THE BIBERBACH CONJECTURE: A SUMMER DRAMA

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ACT 1

It was Brian Twomey who brought me the news: the Biberbach conjecture was false! More than that: a weaker form of it was untrue! We were redundant!

It transpired that Brian had just been on the 'phone to the extern examiner, who was making his rounds of the National University of Ireland Colleges; he was due in Cork in two days time, on Sunday, June 17, 1984, and rang to confirm his arrival time. Brian had no details, other than that X had cracked it. We would have to await the visit of the extern to learn more.

While trying to digest this piece of startling news, I recalled the story of how Briggs was supposed to have reacted when he congratulated Napier on his discovery of logarithms - it is said that he remained speechless in Napier's presence for fifteen minutes - and wondered how much time I should stand open-mouthed at X when I next met him!

The extern duly arrived. No: he had no further information; but he had no reason to doubt his informant, who had heard it from an impeccable source, and, anyway, X was a reputable mathematician.

During the course of a very heavy work-load of examining, the extern received a 'phone call urging him to adopt a less optimistic stand about X's success; there was some doubt about X being able to realise his hope. We should continue working at it! The prize could still be ours'.

A little later in the month, we heard that X had abandoned all hope of patching up his proof.

Things returned to normal.

ACT 2

On July 15, I went to Lancaster University for the NATO-LMS Advanced Study Institute on Operators and Function Theory. Shortly after arriving, I heard that the Bieberbach conjecture was true, news I greeted with considerable surprise and profound scepticism. I was even more sceptical, when I heard who was credited this time with the proof: Louis de Branges, of all people. But, I said, echoing a comment made by others, his reputation is not untarnished: didn't he claim, with a fanfare of trumpets, to have proved the invariant subspace problem many years ago?

My informant's source was unlikely as well. It was the Russians, Peller and Nikolskii, from the Steklov Institute in Leningrad, who were amongst the distinguished gathering, who brought the news. I was intrigued! My natural curiosity was aroused! Why should the news emerge from the U.S.S.R.? Was it some ingenious plot conjured up by the K.G.B. to discredit American mathematicians? If the claim were true, surely the latter would have been the first to know and the first to announce it? Wouldn't they love to bask in the reflected glory of such an achievement?

I consulted my function theory friends. Yes, they had heard the rumour - for that's all it was as far as we knew at that stage - but knew as little as I did. The rumour travelled like wildfire. People speculated as to whether or not it was true; and wondered about the embarrassment it might cause in America, if it were true. Some of the participants had heard it at another conference. Others had heard de Branges himself deliver a lecture in the Netherlands. Surprisingly enough, neither the few experts in the field of univalent functions, nor the other function theorists present at the conference, however, were aware of the news; they too were hearing it for the first time.

Bit by bit the story was unfolded: before departing for a visit to Europe, de Branges had circulated a version of his proof to a handful of his colleagues who were specialists in the field of univalent functions; they were, apparently, still examining the proof, which, by all accounts, involved operator theoretic methods. Everybody seemed to think that that fact plus de Branges' reputation for false claims in the past, explained their silence on the matter. Shortly afterwards, de Branges visited the Steklov Institute and presented an account of his work in a series of talks. Members at the Institute were sufficiently impressed by the work that a group of them, including some very eminent function theorists, got together with de Branges and proceeded to remove all reference to operator theory methods and fashioned a proof along traditional lines. The result was a proof of the conjecture that could be outlined in a 13-page typescript. Peller had a copy of this, in Russian only, and was prepared to make it available for circulation to anybody who was interested, and to give a talk on de Branges' proof, if time could be made available in what was a very crowded programme; and there was some doubt about this.

Representations were made to the organisers of the Conference, and it was decided that Peller should give the talk at 6 p.m. on Friday, July 20; the talk would be followed by the banquet.

