continued without substantial change; though, when Japan entered the war, the predictions for the Japanese stations (then already posted) were returned on the grounds that “they would be of material assistance to the enemy”. The promised replacement of the omitted ‘permanent’ portions of the N.A. did not take place until the publication of the *Explanatory Supplement* in 1961.

I was always critical of the occultation programme, and questioned the calculation and publication of the occultation reduction elements; it seemed to me that the labour of calculating the reduction elements, and publishing them, was then more than the reduction of the actual observations. We later decided to cease the calculation of reduction elements, on the grounds that it would be more efficient to reduce the observed occultations, and we announced in the N.A. that all observed occultations, sent to us with full details, would be reduced by us. I am not, by any means, satisfied with this solution: the amount of correspondence with observers turned out to be colossal, and the recording of all occultations became a serious problem. But it all turned out well when the new lunar ephemeris was introduced, and the reduction could be done by an electronic computer. There is much to be said for waiting a while to discuss observational data when the fundamental ephemeris data with which you compare them are subject to change. [That is not an excuse!]

The *Apparent Places of Fundamental Stars* (A.P.F.S.), which was first published for the year 1941, was continued. If I remember correctly, we continued to receive all the data that we expected from Germany and Spain via Sweden and later Switzerland; but the French were not able to fulfil their commitments, and we had to calculate the apparent places ourselves. I can recall using approximate methods that relied on the possibility of representing apparent places as:

- linear terms (precession, proper motion)
- + annual terms (aberration, nutation)
- + long-period terms (nutation).

By subtraction of the adopted values of the annual terms for one year (the last year available!) the remainder can be interpolated at a wide interval; the annual terms are then reinstated. The method, ad hoc and empirical though it is, worked quite well.

There was no time for any theoretical work, or for the development of the methods of presentation or tabulation other than for the *Air Almanac*. Much though the *Abridged Nautical Almanac* required revision, no change would have been accepted, even if it could have been agreed.

The exchanges with other ephemeris offices continued, but I am conscious that I did not react as actively as I might otherwise have done to the long series of proposals by W. J. Eckert when he took over the directorship of the U.S. Nautical Almanac Office in 1940; I knew that I could not devote the time and attention to them that they deserved, and that they must have low priority. There is, I regret to say, little more that can be recorded about the *normal* work of the Office, that is the routine preparation of the ephemerides required for astronomy and navigation, and the necessary theoretical and analytical work on which they are based.

Mathematical tables

Earlier we had produced at short notice a volume of mathematical tables for the War Office. It was a 5-figure table of natural trigonometric functions with argument at an interval of 10", thus requiring essentially no interpolation. There was not a suitable table available and so we computed one by standard methods of subtabulation on the National machine, and checked it by proof-reading against 7-figure or more tables. It was quicker to interpolate and print than to copy! As far as I can recollect, it was the first (and possibly still is the only) such table at a small interval. It was printed by H.M.S.O., but although the tables went to several editions, and became almost a best-seller, I am still doubtful about their typographical design. Because of the large-page size, I used rules to pick out the pivotal entries; it might have been better to have omitted them. My excuse is that there was no time, or opportunity, for experimentation.

The second table (which was probably much later than the Bomb Ballistic Tables that are discussed later in this chapter) was requested by the Ministry of Supply for the optical industry. It was a hotch-potch collection of 5-figure logarithmic tables, produced by reproduction from three tables: Chappell's tables of logarithms, von Rohr's tables of sines and tangents of the angles $0^\circ - 5^\circ$, and Bremiker's tables of sines, tangents, cotangents and cosines in the range $0^\circ - 45^\circ$. The last two were reproduced under licence from the Custodian of Enemy Property. In spite of the combination of different styles of typography and printing (plates and photolitho) it made a handy and useful set of tables.

War-time astronomical projects

The Office was, however, constantly involved in war-time projects that were directly associated with the astronomical work. Examples include: black-out times, night-illumination diagrams and the work associated with the development and introduction of the astrograph for air navigation.

Black-out times and repayment work

There was an intensification of the usual requests for data on risings and settings (black-out times were in great demand!) and on the state of the Moon (a matter of more than casual interest in a blacked-out world). We were responsible for the provision of black-out times for the newspapers as well as for the Services. I think that this started the 'repayment' work that became, after the war, such an administrative problem. During the war no copyright was charged, but after the war copyright was fixed by H.M. Stationery Office. This took a good portion of the time of one member of staff, especially as it was necessary to check everything completely. This was the case especially in legal cases where the time of black-out in the locality was important. After demands, usually by the defence, that a member of staff should testify in court, we sought and received a form of authority (from the Solicitor General's Office) saying that such attendance was unnecessary and that the relevant facts, as given by us, should be agreed between the two parties. On only one occasion was I personally involved. One evening at about 7 p.m., when I was the only one still in the office, I got a telephone call from the Prime Minister's Office requesting me to give moonrise and moonset, sunrise and sunset, and black-out times for a named location on a particular day. The caller hung on to the telephone while I worked out the very simple sums: but I had no one to check them! But we did not often get such demands.

Ryde night-illumination diagrams

The visit of J. W. Ryde to the Office led to the project of the Ryde Night Illumination Diagrams. Ryde had calculated the illumination from the Sun at various altitudes (mostly negative), the Moon, the planets and the stars. We had complete expressions for the effect of the phase of the Moon, the twilight from the Sun, and the effect of the atmosphere at low altitudes; and he finally had an expression for starlight. His plan, approved by the War Office, was to give an overall picture of night illumination in latitudes appropriate to the war. This consisted of a diagram presenting contours (in blue and yellow) of the total illumination on a horizontal surface due to the Sun, Moon and stars throughout each night. The diagrams were planned in conjunction with the Chart Branch, and were prepared under the direction of W. A. Scott. We did the calculations and the curves were drawn and printed by the Hydrographic Department; they were quite widely circulated to the operational planning units. As far as I can remember a series of charts was provided for every three months, and they were continued for some years after the war. I think that they served their purpose well. Certainly Scott did an excellent job in supervising the whole project (computations, production and distribution). Ryde was an employee of General Electric, and took us to see his laboratory, where he displayed with pride his first experiments on the discharge tube for which he was elected F.R.S..

The astrograph

We were working closely with the Air Ministry on all aspects of air navigation, then almost entirely 'astro'. During the spring of 1940, the Royal Aircraft Establishment (R.A.E.) at Farnborough came up with the idea of the 'astrograph', a photographic device which transmitted, via film, the curves of equal altitude for two stars (and Polaris) directly on to the chart. It was a natural development of the star curves of P. V. H. Weems and, in retrospect, a direct descendent of the I. N. G. Filon star-altitude curves. According to the plan, the curves — drawn against latitude and 'astrograph mean time' (A.M.T.), a kind of mean time of which the key value for each day was

tabulated in the *Air Almanac* — were filmed and reproduced in a series of spools, each of which covered, with an overlap, a range of latitude. In a series of meetings with the staff of R.A.E. (Pritchard and Lamplough), we agreed to calculate the curves, and to design the Astrograph Setting Tables to be given in the *Air Almanac*. This was a major operation in that it necessitated a close link with the Hydrographic Chart Branch, which both drew the charts and traced the diagrams from figures that we gave them. And it was continuous; not only were there numerous tables of latitude but, because of precession, new calculations were required every five years, though it could have been allowed for by shifting the fix. We slipped up once: for the first batch we selected two stars (the most that the astrograph could conveniently cope with) *independent* of declination; once we realized that a star within the zodiac could, on occasion, be interfered with by the Moon, we changed our selection procedure.

Unfortunately, there were problems with the actual projection equipment. It was necessary to project an enlarged image of the photographic film, which carried (in addition to the plotted curves of altitude) lines of latitude, spaced according to the adopted Mercator projection, coupled with a scale of astrograph mean time. A single source of light was provided (adjustable in regard to distance from the film); the choice of bulb was a motor-car headlamp. But, apart from vibration effects, the real snag lay in the distortion of the film lengthwise! Each film covered a number of hours greater than 24^h and had a length of 30 ft! There was an inevitable extension of the film in processing, and we had to allow for this in calculating the curves. As far as I can remember it was a few per cent, but an error in this only extended over the interval between observations.

The programme for the astrograph was one of the major events in the early days of the war and it continued long after the war. It was in the capable hands of W. A. Scott, who organised it from the start, including the discussions with the Hydrographic Department about the drawing of the curves and their reproduction. Speed was essential, and I am pretty sure that we [or perhaps I should say Scott] produced results more quickly than any other organisation would have done.

Coriolis effect

Somewhat later than the first mention of the astrograph, but before it was put in service, the question was raised of the Coriolis effect on the vertical indicated by the bubble. This led to an investigation of the correction to observed altitude for Coriolis acceleration. There was a great dispute as to whether the aircraft flies on a great circle or rhumb-line course, there being instantaneously no apparent difference but a considerable theoretical difference in the amount of the correction. It was queried by Cdr. Hutchings (U.S.A.F.), and referred to me by Air Ministry. At the time I had planned a visit to the R.A.F. station at Boscombe Down, to which H. H. Plaskett, who had been seconded through the Royal Society from his duties in command of an anti-aircraft unit, was attached; he was engaged upon the development of the sextant. We, and his colleague A. G. Weghorn, who was tragically killed later in an aircraft accident, discussed the brief report, and agreed with my draft report. They had their reservations, and so did I, about the course (or track) that an aircraft would fly, according to whether it was steered by hand or under automatic pilot. My paper on the subject (which was reproduced in the *Journal of the Institute of Navigation*, Vol. 1) was generally acknowledged, and formed the basis of the Coriolis corrections that have since been applied. Professor Cox, who was an instructor to the R.A.F. Specialist Navigator

Course, called my definition of the course on which the Coriolis acceleration was zero the 'Sadlerian'! [Cox was a Belgian refugee, then in England, who later became the Director of the Liège Observatory.] I eventually (much later in the war) asked the R.A.F. to take readings of the Coriolis effect when the aircraft was flying a great circle or a rhumb line. The R.A.F. actually ran flight trials at Malvern to test the theory, but owing to poor weather the results were inconclusive.

Twilight for air navigation

The R.A.F. also demanded that the *Air Almanac* gave times of twilight at different heights, without specifying what they meant; "illumination on a horizontal plane" equal to that at ground level at twilight, or some standard of air-to-air visibility that varied with bearing? On the first alternative there is (and for me there still is) almost no information as to the relationship between the amount of scattered light at a height of, say, 5000 ft and that at ground level. I was, and am still, not sufficient of a physicist to derive a theory. So I made an assumption concerning the contributions from different layers in the atmosphere, and calculated the depressions of the Sun at various heights that would correspond to the conditions at ground level for the various twilights. But we also stated that the times so derived were to be regarded as an approximation, and that users should preferably determine the depression that corresponded to the conditions they required by noting the time and calculating it; the latter method was adopted later. I was inundated, both during the war and after the war, by requests for the 'theory', when my 'assumptions' were revealed!

Non-astronomical work of the Office

The non-astronomical work done by the Office during the war must not be overlooked. The first large job was the continuation of the "Winds" project, which had been started in 1936 [see chapter 3]. In spite of the disinclination of those in charge to adopt the analytical model proposed by Davenport, I think that we did a good job — certainly as good as, and probably better than, Scientific Computing Service. But (as with many later jobs) I fear that most, if not all, of our work was wasted; it was impracticable to obtain the data required in the field and, even if obtained approximately, interpolation was difficult — especially with the non-systematic ranges of parameters insisted upon by Major Husskinson.

Bomb Ballistic Tables

The largest job the Office undertook was computing Bomb Ballistic Tables or B.B.T.. I was invited to attend a meeting of a committee of the Ordnance Board attended by representatives of the Air Ministry, R.A.F., Ordnance Board, Army and Navy at which the urgent need for bomb ballistic tables was stressed. Maccoll, knowing that we dealt with the analogous problem of anti-aircraft gun trajectories on the National machines, suggested that N.A.O. might be able to help; he coupled this with an indication that he could not do so because of his priority with A.A. trajectories. None of the large departments directly concerned (e.g., Ordnance Board, R.A.E. Farnborough, etc) was keen to undertake the work, so I agreed that N.A.O. should take on the calculation for the whole of the Bomb Ballistics Tables for the R.A.F.. The theory was crude, and depended merely on the speed and height of the aircraft and the characteristics of the bomb (namely the terminal velocity). Presumably other corrections (e.g. wind) were applied later. The calculation was straightforward, there being standard (experimental) tables of air resistance with density and terminal velocity.

Using a standard atmosphere, and these empirical tables of resistance functions, the work required the construction of triple-entry tables with parameters: height, speed, and terminal velocity of the bomb. The respondents were the quantities required by the bomb-aimer for his bombsight. I can recall severe difficulties in the integration, which we resolved by ad hoc means. One could use an integration for a large height for one with a smaller height, with several minor corrections; this saved a certain amount of work. For high terminal velocity, which meant that the bombs passed through the speed of sound (or close to it) we had to use a smaller interval in integration. But by interpolating between integrations for appropriate terminal velocities, at different heights above the ground, we were able to avoid the use of smaller intervals for the majority of cases. It involved the numerical integration of two standard simultaneous differential equations, but with the added difficulty of the singularity in the resistance function.

