

## Branch Points of Algebraic Functions and the Beginnings of Modern Knot Theory<sup>1</sup>

MORITZ EPPLE

*AG Geschichte der Mathematik, Fachbereich 17—Mathematik, Universität Mainz,  
D-55099 Mainz, Germany*

Many of the key ideas which formed modern topology grew out of “normal research” in one of the mainstream fields of 19th-century mathematical thinking, the theory of complex algebraic functions. These ideas were eventually divorced from their original context. The present study discusses an example illustrating this process. During the years 1895–1905, the Austrian mathematician, Wilhelm Wirtinger, tried to generalize Felix Klein’s view of algebraic functions to the case of several variables. An investigation of the monodromy behavior of such functions in the neighborhood of singular points led to the first computation of a knot group. Modern knot theory was then formed after a shift in mathematical perspective took place regarding the types of problems investigated by Wirtinger, resulting in an elimination of the context of algebraic functions. This shift, clearly visible in Max Dehn’s pioneering work on knot theory, was related to a deeper change in the normative horizon of mathematical practice which brought about mathematical modernity. © 1996 Academic Press, Inc.

Viele der Ideen, die die moderne Topologie geprägt haben, stammen aus ‘normaler Forschung’ in einer der Hauptströmungen des mathematischen Denkens des 19. Jahrhunderts, der Theorie komplexer algebraischer Funktionen, und wurden erst allmählich von ihrem ursprünglichen Kontext abgelöst. Ein Beispiel dieser Entwicklung wird diskutiert. In den Jahren 1895–1905 versuchte der österreichische Mathematiker Wilhelm Wirtinger, Felix Kleins Sicht algebraischer Funktionen auf den Fall mehrerer Variabler zu verallgemeinern. Eine Untersuchung des Monodromieverhaltens solcher Funktionen in der Nähe singularer Punkte führte zur ersten Berechnung einer Knotengruppe. Die moderne Knotentheorie entstand dann nach einer Verschiebung der mathematischen Perspektive auf die von Wirtinger untersuchten Fragen, die zu einer Elimination des Kontexts algebraischer Funktionen führte. Diese Verschiebung, die in Max Dehns knotentheoretischen Pionierarbeiten deutlich sichtbar ist, war mit jener tieferen Veränderung im normativen Horizont der mathematischen Praxis verknüpft, die die mathematische Moderne hervorbrachte. © 1996 Academic Press, Inc.

Beaucoup des idées centrales qui ont formées la topologie moderne ont leurs racines dans la ‘recherche normale’ dans un des courants les plus importants de la pensée mathématique du 19ème siècle. Ces idées ont alors été séparées graduellement de leur contexte originale. Un exemple de ce développement sera discuté. Pendant les années 1895–1905, le mathématicien autrichien, Wilhelm Wirtinger, essayait de généraliser le point de vue de Felix Klein sur les fonctions algébriques complexes dans le cas de plusieurs variables. Une investigation de la monodromie de telles fonctions lui amenait à la première calcul d’un groupe d’un nœud. La théorie des nœuds moderne a alors été formée après un changement de la perspective

---

<sup>1</sup> Earlier versions of this paper have been presented at the Mathematical Colloquium at Heidelberg and the History of Mathematics Meeting in Oberwolfach, April 1994. I have much profited from interesting discussions on both occasions. The paper is part of a larger research project on the history of knot theory.

mathématique sur les problèmes étudiés par Wirtinger. Ce changement, visible distinctement dans le travail de Max Dehn sur la théorie des nœuds, a abouti en une élimination du contexte des fonctions algébriques. Il était relié à un changement plus fondamental de l'horizon normatif de la pratique mathématique qui amenait à ce qui a été appelé la modernité mathématique.

© 1995 Academic Press, Inc.

MSC 1991 subject classifications: 01A55, 01A60, 01A80, 32-03, 57-03.

KEY WORDS: discipline formation, rationality, modernity, knots, algebraic functions.

## INTRODUCTION

Due to the rapid development and application of new knot invariants in mathematics and physics following Vaughan Jones' discovery of a new knot polynomial, knot theory has received growing attention within and even outside the mathematical community.<sup>2</sup> In this context, it has often been asked why the knot problem—of all topological problems—was among the first to be studied by early topologists of our century such as Heinrich Tietze, Max Dehn, James W. Alexander, and Kurt Reidemeister. This question appears all the more puzzling since 19-century work on knots had certainly not been at the cutting edge of mainstream mathematical research—unlike, for instance, the topological problems that arose in connection with the theory of algebraic functions or algebraic geometry. In the following, an answer to this question will be given. Using hitherto unpublished correspondence between the Austrian mathematician, Wilhelm Wirtinger, and Felix Klein, it will be shown that modern knot theory *did* in fact originate from these latter fields. Furthermore, while they were familiar to the pioneers of modern topology, a series of events gradually left these roots forgotten by their followers.

In considering the origins of modern knot theory, I will do more than merely retrace the technical developments. Rather, this formation of a new field of mathematical research illustrates a certain pattern which, in a nutshell, may be characterized as follows:

*Thesis.* What appears, at first sight, to be the invention of a new mathematical discipline, turns out, on closer inspection, to be the outcome of a rather complex process of *differentiation*, and, as I would like to call it, a subsequent *elimination of contexts*.

Here the term “differentiation” is taken from the Weberian tradition in sociology. As is well known, Max Weber has described the formation of modern culture and society as a process of progressive differentiation of cultural “value spheres” and domains of social action, the most important of which are science (which Weber links with technology and industrial production), ethics and religion (linked with the institutions of law), and art.<sup>3</sup> This picture is interesting for the history of science

<sup>2</sup> This breakthrough was first announced in [14]. Since then, a wealth of popular and scientific presentations of knot theory, old and new, have been streaming into the market. Contributors come from all ranks of the scientific hierarchy, including authorities such as Michael Atiyah [37]. Some articles have included historical comments, for instance the nice survey by de la Harpe [47]. Przytycki [55] has given a presentation of some of the combinatorial ideas which led to polynomial knot invariants. The reader should be aware that most of these treatments are not intended to be serious historical studies.

<sup>3</sup> The *locus classicus* for this view is the “Zwischenbetrachtung” in Vol. 1 of [61].

because of Weber's idea of viewing the formation of modernity from the perspective of a history of *rationality*. According to this view, a specific standard of rationality is associated to each of the different "value spheres" which organizes social practice in these respective domains. For Weber, the history of modernity is to a large extent the history of the evolution of these rationality standards.<sup>4</sup> Robert Merton has applied this perspective to an intermediate stage in the process of differentiation of science and religion in his ground-breaking study [53]. In contrast to Weber's and Merton's macrosociological approach, which treats science mostly from an external perspective, the idea of "differentiation" will be used on a microscopic, internal level in the context of the following study. This term will denote the gradual separation of a certain bundle of problems—problems appearing in a well-established field of what Kuhn called normal research—from the mainstream of that field. As we shall see, even on this microscopic level a gradual separation of different standards of rationality is characteristic for such a process of differentiation.

An "elimination of contexts," on the other hand, marks a critical step in mathematical (or, more generally, scientific) research. It puts, so to speak, a previously differentiated complex of problems onto its own feet. As a conscious or unconscious effect of active decisions taken by scientists, it leads to or completes a modification of the network of scientific disciplines. Typically, the decision to accept a new standard of rationality is central in an elimination of contexts. A change of such standards implies a reevaluation and reorganization of the manifold elements of scientific practice, including the perceived architecture of the body of scientific knowledge. On the macroscopic level, an example of this elimination of contexts is the gradual suppression of religious elements in science. We shall see that similar phenomena may be observed on the internal level of mathematical research.

Using these ideas from a history of rationality,<sup>5</sup> we shall be able to trace the influence of the norms guiding the mathematical community not only in the way in which mathematical research is embedded into general scientific and social culture, but also in the regulation of choices determining the constitution of the body of mathematical knowledge itself. In particular, it turns out that we can perceive in the early history of modern knot theory reflections of the broad changes in mathematical culture around the turn of the century. This leads to another aspect which will be central in the following.

The historical narrative to be presented is drawn from the history of topology, that is, from the history of one of those mathematical disciplines which must be called genuinely *modern*—if such a thing exists at all. The events in question

<sup>4</sup> A modern presentation of Weber's theory of modernity along these lines is contained in Jürgen Habermas' influential [46] see in particular Chap. II.

<sup>5</sup> An important difference between a history of rationality on Weberian lines and attempts to give a "rational reconstruction" of the history of science in the spirit of Lakatos [49] should be pointed out: Whereas the latter *import* the relevant standards of rationality from a particular methodology of science, the former considers these standards as *historical data*, to be traced and interpreted by the historian.

occurred during the two decades before and after the turn of the century. They thus overlap in time with Poincaré's writings on *Analysis situs*, which mark the disciplinary threshold of topology. Moreover, they are contemporary with the onset of what Herbert Mehrrens and other writers have called "mathematical modernity," marked by events such as the publication of Hilbert's *Grundlagen der Geometrie* in 1899 and his famous talk on open mathematical problems delivered at Paris in 1900. (We shall see that both events had an influence on the story to be told.) While these connections are striking, the present case study should not be understood as a general theory about the pattern of differentiation and elimination of contexts in the history of mathematics. Still, this pattern might be typical for the creation of some *modern* mathematical theories.

### *Prelude: Poincaré's Fundamental Group*

Let me first give a brief illustration of this pattern. It concerns one of the basic notions of topology, the "fundamental group" of a manifold. As is well known, Poincaré introduced the fundamental group in his paper on *Analysis situs* of 1895 [24] in the context of a discussion of the monodromy behavior of multivalued functions on a manifold.<sup>6</sup> In fact, he gave a *motivation* for his notion in terms of monodromy and then a *definition* in terms of homotopy classes of paths. Instead of simply defining his new notion, he explained the action of closed paths on the set of values of a certain class of multivalued functions at a given point in the manifold. (In more modern terms, he considered the action of the fundamental group on the fiber of the covering associated with a given set of multivalued functions. In fact, the only condition which Poincaré required for the function set implies that this covering is unbranched, so that there are no exceptional fibers.) He then remarked that the resulting group of permutations of these values (which we may call the *global monodromy group* associated with the given class of functions) is always a homomorphic image of the group of path classes, which therefore is rightly considered "fundamental," at least from the point of view of monodromy considerations.

