Reminiscences of My Involvement in de Branges's Proof of the Bieberbach Conjecture

WALTER GAUTSCHI

Around February 3, 1984 (I can't remember the exact date), Louis de Branges came to my office and asked whether he could talk to me for a minute about some work he was doing; perhaps I could be of help. I distinctly remember the first thought that ran through my mind: "Me? Helping de Branges?" We hardly knew each other, never engaged in any mathematical conversation in all the 20 or so years we were at Purdue, and—so I believed—had interests diametrically apart. He sat down and told me that he had a way of proving the Bieberbach conjecture, but needed to establish certain inequalities involving hypergeometric functions. He felt it would be worthwhile, as a first step, to check as many of these inequalities as possible on the computer. Could I do this for him?

Now this was a time when I happened to be under all sorts of pressures. I was expected (and very much wanted) to write a paper for *BIT* to honor Germund Dahlquist on his 60th birthday. Through some mix-up the invitation had reached me just a few days earlier (on February 1)—way past the deadline of December 31, 1983—but I was graciously given an extension through February 29. So I had less than four weeks in which to produce worthwhile results and a publishable paper. At the same time I was in the midst of rewriting a chapter of a survey article for the *MAA Studies in Numerical Analysis* in order to be ready to incorporate the new version on the galley proofs that were to arrive at any time. Also, I was scheduled to leave for Europe on March 7 for lectures in Italy, Yugoslavia, and Germany. As if this were not enough, I had, as the newly appointed managing editor of *Mathematics of Computation*, to deal with a constant stream of manuscripts for this journal. And classes had to be taught also, department committee meetings attended to, etc., etc.

I didn't, of course, tell Louis all these things, but they weighed heavily on my mind when I replied that I would probably not have the time to do anything for him right away. He then told me that he was soon going to give a seminar

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on the subject and asked me to at least attend the seminar and in that way get some more concrete ideas of what was involved.

The seminar took place on February 7, and I managed to attend. I was immediately struck by the clarity, freshness, and elegance of Louis' talk and began to appreciate how those inequalities came about. To my delight, they could be written in terms of orthogonal polynomials—currently a subject very much on my mind. What was needed was to show that for any positive integer n the set of n inequalities

$$(1)\qquad F_{n,k}(x) := \int_0^1 t^{n-k-1/2} P_k^{(2n-2k,1)}(1-2tx)\,dt > 0, \qquad 0 < x < 1,\ k = 0,1,2,\dots,n-1$$

is valid, where $P_k^{(\alpha,\beta)}$ is the Jacobi polynomial of degree k with parameters α, β. (For $k = 0$, the inequality is trivially true.) Louis' theory in fact states that the validity of (1) for some n implies the Bieberbach conjecture for the $(n+1)$st coefficient (but not vice versa). Louis concluded the lecture by showing how he evaluated $F_{n,k}$—a polynomial of degree k—explicitly for the first few values of n (for $n \le 4$, I believe) and how he could verify the correctness of (1) in these cases. Unfortunately, they did not include the largest value of n for which Bieberbach's conjecture had already been proven.

I saw right away how (1) could be verified computationally using Gauss-Jacobi quadrature (with weight function $t^{-1/2}$ on $[0,1]$; but see (2) below), and I pointed this out during the discussion, claiming, with zest, that it would be easy for me to go as far up with n as $n = 100$, if that should be necessary. I was clearly fired up by now and was determined to carry out the computations immediately, no matter what. Having developed reliable software for orthogonal polynomials and Gaussian quadrature during the past few years, I knew that it shouldn't take too much time for me to write the necessary programs.

To begin with, I noted by a simple symmetry argument that one needed only the classical Gauss-Legendre quadrature rule on $[-1,1]$. If $\tau_\nu^{(2m)}, \lambda_\nu^{(2m)}$, $\nu = 1,2,\dots,2m$, are the nodes and weights, respectively, of the $2m$-point Gaussian quadrature rule, with $1 > \tau_1^{(2m)} > \tau_2^{(2m)} > \cdots > \tau_{2m}^{(2m)} > -1$, then in fact

$$(2)\qquad \int_0^1 t^{-1/2} p(t)\,dt = 2\sum_{\nu=1}^{m} \lambda_\nu^{(2m)} p([\tau_\nu^{(2m)}]^2)$$

