BIT 20 (1980), 414–425

IMPRESSIONS OF THE EARLY DAYS OF PROGRAMMING

PETER NAUR

Abstract.

The paper describes the author's experience of working with the Edsac, Cambridge, England, in 1951 and 1953. The author's background in astronomical calculations using hand methods and his plans for spending a year in Cambridge are first described. This is followed by impressions of the Mathematical Laboratory, Cambridge, its staff, the external characteristics of the Edsac, the library of subroutines, documentation for programmers, machine language, paper tape preparation equipment, and organization of the use of the Edsac. The second major part of the paper describes the steps leading to an Edsac-program for calculating the motion of a minor planet by step-by-step integration of the equations of motion, with special attention to the preparation and check of large amounts of input to the program.

Introduction.

The purpose of this report is to record some first-hand experience of programming for a digital computer as this was gained at a time when programming was still a very young activity. The report records the author's impressions, based on memory and existing documents, of his exposure to the unique, new, intellectual activity of programming, as this took place during his work with the Edsac at the Mathematical Laboratory, Cambridge University, England, in 1951 and 1953.

As a contribution to historical documentation the present report is severely limited by being entirely based on the experience of one single person. It may still be of interest because it relates to programming for the Edsac, the first practical stored program computer to be put into operation, in May 1949, and to a period that starts only 21 months after that historical moment. As a further point of interest, the events to be recorded can be fixed fairly accurately in time and can to some extent be supported by the texts of the programs written at the time.

Background and preparations.

I started programming for the Edsac in February 1951, at the age of 22. This step in my activity may be said to be natural, considering my previous experience. As a young person I became interested in astronomy and got fascinated by the power of astronomical computations. I had the fortune to establish contact with

Received Aug. 14, 1980.

the staff of Copenhagen University Observatory, particularly Jens P. Møller, Elis Strömgren, and Bengt Strömgren. This institution supported a powerful tradition of astronomical computation, as applied both to celestial mechanics (the three-body problem, the motion of comets, and computation of orbits for newly discovered objects), and astrophysics (stellar interiors and atmospheres). Guided by the staff of Copenhagen Observatory I gained first-hand experience with a range of computational techniques, including orbit determinations, integration of Newtonian equations of motion, and differential improvement of orbits using the method of least squares. This work was supported by the international literature, e.g. Stracke [17] and Planetary Coordinates for the Years 1940–1960 [16]. A series of my results in these fields were of sufficient interest to be published from 1943 onwards (see, e.g., Naur [6]).

A further activity at the Copenhagen Observatory that prepared my mind for computers was the work of K. A. Thernøe on designing a fully automatic computing machine. Thernøe worked on plans for a purely mechanical device, unpublished as far as I know. In spite of much design effort the plans did not reach the stage of a practical realization before the advent of electronic computing made them obsolete. Even so, the idea that the long and tedious astronomical computing work could be fully automated was clear to me long before I first heard of the successful computers.

After graduating from Copenhagen University in 1949, and while doing my military service, I planned a year's stay at Cambridge as a Research Student. I wanted to profit from the riches of the academic activity of Cambridge over a spectrum of fields ranging from quantum mechanics, over computational physics and astronomy, to observational astronomy. My chief advisor in this planning was Professor Bengt Strömgren. From his contact with Professor Hartree he already knew of the existence of Edsac. My application for admission to Cambridge, written in March 1950 [7], mentions a plan "to study the work on quantum mechanical problems by means of electronic calculators". I first got in direct contact with the Cambridge University Mathematical Laboratory in July 1950. My letter of enquiry, addressed to Dr Wilkes, was answered in his absence in America by D. R. Hartree [3]. His reply was very encouraging and included several reprints of descriptions of the Edsac.

