

REMARKS ON THE HISTORY OF THE CLASSIFICATION OF KNOTS

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About one hundred years ago “modern” topologists began rigorously to examine the tables of knots previously compiled during the last quarter of the Nineteenth Century. It took a lifetime, about seven decades, for them to work their way through to the end. This, it turned out, was just a bare beginning. The first knot was proved to be unknottable in 1906. There are now more tabled knots than people on the planet.

I was pleased to play a part in that remarkable enterprise, despite the fact that I am not a professional mathematician. Some personal background may help the reader understand how and why this came to be.

1. Primary education. Bored in a fifth grade math class, I was passed a copy of *Astounding Science Fiction* magazine which happened to contain a serious article on the Klein bottle. One look at the picture and I was hooked on topology.

2. Secondary education. Never much good at calculations (terrible at arithmetic and at best mediocre in algebra) I managed to squeak into an accelerated math program at my high school despite the fact that I flunked the “algebra aptitude test.” We were taught for three years straight by a teacher who told us on the very first day that (1) there would be no homework, (2) a great deal of ground would be covered in the classroom and (3) we had better pay close attention as he intended to teach it all very fast. This was long before, and quite the opposite of, “no child left behind.”

A decade later he showed up on the platform, as I was graduating from law school, to be honored by Harvard University as one of a handful of America’s most outstanding secondary school educators. Later that afternoon he scolded me ferociously for not pursuing a career in mathematics.

His name was James D. Bristol and his oft repeated motto was “the good mathematician is a lazy mathematician” by which he meant always look for the easiest way to

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the answer. This was advice I was happy to follow being elsewhere constantly criticized, correctly I hoped, as “bright but lazy.”

His teaching and the resultant standardized test scores (second in math in the State of Ohio for both my junior and senior high school years) propelled me on to Princeton where the world’s best topologists were focused on knot theory.

3. College. As an advanced placement freshman I was allowed to take the sophomore mathematics prize exam, where again I came in second. The following year, taking it again as a sophomore, I completely washed out. A greater power was telling me: forget about becoming a first rate mathematician. So I coasted through mostly graduate math courses my junior and senior years (an easy way to get good grades) and went on to law school.

My senior thesis, however, was of considerable interest to my advisor, Ralph Fox, who had posed the problem that it solved in the following terms: “Read the last section of Reidemeister’s book and see if you can figure out how he calculates those linking numbers.” Since calculation ranked lowest in my arsenal of mathematical weapons, I decided to take a really close look at the cut-and-paste geometry of covering spaces. The answer, of course, was clear and obvious, as it usually is in matters geometric, although it took me two days and nights at a blackboard to figure out just what I was looking at.

At Fox’s suggestion, I managed to put this on a computer which spit out linking numbers between branch curves of irregular knot covers faster than anybody knew what to do with them, and in a way that necessarily hid from view the manner in which it was calculating them. (I think there’s no such thing as binary geometry.) It’s an obvious fact that the linking numbers of a product knot will be the sum of those of its factors, so I was quite satisfied with the reliability of my program after feeding it the product of a couple of knots with fractional linking and seeing that it could correctly add fractions.

Fox was not so easily convinced. Having no way himself to confirm my results, he fell back on homology of the covering space, which he could calculate with great difficulty algebraically, and which for fractional linking could provide a partial check (of the denominator only). On more than one occasion I was called to his office because his calculations showed I had to be wrong. I watched (pretending to understand) while he filled several blackboards with algebraic symbols and equations before arriving at a corrected answer that was consistent with the computer’s results.

It was obvious that no one really knew what I was doing. Sometimes even a great mathematical enterprise must rely on trust. My thesis jury had no questions about my methods, although Stallings did ask whether a little drawing behind the title page had any mathematical significance. It didn’t. But still, it was a very good question and it got me thinking that I was missing a lot of good geometric information by letting the computer do all the heavy lifting. Later, at law school, I thought more about it and found some easy ways to “see” the linking of branch curves.

