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Knot Invariants in Vienna and Princeton during the 1920s: Epistemic Configurations of Mathematical Research

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Argument

In 1926 and 1927, James W. Alexander and Kurt Reidemeister claimed to have made "the same" crucial breakthrough in a branch of modern topology which soon thereafter was called knot theory. A detailed comparison of the techniques and objects studied in these two roughly simultaneous episodes of mathematical research shows, however, that the two mathematicians worked in quite different mathematical traditions and that they drew on related, but distinctly different epistemic resources. These traditions and resources were local, not universal elements of mathematical culture. Even certain common features of the main publications such as their modernist, formal style of exposition can be explained by reference to particular constellations in the intellectual and professional environments of Alexander and Reidemeister. In order to analyze the role of such elements and constellations of mathematical research practice, a historiographical perspective is developed which emphasizes parallels with the recent historiography of experiment. In particular, a notion characterizing those "working units of scientific knowledge production" which Hans-Jörg Rheinberger has termed "experimental systems" in the case of empirical sciences proves helpful in understanding research episodes such as those bringing about modern knot theory.

1. Universal or local knowledge?

Recent history of science has taught in great detail that research practice in the experimental sciences is strongly bound to local environments. At least since the seventeenth century, even modestly advanced experiments involved a substantial amount of special apparatus whose manufacture and operation required special knowledge and skills not easily transmitted from one place or researcher to another.[1] As the scale of experimentation increased, the importance of local knowledge and skills was further emphasized. To build, to operate, and to make use of particle accelerators or biological *in vitro* systems is a highly nontrivial practical and intellectual matter

[1] Shapin and Schaffer 1985 has become a standard reference for these matters, see especially their discussion of Huygens's attempts to replicate Boyle's vacuum experiments. Many other studies also underline the importance of local traditions and environments in experimental science. Let just two be mentioned here: Buchwald 1994 and Sibum 1995.

that goes far beyond what can be written down in scientific articles, monographs, or textbooks. Rather than individuals performing single experiments we find groups of scientists working together in complex experimental setups that allow whole series of related experiments to be performed. The expertise required for working in such an experimental configuration typically involves both a variety of theoretical resources and technical skills relating to the particular apparatus used. This combination of theoretical and practical knowledge is usually tied to the particular experimental setup and the group of people working with it. The knowledge, skills, experimental setup, and group of experimenters form an "integral working unit of scientific knowledge production," an "experimental system," as Hans-Jörg Rheinberger puts it (Rheinberger 1997, 27–28). Such experimental systems have their time and place. To repeat similar series of experiments in other places or at other times requires rebuilding completely new experimental systems which will be different in many respects.[2]

On an epistemological level, the fact that many historical studies of experimentation have underlined the importance of local knowledge, traditions, and skills throws up a disturbing problem: if scientific knowledge is produced in highly local settings, how can it possibly attain the universality which is usually ascribed to it? Or is the outcome of an episode of experimental research not universal knowledge at all? A number of diverging answers have been given to this question, but the opinion remains divided.[3] From the point of view of studying the research practices of scientists, the baseline of these debates is that in order to transform knowledge gained in a local experimental setting into universal knowledge in any robust sense, further activities are required – activities of communication, of argument, of writing, of restructuring an existing knowledge corpus, etc.

It might seem that the history of mathematics has little to learn from this development within history of science. A lot of mathematical research is done without a large material apparatus operated by skilled personnel, without complicated experimental setups needing the help of special instrument-makers or operators. Moreover, mathematical ideas seem to represent an extreme kind of scientific object.[4] In the continuum between getting hands dirty with ordinary material things in, say, a chemical laboratory, and pure, abstract thought, mathematical ideas have their place close to one end. Equations and manifolds do not lie on workbenches, one feels tempted to say, they are objects of thought, described in a highly artificial but universal language. They are investigated using methods which themselves have to

[2] Some of these points have been pointed out forcefully in the classic study, Fleck 1935/1980. For more modern accounts, see in particular Pickering 1984, who also makes heavy use of the idea of theoretical resources of experimentation, and Rheinberger 1997. More general overviews of recent historiography of experiment can be found in the collections: Gooding et al. 1989, Buchwald 1995, Wise 1995, Heidelberger and Steinle 1998; see also the survey Hentschel 2000.

[3] See for instance Buchwald's commentary on a variety of approaches (Buchwald 1998).

[4] An interesting collection of attempts to describe the various roles and the historicity of scientific objects has been presented in Daston 2000.

fulfill strict standards of correctness and must be generally accepted. Therefore, one might conclude, precisely those aspects of scientific knowledge production that recent studies of experimentation have emphasized, its dependence on local, technical, as well as theoretical resources, might not play a significant role in mathematics. Due to the abstract nature of mathematical ideas, and due to the universal character of mathematical language, knowledge about mathematical objects seems to be intrinsically non-local. If a mathematical theorem has been correctly stated on the basis of sound definitions, and if it has been proved according to full standards of mathematical rigor, the theorem's validity has been established for all times and places.

Whatever the philosophical merits of this well-known image of mathematics may be, for historians it is a trap.[5] It suggests a bypass around some of the most interesting questions about the production of mathematical knowledge. How were mathematicians at particular places and times led to try out certain definitions or concepts? How did particular mathematical problems emerge? How was it possible to frame conjectures that might eventually become theorems? Which means of proof were available at particular places and times and how did mathematicians put them to use? How did they manage to convince others of the relevance of their definitions as well as of the correctness of their theorems and proofs? How did – in and by all these activities – perceptions of and ideas about particular mathematical notions or objects change? If such questions are seriously posed, the activities of mathematicians appear in a different light. Issues such as the specifics of the mathematical language used in a particular period and region, the possibilities offered and the limits imposed by particular conceptual frameworks or ways of imagination, the differences in proof strategies and standards of rigor, the mathematical and scientific contexts of particular problems, or the social and cultural setting of particular episodes of mathematical work move into focus.[6]

All these factors tend to "localize" the historiography of mathematics, without, however, shifting the perspective simply to traditional accounts of individual achievements or intellectual biographies. Many recent studies in the history of mathematics address the level between universal and individual aspects, the level of intellectual environments and knowledge traditions, of research agendas and research tools shared by relatively small groups of scientists in a particular place and/or period.[7] Precisely the same kinds of issues occupy the recent historiography of experiment

[5] This point seems to find growing acceptance in recent historiography of mathematics. For an earlier, related discussion, see e.g. Corry 1989 and Corry 1996, especially pages 3–5.

[6] If recent studies in the history of mathematics are paying attention to such issues more systematically, this is due to a number of factors. Among these, Sabetai Unguru's forceful criticism of algebraic reformulations of Greek geometrical arguments and his insistence on philological standards in the interpretation of past mathematical language still stands out as a landmark in demonstrating the importance of historical method to historians of mathematics.

[7] This is even true of some studies focusing on a single author such as Bos, who describes the development of Descartes' views of geometrical exactness in an intellectual environment structured by a variety of similar approaches (Bos 2001). While it would be difficult to list all contributions to this kind of "intermediate" historical analysis, there seem to be few studies that systematically address the development of particular local

(disregarding for a moment the material aspect of laboratory work). The present paper aims at exploring these parallels in some more detail. In particular, it attempts to outline what might correspond, in the case of mathematics, to those functional units of knowledge production which Rheinberger has called experimental systems.

One approach to a better understanding of the relations between experimental and mathematical knowledge production is to look at cases in which mathematical knowledge was indeed produced in close contact with an experimental science or, to put it differently, where mathematical work formed part of an experimental system in the proper sense. Such cases exist, in the physical sciences there are even many of them. Nevertheless, very few historical studies of experiment have seriously addressed the role(s) of mathematics in gathering and interpreting experimental data. In some experiments, even the connection between the raw data of an experimental setup with the empirical parameters of the physical theory involved cannot be established without using advanced mathematics in nontrivial new ways.[8] Another approach has been to consider areas of mathematical research which did indeed rely on material cultures. The most prominent such field is numerics, as far as calculating aids are developed and used. In a rather precise sense, these devices form "mathematical laboratories" for numerical mathematics, whose historical roles may be traced in similar ways as in experimental science.[9] In the twentieth century, other fields that rely heavily on this kind of mathematical laboratory include fluid dynamics, optics, and combinatorics. Somewhat less obvious is the case of computerized proving or, to mention an older example, the use of mathematical models in higher geometry (see Mackenzie 1999 and Fischer 1986).[10]

All these approaches have in common the belief that the interfaces between mathematical and experimental research widen the meaning of experimentation to include, e.g., manipulating calculating aids and computers. In the following a different route will be taken. I will consider episodes of mathematical research which are *not* related to experiment or material cultures in any direct sense, in fact episodes which are typical examples of "pure" mathematical research. I will argue that even in this case one can discern the "laboratories" of mathematical thought, laboratories in which local knowledge traditions and technical skills, specific research agendas and patterns of mathematical rationality are amalgamated into effective units of knowledge production.[11]

mathematical milieus. Exceptions include Rowe 1989 for the case of Klein's and Hilbert's Göttingen, and Warwick 2003 for nineteenth-century Cambridge.

[8] This is one of the main points in the interesting recent study, Sichau 2002 (see also Buchwald 1994 and Epple 2002).

[9] This is the perspective of Warwick 1995.

[10] In most of the areas mentioned above there is still much room for future historical studies.

[11] With a similar intention, but using a rather narrow analytical framework, Pickering and Stephanides 1992 and Pickering 1995, chap. 4, have described Hamilton's invention of quaternions as an interplay between various forms of theoretical "agencies." For another recent study trying to show that even "pure" mathematical research

In order to approach this goal, two closely related episodes of research are discussed. In the years 1926 and 1927, two mathematicians in two rather different intellectual environments claimed to have invented the "same" numerical invariants of knots and links, i.e., procedures by which certain numbers could be associated with the topological objects called knots or links such that two knots or links could only be equivalent in an appropriate sense if these numbers were the same. The procedures themselves were the same at least in the sense that the output data (numbers) were the same for the same input data (knots or links). These parallel results constituted a major breakthrough in the study of knots and links, effectively establishing knot theory as a substantial subfield of modern topology.