I arrived in Cambridge on 1950 October 4. As planned I spent my first months there trying to carry out a series of astronomical observations, besides following several courses and taking part in many other activities. By January poor weather had frustrated my plans for astronomical observations. I was still set on doing some astrophysical problem with the aid of the electronic computer. But as no such suitable problem came to my mind I finally switched to a problem with which I was already thoroughly familiar, the integration of the equations of motion for a minor planet, with full account of the attractions from the Sun and the major 7 planets, also known as the problem of special perturbations. This problem had considerable current interest in relation to a project under way at the Copenhagen Observatory, initiated by B. Strömgren. The project was to follow the motion of minor planet 51 Nemausa by calculations of high accuracy and compare the results with observations, also of high accuracy, with the purpose of determining systematic errors in the catalogues of stars used as references when measuring the positions of the planet. Much work on this project had already been done by Ole Møller, while I had myself contributed sufficiently to it to be well familiar with the computational procedure.

According to private letters and diary notes from the time, my decision to prepare this problem for the Edsac was made in February 1951. Until that time my only knowledge of programming came from reading the brief description of the Edsac that Hartree had sent me. They included the list of orders. So far I had not tried to write any program. On the other hand, I remember distinctly feeling at home with the idea of building up computations by series of executions of orders. This general familiarity with the idea of programming was clearly a result of my experience with complex astronomical calculations.

In the following description of my impressions of my first programming experience I have found it convenient to separate the description of the facilities and organization of the Cambridge University Mathematical Laboratory from that of the problem that I set about to solve. This separation does not correspond to any separation in time of the impressions that are the basis of the descriptions.

Impressions of the Cambridge University Mathematical Laboratory.

The "Math. Lab." that I got to know was a place of friendliness, helpfulness, and a keen openness to anything that might contribute to the exciting development that was taking place. Among the senior staff I got mostly in touch with M. V. Wilkes and J. C. P. Miller. Among the junior staff and users of the computer my primary contact was with Eric Mutch, Stan Gill, David Wheeler, Tony Brooker, and Sandy Douglas. In February 1951 a lot of orderly organization and documentation around the use of the Edsac had already been established (cf. [2]). For me personally this proved to be fully adequate so as to enable me to go ahead with the solution of my problem. Even so great improvements were made during the period from 1951 to 1953.

First, the Edsac itself. It was developed as an experimental model, and looked like one (cf. [20]). When I first used it in February 1951 the mercury delay line store was still openly exposed to the environment. This made the correct operation of the store quite dependent on the stability of the room temperature. Consequently it was necessary at short intervals to run a store test and to adjust the basic pulse frequency. This basic weakness of the machine was removed when the store was placed in a closed box with thermostatic control.

Even with the improved temperature control the store was a source of frequent trouble. In 1951 it consisted of 16 delay lines, each holding 32 words of 18 bits, making a total of 512 words. But as a safety precaution, the experienced user would arrange those programs that were to run for long periods to use only 15 delay lines. Before embarking on a long run with such a program, in particular before a long night, one would use a store test that indicated the location of the delay line that first failed and one would then interchange the wires connecting the store control with the actual delay line unit so as to place the least reliable line where it would not be used. The insight into how to do this was gained by word of mouth from one who already knew.

The state of the machine was frequently tested by several test programs that were always lying around as five-hole paper tapes. One test was a prime number calculation that had proved particularly sensitive to the state of the machine. It ran for a few minutes and then produced some numbers that often would be found to be incorrect, and thus indicated that something was amiss. No one had any idea of how to analyse what went wrong in this program [21].

When I first arrived in February 1951 the original, mechanical paper tape reader had recently been replaced by a much faster, photoelectric reader. This worked quite well. The output was still only to be had via a mechanical teleprinter, at a rather low speed. While I was around, I think during the summer of 1951, this was replaced by a tape punch. At the same time the check of output was modified, as follows. The original scheme was to have an echo back from the output unit, which then could be checked by the program. With the installation of the punch the coding was changed so that the decimal digits all were represented by patterns having just two holes in the five positions. In this way any single-hole error would change a digit into a non-digit or vice versa. Thus the most dangerous errors, those that change a digit into another one, would only be caused by two-hole errors. This proved to be a satisfactory solution of the output check problem.