ACT 3

Excitement was at fever pitch when the moment arrived for Peller to begin. The lecture theatre was packed for the historic occasion; participants from other conferences being run at Lancaster were also present; some non-mathematicians were even there. The majority present were non-experts. It was disappointing that so few experts in the field of univalent

functions were there, although several British mathematicians had been informed of the happenings during the week. I feel most people came out of curiosity; everybody had heard about the Biberbach conjecture, even if they couldn't formulate it exactly and didn't appreciate its importance and relevance; some came to discover "the mistake", and leave early to dress for the banquet. I suspect that most of these remained to the end and were late for the banquet! I was one of them!

Peller gave a short history of the problem and recalled a few familiar inequalities. (So far, so good, I thought.) Next, he introduced a system of first order linear differential equations, and asked us to accept certain properties possessed by the solution. (No problem!) He then appealed to the Loewner theory of univalent functions. (Curse it, I'm not familiar with this, I thought, but what he is using can be verified. I'll hang in there.) This was followed by some very crafty manipulations, in which the role of the system of differential equations became apparent. (Several people left at that stage.) Undaunted, Peller proceeded with his task. Eventually, it emerged that everything hinged on the solution of this system having a certain property. Peller proceeded to discuss the solution. (More people left to prepare for the banquet, others because they had tired minds and simply could not absorb any more. I was fast becoming one of them! But I was determined to see it through.) To clinch the result, Peller introduced the final surprise: the property that he wanted the solution to satisfy fell out from a result of Askey and Gasper on hypergeometric polynomials!! Who would have believed it! I was drained. Peller had spoken dispassionately for one and a half hours, taking great care to present all the important points in the proof. I rushed away exhilarated, to change for the banquet. What an occasion to be present at!

In the days that followed, Peller's talk was discussed, and there was general agreement that the proof was correct, though most people were unsure about the details, expecially the role of the hypergeometric polynomials, which took us all by surprise. As one editor of a respected journal said to me afterwards, if he had received the Askey-Gasper paper for his journal, he would have rejected it out of hand.

(There is a lesson there for all of us: we should never dismiss too lightly what may appear to us to be uninteresting and therefore insignificant, simply because it is out of fashion.)

ACT 4

In September, I attended a one-day Conference in Liverpool, and I heard Hayman give a slightly different version of De Branges' proof, in which he took into account some simplifications due to Fitzgerald and Pommerenke.

There could no longer be any doubt: the Bieberbach conjecture had fallen at the hands of Louis de Branges!

We salute him!

EPILOGUE

What follows is an account of de Branges' proof, based on Peller's talk and Hayman's; I have not seen de Branges' original accounts, but I was privileged to see the 13-page Russian typescript, on which Peller based his lecture, and the informal communication circulated by Fitzgerald and Pommerenke during the late summer. While preparing this, Donal Hurley drew my attention to Fitzgerald's own article in the A.M.S. Notices. Readers desiring more information on the history and solution of the problem are advised to consult this, which contains an up-to-the-minute account of the progress that has been made in the past few months, as well as an extensive bibliography.

$\mathcal{S}$ will stand for the class of univalent functions f on the open unit disc D, i.e. $f \in \mathcal{S}$ iff f is one-to-one and analytic on D with Taylor series

$$f(z) = \Sigma a_n(f)z^n \equiv z + \sum_{n=2}^{\infty} a_n(f)z^n, \quad (z \in D)$$

In 1916 Bieberbach conjectured - on the basis of very slim evidence - that

$$(B) \qquad A_n = \sup\{|a_n(f)| : f \in \mathcal{S}\} = n, \quad n = 2,3, \ldots,$$

with equality only if f is of the form $f(z) = K(\lambda z)$ for some λ, with $|\lambda| = 1$, where

$$K(z) = z/(1 - z)^2 = z + \sum_{n=2}^{\infty} nz^n$$

is Koebe's function. This was proved by him for n = 2.