Poincaré's text documents the last step in a process of differentiation. Investigations of the monodromy behaviour of multivalued functions in the neighborhood of singular points had been normal research problems in analytic (or algebraic) function theory on surfaces since the time of Puiseux and Riemann. The term "monodromy group" was coined by Camille Jordan in his *Traité des substitutions et des équations algébriques* of 1870 [15]. The idea of the group had already been implicit in Victor Puiseux's *Recherches sur les fonctions algébriques* of 1850 [27]. Therein, Puiseux had examined the permutations of the roots of a polynomial equation with rational functions as coefficients induced by analytic continuation along small loops around branch points. One year later, Charles Hermite identified

<sup>6</sup> A historical discussion of the developments leading to Poincaré's notion was given by vanden Eynde [45].

this set explicitly with the Galois group of the defining equation [11].<sup>7</sup> The concept of monodromy soon found important applications, for instance in the context of differential equations. In 1895, Poincaré was on the verge of separating a particular complex of questions from this context by introducing the new notion of the fundamental group. He was well aware that it was precisely the *context* which mattered to him and his contemporary mathematicians; this functioned as a source of legitimation for the study of the topological questions involved. Therefore, in the announcement of his work on *Analysis situs* [23], which was intended to convince readers of the importance of the new field for topics like analytic functions or algebraic geometry, Poincaré restricted himself to explaining the fundamental group in terms of the monodromy behavior of multivalued functions on a surface. There, it is simply the “maximal” global monodromy group (belonging to “the most general” set of multivalued functions, associated with what was not yet called the universal covering of a surface). The group of path classes was not even mentioned.<sup>8</sup>

At the same time, Poincaré’s notion opened up the possibility of *eliminating* the context of monodromy considerations. Already the text of [24] allows one to isolate conceptually the notion of the fundamental group from its action on multivalued functions. And this is exactly what happened later on.<sup>9</sup> The central step which led to this elimination was when Poincaré decided to redefine a certain class of manifolds in a purely combinatorial way, a step which he took, motivated by Poul Heegaard’s criticisms, in the first *Complément à l’Analysis situs* of 1899 [26]. Here, we find the first hints at a new standard of rationality for topological argumentation. It was established after the turn of the century by those mathematicians who advocated an axiomatic, purely combinatorial approach to topology. While Poincaré’s first *Complément à l’Analysis situs* did not mention the fundamental group, it was clear that a combinatorial notion of manifolds offered new possibilities for viewing the fundamental group, too. It was Heinrich Tietze who took this step in his *Habilitationschrift* of 1908, by reducing all then-known topological invariants of three-dimensional manifolds to the fundamental group.<sup>10</sup> This group was now introduced and investigated by means of a group presentation associated with a given combinatorial complex. A combinatorial notion of homeomorphism was introduced which enabled Tietze to show the invariance of the fundamental group by means of combinatorial group theory. No mention was made of algebraic or analytic functions

<sup>7</sup> See [63, 118 f.]. It is a pity that Hermite’s short paper escaped vanden Eynde’s notice in [45] since it shows, in fact, that group-theoretic thinking *was* in the air, at least immediately following Puiseux’s work. However, it was not the fundamental group, but the monodromy group which was studied then. It seems to be a general shortcoming of vanden Eynde’s article that she underestimates the role of monodromy considerations in the developments leading to the notion of the fundamental group.

<sup>8</sup> The notion of equivalence classes of paths was familiar to Poincaré by 1883 from his work on the uniformization problem. This problem represents another important source of topological notions in the context of complex function theory. See [45].

<sup>9</sup> A thorough discussion of the relations between the fundamental group and coverings of a given manifold was taken up by Reidemeister [28] and Seifert and Threlfall [30].

<sup>10</sup> [32]; see [38] for a survey of the combinatorial parts of Tietze’s paper.

and their monodromy behavior. (See [38, 160–162].) Only *after* this elimination of the specific motivating context did the notion of the fundamental group of a manifold acquire its broad significance in topological research. In particular, mathematicians interested in the possibilities of the new discipline, such as Tietze and Dehn, could now use the notion without necessarily knowing a good deal of function theory. New problems could be posed and treated which did not presuppose a connection with algebraic functions or algebraic geometry, a famous example being Poincaré’s conjecture regarding the 3-sphere.<sup>11</sup>

This early history of the notion of the fundamental group discloses more than it may seem to at first glance. We shall see that the first steps of modern knot theory were the outcome of a line of thought which is astonishingly close to that just mentioned. Again, investigations of the monodromy behavior of algebraic functions—in this case, of two complex variables—led to a topological notion, which, after an elimination of the original context and a change in thought style, became known as the group of a knot. Seen from a purely mathematical perspective, this may come as no great surprise, since knot groups are special cases of fundamental groups. Seen historically, however, the parallel is rather instructive, since in the beginning at least, the two lines of thought evolved independently. This underlines the relevance of the specific *pattern* of transformation, as well as the importance of late 19th-century research on algebraic functions for the birth of topology.

## DIFFERENTIATION

### *The First Result of Modern Knot Theory: The Impossibility of Disentangling the Trefoil Knot*

Let us begin with an explanation of what “modern knot theory” shall be taken to mean in the following. The problem of classifying knots (or rather, plane knot diagrams) apparently already puzzled Gauss back in the 1820’s (see, e.g., [55]). We also possess a remarkable letter from Betti, who reported on conversations with Riemann that document the importance which Gauss attached to the knot problem in his later days. (Betti to Tardy, 6.10.1863. See [62].) In fact, Gauss regarded this as one of the paradigmatic problems of *Analysis situs*. The next main episode in the history of knot theory was the beginning of knot tabulations by Peter Guthrie Tait and his followers, working in the Scottish context of Lord Kelvin’s speculations about a theory of vortex atoms.<sup>12</sup> (In passing, note that again it was the *context*

<sup>11</sup> For information on the early history of this conjecture, see [60].

<sup>12</sup> From a mathematical point of view, 19th-century knot tabulations have been discussed in detail by Thistlethwaite [59]. On Thomson’s speculations, a standard reference is [58]. So far, no detailed treatment of the connections between this proposal of an atomic model and Tait’s tabulations has been given. A rather interesting line of topological thought links the Scottish physicists to Riemann’s ideas about connectivity. In particular, Thomson’s papers contain, though in a rather vague way, the claim that the first Betti number of knot complements equals one in all cases. I plan to investigate these ideas on another occasion.

that served to legitimize non-normal research.) However, the first serious published proofs of results on knots date from the beginning of our century. They were contained in the above-mentioned article by Tietze and in a series of pioneering papers by Max Dehn that appeared in the years 1910–1914. Soon thereafter, knot theory attracted several talented young mathematicians. After the interruption caused by World War I, Otto Schreier, Kurt Reidemeister, Emil Artin, and James Wadell Alexander were the first to take up the knot problem. It became the subject of journal articles—most of which were published in a new journal with a modern outlook, the *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität*, founded in 1922. Ten years later, the first monograph appeared: Reidemeister's *Knotentheorie* [29]. It was during this period that “modern knot theory”—that is, knot theory as part of mathematical modernity—was formed. (In this study, the problem of whether or when the “modern” period of knot theory ended will be left open.)

In 1908, Heinrich Tietze published the first result of modern knot theory. In a section of his *Habilitationsschrift* which contains a discussion of 3-manifolds embedded in ordinary 3-space (called “developpable dreidimensionale Mannigfaltigkeiten” by Tietze), he presented an argument for the impossibility of disentangling a trefoil knot. He began by mentioning that the fundamental group of a solid torus (embedded in 3-space) is the infinite cyclic group. A homeomorphic manifold is given, he continued, by boring a cylindrical channel out of a solid ball. “If one instead would bore a knotted channel out of the ball as in fig. 3, then the fundamental group of the resulting manifold would be generated by two operations satisfying the relation  $sts = tst$  so that this manifold cannot be homeomorphic with that first mentioned.”<sup>13</sup> (See Fig. 1.)

Tietze gave his result without proof, in fact without even giving a hint at the method used to compute the group. He merely mentioned an earlier note stating this result in a local Viennese journal [31]. A closer reading of his *Habilitationsschrift*, however, indicates where the result came from, namely, from another mathematician who was working in a completely different context. Tietze buried this information in a section far removed from the passage cited above, and without calling attention to the connection between the two passages. Later readers, such as Dehn, who were interested mainly in combinatorial topology, appear to have hardly read

<sup>13</sup> “Greifen wir etwa die von einer einzigen Fläche vom Geschlechte 1 berandeten developpablen Mannigfaltigkeiten heraus. Das einfachste Beispiel einer solchen Mannigfaltigkeit stellt der von einer Torusfläche berandete Teil des  $\mathbb{R}_3$  vor. Die Fundamentalgruppe dieser Mannigfaltigkeit ist die aus einer Operation, für die keine definierende Relation besteht, erzeugte zyklische Gruppe unendlich hoher Ordnung. Eine dieser Mannigfaltigkeit homöomorphe erhält man, wenn man aus einer Kugel einen zylindrischen Kanal ausbohrt. Würde man statt dessen einen verknoteten Kanal wie in Fig. 3 aus der Kugel ausbohren, so wäre die Fundamentalgruppe der so entstandenen Mannigfaltigkeit aus zwei erzeugenden Operationen mit der Relation  $sts = tst$  aufgebaut, so daß diese Mannigfaltigkeit mit der erstgenannten nicht homöomorph sein kann.” [32, 81]

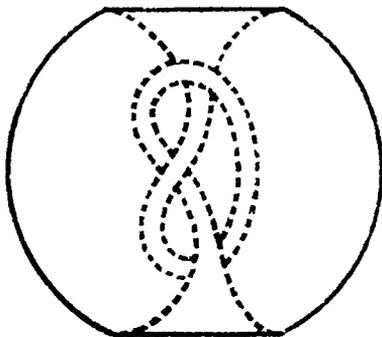


FIG 1. Tietze's Fig. 3.

this section at all.<sup>14</sup> It treats what Tietze (following Heegaard [10]) called “Riemann spaces,” that is, three-dimensional analogues of Riemann surfaces [32, Sect. 18]. There we find the name of Wilhelm Wirtinger, Tietze’s older colleague, who actually had induced him to turn to topology for his *Habilitation*. In an autobiography, Tietze wrote in 1960 about his early career:

[After finishing my dissertation] a strong impression was made on me by Wirtinger who had come from Innsbruck to Vienna and who, when speaking of algebraic functions and their integrals in lectures and seminars, pointed out that topological elements lie at the basis of this theory.<sup>15</sup>

Moreover, in Tietze’s Sect. 18, we find not only the missing technique but also the missing context. Just one single sentence refers to it: the “investigation of the function of two complex variables represented by Cardan’s formula” [32, 105].

Tietze’s text represents the critical step in a striking example of “context-elimination.” His decision to state the knot-theoretical result without specifying the context in which it was rooted led others to credit him with having initiated modern knot theory (at least by having asked the right questions). For about 20 years at least, this virtually eliminated Wirtinger’s work from the collective consciousness of the

<sup>14</sup>Tietze’s article consisted of two parts. In Sects. 1–14, he developed a combinatorial theory of topological invariants (Betti and torsion numbers, the fundamental group). In Sects. 15–22, he discussed problems which did not yet seem tractable in a purely combinatorial fashion (e.g., embeddings of manifolds), admitting that the standard of rigour reached in the first part could not be maintained in the second; see [32, 80 ff]. Most readers appear to have concentrated on the first part. References to the second part are extremely rare in later papers on knot theory, although parts of its content came to be known later on via an oral tradition, see below.

<sup>15</sup> “[Nach meiner Dissertation] erhielt ich einen starken Eindruck von Wirtinger, der von Innsbruck nach Wien gekommen war und in Vorlesungen und “Übungen, wenn er auf algebraische Funktionen und ihre Integrale zu sprechen kam, darauf hindeutete, daß es topologische Momente sind, die dem Aufbau zugrunde liegen.” Quoted from [42, 78]. In a footnote at the beginning of his study (no. 10), Tietze explained that its starting point had been suggested to him by Wirtinger. The latter was also one of the three editors of the Viennese *Monatshefte*, in which Tietze’s *Habilitationsschrift* was published.

mathematical community. Today, most knot theorists know the central parts of Wirtinger's results—but few know that they are due to Wirtinger and even fewer that this piece of mathematics, rather than postdating the invention of knot theory, actually furnished the basic tool for treating the subject, the group of a knot.

*Wirtinger's Approach to Branch Points of Algebraic Functions  
of Two Complex Variables*

Before turning to the piece of mathematics in question, it seems appropriate to include a short characterization of its author. Wilhelm Wirtinger was born in 1865 in the little town of Ybbs in Lower Austria, the son of a physician. Even in school he seems to have read some mathematical classics, including some of Riemann's works. Whatever he may have understood from these, he was to become a renowned specialist in geometric function theory. He took his doctorate in 1887 under the Viennese mathematician, Emil Weyr, and continued his studies during a stay in Berlin and Göttingen. In Göttingen, he participated in Felix Klein's seminar and thus established one of the most important connections of his professional life. In 1890, he habilitated in Vienna, and after a period at the *Technische Hochschule* in Innsbruck, he received a call to Vienna in 1903, where he remained for the rest of his professional career. During his years in Innsbruck, he published widely appreciated papers on Abelian and theta functions. This recognition from his colleagues culminated in 1907 when he was awarded the Sylvester Medal by the Royal Society of London. Wirtinger's further academic career went smoothly (further details are given below), and he retired in 1935. He died in 1945 in his hometown, Ybbs.