The change to output on paper tape increased the speed of output considerably. In addition it made it possible to use the machine to produce results that later were used as input. This was of decisive importance to my application.

Already when I first started using it, in February 1951, the Edsac was supported by an excellent library of subroutines and other programs. This contained subroutines for the basic mathematical functions, often in several versions offering different advantages. Further, the library contained routines for input and output. But as perhaps the most remarkable feature, the library contained a wide variety of diagnostic routines. These covered primitive post-mortem programs that could be applied in an improvised manner at the machine, in addition to tracing routines and programs for monitoring single locations of the store during program execution. The library was very well organized and included systematic documentation. I used many library routines and always found them to work correctly. The most serious weakness I ever found in a library routine was a pair of superfluous instructions in the innermost loop of an input routine.

The most basic documentation for programmers had been collected to form the "Report on the Preparation of Programmes for the Edsac and the Use of the Library of Sub-routines" with contributions from J. M. Bennett, R. A. Brooker,

S. Gill, D. R. Hartree, E. N. Mutch, B. Noble, J. P. Stanley, D. J. Wheeler, M. V. Wilkes, and B. H. Worsley [18]. This existed as a book of sheets that had been produced by means of a spirit duplicator. Later this report was published in the USA [22]. It describes the machine language, including the set of "initial orders", a program of 40 orders written by David Wheeler that in a marvelously flexible manner would translate the external instruction language into internal form. Addresses had to be written relative to one of a handful of base addresses that remained symbolic until the actual program input time (cf. e.g. [2]). Based on this machine language the main bulk of the report gives the full documentation of the library of subroutines as it existed at that time.

The machine language and library organization for the Edsac were very advanced at the time. Several years later, in 1956, when the first Danish computer Dask was being developed as a modified copy of the Swedish computer Besk, I found that a major step forward would be achieved just by taking over these ideas directly (cf. [1]). On the other hand, the very effectiveness of this approach made the staff at the Mathematical Laboratory less open to the further step of an algebraic notation than it might otherwise have been. Thus I remember an incident that happened in the summer of 1951. I met David Wheeler in the hall of the Mathematical Laboratory and said to him: "What about allowing the user to express himself mathematically, so that he could write: sin(x), cos(x), etc.?" His reply was: "That is unnecessary since we have the library of subroutines.".

In agreement with the strong stress on diagnostic work and on the use of the library of subroutines, the Mathematical Laboratory possessed a variety of paper tape handling equipment, installed under favorable circumstances, actually a far wider range of machinery than I have ever encountered elsewhere. In addition to punching machines there were tape comparators, several types of copying machines, and, as the most elaborate device, a tape merger. The presence of these machines supported careful tape preparation, based on double punching and subsequent check comparison of all input. The most serious shortcoming was that there were no printing devices attached to this equipment. All work was concerned with the five-hole tape directly. In this medium it was not possible to carry informal comments along with the instructions.

The activity around the use of the Edsac was controlled by an administrative system that struck a fine balance between systematic order and informality. During day-time the machine was handled by operators from the administrative staff. They would run jobs submitted to a simple queue. At night the machine could be run by one of the "fully authorized" users. In addition, the "partially authorized" users were permitted to use the machine as long as there was a fully authorized user around who would be in charge. The special responsibility of the "fully authorized" user was to make sure that the machine would be switched on and off properly and to be ready to take sensible action in emergencies, such as fire breaking out. I myself was quickly authorized partially, and was promoted to the full status when I began to require long night runs on the machine.

The development of an Edsac program.

The program development described below mostly took place in a first phase from 1951 February 20 to June 24 and a second phase from 1953 May 25 until August 10. The astronomical aspects of the programs, including the problem formulation, the method of solution, and discussions of errors and limitations, are described in two papers that correspond to the two phases, referred to as the 1951-paper [8] and the 1957-paper [13]. In the following description the stress will be on the process of learning and understanding the new activity of programming that the work on the programs involved. The dates of events given in these sections are derived from my diary notes made at the time.