for any $p \in \mathbb{P}_{2m-1}$, $m = 1,2,3,\dots$. Since the integral in (1) is of the form (2), with p a polynomial of degree n, it suffices to take $2m-1 \ge n$ in (2), for example, $m = [n/2]+1$. The Gauss formula involved in (2) can easily be generated for any value of m (this indeed is done by one of the easier parts of my software package), and the polynomial $P_k^{(\alpha,\beta)}$ in (1) is readily and accurately generated by the well-known three-term recurrence relation. I actually found it slightly more convenient to compute

$$(3)\qquad f_{n,k}(x) = \int_0^1 t^{n-k-1/2} \pi_k^{(2n-2k,1)}(1-2tx)\,dt,$$

for $0 \le x \le 1$, $k = 1,2,\dots,n-1$, where $\pi_k^{(\alpha,\beta)}$ is the monic Jacobi polynomial.

My first program ran the next day, on February 8, and "verified" (1) for all $n \le 18$. It cost me \$3.69 in computer time on the CDC 6500. The program, of course, was still fairly primitive; I simply evaluated $f_{n,k}$ for up to 400 equally spaced points on the interval $[0,1]$ and printed the minimum value and corresponding x-value to see whether the minimum was positive (or a "machine zero" when $x = 1$ and k is odd). I took this simple-minded approach because I was fairly sure that I was going to hit a negative minimum for some early value of n, which would render Louis' argument inconclusive for that value of n, and I could quit and go on with my own work. It didn't work out that way!

After this first piece of positive evidence, I began to improve the program, incorporated error-monitoring devices, compared double precision with single precision results, determined all minima and maxima of $f_{n,k}$ on $[0,1]$ to full machine precision using Newton's method, and checked between any two extrema for possible additional oscillations that I may have missed. I then pushed this improved version of the program up to $n = 30$ and found the validity of (1) confirmed in every case. The most expensive run (for $27 \le n \le 30$) still cost me only \$10.84.

At this point I was convinced that (1) is true for all n. I began to play with the idea of writing up this work in a short note entitled, tentatively, "Numerical evidence in support of a conjecture of L. de Branges." (I didn't dare yet to bring Bieberbach into the title!) I even prepared neat photoready printout on our Diablo printer that could be reproduced, together with the program listing, in the microfiche or supplements section of the journal. (I had in mind my own *Mathematics of Computation.*) A brief excerpt from these tables is shown on the next page.

Happy about this encouraging development, I called on February 13 my good friend Luigi Gatteschi at the University of Turin and asked him whether I could possibly give a second lecture in Turin (one had already been scheduled); the title: "La congettura di Bieberbach è (probabilmente) vera." He readily agreed (though I seemed to detect a skeptical tone in his voice) and subsequently arranged the first lecture to be given at the University and the second at the Polytechnic.

Still not completely satisfied with the strictly computational nature of my work, I began to develop complicated analytic criteria that would insure, mathematically, that $f_{n,k}$ could not have any zeros on any given sufficiently small subinterval of $[0,1]$. By applying these criteria in a judicious manner, one could then in principle prove (again with the help of the computer) that the whole interval $[0,1)$ is free of zeros. I spent about a week on efforts along these lines, but did not get very far, since the program I wrote turned out to be extremely slow. About this time, on February 20, Professor Jack Schwartz from the Courant Institute at NYU visited our Computer Sciences Department. I requested beforehand ten minutes of his time to talk to him briefly about this computational