The two episodes serve well to test the argument of this essay. At first sight, they might seem to undermine its intentions. Are we not dealing with a typical instance of a double discovery, showing that differences of local environments or traditions do not matter in mathematics? The next section will present the two cases in detail, showing that as soon as one analyzes the mathematical activity that produced these mathematical achievements, significant differences between the two episodes appear, differences in mathematical approach which point to different local milieus.[12] In the third section, some further descriptive tools are introduced which will then be used in outlining a "synthesis" of the historical developments in the two places that led up to the results of 1926 (section 4). By then I hope to have shown that local knowledge traditions and technical skills were indeed essential ingredients of the research episodes under consideration. Some open ends of the discussion are summarized in the concluding section.[13]

2. Knot Invariants I: Analysis

If modern mathematics is taken to refer to the mathematical culture that gradually took shape in the decades before and after 1900,[14] then topology belongs among the most active fields of modern mathematics. The study of mathematical spaces which have certain continuity properties and the study of continuity properties of figures in such spaces not only represented a paradigmatic field for the formation of the new, abstract, and structural approach of modern mathematics. It also posed challenging problems

may have much in common with the style(s) of experimental science, see Goldstein 2001. She argues that in the practice of the seventeenth-century mathematician Frénicle de Bessy, the Baconian program of scientific method may have served as an alternative to a more traditional "Euclidean" model of doing mathematics.

[12] Other examples of this kind are described in Tinne Hoff Kjeldsen's contextual analysis of various theorems in nonlinear programming and convexity theory (Kjeldsen 2000; Kjeldsen 2001; Kjeldsen 2002).

[13] In its methodological emphasis as well as in its historical case studies, the present article draws heavily on material presented in Epple 1999a, see especially chapters 1, 10 and 11; see also Epple 2000.

[14] See the closing sections of any good standard textbook on history of mathematics. More sophisticated interpretations are proposed and discussed in Mehrtens 1990, Corry 1996, and Epple 1999a, chap. 7.1.

of a new kind. These problems ranged from conceptual clarifications (e.g., what kinds of spaces and figures should actually be considered?) to very concrete problems about particular topological objects (e.g., what are the topological characteristics of ordinary 3-dimensional space?). On both levels, mathematicians learned to conceive new research objects which could no longer be seized by ordinary intuition and which even went far beyond the imaginations of most nineteenth-century mathematicians. To address these problems (some of which still remain unsolved), a large number of new mathematical tools were forged that now pervade most of present mathematics.[15]

One of the earliest problem fields of topology, and at the same time one which was still fairly close to ordinary intuition, was related to knotted and linked curves in standard 3-dimensional space. As objects of scientific interest knots came up in various nineteenth-century contexts. Most important was the attempt of the Scottish natural philosopher Peter Guthrie Tait to list all different types of knotted, closed curves in ordinary space in the decade 1876–1885. The context of his work was William Thomson's speculation that material atoms might be indestructible knotted or linked vortex rings in a perfectly fluid ether. Tait worked out a substantial part of such a list but stopped his work when Thomson's speculation lost its attraction for physicists in the late 1880s. Moreover, it was quite clear to Tait and his contemporaries that his work was mathematically rather informal. Nevertheless, Tait found industrious followers who continued his tabulations to include more complicated knots.[16]

It was only in the 1920s that two mathematicians, James Weddell Alexander in Princeton and Kurt Reidemeister in Vienna and later in Königsberg, made a major breakthrough in dealing with the mathematics of knots. Their work opened a broad range of new research possibilities that allowed the creation of a modern mathematical theory of knots. The basic problem of this theory was still that of classifying different types of knots; their innovation consisted in the successful construction of algorithms for calculating numerical invariants that enabled one to distinguish effectively a large number of different knot types. The late 1920s and 1930s then saw the construction of several other calculable invariants of knots and links, whose scope and limitations were studied in great detail. A comparable activity in research on knot invariants can be witnessed only since the mid-1980s.[17]

For the following, the basic problem of knot classification must be reviewed in more mathematical detail, in the fashion of the 1920s. The common understanding at the time was that a (mathematical) knot should be conceived as a closed curve in Euclidean

[15] Literature on the history of topology is still dominated by the retrospective of members of the discipline, see e.g., the survey Dieudonné 1989 and the collection James 1999. Exceptions will be mentioned in some of the following notes.
[16] For a detailed historical analysis of Thomson's and Tait's enterprises, their scientific context, and the ensuing work on knots, see Epple 1998. A comprehensive survey of the theory of vortex atoms can be found in Kragh 2002.
[17] For some of this work, a Fields medal was awarded to Vaughan F. Jones in 1990. A brief sketch of various important topics in knot theory up to Jones' work is given in Epple 1999b.

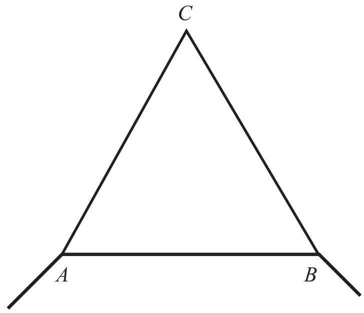

Fig. 1. Elementary deformations of knotted polygons.

3-dimensional space without self-intersections. In order to avoid certain topological difficulties, this understanding was refined (and restricted) by demanding that a knot is a closed polygon in three-dimensional Euclidean space with a finite number of sides and without self-intersections. By taking the number of sides very large, such a polygonal knot (or knotted polygon) could still be imagined to be close to a smooth closed curve in space. Intuitively, two knots could be considered as *equivalent* or *of the same type*, if one could smoothly be deformed into the other without cutting up the curve. In the framework of polygonal knots, this idea was rendered as follows (see fig. 1): let AB be one of the sides of a knotted polygon, and let C be a point in space such that the triangle ABC does not contain any point of the knot except the side AB. Then replace AB by the two sides AC, CB. This operation and its inverse are elementary deformations of a polygonal knot which do not change its type. Two knots were called equivalent or of the same type if and only of they could be deformed into each other by a finite sequence of such elementary deformations.

In the first monograph on the field, the task of knot theory was then stated as follows: "To give a survey of all properties of a knot, i.e., of all deformation invariants of a closed polygon without double points" (Reidemeister 1932, § 1).[18] In a trivial sense, to know all deformation invariants implies to be able to distinguish between all different types of knots, as the type of a knot (the class of knots equivalent to a given one, or the abstract entity imagined to represent this class) is obviously a deformation invariant itself. Another invariant that was known and studied to some extent before Reidemeister's and Alexander's contributions was the so-called group of a knot, the fundamental group (in Poincaré's sense) of the complement of a given knot in space. In 1910, Max Dehn had described a way for deriving a finite presentation of this group for any given knot (Dehn 1910; cf. also Epple 1995). As we shall see, another way was known as well. However, this invariant object was not much less obscure than the knot type itself. Two representatives of the same knot type would in general lead to

[18] Here and in the following, all translations from German are mine.

quite different presentations of the knot group, and no general procedure was known at the time that would have allowed a decision whether or not two such presentations defined isomorphic groups. This problem belonged to the core of another new area of mathematics, the area that came to be called combinatorial group theory.[19] For this reason, the knot group was not of much help for classifying knots.

The crucial problem was to find deformation invariants of knots that could more easily be dealt with and which could actually be calculated for any knot given in a simple manner. Such invariants were called calculable invariants by Reidemeister and Alexander. The central claim made by both was that they had found the first calculable invariants of polygonal knots. This claim was made in print first by Kurt Reidemeister in two papers entitled "Knoten und Gruppen" and "Elementare Begründung der Knotentheorie" in 1926 (hereafter, these texts will be denoted by *R1* and *R2*; *R* will refer to both). A year later, James W. Alexander and his student G. B. Briggs published an article "On types of knotted curves" in which "the same" invariants were discussed (hereafter, denoted by *A/B*). However, in the introductory passages *A/B* claimed that Alexander had calculated these invariants already in 1920 for certain knots, and that it was Reidemeister who had "rediscovered" them. By calculating the new invariants for the knots listed in Tait's and Kirkman's tables, Alexander and Briggs also showed that a substantial part of this informal classification of the late nineteenth century could be saved according to modern standards of rigor.

In order to make his priority claim, Alexander argued that the core of the two mathematical contributions in question was identical. It has already been mentioned that this was true of the algorithms in the following sense: the same input data produced the same output data.[20] The input data of the algorithms were plane diagrams of knots. Both *R2* and *A/B* gave an explanation of how polygonal knots could be represented by suitable plane projections of knots in space endowed with some marking convention for "over-" and "undercrossings." Elementary deformations of knots *in space* could then be replaced by elementary deformations *of diagrams*. Fig. 2 and fig. 3 show typical diagrams of both papers; fig. 4 illustrates the admissible elementary deformations of diagrams in the neighborhood of diagram crossings (obviously allowed changes of the diagrams that do not involve crossings also have to be taken into account).[21] *A/B* also used smooth diagrams in places where no misunderstanding could arise. A seemingly marginal difference between *A/B*'s and *R*'s description of the input data of their algorithms is precisely the marking convention for crossings. *A/B* use a system of dots

[19] For a historical account of combinatorial group theory, see Chandler-Magnus 1982.
[20] The importance of a detailed analysis of algorithmic properties of mathematical constructions for historical understanding has been underlined in Jim Ritter's studies of ancient mathematics (see e.g., Ritter 1995). One can also view the following analysis as an attempt to understand how certain algorithms (in some sense the end products of the episodes of mathematical research discussed here) were built, from what resources, and in what steps.
[21] The elementary deformations of fig. 4 are often named after Reidemeister in today's knot theoretical lingo.