Don’t let anyone tell you that legal reasoning is difficult and time consuming. It’s mostly illogical and thus quite confusing, but the upside is that almost any argument can persuade a judge, when skillfully presented. I recently read, I think in the “Economist”

newspaper, a psychologist's account of a barrister describing the prerequisites of his profession. First, one must be willing to take either side of any dispute, which means a prospective candidate must be totally amoral. Second, one must be highly persuasive. This, the barrister correctly concluded, is the psychological profile of a sociopath.

As I was preparing to leave Princeton for law school, I had a very nice conversation with Fox where he agreed that I was doing the right thing. It seems he had been drawn to a career in music, but recognized it as something he could always do on the side, which he did in a classy quartet with Einstein on the fiddle. He noted that law required professional credentials while anyone interested could practice mathematics without a license. He also mentioned that he intended to write a paper on recent knot theoretical work at Princeton, his own and his students', encouraging me to stay in touch.

4. Law school. Having time on my hands (disdaining to read all those boring legal precedents) I thought about Stalling's question, looked closer at Wirtinger's covering space decomposition, and worked out a way to see from the diagram a surface with one of the branch curves as boundary. Its intersections with the other branch curve were obvious and suddenly I had (in most cases) an easy way "see" the linking between them, all without computers, simultaneous equations, or any real calculations at all.

I wrote to Fox explaining all this in November 1964. He just wasn't interested. (Nor, I later found out, was any serious journal.) I suspect that simple cut-and-paste topology is beneath the dignity of serious mathematicians. Surely there's a greater sense of accomplishment when you do something the hard way, with pages and pages of algebra.

Enjoying the visual geometry, however, I pressed on to look at the 4-fold covers, where meridians were mapped to simple transpositions. These were much more loosely knit and the surfaces I was looking for were almost always obvious. Non-cyclic coverings were in general hard to find, but it turns out that I had already done the work to find these, since they corresponded nicely, if they existed at all, to the 3-fold covers I had already tabulated. The trick was to use the homomorphism of S_4 onto S_3 that was built into notation symbolizing transpositions as $1 = (23)$, $2 = (13)$, $3 = (12)$, and $-1 = (14)$, $-2 = (24)$ and $-3 = (34)$. I wrote to Fox again in January 1965. Still no interest.

As I began to produce tables of linking numbers in these 4-fold covers, however, things changed dramatically. Fox was trying to find a counterexample to the Poincaré conjecture among the limited range of then known non-cyclic covering spaces of knots, and these "simple" 4-fold covers, which nobody had really looked at before, were almost always simply connected (for tabled knots).

In November 1966 I met with him in Princeton and handed him my latest list of linking numbers in about 250 examples (everything through the alternating 11-crossing knots). His response was to mail me a table of over 250 links.

5. Knots on Wall Street. After Law School I took a job with a wonderful law firm, which really stood out from the money-grubbing pack that came to Harvard looking for fresh talent. Firmly rooted in a more gracious past, it offered a daily tea time not unlike that at Princeton, and four weeks of annual vacation – five, if one traveled to Europe by sea.

The firm's big star was John J. McCloy, often referred to as Chairman of the Board of The Eastern Establishment, and its principal client was the Rockefeller family and their various interests such as Chase Manhattan Bank. My lips are sealed by the attorney-client privilege, but I think I am allowed to reveal that I once was sent to the State Department to examine secret files on corporate bribery of a friendly foreign government, which the culprit regularly reported to the U.S. Ambassador. I had previously traveled through most of East Germany, on a trip arranged by its then foreign minister, after I sent him a letter complaining that only group tours were permitted and he promptly made me a group with one member. Yet the only security check between me and the Ambassador's classified reports to headquarters was my answer to the question: "Are you the guy McCloy sent?"

I was pleasantly surprised by how liberating it was to trade schooling for a so-called full time job. Evenings, weekends, holidays and vacations now belonged fully to me, with no need to study or fret about exams. These were without a doubt my most productive years in mathematics.