n	k	x	f(x)	df(x)	ddf(x)
25	23	0	2.992383247e-04	-2.85e-02	2.439457320e+00
		3.205597026e-01	2.341983715e-08	-7.58e-20	7.210458286e-06
		3.417860015e-01	2.391757633e-08	9.68e-20	-5.828107204e-06
		4.340919006e-01	1.072194076e-08	-1.27e-20	3.934615048e-06
		4.662672133e-01	1.136217104e-08	2.70e-20	-3.184816919e-06
		5.522705925e-01	5.785863359e-09	2.36e-20	2.457239610e-06
		5.912254258e-01	6.401691048e-09	-1.98e-20	-2.091412963e-06
		6.679768905e-01	3.545491509e-09	1.17e-20	1.891163295e-06
		7.096583223e-01	4.124040146e-09	-2.57e-21	-1.744348249e-06
		7.744958058e-01	2.406779734e-09	-3.54e-21	1.841974435e-06
		8.146887759e-01	2.974169399e-09	-1.11e-20	-1.903521612e-06
		8.656751077e-01	1.775935986e-09	-1.01e-20	2.376104557e-06
		9.002022063e-01	2.375351261e-09	8.51e-21	-2.907485610e-06
		9.362182846e-01	1.395724928e-09	-7.17e-21	4.540772788e-06
		9.612703112e-01	2.103525418e-09	-1.40e-20	-7.509062965e-06
		9.819556336e-01	1.112364664e-09	-3.14e-20	1.838703966e-05
		9.944763566e-01	2.166255736e-09	1.27e-19	-7.519729386e-05
		1.000000000e+00	4.784809710e-19	-1.02e-06	-3.376022124e-04
25	24	0	1.583271559e-05	-2.13e-03	2.534501112e-01
		8.821620539e-02	8.890073589e-08	6.86e-19	9.519682273e-05
		1.007789679e-01	9.102976075e-08	1.47e-18	-6.626872232e-05
		1.654376237e-01	3.432305315e-08	-4.90e-20	2.621243315e-05
		1.895837701e-01	3.655406944e-08	4.55e-19	-1.841404435e-05
		2.621430762e-01	1.721658961e-08	-6.88e-21	9.824593756e-06
		2.961071425e-01	1.896011080e-08	5.88e-21	-7.338968761e-06
		3.726369664e-01	1.015730680e-08	-9.74e-21	4.916658213e-06
		4.142485505e-01	1.154094862e-08	-6.56e-21	-3.940854645e-06
		4.905874739e-01	6.707130829e-09	-4.35e-20	3.131842409e-06
		5.370536552e-01	7.869709830e-09	1.27e-21	-2.707663255e-06
		6.092538964e-01	4.817924151e-09	-2.91e-21	2.475361689e-06
		6.572727170e-01	5.859388113e-09	-2.44e-21	-2.328490771e-06
		7.218483349e-01	3.696588620e-09	-2.38e-21	2.418952894e-06
		7.678171738e-01	4.693289289e-09	-1.50e-20	-2.516816547e-06
		8.219130195e-01	2.988555425e-09	-4.24e-22	2.987795528e-06
		8.621912336e-01	4.012828437e-09	2.54e-21	-3.552522979e-06
		9.036814275e-01	2.511845675e-09	1.49e-20	4.988334162e-06
		9.348846218e-01	3.660165547e-09	2.51e-20	-7.304448233e-06
		9.624003854e-01	2.141929923e-09	-4.80e-20	1.360061023e-05
		9.817059823e-01	3.622132535e-09	1.46e-19	-3.126465152e-05
		9.945898427e-01	1.627629589e-09	-4.94e-19	1.385542984e-04
		1.000000000e+00	5.412894218e-09	1.82e-06	6.092753732e-04

Extrema of $f_{n,k}(x)$ on $[0,1]$, with first and second derivatives, for $n = 25$ and $k = 23, 24$. (The computer printout is in floating-point E-format, so that $e - xx$ is to be read as 10^{-xx}. Note also that the derivatives at the interior extrema are not exactly zero, but approximately $\varepsilon \cdot f_{n,k}(0)$, where $\varepsilon = 3.55 \times 10^{-15}$ is the machine precision of the CDC computer.)

work on the Bieberbach conjecture. He showed polite interest in the matter, but didn't say much. Only at the end of our brief meeting he casually asked why not use Sturm sequences. I remember how this question caught me by surprise and how I wondered why I hadn't thought of it myself. After all, I used Sturm sequences in a similar setting some eight years ago in connection with Chebyshev-type quadrature rules. On second thought, however, I could understand why Sturm didn't enter my mind: The polynomial $F_{n,k}$ in (1) is given in the form of an integral, and it was not immediately obvious how to generate Sturm sequences in rational form.

