A PERSONAL ACCOUNT OF THE DISCOVERY OF HYPERBOLIC STRUCTURES ON SOME KNOT COMPLEMENTS

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Abstract: I give my view of the early history of the discovery of hyperbolic structures on knot complements from my early work on representations of knot groups into matrix groups to my meeting with William Thurston in 1976.

1. Introduction

I discovered, quite unexpectedly, the phenomenon of hyperbolic structure on three knot complements early in 1974, and managed to get two papers on the topic published in 1975. At some moment between the dates of publication of these papers, William Thurston independently discovered the phenomenon and ran away with the idea. In late June or early July 1976 he learned of my work, and so when we met later in July he immediately told me that he had been trying for about a year to prove the hyperbolization conjecture for Haken 3–manifolds.

Colin Adams published a semipopular account of knot theory in “The Knot Book” [2], and a copy of this came into my hands recently. On page 119 he gives an account of the hyperbolic structure discovery which is just plain wrong. He does get the names of the two people

1This article was written by Robert Riley about ten years before his death in 2000 and never submitted for publication. An explanation of why it is being published now and some information about Riley and this article is given in [BJS] which accompanies this article in this issue of the journal.

2Riley refers here to the first edition of The Knot Book, published in 1994. In §5.3 of the 2004 edition, published by the AMS, there is a concise, corrected account of this discovery, together with an excellent elementary introduction to hyperbolic knots.
concerned and the priority right, but nothing else. The present paper is an attempt to set the record straight. I shall relate what I did, why, and when. There will be too much detail about small matters, but this will convey the spirit of my projects. Indeed, I think my old papers were very open about my project, and a close look at them and their dates of submission should have made the present history unnecessary. Furthermore, Bill Thurston’s account of my work in [13] is entirely fair, except for being too generous about my influence on his thinking.

So below I give the history of my project from its beginning to the moment I met Professor Thurston. The story is told as I saw it, and the emphasis is on motivation and dates. Many dates are only approximate because most entries in my notebooks are undated, but the uncertainties are never more than about a month. I include an intermediate example, worked out between the discoveries of the hyperbolic structures for the figure–eight knot \((4_1)\) and for \(5_2\). This example ought not be on the main line of development, but in fact it was, and it served to undermine my initial expectation that the figure–eight is the only knot which could possibly be hyperbolic. I close with some comments on the early work of H. Gieseking and Max Dehn, and on the article [15] of W. Thurston.

2. The early years

On settling in Amsterdam in October 1966 I wrote off to virtually everyone publishing in knot theory for their reprints and preprints. I recall with gratitude that R.H. Fox and H. Seifert were especially generous. An unassuming little paper by Fox [2] written in Utrecht some 20 miles away, took my fancy. Here Fox advertised the notion of longitude in a knot group by using it, together with representations on the alternating group \(A_5\), to distinguish the square and granny knots. I was intrigued by the success of \(A_5\), and took the first steps toward writing out explicit procedures to find all \(A_5\)–representations of a knot group in 1967–68. When I got my first temporary appointment at Southampton (England) in 1968 this became my main project, with results summarized in [6, 7]. So by 1970 I was after the parabolic representations \((p\text{-reps})\) of a knot group, initially because they were easier to manage than the general non–abelian representations \((nab\text{-reps})\). The 2–bridge case is especially tractable, because the representations are governed by a simple polynomial whose rule of formation is easily programmed in Fortran. This tractability extends to all \(r\)–bridge knots which are symmetric about an \(r\)–fold axis of rotation that cyclically permutes the bridges, but most knots of bridge number \(r > 2\) are not
so symmetric. The explicit algebraic description of the equivalence classes of $p$–reps of an unsymmetric knot is so difficult that only a few examples have been worked explicitly, and I have found the full curve of all $nab$–representations of only one unsymmetric 3–bridge knot, $8_{20}$. Around 1971 I wrote some primitive Fortran programs to find the $p$–reps of a few 3–bridge knots and used the output to discover the commuting trick of [7], [11 I, II], but at the time this topic was mainly pure frustration.