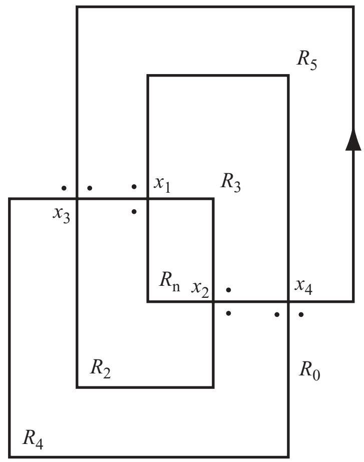

Fig. 2. A knot diagram from *A/B*.

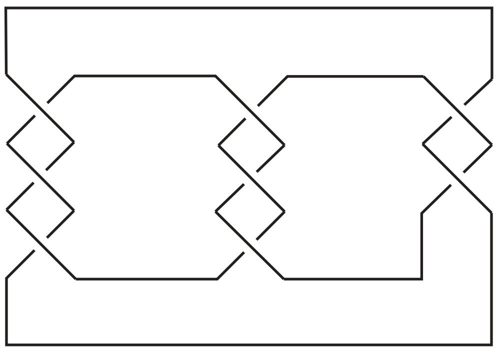

Fig. 3. A knot diagram from *R1*.

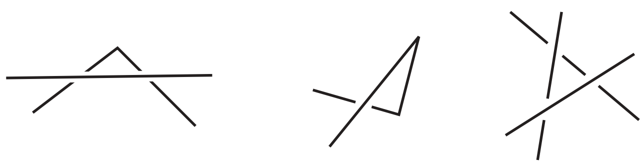

Fig. 4. Elementary local deformations of diagrams (from *R2*). In the diagram to the left, the arc above may be moved away from the arc below, in the middle the loop may be untwisted, and in the diagram to the right any arc may be moved beyond the crossing of the two others.

for determining which arc of a knot diagram lies over another: the knot is traversed in a fixed sense and whenever one arc overpasses another, the two corners to the right of the upper arc receive a dot. *R* simply used an intuitive convention of drawing broken lines for underpassing arcs.

Given a knot diagram (and thus a polygonal knot in space), both *R* and *A/B* showed their readers how one could write down certain matrices whose entries were integer numbers by following some simple rules. In fact, their papers allowed them to define one such matrix for any given knot diagram *D* and any integer *n*.[22] For convenience, these matrices will here be denoted by $R_n(D)$ and $A_n(D)$, respectively. Although defined in a similar fashion, $R_n(D)$ and $A_n(D)$ were in fact different matrices. Nevertheless, by means of standard procedures (explained neither in *A/B* nor in *R*) certain numbers could be calculated which were invariants of these matrices with respect to certain elementary modifications *of matrices*. In *A/B*, these numbers were called "characteristic invariants" or "torsion numbers," in *R*, they were called "Elementarteiler," "Torsionszahlen," or "Poincarésche Zahlen" of the matrix or of the knot. In this way, with each knot diagram *D* and each level *n*, a set of integers was associated, the set of elementary divisors not equal to 1 of either $R_n(D)$ or $A_n(D)$. Both *A/B* and *R* showed that the numbers in this set remained unchanged under elementary deformations of the underlying knot diagram. Consequently, they were knot invariants. Moreover, it did not matter whether they were calculated from $R_n(D)$ or from $A_n(D)$.

These sets of integers different from 1 were the output of the algorithm or, in the terms of the papers, the calculable invariants of the knot given by *D*. Of course, not the whole infinite sequence of invariants was actually calculated. *R* gave virtually no calculations at all. *A/B*, on the other hand, calculated the invariants of levels 2 and 3 for the 84 simplest knots in Tait's and Kirkman's tables. The authors found that using their invariants they could verify the nineteenth-century listings with very few exceptions. By this effort, a substantial part of the older, informal knowledge about knots was reconstructed in the modern paradigm. At the same time, the scope of the new methods was impressively demonstrated.

Mathematical meaning of the algorithms I: techniques

The above description – it would not be difficult to complete it by adding the precise rules for writing down the matrices $R_n(D)$ and $A_n(D)$, and it would only be a little more cumbersome to show that their elementary divisors are indeed invariant under elementary deformations of knot diagrams – treats *A/B*'s and *R*'s algorithms as black boxes. This description does not explain how these algorithms were found or constructed. Moreover, the account is not very faithful to the sources of 1926 and 1927. In fact, it is the result of a highly selective reading of *A/B*'s and *R*'s texts, a reading that

[22] While *A/B* made this explicit, *R* gave only one example for $n = 2$, see (*R2*, sect. 5). For higher *n*, the reader had to understand the definition of the corresponding matrix from other parts of the text. A full description was given in Reidemeister 1932.

only extracts the information necessary for calculating the new invariants.[23] From a historiographical point of view, therefore, our initial account of "the first calculable knot invariants" is defective. Its two most obvious defects – black-boxing and unfaithfulness to the sources – are linked. In order to explain the algorithms to their readers and in order to prove that these algorithms did in fact calculate knot invariants, *A/B* and *R* outlined technical frameworks that went substantially beyond the above summary. In this way, the algorithms were placed in mathematical contexts. As we shall see, *A/B* and *R* chose different technical frameworks for introducing their knot invariants. To some extent, these frameworks reveal information about the construction of the algorithms, and to that extent, the sources permit a look inside the black boxes.[24] The next step in our analysis thus consists in a sketch of the technical frameworks in which Reidemeister and Alexander presented their algorithms.

Reidemeister placed his version of the new knot invariants in the context of combinatorial group theory. The first of his two papers begins with a brief discussion of the difficulties involved in the study of the group of a knot (*R1*, 7). Then he summarized the basics of group definitions by means of finite presentations. He thus chose a mathematical technique which may properly be called a purely symbolical technique: groups were characterized as particular systems of signs ("words" built from certain basic, uninterpreted signs, the "generators") and operation rules (for manipulating "words," codified in the basic group laws and the "relations" defining a particular group). Within this framework, Reidemeister went on to present a combinatorial method for deriving finite presentations of certain subgroups of finitely presented groups. The method could be applied in situations in which a homomorphism from a given, finitely presented group $\mathfrak{F}$ onto a finite group $\mathfrak{G}$ was known (in the sense that the images of all generators of $\mathfrak{F}$ were known). In this situation, a finite presentation of "the normal subgroup $\mathfrak{g}$ of $\mathfrak{F}$ can be given which has $\mathfrak{G}$ as factor group" (*R1*, sect. 3).[25]

This method was then applied to the group of a knot (called $\mathfrak{K}$ by Reidemeister). To this end, Reidemeister "recalled" a definition of this group by means of a finite group presentation that could be read off from a knot diagram (*R1*, sect. 2.2.). The reference was to a paper by Emil Artin and Otto Schreier on the braid group that had been published a year earlier; but in a footnote Reidemeister mentioned that this definition was originally due to the Viennese mathematician Wilhelm Wirtinger (Artin 1925 and *R1*, 15 n. 1). From Wirtinger's presentation it followed (by reading the defining

[23] This reading is only slightly anachronistic. It was a possible "mathematician's reading" (to use Catherine Goldstein's term) right after the publication of the two papers. Except for the notation, it corresponds closely to the presentation of the same material in Reidemeister's monograph of 1932. For a discussion of the operations of reading mathematical texts, essential both in mathematical and historiographical practice, see Goldstein 1995.
[24] The metaphor of black-boxing or of unpacking black boxes by means of historical analysis has, of course, become a standard topos of recent history of science (see e.g., Latour 1987).
[25] After some modifications by Otto Schreier, the method became known as the Reidemeister-Schreier method in combinatorial group theory (see Chandler and Magnus 1982, chap. II.3).

relations as defining relations of a commutative group) that the quotient of the knot group and its commutator subgroup was the additive group of the integers. Therefore, every finite cyclic group was in a canonical way the homomorphic image of the group of a knot. Applying his general method, Reidemeister could thus derive, for any integer g, a presentation of the normal subgroup of the knot group which had the cyclic group of order g as its factor group (in Reidemeister's notation, this subgroup was denoted by $\mathfrak{K}_g$; for convenience, I will write $\mathfrak{K}_g(D)$ in order to express the dependence of the defining group presentation on a knot diagram D). The presentation matrices[26] of the groups $\mathfrak{K}_g(D)$ were R's matrices $R_g(D)$, hence the "torsion numbers" of the knot given by D were the "torsion numbers" or "Poincaréan numbers" of these groups in the sense of combinatorial group theory.

The technical setting in which Alexander and Briggs introduced their version of calculable knot invariants was similar in style but different in content. Rather than working with the symbolic combinatorics of finitely presented groups, *A/B* introduced "linear systems" and "homologies" of symbolic expressions (*A/B*, sect. 5).[27] A "linear system" X was defined to be the set of all finite linear combinations of certain uninterpreted "marks" $x_1, \ldots, x_m$ with integer coefficients. If

$$y_s = \sum_{i=1}^{m} \varepsilon_{si} x_i, \qquad s = 1, \ldots, k, \tag{*}$$

was a system of k elements of X with coefficients ε_{si}, the set of all finite linear combinations of the y_s defined a subsystem Y of X. Two elements x, x' in X were then defined to be "homologous" (mod Y), in signs $x \sim x'$, if and only if the difference $x - x'$ was in Y. The homologies $y_s \sim 0$ were called the "fundamental homologies" defining Y. The set Z of all homology classes in X was then again a "linear domain," and *A/B* reminded their readers that this domain was "completely characterized" by the number and values of the elementary divisors greater than 1 of the coefficient matrix (ε_{si}) defining Y.