As mentioned above, when I started to work with the Edsac I had no direct experience with programming. Getting in contact with the director of the Mathematical Laboratory, M. V. Wilkes, I was very well received, was referred to Erich Mutch as my daily contact, was assigned a desk, and was given my copy of the "Report on the Preparation of Programmes for the Edsac. . .". And so I set going on my problem. As a start I decided to work out and test a subroutine for the central function of my problem, $x^{-3/2}$, the function that will convert the square of a distance between two attracting bodies, r^2, into the inverse cube of that distance, r^{-3}. The inverse cube is needed for converting the vector going from one body to another $(x_2-x_1, y_2-y_1, z_2-z_1)$ into an attraction vector according to Newtonian law of gravity. I started work on this subroutine on 1951 February 20 and was immediately thrown into problems of scale factors, as described in the 1951-paper. I worked on the subroutine also while away from Cambridge from February 26 to March 6. Immediately after my return, on March 7, I punched my first program and before lunch had my first try on the Edsac. Eric Mutch happened to be around at the machine and helped. After a few moments some meaningless characters came as output. Disbelievingly I suggested that the machine might be at fault. Obligingly Erich Mutch put the program through again, with precisely the same result. After that it did not take him long, at glancing at my program, to see what was amiss: I had not understood the primitive way in which the input and output instructions worked; for normal purposes they would be used only in the context of routines from the library. I had my next try the next day, and again found that I had missed something. On the third day, March 9, the test of my $x^{-3/2}$-subroutine worked correctly. Although this made me go ahead with the other parts of my problem, I continued to experiment with that subroutine during the following months, to test its accuracy, to improve its speed, and to save instructions. The routine is based on an iteration and I played about with the starting value and with the stopping criterion. I remember distinctly a situation during a lunch hour when running a test of this routine where I tried to use exact equality of two successive iterands as my stopping criterion, getting output of each approximation. J. C. P. Miller was also present and I explained him what I was doing. When the output started coming out, without convergence, he immediately identified the trouble, saying:

"It swings!" I eventually settled on using a fixed number of iterations in this routine, cf. [8].

During the days following March 9 various parts of my program became ready and were tested in rapid succession. Around March 20 I had a first version of the complete program working. There were three main areas where the problem gave rise to development effort, namely the start of the integration, the input of coordinates of the major planets, and storage economy. As to the first of these, the handling of the start of the integration process caused special problems because, for reasons given in the 1951-paper, I used a fixed-step method based on difference tables. This made a special treatment of the start of the process necessary. In the orbit integration problem at hand this is not too unnatural for the following reason. Mathematically, the start of an integration requires a set of three coordinates and three velocity components of the planet to be given. The astronomical interest is entirely confined to the coordinates, however, while the velocity components remain internal working quantities of the process that are not produced as output. This means that what is available as basis for the start of an integration is several sets of coordinates for successive time steps. This makes the special start procedure comparatively simple.

In the 1951-solution [8, 11] the special start was done by a separate program that would place the initial values in the appropriate locations of the store and then stop. The operator would then have to read in the normal integration program in a separate step. This procedure was inconvenient because both parts of the program also needed access to a tape supplying the coordinates of the major planets. In the 1953-version of the program the start was arranged as a modification of the regular integration program, realized by means of a few jump instructions inserted instead of certain regular instructions. The start procedure was arranged to remove these irregular jumps as it went along, so that finally the regular program would be left without any trace of the special start procedure. The details of this technique emerge clearly from the manuscript documentation of the program.

