[219] The successive minima in the geometry of numbers and the distinction between algebraic and transcendental numbers, *J. Number Theory* **22**, (1986), 147–160 **[Math. Rev. 87d:11043]**
[220] On two analytic functions, *Acta. Arith.* **XLIX**, (1987), 15–20 **[Math. Rev. 89f:30006]**
[221] The representation of squares to the base 3, *Acta Arith.* **LIII**, (1989), 99–106

Appendix II

Fifty Years as a Mathematician II
Kurt Mahler

Abstract

Kurt Mahler FRS, FAA died on 26 February, 1988 after a long and distinguished career devoted primarily to the theory of numbers. The present material is copied from a typescript entitled 'Fifty Years as a Mathematician' written principally in 1971. Remarks [appearing as insertions in the text] are his subsequent handwritten additions. I have made no changes to the manuscript {other than for a few notes} and the minor change in title; in particular, I have adhered to its punctuation and capitalisations. The interested reader will also want to consult Mahler's biographical articles: 'Fifty Years as a Mathematician' (*J. Number Theory* **14** (1982), 121–155)—a rather more mathematical version of the present notes; 'How I became a mathematician' (*Amer. Math. Monthly* **81** (1974), 981–83); and an essay written in the twenties: 'Warum ich eine besondere Vorliebe für die Mathematik habe' (*Jber. d. Deutschen Math.-Verein.* **85** (1983), 50–53). *A. J. van der Poorten* (Macquarie University)

As I have reached the age of 68, it is perhaps appropriate to look back at earlier times in my life and to my occupation with mathematics during these many years.

My family had no academic traditions or connections. All my four grandparents came from small places in the Rhineland in Western Germany, and none of them went to more than an elementary school (*Volksschule*). As Jews they had additional difficulties in the old Prussia of the nineteenth century.

My father and several of my uncles went into the printing and bookbinding trade, beginning at the bottom as apprentices and saving slowly enough money to start small firms of their own early this century.

My parents had eight children, but as was usual in those days four of them died young. My twin sister and I were born in 1903 and were the youngest children. There was also an elder sister who at present is still alive and with her husband and one daughter lives at Venlo in the Netherlands [Lydia Krohne died at the age of 89 in 1984; her husband died about 10 years earlier, but her daughter is still alive in 1986]; and there was an elder brother who with his wife disappeared in a Nazi concentration camp during the second world war.

Like my father, we four surviving children were avid book readers, in a somewhat omnivorous sense. My brother became a printer like his father {Josef joined and eventually took over his father's firm.}, and my elder sister, who was very musical and wanted to become a singer, ended by marrying a printer who was also a musician. My twin sister was perhaps the most capable of us four, at least in business matters. She was excellent at languages, spoke and wrote French and Spanish, and repeatedly visited South America. She finally married into a dry-cleaning firm in Thuringia in Central Germany and made it prosper, but she died in 1934 {Elsewhere, Mahler describes Hilde as being driven to suicide by Nazi persecution.} and her only daughter in 1963.

My sisters and my brother were physically healthy and strong. I was not, but from early childhood suffered from a surgical form of tuberculosis in the right knee. In those days there was no effective treatment of this disease, and it was not healed until I was 35 when the knee cap, after several earlier operations, was removed successfully. Due to my illness I went only for a total of four years to school; for an additional two years I had to stay at home and received some elementary lessons in the three R's. At this time the opinion of my private teacher was that I was "not stupid, but lazy".

I left the elementary school shortly before I was 14 and for the next two years attended day courses at two elementary technical schools as my parents thought that I should try to become a fine-mechanic {This is an over direct translation of 'Fein Mechaniker' which Mahler translates elsewhere as 'precision mechanic'. The point was that this was a job that could be done sitting down.}. However, these technical schools gave me the first instruction in elementary algebra and geometry, and I immediately decided that this is what I really liked. So, during the summer vacation of 1917 when I had just become 14, I bought a logarithm table and enjoyed myself doing problems, and shortly afterwards taught myself some plane and spherical trigonometry. I next acquired a good book on analytic geometry and, in 1918, began with differential and integral calculus. About this time I entered a machine factory in my home town Krefeld as an apprentice, working first for one year at their drawing office and then for not quite two years in the factory itself. The reason for this change of plans was that an apprenticeship of this kind perhaps might enable me to go to a technical university (*Technische Hochschule*) for the study of mathematics. Such a study would not require the very difficult University Entrance Examination (*Arbiturienten-Examen*). Actually, as I shall explain, I did in the end pass this examination!