Over the years, evidence in support of this conjecture was provided. It was shown to be true for various subclasses of $\mathcal{S}$, for all n; and verified for small values of n for the full class. Also, stronger conjectures were advanced. Thus, in 1936, Robertson put forward the conjecture that if g is an *odd* univalent function, then

$$(R) \qquad \sum_{k=1}^{n} |a_k(g)|^2 \leq [(n + 1)/2], \quad n = 1,2, \ldots$$

This is stronger than (B), because if $f \in \mathcal{S}$, the function g defined by $g(z) = \sqrt{f(z^2)}$, $(z \in D)$ belongs to $\mathcal{S}$ and is odd, and the coefficients of g and f are related through the convolutions

$$a_n(f) = \sum_{k=1}^{2n} a_k(g)a_{2n-k}(g), \quad n = 1,2, \ldots$$

An easy application of Schwarz's inequality now shows that (R) forces (B).

In 1955 Hayman proved that the limit

$$\lim_{n\to\infty} \frac{|a_n(f)|}{n}$$

exists and is ≤ 1, for every $f \in S$. Shortly after, he showed that A_n/n converges to a number ≥ 1; and conjectured that the limit was actually equal to 1, expressing the view that this might be easier to prove than (B). (It was this conjecture that X was rumoured to have disproved.)

In the late sixties, Lebedev and Milin derived a powerful inequality for the class S, involving the coefficients of another auxiliary function, viz.,

$$\log[f(z)/z] \quad = \quad 2 \sum_{k=1}^{\infty} \gamma_k(f) z^k \qquad (z \in D)$$

and those of g:

$$\sum_{k=1}^{2n} |a_k(g)|^2 \leq n \exp[\sum_{k=1}^{n} (1 - k/n)(k|\gamma_k(f)|^2 - 1/k)], \quad n = 1,2,\ldots$$

This brings us to Milin's conjecture, which is that

$$(M) \qquad \sum_{k=1}^{n} (n - k)k|\gamma_k(f)|^2 \quad \leq \quad \sum_{k=1}^{n} (n - k)/k, \qquad n = 1,2,\ldots$$

Clearly, it follows from the previous inequality that $(M) \Rightarrow (R) \Rightarrow (B)$. de Branges' remarkable feat was to establish the validity of (M).

The strategy adopted by be Branges was to prove the inequality for a dense set of functions in S - dense in the sense of uniform convergence on compact subsets of D. Fitzgerald and Pommerenke simplified this part of the proof considerably. Both approaches make use of the theory introduced by Loewner in 1923, establishing the existence of an especially useful dense set, which Loewner himself used to prove that $\sup\{|a_3(f)| : f \in S\} = 3$. For our purposes, it is enough to know that there is a dense subset T in S, such that if $f \in T$,

then there is a continuous function $\lambda : [0,\infty) \to \partial D$, such that the solution $f(z,t)$ of the partial differential equation

$$\frac{\partial}{\partial t} f(z,t) \quad = \quad \frac{1 + \lambda(t)z}{1 - \lambda(t)z}\, z \frac{\partial}{\partial z} f(z,t) \qquad (z \in D,\ 0 \leq t < \infty)$$

has the properties that $f(z,0) = f(z)$ and $e^{-t}f(z,t) \in S$, for all $t \in [0,\infty)$.

Given this, let $f \in T$ and $\lambda : [0,\infty) \to \partial D$; and let $f(z,t)$ be the corresponding function. Then, for any $t \geq 0$,

$$\log[e^{-t}f(z,t)/z] \quad = \quad 2\Sigma\gamma_k(f,t)z^k \quad \equiv \quad 2\Sigma\gamma_k z^k,$$

say. Using the differential equation, it is easy to derive the identity

$$1 + 2\sum_{n=1}^{\infty} \dot{\gamma}_n z^n \quad = \quad [1 + 2\sum_{n=1}^{\infty} \lambda^n z^n][1 + 2\sum_{n=1}^{\infty} n\gamma_n z^n] \qquad (z \in D),$$

where the dot denotes differentiation with respect to t and the variable t has been suppressed throughout.