Let us now discuss the piece of mathematical work that led to the first calculation of a knot group. This was the discovery of the connection between knots and the topology of singular points of algebraic curves, a finding usually attributed to Karl Brauner, who published a three-part article on the subject in 1928 in the *Hamburger Abhandlungen* [3].<sup>16</sup> Brauner was one of Wirtinger's students, and this article was his *Habilitationschrift*.<sup>17</sup> Moreover, the central idea of this article was clearly due to Wirtinger, even though the latter never chose to publish it. In his report on Brauner's *Habilitation*, Wirtinger wrote: "More than twenty years ago, the referee showed the way in which these difficult, but basic problems may be dealt with."<sup>18</sup>

The only printed documentation of Wirtinger's earlier work is the title of a talk he gave in 1905 at the annual meeting of the *Deutsche Mathematiker-Vereinigung*: "Über die Verzweigungen bei Funktionen von zwei Veränderlichen" [35]. However, we are in a good position to reconstruct his work. On the one hand, an oral tradition dating back to Wirtinger's lectures in Vienna is documented in several early papers on knot theory by Schreier, Artin, and Reidemeister, in addition to Tietze's and

<sup>16</sup> See for instance [36, Sect. 1.4; 56, 159; 54, 3 f.; 39, 415; or 47, 243].

<sup>17</sup> Brauner was definitely not a first-rate mathematician. After his *Habilitationschrift*, he published nothing of importance. In the 1930s and 1940s, he became a convinced supporter of the Nazis, and in 1945 he was removed from his chair in Graz. See [42, 249].

<sup>18</sup> "Der Berichterstatter hat vor mehr als zwanzig Jahren den Weg angegeben, auf welchem diesen schwer zugänglichen, aber grundlegenden Problemen beizukommen ist." Quoted from [42, 247].

Brauner's texts. Actually, all of these mathematicians had attended Wirtinger's lectures at one time or another [42, 18]. Comparing the ascriptions these texts made to Wirtinger leads to a rather clear picture. Fortunately, this picture is fully confirmed and even extended by a series of letters included in Wirtinger's correspondence with the powerful mathematician who had from early on guided and supported his career, Felix Klein.<sup>19</sup> What follows is a fairly detailed description of Wirtinger's ideas, based on these letters. From them, one can follow the gradual process of differentiation which ended in the first treatments of knot groups.

The first letter to Klein which is relevant here dates from December 22, 1894. It contains a sort of annual report on Wirtinger's work. Among other subjects, he writes about a new research project:

For functions of several variables, I have another project, namely to investigate whether the bilinear differential form in question can be determined in such a way that the real and imaginary parts of such a complex function on an arbitrary manifold remain potentials, too.<sup>20</sup>

This project amounted to nothing less than an extension of Klein's view of algebraic functions, based on the theory of potential functions on surfaces, to the case of several variables. Evidently, Wirtinger's proposal was to view, as Klein had successfully done for one variable in his treatise *Über Riemanns Theorie der algebraischen Funktionen und ihrer Integrale* of 1882, the variety associated with an algebraic function of  $n$  complex variables as a  $2n$ -dimensional real manifold whose complex structure is determined by a Riemannian metric. The class of real parts of algebraic functions defined on such a variety should then—so Wirtinger hoped—be included in the class of harmonic functions on this Riemannian manifold.<sup>21</sup>

At the time, the study of algebraic functions of two or more variables was a still young and flourishing field of research. When Wirtinger conceived his project, only a few studies had addressed this natural extension of Riemann's work on algebraic functions and their integrals. From around 1870 onwards, Max Noether and later Émile Picard had studied algebraic functions  $z$  of two complex variables  $x$  and  $y$  given by a polynomial equation

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

In particular, they had discussed resolutions of singularities by means of rational transformations. Clebsch and several Italian geometers had also considered special

<sup>19</sup> Wirtinger's letters to Klein are contained in the Klein Nachlass in NSUB Göttingen, Cod. Ms. Klein XII, 364–412. This includes ca. 50 letters dating from 1890 to 1924. At present, I do not know whether the other half of this correspondence is still extant.

<sup>20</sup> "Für die Functionen mehrerer Variablen habe ich noch ein Project, nämlich zu untersuchen, ob sich die bewusste bilineare Differentialform nicht so bestimmen lässt, dass der reelle u. imaginäre Theil einer solchen complexen Function auf beliebiger Mannigfaltigkeit auch Potentiale bleiben."

<sup>21</sup> For the plane  $\mathbb{C}^n$ , it had been shown by Poincaré in 1883 that a straightforward extension of the approach to complex functions in one variable via harmonic functions was impossible. There exist harmonic functions of several complex variables which are *not* real parts of analytic functions. Still, the reverse inclusion holds, so that potential theory *can* be applied to complex functions of several variables. See [22; 25].

classes of algebraic functions of two variables, interpreting these as algebraic surfaces. It soon became clear that one of the major differences between algebraic functions of one and of two variables was the much more involved topological situations that arise in the latter case. Algebraic surfaces were complicated objects of four real dimensions immersed in a space of six real dimensions. For instance, the set of singularities consists not of isolated points, as is the case for one variable, but is an algebraic curve, given by the discriminant  $D$  of the defining polynomial  $f$ :

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

In an influential memoir [20], Picard had emphasized this aspect with regard to the singularities of algebraic functions of two variables. When in 1897 he and Georges Simart published the first monograph on such functions, a substantial chapter of their book was devoted to *Analysis situs*.

All this makes clear that Wirtinger's project—however natural in an established line of research—was a formidable one. We shall see that Wirtinger's ambitions soon boiled down to a much more limited domain of questions. He, too, was aware of the topological problems which his project would pose. In his letter to Klein, he continued:

Of course, the faculty of imagination must here be educated and extended essentially. Let me just mention as an example that in 4-dimensional space, a surface of integration and a surface of singularities may be linked together like two rings in the three-dimensional domain. The surface of integration may then be deformed arbitrarily but cannot be reduced to a point. . . . To grasp all this in a typical and general way will not be easy, but it must be done in the end if the consideration of complex functions of several variables will not be restricted to the most elementary facts.<sup>22</sup>

We shall soon see that Wirtinger's suggestion that the topological difficulties of the subject called for a training of mathematical intuition was not just a passing remark.

Exactly one year later, on December 22, 1895, in his next "annual report," Wirtinger could write to Klein about his first successes. The complete text of the letter, which throws an interesting light on his relationship to Klein, too, is given in the appendix. In this letter, we find the first signs of the differentiation of a certain problem from the context of function theory which later turned out to be decisive. What Wirtinger in the letter calls "the core of the whole subject"—the investigation of the branching singularities of algebraic functions—will be transformed into the investigation of knot groups 15 years later. Here I shall try to isolate and interpret those aspects of Wirtinger's statements which pertain to this development.

<sup>22</sup> "Freilich muss hier das Vorstellungsvermögen wesentlich geschult u. erweitert werden. Ich erwähne nur beispielsweise, dass eine Integrationsfläche u. eine Singularitätenfläche im Raum von 4 Dimensionen so ineinander hängen können, wie zwei Ringe im dreidimensionalen Gebiet. Die Integrationsfläche kann dann beliebig verschoben u. verändert werden, aber nicht auf einen Punkt reducirt werden. . . . Alles dieses typisch u. allgemein zu erfassen wird nicht leicht sein, aber doch schliesslich gemacht werden müssen, wenn man die Betrachtung complexer Functionen mehrerer Variablen nicht auf das allerelementarste beschränken will. . . ."

As was usual at this time, Wirtinger viewed algebraic functions of two variables as branched coverings of the complex plane  $\mathbb{C}^2$ , in modern notation,

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

(A similar situation obtains for more than two variables). He then distinguished two kinds of branch points, according to whether the sheets of the covering are permuted cyclically along a closed path around the branch point or not. He realized that at *regular* points of the branch curve, where an analogue of the Puiseux expansion of the algebraic function is available, the sheets are permuted cyclically, while the situation is unclear at its *singular* points. At such points, say  $(x_0, y_0)$ , he interpreted the defining polynomial  $f$  as a polynomial in  $z$  with coefficients in the “Rationalitätsbereich” of power series  $a(x, y)$ , convergent in a neighborhood of  $(x_0, y_0)$  (probably, he meant the quotient field of this ring):

$$f(x, y, z) = a_0(x, y) + a_1(x, y)z + \cdots + a_n(x, y)z^n = 0.$$

It was known that the Galois group of the corresponding equation for the case of *one* variable, over the field of rational functions, coincides with the monodromy group of the unbranched covering of the Riemann number sphere with the set of branch points removed. (This had first been recognized by Hermité; see above). Evidently, Wirtinger extended this insight to his higher-dimensional, local situation, claiming that the Galois group of *his* equation equals the “local” monodromy group of the associated covering, that is, the monodromy group of the covering of a neighborhood of  $(x_0, y_0)$  with the branch curve taken out. Thus he could say that “this group characterizes the branch point”—we should add, topologically.<sup>23</sup>

Wirtinger then gave an example. It involves the general equation of order 3. He considered the algebraic function  $z$  of  $x$  and  $y$  given by

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

The equation of the branch curve then is

$$D_f(x, y) = x^3 + y^2 = 0.$$

This cubic has a cusp in  $(0, 0)$ . Since the Galois group of a general equation is the full symmetric group, there exist closed paths in the neighborhood of  $(0, 0)$ , which induce arbitrary permutations among the three branches of the algebraic function defined by Wirtinger’s equation. Thus the singular branch point is not of the cyclical kind.

This example remained paradigmatic for all later work. We shall see that while

<sup>23</sup> It is not clear to me what Wirtinger meant in the letter by saying that a neighborhood of a singular branch point is not homeomorphic to an  $n$ -cell and has a “certain connectivity.” Is he speaking of a neighborhood of the branch point in the *base*—with the branching manifold taken out—or in the *total space* of the covering? Certainly, there is an argument in the air concerning the first possibility. Since the fundamental group of such a neighborhood has a non-abelian homomorphic image—the Galois group—it cannot be abelian either. However, the sources do not ascribe such an argument to him.

pursuing the questions he now had put to himself—to specify general conditions which a group of permutations had to satisfy in order to occur as the local monodromy group associated to a singular branch point—Wirtinger was led to the first calculation of a knot group, a result made public by Tietze in 1908.

In the following years, however, Wirtinger remained silent about his project. He had enough else to do, for instance in working with Max Noether to prepare an extensive supplement to Riemann's collected works, and writing the article on algebraic functions and their integrals for the *Enzyklopädie der mathematischen Wissenschaften* [34]. Felix Klein, who thought highly of his Austrian admirer, probably had his hands in the arrangement of both tasks. Significantly enough, the *Enzyklopädie* article contained nearly nothing about functions of several variables, and we find Wirtinger writing:

. . . in general, the theory of functions of several variables has not yet been developed very far. In particular, one has not yet succeeded in determining a given algebraic variety by a finite number of data in a similar way as is possible with the different forms of a Riemann surface. These investigations presuppose a thorough treatment of *analysis situs* for several dimensions.<sup>24</sup>

Wirtinger returned to the subject in a letter dated August 26, 1903. Finally, he announced a result which he intended to present publicly:

For the branchings of algebraic functions of several variables I have made a completely elementary study whose only aim is to make clear how it happens, and how it must be imagined topologically, that along the connected discriminant manifold only two branches are connected in general, while in its singular points perhaps arbitrarily many [branches are connected]. I wanted to report on these things in Kassel, but it has again become unclear whether I shall be able to go there.<sup>25</sup>

Actually, Wirtinger did not travel to the annual meeting of the *Deutsche Mathematiker-Vereinigung* in Kassel in September 1903, due to problems with his ear.<sup>26</sup> Another two years passed before he gave a talk on this subject in 1905.

