CHAPTER 11

Geometric Aspects in the Development of Knot Theory

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1. Introduction

§ 1. Among the most widely noticed achievements of knot theory are certainly the famous knot tables produced by the Scottish tabulating tradition in the late 19th century, the polynomial invariant invented by James W. Alexander in the 1920's, and the series of new polynomial invariants that came into existence after Vaughan F. Jones discovered a new knot polynomial in 1984. It might seem that these results easily fit into a story centered around plane knot diagrams, symbolical codings of such diagrams and the operations one can perform with them, and combinatorial techniques to draw conclusions from the information that is thereby encoded.[1] In this contribution, I will first outline such a narrative and then show that it fails to account for important causal and intentional links in the fabric of events in which these achievements were produced. Indeed, a striking feature of knot theory is that, even if a significant number of its results may be stated and proved in a direct, combinatorial fashion, the research that produced those results was often motivated by and directed toward geometric considerations of varying complexity. In many cases, these geometric ideas alone provided the links to other topics of serious mathematical interest and thus could induce mathematicians to devote their time to knots. Moreover, only by taking into account the surrounding geometric aspects can historians reach a position from which they may judge the relations between the steps in the formation of knot theory and the broader mathematical and scientific culture in which these steps were taken. These relations form part of the causal weave that needs analysis in order to attain a historical understanding of Tait's, Alexander's, or Jones's results.

§ 2. In what follows, I will pursue this subject in five steps (corresponding to Sections 2–6). In Section 2, some of the relevant combinatorial aspects of the history of knot theory will be sketched. This account is mainly intended to anchor the events relating to combinatorics in the historical chronicle, and to highlight the kinds of questions that remain unanswered if the history of knot theory is presented in a perspective that concentrates

[1] A sketch along these lines has been published by Przytycki [131].

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exclusively on combinatorial issues. In Section 3, the main geometric ideas in the background of the first mathematical treatments of knots, up to and including the tabulations of Peter Guthrie Tait and his followers, will be discussed. It will be seen that, in an important sense, the knot tables of the 19th century did *not* represent an isolated and curious effort in the hardly existent science of topology. Section 4 will then be devoted to the making of what may be called *modern* knot theory in a historically specific sense, based on the new tools of Poincaré's *Analysis situs*, the fundamental group, torsion invariants, and Alexander's polynomial. Section 5 takes up the difficult task of choosing and describing some developments concerned with the further investigation of knots in the period up to Jones's breakthrough. Besides the background to his new invariant, I will mainly focus on researches formulating innovative ideas on knots as genuinely three-dimensional objects, rather than as objects given by diagrams. I will return to my general theme in the concluding Section 6, in a brief attempt to assess the historical role of geometric aspects in the mathematical treatments of knots.

§ 3. Due to limitations of competence and space, the selection of topics discussed cannot be exhaustive and may perhaps not even be representative with respect to the main theme of this article. This holds both with respect to the description of mathematical ideas and – even more so – with respect to the causal and intentional saturation of the historical narrative.[2] Among the many mathematical issues I have *not* dealt with are results about special classes of knots, investigations relating to the finer structure of knot groups, knots in higher dimensions, and the relations between knots and dynamical systems.[3]

It must also be emphasized that the following remarks are written from the perspective of a historian, and not from that of a mathematician engaged in active research on knots. This raises a particular difficulty when it comes to recent developments. Since there is little or no distance to view these events from, one is hard pressed to find *historical* criteria that would help to order the overwhelming amount of material that could be subjected to historical investigation. Since, on the other hand, it is not the historian's task to side with one or several of the engaged parties of active researchers in the assessment of this material, he is left with a huge and (from his perspective) largely unordered corpus of information. Under these circumstances, the best I can hope for is to propose some points of view that may prove useful for a better structuring and understanding of this corpus in subsequent historical work.[4]

2. A tale of diagram combinatorics

§ 4. It has been suggested that one of the earliest tools of combinatorial knot theory was forged by Carl Friedrich Gauss. Some posthumously published fragments of his *Nachlaß*,

[2] Taking descriptions of a complex of intellectual events in which certain mathematical ideas were produced as elements of the basic chronicle of a historical narrative, a historian has to "saturate" this chronicle in one of various possible ways. The idea that guides me in this enterprise is to produce an account of the *weave of mathematical action* in which these intellectual events actually happened. A historical narrative might thus be called "saturated" with respect to its basic chronicle if the causal and intentional context of the basic events in this weave is adequately captured. See also [42, Introduction].

[3] Interesting survey articles that offer information on these and still other questions are [60, 156, 67].

[4] A fuller treatment of the topics discussed here will be found in some of the papers referred to below and in my book *Die Entstehung der Knotentheorie*, forthcoming.

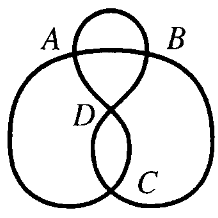

Fig. 1. A "Tractfigur" with crossing sequence $ABCDBADC$.

originally written in the years 1825 and 1844, document that the Göttingen mathematician tried to classify closed plane curves with a finite number of transverse self-intersections, sometimes called "Tractfiguren" (tract figures) by Gauss.[5] To do so, he invented a symbolical coding of such figures. He assigned a number or letter to each crossing and then wrote down the sequence of crossing symbols that resulted from following the curve in a given direction from a given point (see Figure 1).

To some extent, this symbol-sequence captured the characteristic features of the tract figure in the sense of *Geometria situs*, as Gauss preferred to call the as yet unexplored science of topology.[6] Gauss noticed that the sequences arising in this way had to satisfy certain conditions: Each of the symbols representing one of the n crossings had to appear exactly twice, once in an even and once in an odd place of the sequence. As Gauss noticed in 1844, however, these conditions were sufficient only for $n \leqslant 4$. He thus set out to write down a table of the admissible sequences for five crossings, but he did not find a method to solve the general problem of determining exactly which symbol sequences satisfying the above conditions actually represented crossing sequences of "Tractfiguren".[7]

What reasons did Gauss have for looking at this matter? Unfortunately, the fragments themselves do not give a clear answer. From the perspective of later knot theory, Gauss's attempt might look like a first step toward knot tables, but we will see that he had other reasons for studying "Tractfiguren".

Apparently unrelated to these considerations is another posthumous fragment that has often been cited as evidence for Gauss's interest in knots and links. This text, written in 1833, gives a double integral for counting

> the intertwinings of two closed or infinite curves. Let the coordinates of an undetermined point of the first curve be x, y, z; of the second x', y', z', and let
>
> $$\iint \frac{(x'-x)(\mathrm{d}y\mathrm{d}z' - \mathrm{d}z\mathrm{d}y') + (y'-y)(\mathrm{d}z\mathrm{d}x' - \mathrm{d}x\mathrm{d}z') + (z'-z)(\mathrm{d}x\mathrm{d}y' - \mathrm{d}y\mathrm{d}x')}{[(x'-x)^2 + (y'-y)^2 + (z'-z)^2]^{3/2}} = V$$
>
> then this integral taken along both curves is $= 4m\pi$ and m the number of intertwinings.[8]

Here, the situation is different than with the fragments on "Tractfiguren". A number that modern knot theorists might be inclined to calculate from a link diagram by adding "signs"

5 [57, vol. VIII, pp. 271–286].

6 Today, it is known that a reduced projection of a prime knot is indeed determined by its crossing sequence; see [29].

7 This problem received new interest after Gauss's fragments were published in 1900 and described in [33]. An algorithm solving the problem was first published by Max Dehn in [36].

8 [57, vol. V, p. 605].

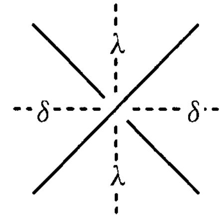

Fig. 2. Marking corners of diagrams.

of diagram crossings, was described by Gauss using *analytical* information. As it stands, also this fragment poses a historical riddle: Why, when, and how was Gauss led to consider linked space curves and this integral?

§ 5. While there is no direct evidence that Gauss actually studied the knot problem, his student and *protegé* Johann Benedikt Listing did.[9] In his 1847 essay, *Vorstudien zur Topologie* (in which he coined the term topology), Listing proposed to study, among other things, "Linearcomplexionen im Raume", roughly corresponding to 1-dimensional cell complexes embedded in ordinary space. The simplest case were knots (in the sense of smooth closed space curves without double points). Listing did not formulate the classification problem explicitly, but the general thrust of his essay suggests that he was interested in topologically invariant characteristics of "Complexionen" like knots. Guided by the Leibnizian idea of a symbolical calculus expressing "situation", as it was understood at the time,[10] Listing associated a "Complexionssymbol" with each knot diagram which, in slightly modernised notation, is a polynomial with integer coefficients in two variables. It was based on a rule for marking the corners of a diagram associated with Figure 2. Connecting two opposite regions by an axis running between the two arcs of the link, these arcs turn around the axis either like a right-handed or a left-handed screw. Accordingly, the regions were marked λ or δ, respectively [94, p. 52]. Listing's symbol was then defined to be the polynomial

$$\sum c_{ij} \lambda^i \delta^j,$$

where each term $c_{ij}\lambda^i\delta^j$ represented all diagram regions with precisely i marks λ and j marks δ; the coefficients c_{ij} were just the numbers of regions of type $\lambda^i\delta^j$, including the outer region. This polynomial was not a knot invariant, however, since diagrams of equivalent knots could have different polynomials. What Listing hoped was that the resulting identities could be made the basis of an *algebraic calculus* with diagram polynomials (in modern terms, one might interpret Listing's idea by considering the quotient of $\mathbb{Z}[\lambda, \delta]$ by the ideal generated by all diagram equivalences). The obvious problem was that the basic identities were unknown as long as the knot problem was unsolved, and Listing was unable to draw any interesting consequences from his definitions.

[9] On Listing, see [21]. A letter of Betti's reporting on his conversations with Riemann gives indirect evidence that during the last years of his life, i.e. after Listing's *Vorstudien* had appeared, Gauss studied knots, though without much success; see [170].

[10] The recurrent appeal to Leibniz' authority on *Analysis situs* is itself a historically interesting phenomenon, see [91, Introduction]. The particular conception of *Analysis situs* that Listing had in mind was in fact due to Euler [43].

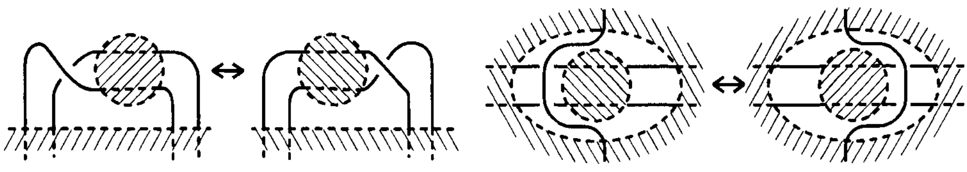

Fig. 3. "Twists" and "two-passes".

§ 6. The next visible scientific enterprise relating to knots was the construction of tables of alternating knots of up to eleven and of non-alternating knots of up to ten crossings by the Edinburgh physicist Peter Guthrie Tait, the Lancashire clergyman and mathematician Thomas P. Kirkman and the American civil engineer Charles N. Little in the last two decades of the 19th century. These tables, all but one published in the journals of the Royal Society of Edinburgh, were the outcome of hard combinatorial work.[11] Tait, who initiated the whole enterprise, outlined the strategy to be followed. It consisted of two separate tasks: first, all possible projections of prime knots (i.e. diagrams where over- and undercrossings were not distinguished) had to be enumerated; second, all possible choices of over- and undercrossings in these projections had to be checked, eliminating diagrams of equivalent knots.

The enumeration of knot projections was the easier part of this strategy. For the lowest crossing numbers, and independently of Gauss's still unpublished ideas, Tait first tried a method based on a refined version of crossing sequences. Later, he settled for a different technique, involving what today is called the "graph" of a knot. For higher crossing numbers, Kirkman took over this project, using another method for enumerating certain four-valent graphs from which knot projections could be derived. The harder task involved searching for duplications among the knot diagrams resulting from the enumerations of knot projections. Tait completed this task for alternating diagrams of up to ten crossings, and Little went on to deal with those having eleven crossings as well as the non-alternating diagrams. Two ideas about how diagrams of equivalent knots were related, implicit in Tait's work but only made explicit by Little, helped them to construct their tables, though both were acutely aware that their results were, to some extent, only tentative. For *alternating* diagrams without "nugatory" crossings,[12] Tait's implicit assumption and Little's explicit claim was that two such diagrams represented the same knot if and only if they could be related by a sequence of "twists" as in Figure 3 (left).[13] Only recently, and based on Jones's new invariant, has this conjecture been proved by Menasco and Thistlethwaite [108]. For *non-alternating* diagrams, Little argued that a sequence of twists and of additional operations, today called "two-passes" and illustrated in Figure 3 (right), would suffice to generate all diagrams of equivalent knots. Unfortunately, this claim was recognised to be wrong when K.A. Perko discovered a duplication in Little's tables in 1974 that the latter had missed because the two are not related by twists and two-passes.

[11] The main publications are [153–155, 88, 89, 95–97]. See [42] for a more detailed description of this work.

[12] A diagram crossing was called "nugatory" by Tait if a simple closed curve existed in the diagram plane intersecting the diagram only at this crossing. Diagrams without such crossings are today called "reduced".

[13] The name "flype" that modern authors tend to attach to this operation was used by Tait for a different operation.

Two obvious historical questions arise. First, why did these men spend so much of their time on knot tabulations? And, second, was their work causally linked to the Göttingen environment of the 1840's, where Gauss and Listing had dealt with similar issues?

§ 7. Not long after Poincaré had created the conceptual tools of modern topology – homological invariants and the fundamental group – presentations of the fundamental group of a knot complement, associated with a knot diagram, became known. In 1910, Max Dehn published a method for finding such a presentation and pointed out that a study of knots using group presentations would require solving some of the basic problems of combinatorial group theory [34]. In this connection, Dehn actually gave the first general and explicit formulations of the word and conjugacy problems in finitely presented groups.[14] Dehn also sketched a technique for treating the word problem by constructing what he called the "Gruppenbild", namely, the Cayley graph of a finitely presented group $G := \langle a_1, \ldots, a_n \mid r_1, \ldots, r_n \rangle$ consisting of the group elements as its vertices and oriented edges connecting group elements of the form g and $a_i g$. The cycles in this graph obviously correspond to all trivial words of the presentation, so that constructing the "Gruppenbild" and solving the word problem of a group presentation are equivalent tasks. Dehn managed to construct the graph of the group of a trefoil knot. The graph showed that this group was non-commutative, and hence a trefoil knot could not be deformed without self-intersections into an unknotted circle.

In the 1920's, Kurt Reidemeister and Emil Artin pointed out that another method for associating a group presentation with a knot diagram had been developed already around 1905 by the Vienna mathematician Wilhelm Wirtinger; and in fact the method had been described in a somewhat disguised fashion by Tietze in a paper of 1908.[15] Again, a question arises: what drew Wirtinger and Dehn to study knots and their groups in the first place? Was it just the wish to apply Poincaré's new tools to a "natural" particular case?

§ 8. The 1920's brought the first effectively calculable invariants of knots, and thus also a means for verifying the knot tables of the 19th century. More or less independently, the Princeton topologist James W. Alexander (together with his student G.B. Briggs), and Kurt Reidemeister, first at Vienna and then at Königsberg, showed how to associate certain matrices with knot diagrams in such a way that the elementary divisors of these matrices were knot invariants.[16] The model for this technique was clearly Poincaré's calculation of the torsion numbers of cell complexes, but both Reidemeister and Alexander presented their results in a completely independent way; Reidemeister even spoke of a new "elementary foundation" for knot theory. Both defined knots as equivalence classes of finite polygons in three-dimensional Euclidean space. Two such polygons were considered equivalent if and only if they could be deformed into each other by a sequence of applications of the following operation and its inverse: two incident edges AB, BC of a polygon may be replaced by an edge AC, provided the triangle ABC contains no further point of the polygon. Reidemeister and Alexander translated this into an equivalence relation between knot (or link) *diagrams*. Instead of just one operation, four had now to be considered: an analogue

[14] For a discussion of the relations between knot theory, topology in general, and early combinatorial group theory, see [28, Chapter I.4].

[15] See [9, Section 6; 160, p. 103f].

[16] See [134, 135, 7].

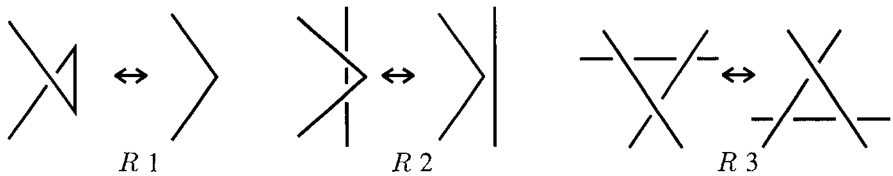

Fig. 4. Reidemeister's diagram moves.

of the above in the plane (where it is allowed that an arc of the polygon without crossings lies "below" or "above" the triangle involved), and three "diagram moves" involving modifications of diagram crossings which since have become known under the name of "Reidemeister moves" (see Figure 4).

Again, diagrams that could be deformed into each other by a finite sequence of such moves represented the same knot, and thus any mathematical object associated with a knot or link diagram invariant under Reidemeister's moves was a knot (or link) invariant. Reidemeister and Alexander showed this to be the case for the nontrivial elementary divisors of the matrices they were considering. Since this helps to understand Alexander's subsequent invention of a knot polynomial, let me present Alexander's version of a matrix associated with a knot diagram in condensed form. Thus, let the ν crossings of a knot diagram be denoted by $c_1, c_2, \ldots, c_\nu$, and let its $\nu + 1$ finite regions be denoted by $r_0, r_1, \ldots, r_\nu$.[17] The region r_0 should have a common border with the infinite region. After selecting an orientation of the diagram, the corners of diagram regions are marked according to the convention that the corners of the two regions to the *left* of each *undercrossing* arc receive a dot. Moreover, an integer $n \geqslant 2$ is chosen. Then an $(n\nu \times n\nu)$-matrix M is defined, consisting of $\nu \times \nu$ blocks a_{ij}, each of size $n \times n$. In order to abbreviate the definition, let I denote the $(n \times n)$-unit matrix, and let x be the $(n \times n)$-block given by

$$x := \begin{pmatrix} 0 & 1 & 0 & \ldots & 0 \\ 0 & 0 & 1 & & \vdots \\ 0 & 0 & 0 & \ddots & \vdots \\ \vdots & & & \ddots & 1 \\ 1 & 0 & \ldots & \ldots & 0 \end{pmatrix}.$$

Then M is defined by the following rules: (1) to each crossing c_i corresponds a row of blocks a_{ij}, and to each region r_j, $j = 1, \ldots, \nu$, corresponds a column of blocks a_{ij} in M; (2) if r_j, r_k, r_l, r_m are the regions incident with a crossing c_i, in cyclical order as one goes round c_i in counterclockwise sense, and such that the dotted corners belong to r_j and r_k, then $a_{ij} = a_{ik} = x$ and $a_{il} = a_{im} = I$.

It was now a matter of straightforward calculation to show that the elementary divisors of M different from zero and one – called the "torsion numbers" of the knot – remained invariant under Reidemeister's diagram changes. Alexander and Briggs calculated the elementary divisors of all of the 168 matrices associated with the 84 knots of nine or less

[17] These notations are taken from [5]. In the earlier [7], different notational conventions were adopted. In both papers, the matrix defined was viewed as a matrix of coefficients of a system of linear equations in the variables r_j.

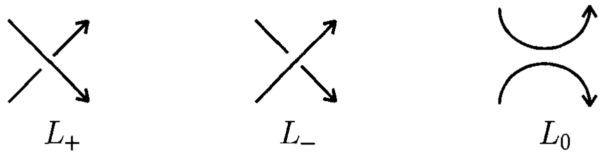

Fig. 5. Local modifications of links.

crossings in Tait's tables, corresponding to $n = 2$ and $n = 3$. Except for three pairs with identical torsion numbers, all of these knots were found to have distinct invariants.

