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Topology, Matter, and Space, I: Topological Notions in 19th-Century Natural Philosophy[1]

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Contents

I. Topological notions in the weave of scientific practice 299
Introduction 299
Tracing topological notions: Dimension and connectivity 305

II. Ether vortices, constitution of matter, and the topology of space regions 309
Helmholtz studies "Wirbelbewegung" 311
Tait reads Helmholtz 316
Thomson speculates about matter 319
Enter Maxwell 324
The Helmholtz-Bertrand controversy 329
Maxwell pursues topology 332
Multiple connectivity and the most general motion of a fluid 341
Issues of reception 350

III. Knot chemistry 353
A periodic table of knots? 354
Graphical formulae for molecules and knots 362

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The tabulating tradition 367
A chemist's interest in topology: Crum Brown 373
Aware of a new discipline:
The British reception of Listing's work 375
The topology of matter: Concluding remarks 379

Appendix 383
References 385

Contents of Part II*

IV. Topology and the possible forms of space 00–00

V. The fourth dimension, knot spiritualism, and the *Unseen Universe* 00–00

VI. Conclusion 00–00

* Part II will appear in one of the next issues of the *Archive for History of Exact Sciences.*

I. Topological notions in the weave of scientific practice

Introduction

§ 1. The present study deals with the larger scientific background to the gradual emergence of the mathematical discipline of topology. Two important and interrelated strands in the practice of the exact sciences in the 19th century will be considered in which topological ideas came to be relevant for natural philosophy. In this way, light can be thrown on a part of the causal weave of events that eventually led to the emergence of topology as a discipline, a part which has largely been neglected up until now in the historical literature. The first of these two strands was concerned with topological issues that arose in the context of a dynamical theory of physical phenomena, a theory advocated in particular by British natural philosophers during the last third of the 19th century. These developments will be discussed in the first part of our study. The second strand of events – related to speculations about the large-scale topological structure of space – will be the focus of the second part of this article.

§ 2. The emergence of an entirely new discipline within mathematics is a rare event in the history of science. The creation of topology – the science of properties of spaces and figures that remain unchanged under continuous deformations – represents a phenomenon of this kind, but of a distinctly modern variety. Topology bears comparison with the calculus, probability theory or number theory in that the first ideas about a new field called *Analysis Situs* or *Geometria Situs* were communicated among a handful of mathematically-minded intellectuals in the late seventeenth and early eighteenth centuries. However, unlike the calculus and number theory, but similar to probability theory, the basic ideas underlying *Analysis Situs* reveal no ancient roots.[2] Notoriously, ancient authors treated questions of continuity hardly at all, and if so, then mainly as physical questions linked to the phenomenon of motion.[3] Moreover, in sharp contrast to these three other fields, during the 18th century no clearly defined domain of mathematical problems was delineated that should and could be treated by *Analysis Situs*. Rather, a vague idea about an analysis which dealt not with magnitude, but "position," left it to individual mathematicians to decide what should belong to the new field. Only gradually over the course of the 19th century was a consensus reached about the nature of problems in topology. Nevertheless, after crossing the threshold to a scientific discipline in the full sense of the word in the first decades of this century, topology became one of the core research fields of mathematics, and topological arguments have come to play a role in virtually every other field in mathematics and the mathematical sciences. If one may reasonably speak of *genuinely modern* mathematical disciplines, then topology certainly belongs among them.

These late beginnings may be one reason why the emergence of topology has only begun to attract historiographical attention comparable to that received by fields like the

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[2] On the early ideas on *Analysis Situs*, see (Freudenthal 1972) and (Pont 1974). Some of Leibniz' fragments on *Analysis Situs* have recently been edited (Leibniz 1995).

[3] The *locus classicus* is Aristotle's *Physics*. For a modern discussion of the ancient idea of continuity, see (Dehn 1936a).

calculus, number theory, or probability theory. While the invention of the calculus has long since been the object of historical study, and while the emergence of number theory and probability theory have recently been treated from a wide variety of perspectives,[4] the number of historical monographs devoted to the formation of topology remains very small. Apart from these, we have a few survey articles, and several research papers dealing with particular topics within or closely related to topology.[5] Most of this literature has focused on describing concepts and mathematical results which we today classify as topological, without undertaking a closer analysis of the concrete circumstances under which these achievements were actually produced. As a consequence, the overall account of the making of topology which emerges from the existing literature is structured by a rather narrow history of mathematical ideas.[6] This account suggests that it was mainly a chain of deep insights into the conceptual architecture of mathematics that eventually opened up this new branch of geometry. These insights were gained and shared by a few great mathematicians, first Leibniz and perhaps Descartes, then Euler and Vandermonde in the 18th century, and finally Gauss and in particular Riemann in the 19th. Only after Poincaré's seminal writings in the 1890's, though, did the field reach maturity. The first serious outstanding problem of topology, according to this account, was the classification of surfaces, a problem which arose in connection with one of the core fields of 19th-century pure mathematics, the theory of complex algebraic functions. Generalizing this problem to higher dimensions then led to the notion of a (differentiable or topological) manifold and the corresponding classification problems, for which the notions of homology and homotopy provided the crucial technical tools.

§ 3. However persuasive such a picture may appear in retrospect, it certainly cannot account for the complex processes that led to the emergence of the discipline of topology in a detailed and realistic way. Knowing that certain concepts were formed and that a number of problems were formulated and treated does not tell us very much about the *reasons* that led scientists to form just these concepts and to treat precisely those problems. Moreover, the account just summarized is silent about the role of topological work in the scientific careers of those involved, the processes of communication of topological ideas, and the embedding of these new ideas into better established areas of scientific knowledge and practice.

One need only focus attention on some of the well-known circumstances involving leading figures in order to see that studies of the emergence of topology may be deepened in significant ways. For instance, when Poincaré referred to celestial mechanics (more

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[4] The origins of number theory are discussed, e.g., in (Weil 1984), (Mahoney 1994), and (Goldstein 1994). On the emergence of probability theory, see (Hacking 1975), (Stigler 1986), (Porter 1986), and (Daston 1988).

[5] The only monograph dealing with the whole period before Poincaré is still (Pont 1974). For the period after Poincaré, the standard reference is (Dieudonné 1989). The substantial study (Scholz 1980) focuses on the development of the concept of manifold. The surveys are (Hirsch 1978) and (Dieudonné 1994); of research papers, (Bollinger 1972), (Johnson 1979 and 1981), (vanden Eynde 1992), (Epple 1995) and several articles in (Greffe et al. 1996) might be mentioned.

[6] Condensed statements of this view can be found in the concluding sections of (Pont 1974) for the period before Poincaré, and the opening paragraphs of (Dieudonné 1989) for the period beginning with Poincaré.

precisely, to the three-body problem) as one of the issues which led him to topology (Poincaré 1921, 101), this can be taken not only as a reference to a crucial point in Poincaré's own career (see Barrow-Green 1997) but also as indicating that areas of mathematics dealing with physically motivated problems might well have been relevant for the emergence of topology in general. Or, to go back to the Enlightenment, when Vandermonde, an intellectual close to Monge who became deeply involved in the French revolution, claimed in 1771 that *Analysis Situs* could be developed as a calculus for the manufacture of textiles (Vandermonde 1771, 566), this contention raises the issue of non-mathematical practices that were linked to the invention of *Analysis Situs*.

As this study will show, similar hints taken from less familiar documentary evidence point in much the same direction. A causally adequate history of the emergence of topology, it seems, must go beyond a history of mathematics *intra muros* and look at the subject in a broader context. Such a history requires more than a description of a chain of topological concepts and results leading to the modern body of topological knowledge. Rather, it has to make clear by a detailed study of the network of practices in the mathematical sciences how a need for topological tools gradually developed, and how these tools were forged in complicated interactions between different domains of scientific practice. After all, the emergence of topology was a process of *mathematization*, and as such dealt with a complex variety of questions – namely those of "position" – which were of relevance not only in important domains of pure mathematics but also in other domains of science and technology. Of course, the formation and use of topological ideas *within* pure mathematics also deserves a closer historical investigation highlighting the ways in which these ideas were situated in actual mathematical practice.

§ 4. The two parts of the present study both deal with one particular area relevant for a broader history of the emergence of topology, namely its relations with mathematical physics in the second half of the 19th century. At first sight, one might not expect to find a rich body of materials for a substantial history around this topic.[7] It is true that, as a

[7] This seems to be confirmed by the limited number of earlier historical studies of topological ideas in 19th-century mathematical physics. Historians of mathematics have addressed the topic occasionally, but mainly in passing. Pont devotes a few pages to the subject which cannot claim to be complete in any respect (Pont 1974, 154 ff.). Some aspects of the issues of the dimension of physical space and also of the Zöllner affair have been discussed in Lützen's recent work (Lützen 1995). Closest to some parts of section II below is Archibald's brief study of the early development of Green's ideas in potential theory (Archibald 1989). On the other hand, many aspects of 19th-century natural philosophy related to the themes discussed here have received detailed and competent treatments in the historical literature on physics. For British dynamical theory, see e.g. (Silliman 1963), (Knudsen 1976), (Buchwald 1977), (Wise 1981), (Buchwald 1985) and the collections (Cantor and Hodge 1981) and (Harman 1985). However, precisely those mathematical subtleties which show the gradual assimilation and refinement of topological notions are usually passed over in the physics literature. For instance, Silliman's discussion of vortex atoms mentions Tait's ensuing knot tabulations only in passing and overlooks the topological arguments in Thomson's hydrodynamical work. Even Harman, who emphasizes the role of topology in Maxwell's work (Harman 1987), preface to (Maxwell 1995), does not pursue the matter in detail. The literature on the space problem has been mainly oriented toward philosophical issues; see for instance (Jammer 1969) and (Torretti 1978).

research discipline in the full sense of the word, topology did not exist before Poincaré published his papers on *Analysis Situs*, and perhaps not even before the next generation of topologists embarked on the project of extending Poincaré's results. Moreover, in the time before the disciplinary threshold was reached, even pure mathematicians had great problems in understanding and refining the topological tools introduced by Riemann and some of his contemporaries. This is indicated, for instance, by the long period that elapsed before the completion of the topological classification of non-orientable surfaces, or by the slow reception of Betti's ideas on higher connectivity numbers. In view of these difficulties, one might be inclined to wonder whether 19th-century natural philosophers were in a position to use the rudimentary tools of pre-disciplinary topology at all and, if they were, whether their work reflected more than a passive reception of the mathematical innovations.

As a first step toward answering these questions, one can point to a document like Peter Guthrie Tait's now-famous tables classifying alternating knots (Fig. 1). Evidently, this gives an example of a natural philosopher who actively pursued topology in the decade 1876–1885. One might still wonder, of course, whether this classification of knots had serious relevance for physics; perhaps it was merely a mathematical recreation for the experimental physicist Tait? After all, it was only in the 1920's that Tait's knot tables could be verified by rigorous methods (using Poincaré's torsion invariants and, a little later, the polynomial knot invariant introduced by Alexander). Nevertheless, I wish to show that topological notions *did* play a crucial role in certain areas of physics and that, in particular, Tait's enterprise was deeply anchored in the natural philosophy of its time.

More generally, I will argue that the interaction between mathematics and natural philosophy around topological notions was far from being just a process of sporadic reception of the mathematicians' work by physicists.[8] The interaction was of a much more complicated form. At least three aspects deserve emphasis and will be discussed in the following. First, there was significant overlap between the communities and discourses of mathematicians and physicists in which topological ideas were exchanged and elaborated. Physicists like Helmholtz, W. Thomson, Maxwell or Tait were highly sensitive to new ideas in mathematics. By the same token, mathematicians like Clifford or Klein did not separate their investigations from thinking or speculating about the physical world. Second, along with the exchange of topological ideas, a transfer of legitimacy from physical theory to mathematical innovation took place: "topology mattered" in an almost literal sense. Third, physical thinking even led to new mathematical problems and, consequently, to new results. All these observations call into question the idea that the emergence of topology was merely a result of developments *within* the disciplinary architecture of mathematics. On a more general level, they also qualify assertions about the degree of differentiation between the disciplines of mathematics and physics in the period considered.

§ 5. Before proceeding further, let me give a short survey of the contents of the present study. After some methodological remarks, I turn in Section II to the role topol-

[8] That there was at least some reception of topological ideas among physicists has been noted occasionally. See for instance (Harman 1987, 285 ff.).

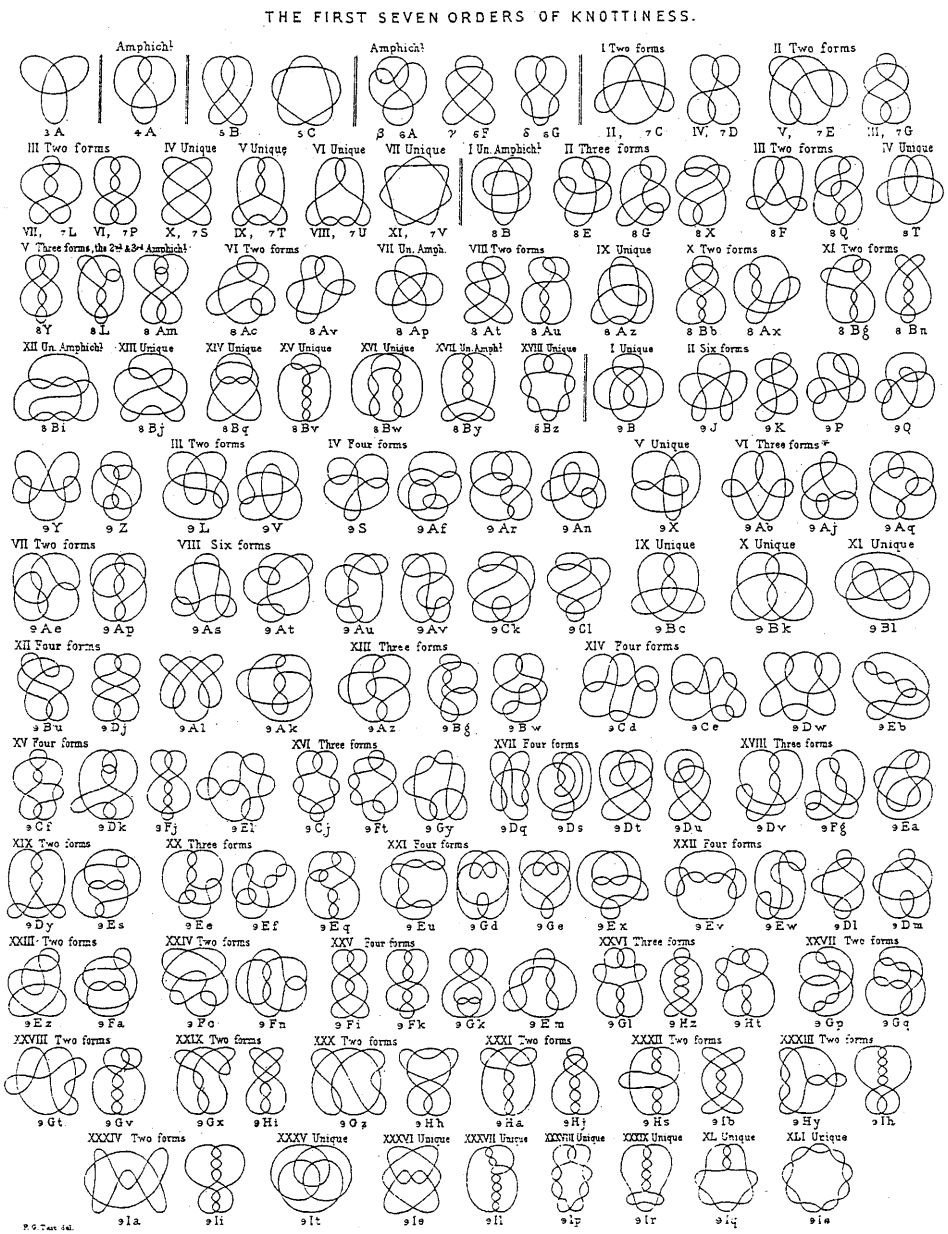


Fig. 1. One page from Tait's tables of knots (Tait 1884c)

ogy played in speculations on the constitution of matter. Since much of 19th-century natural philosophy sought to explain physical phenomena in terms of a complicated dynamics of a continuous medium (be it in the form of an ether theory or in the more sophisticated version of a dynamical analogy based on the formalism of Lagrangian mechanics[9]), it should come as no surprise that topological aspects of this dynamics were investigated and discussed. The basic phenomenon to be treated here is vortex motion. In a famous paper written in 1858, Hermann v. Helmholtz initiated a rich line of research which not only soon entered hydrodynamics textbooks but which also was taken up by William Thomson in his famous speculations about vortex atoms in 1867 and the years following. Thomson was acutely aware of the topological difficulties of the topic and his hydrodynamical papers, written while he was still struggling to make his notions precise, in fact contain some non-trivial topological results. Technically speaking, Thomson's investigations dealt with "irrotational flows" (in modern terms: harmonic vector fields) in multiply connected space regions. Two other scientists, Peter Guthrie Tait and James Clerk Maxwell, were closely involved in this research. Maxwell, who was genuinely interested in learning and extending the topological ideas developed by the Göttingen mathematicians, is relevant for our discussion for two reasons. First, because he engaged in an extensive discussion about topological matters in his correspondence with Thomson and Tait around 1868, and second, because his *Treatise on Electricity and Magnetism* of 1873 collected those topological ideas which he thought important for the development of natural philosophy. Tait, on the other hand, emerged among these Scottish natural philosophers as the one who made the most original contribution to topology. Motivated by Thomson's speculations about vortex atoms and encouraged by Maxwell's interest, he embarked on a remarkable project, the classification of alternating knots of up to ten crossings. With surprising success he produced complete tables of such knots, thereby founding a tradition of knot tabulation which survived even into the first decades of the 20th century. Tait's achievement, discussed in Section III, must be seen in the context of several other developments of the day, like combinatorics, the beginnings of graph theory, and speculations about chemical structure.

In the second part of this study, comprising Sections IV–VI, I present a summary of the topological issues involved in another strand of the debates linking natural philosophy with mathematics in the late 19th century: those concerned with the so-called space problem. While these discussions have often been described as leading to a fundamental transformation in the understanding of the relations between geometry and physical experience, there is also a story to be told about what could be called the *topological space problem*, an issue underlying, but distinguishable from the geometrical space problem. Besides the issue of the dimension of physical space, the technical core of this story, treated in Section IV, concerns the topological differences between what came to be known as Clifford-Klein space forms. The novelty of the topological speculations about space and matter was felt very distinctly by teachers and students of 19th-century natural philosophy. Little wonder, then, that metaphysical and even supernatural speculations accompanied the conceptual innovations. In Section V, I briefly review both the scandals surrounding the astrophysicist Zöllner's engagement with spiritualism and Tait's and

[9] Compare the discussion in (Buchwald 1985, 20 ff.).

Stewart's popular books *The Unseen Universe* and *Paradoxical Philosophy*. It will be shown that many of the topological ideas which played a role in the debates of the time on natural philosophy – both those relating to the structure of space and those involved in theories of matter – were woven into this little tangle of spiritualism and immaterialism. In the last section, the topological speculations of 19th-century natural philosophy are connected with some developments in early 20th-century field theory. I conclude with an attempt to summarize the main features of the strands of scientific practice described, including a discussion of their role in the formation of modern topology.[10]

Tracing topological notions: Dimension and connectivity

§ 6. The method used in the following to trace topological ideas in 19th-century natural philosophy is based on a rather simple analysis of the occurences of the two most important topological notions in natural philosophy during the period described. The first of these notions was *dimension*, in particular, the dimension of physical space. The second, in some ways both deeper and vaguer idea was that of *connectivity* (*Zusammenhang*). By following the traces which the uses of these notions left in published texts, manuscripts and correspondence, and by situating these uses in their respective contexts, it becomes possible to establish a significant part of the interactions relating the work of mathematicians and natural philosophers around topological innovations in the period between Riemann and Poincaré. As mentioned before, the idea behind this procedure is not so much to write a conceptual history but rather to use dimension and connectivity as indicators for causal connections between the actions and events constituting the practice of the scientists involved. Both ideas, the notion of dimension as applied to physical space, and the even more complicated idea of connectivity, are

[10] I would like to emphasize that the topic of the broader scientific context of the emergence of topology is by no means exhausted by the following study. Of developments before the period considered here, at least three deserve historical attention in their own right. The first concerns Gauss, whose topological interests were also motivated in part by contexts like astronomy, geodesy, and electromagnetism. For a glimpse into Gauss' ideas, see (Epple 1997). I hope to present a full treatment on a future occasion. Secondly, Listing's essay *Vorstudien zur Topologie* of 1847 merits a detailed study highlighting his attempts to convince scientists of the relevance of this new mathematical discipline for fields like crystallography, biology, astronomy, etc. The third development, particularly relevant for the discussion in Section II, concerns the topological ideas in Ampère's and Faraday's researches in electricity and magnetism. Some hints may be gathered from (Grattan-Guinness 1990, ch. 14). Another serious omission even in the period considered here are the Italian links to the events described below. In particular, Betti's hydrodynamics dealt with topological issues very much along the same lines as Thomson's work. On another Italian mathematical physicist, Beltrami, and his mathematical ideas on ether see (Tazzioli 1993). Finally, I have not ventured to enter into a detailed discussion of the broader scientific contexts of Poincaré's topological work. Poincaré will only briefly be discussed in the fourth section with respect to the topological aspects of the space problem, but there is much more to say. The role, for instance, of Poincaré's topological ideas in celestial mechanics and the three-body problem has been discussed in (Goroff 1993), (Andersson 1994) and (Barrow-Green 1997).

sufficiently specific that it makes sense to proceed in this way. In view of the novelty of these ideas in 19th-century scientific thinking, it is hardly conceivable that a scientist used one of them without having been in intellectual contact with earlier developments involving them, and in fact it is possible in many cases to establish chains of personal communication in which these ideas were transmitted, and often modified.

Already at this level, the picture of the emergence of topology suggested by the existing historical literature will be shown to require modification. According to this standard view, the notion of connectivity, introduced in Riemann's function theoretic papers, played a key role in the classification of surfaces and in Betti's attempts to generalize Riemanns *Zusammenhangsordnung* to higher dimensions (Pont 1974, ch. 2). By the 1870's, both the notion of connectivity and the related notion of genus began to take on major significance for algebraic geometry.[11] Efforts to explore the connectivity of higher-dimensional algebraic varieties, mostly by Picard, form part of the backdrop leading to Poincaré's breakthrough in defining homological invariants and the fundamental group of a manifold (see Scholz 1980, Section VI.2.3 and ch. VII). However, as will be described in detail below, the complicated notion of connectivity also passed through different channels, leading to a link between Göttingen mathematicians and Scottish natural philosophers. Initiated by Helmholtz's hydrodynamical work, this reception eventually led to mathematical arguments which in modern mathematical language can be described as situated within the intersection of the theory of harmonic differential forms and three-dimensional topology. Of particular importance were arguments that arose in attempting to clarify the notion of connectivity itself. Both Riemann and Poincaré wavered at times between what later came to be distinguished as the homological and homotopical aspects of this notion,[12] so it is hardly surprising that physicists like Maxwell and Thomson struggled with this ambiguity as well. Nevertheless, they were able to make substantial uses of the idea of connectivity in studying physical problems where this notion seemed relevant to them. The main body of the following study will be devoted to these developments and their implications.