<sup>24</sup> “. . . dass die Theorie der Funktionen mehrerer Variablen überhaupt noch wenig ausgebildet ist. Es ist im besondern noch nicht gelungen, das einzelne algebraische Gebilde durch eine endliche Anzahl von Bestimmungsstücken in ähnlicher Weise festzulegen, wie dies bei den verschiedenen Formen der Riemannschen Fläche möglich ist. Die Untersuchungen selbst setzen eine eingehende Bearbeitung der Analysis situs für mehrere Dimensionen voraus” [34, 174]. For this last task, Wirtinger refers to Poincaré's texts [24; 26]. The middle sentence contains an allusion to his own work. One way to determine a Riemann surface had been studied by Hurwitz [13]: by specifying the number of sheets, the position of its branch points, and the local monodromy behavior at these points, i.e., a certain sheet permutation associated with each branch point. Wirtinger's thoughts focused on a generalization of the last of these three aspects.

<sup>25</sup> “. . . Über die Verzweigungen algebraischer Functionen mehrerer Variablen habe ich eine ganz elementare Untersuchung angestellt, welche nur den Zweck hat, klar zu stellen, wie es kommt u. wie man sich topologisch vorzustellen hat, dass längs der zusammenhängenden Discriminantenmannigfaltigkeit überall im allgemeinen nur zwei Zweige zusammenhängen, dagegen in deren singulären Punkten unter Umständen beliebig viele. Über diese Dinge wollte ich in Cassel berichten, ob es mir aber möglich sein wird hinzukommen ist wieder zweifelhaft geworden. . . .”

<sup>26</sup> Wirtinger to Klein, Cod. Ms. Klein XII, 409; probably autumn 1903.

Taking together what we find in [1; 3; 32], it is not too difficult to reconstruct the contents of that lecture. The main source is Sect. 18 of Tietze's *Habilitationsschrift*; Brauner's paper also makes it possible to add some computational details. In the talk, a crucial new idea came into play which, according to Tietze, Wirtinger had taken from Heegaard's dissertation of 1898, entitled *Forstudier til en topologisk teori for de algebraiske fladers sammenhæng* [10]. As the title indicates, Heegaard had developed similar interests to those of Wirtinger in a topological treatment of algebraic functions of two complex variables, but unlike Wirtinger he preferred the viewpoint of algebraic geometry. Heegaard's idea was to study singular points of algebraic surfaces by looking at the restriction of the branched covering of  $\mathbb{C}^2$  defined by the equation of the surface to a 3-sphere bounding a small neighborhood of the singular point in question. In this way, one gets a branched covering of the 3-sphere, which is precisely what Heegaard called a "Riemann space" [10, Sect. 13]. In the situation of the paradigmatic example, one obtains (for small positive  $c$ )

$$\begin{aligned} \{(x, y, z) \in \mathbb{C}^3 : z^3 + 3zx + 2y = 0 \text{ \& } |x|^2 + |y|^2 = c\} \\ p \downarrow (x, y, z) \mapsto (x, y) \\ \{(x, y) \in \mathbb{C}^2 : |x|^2 + |y|^2 = c\} \end{aligned}$$

It was now a matter of straightforward calculation for Wirtinger to see that *the restriction of the branching manifold to the 3-sphere is a trefoil knot!* One simply had to solve the system of equations

$$x^3 + y^2 = 0 \quad \text{and} \quad |x|^2 + |y|^2 = c.$$

The calculation makes clear that the result is a curve which lies on a torus and winds twice round the first meridian and three times round the second.

Moreover, the restriction transforms the local monodromy group characterizing the original singularity into the global monodromy group of the three-sheeted covering of the exterior of the trefoil knot. Thus in order to study the topological situation at the branch point, it was enough to look at the three-sheeted covering of the exterior of the trefoil. As was usual in the case of Riemann surfaces, Wirtinger now applied more or less intuitive cutting and pasting arguments in order to obtain generators and relations for this monodromy group. There is a picture in Artin's article [1] which illustrates these techniques (Fig. 2). The same picture had been described in words by Tietze, and it reappeared in Brauner's article. Finally, it was reprinted in Reidemeister's *Knotentheorie*. In all of these cases, the use of this picture in order to derive a presentation of the knot group is ascribed to Wirtinger.<sup>27</sup> (The idea of the picture itself was again taken from Heegaard's dissertation, where it had been used to construct what Heegaard had called the "diagram" of a Riemann space.) This alone suffices to establish the existence of an oral tradition—which in this case transmitted a picture, an intuition!<sup>28</sup>

<sup>27</sup> In Artin's case, Schreier was the go-between; see [1, 58].

<sup>28</sup> See [44] for another example and a more philosophical discussion of the importance of cognitive acts related to such intuitions in the creation of mathematical knowledge.

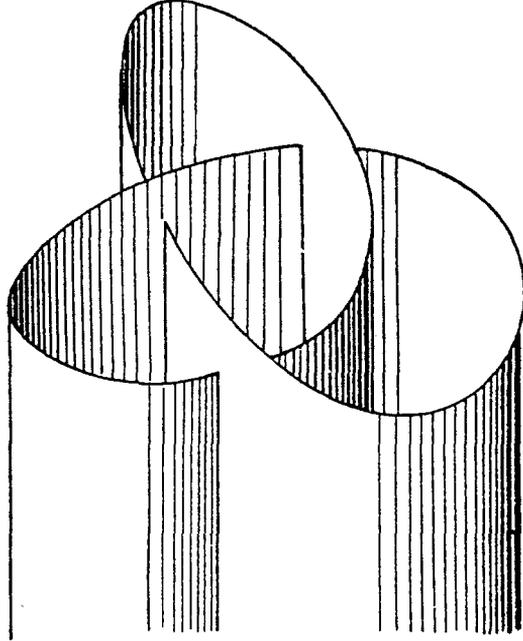


FIG 2. Wirtinger's semicylinder.

The first step toward determining a presentation of the monodromy group was to cut the covering into three simply connected sheets. This was achieved by joining the points on the branch curve by straight lines to a point (conveniently chosen at infinity in a direction along which the knot projects to a regular knot diagram) and cutting along the resulting semicylinder with self-intersections.<sup>29</sup> Evidently, the monodromy group is then generated by the sheet permutations associated to penetrations of this semicylinder. In his example, Wirtinger used six generators, associated with the six parts of the semicylinder which lie between the three lines of self-intersection. By looking at small circles which do not circle around the trefoil, the group relations could be determined. Three cases were to be distinguished: (i) only one part of the semicylinder is traversed, giving no relation; (ii) two parts of the semicylinder are penetrated (this may happen *above* a line of self-intersection and reduces the number of generators to three, say  $r$ ,  $s$ ,  $t$ , associated to the three arcs in the knot diagram); (iii) four parts of the semicylinder are passed through. This leads to those relations which today are still called Wirtinger's relations. For the trefoil, they are given by

$$1 = rsr^{-1}t^{-1} = st^{-1}s^{-1}r = tr^{-1}t^{-1}s.$$

<sup>29</sup> In one dimension lower, the same idea had been used in the 19th century, e.g., by Hurwitz [13].

Eliminating  $r = sts^{-1}$ , one arrives at the following relation for the sheet permutations of the covering:  $sts = tst$ —which is exactly the relation which appeared in Tietze’s argument for the knottedness of the trefoil. In fact, it is clear from Wirtinger’s argument that  $\langle s, t \mid st = tst \rangle$  is a presentation of the fundamental group of the exterior of the trefoil knot. As an immediate corollary, we obtain an argument for the fact that this group is not infinite cyclic. By construction, it has the symmetric group of order three (the monodromy group) as a homomorphic image. In this way, Wirtinger had not only characterized the complicated singularity of his example by means of the group of the trefoil,<sup>30</sup> but had also shown how to form a very intuitive picture of the topological situation around the branch point. The remarks to Klein in his letter of 1903 were thus fully justified.

From the sources, it is not quite clear whether Wirtinger was fully aware of the fact that he had actually developed much more than what he originally had been looking for. What he had given was a method for deriving not just necessary conditions on the monodromy group in question, but a presentation of the fundamental group of the exterior of an *arbitrary* knot! While it is not very probable that anyone who had read Poincaré would overlook this difference, it is astonishing to realize that even in 1928 Brauner seems to confuse exactly these two aspects of Wirtinger’s procedure.<sup>31</sup> The method became generally known under Wirtinger’s name when Artin described it in his widely read article on the braid group [1]. It is interesting that Artin makes no reference to Tietze’s *Habilitationschrift*, where the method already had been described in detail. This once more suggests that Tietze’s reference to his advisor escaped the notice of the mathematical community. In any case, by 1908 the separation from its original context of the central problem which was to constitute modern knot theory was complete.<sup>32</sup>

### *Wirtinger’s Debt to Klein*

Before turning to knot theory proper and the final elimination of contexts, it should be emphasized that Wirtinger’s commitment to the context of a certain view of algebraic functions was not just a question of mathematics. We have seen that when he began work on his project he viewed it as a natural generalization of an approach to algebraic functions advocated by one of the mighty figures on the

<sup>30</sup> To be precise, he had shown the way to arrive at a topological classification of singular points of plane algebraic curves. Wirtinger’s final result no longer concerned the *covering* he originally had intended to consider, but the topology of the *base* of this covering. See the *Prelude* above.

<sup>31</sup> For Brauner, *Wirtinger’s* relations are still *monodromy relations* and not relations in the fundamental group of the knot complement. See [3, 4 ff].

<sup>32</sup> Wirtinger’s investigation not only made the connection between singularities and knots; it also contained the first example of a knotted surface in a manifold of four real dimensions. This is exactly the position of the complex branch curve in the complex plane associated with the given algebraic function when looked at from the point of view of real manifolds. It is evident that Artin had this example in mind when he inaugurated the study of knotted surfaces in his short paper [2]. Once more, we face an elimination of contexts. A similar situation obtains in the case of Artin’s alleged “invention” of braids in [1]; they had originally been treated in the context of Riemann surfaces by Hurwitz in his remarkable paper [13]. For details, see [40; 43].

German mathematical scene. Like many others, Wirtinger was very clear as to the influence which Klein had on his professional career. Thus, it is hardly surprising that he informed Klein about progress and promising perspectives in his research. It may even be the case that Klein expected something of this kind from mathematicians under his protection. We have also seen that Wirtinger never chose to leave the context of algebraic functions which was so dear to Klein.

In his general views on mathematics, too, he was strongly influenced by his mentor. His letters to Klein express a strong adherence to Klein's values concerning mathematical practice and the place of mathematics in culture. Wirtinger shared Kleinian values with respect to the status of geometric intuition—as evidenced by the way in which Wirtinger approached his project on algebraic functions—as well as with regard to the social importance of higher mathematical education. In one of a series of letters dealing with the issues put forward by Klein in his talk on “arithmetization” [17], Wirtinger expressed his general agreement by using a nice metaphor to illustrate the mathematician's task. He imagined the mathematician of the 20th century, he said, like a painter who looks at the world with a painter's eyes, thinking about the way in which he would like to paint it. Correspondingly, the mathematician should try to “see the mathematical problem” in whatever form she or he encounters it. This perceptive faculty should be the result of general mathematical education. Wirtinger then joins in with Klein's critique of the growing trend toward abstraction in mathematics; an abstract definition, he says, is nothing but a *résumé* of a series of concrete instances, in which the real mathematical interest must lie.<sup>33</sup>

It is certainly apt to call Wirtinger a convinced supporter of a “Kleinian style” of mathematical practice. Wirtinger made this very explicit not only in his letters but also in an article written on the occasion of Klein's 70th birthday, entitled *Klein und die Mathematik der letzten 50 Jahre* [35].

Of particular importance in the present context is Wirtinger's reluctance to accept and to promote the general tendencies toward a growing differentiation of mathematical fields. This hesitation, central to the style of mathematical practice advocated by Klein and Wirtinger, is clearly expressed in the following passage from Klein's *Entwicklung der Mathematik im 19. Jahrhundert* which refers to the differentiation of problem fields and methodological schools in connection with algebraic functions:

This tendency to dissect science not only into an ever greater number of individual disciplines, but also to create schools based on differences with regard to methodology would, if it should become prevalent, lead to the death of science. We have always aimed at the opposite ourselves.