Writing

$$b_k \equiv b_k(t) \quad = \quad \sum_{\nu=1}^{n} \nu\gamma_\nu(t)(\lambda(t))^{-\nu}, \qquad k = 1,2,\ \ldots$$

and equating coefficients in the above identity, we see that

$$k\gamma_k \quad = \quad \lambda^k(b_k - b_{k-1}), \text{ and } \dot{\gamma}_k \quad = \quad \lambda^k(b_k + b_{k-1} + 1),$$

$$k = 1,2,\ \ldots$$

Now fix n and consider

$$\phi(t) \quad = \quad \sum_{k=1}^{n} (k|\gamma_k(t)|^2 - 1/k)\tau_k(t),$$

where $\tau_1, \tau_2, \ldots, \tau_n$ are the solutions of the system of differential equations

$$\tau_k - \tau_{k+1} = -\dot{\tau}_k/k - \dot{\tau}_{k+1}/(k+1), \quad k = 1,2,\ldots,n,$$

$$\tau_{n+1} \equiv 0, \text{ and } \tau_k(0) = n + 1 - k, \quad k = 1,2,\ldots,n.$$

We want to show that $\phi(0) \leq 0$. To achieve this, it will be sufficient to show that ϕ is increasing on $[0,\infty)$ and that $\phi(t) \to 0$ as $t \to \infty$. We proceed to show that these properties can be detected from the τ_k. Indeed, a fairly straightforward computation shows that

$$\begin{aligned}\phi &= \Sigma\tau_k[|b_k - b_{k-1}|^2 - 1]/k + 2\Sigma\tau_k \mathrm{Re}[k\gamma_k\dot{\gamma}_k] \\ &= \Sigma\dot{\tau}_k[|b_k - b_{k-1}|^2 - 1]/k + 2\Sigma[|b_k|^2 + \mathrm{Re}\, b_k](\tau_k - \tau_{k+1}) \\ &= -\Sigma\dot{\tau}_k|b_k + b_{k+1} + 1|^2/k, \\ &\geq 0,\end{aligned}$$

if $\dot{\tau}_k \leq 0$, for $k = 1,2,\ldots,n$ (The range of summation is from $k=1$ to $k=n$.)

The remarkable fact is that this is true! And this is where the results of Askey and Gasper enter the picture: the $\dot{\tau}_k$ can be expressed as *non-positive* polynomials in e^{-t}! Thus ϕ is increasing on $[0,\infty)$. It is easy to see that $\tau_k(t) \to 0$ as $t \to \infty$, for $k = 1,2,\ldots,n$. Also, the $\gamma_k(f,t)$ can be shown to be bounded with respect to t. Hence $\phi(0) \leq \lim \phi(t) = 0$, and de Branges' proof of the Bieberbach conjecture is complete.

REFERENCES

1. ASKEY, R. and GASPER, G.
"Positive Jacobi Polynomial Sums II", *Amer. J. Math.*, 98 (1976) 7090737.

2. BIEBERBACH, L.
'Über die Koeffizienten derjenigen Potentzreihen, welche

eine schlichte Abbildung des Einheitskreises vermitteln', S.-B. Preuss, AKAD WISS (1916) 940-955.

3. DE BRANGES, L.
"A Proof of the Bieberbach Conjecture", to appear.

4. -------------,
'Square Summable Power Series', 2nd ed., to appear.

5. ROBERTSON, M.S.
"A Remark on the Odd Schlicht Functions", *Bull. Amer. Math. Soc.* (1936) 366-370.

6. HAYMAN, W.K.
"The Asymptotic Behaviour of p-Valent Functions", *Proc. London Math. Soc.* 5 (3) (1955) 257-284.

7. MILIN, I.M.
"Univalent Functions and Orthonormal Systems", English Translation, *Amer. Math. Soc.*, Providence, R.I. (1977).

8. FITZGERALD, C.H. and POMMERENKE, Ch.
"The De Branges Theorem on Univalent Functions", *Trans. Amer. Math. Soc.*, to appear.

9. FITZGERALD, C.H.
"The Bieberbach Conjecture: Retrospective", *Amer. Math. Soc. Notices*, Issue No. 239, Vol. 32 (1985) 2-6.

10. POMMERENKE, Ch.
'Univalent Functions', Vandenhoeck and Ruprecht, Gottingen, 1975.

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