The second major area of design development of the integration program was the handling of the coordinates of the major planets that had to be supplied as input. For each integration step covering 20 days of motion of the planet, data had to be provided corresponding to 7 major planets each having 3 coordinates of 4 to 7 decimal digits. In 1953 I wanted to compute the motion of a planet over 100 years. The preparation of planetary coordinates for 1800 dates on paper tape, in a suitable form and without errors, required a substantial development effort. In the first version of the integration program the major planet data were used in precisely the same way as in the hand calculations with which I was so familiar. As the basis for this work the Nautical Almanac Office had already several years before published an excellent collection of tables, Planetary Coordinates for the Years 1940–1960 [16]. These tables contained not only the rectangular coordinates of each major planet, but also the so-called indirect acceleration term of the equations of motion, which depends only on the coordinates of each major planet. Including the indirect acceleration term as input to the integration program meant that six, and not only three, numbers had to be supplied for each major planet for each date. A special problem was created because the Planetary Coordinates included a factor depending on the interval of integration in the indirect terms. Finding ways of handling these indirect terms and their interval-dependent factors became a major part of the difficulty of establishing the first version of the integration program. It is probably characteristic of the programming activity that no sooner did I manage to get a solution of these problems to work correctly than it occurred to me that the supply of these terms as input was entirely unnecessary: they could readily be computed from the coordinates of the major planets, mostly using program logic that was available anyway. The moment of realization that in this way half of the input became unnecessary was the happiest one of many joyful moments of this program development. Luckily this realization occurred sufficiently early in the project to allow large overall savings.

The realization of the revised handling of the indirect acceleration terms was about to be ready when the Edsac was closed down around Easter, 1951 March 23–26. During the Easter days I could therefore do nothing better than check my new program version carefully by means of desk methods. This proved to be a highly profitable activity; when finally I got on to the machine on the day after Easter the revised program worked immediately. This positive experience has continued to influence my attitude toward methods for avoiding program errors ever since.

The preparation of planetary coordinate tapes became a major separate effort, in magnitude surpassing what data preparation had so far been done at the Mathematical Laboratory. Fortunately my previous experience in astronomical calculations gave me a solid basis of methods and understanding of the difficulties. In the final, large-scale effort during the summer of 1953 the steps were as follows. First, separately for two groups of the seven major planets, the coordinates for a suitable period were punched twice independently. The two versions were compared by means of a tape comparator, and a corrected third version produced, partly by gluing together the parts of the two punched versions. The corrected version was then checked by differencing, using the Edsac, and remaining errors corrected. In a further run on the Edsac the tape was again checked by differencing, at the same time as a copy of the tape, with a check sum for each date added, was produced as output. When the tapes for each of the two groups of planets had been prepared in this way they were finally merged into one tape, using the separate tape merger. When the final tape was read during integration runs, the check sums were verified.

This procedure for preparing the planetary coordinate tapes made use of auxiliary Edsac programs for checking higher order differences and for forming check sums. The lack of suitable published data for the planet Mars for the years

1858 to 1919 made one further auxiliary program necessary. This program converted input consisting of the longitude and radius vector of the planet at 100 days' interval into rectangular coordinates at 20 days' interval, with the aid of interpolation and coordinate transformation [12].

I executed this production process for more than 200 000 final decimal digits single-handedly during June and July 1953. This was tiresome work, but the design of the production process proved effective, progress was steady, and I was rewarded by the fact that the final integration runs ran smoothly and produced results that satisfied a strict difference check. Actually, during the long nightly integration runs with the Edsac in late July 1953 it happened just once that the machine stopped because the sum check on the input failed. As planned in the design of the program I moved the tape back one block and let the machine read the same data once more. Again the check failed. So I picked up the tape, looked at it closely, and found an irregularity in the paper looking like a small round window, situated in the position of a hole. I decided that this was likely to be the cause of the trouble, so I took my pen and covered the little window thoroughly with an ink spot. On reading the same block again the machine continued smoothly; the trouble had been cured.

