EXTRACT FROM

A PERSONAL HISTORY OF H.M. NAUTICAL ALMANAC OFFICE

30 October 1930 - 18 February 1972

By Donald H. Sadler

Edited by George A. Wilkins

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A Personal History of H. M. Nautical Almanac Office by Dr. Donald H. Sadler, Superintendent of HMNAO 1936-1970, is a personal memoir; it does not represent the views of, and is not endorsed by, HMNAO or the United Kingdom Hydrographic Office.

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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 $0^o - 5^o$, and Bremiker's tables of sines, tangents, cotangents and cosines in the range $0^o - 45^o$. 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 Ensleigh) 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 Tripos 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 (? $0^{s}.0001$) 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 armlets. 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.