In 1971 a plea for help from me was passed on to Professor G.E. Collins, the instigator of the SAC–1 file of Fortran routines for doing the kind of algebraic calculations I needed. He sent me a pile of very poorly printed manuals containing the program listings, lots of errata slips, and the advice that the 24 bit word size of the Southampton University computer would require some doubly recursive programming in assembly language. (The reference count field in a SAC atom would be too small without this recursion, and hence impose a strict limit on the allowed complexity of calculations). He also mentioned that I would need to get someone to punch up the 6000 or so cards of the 1971 SAC. Well, that someone had to be me, but fortunately only some 4000 cards, plus the assembly language parts, were needed for my application. It took about eight months to do all this, and I never did get the double recursion for the reference count right. So my more ambitious calculations were killed as soon as the reference count tried to reach 128, but I still managed to do most of what I wanted. By 1 October 1972, the day my fourth temporary appointment at Southampton ceased, I had done the elimination–of–variables part of the solving for an algebraic description of the set of $p$–reps for several 3–bridge knots, including $9_{35}$. Each SAC run required several hours of CPU time, and could not have been attempted during term time. Perhaps some distorted memory of this story is the source of the “immense computer program that was designed to attempt to show that some knots are hyperbolic” bit in Adams’ account. In fact, the PNCRE package which does just this was developed from 1976, and it was always fast enough for term time, even during the day on a grossly overloaded 1960’s computer.

3. The preparation

In October 1972 I had a large pile of SAC output which needed more computer analysis to become meaningful, and no prospect of further employment. So I spent the next three months walking the Pennine Way and walking in Wales until the prospect of a six month appointment
in Strasbourg opened up. While I was walking in the Vosges this materialized, and I was able to complete the algebraic description of the equivalence classes of $p$–reps for several knots, including $9_{35}$, cf. [11]. (I recall a puzzling difficulty with $9_{32}$ that was explained a decade later as the consequence of dropping the deck of data cards, perhaps in 1971, and reassembling it almost exactly right).

The knot $9_{35}$ has a large symmetry group (dihedral of order 12, [11]), and also an unusually large number of algebraic equivalence classes of $p$–reps, facts which I believe are related. The SAC calculations had given me a polynomial $p(x) \in \mathbb{Z}[x]$ of degree 25 which I had to factor as the first step. When one has no symbolic manipulation package available this is done by finding the roots of $p(x) = 0$ and examining them for clues. The polynomial $p(x)$ (and its relative for $9_{48}$ which was even worse) defeated several commercially produced root–finding routines, but a final resort routine succeeded, sort of, and I was able to infer factors

$$p_1 = 1 + x, \quad p_2 = 1 + 2x + 7x^2 + 5x^3 + x^4, \quad p_3 = \cdots,$$

and soon

$$p(x) = (1 + x)^{10}p_2(x)^2 \cdots .$$

Only the cubic factor remained unguessed, and of course it turned out to be the one giving the hyperbolic structure four years later. Each factor $p_k(x)$ of $p(x)$ had to be tested to see if it gave an equivalence class of $p$–reps or was spurious, and I expected $1 + x$ to be spurious. To my surprise it gave $p$–reps on

$$G_i = \left< \begin{bmatrix} 1 & 1 \\ 0 & 1 \end{bmatrix}, \begin{bmatrix} 1 & 0 \\ -1 & 1 \end{bmatrix}, A_i \begin{bmatrix} 1 & 0 \\ -1 & 1 \end{bmatrix} A_i^{-1} \right> \subset SL_2(\mathbb{Z}[i]), \quad A_i = \begin{bmatrix} 1 & i \\ 0 & 1 \end{bmatrix},$$

where $i = \sqrt{-1}$. This was in June 1973, and I probably did not understand what a Kleinian group is at the time, but I could see $G_i$ is discrete and wondered what its presentation was. Also, as I watched the printout emerge from the line printer I guessed that these $p$–reps must be an instance of an undiscovered theorem, and the same evening stated and proved the theorem. (Writing it up for publication is taking longer. In December 1991 I used Maple to extend the theorem to algebraic varieties of $nab$–reps and add some new material. In 1993 I told Tomotada Ohtsuki about this, giving no detail, and he promptly found a better proof and more new material. I hope to proceed to a joint paper soon.)