Of course, the above description makes Alexander's construction of invariant numbers appear historically opaque. How could he possibly have found all this machinery? Why were these invariants called torsion numbers?

§ 9. In Alexander's and Briggs's first paper, the block structure of the above matrix was not introduced explicitly. Once it was recognized (and no doubt it was recognized during the extensive calculations needed for checking Tait's tables), it was but a small step to see that one could view the $(\nu \times \nu)$-matrix of *blocks* as a matrix with polynomials in the formal variable x as its entries (I being identified with 1). This step was taken in [5], where virtually the same arguments as before showed that the nontrivial elementary divisors of this new matrix, and therefore in particular its determinant $\Delta(x)$, were invariants under the Reidemeister moves up to a factor $\pm x^k$ ($k \in \mathbb{Z}$).[18] Normalizing $\Delta(x)$ by the requirement that the term of lowest degree became a positive constant, Alexander obtained the polynomial invariant of oriented knots that today carries his name. Again, the knots in Tait's tables were used to test the force of the new invariant. Alexander found that the polynomial, though of course much easier to calculate, was only slightly less effective in distinguishing knots than the torsion numbers. It turned out that both, however, could not distinguish knots from their reverse knots (obtained by reversing the orientation) or mirror images (obtained by switching all crossings).

Toward the end of his paper, Alexander included a side remark which probably resulted from his experiences with calculations of $\Delta(x)$. After noticing that his definition could equally well be applied to link diagrams (in this case, it gave rise both to a one-variable polynomial of oriented links, defined by the same rules as above, and a polynomial in as many variables as the link had components), Alexander established a relation of the one-variable polynomials of "three closely related links" [5, p. 301]. Using the modern subscripts L_+, L_-, and L_0 for oriented link diagrams that only differ at one crossing in the way indicated in Figure 5, Alexander's relation can be written as

$$\Delta_{L_-}(x) - \Delta_{L_+}(x) = (1 - x)\Delta_{L_0}(x). \qquad (\star)$$

For the time being, however, nothing was made of this relation.

§ 10. In 1961, Wolfgang Haken published a long and difficult paper in which an algorithm was described that allowed one to decide whether or not a given knot was equivalent

[18] Actually, Alexander used both this matrix and an equivalent one, in which certain signs were added to take care of orientations.

to the unknot [64]. Although this algorithm was extremely impractical, its existence made it probable that the classification of knots was possible by algorithmic means. Accordingly, and enhanced by the availability of powerful computers, the interest in computerised knot tabulations increased significantly during the 1960's. At a conference in 1967, John Conway surprised the tabulators by presenting an algorithm for enumerating knots and links which was much more effective than those used by the 19th-century tabulators and which enabled him to enumerate knots of up to eleven crossings and links of up to ten crossings by hand [31]. The main tool was a calculus of "tangles", parts of link diagrams with four open ends, that could be used to survey their possible combinations and closings. In order to distinguish the various links enumerated in this way, Conway had to calculate invariants, too. This led him to reconsider the Alexander polynomial, and he redefined it by a change of variables and a new normalization. Apparently without having read Alexander's earlier remark, Conway pointed out that his version of the polynomial satisfied an equivalent of ($\star$) and similar relations which he later came to call "skein relations".[19] In view of their usefulness in calculations, he counted these relations among "the most important and valuable properties" of his version of the Alexander polynomial, but he did not try to *define* the polynomial using a variant of ($\star$).

This was done in 1981 by L.H. Kauffman. He observed that up to a suitable normalization, Alexander's one-variable polynomial $\Delta_L(t)$ of oriented links L was uniquely determined as a symmetric element of $\mathbb{Z}[t^{1/2}, t^{-1/2}]$ (in the case of proper knots even of $\mathbb{Z}[t, t^{-1}]$) by the following two conditions (the symbol $\bigcirc$ represents the unknot):[20]

$$\Delta_{\bigcirc}(t) = 1, \tag{1}$$

$$\Delta_{L_+}(t) - \Delta_{L_-}(t) + \left(t^{1/2} - t^{-1/2}\right)\Delta_{L_0}(t) = 0. \tag{2}$$

Since any link diagram could be changed into a trivial one by switching its crossings, it was not difficult to see that these two rules would suffice to *calculate* the polynomial inductively, provided it was well-defined. This was shown to be the case using yet another description of $\Delta(x)$ as the determinant of a matrix associated with an oriented link diagram. Still, no further analysis of this seemingly peculiar property of Alexander's polynomial was undertaken, and Kauffman expressed his astonishment about the approach: "It seems nothing short of miraculous that such a scheme should produce good invariants" [83, p. 102].

§ 11. The view of the matter changed dramatically when Jones discovered his new polynomial invariant of oriented links in 1984 [77]. In discussions, Jones and Joan Birman found that also this invariant satisfied a skein relation similar to the one found by Alexander. In fact, if $V_L(t)$ denotes the Jones polynomial associated with a link L, then $V_L(t)$ was uniquely determined by the conditions

(i) $V_{\bigcirc}(t) = 1$,
(ii) $t V_{L_+}(t) - t^{-1} V_{L_-}(t) + \left(t^{1/2} - t^{-1/2}\right) V_{L_0}(t) = 0$.

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[19] According to Lickorish [92]. In [31], this terminology was not used.

[20] Conventions on signs and variables in the literature on knot polynomials are far from consistent. I follow here [54, 67]. In [83], the polynomial $\Omega(t) := \Delta(t^2)$ was considered.

Fig. 6. Local modifications of unoriented links.

This striking similarity induced several mathematicians to investigate the conditions under which a *general* skein relation would define a link invariant. Almost simultaneously, at least eight mathematicians found an equivalent answer to this question. In the joint article (Freyd et al. [54]),[21] this answer was stated as follows: there is a unique invariant $P_L(x, y, z)$ of oriented links with values in the homogeneous Laurent polynomials of degree 0, satisfying

$$\begin{aligned} &\text{(I)} \quad P_{\bigcirc}(x, y, z) = 1, \\ &\text{(II)} \quad xP_{L_+}(x, y, z) + yP_{L_-}(x, y, z) + zP_{L_0}(x, y, z) = 0. \end{aligned}$$

Moreover, this invariant is universal in the sense that *any* link invariant Q with values in a commutative ring A that equals one for the unknot and satisfies a linear skein relation $aQ_{L_+} + bQ_{L_-} + cQ_{L_0} = 0$, for arbitrary invertible coefficients $a, b, c \in A$, can be obtained from P by the canonical ring homomorphism from $\mathbb{Z}[x, x^{-1}, y, y^{-1}, z, z^{-1}]$ to A which sends x, y, z to a, b, c, respectively. In particular, both Alexander's and Jones's polynomials can be obtained from P (which is often expressed as the *inhomogeneous two-variable* polynomial $P_L(l, l^{-1}, m)$) by suitable substitutions of the variables.

It turned out that Jones's polynomial and its generalization were much stronger invariants than the Alexander polynomial. In many cases, these polynomials distinguished knots from their mirror images, and up to the time of writing, no nontrivial knot seems to be known with the Jones polynomial of the unknot.

Of course, once the surprising force of skein relations was recognized, variations of this combinatorial theme seemed promising and a whole series of related polynomials were found.[22] Kauffman's investigations were again particularly successful in this respect. By considering the *four* possible local modifications of *unoriented* links (see Figure 6) he found not only a new and extremely simple definition of Jones's polynomial (Kauffman [84]) but also a two-variable polynomial invariant of oriented links that was seen to be independent of P (Kauffman [85]).

§ 12. It is a historiographical commonplace that quite different historical narratives based on the same documentary material are possible. The above outline of some important combinatorial aspects in the development of knot theory is one of the stories that can be told about mathematical treatments of knots and links. Homogeneous as it may seem, though, it is clear that crucial historical questions remain unanswered and important parts of the documentary evidence have been passed over in silence. What were the actual motivating backgrounds for those contributing to this development? For whom, and in what contexts, did they work on knots? How, precisely, were physicists like Tait and mathematicians such as Dehn, Reidemeister, or Alexander led to form their ideas? How could an operator algebraist like Jones hit on a topological invariant of links? It is hardly imaginable that an

[21] See also [132].

[22] A concise survey is given in [93].

interest in the combinatorics of knot diagrams alone provided enough motivation and a sufficiently elaborated intellectual framework for constructing knot tables, for studying the knot group, or for inventing polynomial invariants. And indeed, in most cases, it turns out on closer inspection that quite varied and much richer impulses were at work in the historical development. Thus, a story about the mathematical study of knots can be told which is quite different from the above.

3. Mathematical treatments of knots before 1900

§ 13. Since prehistoric times, knots and interlacing patterns have been used in human cultures for practical, ornamental, and symbolical purposes. Against this background, the beginning of a *mathematical* interest for knots in the late 18th century marks a striking discontinuity. It has to be understood within the general context of the progressing mathematization of many domains of human knowledge and practice that characterizes this epoch. More precisely, knots and interlacing patterns found the attention of those few mathematicians that were interested in a vaguely conceived new exact science, tentatively called the "science of situation", *Analysis situs* or *Geometria situs*.[23] In fact, the first but vigorous attempt to bring knots within the reach of mathematical treatment bears all the marks of a typical Enlightenment attempt to mathematize a domain of human practice. It was made in 1771 by the Paris intellectual, A.T. Vandermonde, later a decided supporter of the French Revolution. In a short paper entitled "Remarques sur les problèmes de situation", he wrote:

> Whatever the convolutions of one or several threads in space may be, one can always obtain an expression for them by the calculus of magnitudes; but this expression would not be of any use in the arts. The worker who makes a *braid*, a *net*, or *knots*, does not conceive of them by relations of magnitude, but by those of situation: what he sees is the order in which the threads are interlaced. It would thus be useful to have a system of calculation that conforms better to the course of the worker's mind, a notation which would only represent the idea which he forms of his product, and which could suffice to reproduce a similar one for all times.[24]

Besides showing how some symmetrical weaving patterns (ones actually used in textile manufacture) could be described by means of a symbolical notation, Vandermonde did little to advance a veritable "system of calculation" relating to knotted or linked space curves. Nevertheless, it is significant that this kind of problem was incorporated into *Geometria situs* long before, say, the classification of surfaces became an important issue.

§ 14. Also in Gauss's case, it seems to have been the *uses* of the new science of *Geometria situs* that captivated his interest in the topic. For him, however, these uses were concerned not with the practical arts but rather the exact sciences, including traditional pure mathematics as well as sciences like astronomy, geodesy, and the theory of electromagnetism.[25] Gauss encountered linked space curves for the first time in his scientific career in an astronomical context. This happened in 1804, some twenty years before the first of the fragments described in the last section was written. After Gauss's successful

[23] See [129, 53].

[24] [163, p. 566]; my (rather literal) translation.

[25] The following paragraphs are a condensed version of [41]. For details and full references, the reader is referred to this article.

calculation of the orbit of the first observed asteroid, Ceres, had spread his fame over Europe in 1801, he continued to follow the discoveries of several other "small planets" made soon thereafter with increasing rapidity. In this connection, he published a small treatise entitled *Über die Grenzen der geocentrischen Örter der Planeten*, which took up a rather practical question, namely the determination of the celestial region in which a given new "planet" might possibly appear.[26] Taking the liberty of presenting Gauss's arguments in modern mathematical language, the problem of this paper can be described as follows. If the orbit of the earth's motion around the sun is given by $X \subset \mathbb{R}^3$, and if $X' \subset \mathbb{R}^3$ is the orbit of another celestial body (the sun being at the center of a suitable system of Cartesian coordinates), Gauss wanted to determine the region on the sphere given by

$$\left\{ \frac{\vec{x} - \vec{x}'}{\|\vec{x} - \vec{x}'\|} \in S^2 \,\Big|\, \vec{x} \in X,\ \vec{x}' \in X' \right\}.$$

This region he called the *zodiacus* of the celestial body in question. Its determination helped to limit the effort needed both in the observation of the celestial body and in the production of an atlas of the smallest part of the celestial sphere on which the orbit of the body could be drawn. In order to solve this problem, Gauss derived a differential equation for the *boundary curve or curves* of the *zodiacus*, implicitly assuming the orbits to be smooth curves. If $\vec{x} = (x, y, z) \in X$ and $\vec{x}' = (x', y', z') \in X'$ denote the coordinates of orbit points, a necessary condition that a pair of points $(\vec{x}, \vec{x}')$ corresponds to a boundary point of the *zodiacus* is that the triple consisting of the two tangent vectors to the orbits at $\vec{x}$ and $\vec{x}'$ and the distance vector $\vec{r} := \vec{x}' - \vec{x}$ is linearly dependent. Gauss expressed this condition by saying that the two tangents at $\vec{x}$ and $\vec{x}'$ had to be coplanar. Translating the condition into a formula led to the differential equation

$$\begin{aligned}(x' - x)(\mathrm{d}y'\mathrm{d}z - \mathrm{d}y\mathrm{d}z') + (y' - y)(\mathrm{d}z'\mathrm{d}x - \mathrm{d}z\mathrm{d}x') \\ + (z' - z)(\mathrm{d}x'\mathrm{d}y - \mathrm{d}x\mathrm{d}y') = 0.\end{aligned}$$

Obviously, the differential form on the left-hand side is, up to a change of sign, nothing but the numerator of the integrand in the linking integral! At this point, Gauss inserted a typical remark: He had undertaken a mathematical study of this equation in its own right, but for the sake of brevity he did not wish to go into that now. However, Gauss pointed out that one case was of particular importance, namely that in which the two orbits were *linked*. (Even this was not just a mathematical fancy: While none of the orbits of the known "planets" was linked with that of the earth, Gauss reminded his readers that "comets of the sort exist in abundance".[27]) In this case, the *zodiacus* was, "for reasons of the geometry of situation", the whole celestial sphere.

As I have described elsewhere, it is probable that already in his study of the equation determining the boundary of the *zodiacus* Gauss began to understand the connection between the geometry of linked space curves and the integer calculated by his double integral – an integer which in modern mathematical language may also be described as the mapping degree of the mapping defining the *zodiacus*. Thus, geometric considerations that came up

[26] The article is reprinted in [57, vol. VI, pp. 106–118].

[27] [57, vol. VI, p. 111f] – in 1847, Listing counted 25 pairs of *asteroids*, whose orbits were known to be linked by then [94, p. 64f].

in a scientific context highly appreciated at its time induced Gauss to think about this kind of topological phenomenon.

A similar connection involving an exact science and *Geometria situs* probably first aroused Gauss's interest in tract figures. During the 1820's, his geodetic work related to the triangulation of the Kingdom of Hanover induced him to develop once again some new mathematics. While Gauss was directing this lucrative enterprise, he also worked on the *Disquisitiones generales circa superficies curvas*, published in 1827. In this concise treatise, he developed the basic ideas on curvature and geodesics on surfaces that formed the starting point of modern, intrinsic differential geometry. The crucial tool for studying curvature, however, depended on the consideration of surfaces *embedded* in ordinary space. This tool was a mapping that today carries Gauss's name: it associated to each point on a curved surface the direction of the surface normal at that point; this direction was then represented as a point on an auxiliary sphere. Using this mapping, Gauss introduced the notion of the total curvature of a portion of the surface bounded by a simple closed curve. By definition, this curvature was given by the area enclosed by the image of the boundary curve on the auxiliary sphere. Here, however, a problem arose: the image curve could have singularities – i.e. it could be a tract figure on the sphere (or even worse). Thus one had to clarify what "the area enclosed" by such a figure actually meant. In this way, Gauss was led to look at the topology of closed plane curves in more detail, and it was amidst his work on the *Disquisitiones* that he sketched his first ideas about tract figures. In the published treatise, he only alluded to this work (and the solution of the problem of defining the area enclosed by a tract figure by means of "characteristic" numbers given to the various regions of the figure). But in a letter to his friend Schumacher, he complained:

> Some time ago I started to take up again a part of my general investigations on curved surfaces, which shall become the foundation of my projected work on higher geodesy. [...] Unfortunately, I find that I will have to go *very far* afield [...]. One has to follow the tree down to all its root threads, and some of this costs me week-long intense thought. Much of it even belongs to *geometria situs*, an almost unexploited field.[28]

At about this time, Gauss also spent some thought on another peculiar object of *Geometria situs* – a four-strand braid. While the page in one of Gauss's notebooks documenting this astonishing attempt reveals that he knew how to determine the linking number of two curves by counting signs of diagram crossings, it remains unclear how this fragment relates to Gauss's other mathematical activities.[29]

The third exact science which brought Gauss back to the study of linked space curves was the theory of electromagnetism, which drew considerable scientific and public attention after Oersted's and Faraday's discovery of electromagnetism and electromagnetic induction. As is well known, Gauss was involved together with his friend and colleague, the physicist Wilhelm Weber, in setting up the first telegraph in Göttingen in April 1833. In connection with this work, Gauss studied intensively the mathematical formulation of the laws of electromagnetical induction. It could not have escaped his notice that the law describing the magnetic force induced by an electric current was governed by precisely the same differential form which he had encountered in his earlier investigation of the *zodiacus*. Conceiving magnetic forces as acting on particles, which behave mathematically like monopoles in some "magnetic fluid", the linking integral could be interpreted as ex-

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[28] Gauss to Schumacher, 21 November 1825, in: [57, vol. VIII, p. 400].

[29] The fragment has been published and discussed in [41].

pressing the work needed to carry an "element of magnetic fluid" along a closed path in the magnetic field induced by a current running through another closed curve. Still, when Gauss wrote the passage on the linking integral in his notebook a few months before the telegraph was finished, he made no explicit remarks about electromagnetism.

Those with close contact to Gauss's work, including Wilhelm Weber and Ernst Schering, the later editor of Gauss's writings on electromagnetism, knew that he had thought of topological issues in connection with electromagnetism, and Schering thus decided that the fragment on the linking integral should be published in the fifth volume of Gauss's *Werke*, which appeared in 1867 and contained his unpublished notes on electromagnetic induction. It was there that another physicist learned of Gauss's interest in *Geometria situs*: James Clerk Maxwell. In his masterpiece, the *Treatise on Electricity and Magnetism* of 1873, Maxwell went to considerable lengths to explain the physical content of the linking integral [104, §§409–422].

From several passages in Gauss's letters as well as from certain writings of his scientific friends, we know that Gauss held the still barely existent science of *Geometria situs* in very high esteem and expected great developments to come from future research in this field. The reasons for his expectations certainly had little to do with his inconclusive results on tract figures or similar combinatorial ideas. Rather, they derived from his experience that certain types of objects and problems, like linked space curves and tract figures, that were geometric in nature but independent of "magnitude", continually reappeared in some of the leading sciences of his day, ranging from pure mathematics to electromagnetism.

§ 15. A similarly close relationship between important issues in the exact sciences and new ideas related to knots continued to hold throughout the 19th century. When Tait embarked on his tabulation enterprise, he was motivated by developments in natural philosophy in which topological ideas played a very fundamental role. The crucial mathematical device that brought topology into play came from Germany: potential theory in multiply connected domains. Thus, Tait's enterprise and the earlier topological ideas shaped under Gauss's hegemony at Göttingen were actually connected in the fabric of scientific practice, although in an indirect way. Three elements must be put together in order to understand this connection: the dynamical theories that many British natural philosophers held in the second half of the last century, H. von Helmholtz' researches on vortex motion in perfect fluids, and Riemann's notion of *connectivity*.[30]

Guided by the many "mathematical analogies" between physical phenomena that could be described by means of the Laplace equation (electrostatics, heat flow, etc.), many of the leading British physicists in the second half of the last century believed that all of physical theory could and should ultimately be based on some kind of Lagrangian dynamics that governed the flow of a continuous medium (or several media). However, an important challenge to this conception emerged with the continuous rise of atomistic conceptions in chemistry. From the 1860's onward, atomism was forcefully supported by what was perhaps the most advanced experimental technology of the time, spectrum analysis. This posed an immediate problem: how could the smallest units of matter possibly be explained by the dynamics of a continuous medium? One hint came from experiments with magnetism that seemed to imply that, on the molecular level, some kind of rotary motion

[30] Full references and a detailed account of the events described in the following paragraphs may be found in [42].

took place. Already in the 1850's, one of the important proponents of dynamical theories, William Thomson, the later Lord Kelvin, hoped to solve the riddle of atoms by detecting some kind of stable (presumably rotary) dynamical configuration in the motion of the universal medium (ether).