At about the same time, one evening when my program was running merrily Maurice Wilkes came by to see how things were going. I gave him a brief summary of the progress and still remember the essence of his response, spoken with a smiling glance at the maze of racks and overhead wires that filled the room: "So this is now a model of the Solar system! Let this be a warning against drawing conclusions from the external appearance of our models!"

The third major area of concern in the design of the integration program was storage capacity. It goes without saying that in a project like mine the capacity of the store of the Edsac was soon felt as a limitation. In 1951 the wise program designer only used 480 locations. Under such pressure I started to look round for ways to economize on store for program. In a program like mine, where a large part of the processing has to be done three times in sequence, once for each of the three coordinates of the planets, the idea lies close at hand to do this by means of the same program text. The Edsac had no built-in address modifications of any kind, however. As the way out I developed a subroutine that would control any piece of program given to it as parameter, in such a manner that it was executed the proper three times, with the appropriate address modifications done in between. This subroutine is remarkable for the fact that it handles its parameter according to the call-by-name mechanism of Algol 60. The documentation of this subroutine still exists and has been described separately [14]. It was used until June 1951. When I came back in 1953 the store of the Edsac had been doubled so that I could avoid this space-saving but time-consuming device.

In any case the speed of the program was highly gratifying. From many hours of work I knew that when using a desk calculator one integration step could at best be accomplished in about two hours. Using the Edsac this time was reduced, in 1951 to 45 seconds, and in 1953 to 22 seconds. This latter figure was particularly significant since in July and August 1953 the Edsac was so reliable that nightly faultless runs of several hours were possible quite regularly.

Professional reactions to the integration program.

The successful operation of the integration program was greeted with warm recognition by several astronomers in the field of celestial mechanics. Mr D. H. Sadler of the Nautical Almanac Office, Royal Greenwich Observatory, followed and supported the work from the early stage, and on June 7, 1951, came to Cambridge to see the program at work. Dr G. Merton of the University Observatory, Oxford, expressed his enthusiasm and immediately arranged that I use the program to check a hand calculation of the motion of a comet through 8 years, in an attempt to clarify earlier controversial results [4, 5].

With this encouragement I wrote a paper about the methods used in the program. This was finished in a first draft on 1951 June 22 and was accepted for publication in the Monthly Notices of the Royal Astronomical Society on October 8 the same year [8]. In addition I prepared a more detailed description of the way to use the program, as a documentation of the set of program and planet tapes that I left behind in the program library of the Mathematical Laboratory after going away in June 1951 [11].

In September 1951 I had a letter from Professor J. H. Oort, director of the Leiden Observatory [15]. He had learned about my program from Dr Merton and inquired into the possible use of it for calculating the motion of comets. In my reply [9] I stressed the practical problems of preparing the planet tapes. At the same time I could tell about my owns plans for large scale use of the program for calculating the motion of minor planet 51 Nemausa during 100 years. At that time I hoped to be able to go ahead with this project during December 1951 and January 1952 [10].

My plans for a large-scale use of the program had to be postponed because of extensive rebuilding of the Edsac which was done during the summer and autumn of 1951 and the consequent accumulation of work for the machine [19]. Eventually my project was carried out after I had been to the USA for a year, during the summer of 1953. This postponement most likely is the reason why the plan to use the program for calculating orbits of comets was not realized.

Concluding comments.

If general lessons are to be derived from the previous observations of the development of an application program for the Edsac, the first would be related to the active development at the Mathematical Laboratory of supports for the computer, in the areas of subroutine organization, building up a library of programs, and aids to programming error diagnosis. These supporting developments were of great importance to the success of the application program development and may be seen as an early demonstration of the significance of software as a component in computer applications.

As a second point, it is clear that the success of the application program depended significantly on a close attention to the best use of the limited storage capacity of the computer and to such checks and restart facilities as made it possible to make progress with the application in spite of a high frequency of hardware failures. The lesson is that even grave limitations and imperfections of the computer may be overcome if they are faced squarely and counteracted actively during the program design.