After the summer vacation of 1973 when I returned to Southampton, the professors of the mathematics department granted me the use of
an office and all university facilities, except the computer which was heavily overloaded. By then I had learned by some osmosis what a Kleinian group is and read Maskit’s paper [4] on Poincaré’s Theorem on Fundamental Polyhedra. This made progress on $G_i$ above possible, and I soon had its presentation. (I also found that Fricke and Klein had considered $G_i$, or something very like it, cf. Fig. 151 on page 452 of [3].) Success with $G_i$ led to success with the image $\pi K\theta$ of a $p$–rep of the figure–eight knot group in November 1973. Recall that

$$\pi K\theta = \left\langle \begin{bmatrix} 1 & 1 \\ 0 & 1 \end{bmatrix}, \begin{bmatrix} 1 & 0 \\ -\omega & 1 \end{bmatrix} \right\rangle, \quad \omega = \frac{-1 + \sqrt{-3}}{2},$$

so the group is obviously discrete and only its presentation was in doubt. I remember my surprise at finding this $p$–rep is faithful. The first version of my account [8] of this was received by the Editors on 30 November 1973, and it didn’t mention the orbit space $\mathbb{H}^3/\pi K\theta$ because I had not even thought of it.

Why not?! Well, the result was perhaps a fortnight old, and I didn’t have a premonition of hyperbolic structure on knot complements. Years later I learned that it had not only been thought of, but attempted and discussed privately by the Kleinian groupies since 1968. Nothing had been written and none of this had reached me. The key to seeing that the orbit space of $\pi K\theta$ had to be the figure–eight complement was seeing the peripheral torus in the orbit space. This torus occurs as the image of Euclidian plane $\Pi(t) = \{(z, t) : z \in \mathbb{C}\} \subset \mathbb{H}^3$ for any $t > 1$. In my diagram $\Pi(t)$ meets the fundamental domain not in a parallelogram but in a zigzag shape (four hexagonal discs), and perhaps the zigzag temporarily prevented me from seeing the torus. This is silly, because the stabilizer of the torus is the free abelian group $(\pi K\theta)_\infty$ generated by $z \mapsto z + 1$, $z \mapsto z + 2\sqrt{-3}$, and $(\pi K\theta)_\infty$ has to be considered explicitly during the verification that Poincaré’s theorem applies to my supposed Ford fundamental domain. But silly or not, it took perhaps seven weeks, till January 1974, for me to see the torus. Verification that $\mathbb{H}^3/\pi K\theta = S^3 – \text{fig–eight}$ took perhaps a day, and consisted of looking at my reprint of Waldhausen’s paper [16]. It seems unfortunate that this was too easy, and that I should have been forced to develop a direct geometrical argument, but once the pressure was off I didn’t want to do it. I expect that a direct geometrical construction works for all non–torus two bridge knots, and that it would prove the conjectures of [12, §4], so the matter will not be a waste of effort.
The figure–eight discovery was not decisive for me as it was for Thurston. I expected that Shimizu’s lemma, viz. 
\[
\left\langle \begin{bmatrix} 1 & 1 \\ 0 & 1 \end{bmatrix}, \begin{bmatrix} a & b \\ c & d \end{bmatrix} \right\rangle
\]
is not discrete when \(ad - bc = 1, \ 0 < |c| < 1,\) would preclude the discreteness of the images \(\pi K\theta\) of the potentially faithful \(p\text{-}reps\) \(\theta\) for all other knots. (In particular, I predicted Alan Reid’s theorem \([5]\) that the figure–eight is the only arithmetic hyperbolic knot). However, by the time I mailed off the revised version of \([8]\) that was actually accepted I had recognized the true situation, but, I suppose out of laziness, I didn’t revise \([8]\) again to make an announcement.

R.H. Fox died within a few days of the figure–eight discovery.

4. THE INTERMEDIATE EXAMPLE

I now had a beautiful discovery, and a certain fear of testing whether something similar was true for the obvious next case, the knot 52 of two–bridge types \((7, 3), (7, 5)\). Instead of going for 52 directly I temporized by taking up a different kind of example, the groups \(\pi K\theta\) associated to a cubic factor \(f(u)\) of the \(p\text{-}rep\) polynomial for the knot 811 of two–bridge types \((27, 17), (27, 19)\). To give an account of this we need to recall the basics of two–bridge knot groups and their \(p\text{-}reps\).