A crucial piece of knowledge for Thomson's pursuit of this line of thought was provided when Hermann von Helmholtz published a ground-breaking paper in which the dynamics of a perfect (i.e. incompressible and friction-free) fluid was investigated without supposing, as had been done earlier, that such a flow could be described by a globally defined potential function (Helmholtz 1858). In modern mathematical notation, Helmholtz discussed solutions $\vec{v}$ of the Euler equations,

$$\vec{F} - \frac{1}{h}\nabla p = \frac{\partial \vec{v}}{\partial t} + (\vec{v} \cdot \nabla)\vec{v},$$

$$0 = \nabla \cdot \vec{v},$$

for which the field of rotation, rot $\vec{v}$, did not necessarily vanish.[31] Helmholtz observed that the integral curves of rot $\vec{v}$ – called "Wirbellinien" (vortex lines) – possessed a kind of dynamical stability. During the motion of the fluid, the particles constituting a vortex line would continue to do so throughout the motion. In particular, if a vortex line was closed, it would remain closed, however altered in shape. This suggested looking for a particular kind of solution to the Euler equations. Helmholtz supposed the dynamics of a finite number of closed vortex lines (or vortex tubes, i.e. tubular bundles of vortex lines emerging from a small area) to be given. He then asked: could a solution to the Euler equations be found with precisely these vortex lines or tubes as its (discontinuous) rotation field? Since *outside* the vortices the rotation had to vanish, this problem amounted to finding a solution of the Laplace equation $\Delta\varphi = 0$ in the multiply connected complement of the vortices (possibly bounded by some closed surface). Here, a solution was understood to be a *many-valued* function φ defined on the complement of the vortices, the branches of which satisfied the Laplace equation locally. By invoking the analogy between the mathematics of hydrodynamics and of electromagnetism, Helmholtz made it clear that such solutions always existed. The mathematics of the situation corresponded to that of a system of closed currents (playing the role of the vortices) which induced a magnetic field (assuming the role of the flow) in their complement.[32] In fact it was easy to write down integral formulae representing the solutions locally. Helmholtz illustrated his results, which he viewed as a three-dimensional analogy to the "Abelian integrals of the first kind" on Riemann surfaces, by giving explicit formulae for a few concrete cases like circular vortex rings.

After a delay of more than eight years, and through the mediation of some experimental illustrations of Helmholtz' results by the Edinburgh physicist Peter Guthrie Tait, Thomson eventually understood that these findings provided one of the missing links in his earlier speculations on atomism. Were not these sorts of closed vortices the kind of stable dynamical configuration in the ether that made up atoms? Once this idea had taken shape in

[31] As a matter of fact, the vectorial notations for the rotation field and for the equations of fluid motion were indirectly inspired by Helmholtz' paper. When Tait read this paper in the fall of 1858, he was reminded of certain quaternionic formulae he had seen earlier in Hamilton's writings; this induced him to start a crusade for the use of quaternions – and thus, also vectorial notations – in physics.

[32] The analogy is strictly correct only in the stationary case.

early 1867, Thomson set out to pursue it with surprising energy. Two things were clear: first, if his speculation was correct, then the different topological types of knots and links provided a wealth of forms that should account for the variety of chemical elements. Second, if it was legitimate to hope for a precise mathematical treatment of "vortex atoms", as Thomson's ether singularities were now called, it would be necessary to extend Helmholtz' theory of vortex motion to a considerable degree.

Thomson took up the second of these tasks. Among the first goals he set himself was to determine the solution space of the problem Helmholtz had considered. Thus, given a multiply connected domain in three-dimensional space (the complement of a system of vortices), how many linearly independent solutions of the Euler equations for a perfect fluid existed with given boundary conditions (e.g., with the fluid flow tangential to all boundary surfaces)? The surprising answer that Thomson found was that the number of parameters determining a solution was dependent only on the topology of the domain considered. It equalled the "order of continuity" of this domain, as Thomson called it. In modern terms: The dimension of the linear space of harmonic vector fields in a given domain, with fixed boundary conditions, was equal to the domain's first Betti number. Thomson was aware of the fact that his result provided an analogy between integrals of the Euler equations for a perfect fluid and Abelian integrals on Riemann surfaces, an even closer analogy than that Helmholtz had seen earlier. Much later, this insight would be explored in a different direction by Hodge's theory of harmonic integrals.

Due to Thomson's theory, interest among British physicists in topological ideas began to surge. In late 1867 and during the following year, Maxwell also began to think about the topological issues involved in the theory of vortex atoms, although his interest stemmed perhaps more from the relevance of the mathematics of vortex motion for electromagnetism rather than because he believed it could be used to explain the structure of matter. Maxwell produced several manuscripts in which he sketched some of the topological ideas needed for dynamical theory.[33] One basic proposition concerned the first Betti number of a region in ordinary space, a "solid with holes" as he described such a region intuitively. If a solid with holes was bounded by one external surface of genus n_1 and several internal bounding surfaces of genus $n_2, \dots, n_m$, then the first Betti number of the region was $b = n_1 + n_2 + \cdots + n_m$.

Both Thomson and Maxwell did not yet have a sufficiently clear language to give precise formulations and proofs of their topological results; neither the notion of the genus of a surface nor that of the "order of connectivity" of a space region were completely clear in their work. Maxwell and Thomson tried to explain their ideas mainly in terms of "irreconcilable curves" in a domain, but "reconcilability" meant for them something closer to *homotopical* rather than *homological* equivalence. Thus, a particular difficulty in determining the first Betti number of a space region arose again from knotting: why was the "order of connectivity" of a link complement equal to the number of components of the link, as Maxwell's result implied? A closer analysis shows that it was *physical thinking* rather than mathematical precision that helped Thomson and Maxwell to find the correct results.[34] In any case, an understanding of the topology of knots and links became a requisite part of their physical theories.

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33 These manuscripts were published only recently in [106, vol. 2].

34 Briefly put: In technical arguments on multiply connected domains, cutting surfaces (interpreted as membranes stopping fluid motion) were used rather than "irreconcilable curves". See [42, Section II] for details.

In one of his manuscripts, and to the best of my knowledge for the first time, Maxwell explicitly formulated the classification problem for knots and links. Independently of Gauss's and Listing's earlier attempts, he then developed a method to represent link diagrams symbolically, and went so far as to look for obvious diagram modifications ordered according to the number of diagram crossings involved. Not surprisingly, this led him to uncover the "Reidemeister moves" – without, however, considering the question of whether or not these moves would generate all diagram equivalences.[35] Apparently, Maxwell did not pursue his reflections on knots very far in the years around 1868. However, in a very favourable review of Thomson's theory of vortex atoms, written for the ninth edition of the *Encyclopedia Britannica* in 1875, he pointed out that the classification of knots might actually turn out to be rather complicated: "The number of essentially different implications of vortex rings [that is: knot types] may be very great without supposing the degree of implication of any of them very high" [105, p. 471].

Soon afterward, Tait began to investigate the classification problem of knots along the lines described in Section 2. Throughout his work on the tabulations, Tait was motivated by the possible contributions these tables could make to the theory of vortex atoms, and he dropped his work when he felt the tables were sufficiently extensive to be compared with the requirements of physics – be it with a positive or, as became more and more probable, with a negative result.[36] It should be emphasized, however, that the scientific background of Tait's tabulation enterprise was anything but a scientific curiosity. Given the beliefs and methods of the period, Thomson's theory was considered a serious and even promising speculation. The fact that several of the leading British natural philosophers, including Maxwell, followed Thomson's ideas with interest, in itself offers ample evidence of this. Moreover, even if unsuccessful, the theory of vortex atoms was the first serious attempt to explain atoms on the basis of fundamental laws of motion rather than by postulating additional theoretical entities, like force centres or the like. Finally, the *mathematics* that had to be developed in order to pursue the theory was clearly perceived to be important, even if the physical core of the theory should turn out to be incorrect. One final point deserves attention. As in all earlier contributions to the mathematical study of knots, knots were thought of as physical objects in ordinary space. While Tait and his followers used diagrams to deal with these objects, the physical context implied that the *complement* of a knot or link was at least as interesting as the link itself. Thus, in connection with vortex atoms, it was the geometry of this spatial domain rather than the combinatorics of diagrams that "mattered".

4. The formation of "modern" knot theory

§ 16. By the time mathematicians of the twentieth century turned again to the investigation of knots and links, both the status of topology and the general horizon of mathematical culture had changed deeply – a new epoch of mathematics had dawned that may reasonably be called "mathematical modernity". Two aspects of these changes are particularly

[35] Cf. [42, §20; 104, vol. 2, pp. 433–438].

[36] During the 1880's, Thomson himself gradually abandoned the theory of vortex atoms. He repeatedly failed to prove that vortices possessed kinetic stability, and he began to feel that the difficulties to include other physical phenomena like light and gravitation into the picture were unsurmountable. See [152] for a concise description of Thomson's changing views.

relevant for our story. On the one hand, Poincaré's writings on *Analysis Situs* offered new ways to conceive topological objects and new mathematical tools to deal with them, however vague some of his proposals still were on the technical level.[37] On the other hand, the emergence of the modern, axiomatic style in mathematics, the power of which had been impressively demonstrated in Hilbert's *Grundlagen der Geometrie* of 1899, underlined the intellectual autonomy of mathematics and its increasing conceptual separation from the exact sciences. In such an environment, a continuation of the study of knots along the lines followed by Tait, Kirkman and Little seemed hardly promising.[38] Indeed, two quite different lines of thought brought knots to the fore of modern mathematics: the study of singularities of complex algebraic curves and surfaces, and Poincaré's attempt to give a topological characterization of ordinary three-dimensional space, known as the "Poincaré conjecture".[39]

§ 17. Around 1895, the Austrian function-theorist Wilhelm Wirtinger began to think about ways to generalize the approach to algebraic functions of a single complex variable based on harmonic functions on Riemann surfaces to the case of algebraic functions of two complex variables, i.e. "functions" $z = z(x, y)$ defined by a polynomial equation

$$f(x, y, z) = 0, \quad x, y, z \in \mathbb{C}.$$

Such an approach was very much in the spirit of Felix Klein's views on algebraic functions, and indeed Wirtinger regularly reported on his ideas in his correspondence with Klein. Soon, however, Wirtinger realized that among the many difficulties that had to be overcome, the *topological* ones were crucial. Viewing algebraic functions of two variables as branched coverings of $\mathbb{C}^2$,

$$p : \{(x, y, z) \in \mathbb{C}^3 \colon f(x, y, z) = 0\} \to \mathbb{C}^2, \quad (x, y, z) \mapsto (x, y),$$

Wirtinger tried to characterize the topological situation along the singularity set of such a function (a curve given by the discriminant of f). In particular, Wirtinger was interested in the *local monodromy* of such a covering along the branch curve, i.e. the group of permutations of the values $p^{-1}(x_0, y_0)$ over a point (x_0, y_0), induced by analytic continuation of the function values along small closed loops starting and ending at (x_0, y_0) and avoiding the branch curve. In modern terms, this meant considering, for a neighbourhood U of a branch point with the branch curve removed, the image of the fundamental group $\pi_1(U, (x_0, y_0))$ under the canonical mapping to the symmetric group acting on the fibre $p^{-1}(x_0, y_0)$. However, it should be emphasized that, in the beginning at least, Wirtinger's work was independent of Poincaré's, and the notion of a fundamental group did not appear explicitly.

While Wirtinger noticed that along *regular* pieces of the branch curve, the sheets of the covering were permuted in cyclical order, he recognized that at *singular* points of the

[37] On Poincaré, see [141, 37, 166], and Chapter 6 in this volume.

[38] The exception confirms the rule: In 1918, Mary G. Haseman of Bryn Mawr College published her dissertation on amphicheiral knots of 12 crossings in the footsteps of Tait, Kirkman and Little. She did not mention any of the modern contributions to knots by Tietze and Dehn that had appeared in the meantime.

[39] The following paragraphs are mainly based on [40]. For a description of early work related to the Poincaré conjecture, see also [166].

branch curve the sheets could be connected in a more complicated way. Several years went by, however, before Wirtinger managed to work out a paradigmatic example, that given by the equation

$$f(x, y, z) = z^3 + 3xz + 2y = 0.$$

The discriminant of this polynomial is $D_f(x, y) = x^3 + y^2$, which yields a cubic with a cusp as branch curve. In 1905, Wirtinger presented this example to the annual meeting of the *Deutsche Mathematiker-Vereinigung*. The proceedings of the meeting give only the title of his talk, but from his correspondence with Klein and various remarks of later authors on Wirtinger's ideas the gist of what he said on that occasion is quite clear. In order to characterize the topological behaviour of a function like the one above in the neighbourhood of a singular point of its branch curve, Wirtinger brought a new idea into play which he probably had learned from Poul Heegaard's dissertation [69]. Therein, Heegaard described a similar program for a topological study of algebraic surfaces.[40] The idea that interested Wirtinger was to restrict the covering p to the boundary of a small 4-ball around the point in question, that is, to a covering of the sphere S^3, branched along a certain set K of real dimension one, namely the intersection of S^3 with the branch curve of the given algebraic function. In the particular example considered, this covering turned out to be a three-sheeted covering of S^3, branched along the trefoil knot [69, p. 85]! The point of this restriction was that it had the same monodromy as the algebraic function itself, and, moreover, that it allowed one to compute the monodromy group, as a matter of fact, to compute the fundamental group of the base space $S^3 - K$ of the restricted, *unbranched* covering. For his example, Wirtinger obtained the full symmetric group on three elements as monodromy group – and thus the first serious proof that the trefoil knot was actually knotted. It was soon realized, either by Wirtinger himself or by his younger Vienna colleague Heinrich Tietze, that Wirtinger's method actually gave a way to write down a presentation of the fundamental group of the complement of arbitrary knots and links, and not just of the trefoil knot. Moreover, it became clear that this approach could be used to describe the topology of singularities of algebraic curves (algebraic functions of *one* variable) by disregarding the covering obtained by Wirtinger and by taking the branch curve itself as the basic object to be studied.

The importance of the whole argument for the emergence of modern knot theory can hardly be overestimated. Not only had knots appeared in one of the central areas of mathematical interest, but the situation suggested a whole set of new ideas and questions. Together with a knot and its complement, *covering spaces* of knots – either coverings of the 3-sphere branched along a knot or unbranched coverings of knot complements – had come into the picture, including homomorphisms from the knot group to permutation groups. Among the obvious questions were: what kinds of knots could arise in situations like those considered by Wirtinger? What kinds of covering spaces could be obtained in such cases?

Since Wirtinger did not publish his ideas, it took some time before these problems were taken up by others. Wirtinger's basic insight and the main ingredients of the answer to the first question have often been attributed to Karl Brauner, who published a three-part article on the subject in 1928, based on his *Habilitationsschrift* under Wirtinger. Following Brauner, Kähler, Zariski, and Burau simplified and rounded off Brauner's arguments

[40] More information on Heegaard may be found in Chapter 34 in this volume.

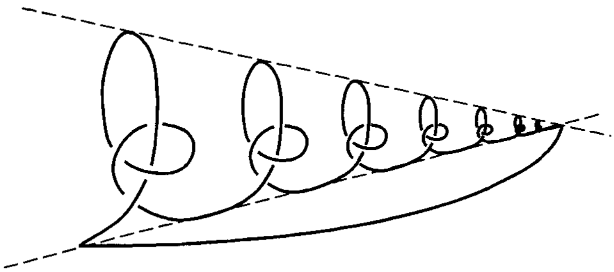

Fig. 7. Tietze's wild knot.

to obtain the final result that singularities of irreducible curves were topologically characterized by iterated torus knots while for reducible curves, links of such knots had to be considered.[41] Moreover, the knots and links arising from singularities of algebraic curves were classified by the pairs of integers determining the Puiseux expansions of the curve branches around the singularity. However, Wirtinger was certainly right when he pointed out in his (unpublished) review of Brauner's *Habilitationsschrift*: "More than twenty years ago, the reviewer has shown the way in which these difficult, but basic problems may be dealt with".[42]

Not only Brauner, but several other mathematicians who made significant contributions to modern knot theory in its early years were also inspired by Wirtinger's insights. Heinrich Tietze, Otto Schreier, Emil Artin, and Kurt Reidemeister all came in direct contact with Wirtinger at some time, and it will become clear below to what extent their ideas were influenced by Wirtinger's.

§ 18. Twenty years before Brauner, another young mathematician presented a *Habilitationsschrift* on topology, guided by Wirtinger in Vienna: Heinrich Tietze. His paper [162] marked a crucial step toward a clear technical understanding of Poincaré's topological ideas. Following a rather coherent, combinatorial approach to the topology of three-dimensional manifolds, Tietze re-established Poincaré's results, emphasizing that all then known invariants of three-dimensional manifolds could be derived from the fundamental group. Among the examples he discussed was Wirtinger's method for finding a presentation of the fundamental group of knot complements, including the example discussed by Wirtinger.[43] In addition, and in the thorough, critical spirit which marks the whole paper, Tietze formulated several basic questions related to knots and three-dimensional manifolds whose answers were unknown at the time.

First, Tietze pointed out in a discussion of Poincaré's definition of the homological invariants of manifolds that certain curves required special attention: For instance, a curve like that of Figure 7 could not be said to bound a finite two-dimensional cell complex in S^3 in Poincaré's sense. This example also made clear that the notion of a knot and of knot equivalence itself required additional care if "wild knots" were to be avoided.

[41] See [20, 82, 175, 25, 26].

[42] Quoted from [38, p. 247].

[43] However, Tietze's description of Wirtinger's ideas was scattered in different passages of his paper which made it hard for his readers to see just what Wirtinger's contribution had been. See [162, §§15, 18].

The second group of questions was inspired by Wirtinger's calculation of the group of the complement of the trefoil knot. Tietze viewed this as a region in $\mathbb{R}^3$, bounded by a torus which bounds on its other side a tubular neigbourhood of the knot. Clearly, both the right-handed and the left-handed trefoil had homeomorphic complements (and, consequently, isomorphic groups), but what about the converse? Could two knot complements be homeomorphic without one knot being isotopic to the other or its mirror image? As Tietze remarked, it was not even clear whether all submanifolds of $\mathbb{R}^3$ bounded by a torus were knot complements [162, § 15].

A third complex of questions arose from Tietze's consideration of the group of self-homeomorphisms of a (closed or bounded) manifold and its quotient by the group of those self-homeomorphisms homotopic to the identity [162, §16]. For oriented manifolds, one could also consider just the orientation-preserving self-homeomorphisms. These groups acted in a canonical way on the fundamental group of the manifold as well as on the fundamental group of its boundary. In the case of several boundary components, the group of permutations of these components induced by this action might also carry interesting information. In this way, a whole new set of topological invariants arose about which very little was known. Tietze illustrated these concepts by considering the complements of collections of disjoint right- and left-handed trefoil knots, pointing out that not even the intuitive belief that the two trefoil knots were inequivalent (a belief that he used in his illustrations) had been rigorously proved.

Finally, Wirtinger's construction suggested yet another way of looking at three-dimensional manifolds, namely as coverings of S^3, branched over a link. Manifolds described in this way were called "Riemann spaces" at the time, generalizing the idea of a Riemann surface (viewed as a branched covering of S^2). It was known that all closed, orientable surfaces could be described in this way; but, Tietze asked, could all closed, orientable 3-manifolds be described as Riemann spaces [162, § 18]?

All of Tietze's questions stressed the relations between knots (or links) and the general study of three-dimensional manifolds. In at least two ways, knots and links gave rise to interesting classes of such manifolds: by their complements, and by covering spaces. It turned out that some questions of Tietze's could be answered rather quickly by the next generation of topologists, while others resisted a solution until very recently.