<sup>33</sup> “Ich stelle mir den Mathematiker des 20<sup>r</sup> Jahrhunderts so vor, dass er, wie der Maler, so oft er will, die Welt malerisch sieht u. denkt wie er sie malen würde (u. nicht blos an classische Galeriebilder), auch so oft er will das mathematische Problem sieht, wo u. in welcher Gestalt immer es entgegentritt. Als Resultat der allgemeinen mathematischen Bildung, denke ich mir nun die Fähigkeit dieses Sehens, wenigstens im Princip. Es scheint mir, dass Sie mit der Bemerkung auf pag. 8 über die zu grosse Abstraction, die nur hindert ein concretes Problem zu erfassen, die Wurzel des Übel's bezeichnet haben. Mir persönlich war die Verbaldefinition nichts anderes als das Resumée über eine Reihe concreter Fälle u. ohne Kenntnis derselben ganz ohne Interesse.” [Wirtinger to Klein, May 22, 1896]

In our generation, we have kept 1. the theory of invariants, 2. the theory of equations, 3. function theory, 4. geometry and 5. number theory more or less in contact and this was our special pride.<sup>34</sup>

Elsewhere I have tried to show not only that this attitude represents a strong normative commitment with respect to the organization of mathematical research, but also that it was connected to a particular style of mathematical argumentation which drew heavily on—and sought to draw on—connections between different areas of mathematics [43]. As such, Klein's statement may be interpreted as expressing one aspect of a specific standard of rationality in mathematical practice. This standard is "integrative" in a strong sense. It is regarded as rational to promote the coherence of mathematics as a whole, in professional politics as well as in doing research which links different mathematical fields. It may well be that Wirtinger's adherence to a similar standard was one of the factors which prevented him from pursuing and publishing the purely topological parts of the results which he obtained in his investigation of branch points of algebraic functions.

### ELIMINATION OF CONTEXTS

#### *Max Dehn's Research on Knots*

At about the time when his *Habilitationsschrift* was published, Heinrich Tietze had an encounter in Rome with another aspiring young mathematician interested in topology and knot theory, Max Dehn. At the time, Dehn was convinced that he knew of a topological characterization of ordinary 3-space which would have been more or less equivalent to a proof of the Poincaré conjecture.<sup>35</sup> Tietze pointed out the error in Dehn's argument, and Dehn had to withdraw a paper which he already had sent to Hilbert with the urgent request for speedy publication in the *Göttinger Nachrichten*.<sup>36</sup> Dehn's misconception prevented him from publishing the other half of the paper immediately, and only in 1910 did the first of a series of papers appear which—among other things—definitively established knot theory as a promising subfield of what was then called combinatorial topology ([5–8]). In fact, this paper also contained a serious gap which would not be filled during Dehn's lifetime, the notorious "Dehn lemma" (see, e.g., [51]).

The central notion in this work was again that of the fundamental group of the

<sup>34</sup> "Diese Tendenz, die Wissenschaft nicht nur in immer zahlreichere Einzelkapitel zu zerlegen, sondern Schulunterschiede nach der Art der Behandlung zu schaffen, würde, wenn sie einseitig zur Geltung käme, den Tod der Wissenschaft herbeiführen. Wir selbst haben immer das Umgekehrte angestrebt. In unserer Generation haben wir 1. Invariantentheorie, 2. Gleichungstheorie, 3. Funktionentheorie, 4. Geometrie und 5. Zahlentheorie mehr oder weniger in Kontakt gehalten, und das war unser besonderer Stolz." [18, 327]

<sup>35</sup> The key to this characterization would have been "daß der gewöhnliche Raum die einzige 3dim. Mannigfaltigkeit ist, in der jeder 'geschlossene Flächenkomplex' [as Dehn had defined it] zerstückelt" (Dehn to Hilbert, February 12, 1908). Compare also the final paragraph in [5].

<sup>36</sup> Dehn to Hilbert, February 12, and April 16, 1908. Dehn had feared that somebody, perhaps Poincaré himself, might anticipate his result.

exterior of a knot, which Dehn simply called “the group of the knot.” One of the famous results of the first paper is the statement (which hinges upon “Dehn’s lemma”) that a knot may be disentangled, i.e., is equivalent to the trivial knot, if and only if its group is abelian.<sup>37</sup> Even more impressive was the proof, contained in the fourth paper, showing that a left-handed trefoil knot cannot be deformed into its right-handed mirror image. The proof involved classifying the automorphisms of the group of the trefoil, which, as we have seen, had already been determined in Wirtinger’s and Tietze’s work. Nevertheless, Dehn did not use their techniques. In Dehn’s papers, no idea related to algebraic functions or at least coverings of the exterior of a knot was mentioned, and he ignored Wirtinger’s method of deriving a presentation of the knot group. Instead, Dehn gave another presentation starting completely from scratch.<sup>38</sup> The methods he used to prove his deeper results were devoid of all vestiges of analysis, and consisted in a highly original fusion of combinatorial group theory and hyperbolic geometry (which contributed to topology and group theory in almost equal parts).

Why did Dehn not take up the thread offered to him by Tietze? It is clear that he had read at least those parts of Tietze’s paper which seemed important to him.<sup>39</sup> The answer to this question accounts for why the context of algebraic functions is no longer present in the first period of modern knot theory. Moreover, it also reveals how this particular example of context-elimination reflects the rise of a new style of mathematical practice dominated by another mighty figure on the mathematical scene, David Hilbert, and connected to a new standard of mathematical rationality. In order to explain this fully, we must once more work our way backwards in time. We begin with a detailed description of Dehn’s way of defining knots and *his* way of proving the knottedness of the trefoil. Since Dehn’s work in topology is better known than Wirtinger’s, we need not go into the same detail as in the previous section.<sup>40</sup>

Dehn’s paper of 1910 began with some considerations which proved to be of great influence for the development of combinatorial group theory. Dehn’s subject was finite group presentations, and he stated clearly the algorithmic problems associated with them: to find methods that would enable one to decide in a finite number of steps whether or not two words (a) represent the same group element, or (b) represent conjugate elements. (A third algorithmic problem, namely, to decide whether two given presentations determine isomorphic groups, had already been stated and treated by Tietze in the context of showing the combinatorial invariance of the fundamental group [32]; see above.) Dehn went on to translate the first of these problems—the “Identitätsproblem” as he called it in [6]—into

<sup>37</sup> Another aim of this paper was to construct examples of 3-manifolds—in particular, of homology spheres—using what was later called surgery on a knot.

<sup>38</sup> For some time, the group of a knot was even called “Dehn’s group,” for instance, by Veblen [33] and in a letter of Reidemeister to Hellmuth Kneser of 1925. The picture was corrected by Artin [1].

<sup>39</sup> This is evident from the group-theoretical parts of his papers, for instance [6]. See also [40, 17 ff.].

<sup>40</sup> For a historical treatment of Dehn’s topological papers, see, e.g., [51] or [40]. Bollinger [38] and vanden Eynde [45] have also devoted some attention to Dehn’s work.

another combinatorial problem, the construction of the “Gruppenbild” or Cayley graph associated to a group presentation  $G := \langle a_1, \dots, a_n \mid r_1, \dots, r_n \rangle$ . The vertices of this graph are the group elements. Two vertices  $g, h \in G$  are connected by an edge of the graph if and only if  $h = a_i g$  holds for some generator  $a_i$ . Consequently, each closed path in the graph represents a relation in the group. To solve the word problem and to construct the graph are therefore equivalent.<sup>41</sup> The “Gruppenbild” is an example of what Dehn called a “Streckenkomplex,” that is, an assembly of basic elements (here, group elements) together with a set of pairings (here, equations  $h = a_i g$ ). We shall see shortly how this combinatorial notion could be used to treat knot groups.

After some further preparations (including his “lemma”), Dehn defined knots as what he called “closed nonsingular curves,” embedded in 3-space [5, 153]. A closer look shows that a “curve” is, for him, another example of a “Streckenkomplex.”<sup>42</sup> It is a polygonal curve, given by a set of points in 3-space, paired according to the line segments which make up the polygon. (For Wirtinger, by contrast, the trefoil had been given by the intersection of an algebraic curve with a 3-sphere in the complex plane!) A curve is nonsingular if no two line segments meet (except neighboring segments at the vertices of the polygon). The group of a knot was then introduced in the following way. Dehn’s starting point was the “Streckenkomplex” given by a regular projection of a knot (a knot diagram with self-crossings). To such a graph, he had associated earlier in the paper [5, Part I, Sect. 2] a group presentation which was nothing but the fundamental group of the surface bounding a tubular neighborhood of the projection. Then, for each crossing of the projection, new relations were introduced which account for the fact that, in contrast to its projection, the knot has no self-crossings in 3-space. In this way, Dehn arrived at a presentation of the group of equivalence classes of paths on the torus which bounds a tubular neighborhood of the knot, where two such paths are considered equivalent if and only if they are homotopic *in the knot complement* [5, 157]. This completed Dehn’s definition of the group of a knot. For the trefoil, Dehn found a presentation that is easily shown to be equivalent to Wirtinger’s presentation:

$$\langle c_1, c_2, c_3, c_4 \mid c_1 c_4^{-1} c_2, c_2 c_4^{-1} c_3, c_3 c_4^{-1} c_1 \rangle.$$

Note that the knot group was introduced on a purely combinatorial basis, starting from a “Streckenkomplex” and using no further information. It was *not* given beforehand and then shown to possess a certain presentation, as in Wirtinger’s case. That it has an obvious *interpretation* in terms of equivalence classes of paths is not essential for the definition itself. Interestingly enough, Dehn makes no attempt to show that his group is in fact an invariant under a combinatorial version of isotopy. This leads one to wonder how Dehn could have shown that a given knot

<sup>41</sup> For further information on the “Gruppenbild” and Dehn’s use of it, see [40].

<sup>42</sup> In [5], the reader is simply referred to the *Enzyklopädie* article [4]; see below.

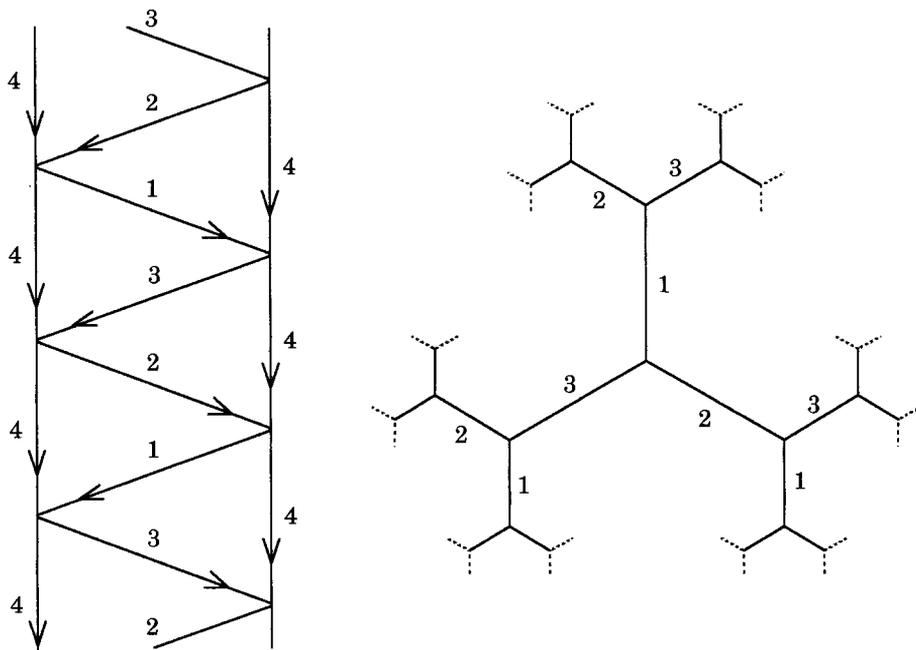


FIG 3. Dehn's "Gruppenbild" of the trefoil.

is non-trivial. Dehn did so by using the correct, but insufficiently proven result that a knot is trivial if and only if its group is abelian. (Since the "only if" part is enough for showing knottedness, the gap in Dehn's proof is not relevant here.) Let us consider how this result appears in the case of the trefoil.