The job was urgent, the *real* accuracy was low (as compared to the nominal precision, since neither the resistance function nor the terminal velocity was accurately known and the speed and height of the aircraft were subject to considerable uncertainties) and mathematical elegance was unnecessary. We finished the job very quickly, at the expense of long hours of overtime. Later we added tables for the low terminal velocities that were easier to calculate, but probably of considerably less real accuracy. As far as I know, the N.A.O.-computed Bomb Ballistic Tables were in use by the R.A.F. at least until the last year of the war.

{Mrs Sadler states that she and the Daniels brothers did the work but Sadler gave the following account of it; presumably he was confusing this job with another one. Ed.}

I used two tricks to shorten the work: having integrated the equations for several sets of parameters accurately at a small interval (to overcome the singularities), we essentially integrated the differences between these and the intermediate sets to facilitate interpolations; by using height as independent variable, for the later part of the trajectories, the respondents for *differences* of height could be interpolated at a wide interval, thus almost eliminating one of the three parameters. Both were quite empirical and numerical; and, although of doubtful mathematical legitimacy, were, I am sure, adequate for their purpose. I successfully taught several of the girls to do the integrations — Miss Rodgers, Miss Hitches (who was good, and picked up the principles quickly) and others.

A job for Massey

Sometime before this, I had a contact with Professor (later Sir) Harrie Massey, who had been seconded from his place as University Professor at University College London, and who was in charge of the establishment (at Portsmouth?) concerned with magnetic mines and the protection against them.. They (his assistant was Buckingham, who later became Director of the University Computing Laboratory) said that they required assistance in an application of Green's theorem, which involved the triple integration of magnetic charges over the whole ship. They were using a Brunsviga machine, and found the integration most tiresome. A. E. Carter and I went to Portsmouth (whether in my own car, with petrol coupons from the Admiralty, I cannot remember) to see their work. I will not go into detail (I doubt whether I can remember them), but it was obvious to us that the Brunsviga was quite inefficient. Other than the comptometer (that did multiplication in the hands of a skilled operator even though it

was an adding machine), the only methods open to us were to do the multiplications mentally or to use Zimmermann's tables, which involved page turning. Two large sheets (a yard across) gave numerical data to 2 figures. It was relatively easy for Carter and me to make the multiplications mentally. It was hard work, but we finished it in an afternoon. I cannot remember whether we took subsequent cases home with us or whether Buckingham and his team did them. Harrie Massey never forgot this! Carter was magnificent, rapidly developing his 2 x 2 table (and rounding off to two figures) mentally; it is not difficult to get within 3 of the right figure — which in this case was quite good enough.

Security at Laggan and war service

We had of necessity to organise the security of Laggan against enemy action. The Admiral and most of his (retired) staff were too old, but George Harding had had Cadet Corps training at school, had acquired the appropriate certificates, and possessed a calm and efficient personality. He was therefore put in charge of all security arrangements, and took charge of Laggan's "defence force", which consisted of a Vice-Admiral, several senior R.N. Captains and Commanders, as well as lesser mortals. He organized drills, prepared petrol bombs for throwing in tanks if they approached up College Road, and planned exercises in the large garden. It was marvellous how the Admiral and his staff reacted to his instructions; I think that they thoroughly enjoyed it!

All the staff were initially in "reserved" occupations, and of the pre-war staff only George Harding joined the armed forces. He was commissioned in the Army, and had much overseas service. From the reports that filtered to me, it was clear that he was a great success. Throughout his service, he never once forgot to write a monthly letter to the staff reporting to us, as much as security allowed, the highlights of his job. The staff wrote joint letters to him, each penning a few lines. We were all greatly gratified by his regular letters and we welcomed him back after demobilisation.

At approximately the same time enquiries were made in relation to W. E. Candler and Miss F. McBain. Candler was only a temporary Assistant, and we felt that (as most of his work for the A.N.T.s was over) we could not object to his transfer. He was transferred to the R.A.F., Army and Navy testing range at Shoeburyness, which I am pretty certain (from his letters to me) he did not like. Later he was moved to Helensburgh (near Glasgow) where there was a naval armament centre. He then ceased to write to me; but I then had a confidential letter from the Director, to some extent criticising his work. I wrote back saying that we had no criticism of his work, but that maybe his extraordinary carelessness, particularly on clothes, might lead to this conclusion. I heard from him after the war that he had immediately resigned his appointment.

Miss McBain was asked by the R.A.F. if she would consider recruitment as a W.R.A.F. Officer to work on a secret project (so hush-hush she couldn't be told what it was, but it turned out to be Radar). She was interviewed by a board and medically examined. At first she was considered underweight but then accepted, but the Admiralty objected. The chairman of the board was Sir Robert Watson Watt, who later approached the Admiralty to ask if she could be released to work in his team. I said she could only be released if Candler could return to the Office, but this proved impossible.

Life in war-time

Life for the staff was pretty grim at times, in spite of the relative “comfort” of Laggan. Salaries, especially for the junior staff, were extremely low; and living in a strange (and somewhat expensive) town, away from home and family, perhaps with a not satisfactory billet, with a black-out, with food rationing and shortages, with constant air-raid warnings ..., was not pleasant. The Office hours were constantly being increased from the pre-war 36 (excluding lunch hours) to, at some times, 51 a week; and, in the NAO, there were often (especially later in the war) rush jobs that required lengthy overtime. It was perhaps these extreme conditions that led to a remarkable comradeship among staff, particularly the young girls uprooted from their homes. Much was due to the magnificent qualities of Marion Rodgers, whose steadfastness, integrity and real friendliness made her their life-long friend.

We were all engaged in some form of civil defence, and took periodical courses on appropriate subjects: Local Defence Volunteers (later called Home Guards), Wardens, First Aiders, Fire-watchers, ...; we did everything. We had some appalling weather, with severe winters. Later we had Double Summer Time, which meant that for many the journeys to and from the Office were both made in darkness in winter. At Laggan we spent a considerable total time in the cellars during the frequent daylight air-raid warnings, but we later had roof-watchers to give warning of imminent attack.

I saw some of the British troops who had been evacuated from Dunkirk on one of my trips to the R.A.E. at Farnborough; the line used (Reading - Farnborough - Guildford - Dorking - Ashford - and the Channel ports) was one of the great military lines, and there was a terrible lot of the traffic on that day!

A storm at Bath

We had one freak frost while we were at Laggan (at the beginning of 1940) when the temperature fell suddenly after heavy rain, which froze on everything. There was half-inch-thick ice on leaves, for example, bringing down hundreds of branches of trees; this added to icy roads to form a night of almost terror. The destruction of trees was enormous; many members of the staff who were out in Bath that evening came home with trees crashing down under the weight of ice. It was, however, in the morning a sight of rare beauty; the extensive garden at Laggan was a glorious sight.

Bombing of Hammond’s printing works

At the beginning of November 1940, we heard that Hammond’s printing works had been bombed, with the loss of a complete set of type and stereo plates for the N.A.. We fortunately had uncorrected proof copies, and we felt competent enough to correct them by pasting over the erroneous figures. This was a great burden on the staff, but the corrections were pin-pointed by Richards and Scott, and were carried out very carefully by members of the staff. The N.A. was then printed by photolithography. This was, I think, the only occasion on which enemy action directly interfered with the work of the Office. Subsequently, we took precautions to ensure that a set of proofs of each publication, at each stage, was deposited in a safe place.

CHAPTER 8

From the move to Ensleigh until the end of the war

The move to Ensleigh

Towards the end of a very grim 1941 I can remember the shock and dismay that went through us all at the Admiralty when we heard the news of the sinking of the aircraft-carrier *Ark Royal* and less than a month later of the sinking of two battle-cruisers, *Repulse* and *The Prince of Wales*, by Japanese air attack. Between these events we moved (17 November) to new office accommodation in Block E, Ensleigh Hutments.

The move to Ensleigh was made without trouble. We took the opportunity of ending the loan of our desks to the Hydrographic Department, and we got a whole spur of Block E to ourselves. We had reasonable space, but we later expanded to use other rooms, and, eventually, we moved to another spur where the Office stayed until 1949.

The Hutments were built by Laing and for their purpose they were superb. We do not have any record of complaints about them. The accommodation was excellent, but the situation, at the top of Lansdown Hill overlooking Bath, was awkward of access and very exposed to the north-easterly winter winds. On a clear misty morning, however, we were above the mist, and we had magnificent views of Bath. Access involved a walk of a few hundred yards from the special bus stop, which had a shelter. On some days I had the greatest difficulty in fighting my way against the blizzard; how the girls managed I do not know. I can remember the walk to Block E on one cold frosty morning against a northeast wind; I was literally exhausted when I reached my office. At this time there was no petrol; I could not get an allowance since I was within a bus ride (or two bus rides) from the office. There was a central canteen, but it was almost as far away as the bus stop. Rear Admiral Jackson, Assistant Hydrographer, used to order an official car from the centre of Bath to take him to the canteen from Block E in wet weather. He was duly reprimanded! At weekends there was little heating (only background) and electric fires were absolutely forbidden. I worked most Sundays, often in an overcoat.

The bombing of Bath in 1942

On 25/26 April 1942 Bath was bombed during the so-called Baedeker raids and the town was badly damaged, with several hundred killed. The Admiralty (not I think a special target) was not seriously affected, and not many staff were killed or hurt; but most staff had minor or major difficulties. But it was amazing how soon people returned to normal working, even when without services of water, gas and electricity. The N.A.O. staff escaped relatively well, though all had their experiences.

Perhaps I was the most affected since Villa Julia was practically destroyed when a bomb made a direct hit on the next (unoccupied) house. I had that day (the second of the two days of raid) taken Mrs Thornton to her brother's home in the country, and the two elderly servants and myself were in the cellar when the bomb brought part of the house down on top of us. But the roof held, and we were not injured. If we had stayed in our beds, we would have been killed — at least I would have been, as the bed was smashed by a roof-beam. The garage was wrecked, but my car was not.

I spent much time getting the two servants out of Bath to friends in the country, after digging out my car from the practically collapsed garage, and taking charge of some of the more valuable of Mrs Thornton's possessions (including her collection of Russian icons). I spent the following couple of nights sleeping on the floor of my office at Ensleigh, but then Farquharson (Superintendent of Tides) invited me to live with him and his family until I could find somewhere permanent. I recall, with some amusement, that on the morning following my move to the Farquharson's I produced, at breakfast, the 1-lb jar of marmalade I had been hoarding. There was a howl of delight from the children: "marmalade!" and it disappeared.

A billet was found for me in a gardener's cottage, attached to a large house in Bath, and I stayed there until Villa Julia was repaired and Mrs Thornton came back. The gardener's cottage was minimal, yet the family was marvellous to me. I had never eaten so many vegetables before in my life! I cannot remember their names now, though I did keep in touch with them while we were in Bath.

Grimwood moved his family out of Bath for safety, and then had considerable difficulty in getting in to Bath. I could not get *any* allowance of petrol, but he could, and my car (a 1932 Austin 7) was standing unused at Ensleigh. Grimwood knocked my price down to a very low level and then promised to pay in instalments! A few days later he told me he had sold the car-clock for a good proportion of the price, and a few years later he gleefully told me that he had sold the car for twice what he had paid me! I was amused rather than annoyed!

On the whole the staff showed commendable initiative in getting their own accommodation (house, flats or rooms) independent of the official billeting scheme. But I was quite content to have a place to sleep in; and I spent much time in the office, and I was away in London or elsewhere a lot.

Staff changes

In late 1936 Miss Roberts, who had been Comrie's secretary, resigned to take up a much better position. We had by this time a Clerical Officer to act as 'secretary', but we still needed a shorthand typist. My C.O. explained that we should see what the Superintendent of Typists in the Admiralty could supply. She, in due course, suggested Miss V. M. Hooper, who was having some sort of difficulty in the typing pool, but was a very good shorthand typist. She reported for duty and gave me quite good service. But her mental troubles were reserved for other members of the staff. She moved with us to Devonport House and to Bath. But after the move to Ensleigh Hutments, she became more insecure mentally, and I reported to Superintendent of Typists (in Bath) that we could no longer put up with her, even though her work was satisfactory. After a full investigation by welfare workers, etc., Miss Hooper was transferred to a typing pool. She was replaced (after a short stay by Miss Marjorie Height) by Miss Joan E. Perry, who stayed with the N.A.O. as shorthand-typist, and later as secretary, for many years. (Until 1965 when she was promoted to take charge of the R.G.O. Library at Herstmonceux.) Throughout these 25 years (about) she was the model of efficiency, with her tidy mind and exceptionally neat handwriting; she kept the Office records and files in immaculate condition, and her memory was infallible. In particular, she organised, and was instrumental in recording and classifying the 100 or so 'jobs' (projects) under the Admiralty Computing Service. There were often 10 or more jobs in progress simultaneously with varying priorities and she kept the current record of progress up-to-date. She became the successor to my C.O..

There was an enormous turn-over of junior temporary staff taken on to cope with the increased work-load, particularly after the start of the Admiralty Computing Service (A.C.S.) (see chapter 9). (They are, I hope, all recorded in our files.) They were almost all Temporary Clerical Assistants Grade III, sent to us by C.E. Branch without apparent consideration of their qualifications and often (to my expressed annoyance!) without prior information. I can remember the exasperation when I got approval for an additional T.C.A.; C.E. Branch (in Bath) then sent to me, without notification, a T.C.A. who presented herself to me with statement that she was to work for us. They varied from Mrs E. N. Fox (whose husband, a professor of engineering, was a contemporary of mine in Cambridge), who was a graduate and a housewife who had to pay her char more than she earned, to Olga Kevelos, who could not do arithmetic and terrified people by stalking around with a large knife in her belt. (But she seemed an interesting woman, and after the war, became a motor-cycle racing champion!) Most were reasonably competent and one or two were exceptionally good, but a few were hopeless — Olga Kevelos could not add + and – signs together. Still we survived! — with the major help of Miss Rodgers who was landed with the job of training these girls. Those who stayed with us after the war, and some of whom moved to Herstmonceux, were established and were extremely good.