It soon occurred to me, however, that the explicit power representation of $F_{n,k}$ can be obtained (in rational form) rather easily by substituting the representation

$$(4)\quad P_k^{(\alpha,\beta)}(u) = \frac{\Gamma(\alpha+k+1)}{k!\Gamma(\alpha+\beta+k+1)} \sum_{m=0}^{k} \binom{k}{m} \frac{\Gamma(\alpha+\beta+k+m+1)}{2^m\Gamma(\alpha+m+1)} (u-1)^m$$

into (1). Indeed, with $u = 1 - 2tx$, one gets $u - 1 = -2tx$, and the integral in (1), using (4), is easily evaluated, yielding a polynomial with coefficients in the form of ratios of factorials (in fact, a ${}_3F_2$). So the work to be done from now on was clearly mapped out for me: Apply the Sturm sequence algorithm to (1) [or alternatively, to (3)] on the interval $[0,1]$ in rational arithmetic—for example, using the MACSYMA system—and in this way show compellingly, once and for all, and for as many n as desired, that $F_{n,k}$ cannot vanish on $[0,1)$ and therefore, since $F_{n,k}(0) > 0$, that it remains positive on $(0,1)$. Time, however, was getting short, and I decided to postpone this work until after my return from Europe. In the meantime, I programmed the Sturm sequence algorithm for (1) in double precision FORTRAN in order to check out the algorithmic details and to make it easier for me, upon my return, to transcribe the program into the MACSYMA language (a system I still had to get better acquainted with). By February 26 (a Sunday) I had the program running satisfactorily on the CDC computer and producing results as expected.

A week earlier, incidentally, I managed to complete my paper for Dahlquist and sent it off to the editors.

My departure for Europe was becoming imminent. Since my verification work seemed well under way, and in good shape now, I let it rest for a while and turned my attention to the lectures I was to give in Europe. Just to set my mind at ease, I wanted to make sure, however, that the inequality (1) was not by chance already known in the literature. Actually, looking at the rather delicate behavior of $f_{n,k}(x)$ for x near 1 and k near n, as exemplified in the short table above, I rather doubted that analytical techniques could be sharp enough to provide a proof of (1). But it didn't hurt to check. I knew there was only a handful of mathematicians in the world who could possibly be familiar with a result of the type (1) and even come up with a proof of it, among them Dick Askey at the University of Wisconsin, whom I knew best. So I called him on February 29 and told him of the inequality (1) and what it implied according to de Branges. He immediately interrupted me with an emphatic: "I don't believe it!" and recounted some rather outrageous claims that had been made in the past by a number of people. I countered that de Branges was a serious mathematician and that we were dealing here with first-rate work. Even if Louis' implication should not hold tight, I said, the inequalities (1) were quite interesting in their own right and ought to be scrutinized further. Besides, I was fully convinced of their validity. Dick now agreed to look into it.

I was working late at home on my lectures, that same night, when the phone rang and I heard Dick Askey's triumphant voice on the other end of the line: "The inequality is not a conjecture—it's a theorem!" He then pointed out a result in a joint 1976 paper with George Gasper that contains (1) as a special case. I was, of course, delighted to hear this incredible news, but also disappointed, realizing that all my hectic work had been in vain. After I checked and confirmed the result myself, I saw Louis the next morning and told him the good news. He replied, rather matter-of-factly: "Well, that proves Bieberbach's conjecture."

Immediately after I talked to Louis, I called Luigi Gatteschi in Turin and asked him to change the title of my second lecture. There was no point anymore to talk about numerical evidence for a conjecture that had turned into a theorem, and I proposed, instead, to talk about the work I did in the paper for Dahlquist. I told him that I would explain everything when I was in Turin. He agreed to send out a change of title notice, and he scheduled this second lecture to be held also at the University on March 13.

It was during the first ten minutes of this lecture that I first apologized for the change of subject and then briefly announced the validity of the Bieberbach conjecture subject to verification of de Branges's work. This was probably the first time that the word got out in Europe, but it was a small audience, consisting largely of graduate students and only a few faculty members. A week later, I attended a conference in Munich celebrating the 25th anniversary of the journal *Numerische Mathematik*. There I saw another good friend of mine, Dick Varga, and told him informally of de Branges's proof of the Bieberbach conjecture. I knew he was going to give a talk himself about a number of conjectures, including the Riemann hypothesis. At the end of the discussion period he turned towards me and put me on the spot with: "Speaking of conjectures, how about Bieberbach's conjecture, Professor Gautschi?" So I went to the blackboard and announced again de Branges's proof of the conjecture and the role played by the inequality of Askey and Gasper. But this time, it was before a large international audience of experts, and I felt the enormous impact of my brief presentation. The word now spread to different parts of Europe.