In 1967 I received a draft of Fox's paper which appeared in 1970, along with Conway's which corrected and expanded the Nineteenth Century knot tables — new grist for my mill just as I was beginning to enjoy seeing my name in print. It was clear from Fox's paper that a lot more could be said about non-cyclic covering spaces and that his program for classifying tabled knots with the help of his students' computerized invariants was running out of steam. While continuing to send him new theoretical results, arrived at as a byproduct of calculating linking numbers not previously looked at, it occurred to me that I had the necessary tools to tackle for myself the problem of distinguishing most if not all of the 10-crossing knots.

This was a task that most mathematicians weren't at all interested in. It offered no new or generally applicable insights and certainly smelled like the dregs of their profession — applied mathematics. Recall that this was long before the computer geeks were taking home billions. Even Reidemeister, when I met him in the Summer of 1964, sort of brushed aside the work of Bankwitz (and Schumann) in completing the classification of tabled knots through 9 crossings. He was nonetheless a delightful human being, having immediately invited me to his home for tea on the strength of a phone call saying Fox suggested I meet him.

In March 1972 I got a letter from Fox enclosing comments by Murasugi on all the stuff I had been sending him, adding: "It seems to us that your results are worth publishing. Since you haven't had any graduate training in mathematics, and since you don't have a mathematical library at hand, this may be a difficult thing for you to do, but I have a suspicion that you would like to try it, and if you do want to do it I will be here to help you when you want help." This was the beginning of my post-undergraduate mathematical education — a correspondence school with an illustrious faculty that originally consisted of Fox and Murasugi and ultimately expanded to include Birman, Cappell, Caudron, Lickorish, Montesinos, Siebenmann, Thistlethwaite and Trotter.

Fox took a look at my work relating 3- and 4-fold coverings of 3-bridged knots and extended it to the general case; Murasugi and I exchanged tables of linking numbers

and signatures, which soon were found to be related for 2-bridged knots; Montesinos had a different way of numbering the sheets in a maximal (regular) dihedral covering, which led me, by comparing it with my own, to find a nice formula relating the linking numbers in any irregular dihedral cover; Caudron and I exchanged data on duplications and omissions in his and Conway's 11-crossing knot and link tables; and Thistlethwaite, finally computerizing the enumeration and classification of knots, found a number of mistakes in my numerical results, not because my methods were flawed but simply because I looked carelessly at the pictures. It was an exciting ride for me and I learned what I could from experts working way above my level.

Trotter, Cappell, Birman, Lickorish, and Siebenmann offered mostly encouragement. Collectively they covered all the major bases of mid-20th Century knot theory research. Trotter (who I'm told refereed an early paper, and probably had to convince some skeptical editor that this uncredentialed upstart knew what he was talking about) kept me abreast of the work done at Princeton, while Cappell mentioned my results in several important papers, sparking a good bit of interest from others. Birman, Siebenmann and Lickorish invited me to explain it in seminars at Columbia, Orsay and Cambridge (where I first met Conway) and Siebenmann was quick to apprise me of his and Bonahon's great work on arborescent knots, which he knew, or thought probably, would enable me to complete the classification of all remaining tabled knots in 1979. But I am getting ahead of myself. Let's go back to Fox's suggestion that my work should be published.

6. Writing mathematics. In April 1972 I sent Fox a 23 page draft — a rambling discourse on a variety of topics, stitched together only by the fact that they all involved looking at non-cyclic covering spaces of knots. In retrospect, it's quite an embarrassment. Screwed up in its terminology, short on obvious generalizations and long on pictures and examples, it was hardly the sort of thing that anyone would want to put in print. Its main result was the unsurprising finding that the only amphicheiral knots through 9 crossings were those already known.