On the N.A.O. staff we had a Cambridge graduate, Miss A. M. (Peggy) Hathaway, who surprised me (and, I think, most of the staff) by marrying a young T.C.A. III, Len Macey, without any particular qualifications. He was, I seem to recall, the son of a printer. He was about 18 and so was soon called up; he had a most successful career in the Navy. He took a mathematical degree after demobilisation, lectured for some time at Bristol University, and then went into the Colonial Service. They served, adventurously, in Sarawak, where he was a surveyor, and the last I heard of them they were in Cyprus.

In 1943 or 1944, Doreen Ifield was engaged to an R.A.F. navigator, Paddy Doyle, and was to be married shortly after his tour of duty; he was the navigator of a Mosquito which failed to return. Doreen (whom everyone admired and loved) came through the ordeal exceptionally well. She is now married to Ben Barrett and is in touch with Flora and myself. She was the first person (aged 16) that I took on in 1936!

[But I must not attempt to recall all the many war-time staff; those who stayed on after the war will be referred to later.]

Duties of the staff

With the additional work on the *Air Almanac* and the *Astronomical Navigation Tables* (A.N.T.s), the considerable work on astronavigation, including our involvement with the specialist navigation course, and the preparation and publication of the *Apparent Places of Fundamental Stars* (A.P.F.S.), the Office staff had their full share of work. With my involvement with the A.C.S. (see chapter 9) — not to mention my duties and responsibilities as Secretary of the R.A.S. — they had a heavy task. As Assistants, Miss McBain and Richards played a dominant role. Miss McBain dealt with the exasperating ‘business’ of receiving (and reducing) the occultations that had been received and Richards coped with the preparation of the copy for A.P.F.S.. This was in addition to their normal functions for the N.A. & A.N.A.. The bulk of the additional work was done by Scott, who was at that time a Junior Assistant (Higher Grade), and Miss Rodgers, who was a Junior Assistant. The Daniels, both Junior Assistants (Higher Grade), were immaculate in their proof-reading and largely took the responsibility for

the accuracy of the printing process. At one time, the hours of duty rose (I think) to 51.5 hours a week, but all the staff accepted this as our war effort; but it was too long for the computing that we had to do. At one time every member of the staff needed to read 20 pages of proofs a week. The standard procedures were:

reading the first proofs (labelled P1-6)

checking the revised proofs (labelled R1-6)

and reading the stereo proofs or printed pages (labelled S1-6).

The proofs were read in duplicate, one by a 'senior' and one by a 'junior'. Miss McBain arranged the schedule and Miss Perry checked that the proofs were returned on time or nearly on time!

Fortunately the *Abridged Nautical Almanac* (A.N.A.) required no revision. In spite of the emphasis on rapid reduction methods, the Admiralty was stubborn in its refusal to change either the A.N.A. or cosine-haversine method of reduction. Consequently, we did nothing but produce the A.N.A., as a routine task, in its original form until after the war.

Calculating and punched-card machines

The stock of desk calculating machines gradually increased, mainly by the addition of Brunsviga 20s; but, I think, we also acquired some electrical machines such as the Marchant and Friden. A constant source of concern was the state of the old 45-column round-hole Hollerith cards, which had been much used for the lunar ephemeris and which had been man-handled at least twice during the moves. There was a fairly urgent need for the final stage of combining the sums of the many terms to give the final longitude, while much cyclic summation remained to complete the ephemeris to 2000. We used a tabulator in the accounts division, double-plugging the 80-column reading brushes to read the 45-column cards; very messy, but it served its purpose. Later, we were able to use machines at B.T.M.C.'s service station at Cirencester with rather more success, since we had (I think) duplicated many of the cards by converting them from 45 to 80 columns. [It is my recollection that the reading brushes of the reproducer were easier to adjust than those of the tabulator, and the results of mis-reading were easier to correct.]

There was a project that could not be delayed. Way back before I joined the Office, Comrie had punched Hollerith cards (45-hole cards) for the summation of the half-daily values of the sums (Σ_1 , Σ_2 , etc) of the periodic terms entering into the Moon's longitude and latitude. It was planned to complete the work until the year 2000, but it was never finished because the rental period of the machines ran out. In my early days in the Office, we did a little on the machines of H.M. Stationery Office — usually a half-day when they were free. The method was then new and made possible by Brown's ingenious design of his tables. I applied the method to the periodic terms in Newcomb's *Tables of the Sun*, and to some others, notably, the nutation in longitude and latitude. The obstacle to completion was the virtual disappearance of 45-column cards and their replacement by 80-column cards. There was a scheme for the transcription of 45 columns to 80 columns on a reproducer, and we got the reproduction done on a reproducer in C.E. Branch on Sundays. But they could not provide the tabulator time to carry out the additions. We used contract work (by B.T.M.C. service machines at Cirencester) to complete the work to 2000. Richards was in charge of the work, but the actual operation was done by other members of the staff. Our relations with B.T.M.C.

were quite good and I think that we got the use of the machines (with our own staff) at a nominal cost.

Specialist course for air navigation

Perhaps the most memorable contribution to the R.A.F. arose through the suggestion of Squadron Leader Kelly Barnes to set up the Specialist Navigation Course (Spec.N.). He was then the editor of the (classified) first edition of *The Manual of Air Navigation* for the R.A.F.; this was known as the 'Alice edition' because each chapter was headed by an appropriate quotation from *Alice in Wonderland*. {See preface of *Man is not Lost* re Barnes and the device on the cover of *The Air Almanac*. See also below. Ed.} He sought my opinion on 'astro' and I agreed that we could contribute. Our contribution was, finally, a two-week course at the N.A.O., during which we could cover the theory of astro in depth. We worked closely with the headquarters of the course at Cranage, where I gave occasional lectures; I was picked up by air from a neighbouring airfield. We also had many contacts with the Pathfinder course (mainly practical and operational) under D. C. T. Bennett, with E. W. Anderson and Francis Chichester. Thus all the outstanding navigators of the R.A.F., and some from R.C.A.F. and R.A.A.F., passed through our hands. The majority, especially in the early courses, were killed acting as pathfinders over Germany. The standard was incredibly high, as regards intelligence, ability and personality.

The first course was held at Cranage, with Wing Commander J. V. Branch and Squadron Leader A. G. Hagger in charge. Branch was the secretary of the first committee which, after the war, was responsible for forming the Institute of Navigation. Hagger, a master at Wellington College, returned there after the war.

We had some magnificent men, but many of them died in operations; some survived to become the leading navigators of the R.A.F. (Ken Maclure, W. H. McKinley, A. H. Jessell, ...), and some of them are still friends of mine. They came from Australia and Canada as well as the United Kingdom. Maclure, Greenaway and Knight, the Canadians, all reached high ranking in the R.C.A.F.. Maclure became a diplomat, and (second) High Commissioner in London. Edwards, founder of the Australian Institute of Navigation, was an enthusiastic astronomer, whom we taught (after the war) to compute cometary orbits. There were many others from the United Kingdom who became Air Commodores, or better, or who made high positions in industry. One was J. B. Parker, who joined the staff for a short period and later became a P.S.O. at Aldermaston, where he was engaged on the Monte Carlo process for designing atomic bombs. Among those whom I can remember was Doug Fraser, who became a high-up in English Electric.

Most of the staff helped with the courses by giving demonstrations or lectures. On one occasion I persuaded the Astronomer Royal to talk to them [he talked on 'time' for more than 2 hours], and on another I got Professor W. M. Smart (from Glasgow) to give them the seamen's point of view. On that occasion, I was due to meet him on the Sunday, but I could not possibly meet the train as I could not walk since I had torn the ligaments of my knee in playing hockey for Bath on a Saturday. No one else knew him, so I sent Miss McBain with a partial description of him; she said it was inadequate, but she managed! (Incidentally, my knee troubled me for a long time, but, fortunately, Admiral Nares had a lift by an Admiralty car (driven by a WREN) to take him to work, and he came to pick me up in the morning.) Apart from the encouragement that such

visits gave to the staff (and I think they all enjoyed them), the contacts thus made were, and probably still are, even if less directly, extremely valuable to the Office.

Probably what we tried to do was not, in practice, a great success; but we did try to instil in them the basic principles without too much detail. Perhaps they obtained a better understanding of the precision of astro, which was of low standard, because of the limitation of the bubble sextant and of the acceleration of the aircraft. And they thoroughly enjoyed themselves! — thanks, maybe, to the girls in the Office. We drew up a series of lectures and demonstrations, including, with the cooperation of the Hydrographer, a visit to Chart Branch. Scott was the chief instructor, telling them in detail how, and why, the *Air Almanac* was constructed; I concentrated more on principles. But all the staff helped in some way or another.

This gave a wonderful sense of belonging to the war effort, and the knowledge that we had a place in the work of the R.A.F.. We had several trips by air to Cranage and later to Shawbury, mainly by myself alone, but Scott came with me once and Miss McBain came with me on 2 or 3 occasions and for a celebration after the war. The course had a plane at its disposal, and several pilots made the trip to Corsham (or Bristol) airports to pick us up. On one occasion Sq. Ldr. Hagger (who was stated by the students to be the worst pilot of them all, but who was, nevertheless, a much liked and charming individual) flew down to Bristol (since Corsham was out of action through fog) and had great difficulty in landing; he made several overshoots and landed about 50 ft beyond the end of the runway in thick mud! Of the keen pilots who flew us, one of the best was Sq. Ldr. McKinley, who flew Miss McBain and me in an enormous bomber (a Stirling), with us in parachute harness, to Shawbury and then brought us back in a trainer. All the staff at Shawbury were horrified that we had put on parachute harness — with the quip that McKinley could put down the trainer on a handkerchief. All was not a joy ride as on each occasion (except the last after the war) there was a conference or lectures at which we made a contribution. [McKinley became an Air Marshall; when he retired, he took a course in bricklaying and built himself a lovely house in the West Country.]

Relations with the Royal Air Force

We met E. W. Anderson and Francis Chichester through the course, though they were not on the course. Both were attached to the Pathfinder course (run by Don Bennett); and both were brilliant, but unconventional navigators. [We were asked down to the Pathfinder course to assess the worth of Anderson's proposal for a rapid form of astro.] They were good friends of ours. Anderson was appointed to Shawbury after the war, and was instrumental in inviting us down. Incidentally, the polar flight of 1945 (Aries) to reach the north magnetic pole was piloted by McKinley, with navigators Anderson and Maclure; we advised them on certain aspects of navigation, including the limited form of astro close to the pole.

Incidentally, we must not forget the debt of the R.A.F. to the liaison between the Air Ministry and ourselves. Starting before the war, in 1936, the Air Ministry had, with one or two exceptions, a series of exceptional men as Head of O.R.3 (Operational Requirements Division 3). (Sq. Ldr. Vielle was the one I liked least, largely because of his casual acceptance of anything I proposed: I was used to considered views. But he became a Group Captain and he claimed to be an industrialist with a large house in Switzerland.) They usually had 6 months or a year in O.R.3 before being promoted. I cannot remember them all. Mackworth became an Air Commodore. Wing Commander

(Kelly) Barnes died young; he was famous for landing in Reykjavik in a flying-boat on an operation and being interned; he was treated kindly and given complete freedom provided he did not escape. He accordingly flew to England, married and bought a motor-cycle, and then flew back! Chilton (later Air Chief Marshall Sir Edward) was extremely efficient in anything he did; we used to meet at the A.G.M. of the R.I.N.. Finally, after the war, there was Banks, who became an Air Commodore.

[We had some connection with the Air Force before the war when we advised the pilot of Scylla and Charybdis — the combined aircraft with one plane superimposed on top of the other — on the astro required in a trip across the Atlantic. W. H. Cockson was the pilot, and we drew up a series of sights for him to take. He became a Group Captain during the war; he was the navigator who, shortly after D-Day, dropped his cargo of bombs on the British! Although cleared, because of signals failure, he was clearly very distressed afterwards.]

At this period we were inundated by requests from O.R.3 of the Air Ministry for opinions on devices and methods for simplifying astro; there must be 20 or 30 bright, but impracticable or unsound, such proposals hidden in the Office files.

Computations for the DECCA Navigation system

Mainly through the reputation the A.C.S. had received throughout the Admiralty, we were consulted by a Lieut. Cdr. R. B. Michel and I attended a meeting at the Admiralty Signals Establishment to discuss the top secret matter of the calculation of data for the hyperbolic lattices used in the Decca short-range radio navigational system. I said that we could undertake the work, provided the Hydrographer (who was also represented at the meeting) could draw the charts. Decca was first used (apart from trials) on the D-Day invasion across the English Channel in 1944. We did not do (presumably because of security) these calculations, but the range of coverage was small for the short crossing, and the relatively simple calculations were carried out by the Decca company.

Our first request was for a chart that spanned the Scheldt estuary, and was to help the R.N. to navigate the river. The positions of the stations had to be surveyed as the advance continued; and once these were surveyed the calculations could start. The data were required IMMEDIATELY. Preliminary calculations could be prepared, but the actual intersections (involving an inverse interpolation in values on a meridian or parallel of latitude) had to be made. With the assistance of the A.C.S. personnel, the Office managed to complete the work in 48 hours of receiving the coordinates, taking an estimated total time of 800 hours! We also calculated the three (Decca) coordinates on the mine-free track along the river; whether this was used I do not know, but the technique became standard later. I later amused myself by calculating the Decca coordinates of equidistant points along the main channel of the Scheldt and I understand this proved useful.