Concerning the notion of dimension, it has often been pointed out that at mid century it was quite difficult to speak, like Grassmann or Riemann, of n-fold extended quantities without entering into conflict with the traditional idea that physical space has no more than three dimensions. A related, but different task, however, is to describe how ideas about a space or spaces of higher dimensions finally found their way into physics lectures and books, sometimes combined with a defence of the number 3 or, occasionally, with brief speculations about how the world would look if real space had more than three dimensions. Moreover, also the notion of connectivity came to be applied to the issue of the topological structure of physical space, first by Clifford, and later by Klein and Killing. As we shall see, within this strand of the transmission of topological notions, the need for technical clarifications also grew significantly.[13]

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[11] See (Scholz 1980, Section IV.2) on the contributions of Klein and Schläfli.

[12] See e.g. (Hirsch 1978, Section 10.2.3).

[13] A third topological notion, the uses of which could be traced in the interaction between natural philosophy and mathematics, is the fundamental notion of continuity. Also this would make an interesting subject of a narrative extending the perspective of the present study. Since, however, the uses of this notion – for instance, in Clifford's and Poincaré's writings on the philosophy of

§ 7. Let me now give a more concrete description of the procedure which I have tried to follow. In the first place, specific and substantial uses of the notion of dimension or connectivity in the context of natural philosophy were taken up into what might be called the *basic chronicle* underlying my historical narrative.[14] At the beginning of each section, the respective part of this basic chronicle will be given. However, such lists of events are in themselves not a sufficient basis for producing a coherent historical narrative. Detailed descriptions of the events listed in the basic chronicle invariably suggest causal and motivational relationships with further historical events which did not necessarily have a topological cognitive content. In fact, in the present study they range from simple social events like the first encounter of Helmholtz and Thomson in Bad Kreuznach in the summer of 1856 to experimental illustrations of vortex motion, the Cambridge Tripos, and the invention of a graphical notation for chemical structure. These events were added to the basic chronicle in order to form what I will call the *extended chronicle*. This chronicle will be implicit in the text; it is not given as a separate list. The third step, then, consisted in constructing a narrative exhibiting the relations between the events in the basic chronicle and those in the extended chronicle. In particular, the narrative tries to account in a specific way for the cognitive contents of the events in the basic chronicle. In practice, these steps had to be iterated and adjusted to each other several times. In all three steps, there were choices to be made, in particular, to decide at what level of "thickness" (to use a notion made popular by (Geertz 1973)) to stop.

One of the criteria which guides the type of historical inquiry carried out below is the achievement of a certain form of *causal coherence* in the historical narrative. A narrative is causally coherent, if the events forming its basic chronicle are given an interpretation that shows their place in a causally connected course of scientific action. To satisfy this criterion in the present case, I found it necessary to understand the occurences of the notions of dimension and connectivity in natural philosophy in the spirit of what Buchwald and Schweber have recently called "pragmatic realism" in the history of science (Buchwald 1995, 345 ff.). Adapted to the case of mathematical practice in the domain of pure mathematics as well as in natural philosophy, pragmatic realism means that the historian insists on *both* the objective character of mathematical knowledge *and* the fact that this knowledge was constructed in a fabric of social and communicative action. It is the weave of concrete scientific action rather than an abstract life of mathematical ideas that historians need to analyze in a detailed and realistic way if they wish to adhere to the goal of producing a causally coherent account of developments like the emergence of topology.

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science – are more relevant for exploring the scientific contexts of the emergence of set-theoretic topology rather than geometric and algebraic topology, it will only occasionally be touched upon in the following.

[14] Here I make use of the historiographic distinction between chronicle and narrative, rather traditional in general history. Even if often criticised, this distinction makes much sense if it is understood not as referring to two different kinds of historical writing, but rather as denoting two layers present in *any* historical text. As such, it has been made the core of Hayden White's impressive project of a *Meta-History* (White 1973, White 1987), the full implications of which for the history of science still remain to be drawn.

It seems necessary to add a remark on what historical causality is taken to mean in the following. Causal explanations of historical events have often been thought to be connected with formulations of historical laws accounting for individual events. Cutting short the long debate which this position has raised, I wish to emphasize that I take a different view of historical causality. Without further argument I take as starting point a Weberian picture of historical, social practice as composed of individual actions in situations structured both by material boundary conditions and an intellectual horizon shaping individual intentions.[15] Material conditions and *Sinnhorizont* of scientific work are themselves, at least partially, results of social practice. The events making up what I called above the chronicle of a historical narrative are particular events in this practice, especially intellectual actions with a substantial cognitive content. Historical causality, then, is a complex network of relations between such individual action-events, including their concrete motivations and intentions. The latter, in turn, are causally related to previous events in the weave of scientific or social practice. In particular, causal relations are not necessarily governed by general historical laws. This view of causality conforms to the Weberian position that "a correct causal interpretation of a concrete action means: that both the external course and the motive [of the action] are recognized as accurate and at the same time are understandable as meaningful."[16]

Of course, there is no easy access to this level of historical reality, nor can one expect to find some unique account, since no complex event in scientific practice admits just one adequate description. It should be clear, then, that the kind of realism advocated here does not preclude a certain degree of perspectivism on the side of the historian. Nevertheless, I cannot follow the view that in consequence of this difficulty, historiography should dispense with the attempt to provide realistic, causally coherent narratives altogether. Different realistic stories about the same tangles of scientific practice are not only possible but desirable and stimulating for historical discourse.

On the historiographic level, pragmatic realism has consequences on a number of different levels. On the one hand, it requires close attention to technical issues, at least in some places. If the basic chronicle, as in the present case, is to include events like proofs of mathematical results, a narrative which situates them in mathematical practice must show at least the main lines of how these results came about, in what ways they were based on previous knowledge, and what kinds of aims motivated these findings. On the other hand, the tendency to view the modern, systematic motivations for posing and solving particular problems as driving forces behind the actual historical development – still prevalent in many contributions to the history of topology – has to be replaced by paying attention to the actual intentional context of historical research. Under the restriction to explicitly documented intentions, legitimations, etc., even a sceptic about intentional history might consider the result as a form of "discourse analysis", namely of the discourse about intentions, the function of which in scientific practice was the negotiation of legitimacy for a particular area of research.

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[15] Weber's basic framework is concisely described in (Weber 1921, ch. I).

[16] See (Weber 1921, ch. I.1, § 1.7). Here and in the following, all additions in square brackets are mine.

§ 8. Such a historical analysis allows one to show, to take a specific example, that two events like Listing's treatment of knots in his *Vorstudien zur Topologie* and Tait's classification project were not just related by the same topic (a relationship that does not necessarily imply a causal connection) or the fact that Tait eventually came to read Listing and subsequently used the latter's remarks for his own research. Rather, we can follow a much more complex series of historical events involving several other scientists (including Gauss, Riemann, Helmholtz, Thomson, and Maxwell) and research interests (including vortex motion, electrodynamics, and atomism) that enables us to explain how topological ideas actually found their way into the Scottish context and motivated Tait in his actions.

In a similar way, I hope the present study shows at least part of the extension of the chronicle that is necessary for giving a causally coherent history of the complicated process of discipline formation leading to modern topology. Further studies are required to complement this extension of the chronicle with a better causal understanding of the developments within pure mathematics, first and foremost in algebraic function theory and algebraic geometry. In the end, it may turn out that in some respects, for instance concerning the relative importance of various developments, the existing account of the emergence of topology can be considered as substantially correct. Even if so, however, this should be the result of thorough historical studies rather than an implicit assumption drawn from the conceptual organisation of today's mathematical knowledge.

II. Ether vortices, constitution of matter, and the topology of space regions

Chronicle: Ether vortices and topology	
1847	Listing's essay *Vorstudien zur Topologie* tries to convince scientists of the importance of topology. The phenomena of orientation and knotting play a central role.
1851, 1857	Riemann's papers on complex analysis present his ideas on connectivity.
1858	Helmholtz studies vortex motion of perfect fluids and mentions connectivity numbers of spatial regions.
1860	Tait reads Helmholtz and begins thinking about quaternion analysis.
1861	Listing publishes his *Census der räumlichen Complexe*.
1867	Gauss's fragments on electromagnetism are published, including the linking integral. Tait performs smoke ring experiments in W. Thomson's presence. Thomson speculates about vortex atoms. Tait publishes his translation of Helmholtz's *Wirbelbewegung*. Maxwell, Thomson and Tait begin to correspond on topological matters.
1868-1869	Thomson reproves and extends Helmholtz's results on vortex motion, with special emphasis on irrotational flows (in modern terms: harmonic vector fields) in multiply connected space regions.
1869	Maxwell reports on Listing's *Census der räumlichen Complexe* to the London Mathematical Society.
1870	Tait writes *On Green's and other allied theorems*, combining quaternion methods and ideas on fluid motion. Maxwell discusses the topology of graphs in papers on statics.
1873	Maxwell publishes his *Treatise*, including Listing's topological ideas, Thomson's results on flows in multiply connected regions and Gauss's linking integral.

Chronicle: Ether vortices and topology	
1870's	Thomson tries to prove the dynamical stability of simple vortex configurations and to determine their fundamental vibrations.
1882	J. J. Thomson receives the Adams Prize for an essay on vortex motion which includes an attempt to explain chemical valency and chemical compounds in terms of vortex atoms.
late 1880's	W. Thomson gradually abandons the vortex atom theory.

§ 9. For one or two decades in the late 19th century, some speculations on the constitution of matter were intensely discussed among British natural philosophers in which topological ideas entered at a very fundamental level. These speculations, initiated by William Thomson in 1867, tried to integrate two fundamental tenets of mid-19th-century natural philosophy into a unified picture of matter and motion.[17] The first of these was a mechanical, or rather dynamical world view; the second an atomistic approach, forcefully supported first by chemistry, then by spectrum analysis and statistical mechanics, both of which began to blossom in the 1860's. These two visions did not, at first, appear easy to integrate. Many natural philosophers up to the middle of the 19th century conceived of atoms as small material (or perhaps even immaterial) objects, interacting with each other by some sort of action at a distance. Of particular influence was the theory proposed by Boscovich in the late 18th century which made atoms simply force centres, with a force law to be adjusted to save the phenomena. A dynamical theory, on the other hand, would try to explain matter and its interactions by a Lagrangian formalism describing the motion of a continuous medium; in such a medium, actions would be propagated by the contiguity of the smallest parts of the medium. For those adhering to the latter view, like William Thomson, in many ways the leading figure in British natural philosophy during this period, the idea of indestructible atoms therefore represented an important challenge.

One way to reconcile atomism and dynamical theory was to look for stable dynamical configurations in a universal medium that could be regarded as atoms. Already in the 1850's, Thomson had considered the possibility that such configurations could arise from rotary motion in the ether. Due, however, to the lack of both empirical evidence of the permanence of vortex motion in real fluids as well as mathematical methods to treat this kind of motion, Thomson remained sceptical and the idea was not pursued for several years. When Thomson took up his speculations again in the years following 1867 and elaborated them into an ambitious but finally unsuccessful research program, the crucial new ingredient was the idea that the stability of atoms could perhaps be explained by the permanence of certain *topological* properties of fluid motion.

In this section, the origins of this idea and its implications for the research of British natural philosophers, mainly Thomson, Tait, and Maxwell, will be discussed. For the developments to be described, Riemann's notion of connectivity became both an important technical tool and a notion inspiring far-reaching ideas on the variety of chemical elements. Moreover, the transmission of topological ideas from their Göttingen ori-

[17] For treatments of these developments from a physical point of view, see (Whittaker 1951), (Silliman 1963), (Smith and Wise 1989), and in particular the concise study (Siegel 1981).

gins to Scotland went well beyond a mere awareness of the literature on the part of the Scottish physicists. Oral and written communication were involved, the messenger from Germany being first Helmholtz and later Listing. An intense exchange of ideas in correspondence accompanied the efforts to understand the new ideas. In the course of these communications and subsequent uses of topological notions and techniques, the Scottish physicists became more and more aware of the fact that they were involved in an ongoing process of the formation of a new mathematical field, and they gradually produced a significant body of pre-disciplinary topological results.

Helmholtz studies "Wirbelbewegung"

§ 10. The event which in many ways triggered the developments in question was the publication of an article by Hermann v. Helmholtz in the 1858 volume of Crelle's Journal, *Ueber Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen.*[18] In this article, Helmholtz dealt with the equations of the motion of a perfect fluid, i.e. an incompressible fluid without friction, in the case where the global existence of a velocity potential was not assumed. Helmholtz wrote these equations (often referred to as Euler's equations) in the form

$$\begin{aligned} X - \frac{1}{h}\frac{dp}{dx} &= \frac{du}{dt} + u\frac{du}{dx} + v\frac{du}{dy} + w\frac{du}{dz} \\ Y - \frac{1}{h}\frac{dp}{dy} &= \frac{dv}{dt} + u\frac{dv}{dx} + v\frac{dv}{dy} + w\frac{dv}{dz} \\ Z - \frac{1}{h}\frac{dp}{dz} &= \frac{dw}{dt} + u\frac{dw}{dx} + v\frac{dw}{dy} + w\frac{dw}{dz} \\ 0 &= \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}, \end{aligned}$$

where u, v, w are the velocity components of the fluid at a point with coordinates x, y, z, the components of the external force being given by X, Y, Z whereas h and p denote the density and the pressure of the fluid, respectively. All functions involved were implicitly supposed to be as smooth as calculations required.[19] In earlier treatments, mainly by Euler and Lagrange, it had usually been supposed that the fluid velocity derives from a potential function, i.e. that there exists a function φ such that

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[18] Brief discussions of Helmholtz's article can be found in (Silliman 1963), (Siegel 1981), (Buchwald 1985, appendix 6), (Archibald 1989).

[19] How this system of equations came to be written in a modern, vectorial form like

$$\begin{aligned} \vec{F} - \frac{1}{h}\nabla p &= \frac{\partial \vec{v}}{\partial t} + (\vec{v}\cdot\nabla)\vec{v}\,, \\ 0 &= \nabla\cdot\vec{v}\,, \end{aligned}$$

is part of the story to be told, so unless stated otherwise, I adhere to the original notations.

$$u = \frac{d\varphi}{dx}, \quad v = \frac{d\varphi}{dy}, \quad w = \frac{d\varphi}{dz}.$$[20]

In this case of potential flows, the solution of the equations of motion could be reduced to finding appropriate solutions φ of the Laplace equation which results in this case from the last of the Euler equations. The pressure function p could then be found by means of simple integrations. At this point, Helmholtz added a remark and a footnote which came to be of importance later on. "It is well known," he wrote, "that every function φ, which satisfies the above [i.e. the Laplace] equation in a simply connected space may be expressed as the potential of a certain distribution of magnetic masses on the surface of the space." (Helmholtz 1858, 105 f.) This remark not only connected fluid motion and magnetism, but also introduced a notion which at that time was probably unknown to most of his readers: that of *simple connectivity*. Already in the introduction, which presented a survey of the novelties of the article, Helmholtz had given a definition of simple connectivity, extending Riemann's earlier notion to three dimensions:

> I take this expression in the same sense in which Riemann [in his article on Abelian functions, published in the previous volume of Crelle's Journal (Riemann 1857)] speaks of simply and multiply connected surfaces. An n-ply connected space is thus one in which $n - 1$, but no more cutting surfaces [*Schnittflächen*] can be placed without dissecting the space into two completely disjoint parts. A [solid] ring is thus, in this sense, a doubly connected space. The cutting surfaces must be completely bounded by the line in which they cut the surface bounding the space. (Helmholtz 1858, 103.)

The footnote which Helmholtz added to his remark on potentials in simply connected spaces continued by pointing to a mathematical difficulty that arose if one considered *multiply connected* regions of space. In this case, he argued, there exist multi-valued functions satisfying the Laplace equation (a local condition), and for such functions Green's basic integral formula as well as various of its consequences were no longer valid.

This remark about potentials in topologically nontrivial regions actually had to do with Helmholtz's problem proper, the study of flows which were *not* supposed to be potential flows, as he set out to show. Locally, a potential function existed if and only if the set of equations

$$\frac{du}{dy} - \frac{dv}{dx} = 0, \quad \frac{dv}{dz} - \frac{dw}{dy} = 0, \quad \frac{dw}{dx} - \frac{du}{dz} = 0$$

was satisfied. Accordingly, Helmholtz began his investigation by developing a mechanical interpretation for these equations. Using infinitesimal considerations based on Euler's equations, he showed that the instantaneous movement of an infinitely small portion of the fluid could be decomposed into a *translation*, an *expansion* and a *compression* along three principal, mutually orthogonal directions, as well as a *rotation* around an axis with direction cosines proportional to

$$\frac{dv}{dz} - \frac{dw}{dy} =: \xi, \quad \frac{dw}{dx} - \frac{du}{dz} =: \eta, \quad \frac{du}{dy} - \frac{dv}{dx} =: \zeta,$$

[20] Euler, though, had also pointed to solutions without a potential; see (Helmholtz 1858, 103 f.).

and with angular velocity proportional to the square root of $\xi^2 + \eta^2 + \zeta^2$.[21] This consideration must be viewed as the origin of the notion of the rotation of a vector field; how vectors came in will be discussed below.

A flow that did not everywhere admit local potential functions was thus rightly called a *Wirbelbewegung*: At least in some points of the fluid, the fluid particles experienced an infinitesimal rotation as just described. To capture these rotations, Helmholtz introduced two new terms: *vortex lines (Wirbel fäden)*, defined as lines the direction of which coincided everywhere with the local direction of the axis of rotation of the fluid (in modern terms: integral curves of the rotation field), and *vortex tubes (Wirbelfäden)*, bundles of vortex lines emerging from infinitely small area elements transverse to the rotation axes (see Fig. 3). Helmholtz went on to establish three important propositions about this rotary motion under the supposition that the external forces were derived from a potential function. First, no particle of the fluid which was not in rotation initially would ever begin to rotate. Second, those fluid particles constituting a vortex line at a given moment would constitute a vortex line for all times. Thus it was possible to speak of "the same" vortex line moving along in the fluid. Third, the product of the area of a cross section of a vortex tube and the angular velocity of the rotation at that point (called the *strength* of the vortex tube) was constant along the tube and in time. From this last proposition, Helmholtz concluded that vortex tubes either had to run back into themselves or else end at the boundary of the space in which the whole fluid was contained (Helmholtz 1858, 103).

That Helmholtz was ready to draw this last conclusion shows that he had not considered the topological features of fluid flows very seriously. In fact, viewed from a modern perspective, Helmholtz's three propositions are sound theorems even for *finite* vortex tubes, but the assertion about the closing of vortex tubes requires further qualifications since topological complications like branching or aperiodic vortex lines may arise (see Fig 2.)[22]

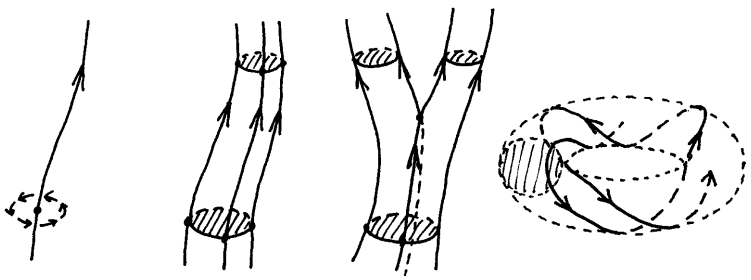


Fig. 2. Vortex lines, vortex tubes and topological complications (small arrows indicate the motion of the fluid)

[21] Here and in the following, I have suppressed a factor 2 which occurs on the right hand sides of these equations. In modern terms, the second and third component of Helmholtz's decomposition correspond to the symmetric and the antisymmetric part of the differential of the flow map, $d\Phi_t : \mathbb{R}^3 \to \mathbb{R}^3$, in the limit $t \to 0$.

[22] This criticism seems to be fairly recent. See (Chorin and Marsden 1992, 27).

Unaware of such difficulties, the hydrodynamical textbook tradition of the 19th century typically regarded the implication of Helmholtz's third theorem as a proved proposition.[23] In any case, Helmholtz's theorems correctly showed that if a closed vortex line existed at a given instant in a perfect fluid, those fluid particles of which it consisted would forever remain on a closed vortex line, however distorted in shape.

§ 11. The above theorems led Helmholtz to look at the problem of vortex motion in the following way: Suppose the dynamics of the vortex lines, i.e. the field (ξ, η, ζ), are known. Was it then possible to determine the motion of a fluid as a whole? The answer was yes and the solution again depended on a simple application of potential theory. For a fixed point of time, Helmholtz looked for new quantities P, L, N, M satisfying

$$\begin{aligned}
\frac{d^2L}{dx^2} + \frac{d^2L}{dy^2} + \frac{d^2L}{dz^2} &= \xi \\
\frac{d^2M}{dx^2} + \frac{d^2M}{dy^2} + \frac{d^2M}{dz^2} &= \eta \\
\frac{d^2N}{dx^2} + \frac{d^2N}{dy^2} + \frac{d^2N}{dz^2} &= \zeta \\
\frac{d^2P}{dx^2} + \frac{d^2P}{dy^2} + \frac{d^2P}{dz^2} &= 0
\end{aligned}$$

and appropriate boundary conditions. Then a solution of the Euler equations was given by:

$$\begin{aligned}
u &= \frac{dP}{dx} + \frac{dN}{dy} - \frac{dM}{dz} \\
v &= \frac{dP}{dy} + \frac{dL}{dz} - \frac{dN}{dx} \\
w &= \frac{dP}{dz} + \frac{dM}{dx} - \frac{dL}{dy}\,.
\end{aligned}$$

Of course, Helmholtz knew how to find L, M, N and P by the standard integral formulas.[24]

The point of this solution was that it allowed Helmholtz to extend the hydrodynamic-electromagnetical analogy to the case of flows without a velocity potential. The motion of the fluid at a given point of time turned out to be of exactly the same form as the magnetic field induced by a stationary distribution of electrical currents corresponding to the given distribution of vortex lines in the fluid. Due to developments which will be described below, this analogy is nowadays simply expressed by the corresponding Maxwell equation for the vacuum (ignoring constants):

$$\operatorname{rot} \vec{H} = \vec{j}\,.$$

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[23] See for instance (Kirchhoff 1876, 169), (Lamb 1879, 149), (Love 1887, 326).

[24] See (Helmholtz 1858, 116). In modern terms, Helmholtz decomposed the velocity field into a gradient and a pure rotation $\vec{v} = \nabla P + \operatorname{rot} \vec{A}$, where $\vec{A} = (L, M, N)$ denotes what we would call a vector potential. Modern authors call this the "Helmholtz decomposition" of vector fields; as such it forms one of the starting points of Hodge theory; see e.g. (Schwarz 1995, 1).

Helmholtz worked it out by verifying the analogue of the law for the force exerted on a "magnetic particle" (a magnetic monopole) by an infinitesimal current element.[25] The analogy could be made both ways: it could be used to visualize vortex motion in fluids, but it could also be used to visualize electromagnetical induction by fluid motion. These possibilities, which form part of a general tendency in 19th-century natural philosophy to think in terms of analogies between different domains of physical phenomena, would become important later on when they piqued Maxwell's interest in vortex motion.[26]

The hydrodynamic-electromagnetical analogy induced Helmholtz to consider the following special type of vortex motion. He supposed the vortex lines of a perfect fluid to be confined to a certain finite number of distinct vortex tubes (which need not be infinitesimally thin). In this situation, the region *outside* these vortex tubes was multiply connected, while the flow in this region satisfied

$$\xi = \eta = \zeta = 0,$$

so that locally a velocity potential existed. Supposing the motion and strength of the vortex tubes to be known, the solution of the Euler equations amounted to finding what contemporary authors called a many-valued potential function in the region surrounding the moving vortices. In addition, appropriate boundary conditions on the surfaces of the vortex tubes and on the bounding surface of the spatial region under consideration had to be satisfied. Thus, for a great number of cases, the study of vortex motion was equivalent to potential theory in multiply connected regions.