When returning home from the factory, during my period at the machine factory, I attended evening classes on technical courses, and these included some more elementary mathematics. However, most of my free time I spent on my mathematical readings. I bought a good course on advanced calculus (Césaro-Kowalewski, *Infinitesimalrechnung*) and the first volume of

Pascal's *Repertorium der Mathematik.* From the first work I learned roughly the contents of a first year honours algebra and calculus course, and from the second one, which gave only results and no proofs, and was very hard reading, I learned about groups, matrices, invariants, and other topics. The next books I acquired were some volumes of Bachmann's *Zahlentheorie*, Landau's *Primzahlen*, Knopp's little *Funktionentheorie*, and the Klein-Fricke volumes on the *Ikosaeder*, *Modulfunktionen*, and *Automorphe Funktionen*; further *Hilbert's Grundlagen der Geometrie* and advanced texts on Analytic geometry. Thus I became acquainted with the different types of non-euclidean geometry. Naturally, at this time I had none to help me with those studies. But I met fellow students at the evening classes, and I tried to teach one or two of them some of the pretty things which I had just learned and hoped to understand better by talking about them!

The great moment came about 1921. My father, without my knowledge, sent some of my mathematical attempts to the director of the Realschule in Krefeld Dr. J. Junker who was then about 50. He had originally been a student of Christoffel and had obtained his Ph.D. in the 1890's with a thesis in invariant theory. Dr. Junker immediately decided to help me. He repeatedly sent my attempts in mathematics to Felix Klein in Göttingen who gave them to his assistants for a report. C. L. Siegel, who at about this time began his great career in Göttingen gave the first report and suggested that I should be helped to pass the university entrance examination and so be enabled to go to the university.

So I left the factory and for the next two years took lessons in certain required subjects, in particular German, French, and English. These lessons were given by teachers at Dr. Junker's school and naturally were a great help. They did not stop my private studies in mathematics which by now dealt with subjects at the university level.

The Prussian government which was very liberal at this time allowed me to take the examination in my home town Krefeld. I did so in the fall of 1923, during one of the worst periods of the big German Inflation and while there was a lot of political trouble due to the Ruhr occupation. (Needless to say that I was at this time and long into the 1930's still a very patriotic German!) There were just two of us external students who took the examination. It lasted from Monday Morning to Friday Afternoon and covered in written papers the three languages, mathematics, and physics; and on Friday afternoon there was an additional oral in these and some other subjects. My fellow sufferer was then told that he had failed, but I just scraped through!

Thus, when I had reached the age of 20, I was able to go to the university. I had originally intended to study in Göttingen. But at about this time Siegel had become professor at the university of Frankfurt, and he invited me to come there for my first semesters. Frankfurt had the advantage of having only few students, while the mathematical faculty consisted at this time of Dehn, Hellinger, Epstein, Szass, and Siegel, all distinguished in their subjects. Also the department at this time was the only German university to hold a weekly seminar on the history of mathematics.

I well remember my first semester. I took the second part of calculus given by Siegel (I never took the first part, but gave it myself later many times at Manchester); a very difficult course on *analysis situs* (topology) by Dehn; elliptic functions by Hellinger; and some non-mathematical lectures which I liked much less. In addition I took part in the historical seminar [History of Mathematics], and also in a seminar on *Kreisteilung* (cyclotomics) where I in fact gave several of the lectures. It was at this time the rule that every member of the Seminar had to write a short paper for the Seminar Book of the Frankfurt Mathematical Department, and so several of my contributions can be found in it. [This book unfortunately was lost during the Second World War. Copies are with the Hebrew University in Jerusalem.]