Later we were asked to provide the whole coverage for the chain set up in Belgium to provide for supplies to Antwerp and the advancing army. The requirement was for complete charts *within a few days* of the Army surveyors fixing the coordinates of the stations. We had approximate positions, which enabled us to plan the calculations (i.e., whether to plot along parallels or meridians) before they were occupied; and Superintendent of Charts (in the same block at Ensligh) was also fully prepared. My estimate was that the number of man-hours involved in the calculations was about 800

(the plotting involved considerably less) and we finished our part within 3 days of getting the signal giving the coordinates.

As soon as the war was over, the Decca Navigator Company was formed. They, with agreement from the Admiralty (I do not know what financial arrangements were made!), entrusted the calculations to the N.A.O. and the plotting of the special over-printed charts that were essential to its use to Chart Branch. N.A.O. did all the calculations for many years, working closely with Chart Branch and Decca. The basic 'theory' was simple, though allowances had to be made for different speeds of propagation as well as for the figure of the Earth. It was an 'unsatisfactory' job in that it involved switching (and overlapping) from calculations along meridians or parallels; direct calculation of lane numbers had to be converted, by systematic inverse interpolation, to coordinates corresponding to integral lane numbers. There were iterative methods of going direct from lane intersections to geographical coordinates (or grid coordinates), but they were essentially computational tricks that could not be used, with desk calculating machines, for systematic work. Much experimentation confirmed that the best way was sheer slog, using the National machines at every possible stage. As with many similar jobs, Scott took over the project from start to finish, dealing with both the administration (contacts with Chart Branch and Decca), planning the chain (an important preliminary), the actual computations and the preparation of copy. He did an excellent job with considerable assistance from Carter, who was in charge of the National machines. This went on for many years until a computer program was developed. J. B. Parker and I wrote up a comprehensive treatment of the problem, with emphasis on the direct evaluation of longitude and latitude from the Decca coordinates. This was published (in part) in the *International Hydrographic Journal*.

A personal note

Sometime during the war (June 1943) I had what threatened to be a breakdown due to pressure of work and exceptionally long hours. My doctor told me that I must have a holiday, and I appealed to my friend Professor Harold Davenport. He came over to Bath (from Bangor), looked up for me possible hotels and we fixed up a week's holiday at Church Stretton. We had a long series of walks on the Long Mynd, recalling the author of *The Shropshire Lad*, A. E. Houseman. This, or Harold's discussion of his lattice problem (in number theory), cured me; and I was able to return to Bath. My doctor subsequently prescribed for me phenobarbitone, and I think this saved me from a subsequent depression. Of course, I would not put the blame on my recent decision to stop smoking (on my doctor's advice!). I was not the only one on phenobarbitone; H. R. Hulme (my co-secretary of the R.A.S.) was on the same drug.

CHAPTER 9

The Admiralty Computing Service

Preamble

A major disruption in the normal affairs of the Office (which I shamefully neglected) was the introduction of the Admiralty Computing Service (A.C.S.). Strictly speaking it has nothing to do with the history of the N.A.O., since it was not astronomical or nautical. It was, however, the largest contribution made by the Office to the war-effort. I do not have the A.C.S. publications readily available, so that the following account is in general terms only; dates and other facts can readily be corrected by reference to the A.C.S. files, or to the A.C.S. reports. This is no place to discuss the A.C.S. or its work in detail; an account of A.C.S., its history and achievements have been described in two articles by myself and John Todd in *Nature* in 1946 and in *Mathematical Tables and other Aids to Computation* in 1947. The A.C.S. has been described, in even greater detail, in a doctoral thesis by Mary Croarken, who consulted a great number of the people concerned, including myself. When I wrote the following account, I had forgotten that I had made to the A.R. a submission that may have started the A.C.S..

{Sadler sent Mary Croarken a note about A.C.S. in 1984 and he wrote a complementary note at about the same time; copies of these two notes are given later in Appendix 3. She has given some of this material in her book on *Early Scientific Computing in Britain*. Ed.}

The beginning of the Admiralty Computing Service

The Deputy Director of the Admiralty's Department of Science and Research (D.S.R.), under Sir Charles Wright, was Professor J. A. Carroll (later Sir John Carroll, Deputy Controller to the Royal Navy); he was an astronomer on secondment from the professorship of Natural Philosophy in Aberdeen and had been Miss McBain's tutor. On his staff was John Todd, a Cambridge mathematician whom I knew; he suggested to Carroll that mathematical and computing work in the various Admiralty research stations should be centralised, with the N.A.O. being asked to undertake the numerical work. Carroll, as an astronomer, knew the potentialities of the N.A.O. and approached me to see whether I would take on the computational work arising from the research being done in Admiralty establishments and, incidentally, advise them on their own computing facilities. I consulted the Astronomer Royal; he agreed, and an arrangement was reached very quickly.

Todd had married an Austrian refugee, Olga Taussky, whom I had met when she arrived in London from Vienna in c. 1933. Olga was a mathematics teacher at Westfield College, a part of the University of London, and was later seconded to the National Physical Laboratory. The Todds both became professors at the California Institute of Technology, and leading personalities in the field of numerical analysis. Of the two, Olga was the more accomplished, and (although much crippled) was in great demand at international conferences.

Todd acted as the ‘front man’, who made the contacts with the various stations and departments, and who organised such mathematical investigations as seemed desirable. His wife, Olga Taussky-Todd, was the inspiration behind much of the abstruse mathematics and was responsible for recruiting many leading mathematicians to write *practical* handbooks on techniques (such as Fourier transforms) for the use of departments. [Erdélyi (Professor of Mathematics at Edinburgh and a successor to Aitken) was one of those who made outstanding contributions; he died in 1977.] The arrangement worked well. Todd, with his Irish charm of manner, was excellent in his personal relations with the heads of establishments (Departments or Research Stations), who were at first reluctant to delegate work.

The first job — on the Taylor bubble

Early in 1943, before I had recruited staff, a preliminary notice was sent out by D.S.R. to all departments saying that an Admiralty Computing Service was being set up and the N.A.O. would be the computing agency and would be responsible for any computations that were required. That Saturday morning, I received a telephone call from the Under-Water Research Station at Fairlie (on the Clyde coast). The caller asked (more or less as a joke, as I later discovered) whether we could predict the pressure wave arising from the explosion of a depth-charge, of given power, at a given depth, so that they could assess its likely effect on an old destroyer immediately above it. A test was to be done on the Monday — could I help? As he said, no great harm would be done if the destroyer was sunk, but they would like to know beforehand if possible. We discussed details: they really wanted solutions to two non-linear differential equations for ‘the Taylor bubble’, and they could then work out the effect of the pressure wave on the ship.

Fortunately, the theory of the Taylor bubble had recently been given by (Sir) G. I. Taylor and I had a copy of the (classified) paper, but without any clue as to how to solve it. [I can remember saying in Cambridge that the question asked in the *Tripes* was sufficiently answered by a series of differential equations; no one was really ready to solve them, except in the simplest cases.] The two non-linear simultaneous differential equations that describe the rise of the oscillating gas bubble are, however, easy to integrate provided the correct numerical technique is used — and we had plenty of experience. As far as I can recall, I took the necessary data down on the telephone, and then I sketched out a method using an estimated value for the next step with later corrections. I spent the weekend integrating the equations. The main snag was the small interval ($\approx 0^s.0001$) at the start of the explosion; later this stage was dealt with theoretically. It worked very well and I finished the calculation on Sunday, ready for telephoning on Monday. I had worked in artificial units of pressure and I had no idea of the effect of the pressure on the destroyer. But, when I telephoned my results on the Monday, I was told that they indicated that the destroyer would not be sunk; they exploded the device on the Tuesday, and it did not sink. A.C.S. did several later calculations involving the Taylor-bubble theory, so that our procedure became standardised. Vi Hitches (one of the ANTs — see chapter 4) did one of the integrations, I seem to remember. After the transfer of the A.C.S. staff to N.P.L. Mathematics Division, the same procedure was used for the biggest Taylor bubble on record — the U.K. under-water atomic-test explosion!

Recruitment of staff

Generally, however, jobs came in slowly at first and we were able to recruit staff to keep pace with the gradually increasing demand. We clearly required C.E. Branch approval for the additional staff that we needed, but D.S.R. had sufficient standing in the Admiralty (more so than the Hydrographer) to ensure that approval to take on staff as needed was given without delay. Although there was some difficulty in getting appropriate gradings for the new staff, we were very fortunate in recruiting a most efficient team. I suppose that, at its peak, we must have had 15-20 A.C.S. staff. Except for special jobs, or tasks, the senior members of the N.A.O. staff (Miss McBain, Richards, Scott, the Daniels) took little part in the A.C.S. work, but the junior staffs were shared, when necessary; Miss Perry kept the general records of the work.

For A.C.S. we eventually got approval to recruit at a higher level than Temporary Clerical Assistant, though I was never able to compete in the 'market' with larger departments. I attribute this largely to the lack of enterprise of Walter (the Civil Chief Assistant (C.C.A.) to Hydrographer), through whom my representations to C.E. Branch had to go. Fortunately the Admiralty later recruited some university staff to administrative positions and I was able to talk to them. J. Wishart (a member of the British Association Mathematical Tables Committee) became an Assistant Secretary, and so did the historian, Alec Clifton-Taylor. However, either through D.S.R. Admiralty or direct, we managed to recruit a pretty good team: E. T. Goodwin (later Superintendent of Mathematics Division, N.P.L.), L. Fox (*the* outstanding specialist on relaxation methods and later Professor of Numerical Analysis and Director of the Computing Laboratory in Oxford), F. W. J. Olver (later professor of numerical analysis in the U.S.A.), H. H. Robertson (a Scot who played full-back for Bath and the county, and later did extremely well in industry as a mathematician), R. G. Taylor (who got good university posts in London after the war) and E. M. Wilson (who went to the Admiralty Research Laboratory). There were others, including W. J. Ferguson, a taciturn, almost speechless, short and stocky Welshman, who was incredibly incompetent; his tenuous claim to fame was his ability to run fast (in spite of his appearance) and he made full use of this by entering, and winning, professional races!

We also recruited a number of intermediate staff, and young graduates (women), whose grades I cannot now remember. Among the A.C.S. staff were: P. H. Haines, an actuary with some small physical disability and a diffident manner; he was, however, extremely conscientious, and later became scientific librarian to Mathematics Division of N.P.L.; Kathleen Blunt, from Westfield College, was efficient in everything that she did; Mrs Ledsham (she married a scientist at R.A.R.D.E. at Fort Halstead) was really an exceptionally competent person, with a personality to match.

Kathleen Blunt, who was just about to take her degree, was recommended to me by Olga Taussky-Todd, then at Westfield College, which had been evacuated to Cambridge. Miss McBain went to Cambridge to interview her and Joan Slater, and had the doubtful pleasure of meeting the Principal of the College, Mary (later Lady) Stocks; she was very doubtful about the job that Miss McBain was offering and quizzed her about the details, but Miss McBain did not know anything beyond what I knew, which was *nothing*. K.B. (as she is still known!) took the post and became one of the great successes. She tackled every job with enthusiasm; and her understanding of mathematics, computation and presentation (and her clarity) is permanently preserved in the many (30 to 40) A.C.S. Reports which she prepared in manuscript for photo-

reproduction; some of the tabular material was in the form of National print-out. Many of them were confidential, involved mathematics and did not require more than a dozen copies. Her handwriting was marvellous; she wrote the report from a rough draft and it was then duplicated. This was then, and perhaps even today, the quickest method of getting out a report. We had no large organisation for the production of reports (with which other departments engulfed us), and we required small circulations with minimum delay. I am certain that the A.C.S. Reports, involving elaborate mathematical expressions, written, with the text, in K.B.'s neat and legible manuscript, compare favourably with the most elaborately-produced typescript reports (and with printed ones!). Yet these were produced in the shortest possible time, with the minimum drafting by the 'author'; there was no need to produce a detailed draft.

Range and background of A.C.S. jobs

The jobs, and the nature of the computations arising therefrom, were carried out over almost the whole range:

from 'open' tables to 'top-secret' investigations in which every computing sheet was so classified;

from the 'trivial' tables to quite advanced mathematical techniques;

from the trivial tables to difficult numerical techniques, many of them new, such as integral equations;

from single-figure answers, through graphs and nomograms to elaborate tabulations; and

from long-term background jobs to top-priority operational requirements.

In its short life A.C.S. produced well over 100 reports, many of them were substantial and some were of permanent value. On the computational side we probably used all of the techniques of practical numerical analysis then available, though we did not have much use for matrix inversion and characteristic roots, which entered so much into aerodynamics and aircraft design. We did, however, do some work in this field.

One feature of the A.C.S. work must be mentioned: namely, the relationship between client and computer. Far too often the clients presented problems that were poorly formulated, rarely (possibly for security reasons) gave their applications, and demanded obviously unrealistic accuracies; moreover, the clients were too remote. It was to me galling in the extreme not to get even an acknowledgment of the receipt of calculations that had taken hundreds of hours of work. Of course, it was war-time, the individual concerned had lost interest in the problem, or it been solved in another way, or he had been transferred, but I regarded it as the responsibility of the Head of the Establishment to authorise, and accept responsibility for, such work. I said so and there was some little improvement, but we had to adopt a standard procedure of checking every request for self-consistency of the data and, where information was available, whether the stated requirement was the most suitable for the solution of the problem. Consequently, we found many errors of presentation that were corrected before we started.