It was at this point that topology entered the discussion in a crucial way. Helmholtz was quite aware of this implication, as is indicated by a further gesture toward Riemann that he made in discussing his results. Integrals of the hydrodynamical equations, he wrote, which were based on a single-valued velocity potential, could be called "integrals of the first kind" of the fluid equations, whereas he proposed to call solutions with many-valued potentials belonging to the cases just discussed "integrals of the second kind." (Helmholtz 1858, 120.) This extended Riemann's terminology for the classification of Abelian integrals to the three-dimensional situation of potential flows.

§ 12. Helmholtz left the general discussion at that point. We shall see that the Scottish physicists took it up again and precisely at the point where Helmholtz had left it, giving the problem the title of the "most general motion of a fluid." Helmholtz concluded his paper by discussing some special cases. All of them were of the type described above. In particular, he treated the case where rotational motion was confined

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[25] See (Helmholtz 1858, 118). Helmholtz did not attribute this law, originally formulated by the French physicists Biot and Savart and further discussed by Ampère, to some particular scientist. It is thus unclear from whom Helmholtz took the idea. The interaction between current elements and "magnetic particles" had received various discussions in the German context, for instance by Gauss and W. Weber.

[26] See below, § 16. For a discussion of physical analogies, see (Knudsen 1976), (Siegel 1981, 240 ff.), (Wise 1981), and (Knudsen 1985). In most cases, the analogy consisted in the fact that phenomena were described by solutions of Laplace's equation. Consequently, these phenomena admitted a description in terms of potential flows. Helmholtz also referred to this wider context of analogies; see (Helmholtz 1858, 119 f.).

to straight, parallel vortex tubes, and the case of a closed circular vortex ring. In finding the concrete solutions, Helmholtz used another idea which played a major role in his thinking, the conservation of energy. To deal with the circular vortex ring, Helmholtz also had to bring in elliptic integrals. In both cases, the solutions implied that straight or circular vortex tubes could exist only together with a motion of the parts of the fluid at infinite distance from the vortices; a fact which could be interpreted as saying that the vortex tubes acted at a distance. In particular, two parallel, straight vortex tubes would revolve around each other with a certain speed determined by their vortex strengths, and a single circular ring would move along its symmetry axis with a speed depending on its vortex strength. Helmholtz added a description of the behaviour to be expected from the movement of two circular vortex rings situated on the same symmetry axis:

> If they both have the same direction of rotation they will proceed in the same sense, and the ring in front will enlarge itself and move slower, while the second one will shrink and move faster; if the velocities of translation are not too different, the second will finally reach the first and pass through it. Then the same game will be repeated with the other ring, so that the rings will pass alternately one through the other. (Helmholtz 1858, 133.)

In the case of rotation in opposite directions, both rings slow down and enlarge. In the completely symmetric case of two equal rings on a line revolving in opposite directions, the normal component of the fluid motion in the symmetry plane is zero and one could thus imagine a blockading wall situated there. While Helmholtz hinted at how to produce nice semi-circular vortices with a spoon in a tea cup, he apparently did not think of performing more elaborate experiments to confirm his calculations.

Tait reads Helmholtz

§ 13. A Scottish physicist, Peter Guthrie Tait, however, did, and with great success. Tait read Helmholtz's article in July 1858 and immediately made an English translation for his personal use. At this juncture, it was not the study of vortex rings which aroused his interest, but the mathematics of the opening paragraphs of the paper. Helmholtz's discussion of the infinitesimal motion of a perfect fluid reminded him of some formulae he had read five years earlier in Hamilton's *Lectures on Quaternions*. Sparked by this connection, Tait began to think seriously about quaternion analysis and its use in mathematical physics. In August 1858, he began an extensive correspondence with Hamilton which represents an important step in the history of the emergence of vector calculi.[27] Some months later, Tait described the event in a letter to Hamilton. The latter had asked Tait to tell him how he got involved with quaternions. Answering this query, Tait reported that he had begun to read Hamilton's *Lectures* already in 1853 but that other interests (more on the physical and experimental side) had prevented him from pursuing quaternions further. Then he continued as follows:

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[27] See (Knott 1911, ch. 4), (Crowe 1967, ch. 4), (Ewertz 1995).

> [...] it was only in August last that I suddenly bethought me of certain formulae I had admired years ago at p. 610 of your Lectures – and which I thought (and still think) likely to serve my purpose exactly. (The matter which more immediately suggested this to me was a paper by Helmholtz's in Crelle's Journal (Vol. LV) which I was reading in July last as soon as we received it, and which put the subject of Potentials before me in a very clear light. The title (in German) I forget – but an MS translation of my own which I have now beside me is headed 'Vortex Motion.' It refers to the integration of the general equations in Hydrodynamics when $udx + vdy + wdz$ is not a perfect differential.)[28]

What apparently captured Tait's attention was the physical picture Helmholtz's paper provided for the quaternion formulae he had learned earlier. Tait recognized that Helmholtz's decomposition of the motion of a perfect fluid into translation, rotation and deformation was expressible by quaternions in a simple and elegant way. In the following years, Tait worked out this insight and published the results in a series of notes, many of which appeared in the *Proceedings of the Royal Society of Edinburgh*, a journal which would become the publication forum for many of the contributions to our story.

It would take us too far away from the present subject to describe Tait's quaternionic arguments in any detail. Suffice it to say that the field of Helmholtz's vortex line elements admitted a particularly simple redescription. Using the quaternionic notation then in use – with i, j, k representing the imaginary units of the quaternions, $V\alpha$, $S\alpha$, $T\alpha$ denoting the vector part, scalar part, and length of a quaternion α, respectively, and

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz}$$

standing for Hamilton's differential operator – the velocity field of a fluid (u, v, w) was interpreted as what Hamilton and Tait called a "vector function," i.e. a function $\sigma = ui + vj + wk$ defined on a region of Euclidean space with values in the quaternions and vanishing scalar part. The local rotations were then determined by the direction and length of the vector part of Hamilton's differential operator, applied to the given velocity field:

$$V.\nabla\sigma = -(\frac{dv}{dz} - \frac{dw}{dy})i - (\frac{dw}{dx} - \frac{du}{dz})j - (\frac{du}{dy} - \frac{dv}{dx})k\ .$$

Similarly, the scalar part of the same quantity,

$$S.\nabla\sigma = -(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz})\ ,$$

represented what Tait called the "cubical compression" during the infinitesimal motion of a fluid with velocity field σ.[29]

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[28] Tait to Hamilton, 7 December 1858; quoted in (Knott 1911, 127).

[29] When preparing his *Treatise on Electricity and Magnetism*, Maxwell proposed to call $V.\nabla\sigma$ the "curl" of the vector field σ; in the second edition he changed this name to "rotation." For $S.\nabla\sigma$, Maxwell preferred "convergence," and Clifford was responsible for a change of sign and the modern term "divergence." See (Crowe 1967, 131–135).

In this way, Hamilton's calculus, and in particular the differential calculus associated with it, admitted a very intuitive physical interpretation: the basic differential operations on a "vector function" could be visualized by the kinematics of the fluid flow that had the given function as its velocity field. But not only quaternion analysis profited from acquiring a new, physical meaning. Quaternion formulae also helped to grasp physical situations which could be described in terms of fluid motion more easily. For instance, Tait reworked Helmholtz's hydrodynamic-electromagnetical analogy in quaternion language. In particular, he rewrote the differential form involved in Biot and Savart's law for the force exerted by an infinitesimal current element α on a magnetic particle in the quaternion form

$$\sigma = -\frac{V.\alpha\rho}{T\rho^3} ,$$

where ρ denoted the distance vector between current and magnetic particle (Tait 1860, 23). Once quaternion expressions were understood, such a formula captured the geometry of the situation very clearly. Given the physical beliefs of the time, which relied heavily on all kinds of analogies to flow phenomena, quaternion analysis seemed to Tait an ideal mathematical instrument, and he felt justified in asserting "that the next grand extensions of mathematical physics will, in all likelihood, be furnished by quaternions." (Tait 1863, 117.) The amalgamation of quaternion analysis and flow thinking, made possible by Helmholtz's treatment of vortex motion, thus served as the central motivation behind the crusade for quaternion methods which Tait was about to initiate.

After Helmholtz's results had found Thomson's interest for reasons that will be described below, Tait not only published his translation of Helmholtz's paper (Tait 1867) but he also showed how to write the basic equations of fluid motion in a condensed form using quaternions. In a short note entitled *On the most general motion of an incompressible perfect fluid* (Tait 1870a), he presented them as follows:

$$\begin{aligned} \nabla P - \tfrac{1}{r}\nabla p &= D_\sigma \sigma \\ S\nabla\sigma &= 0 , \end{aligned}$$

where r was the density of the fluid, P the potential of the applied forces, and D_σ denoted, for a given vector $\sigma = ui + vj + wk$, the differential operator

$$D_\sigma = \frac{d}{dt} + u\frac{d}{dx} + v\frac{d}{dy} + w\frac{d}{dz} .$$

This rewriting of the fluid equations represented an important step toward the modern, vectorial formulation. With great ease, Tait could now rederive some of Helmholtz's statements from some general formulae of vector analysis (or, more precisely, quaternion analysis). An example was the fact that $D_\sigma \nabla\sigma$ vanishes whenever $\nabla\sigma$ does, which implied (by taking vector parts) that fluid particles moving irrotationally at one instant would continue to move irrotationally forever.

In the same year, Tait published a treatment of the basic mathematical tools for fluid motion and potential theory, entitled *On Green's and other allied theorems* (Tait 1870b). Again, the physical interpretation of Hamilton's calculus enabled Tait to reduce the respective results to one basic quaternion formula. This earned Tait great praise from Maxwell's side. In an undated letter sent to Thomson sometime in 1871, Maxwell wrote:

> You should let the world know that the true source of mathematical methods as applicable to physics is to be found in the Proceedings of the Edinburgh F.R.S.E's. The volume- surface- and line-integrals of vectors and quaternions and their properties as in the course of being worked out by T' is worth all that is going on in other seats of learning.[30]

Those British scientists who followed Tait's innovations, either with sympathy like Maxwell or Clifford, or with reservation like Thomson, were aware of their roots in Helmholtz's treatment of vortex motion. For instance, Clifford, whose interest in quaternions will play an important role in the second part of this study, noted in 1877 that Helmholtz's procedure for finding the velocity of a fluid given its vorticity came down to solving the basic quaternionic integration problem, i.e. to determine a quaternion function, $\sigma : \mathbb{R}^3 \to \mathbb{H}$, such that $\nabla\sigma$ equals a given quaternion function (Clifford 1877, 407).[31]

Thomson speculates about matter

§ 14. Quaternion or vector methods (still subsumed under the former) helped Tait and others to elaborate on the mathematical description of vortex motion of a continuous medium. In this way, the ground was also prepared for an understanding of some topological features of fluid motion. The crucial step in this respect was taken by William Thomson, who, however, did not share Tait's enthusiasm concerning quaternion methods.[32] In order to pique Thomson's interest, a less mathematical and more experimental approach to vortices was needed. Again, Tait played the key role. Tait had been so impressed by Helmholtz's general theorems on the dynamical invariance of vortices that he eventually decided to illustrate these in his physics lectures by carefully devised experiments with smoke rings.[33] Tait used two boxes with a circular hole on one side and a rubber diaphragm on the opposite side. Within the boxes, a chemical agent (magnesium sulfate) produced a thick, white smoke. When struck on the rubber diaphragm, circular smoke rings shot out of the holes. The boxes could be placed in various positions, causing the smoke rings to interact just as Helmholtz had indicated (see Fig. 3).

Since Thomson's and Tait's own account, the story that one of Tait's earliest performances of the smoke-ring experiments inspired Thomson to his far-reaching speculation on vortex atoms belongs to the stock of history of physics.[34] Seeing how the smoke rings preserved their identity when interacting with each other, Thomson hit upon an idea:

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[30] Maxwell to Thomson, ca. 1871. Glasgow University Library, Kelvin Papers, M 32. This collection will in the following be denoted by Kelvin Papers Glasgow.

[31] For further details on Tait's campaign for quaternion methods and their physical applications see (Ewertz 1995).

[32] See e.g. (Knott 1911, 185), (Crowe 1967, 119 f.).

[33] Quite generally, Tait placed high value on experimental illustrations as a means of teaching; cf. his *On the teaching of natural philosophy* (Tait 1878). On Tait's teaching, see also (Wilson 1991).

[34] See (Thomson 1867), Tait (1876a, 290 ff.), (Thompson 1910, 510 ff.), and (Knott 1911, 68 f.).

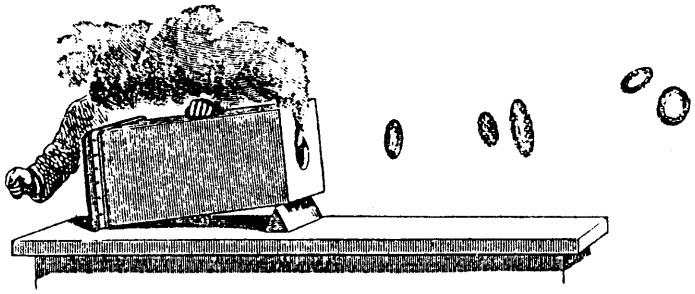


Fig. 3. Tait's smoke rings (Tait 1876a, 292)

why should not closed vortices provide the kind of stable dynamical configuration in a universal medium in accordance with his general beliefs regarding the nature of atoms?

As a matter of fact, the development of Thomson's ideas was slightly more complicated. Recent authors have pointed out that since the mid-1850's, Thomson had explored various ways of conceiving the space between the smallest parts of matter as filled with a continuous and material medium undergoing rotary motions around material atoms and molecules, "vortical or other."[35] These ideas drew on earlier ideas by the British physicist and engineer W. J. M. Rankine and on Thomson's own interpretation of Faraday's discovery of the influence of magnetism on the polarisation of light. At that time, Thomson was still undecided about whether to consider material atoms as a different kind of body from those comprising a continuous space-filling medium, or as parts of the fluid medium itself. As there was no clear empirical evidence indicating how to make these ideas precise, and since sufficiently developed mathematical methods to treat such a complicated dynamics did not exist, Thomson's attempts to work out his speculations into a solid theory failed for the time being.[36] Given these circumstances, it may seem surprising that Thomson did not immediately pursue Helmholtz's ideas after becoming aware of them in 1858.[37] Thomson's delayed reaction may have been due to his occupation with other projects at the time, like the atlantic cable project or the writing of the *Treatise on Natural Philosophy* together with Tait. During the 1860's, however, general interest in atomistic conceptions was greatly enhanced by the progress in experimental

[35] (Thomson 1856, 200). See the studies by (Siegel 1981), (Knudsen 1985), and (Smith and Wise 1989, chs. 11 and 12), on which the following discussion is based.

[36] In the correspondence between Thomson and Stokes during these years, Stokes repeatedly took the position of a sceptic of Thomson's ideas; see (Smith and Wise 1989, 408–412).

[37] The two had first met each other during a stay in Bad Kreuznach, a health resort in the Rhineland, in the summer of 1856, and afterwards they developed a close personal relationship. Thomson read Helmholtz's paper in late 1858 and intended to discuss its contents with him, as he wrote to Helmholtz in May 1859; see (Thompson 1910, 402). Both advocated what might be called a hydrodynamical world view, and their correspondence repeatedly addressed hydrodynamical matters. For instance, in 1859, Helmholtz wrote to Thomson about the basic equations of fluid motion with friction, proposing a set of equations generalizing the one he had used in his 1858 paper (Cambridge University Library, Kelvin Papers, Add. 7342, H 65; in the following, I will refer to this collection as Kelvin Papers Cambridge).

spectrum analysis and statistical thermodynamics which made the need for a theory of atomic structure more and more urgent.

Ironically enough, another reason for the delay may have resulted from Tait's quaternionic reformulation of Helmholtz's ideas. By focussing on infinitesimal considerations, Tait's treatment may well have given Thomson the impression that Helmholtz's theory had an essentially abstract mathematical character rather than being of serious physical significance. In particular, Tait had never emphasized the global implication of Helmholtz's results, namely the permanence of the topological type of closed vortex tubes in the motion of an incompressible perfect fluid. It seems that it was precisely this idea of topological stability that Thomson finally grasped on the occasion of viewing Tait's experimental illustrations. Once Thomson recognized that Helmholtz's theorems enabled him to work out one of the variants of his earlier ether speculations, he became seriously engaged in developing a theory of vortex atoms in which topological considerations came to play a crucial role. This is documented already in the first, enthusiastic letter on his new ideas that Thomson wrote to Helmholtz in January 1867:

> Just now, however, Wirbelbewegungen have displaced everything else, since a few days ago Tait showed me in Edinburgh a magnificient way of producing them. [...] We sometimes can make one ring shoot through another, illustrating perfectly your description; when one ring passes near another, each is much disturbed, and is seen to be in a state of violent vibration for a few seconds, till it settles again into its circular form. [...] The vibrations make a beautiful subject for mathematical work. The solution for the longitudinal vibration of a straight vortex column comes out easily enough. The absolute permanence of the rotation, and the unchangeable relation you have proved between it and the portion of the fluid once acquiring such motion in a perfect fluid, shows that if there is a perfect fluid all through space, constituting the substance of all matter, a vortex ring would be as permanent as the solid hard atoms assumed by Lucretius and his followers (and predecessors) to account for the permanent properties of bodies (as gold, lead, etc.) and the differences of their characters. Thus, if two vortex rings were once created in a perfect fluid, passing through one another like links of a chain, they never could come into collision, or break one another, they would form an indestructible atom; every variety of combinations might exist. Thus a long chain of vortex rings, or three rings, each running through each of the other, would give each very characteristic reactions upon other such kinetic atoms.[38]

In these lines, Thomson clearly spelled out the core of his speculations, namely that closed vortex lines or tubes manifested the required permanence and stability. Thus, topologically different, linked or knotted vortices might account for the variety of chemical elements. At the same time, the smoke ring experiment showed that collisions between vortex rings produced vibrations and thus hinted at a possible explanation of spectra. As Maxwell later pointed out in a very favourable review of Thomson's theory in the

[38] Thomson to Helmholtz, 22 January 1867; quoted in (Thompson 1910, 513–516).

Encyclopedia Britannica, these ideas represented the first candidate for an atomic theory which "saved appearances," and did so not by introducing new hypotheses for every aspect of atoms to be accounted for but by deriving them from the laws of motion of a more primitive object, the universal medium (Maxwell 1875, 471). In particular, the unity of a dynamical theory of physical phenomena was preserved. Actually, Thomson felt his ideas had one additional feature that recommended them, namely they left a role open for a creative power who could once and for all institute vortex motion in the universal plenum. Thus not only physical appearances and dynamical theory could be saved but theology as well![39]

§ 15. Thomson's bold speculations were more akin to a huge research program than a specific, well-pondered physical conjecture. The main task of working out the program would lie in the hands of "pure mathematical analysis," as Maxwell later put it. It was clear that the mathematical methods required would be difficult to employ and, in parts, they would even have to be created virtually from scratch. By 1875, Maxwell's verdict was still: "the difficulties of this method are enormous, but the glory of surmounting them would be unique." (Maxwell 1875, 472.) Very much the same feeling was expressed in Thomson's first paper on vortex atoms (Thomson 1867, 2). There, Thomson also pointed out that one part of the mathematical difficulties he anticipated had to do with the topological features of vortex motion.

> Diagrams and wire models were shown to the Society to illustrate knotted or knitted vortex atoms, the endless variety of which is infinitely more than sufficient to explain the varieties and allotropies of known simple bodies and their mutual affinities. It is to be remarked that two ring atoms linked together or one knotted in any manner with its ends meeting, constitute a system which, however it may be altered in shape, can never deviate from its own peculiarity of multiple continuity. Thomson 1867, 3.)

The last six words represent a first attempt to hint at what we would call the topological type of the knot or link in space. It is clear from Thomson's work that he did not distinguish this from the topological type of the space region *surrounding* the knot or link, i.e. its complement.[40] In the same paper, Thomson tried to distinguish between "degree and quality of multiple continuity." While the former notion, later translated by Tait as "degree of connectivity" in order to avoid confusion with the usual idea of continuity,[41] referred to Riemann's and Helmholtz's *Zusammenhangsordnung*, the latter expression apparently was intended to capture those aspects of knots and links which

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[39] See (Thomson 1867, 1). For further information on the metaphysical background of vortex atoms, see Section V below.

[40] The 19th-century physicists had definitely no means to imagine that the topological type of two knot or link complements might be the same while the knots or links themselves were inequivalent. The question was first raised by (Tietze 1908). (Whitehead 1937) then pointed out by a simple example that different *links* could indeed have homeomorphic complements, but it took a long time before (Gordon and Luecke 1989) proved that this cannot happen for *knots*.

[41] See Tait to Thomson, 18 May 1867; Kelvin Papers Glasgow, T 81. In this letter, Tait reported on his decision to translate the German *zusammenhängend* as 'connected' rather than 'continuous;' moreover Tait indicated that he had been in touch with Helmholtz concerning the translation.

were *not* determined by the *Zusammenhangsordnung*. Thomson was prepared to admit that he was "not yet sufficiently acquainted with Riemann's remarkable researches on this branch of analytical geometry to know whether or not all the kinds of multiple continuity now suggested are included in his classification and nomenclature." (Thomson 1867, 10.) Thomson soon learned that "this kind of analytical geometry" – for the case of three dimensions – had not yet advanced beyond Helmholtz's adaptation of Riemann's *Zusammenhangsordnung* to regions in space, and that vortex atom theorists would have to work out the subject for themselves to the extent it was needed.

Tait, whose experiments had suggested the whole project to Thomson, seems to have been a little sceptical about this flight of imagination initially. Before Thomson presented his first paper at the crucial session of the Royal Society of Edinburgh, Tait pointed out to him what Helmholtz had already emphasized: "Your atoms act on each other at a distance as *two small magnets*. The true application of Vortex atoms is to *electricity*."[42] Nevertheless, Tait promised that the paper would be read, "amply illustrated by drawings and experiments – in both of which Crum Brown will assist." In this way, also Tait and his brother-in-law, the Edinburgh chemist Alexander Crum Brown, came face to face with topological issues for the first time. Tait's drawings of knots, printed with Thomson's second paper on vortex atoms (see Fig. 4), display the same perfection so characteristic of his later tables of knots. For the meeting, Tait also perfected his smoke ring experiments. Tait's letter of 11 February 1867 captures nicely the spirit in which he was working: "Have you ever tried plain air in *one* of your two boxes. The effect is very surprising. – But eschew NO_5 and Zn. *The true thing* is $SO_3 + NaCl$. Have the NH_3 rather in excess – and the fumes are very dense + not unpleasant. NO_5 is DANGEROUS. – Put your head into a ring and feel the draught." (L.c.) The session was successfully held on 18 February, and by April 1867, the scientific public could read Thomson's bold claims in the *Proceedings* of the R.S.E.

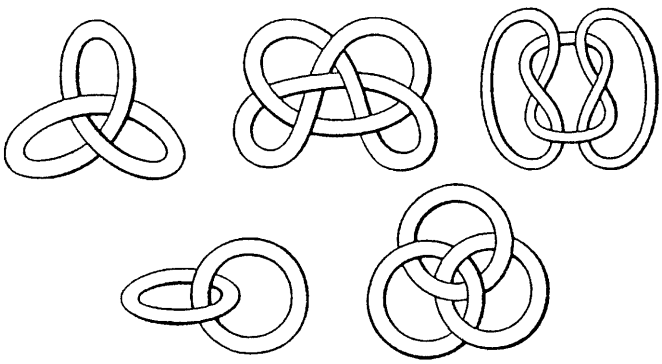


Fig. 4. Candidates for atoms? Knots and links from (Thomson 1869)

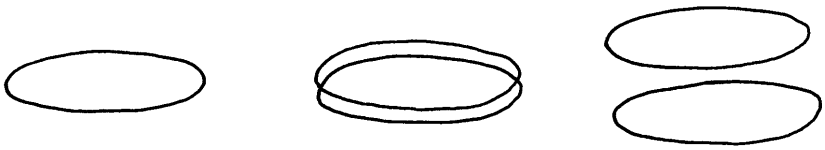
[42] Tait to Thomson, 11 February 1867; Kelvin Papers Glasgow, T 74. Here and in the following, all emphasis is in the originals.