A two–bridge knot normal form corresponds to a pair \((\alpha, \beta)\) of integers, where \(\alpha > 1\) is odd, \(\beta\) is odd, \(\gcd(\alpha, \beta) = 1\), and \(-\alpha < \beta < \alpha\). The knot group \(\pi K\) for \((\alpha, \beta)\) depends not on \(\beta\) itself but on \(|\beta|\), so we may as well assume \(0 < \beta < \alpha\). Then
\[
\pi K = \langle x_1, x_2 : wx_1 = x_2w, \ w = x_1^{\epsilon_1} x_2^{\epsilon_2} \cdots x_2^{\epsilon_{\alpha-1}} \rangle, \tag{3.1}
\]
where \(\epsilon_j = \epsilon_{\alpha-j} = \pm 1\), and the exponent sequence \(\hat{\epsilon} = (\epsilon_1, \cdots, \epsilon_{\alpha-1})\) is determined by a simple rule, cf. \([7, 12]\). A longitude \(\gamma_1\) in the peripheral subgroup \(\langle x_1, \gamma_1 \rangle\) of \(x_1\) is a certain word \(\hat{w}^{-1} wx_2^\sigma\) on \(x_1, x_2\).

A normalized \(p\text{-}rep\) \(\theta = \theta(\omega)\) of \(\pi K\) is a homomorphism such that
\[
x_1\theta = A = \begin{bmatrix} 1 & 1 \\ 0 & 1 \end{bmatrix}, \quad x_2\theta = B = B_\omega = \begin{bmatrix} 1 & 0 \\ -\omega & 1 \end{bmatrix}, \tag{3.2}
\]
where \(\omega \in \mathbb{C}\). Indeed, \(\omega\) is a root of the \(p\text{-}rep\) polynomial \(\Lambda[u] \in \mathbb{Z}[u]\) which may be reducible but which has no repeated roots. Then the longitude entry \(g(\theta)\) or \(g(\omega)\) for \(\theta(\omega)\) is found by
\[
\gamma_1\theta = \begin{bmatrix} -1 & g(\theta) \\ 0 & -1 \end{bmatrix},
\]
and is readily computable once \( \omega \) is known. To factor \( \Lambda(u) \) without a system like SAC, Macsyma, or Maple but when a polynomial root finding package is available, find the roots and list the pairs \((\omega, g(\omega))\). Factors stand out as having pairs where \( g(\omega) \) evidently belongs to a proper subfield of \( \mathbb{Q}(\omega) \). In the case of \( 8_{11} \) we found the factor

\[
f(u) = -1 + u(1 + u)^2
\]

by \( g(\theta) = -6 \) for its roots. The roots of \( f(u) \) are

\[
\omega_1 = -1.23278 + 0.79255i, \quad \omega_2 = \bar{\omega}_1, \quad \omega_3 = 0.46557,
\]

(rounded to 5 decimal accuracy). Today this factor is explained as an instance of Theorem B of [12] and it clearly had something to do with the discovery of the theorem. I had \( f(u) \) by 1971.

By February 1974 my worries about the figure–eight knot brought me to consider the group \( \Gamma = \langle A, B \rangle, \ B = B_\omega \), where \( \omega \) is the \( \omega_1 \) of (3.3). I simply went for a Ford domain \( \mathcal{D} \) of \( \Gamma \) using graph paper, compass and ruler, and the first programmable calculator available at Southampton. (That would have cost about two months gross salary if I had still been employed). It didn’t take long to get the diagram of Fig. 1, and when the time came to think about proof the closing trick and angle sum trick of [10] came to mind automatically. As far as I know this group \( \Gamma \) is the first group proved discrete by Poincaré’s theorem where these tricks are necessary. Perhaps the first people to wonder about using Poincaré’s theorem for computation with potentially discrete groups didn’t see these simple tricks in advance, didn’t have a specific example they really needed, and shied away from getting too involved.

We give a little more detail on \( \Gamma \) and its Ford domain \( \mathcal{D} \) illustrated in Fig. 1. This is taken from an unpublished paper CPG, written in late 1974 and early 1975, doing all the discrete non–Fuchsian cases where the group \( \pi K \theta \) corresponds to a root of a cubic polynomial, viz. \( 5_2, 7_4, \) and \( 8_{11} \). The case \( 5_2 \) is worked in [11], and \( 7_4 \) is similar to but easier than \( 7_7 \), also worked in [11] but much easier.