After Easter 1925 Prof. Siegel went on leave for foreign travel and studies. During the three semesters till then he had given me much personal help and supervision and taught me methods useful for my research. I therefore went in the summer of 1925 to the University of Göttingen and stayed there in fact until the Nazi revolution of 1933.

Göttingen was then still the centre of world mathematics. Klein died shortly after my arrival. But Hilbert was still working and lecturing, and there were Landau, Courant, Emmy Noether, Ostrowski, and at a slightly later date Herglotz and Weyl. I had little contact with Landau at this time since he was then giving a calculus course for beginners. At a later date I belonged to the group of advanced students who helped with his students, and he became more interested in my work. Our personal relations were always friendly, and I enjoyed parties at his house.

Herglotz gave the most perfect lectures, usually on subjects on the boundary between pure and applied mathematics. It was difficult to add anything to what he said about his problems; so he had few research students.

Quite different were the lectures by Courant. I found them rather vague and was never quite sure whether he had in fact proved his assertions. However, I learned from him about the direct methods in the calculus of variations, and many years later this enabled me to apply analogous ideas in the geometry of numbers of general point sets.

I learned also much from Emmy Noether. Her lectures were usually badly prepared, and she spoke much too fast and wiped the blackboard far too often. Most of the time I could not understand her during her lecture, and I had to allow her ideas to simmer in my head for quite a period before

I could understand them. But I learned from her about general fields and in particular the p-adic field, and was in later years to apply these notions many times. I never took to the axiomatic method; it seemed to be so much more fun to deal with naturally occurring rather than constructed problems in mathematics.

During my stay in Göttingen, there were many foreign visitors, topologists like Alexandroff and Hopf, algebraists like van der Waerden and A. Weil, number theoreticians like Schnirelmann and Gelfond, and many others. Let me mention in particular Norbert Wiener because I acted as his (unpaid) assistant in 1926 when his German was not yet as good as in later years {A result of this assistantship was Mahler's first paper [1] which appears, in English! as an appendix to a paper of Wiener}; L. J. Mordell who was to play such an important role in my life; and in the early 30's my dear friends J. F. Koksma and J. Popken from the Netherlands.

I became very ill in 1927 and had one kidney removed. This kept me at home in Krefeld for the summer semester of 1927 and continued to trouble me for several more years. The illness delayed my Ph.D. examination for several months, until it took place in December 1927 at the University of Frankfurt. The thesis {'Ueber die Nullstellen der unvollständigen Gammafunktionen' with Referent: Prof. Dr. O. Szász and Korreferent: Prof. Dr. C. Siegel; dedication: Herrn Professor Dr. Josef Junker in Dankbarheit und Verehrung gewidmet} dealt with the zeros of the incomplete Gamma Functions, thus with the zeros of a class of functions tending to a function without zeros. Ostrowski was not very impressed with this work and told me to do less easy mathematics. I hope that I have followed his advice at least in some of my later research.

While I was ill at home in 1927, I succeeded in proving the transcendency of $z + z^2 + z^4 + z^8 + \cdots$ for algebraic z satisfying $0 < |z| < 1$. The method was new and depended on the functional equation $f(z) = z + f(z^2)$ for the series. Several papers of mine generalised this result, and I could in particular establish the transcendency of $\sum_{n=1}^{\infty} [n\alpha] z^n$ for all real quadratic irrationalities α and all algebraic numbers z satisfying $0 < |z| < 1$; in fact, all derivatives of this function of z have the same transcendency property.

E. Landau did not show much interest in these results. So I next turned to a closer study of the approximation properties of e and π. I found my classification of the transcendental numbers into the three classes S, T, and U. It allowed me to prove that none of the numbers e, π, or $\log 2$ can be algebraically dependent on a Liouville number. This settled a question which Perron had asked in his book '*Irrationalzahlen*'. It was also this work which brought me into close contact with Koksma and Popken when they spent a summer semester in Göttingen in the late {This must be a mistype for 'early'.} 1930's.