Enter Maxwell

§ 16. Tait's warning that perhaps vortex motion was more relevant for electromagnetism than for the constitution of matter apparently did not impress Thomson very much. For Tait's former schoolmate, James Clerk Maxwell, the warning would hardly have been necessary since electricity and magnetism were the focus of his attention anyway. Soon after Thomson and Tait started to discuss vortex atoms and their possible topological complications, Maxwell became interested, too. His own kind of analogical thinking had made him sensitive to connections between fluid dynamics and electromagnetism, and he had been aware of the hydrodynamic-electrodynamical analogy already for some years.[43] Thus when Maxwell learned of Thomson's speculation, he tried to extract and elaborate what was new to him. The main new ingredient, however, consisted in the topological ideas. In the course of a few months, Maxwell systematically learned what was known about this branch of mathematics, documenting his newly gained insights in several manuscripts. Since this material is not very well known, a fairly detailed description of Maxwell's engagement with topology will be included in this section.

In November 1867, Maxwell wrote to Tait:[44]

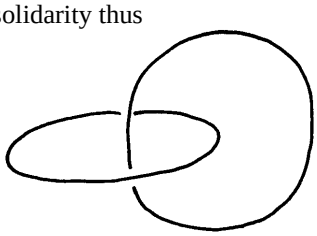
> Dear Tait,
>
> If you have any spare copies of your translation of Helmholtz on 'Water twists' I should be obliged if you could send me one. I set the Helmholtz dogma to the Senate house [i.e. the Cambridge Tripos] in '66, and got it very nearly done by some men, completely as to the calculation, nearly as to the interpretation. Thomson has set himself to spin the chains of destiny out of a fluid plenum as M. Scott set an eminent person to spin ropes from the sea sand, and I saw you had put your calculus in it too. May you both prosper and disentangle your formulae in proportion as you entangle your worbles. But I fear that the simplest indivisible whirl is either two embracing worbles or a worble embracing itself. For a simple closed worble may be easily split and the parts separated



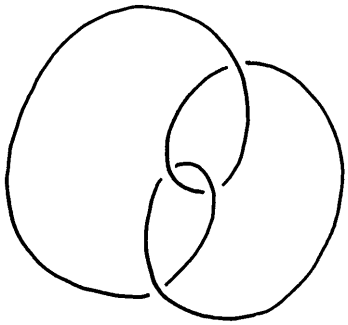
[43] In a note to (Maxwell 1861), Helmholtz's analogy was mentioned. See also (Siegel 1985, 191 f.) and item no. 254 in (Maxwell 1995).

[44] The Maxwell-Tait correspondence is available from Cambridge University Library as a microfilm, Add 7655. Harman's edition (Maxwell 1990/1995) gives the Maxwell part; a few of Tait's letters have been published in (Knott 1911). In the following, only the dates of their letters will be given.

but two embracing worbles preserve each others solidarity thus



though each may split into many, every one of the one set must embrace every one of the other. So does a knotted one.



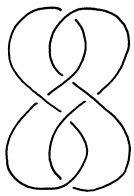
yours truly
J. Clerk Maxwell.[45]

Tait immediately passed the letter on to Thomson, commenting: "How about the enclosed from Maxwell? He doesn't know that we had done all sorts of knots – but his splitting up suggestion seems curious if true."[46] In his next letter, Maxwell reported on further thoughts he had devoted to the issue. This time he made it clear that electromagnetism provided the physical background of his interest:

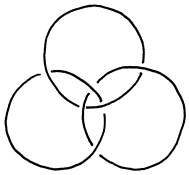
> I have amused myself with knotted curves for a day or two. It follows from electromagnetism that if ds and $d\sigma$ are elements of two closed curves and r the distance between them and if $l\ m\ n$, $\lambda\ \mu\ \nu$, and $L\ M\ N$ are the direction cosines of $ds\ d\sigma$ & r respectively then
>
> $$\int\int \frac{ds\ d\sigma}{r^2} \begin{vmatrix} L & M & N \\ l & m & n \\ \lambda & \mu & \nu \end{vmatrix} = [\ldots] = \ 4\pi n$$
>
> the integration being extended round both the curves and n being the algebraical number of times that one curve embraces the other in the same direction. If the curves are not linked together $n = 0$ but if $n = 0$ the curves are not necessarily independent. In [a] the two closed curves are inseparable but $n = 0$. In [b] the 3 closed curves are inseparable but $n = 0$ for every pair of them. [c] is the simplest single knot on a single curve. The simplest equation I can find for it is

[45] Maxwell to Tait, 13 November 1867; quoted in (Knott 1911, 106).
[46] Tait to Thomson, 18 November 1867; Kelvin Papers Glasgow, T 85.

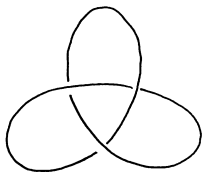
$r = b + a\cos\frac{3}{2}\theta \quad z = \sin\frac{3}{2}\theta$, when c is $-^{\text{ve}}$ as in the figure the knot is right handed, when c is $+^{\text{ve}}$ it is left handed. A right handed knot cannot be changed into a left handed one.



[a]

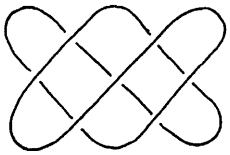


[b]

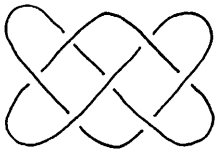


[c]

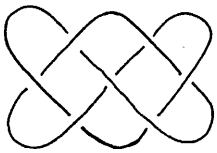
The curve $x = \sin 2\theta, \quad y = \sin 3\theta, \quad z = \sin(5\theta + \gamma)$ is knotted in different degrees according to the value of γ. When $\gamma = 0$ it is not knotted at all, when $\gamma = \frac{\pi}{3}$ it begins to be knotted and when $\gamma = \frac{7}{12}\pi$ it is knotted in a different way but to the same degree.



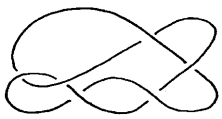
$\gamma = 0$ no knot



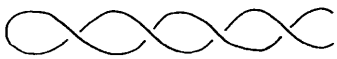
$\gamma = \frac{\pi}{3}$



$\gamma = \frac{7}{12}\pi$



$\gamma = \frac{\pi}{3}$, a knot = a righthanded twist of $4\frac{1}{2}\pi$ and then the ends linked together



The twist of $\frac{9}{2}\pi$

Yours truly
J. Clerk Maxwell[47]

These passages are remarkable in several respects. First, Maxwell singled out knots and links as objects deserving of mathematical study. Second, he pointed out that "from

[47] Maxwell to Tait, 4 December 1867.

electromagnetism" one could derive a number distinguishing links of different types. Here, Maxwell reproduced or rediscovered one of Gauss's earlier topological insights, an integral formula counting the linking number of two closed or infinite curves which Gauss recorded without proof in one of his notebooks in 1833. Gauss's formula represents the first topological invariant of links ever conceived, and he was perfectly aware of the relevance of his result. The fragment, however, was published only in 1867. The editor, the Göttingen physicist Schering, chose to place it among several other fragments relating to the law on electromagnetic induction since the latter involves the same differential form as the formula for the linking number. Actually, the double integral expresses precisely the work done by an imaginary magnetic particle when moved along a closed curve in the magnetic field induced by a closed circuit.[48] Maxwell's correspondence contains no hint that he was aware of Schering's edition at the time of writing his letter to Tait.[49] Thus it cannot be ascertained whether Maxwell merely recognized the relevance of Gauss's idea when it was published, or whether he invented the invariant for himself, drawing on his detailed knowledge of electromagnetic induction. In any case, his remark that there might be linked curves with vanishing linking number went beyond Gauss's earlier remarks.[50] Finally, Maxwell brought a new idea to the topic: to look for *equations* which describe knots or links. His examples all represented algebraic space curves, a class of curves that had rarely been singled out for detailed study by earlier mathematicians and certainly not from a topological point of view.

§ 17. Tait was pleased by Maxwell's interest in knots, although he had not yet seriously thought of dealing with the topic. He immediately replied by inviting Maxwell to send a paper on knots to the R.S.E.:

> Dear Maxwell,
> [. . .] Please to remember that you are a fellow of the R.S.E., and be good enough to send us a paper on Knots & their possible equations in 3 dimensions. We devised all your figures (and many more) long ago – (Crum Brown and I, working for Thomson) – but we never tried EQUATIONS. Give us a paper on them like a good fellow; whether for the *Trans.* or merely for the *Proc.*[51]

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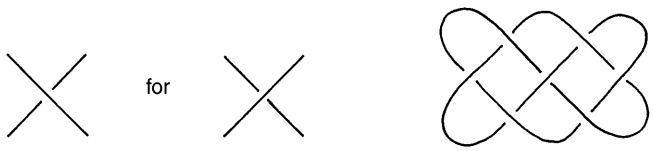
[48] See (Gauss 1867, 605); a discussion of this fragment is given in (Epple 1997). There I have shown that Schering's editorial choice was reasonable though not beyond criticism. Already in 1804, Gauss had encountered the phenomenon of linking in the context of astronomy. Tait, who had already rewritten the law of electromagnetic induction in quaternion form (see above, § 13), could easily translate Maxwell's argument into quaternion language. In fact, after Tait had encountered the linking number again in his work on knots (see below, § 35), this example furnished him with another paradigmatic illustration of the power of quaternion methods; see e.g. (Tait 1890).

[49] In later writings, he referred to Gauss's fragment, first in an undated excerpt, probably written in December 1868, which will be discussed below, § 26.

[50] The introduction of the linking integral in Maxwell's treatise might represent a way in which the formula was indeed found independently of Gauss. See below, § 45.

[51] Tait to Maxwell, 6 December 1867.

Tait was serious about the invitation.[52] Unfortunately for him, however, Maxwell did not send the solicited paper. His next letter only contained a short remark about Tait's proposal:

> With regard to knots I have drawn stereoscopically $x = \sin 2\theta$, $y = \sin 3\theta$, $z = \cos 7\theta$ which is the first real web [read: alternating knot] that I have got. If the middle crossing be reversed
>
> 
>
>
> it becomes a knot of the simplest kind.
>
> I have not got any R.S.E. matter on this but if they would like could knit net again. I have considerably improved the theory of reciprocal figures & diagrams of forces, which appeared in Phil. Mag. Ap. 1864.[53]

In the last paragraph, Maxwell referred to his work in graphical statics, which was based on the consideration of plane projections of polyhedral graphs in space (representing systems of forces) and projective duality.[54] Prior to 1867, this work was only superficially related to topology: Maxwell's first paper on the subject (Maxwell 1864) vaguely alluded to Euler's theorem on simply connected polyhedra. But when Maxwell finally kept his promise and sent a paper to the R.S.E. *Transactions* (Maxwell 1870a), the situation had changed. In the meantime, Maxwell had formed a clear conception of multiply connected polyhedra and was ready to apply it to diagrams of forces. Maxwell's work in graphical statics thus confirms that in the period before November 1867, topological ideas had not yet begun to play a central role in his research but that they did so only a short time later. To the best of my knowledge, no traces of a particular interest in *Analysis Situs* are present in his correspondence prior to the letters discussed above. Therefore, we may suppose that Maxwell's interest in topology was first raised by Thomson's speculations on vortex atoms. These ideas had pointed to the possibility that topological results could become an important factor in research on vortex motion in fluids, the constitution of matter and, most importantly for Maxwell's own work, for electromagnetism. In the course of 1868, Maxwell continued to follow Thomson's work on vortices, paying particular attention to the hydrodynamic-electromagnetical analogy and topology.

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[52] In a postscript to the letter, Tait added: "Ponder this proposition. A man of your *originality*, and *fertility*, and *leisure*, is undoubtedly bound to furnish to the chief Society of his native land, numerous papers, however short." (L.c., emphasis by Tait.)

[53] Maxwell to Tait, 11 December 1867.

[54] See (Scholz 1989, 181 ff.).

The Helmholtz-Bertrand controversy

§ 18. During the year 1867, Thomson set out to develop some mathematical parts of his new theory, while Tait prepared his translation of Helmholtz's memoir for publication in the *Philosophical Magazine.*[55] On April 29, Thomson read his second paper to the Royal Society of Edinburgh. On this occasion, Thomson embarked on the mathematics of vortex motion and the vibrations of vortices. The paper, however, was not published immediately. In July, Tait reminded Thomson to hand in a written contribution to the R.S.E., but apparently Thomson encountered significant difficulties in working out the material.[56] For more than one year, nothing became of Thomson's second paper, though he continued to work on it. Then, right in the middle of this work, he learned of a harsh criticism of the foundations on which the whole theory relied: the French mathematician Joseph Bertrand claimed that Helmholtz's 1858 paper was flawed from the outset. In a short note presented to the Paris *Académie des sciences* on June 22, 1868, Bertrand attacked Helmholtz's decomposition of the infinitesimal motion of a fluid into a translation, a deformation and a rotation (Bertrand 1868). As soon as Tait learned of Bertrand's criticism, he wrote to Thomson, who was spending a few days in the Bavarian health resort Bad Kissingen on the way back from Italy, where his wife had taken a cure:

> Do you see the Comptes Rendus? In that for the 22nd June (I think) Bertrand states that there is a mistake in the beginning of H^2's Vortex paper which renders ALL HIS RESULTS ERRONEOUS. So, of course, you may drop your vortex-atom-paper, and come home to useful work. B. does not point out the mistake, nor have I been able to find it – but H^2 will perhaps be able to tell you.[57]

Thomson, however, was already too deeply involved with the possibilities of his new theory to give up everything completely. Three days later, he sent his reply, focusing on the topological and energy considerations which stood at the core of his conception:

> It is a pity that H^2 is all wrong and we all dragged so deep in the mud after him. However we ought not to grudge France the glory (for once) of making an application of their characteristic exactness and clearsightedness so useful to others as this will be. Till yesterday afternoon when your letter came I had been seeing from time to time as I thought a little more about vortex motion, to shorten my paper [...] but, as you say, I may now discard the whole affair. I proposed to begin with irrotational motion and show the reform in its theory required by the footnote of H^2 tr^n p 488. I should have begun with the division irrotational non-cyclic/cyclic. Cyclic requires a core with double or multiple continuity.[58]

Thus, Thomson evidently intended to take up the discussion of vortex motion where Helmholtz had left it, starting from the special cases where vortex lines were confined

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[55] See Tait to Thomson, 18 May 1867, Kelvin Papers Glasgow, T 81.

[56] "Can't you send some speculations [...] on matter as made of  such things?" Tait to Thomson, 4 July 1867; Kelvin Papers Glasgow, T 84.

[57] Tait to Thomson, 2 July 1868; Kelvin Papers Glasgow, T 89. See also T 96.

[58] Thomson to Tait, 5 July 1868; Kelvin Papers Glasgow, T 90.

to a number of bounded regions in the fluid. Outside these, in a space which might be multiply connected, the motion was irrotational, i.e. locally a potential flow. Thomson went to some lengths in sketching qualitatively his findings on the latter "w^{h} would have been proved had the whole foundation not been cut away" (l.c.). Using *ad hoc* terms like the "degree of multiplicity of their hanging-together-ish-ness" for multiply connected regions, he developed his main result on irrotational flows in a region bounded by some external surface and the surfaces of several solids within the fluid. The latter had to be viewed as surfaces of the regions containing vortex lines and could therefore themselves be multiply connected. In a first step, Thomson discussed the crucial case of a fluid moving irrotationally around a ring-shaped solid in a large finite container:

> Let one of the solids be a ring stopped by an extensible membrane. By proper determinate pressure on the membrane (which is simple uniform all over it) institute a motion as that the velocity of the fluid will be equal on the two sides of the membrane. Then dissolve the latter into perfect liquid. A cyclic irrotational motion is thus instituted, and continues for ever unless again a membrane closes the ring + this time stops instead of starts the motion. Move the ring about, bend it, draw it out (its volume of course remaining constant) the cyclic motion has a constant quality, viz. the difference of fluid-velocity-potential in going once round any closed curve through the ring, remains always the same. This wants a name (? cyclic constant? only temporary).[59]

In the second step, Thomson discussed more complicated arrangements of internal bounding solids, stating qualitatively that they behave as if formed by a finite number of independent cyclic motions as above. Thus, for every arrangement of multiply connected solids in a large container, the possible irrotational flows of a perfect fluid around these solids were determined by some fixed number of parameters (provisionally named "cyclic constants") which only depended on the topology of the situation, not on the particular shapes of the bounding surfaces.

Here, in qualitative physical terms, Thomson hinted at a general theorem relating flows admitting locally a potential (in modern terminology: harmonic vector fields or 1-forms) in a multiply connected region of space and the topological type of such regions. It is unclear to what extent Thomson already understood this relation, but at least he had the crucial technical idea of determining the variety of possible flows by employing virtual membranes that stop fluid motion, thereby lowering the degree of connectivity of the region under consideration. Thomson's reasoning makes clear that with each such membrane he associated one cyclic constant characterizing the flow across it. On the other hand, it was not yet quite clear how to *use* this idea in the general situation of a region of space bounded by one external and several internal boundary surfaces of arbitrary shape. The key question remained: how many membranes with associated cyclic constants were actually needed in order to determine an irrotational flow?[60]

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[59] Thomson to Tait, 5 July 1868. Kelvin Papers Glasgow, T 90.

[60] The second strand in Thomson's sketch concerned energy considerations; these need not detain us here though they were crucial for Thomson's later studies of both the vibrations and the stability of vortices.

§ 19. At the end of his long report from Bad Kissingen, Thomson mentioned that Maxwell could perhaps help in working out the the ideas of his letter: "If you think it worthwhile and think he would make it out, you might send the above to Maxwell, as on a recent occasion he manifested a spirit of enquiry regarding vortices."[61] Tait followed Thomson's suggestion. Two weeks later, he received Maxwell's reaction, reassuring Tait about Bertrand's critique:

> I have received Thomson on vortices which I will return in a little, as I am reading it with pains. I do not see the Comptes Rendus, nor do I perceive, without the aid of Bertrand, the 'légère faute' in H^2. In fact I consider it impossible to commit one at the beginning of such a theory. You must either tell a 'rousing whid' or be infallible. If equations 3 and 3a have anything the matter with them, may ξ stick in H^2's throat. I not only believe them myself but set them to senate house men who did them.[62]

On the same day, Maxwell also wrote to Thomson, taking up the latter's discussion and presenting his own treatment of some special cases, such as a vortex cylinder or a vortex ring, based on the hydrodynamic-electromagnetical analogy and his 1865 paper *A dynamical theory of the electromagnetic field*. Maxwell included a long list of corresponding notions and propositions in electromagnetism and vortices. Moreover, he also reassured Thomson about Helmholtz's paper: "I have not seen Bertrand's refutation of Helmholtz so I will proceed as if he were still existing."[63] A few days later, Thomson wrote again to Tait, reporting some further progress on vortex motion. He introduced his ideas by stating that Euler's equations of fluid motion "include all Natural Philosophy & Chemistry, and everything else too acc[ording] to [Emil] Du Bois Reymond..."[64]

The Bertrand affair, however, was still unsettled. As it turned out, Helmholtz was able to show that Bertrand's original criticisms were unfounded since they resulted from a different description of the infinitesimal motion which was nevertheless compatible with Helmholtz's approximation. His response to Bertrand was published in the *Comptes Rendus* later in 1868, but in the same volume, which reached Tait in early September, Bertrand reiterated his criticisms. This time, it was the assumption of a continuous velocity field in the fluid which caused trouble. Tait reported to Thomson: "D^r T, B. has put his foot into it fairly at least. He *proves* (or thinks he does) that H^2's effect of an element of a vortex on a particle of the fluid is altogether wrong. Won't he catch it?"[65] The condition of continuity (or rather, smoothness) of the flow of a perfect fluid was indeed a delicate issue. From a mathematical point of view, it was clearly a necessary assumption; from a physical point of view, however, it was rather unclear whether it could be supposed to hold in a perfect fluid or not. Thomson, Tait, and Helmholtz corresponded

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[61] Thomson to Tait, 5 July 1868; Kelvin Papers Glasgow, T 90

[62] Maxwell to Tait, 18 July 1868. Equations (3) and (3a) in (Helmholtz 1858) determined the time derivative of the rotation field (ξ, η, ζ) (in Helmholtz's notation), showing that zero rotation at one instant implied zero rotation at every instant.

[63] Maxwell to Thomson, 18 July 1868; in (Maxwell 1995, 398–406).

[64] Thomson to Tait, 22 July 1868; Kelvin Papers Cambridge, T 7.

[65] Tait to Thomson, 6 September 1868. Kelvin Papers Glasgow, T 96.

about the matter in September and October 1868, and the issue repeatedly appeared in later correspondence between Tait, Stokes and Thomson. Throughout these exchanges, Thomson maintained the position that continuity or smoothness cannot be broken by natural actions on a perfect fluid, initially in a state of rest or continuous flow in a region of space, and therefore that Helmholtz's reasoning was sound.[66] In any case, Thomson worked hard to get his long-planned paper on the mathematics of vortex motion into a form which could be printed.

Maxwell pursues topology

§ 20. Meanwhile, Maxwell continued to think about the topological matters which Thomson's ideas had brought to his attention. In the autumn of 1868, he produced several manuscripts which now displayed a more systematic approach to the field.[67] In them, he tried to fix basic concepts and collected some elementary observations and problems. In the course of these efforts, he solved the topological problem raised by Thomson's reflections on irrotational flows in multiply connected regions, i.e. to determine the degree of connectivity of such regions with given boundary surfaces.

The first manuscript set out by defining the notions of function and of continuity. For Maxwell, the latter notion implied that at each point, a finite differential coefficient exists which, however, was not required to vary continuously. Then curves were defined as the

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[66] As early as 1858, Stokes had doubted that fluid motion was necessarily continuous (Stokes to Thomson, 12 February 1858; Kelvin Papers Cambridge, S 391). For Thomson's exchange with Helmholtz, see e.g. Thomson to Helmholtz, 3 October 1868; Kelvin Papers Cambridge, H 71. In 1870, Tait wrote to Thomson: "O. T., Tell me 'in so many words' (as the phrase is) *why* a vortex filament *in a perfect fluid* 'bedingt' (as H^2 has it) a certain velocity in each other part of ye same. It seems to me to be a mere gratuitous assumption of continuity. I know you laughed at this idea years ago, but I persist in it until I can get an explanation w^h will satisfy me. Otherwise (f.e. if friction come[s] in) I allow it at once – but how *without* friction. Das Ganze ist nur Bosh. Y^{rs} T' " (Tait to Thomson, 9 March 1870; Kelvin Papers Cambridge, T 13.) In 1880, Stokes raised the issue again in correspondence with Tait (see Tait to Stokes, 16 August 1880; Cambridge University Library, Add. 7656, T 93). In 1884, Tait even brought the problem before the R.S.E. in a note entitled *On vortex motion* which was abstracted as follows: "This paper contained a discussion of the consequences of the *assumption* of continuity of motion throughout a perfect fluid; one of the bases of von Helmholtz's grand investigation, on which W. Thomson founded his theory of vortex-atoms." (Tait 1884b) In 1887, Stokes came back to the topic in his correspondence with Thomson, using a nice illustration: What happens if a drop or wave of perfect fluid falls into a mass of perfect fluid at rest? Thomson again insisted on the continuity of the velocity field; see (Wilson 1990, vol. 2, 586–591).