But we had little idea of the background of many of the jobs. One of the most 'famous' (within a limited circle) was a 'top-secret' demand for the solution of an integral equation from the minimum data; no other information was available, nor, as far as I know, ever became available. We got a solution, well within the time limit, but

extremely luckily (as we discovered later) since the empirical methods we used only converged if our original 'guess' was within certain limits. Many of the jobs, connected with radar propagation and the design of wave-guides, involved complex variable theory and calculations. We had no means of assessing whether our work was useful, except that in one case we got several repeat orders! One interesting job, that caused much concern, was the design of camouflage for ships (destroyers, I think) in various operational circumstances. The theory was fairly simple, but it was required to give the officer concerned an immediate guide. The design of nomograms with many variables is an art! And there were many more.

The A.C.S. took up a lot of my time even though I had a very good team of mathematicians and numerical analysts (the word had not been coined then!). Not only did I do all the administrative work, I checked the reports and prescribed priorities. Miss Joan Perry, my secretary, made up a sheet (or sheets) describing every job, and these descriptions formed the background to my task of allocating different jobs to different teams, with the priorities attached. But also, as part of A.C.S. work, we gave advice on machines and methods to different establishments for D.S.R.. This involved discussions with the Treasury, whose Organisation and Methods Branch was typically unhelpful. In one of my trips to the Admiralty Signals Establishment (A.S.E.) I ran into Fred Hoyle, whose description of a cathode-ray tube and of its operation, was a model of lucidity. But we also discussed astronomy, and I made some effort to heal the divide between the R.A.S. and himself. He then told me that, as soon as possible after the war, he wanted to go to Mount Palomar and get the results from the 200-inch telescope.

CHAPTER 10

The post-war period (to about the end of 1947)

A visit to Germany

After the collapse of Germany in 1945 I was asked by D.S.R. to accompany a T-Team to go to Germany to investigate mathematical and computational progress during the war. (The two main fields of enquiry were developments in mathematical analysis and numerical techniques, and the progress that the Germans had made in developing 'automatic' digital computers, if any, for use with the V2 rockets.) I was asked to lead the team, but, as I was given only about three days notice, I was very ill-prepared for this task. The team consisted of Todd (who arranged the visit), Reuter (an ex-German, now Professor of Mathematics in Bristol), Baxter (an astronomer from Aberdeen University) and Fred Hoyle. [Baxter, who specialised in optics, was sadly killed in the Dakar air crash in 1947 when on the way to the eclipse of 1947 May 20 in Brazil. Another man, Strong, was also killed in the crash and Alan Hunter was severely injured.] I became a full Commander R.N.V.R. and was the senior officer. We went in army battle dress, with a navy cap and armllets. I was duly fitted up at a depot near London. We flew from Stansted and picked up a Ford transit van; but this developed trouble, and I was the only person who could drive it without stalling. Our request for a replacement van met with the supply of a U.S. armoured car with a regular driver. He was marvellous and slept in the car!

I had earlier persuaded the R.A.F. to organize a 'training flight' to observe the eclipse of 1945 July 9 over Greenland, with an increased totality by flying along the track, but on my return to England from Germany I found that the flight had been cancelled by the Air Council on the ground that it was politically undesirable as the war with Japan was not yet over. Many years later I did have a successful flight to observe an eclipse. I have a memento of the R.A.F. flight to observe the eclipse of 1954 June 3. It is a copy of the N.A.O. leaflet about the eclipse and it was autographed by all on the flight.

The report of the Group was written up for (later) publication by Todd, but there was nothing of interest to us. The calculation of the trajectories of the V2 rockets was crude in the extreme; an automatic differential analyser (on Hartree's model) was found, and immediately shipped to N.P.L., where I don't think it was used! I returned from Germany before the others (though not before Hoyle, who had to be sent back after a few days because of illness, due to vaccinations, etc.). The team had much greater success in Bavaria where they discovered a mathematics team devoted to algebraic mathematics. Todd was interested, and helped them (I think because his wife, Olga Taussky, was, and is, the foremost algebraicist in the world) to form an institute which still flourishes.

The story of the trip to Germany is not part of the history of the N.A.O., but nevertheless, it was an experience that I shall not forget. In many ways it was chaotic in the extreme, without apparent plan or organisation; our 'orders', which were signed by a member of the Board, probably D.C.N.S., were adequate to command the fullest cooperation of local commanders, British, French or American. Nothing much accrued

to N.A.O. from my visit, except that we 'acquired' a number of Brunsviga 20s. Unfortunately, I had no opportunity of seeking out Kopff and the Astronomisches Rechen-Institut (A.R.I.), though I did see (and help) astronomers in Heidelberg and Göttingen. I would have liked to have found out what had happened to A.R.I. (which we did not know had been bombed out of Berlin-Dahlen, and transferred to Magdeburg in imminent 'danger' of being transferred to the Russian zone). But Todd made many useful contacts, and was very helpful to the German mathematicians.

The Astronomer Royal (Spencer Jones) and H. M. Smith later made a similar visit to Germany, and were able to arrange for the setting up of the A.R.I. in Heidelberg.

One incident occurred in Heidelberg when we were in a new block of flats. [I can remember that when Clemence and I visited Fricke in about 1954 (before the A.R.I. had moved to the new premises in Mönchhofstrasse) I was surprised to find that he then occupied the flat in which we had stayed in 1945! Fricke does not know of this!] While in Heidelberg, we learned that an 'old' friend, Helmut Hasse, was living there. He was a mathematician with whom I had stayed (with Harold Davenport) in 1932 and 1934; he was then anti-Nazi, but up to 1939 he became more and more Nazi, and we 'wrote him off'. I asked Hoyle if he could find out whether Hasse was living there now; he said that he had made enquiries and the answer was 'No'. I therefore took the armoured car to call on his wife, Clare, and daughter in order to confirm that they were OK. To my utter astonishment the door was opened by Helmut! I was in uniform, with a revolver, but he started to bargain with me. He said that he would reveal all his secrets of his work with O.K.M. (Admiralty) provided that we could ensure his passage to U.S.A., out of danger of being passed over to the Russians. I saw Clare to whom I gave some PK rations, but reported my interview to the local C.O..

The post-war period in Bath

After the end of the war in 1945 it became clear that the days of A.C.S. were numbered, and the work-load gradually tailed off, preparatory to its disbandment at the end of the year. A few small jobs continued, and N.A.O. received requests for information (and occasional added calculations) in respect of some of the more fundamental calculations undertaken; for example, the Incomplete Airy Integral.

The main demand at the end of the war was, however, to get down to the Office's normal work after six years of calculated neglect. I had received about six or seven letters from W. J. Eckert (then Director of the U.S. Nautical Almanac Office) calling my attention to the shortcomings of Brown's *Tables of the Moon* and numerous other matters for discussion about the *Nautical Almanac*. I replied to these letters saying that, in the circumstances of the war, it was not possible for me then to give positive replies. It was impossible to get in contact with Kopff (Director of the A.R.I. in Germany) and difficult to communicate with Fayet (Director of the French Bureau de Longitude); in fact, these difficulties meant that we had to do more work on the A.P.F.S. ourselves. There was therefore no means of reaching international agreement. These difficulties continued long after the war. It was not until Gerald M. Clemence succeeded Eckert that we were able (in 1947) to meet and discuss the issues. Eckert was an expert on applying I.B.M. machines to scientific work, much as Comrie had been over here, and he went to I.B.M. as Director of Pure Science.

The 'normal' work of the Office had been continued, in its basic essentials, throughout the war; but almost everything beyond the normal minimum had been

postponed. The three most urgent matters, as I recall, were: the occultation programme in which the war-time offer to reduce observations (instead of publishing reduction elements) as from 1943 had to be fulfilled; the revision of the *Abridged Nautical Almanac* and the provision for astronomical navigation; and the completion of the lunar ephemeris (to which reference has already been made). I turned what spare time I had to what seemed to me the 'fundamental' work of the Office, namely the provision of almanacs and tables for surface navigation.

The occultation programme was now a major task because we were not only providing predictions of occultations, but we were obtaining new observations from all over the world, then reducing them, and then analysing them. By its nature it was a basically unsatisfactory project because of the disproportionate amount of 'clerical' work involved in obtaining, and verifying, the observations. Added to this was the considerable work of the actual reduction of the observations, which originated from a large number of distinct observers whose positions had to be checked and incorporated. It was only possible to continue the relatively crude annual discussions started by Brouwer, pending a much increased effort. It was not practicable to inform observers of the accuracy of their observations until about 2 years afterwards. This was partly because many of them sent them in, or published, their observations very late and partly because no assessment could be made until after the annual discussion had been made. It was only after many years that the I.C.T. 1909 computer allowed the possibility of 'instantaneous' reduction and print-out, thus making possible a very rapid 'acknowledgement and assessment' service to observers. However, both the prediction and reduction programmes were continued as planned; and the numbers of observations gradually increased. The amount of work that it involved was very great and, although the reductions have long since been recalculated on the computer with reference to the improved lunar ephemeris, a large proportion of the work (that is the collection of the observational data in acceptable form) is of permanent value.

Post-war changes in staff

The end of the war in Europe brought an immediate reduction in the considerable pressure on the N.A.O. and its staff. There was a reduction in the hours of work and a lessening of the restriction on leave, but no relaxation of the severe conditions (rationing continued for many years) under which we lived. It took some time (I cannot give even an approximate timetable) to return to the 'normal' staffing and organisation of the Office. There was great upheaval in the staff. Many of the A.C.S. staff (other than the junior locally-recruited 'Temporary Clerks') transferred to the newly formed Mathematics Division of the National Physical Laboratory from August 1945 onwards or took other jobs, of which there were plenty on offer. The temporary war-time staff almost all left as their terms of duty expired, and so we were back to the pre-1939 staff. Most of the senior staff remained but we were left with the problem of recruiting staff who would stay. Harding returned from the Army, and we took on, as a Temporary Assistant, J. B. Parker, who had been recently 'demobbed' from the R.A.F., in which he was a Specialist Navigator; he had been the youngest member of the last Spec. N. course that had visited the Office. He stayed for about 2 years before moving on to the Ministry of Civil Aviation and, eventually, to Aldermaston where he is (1977) in charge of the statistical branch concerned with Monte Carlo computing methods: statistics was always his main interest. He did valuable work on a number of mathematical problems, particularly on the theory of the Decca lattices, and the accuracy of astro fixes. Miss McBain was in charge of the occultation programme, with the assistance of Miss

Rodgers. Richards was in charge of A.P.F.S. and Scott many jobs (Decca, Ryde night illumination diagrams, the astrograph and the *Air Almanac*). The Daniels were in charge of the many proofs that were read. So we had our problems. {This list omits the NA and ANA! Ed.}

Formation of the Mathematics Division at N.P.L.

I think it was the demand for the N.A.O. to work on the Bomb Ballistic Tables that impelled me to put forward, officially, through the Astronomer Royal and the Hydrographer, to C.E. Branch and thence to higher authorities (such was my ignorance) a formal proposal for the setting up of an organisation for the centralisation of all governmental computation work arising from the war effort. About two or three years later (my memory is vague about dates) the Principal Under Secretary in the Admiralty (McLeod) rather shame-facedly produced the file containing my proposal, with the remark that “since the Admiralty Computing Service was in existence and doing such an excellent job, perhaps the file, which has been on my desk for so long, might now be annotated with ‘action no longer needed’ ”!

It was later, towards the end of the war, that I put forward, through the Astronomer Royal, but direct to a Scientific Committee (the name of which I forget), the proposal for a national mathematical and computational laboratory. As a result of an approach by Carroll a committee was set up in 1944 by D.S.I.R. under the chairmanship of Sir Edward Appleton. I recall that the first meeting of the committee was fixed for the second Friday of a month. This was an R.A.S. meeting day and was one of the very few occasions that I missed the meeting of the Council while I was Secretary. Spencer Jones was Treasurer of the R.A.S., and he also attended the committee meeting. Lots of people were present, including representatives of statistical bodies and tax officials, as well as those devoted to computing. Sir Charles Darwin (then the Director of N.P.L.) offered to set up, for the purpose, the Mathematics Division of the National Physical Laboratory. This seemed exactly what was proposed, and so it was agreed.

In due course, I was invited by Sir Charles Darwin to apply for the post of Superintendent of the new Division. I did not want to leave the N.A.O. but Todd told me that it was stupid not to apply and so I allowed my application to go forward. There was, I think, only one other applicant, J. R. Womersley, who entered this story in 1936 when he was in charge of the Army statistical research unit. He had the ability to speak well and he made a powerful speech at the meeting, on behalf of a statistical organisation (mainly on quality control). I did not have such powerful advocacy and he got the job. I was greatly relieved at not having to decide whether to accept or not, but I doubt whether I would have accepted it.

Womersley was appointed Superintendent of the new N.P.L. Mathematics Division on 1 April 1945. It included many competent staff from A.C.S., but Womersley was not very successful and he did not stay long. The next Director of N.P.L. (Sir Edward Bullard), on being asked by Womersley whether he should accept an offer from B.T.M.C. (later I.C.T.) to direct their computer development programme, is reported to have said “I have no wish to stand in your way”. The new Superintendent was E. T. Goodwin, who had been in the A.C.S., and with whom we had many years of fruitful cooperation.