This machinery was then applied to knot diagrams as follows (*A/B*, sect. 6). After choosing a fixed integer n, with each of the ν crossings of a given knot diagram D, n uninterpreted marks were associated, defining a linear system X with a total of νn base marks. Then for each region of the diagram, n fundamental homologies defining a subsystem Y were written down, involving the marks associated with the corners of this region and coefficients ε_{si} depending on the dots distributed in these corners in order to mark over- and undercrossings (see above). The matrix denoted above by $A_n(D)$ was nothing but the resulting matrix of coefficients (ε_{si}), and the "torsion numbers" of the knot given by D were just its elementary divisors greater than 1. In

[26] In the presentation matrix (c_{ij}) of a group presented by generators $a_1, \ldots, a_m$ and relations $r_1, \ldots, r_n$, entry c_{ij} is the sum of the exponents of all occurrences of a_i in relation r_j.

[27] For a semiotic analysis of similar texts of Alexander on linear systems, see Herreman 1997.

A/B's framework, these numbers characterized the domain of homology classes in X modulo Y.

Comparing the technical frameworks in which *R* and *A/B* introduced their numerical invariants of knots, both similarities and differences appear. Both papers outlined more general techniques of abstract, symbolic mathematics, and both connected these techniques with knot diagrams. However, the techniques themselves were quite different. While *R* used combinatorial group theory, *A/B* employed "homologies" of "linear systems." A further similarity is that certain key words of two crucial mathematical techniques introduced in Poincaré's ground-breaking papers on *Analysis situs* or topology appear in both papers.[28] The knot group was referred to as the fundamental group of the complement of a knot in 3-dimensional space, "homologies" and "torsion numbers" or "Poincaréan numbers" of course referred to Poincaré's homological invariants of manifolds. Already at this point one may guess that *A/B* and *R* each preferred one of these two techniques over the other.

However, the connection to Poincaré's work is not (yet) very clear. There is a significant difference in style: so far, neither *A/B* nor *R* spoke of *manifolds*, i.e., of those mathematical objects for whose treatment Poincaré had developed his techniques. Reidemeister's groups were just introduced by means of presentations; similarly, Alexander's and Briggs' homologies were introduced as purely symbolic devices. To some extent, therefore, we have opened the black box of *A/B*'s and *R*'s knot invariants, and to some extent, it still remains closed. We can see how the authors made specific uses of known techniques in the definition of their invariants. We can also see a similar preference for writing mathematical articles in terms of formal, combinatorial mathematics. But the techniques themselves, and the writing style, still appear to fall from mathematical heaven. How could Poincaré's original techniques of manifold topology be modified and adapted to the new field of knots given by diagrams? How should we explain the particular formal style of *A/B*'s and *R*'s papers?

Mathematical meaning of the algorithms II: objects

A partial solution to this little riddle can be found by looking for the ingredients of Poincaré's manifold topology that are missing from the accounts of the knot theorists, as far as we have described them up to this point. Clearly, the crucial missing items are geometric objects to which the techniques of the fundamental group or of homology could be applied, i.e., manifolds. As several detailed studies have shown, Poincaré's own perspective on manifolds was changing between a number of different approaches, most of which were technically vague to a greater or lesser degree.[29] To the extent to

[28] On Poincaré's topological work, see e.g., Scholz 1980, Dieudonné 1989, Herreman 1997, Sarkaria 1999, and Volkert 2002.

[29] See the literature cited in the previous note and Epple 1999a, chap. 7.2.

which Poincaré wanted to "calculate" topological invariants of a manifold, however, he developed a clear preference for describing manifolds by means of cell decompositions. Based on the general idea of viewing a manifold as composed of simple, bounded patches of space of different dimensions, attached to each other in a well-determined fashion, this kind of description was vague in itself, a vagueness that gave rise to the long-winded history of (cell) complexes in topology.[30] Nevertheless, Poincaré's papers sketched workable methods for deriving presentations of the fundamental group of a manifold and of the ("reduced") homologies of a manifold, if this manifold was given by a suitable kind of cell decomposition.[31]

Comparing these Poincaréan methods and the work of *A/B* and *R* one finds that whereas *A/B* and *R* proceeded by introducing machineries of uninterpreted symbols, Poincaré developed his methods in a highly intuitive, geometric fashion. In his papers, the "algebraic" symbols used had a clear geometric meaning. The generators of his presentations of fundamental groups represented (classes of) so-called fundamental paths in manifolds, while the defining relations were associated with 1-dimensional cells in a given cell decomposition of a manifold. Similarly, his (1-dimensional) homologies were expressing bounding relations between 1-dimensional and 2-dimensional cells.

Did the topologists of the 1920s disregard this geometric content, retaining just the symbolical tools in order to use them in new, more general, maybe more abstract situations? If so, Alexander's and Reidemeister's work would have involved a substantial amount of ingenious guesswork since using Poincaré's tools in the intended way required a geometric interpretation of the symbols. To be able to apply the methods in a particular situation meant to understand the way in which some manifold was built up from simple cells.[32] Another look into the sources reveals, however, that despite all symbolic methods geometric objects were present, if a little hidden, in the knot theorists' papers. In surprisingly parallel fashion, manifolds and cell decompositions were introduced as a means of "interpreting" the more formal definition of knot invariants. A suspicion arises whether these apparently secondary "interpretations" had not been, in fact, the decisive clues that had allowed the construction of knot invariants in the first place.

Let us begin by considering *R*. Just after introducing the crucial groups $\mathfrak{K}_g(D)$ (whose Poincaréan numbers were *R*'s knot invariants) a section was added entitled "Geometrische Deutungen" (1926a, sect. 3). At its beginning, Reidemeister referred back to his introduction of the knot group as a fundamental group of the knot complement. As mentioned above, he had taken this introduction essentially from

[30] A good historical account of this interesting history of a crucial mathematical tool is missing. For treatments by mathematicians, see Dieudonné 1989, Burde and Zieschang 1999.
[31] A brief description of these methods is given in Epple 1999a, § 70.
[32] Of course one could try to avoid geometrical interpretations by turning the whole process of forming a cell complex into a purely formal procedure. This was attempted by some of the early readers of Poincaré, such as Dehn and Veblen. See Herreman 1997 for a discussion of some of the steps toward a formalization of homology.

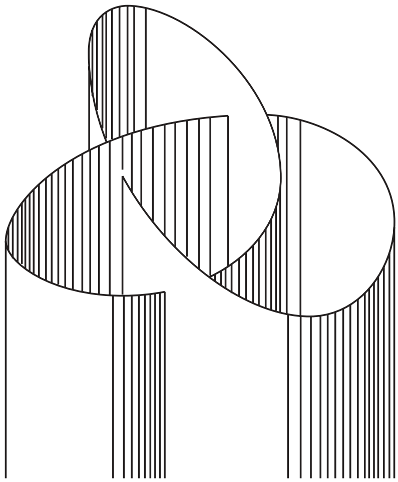

Fig. 5. Wirtinger's semi-cylinder (printed in Artin 1925, 58).

Artin and Schreier, indicating that it was even going back to Wirtinger. In Wirtinger's construction, a picture of a way of cutting up the complement of a given knot played the crucial role (see fig. 5).

Imagining the knot in space, in a position close to a plane onto which its diagram could be projected (roughly horizontally in the figure, not drawn), Wirtinger (and Artin and Reidemeister) considered a self-intersecting semi-cylinder Z bounded by the knot and generated by parallel rays orthogonal to the projection plane.[33] This cylinder was again subdivided into as many parts as the diagram had arcs (in the figure, the three surface parts, each of which is bounded by two lines of self-intersection of the cylinder and one knot arc). The (simply connected) complement of the cylinder, these (simply connected) surface parts, the lines of self-intersection of the cylinder, and (possibly) a point at infinity could then be viewed as a cell decomposition of the knot complement. What *R* did not mention was the fact that Wirtinger's presentation of the knot group was nothing but the group presentation obtained by applying Poincaré's method to this cell decomposition.[34]

In order to interpret the groups $\mathfrak{K}_g(D)$, Reidemeister extended Wirtinger's construction. Another, somewhat more complex geometric object was introduced:

[33] I owe an apology to all those who have heard me emphasizing the crucial role of this "object," or more precisely, of this imagination in the history of knot theory for about ten years now. A comparison of the present arguments with my first discussion of it in print (Epple 1995) will show that it played quite different roles in this history. For more information, see section 4 below. The present paper may also be read as an explanation of why imaginations or objects of this kind matter in the history of mathematics.

[34] Considering the knot as a 1-dimensional polygon or curve, the knot complement is an open manifold and thus not exactly what Poincaré had considered. In Reidemeister's practice, however, this made no big difference. The Poincaréan methods could easily be stretched to apply in this slightly different situation.

> Basically the same illustration [*Veranschaulichung*] is to cut up space along the semi-cylinder Z and to attach g copies $R_1, R_2, \ldots, R_g$ of this space to each other along Z, in such a way that starting from R_1 and passing once through Z one reaches R_2, after surrounding the knot and a further passage through Z one gets to R_3, and so on to $R_4, \ldots, R_g$, but from there back again to R_1. $\mathfrak{K}_g$ is the fundamental group of this g times covered exterior space, A_g. (*R1*, 16)

This new space A_g, the g-fold cyclic covering of the complement of a knot, described here in very intuitive terms, is the geometric object at the core of *R*'s argument. In a canonical fashion, Wirtinger's decomposition of the knot complement gives rise to a cell decomposition of the (open) manifold A_g. Applying Poincaré's technique for presenting the fundamental group of A_g to this cell decomposition produces exactly the group presentation of $\mathfrak{K}_g$ used in Reidemeister's paper.

It is hard to believe that this geometric interpretation was but an afterthought to an otherwise formal manipulation of symbolic machinery. In the passages of *R* discussing these things, several historical references interfere with mathematical argument. These references point to a much earlier stage of research – in fact Wirtinger had been working on his construction before 1904. He had even considered certain covering spaces (see section 4 below). The most important point, however, is that the construction of knot invariants can easily be understood as a product of a meaningful research activity if it was derived in this way – by applying known, Poincaréan techniques to an object, A_g, that was very close (to say the least) to objects also known beforehand.