[67] Harman includes three such MSS into his edition, moreover there is a further undated MS collecting material from Listing and Gauss, not edited by Harman. Two manuscripts were probably written in September or October 1868 as Maxwell mentioned part of his work in a letter to Thomson in late September. It is difficult to tell with certainty which one was written first but Harman's order seems plausible. I cannot agree, however, with the dating he gives for the second manuscript (no. 305 in Harman's edition), and the order in which he edited the two extant drafts of this manuscript. See below for details.

locus of a continuously varying point in space. Maxwell pointed out that the position of a point on a closed curve is a periodic function of curve length, measured from a given base point. Next, Maxwell passed to surfaces, defining a closed surface as "a finite surface enclosing a space so that a point cannot pass from within the surface to the space outside without passing through the surface." (Maxwell 1995, 434.) Evidently, he had only orientable, embedded surfaces in mind. The paradigmatic case was that of the surface of a solid body with several tubular "holes" through it. Turning to connectivity, Maxwell introduced a terminology similar to that of Riemann's short note *Lehrsätze aus der analysis situs...* of 1857:

> A closed surface may either be simply connected like that of a sphere or complexly connected like that of a ring or a solid body pierced with holes. [...] In a surface of n connexions $n - 1$ closed curves may be drawn so that [it] may be possible to pass from any one point to any other without crossing any of these curves. (Maxwell 1995, 434 f.)

It is not clear whether Maxwell had actually read Riemann's note or whether he had merely taken the idea from Helmholtz's and Thomson's writings. In fact, Maxwell's remark that the surface "of a ring," i.e. of a solid torus, was "doubly connected" reveals that he had not yet grasped the situation completely, and it also makes clear that he could not have read Riemann's note very closely. While Maxwell even remarked that both a longitudinal and a transversal meridian would serve the purpose, he realized his error only later and gave the right number of non-dissecting curves, i.e. two.[68]
After a short remark concerning "finite spaces" (which did not, however, address multiple connectivity), Maxwell turned to what he considered one of the basic problems of "geometry of position", as the draft was entitled:

> Let any system of closed curves in space be given and let them be supposed capable of having their forms changed in any continuous manner, provided that no two curves or branches of a curve ever pass through the same point of space, we propose to investigate the necessary relations between the positions of the curves and the degree of complication of the different curves of the system. (Maxwell 1995, 435.)

Within the limits of the mathematical language available to Maxwell, this represents a formulation of the classification problem for links. Apparently, Maxwell was still unaware of Johann Benedikt Listing's *Vorstudien* of 1847, in which the same problem had been posed at least implicitly. Most probably, Maxwell was inspired to consider this problem by the role of knots and links in Thomson's speculations.

Maxwell proceeded to discuss some simple ways of deforming links without changing their type. In order to do so, he looked at plane projections of such links, implicitly supposing that these projections only contained transversal double points. Next, he developed his own symbolic coding of such diagrams. For each of the closed curves

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[68] See (Maxwell 1995, 434 f.) In his note, Riemann had explicitly mentioned that the surface of a solid torus is *triply* connected.

$a, b, c, \ldots$ constituting a link in space, he designated by A_p ($p = 1, \ldots, \alpha$) those points on the curve a lying over one of the double points of the link projection. Similarly, B_q ($q = 1, \ldots, \beta$) denoted the corresponding points for component b, etc. Maxwell observed that the total numbers $\alpha, \beta, \ldots$ of these points were always even, a fact which would also serve as Tait's starting point eight years later. To each crossing of the projection, Maxwell assigned a symbol $\frac{A_p}{B_q}$, if curve a crossed *over* curve b with respect to a given orientation of space. The reversed crossing would then be $\frac{B_q}{A_p}$; a crossing of a with itself was denoted by a symbol of the form $\frac{A_p}{A_{p'}}$, etc. Of course, the projection together with these symbols determined the link completely (see Fig. 5).

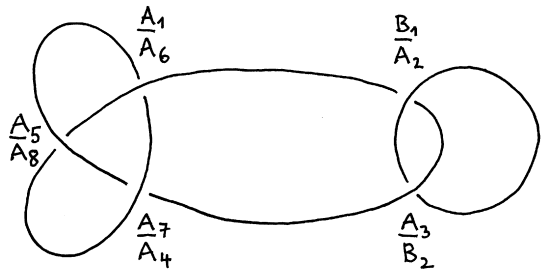


Fig. 5. Maxwell's coding of link diagrams

To find modifications of these diagrams which did not change the type of the link, Maxwell first showed that every projection necessarily contained regions bounded by less than four arcs of the projection (taking an arc to be bounded by two consecutive points A_p, A_{p+1}). This was an easy corollary of Euler's formula for polyhedra, applied to the plane graph given by a connected link diagram.[69] The number of edges of this graph equalled the total number $l = \alpha + \beta + \gamma + \ldots$ of the points A_p, B_q, etc., while the number of corners (i.e. crossings of the projection) was obviously given by $s = \frac{l}{2}$. Euler's formula then implied that there were $s + 2$ regions in the projection, one of which was the infinite region. Denoting the number of arcs bounding the i-th region by n_i, Maxwell finally obtained

$$n_1 + n_2 + \ldots + n_{s+2} = 2l = 4s \,,$$

thus at least one of the $s + 2$ integers n_i had to be less than 4.

Maxwell next proceeded to consider the types of deformations that could be carried out in regions bounded by less than four arcs. In the case of a region bounded by *one* arc only (cf. Fig.6a), the crossing symbol was of form $\frac{A_p}{A_{p\pm1}}$, and the whole region "may be made to disappear by uncoiling the curve", as Maxwell expressed it. In the case of a region bounded by *two* arcs, there were two possibilities (illustrated in Figs. 6b, 6c). Either the two symbols involved were of the form $\frac{A_p}{B_q}$ and $\frac{A_{p\pm1}}{B_{q\pm1}}$, in which case "the two loops may be separated and the symbols belonging to them may be cancelled," or

[69] An extension of Euler's formula to the case of curvilinear plane figures had been described by (Cayley 1861). In later writings, Maxwell repeatedly referred to this paper.

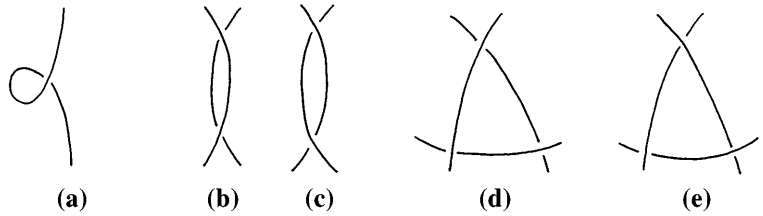


Fig. 6a-e. Regions bounded by less than four arcs

else the symbols would be of the form $\frac{A_p}{B_q}$ and then $\frac{B_{q\pm1}}{A_{p\pm1}}$, so that "the curves are linked together and cannot be separated without moving other parts of the system." If, finally, an area was bounded by three arcs, there were again two possibilities (cf. Figs. 6d, 6e). "In the first case," Maxwell wrote, "any one curve can be moved past the intersection of the other two without disturbing them. In the second case this cannot be done and the intersection of two curves is a bar to the motion of the third in that direction." (Maxwell 1995, 437.) Maxwell went on to add a few inconclusive remarks about regions bounded by more than three arcs, culminating these reflections with the incorrect statement that "if a polygon [i.e. region of the projection] is partly right handed and partly left handed it may be reduced." (Maxwell 1995, 438.) The remark shows that Maxwell was unaware of non-alternating links.[70]

Maxwell's discussion represents an early and inconclusive venture into the domain of knot theory. The diagram changes Maxwell discussed later came to be known under the name of "Reidemeister moves;" today they play a fundamental role in the combinatorial approach to knot theory. Even if Maxwell's discussion shows a clear deliberation to build up complex modifications of link projections from simpler ones or at least to order them according to increasing complexity, he did *not* claim or prove that all diagram changes can be built up from those he explicitly discussed. It was only in the 1920's that this was shown to be the case by Reidemeister and Alexander.[71]

§ 21. Thomson became aware of Maxwell's renewed interest in topology in late September 1868, when Maxwell reported about a new subject, the connectivity of bounded space regions. Writing to his Glasgow colleague, Maxwell announced a proposition that would become of great interest to Thomson:

> I have been making a statement about the continuity discontinuity periodicity and multiplicity of functions generally and of lines surfaces and solids. Here is the upshot in connected form.
>
> Take a solid without any hollows in it or holes through it. It is a simply connected space bounded by one simply connected closed surface. Now bore *n*

[70] A vague statement which might have been interpreted in this way was made by (Listing 1847, 55). However, the difference in terminology and outlook in Maxwell's manuscript, and the absence of Listing's more interesting ideas on knots, make it improbable that Listing's essay was the source of Maxwell's claim.

[71] See (Reidemeister 1926) and (Alexander and Briggs 1927).

> holes right through the solid. It is now a space of n connections bounded by an n-ly connected closed surface.
>
> The infinite space outside is also n-ly connected. Now let there be m hollow spaces within the solid and let these be bounded by closed surfaces whose connections are $n_1 n_2 n_3 \ldots n_m$. Then the solid will be an $(m+1)$-ly *bounded* space and its connexions will be $n + n_1 + \&\ \mathrm{c} + n_m$.[72]

Maxwell's wording shows that he was still struggling to form a clear picture of the situation, and his pronouncements here are inconsistent with the terminology of his earlier topological manuscript. According to this letter, both a sphere and a torus would be regarded as a *simply* connected surface. By contrast, following Riemann's terminology, a solid with n "holes" through it should have as its degree of connectivity $n+1$ rather than n, and the connectivity of its bounding surface would be $2n+1$. The number n itself corresponds to what came to be called the *genus* of such a surface. Nevertheless, Maxwell was in a position to formulate a correct assertion. About a week later, he recognized his terminological error. On 6 October, Maxwell thanked Thomson for the first 6 pages of the latter's paper on *Vortex Motion* and informed him that he had been studying the dynamics of three "Helmholtz rings" moving on the same symmetry axis and had illustrated the case in his "wheel of life."[73] On the next day, Maxwell again wrote to Thomson:

> I find that I made a mistake about the connectedness of hollow solids. If the solid is bounded by m surfaces of which one is external and the rest internal and if the connectedness of these are $n_1 n_2 \ldots n_m$ Then if n_1 belongs to the external surface it introduces n_1 connexions into the solid, but if n_2 belongs to an internal surface it introduces only $n_2 - 1$ *new* connexions. Hence the whole number of connexions is $[\ldots] \sum(n) - m + 1$.[74]

Given Maxwell's erroneous treatment of surfaces, this statement has to be read with care. It is wrong if we read, as he still explicitly required at that time, a surface "of n connexions" to be a surface in which at most $n-1$ non-dissecting closed curves can be drawn. But the numbers $n_i - 1$ *intended* by Maxwell were not the maximal numbers of non-dissecting closed curves but rather the *genera* of the surfaces. Thus Maxwell's statement amounts to the correct proposition that the order of connectivity of a space region bounded by a finite number of closed surfaces is one higher than the sum of their *genera*. No proof or argument for this claim was given in the letter to Thomson.

§ 22. Around this time, Maxwell also wrote a second manuscript on topology, including the new result in the corrected form communicated to Thomson. Two drafts of this manuscript have survived. The first virtually repeated the phrasing of the letter to Thomson while the second is significantly more detailed and introduced new terminology which Maxwell later incorporated into his *Treatise*.[75] The manuscript was written in

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[72] Maxwell to Thomson, 28 September 1868; in (Maxwell 1995, 443).

[73] See the photograph in (Maxwell 1995), opposite page 446.

[74] Maxwell to Thomson, 7 October 1868; in (Maxwell 1995, 449).

[75] Harman's edition of the two drafts of this manuscript gives what obviously was the first draft as the second (Maxwell 1995, 450) and the revise as the first (Maxwell 1995, 440–442). That the

a style which would have suited the introductory sections of a textbook on mathematical physics, devoted to topological basics.[76] Again, Maxwell started with a discussion of continuity, this time emphasizing the origin of this notion, as he conceived it, in *physics* rather than mathematics:

> The idea of physical continuity is best conceived under the example of the continuous existence of matter in time and space. A material particle, during the whole time of its existence must have a determinate position. Hence its path is a continuous line and its coordinates are continuous functions of the time. (Maxwell 1995, 439.)

Maxwell emphasized physics in this connection because he still conceived *mathematical* continuity along 18th-century lines, making the distinction in almost perfect agreement with Euler's views on the subject: "The idea of mathematical continuity refers rather to the form of the function than to its particular values, whereas a function may be physically continuous though its form may be different for different values of the variables."[77] As in the first manuscript, Maxwell required physically continuous functions to be differentiable as well. After setting forth this distinction, he then introduced topology in a remarkable way:

> Most important applications of the idea of physical continuity to geometry have been made by Riemann (Crelle). The ideas of Riemann have been employed by Helmholtz Betti Thomson &c in physical researches and I have found it necessary for my own purposes to employ a system of nomenclature of spaces, surfaces and lines which I shall now explain.[78]

The core notion of topology, Maxwell implied, was physical, not mathematical continuity. It would appear that Maxwell saw Riemann's efforts to utilize this physical notion in geometry as significant mainly because this furnished mathematical physicists with new tools for their investigations.

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correct order is different from Harman's can be seen from the error on the connectivity of closed surfaces which, while repeated both in the draft p. 450 and the letter to Thomson of 7 October to which Harman appended it, is only corrected in what Harman edited as the "first" draft. Moreover, the amount of detail and the new terminology used in this draft show that it was indeed the revised version. Harman's reason for his ordering – the mistake Maxwell made in reporting on his results to Thomson (see above) – has in fact nothing to do with the two drafts since it is contained in neither of them. Consequently, also Harman's dating of the two drafts has to be modified. While there is a chance that Maxwell wrote the first draft before his letter of 28 September, and simply got his own statements wrong in writing to Thomson, it seems more probable that both drafts of this MS were only written in October when Maxwell had come to realize the error in his statement to Thomson on 28 September.

[76] Harman views this MS as an early draft for the book. The crucial reference to Listing, however, is still missing; see below.

[77] (Maxwell 1995, 439.) On Euler's distinction between mathematical and mechanical continuity, see (Youschkevitch 1976, 64–69).

[78] (Maxwell 1995, 439.) Note that Maxwell did not yet mention Listing, as in various places of the *Treatise*; see § 45 below.

Maxwell then turned to discuss the connectivity of finite space regions and the closed surfaces bounding such regions, using almost the same words as in the letter to Thomson of 7 October.[79] Initially, he still continued to use his problematic terminology that a surface of genus n is $(n+1)$-ly connected or continuous. But now he came to see that this was misleading, and he revised the second part of his manuscript thoroughly. Instead of "connexions" he decided to speak of "cycles", and the discussion of surfaces was finally put right. The crucial passage in this revision will be given here in full since it also enables us to discern at least the outline of an argument for Maxwell's proposition about the connectivity of solids:

> Let any closed curve be drawn on the limiting surface [of a finite space region] and let a surface be drawn within the space bounded by the closed curve then in the case of a space of simple continuity this surface will divide the space into two distinct regions so that a point cannot travel from one to the other without crossing the surface.
>
> Any solid body without any holes through it is an example of simple continuity. Now let a hole be bored through the solid converting it into a ring, and let a surface be drawn meeting the limiting surface along one side of the hole and round one side of the solid. A point can still travel from one side of this surface to the other by going round the other side of the hole. If n holes had been bored through the solid, n such surfaces may be drawn without separating one part of the space from the rest. Such a space would be called if we follow the method of Riemann, an $(n+1)$ly connected space. I prefer however for reasons which will appear as we proceed to call it an n-cyclic space.
>
> If a finite space bounded by a single continuous [i.e. connected] surface is n-cyclic the bounding surface is also n-cyclic and the infinite space outside the surface is also n-cyclic as far as that bounding surface is concerned. If we consider the finite space as solid with n holes in it, then the infinite space has n channels by which it embraces the finite space and the finite space has also n channels by which it embraces the infinite space.
>
> If the expression $Xdx+Ydy+Zdz = dV$ be a complete differential at every point within the finite space then in a simply connected space which we may call acyclic V can only have one value for each point of the space but in an n-cyclic space V may have values infinite in number of the form
>
> $$A = V_0 + p_1 P_1 + ... + p_n P_n$$
>
> where V_0 is one of the values and $p_1 \ldots p_n$ are integral numbers positive negative or zero and $P_1 \ldots P_n$ are the values of $\int (X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds})ds$ taken round a closed curve drawn round each of the n channels belonging to the finite space. The quantities $P_1 \ldots P_n$ may be called the cyclic constants. They are important in the theory of Vortices and in Electromagnetism.
>
> If a space be bounded by several surfaces the number of cycles belonging to the space will be the sum of the number of cycles belonging to the different bounding surfaces. (Maxwell1995, 440 f.)

[79] See (Maxwell 1995, 450). As mentioned above, this version must be taken as the first draft of the manuscript.

Maxwell added a short section on surfaces where he pointed out that for closed surfaces which he called n-cyclic, not n but $2n$ closed curves may be drawn which do not separate the surface into disconnected parts. In other words, a closed n-cyclic surface, or what would later be called a surface of genus n embedded in 3-space, has degree of connectivity $2n + 1$.

It is important to notice that Maxwell did not give a precise definition of the crucial new technical term: "cycle." His discussion of complete differentials dV suggests that he was interested in integration domains, or, more precisely, in systems of paths that generate all possible values of path integrals $\int dV$ of such a differential form. On the other hand, this interpretation does *not* fit his discussion of "cycles" in surfaces; Maxwell seems to have been unaware of Riemann's corresponding discussion in the main part of the paper on Abelian functions of 1857.[80] Certainly, Maxwell's terminology derived from the physical situation he and Thomson wanted to treat, i.e. fluid motion or electromagnetic fields and their path integrals in *three-dimensional* regions. Therefore, closed surfaces were not conceived in isolation but always, at least implicitly, as boundaries of "solids with holes."

Based on Maxwell's new terminology, we can sketch an intuitive argument for his main result which states that a space region bounded by one external surface of genus n and m internal surfaces with genera $n_1, n_2, \ldots n_m$ will possess $N = n+n_1+n_2+\ldots+n_m$ "cycles," or, in other words, it will be $(N + 1)$-ly connected. Starting with the interior of the external surface, viewed as the surface of a solid with tubular holes, Maxwell seems to have thought of a successive removal of further solids of the same kind from this region. The first region has n "cycles" if its bounding surface is of genus n, i.e. the space enclosed has n tubular holes; a "cycle" might here be conceived as a closed curve which runs once round one of the holes of the solid. From the dual statement that the exterior of a solid with n tubular holes is also n-cyclic (see the quotation above), Maxwell may have concluded that removing a solid with a surface of genus n_1 from this bounded region of 3-space will introduce n_1 new "cycles" (one closed curve for each hole, running once through the hole). Since each of these curves can be deformed into a point before removing the internal solid, these "cycles" are *not* reducible to the n "cycles" accounting for the original connectivity. Thus the connectivity of the new region is $n + n_1$. Finite induction then leads to the desired result.

It seems very probable that Maxwell derived his claim using an argument similar to the one just given. The crucial duality lemma relating the connectivity of a solid with holes to that of its complement in space reappears in virtually all of his later writings on the topic.[81] Plainly, however, such an argument cannot be regarded as a serious proof, since it is based on a consideration of paradigmatic cases of the form depicted in the figure below where all the bounding surfaces are in a "standard" position, neither knotted nor linked with each other.

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[80] See (Riemann 1857, § 4). A reader more interested in the notion of connectivity than in algebraic functions might well have contented himself with the short note *Lehrsätze aus der analysis situs...*, prefixed to the main, and difficult, part of the paper, the *Theorie der Abel'schen Functionen*.

[81] See especially § 18 in the *Treatise*.

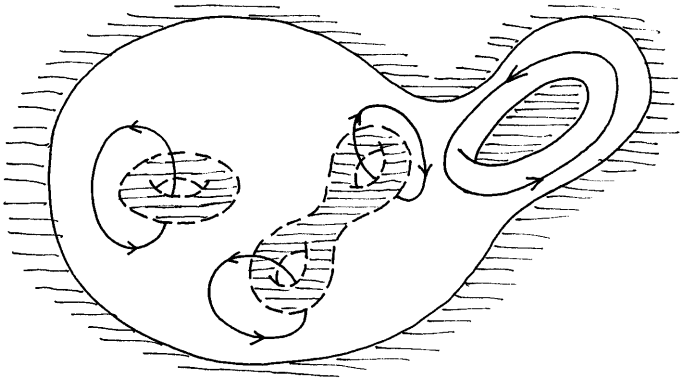


Fig. 7. Complexly connected solid with "cycles"

The restriction to such cases and the use of the vague notion of "cycles," however, led to a difficulty which soon came to play a role in the work of the Scottish physicists. How, in fact, could Maxwell be sure that not *more* than n_1 independent new "cycles" were introduced by removing an n_1-cyclic solid which was situated in a more complicated way inside the original one, such as, for instance, a knotted vortex tube? How did Maxwell know that a space surrounding a knotted channel, like that drawn in Fig. 8, was just *doubly* connected?

In order to show this, Maxwell would have had to develop a technique enabling him to show that two curves like α and β in Fig. 8 may indeed be considered as equivalent "cycles," or that his duality lemma holds in such cases as well. Nothing in Maxwell's manuscript indicates that he thought about such a technique.

Here Maxwell fell victim to the general ambiguity in many topological arguments of the period, a difficulty that resulted from the lack of a clear distinction between what today are called homological and homotopical aspects of a given problem. Very often in this period, the relevant variations of one-dimensional integration domains were viewed

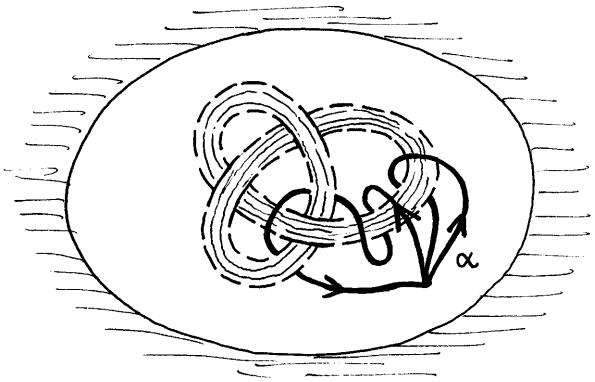


Fig. 8. A doubly connected space?

intuitively as gradual changes in position, i.e. as homotopies.[82] This was the case with Maxwell and Thomson, too, whose discussion of path deformations will be presented below. Following such a view, however, the difficulties illustrated by Fig. 8 become crucial. Intuition suggests that the curves α and β *cannot* be gradually deformed into each other without traversing the knotted channel, and indeed they are not *homotopic* in the modern sense. On the other hand, a *homological* equivalence between the two exists, i.e. a surface can be found avoiding the knotted channel and bounded precisely by α and β. But to establish this equivalence requires both a suitable conceptual framework and some insight into the complexities that can arise, of which no traces can be found in Maxwell's writings. Alternatively, the difficulty of knotted and linked channels could have been avoided by strictly adhering to the Riemannian point of view of counting the number of non-dissecting two-dimensional cuts which a three-dimensional region admits; we have seen that Maxwell explicitly decided not to follow this approach.