In this instance, Dehn actually managed to construct the "Gruppenbild." It can be built from infinitely many "strips" of the form shown in Fig. 3 (left), where vertical edges always represent  $c_4$ , while oblique edges represent  $c_1$ ,  $c_2$ , and  $c_3$  as indicated in the figure. Different copies of such strips must then be pasted together according to the scheme shown in Fig. 3 (right). Here, every line segment represents one copy of the strip, as "seen from above," and in pasting one has to ensure that at each vertex all four types of edges meet (this is indicated by the numbers).

It is easy to see that the resulting graph is in fact the "Gruppenbild" of the trefoil group. Dehn's argument for the knottedness of the trefoil is now a matter of inspection. In his own words: "Now we recognize immediately that the group is not isomorphic to the group  $\{S^\alpha\}$  [i.e.,  $\mathbb{Z}$ ], that it is not abelian. For example, the polygonal tract  $c_1c_4c_1^{-1}c_4^{-1}$  is not closed."<sup>43</sup>

<sup>43</sup> "Wir erkennen nun sofort, daß die Gruppe nicht isomorph mit der Gruppe  $\{S^\alpha\}$ , daß sie nicht abelsch ist. Zum Beispiel ist der Streckenzug  $c_1c_4c_1^{-1}c_4^{-1}$  nicht geschlossen" [5, 160]. Looking through the right glasses, the figure also shows that the group of the trefoil acts by isometries on the hyperbolic plane, a fact heavily exploited in Dehn's article of 1914 [8].

### *A Hilbertian Approach to Topology*

Evidently, Dehn's way of dealing with knots and their groups depended on a completely different conceptual outlook than Wirtinger's. This conceptual framework was taken from an article that Dehn had written together with Poul Heegaard for the *Enzyklopädie der mathematischen Wissenschaften* in 1907. There, they sketched a rigorously combinatorial approach to topology while attempting to reformulate the classical problems of 19th-century topology in this setting.

Among these classical topics, the problem of classifying knots was taken up and given a systematic place in the hierarchy of topological problems. Dehn and Heegaard distinguished between problems of "Nexus" and problems of "Connexus." The former are problems of classifying manifolds up to homeomorphism, whereas the latter deal with embeddings of manifolds of various dimensions into each other [4, 170]. Among "Connexus" problems, one finds problems of *homotopy* (Dehn and Heegaard only mention closed curves in  $n$ -dimensional manifolds, which give the problem of calculating the fundamental group), and problems of *isotopy*. According to Dehn and Heegaard, the first interesting case of the latter type of problems is the knot problem [4, 207 ff.].<sup>44</sup> They even sketched a (rather trivial) "arithmetization" of the knot problem. This "arithmetization" consisted in considering knots as chains of nearest neighbors in a three-dimensional cubic lattice, equivalence of knots being given by the obvious elementary deformations.<sup>45</sup>

More interesting than the remarks on knots, however, is the general perspective on topology which was advocated in the article. Unlike nearly all the other articles in the *Enzyklopädie*, the systematic parts of Dehn and Heegaard's text (for which Dehn was primarily responsible [4, 153]) were written with an evident dependence on Hilbert's epoch-making *Grundlagen der Geometrie*. Like Hilbert's geometry, *Analysis situs* was presented as a theory dealing with aggregates of uninterpreted elements, for which only combinatorial rules are specified.<sup>46</sup> Some of these rules were taken directly from the "topological" parts of Hilbert's book, as for example the notion of a "Streckenkomplex," which is a direct generalization of what Hilbert had called a "Streckenzug."<sup>47</sup> Dehn and Heegaard even formulated axioms (in Sect. 8), without, however, treating problems of consistency or uniqueness. These axioms were thought of rather as conditions which the combinatorial definitions had to satisfy in order to allow for an intuitive interpretation of the theory, to give it an "Anschauungssubstrat." After introducing this formal apparatus, the authors went on to characterize *Analysis situs* as a "part of combinatorics, characterized

<sup>44</sup> Even though Dehn had worked on the Jordan curve theorem earlier, the isotopy problem of closed curves on a surface was not mentioned.

<sup>45</sup> Eighteen years later, Artin also called his discussion of the braid group an "arithmetization" of (topological) braids.

<sup>46</sup> For further details, see [38, 144–147].

<sup>47</sup> According to Hilbert [12, Sects. 3–6], a line segment is defined by a pair  $(AB)$  of points  $A, B$ ; a "Streckenzug" is a system of line segments  $((AB)(BC)(CD) \dots (KL))$ . Admitting arbitrary pairings instead of chains yields a "Streckenkomplex" as defined by Dehn and Heegaard [4, 156].

by its intuitive meaning.” Moreover, in another striking parallel to Hilbert’s *Grundlagen der Geometrie*, Dehn and Heegaard viewed *Analysis situs* as occupying a rather fundamental place in the architecture of mathematical disciplines, namely, as “the most primitive section of geometry, where the notion of a limit is still of no importance.”<sup>48</sup> Consequently, not even the analytical notion of continuity has a systematic place in Dehn and Heegaard’s treatment. In sum, the mathematical discipline of topology which Dehn had in mind was from the beginning conceived as an axiomatic, self-contained theory, very fundamental and far removed from analytic contexts.

Compared to Wirtinger’s and even Poincaré’s ideas about the role of topological problems in mathematics, Dehn and Heegaard’s approach represents a new start. The decision to present topology as a part of combinatorics amounted to establishing a new standard of rationality in dealing with topological questions. On the one hand, this standard aimed at perfect rigor. Topological arguments could be *guided* by intuition, but essentially they should be reducible to arguments dealing with combinatorial data on a formal, axiomatic basis. On the other hand, the new standard *differentiates* topology from other mathematical disciplines. Topology has concepts, techniques, and a hierarchy of problems defined *internally*, without reference to either the origin of its basic concepts in or its application to other fields, like complex function theory or algebraic geometry. Of course, this differentiated status of topology does not preclude, and very probably was not intended to preclude, applications of topological ideas to other mathematical fields. What had changed, however, was the *type of relations* between these fields, and the way of conceiving topology as a subject on its own.<sup>49</sup>

When, one year after Dehn and Heegaard’s article, Tietze published his *Habilitationsschrift*, we find him wavering between the new standard and a more traditional outlook on topology. As far as possible, he tried to follow a rigorous combinatorial approach. However, he was not yet prepared to give up completely connections with ideas originating in fields like algebraic functions or the kind of intuitive arguments that had been usual in earlier treatments. His personal solution to this conflict of rationality standards was diplomatic. He proposed to consider those topological issues not yet tractable from the combinatorial point of view—among them “Riemann spaces” and Wirtinger’s approach to the knot group—as suggesting the type of problems to be dealt with in the future [32,

<sup>48</sup> *Analysis situs* is a “... durch seine anschauliche Bedeutung ausgezeichnete Teil der Kombinatorik, ... der primitivste Abschnitt der Geometrie, wo der Grenzbegriff noch nirgendwo von Bedeutung ist” [4, 170 ff.].

<sup>49</sup> It would be interesting to compare this outlook on topology with that advocated by Listing 60 years earlier [19]. In 1847, not even the separation of mathematics and physics was an established fact. Accordingly, Listing’s efforts to promote a new mathematical discipline included the attempt to convince scientists of all sorts that topology had significant insights to offer in their contexts: in crystallography, biology, physics, etc. *Two* levels of differentiation separate Listing’s project from Dehn and Heegaard’s: first, the differentiation of pure mathematics from other scientific contexts; and second, the differentiation of topology from its contexts in pure mathematics.

80 ff.].<sup>50</sup> Whatever the merits of this early attempt to give topology an axiomatic, combinatorial foundation may have been, one thing was clear: once Dehn, Heegaard, and Tietze had published their papers, no reader of them would have denied that topology had emerged as a discipline in its own right, endowed with its own problems and standards of rigor.

It is easy enough to see where Dehn got the orientation which determined his views on topology. Around the turn of the century, he had been one of Hilbert's model pupils. In a letter to Hurwitz, Hilbert spoke in enthusiastic terms about the results of Dehn's thesis on the foundations of geometry.<sup>51</sup> (It was in this connection that Dehn first began working on hyperbolic geometry, an interest which proved useful to him later on.) After having solved Hilbert's third problem on polyhedra, Dehn could be sure of the future support of his teacher.<sup>52</sup> From the correspondence between the two it is clear that Dehn was deeply involved in the revisions that led to several new editions of *Grundlagen der Geometrie*.<sup>53</sup> He even considered writing a book on the foundations of geometry himself.<sup>54</sup> Although this project was never realized, he later did write an historical appendix to the second edition of Pasch's classic *Vorlesungen über neuere Geometrie* in which he sought to emphasize the lines of thought leading to Hilbert's foundations of geometry [9]. Hilbert, in turn, contributed to the career of his pupil by writing letters of recommendation.<sup>55</sup> Thus, when in the years following 1908 Dehn turned to topology and knot theory, it was clear that a genuine follower of Hilbert's axiomatic program in geometry had begun research on knots. Little wonder, then, that knot theory—like combinatorial topology as a whole—was seen as a fundamental, self-contained theory which *a priori* had nothing to do with higher analysis.<sup>56</sup>

<sup>50</sup> In a way, Tietze's ambivalence was not new, and it was never resolved completely. Dehn and Heegaard's radical hope to establish topology as a subfield of combinatorics never quite got off the ground. During the 1920s and 1930s, a competition dominated the scene between topologists favoring the combinatorial approach and topologists who elaborated analytical notions and methods. Even when the possibilities of the different approaches could be assessed more clearly in the language of categories, this still did not bring to an end the conflict between combinatorial and analytic orientations shared by different members of the topological community. This ongoing debate points to a conflict between what Gerald Holton has called a pair of antithetical themata in scientific thought [48, Chapter 1].

<sup>51</sup> Hilbert to Hurwitz, 5. and 12.11.1899, contained in the Hurwitz Nachlass in NSUB Göttingen.

<sup>52</sup> See [51] for details.

<sup>53</sup> The correspondence is contained in the Hilbert Nachlass in NSUB Göttingen and the Dehn Nachlass in Austin, Texas.

<sup>54</sup> Dehn to Hilbert, January 19, 1903.

<sup>55</sup> Dehn to Hilbert, April 3, 1911, concerning Hilbert's recommendation of Dehn's call to Kiel; March 9 and July 11, 1913, concerning an unsuccessful attempt to promote Dehn in Kiel and his call to Breslau.

<sup>56</sup> Still another influence of Hilbert on Dehn resulted in the awareness of *algorithmic* problems in combinatorial disciplines. When, in 1910, Dehn decided to begin his topological paper with a statement of the word and conjugacy problems of combinatorial group theory, this may well have been a response to Hilbert's quest for an algorithmic solution of problems in number theory, expressed in Hilbert's 10th problem. See [40, 54 ff.].

*Wirtinger, Dehn and the Memory of the Mathematical Community*

This background allows us to understand why Dehn did not take up the line of thought on knots which had been pursued by Wirtinger and his younger colleague, Tietze. He simply was educated and accustomed to think along a completely different horizon of mathematical values. Dehn adhered to a different standard of mathematical rationality. He also was moving in a different social setting in the mathematical community, and he was influenced by a different constellation of power relations.<sup>57</sup> Probably, Dehn simply did not read passages in Tietze's *Habilitationschrift* which had to do with things like "Riemann spaces," the more so since Tietze himself admitted that they were not treated in a rigorous combinatorial fashion but rather depended heavily on intuitive arguments, situated in a peculiar way between different mathematical theories. And even if Dehn *had* read these passages, it must have been clear to him that there was no easy way to adapt their contents into the picture of combinatorial topology to which he adhered.