While still in Frankfurt, I learned from Siegel his proof and improvement of Thue's theorem on the approximation of algebraic numbers, and I had studied his earlier papers on this subject and also his great Academy Paper on lattice points on algebraic curves. I naturally was also acquainted with the treatment of Thue's theorem in Landau's *Vorlesungen*. I think it was during the Whitsun holidays of 1930 when bad weather forced me to stay at my lodgings on a small North Sea island that I got the idea of establishing an analogous result for p-adic numbers, for I had at last learned how to handle them. I could easily extend my classification of S, T, and U-numbers to p-adic numbers, but at that time was not yet able to decide whether the p-adic exponential function e^z, assumed to be convergent, was transcendental for algebraic $z \neq 0$.

In the autumn of that year I attended for the first time a meeting of the Deutsche Mathematiker-Vereiningung and there reported on these p-adic problems and what I had so far been able to prove. Not long afterwards I learned how to generalise my p-adic analogue of the Thue-Siegel theorem to the case when roots of the same algebraic equation in several of the p-adic completions and in the real completion of the rational field are approximated simultaneously by the same rational number. This leads to results like the following one. If $F(x, y)$ is an irreducible binary form of degree at least 3 with integral coefficients, then the number of integral solutions x, y of $F(x, y) = k$, $(x, y) = 1$, does not exceed c^{t+1}, where $c > 0$ does not depend on k, and t denotes the number of distinct prime factors of k. Siegel called these results "schön und wertvoll", and I myself considered them as my first important contribution to mathematics.

I lectured on these p-adic results before K. Hensel in Marburg who was very pleased to learn of a new application of his p-adic numbers; and I gave similar lectures in the seminar of I. Schur at Berlin. During this visit to Berlin, von Papen came to power, so preparing for Hitler.

It was during these last years at Göttingen, shortly before I reached the age of 30, that I had my first income (until then, my parents and even more members of the Krefeld Jewish Community had helped me with my studies). I was given a research fellowship by the Notgemeinschaft der Deutschen Wissenschaften of not quite \$500 for a little more than two years. This was for me a very large income, and I could save some of it for the coming troublesome years. Also, about the end of 1932, I was appointed to an assistantship in the Mathematics Department of the University of Königsberg in the far East of Germany. It was to start in the summer of 1933, but naturally I never took it up.

Hitler came to power in early 1933, and I knew at once that I had to find a home abroad. During the summer of 1933 I went for six weeks to Amsterdam for research and for discussions with Koksma and Popken and with van der Corput who had been their teacher. About this time Mordell obtained for me a small research fellowship and invited me to come to Manchester during the session 1933–34 which I did in September of that year.

During this first visit to Great Britain my English was still somewhat defective. However, immediately on my arrival I was put before a blackboard and told to give a seminar lecture, a drastic but helpful method! I still have a suspicion that my listeners suffered even more from the lecture than I did! Until then, I only had a reading knowledge of English. Now I slowly learned to speak and understand it, and even to understand the language as spoken by the Mancunians.

During this stay at Manchester I learned that, thanks to van der Corput I had been given a stipend by a Dutch Jewish group to enable me to work for the next two years at the University of Groningen in the East of the Netherlands. I went to Groningen first for a short visit in May to June 1934 and then stayed there from the fall of 1934 till the summer of 1936. I worked in the Department of Mathematics of Groningen University. I did mainly research, but I also gave during the two years a course on recent work in Diophantine Approximations and Minkowski's Geometry of Numbers. Regrettably, the rules of the university did not allow me to attend the Dutch lectures in the department without inscribing as a student; therefore I never learned to speak Dutch well, but only acquired a reading knowledge.

My research at Groningen dealt with pseudo-valuations; with the p-adic analogue of the Gelfond-Schneider theorem on the transcendency of α^β; with geometry of numbers and its applications to transfer theorems; and with the Taylor coefficients of rational functions.