§ 19. More or less simultaneously with Tietze, Max Dehn, a student of Hilbert who had started his mathematical career with brilliant results on the foundations of geometry, turned to an investigation of 3-manifolds which led him to study knots.[44] In the beginning, Dehn hoped to be able to prove an equivalent to Poincaré's conjecture that S^3 was the only closed, orientable 3-manifold with trivial fundamental group. However, a discussion with Tietze at the International Congress of Mathematicians in Rome in 1908 made clear to Dehn that his arguments were flawed.[45] Nevertheless, he continued to work on the topic. In 1910, he published a paper whose title "Über die Topologie des dreidimensionalen Raumes" indicated that he still hoped to find a topological characterization of ordinary 3-space or the 3-sphere. Instead of trying to prove the Poincaré conjecture, however, he showed how to construct infinitely many "Poincaré spaces", i.e. orientable 3-manifolds bounded by a two-sphere, with vanishing homological invariants but nontrivial fundamental group. In the

[44] On Dehn's career, see Stillwell's contribution to this volume.

[45] See [40] for more details on this.

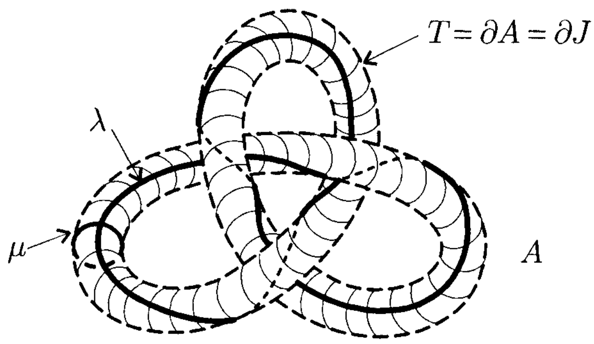

Fig. 8. Dehn's setup.

closing paragraph, Dehn outlined an argument by which he hoped to prove the Poincaré conjecture, directing the attention of his readers to the crucial gap.

While Dehn's paper made clear that the Poincaré conjecture was difficult, it broke new ground both for the study of knots and for combinatorial group theory. In order to construct his examples of "Poincaré spaces", Dehn introduced a presentation of the group of a knot different from Wirtinger's and a new criterion for knottedness. A knot is trivial, he claimed, if and only if its group is commutative. While the "only if" was obvious, the other implication required proof. Here, Dehn argued as follows (compare Figure 8): for all knots K, there exists a longitudinal curve λ on a torus T which bounds a tubular neighbourhood J of the knot, such that λ bounds in the knot complement $A := S^3 - J$.[46] If, in addition, the fundamental group of the knot complement is commutative, then λ must actually be null-homotopic in A. Hence it bounds a singular disk in the knot complement, in such a way, however, that all singularities may be removed from the boundary of the disk. At this point, Dehn invoked the famous, insufficiently proved "lemma" which today carries his name: if a closed curve in S^3 bounds a (piecewise linear) singular disk in such a way that an annulus along the boundary is free of singularities, then this curve even bounds a regularly embedded disk.[47] From the "lemma", then, it followed that λ and hence the knot itself were trivial. Regardless of the difficulties with the lemma, Dehn's criterion could be used to prove that certain knots were non-trivial by showing that their group was non-commutative.

Then Dehn proceeded to consider manifolds arising from the following construction. Let K be a knot in S^3, and let λ, J, T, and A be as above. The generators of the fundamental group of T may be represented by the longitude λ and a curve μ bounding a transversal disk in $J \cup T$. Any other element in this (commutative) group may then be represented by a curve $\lambda^l \mu^m$, or by the pair of integers, (l, m). Dehn now chose a curve ρ of class $(l, 1)$ in T and formed a new manifold $\Phi = \Phi_K(l, 1)$ by attaching to A a thickened disk D (i.e. a 3-cell, whose boundary is considered as the union of an annulus and two disks, see Figure 8) along a small strip on T that forms a neighbourhood of ρ. By construction, $\Phi_K(l, 1)$ is a manifold bounded by a sphere and with trivial homology, i.e. a "Poincaré

[46] My notation is a slightly modernized version of Dehn's.

[47] The gap in Dehn's rather involved argument was recognized in the 1920's, both by H. Kneser and Dehn himself. A sound proof was only given by Papakyriakopoulos [127]. For more details on this, see Chapter 36 in the present volume.

space". Its fundamental group arises from adjoining to the group of the knot the relation expressing the contractibility of ρ. Dehn's presentation of the knot group allowed him to express this relation in a straightforward fashion so that a presentation of the fundamental group of $\Phi_K(l, 1)$ could actually be found.

For the trefoil knot, Dehn managed to construct the graphs of the resulting groups. Of course, $\phi_K(0, 1)$ was just a 3-cell. The group of $\phi_K(\pm 1, 1)$ turned out to be a finite group of order 120, a binary extension of the group of rotational symmetries of the icosahedron.[48] All other manifolds possessed infinite groups. In this way, Dehn found an infinite family of "Poincaré spaces". Moreover, he observed that all of their fundamental groups (like the group of a trefoil knot itself) acted on the hyperbolic plane in a canonical way. Thus, he established a link between knot groups and hyperbolic geometry, a link that he exploited again in a paper of 1914 answering one of Tietze's questions. By a detailed consideration of the automorphisms of the group of a trefoil knot and their actions on longitudes λ and meridians μ of the knot, Dehn showed that the right- and left-handed trefoils were not isotopic.

One should note that Dehn's construction of "Poincaré spaces" is not *quite* the same as what today is usually called "Dehn surgery", since Dehn considered *bounded* manifolds and restricted himself to the case of attaching curves of type $(l, 1)$, a restriction that guaranteed that all manifolds obtained by his construction from knot complements were homologically trivial. I will describe below how the change to the modern point of view came about. It should also be noted that Dehn did *not* ask whether his construction might eventually produce not just a homology cell but even a counterexample to the Poincaré conjecture, i.e. a manifold bounded by a 2-sphere and with trivial fundamental group but topologically different from the 3-ball. Clearly, he still hoped he was on his way toward a proof of this conjecture, rather than a refutation.

§ 20. The geometric motivation behind Wirtinger's, Heegaard's, Tietze's, and Dehn's contributions is obvious. Neither of these mathematicians was motivated by building up a theory of knots *per se*. Rather, they were led to study knots by their research in other areas: research on the singularities of algebraic curves and surfaces, and investigations of three-dimensional manifolds as they had become tractable by Poincaré's new techniques of *Analysis situs*. Knots thus appeared in a rich geometric context, involving covering spaces or Dehn's method for constructing "Poincaré spaces". In both approaches, the knot group played a crucial role, but with different additional structures involved. Some of the problems related to these objects and structures turned out to be quite deep, and several were even too difficult to admit solutions with the methods available at the time. In many ways, later geometric-oriented research on knots, links, and in part also on 3-manifolds, tried to sort out and answer the questions raised in this first phase of a modern mathematical treatment of knots. Some crucial problems remain open even today, as we shall see.

World War I interrupted both Dehn's work and that of the Vienna mathematicians. After the war, two young mathematicians, James W. Alexander and Kurt Reidemeister, became increasingly interested in knots. In several respects, Alexander's and Reidemeister's work were strikingly parallel. Both were led to the same, indeed the first, effectively calculable invariants of knots in the mid-twenties. Moreover, both chose to present their results on the

[48] Using his theory of fibered 3-manifolds, Seifert later showed that Poincaré's original example of a homology sphere was homeomorphic to the closed version of Dehn's manifold $\phi_K(\pm 1, 1)$ [148, pp. 204ff.].

basis of the elementary, combinatorial approach to knots that has been sketched in the second section. To some extent, this parallelism may be traced to the common inspiration they found in Tietze's and Wirtinger's earlier ideas, and in particular to the idea of studying covering spaces of knots or links. On closer inspection, however, their approaches also reveal a basic difference. Probably guided by his earlier work, Alexander was mainly interested in *homological* invariants of covering spaces in his relevant contributions. Reidemeister's crucial insights, on the other hand, were concerned with the *fundamental groups* of such spaces.

§ 21. Alexander's contributions to knot theory began with several clarifications of issues Tietze had raised. During the war years, the Princeton topologist, who had already shown his talents in improving Poincaré's homological results, stayed as a volunteer in Paris and assisted in preparing a French translation of Heegaard's thesis [69].[49] It appears to have been around this time that the problems on 3-manifolds described in Tietze's paper of 1908 caught his attention. The first of Alexander's clarifications was only indirectly related to knots. Still in Paris, he showed that Tietze had been correct in conjecturing that the two "lens spaces" $L(5, 1)$ and $L(5, 2)$ were not homeomorphic [2]. Alexander defined these spaces in a way clearly influenced by Heegaard's dissertation and Dehn's paper of 1910, namely as the manifolds obtained by an identification of the boundaries of two solid tori in such a way that the meridian of one of them gets identified with a curve of type (5, 1) or (5, 2), respectively, in the boundary of the other. Since the fundamental group of both was cyclic of order 5, this showed that the fundamental group was not sufficient to distinguish 3-manifolds in all cases.

About a year later, Alexander claimed in a brief note that every closed, oriented 3-manifold given by a triangulation could indeed, as Tietze had suggested, be obtained as a covering of S^3 branched over a link [3]. His argument was strikingly simple, but incomplete. With each vertex of the triangulation, he associated a point in S^3 such that no four of these points were coplanar. By mapping the simplices of the triangulation onto the simplices of S^3 given by the corresponding vertices and respecting the orientations, Alexander obtained a covering of S^3 branched over a subcomplex of the 1-complex given by the chosen points in S^3 and the edges joining them. "It is easy to show", he continued, "that, without modifying the topology of the space, the branch system may be replaced by a set of simple, non-intersecting, closed curves such that only two sheets come together at a curve. These curves may, however, be knotted and linking" [3, p. 372]. As R.H. Fox later pointed out, the missing part of the argument could be filled in by appealing to a classical argument given by Clifford which showed that every closed Riemann surface – viewed as a branched covering of the complex number sphere – could be deformed into a covering in which only simple branch points of order 2 occur. Alexander's conclusion followed by applying this argument to a continuous family of generic plane sections of the covering obtained in the first step of his argument.[50]

Brief as the argument was, it gave a new and general construction technique for 3-manifolds. Such techniques were still rare and difficult, since triangulations were in some sense too general while the only other known method, Heegaard's decomposition

[49] On Alexander, see [90] and Chapter 32 in this volume.

[50] See [48, p. 213]. Other proofs of Alexander's claim were given by Birman, Hilden, and Montesinos, leading to sharper results, see [72, 117, 73, 118]. Today it is known that there even exist "universal knots", i.e. knots whose branched coverings exhaust all closed, orientable 3-manifolds (Hilden et al. [74]).

of a 3-manifold into two handlebodies, did not seem easy to use except in special cases. Accordingly, Alexander's result whet his interest in links and their covering spaces. Indeed, in November 1920, he presented a new idea for studying *finite cyclic* branched coverings of knots by calculating their torsion numbers. The paper, read to the US National Academy of Sciences, was not published, so that it is difficult to tell precisely what it contained. According to Alexander's own later account, he pointed out that these torsion numbers were actually invariants of the knot or link itself, and he calculated them for a few of the simpler knots. It remains unclear, however, whether he had developed a *general* method to calculate the new invariants.[51]

In 1923, Alexander further refined his picture of "Riemann spaces" by establishing a lemma showing that every link could be deformed into what was later called a closed braid. This lemma had already been demonstrated by Heinrich Brunn at the ICM in Zürich 1897, but Alexander was apparently unaware of Brunn's short note [23]. The implication of the lemma was that "every 3-dimensional closed orientable manifold may be generated by rotation about an axis of a Riemann surface with a fixed number of simple branch points, such that no branch point ever crosses the axis or merges into another" [4, p. 94].[52] A year later, Alexander settled yet another open question of Tietze's by showing that a piecewise linearly embedded torus in S^3 bounds a solid torus on at least one side, making the other side into a knot complement (Alexander 1924).

Up to this point, Alexander was clearly more interested in the 3-manifolds arising from knots or links than in the classification of links themselves. But this changed after Reidemeister's first papers appeared in 1926, describing both a general method for calculating the torsion numbers of a knot from a diagram and the "elementary foundation" of knot theory by diagram moves. In April 1827, Alexander and Briggs submitted their paper "On types of knotted curves" to the *Annals of Mathematics*, describing their own approach to torsion numbers. Although this method was presented in a combinatorial fashion, a closer analysis of the paper makes it clear that Alexander and Briggs were actually guided by Alexander's earlier ideas, and that the calculation was based on an analysis of a suitable cell decomposition of the branched cyclic covering spaces of a knot. I have described in Section 2 how this approach to torsion numbers led to the invention of the first polynomial invariant for knots. As the *infinite* cyclic covering of a knot does *not* appear in [5], it may well be that here, for the first time, Alexander was guided by the combinatorial approach rather than by a geometric one.

§ 22. Also in Reidemeister's case, the combinatorial presentation of his results gives a misleading picture of the actual course of his research. For him, it was an insight into the relation between the unbranched covering spaces of knot complements and the corresponding subgroups of the knot group that opened the way to calculable knot invariants. In 1922, Reidemeister obtained his first professorship in Vienna, and soon afterward he learned of his older colleague Wirtinger's ideas on knots. He began to study Poincaré's writings on *Analysis situs* and organized a seminar on topology and algebra in which he encountered

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[51] See [7, p. 562]. This account figures in an argument with Reidemeister on priority and must thus be taken with some caution.

[52] Also this conclusion was mathematically related to an earlier idea which Alexander may or may not have known: In 1891, Hurwitz had published a substantial paper studying the deformations of Riemann surfaces (viewed as branched coverings of the complex number sphere) arising from braid-like deformations of their branch points. See below, § 23.

the young Otto Schreier, who was full of ideas about combinatorial group theory.[53] The breakthrough came in 1925, soon after he had accepted a position in Konigsberg (Kaliningrad). In correspondence with H. Kneser, Reidemeister announced that he had found a subgroup of the knot group that possessed nontrivial torsion invariants. This group was in fact the fundamental group of the double cyclic covering of the knot complement. In the following year, Reidemeister worked up his idea into a general method for writing down a presentation of the fundamental groups of finite cyclic coverings of knot complements. The method was based on a combination of Wirtinger's method for presenting the knot group and Poincare's method for calculating the fundamental group of a 3-manifold given by a cell decomposition. The surprising fact was that, in contrast to the knot group itself, many of the subgroups obtained in this way had nontrivial torsion invariants.

In preparing the publication of his results, Reidemeister tried to present his ideas in as abstract a fashion as possible. This led him to recognize that his method for calculating subgroups of the knot group could actually be made into the method for calculating subgroups of finitely presented groups that today is known as the "Reidemeister-Schreier method" (Reidemeister [134]).[54] Moreover, he developed his "elementary foundation" of knot theory [135], a manner of presentation that was at least partially motivated by his philosophical interests in the foundations of mathematics. In Vienna, Reidemeister had become one of the early members of the philosophical circle around Hans Hahn and Moritz Schlick. During the foundational debates of the twenties, he was engaged as a convinced "modernist", emphasizing that all exact knowledge (i.e. in his view, mathematics and logic) was ultimately rooted in "combinatorial facts" about signs.[55] Little wonder, then, that Reidemeister favoured a combinatorial approach to topology.[56]

§ 23. Before arriving in Vienna, Reidemeister had held an assistant professorship in Hamburg, where a new university had been founded in 1919. Its mathematical department was directed by Wilhelm Blaschke, who received his doctorate in Vienna under Wirtinger, and by Erich Hecke, a student of Hilbert. Hamburg quickly emerged as a lively mathematical center during the 1920's, and in 1922, the department began to publish its own journal, the *Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universitat.* Reidemeister's papers of 1926 were published in this journal, and it became the main forum for knot theoretical research during the following years. Hamburg's ties with Vienna were also particularly close. At about the time when Reidemeister left for Vienna, another Viennese mathematician came to Hamburg, Emil Artin, followed soon afterward by Otto Schreier. For a short period, the two worked together on a group-theoretical problem related to knots: the classification of braids, or the word problem in the braid group.

Since the paper describing the fruits of this work, [9], has often been taken as documenting the invention of the braid group, a few words should be said about earlier interest in braid-like topological objects and related groups. As pointed out in Section 3, Gauss was probably the first to consider braids (i.e. a collection of n disjoint, smooth curves in

[53] An outcome of this seminar was Schreier's very simple group-theoretical proof that the two trefoils were inequivalent [142]. On Schreier, who died in 1929 at age 28, see [28, Chapter II.3].

[54] For a historical description of the various stages in which this method reached its final shape, see [28, Chapter H.3].

[55] A pronounced statement of Reidemeister's philosophical views can be found in [136].

56 jke "purely combinatorial" approach to topology was first advocated by Dehn and Heegaard [33], strongly inspired by the style of Hilbert's *Grundlagen der Geometrie.*

Euclidean space such that every member of a continuous family of parallel planes intersects each curve in precisely one point) as objects of topological interest. His unpublished fragment may actually be read as posing the problem of classifying braids up to a suitable notion of equivalence. Later, both Listing and Tait were interested in similar geometric objects but failed to prove substantial results about them. As with knots, it was the wish for a geometric understanding of algebraic functions that motivated mathematicians to dig deeper. Since the appearance of Puiseux's contributions [133], the idea became commonplace that the behaviour of an algebraic function of one complex variable, given by a polynomial equation *f(x, y)* = 0, could be studied by looking at the simultaneous motions of the finitely many values *y* e C that arise when the argument *x* describes loops starting and ending at a given point *a* e X, where *X* c C is the complement of the set of branch points of *f(x, y)* = 0. Puiseux and most authors following him were interested in the "monodromy group" of *f(x, y)* = 0, i.e. the group of permutations of the *n* roots of *f(a,y)* = 0 arising from all such loops. Once the conceptual apparatus of the fundamental group and the braid group became available, it was easy to see that the propositions proved by Puiseux actually yield homomorphisms

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where B_n is the n-strand braid group, E_n is the symmetric group on *n* elements, and the image of the composite homomorphism is the monodromy group. In other words, even if the notion of the braid group had not yet been defined, monodromy considerations led to knowledge concerning motions of configurations of complex numbers (we might say "braid motions") that was later encoded in the braid group.

In 1891, Adolf Hurwitz published a paper on (closed) Riemann surfaces, understood as branched coverings of the complex number sphere with finitely many sheets and a finite number *n* of branch points. Among other things, he investigated deformations of such coverings arising by a continuous change of the configuration of branch points, starting and ending at a given configuration. Thus he was again led to consider both braid motions and the special kind of braid motions where each point returns to its original position (in modern terms: motions corresponding to *pure* braids). Hurwitz went a step further than earlier authors by considering pure braid motions as loops in the configuration space

C^n (JCl. . . . JC,,) € C^n |]"](*/ - * /) = 0

whereas he thought of general braid motions as loops in the quotient of this space by the canonical action of the symmetric group E_n. Still, the braid group did not appear explicitly in his paper. Instead, for two given natural numbers *n* and m, Hurwitz considered the set of Riemann surfaces with *m* sheets and *n* branch points, each surface being specified by the *n* sheet permutations *s*\,..., s_n e E_m associated with the *n* branch points. He managed to give rules for determining the "monodromy groups" of permutations of the surfaces, induced by either braid or pure braid motions. Like Wirtinger, who seems to have studied the monodromy of coverings of a knot complement without explicitly discussing the knot group, Hurwitz seems to have been unaware that his rules actually determined the braid and pure braid group itself. In the case of braid motions, his result was that the "monodromy group" in question was generated by the permutations of surfaces,

$$\sigma_i := \begin{pmatrix} s_1 & \dots & s_i & s_{i+1} & \dots & s_n \\ s_1 & \dots & s_i s_{i+1} s_i^{-1} & s_i & \dots & s_n \end{pmatrix}, \quad 1 \leqslant i \leqslant n-1. \qquad (\star\star)$$

Since the sheet permutations s_k were left unspecified in Hurwitz' argument, the group generated by the σ_i may be understood as a group of automorphisms of the free group on n generators which is in fact a faithful representation of the braid group as Artin would show. Again, while the notion of braid group itself was absent, insights were developed that could immediately be transformed into knowledge about this group once it was defined.