Fox and Murasugi made numerous suggestions for improving it and subsequent drafts, while I proceeded to crank out more examples, focusing on distinguishing the few then remaining undistinguished 10-crossing knots. In January 1973 I hit pay dirt: a duplication in Little's non-alternating list and successful classification of all the rest. This struck me as rather important. Fox and Murasugi, not so much. While they continued to offer helpful comments, I looked for a way to stake my claim promptly to this surprising result.

I tried submitting a short announcement to AMS, but wasn't a member. A friend of a friend who was wouldn't sponsor me in the Notices, since she was unfamiliar with knot theory and concerned that what I had to say might already be known. She suggested that I join on my own. It turns out you not only don't need a license to practice mathematics, you can actually publish, unedited, without one, provided you have enough spare change to pay your AMS dues.

Meanwhile, I attended Fox's 60th birthday celebration in Princeton and hobnobbed with his colleagues and former students. I vividly recall his short thank-you speech, where he waxed philosophical on the meaning of life, quoting the insight of some oriental guru. The universally long sought answer was, it turns out, "nothing."

Fox passed away at the end of that year, but not before giving my efforts to publish an important, if somewhat reluctant, boost.

At one point in our correspondence he observed that the results I was getting were so unrelated that he was “tempted to suggest” a number of short papers. That was all I needed to hear. With my wife’s encouragement, I shot off an initial round of two short papers: one, which was just the methodology of my thesis, to a far away journal that I had never heard of, and the other, the good one, to AMS Transactions. The latter was promptly returned as too short. The first went to Fox to referee.

Pronouncing it “pretty thin” in correspondence with me, he nonetheless blessed it for publication, noting to me, in words that sound good when quoted entirely out of context: “I couldn’t recommend it to a more serious journal.”

AMS Proceedings accepted my second paper, a self-serving announcement of which had already appeared in the June 1973 Notices, and soon I was receiving requests for preprints from an impressive array of experts. My favorite was the U.S. Commerce Department’s National Bureau of Standards. I guess they like to keep track of everything. They never followed up, however, with requests for any of my later papers, one of which actually attracted the attention of the I. V. Kurchatov Institute of Atomic Energy in Moscow. I sent it out hoping it might distract them from more deadly serious work.

When Fox passed away in December 1973, Neuwirth put out a call for papers from former colleagues and students for a memorial volume of *Annals of Mathematics Studies*. I offered a third short piece on octahedral coverings, incorporating with attribution Fox’s suggestions and wrapping it in some not all that relevant references to important work of Hilden and Montesinos — a tawdry form of mathematical piggy-backing that seems to work well for the pros. Now I find its result referred to as “a classical theorem of Perko.”

My fourth paper traveled a bumpier road to publication, having been rejected by two separate journals. The first was kind enough to suggest that it was facing page limitations and its actions ought not be interpreted as any reflection on the merits of the piece. This had to be a form letter. The paper was six pages long.

The second rejection came from a journal that apparently regarded itself as more selective than *Inventiones Mathematicae*, where the paper ultimately appeared. Where are the scholarly articles that discuss how to wend your way through this crazy maze of journals? I’m reminded of the strange question once asked of me: “Why did you go to law school at Harvard?” The answer, stranger still, is: it was the only one that accepted me.

Let me digress for some history on the subject of that fourth little paper.

7. Die Entstehung und Vollendung der Diederischenknotenüberlagerungsverschlingungszahlentheorie. Reidemeister was the first to recognize the value of non-cyclic knot coverings. He used them (and the results obtained by Bankwitz and Schumann) to reach beyond the cyclic invariants of Alexander and Briggs and distinguish three previously undistinguishable polynomial pairs in their table of knots. His work in this regard was limited to those classes of knots for which the fundamental group was then relatively well understood (2-bridged, torus and pretzel knots) but the full range of those coverings, arising from representations on dihedral groups, extended well beyond

that. He knew this but must have run into the same algebraic complications as Fox, who wrestled with them with limited success for many years, looking for a counterexample to the Poincaré conjecture.