Towards the end of my stay at Groningen I was run into by a bicycle rider. This was disastrous, for the tuberculosis in my right knee became again active. I was unable to walk for some time and had to stay at Krefeld. There I had several operations, and finally the right knee cap was removed. This had the desired effect of removing the infection; it died out completely in the next few years.

The period after the last operation was very painful, and for several months early in 1937 I was given injections of morphine. At the end of this time when the pain had lessened and the injections were stopped, I found that the drug had not affected my brain, for I could show the transcendency of the decimal fraction 0.1234... {That is 0.1234567891011121314... } and of infinitely many similar fractions. The proof depended on a generalisation of a theorem by Schneider on the rational approximations of algebraic numbers.

During the summers of 1937 and 1938 I spent three months each in a hospital for sun treatment of tuberculosis at Montana, Valais, Switzerland. This treatment probably contributed much to the final recovery from my disease.

Afterwards, in the fall of 1937, I returned to Manchester University at the invitation of Mordell to spend the remainder of my small fellowship. As events were to show, I remained at this university until 1963, a total of 26 years.

I depended originally on the small Bishop Harvey Goodwin Fellowship. However, shortly after my return in 1937, a post became vacant in the department and I was appointed for the session as a temporary assistant lecturer. For this the British Government had to give its permission because at this time there still severe restrictions on the employment of aliens.

Thus, at the age of 34, I started on my first lecture courses to British Undergraduates. My English at this time had slightly improved since 1933, but still left much to be desired. Fortunately there were few people round me who spoke German, and so I had perforce to speak, write, and learn to understand English and to acquire its rather difficult idiom.

The period from the fall of 1937 to the summer of 1940 and the end of the phony war was of great importance for the progress of number theory at Manchester University. Mordell's seminar on number theory included at this time for variable periods mathematicians like Davenport, Young, Erdős, Chao Ko, Lehmer, Billing, Zelinskas, and myself. All the new progress in number theory was discussed at the weekly seminar, and much was contributed to this progress by its members. The outstanding event was a series of papers by Davenport on the products of three linear forms in three integral variables; he established the exact minima when either all three forms were real, or when one form was real and the other two were complex conjugate. This was the first breakthrough in a more-dimensional non-convex problem in the geometry of numbers. Davenport himself of course used arithmetical rather than geometrical methods. But in 1940 Mordell found a geometrical approach to these questions and so could give a new proof for Davenport's results. In the next years Mordell could generalise these ideas and apply them to general classes of two-dimensional non-convex star domains. Davenport, who in the fall of 1941 had gone to Bangor, also made further important contributions to their theory.

Let me go back to 1938. When I had concluded my university session as a temporary assistant lecturer, I worked for a term again on research alone. But then, for the last two terms of the session of 1938-39, I once more had an appointment at Manchester as a temporary assistant lecturer. But between the fall of 1939 and that of 1941 I had no appointment and did research, while living on the balance of my small fellowship and on my savings from the two lecturerships.

At the summer of 1939 Chao Ko had just gone back to China where he was a professor at the University of Szechuan, and through his efforts I was offered a professorship at this university. China was of course at this time at war with Japan. I accepted the appointment; but my internment in the following summer and the war difficulties made the journey to Western China impossible. However, I became interested in Chinese and for the session 1938-39 had one hour a week instruction by the Reader in Chinese at Manchester University. I never learned to speak. But I acquired enough characters, grammar, and idiom, in the following years to be able to read a little. The reading matter consisted mainly of the old novels like the '*History of the three Kingdoms*', and of some history. In this way I obtained perhaps a slightly better understanding of the Chinese.

From June to September 1939, into the outbreak of war, I had a glorious vacation on a small island in the Scilly Isles west of Southern England. It was good for my health, and I also could do some research. In particular, I could at last solve a small problem in combinatorial analysis which had puzzled me since my Frankfurt days. Other research at this time and for the next few years dealt with special classes of Diophantine equations, with Hlawka's theorem on the product of two linear polynomials in two variables with complex coefficients, and with the related problem for quaternions, etc.