The group-theoretic structure ($\star\star$) obtained by Hurwitz reappeared with a different interpretation in another context of ideas, relating to transformations of Riemann surfaces onto themselves Fricke and Klein [55, pp. 299ff.]. In this work, the idea of "braid motions" was less visible, but soon after the explicit definition of the braid group, Wilhelm Magnus showed how some of Fricke's and Klein's ideas could be translated into a connection between the mapping class group of the n times punctured plane or sphere and the braid group [98].

In the early twentieth century, the idea of what I have called "braid motions" was certainly well known to most mathematicians interested in algebraic functions and related issues. We have seen that it served Alexander to study 3-manifolds, and certainly Artin and Schreier were acquainted with it, too. In this light, Artin's geometric definition of the n-strand braid group B_n and his presentation of B_n as the group generated by $n-1$ elements $\sigma_1, \dots, \sigma_{n-1}$, with relations

$$\sigma_k \sigma_l = \sigma_l \sigma_k \quad \text{if } |k-l| \geqslant 2;$$

$$\sigma_k \sigma_{k+1} \sigma_k = \sigma_{k+1} \sigma_k \sigma_{k+1} \quad \text{for } k = 1, \dots, n-2,$$

appears less as an invention out of the blue but rather as a properly topological or group-theoretical definition of a known structure. The emphasis of Artin's paper was clearly on translating the geometric questions about braid motions into purely group-theoretical questions. In particular, the classification of braids up to the appropriate kind of isotopy was restated as the word problem of the braid group, while the classification of *closed* braids amounted to the conjugacy problem of the braid group. On the other hand, Artin probably knew Hurwitz' paper and its geometric techniques. In order to solve the main problem of his paper, the word problem in B_n, Artin used precisely the representation of braids as automorphisms of the free group on n generators that Hurwitz had (almost) defined. The crucial step was to show that this representation is indeed faithful, and here Artin relied on a topological argument quite close to some of Hurwitz' ideas. Moreover, when looking at closed braids, Artin used Wirtinger's presentation of a knot group as it had been communicated to him by Schreier.

Thus, on the whole it is clear that, as with the case of knots, interest in braids was closely tied to a geometric approach to algebraic functions; the latter provided the background for the investigations of the combinatorial and group-theoretic aspects that came into focus after Artin had published his paper.

§ 24. After Alexander's homological and Reidemeister's and Artin's group-theoretical contributions had shown how to construct calculable invariants of knots and links, the prospects for developing a "theory of knots" in its own right seemed promising. Many problems seemed tractable, and Dehn's and Alexander's results connecting knots with 3-manifolds, as well as the work inspired by Wirtinger on singularities of algebraic functions established sufficiently many links to other fields to convince others that knot theory was an interesting subject. At the same time, the piecewise linear, combinatorial approach to knots used by Reidemeister and Alexander made it possible to develop knot theory without entering the intricacies of these other fields too deeply. This enabled newcomers to join the enterprise. Indeed, both at Königsberg and Hamburg a number of students of Reidemeister and Artin started to work on knots, and the flow of papers to the *Hamburger Abhandlungen* increased steadily. In 1932, Reidemeister's monograph *Knotentheorie* summarized the results obtained until then (leaving out most connections to other fields, however) and provided a kind of "paradigm" in the sense of Thomas Kuhn for the young field. On the mathematical level, this period of flourishing activity was oriented toward a finer study of particular classes of knots or links (such as the links arising from singularities), a better understanding of the invariants that had been constructed, and a discussion of their power in distinguishing knots and links.

A significant contribution to the understanding of Alexander's invariants was made by Herbert Seifert, exploiting a geometric idea that lay dormant since Tait's days. While it had long been known that surfaces embedded in space and bounded by an arbitrary knot could be found (this followed for instance from Tait's observation that every knot diagram could be coloured in a chequerboard-like fashion), an additional argument was needed to show that *oriented* surfaces bounded by a given knot existed as well. A procedure to find such a surface was described by Frankl and Pontrjagin [52]. Seifert saw that one could use such surfaces – today called Seifert surfaces – for the construction of cyclic coverings of a knot complement and hence for a calculation of homological knot invariants [150]. In particular, Seifert was the first to describe the Alexander polynomial in terms of the first homology group of the *infinite* cyclic covering of a knot complement. He showed that this group could be viewed as a module over the ring $\mathbb{Z}[x, x^{-1}]$, and that the Alexander polynomial was given by the determinant of a presentation matrix of this module. Seifert's construction also made it possible to obtain information about the minimal genus g_K of Seifert surfaces, an invariant of the knot K which he called its "genus". For all knots, the degree of the Alexander polynomial was a lower bound for $2g_K$. Since a more or less sharp upper bound on g_K could be read off a diagram, this enabled calculations of the genus of many knots such as the torus knots and all knots of up to 9 crossings. Moreover, Seifert was able to describe a nontrivial knot all of whose cyclic coverings were homology spheres. This showed that the Alexander polynomial was not sufficient to detect knottedness [150, § 4].

Already in an earlier paper, Seifert had observed that the two composite knots presented in Figure 9, without being mirror images of each other, had the same group. Seifert distinguished these knots by a new type of "linking invariants", computed from the torsion subgroup of the first homology group of cyclic coverings of the knots [149]. This showed that the group of a knot was not a complete invariant, at least for composite knots. Moreover, Tietze's question whether a knot was determined by the homeomorphism type of its complement also remained a mystery. By a rather simple example, J.H.C. Whitehead pointed out in 1936 that the analogous statement for the case of *links* was false [171], and thus the answer to Tietze's question seemed quite unclear (see Figure 10).

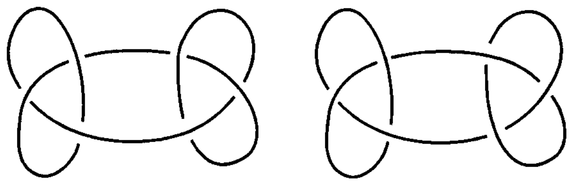

Fig. 9. Knots with the same group.

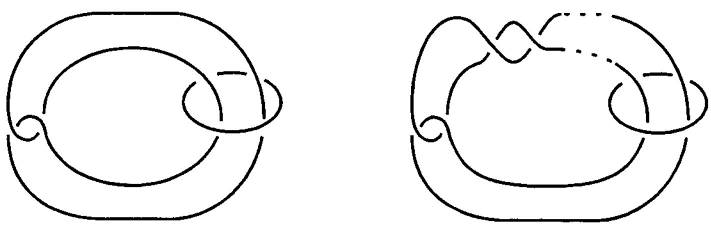

Fig. 10. Whitehead's links having homeomorphic complements.

Yet another way of looking at the Alexander polynomial came into view when Werner Burau found a rather surprising connection of this invariant to braids. Burau showed how the Alexander matrix of a link, represented by a closed braid of n strands, could be calculated from a linear representation of the braid group,

$$\beta : B_n \to GL(n, \mathbb{Z}[x, x^{-1}]),$$

that today carries his name [24].[57] In particular, if a knot K could be represented by closing a braid w, then up to a normalization, its Alexander polynomial was given by $\Delta_K(x) = \det(\beta(w) - I)$. Further light was thrown on the relation between braids and knots by a conjecture of the Russian mathematician A.A. Markov at the congress on topology in Moscow in 1935 [102]. He claimed that two closed braids, given by elements $v \in B_m$ and $w \in B_n$ in different braid groups, represented isotopic links if and only if v and w could be related by a sequence of modifications

$$a \leftrightarrow bab^{-1}\ (a, b \in B_k) \quad \text{or} \quad a \in B_k \leftrightarrow a\sigma_k^{\pm 1} \in B_{k+1}.$$

At the time, Markov's conjecture was not seriously pursued nor was it related to Burau's results. Only much later, Joan Birman included a full proof of it in her book [17].[58]

In Germany, this period of a rapid development of the young field was ended by the consequences of the Nazi regime's takeover. Already in April 1933, Reidemeister lost his professorship in Königsberg for being "politically unreliable".[59] After a lapse of a year, he obtained a new position in Marburg, but he had lost most of his Königsberg students and spent his Marburg years in growing isolation. Some of Reidemeister's students moved

[57] Joan Birman reports that Burau had learned of this representation either from Reidemeister or from Artin.

[58] The proof was based on notes taken at a seminar at Princeton University in 1954 [17, p. 49].

[59] See [39] for a description of the circumstances.

to Hamburg, but there, the situation was difficult as well. In 1937, Artin and his wife had to leave Germany because she was Jewish. After the pogroms of November 1938, Dehn, too, was forced to flee from Frankfurt under rather dramatic circumstances (Siegel [151]). Seifert was ordered by the German ministry of education to go to Heidelberg in 1937. There, the Nazis had driven the two Jewish professors of mathematics, Liebmann and Rosenthal, out of their positions. This interrupted Seifert's productive collaboration with William Threlfall. After the war broke out, research on knot theory was also abandoned outside Germany. Topologists like Alexander and Whitehead took over new tasks in the military and left knots and links behind.

§ 25. A look back on the events described in this section shows how deep the changes in the mathematical treatment of knots were that occurred between the late nineteenth and the early twentieth century. Research on knots needed no longer to be justified by its function in scientific contexts beyond mathematics. New kinds of mathematical objects and techniques definitely transcended the limits imposed by thinking of knots and knot complements as figures or regions in physical space. Moreover, an impressive range of problems could be dealt with in a rather rigorous way and with promising results. All these aspects point to the modernity which the new field shared with much contemporaneous mathematics.

A particular shade of this modernity is also visible in Reidemeister's successful attempt to build up knot theory in a very autonomous, combinatorial fashion, the Hilbertian roots of which can easily be discerned. Nevertheless, I hope to have made clear that both the main motivations and the complex mathematical objects that allowed mathematicians to reach a deeper understanding of knots did not originate in this "elementary" way. They came from the highly valued field of algebraic functions and from Poincaré's ideas on three-dimensional manifolds. Neither Alexander's nor Reidemeister's nor Artin's innovations would have been possible had they not been acquainted with the corresponding ideas of mathematicians like Hurwitz, Wirtinger, Dehn, or Tietze. For this reason, the combinatorial shade of modernity should not be overplayed in our understanding of the emergence of modern knot theory.

5. Some geometric topics in knot theory after 1945

§ 26. While the emergence of modern knot theory in the early decades of the 20th century can be described as a relatively coherent fabric of events, the further development of knot theory becomes increasingly complex. In part, this results from the fact that knot theory did not attain the status of a self-sustaining subfield of mathematics, with its own separate domain of problems and methods, with its own publication forums, institutional networks, etc. Rather, research on knots remained tied to the broader development of low-dimensional topology, especially the theory of 3-manifolds. This holds both with respect to mathematical ideas and with respect to the social setting of work on knots. Although there emerged a group of experts in knot theory, most of the important work was done by mathematicians who had interests in other areas as well. Consequently, a historical narrative sensitive to issues of motivation and context cannot isolate knot theory after 1945 from the spectrum of related mathematical activities. This makes our subject both interesting and difficult. The overwhelming proliferation of mathematical research during the second half of this century, in which the discipline of topology played a crucial role, is reflected in the

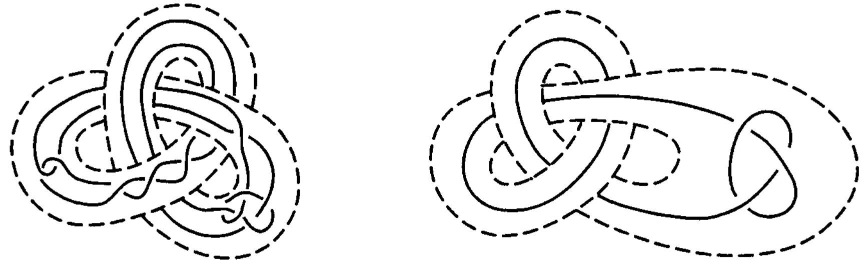

Fig. 11. Two satellites of the trefoil knot.

broad range of new concepts and techniques that were formed in order to deal with knots. The following paragraphs try to capture a part of this complexity. Guided by the main theme of this article, the following groups of issues will be discussed: the systematization of the categorical framework of knot theory by Ralph H. Fox and his Princeton group (§ 27); John Milnor's work related to knots (§ 28); the use of surgery techniques and the relation between knots and the Poincaré conjecture (§ 29); the ideas of Wolfgang Haken and others that led to a proof that knots can be classified algorithmically (§ 30); the discovery of the hyperbolic structure of most knot complements by Robert Riley and William Thurston (§ 31); and the path leading from von Neumann's construction of the "hyperfinite II_1 factor" to Vaughan Jones's new knot invariant (§ 32). Several further developments are passed over in silence, but the narrative should enable the reader to perceive the main geometric impulses that induced 20th-century mathematicians to investigate knots.

§ 27. For many years after the defeat of the Nazi regime in 1945, knots and links did not play a significant role in mathematical research in Germany. Reidemeister gathered a new group of research students with interest in low-dimensional topology and knot theory only after accepting a professorship in Göttingen in 1955, at the age of 61. In Heidelberg, where Seifert was teaching, Horst Schubert stood out as the major exception from the rule that mathematics in Germany now had other concerns than knots. Schubert had begun his studies during the war with Threlfall in Frankfurt, and followed him when Threlfall received a call to Heidelberg in 1946. In his dissertation, Schubert showed that knots formed a commutative semigroup with unique prime decomposition under the product operation given by tying two separate knots on the same string [143]. Schubert's *Habilitationsschrift* [144] treated knots K embedded in a solid torus J that formed a tubular neigbourhood of another non-trivial knot K', such that K could not be deformed into K' or the unknot by isotopies within J. The knot K' was called a "companion knot" of K by Schubert, while K later became to be called a "satellite" of K' (see Figure 11 for two examples; note that the right one indicates that all product knots are satellite knots). Schubert showed how certain invariants of satellite knots, such as their genus, were related to the corresponding invariants of their companions. A year later, Schubert introduced a new invariant, the "bridge index" of a knot [145]. This invariant could be defined from knot diagrams, namely as the least number of diagram arcs that extend from one undercrossing to the next while passing at least one overcrossing in between. Like the crossing number or the genus of a knot, the new invariant was in general difficult to calculate, but Schubert was able to give a complete classification of knots with bridge index 2 [146].

In contrast to the situation in Germany, knots quickly received attention in the United States. Soon after the war, a new center for research in knot theory developed in Princeton. There, the earlier local tradition in topology, the emigration of German mathematicians, and the new international contacts created a favourable environment for research on knots and related topics. From 1946 to 1958, Artin taught at Princeton University, and in the late forties, Reidemeister and Seifert also stayed at the Institute for Advanced Study for some time. But it was Ralph H. Fox who became the central figure in a group of young mathematicians interested in knots, links, and three-dimensional topology. Fox had obtained his doctorate under Solomon Lefschetz before joining the Princeton faculty in 1945. For a certain period, he closely collaborated with Artin, who was then reconsidering his earlier work on the braid group. Together, they published an article on "Some wild cells and spheres in three-dimensional space" which raised at least two important issues. On the one hand, they asked for a clear delineation of the domain of knot theory within three-dimensional topology; on the other, it hinted at a relation between the complements of (maybe wild) knots or knotted arcs and the Poincaré conjecture [49].[60] During the following years, many of Fox's students started their careers with contributions to knots and links, often with a view toward 3-manifold theory.[61]

At the International Congress of Mathematicians in 1950, Fox presented a first survey on the work of his group, which began with a criticism of the combinatorial fashion in which knot theory was conceived during the 1920's and 1930's:

> This description of what I may call classical knot theory tends, by its narrowness, to isolate the subject from the rest of topology. It is to be hoped that the various special theorems which make up classical knot theory will eventually turn out to be particular cases of general topological theorems. In working toward this end the following principles seem almost obvious: (A) *The class of polygons should be replaced by a suitable topologically defined class of curves.* [...] (B) *Euclidean 3-space should be replaced by other compact 3-manifolds.*[62]

The interest of Fox and some of his students in wild arcs was tied to this desire to redefine the objects of knot theoretical studies. When in the early 1950's, Edwin Moise proved that topological 3-manifolds could be triangulated and that, moreover, the "Hauptvermutung" of combinatorial topology was true in this case, it became clear that "classical" knot theory could indeed be reformulated according to Fox's ideas as the theory of (orientation preserving) homeomorphism classes of (oriented) tame simple closed curves in S^3 (or a different 3-manifold).[63] Before these clarifications, Fox had proposed to work with isotopy classes of smooth curves and conjectured that every smooth curve was actually tame. A proof of this conjecture was later included in [32].

This successful effort to readjust the foundation of knot theory must be seen in the context of a general reaction to the earlier, purely combinatorial style of low-dimensional topology. As R.H. Bing put the matter in an inspiring paper that will be discussed below,

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[60] The terminology of "tame" and "wild" curves in a 3-sphere was introduced in this paper. A curve, surface, or domain in S^3 was said to be "tame" if and only if it could be transformed into a simple polygon, polyhedral surface, or solid polyhedron by a self-homeomorphism of S^3, respectively, and "wild" if this was not the case.

[61] A list of Fox's research students is given in the second volume of Milnor's *Collected Papers*, dedicated to Fox [114, vol. 2, p. xi]. A look at the bibliography of Burde and Zieschang [27] shows that almost all of them worked on topics related to knots, links, braids, or higher dimensional analogues.

[62] [45, p. 453]. Emphasis in the original.

[63] This was pointed out by Moise himself at the ICM 1954 [115]; a proof appeared the same year [116].

the trend was to "regard a 3-manifold as a concrete object [described by appropriate topological constructions] rather than an abstraction of combinatorially equivalent systems of symbols" [12, p. 17]. Of course, this desideratum was particularly easy to fulfill in the three-dimensional case, once it was clear that triangulations existed and the "Hauptvermutung" was true. At any rate, the new perspective on knots advocated by Fox tended to make explicit the integration of knot theory into the broader field of low-dimensional topology. As I have described above, a similar view had also guided the research of the pioneers of modern knot theory, but this perspective had more or less vanished from the printed texts of "classical" knot theory of the 1920's and 30's (note how quick a "modern" approach had become "classical").

In his talk at the ICM in 1950, Fox also reported on certain new ideas developed at Princeton concerning the algebraic structure of the knot group and their presentations. At the time of his talk, these ideas had not yet appeared in print, but during the following years Fox gradually unfolded them in a series of articles. One of his guiding ideas, for a knot in a 3-manifold M represented by a knotted solid torus J with boundary ∂J, was to consider the commuting diagram of homomorphisms:

$$\begin{array}{ccc} \pi_1(\partial J) & \rightarrow & \pi_1(J) \\ \downarrow & & \downarrow \\ \pi_1(M-J) & \rightarrow & \pi_1(M) \end{array}$$

where the arrows were given up to a conjugation in the respective image by the canonical embeddings of manifolds. In the case of knots in S^3, the information contained in such a diagram was already captured by the conjugacy class of subgroups of the knot group generated by the homotopy classes of a meridian and a longitude of the knot. Such subgroups Fox called the (maximal) peripheral subgroups of the knot group. He conjectured that all known knot invariants could be derived from the knot group together with the class of its peripheral subgroups. He also mentioned that Dehn's proof of the inequivalence of the two trefoil knots could be interpreted as an argument about peripheral subgroups. Moreover, he reported that he had been able to show that no automorphism of the group of the two knots discussed by Seifert (see Figure 9) preserved peripheral subgroups.[64] Fox further suggested that a proof of his conjecture might possibly depend on a proof of Dehn's lemma. As it turned out, he was right, but even after Dehn's lemma had been saved by C.D. Papakyriakopoulos in 1957 it took a long time and hard work to establish that the answer to Fox's question was affirmative, as will become clear from what follows.