In the case of *A/B*, the situation is even clearer. Toward the end of the paper, after the full presentation of their version of knot invariants, the authors remarked: "Before bringing the discussion to a close, we shall indicate how the torsion numbers of a knot K, for any given determination of the integer n, may be interpreted in terms of the Betti numbers and coefficients of torsion of an n-sheeted covering J_n with a branch curve of order n-1 covering the knot" (*A/B*, sect. 10). What A_g was for Reidemeister, J_n was for Alexander and Briggs. In a very detailed manner, *A/B* described a "cellular subdivision" adapted to a given knot, first of the 3-dimensional sphere, then of its branched, cyclic covering space J_n. This cell decomposition was then modified, reducing it to a cell decomposition with fewer cells but with the same homological invariants in the Poincaréan sense. The (1-dimensional, reduced) homologies of this modified cell decomposition of J_n, calculated according to Poincaré's method, led to exactly those homologies (*) which were codified in the earlier parts of the paper. Even the rather peculiar dotting convention by which *A/B* marked over- and undercrossings in knot diagrams acquired a new meaning: the system of dots encoded information about the way in which the different layers of the covering space were attached to each other in the cell decomposition used for calculating homologies.

Hence the uninterpreted marks of *A/B*'s approach to knot invariants actually had an interpretation, in fact a strikingly coherent one. As in *R*, the introduction of the covering space reveals what probably was at the core of Alexander's and Briggs' research activity. Taking it into account, a smooth order of reasoning becomes visible that essentially reverses the order in which the authors had arranged their material in writing. The text even said as much:

> A number of years ago, one of the authors of the present paper pointed out that if the space of a knotted curve be covered by an *n*-sheeted "Riemann 3-spread" (the three-dimensional analogue of a Riemann surface) with a branch curve of order *n-1* covering the knot itself, then, the topological invariants of the covering spread will also be invariants of the knot. He further calculated the Betti numbers and coefficients of torsion of the covering spreads determined by some of the simpler knots and found these invariants to be sufficient, in the cases actually examined, to distinguish one type of knot from another. (*A/B*, sect. 1)

In this tiny story, the formal, combinatorial definition of torsion invariants of knots as presented in *A/B* does not occur at all. Despite forming the content of the first sections of the paper, in practice the formal perspective obviously came last. After making its priority claim the text continues:

> In this paper, we propose, first of all, to obtain the torsion invariants of a knot by direct, elementary considerations, without appealing to the idea of a Riemann covering spread. Next, we shall prove, with the aid of these invariants alone, that all types of knots of eight or less crossings listed as distinct in the knot tables of Tait and Kirkman actually are distinct . . . Finally we shall describe, briefly, the method of obtaining the torsion numbers of a knot from its associated Riemann covering spread, after the manner in which the invariants were originally discovered. (*A/B*, sect. 1)

Hence the written order of the final paper actually reversed the temporal order of events in research practice. I feel tempted to convert this reversion into a historiographical principle (to be taken with a grain of salt): for a historical reconstruction of successive events in mathematical research, it may help to read the papers documenting the results from end to beginning rather than in the usual way. The streamlined and polished approach to a mathematical problem which is offered at the beginning of a final paper may have been the last step in an episode of mathematical research while the applications, concrete examples, and connections to other mathematical or scientific topics with which many papers close may well have stood at the beginning of this research episode.

3. Epistemic configurations of mathematical research

The analysis of the last section has shown that despite all similarities, two quite different episodes of mathematical research led Alexander and Reidemeister to their respective version of calculable knot invariants. We found that the formal, symbolic presentation of these invariants was only the last step in an activity which probably began as an investigation of very specific geometrical objects by means of techniques that were derived from Poincaré's techniques of manifold topology. Alexander studied "Riemann spreads" or "Riemann spaces," closed covering manifolds of the 3-dimensional sphere branched over a knot or link, using a modified version of Poincaré's "reduced homology." Reidemeister, on the other hand, looked at (open) unbranched coverings of knot or link complements, and used a modified version of Poincaré's technique for deriving a presentation of their fundamental group. In addition to the fact that both authors treated different geometric objects, they relied on two quite different descriptions of their objects: each author used a particular cell decomposition of the spaces under investigation that was suited for the application of the technique he was employing. While these differences may appear unimportant to a modern mathematician fully aware of the mathematical relations between the spaces and techniques mentioned, they made a significant difference in Alexander's and Reidemeister's mathematical practice.[35] They indicate that these mathematicians worked in different *epistemic configurations*, as I will say.

An epistemic configuration of mathematical research is the entirety of the intellectual resources that are involved in a particular research episode. It comprises the mathematical language, the skills and techniques at the disposal of the mathematician or the group of mathematicians engaged in this research, the set of research topics and open problems under consideration, the horizon of aims and more general heuristic guidelines followed by the researchers, etc. It either enables the work of a single mathematician or is shared by a small group of mathematicians working together. An epistemic configuration of mathematical research, together with the mathematician(s) working in and with them, thus constitutes a (usually rather small) working unit for the production of mathematical knowledge. Consequently, it may be taken to correspond to what Rheinberger has called the experimental systems of empirical sciences.

This correspondence may be made even closer by taking up a functional distinction used by Rheinberger to describe the dynamics of knowledge production in experimental systems.[36] Generating new knowledge means posing and answering new, previously unthought questions, or answering old questions in new, previously unimagined ways. Accordingly, there are two kinds of elements involved in the cognitive practice of scientists: elements that induce questions, that open up the future of research, and elements that generate answers, that produce a stable past for ongoing

[35] That mathematical theories which are more or less "equivalent in theory" (especially in modern retrospective) may be "non-equivalent in practice" has nicely been pointed out by Skuli Sigurdsson with reference to Newtonian and Leibnizian calculus (Sigurdsson 1992). Further use of this idea has been made by Guicciardini 1999.

[36] For the following, see Rheinberger 1997, 28–31. Rheinberger tends to conceive experimental systems as "the smallest integral working units of research." Whether it makes sense to speak of "smallest" of such units in the case of mathematics depends on the meanings given to the other terms in the quotation. In any case it is necessary to consider research episodes sufficiently limited in time and space.

research activity. With a view to experimental research, Rheinberger calls these two kinds of elements the "epistemic things" and the "technical objects" involved in experimental systems. Epistemic things are objects of active investigation, intellectual constructions, or material things or processes that are referred to as "objects" by scientists even if they are not ready-made, inert entities. They induce questions only if and insofar as they are vague, partially understood, and partially obscure. As research progresses, certain aspects of the epistemic objects under investigation become clearer while other, previously unseen aspects again emerge as vague and problematic, posing new challenges to research. Typical examples would be the structure of DNA before its clarification in the work of Watson, Crick, and others, or enzymatic sequencing of DNA before its transformation into a generally used laboratory technique. Another example, crossing the boundaries between physics and mathematics, might be Brownian motion, both before and during its mathematization in Norbert Wiener's work.[37] Technical objects, on the other hand, are procedures or techniques – such as enzymatic sequencing *after* its transformation into a standard tool or (mathematized) Brownian motion as used in later theories of stochastic integration – that allow one to determine some of the vague features of research objects. They either help to describe research objects in a way that enables scientists to handle them effectively and ask precise questions about them, or they provide the tools to answer some of these problems. Technical objects are stable, finished products of earlier activities. In an experimental setting, such techniques and procedures are typically tied to (but not identical with) material apparata such as microscopes, computers, or all kinds of measuring devices.

Of course the examples that Rheinberger has in mind mainly belong to laboratory science. However, the description of epistemic and technical objects given by Rheinberger is deliberately coined as a functional one and hence conceptually independent of any distinction between material and intellectual aspects of scientific practice (see e.g. Rheinberger 1997, 30). For this reason, it can be applied in situations where material practices do not extend far beyond reading, writing, talking, and listening. Quite obviously, mathematical research also revolves around partially understood objects of investigation, around objects that generate problems, and whose vagueness and intricacies point to an unforeseen future. And the object-defining and question-answering tools that a mathematician or a group of mathematicians has at hand in a particular research episode may evidently be taken to correspond to the laboratory equipment of an experimental scientist or, more precisely, to the technical procedures employed by the experimentalist to determine her or his phenomena. (By the way, also in the case of an experimentalist, certain cognitive instruments, certain techniques of thinking must be considered as part of the laboratory.)[38]

[37] At the time, the most challenging obscure aspects of this microscopic phenomenon were the strange properties of paths of individual particles.

[38] The relation between laboratory sciences and mathematics has also been discussed in Heintz 2000, especially pages 110–119. Independently of Epple 1999, Heintz proposes a very similar application of Rheinberger's

In order to underline the conceptual, not necessarily factual, independence of the answer-generating elements of research practice from material things, I will speak of epistemic techniques rather than of technical objects. An epistemic configuration of mathematical research, then, consists in a particular constellation of epistemic objects and epistemic techniques. This way of expression remains close to the usage of mathematicians. However, it must be kept in mind that the epistemic objects and techniques of mathematical research are not the immutable abstract objects or timeless methods mathematical Platonism or similar philosophies speak about. They are mathematical objects and techniques not "as such" but as conceived, imagined, or used by particular scientists in particular research episodes, as "targets" and resources "of epistemic activity" (compare Rheinberger 2000, 274). By definition, then, they exist only in particular places and times and undergo a permanent modification in research practice. Every partial result and any new heuristic idea about a problematic object changes it. Well understood epistemic objects may later turn into ingredients of stable research techniques, while techniques applied in unintended situations may become problematic and hence change into epistemic objects. On the other hand, both epistemic objects and techniques usually transcend the conceptions, imaginations, or uses of individual mathematicians – to the very degree that the research episode in question transcends the research of an individual.[39]

The mathematical objects that Reidemeister or Alexander tackled while working out their knot invariants were typical epistemic things. In particular, the key objects of their constructions, the "Riemann spaces" or covering spaces associated with knots were far from being fully understood mathematical constructions at the time of their research. Just as Riemann surfaces a few decades earlier, these 3-dimensional manifolds posed intriguing challenges to those studying them. No fixed nomenclature and technical language existed that allowed researchers to speak about them in a clear-cut way. Consequently, Alexander, Briggs, and Reidemeister had to develop their own devices to deal with these objects – they had to set up productive epistemic

categories to the case of mathematics. While she places this proposal in the context of an interesting sociological discussion of mathematics, Heintz does not try to give detailed analyses of particular episodes of mathematical research based on such a framework.