The absence of the context of algebraic functions in Dehn's pioneering work on knots may be explained along these same lines. This elimination of contexts need not necessarily have been the result of a conscious decision. Rather, it was probably the unintended effect of a *prior* decision, the decision to accept a Hilbertian style in mathematical practice together with its inherent standard of rationality. Relative to that standard, the elimination was itself "rational." The combinatorial approach to knots was simpler and more streamlined since it carried no ballast from the theory of algebraic functions. It promised a different level of rigor in proofs as well as a clearcut separation between the knot problem and other mathematical problems related to it. The tendency of the new style to foster a differentiation within mathematical disciplines made it also especially attractive for a *young* mathematician like Dehn. It allowed such a person to reach the frontiers of research without the long years of education necessary to survey a whole network of mathematical fields as Klein would have liked. Dehn could do knot theory without knowing much about Galois groups, analytic continuation, or singular points of algebraic curves. It was no longer necessary to delve deeply into the classics of 19th-century mathematical literature, reading works by Puiseux, Riemann, Jordan, etc.

It is equally understandable that when, after the interruption caused by World War I, the next generation of mathematicians—Schreier, Reidemeister, and Artin—entered the scene, they started from Dehn's combinatorial approach, and not from Wirtinger's (still unpublished) ideas. Thus, in his Cambridge Colloquium on *Analysis situs*, Oswald Veblen summarized the history of the knot problem in the following words:

A large number of types of knots have been described by Tait and others and a list of references may be found in the Enzyklopädie article on *Analysis situs*. But a more important step towards developing a theory of knots was taken by M. Dehn, who introduced the notion of the group of the knot, which is essentially the group of the generalized three-dimensional complex [sic!]

<sup>57</sup> The notion of "power relations" should be understood here in a neutral and descriptive way, referring equally to the asymmetry of social relations in a community and to scientific leadership.

obtained by leaving out the knot from the three-dimensional space. Dehn gave a method for obtaining the group of a knot explicitly . . . [33, 150]

This passage suggests that by 1922 not only was the mathematical context of algebraic functions eliminated from knot theory but also that 10 years of an individual's efforts had been—for the time being—deleted from the collective memory of the mathematical community.

However, this was not quite the truth. As mentioned earlier, Reidemeister, Schreier, and Artin had attended Wirtinger's lectures in Vienna. In fact, there was a most intimate connection between the newly founded "Mathematisches Seminar" in Hamburg and the mathematicians in Vienna. Kurt Reidemeister, for example, went to Vienna following the recommendation of Hamburg's Wilhelm Blaschke, who himself had received most of his education in Vienna (in fact, Blaschke had written his dissertation under Wirtinger). It was there that Reidemeister decided to turn to knot theory, and a closer look at *his* mathematical contributions reveals that he absorbed Wirtinger's approach to knots. In fact, it was precisely this knowledge that enabled him to go a step further than Dehn by giving the first effectively computable knot invariants, the torsion numbers of *cyclic coverings of knot exteriors*. Interestingly enough, even this remnant of the original context which had led to the notion of a knot group was relegated to the last sections of Reidemeister's *Knotentheorie* of 1932. There, too, Reidemeister's new invariants were introduced in a purely combinatorial way, and their connection to covering spaces only appeared as a secondary, though interesting, interpretation.<sup>58</sup>

### CONCLUSION

Why was Wirtinger's work presented here in such detail? Certainly not to engage in priority debates. To ask whether or not Wirtinger was the "real father" of modern knot theory misses the point of the present study. Surely, it would be misguided to accuse Tietze or Dehn of having temporarily suppressed an interesting piece of mathematics. Rather, my intention has been to re-contextualize early work in modern knot theory. Knot theory was neither an invention out of thin air nor an application of general topological notions to a particular problem.<sup>59</sup> It emerged as part of a gradual process of differentiation out of one of the mainstream disciplines of 19th-century mathematical research, the theory of algebraic functions. With regard to *that* process, Wirtinger was the key figure. Moreover, the status of knot theory as a separate subfield of the new discipline of topology was attained only after an elimination of the context which originally served to legitimize Wirtinger's project. (Actually, algebraic function theory was not the only context of 19th-century work on the knot problem which moved into the background during the constitution of modern knot theory. Another was the *physical* context, from which Tait had derived his justification for tabulating knots.)

An attempt to redescribe the invention of a mathematical theory or discipline

<sup>58</sup> I hope soon to present a study of this period in the formation of modern knot theory.

<sup>59</sup> As such, it appears in Dieudonné's presentation [41, 307–310].

as a process of differentiation and/or context-elimination should not be viewed as a reassessment of the achievements of the pioneering figures. Rather, it places these achievements in a different light by exhibiting some of the causal links between mathematical life before and after the disciplinary threshold has been reached. Moreover, this approach may direct historians' attention to that aspect of mathematical culture which is responsible for most of its deeper changes, the domain of the norms and values guiding mathematical research, including the field of power constellations connected with the normative structure of every community. I hope to have shown that, in the case of early modern knot theory, this domain had an influence not only on the role and status of mathematical knowledge in scientific culture as a whole, as has often been discussed, but also on the internal constitution of the body of knowledge itself. When the complex of problems within the theory of algebraic functions which had led Wirtinger to his topological investigation of the complement of certain knots eventually differentiated into a new topological (sub-)discipline, centered around a combinatorial treatment of the knot group, this reflected a shift in the relative valuations of mathematical problems and theory-constructing techniques. The context-elimination which is so evident in Dehn's and Reidemeister's later work was the consequence of a series of normative decisions, decisions which departed from a Kleinian view of mathematics and reinforced a "modernist" view of mathematics as a complex of more or less self-contained, axiomatized theories. Moreover, these decisions were not taken in a social vacuum, but in a social space structured by complex normative horizons and power relations.<sup>60</sup>

#### APPENDIX

This appendix presents the complete text of Wirtinger's letter to Klein, dated 22.12.1895. The letter is contained in the Klein Nachlass in Göttingen, filed in Cod. Ms. Klein XII, 391. Orthography and punctuation are left unchanged.

Innsbruck, 22./XII 1895.

Hochgeehrter Herr Collega!

Diesen Brief muss ich mit einer Entschuldigung einleiten. Ich habe nämlich bis jetzt gehofft, das Manuscript der Arbeit über Differentialgleichungen bis Neujahr fertigstellen zu können, nun aber sehe ich, dass es doch nicht geht. Der Entwurf, ziemlich ausgearbeitet, ist fertig. Die Schlussredaction werde ich hoffentlich im Jänner fertigmachen können. Vorlesungen, ein längeres Unwohlsein, Prüfungen u. was derlei schöne Dinge mehr sind haben mich nicht dazu kommen lassen. Vergebliche Versuche diese und jene berührte Frage weiter zu führen als es

<sup>60</sup> I leave it to the reader to draw the obvious connections from my narrative to Herbert Mehrtens' discussion of the opposition between what he calls "modern" and "counter-modern" trends in mathematical practice around the turn of the century. One could even go further and speculate whether the shift from an *integrative* standard of mathematical rationality as expressed by Klein toward a *differentiative* standard as implicit in (at least the reception of) Hilbert's *Grundlagen der Geometrie* reflects the general trend of modern culture toward differentiation noticed by Max Weber and others. For further information on Klein, Hilbert and general matters, I refer to work by Herbert Mehrtens [52] and David Rowe [57]. It will be clear that my perspective on the developments presented in this article owes much to both of them, a debt which I gratefully acknowledge.

geschehen ist haben auch ihren Anteil daran. Nun aber ist es wenigstens soweit, dass ich nur mehr die letzte Redaction vor mir habe.

Die Functionen mehrerer Variablen sind auch wenig vorgerückt. Aber einiges habe ich mir doch klar gemacht. Um Directiven für die allgemeinen Fragen zu gewinnen, bin ich daran gegangen, mir das Verhalten gewöhnlicher algebraischer Functionen längs Verzweigungsmannigfaltigkeiten genauer auszudenken. Mein Ziel ist dabei zu erweisen, dass man ein beliebiges System algebraischer Gebilde von  $n - 1$  Dimensionen mit zugehörigem Verzweigungsschema immer als System von Verzweigungsmannigfaltigkeiten einer Function von  $n$  Variablen auffassen kann. Das gibt verschiedene algebraische und transcendente Formulierungen, die erst fassbar werden, wenn ich über die singulären Stellen eine deutliche Vorstellung gewonnen habe. Der Kern der ganzen Sache ist mir nun ziemlich deutlich. Bei einer Variablen liegt die Frage deshalb so einfach, weil in der Nähe eines Verzweigungspunktes die cyclischen Functionen der Werte eindeutig werden, also die  $n$ -te Wurzel der Variablen selbst eindeutig ist. In dieser Fassung liegt der Keim der Verallgemeinerung.

Das cyclische Verhalten bleibt aufrecht für beliebige Verzweigungsmannigfaltigkeiten die durch Nullsetzen einer Potenzreihe mit Gliedern erster Ordnung gewonnen werden. Mit Hilfe von  $P^{1/\nu}$  kann man dann alle Functionen darstellen. Sind mehrere solche  $P$  an einer Stelle Null, so schadet das nichts. Anstelle des Schemas der Auflösung der einfachen Abel'schen Gleichungen tritt dann nur das Schema der 'mehrfältigen' Abel'schen Gleichungen.

Ganz anders ist es aber wenn  $P$  keine Glieder erster Ordnung hat u. auch nicht zerlegbar ist in Reihen mit Gliedern erster Ordnung. Dann ist nämlich jede Galoissche Gruppe möglich. Und eben diese Gruppe charakterisiert den Verzweigungspunkt. Beispielsweise  $z^3 + 3zx + 2y = 0 - z$  als Function von  $x$  u.  $y$ —hat in der Nähe der Stelle 0 nur die symmetrische Gruppe.

Eine Darstellung aller Functionen, die auf der gegebenen Mannigfaltigkeit in der Umgebung einer Stelle eindeutig sind ist dann möglich durch eine Wurzel der Galoisschen Resolvente, u. diese Wurzel ist es, welche an die Stelle von  $x^{1/\nu}$  in der Ebene tritt. Der Rationalitätsbereich ist hier der aller convergenten Potenzreihen von  $n$  Variablen.

Es ist im allgemeinen nicht möglich, die Umgebung einer solchen Stelle stetig auf ein einfach zusammenhängendes Raumstück von  $n$  Dimensionen abzubilden, sondern die Umgebung einer solchen Stelle hat selbst einen gewissen Zusammenhang!<sup>61</sup>

Aus diesem Grunde halte ich eine allgemeine, überall bestimmte, Parameterdarstellung des Gebildes in der Nähe der betrachteten Stelle, welche ausserdem jede Stelle nur einmal liefert, für unmöglich. Dagegen können sehr wohl Darstellungen möglich sein, welche das Gebilde mehrfach überdecken, oder ausserwesentliche Singularitäten aufweisen. In wie fern man vielleicht die letzteren durch Benutzung homogener Variablen unschädlich machen kann ist mir noch nicht ganz klar, ich setze aber nicht viel Hoffnung darauf.

Der Kern der ganzen Sache liegt jetzt für mich in der Eruirung der Gruppe eines Verzweigungspunktes, also eigentlich in einer Irreduzibilitätsfrage im Gebiete der Potenzreihen einerseits, andererseits in der Frage: Kann man diese Gruppe willkürlich vorgeben, oder ist sie an Bedingungen gebunden, damit zugehörige Functionen existieren?

Das sind die Fragen und Gesichtspunkte, die ich nun genauer durcharbeiten will.