Womersley had little more than a year with B.T.M.C.. Some said that the comparative failure of all earlier B.T.M.C. computers was due to his leadership. [I was

told by Dickens of B.T.M.C. that its slow start in computer design and production was, partially at least, attributed to Womersley's appointment.] Womersley was later (I think) attached to the British Scientific Staff in Washington, but died suddenly leaving his widow in straitened circumstances in Washington as he had left her nothing! I was asked by the British scientific representative in Washington to suggest possible sources of help for Mrs Womersley. She asked me seek government aid; I did what I could, but the case was poor. The government did, however, make an *ex gratia* payment.

Captain Schmidt and Decca for Denmark

Shortly after the war a Dane, Captain Axel Schmidt, then in command of the Danish Royal Yacht, was interested in Decca, and he obtained permission to come over to see the Hydrographer, Decca, and ourselves, as we did all the calculations. He was a most cultured man, who was interested in geodesy, and who did not mind working on detail. The Hydrographer was in London but, oddly enough, the Hydrographic staff in Bath did nothing about finding him accommodation in Bath. So when he arrived, I was placed in the position that I had to find accommodation quickly. I solved this problem by the courtesy of Mrs Thornton, who was then quite happy to have a visiting Naval Officer (and his contact with the King and Queen of Denmark) to stay with her. He spent several days [about a month?] with us, going through the complete procedure for calculating the Decca lattices. He was a most charming, and efficient, man. He subsequently started a computing centre in Denmark (the chief being E. Moller, an astronomer whom I had known) and he invited me over to Denmark to start it up. I had four days in Copenhagen in 1946 [?] to get the Danish Hydrographic staff started on their calculations and I never worked so hard. I there resumed my friendship with Miss Vinter-Hanson at the Observatory. Axel Schmidt (and his wife) were great friends of mine until he died in the early 1970s.

Cooperation with the Spanish Almanac Office

In a series of letters during 1946 Captain de la Puente, then Deputy Director Spanish Observatorio de Marina [Naval Observatory] at San Fernando, near Cadiz, enquired what calculating machines we had and how we used them in calculating the positions of a stars. At that time the office of the *Almanac Nautico* (A.N.) contributed a portion of the 10-day stars in A.P.F.S. I explained our present set-up (including a substantial addition to our collection of Brunsviga 20s, which I obtained from Germany) and I mentioned the important part played by our National machines. He obtained from me the essential specification of the machine, and its distribution in this country, and he placed an order. He then explained to me that he had no one capable of using them, and he requested that we should take two of his staff for 2 or 3 months, and show them our methods of calculation, particularly using the National machine. We agreed to do this, and he brought over his two staff. He was not particularly interested in the calculation, and his English was not very good, and so he spent his time at the Spanish Embassy and visited them once or twice. The two Spaniards were delightful; they spoke very little English, but they got on well with the staff and I think that they enjoyed their visit. I do not know how much they learned; we certainly told them how to use the National machine for differencing and integration.

An amusing incident occurred towards the end of their visit. We had entertained de la Puente (and his two computers) to a meal in Bath and the staff and I had entertained the two Spanish on other occasions. Then de la Puente invited me to join him for lunch in London. We met at Martinez (the Spanish restaurant, which I knew

well) and de la Puente called the waiter over and ordered our food in a string of Spanish. But the waiter said that he (and the other waiters) could not speak Spanish and so I had to order! It was, I believe, during this meal that I suddenly realized that he was looking forward to using the National machine to calculate the apparent places of stars, which formed the main contribution of Spain to the pool of the international ephemerides and A.P.F.S.. But we did not compute apparent places of stars on the National machine, apart from certain stages of preparation of data and for war-time approximate methods.

In desperation, I thought up a method that might work, and with Richards' help (he was in charge of A.P.F.S.) constructed an example, and compared it with the normal methods. The method was based on building up the apparent place by the continuous summation of small multiples (second differences of the day numbers) of the star constants; a good deal of preparation was required, but the final print out could be guaranteed correct if the full 11-figure last value agreed exactly with the pre-computed value. I cannot recall whether the multiples were stored in registers, or calculated by auxiliary Brunsviga. The main thing is that the method worked, and, in fact, was later much used in the Office. After some modification we wrote up the set-up for the National machine, and gave all the operating instructions. We then explained it to the Spaniards, and got them to try it out, but whether they understood it I do not know. Richards and I presented a short paper describing the method in a paper to the R.A.S. (1948). I do not think that we would have developed the method on the National machine without the inducement of their visit.

The Star Almanac

In August 1947 there was a Conference of Commonwealth Surveyors in Oxford, to which myself and Richards were invited, primarily to discuss the provision of astronomical data for land surveying. The N.A. was unsuitable, and the A.N.A. was inadequate for their accuracy. Dr de Graff Hunter was the leading exponent, and he gave a draft outline of the contents of a special publication. Afterwards we drew up detailed proposals and specimens for *The Star Almanac for Land Surveyors*. They were considered at length by a technical committee of surveyors and eventually agreement was reached on the general content and layout (with only very small changes from our proposals). A full description, with specimen pages, was given a wide circulation to surveyors throughout the Commonwealth. The emphasis was on something that could be carried by the surveyor, and it therefore should be small and light. It thus could not use the G.H.A. method, and was therefore based on the old *E* and *R* method, which found favour with the rather old-fashioned surveyors. In an 80-page paper-covered volume, this provided all the data the surveyor wanted, including the positions of the brightest 650 stars. Copy was prepared for the first edition for the year 1951. {See chapter 12.}

The Institute of Navigation

The formation of the [Royal] Institute of Navigation in 1947 was to have significant effects on my life and on the Office. I was a member of the Steering Committee, and played a significant part in getting it started. Following my experience in the R.A.S., I drafted the Bye-Laws of the Institute as if it were a scientific society; in spite of the decision to have Fellows as well as Members, the concept (of a scientific society) has remained unchanged. [A proposal by the Way Ahead Committee in 1984 to change the Constitution was approved by the Council, but the members referred it back

and it was not adopted.] I was also responsible for approaching Sir Harold Spencer Jones to accept nomination as the first President. The R.I.N. (it received the ‘Royal’ through the later intervention of our Patron, the Duke of Edinburgh) took up a lot of my time in 1947/48 (and for many years thereafter) when I was helping the ‘new’ Secretary, Michael Richey, with the Journal and with the general organisation. I gave up the secretaryship of the R.A.S. in February 1947, but thereafter for many years I did (in an unofficial and advisory capacity) almost the same duties for the I. of N., which had no honorary secretaries. It will be noticed that the Constitution and Bye-Laws of the Institute owe much to the R.A.S. and they have proved equally enduring. The Journal of the Institute [now the *Journal of Navigation*] has been the medium for the publication of a large number of N.A.O. papers under my authorship, under collective authorship and by several of the N.A.O. staff.

Honours

As a kind of last fling of the A.C.S., the Admiralty organised a two-week course on numerical computation in London and I gave a number of lectures. While in London I was informed of the award of the O.B.E., though, of course, it was not announced until the Honours List was published in the 1948 Birthday Honours. This was in recognition of *our* (the N.A.O.’s) service to air navigation in the R.A.F., and was a great tribute to members of the staff of the Office as much as it was to me. It wasn’t until much later that I was told by Chilton (now Air Marshal Sir Edward Chilton), in strict confidence, that my name had been put forward for the award of the O.B.E. by the Air Ministry, but (since I was an Admiralty civil servant) it had to be referred to the Admiralty for approval. It was turned down by Spencer Jones, but was approved at the second time! [I personally thought the work on the Admiralty Computing Service (which was outside of and additional to the normal work of the Office), coupled with other jobs such as the B.B.T., would have been more worthy than the work for the R.A.F.]

A second award later was more in keeping with the work we had done for air navigation. The Thurlow Award of the U.S. Institute of Navigation was awarded to me in 1948. The Award is the highest honour of the U.S. Institute and consists of a plaque (very heavy!) depicting Commander Thomas L. Thurlow, who was killed during the war. The plaque used to hang in my office! Both of these awards were in recognition of work done by the Office as a whole, and I hope I made this clear at the time.

In neither case did I personally receive the award; the King was too ill, or too busy, and so a large number of awards of O.B.E. and M.B.E. were sent by post to recipients. The 1948 I.A.U. General Assembly made it impossible for me to go to New York for the presentation of the Thurlow Award. It was kindly received on my behalf by a Captain who was going to New York on DECCA business. He was a most generous and kind-hearted man; he was responsible for the foundation of the Hon. Company of Master Mariners and (anonymously) provided the basic funds on which the Institute was founded.

Other events of the post-war period

Other events that I recall include the following.

Miss McBain was appointed Editor of *Monthly Notices of the Royal Astronomical Society* in 1947, a new post designed to assist the secretaries to catch up on the arrears due to printing difficulties allied to the ‘peak’ of papers arising from the ‘troughs’ of the war years.

One interesting arrangement, connected with the provision for astronomical navigation, was the agreement with the Hydrographer to send a member of N.A.O. staff to sea for several months to get experience of practical requirements. As a result G. A. Harding (who had returned from military service to work in the navigation section under Scott) spent three months in early 1949 mainly in the Mediterranean base at Malta, on the surveying ship H.M.S. Dalrymple. He enjoyed himself and learned a lot; he also was most diligent in taking and analysing observations. He made frequent reports (as well as keeping the staff fully informed of his activities) and wrote several papers. Although no great results emerged that suggested fundamental changes, considerable benefits accrued in mutual understanding and appreciation. The Navy (particularly the Captain of Dalrymple, later Sir Archibald Day) was impressed by Harding and he was equally impressed by the skill of the navigators in taking sights. Harding tells the story of Day (himself) getting 5 star sights with a spread of 2 miles when he (Harding) could not stand on deck.

About this time we had our first 'vacation student' since Miss McBain in 1937. Professor McCrea was at Royal Holloway College and recommended this girl to us since she was a first-class student. Dr Joyce Gardner was a delightful girl, who made herself at home with all members of the staff, and was an accomplished mathematician. Miss McBain kept up with her after her marriage to a physics student (Billings); they emigrated to Perth where she became a lecturer and later sub-dean of Perth University.

Throughout this period the Office staff endured the rigours of post-war Bath and its appalling weather with fortitude, good humour and good companionship. We had some very extreme winters, made more difficult to endure by fuel, energy and other shortages, including rationing. The annual outing was resumed, and I can recall visits to Cheddar Gorge and Lyme Regis among others.

CHAPTER 11

Major changes in the post-war period

Visit to the U.S.A. in 1947

In late 1946, or earlier, we submitted a proposal to the Admiralty (through the usual channels of A.R. and Hydrographer) that the N.A.O. should rent (since purchase was not then a practicable, or even possible, alternative) punched-card equipment. The Admiralty decided that it must have further information on the application of punched-card machines to astronomical computation and on developments in automatic digital computers. For this reason the Astronomer Royal and Hydrographer approved my visit to the U.S.A. in April/May 1947. My main reason for going was to consult Clemence, but I could not get permission for that only so I had to draw up a list of visits to centres of computation with a view to studying their progress in the design and use of automatic computers. I spent most of the time with Clemence at U.S.N.O., but I did visit Harvard, Yale, Princeton (where I met von Neumann for the first time), as well as military establishments at Aberdeen, etc.. None of the work being done on automatic digital computers, all at an experimental level, seemed of relevance to the Office work, so I concentrated on the equipment at U.S.N.O. and its applications. I also spent some time with Eckert and I.B.M., discussing the possibility of acquiring a card-controlled typewriter. The reports that I presented on my return led, eventually, (and it was a long time!) to the installation of the I.B.M. 602A calculating punch, the I.B.M. card-controlled typewriter and other equipment. I made extremely valuable contacts and agreements with Clemence and U.S. Naval Observatory, but otherwise the visit was largely a waste of time. For instance, I can remember T. E. Stern's comment at Harvard: you and Clemence had better shut up shop as in a few years time no one will use a printed ephemeris since the data will be calculated and stored by automatic digital computing machines. That was nearly 40 years ago!

Hydrographer arranged my trip through the Scientific Attaché in Washington, but did not tell me of the arrangements! I duly turned up at Southampton to board the Queen Elizabeth without a ticket, or reservation, but I was assured that someone would be there to meet me. It worked, much to my relief! Then the Queen Elizabeth was stuck on a sand-bank and the departure was delayed for 24 hours. During that time in harbour I was treated as though I were at sea, without any of the continuing wartime restrictions on food. I soon found out that there was on board a group of delegates to the second IMRAN conference (International Meeting on Radio Aids to Navigation), of which I knew several of the (British) delegation through my connection with DECCA. I actually attended a meeting (concerned with DECCA) and spoke on the calculations. One of the delegation, Dick Michell, organised a chess competition among the delegates; I won through to the final where I played a Frenchman, who had served four years in a POW camp. He, being a rook and a bishop down, took 50 minutes considering his next move (his excuse being that when playing chess in his POW camp there were no time limits); I then left my queen *en prise*!

I was duly met in New York by a member of the staff of the Embassy, who proceeded to see me through customs, to give me a large sum in dollars and to escort me to Pennsylvania station for my visit to Washington. Clemence had arranged to meet

me at Union station in Washington but I missed him. So I took a taxi to his house where I met Mrs Clemence and her two sons. Clemence arrived later, much mystified by my non-arrival.