In a 1956 lecture at Haverford College Fox explained in simple terms how to find all dihedral representations of any knot. The method, now known as “Fox coloring,” is to symbolize half the elements of the dihedral group of order $2n$ as integers (mod n) and look for an assignment of an integer to each segment of a knot diagram such that at each crossing the sum of those assigned to the underpasses equals (mod n) twice that of the overpass. He hid this quite well, however, in his textbook on knot theory, where the only hint of it (in a haystack of polynomial algebra) is Exercise 8 on page 133. I managed to pick this up in class, however, and used it with geometry to look at the construction of, and linking of branch curves in, such coverings.

At the time non-cyclic coverings were rarely studied. Burde, Montesinos and Riley seemed to be the only ones looking at them: Burde, algebraically; Riley, through the dark lens of computer calculations; and Montesinos, bless him, following the yellow brick road of geometry.

In October 1973 Montesinos wrote to me to request a copy of my 1964 senior thesis. (I take great pride in the fact that he and Thistlethwaite were, to my knowledge, the only two scholars to delve into that document early in their careers and walk away with a full understanding of just how simple it was; the latter later writing an expository paper to explain it.) This was the beginning of a correspondence with Montesinos from which I derived enormous benefit. A difference so simple as his way of numbering the sheets in a regular dihedral covering led me immediately to a full understanding of the geometric relationships among all dihedral coverings and the obvious equations relating their linking numbers.

My advice to all aspiring mathematicians, which the best already know: “Talk amongst yourselves.”

8. Those elusive 11-crossing knots. Having finished off the 10’s, I turned my attention upwards. The first problem was that Conway’s table was incomplete, although nobody wanted to talk about it. His work was just so great (and great it was) that looking at what he left out seemed to be an impermissible form of lese majeste.

Knowing where I was going, in 1976 Trotter sent me the results of David Lombardero’s 1968 Princeton senior thesis, explaining that it was based on an unpublished draft of Conway’s 1970 table that Conway had sent to Fox. (How nice it was to be part of the Princeton topological grapevine — once described to me by Murasugi as “our group.”) Comparison of the two, and calculation of a few invariants, quickly revealed that a couple of knots were missing from Conway’s published list. One of them, perhaps significantly, the one distinguished in Lombardero’s list by its Alexander polynomial, can be matched to the typographical duplication included among Conway’s non-alternating 11-crossing knots. The other was apparently just dropped by the wayside, along with a handful of others in Conway’s draft that actually deserved such treatment.

Three of those knots that Conway correctly discarded were duplications of the same type that I found three years later in Little’s table of 10-crossing non-alternates — i.e.,

counterexamples to Little's false theorem on the invariance of writhe. This false theorem, blessed by Tait, Dehn and Heegaard, is now widely known as a "Tait conjecture" although without all those misbehaving non-alternating knots.

Ge-kwan in a Chinese blog, drawing from millennia of accumulated wisdom, has described my discovery of the duplication in Little's 1899 paper as the result of "strong interest in mathematics and extraordinary luck." No one can argue with that; not because the discovery wasn't inevitable, but because Conway didn't do it first.

I quickly spread the word that there was reason to question the completeness of Conway's table of 11-crossing knots, and in the Fall of 1978 received correspondence from Alain Caudron in Tunisia, which included preliminary versions of his tables of knots and links. Checking them out, I saw a duplication in the link table and a couple of new knots, not listed by Conway or Lombardero, which were easily distinguishable from those that were.

In subsequent years I've read many different accounts of the tabulation of 11-crossing non-alternating knots; some saying Conway listed them all, others that Caudron found four missing knots, and even one that attributes the four to me, I guess because I was the first to write about them. Thoughtful readers will not be surprised that in such a situation the truth is "none of the above."

9. Classifying the 11's. Toward the end of the 1970's I had pretty much given up on classifying all the 11-crossing knots. There were just too many polynomial duplicates, the invariants I knew weren't applicable to all of them, they had yet to be proven prime (which wasn't easy in a lot of cases) and my interest in the entire enterprise was beginning to wear thin.