Looking back at this episode, I cannot help concluding with a few philosophical remarks. 1. *The computer is an important aid in theorem proving.* In our case, the computer could have been used to prove Bieberbach's conjecture (using Sturm sequences in MACSYMA) if not for all n, then at least for as many n as desirable and practicable. Equally importantly, my computer results gave Louis confidence in his overall proof strategy; his approach indeed seemed capable of proving the complete Bieberbach conjecture. 2. *The availability of high-quality mathematical software is of the utmost importance in scientific research.* While this statement is undisputed among computer scientists, it deserves to be better understood and appreciated by the mathematical community. Had I not had available my own software package on orthogonal polynomials, I would probably not have undertaken these computations, given the severe time constraints under

which I was operating. 3. *One should never underestimate the usefulness of a result in pure mathematics.* No one in his wildest dreams, least of all the authors, could have imagined that the Askey/Gasper nonnegativity result would provide a critical link in the proof of the Bieberbach conjecture. *Inequalities,* in particular, *are always potentially useful.* I am fortunate to have inherited a love for inequalities from my teacher, Professor Alexander Ostrowski, who was a master at them, and from Professor Mauro Picone, who openly confessed to me his fondness for inequalities. Perhaps it is an auspicious omen that the Bieberbach conjecture itself—now de Branges's theorem—consists of a set of inequalities.

My Reaction to de Branges's Proof of the Bieberbach Conjecture

RICHARD ASKEY

On February 29, 1984, Walter Gautschi called to ask about an inequality. He prefaced his request with the statement that Louis de Branges had reduced the Bieberbach conjecture to showing that an integral of a class of Jacobi polynomials was positive. I said that was preposterous, since the Bieberbach conjecture was a complex inequality, and it could not be proved from a real inequality for a real function. Gautschi gave me the inequality and said it seemed to be true, but that it was probably very hard to prove. He had put some special cases on a computer and they seemed to be true, but there were times when the polynomials were very small. I said I knew something about these questions and would look at it. That evening I computed the integral to see what ${}_3F_2$ it was and then looked at a paper George Gasper and I wrote about ten years earlier. It appeared in the *American Journal of Mathematics* in 1976. To my surprise, the inequality de Branges wanted was proved in this paper. I called Gautschi to tell him that the inequality was true, but that I was almost sure it would not prove the Bieberbach conjecture. In addition to my earlier reservation, there was another good reason why I felt this way. The ${}_3F_2$ inequality is equivalent to the positivity of a sum of Jacobi polynomials, and Gasper had proven the positivity of another sum of Jacobi polynomials that is a fractional derivative of order one-half of the inequality de Branges needed. Thus while this inequality is sharp in some senses, as Gautschi realized from his computations, in other ways there were much stronger inequalities. I did not think the Bieberbach conjecture would follow from anything except a very sharp inequality.

Two days later de Branges called to ask me if I knew the consequences of this inequality. I said I knew what Gautschi had told me, but that I had serious doubts and told him what they were. In less than two weeks de Branges sent his manuscript, *Square Summable Power Series.* Before that I called Peter Duren, and he said he had been informed and had suggested that de Branges circulate his proof widely. At that time I felt there was something like a 2 percent chance that de Branges had proven the Bieberbach conjecture. The reason this was greater than zero was that he had discovered a deep inequality without knowing it before, so he clearly had something. It seemed likely he would be able to prove the Bieberbach conjecture for a subclass of univalent functions, since weaker inequalities for trigonometric polynomials had been used for that purpose before.

I tried to read the last chapter of de Branges's book, but was unable to follow his arguments. The next thing I heard was from Walter Rudin, when he returned in the summer from a meeting in England. He called to say that de Branges had proved the Bieberbach conjecture. After I told Rudin of my doubts, he told me there was a new version of de Branges's proof, with all the operator theory removed, and that the details had been checked by a number of very good mathematicians. He gave me a Russian version from Leningrad. After a week I had reached the point of getting out a Russian dictionary when David Drasin called to tell me the latest and to ask if the inequality Gasper and I had published was really true. He said that Pommerenke had checked the rest of the proof. I assured him that our inequality was true, and that there were two proofs. The first appeared in our paper, and I had earlier included a proof of

$$(1) \qquad \sum_{k=0}^{n} P_k^{(\alpha,0)}(x) > 0, \qquad -1 < x \leq 1, \ a > -2$$

in my Regional Conference Lectures: *Orthogonal Polynomials and Special Functions*, SIAM, 1975. When $\alpha = 2, 4, \ldots$, this is equivalent to the inequality de Branges needed. This is an appropriate place to recall something I first mentioned in these Lectures. The proof of (1) given there was found by Gasper. Gasper has found a second proof of (1) which is shorter than his first proof, but not as well motivated. Actually there are now five proofs, four due to Gasper and one found by Koornwinder.