The Scientific Attaché office in Washington had arranged appointments for me to visit such places as Aberdeen (the armaments research centre), Princeton (von Neumann), I.B.M., Harvard Observatory, Yale Observatory, and certain research centres working on computers. Nothing I saw was particularly interesting to me as far as computing was concerned! The principal research was in the design of a fast drum, although there was a hint of a new development of a mercury delay line. Yet I met many astronomers at Harvard and Yale, some of whom I got to know much better later. Either then, or in a subsequent visit or visits, there was an informal conference of astronomers from the eastern part of the U.S.A.; my visits usually coincided with one of them as I got to know about them through Clemence. The former Superintendent's house at the U.S. Naval Observatory was taken over by the Navy for the residence of the Chief of Naval Staff; I treasure the memory that the flowers on my desk at the Naval Observatory came through the courtesy of Mrs Nimitz!

The substance of my visit was my talks with Clemence, and primarily concerned the redesign of the *Abridged Nautical Almanac* and the provision of sight reduction tables for navigation at sea.

Revision of the *Abridged Nautical Almanac*

After a tiring day, involving disembarking from the Queen Elizabeth, the journey from New York to Washington by train, missing Clemence at Union Station and having dinner, Clemence asked me if I would like to see the Nautical Almanac Office in the U.S. Naval Observatory. We went at about 10 pm, and after showing me round, he asked me for my views on the redesign of the *Abridged Nautical Almanac*, both British and American. I had studied this matter in some detail, and I gave my opinion on the general layout and content. There were four main possibilities: the E and R form of the Almanac, extending E for the Sun to all other bodies except the stars; the direct tabulation of G.H.A. at a small interval for all bodies; the tabulation of S.H.A. for all bodies combined with tabulation of G.H.A. Aries (or R) at a small interval; and a mixture of the last two. I found it very difficult to decide between these and, in fact, had not definitely made up my mind when I went to visit Clemence. I had a slight preference for the mixed method, namely G.H.A. for Aries and all bodies other than the stars together with S.H.A. for the stars. After about an hour of discussion Clemence agreed not only to redesign the *American Nautical Almanac* to conform, but also to replace the name Greenwich Civil Time (G.C.T.) by Greenwich Mean Time (G.M.T.). At that time our agreement concerned only the principles, and did not cover the general layout. The agreement to make the almanacs identical came later.

This was my first meeting with Clemence, and was an auspicious beginning to a much valued friendship and a most productive cooperation between U.S.N.A.O. and the Office. The agreement was, of course, subject to confirmation and approval by our respective users. In the U.K. the proposals were referred to the newly-founded Institute of Navigation by the Admiralty, but the R.A.S. was not consulted. They were fully discussed at a meeting of the Institute on 7 May 1948, and considered by a Committee that reported to the Hydrographer; although there were some reservations on matters of detail the general principle was approved. Later the presentation was changed when copy was prepared on the card-controlled typewriter, but the principle still remains.

Tables for marine navigation

Clemence and I also discussed the position of the sight reduction tables and, in particular, the reproduction of H.O. 214 for British use. I had proposed to the Admiralty a complete set of new tables (on the lines of N.P. 401 - now U.S. Pub. 229), but this was turned down. Instead, my alternative suggestion, that we should approach the U.S. Hydrographer for permission to reproduce H.O. 214, was agreed. This was a bitter pill for me to swallow, since H.O. 214 contains at least one *fundamental* error (which could not be corrected), some systematic errors (which could) and many accidental errors (proofreading mainly, which could). But it was the quickest possible way of providing the Navy with the tables they desired. At this point it should be emphasised that in their statement of requirements the Navy had specified a nominal accuracy of 0'.1 and plotting from the D.R. position.

Clemence therefore arranged an interview with U.S. Hydrographer, in which I put forward the case for reproduction of H.O. 214, subject to our correcting their mistakes; I think that the U.S. Hydrographer was aware of my criticism of the first version. [In early 1937 the then Hydrographer J. A. Edgell brought the U.S. Hydrographer to visit N.A.O.; he displayed with pride a copy of the first edition of H.O. 214, and left it with me for comment. I found 20-30 proofreading errors on the first page at which I looked, and further examination confirmed that the standard of accuracy was unacceptably low. U.S. Hydrographer immediately cabled to Washington and the first edition was withdrawn.] I was pleasantly surprised when he agreed and welcomed the fact that the British Navy would be using the same tables and, much more, that this would sell the volume to the British Merchant Navy! I disliked, and still dislike, H.O. 214; it is poorly designed and was badly executed, and it was with the greatest disgust that I was more-or-less forced to make this just-better-than-nothing suggestion in order to complement the new form of *Abridged Nautical Almanac* introduced in 1952. We proof-read much of H.O. 214, and made many corrections with scissors and paste, before photolitho reproduction; in some cases whole columns were replaced. I insisted on rewriting the Introduction, so that I could warn users of the design fault, namely the recommended use of the *nearest* tabular hour-angle for interpolation with the variation calculated for the mid-point of the following interval!

A subsequent incident angered me at the time. I had agreed with the Hydrographer that I would personally discuss the reproduction of H.O. 214 with the U.S. Hydrographer when I was in Washington. I did so *with the foreknowledge* (supplied by Clemence) that the U.S. Government had no copyright on any of its publications. Thus there was no difficulty in reaching an amicable agreement to reproduce it as H.D. 486, with full acknowledgement and cooperation, and with no restriction on sale. There was no law in the U.S.A. that prohibits this; all government publications must be freely available for reproduction. My arrangement was only a matter of courtesy, but I later discovered that the Chief Civil Assistant to Hydrographer had boasted of his triumph in obtaining permission to reproduce and had (in the formal exchange of letters confirming the oral agreement) quite unnecessarily stated that the tables would be for the use of the Royal Navy only and would not be put on sale! The first edition was therefore issued under a Restricted classification, totally destroying much of its purpose. I am afraid that I told him what I thought! This condition was not removed until 1954, by which time other countries had received permission to reproduce without such a restriction.

Still my visit to the U.S. Hydrographic Office was a pleasant surprise. Of the large war-time staff, Albert M. Moody and Henrietta Swope were still there. They became great friends of mine, one in the navigation field and the other in the astronomy field. I got on fairly well with the future head of the navigation section J. H. Blythe, but he was a trifle ponderous, especially in his explanation! We discussed the first edition of H.O. 249, and gave preliminary consideration to the three volumes of the final edition. We subsequently played a large share in their production, not only of the British edition A.P. 3270, but also of H.O. 249. When I think of the proofreading that the members of the Office staff did in connection with H.D. 486 and A.P. 3270, in addition to all the other proofreading, I am amazed at their patience and accuracy.

Further comments on tables for navigation at sea

The redesign of the *Abridged Nautical Almanac* was very long overdue, as was, in my view, the whole provision for astronomical navigation at sea. There can be no doubt that almost since the revision of the N.A. in 1834, the requirements of the seamen had been less than fully met. After the *Requisite Tables* ..., and its immediate successors, the Office (either deliberately or by default) took the view that the purpose of the N.A. was to provide the seaman with the astronomical data he required and that how he used them was not its concern. The *Abridged Nautical Almanac* (itself a rather reluctant addition to the N.A. itself) contained nothing relating to the reduction of observations. Comrie told me once that it was the official policy of the Admiralty NOT to include such material as it was the prerogative of Inman's Tables, the 'official' issue to H.M. Ships. I took the view (in 1946) that, if this were so, the sooner the policy was changed, the better it would be. Consequently, I undertook a very detailed investigation into the whole question of both almanac and reduction tables. As far as the latter were concerned, the flexibility of choice was severely reduced by the Admiralty's repeated insistence that the main recommended method (then the cosine-haversine method provided by the tables given in Inman) must allow for the use of the D.R. position as the origin for the calculated intercept. This essentially ruled out of consideration all 'short' methods that depended on the use of tabular values of certain quantities and direct tabulation with these arguments. The cosine-haversine method is a good one for altitude, but it is not the best as it does not readily provide azimuth; my comprehensive analysis, using my experience of tabulation, suggested that several methods were marginally better. [Aquino's logsec+logtan method is particularly convenient.] It was obvious, however, that it would be undesirable to change established practice for what must, inevitably, be a limited period before the Admiralty saw sense.

I put forward my views at some length in a paper presented at the Institute of Navigation in 1948, in which I made two suggestions that are now relevant. The first was for triple-entry tables of calculated altitude and azimuth that, after 20 years, are now available as *Sight Reduction Tables for Marine Navigation* (H.O. 229 in U.S.A. and N.P. 401 in U.K.). [The rejection of this idea by the Admiralty in 1947 led to the adoption of H.O. 214 as the basis of H.D. 486, which was published in the early 1950s.] The second was the possibility of dividing the navigational triangle by a perpendicular from the pole to the opposite side and the desirability that it be further explored.

The division of the PZS triangle by a perpendicular from P to ZS was used by a Yugoslav, Flegs, after reading my 1948 paper, but the method was not good. The difficulties appeared to be considerable, but I always thought that they could be overcome by tabulation techniques. There was no future in such methods after H.D. 486

(H.O. 214) came into general use, but I did ‘play’ unsuccessfully with the idea from time-to-time. It was, however, not until 1976 that I made a serious effort and came up with what I think is the best ‘short-method’ for the solution of the PZS triangle. It is described in “A note on short-method tables” in the *Journal of Navigation* 29, 290, 1976. This was a deliberately short note, without specimen tables (though I designed them), for the optimum tabular method of sight-reduction using the D.R. position, but this was many years too late!

New equipment and other topics of discussion

We also discussed the card-controlled typewriter (CCT), which Eckert had designed in consultation with I.B.M.. I was most impressed by the innate accuracy of the method of producing copy direct for photography. It promised to provide an end to proofreading. I came back with the idea of getting B.T.M.C. or I.B.M. to make us one. I was impressed by their punched-card installation and, in particular, by the I.B.M. 602A calculating punch as this was such an improvement on the old form of the B.T.M.C. calculator.

We turned to the substance of the *Nautical Almanac* and to the future of the I.A.U., whose next meeting was to be in Zürich in 1948. There were many problems and most of them were ours. But Clemence was working on his first-order theory of Mars and there was a brief discussion between Clemence and Brouwer on the fundamental constants.

The return journey from the U.S.A.

I cannot remember details of these discussions, but I can recall episodes on the return journey. There was no space available on direct routes from New York, and I was therefore booked on the Aquitania sailing from Halifax. I got on the late train from Montreal to Halifax, and then decided I needed a drink before turning in. I walked along to the ‘Parlour Car’, only to be told that there was no licence on board a moving train. Then a man flopped down on the seat beside me; he made the same request to the steward and received the same reply. It was Beeching (later Lord Beeching), whom I had known in Greenwich when he was doing research at (I think) the Building Research Station; we lived in the same house for several years, and he generally gave me a lift to the Office. On advice we got off at the appropriate stop at the station the next morning and walked a hundred yards to buy a liquor licence.

It was as a result of meeting him, and learning that his Ministry was sending a car to Southampton to meet him, that I sent a cable to Miss Ifield requesting the Admiralty to send a car for me. She succeeded, with great difficulty, as she subsequently confessed. [Beeching had left his Greenwich job, and had joined a chemical firm which was later taken over by I.C.I.; he was then a temporary Assistant Director of armament procurement at the War Office.] On arrival at Southampton, even though I had my luggage under the initial ‘Q’, I declared what goods I had purchased abroad (mainly nylons for the girls in the Office!) and was duly assessed for customs duty. I then paid at the cash desk, and then had to wait a long time before I could attract the attention of the customs officer. He then demanded that I open my cases; I told him to open one: he did so, but he chose the wrong one!

The General Assembly of the I.A.U. in Zürich in 1948

The General Assembly of the International Astronomical Union in Zürich in 1948 was an important event. It was the first meeting after the war, and since 1938 many

things had been happening in international cooperation between the almanac offices. Miss McBain and I attended from the Office and I think we did an excellent job in revitalising and reorganising the exchange arrangements and cooperation between the ephemeris offices. Clemence and I had discussed details of the problems at our meeting in 1947. Fayet was the President of Commission 4 (Ephemerides) and we knew, or suspected, that his interest had expanded during the war. He was now living part of the time in Nice; he had reneged his share (or rather the share of the office of the *Connaissance des Temps*) of the contributions to the A.P.F.S. and I think that Clemence had had difficulty in getting the data on the satellites of Jupiter from him. One of the most important resolutions adopted by Commission 4 was one on Universal Time that recommended that astronomers use this name exclusively for mean solar time on the meridian of Greenwich beginning at midnight.

I decided to hold a luncheon party for the Directors of the Ephemerides (and their guests) at which we agreed on a redistribution of the calculations. For the first time these included contributions from the Institute of Theoretical Astronomy in Leningrad, which produced the Russian *Astronomy Almanac*. Subbotin, the Director of the Institute, was not there, but A. A. Mikhailov (leading the U.S.S.R. delegation) had been authorised to act on behalf of I.T.A.. Subbotin was said to be ill, but in any case he rarely went abroad. I arranged the party at an hotel in Zürich, and got some extra foreign currency to cover the cost. [But the Admiralty failed to give me even a token sum for the lunch!] It was there that we first met Mikhailov. He was truly magnificent at that party — he told tales in English, followed by their translation into French and German. He made the party a huge success and contributed to the successful conclusion of our experiment regarding the exchange of data for publications.