As I said already, I was unemployed during the first two years of the war and spent my time on research. Like most other German refugees I was interned for three months during the summer of 1940, a rather uncomfortable time. During the first six weeks, when it was very rainy, we were in a camp under tents at the border of Wales; but later we lived in empty boarding houses on the Isle of Man. Here a group of us former university lecturers and students got together and we opened a university for the interned! I myself gave a course on the construction of real numbers by means of Cauchy sequences of rational numbers. Later on, I started many first year honours analysis courses at Manchester University in this fashion.

After my return from internment in the fall of 1940 I began to work on Farey Sections in complex number fields. This work was completed only after the war with the cooperation by Ledermann and Cassels and published in a joint paper. At about this time I received an invitation as lecturer to the University of Cape Town where L.C. Young was then professor of mathematics. On his advice I declined this invitation.

For I knew then already that Davenport, who until 1941 was an assistant lecturer at Manchester, but who had been elected to the Royal Society before the war, was to leave for his professorship at Bangor. The British Government allowed my appointment to the vacant post as assistant lecturer, and

I remained on the staff of Manchester University from the fall of 1941 until that of 1963, successively as assistant lecturer, lecturer, senior lecturer, reader, and finally as personal professor. The professorship was the first such appointment to be made in the mathematics department of Manchester University.

I think it was about 1941 when, as a consequence of the work by Davenport and Mordell already mentioned, I myself became interested in the general geometry of numbers of non-convex sets. My first, rather long, paper on the subject dealt with the case of bounded two-dimensional star domains, and for these I gave a finite algorithm for finding their critical lattices and the lattice constant. I finished this paper while I was on Xmas vacation at a boarding house in a small village near Lancaster, and while there divided my time most agreeably between working, walking, and reading. On later visits I was never again as successful in combining these occupations!

I soon extended my results to the case of unbounded star domains, and this led logically to the study of lattice point problems for any n-dimensional star bodies and even more general sets. My compactness theorem for bounded sets of lattices was the main tool in these investigations and led to existence and other general results. This was perhaps the second time that I made an important contribution to mathematics. My method was soon adopted by other mathematicians, in particular by Davenport, C.A. Rogers, and I.W.S. Cassels, who all used it with great success.

When the war ended in 1945, I applied for British Naturalisation. The application was granted in 1946, and in 1948 I was elected to the Royal Society. At about this time, Mordell had already left for Cambridge, and M. H. A. Newman had become his successor as head of the Mathematics Department at Manchester.

The next year, from January to September 1949, I had my first leave from Manchester University. I made then my first visit to the United States. For the greater part of my leave I went to the Princeton Institute of Advanced Study. But during the summer, when the heat and humidity of Princeton became intolerable, I went for a few days to the University of Colorado, for a week to the University of California at Los Angeles, for six weeks to the University of California at Berkeley, and for three weeks to the University of British Columbia at Vancouver. At Boulder, Colorado, B. W. Jones invited me to come again the next year during the summer vacation and to give then a course on the geometry of numbers, before attending the International Congress of Mathematicians due to take place at Harvard in the fall of 1950.

I almost did not make it. For during the Xmas vacation of 1949–50 I contracted Diphtheria, and I returned to Manchester from the Netherlands suffering from this disease and all kinds of complications, including pneumonia. As a child I had of course never been immunised against diphtheria because this was not yet done at the beginning of the century. I was ill for some three months, but thanks to antitoxin, sulfonamides, and penicillin, I finally recovered. However, immediately afterwards I was attacked by a delayed consequence of the diphtheria, and I had to endure another three months suffering of peripheral neuritis which was almost worse and certainly more painful. Fortunately, I recovered from it just in time to make my proposed trip to the University of Colorado. I spent a most delightful summer there lecturing on geometry of numbers and making many trips into the Rocky mountains. Afterwards I went to the International Mathematical Congress at Harvard. This was the third international congress which I attended. For before the war I had already gone to the congresses at Zurich and at Oslo; in later years I was also to attend the congresses at Amsterdam and Edinburgh.