Drasin said there was another version of de Branges's proof written by Fitz-Gerald and Pommerenke. He sent a copy, and after reading it, I was convinced that my original reservations had come because I did not know enough. It was also clear that de Branges's marvelous proof should lead to deeper inequalities than Milin's conjecture. Koornwinder has found a deeper inequality in one direction, and there should be more in other directions.

There are a number of things to learn from this work. First, on the technical level, de Branges has shown the true depth of Loewner's work. It was always known to be deep, but I doubt if anyone except de Branges really appreciated how deep. Second, there is a lot to be said for doing two things de Branges did. He worked on a hard problem, has worked on other hard problems in the past, and was not discouraged by past failures. Also it often pays not to talk with experts in an area, or at least not to take their pessimism too seriously. Experts in univalent functions would have told de Branges that his method could not work, and they would have been wrong. My reaction was similar. I knew too much in one direction, where my optimism had led to a number of deep inequalities, and I did not know enough in another area. These two led to a skepticism that was unwarranted. Do not take the pessimistic views of an expert

very seriously, but optimistic views should be taken very seriously, for they often lead to something important. Third, if a newspaper reporter calls, say you want ten minutes to think and ask them to call back, or offer to return their call. I have been disappointed by the press coverage of this work. The important fact is de Branges's proof, not whether he had published false proofs of other conjectures. The fact that de Branges was an outsider in univalent functions is important, but no one who knows his early work can say he was on the fringe of the research community. If one does not know, then it is best to say nothing and refer the reporter to someone who does. Finally, the polynomial inequalities Gasper and I proved (both jointly and separately) were primarily conjectured while trying to understand papers of Fejér that I was reading in his *Gesammelte Arbeiten*. Collected papers of great mathematicians are very valuable, and every mathematician should own some and read seriously in at least one. The time and money spent is a very good investment.

The Story of the Verification of the Bieberbach Conjecture

LOUIS DE BRANGES

You do not have to know much about the Bieberbach conjecture to be able to read this story. All you have to know is that it was, until recently, one of the most famous unsolved problems of analysis. It was proposed in 1916 by the German mathematician Ludwig Bieberbach.

Assume that a power series $z + a_2z^2 + a_3z^3 + \cdots$ represents a function which has distinct values at distinct points of the unit disk. Bieberbach conjectured that the inequality

$$|a_n| \leq n$$

holds for every $n > 1$.

The Bieberbach conjecture was verified for the second coefficient in 1916, for the third coefficient in 1923, for the fourth coefficient in 1955, for the sixth coefficient in 1968, and for the fifth coefficient in 1972.

No other case of the Bieberbach conjecture was known when I distributed a manuscript, on the first of March 1984, claiming to contain a proof of the Bieberbach conjecture for all coefficients.

At that time I had been working on the Bieberbach conjecture for nearly seven years, but I was not an acknowledged expert on that subject. Most of my research, over a period of twenty-five years, had been on other problems.

The final assault on the Bieberbach conjecture was made in a round-about way. I spent the year 1983 writing a book on *Square Summable Power Series.* Only one of the six chapters is concerned with the Bieberbach conjecture. The other five are about apparently unrelated problems on invariant subspaces, perturbation theory, polynomial approximation, and substitution transformations.

So the chapter on the Bieberbach conjecture was only a part of a much larger project. I was delayed in writing the chapter because my wife Tatiana and I translated into English a Russian manuscript related to the invariant subspace theory in the second chapter. We accepted the translation because its author was a student of M. S. Livšic, a personal friend and one of the creative geniuses of invariant subspace theory.

The chapter on the Bieberbach conjecture was completed only at the end of January 1984. The chapter improves on an estimation theory which I had obtained in the fall of 1982. I was very much surprised by the outcome of my calculations, because they were very close to a general proof of the Bieberbach conjecture.