[39] Here an important disclaimer has to be added to prevent misunderstandings. The terminology and perspective employed above are not necessarily intended to do away with philosophical claims about timeless mathematical objects, structures, or methods. They are intended to describe something else, namely what mathematicians do in their research. Even if there exists some timeless abstract object that may rightly be called "the 3-dimensional sphere," this object meant and will mean different things to different mathematicians engaged in topological research. These differences begin with, but are by no means restricted to, the fact that any definition of a sphere is relative to a topological theory of choice. As long as problems such as the (classical) Poincaré conjecture remain unsolved, "the 3-sphere" will also remain an epistemic object for some – partially understood but in some crucial aspects still obscure, an object of active research. (If, by the time of reading, this problem is solved, choose another example.) – My attempt to remain neutral with respect to philosophical issues is slightly different from a trend in recent history of science which views approaches to scientific objects similar to the one sketched above as undermining philosophical debates about realism or constructivism, see. e.g. the introduction to Daston 2000.

configurations in which "Riemann spaces" could be treated. While both chose to draw on Poincaréan techniques – there was hardly another way to go – the endowment of their mathematical laboratories differed quite substantially in the details. Reidemeister made a strong effort to build up a technical apparatus around combinatorial group theory. Poincaré's approach to the fundamental group of a manifold was integrated in this apparatus by means of the crucial description technique for the kind of epistemic objects considered, namely cell decompositions. Alexander, on the other hand, worked in a laboratory whose most powerful tools were homological ones. He also refined the technique of describing manifolds by cell decompositions in such a way that Poincaré's method of determining the reduced homologies of a manifold involved less complex symbolical manipulations.[40] Accordingly, these mathematicians developed quite different skills that they could bring to bear on their epistemic objects.

A striking feature of both *A/B*'s and *R2*'s introduction of knot invariants was that both articles also sketched another, new epistemic configuration for doing mathematical research on knots. The objects of this new configuration were knots, described by means of plane diagrams, and its epistemic techniques were symbolic devices (matrices, groups, etc.) translating diagram equivalences into combinatorial or algebraic equivalences of the symbolism. In this configuration, 3-dimensional manifolds no longer played the role of key objects. Readers who skipped the corresponding parts of the papers would not thereby have missed essential information for dealing with knots in the new fashion. This reconstellation of research on knots was the main thrust of Reidemeister's "elementary foundation" for knot theory (*R2*), and one cannot avoid the impression that at least this move inspired Alexander and his student Briggs to proceed in a similar style, notwithstanding Alexander's claim to priority in discussing the torsion invariants of *Riemann spaces*. The further development of knot theory in the late 1920s and 1930s shows that the new epistemic configuration of knot theory, built around diagram combinatorics rather than manifold topology, was indeed effective and allowed the production of new knowledge about knots.[41] This shift also illustrates the dynamics of epistemic configurations in research processes. Elements of these configurations may change places. The epistemic objects of one research episode may turn into tools for another or they may vanish from a mathematical laboratory altogether. Techniques may themselves move in the focus of research interest and become modified for new tasks.

We have now reached a point where we can turn from an analysis of our main sources to a historical synthesis, as it were, of Alexander's and Reidemeister's research practice. By localizing the objects and tools of their work in their particular epistemic configurations, questions concerning the historicity of these objects, tools, and configurations can be posed and answered. How, when, and in what local milieus

[40] As mentioned above, this was done mainly by reducing the number of cells involved in the description of a manifold in a systematic fashion.
[41] A detailed account is given in Epple 1999a, chap. 12.

did Reidemeister and Alexander learn about their techniques and objects? A closer look at their roles in their respective local milieus also reveals different intellectual orientations and mathematical agendas which can help to account for the main phenomenon which our previous analysis has left unexplained, the move toward a "modern," formal mathematical style.

4. Knot Invariants II: Synthesis

Reidemeister

Kurt Reidemeister turned to topology soon after his arrival in Vienna in late 1922, where he had just obtained his first professorship.[42] Earlier he had been working on algebraic number theory and differential geometry, so his professional move was connected with a reorientation of his research interests. His correspondence during this period shows that he made himself acquainted with two main areas, Poincaréan manifold topology and combinatorial group theory. Both areas were actively represented in Vienna's local mathematical tradition. In this respect, the key figure among the senior professors whom Reidemeister met in Vienna was Wilhelm Wirtinger, born in 1865. Wirtinger lectured mainly on complex analysis and algebraic function theory, and in this context, he gave Poincaré's topological ideas a prominent role.[43] In particular, Wirtinger intensively studied singularities of algebraic functions of two complex variables during the decade between 1895 and 1904. In this work, drawing heavily on Riemannian function theory and the imaginative topological work of the Danish mathematician Poul Heegaard,[44] Wirtinger showed that certain features of such singularities – another characteristic epistemic object of mathematical research – could be investigated by means of a mathematical technique that transformed the singularities into 3-dimensional analogues of Riemann surfaces, i.e., those objects which Heegaard, Alexander, and others called "Riemann spaces." In this way, knots, Wirtinger's semi-cylinder construction, and the knot group appeared on the Viennese mathematical stage. For Wirtinger, these items played the role of research instruments: he used them to answer problems related to singularities. However, he refrained from publishing any final results of his research (for details, see Epple 1999a, chap. 8.2).

One explanation for this may be that the instruments were difficult to handle. They began to generate problems of their own. How could one tell different Riemann spaces apart? How could one tell different knot groups apart? As soon as such problems were raised, Wirtinger's items turned into epistemic objects. This step can be

[42] For more biographical information, see Epple 1999a, § 91.

[43] See in particular a remark in an unpublished autobiography of Heinrich Tietze (quoted in Epple 1999a, 240).

[44] Heegaard's thesis of 1898 played a crucial role in the clarification of Poincaré's homological notions. For a discussion of its contents and its role in Wirtinger's and Tietze's research, see Epple 1999a, § 77.

traced in a long and influential paper written by another mathematician who studied with Wirtinger in Vienna for some time, Heinrich Tietze's habilitation thesis "Über die Topologie dreidimensionaler Mannigfaltigkeiten" (Tietze 1908).[45] Tietze's main emphasis was to rework the foundation of Poincaré's manifold topology in a more consistent, combinatorial fashion. Two techniques were heavily used and developed: cell decompositions (according to Tietze's own account, this part of his thesis drew on direct suggestions by Wirtinger) and combinatorial group theory. Moreover, Tietze's article may be said to be an inventory of open topological problems, several of which were drawn from the tradition of geometric function theory. In this context, Wirtinger's constructions were mentioned – not as tools for algebraic function theory, but as problematic objects in their own right. One of the problems Tietze raised without answering it was the following: Could *all* closed, 3-dimensional manifolds be represented by a Riemann space in Wirtinger's fashion, i.e. as a covering of the 3-dimensional sphere branched along some knot or link (Tietze 1908, § 18)? We will return to the answer below.

From this brief account it is clear that there existed an awareness in Vienna's local tradition that this area of mathematics offered interesting possibilities for future research. Moreover, several of the crucial elements of the epistemic configuration in which Reidemeister worked out his knot invariants were present in this tradition prior to Reidemeister's involvement. In January 1923, Reidemeister wrote from Vienna to Hellmuth Kneser, who had taken his doctorate with David Hilbert and who also turned to topology at the time, that he planned to read Poincaré's papers on topology and that he intended to work on graphs and knots: "Whether something will come out of my present graph-knot-plans I can probably decide by Easter."[46] Five months later, Reidemeister reported that he gave courses on topological and group-theoretical topics, among them the theory of elementary divisors. In the same letter, Reidemeister mentioned that Max Dehn, the main expert on knots and groups in the period before World War I, had given lectures in Vienna, adding: "Soon, I will also fire a group cannon for shooting all invariants; at the moment the battery position is being dug out, transports of ammunition are directed etc., but everything is feverishly excited."[47] Reidemeister's excitement marks the turning point from learning new mathematical tools to the production of new knowledge. At least he now felt that he had a tool – the "group cannon" – which would help him in solving actual problems.

A first sign that his mathematical laboratory was beginning to be productive is the fact that one of the Vienna doctoral students, Otto Schreier, who participated in Reidemeister's seminar, was able to reprove and generalize an important earlier result of Max Dehn's classifying the groups of the so-called torus knots using group

[45] A discussion of Tietze's work is given in Epple 1999a, chap. 8.3.
[46] Reidemeister to Kneser, 6.1.1923 (translated from Epple 1999a, 301).
[47] Reidemeister to Kneser, 17.6.1923 (translated from Epple 1999a, 301).

theoretical tools where Dehn had relied on geometric arguments.[48] Schreier, who wrote his dissertation under Philipp Furtwängler, at the time Vienna's leading algebraic number theorist, shared Reidemeister's fascination with combinatorial group theory and seems to have functioned as a mediator for the transfer of older Viennese knowledge about knots and groups to Reidemeister. In late 1923 or early 1924, after receiving his doctorate, Schreier joined the mathematical seminar at Hamburg University and began working with Emil Artin (another Viennese mathematician who had come to Hamburg). As mentioned above, Schreier was also responsible for bringing Wirtinger's ideas into this collaboration.[49]

In 1924, Reidemeister presented his first group theoretical results at a mathematical colloquium at the university of Hamburg.[50] However, it seems that he still struggled with bringing his (somewhat unwieldy) group cannon to "shoot" topological invariants. A year later, after moving to Königsberg where he got his first full professorship, Reidemeister wrote to Kneser about a major breakthrough: "Concerning knots, it is violently roaring in my head, a horribly exhausting activity. I just want to make you a little curious. I have discovered a new knot group, a subgroup of Dehn's group."[51] The rest of the letter shows that this subgroup was the fundamental group of the (unbranched) double covering of the knot complement, i.e., the first in the series of groups whose Poincaréan numbers were presented as Reidemeister's new knot invariants.