Then along came Bonahon and Siebenmann, who turned the geometric insight of Conway and Caudron into actual topological invariants, classifying in one stroke (of 351 pages) the vast majority of tabled knots — i.e., the "algebraic" or "arborescent" ones. This was a breakthrough tantamount to the complete understanding of 2-bridged knots, arrived at by Schubert decades before.

At Siebenmann's urging, I finished off distinguishing the rest in March 1979 and finally proved primeness for the remaining 4-bridged knots. The latter result, which I thought trivial but seemed to be of interest to others, was arrived at by what is still my favorite proof (by contradiction): it concludes by demonstrating geometrically that if a particular covering space torsion number cannot be divided by 3, then it is 3.

That was the end of my mathematical work, except for fixing up a handful of miscalculated invariants that Thistlethwaite (privately) called to my attention, his computers being much better than my eyesight at reading numbers off of knot diagrams.

Thereafter, I reviewed papers for MR for about a decade, until they dropped me for trying to out a referee, whose fingerprints were all over suggested changes attributed to him anonymously. This was not unwelcome at the time, as I was falling far behind in efforts to keep up with a fast growing subject.

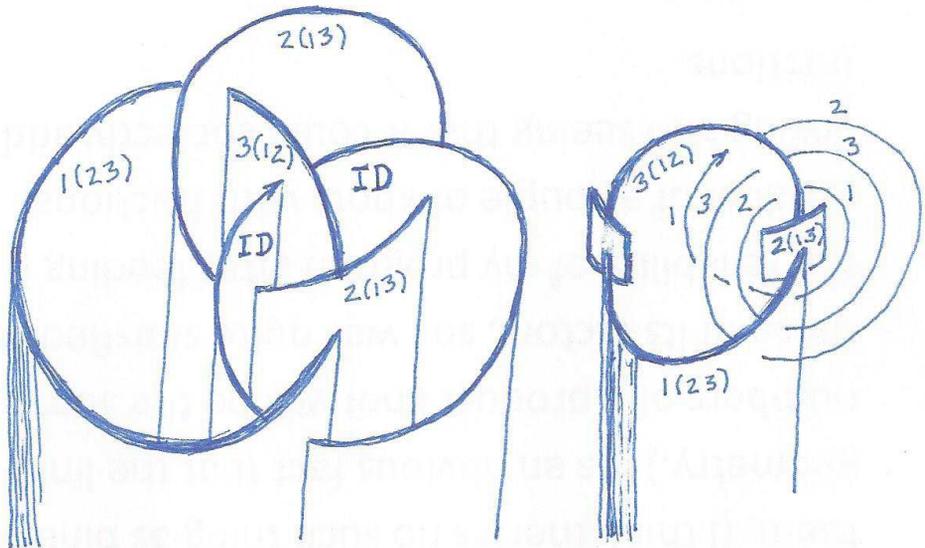
Conclusion. Counting the knots has always been a collaborative effort, starting with Tait, Kirkman and Little and continuing, through Reidemeister, Bankwitz and Schumann, to the present day. My methods had the advantage of what Conway once called “the random junk property” which left me as the clean-up hitter, until the big computers took over.

It’s nice to look back on how much could be accomplished with pencil and paper, and in a way that’s really not all that hard to understand. It’s also nice to think that the results will last as long as anyone is interested in knots.

Appendix: Borromean rings example

Admiring the prose with which Reidemeister disposed of the 9-crossing knot problem (in beginning the final paragraph of “Knotentheorie”), we wrote in Zbl 282.55002 (1975): “Ubrigens lassen sich die Borromaischen Ringe durch die Zahl der Verzweigungen einer geeigneten dreifachen Überlagerung als untrennbar erkennen.”

Here’s what we had in mind, “ID” representing the identity permutation (1)(2)(3).



Pay no attention to linking numbers, which are easy to see but all zero and no help.

Instead, just count the (surprising) number of branch curves.

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