Given that most 19th-century mathematicians did not clearly distinguish between homological and homotopical equivalences, the vagueness of the physicist Maxwell's remarks is certainly not surprising.[83] It should be noted, however, that Maxwell was dealing with a three-dimensional situation where a recognition of the difference between homological and homotopical properties would indeed have been of fundamental importance. Whereas two space regions of the kind illustrated in figure 8, but with differently knotted channels removed, cannot be distinguished by homological information, they are in general very different from a homotopical point of view, as modern knot theorists would amply show in the 20th century. As we shall see, it is not completely anachronistic to raise this issue. Maxwell's correspondent, Thomson, whose occupation with vortex atoms had already made him sensitive to the distinction between the "degree" and the "quality of multiple continuity," was to struggle with these thorny matters and eventually he claimed to have found a way to deal with the kinds of problems Maxwell had evaded.

Multiple continuity and the most general irrotational motion of a fluid

§ 23. While the Bertrand affair was still causing headaches for the vorticists, Thomson finally began sending Tait the first pages of his paper *On vortex motion* for publication in the *Transactions* of the R.S.E. (Thomson 1869). The first 58 paragraphs of the paper were "recast and augmented 28th August to 12th November, 1868."[84] Thus Thomson could profit both from Maxwell's reassuring remarks about Helmholtz's paper and from Maxwell's discussion of connectivity. Another year went by, however, before the second part (§§ 59–64) of the paper was delivered, and both parts were then printed together in the volume of the *Transactions* for 1869. In the meantime, Maxwell continued his

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[82] See (vanden Eynde 1992) for a survey of contemporary views on deformations of paths.

[83] The difference was not fully clarified until Poincaré's work; see (vanden Eynde 1992, 159 ff.).

[84] See (Thomson 1869, 13). Thomson's collected papers give "§§ 1–59 recast . . . "; this must be a misprint as the remark before § 59 shows (Thomson 1869, 46).

topological studies and in particular read what was available from Listing and Gauss on the subject; we will find traces of this reception also in Thomson's paper.

To a large extent, the paper was devoted to a discussion of impulse and energy in fluid motion, the central idea being based on Helmholtz's results. For a large class of solutions to the Euler equations, Thomson argued, vortices might be conceived as equivalent to a number of "bodies" of variable, but topologically invariant shape, embedded in the fluid (Thomson 1869, § 20). Therefore, the crucial point was to study irrotational flows of a perfect fluid in a space containing "bodies" replacing the vortices, taking into account the resulting interactions between the fluid and these "bodies." A complete presentation of the results that Thomson obtained on the basis of this approach would lead us too far astray. However, those passages of his article which dealt with the notion of connectivity must be treated in detail.

The main problem which Thomson addressed and eventually solved in these passages was the one hinted at in his long letter to Tait: given a multiply connected space region, bounded by one external and several internal surfaces, to determine the set of irrotational flows in this region satisfying certain given boundary conditions. This was dubbed the problem of "the most general irrotational motion" of a perfect fluid (Thomson 1869, § 61).[85] Its solution depended upon finding a workable notion of multiple connectivity. It turns out that, like Maxwell, Thomson, too, wavered between two versions of the notion of connectivity – one homological, the other homotopical. In his paper, however, the difference between the notions is much more explicit than in Maxwell's manuscripts, and it is possible to understand quite clearly on which of the two Thomson's technical arguments relied.

§ 24. The starting point in Thomson's argument was the well-known result that in a simply connected region bounded by a closed surface, irrotational fluid motion was uniquely determined by boundary conditions. In particular, if the normal component of the fluid velocity along the boundary vanished, then the fluid had to be at rest. The canonical proof, included for instance in Helmholtz's 1858 paper, relied on one of Green's integral theorems.[86] However, Thomson reminded the reader:

> Helmholtz, in his splendid paper on Vortex Motion, has made the very important remark, that a certain fundamental theorem of Green's, which has been used to demonstrate the determinateness of solutions in hydrokinetics, is subject to exception when the functions involved have multiple values. This calls for a serious correction and extension of elementary hydrokinetics to which I now proceed. (Thomson 1869, § 54.)

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[85] This global problem must be distinguished from the local problem of the "most general motion of a fluid" which consisted in finding suitable approximations and decompositions of the infinitesimal motion of a fluid. The latter problem was the subject of the Helmholtz-Bertrand controversy.

[86] Existence theorems in potential theory were not rigorously dealt with at the time. Though the topic was carefully studied in two dimensions where it had been recognized to be important for the Riemannian approach to complex analysis, the situation was considered difficult in 3 dimensions, as H. A. Schwarz pointed out in a letter to Klein of 28 March 1882, NSUB Göttingen, Cod. MS. Klein, XI, 936.

Thomson formulated the version of Green's result which he had in mind as follows. For two *single-valued* functions ϕ and ϕ' in a space region bounded by a closed surface of arbitrary genus, one obtains:

$$\begin{aligned}\iiint \left(\frac{d\phi}{dx}\,\frac{d\phi'}{dx}+\frac{d\phi}{dy}\,\frac{d\phi'}{dy}+\frac{d\phi}{dz}\,\frac{d\phi'}{dz}\right) dx\,dy\,dz \\ = \iint d\sigma\;\phi\mathfrak{d}\phi' - \iiint dx\,dy\,dz\;\phi\nabla^2\phi' \\ = \iint d\sigma\;\phi'\mathfrak{d}\phi - \iiint dx\,dy\,dz\;\phi'\nabla^2\phi\,,\end{aligned}$$

where $\iiint dxdydz$ denotes integration over the given region, $\iint d\sigma$ denotes integration over the bounding surface, and $\mathfrak{d}$ stands for the normal derivative at a point of this surface. Implicitly supposing sufficient conditions of smoothness, Thomson held this result to be "true without exception" (Thomson 1869, § 55.) The uniqueness of irrotational flows in simply connected regions could now be derived by observing that (1) the solution space of the corresponding Euler equation was linear; (2) every solution was the gradient flow $\nabla\phi$ of a potential function ϕ; and (3) Green's formula implied, by taking $\phi = \phi'$, that the only solution with zero normal component at the boundary was $\nabla\phi = 0$.

This argument, however, broke down as soon as multiply connected regions were considered. Irrotational flows in such regions still formed a linear space, but they were no longer necessarily gradient flows of *single-valued* potential functions. As a consequence, if either ϕ or ϕ' was a many-valued function arising from a vector field $\vec{v}$ by definite integration, i.e.

$$\phi(x) = \int_{x_0}^{x} \vec{v}\,d\vec{s}\,; \quad \text{so that} \quad \nabla\phi = \vec{v}\,,$$

then the integrals in the second or third term of Green's formula were ambiguous, as Thomson showed by an example.[87] He chose ϕ to be the many-valued potential of an irrotational fluid motion taking place between two concentric cylinders and bounded by two perpendicular planes which, in a suitable coordinate system, was given by:

$$\phi(x, y, z) = \tan^{-1}\frac{y}{x}$$

(here all branches of $\tan^{-1}$ must be taken into account). He then pointed out that the ambiguity of Green's formula arises from the fact that if $\vec{v}$ denotes the vector field $\nabla\phi$, the line integral $\int \vec{v}\,d\vec{s}$ might take different values along different paths in the region considered. As this example indicated, such cases arouse precisely in the hydrokinetic situation Thomson wanted to treat, fluid motion in multiply connected (or "multiply

[87] Thomson never used vector language, and he chose to write all his formulas in Cartesian coordinates, in accordance with his general aversion against vector or quaternion methods. As I want to focus on other issues here, I will abbreviate Thomson's formulae occasionally by using vector notation. When, in 1870, Tait also turned to Green's theorem and its generalization to multiply connected regions, he used quaternion language, thus making the first step toward a vectorial form. See § 13 above.

continuous", as Thomson prefered to say) regions of space. In such regions, he noted that one could get rid of the ambiguity, "if we annex to [the system of bounding surfaces] S a surface or surfaces stopping every aperture or passage on the openness of which its multiple continuity depends." (Thomson 1869, § 57.) As already indicated in the letter to Tait of 5 July 1868, Thomson clearly conceived of this process in terms of virtual barriers that would stop the motion of the fluid. This physical idea, however, fitted well with Riemann's or Helmholtz's idea of cutting surfaces.

Choosing a system $\beta_1, \ldots, \beta_n$ of such blockading surfaces that completely stopped the flow thus produced a simply connected region. Thomson then remarked that for any given flow in the original region, a certain constant could be associated with each surface β_i. In fact, the value of $\int \nabla\phi \, d\vec{s}$, taken along a path through the remaining simply connected region, starting from a point P in β_i to one side and returning to P from the other, was a constant κ_i, independent of the starting point P in β_i. Using these constants, Thomson gave a modified version of Green's formula, valid for many-valued functions whose gradients were smooth, single-valued vector fields:

$$\begin{aligned}
&\iiint \left(\frac{d\phi}{dx}\frac{d\phi'}{dx} + \frac{d\phi}{dy}\frac{d\phi'}{dy} + \frac{d\phi}{dz}\frac{d\phi'}{dz} \right) dx\, dy\, dz \\
&= \iint d\sigma\, \phi \mathfrak{d}\phi' + \sum_i \kappa_i \iint_{\beta_i} d\sigma\, \mathfrak{d}\phi' - \iiint dx\, dy\, dz\, \phi \nabla^2 \phi' \\
&= \iint d\sigma\, \phi' \mathfrak{d}\phi + \sum_i \kappa_i' \iint_{\beta_i} d\sigma\, \mathfrak{d}\phi - \iiint dx\, dy\, dz\, \phi' \nabla^2 \phi \,.
\end{aligned}$$

Thomson's claim is easily derived from the original form of Green's formula, applied to the simply connected region obtained by introducing the β_i, if technical difficulties relating to the smoothness of the functions and boundaries are passed over. The additional middle terms arise by evaluating the integrals containing the normal derivatives along the surfaces β_i.

§ 25. Thomson's extension of Green's formula dealt with a phenomenon still rather unusual in the mathematical physics of this time. Some of this novelty was felt by Thomson himself, who concluded the first part of his paper with a paragraph elaborating and illustrating his views on multiply connected regions of space. His attempt at a definition was the following:

> Adopting the terminology of Riemann, as known to me through Helmholtz, I shall call a finite portion[88] of space n-ply continuous when its bounding surface is such that there are n irreconcilable paths between any two points in it. To prevent any misunderstanding, I add (1), that by *a* portion of space I mean such a portion that any point of it may be travelled to from any other point of it, without cutting the bounding surface; (2), that the 'paths' spoken of all lie within the portion of space referred to; and (3), that by irreconcilable paths between two points P and Q, I mean paths such, that a line drawn first along one of them cannot be gradually changed till it coincides with the other, being always kept passing

[88] The reprint in Thomson's *Collected Papers* has "position" for "portion", probably a misprint.

through P and Q, and always wholly within the portion of space considered. (Thomson 1869, § 58.)

Thomson went on to illustrate this "definition" by means of solids with a number of tunnels – either opening on the boundary or located entirely in the interior – bored out of them. If the solid were simply continuous (to use Thomson's language) to begin with, then Thomson claimed that boring out n tunnels would lead to a solid of "$(n + 1)$ply continuity." Up to this point, Thomson's approach agreed more or less with Maxwell's way of looking at the situation. However, Thomson explicitly entered into the problem of knotted tunnels. He argued that if the tunnels were knotted or linked in complicated ways (as illustrated in the drawings by Tait and Crum Brown, reprinted in figure 4), this would give rise to "varieties of multiple continuity curiously different from that illustrated by a single ordinary straight or bent tunnel" (l.c.). Nevertheless, Thomson ventured the claim that "no amount of knotting or knitting, however complex, in the cord whose axis indicates the line of tunnel, complicates in any way the continuity of the space considered" (l.c.). In other words: while the "quality" or "variety" of multiple continuity could be different, its "degree" was always the same. In modern terms, this amounts to the statement that the first Betti number of a knot complement equals one for all knots.

The whole discussion was, to be sure, quite unsatisfactory if the wording of Thomson's definition of multiple connectivity was taken literally. His intuitive description of "irreconcilable paths" clearly suggests a notion involving homotopy rather than homology. Therefore, even a single tunnel would, in fact, create *infinite* continuity in Thomson's sense, since there are infinitely many "irreconcilable" paths in a doubly connected region like the interior of a solid torus. Missing was the idea of a composition of paths, which would have enabled Thomson to consider only "fundamental" paths from which all others could be generated up to reconcilability. However, the above presentation shows quite clearly that Thomson's extension of Green's result did *not* rely on the notion of multiple continuity as he just had defined it. Rather, his proof involved the notion of virtual membranes stopping the flow, i.e. of cutting surfaces that made the region considered simply connected. Moreover, it was this homological idea rather than that of irreconcilable paths which justified Thomson's claim regarding the independence of the "amount of continuity" from the knotting or linking the tunnels in a solid. In fact, Thomson saw his claim as confirmed by *experiments* with such membranes: "A single simple knot, though giving only double continuity, requires a curiously self-cutting surface for stopping barrier: which, in its form of minimum area, is beautifully shown by the liquid film adhering to an endless wire, like the first figure [in Fig. 4 above], dipped in a soap solution and removed." (Thomson 1869, l.c.)

It seems very unlikely that Thomson, or Maxwell before him, actually had any topological evidence for this claim other than the paradigmatic example of unknotted and unlinked tunnels, together with such experimental illustrations which indeed indicate that for every knot there is a connected "surface" bounded by the knot and "stopping every aperture or passage" through it. Thomson's remarks have to be taken with caution, however, as a little experimenting following his suggestion shows. Even for the standard form of the trefoil knot, and similarly for other knots, the soap film usually obtained is *not* an immersed disc but branches along certain lines (see Fig. 9a for the case of the

trefoil; the branching lines are dotted in the figure). Such a surface therefore could *not* serve as a "stopping barrier" since the quantity κ defined by Thomson would differ in the middle and outer parts of this surface (it is proportional to the linking number between the knot and a closed curve intersecting the surface in one given point). Moreover, in some cases, a Möbius band is obtained as in Fig. 9b; whereas bending the knot to a different form leads to still further types of minimal surfaces.

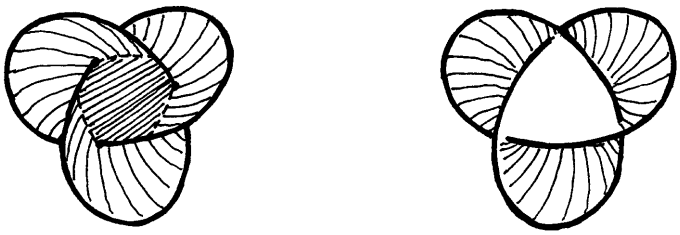


Fig. 9. Minimal surfaces bounded by a trefoil knot

In short, it was the intuitive idea of cutting surfaces rather than the idea of "irreconcilable paths" that informed Thomson's mathematical claims concerning multiply connected regions. While Thomson tended to *conceptualize* multiple connectivity by using path classes, *in arguments* about flows he resorted to the use of "stopping barriers" or cutting surfaces. This wavering between the homotopical and homological aspects of a situation, even more explicit in Thomson's work than in Maxwell's manuscripts, was characteristic of the fluidity which the notion of connectivity still had during this period.

§ 26. In late 1868, at about the time when Thomson completed the first part of his paper, Maxwell's interests in topology took a new turn, following his reading of the *Census der räumlichen Complexe*, a topological essay written by the Göttingen physicist Johann Benedikt Listing. It is not quite clear how Maxwell got to know this text, but it seems possible to date his reception fairly precisely. Most probably, it took place between October 1868 when he wrote his second topological manuscript and 29 December 1868, which is the date Maxwell gave to his third manuscript of topological content (Maxwell 1995, 466–469). While the earlier manuscripts were independent both in content and terminology of Listing's writings, the third manuscript gave a summary of the main ideas of Listing's *Census*. Like Listing's essay, Maxwell's manuscript dealt with cell decompositions of three-dimensional space and various generalizations of Euler's formula on polyhedra which could be drawn from such decompositions.[89] On 21 January 1869, Maxwell posed a corresponding problem to the Cambridge Mathematical Tripos, and four weeks later, he gave an account of Listing's ideas to the London Mathematical

[89] More precisely, Maxwell summarized Listing's main result which related the alternating sum of the numbers of vertices, edges, faces, and cells of a given cell complex to the connectivity numbers of these constituents.

Society.[90] From this time onward, Maxwell constantly referred to Listing both in his correspondence and published work whenever he treated topological matters.[91] In particular, he adopted Listing's terminology in the introductory chapter of his *Treatise on Electricity and Magnetism*. Maxwell's reading of Listing's essay apparently represents the earliest reception of this work in British scientific circles. It would become influential some years later when Tait started to think seriously about knots; by then, Maxwell had also learned of Listing's earlier *Vorstudien zur Topologie*.

§ 27. The discussion of the connectivity of space regions described above formed the end of the first part of Thomson's paper. Although the modification of Green's formula for such regions had been given, the intended application of this result to the determination of irrotational flows in multiply connected space regions was still missing. Thomson continued to work out his ideas and in late 1869, the second part of his paper took up the discussion at the point where he had left it. Apparently as a result of an exchange with Maxwell and perhaps of rereading Helmholtz, Thomson felt he should redo his topological discussion. As a new element, he introduced the notion of a "circuit" which comes closest to what today would be called a nontrivial homotopy class of closed paths in a region: "Henceforth I shall call a *circuit* any closed curve not continuously reducible to a point, in a multiply continuous space. I shall call *different circuits*, any two such closed curves if mutually irreconcilable (§ 58), but different mutually reconcilable curves will not be called different circuits." (Thomson 1869, § 60 (s).) Thomson then repeated his earlier, insufficient definition of the "degree of continuity," adding a telling historical comment:

> Thus $(n + 1)$ply continuous space, is a space for which there are n, and only n, different circuits. This is merely the definition of § 58, abbreviated by the definite use of the word circuit, which I now propose. The general terminology regarding simply and multiply continuous spaces is, as I have found since § 58 was written, altogether due to Helmholtz; Riemann's suggestion, to which he refers, having been confined to two-dimensional space. I have deviated somewhat from the form of definition originally given by Helmholtz, involving, as it does, the difficult conception of a stopping barrier [here a footnote was inserted, see below]; and substituted for it the definition by reconcilable and irreconcilable paths. It is not easy to conceive the stopping barrier of any of the first three diagrams of § 58, or to understand its singleness; but it is easy to see that in each of these three cases, any two closed curves drawn round the solid wire represented in the diagrams are reconcilable, according to the definition of this term given in § 58, and therefore, that the presence of any such solids adds only one to the degree of continuity of the space in which it is placed. (Thomson 1869, § 60 (t).)

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[90] The undated excerpt both of Listing's essay and of Gauss' fragment on the linking integral mentioned earlier (Cambridge University Library, Add. 7655. Vc. 40) might either belong to the period of Maxwell's first reception of these texts or else to the preparation of his talk to the London Mathematical Society. The problem for the Tripos is reprinted in (Maxwell 1995, 466); for the contents of the talk see below, § 45.

[91] Instances are his papers *On hills and dales* (Maxwell 1870b) and on graphical statics (Maxwell 1870a); for the latter, see also (Scholz 1989, 189 ff.).

Thomson did not elaborate on these risky claims which every critical reader of his paper must have found either incomprehensible or false. For if Thomson really insisted on taking the reconcilability of paths as depending on continuous deformation, then it was not only far from "easy to see" that two paths like α and β in Fig. 9 above are reconcilable, it was actually impossible to do so! Thus, had Thomson considered his own words more seriously, he would have recognized the necessity to clarify his ideas, either by emphasizing that his technique of "reconciling" paths was what in modern mathematics would be called a homological procedure, or else by noticing that with respect to continuous deformations of paths, knot and link complements were much more involved than his remarks suggested. It was at this point that Thomson came closest to recognizing the need to distinguish between different kinds of path equivalences or at least between the idea of reconcilable paths and Riemann's idea of cutting surfaces. In the footnote left out in the quotation above, Thomson explicitly acknowledged that in technical arguments he relied not on "circuits" but on "stopping barriers." Here he wrote that "without this conception [of stopping barriers] we can make no use of the theory of multiple continuity in hydrokinetics [...], and Helmholtz's definition is, therefore, perhaps preferable after all to that which I have substituted for it." (Thomson 1869, § 60 (t), note.) Moreover, Thomson explained that Maxwell had referred him to Listing's *Census* for "a very complete" treatment of "the subject of multiple continuity", and to Cayley's adaptation of Euler's formula to the case of curvilinear plane figures (Cayley 1861).

Finally, Thomson proceeded to solve the global problem of the irrotational motions of a fluid. After some preparations, he stated his result as follows:

> (PROP.) The motion of a liquid moving irrotationally within an $(n+1)$ply continuous space is determinate when the normal velocity at every point of the boundary, and the values of the circulations in the n circuits, are given. (Thomson 1869, § 63.)

The proof of this proposition followed from an easy application of his modified Green's formula. Introducing all necessary "stopping barriers" $\beta_1, \ldots, \beta_n$ (see § 24 above), every possible irrotational motion was uniquely determined by its potential ϕ, a single-valued function in the simply connected region obtained by cutting along the β_i, and satisfying the given boundary conditions. Moreover, ϕ had to satisfy the additional condition that its values on the two sides of every surface β_i differed by the given "cyclic constant," which Thomson now called the "circulation."[92] As fluid motions could be linearly superposed, the difference between two such motions satisfying the given conditions was again a possible fluid motion, but determined by a potential ψ whose normal deriviative at the boundary and cyclic constants vanish. Therefore,

[92] This condition had to hold not only in the limit approaching a point in β_i but in a whole neighbourhood of each such point, in order to guarantee a smooth gradient field across each "stopping barrier."

$$\iiint \left(\left(\frac{d\psi}{dx} \right)^2 + \left(\frac{d\psi}{dy} \right)^2 + \left(\frac{d\psi}{dz} \right)^2 \right) dx\, dy\, dz$$

$$= \iint d\sigma\ \psi \mathfrak{d}\psi + \sum_i \kappa_i \iint_{\beta_i} d\sigma\ \mathfrak{d}\psi - \iiint dx\, dy\, dz\ \psi \nabla^2 \psi = 0\,,$$

implying that ψ had to vanish identically, and the proposition was proved.

From the point of view of this last proposition the problem of determining the degree of connectivity of a knot or link complement appears in a new perspective. Thomson's result showed that this degree was just the number of linearly independent irrotational flows satisfying fixed boundary conditions, or – seen in terms of the analogy between vortex motion and electromagnetism – of magnetic fields induced by currents in the given link. Thus, a seemingly quite complicated physical question, expressed in mathematical language as a question about potentials, now received a surprisingly simple answer in terms of a purely topological concept. Before stating his result, Thomson accordingly underscored that "hitherto we have seen no reason even to suspect the following proposition" (Thomson 1969, § 62). On the other hand, once the proposition was established, it could be read in the reverse direction, in which case the physical interpretation of the result suggested that the degree of connectivity of a space region equalled the number of essential physical parameters characterizing the possible flows the region admitted. Since, in addition to boundary conditions, the only available parameters in a system of n linked vortex tubes or currents were the n vortex or current strengths, the degree of connectivity of the given region had to be $n + 1$ no matter how the vortices or currents were linked or knotted. A physical argument of this kind may well have been a further reason for Thomson's and Maxwell's otherwise rather vaguely justified belief that knotting or linking of boundary surfaces does not change the degree of connectivity of the region enclosed.