Unfortunately we do not have any "laboratory notebooks" documenting this breakthrough in more detail. However, our previous analysis makes clear what must have caused the storm in Reidemeister's brain: the consideration of Wirtinger's geometric objects. The techniques he had learned to master in Vienna, Poincaré's and Wirtinger's cell decompositions and fundamental groups, Tietze's and Schreier's combinatorial group theory now all fitted together in the study of covering spaces. Calculating their fundamental group according to Poincaré's method and using Wirtinger's cell decomposition gave the new subgroup and with it, numerical invariants.[52] Hence, a hybridization of at least three strands of knowledge, two of which formed part of Vienna's local mathematical tradition and one of which consisted in a conscious and cooperative appropriation of written knowledge, was responsible for Reidemeister's innovation.[53] For quite some time, the epistemic configuration thus built up was highly productive. Not only would Reidemeister become the leading authority on knots in Europe for several years, he also worked out a number of

[48] Schreier's debt to Reidemeister's seminar is mentioned in Schreier 1924, 167 n.1.
[49] On Schreier's short life and career, see Chandler and Magnus 1982, 92–93.
[50] Published only later in Reidemeister 1926c; see the remarks at the beginning of this paper.
[51] Reidemeister to Kneser, 30.7.1925 (translated from Epple 1999a, 302).
[52] A full account of the claim made in this sentence is given in Epple 1999a, § 93.
[53] Here we recover another important aspect of Rheinberger's analysis of experimental systems. In many cases, their potential to produce new knowledge relies on the merging of elements from different research activities or experimental systems into a new one (cf. Rheinberger 1997, 135 f.; see also below).

further, more general results about the relation between the covering spaces and the fundamental group of a manifold.[54]

The influence of the city of Mahler, Schönberg, and Freud, of Wittgenstein and the Vienna circle in philosophy, of waltzes and anti-semitism on Reidemeister's intellectual life was not limited to the mathematical tradition(s) that entered his topological research.[55] In his university studies, Reidemeister had graduated not just in mathematics, but in philosophy (and three other subjects) as well. Edmund Husserl had been among his teachers. After coming to Vienna, Reidemeister soon found himself involved in the mathematico-philosophical activities that would later condense into the Vienna circle. In particular, he became a close friend of Hans Hahn, who had been one of Heinrich Tietze's closest friends before World War I. Together with the philosopher Moritz Schlick and the economist Otto Neurath, Hahn was one of the main figures in the early Vienna circle. Hahn's sister Olga was Neurath's first wife; Reidemeister's sister Marie would later flee with Neurath from Nazi persecution to England and become his second wife. Among the younger students involved in the group during the years Reidemeister spent in Vienna were Otto Schreier and Karl Menger. In other words, most of Reidemeister's communication partners in mathematics (with the exception of his older colleague Wirtinger) were also engaged with him in philosophical, cultural, and political discourses. When Reidemeister left Vienna in 1925 for Königsberg, he could rightly be regarded as an outpost of the Vienna circle. In 1930, he functioned as the local organizer and moderator of one of the circle's most famous meetings, the Königsberg meeting in which the famous triad of logicist, intuitionist, and formalist approaches to the foundations of mathematics was discussed by Carnap, Brouwer and von Neumann, and Gödel announced his famous undecidability results.[56]

Already in the mid-twenties, the foundations of mathematics as outlined in Hilbert's recent "formalist" program (Hilbert 1922) and the relevance of this approach to epistemology at large were discussed in Vienna. Two texts, published in 1928 and 1929, respectively, document some results of these discussions. The first was written by Reidemeister, the second by Hahn. This latter text, entitled "Empiricism, mathematics, and logic," sought to combine a formalist view of logic and mathematics with empiricism. According to Hahn, empirical knowledge arose from speaking about the world by means of a suitable symbolism. However, the signs of this symbolism did not correspond "uniquely and isomorphically" to that which they should signify. Hence a science was necessary which studied those transformations of symbolic expressions that did not change their relation to empirical content. This science was (formal) logic.

[54] See in particular Reidemeister 1928a, which gave a general classification of coverings of 3-dimensional manifolds endowed with a cell decomposition, and Reidemeister 1935, where a new topological invariant was introduced that came to be called "Reidemeister torsion."

[55] For Vienna's intellectual life, Janik and Toulmin 1973 and Schorske 1979 remain essential reading.

[56] Full references for the last paragraph are given in Epple 1999a, § 91 and § 97.

Mathematics, in turn, was nothing but a special branch of logic, particularly suited to the needs of empirical science (Hahn [1929] 1988). Reidemeister's philosophical manifesto, entitled "Exact thinking," also stressed the fundamental role of analyzing the combinatorics of sign systems. In his view, the "combinatorial facts" about dealing with "concrete signs" had their place in the "at the beginning of the construction of exact science, before logic" (Reidemeister 1928b, 35). For Reidemeister, exact science meant to establish relations between certain "combinatorial objects" only on the basis of previously fixed "combinatorial rules." The relations of the domain of exact thinking to the empirical world, however, were problematical: "The immediate grasp of these relations never leads to exact knowledge. It only gives rise to certain exact constructions" (Reidemeister 1928b, 37).

It is striking how well both Hahn's and Reidemeister's formulations capture the approach to knot invariants as published by Reidemeister in 1926. There, mathematical knowledge about knots was indeed presented as knowledge about the combinatorics of symbol systems, in fact as knowledge about certain symbolic expressions that remained invariant under transformations of the diagram symbolism describing knots in space. Hence knot theory was a part of "exact" combinatorics, but a part that had a fairly clear intuitive meaning, i.e. a relation to the empirical world, however problematic it might be. In this way, Vienna's modernist philosophical culture of the 1920s, the exchanges with Hahn and others, may well have contributed to shaping the formal, combinatorial style of Reidemeister's mathematics. As mentioned earlier, this writing style both documented and advanced a significant change in the epistemic configuration of doing research on knots. Thus we have a nice example of how an intellectual milieu can contribute to shaping a new epistemic configuration of mathematical research.

Alexander

James W. Alexander's career as a topologist was closely tied with Princeton's emergence as a renowned center of mathematical research. Born in 1888, Alexander took his degree and doctorate at Princeton University. There "he came under the guidance of Oswald Veblen," as his later colleague Solomon Lefschetz put it in his obituary (Lefschetz 1974). At the time, Veblen was the main American propagator of Hilbertian ideas and "founder of the Princeton research tradition in topology" (Parshall and Rowe 1994, 449).[57] Soon after graduating, Alexander got involved in polishing Poincaré's approach to homology based on cell decompositions of manifolds. His first major paper, coauthored by Veblen, gave a duality theorem on non-orientable manifolds. In its proof, a technique first introduced in Tietze's paper of 1908 was heavily used (Alexander and Veblen 1913).[58] In 1915, he closed an essential gap in Poincaré's

[57] On Princeton's rise as a mathematical center, see also Aspray 1988 and Borel 1988.
[58] The technique in question was the use of chains with coefficients mod 2.

arguments by giving a detailed proof of Poincaré's rather vaguely justified claim that two different cell decompositions of the same manifold gave rise to the same invariants of (reduced) homology. By the end of World War I, Alexander had developed an impressive virtuosity in handling the homology of cell-decomposed manifolds. Given Veblen's continuing interest (documented especially in Veblen 1922), one can clearly speak of a local knowledge tradition in Princeton in this regard.

In 1918, Alexander's researches took a new turn. Apparently inspired by the long list of open problems related to 3-dimensional manifolds in Tietze's paper and the geometric ideas discussed in Heegaard's thesis, which Alexander had helped to translate into French in 1916 (Heegaard 1916, 163), Alexander made 3-dimensional manifolds his favorite epistemic objects. Over the next few years, an impressive sequence of research results appeared in print which explored 3-dimensional manifolds in various new directions (including, where possible, higher-dimensional cases).[59] He defined new invariants, discussed the differences between the topological constructions within the 3-dimensional sphere that were possible either with finite "cellular subdivisions" or with wild, infinite subdivisions,[60] found and proved a new duality theorem and thought about new ways to construct 3-dimensional manifolds.

An important step in this last respect was Alexander's successful attempt at answering one of Tietze's open problems concerning Riemann spaces, i.e., branched coverings of the 3-dimensional sphere. Using once more cell decompositions and (most probably) intuitive thinking in the style of nineteenth-century's geometric function theory, Alexander "showed" that Tietze was right in conjecturing that every closed orientable 3-dimensional manifold could be represented as a covering of the 3-dimensional sphere branched over a knot or link (Alexander 1920).[61] This indicated that Riemann spaces might become an essential element in a new general description technique for 3-dimensional manifolds, and it appears that Alexander had high expectations for this technique (Lefschetz 1974, 112).

This insight added importance to the study of Riemann spaces; in other words, it added weight to their status as epistemic objects. In November 1920, Alexander gave a talk at the National Academy of Sciences that contained the material that would later form the basis of his priority claim about knot invariants. Our only sources about this talk are Alexander's own account, quoted above, and brief remarks in Veblen's 1922 monograph on *Analysis situ*s. According to these, the main thrust of this talk was the statement that all topological invariants of a Riemann space were also invariants of the system of its branch curves (in a suitable sense) (Veblen 1922, chap. V, § 44).