Mit meiner Lehrthätigkeit in Innsbruck hat es eine eigene Bewandniss. Der Menschenschlag ist hier sehr zäh, fleissig aber ungemein widerstandsfähig gegen jeden Versuch den Gesichtskreis zu erweitern. Die Vorbildung lässt viel zu wünschen übrig. Ich musste z.B. letzthin 3 Seminarstunden darauf verwenden, um die Elemente der Kettenbrüche vorzuführen, u. das Leuten aus dem VII u. VIII Semester! Von Geometrie will ich gar nicht reden, wurde ich doch letzthin angegangen—von den nämlichen Semestern—zu erklären was eine *Involution* ist!! Da können Sie denken, wie viel Geduld ein auch nur geringer Lehrerfolg braucht.

<sup>61</sup> Here Wirtinger amended "Zusammenhangszahl" into "Zusammenhang," showing that he was aware of the nontrivial topological situation he was considering.

Nun aber wünsche ich Ihnen und dem mathematischen Lexikon besten Erfolg im neuen Jahr. Für mich war das vergangene Jahr nach aussen hin ein Glücksjahr u. das verdanke ich zum grösstentheil Ihnen. Lassen Sie mich also nochmals recht herzlich für Ihre Freundschaft u. Förderung danken u. bewahren Sie mir dieselbe auch im kommenden Jahr, wo ich Sie in Frankfurt a.M. wieder zu sehen hoffe.

Die Herren Hilbert, Burkhardt, Sommerfeld bitte ich vielleicht bei Gelegenheit von mir zu grüssen.

Mit den herzlichsten Wünschen für Ihre Gesundheit verbleibe ich

Ihr dankbar ergebener  
Wirtinger

## REFERENCES

### *Published Sources*

1. Emil Artin, Theorie der Zöpfe, *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität* **4** (1925), 47–72.
2. Emil Artin, Zur Isotopie zweidimensionaler Flächen im  $\mathbb{R}_4$ , *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität* **4** (1925), 174–177.
3. Karl Brauner, Zur Geometrie der Funktionen zweier komplexer Veränderlicher, *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität* **6** (1928), 1–55.
4. Max Dehn and Poul Heegaard, Analysis situs, in *Enzyklopädie der mathematischen Wissenschaften III*, AB 3, Leipzig: Teubner Verlag, 1907, pp. 153–220.
5. Max Dehn, “Über die Topologie des dreidimensionalen Raumes, *Mathematische Annalen* **69** (1910), 137–168.
6. Max Dehn, “Über unendliche diskontinuierliche Gruppen, *Mathematische Annalen* **71** (1911), 116–144.
7. Max Dehn, Transformation der Kurven auf zweiseitigen Flächen, *Mathematische Annalen* **72** (1912), 413–421.
8. Max Dehn, Die beiden Kleeblattschlingen, *Mathematische Annalen* **75** (1914), 1–12.
9. Max Dehn, Die Grundlegung der Geometrie in historischer Entwicklung, appendix to M. Pasch, *Vorlesungen über neuere Geometrie*, Berlin: Springer-Verlag, 1926.
10. Poul Heegaard, *Forstudier til en topologisk teori for de algebraiske fladers sammenhaeng*, Copenhagen, Det Nordiske Forlag, 1898.
11. Charles Hermite, Sur les fonctions algébriques, *Comptes rendus* **32** (1851), 458–461.
12. David Hilbert, *Grundlagen der Geometrie*, Leipzig: Teubner Verlag, 1899.
13. Adolf Hurwitz, Über Riemannsche Flächen mit gegebenen Verzweigungspunkten, *Mathematische Annalen* **39** (1891), 1–61.
14. Vaughan F. R. Jones, A Polynomial Invariant for Knots via von Neumann Algebras, *Bulletin of the American Mathematical Society* **12** (1985), 103–111.
15. Camille Jordan, *Traité des substitutions et des équations algébriques*, Paris: Gauthier-Villars, 1870.
16. Felix Klein, *Ueber Riemann's Theorie der algebraischen Functionen und ihrer Integrale*, Leipzig: Teubner Verlag, 1882.
17. Felix Klein, Über Arithmetisierung der Mathematik, reprinted in *Gesammelte mathematische Abhandlungen*, 3 vols., Berlin: Springer-Verlag, 1921–1923, 2: 232–240.
18. Felix Klein, *Vorlesungen über die Entwicklung der Mathematik in 19. Jahrhundert*, Teil 1, Berlin: Springer-Verlag, 1926.
19. Johann Benedikt Listing, *Vorstudien zur Topologie*, Göttinger Studien, 1847.
20. Emile Picard, Mémoire sur la théorie des fonctions algébriques de deux variables, *Journal des mathématiques pures et appliquées (IV)* **5** (1889), 135–319.

21. Emile Picard and Georges Simart, *Théorie des fonctions algébriques de deux variables indépendantes*, Vol. I, Paris: Gauthier–Villars, 1897.
22. Henri Poincaré, Sur les fonctions de deux variables, *Acta Mathematica* **2** (1883), 97–113.
23. Henri Poincaré, Sur l'Analysis situs, *Comptes rendus* **115** (1892), 633–636.
24. Henri Poincaré, Analysis situs, *Journal de l'École Polytechnique* **1** (1895), 1–121.
25. Henri Poincaré, Sur les propriétés du potentiel et sur les fonctions Abéliennes, *Acta Mathematica* **22** (1898), 89–178.
26. Henri Poincaré, Complément à l'Analysis situs, *Rendiconti del Circolo matematico di Palermo* **13** (1899), 285–343.
27. Victor Puiseux, Recherches sur les fonctions algébriques, *Journal des mathématiques pures et appliquées* (I) **15** (1850), 365–480.
28. Kurt Reidemeister, Fundamentalgruppe und Überlagerungsräume, *Nachrichten der Akademie der Wissenschaften Göttingen, Mathematisch–Physikalische Klasse* **II** (1928), 69–76.
29. Kurt Reidemeister, *Knotentheorie*, Berlin: Springer-Verlag, 1932.
30. Herbert Seifert and William Threlfall, *Lehrbuch der Topologie*, Leipzig: Teubner Verlag, 1934.
31. Heinrich Tietze, Zur Analysis situs mehrdimensionaler Mannigfaltigkeiten, *Sitzungsberichte der Wiener Akademie* **115** (1906), 841–846.
32. Heinrich Tietze, “Über die topologischen Invarianten mehrdimensionaler Mannigfaltigkeiten, *Monatshefte für Mathematik und Physik* **19** (1908), 1–118.
33. Oswald Veblen, *The Cambridge Colloquium 1916, II. Analysis Situs*, American Mathematical Society Colloquium Lectures, vol. 5, New York: American Mathematical Society, 1922.
34. Wilhelm Wirtinger, Algebraische Funktionen und ihre Integrale, in *Enzyklopädie der mathematischen Wissenschaften II*, 2 B 2, Leipzig: Teubner Verlag, 1901, pp. 115–175.
35. Wilhelm Wirtinger, Über die Verzweigungen bei Funktionen von zwei Veränderlichen, *Jahresberichte der Deutschen Mathematiker-Vereinigung* **14** (1905), 517.
35. Wilhelm Wirtinger, Klein und die Mathematik der letzten 50 Jahre, *Die Naturwissenschaften* **7** (1919), 287–288.
36. Oscar Zariski, *Algebraic Surfaces*, Berlin: Springer-Verlag, 1935.

### Secondary Literature

37. Michael Atiyah, *The Geometry and Physics of Knots*, Cambridge: Univ. Press, 1990.
38. Maja Bollinger, Geschichtliche Entwicklung des Homologiebegriffs, *Archive for the History of Exact Sciences* **9** (1972), 94–170.
39. Egbert Brieskorn and Horst Knörrer, *Plane Algebraic Curves*, Basel: Birkhäuser, 1986.
40. Bruce Chandler and Wilhelm Magnus, *The History of Combinatorial Group Theory*, New York: Springer-Verlag, 1982.
41. Jean Dieudonné, *A History of Differential and Algebraic Topology, 1900–1960*, Boston: Birkhäuser, 1989.
42. Rudolf Einhorn, *Vertreter der Mathematik und Geometrie an den Wiener Hochschulen, 1900–1940*, Dissertation, Technische Universität Wien, 1983.
43. Moritz Epple, Styles of Argumentation in Late 19th-Century Geometry and the Structure of Mathematical Modernity, in “Analysis and Synthesis: History and Philosophy,” (Michael Otte and Marco Panza, Eds.), Dordrecht: Kluwer, to appear.
44. Moritz Epple, Mathematical Inventions: Poincaré on a “Wittgensteinian” Topic, preprint, Mainz, 1994, *Proceedings of the International Congress Henri Poincaré, Nancy, 1994*, to appear.
45. Ria vanden Eynde, Historical Evolution of the Concept of Homotopic Paths, *Archive for History of Exact Sciences* **45** (1992), 127–188.

46. Jürgen Habermas, *Theorie des kommunikativen Handelns*, 2 vols., Frankfurt-am-Main: Suhrkamp, 1981.
47. Pierre de la Harpe, Introduction to Knot and Link Polynomials, in *Fractals, Quasicrystals, Chaos, Knots and Algebraic Quantum Mechanics*, Proceedings Acquafredda di Maratea 1987, ed. A. Amann *et al.*, Dordrecht: Kluwer, 1988, pp. 233–263.
48. Gerald Holton, *The Scientific Imagination: Case Studies*, Cambridge: Univ. Press, 1978.
49. Imre Lakatos, History of Science and Its Rational Reconstructions, in *Method and Appraisal in the Physical Sciences*, ed. Colin Howson, Cambridge: Univ. Press, 1976, pp. 1–40.
50. Wilhelm Magnus, Braid Groups: A Survey, in *Proceedings of the 2nd International Conference on the Theory of Groups*, Lecture Notes in Mathematics, vol. 372. Berlin: Springer-Verlag, 1974, pp. 463–487.
51. Wilhelm Magnus, Max Dehn, *The Mathematical Intelligencer* **1** (1978), 132–142.
52. Herbert Mehrtens, *Moderne—Sprache—Mathematik*, Frankfurt-am-Main: Suhrkamp, 1990.
53. Robert K. Merton, Science, Technology and Society in Seventeenth-Century England, *Osiris* **4** (1938); reprint ed., New York: Howard Fertig, 1970.
54. John W. Milnor, *Singular Points of Complex Hypersurfaces*, Annals of Mathematics Studies, vol. 61, Princeton: Princeton Univ. Press, 1968.
55. Jozef Przytycki, History of Knot Theory from Vandermonde to Jones, *Aportaciones matemáticas comunicaciones* **11** (1992), 173–185.
56. John E. Reeve, A Summary of Results in the Topological Classification of Plane Algebroid Singularities, *Rendiconti del Seminario matematico de Torino* (1954), pp. 159–187.
57. David E. Rowe, Klein, Hilbert, and the Göttingen Mathematical Tradition, *Osiris (New Series)* **5** (1989), 186–213.
58. R. H. Silliman, William Thomson: Smoke Rings and Nineteenth-Century Atomism, *Isis* **54** (1963), 461–474.
59. Morven B. Thistlethwaite, Knot Tabulations and Related Topics, in *Aspects of Topology. In Memory of Hugh Dowker 1912–1982*, ed. I. M. James and E. H. Kronheimer, Cambridge: Univ. Press, 1985.
60. Klaus Volkert, *The Early History of Poincaré's Conjecture*, Heidelberg, 1994, preprint.
61. Max Weber, *Gesammelte Aufsätze zur Religionssoziologie*, 5th ed., 3 vols., Tübingen: Mohr (Siebeck), 1963.
62. André Weil, Riemann, Betti and the Birth of Topology, *Archive for History of Exact Sciences* **20** (1979), 91–96.
63. Hans Wussing, *Die Genesis des abstrakten Gruppenbegriffs*, Berlin: VEB Deutscher Verlag der Wissenschaften, 1969.