As the third point, the brevity of the time span, one month, from the start of the project on the basis of no computer experience to the successful completion of the first version of the application program, deserves some comment. This time span reflects the transfer to the computer of a problem that already was completely algorithmic, although not expressed in a formal algorithmic notation, by someone who was thoroughly familiar with it. The brevity of the time required for the transfer may be taken as a measure of the simplicity and effectiveness of the computer and software facilities established by the Edsac-group, particularly in relation to computational problems in science.

Acknowledgements.

In preparing this report I have received helpful criticism from M. V. Wilkes and M. Campbell-Kelly, which I gratefully acknowledge.

As a whole, the report should be taken as a modest tribute to the group of people centered around the Cambridge University Mathematical Laboratory during 1951–1953, to their outstanding scientific and technical contribution and to their spirit of openness and helpfulness that to the author remain an unforgettable inspiration.

REFERENCES

1. Chr. Andersen, N. I. Bech and O. Møller 1958. *Lærebog i kodning for Dask.* Regnecentralen, Copenhagen.
2. M. Campbell-Kelly, *Early programming activity at the University of Cambridge.* Annals of the History of Computing, Vol. 2, No. 1, (1978), 7–36.
3. D. R. Hartree. Letter to P. Naur. 1950 July 27. 1 page.
4. G. Merton. Letter to P. Naur. 1951 April 10. 2 pages.
5. G. Merton. Letter to P. Naur. 1951 July 30. 1 page.
6. P. Naur, *The orbit of comet du Toit-Neujmin-Delporte.* Det kgl. Danske Videnskabernes Selskab, Mat. fys. Medd. Bind XXII, nr. 1 (1945).
7. P. Naur. Letter of application to Cambridge University. 1950 March 12.
8. P. Naur. *Computation of special perturbations by an electronic calculator.* Monthly Notices of the Royal Astronomical Society, Vol. 111, no. 6, (1951) 107–116.
9. P. Naur. Letter to J. H. Oort. 1951, Oct. 5. 1 page.
10. P. Naur. Letter to M. V. Wilkes. 1951. Oct. 14. 3 pages.

11. P. Naur. *The motion of a minor planet or a comet.* EDSAC Library Specification Sheet, University Mathematical Laboratory Cambridge, June 1951, 7+4 pages.
12. P. Naur. *Mars-coordinates from L and r.* Hand-written notes. 1953. 5 pages.
13. P. Naur. *Minor planet* 51 *Nemausa and the fundamental system of declinations.* Thesis, Copenhagen University. (1957), 67 pages.
14. P. Naur. *A vector-handling subroutine using call by name written for EDSAC in* 1951. (1977) 12 pages.
15. J. H. Oort. Letter to P. Naur. 1951 Sept. 26. 1 page.
16. *Planetary Coordinates for the Years* 1940–1960. H. M. Nautical Almanac Office, H. M. Stationery Office, London (1939).
17. G. Stracke. *Bahnbestimmung der Planeten und Kometen.* Springer, Berlin (1929).
18. University Mathematical Laboratory Cambridge. *Report on the preparation of programmes for the EDSAC and the use of the library of sub-routines.* Sept. 1950. 41+26+39+11 pages.
19. M. V. Wilkes. Letter to P. Naur. 1951 Oct. 19. 1 page.
20. M. V. Wilkes. *Early computer developments at Cambridge: The EDSAC.* The Radio and Electronic Engineer, Vol. 45, no. 7, 1975, pp. 332–335.
21. M. V. Wilkes. Letter to P. Naur. 1978 Oct. 11. 1 page.
22. M. V. Wilkes, D. J. Wheeler and S. Gill. *The preparation of programs for an electronic digital computer, with special references to the EDSAC and the use of a library of sub-routines.* Addison-Wesley, Reading, Massachusetts (1951).

DATALOGISK INSTITUT
SIGURDSGADE 41
2200 KØBENHAVN N
DENMARK