[59] For details, see Epple 1999a, §§ 99–101. A good biographical study or even a detailed obituary of Alexander is not available at present. The most important short obituary describing some main achievements and goals of Alexander's topological work is Lefschetz 1974.

[60] This research was inspired by another French paper, Antoine 1921.

[61] He came back to this topic in Alexander 1923 where he showed that the system of branch curves could be chosen to be what was later called a closed braid.

Probably, Alexander viewed this result as a byproduct of his study of Riemann spaces, and not as an important insight for the study of knots or links. In any case, the results discussed in the talk were not published. Instead, Alexander continued his investigations of 3-dimensional manifolds. Only after reading Reidemeister's paper, it appears, did Alexander take up the knot-theoretic leads of his earlier ideas, asking Briggs to help him work them out into an alternative approach to the torsion numbers of knots. Once this was done, Alexander found he could go an important step beyond Reidemeister's results. Less than a year after his joint paper with Briggs, he published his second major paper on knot invariants, introducing the polynomial invariant that today bears his name (Alexander 1928). A closer analysis reveals that this work very probably represents the first actual research performed in the new epistemic configuration of knot theory outlined both in *R* and *A/B* (Epple 1999a, § 103).

The local tradition Alexander drew upon in this research was clearly the homology of cell complexes he had been refining with Veblen since his earliest steps as a mathematician. This tradition merged with knowledge about the geometric objects of Heegaard and Vienna's earlier topological tradition and in particular, about Riemann spaces. Apparently, this knowledge was appropriated by active reading. In the period considered here, i.e. the years after World War I, Alexander's general agenda can be described as a highly successful (if necessarily incomplete) attempt at building up a powerful archive of mathematical tools for dealing with 3- and higher-dimensional manifolds. This agenda encompassed more than just a productive epistemic configuration for a particular research episode. It was directed toward turning the whole field of combinatorial topology of manifolds into a productive subdiscipline of modern mathematics, satisfying modern standards of rigor. This attitude was concisely summarized in a talk at the International Congress of Mathematicians of 1932 which may be considered as Alexander's mathematical manifesto of this period (Alexander 1932). In it, Alexander discussed rival views of the field of topology, weighing the possibilities and limitations of different approaches to its foundations. His own variant of the topology of cell complexes and cell-decomposed manifolds, "flat analysis situs,"[62] was presented as a wise compromise between set-theoretic, geometric and purely combinatorial foundations of topology. Illustrations of the possibilities of the field were taken, among other things, from knot theory.

Unlike Reidemeister, Alexander was not a philosophical mind. He was first and foremost a professional topologist. Up to World War II, virtually all of his research was devoted to topology. It earned him a full professorship at Princeton University in 1928, and in 1933 he became one of the first professors of the newly founded Institute for Advanced Study at Princeton. The key notion characterizing both aspects of Alexander's work – research and career – is clearly that of professionalization. Here again, Veblen's influence seems important. Around 1900, Veblen had been a

[62] The most adequate translation of this term into present-day mathematical language would be piecewise linear topology.

doctoral student of E. H. Moore in Chicago, one of the most important centers of the professionalization of mathematics in the United States one generation earlier (see Parshall and Rowe 1994, chap. 9). There, Veblen also came in touch with Hilbert's modern axiomatic approach to geometry. When Veblen came to Princeton a few years later, he clearly saw the advancement of axiomatic, formal mathematics as a pathway to professional mathematical culture. Princeton provided a very appropriate setting for these tendencies within the mathematical culture of the 1920s. New fields such as topology seemed ideally suited for carrying such a program to the next level of professional differentiation, and Alexander was just the right person to take over this particular task. Without being able to present more direct evidence, I would suggest that this aspect of scientific modernity played a similar role in Alexander's research practice as the intellectual avantgardism of Reidemeister's years in Vienna. To present knot invariants in the streamlined fashion of symbolic mathematics, joining simple manipulations of knot diagrams with formal manipulations of algebraic devices must have appeared attractive to a mathematician of Alexander's professional outlook.

5. Conclusion

The focus on the material aspect of experimentation is misleading when it comes to comparing experimental sciences with mathematics. What is needed is an analysis of the different functions of elements of research practice for the production of scientific knowledge. Once this move is made, as suggested e.g. in Rheinberger's historical studies of molecular biology, a number of interesting similarities between the dynamics of mathematical and experimental research become visible. The clue to such an analysis lies in a consistent temporalization and localization of the perspective on past scientific research. Rather than looking at an abstract, eternal world of mathematical objects and procedures so dear to some philosophers and mathematicians, historians can focus on the objects and techniques as given, employed, and modified in particular episodes of research.

Our analysis of two influential contributions to early twentieth-century topology has shown that even in a case in which two rather different mathematicians appear to produce the same fragment of mathematical knowledge at roughly the same time in two different locations, the technical resources and skills, the problems actually dealt with, and the general scientific agendas responsible for this work can be quite different. Both Reidemeister's and Alexander's research practice had its time and place. As intellectual enterprises, their work was not interchangeable.

This is mainly due to the fact that the production of mathematical knowledge also requires elements that do not travel instantaneously through space and time, in spite of their not being material in a straightforward sense. On the one hand, these elements consist in more or less stable mathematical techniques that have to be mastered before beginning work on a particular area of mathematical problems. In the episodes we have considered, this was not done by a simple and quick reading of mathematical texts. The tools of cell decompositions, combinatorial group theory, or homology were appropriated by Reidemeister and Alexander only in time-consuming processes that would probably have been impossible without the existence of a communicative environment in which technical skills were exchanged and probed in direct interactions. Reidemeister made himself acquainted with the tools he needed in Vienna, Alexander did the same, but with other tools, in Princeton. The second kind of elements tied to particular places and times are the epistemic objects of research. In both Reidemeister's and Alexander's work on knots, the crucial objects whose investigation allowed the introduction of calculable knot invariants were complicated geometric imaginations whose paths through early twentieth-century mathematics can be traced in almost complete detail. They had first been considered in late nineteenth-century work on the singularities of complex algebraic functions, notably in Heegaard's dissertation and then, decisively, in studies of Wilhelm Wirtinger around 1900. Wirtinger's students, in particular Tietze and later Schreier, made some of this work known to others, either in print or in direct communication. Only after learning of these geometric imaginations could Alexander and Reidemeister become aware of the research possibilities that would be opened up if the tools they had mastered were applied to these objects.

The analysis sketched in the second section has also shown that the texts documenting the results of Alexander's and Reidemeister's efforts did not openly reveal the epistemic configurations in which this research happened. In fact, they effectively sketched a different epistemic configuration in which mathematical work on knots and links could be done in the future. In this new configuration, the complicated geometric objects of the Vienna tradition no longer played a crucial role. Instead, the objects and techniques of the new setup revolved around knot diagrams and formal, symbolical manipulations of algebraic devices associated with these diagrams. In order to account for this remarkable move toward a modernist style of mathematics, I have sketched the intellectual and professional milieus in which Reidemeister and Alexander worked. Once more we find quite different environments. Whereas Reidemeister was immersed in the avantgard philosophical circles of Vienna's science-oriented intellectuals, Alexander was mainly acting in an environment of energetic professionalization of the kind of research he was interested in. However, these environments also shared certain features: they represented different shades of modernism in mathematics. In both milieus, Hilbert's axiomatic approach to mathematics was regarded as paradigmatic, and the kind of intuitive argumentation characteristic of, e.g. Heegaard's and Wirtinger's work was considered obsolete. Both in Veblen's environment and in the Vienna circle, the combinatorics of symbol systems was judged to be a rigorous or exact foundation for mathematics in general and topology in particular. Due to these factors, the intellectual environments of Alexander and Reidemeister exerted a similar pressure on the style in which they presented their results. If this is correct, it underlines once again the relevance of Rheinberger's observation of the hybrid character of experimental systems for configurations of mathematical research: "they are at once local, social, technical, institutional, instrumental, and epistemic settings" (Rheinberger 1997, 34).

Together with the shift of epistemic configurations, the debts of Alexander and Reidemeister to their local environments were obscured in their written papers. Going back to the first steps of our analysis we see that the final algorithms for calculating invariants, including their proofs, were largely detached from information concerning their coming about. Readers could take these final results and use them as models for further work on knots without even knowing about the epistemic objects and techniques of Alexander's and Reidemeister's own earlier research. To a superficial reader it could thus seem that the similarities in *A/B* and *R* – in results, in technical detail, in style – by far outweighed the remaining differences. A new piece of universal mathematics seemed to have been unraveled, as it happened in two places at more or less the same time.

But as we have seen, even this final similarity may be explained by referring to the shared features of the authors' local milieus. When Alexander read *R*, he not only recognized the epistemic objects of his own earlier research, but he will also have realized that Reidemeister's style of writing followed norms similar to his own. Given that he wanted to show that his earlier work had covered "the same" mathematics anyway, it is not surprising that *A/B* was written in a fashion closely resembling *R*. In the process of presenting their results, Reidemeister, Alexander, and Briggs acted on a level of mathematical culture that happened to be less determined by local differences. Writing articles meant acting in a space of communication that was wider than Vienna or Princeton. General knowledge claims had to be defended, and every article positioned its author in the spectrum of mathematical styles available at the time. Princeton and Vienna, being centers of a decidedly modernist mathematical culture, were in fact placed rather close to each other within this spectrum.

In this way, in connection with a priority quarrel and with the move toward a modern mathematical style, the outcome of local research activities was transformed into a more universal, maybe more stable, form of mathematical knowledge. This is not meant to suggest a general answer to the problem of how local mathematical knowledge becomes universal, but as another indication that even if the weave of intellectual fictions constructed by mathematicians acquires a remarkable stability over space and time, both the activity of weaving and the generation of stability happen in contingent, local historical processes.

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