So far as I can remember, I wrote the report of the commission meeting in the Transactions, even though it was signed by someone else. The Assembly was held at a time when there was rationing at home and the shops were almost empty and so it was a great tonic to us all. However, I nearly spoilt it! The delegates from the U.S.S.R. were very much on the defensive, and at the dinner we were all placed at separate tables, with a mixed arrangement of nationalities. I had, I think, representatives of U.S.S.R., Italy, Sweden, and France on my table. From want of something to say, I drew the table's attention to the fact that there were (say) 38 flags of the different nations present, when the attendance at the General Assembly was stated to be 39. The U.S.S.R. man at my table (Kukarkov, I later discovered, who became a great friend) noted that the missing flag was that of the U.S.S.R.; he got up, consulted the leader of the delegation (or perhaps Commission) and the whole delegation walked out! After a brief interval, the U.S.S.R. flag was restored, and they came back.

I was nominated as a member of the I.A.U. Finance Committee by the British National Committee of Astronomy; and by that body as member of the small sub-committee which investigated the accounts in detail. The Chairman was C. S. Beals, Director of the Dominion Observatory in Ottawa, but he had to leave Zürich (either some illness, or some call to an ill family), and he appointed me Chairman of the sub-committee and of the whole Finance Committee. This was the first opportunity I had of getting to know the workings of the I.A.U., and the first time that I contributed to the administration of the I.A.U..

Concern as to the future site of the Office

Before the war the Astronomer Royal had obtained Admiralty approval, in principle, to move the Royal Observatory from Greenwich, where the observational conditions had become impossible, both on account of the increasing illumination of the night-sky from street-lighting and because of atmospheric pollution. The outbreak of the war put a temporary stop to the search for a new site, but this was resumed in 1945. It had been agreed that the Office would join the rest of the Observatory at its new site; there was obviously no room at Greenwich. Naturally all the staff were anxious to know of the new site as soon as possible so that they could make plans for the move. We knew that the Astronomer Royal was exploring sites; he, rightly, believed that the Government could not afford to build and that a large country house (which was then going cheap) would form the best site for the Royal Observatory. Once the office space was provided for, the buildings and domes for the telescopes would come later.

We were not consulted and, in spite of several requests for information, we were not informed of the decision until, I am pretty sure, almost everyone else knew. [I personally heard of the rumour of a decision to move to Herstmonceux from friends (Mr & Mrs King) in Abinger Hammer, where the Spencer Jones' had a war-time house and office; they had been told by Lady Spencer Jones, who had described to them their accommodation in the Residence and the problems involved.] The Astronomer Royal visited us once, or at most twice, a year and little information came to us. He visited N.A.O. one afternoon after he had been to the Chronometer Department in Bradford-on-Avon in the morning and I asked him for confirmation that a decision had been made. He then told me the precise position, and expressed great surprise (quite genuine) that we were not fully informed; he assumed that we all knew of the choice of Herstmonceux Castle. Everybody else in the Royal Observatory knew! He authorised me to pass on the information to the staff, including his estimates of timetables for the necessary completion of purchase, alterations, etc. and the moves. He told me that the administrative staff would move first, once there was adequate living accommodation within the Castle; there would have to be quite a lot of work to make suitable accommodation for the staff. The N.A.O. would probably be the first, followed by the Chronometer Department from Bradford-on-Avon, and then the Time Department from Abinger. And he gave an approximate date for our move, but this turned out to be at least one year early! We certainly got the impression that N.A.O. would be moved, probably first, within a year, or two, at the very latest. On this basis Scott bought a house in Bexhill and moved his family there, only to have to commute, weekly, for about two years. Other members of the staff (Porter and Richards) made similar, but less precipitate arrangements. It was not to be until some months later that the Government agreed to the move; I was present, in the R.A.S., when the Admiralty rang Spencer Jones. He then made his announcement of the move on the radio. {Subsequently, in 1948, it was announced that the Observatory would be known as the Royal Greenwich Observatory, Herstmonceux.}

At an early date, Grimwood had made a visit to Herstmonceux and reported to us in the N.A.O. his findings. Apart from the Castle, there were about six hutments that had been used by the war-time occupants, the Heart of Oak Friendly Society. His report was excellent in its details. Later I visited, with the A.R., the recent owner, Sir Paul Latham, who was then living in Herstmonceux Place, a large house about half-a-mile north of the Castle. I did not inspect the Castle in detail; most of the time was spent at lunch, discussing details of furniture, tapestries etc, which were to be left in the Castle.

At some stage (I cannot remember the precise date) a party of about 5 people from the Office were invited to visit Herstmonceux and discuss the arrangements for office accommodation. The A.R. had planned that the N.A.O. should occupy the second-floor rooms in the north side of the Castle. These were then the servant's bedrooms and it was at once obvious that they were completely inadequate and unsuitable for the purpose. These bedrooms were later used to provide a women's hostel. As soon as I got back to Bath, I wrote to the A.R., pointed out the objections, and proposed that the N.A.O. should be accommodated in the huts then in the South Courtyard, as left by the Heart of Oak Friendly Society, until new buildings could be erected. Our proposal was eventually accepted, I think very reluctantly.

The new Scientific Civil Service

At the beginning of 1946, the new Scientific Civil Service came into being with the complete regrading of all scientific staffs; the staff of the N.A.O. was incorporated into it. On the whole the staff came out of the regrading exercise fairly well. I was graded S.P.S.O. (Senior Principal Scientific Officer), and Miss McBain (Assistant) a P.S.O. — but there were no S.S.O.s (Senior Scientific Officer) nor S.O.s (Scientific Officer) due to a failure to recruit during the war. The J.A.(H.G.)s, namely the two Daniels, Richards and Scott, were regraded as S.E.O. (Senior Experimental Officer), the J.A.(L.G.)s, namely Carter, Miss Rodgers, Grimwood and Smith, became E.O.s (Experimental Officer), except that, in spite of my efforts, Harding was appointed as an A.E.O. (Assistant Experimental Officer). There was (as far as I can remember) no other A.E.O.. The more junior staff became Scientific Assistants.

The proposals for the regrading were made by the Astronomer Royal, in conjunction with those for the staff of the Royal Observatory. I noticed that a young Junior Assistant (B. R. Leaton) was proposed as an E.O., and I objected to this as in my opinion Harding was very deserving of promotion. The A.R. upheld my point, and proposed that both should be regraded as A.E.O. and considered for promotion at the end of 1949. This was done — and I cannot think that anyone deserved promotion more thoroughly. It was a step in the right direction.

[The list is possibly incomplete, because I do not pretend to remember the ebb and flow of the many staff (permanent, temporary, or A.C.S.) after the war. Most dispersed, many of the girls either married or leaving to get married. I may have the details wrong; possibly not all those named were *initially* graded as E.O.s, but were promoted shortly afterwards. I recall Harding's case because Spencer Jones said he was too young.]

It should be remembered that, in the late 1940s and early 1950s, establishment was still fairly difficult to obtain, and there was considerable competition. There were obvious gaps in the balance of the various grades, particularly as regards the S.O. class, and A.E.O.s. The complement (i.e., the number of staff in the various grades) was adequate, largely because C.E. Branch did not make the full reduction for temporary work (A.N.T.s and extensions to the *Air Almanac*) that essentially ceased, but it did allow for the continuing additional work for the Decca lattices etc.. The difficulty was to recruit suitable staff at all levels except S.E.O. and E.O., in which grades we were admirably suited. We recruited junior staff (in the Scientific Assistant grade) to replace those who left to get married or because they were temporary. But we had difficulty in getting suitable A.E.O.s and S.O.s, for which posts there was a national shortage.

A search for a 'celestial mechanic'

On my return to the Office from Zürich there was the requirement to appoint a new P.S.O. to do research work. The vacant P.S.O. post was nominally for a 'navigational' P.S.O., but I wanted someone to work in the field of celestial mechanics. I tried to get in touch with W. E. Candler, but he failed to reply to my letters {see chapter 4}. We advertised through normal Civil Service Commission channels (including trawls of other departments), but had rather a disappointing response. We turned down obviously unsuitable candidates and interviewed the remainder. I don't remember the constitution of the panel, but I think that the A.R., or Atkinson, who was his Chief Assistant, was on in addition to the Civil Service Commission nominee. J. G. Porter was the chief contender but, although he was very competent in the orbits of meteors and comets, he did not, in my view, have, nor pretend to have, the background in celestial mechanics that I would have liked. However, as there was no one else, I agreed with the rest of the panel that he should be offered the job. He joined the staff in Bath. I had been tempted to offer the P.S.O. job to Scott, who had done so much work during the war and after the war. But the promotion of an S.E.O. to P.S.O. was unheard of in those days. So an opportunity for the Office to make some positive contribution to the theory of the motions within the Solar System was lost. We could, at that stage, have afforded one research post from the complement.

I should make it clear that I had, personally, long recognised that it was too late for me to make any original contribution. I had done NO celestial mechanics of any kind (I do not count the routine calculation of cometary orbits by special perturbations as celestial mechanics) since the purely theoretical lectures in Cambridge in 1929, and I was aware of my mathematical limitations even though I thought I could deal with numerical problems. The Office had done NO theoretical work since Cowell had been rebuffed by the Admiralty, though Comrie (who was not interested in celestial mechanics) had made considerable contributions to the practical application of special perturbations. It had always been my hope that the Office could, at some time, cease to be dependent on the Tables of Newcomb and Hill for the fundamental ephemerides for which, under international agreements, it was (and still is) responsible. The task was immense, both theoretically and numerically, as well as demanding the search for and analysis of all observations since 1900.

In the U.S.A. Eckert, who succeeded Robertson in 1940, was (as we were) primarily concerned with getting current work done and meeting the demands of the war; he also put a great deal of effort into mechanising the computations and the handling of data by punched cards. He more-or-less designed the card-controlled typewriter, which was so successfully used, both at the U.S.N.O and in the Office, for preparing copy for photolithographic reproduction. He temporarily suspended his long-term verification of Brown's lunar theory. It was not until he joined I.B.M. as its scientific advisor that he was able to evaluate Brown's theory directly, and much later to complete its verification. In the meantime, Clemence became Director of the U.S. Nautical Almanac Office and made a determined effort to improve on Newcomb's theories. The length of time taken over the theory of Mars, and still more so the introduction of the theory into the ephemerides, is, however, a measure of the immensity of the task. The corresponding theory of the motion of the Earth was left unfinished at his death. With Clemence's mastery of celestial mechanics, with his dedication to the task, and with the considerable resources (both man-power and machines) available to him, the time-scale for the theory of one planet was of the order

of 10 years, or more like 20 years in practice. I doubt very much whether the Office could have successfully finished such a task, even if the right person had been available.

Other staff changes

We had several more additions to the staff. We recruited G. E. Taylor, who had applied for a transfer from the Air Ministry, where he had been engaged on routine meteorological observations in the field; he joined the staff as an Scientific Assistant. Dr J. G. Porter and M. P. Candy, a Bathonian, also joined the Office in Bath, as did Miss D. Fooks, from Bath, Miss M. Hawkes, and Miss M. M. S. Gibson and Miss A. M. James from Scotland (see next chapter).

We expected to lose most of the temporary staff when the Office moved to Herstmonceux, and so we advertised in Bexhill (and Eastbourne) for Scientific Assistants. In a succession of interviews I took on two staff; one was Miss Grove and the other was Miss B. Hyne [?], who became homesick and did not stay long. I picked them up in my car, after starting from Blackheath where I still had my room, and took them to Bath where the billeting officer had found accommodation for them. On the journey we actually stopped and looked at Stonehenge.

Planetary Co-ordinates

As already mentioned, some of the outstanding work on the final stages of the lunar ephemeris was done, mainly by Richards, on the service machines of B.T.M.C. in Cirencester. The heliocentric ephemerides of the planets had been completed well before the war, together with the geocentric rectangular coordinates. This was all done as part of the large operation, during the course of which (almost as a by-product!) planetary coordinates referred to the equinox of 1950.0 were calculated for the years 1940–1960. Copy for the 1940–1960 volume of *Planetary Co-ordinates* was sent to the printer early in 1939, and the volume was published about, or slightly after, the move to Bath. In November 1940 all plates, type and stock at the printers were destroyed. The issue of a photographic reprint had to be deferred until 1946; during the intervening years we endeavoured to meet the requirements by the loan of N.A.O. copies. Porter, who was a ‘comet man’, was naturally allocated the job of preparing the succeeding volume covering the years 1960–1980; and he started investigating, in greater depth than before, various methods of calculating special perturbations, including the elegant but onerous methods of variation of elements. He started this work at Bath, but the bulk of the work on it was done at Herstmonceux.

The move to Herstmonceux

On the whole, I do not think that we could grumble unduly at our ten-year sojourn in Bath. We could have done very much worse.

I cannot remember anything else of particular interest that happened in 1948–1949, other than the difficulty of not knowing when and how we were to move. There was to be a meeting in the U.S.A. in October 1949, and I was anxious to move first. The N.A.O. was, however, not the first to move. The Astronomer Royal and his secretarial staff first moved (from Abinger) into rooms on the ground floor of the Castle East Wing, followed by the Chronometer Department, and (I think) the Solar Department, which moved into a large room on the first floor of the East Wing, and the Magnetic and Meteorological Department, which had an office over the canteen in the South Wing. The Office eventually moved to Herstmonceux over a period of a weekend in

early October 1949. The move was trouble-free and my visit to the U.S.A. took place shortly afterwards. One block of the huts (that nearest to the Castle on the south-east side) was used for ordinary office accommodation, while part of the hut on west side was used for the Hollerith machines, which were delivered later. The accommodation proved to be excellent.

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.