But to obtain the Bieberbach conjecture, I needed to know that certain polynomials are positive in the unit interval. For the $(n+1)$st coefficient, there are n polynomials, of degrees 1 through n. The polynomials are hypergeometric series with rational coefficients. So they are easily computable, and by examining them I could verify the Bieberbach conjecture for the second, third, and fourth coefficients. But the fifth coefficient made me work and I nearly did not make my way through the calculations for the sixth coefficient. So I was very happy when Walter Gautschi, seemingly without effort, ran off the calculations on the Purdue computer to verify the Bieberbach conjecture up to the twenty-fifth coefficient.

I leave it to him to tell his own story of how that happened. In fact he went farther than twenty-five, but I was already impressed at twenty-five because the Bieberbach conjecture was doubted by experts for odd coefficients, beginning around the nineteenth. I concluded that these doubts were unfounded and began searching for a general proof of the inequality.

At this point there occurred one of two remarkable coincidences connected with the proof of the Bieberbach conjecture. Walter Gautschi consulted Richard Askey at the University of Wisconsin about the calculations. It turned out that the inequality being tested on the computer was a theorem which Askey had obtained in 1976 in joint work with George Gasper of Northwestern University. That completed the proof of the Bieberbach conjecture.

That allowed me to complete my manuscript on *Square Summable Power Series.* I mailed more than a dozen copies to experts for verification. That was in the beginning of March. To my disappointment every one of them wrote back giving me a good reason why he could not check the proof at that time.

It was at this point that a second remarkable coincidence occurred. This happened because I was scheduled for a three-month visit to the Soviet Union under the exchange agreement between the National Academy of Sciences and the Academy of Sciences of the USSR.

Here I need to explain that I had proved a stronger conjecture than the Bieberbach conjecture. I had proved a conjecture of I. M. Milin, which implies the Bieberbach conjecture. I made a telephone call to my hosts at the Steklov Mathematical Institute in Leningrad. Sergei Kisliakov told me that Milin was in Leningrad.

On the twentieth of April, I took Lufthansa flights from Chicago to Frankfurt and from Frankfurt to Leningrad. In one hand I carried my manuscript on *Square Summable Power Series*, neatly bound in a black folder. A bleak winter landscape greeted me on arrival, and I felt unable to speak a word of Russian

despite an excellent reading knowledge of the language and the ability to understand what was said when it was spoken. My host Sergei Khruschev greeted me at customs and took me through the city to a three-room apartment which was to be mine.

Then we went by subway to the apartment of N. K. Nikol′skiĭ, the leader of the seminar in functional analysis. Present also were V. P. Havin and Mrs. Nikol′skiĭ. After a meal which she served, we scheduled my talks to the functional analysis seminar in April and May. There was just enough time to cover the five chapters of my book on *Square Summable Power Series* which were not related to the Bieberbach conjecture. That part was left to the seminar in geometric function theory.

My stay in Leningrad was one of the happiest periods of my life. A typical day began with a walk through a huge park nearby. Then after bathing, eating, and shopping, I was free to do whatever I wanted. Tuesdays and Thursdays were seminar days. On those days I took the subway downtown and walked along Nevskiĭ prospect until I reached the Mathematical Institute, which is located along a bank of one of the canals passing through the city.

Functional analysis has an impressive seminar room which must hold fifty people. A meeting of the seminar ordinarily consists of two hour-and-a-half sessions interrupted by a break for tea. I spoke in English, doing much writing on the blackboard. My work was received with interest and appropriate questions in Russian and in English. At the end of the five lectures it seemed to me that every member of the seminar had spoken to me of some work of his own which was related to my lectures.

The verification of the proof of the Bieberbach conjecture was made in a smaller room with a smaller audience whose members were less familar with English. I was introduced to Professor Milin by Asya Greenshpan, a professional translator and the wife of Arkady Greenshpan. Milin had an amused look which I interpreted as doubts about the soundness of my undertaking. This seemed to be confirmed a few minutes later when a question was asked about my work on the Peterson conjecture, which contains an uncorrected error. I had to explain that I did not know whether my argument could be saved and that I had not tried to do so because the Peterson conjecture was contained in the Weil conjecture which had subsequently been proved by Deligne.

No one seemed to believe a word of my lecture, but the situation was better than it seemed. The reason was that Khrushchev had visited Purdue University during the fall semester 1982 and had brought back to Leningrad a manuscript which I had written at that time. After my telephone call, the results of the manuscript were presented to the seminar in geometric function theory by a student named Emel′ianov. This occurred some three weeks before my arrival. Already in my second meeting with the seminar, Genia Emel′ianov was saying that the proof was correct. A meeting of the seminar then occurred in which I was not present. In my third meeting with the seminar, Professor Milin shook my hand, without losing the glint of humor in his eye, and said that I was a very talented mathematician. He also said that my argument was elegant.