During the next seven years I stayed at Manchester without any long leaves. But I made a long visit to the Hebrew University in 1951, to the University of Göttingen in 1952, to that of Brussels in 1953, and to that of Vienna in 1954, always during the vacations. During the long vacations in these years I usually went for up to 7 weeks to some seaside place in Great Britain, later in particular to Herm Island in the Channel Isles. In those days this island was still very quiet and unspoiled, but as I found out this year on a short visit, it is no longer so.

My research during these years dealt with various questions. I mention a generalisation of Siegel's theorem on lattice points on algebraic curves, and some general results on the geometry of numbers of compound and associated convex bodies. The latter have recently been applied to simultaneous rational approximations of algebraic numbers by W. Schmidt.

Then, during the spring and fall of 1957, I was once again granted a long leave by Manchester University. For the first two months I had an appointment as traveling lecturer for the Mathematical Association of America, visiting a great number of colleges and universities in the Eastern half of the U.S.A. The summer I spent once again at the University of Colorado, and during the fall I was for the first time at the University of Notre Dame. I lectured at both places on the p-adic generalisations of Roth's theorem on the rational approximations of algebraic numbers. By early 1959 I had collected these lectures into a little book which finally appeared after a long delay in 1961 at the University of Notre Dame Press (*Diophantine Approximation* I; the second part has not so far appeared and probably never will!).

At Notre Dame I found a friend in A. E. Ross who was then the head of the Mathematics Department. On his invitation I went during several summers in the next years as a visiting professor to Notre Dame, until 1963 when I left Manchester for Australia, and Ross soon after went as head of the Department of mathematics to the Ohio State University at Columbus.

Since 1938 I had, except for a short interruption in 1940–41, been living at Donner House, Fallowfield, Manchester. This was a very comfortable house for Staff, Research workers, and Visitors, of the University of Manchester. It was lying among trees and not far from a pretty park, and it had the great advantage of bringing together members of different departments of the university. But towards the end of the 1950's the University decided to tear down Donner House and use the grounds for Student Halls (which turned out to be rather ugly and uncomfortable!). So, in 1958, I bought a small semi-detached house in Withington and lived there by myself until 1963.

At about this time most of my colleagues in the Mathematics Department started to accept appointments at other universities, and it became for me rather lonely in Manchester; in particular, there was now nobody left with interest in number theory.

Once again I was given leave by the University of Manchester from the summer of 1962 to early 1963. This time I spent only short periods at the Universities of Colorado and Notre Dame, but for the first time went from October 1962 until the end of the year to the Institute of Advanced Studies at the Australian National University in Canberra, Australia. Here my former colleague B. H. Neumann had become the head of the Mathematics Department at the I.A.S.. I immediately took to Australia and to Canberra and was very happy to accept a professorship at the Institute to start in September 1963. The sunny climate and the beautiful situation were so different from the ugliness of Manchester. At this time, Canberra did not yet have its lake; it was filled only after my arrival, early in 1964.

I took up my new appointment in September 1963 and I remained at the Australian National University for five years, until I reached the rather early retirement age limit of 65 in 1968. It was a very stimulating and useful time, and I liked very much my Australian colleagues at the different Australian universities. When I was not on short vacations or at meetings of the Australian Mathematical Society, I was working on research at the Institute in Canberra.

There was at this time no teaching of number theory in the undergraduate school (School of General Studies) at the ANU. I therefore gave a course on this subject to second and third year students at the SGS, probably the first one ever in Canberra. One of my undergraduates, Coates, asked me to introduce him to research. I provided him with problems to work on, and by the time he obtained his B.Sc., he had already several papers published or in print. He is now a Ph.D. and has a professorship at Harvard, and he has published a number of very good papers dealing with Diophantine Approximations and Transcendency. [Coates was elected to the Royal Society in 1985 and became the successor of Cassels at Cambridge.]