Let \( \pi K \) be the group of \((27,17)\) presented as in (3.1), so \( \Gamma = \pi K \theta \) as in (3.2). We have words

\[
u := x_1^{-1}x_2x_1, \quad v_1 := ux_2^{-1}x_1x_2^{-1}, \quad w_1 := v_1x_1^{-1}v_1^{-2}x_2^{-1}v_1x_1^{-1}.
\]

The word \( w \) of (3.1) is \( w_1x_1 \), so \( w_1x_1 = x_2w_1 \) holds in \( \pi K \). These words \( u, v_1 \) were found by straightforward search of subsegments of \( w \) to correspond to spheres carrying sides of tentative Ford domains. The search for the sides of a fundamental domain has to be guided by some
Figure 1. The Ford domain \( \mathcal{D} \), copied from G.P.G (I get a new plotter soon).
principle, since a Cantorian exhaustion is too slow, and segments of \( w \)
worked well, both here and later for all two bridge knots.

We found easily that the elements

\[
A = x_1 \theta, \quad U = u \theta, \quad V_1 = v_1 \theta, \quad W_1 = w_1 \theta, \quad V_2 = U^{-1} W_1
\]

seem to be the side pairing transformations of the tentative Ford domain \( D \) of Fig. 1. Thus we read off from Fig. 1 a proposed presentation for \( \Gamma \): generators \( A, U, V_1, V_2, W_1 \). relations

\[
W_1^2 = V_1^3 = V_2^3 = (A^{-1} V_1)^2 = (A^{-1} V_2)^2 = E,
V_1 = W_1 U^{-1}, \quad V_2 = U^{-1} W_1, \quad U = A^{-1} W_1 A W_1 A^{-1}.
\]

To use the closing trick and angle sum tricks of [10] it is necessary to verify directly that these relations hold in \( \Gamma \). For this it helps to see copies of the modular group \( SL_2(\mathbb{Z}) \) in \( \Gamma \). Let

\[
A_* := \begin{bmatrix} 1 & u + u^2 \\ 0 & 1 \end{bmatrix},
\]

then

\[
V_1 \equiv A_*^{-1} \begin{bmatrix} 0 & -1 \\ 1 & 1 \end{bmatrix} A_* \quad \text{and} \quad V_2 \equiv A_* \begin{bmatrix} 1 & -1 \\ 1 & 0 \end{bmatrix} A_*^{-1} \pmod{f(u)}.
\]

So \( \langle A, V_1 \rangle \) and \( \langle A, V_2 \rangle \) are conjugate to \( SL_2(\mathbb{Z}) \) in \( SL_2(\mathbb{Z}[\omega]) \). All the proposed relations now can be verified by straightforward computation in \( SL_2(\mathbb{Z}[u]) \) modulo \( f(u) \). Then the arguments of [10] show that \( \Gamma \) is discrete, \( D \) is a fundamental domain for it, and that these relations present the group. This made a good confidence–building exercise for me, and might do the same for other people. Note that this \( D \) is simpler than the Ford domain for \( 5_2 \) discussed in [11], so \( \Gamma \) really is an intermediate example.

5. Completion of the discovery

This procrastination had now given me a bigger worry which can be put thus: Why should the Great Lord have performed a unique miracle to make \( \Gamma \) discrete, for no visible reason at all?! The answer is compelling: He didn’t! If \( \Gamma \) is discrete then many other groups have to be discrete, in direct defiance of Shimizu’s lemma, and, since each case of discreteness requires a good reason, there must be general theorems explaining this discreteness. It is a little ironic that this prediction was amply vindicated for (suitable) 3–manifold groups, but, at this writing, the general theorem explaining the discreteness of \( \Gamma \) has not been stated, let alone proved.
During a few weeks further procrastination the above considerations compelled me to predict that a knot in $S^3$ is hyperbolic unless it clearly was not. Early in March 1974, I think, I finally went to work on $5_2$ and in a few hours had confirmed my prediction. This completed the essential part of my discovery, and all later cases, such as $7_4$ and several links, were just routine examples at most illustrating matters of secondary importance, such as the symmetries of a knot. In fact, for a while I was confused by the symmetries and thought that a too–rich symmetry group would preclude the hyperbolic structure, but I eventually found my mistake. So by late 1974 I had gotten it right: a knot is hyperbolic unless its group contains a noncyclic abelian subgroup which is not peripheral. Making bold sweeping conjectures is unnatural for me, and I didn’t venture to predict anything about arbitrary 3–manifolds. I suppose that I might have predicted which 3–manifolds were hyperbolic had someone pressed me on the issue in conversation, but I was too isolated and unknown for that to happen. The locals at Southampton were rather cool about the whole project, except for David Singerman. He liked it enough to propose that we try to get the Science Research Council (of Great Britain) to support me on a hyperbolic project at Southampton University while I got my Ph.D. and looked for a permanent job. His plan was to time the submission of the proposal so that the referee would be at the summer 1975 conference on Kleinian groups at Cambridge where I would publicize hyperbolic structure. Whether or not the plan worked, the Kleinian groupies liked my examples, especially because these examples pointed up the importance of their own work. The SRC did fund the project generously, ultimately for four years 1976–1979.