Both Emel'ianov and Milin submitted written reports confirming the proof of the Bieberbach conjecture and presenting variants of the argument considered to be advantageous. The reports were accepted by the other members of the seminar, including Arkady Greenshpan and the seminar leader, G. V. Kuz'mina, as correct.

Spring had come to Leningrad and it was now June. Every day was an ideal twenty degrees Celsius. No more seminars met. Everyone went on sightseeing expeditions to the Winter Palace and to other palaces outside the city such as Peterhof and Pushkin. But during that time I worked for long hours with Professor Kuz'mina to prepare the preprint, in Russian and in English, which contains the findings of the seminar.

She was extremely conscientious in her capacity as an editor. The manuscript underwent constant revisions not only for mathematical content but also for details of expressions and for historical accuracy. She was, for example, concerned that the cases of equality be included in the preprint. I assumed at the time that this work was done by Emel'ianov, but this may have reflected modesty on her part.

Our working sessions began at two in the afternoon and ended at nine or ten in the evening. The only break was for tea. When I left, she complained about the adjustments to living without the Bieberbach conjecture, which had become such an absorbing part of her life.

During the second week of June, I had made a visit to the Gonchar seminar at the Steklov Mathematical Institute in Moscow. Afterwards Professor Gonchar let it be known that he thought the proof should be published in *Mat. Sbornik.* I agreed that was appropriate in view of the contribution of the Leningrad seminar in geometric function theory to the verification of the proof. As I left, Professor Kuz'mina and I promised to keep working together to bring the manuscript into suitable form for such publication.

The Soviet Academy of Sciences granted me permission to leave Leningrad two weeks earlier than scheduled so that I would have a chance to present my proof of the Bieberbach conjecture in Western Europe before catching my return flight from Frankfurt to Chicago. It also paid me for those two weeks, which allowed me to take an Aeroflot flight from Leningrad to East Berlin.

Sergei Khrushchev accompanied me to the Leningrad airport on the morning of July first. One of the things which was different on leaving Leningrad was that by that time I could speak Russian.

My first reaction on arriving in West Berlin was to have a normal meal. I then went by train to Heidelberg and rested for several days at my father's house. I learned from his scales that I had lost five kilos during the ten weeks in Leningrad.

In the first two weeks of July, I presented the proof of the Bieberbach conjecture at the University of Würzburg, the University of Hannover, and the Free University in Amsterdam. The proof was also presented by Sergei Khrushchev at an international symposium on operator theory in Romania in the second week of June. As I was catching my return flight to Chicago on July fifteenth, Volodya Peller of the Leningrad seminar in functional analysis was about to present the proof of the Bieberbach conjecture at an international symposium on operator theory in Lancaster, England.

Everywhere that the proof of the Bieberbach conjecture was presented, it was received with great excitement. With all that had happened, it was several weeks after my return to Lafayette that I could resume my work.

As soon as I was back to my typewriter and to my usual means of producing manuscripts, I became dissatisfied with the publication arrangements made in the Soviet Union. My main concern in Leningrad had been to get the seminar to say yes. I tried to do nothing which would prevent or delay that important affirmation. However, during the month of June, I found improvements in technique which could not have been incorporated in the preprint during the remaining time of my visit. The mails to Leningrad were too slow and the rules of *Mat. Sbornik* were too strict to make and check out all the needed changes later.

I decided to begin a new and final version of the proof of the Bieberbach conjecture. As I was working on it, I had the feeling that I was producing a historical document. I put other concerns aside and gave myself entirely to the writing of the paper, without any hurry.

I asked friends and colleagues about the best journal for publication. The unanimous answer was *Acta Mathematica.*

The paper was completed at the end of August, just as the new semester was beginning at Purdue University. The manuscript was mailed to Stockholm in the middle of September. Professor Hörmander accepted it for publication in the middle of October. Publication occurred within four months of acceptance.

Note: Other accounts of the verification have been written by I. M. Milin in these proceedings and by G. V. Kuz'minä (in a joint article with O. M. Fomenko) in the *Mathematical Intelligencer*, 8 (1986), No. 1, pp. 40–47.