My own work at Canberra concerned mainly two subjects. One was the global approximation theory of algebraic numbers in a number field. My paper J. Austral. Math. Soc. 4 (1964), 425–448 should be useful for the deeper study of such fields; but the super-abstract algebraists of to-day probably find it too specialised! [It has since then found applications.]

My other work centered on transcendental numbers. The fundamental papers by Shidlovski, and by Baker, became available at this time. I carefully worked through them and somewhat simplified their methods. I prepared detailed lecture notes which, perhaps, some day will become a book. { They did, see [200].}

One fruit of my study of Shidlovski's papers was that, during a short week in 1968, I thought I had proved the transcendency of Euler's Constant. But the proof was false, and I could only prove a much less interesting result!

During the American summer in 1965 I once again was at the University of Colorado, and I also made a short visit to Ohio State University where Ross was now the Head of the Mathematics Department. Two years later I spent four months, from September 1967 to January 1968, at the University of Arizona at Tucson, lecturing on transcendental numbers, and afterwards, for about two months, visited again Ohio State University. It was at this time that I accepted an appointment as professor at OSU, to start in the fall of 1968 when my appointment at Canberra was due to end. Since the fall of 1968 I have in fact been here in Columbus at OSU. But I was on leave for three months, from November 1968 to March 1968, and again this year (1971) from the end of March to that of June. The first leave I spent again at the ANU in Canberra, and the second one I used to lecture at the University of Sussex, Brighton, in England.

At this moment, I expect to spend the time from November 1971 to March 1972 at the University of California at San Diego (La Jolla), and then to return to Canberra for my final retirement. I am feeling my age more and more and am troubled by my eyes; so it is perhaps time for it.

While still in Canberra, I was in 1965 elected to the Australian Academy of Science. And in June of this year I was awarded the de Morgan Medal of the London Mathematical Society.

It was a great loss to me when shortly one after the other Koksma, Davenport and Popken died in recent years. All three were dear friends to me for many years.

As a mathematician, my greatest debt was due to Siegel and Mordell. The first taught me much in research, and the second one in teaching and the writing of mathematics. Until Mordell left Manchester in 1945, he corrected the English and the mathematical style of almost all my papers. But in exchange I used to prepare the drawings for his papers in geometry of numbers!

During my fifty years as a mathematician, I have seen immense progress in mathematics and in all the more exact parts of science, including the biological sciences. I feel that in almost everything else there has been a regression. With the extremists on both sides of the spectrum trying to destroy basic research and replacing it by their dogmas of intolerance and power madness, we seem to be on the way to new dark ages.

COLUMBUS, 9 SEPTEMBER, 1971

Addenda

While in the U.S.A. last year (1973), I bought an HP45 electronic calculator and have become interested in computers. Our department at IAS, ANU is due to obtain for me the improved programmable HP65 calculator, and I am looking forward to working with it. It is amazing how much progress in this domain has been made and how prices are coming down, this at a time of big inflation.

I was very pleased by all this progress in Astronomy and Astrophysics during the last years. It gave me particular pleasure to learn that Mars once seems to have had a lot of water and may perhaps become again livable. I was also glad that the schoolbook teachings about the noble gases were disproved when the first compounds of these were obtained.

The progress in my lifetime in the exact sciences, including biology has been impressive, and the years of this century must be unique in history in this regard. Unfortunately, no such progress has been made in the social sciences and certainly not in economics and politics in general. Our world would be in a better state if all the politicians of all the countries could be collected and sent to the moon! And the same should be done to all those representatives of the news media and all the intolerant dogmatists of religion. One needs only think of the two world wars and of the present events in Ireland to see how little christianity does for morality; and the events in Pakistan, not to mention the Near East, prove the same for Islam. Our world would be in a better state if the teachings of Confucius were applied!

APRIL 1974

In 1976 I bought a Texas Instruments RE 52 Calculator, and in 1977 a TI 59. These programmable calculators are very powerful and have been used by me to study the solutions of $h(z^2) - h(z)^2 + c = 0$ which have a simple pole of residue 1 at $z = 0$. I have not tried to learn to work at computers.

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