§ 28. Both in terms of its content and the techniques of proof, Thomson's proposition appears as an analogue to Riemann's description of "Abelian integrals of the first kind" in the three-dimensional situation Thomson was considering, just as Helmholtz had indicated with his earlier remark that irrotational flows in multiply connected regions should be regarded as "integrals of the first kind" of the equations of fluid motion. Thomson himself saw the analogy, too, as he pointed out in his long report from Bad Kissingen. From a modern mathematical perspective, these results represented the first steps toward a theory relating the space of harmonic forms on a Riemannian manifold to its topology, culminating in Hodge's theory of harmonic integrals (Hodge 1941). It is beyond the scope of this study to follow the paths leading from Riemann's and Thomson's results to this modern theory, but a few hints may be given in order to show that the point of view developed by the Scottish physicists had an influence on later approaches.

First, Thomson's hydrodynamic insight into the relation between irrotational flows and topology soon found its way both into Maxwell's *Treatise* and into hydrodynamical textbooks like (Lamb 1879). In this manner, Thomson's ideas helped to make mathematicians aware of the fundamental relation between potential theory and topology. For Riemann surfaces, the interconnections between complex analytic functions, potentials, flows, and topology were made popular by Felix Klein's little monograph *Über*

Riemanns Theorie der algebraischen Funktionen und ihrer Integrale of 1882. While no reference to Thomson was made in the booklet, Maxwell's treatment of complex analytic functions along hydrodynamical lines provided the starting point of Klein's presentation (Klein 1882, § 1, note). When dealing with the central problem of the "most general stationary flow" on a Riemann surface, Klein pointed out that a more rigorous treatment relied on an application of Green's formula, and he referred to Tait's hydrodynamical justification of the latter (l.c., § 10). Therefore, it seems that Klein's particular interpretation of Riemann's theory was inspired at least as much by the British physicists as it was by Riemann himself, though he wanted his readers to believe that his views represented Riemann's original line of thought (l.c., iii–vi.). The next main step toward the modern understanding of the situation was then taken by Hermann Weyl in his *Die Idee der Riemannschen Fläche* of 1913. Weyl, who had discussed the material of his book with Klein, acknowledged both the heuristical force of the hydrodynamical interpretation of complex functions on a Riemann surface and the British background to it.[93] In this way, the physical background of dynamical theory, motivating Thomson's proposition on irrotational flows in a space region, contributed to shape the directions in which later explorations of the relation between potentials and topology continued to move.

Issues of reception

§ 29. Thomson's mathematical investigations, while providing an impressive starting point for further studies of vortex motion, failed to develop a sufficient mathematical basis for the physical speculation that motivated them. Indeed, his results were still far from offering a satisfying theory of the dynamics of vortex atoms. One of the problems yet to be addressed was that of the dynamical stability of vortices. While Thomson initially hoped that energy considerations would lead to the desired results, he had to admit in 1876 that not even the simplest cases had been solved:

> Hitherto I have not indeed succeeded in rigorously demonstrating the stability of the Helmholtz ring in any case. [...] The known phenomena of steam-rings and smoke-rings show us enough of, as it were, the natural history of the subject to convince us beforehand that the steady configuration [...] is stable. [...] But at present neither natural history nor mathematics gives us perfect assurance of stability when the cross section [of a Helmholtz ring] is considerable in proportion to the area of aperture.[94]

A second important gap in the theory concerned the investigation of the fundamental modes of vibrations of vortices, which were crucial for explaining the observed spectra of chemical elements. In several minor papers, Thomson dealt with a few special cases, such as the vibrations of an infinitely thin columnar vortex or a circular vortex ring with

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[93] Weyl even called the basic minimum problem whose solution guaranteed the existence of potentials on Riemann surfaces the "Thomson-Dirichlet principle." See (Weyl 1913, § 14.)

[94] (Thomson 1875, §§ 19–20.) See also (Smith and Wise 1989, 431 ff.)

an infinitely thin core.[95] These treatments, however, failed to lead to results which could be related to empirical data.

Even if it were granted that such mathematical difficulties could be overcome, imposing physical problems remained as well. How could the basic physical phenomena of gravitation, electromagnetism and light be explained on the basis of vortex atoms? Here Thomson was led to ever more complicated assumptions about ether vortices. Regarding gravitation, he sought to adapt Le Sage's explanation of gravitation by means of tiny "ultramundane" corpuscles, which were conceived as vortices much smaller than the atoms of matter. Thomson hoped that the transmission of electromagnetic waves and light in ether could be accounted for by assuming that the space between material atoms also carried a regular vortex structure that formed into an elastic "vortex sponge."[96]

Until well into the 1880's, these mathematical and physical shortcomings were not considered serious obstacles to pursuing the idea of vortex atoms. A few years after his first public announcement of the theory, Thomson's approach had become quite well established as a promising candidate for a future atomic theory. Maxwell's favourable discussion in the 9th edition of the *Encyclopedia Britannica* (Maxwell 1875) has already been mentioned. In that article, Maxwell even went to some length in laying out the mathematics of vortex motion. Regarding the topology of vortices, Maxwell hinted at the great number of knots and links which might account for the different types of atoms: "The number of essentially different implications of vortex rings may be very great without supposing the degree of implication of any of them very high." (Maxwell 1875, 471.) Shortly after the appearance of this article, Thomson's collaborator, Tait, would take up this suggestion.

In 1882, the theory appeared to have reached a state worthy of institutional encouragement. For the University of Cambridge's Adams Prize for 1882, the subject selected was "A general investigation of the action upon each other of two closed vortices in a perfect incompressible fluid." The prize was awarded to J. J. Thomson, whose essay not only discussed stability and vibrations of symmetric groups of ring vortices, but, in its most original parts, also hinted at an explanation of chemical valency and chemical combination via interactions of (possibly linked) groups of ring vortices. With hindsight, one perceives in this study some elements of J. J. Thomson's later atomic model.[97] Outside Britain, William Thomson's speculation also received some attention. For the German journal *Mathematische Annalen*, A. E. H. Love contributed a survey *On recent English researches in vortex-motion*, emphasizing that "the chief physical interest of vortex-motion lies in the speculations of Sir W. Thomson as to the ultimate constitution of matter" (Love 1887, 326). In France, Thomson's admirer L. M. Brillouin intended to publish a series of papers on vortex atoms.[98]

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[95] See e.g. (Thomson 1880). Not all of these papers were published; cf. e.g. the title of Thomson's talk to the R.S.E., 15 April 1878, in the appendix.

[96] For details, see (Silliman 1963), (Siegel 1981, 256 ff.), and (Smith and Wise 1989, 425 ff., 438 ff.).

[97] (J. J. Thomson 1883); see also (Silliman 1963).

[98] This is mentioned by (Love 1887, 327, note).

In all such presentations of Thomson's program, the role of topology was recognized and acknowledged but, with the exception of Tait, not seriously pursued.[99] By the end of the 1880's, however, the physical difficulties of the theory seemed more and more pressing, and these ultimately proved fatal. The problems of explaining mass, gravitation, electromagnetism, and light on the basis of vortex atoms resisted any quantitative treatment. Moreover, no real progress was made in proving the dynamical stability of vortices in a perfect fluid, and virtually all calculations of modes of vortex vibrations showed the periods of vibration to be dependent on the energy of the vortex, a result that stood in blatant contradiction with the experiences of spectrum analysis. In 1887, Thomson had to admit that "the most favourable verdict I can ask [...] is the Scottish verdict of *not proven*," and in the years to follow he gradually abandoned his brilliant speculation.[100]

§ 30. If vortex atoms turned out to be a dead end for physics, the same cannot be said for the mathematical ideas that accompanied this theory. While this has been acknowledged since the 19th century with respect to Thomson's hydrodynamical innovations, our narrative also makes clear that in the line of research leading from Helmholtz's investigation of vortex motion to Thomson's theory of vortex atoms, topology entered a basic physical theory in a highly nontrivial way. The Scottish mathematical physicists, notably Maxwell and Thomson, were acutely aware of this fact. Their correspondence and writings during this period show that they struggled with at least three interconnected problems of a topological nature which would require a solution if Thomson's theory were to become successful. The central one, motivated by Thomson's idea to replace vortices in a perfect fluid by bodies of arbitrary shape, was to determine the set of irrotational flows in multiply connected space regions. This problem required an understanding of the topology of such regions and, in particular, a determination of their degree of connectivity. Maxwell's formula, expressing this number in terms of the genus of the boundary components, provided a means to calculate it without, however, giving more than an intuitive argument to justify his finding. Thomson's discussion of Maxwell's result then tried to spell out at least some of the potential difficulties that arise when regions which are complements of a system of knotted or linked channels are taken into account. I have tried to show that it was the physical content of Thomson's proposition on irrotational flows, rather than a precise mathematical analysis, that lent support to Thomson's belief in Maxwell's formula. In this work, a difference was discerned between the degree of connectivity of a knot or link complement and what Thomson called its "quality of connectivity." This subject reached well beyond fluid dynamics and the technical tools Maxwell and Thomson had employed up till then, and its elaboration would have required a direct attack on the problem of knot and link classification. Maxwell's writings of the late 1860's show that he was aware of the problem, but he apparently decided not to tackle it seriously. Soon afterwards, Peter Guthrie Tait did.

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[99] A typical statement on the topological aspects of the problem may be found in (Love 1887, 327).

[100] (Thomson 1887, 320); further details regarding Thomson's disengagement can be found in the literature cited in previous footnotes.

III. Knot chemistry

Chronicle: Knot chemistry	
1876	Tait embarks on knot classification and begins to publish a series of related papers.
1877	Pursuing a hint from Maxwell, Tait reads Listing's *Vorstudien* and reports on it to the R.S.E.; a long paper *On knots* collects Tait's results.
1883	Tait describes *Listing's Topologie* in a paper for the *Philosophical Magazine*, calling attention to knot classification.
1884	Reverend Kirkman redoes Tait's results in terms of "polyhedra" and provides Tait with "polyhedral data" for knots with 8 and 9 crossings. Using Kirkman's results, Tait extends his classification in *On knots II*. First knot tables are published.
1885	*On knots III* tabulates alternating knots of order 10, again drawing on Kirkman's work.
1885	The American engineer Little starts to produce knot tables.
1886	The Edinburgh Chemist Crum Brown publishes short papers on topological issues.
1889	Little tabulates non-alternating knots of orders 8 and 9.
1890	Little tabulates alternating knots of order 11.
1899	Little tabulates non-alternating knots of order 10.
1917	Mary Haseman tabulates amphicheiral knots of order 12.

§ 31. Among other things, William Thomson's version of dynamical atomism intended to shed light on the problem of explaining the variety of chemical elements and, if possible, the nature of chemical structure. The latter topic received growing attention during the late 19th century, mainly in consequence of progress in organic chemistry, including Kekulé's discussion of the structure of benzene in 1865 and subsequent work on the isomerism of hydrocarbons. However, in the late 1860's, Thomson and Maxwell only hinted at the task of classifying the possible forms of knots and links which would serve to represent the various chemical elements on the basis of the vortex atom theory. This task clearly represented an important step in the extension of Thomson's program in the direction of chemistry.

It was Peter Guthrie Tait who, in 1876, decided to make a serious effort toward the classification of knots. In this way, Tait hoped at least to lay the groundwork for a vorticist approach to the problems of chemistry. On the one hand, a list of knots (and perhaps links) would provide a universe of possible forms for vortices from which those forms could be selected which might actually represent chemical elements. On the other, a study of the various forms of knots and links might perhaps even be useful in approaching the problem of chemical structure, if the latter was also conceived in topological terms.

Tait's research may be seen as related to a broader interest in representing the structure of molecules by means of symbolical graphs. This interest had arisen during the 1860's, and by the 1870's, several mathematicians, including Arthur Cayley and Joseph J. Sylvester, had become involved in developing graph-theoretic methods applicable to chemical structure. The problem of classifying knots and links, or, more precisely, knot and link *diagrams*, seemed to fit naturally into this context. Along with his work on knots, Tait more than once exchanged ideas with his brother-in-law, the Edinburgh chemist Alexander Crum Brown, whose version of a graphical notation of chemical structure, proposed in the early 1860's, had since come into general use. While Tait

eventually took up Crum Brown's method of notation in his classification enterprise, the chemist became interested in topology as well. In the mid-1880's, Tait's classification project developed into a modest tradition of knot tabulations. Contributions to this project were made by the combinatorially-minded Lancashire clergyman, Thomas P. Kirkman, the American professor of civil engineering Charles N. Little, and, as late as 1917, by Mary G. Haseman, who wrote a dissertation on knots at Bryn Mawr College. In the following, the development and content of Tait's classification project and its main contexts will be briefly sketched.[101]

A periodic table of knots?

§ 32. Nine years elapsed between Tait's smoke ring experiments and his attack on the problem of knot classification. We have seen that Tait had initially been somewhat sceptical about Thomson's idea of vortex atoms, but in the early 1870's he began to include Thomson's theory into his regular lecture courses on natural philosophy, and he also addressed the topic repeatedly in popular lectures before different audiences. Usually he discussed the various hypotheses on the constitution of matter at the outset of his courses. Thus, a typical presentation would include a description of Lucretius' hypothesis of elastic balls, of Boscovich's centre of force hypothesis, and, as preparation for the discussion of Thomson's ideas, a qualitative review of Helmholtz's results on vortex motion.[102] Tait would then perform his smoke ring experiments, emphasizing that the idea of vortex atoms was of a very fundamental character. That he made his own views plainly known can be seen from notes taken by of one of his students in the 1871-72 lectures. In connection with Tait's smoke ring experiments, the student described an "Example of Vortex ring, formed by the smoke, arising from the ignited gunpowder, as it curls up on the air, and spreads along the ceiling. One of the most plausible hypotheses yet made in regard to matter is, that matter is nothing but energy."[103] In later lectures of the course, reference to vortex atoms would be made now and then in other contexts in order to make an argument.[104]

Of course, Tait also saw the mathematical difficulties which stood in the way of a straightforward pursuit of Thomson's programme. In their common metaphysical manifesto of 1875, *The Unseen Universe* (to be discussed in Section V), Tait and the Belfast

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[101] It would take too much space to give a full treatment of Tait's endeavours here. See my *History of Knot Theory*, forthcoming, for a more detailed discussion.

[102] To leave out advanced mathematics was part of the general style of Tait's lectures; see (Wilson 1991).

[103] Tait, *Lectures on natural philosophy*, Edinburgh University 1871–1872. Notes taken by I. Gray, Edinburgh University Library, Gen 1408.

[104] Instances of Tait's presentation of vortex atoms may be found in the lecture notes of his regular courses by I. Gray (1871–1872), Andrew D. Sloan (1881–1882), P. P. Easterbrook (1885-1886), all in the University Library at Edinburgh, and by G. M. Barrie (1880–1881), in the National Library of Scotland, MS 6654. The argument was also presented in Tait's popular lectures on *Recent Advances in Physical theory* (Tait 1876a) and in his "Lectures for Ladies" (1875–1876), see the notes by Elisabeth Haldane, National Library of Scotland, MS 20200.

experimental physicist Balfour Stewart included a passage describing Helmholtz's results on vortex motion, declaring that very little had been achieved mathematically on this topic in the meantime. Thomson's speculation, they wrote,

> promises to be very valuable from one point at least, viz., the extension and improvement of mathematical methods; for in the treatment of its very elements it requires the application of the most powerful of hitherto invented processes, and even with their aid, the mutual action of two ring-vortices (the simplest possible space-form [read: knot or link]) has not yet been investigated except in the special cases of symmetrical disposition about an axis. (Stewart and Tait 1875, § 134.)

In the margin of his private copy of the first edition of *The Unseen Universe*, Tait added: "Nay more; even the undisturbed form of the simplest knotted vortex (that which is drawn on our title page) has not yet been investigated. If any competent mathematician were to devote *his whole life* to this study of this one form."[105] At about the time Tait wrote this comment, he himself embarked on the project of knot classification.

The first documented manifestation of Tait's interest in knots came in 1876 in the form of a short communication to the mathematical section of the annual meeting of the British Association for the Advancement of Science. This note contained an observation which Tait had made earlier on the occasion of "designing Vortex atoms of various forms" for Thomson, namely that two closed plane curves which have only a finite number of transverse intersections (which may be self-intersections) always meet in an even number of points.[106] Tait pointed out two immediate implications: first, that every plane curve with at most finitely many transverse self-intersections could be viewed as a plane projection of a knot in which the double points of the curve alternately represent over- and undercrossings of the arcs of the knot. Thus it seemed possible, at least for such alternating knots, to base the classification of knots on a preliminary classification of closed plane curves with finitely many double points. Supposing this could be done, the crucial problem that remained was: what are the conditions under which two closed plane curves represent projections of knots which can be deformed continuously into one other? The second consequence mentioned by Tait was that the regions of any regular knot projection could be coloured black and white in a chequerboard-like fashion, a fact which came to play a technical role in Tait's later work. Tait concluded his short communication with the words: "The development of this subject promises absolutely endless work – but work of a very interesting and useful kind – because it is intimately connected with the theory of knots, which (especially as applied in Sir W. Thomson's Theory of *Vortex Atoms*) is likely soon to become an important branch of mathematics." (Tait 1876b, 272.)

During the academic year 1876–1877, Tait devoted himself intensively to the study of knots. On 16 October 1876, he submitted a sealed envelope to the Royal Society of

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[105] Edinburgh University Library, Df. 3. 87, note to § 134.

[106] See (Tait 1876b); the historical remark is in (Tait 1877b, 309 f.). Of course, this proposition, a consequence of the Jordan curve theorem, was not and could not have been proved in any modern sense by Tait.

Edinburgh containing some remarks on knot diagrams which he apparently regarded as marking a breakthrough in knot classification.[107] In December 1876, he read a paper to the Royal Society of Edinburgh, the first in a series of seven which were then reworked into a long article (Tait 1877g). These seven papers, all published in the R.S.E. *Proceedings*, make possible to give a a detailed analysis of the development of Tait's endeavours. In the following, I sketch the various topics Tait addressed in the order in which they appear in these communications, adding remarks on his later achievements regarding the same topics.

§ 33. As with Maxwell's earlier work, which he seems not to have known, Tait's first step was to find a suitable symbolic notation for knot diagrams which could serve as the basis for a combinatorial treatment. His method, though similar to Maxwell's, was closer to one which had been employed much earlier by Gauss.[108] Starting with a knot diagram, i.e. a closed plane curve with finitely many double points or crossings, Tait proposed to choose an initial point and an orientation (see Fig. 10), and then to label the first, third, fifth, etc., double point by letters $A, B, C, \ldots$. In this way, all double points would be labeled uniquely. The series of letters attached to the crossings, read along the diagram and including the even places, Tait called the *scheme* of the knot.[109]

Evidently, a scheme was determined by the sequence of letters in the *even* places (we may call this the *condensed scheme*). However, not every sequence of letters could actually arise, and some sequences led to obviously reducible diagrams. If, for instance, the letter A occurred in the first or last place of the condensed scheme, the diagram in the neighbourhood of A had to appear like Fig. 11 (left). In this instance, A could be removed from the diagram by untwisting the loop. More generally, a symbol was considered "nugatory" if it corresponded to a diagram crossing which separated two completely distinct parts of the knot as in figure 11 (right).

The scheme of a diagram depended on the choice of the initial point and the orientation. This gave rise to an equivalence between schemes which could easily be translated into purely combinatorial terms.[110]

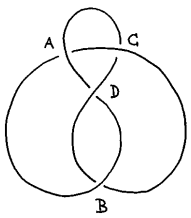


Fig. 10. A knot with scheme $ACBDCADB$

[107] National Library of Scotland, Acc. 1000, no. 376. The most important remark on the sheet of paper in the envelope will be discussed in § 34 below. The envelope was opened for the first time in 1987.

[108] The relevant fragments of Gauss' *Nachlaß* were unkown to Tait as they were only published in 1900 in the eighth volume of Gauss' *Werke*.

[109] See (Tait 1876c, 238), (Tait 1877g, § 5).

[110] Tait's clearest explanation of this is in (Tait 1877g, § 5).

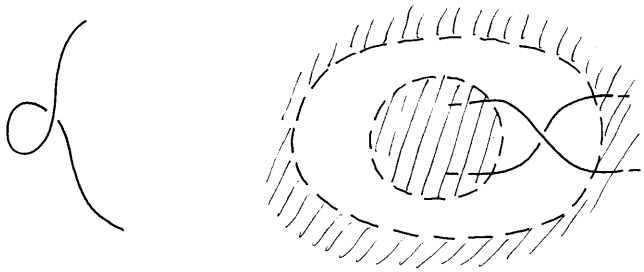


Fig. 11. Nugatory crossings

By introducing this scheme, Tait could devise a strategy for classifying knots as follows: (1) find all not obviously nugatory condensed schemes, i.e. all permutations of n distinct signs $A, B, C, \ldots$ such that A is not in the last or first place, B is not in the first or second, etc.; (2) find all combinatorially equivalent schemes, and retain only one scheme of each equivalence class; (3) of the remaining schemes, single out those which actually represent knot diagrams; (4) determine which diagrams of the list obtained in the third step represent equivalent knots. Strictly speaking, this last step consisted in answering two questions: (4a) given a knot diagram, which choices of over- and undercrossings give rise to irreducible knots, i.e. knots which do not possess a diagram with fewer crossings? (4b) which among all the irreducible knots resulting from diagrams with a given number n of crossings are equivalent?[111]

The first two steps were purely combinatorial problems, and Tait succeeded in attracting Cayley's and Muir's interest to the first of these in early 1877. Using determinant methods, Muir gave a formula for the numbers u_n of schemes satisfying condition (1). For three to eight crossings, Muir's formula gave the numbers:

$$u_3 = 1,\ u_4 = 2,\ u_5 = 13,\ u_6 = 80,\ u_7 = 579,\ u_8 = 4738,$$

showing that the number of schemes to be tested increased rapidly.[112] The third step in Tait's procedure was partly a combinatorial task (involving several tests based on the idea of looking at subcycles of schemes), and partly a matter of empirical verification by drawing the associated diagrams. If done with care, this process would lead to a complete list of knot diagrams with a given number of double points; Tait managed to work out this step up to and including the case of seven crossings (see below). However, Tait and his followers, like Gauss earlier, were not able to provide efficient algorithms for this part of the problem.[113] As it turned out, the fourth step emerged as the hardest and least clear part of the whole procedure. For most of his work, Tait only considered *alternating* choices of over- and undercrossings, thus bypassing step (4a).

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[111] This overall strategy was only vaguely hinted at in the published abstract of Tait's first paper. However, Tait clearly *followed* it when discussing the simplest types of knots. It was then spelled out in those parts of (Tait 1877g) which present the arguments of the earlier paper in more detail.

[112] See (Muir 1877). Cayley simplified Muirs results by studying the generating function of the problem, $u_3 + u_4 x + u_5 x^2 + \cdots$, cf. (Cayley 1877a).

[113] This gap was only filled by (Dehn 1936b).

§ 34. From the beginning, Tait was also interested in finding numerical invariants of knots that could express something like their degree of complexity. Both Thomson and Maxwell had thought about such a measure, and the degree of connectivity of space regions suggested that something similar might be attained for knots.[114] The first such possibility Tait discussed was the minimal number of crossings any diagram of a given knot type might have. Already the sealed envelope handed in to the R.S.E. stated a conjecture about this number which has since become known as "Tait's first conjecture."[115] The cryptic statement "If the simplest is $+-+-+-$ then irreducible" given in the envelope meant that an alternating diagram (the signs in the quote refer to over- and under-crossings) without nugatory crossings would necessarily have the minimal number of crossings. Tait gave no argument for this claim, and even his long paper of 1877 qualified the proposition as "obvious." Nevertheless, in this paper he added a sort of intuitive justification: "For the only way of getting rid of such alternations of $+$ and $-$ along the same cord is by *untwisting*; and this process, except in the essentially nugatory cases, gets rid of a crossing at one place only by introducing it at another." (Tait 1877g, § 4.)