Next, Fox mentioned an algebraic tool for the study of group presentations, his so-called "free differential calculus", by which not only Alexander's polynomial could be investigated but also the finer structure of the "elementary ideals" of the group ring $\mathbb{Z}[x, x^{-1}]$ of the abelianized knot group associated with a given presentation of the knot group. This calculus, first discussed in a series of papers starting to appear in 1953, was made popular by two publications that did much to disseminate the Princeton group's work on knot theory: Fox's "A quick trip through knot theory" (1962) and the *Introduction to Knot Theory* by Fox and his former student R.H. Crowell (1963), the first monograph on knot theory since Reidemeister's book. Together with his "Quick Trip", Fox published a list of open problems on knots. The two most fundamental were: (1) Tietze's old question, "Is the type

[64] The argument, based on a discussion of the representations of this group in the symmetric group on five elements, was published in [46].

of a knot determined by the topological type of its complement?", and (2), the new, complementary problem whether the topological type of a knot complement was determined by the knot group and its peripheral subgroups.

§ 28. Toward the end of his 1950 talk, Fox had also reported on the work of a then 19-year-old student that fell somewhat outside the range of topics otherwise described: John W. Milnor's study of the total curvature $\kappa(K)$ of a knot K [110]. Using a definition of total curvature applicable to *any* continuous closed curve, Milnor showed that, when K varied in its isotopy class $\mathfrak{K}$, the greatest lower bound of $\kappa(K)$ was a positive integer multiple of 2π, and equalled 2π only if the knot was isotopic to a circle.[65] Thus the integer

$$\mu_K := \inf_{K \in \mathfrak{K}} \frac{\kappa(K)}{2\pi}$$

was a knot invariant that, for the first time in the development of knot theory, involved a notion from differential geometry. Milnor showed how to relate this invariant to a Morse-theoretic view of knots. He began with the observation that every knot in a generic position in space attains a finite number of height maxima with respect to a given axis. The minimum number of such maxima, which Milnor called the "crookedness" of a knot, was just μ_K. As a matter of fact, it was not difficult to see that the crookedness of a knot and its bridge index (defined by Schubert a little later) were the same numbers. This gave a nice example of how a combinatorial knot invariant could have a geometric meaning.[66]

In his master's and doctoral theses [111, 112], written under Fox's direction, Milnor dealt with a new geometric idea concerning links. In order to describe this, it may help to look back at Gauss's linking number briefly. It was clear that this number was not only invariant under ambient isotopies of the link, but also under deformations where each component of the link might cross itself, but no two components were allowed to have mutual intersections. Such deformations were called "link homotopies" by Milnor. Invariants under this kind of deformation captured information about the proper "linking phenomena" in links, disregarding the possible knotting of individual link components.[67] By considering the factor group G/G_q of the fundamental group G of the link complement by its qth lower central subgroup, Milnor was able to define certain new numerical invariants of link homotopy, depending not just on two components of a link but on finitely many. These "higher linking numbers" represented a generalization of Gauss's invariant, i.e. for the special case where only two link components were considered the definitions were equivalent. A geometric ingredient in Milnor's technical arguments that documents the influence of Fox's ideas was the essential use of longitudes and meridians of the link components.

In 1957, Fox and Milnor together published a short note in the *Bulletin of the AMS* in which a new research theme was announced that would occupy Milnor's attention repeatedly during the following years. It concerned the relation between knots and singular points

[65] This confirmed the conjecture of Borsuk [19] that the total curvature of a non-trivial knot was bounded from below by 4π. Borsuk's conjecture was proved independently by Fáry [44].

[66] Strangely enough, Schubert originally claimed that his bridge number was independent of Milnor's crookedness [145, p. 245].

[67] The idea to look at this kind of deformations had already appeared in a dissertation by Erika Pannwitz in 1931, written under the direction of Otto Toeplitz. Using the idea, Pannwitz showed that there always exist lines in space intersecting a non-trivial two component link (or a knot) in at least four points [126].

of surfaces in a four-dimensional manifold, a problem that had stood at the beginnings of modern knot theory as we have seen in Section 3. Also with respect to this topic, Artin functioned as a mediator between the earlier generation of knot theorists and the Princeton mathematicians. In 1925, Artin wrote a brief paper in which he discussed the purely topological aspects of the situation as considered earlier by Heegaard and Wirtinger [10]. Artin pointed out that, contrary to the beliefs of some of his contemporaries, knotted surfaces in $\mathbb{R}^4$ (in particular, knotted spheres) did exist. To construct examples, he introduced a technique later called "spinning": a knot or a knotted arc in a half space in $\mathbb{R}^3$, bounded by a plane E, was "rotated" in $\mathbb{R}^4$ about E. The surface covered by the moving knot was then a knotted surface in 4-space. Moreover, Artin pointed out that the kind of singularities discussed by Heegaard and Wirtinger could be described in purely topological terms, without reference to algebraic functions. Given a point in a piecewise linear, closed surface F embedded in $\mathbb{R}^4$, the intersection of F with the boundary S^3 of a small 4-ball around the given point was a knot whose isotopy class in S^3 was a complete invariant of the surface point with respect to deformations in $\mathbb{R}^4$.[68] If the knot was non-trivial, the point could be considered as a "combinatorial singularity" of F, as Artin called it. Examples could be obtained by forming the "cone" on a given knot in $\mathbb{R}^3$, i.e. by joining all points of the knot by straight line segments with a vertex in $\mathbb{R}^4$ outside the hyperplane containing the knot.

In their research announcement, Fox and Milnor proposed to study these kinds of local singularities of piecewise linear embeddings of oriented 2-dimensional manifolds into piecewise linear, oriented 4-dimensional manifolds more closely. They claimed that a collection of knots $K_1, K_2, \ldots, K_n$ could arise from singularities of a 2-sphere in $\mathbb{R}^4$ if and only if the product knot $K_1 K_2 \ldots K_n$ could be obtained from a single singularity. This gave rise to the introduction of a new concept and a new equivalence relation among knots. A knot obtained from a *single* singularity of a 2-sphere, or, equivalently, as the boundary of a non-singular disc, embedded in a half space of $\mathbb{R}^4$ bounded by a hyperplane containing the knot, was called a "slice knot".[69] Two knots K_1 and K_2 were called equivalent, if and only if the product $K_1(-K_2)$ of K_1 with the "inverse" of K_2 (i.e. its mirror image with reversed orientation) was a slice knot. The equivalence classes of knots under this relation formed a commutative group. Fox and Milnor remarked that a necessary condition for a knot K to be a slice knot was that its Alexander polynomial had the form $\Delta_K(x) = p(x)p(x^{-1})$ for some $p \in \mathbb{Z}[x]$. This allowed them to conclude that the new group was not finitely generated.

In 1966, Fox and Milnor published a more detailed paper summarizing their ideas in a revised and extended form. There, they also showed that the new equivalence relation could be regarded as a kind of relative cobordism relation between knots: two oriented knots were equivalent if and only if they could be placed in two parallel hyperplanes in $\mathbb{R}^4$ such that in the region of 4-space between these hyperplanes, a non-singular, oriented annulus could be found which was bounded by the two knots (with correct orientations). Accordingly, Fox and Milnor proposed to call their group the *knot cobordism group*.

In the years between the authors' first announcement and the paper of 1966, their ideas on knot cobordism had been communicated to several other people, and in particular, to a group of mathematicians working in Japan. This connection had been established in the

[68] Here, Artin's claim was necessarily vague. As Fox and Milnor [51] pointed out, it was only clear that the knot was a "combinatorial" invariant of the embedding, i.e. unchanged by piecewise linear deformations.

[69] This last term was actually absent from Fox's and Milnor's announcement, but was introduced in Fox's "Quick Trip".

late 1950's by Fox, and during the 1960's a great number of articles on knots appeared in the *Osaka Journal of Mathematics*. Many of them focused on the brand new topic of slice knots. Perhaps the most important outcome of this research was a paper by Kunio Murasugi in which the signature of knots – the signature of a quadratic form derived from the first homology group of a Seifert surface of minimal genus – was discussed and shown to be a cobordism invariant [120]. Since then, slice knots and knot cobordism continued to form a focus of research at the interface between knot theory and 4-manifolds.[70]

Milnor's interest in the relation between knots and singularities took a new turn after Egbert Brieskorn, using techniques analogous to the Heegaard–Wirtinger construction, showed that certain algebraic varieties yielded examples of exotic spheres. Brieskorn considered the intersection of the varieties

$$V_n := \left\{(z_1, \ldots, z_{n+1}) \in \mathbb{C}^{n+1} \,\middle|\, z_1^3 + z_2^2 + \cdots + z_{n+1}^2 = 0\right\}$$

with the boundary S^{2n+1} of a ball centered at the origin, giving rise to smooth manifolds homeomorphic to S^{2n-1} and knotted in S^{2n+1}. Brieskorn showed that for certain n, for instance $n = 5$, these knotted spheres were even exotic, i.e. their differentiable structure inherited from $\mathbb{C}^{n+1}$ was inequivalent to the standard differentiable structure on S^{2n-1} [22]. Milnor set out to study the singularities of complex hypersurfaces, i.e. zero sets of polynomials, along similar lines [113]. His basic result was a fibration theorem: if S_ε was a sphere of sufficiently small radius ε around an arbitrary point $\vec{z}^0 = (z_1^0, \ldots, z_{n+1}^0)$ of a complex hypersurface V given by $f(\vec{z}) = 0$, and if K denoted the intersection $V \cap S_\varepsilon$, then $S_\varepsilon - K$ was a smooth fibre bundle over S^1, with projection mapping $\phi(\vec{z}) = f(\vec{z})/|f(\vec{z})|$, having a smooth, parallelizable $2n$-manifold F as fibre. From this theorem, further information on the algebraic topology of the singularity could be drawn. In the "classical case" of an isolated singularity of an irreducible, complex algebraic curve characterized by a knot K, Milnor's theorem implied that the complement $S^3 - K$ admitted a fibration by Seifert surfaces of minimal genus. Using another deep result of the Princeton school, a theorem of Neuwirth and Stallings characterizing knots with complements fibred over S^1, Milnor concluded that the commutator subgroup of the group of K was a finitely generated free group whose rank μ equalled the degree of the Alexander polynomial of K. Moreover, μ was twice the genus of the fibre F, i.e. the genus of the knot [113, p. 84]. Milnor's main interest, though, concerned higher-dimensional generalizations of this situation.

In all of Milnor's contributions to knot theory, a strong component of geometric thinking is clearly visible. The essential new ideas – curvature, link homotopy, knots as invariants of local singularities of surfaces in 4-manifolds, knot cobordism, Milnor's fibration – were all of a geometric character. There can be no doubt that it was this aspect that made his work so fruitful in stimulating further research. Also Brieskorn's examples, weaving together algebraic geometry, knotted spheres in higher dimensions, and exotic differentiable structures, gave a significant impulse to research in all fields concerned.[71]

§ 29. Another series of new researches on knots and their role in the theory of three-dimensional manifolds was initiated when Dehn's technique for constructing "Poincaré spaces" was elaborated in the early 1960's. As in Dehn's case, the main impulse to do this

[70] See, for instance, the long list of problems relating to this topic in [87].

[71] On the topology of singularities, see also Chapter 13 in this volume.

came from renewed attempts to decide the Poincaré conjecture (here always taken to refer to the three-dimensional case). In 1957, Christos D. Papakyriakopoulos, a mathematician supported by Fox although he had apparently been unproductive for several years, published proofs of Dehn's lemma and two other fundamental theorems, the loop theorem and the sphere theorem. These theorems provided new tools to draw geometric information on 3-manifolds from knowledge of their algebraic topology, and specialists in the field agree that they marked "the beginning of the modern period of growth in 3-dimensional topology" [114, p. xi]. In particular, substantial progress toward a resolution of Poincaré's long-standing problem seemed possible. In the late 1950's, rumours spread in Princeton that several independent proofs were on the way (Bing [14, p. 124]).

In 1958, a paper of Bing on "Necessary and sufficient conditions that a manifold be S^3" brought knots back into the discussion on Poincaré's conjecture. As a matter of fact, Bing, who came like Moise from R.L. Moore's school of general topology, tended to *disbelieve* the conjecture: "The conjecture has not been proved, and I suspect that perhaps being simply connected is not enough to insure that [a closed, orientable 3-manifold] M is topologically S^3".[72] Bing introduced his paper by describing an example of Whitehead of an open, bounded, simply connected subset U of $\mathbb{R}^3$ with connected boundary that nevertheless was topologically different from $\mathbb{R}^3$. In fact, Bing noticed that this open set failed to satisfy a topological property of ordinary 3-space that Artin and Fox had described in their paper on wild cells and arcs, for U contained a simple closed curve that could not be enclosed in a "topological cube", i.e. a 3-ball. Bing's main theorem then asserted that this property – that every simple closed curve can be enclosed in a "topological cube" – was indeed necessary and sufficient to conclude that a closed, connected 3-manifold was homeomorphic to S^3. Knots came into play both in the form of an ingenious trick in Bing's proof of this theorem (see [12, § 5]) as well as in his concluding discussion of various constructions that could perhaps produce counterexamples to the Poincaré conjecture. After discussing handlebody decompositions of 3-manifolds,[73] Bing considered 3-manifolds that could be decomposed into a solid torus and the complement of a tubular neighbourhood of the trefoil knot ("a cube with a knotted hole"). Such manifolds could be thought of as formed by removing a knotted solid torus J from S^3 and "sewing it back" in a different fashion by identifying the boundary torus of both components in various ways. The possible identifications were determined by the image of a meridian of the solid torus J on the boundary torus of $S^3 - J$. Indeed, the resulting manifolds were just Dehn's $\Phi_K(l, m)$ with a 3-sphere filled in to close the manifold. In contrast to Dehn, Bing now considered, for K the trefoil knot, *all* possibilities for the attaching curve and not just those with $m = 1$. (In the following, I will denote the *closed* manifold by $\Phi_K(l, m)$, too, and the construction will be referred to as "Dehn surgery on K".) A presentation of the fundamental group of the resulting manifold could easily be found by adding the relation that expressed the contractibility of the attaching curve to the relations defining the knot group. By analyzing these presentations, Bing showed that $\Phi_K(l, m)$ was simply connected if and only if $m = \pm 1$ and $l = 0$. Moreover, in these cases the manifold was homeomorphic to S^3. Thus, from Dehn surgery on the trefoil knot, no counterexample to the Poincaré conjecture could be formed.

Bing closed his paper with a series of questions. Papakyriakopoulos had informed him, he reported, that in the above construction, the trefoil knot could be replaced by an *arbi-*

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72 [12, p. 18]. On Moore's school, see [173].

73 In particular, Bing pointed out that no manifold with a decomposition into handlebodies of genus one could lead to a counterexample to the Poincaré conjecture.

trary knot K with the result that any simply connected $\Phi_K(l, m)$ would still be homeomorphic to S^3. Was the same true, Bing asked, for manifolds from which two or more knotted and perhaps linked solid tori were removed and replaced differently? Moreover, did every simply connected compact 3-manifold belong to this class? If the answer to both questions were yes, the Poincaré conjecture would have been proved. If, on the other hand, the answer to either question were no, a counterexample might eventually be constructed.[74]

It turned out that the first question was difficult to answer. In fact, even Papakyriakopoulos' claim was only a conjecture as Bing pointed out in a correction to his paper [13]. The second question, however, was quickly answered in the affirmative. Using a general machinery of "modifications" of differentiable 4-manifolds, A.H. Wallace showed that every differentiable, closed and orientable 3-manifold could indeed be obtained by a finite number of Dehn surgeries on a link of disjoint solid tori [169]. Soon afterward, W.B.R. Lickorish gave an elementary and very geometric proof that the same could be shown in the piecewise linear category [92]. Lickorish's basic idea was to decompose a given oriented 3-manifold into two handlebodies and then to use a sequence of Dehn surgeries to simplify the boundary identification of these handlebodies until a 3-sphere was obtained.[75] In this way, a new technique for constructing and handling closed orientable 3-manifolds was established. The necessary data (what came to be called a "surgery description" of the manifold) were a link and, associated with each of its components, a rational number $r = m/l$ specifying the type of the surgery on a small tubular neighbourhood of this component.[76] It was quickly realized that Dehn surgery could be used to calculate invariants of 3-manifolds by controlling the effect of the surgery operations on the invariants in question. In particular, it became clear that Dehn surgery gave a powerful method for calculating homological knot invariants like the first homology group of the infinite cyclic covering of a knot complement, from which the Alexander polynomial could be derived. This method was heavily exploited in Rolfsen's textbook [140]. In 1978, Robion Kirby was even able to describe an equivalence relation on surgery descriptions, generated by two simple "diagram moves", which corresponded to orientation-preserving homeomorphism between the 3-manifolds thus defined.[77]

The result of Wallace and Lickorish also heightened the interest in Bing's other question: for which knots besides the unknot and the trefoil knot could one show that no Dehn surgery would ever produce a counterexample to the Poincaré conjecture? In 1971, Bing and Martin summarized the results obtained thus far. If the following two propositions about a given knot K were true, the knot was said to have "property P": (1) if Dehn surgery

[74] Soon afterwards, Fox reminded the community of low-dimensional topologists that there was, besides handlebody decompositions and surgery on links, a third way of constructing simply connected 3-manifolds, namely that indicated by Tietze and Alexander, using coverings of the sphere branched over a suitable link. Fox's free calculus allowed to give algebraic conditions on the sheet permutations of the covering that implied its simple connectivity [48].

[75] In order to show that this idea worked, Lickorish established a basic theorem on self-homeomorphisms of closed, orientable surfaces: every such homeomorphism is isotopic to a sequence of elementary self-homeomorphisms called "Dehn twists". Moreover, Lickorish showed that Dehn twists in the splitting surface of a given 3-manifold can be produced by Dehn surgeries.

[76] It is not hard to see that $|l|$ and $|m|$ have to be relatively prime since the corresponding curve must be simple. Moreover, only the quotient of the signs is relevant to fix the relative orientation of the two tori involved in the surgery. The "rational" notation seems to be due to Rolfsen [140].

[77] See [86]. More information on the developments initiated by Dehn surgery on 3-manifolds may be found in the article by Cameron McA. Gordon in this volume.

on K leads to a simply connected manifold Φ, then Φ is homeomorphic to the 3-sphere; (2) any piecewise linear homeomorphism of $S^3 - J$, where J is a small tubular neighbourhood of K, into S^3 can be extended to a piecewise linear self-homeomorphism of S^3. The first condition meant that no counterexample to the Poincaré conjecture could be obtained by Dehn surgery on K, while the second meant that the homeomorphism type of the complement of K determined the knot (up to orientations). Due to Alexander's theorem on embedded tori in S^3, property P could be reformulated as follows: a knot K had property P if and only if, for all nontrivial surgeries (i.e. for $l \neq 0$), the manifold $\Phi_K(l, m)$ was not simply connected. Since a presentation of $\pi_1(\Phi_K(l, m))$ could be found, this reduced the question to combinatorial group theory. By rather tricky constructions of homomorphisms onto known non-trivial groups, Bing and Martin showed that several classes of knots had property P, including twist knots, doubled knots, and all product knots (in this last case, a more geometric argument was used).[78] At the end of their article, Bing and Martin pointed out that in many cases, like that of the trefoil knot, a properly *geometric* understanding had not yet been reached for the fact that no non-trivial Dehn surgery yielded a simply connected manifold. Later work by various authors changed this to some extent. For instance, David Gabai showed by an argument involving foliations of 3-manifolds that all torus and satellite knots possessed property P [56].[79] It remains unclear, however, whether or not *all* knots share the property.

After a long series of partial results obtained by various authors, and relying on certain techniques of Gabai for studying foliations, Cameron McA. Gordon and John Luecke finally showed that no non-trivial Dehn surgery on a knot yields S^3 [62]. While this did not resolve the problem of property P, it answered Tietze's long-standing question: the topological type of a knot complement does indeed determine the type of a knot (with or without orientations).[80] Therefore, Gordon's and Luecke's result implied that the second clause in Bing's and Martin's original definition of property P could be dropped, so that the truth of the Poincaré conjecture would imply that all knots have property P. On the other hand, if a single knot could be found such that some Dehn surgery on it yielded a simply-connected manifold, a counterexample to the Poincaré conjecture would have been found, too. Thus, property P is still considered by several mathematicians as one of the major open problems of knot theory.