The first two years of the project were devoted to the development of the system PNCRE \cite{10}, a file of Fortran subroutines to compute with explicit subgroups of $SL_2(\mathbb{C})$. PNCRE was not easy to develop and its first output came early in 1977. Meanwhile, about March 1976, a colleague gave me a preprint of Thurston’s lecture \cite{13} on foliations of surfaces. This was the first I heard of him, and I recall that on reading it I became certain that he and I would never share any common mathematical interest. In late June 1976 a friend drove me up to the University of Warwick to hear a lecture by J. Milnor on topics like Sarkovskii’s theorem. Directly he was finished I very nervously (read: scared stiff) introduced myself to him and told him about examples of hyperbolic knots/links. He was interested, and asked a number of direct questions, so that in a minute he understood the status of my project (examples only). I did not guess that he already knew something about
the matter. I was so scared that when he asked me to repeat my name I simply ran away. But perhaps even before we got back to Southampton that evening, Milnor had asked the locals who in Britain was interested in hyperbolic structure on knot complements, and directly afterwards Thurston had his hands on my two papers. If not, he did when I sent my papers to Milnor the next week (early July 1976).

Later that month I was invited, to put it mildly, to spend a week in David Fowler’s home in Warwick. His wife is French, and she felt that that year she simply had to bring the children to France to meet their relatives. She naturally had the house and garden filled with beautiful plants which need constant watering. The summer of ’76 was a famous drought in which the water shortage was so severe that the only legal water for plants was used bath water. Hence the urgent need to have the Fowler’s home occupied every night, and David Rand, who had been a student at Southampton and was taking up a lectureship at Warwick, put me down for one week.

On my arrival in the common room of the Warwick Mathematics Department, David Epstein sprang up and asked me who I was. He had seen my face on numerous occasions over the years, most recently when I sat directly behind him in Milnor’s lecture, and wanted to know. On hearing my name, a tall man sprawled over three chairs sprang up. He said he was Bill Thurston, that he wanted to meet me, and that for about a year he had been working on a general conjecture which included everything I was doing. The shock was immense. I am afraid that I react badly to surprises, and I became quite unpleasant for the rest of the week. Fortunately Bill didn’t hold it against me later. His later statement (page 177 of [15]),

“...; and I have not actively or effectively promoted the field or the careers of the excellent people in it.”

was either not written with me in mind or he judges me not to satisfy the qualification. He certainly did advance my career actively: strong letters of recommendation, several thousands of dollars from his Waterman Fellowship, inclusion in the 1980–81 Thurston–Sullivan NSF project at Boulder, and a trip to Binghamton at my request. I owe everything to the people who have so generously supported me over the years when I needed help most: H.B. Griffiths, David Singerman, and Bill Thurston, and I am deeply grateful to them all.
6. Hindsight

The question is: Why did the explicit discovery of hyperbolic structure on at least some knot complements wait until 1974? Wilhelm Magnus told us that H. Gieseking, in a thesis written in 1912 under the direction of Max Dehn, considered a group $G_1$ of hyperbolic isometries of a ball model $B^3$ of $\mathbb{H}^3$ and certain of its subgroups. The fundamental domain for $G_1$ is a regular ideal tetrahedron $T_1$, and $G_1$ contains orientation reversing elements. Gieseking considered the orientation preserving subgroup $G_2$ of index two whose fundamental domain is two tetrahedra glued together along a face, without recognizing that $G_2$ is isomorphic to the figure–eight knot group and its orbit space is the figure–eight complement. Magnus told me that Dehn considered these groups only as exercises in geometric symmetry: the geometric description of $G_1$, $T_1$ is so simple that Poincaré’s theorem simply has to apply directly. If Dehn had known that the figure–eight knot was involved he certainly would have had Gieseking publish, he would have given the matter the greatest publicity, and the development of 3–manifold theory would have gone very differently. So why did they not recognize the figure–eight complement?