§ 30. By the end of the seventies, a further development in 3-manifold theory came to a certain end which had fundamental implications for knot theory: the general classification problem of knots was recognized to be solvable by algorithmic means. Following the first undecidability results in mathematical logic in the 1930's, logicians raised the question as to whether certain topological problems, among them the classification of knots, might

[78] In the proof for twist knots, matrix representations of Coxeter groups were used for this purpose. These representations were generated by certain matrices in which complex square roots of the numbers $4\cos^2(\pi/n)$, $n = 3, 4, 5, \ldots$ occurred. After Jones showed that these numbers were just the possible discrete values of the index of subfactors of the hyperfinite II_1 factor (see below, § 32), a relation between subfactors and Coxeter groups was immediately recognized, see, e.g., [77, p. 104]. I am not aware, however, of work relating this to the problems studied by Bing and Martin.

[79] For torus knots, a purely group-theoretical proof of property P had already been included in the textbook of Burde and Zieschang [27, Section 15.6].

[80] If a knot complement $S^3 - K_1$ would be homeomorphic to another, $S^3 - K_2$, without a homeomorphism of pairs $(S^3, K_1) \to (S^3, K_2)$, there would be a non-trivial Dehn surgery on K_1 yielding S^3. A survey of further known properties of Dehn surgeries on knots can be found in [61].

be algorithmically unsolvable as well (Church [30]). The reason for posing such a question was that Reidemeister's purely combinatorial approach had given the knot problem a form very much resembling a kind of word (or transformation) problem in symbolic calculi. When, in the mid-fifties, P.S. Novikov and W.W. Boone independently showed that the general word problem in finitely presented groups was unsolvable, Markov soon thereafter pointed out that this implied the algorithmic unsolvability of the general classification problem of manifolds of dimension greater than three [103]. Due to these circumstances, the case of three dimensions, and knot classification, gained even more interest. Reidemeister, at least, was prepared to wager that the problem of deciding whether or not two given knots were equivalent was solvable (see [11, p. 97]).

Notwithstanding such hopes, undecidability results rather than decidability proofs were high on the agenda of mathematical logicians, and Boone even seems to have tried to prove that the knot problem was unsolvable. Therefore, when Wolfgang Haken, then an almost complete outsider in the community of topologists, announced a theory which allowed to decide algorithmically whether or not a given knot was isotopic to an unknotted circle, he could be sure both of attention and of a certain amount of scepticism about the correctness of his results. Haken, who first presented his ideas at the ICM in 1954, was asked to work out his ideas in full detail. This task took him several years, but in 1961, his long and technically demanding "Theorie der Normalflächen" was finally published in *Acta Mathematica*. In the same year, a somewhat simplified and more intuitive presentation of Haken's ideas was given by Schubert [147]. In his article, Haken described an algorithm which enabled one to construct, for a given compact, triangulated 3-manifold, a finite set of "normal surfaces", characteristic of the manifold's topology. In the case of a knot complement (bounded by a torus along the knot), the algorithm could be adapted to produce a Seifert surface of minimal genus. Thus, in principle, the genus of the knot was computable and in particular, it was decidable whether or not the knot was trivial. However, even today Haken's highly complicated algorithm remains beyond the requirements of practical computation. Haken's result found great appreciation among logicians, however, and a few weeks after his paper appeared, he was offered a position at Urbana, Illinois, where Boone was gathering a research group working on the decidability of mathematical problems.[81]

In 1962, Haken announced that he could modify his algorithm in such a way that it could be used to classify a large number of 3-manifolds, including all knot complements [65]. The basic idea was to employ the algorithm to find so-called "incompressible surfaces" in a given manifold M along which the manifold could be split into pieces.[82] Haken sought to determine a class $\mathfrak{K}$ of compact, orientable 3-manifolds for which the process of finding such splitting surfaces could be iterated, decomposing the manifold after finitely many steps into a collection of 3-balls. Moreover, the class of manifolds should be such that the algorithm allowed, for each of the pieces obtained at a given stage, only finitely

[81] The historical details of this paragraph have been taken from an interview with Haken, conducted by T. Dale in 1994, that has kindly been communicated to me by D. MacKenzie.

[82] The technical definition of an incompressible surface underwent several modifications throughout the following years. Intuitively speaking, an incompressible surface cannot be simplified within M by cutting open handles or by deleting 2-spheres that bound a 3-ball. In [167], the following definition was chosen: An incompressible surface F in a compact, orientable 3-manifold M is either a properly embedded, compact surface (i.e. $F \cap \partial M = \partial F$) or a component of ∂M, such that the following two conditions are satisfied: (1) there does not exist an embedded disk D in the interior of M, bounded by a curve $\partial D \subset F$ which is not contractible in F; (2) no component of F is a 2-sphere bounding a 3-ball in M. See also [66] for a readable description of his procedure.

many possibilities for the next splitting surface (the splitting process thus had the structure of a finite, rooted tree). For two manifolds in such a class $\mathfrak{K}$, it could then be decided if they were homeomorphic by comparing the finitely many splitting trees (called "hierarchies" in the technical literature). The manifolds were topologically equivalent if and only if two of them ran completely parallel. However, in order to apply the algorithm to a given manifold M at all, it had to be known beforehand that 2-sided, incompressible surfaces $F \subset M$ existed (with $\partial F \subset \partial M$, if F was bounded), and, moreover, that M was irreducible, i.e. that every $S^2 \subset M$ bounded a 3-ball (otherwise, the unproved Poincaré conjecture would have prevented recognizing the 3-balls at the end of a splitting hierarchy). Thus it was reasonable to conjecture, and Haken in fact claimed, that $\mathfrak{K}$ could be taken to include all manifolds satisfying these two conditions (following Thurston, such manifolds are usually called "Haken manifolds" today). Among them were all knot complements, so that the algorithm implied a decision procedure for the homeomorphism problem of knot complements.

Unfortunately, Haken did not spell out the proofs of all the claims he made in his paper, so that the scope of his results was not completely clear. An announced sequel to his article, which should have given the missing technical details, never appeared. Indeed, further research by Haken and Friedhelm Waldhausen made clear that Haken's original arguments required either an additional restriction on the class $\mathfrak{K}$ or an algorithmic solution of the conjugacy problem in the group of isotopy classes of self-homeomorphisms of a compact, bounded surface (with respect to isotopies fixing the boundary).[83] It took another decade before a co-worker of Waldhausen, Geoffrey Hemion, solved this additional problem and thus established Haken's original claim as correct [71]. In a widely read survey article, Waldhausen summarized the overall results of the development. While these results were of great importance for 3-manifold theory in general, they had particularly striking consequences for knot theory. By a slight modification, Haken's procedure would not only classify knot complements and knot groups, but actually knots themselves.[84] A further consequence of Waldhausen's own contributions to the subject was the proof of Fox's conjecture that two knots whose groups could be mapped by an isomorphism respecting peripheral subgroups had homeomorphic complements. In view of Gordon's and Luecke's theorem, this implies that the knots are equivalent. The result can be strengthened in the case of prime knots: up to orientation, these knots are determined by their groups.[85]

So far, it seems, Haken's unwieldy algorithm itself has been less useful in further research on knots and 3-manifolds than the general theorems drawn from it by Waldhausen and others. Haken's contribution must thus first and foremost be viewed as a decidability proof. Nevertheless, the results established by working out Haken's ideas changed the outlook on knot theory. The search for simpler classifying algorithms or complete knot invariants was shown to be a meaningful enterprise. What should be emphasized in the present context is the fact that genuine three-dimensional ideas guided this line of research, and

[83] See [167, 66, 168, § 4]. The difficulty arose from the possibility that during the decomposition process, a fibre bundle over S^1 could arise, fibred by incompressible surfaces, with incompressible boundary, and containing only incompressible surfaces isotopic to a fibre or a boundary component. In this case, the decomposition process would be blocked. Since such manifolds were known to be representable as mapping tori of self-homeomorphisms of their fibre, a solution to the above-mentioned problem was required.

[84] See [168, § 4]. In carrying out the splitting procedure, one had to keep track of a meridian of the knot considered.

[85] See [168, p. 26], [172].

highly specific geometric tools, such as the notion of an incompressible surface, were provided by it. Once again, it was not by means of diagram combinatorics that a deep insight was found, and once again, knot theory profited from its status as a specialty within the well-established field of 3-manifold theory.

§ 31. In the mid-seventies, the mathematical community was surprised by a revival of the connection between knots, 3-manifolds, and hyperbolic geometry which had already been touched upon by Dehn. In 1973, a Ph.D. student at Southampton, Robert Riley, found that the complement $S^3 - K_4$ of the figure eight knot K_4 (see Figure 1) had a hyperbolic structure, i.e. it admitted a complete Riemannian metric of constant sectional curvature -1.[86] In fact, Riley showed that $S^3 - K_4$ was homeomorphic to a quotient $\mathbb{H}^3/G$ of three-dimensional hyperbolic space $\mathbb{H}^3$ by a discrete group G of hyperbolic isometries, acting freely on $\mathbb{H}^3$ and isomorphic to $\pi_1(S^3 - K_4)$ [138]. The proof relied essentially on Waldhausen's theorems on Haken manifolds. Riley then went on to construct, with the help of a computer, similar examples of hyperbolic structures in certain other knot complements. He conjectured that the complements of all knots except torus and satellite knots could be endowed with such a structure.[87]

In 1977, Riley met William Thurston, who was then in the course of working out his general programme of finding geometric structures on 3-manifolds, an outline of which began to circulate in the form of notes of Thurston's lectures at Princeton about a year later [160]. Riley's results inspired Thurston to look systematically for hyperbolic structures in knot complements and related 3-manifolds. Among many other things, Thurston pointed out in his lecture notes that Riley's example was closely related to a hyperbolic manifold that Hugo Gieseking, a student of Max Dehn, had discussed in 1912. In his dissertation, Gieseking had constructed a manifold whose fundamental group contained an isomorphic copy of the group of the figure eight knot as a subgroup of index two.[88] This manifold was constructed from a regular tetrahedron in three-dimensional hyperbolic space, all of whose vertices were on the sphere at infinity. By identifying the sides of this tetrahedron two by two, Gieseking had obtained a non-compact manifold with a complete hyperbolic metric, and with finite hyperbolic volume. Thurston now showed that the natural conjecture, suggested by the structure of the fundamental group of Gieseking's manifold, was indeed true: $S^3 - K_4$ was the twofold orientable covering of Gieseking's example. In particular, $S^3 - K_4$ could be decomposed into two copies of the hyperbolic tetrahedron defining Gieseking's manifold.

Thurston went on to prove a general result on the existence of hyperbolic structures on certain compact, bounded 3-manifolds which implied that a knot complement $S^3 - K$ (or, equivalently, the interior of the compact, bounded manifold obtained by removing an open tubular neighbourhood of K from S^3) admitted a such a structure if and only if K was not a torus knot or a satellite knot, as Riley had conjectured. If K was a torus knot, then its complement could be given a different geometrical structure, while if K was a satellite of a non-trivial knot K', the question of endowing the knot complement with a geometric structure could be asked separately for the two (simpler) pieces obtained by splitting $S^3 - K$ along a torus, bounding a tubular neighbourhood of K' and containing K.

86 A metric is called complete if every geodesic may be extended indefinitely.

87 See [139, 161, pp. 360, 366ff].

88 See [59]. A description of Gieseking's example was also given in [99, pp. 153ff]. For the delicate question how much Gieseking or Dehn knew about the relation with K_4, see [101, pp. 39f.].

Thus, knot complements provided a striking illustration of Thurston's main conjecture that "the interior of every compact 3-manifold has a canonical decomposition into pieces which have a geometric structure" [161, Conjecture 1.1]. A proof of this conjecture for the class of compact Haken manifolds, together with several surprising applications, earned Thurston a Fields medal in 1982, despite the fact that full details of the proofs had not yet appeared in print.

A direct application of Thurston's results to knot theory was made possible by another fundamental result on hyperbolic 3-manifolds that had been proved in the early 1970's, the "rigidity theorem" of hyperbolic manifolds.[89] It stated that if two 3-manifolds M and N of dimension $\geqslant 3$ with a hyperbolic structure of finite volume have isomorphic fundamental groups, then M and N are not only homeomorphic but even isometric to each other. This implied that every *isometric* invariant of such a manifold, for instance the volume, was necessarily also a *topological* invariant. Since all hyperbolic knot complements had finite volume, the rigidity theorem provided a way to introduce a whole basket of new invariants for knots with hyperbolic complements. Many of these new invariants turned out to be calculable by means of computers. Jeffrey Weeks, a student of Thurston, was particularly successful in this respect. In his Ph.D. thesis, he described an algorithm for calculating various hyperbolic knot invariants that has since been very useful in extending knot tables to ever higher crossing numbers.[90] Already the volume of a knot turned out to be a rather fine (though not complete) invariant of knots with hyperbolic complements. It seems to measure a kind of geometric complexity of knots, but not much is known about this as yet. Thurston has conjectured that the complement of the figure eight knot K_4 might be the hyperbolic manifold with the least volume [161, p. 365].

Evidently, the rigidity theorem and the work of Riley and Thurston not only related knot theory to 3-dimensional topology in a deeper way but also led to a variety of more specifically geometric issues. For instance, representations of knot groups by discrete subgroups of $PSL(2, \mathbb{C})$ can be investigated, or the details of the hyperbolic structure of knot complements may be looked at. Here, too, Dehn surgery turns out to be a particularly helpful tool. It allows to construct new hyperbolic manifolds from given ones, and to address questions such as: which Dehn surgeries on a given knot do produce hyperbolic manifolds and which do not? The connection between knot theory and hyperbolic geometry has opened up a rich and still rather unsurveyable field of inquiry.

§ 32. Up to this point, the "geometry" involved in the investigation of knots and links was mainly that of three-dimensional manifolds associated with knots, be it in the sense of their topological structure or, as in the last paragraph, in the more specific sense of a Riemannian metric on the knot complement. In Vaughan Jones's discovery of a new knot polynomial, a completely different kind of geometry came into play: that of lattices of projections on a Hilbert space and the algebras generated by them. In order to explain why this may with reason be called a variety of *geometry*, a short look back to the beginnings of the field in which Jones was working is necessary. In the 1930's, John von Neumann and his collaborator Francis J. Murray embarked on a programme investigating what they called "rings of operators" on a separable Hilbert space (today called von Neumann algebras). In the course of this work, they invented a mathematical object that represented a close

[89] See [119] for a proof in the case of compact manifolds and [130] for the non-compact case.

[90] See [1]. Recently, tables of prime knots of up to 16 crossings have been constructed by Thistlethwaite, Hoste and Weeks, using a modification of Weeks's program.

infinite-dimensional analogy to complex projective space, although in an important respect it had very different properties. Since it was an investigation of the fine structure of this object which led to Jones's breakthrough, a more detailed account is necessary.[91]

Von Neumann's and Murray's work was motivated by earlier research on the spectral theory of linear operators and the wish to understand the mathematical foundations of quantum mechanics. It concentrated on so-called "factors" of operator rings, i.e. rings $\mathcal{M} \subseteq B(\mathcal{H})$ of bounded operators acting on a Hilbert space $\mathcal{H}$, closed under the adjoining operation * and under pointwise convergence on $\mathcal{H}$, containing the identity operator $\mathbf{I} \in B(\mathcal{H})$, and with a trivial center. Obvious examples of factors were the rings of *all* operators on a (separable) Hilbert space. Up to algebraic isomorphism, these factors were classified by the dimension of the underlying Hilbert space, i.e. they were all isomorphic to the rings $M_n(\mathbb{C})$ of all $(n \times n)$-matrices over the complex numbers or to the set $B(\mathcal{H})$ of all bounded linear operators on a separable, infinite-dimensional Hilbert space $\mathcal{H}$. However, the theory acquired depth by the fact that more, and different, examples could be constructed. In particular, Murray and von Neumann described a class of factors which in their view represented in many ways a better analogy to the finite-dimensional factors $M_n(\mathbb{C})$ than $B(\mathcal{H})$. Using a simplified construction method that Murray and von Neumann published in 1943, these factors can be defined as follows. For a finite or countably infinite group G, the Hilbert space $l^2(G)$, consisting of all square-summable sequences of complex numbers indexed by the elements of G may be formed. On this Hilbert space, G acts by its left regular representation U, given by

$$(U_g\xi)_h := \xi_{gh} \quad \text{for all } \xi \in l^2(G);\ g, h \in G.$$

Then the smallest closed subring $\mathcal{M} \subseteq B(l^2(G))$ containing all operators U_g is a von Neumann algebra, whose elements can all be represented in the form $\sum_{g\in G} \eta_g U_g$ for certain $\eta \in l^2(G)$. Murray and von Neumann pointed out that for finite G, the ring $\mathcal{M}$ was equivalent to Frobenius' "group numbers" (in today's language, the group ring $\mathbb{C}G$). In contrast, it was not too difficult to show that for countably infinite groups, $\mathcal{M}$ was a factor if and only if all conjugacy classes of G were infinite [122, § 5.3].

The rings constructed in this way had a very particular property, though. The function $\mathrm{tr}(\sum_{g\in G} \eta_g U_g) := \eta_e$, where e was the neutral element in G, defined a *finite trace*, i.e. a linear function $\mathrm{tr}: \mathcal{M} \to \mathbb{C}$, satisfying $\mathrm{tr}(\mathbf{I}) = 1$, $\mathrm{tr}(x^*x) \geqslant 0$, and $\mathrm{tr}(xy) = \mathrm{tr}(yx)$, for all $x, y \in \mathcal{M}$. This, in turn, made it possible to define a *dimension function* (relative to $\mathcal{M}$) on the lattice of all closed linear subspaces $E \subseteq l^2(G)$ of the form $E = p_E(l^2(G))$ for some orthogonal projection $p_E \in \mathcal{M}$, by putting

$$\dim_{\mathcal{M}}(E) := \mathrm{tr}(p_E).$$

For finite groups of order n, this function measured the dimension of a subspace of $\mathbb{C}G$, normalized in the sense that for a subspace E of dimension k, $\dim_{\mathcal{M}}(E) = k/n$. For the factors constructed from groups with infinite conjugacy classes, however, the range of this dimension function was the closed interval $[0, 1]$. In general, Murray and von Neumann showed that for all factors a similar dimension function could be constructed, with but few possibilities for the range of its values [121, § 8.4]. Corresponding to these possibilities,

[91] Based mainly on [121, 122]. Again, notations have been slightly modernized.

factors with a dimension function like the above (or, equivalently, infinite-dimensional factors with a finite trace) were called factors of type II_1.

Von Neumann realized that this construction came very close to a view of projective geometry that had been advocated by Karl Menger and Garrett Birkhoff a few years earlier. In 1928, Menger, like Reidemeister a mathematician with strong ties to the Vienna circle in philosophy, had proposed to reformulate projective geometry as the theory of linear subspaces of a finite-dimensional vector space [109]. Through the use of homogeneous coordinates, this idea had been implicit in many researches of 19th-century analytic geometers such as Felix Klein, but it was only under the influence of Hilbert's axiomatics that Menger proposed to shift the perspective on projective geometry and to make the properties of linear subspaces of a vector space the basis of the theory. Accordingly, he characterized these by a suitable system of axioms. Besides axioms governing the intersection and linear span of two subspaces, a crucial ingredient of Menger's approach was an axiom asserting the existence of a *dimension function*, associating with each subspace a positive integer that behaved correctly under intersection and linear span of subspaces. The value of this function then specified whether a given subspace corresponded to a point, or to a plane, etc. Seven years later, and independently of Menger's work, Garrett Birkhoff pointed out that the system of subspaces of a finite-dimensional vector space defining "a projective geometry" represented a particular kind of what he had come to call a *lattice* [16].[92] The lattice of projections of a factor of type II_1 satisfied virtually all of Menger's or Birkhoff's axioms except those securing that the structure defined was finite-dimensional. In this perspective, these factors represented a kind of infinite-dimensional complex projective space, or else, a geometry "without points", since no elements of least dimension existed. Von Neumann set out to show that one could indeed characterize the lattices of subspaces arising in the above way in an abstract fashion [123, 124]. For some time, he had great hopes that these "continuous geometries", as he decided to call them, provided the right framework to do infinite-dimensional projective geometry, and even quantum mechanics.[93] Accordingly, he devoted a significant effort to the further investigation of factors of type II_1.

In 1943, Murray and von Neumann were able to show that not all II_1-factors constructed as above were algebraically isomorphic, depending on the properties of the group G used in the construction. If G was the set theoretic union of an ascending sequence of finite groups, then, and only then, the associated factor M was "approximately finite" (or, in today's terminology, hyperfinite), i.e. generated by an ascending sequence of finite-dimensional algebras. Moreover, all such factors were algebraically isomorphic. In other words, up to isomorphism, there was just *one* of them, say, the factor $\mathfrak{R}$ constructed from the group Σ_∞ of permutations of the integers such that each $\sigma \in \Sigma_\infty$ permuted only finitely many integers. Since Σ_∞ was the union of the finite symmetric groups, $\Sigma_1 \subset \Sigma_2 \subset \Sigma_3 \subset \cdots$, the factor $\mathfrak{R}$ was indeed hyperfinite; the group rings $\mathbb{C}\Sigma_n$ could be taken as the approximating sequence of finite-dimensional algebras. If, on the other hand, G was taken to be a free group on two generators, then the associated factor was not hyperfinite [122, § 6.2]. Thus, the hyperfinite II_1 factor $\mathfrak{R}$ had acquired a rather singular position in the theory. It represented, so to speak, the closest infinite-dimensional analogue to the finite-dimensional factors $M_n(\mathbb{C})$; in other words, its lattice of projections represented the closest analogue to the lattice of subspaces of a finite-dimensional, complex vector space. Moreover, its construction showed that it had a rich but complicated inner structure.

92 See [107] for information on the origins of the theory of lattices.

93 See, e.g., the introduction to [121, 125].

For a long period after World War II, the attention of operator algebraists turned to more general issues, and the "continuous geometry" of $\mathfrak{R}$ moved in the background. A crowning achievement of much of this work was Alain Connes's completion of the classification of factors up to algebraic isomorphism, which earned him a Fields medal in 1982. Connes also took up the study of $\mathfrak{R}$ again by classifying its automorphisms.[94] Finally, the time seemed ripe to look at the inner structure of $\mathfrak{R}$ in more detail. It was Vaughan Jones who set himself the task of investigating *subfactors* of $\mathfrak{R}$, i.e. other infinite-dimensional factors $\mathcal{N}$ embedded in $\mathfrak{R}$. As such subfactors were automatically equipped with a finite trace, they also were of type II_1. In such a situation, i.e. given a pair of II_1 factors $\mathcal{N} \subseteq \mathcal{M}$ with the same unit, von Neumann's theory of dimension functions could be used to define an "index" $[\mathcal{M} : \mathcal{N}]$ which equalled the index of groups, $[G : H]$, if the factors $\mathcal{N}$ and $\mathcal{M}$ were constructed from groups $H \subseteq G$ as above. The suprising result found by Jones was that for II_1 subfactors of $\mathfrak{R}$, the possible values of this index did not consist of the interval $[1, \infty)$, as the definition of the index would have allowed, but only of the continuous interval $[4, \infty)$ and the discrete set $\{4\cos^2 \pi/n \mid n = 3, 4, 5, \ldots\}$ [76].

In the proof of his result, Jones calculated the index in a different way. If a pair of II_1 factors $\mathcal{N} \subseteq \mathcal{M}$ with the same unit was given, the inner product on $\mathcal{M}$ given by $(x, y) \mapsto \mathrm{tr}(y^*x)$ allowed for a completion of $\mathcal{M}$ to a Hilbert space, denoted by $L^2(\mathcal{M}, \mathrm{tr})$. On this Hilbert space, $\mathcal{M}$ acted by the left regular representation, given by left multiplication on the dense subspace $\mathcal{M}$. Similarly, $L^2(\mathcal{N}, \mathrm{tr})$ could be formed as a closed linear subspace of $L^2(\mathcal{M}, \mathrm{tr})$. Introducing the projection $e_{\mathcal{N}} : L^2(\mathcal{M}, \mathrm{tr}) \to L^2(\mathcal{N}, \mathrm{tr})$, Jones considered the von Neumann algebra $\mathcal{M}_1 \subset B(L^2(\mathcal{M}, \mathrm{tr}))$, generated by $\mathcal{M}$ and $e_{\mathcal{N}}$. It turned out that $\mathcal{M}_1$ was again a II_1 factor, with a trace extending the trace on $\mathcal{M}$, and such that $[\mathcal{M}_1 : \mathcal{M}] = [\mathcal{M} : \mathcal{N}] = \beta$, where $\beta^{-1} = \mathrm{tr}(e_{\mathcal{N}})$. By iterating this construction (which had already been studied by C. Skau and E. Christensen in the late 1970's), Jones was able to find the possible values of the index. Repeating the process by which $\mathcal{M}_1$ had been formed, Jones obtained both an infinite tower of II_1-factors $\mathcal{M}_i$ $(i = 1, 2, \ldots)$ and an infinite sequence of orthogonal projections $e_{i+1} : L^2(\mathcal{M}_i, \mathrm{tr}) \to L^2(\mathcal{M}_{i-1}, \mathrm{tr})$ (here, $\mathcal{M}_0 := \mathcal{M}$ and $e_1 := e_{\mathcal{N}}$). These orthogonal projections satisfied a remarkable set of relations:

$$e_i e_{i\pm 1} e_i = \beta^{-1} e_i, \qquad e_i e_j = e_j e_i \quad \text{for } |i - j| \geqslant 2;$$

moreover, for all words w in $\mathbf{I}, e_1, \ldots, e_{i-1}$, the relation

$$\mathrm{tr}(we_i) = \beta^{-1}\mathrm{tr}(w) \tag{$\star\star\star$}$$

held, and β was restricted to the set of values mentioned above. Thus, a necessary condition on the values of the index $[\mathcal{M} : \mathcal{N}]$ had been found. But more than that: Jones showed that whenever $\mathcal{P}$ was the von Neumann algebra generated by a system of orthogonal projections satisfying the above relations, then $\mathcal{P}$ was isomorphic to $\mathfrak{R}$ – it was approximated by the ascending sequence of the canonical images $\overline{\mathcal{A}}_{\beta,n} \subseteq \mathcal{P}$ of the abstract finite-dimensional algebras $\mathcal{A}_{\beta,n}$, generated by $\mathbf{I}, e_1, \ldots, e_n$ and satisfying the above relations –, and the double commutant of the set $\{e_2, e_3, \ldots\}$ in $\mathcal{P} = \mathfrak{R}$ was a II_1 subfactor with index β. Consequently, the condition also was sufficient.

[94] See [8] for a brief description of Connes's work.

The system of projections arising in this argument and the finite-dimensional algebras $\mathcal{A}_{\beta,n}$ soon turned out to form the core of a web of surprising relations to other mathematical topics. In fact, these algebras had been encountered by several other people in quite different fields. For instance, H. Temperley and Elliott H. Lieb had used a representation of $\mathcal{A}_{\beta,n}$ on $\mathbb{C}^{2n+2}$ in a study of certain models of statistical mechanics already in 1971. Moreover, for the discrete values of β, the algebras $\mathcal{A}_{\beta,n}$ were in some way or other related to Coxeter groups.[95] Finally, and most important, it was pointed out to Jones by D. Hatt and Pierre de la Harpe that the relations bore a strong resemblance to those defining the braid groups B_n. In fact, it was not difficult to show that after a change of variables,

$$g_i := te_i - (1 - e_i), \quad 2 + t + t^{-1} = \beta,$$

the algebras $\mathcal{A}_{\beta,n}$ were presented by the relations

$$g_i^2 = (t-1)g_i + t,$$

$$g_i g_{i+1} g_i = g_{i+1} g_i g_{i+1},$$

$$g_i g_j = g_j g_i \quad \text{for } |i - j| \geqslant 2,$$

$$g_i g_{i+1} g_i + g_i g_{i+1} + g_{i+1} g_i + g_i + g_{i+1} + 1 = 0.$$

Consequently, the mapping ρ sending a braid generator $\sigma_i \in B_n$ to the element $\rho(\sigma_i) := g_i \in \mathcal{A}_{\beta,n}$ defined a representation of the braid group B_{n+1} within $\mathcal{A}_{\beta,n}$ or, similarly, within $\overline{\mathcal{A}}_{\beta,n} \subseteq \mathfrak{R}$. In this way, a connection to topology was opened up which no one had expected. "For the first time", Jones remarked in a contribution to a conference in July 1983, "II_1 factors have begun to exhibit their geometric and combinatorial nature. This rich structure can only be expected to deepen as one answers further simple questions about subfactors of finite index" [78, p. 270]. It was not immediately clear, though, how to exploit this connection, as the same paper shows. For a short period, Jones hoped that the determinant of his family of representations of B_n could be used in a way similar to that in which the Burau representation had been used to get new information about the Alexander polynomial or perhaps a related invariant of links.[96] To discuss this question, Jones turned to an expert in braid groups, Joan Birman. In her earlier book [17], written shortly after Garside had solved the conjugacy problem for the braid group, Birman had collected and refined the available knowledge for the study of knots and links via an analysis of the relation between links and closed braids. She was therefore a natural partner for discussing Jones's new ideas. In the discussions, however, Birman pointed out to Jones that his first idea would not work out.[97] In a popular article, Jones later recalled:

> I went home somewhat depressed after a long day of discussions with Birman. It did not seem that my ideas were at all relevant to the Alexander polynomial or to anything else in knot theory. But one night the following week I found myself sitting up in bed and running off to do a few calculations. Success came with a much simpler approach than the one I had been trying. I realized I had generated a polynomial invariant of knots.[98]

[95] See footnote 78 above.

[96] See above, § 24, and [78, p. 244].

[97] See note (2), added in proof, in [78, pp. 244 and 273].

[98] From [81].

It is not difficult to tell what Jones had found. Among other things, Birman had explained to Jones Markov's equivalence relation, characterizing the braid words that represented isotopic knots or links as a closed braid (see above, § 24). Jones realized that the traces $\mathrm{tr} : \mathcal{A}_{\beta,n} \to \mathbb{C}$ furnished by his theory of subfactors automatically satisfied

$$\mathrm{tr}\big(vwv^{-1}\big) = \mathrm{tr}(w) \quad \text{and} \quad \mathrm{tr}\big(wg_{i+1}^{\pm 1}\big) = \beta^{\mp 1}\mathrm{tr}(w),$$

for all words v, w in the generators $g_1, g_2, \ldots, g_i$, since two arguments of the trace could be interchanged and since it satisfied property (⋆⋆⋆). Thus, only a slight correction was needed in order to make the trace itself into an invariant of links. Indeed, Jones showed that if $w \in B_n$ was a word with exponent sum e in the braid group generators σ_i, representing an oriented link L, then

$$V_L(t) := \left(-\frac{t+1}{\sqrt{t}}\right)^{n-1} t^{e/2}\,\mathrm{tr}\big(\rho(w)\big)$$

was invariant under the moves generating Markov's equivalence relation and thus $V_L(t)$ was an isotopy invariant of oriented links. Moreover, the finite-dimensional algebra involved showed that V_L was a Laurent polynomial in the variable t for knots and links with an odd number of components, while it was a Laurent polynomial in $\sqrt{t}$ for links with an even number of components. Further discussions with Birman brought the next surprise. Examples showed that the new invariant was *not* equivalent to the Alexander polynomial. However, Birman and Jones found that V_L satisfied a skein relation similar to the Alexander polynomial, as described in § 11 above.[99] This meant that Jones's polynomial could be defined independently of its original context in von Neumann algebras, a fact heavily exploited in subsequent work.

Let me reconsider the remarkable chain of arguments leading from von Neumann's construction of the hyperfinite II_1 factor $\mathfrak{R}$ to Jones's new link invariant. While completely independent of low-dimensional topology, the beginnings of this development were clearly motivated by the wish to understand a particular kind of infinite-dimensional geometry, extending the approach to projective geometry by Menger and Birkhoff. Moreover, these beginnings were related to von Neumann's attempt to clarify the mathematical basis of quantum mechanics. Jones took up the problem of subfactors of $\mathfrak{R}$, continuing this investigation along lines close to those indicated by Murray's and von Neumann's work. When Jones found his towers of finite-dimensional algebras (the canonical images of $\mathcal{A}_{\beta,n}$ inside $\mathfrak{R}$), he was inclined to think of them in terms of von Neumann's variety of geometry: "The situation is thus very geometric and [the] relations [defining $\mathcal{A}_{\beta,n}$] can be thought of as defining special configurations of subspaces" [79, p. 377]. From this point of view, however, the outcome of Jones's research generated perhaps even more riddles than it solved. By arguments which in the end boiled down to exploiting a surprising similarity in the combinatorial structures of $\mathfrak{R}$ and the braid groups, a relation between the geometry of configurations of subspaces of a Hilbert space and the topology of low-dimensional objects such as braids and links was established. But what was – apart from this combinatorial resemblance – the geometric reason for this connection? Was there a kind of structure

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99 Interview with Joan Birman, Oberwolfach 1995. For this interview and further private communications about her involvement in the invention of the new polynomial invariants, I wish to express my sincere thanks to Joan Birman.

which bridged the algebra and the topology in question, in a similar way than the homology of cyclic coverings related knots and links to the Alexander polynomial? In the years following Jones's breakthrough, such questions were asked repeatedly. In a contribution on statistical mechanical models of link invariants, published in 1989, Jones himself conceded that the riddle was still unsolved: "Our main reason for doing this work was as a step towards a useful and genuinely three-dimensional understanding of the invariants. So far we have not succeeded. The situation is the same as that of the poor prisoners in Plato's allegory of the cave" [80, p. 312].[100]

While Jones's new invariant has been used and generalized by many people in a broad spectrum of directions (such as: further polynomial link invariants, statistical mechanical models, quantum field theory, invariants of 3-manifolds constructed on the basis of Kirby's calculus of surgery descriptions), it seems that a deeper understanding of the relations between the two kinds of geometry involved is still lacking. However, an important new idea which might eventually change the situation came into play through work of V.A. Vassiliev [164, 165]. Following a general approach outlined by V.I. Arnold, Vassiliev proposed to study the space $\mathcal{V}$ of all smooth mappings $S^1 \to S^3$. In this space, the isotopy classes of knots are separated by a system Σ of "walls" representing *singular* maps, and thus the homology of $\mathcal{V} - \Sigma$ in dimension zero, which can be studied by means of a spectral sequence, characterizes all numerical knot invariants. After Birman and X.-S. Lin found a connection between Jones's and Vassiliev's ideas in fall 1990, a substantial amount of research was done on this connection which might provide the starting point for a better understanding of the topology underlying the new link polynomials.[101]

6. Conclusion

§ 33. From the account given in the previous three sections it will be clear that a "tale of diagram combinatorics" such as that told in Section 2 reduces the complex weave of scientific and mathematical practice in which knot theory was formed to a rather thin narrative, in which the intentional and causal aspects of the development become almost unrecognizable. This can already be seen from the periodization which is suggested by the developments discussed. Four major stages of the history of knot theory can be discerned. In the first stage, extending from Vandermonde's first remarks to the late 19th-century tabulations, the mathematization of the knot problem stood in the foreground. This mathematization was called for by various developments in the exact sciences, ranging from astronomy and the theory of electromagnetism to Thomson's speculations on the structure of matter. In the second period, from 1900 to the late 1930's, modern knot theory emerged as a subfield of the discipline of topology, culminating in Alexander's, Reidemeister's and Seifert's contributions. On the one hand, we have seen that this emergence of modern knot theory was motivated by the desire to understand singularities of algebraic curves and surfaces – a topic deeply rooted in 19th-century pure mathematics – and to solve several major problems thrown up by Poincaré's new *Analysis situs*. On the other hand, the formation of knot theory was influenced by the modernist impulse toward autonomous, formal theories, an impulse which found its clearest expression within the developments considered here in Reidemeister's *Knotentheorie* of 1932. The third period, extending roughly from 1945 to

[100] For statements in a similar spirit, see [18].

[101] See the survey of this development in [18].

Jones's invention of a new knot polynomial, is characterized by the close interplay between knot theory and the growing field of low-dimensional topology. The various ways in which knots gave rise to 3-manifolds were explored in detail, and the surprising resistance of the three-dimensional Poincaré conjecture only contributed to motivate topologists to clarify the structure of knot complements, manifolds obtained from those by Dehn surgery, and 3-manifolds in general. The fourth period set on with Jones's discovery, a breakthrough which remains surprising even today, and which changed the structure of the field very deeply. Knot theory is no longer more or less exclusively tied to low-dimensional topology, but also to a variety of other fields among which mathematical physics certainly stands out.

A look at the intellectual contexts which I have touched upon (restricting mainly to the mathematical ideas involved) allows us to recognize that the actual motivations for mathematical investigations of knots and links were very complex. In a more or less direct way, and like so many other fields of mathematics, research on knots was related to the small sets of highly appreciated and contested research themes, the "big issues" that occupied the attention of the scientists of a given period. What is the nature of the small planets, and what are their orbits? What is an atom, and how are observed spectra to be explained? How do algebraic curves or surfaces behave at singular points? What are the objects of the new science of topology and how can ordinary space be characterized in purely topological terms?[102] What is the right mathematical framework to be used in quantum physics? Which mathematical problems are solvable by algorithmic means? The appreciation of such problems, and even more of the candidates for their solutions, has continually changed and will often be found not to coincide with today's valuations. William Thomson's theory of vortex atoms which inspired Tait's tabulation enterprise did not sustain its original attraction for long. Nevertheless, it was in relation to such larger themes that the knot problem has continued to occupy the attention of mathematical minds. In their day, and for a shorter or longer period, they represented hard and deep problems in rich intellectual constellations; constellations which reached far beyond the narrow focus of a particular piece of knot-theoretical work. Moreover, a study of the temporal modifications of the interplay between the grand scientific themes and more concrete research allows us to gain a deeper insight into the historical changes influencing the development of mathematics. It is significant that after 1900 the interest in knots no longer arose from physics but from pure mathematics, and that in the wake of Jones's work, mathematical physics again came to play a major role in motivating research on knots and related topics. I have also indicated in which way the move toward a combinatorial style of "modern" knot theory (or "classical", depending on the perspective) was at least partially inspired by philosophical debates on the foundations of mathematics. To spell out all these influences and interrelations in the details of mathematical, scientific, and cultural practice would mean to produce yet another, and still much "thicker" historical narrative on the formation of knot theory than the one I have presented here.[103]

Returning to the proper subject of this contribution, let me close by recalling the truism that it is not the historian's task to predict the future. However, it is less a prediction than a reasonable expectation to suppose that geometric aspects will continue to play a crucial role in the further development of knot theory. After all, the hierarchy of knots in ordinary

[102] A more detailed analysis would show that around 1900, the second half of this theme was not without cosmological overtones.

[103] For the interesting notion of "thick narratives", see [58].

space or of similar placements in manifolds will continue to remain, first and foremost, a hierarchy of *geometrical complexity* of a certain kind. This hierarchy remains only very partially understood. The bare fact that it is possible in principle to enumerate all types of knots and links in that hierarchy does not tell too much about its finer structure (the comparison has been made with a listing of all prime numbers and a deeper understanding of number theory).[104] Thus, knot theory will continue to be interesting and useful in all situations within and outside mathematics where this kind of geometrical complexity is involved. If the history of knot theory tells us anything, it is that this has always been the case.

Bibliography

The following abbreviations have been used: Abhandlungen MSHU = Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universität; AMS = American Mathematical Society; DMV = Deutsche Mathematiker-Vereinigung, LMS = London Mathematical Society; NAS = National Academy of Sciences USA; RSE = Royal Society of Edinburgh.

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[104] See, e.g., [159].

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