I propose an answer to this question analogous to my own experience: I didn’t see the peripheral torus for several weeks but when I did I knew what I had to have. I began with the figure–eight and had it in mind. They began with an exercise in symmetry and had nothing further in mind. Furthermore they would have to ask the question: for $\epsilon > 0$ let $S_\epsilon$ be the 2–sphere in $B^3$ with centre 0 and radius $1 - \epsilon$. Then $G_1$ maps $S_\epsilon$ to itself, so what is the orbit space $S_\epsilon/G_1$? With hindsight the answer is obvious: a Klein bottle. Dehn would have answered this question easily once it had been raised, and I feel certain the Klein bottle would have disturbed him deeply. The result would have burned within him until he was driven to get to the bottom of the matter, and somehow he would have found the figure–eight. They had about two years to do this before the Great War of 1914–18 swept Gieseking to his doom. Dehn’s good students were probably all destroyed, and most likely Dehn was so distraught at their loss that he couldn’t bear to think about his joint projects with them any longer.

As far as I know, the next time critical examples of hyperbolic structure on 3–manifolds should have been found was in the late 1950’s, during the period of euphoria caused by Papakyrikopoulos’ breakthrough with his proofs of Dehn’s lemma etc. The topic was definitely thought of, but nothing happened, perhaps because the man concerned did not
have anything specific to work on, and he certainly had a lot of other important projects to pursue. In 1968 a Kleinian groupie wondered whether a knot complement could be hyperbolic, and chose as example to test this idea the trefoil knot. He soon found it didn’t work and was discouraged. (Actually, the trefoil complement does carry hyperbolic orbifold structures of infinite volume, but nobody wanted that). In the early 1970’s he actually visited Southampton University and met me, but somehow the crucial topic didn’t come up in the discussion. If it had I would have put him onto the figure–eight and even given him the exact matrices to use. I could not have done the calculation with Poincaré’s theorem at the time (he could), but I did have Waldhausen’s paper to help with identifying the orbit space.

I would like to close by quoting a paragraph from page 175 of Thurston’s essay [15].

“Neither the geometrization conjecture nor its proof for Haken manifolds was in the path of any group of mathematicians at the time — it went against the trends in topology for the preceding 30 years, and it took people by surprise. To most topologists at the time, hyperbolic geometry was an arcane side branch of mathematics, although there were other groups of mathematicians such as differential geometers who did understand it from certain points of view. It took topologists a while just to understand what the geometrization conjecture meant, what it was good for, and why it was relevant.”

Well, this is not quite right. For one thing, it is too strongly put. When I met them in 1975 the Kleinian groupies had been knowledgeable about the hyperbolization conjecture for Haken manifolds for at least a couple of years, but they saw it as too much for themselves. For another, what really took people aback was the speed with which the task was completed (excepting the write–up). Thurston simply didn’t give anyone starting from my examples the time to get involved. And few serious mathematicians would look at one modest example of something pretty and immediately formulate the most sweeping conjecture for 3–manifolds which could possibly be true, and then plunge in. Thurston’s success at doing this is his own personal triumph, and not a closing out of a golden opportunity that the rest of us were fool enough to lose.

I had thought of saying something about the history of Bill Thurston’s thinking about hyperbolic structure in the two years before we met, but I am afraid to repeat Colin Adam’s mistake. There are rumours that he initially thought the hyperbolic structure for the figure–eight was impossible, because of difficulties with the lift of a Seifert surface to $\mathbb{H}^3$. 
and that he discussed these matters with William Jaco at a conference. The story continues that when Bill got back to Princeton he found his supposed contradiction disappear (the lift of the Seifert surface meets the sphere at infinity in a Peano curve), that this completely reversed his expectations, and that he first got the figure–eight out of an example of Troels Jørgensen. I cannot vouch for any of this.

References

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