The interpretation this argument requires some care. In this passage, Tait came close to formulating a second conjecture which provides a mathematical justification not only for his statement on the crossing number of reduced alternating diagrams but also for several of Tait's further claims. We shall see below that from his reading of Listing's *Vorstudien zur Topologie*, Tait was inspired to consider operations on a knot diagram which "twist" a part of the diagram by 180 degrees as in the following Fig. 12.

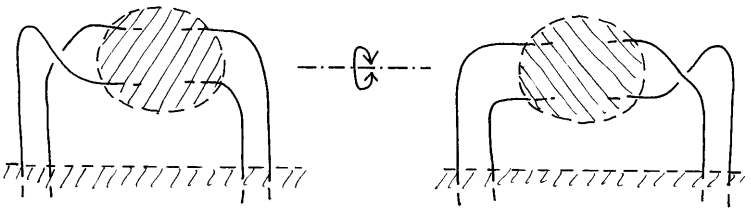


Fig. 12. A "twist" (the shaded regions may contain arbitrary completions of the diagram; the lower part remains fixed)

A reasonable reading of Tait's claim is that a non-nugatory crossing of an alternating diagram may only be removed by a twist, and therefore the total number of crossings remains the same. In this form, however, Tait's argument is inconclusive since it does not rule out the possibility of reducing the number of crossings through a series of diagram deformations which *increase* the number of crossings in the first stages. Thus one might be inclined to favour a stronger interpretation of Tait's argument, saying that *any two reduced alternating diagrams of the same prime knot are related by a sequence of twists.*[116] This latter assertion is what modern knot theorists have come to call "Tait's

[114] A similar interest in a numerical degree of knottedness is documented by (Klein 1876). See below, Section V.

[115] See e.g. (de la Harpe, Kervaire and Weber 1986, § 9). It was first publicly stated in (Tait 1876c, 239) and then again in (Tait 1877g, § 4.)

[116] A knot is called "prime" if it is not composed of two separate knots tied on the same string.

second conjecture." It is not clear whether Tait actually held this conjecture, today known to be a valid theorem.[117] Indeed, it never appears explicitly in Tait's writings and some of his remarks make it appear probable that he did *not* think that twists were sufficient to generate all reduced alternating diagrams of a given alternating knot. On the other hand, the combination of this conjecture with its corollary on the crossing number of reduced alternating diagrams provides a solution to step (4) in Tait's classification strategy in the case of alternating knot diagrams. Consequently, together with an algorithm for enumerating diagrams, these conjectures solve the classification problem for alternating knots by a procedure which, for every given crossing number, consists of finitely many steps. Thus, although Tait never explicitly formulated his so-called "second conjecture," we may understand why a consideration of the twisting operation allowed Tait to construct correct and, for the cases he studied, complete tables of alternating knots.

§ 35. Apparently, Tait hoped initially that the natural measure of the complexity of a knot was not the minimal crossing number of its diagrams but a different invariant which he termed the "beknottedness" of a knot. His first, tentative definition involved a distinction between the possible orientations of diagram crossings. For this purpose, he invented a method of "going round the curve" while throwing silver and copper coins into the corners of the various diagram regions reached ("silver to the right when crossing over, to the left when crossing under;" the other corner received a copper coin). The method was independent of the orientation chosen and led to a distinction between "silver" and "copper" crossings (see Fig. 13) (Tait 1877a, 290).

"The excess of the silver over the copper crossings" seemed to Tait to be the natural measure of beknottedness he was looking for. As the possibility of twisting an arc into a little loop shows, this number (later called "the twist" of a knot diagram by C. N. Little[118]) had to be calculated using *reduced* diagrams, i.e. diagrams without nugatory crossings. Tait and his followers wrongly believed that the twist number so defined was a knot invariant in the full sense; Little would even state and "prove" this as a theorem (see § 41 below). Still, the twist number is indeed an invariant of reduced *alternating* diagrams in consequence of the twisting theorem ("Tait's second conjecture").



Fig. 13. Silver (left) and copper (right) crossings

[117] After having been one of the challenging open problems of knot theory for some time, the twisting conjecture was proved by (Menasco and Thistlethwaite 1993). Following (Conway 1970), the operations here called twists are often called "flypes" today, a term used by Tait to denote a different operation on a diagram (corresponding to the inversion of a knot with respect to a 2-sphere in space). Tait's first conjecture had already been proved earlier by (Murasugi 1987). The modern proofs of both conjectures rely heavily on Vaughan Jones's new polynomial invariant for knots.

[118] In modern texts one also finds "writhe."

By considering two-component *links* instead of knots and by only counting crossings where *both* components meet, Tait obtained a similar number which he knew well from Maxwell's work and which may even have inspired him to try something similar for knots: Gauss' linking number. This connection inspired him to look for an electromagnetic interpretation of his measure of beknottedness by considering the work done when a magnetic particle (a "pole") is carried along a current in a knotted circuit. He immediately observed, however, that such an interpretation could not be defined unambiguously since one had to introduce a convention in order to determine the exact path of the particle.[119]

As Tait realized, changing an overcrossing into an undercrossing or vice-versa increases or decreases the twist number of a diagram by two. Looking at some examples of alternating knots, Tait was led to wonder whether half the twist number was equal to another measure of the complexity of knots. "It is probable after all," he wrote, "that the true measure of beknottedness is the smallest number of signs in a scheme [indicating over- and undercrossings] which must be altered in order that the wire may cease to be knotted." (Tait 1877a, 294.) The idea of relating the easily calculable twist number to this new notion of beknottedness was, to say the least, premature, as Tait immediately realized. For instance, the well-known knot with four crossings (see Fig. 1) had twist number zero, whereas at least one crossing had to be changed in order to unknot it. This induced Tait to ask a new question: Under what conditions would a knot diagram have zero twist number? According to his own account, this led him "to see that there is a class of knots which are *capable of being changed from right-handed to left-handed, without change of form*, by the ordinary processes of deformation." (Tait 1877a, 295.) To these knots, Tait gave the name of *amphicheirals*. If the twist number of reduced diagrams was indeed an invariant, as Tait supposed, diagrams of amphicheiral knots would necessarily have vanishing twist number since a reversal of all crossings leads to the negative of the original twist number. Once more Tait was able to come up with an assertion which was correct if restricted to reduced alternating diagrams like that of the four-crossing knot. For knots with such diagrams, the twist number is an invariant and thus amphicheirality does indeed imply that this number is zero.

The obvious difficulties with the twist number induced Tait to invent various *ad hoc* methods to make "corrections" of this number in order to save its relation to "beknottedness" in the new sense. None of these methods, however, led to a satisfactory general result. In the end he had to admit: "There must be some very simple method of determining the amount of beknottedness for any given knot; but I have not hit upon it."[120] While Tait was still struggling with these ideas, he also noticed that the minimal number of crossing changes needed to open a knot behaved quite differently than the minimal

[119] (Tait 1877a, 290 f.). Since Tait's time, several attempts have been made to define an appropriate "self-linking number" of knots along the lines indicated by Tait, but none of these led to a topological invariant of knots. A natural convention about the path of the magnetic particle can be made if the knot is endowed with a *framing*, i.e. a smooth field of normal vectors. In this way, an invariant for "framed knots" is obtained which came to play a role in the characterization of 3-manifolds by surgery on links by Lickorish and Kirby in the 1960's and 1970's; see e.g. the discussion in (Kauffman 1991, 250 ff.).

[120] See (Tait 1877g, § 42). To the best of my knowledge the situation has not changed up to the time of writing (July 1997).

number of diagram crossings; in particular, sometimes diagrams with fewer crossings had greater beknottedness. This induced him to distinguish these two quantities by introducing the term *knottiness* for the minimal crossing number (Tait 1877a, 296, note).

§ 36. When Tait's second communication to the R.S.E. containing his reflections on a measure of beknottedness was printed, he added the following remark:

> Professor Clerk-Maxwell, to whom I sent some of the above results (and to whom, as well as to Sir W. Thomson, I am indebted for various hints, usually in the especially valuable form of criticisms and reasons for doubt), has lately called my attention to a paper by Listing, of date 1847, part of which is devoted to the subject of knots. [... The author] virtually shows, by giving a particular case, that the method of deformation which I employ does not always give all possible forms of a completely knotted wire. [...] I propose to give the Society an account of Listing's method and results on the earliest opportunity.[121]

Tait's following communication kept this promise by explaining in detail Listing's ideas on knots and, in particular, the latter's method of representing a knot by a so-called "type symbol." Moreover, Tait reconsidered an example Listing had given of two diagrams of the same knot (see Fig. 14), and he found that these diagrams were related by the kind of deformation by twisting as discussed above. It was this example which gave Tait the general idea to use twisting as a method for transforming diagrams.

As his concluding remarks show, Tait was rather puzzled by this new aspect of knots. Realizing that there were even more combinatorial problems involved than he had expected, he even thought about quitting the field:

> In conclusion, it appears that the problem of finding all the absolutely distinct forms of knots, with a given number of intersections, is a much more difficult one than I at first thought; and it is so because the number of really distinct species of each order is very much *less* than I was prepared to find it. The question now belongs more to quantitative than to qualitative relations. It resembles, in fact,

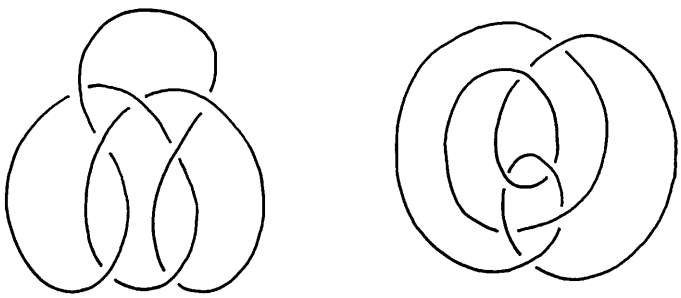


Fig. 14. Listing's equivalent knots

[121] See (Tait 1877a, 297 f.). The remark was dated 27 January 1877. Only a few days earlier, Tait had received Maxwell's suggestion; see Maxwell to Tait, 22 and 24 January 1877.

> the species of problem originally suggested by Crum Brown, and resolved by Sylvester and Cayley, of determining the number of conceivable Hydrocarbons under given conditions of limitation. And here I am glad to leave it, for at this stage it is entirely out of my usual sphere of work, and it has already occupied too much of my time. (Tait 1877b, 315 f.)

Apparently, Tait hoped that scientists interested in the combinatorics of graphs representing the structure of chemical compounds would take up the issue. In fact, the years following 1874 had seen an increasing activity in this domain. Based on Crum Brown's graphical notation of chemical structure (which is essentially the modern one),[122] Arthur Cayley wrote a series of articles enumerating the graphs representing the possible isomers of various series of hydrocarbons. A little later, William Kingdon Clifford and James Joseph Sylvester also published contributions on the problem based on the theory of invariants (in this way, the method of graphical notation also entered the latter field).[123] Tait's suggestion that one of the experts in this kind of questions should turn to knot diagrams was certainly reasonable in view of the types of problems that had to be solved. Moreover, it made sense from a disciplinary point of view. To classify knots and links should, Tait hoped, be as useful for chemistry as for physics or mathematics, just as the enumeration of hydrocarbon isomers had been.

Graphical formulae for molecules and knots

§ 37. Indeed, Tait's disillusionment with the classification of knots proved to be only a momentary lapse of interest. Just two weeks after expressing his discontent with the state of the subject, in fact, he was back in the fray. In his next communication, he entered into a further discussion of Listing's type symbol, especially as applied to links. Moreover, he presented a new method for enumerating knot and link diagrams, inspired by Crum Brown's graphical notation of chemical compounds.

Listing's symbolic representation of knots had been based on a rule for marking the corners of a knot (or link) diagram associated with the following Fig. 15. Connecting two opposite regions by an axis running between the two arcs of the link, these arcs turn around the axis either like a right-handed or a left-handed screw. Accordingly, the regions were marked r or l, respectively.[124] In slightly modernized notation, Listing's symbol – originally called the "Complexions-Symbol" – was defined to be a polynomial of the form:

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[122] Brown's notation was first published in the R.S.E. *Transactions* in 1864 and made popular by an introductory monograph written by Edward Frankland (Frankland 1866). Other versions of graphical notation had been used by various other chemists; see (Russell 1971).

[123] See (Cayley 1874), (Cayley 1875), (Cayley 1877b), (Sylvester 1878a), (Sylvester 1878b), and (Clifford 1878). For a discussion of these contributions to chemical graphs, see (Biggs, Lloyd and Wilson 1976, 60 ff.).

[124] The notation is Tait's. Listing had actually interchanged left and right in his notation since he had used an orientation of space different from the modern convention; see (Listing 1847, 52). When Maxwell prepared his *Treatise*, he established a consensus in the London Mathematical Society for adopting the system based on the "right hand rule;" see below, § 45.

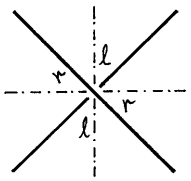


Fig. 15. Marking corners of diagrams

$$\sum \alpha_{ij}\, r^i l^j,$$

where each term $\alpha_{ij} r^i l^j$ represented all diagram regions with precisely i marks r and j marks l; the coefficients α_{ij} were just the number of regions of type $r^i l^j$, including the outer region (see Fig. 16 below for an example).

While Listing had been interested in relations between the polynomials of different diagrams of the same knot, Tait soon found that diagrams of different knots or links might lead to the same "type-symbol," as he preferred to call it.[125] In particular, Listing's symbol might equally well represent a link or a knot, as Tait illustrated by several examples. Still, Tait found Listing's idea of basing a symbolic representation on diagram *regions* rather than diagram *crossings* attractive and tried to improve it, drawing further inspiration from chemical graphs:

> There is no connection between the type-symbol, as Listing gives it, and the singleness or complexity of the curve represented, but it is possible to make analogous symbols capable of expressing everything of this kind. Only we must now adopt something very much resembling Crum Brown's Graphical Formulae for chemical composition. Some very remarkable relations follow from this process, but I can only allude to a few of the simpler of them in this abstract. (Tait 1877c, 326.)

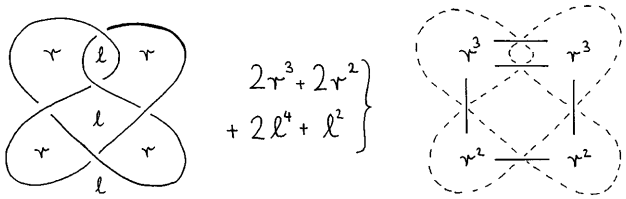


Fig. 16. A five-crossing knot, its type symbol, and its graphical formula

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[125] Apparently, Listing's hope in introducing "Compexions-Symbole" had been to establish a calculus in which polynomials representing different diagrams of the same knot were considered equivalent. Since the "defining relations" were unknown, however, the idea was not easy to work out; see (Listing 1847, 58). Given these difficulties, Tait's renaming of Listing's symbol as a "type-symbol" does not seem very happy.

Tait's new method was restricted to alternating diagrams or, equivalently, to link projections in which over- and undercrossings were not distinguished. In fact, in alternating diagrams all marks of a given region were identical, giving rise to a checkerboard distribution of "right-handed" (r) and "left-handed" (l) regions (a fact which Listing had already observed). Focusing on one kind of region only, say the right-handed ones, Tait encoded their arrangement in a graphical formula similar to a chemical structure formula, in which each region was represented by its symbol r^i (in later versions, only by the number i), while each crossing joining two regions r^i and r^j was represented by a dash (see Fig. 16). Any region of type r^i was connected to others by exactly i crossings, and thus the numbers i represented the valencies of the vertices of the graphical formula representing a given diagram. From the new graphical formula, the diagram itself could easily be recovered by joining mid-points of the edges as indicated in the figure below. Moreover, the *dual* graphical formula obtained by interchanging vertices and regions gave the arrangement of left-handed diagram regions.[126]

These properties of the new formulae suggested that they could be used in the enumeration of knot and link diagrams. The only restrictions on the valencies i were that their sum had to be even and no i could be less than 2 or greater than the sum of the others. Hence an alternative strategy for making a complete list of knot and link projections arose:[127] first, find all the ways in which an even number $2n$ can be written as a sum in which no summand is less than 2 or greater than n; and second, determine whether the resulting sets of numbers can be realized as the valencies of a "graphical formula," thereby enabling one to draw the corresponding diagrams. Of course the problem of checking whether the resulting diagrams actually correspond to different knots remained the same as in the earlier approach. Motivated by his new ideas, Tait remarked: "I propose, when I have sufficient leisure, to re-investigate the whole subject from this point of view." (Tait 1877c, 330.) From this point onward, Tait's articles abound with graphical formulas very much like the structure formulas found in contemporary papers of organic chemistry.

Tait concluded his present paper by describing all knots of sixfold knottiness, based however not on the new method of graphical formulae but on a consideration of the 80 schemes of six letters that are not obviously nugatory. It turned out that only *four* distinct alternating knot types occurred, one of them being the composite knot made up of two trefoils. (See Fig. 1 for the three prime knots. Tait did not mention the non-alternating version of the composite form.)

§ 38. Up to this point, the results of Tait's endeavours proved rather unsatisfactory with respect to the vortex atom theory, as too few distinct knot types had been found. Thus it seemed necessary to proceed to the study of more complicated knots. Tait's next communication, discussing *Sevenfold knottiness*, addressed Thomson's theory in its first

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[126] Tait did not use the term "dual" although he probably knew it from Maxwell's paper on graphical statics which was read to the R.S.E. in 1870 and published in the R.S.E. *Transactions*. Today, the graph underlying Tait's formula is often called the graph associated with a link; see e.g. (Kauffman 1991, 47 f.).

[127] Explicitly stated in Tait 1877g, § 21.

lines. Here, he claimed that the small number of distinct knots with few crossings was actually not a vice, but a virtue of the theory, for as he explained:

> From the point of view of the Hypothesis of Vortex Atoms, it becomes a question of great importance to find how many distinct forms there are of knots with a given amount of knottiness. The enormous numbers of lines in the spectra of certain elementary substances show that the form of the corresponding Vortex Atoms cannot be regarded as very simple. But this is no objection against, it is rather an argument in favour of the truth of, the Hypothesis. For not only are the great majority of possible knots not stable forms for vortices; but altogether independently of the question of kinetic stability, the number of distinct forms with each degree of knottiness is exceedingly small. (Tait 1877d, 363 f.)

The idea behind this slightly involved argument seems to have been that since there were only surprisingly few types of knots with a small number of crossings (some of which could even lack dynamical stability), any explanation of chemical elements by means of knotted vortices would necessarily involve rather complicated knots – but this was just what the complicated structure of observed spectra would make one expect. Using a combination of his two methods, Tait now found that there were seven prime knots and a single composite alternating one with crossing number seven, and he listed them in the new notation (see Fig. 17). Again he did not consider the possibility of non-alternating forms, a gap which he only filled in his next major paper (Tait 1877g, § 13).

Still, the number of knot types was too small for the purposes of vortex chemistry, and Tait felt that the labour involved in extending the classification would be too great for him: “Eight and higher numbers [of knottiness] are not likely to be attacked by a rigorous process until the methods are immensely simplified.” (Tait 1877d, 364.)

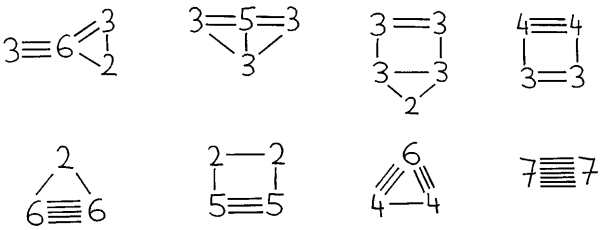


Fig. 17. Formulae for sevenfold knottiness

§ 39. In May 1877, Tait summed up and extended his previous findings in a long paper for the R.S.E. *Transactions*. Beyond the topics already discussed, this paper was full of all kinds of ideas and suggestions (e.g., on amphicheiral knots, on “plaits,” which is Tait’s term for braids, and on Möbius type bands), some of them full of insight, some of them miscarried if not erroneous.[128] Perhaps the most valuable group of remarks

[128] Tait asked Maxwell to read the proofs of the paper, but their correspondence does not tell whether Maxwell actually did Tait the favour. See Tait to Maxwell, 13 and 30 June 1877; Maxwell to Tait, 13 July 1877.

pertained to his collection of various ways of deforming a knot.[129] These served as the basis for the final step in Tait's classification strategies. He freely admitted, however, that he had "not been able as yet to find a general method" of finding all possible diagrams of a given knot, nor had he "discovered, what would probably solve this difficulty, any perfectly general method of pronouncing at once from an inspection of its scheme or otherwise, whether a knot is reducible or not" (Tait 1877g, § 28). This again indicates that Tait probably did not explicitly hold the twisting (or flyping) conjecture.

Tait's long paper marks the end of his first involvement with knots. In the months to follow, his interests turned back to other physical topics (cf. the chronicle of his contributions to knots in the Appendix).[130] The outcome of his work for his original aim, to extend Thomson's theory of vortex atoms in the direction of producing a table of chemical elements, was ambiguous. While in the introduction to the paper, Tait again emphasized the physical motivation, his results did not offer satisfactory findings from this point of view. Before a judgement about the value of knot classification for this kind of atomic physics could be made, the project had to be extended to higher crossing numbers, and this seemed beyond Tait's capabilities. The labour involved, he felt, "rises at a fearful rate." Apparently, Tait's only hope was the possibility of transferring this kind of labour to a suitable machinery: "In fact it is probable that the solution of these and similar problems would be much easier to effect by means of special (not very complex) machinery than by direct analysis. This view of the case deserves careful attention." (Tait 1877g, § 12.) This Babbagian utopia, however, was not pursued seriously, and the project of knot classification reached a virtual standstill for several years.[131]

In late 1883, on the occasion of an address to the recently founded Edinburgh Mathematical Society on *Listing's Topologie* which will be discussed in more detail below (see § 46), Tait turned back to the possibilities of renewed human effort to advance his project of knot classification:

> We find that it becomes a mere question of skilled labour to draw all the possible knots having any assigned number of crossings. The requisite labour increases with extreme rapidity as the number of crossings is increased. [. . .] I have not

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[129] Given in §§ 14, 15, and 28–34 of (Tait 1877g).

[130] Although Tait dropped the subject of knots, his interest in plane graphs continued. When Alfred Bray Kempe announced his supposed proof of the four-colour conjecture in 1879, Tait took the matter up and claimed to have an alternative proof of the conjecture based on a method for colouring the edges of a trivalent graph using three colours only, such that at each vertex all three colours meet (Tait 1880). While the four-colour conjecture is indeed equivalent to the existence of such a colouring, Tait's supposed inductive proofs of the latter were fallacious; see (Biggs, Lloyd and Wilson 1976, 94 ff.). However, Tait did not connect these graph-theoretic ideas with his work on knots. It seems that the idea of colouring a knot diagram, leading to what is perhaps the simplest proof of the fact that the trefoil knot is actually knotted, emerged from Fox's work in the early 1960's as a simple way of coding homomorphisms from the knot group to symmetric groups (Fox 1962, Section 10). Such homomorphisms were first considered by Wirtinger in the context of monodromy investigations of singularities of algebraic surfaces; see (Epple 1995).

[131] Computer algorithms for the tabulation of knots have been devised since the 1960's; see (Trotter 1970) and (Thistlethwaite 1985). Computerized knot tabulations have recently been carried to knots with 16 crossings by Thistlethwaite, Hoste and Weeks (forthcoming).

> been able to find time to carry out this process further than the knots with *seven* crossings. [. . .] It is greatly to be desired that some one, with the requisite leisure, should try to extend this list, if possible up to 11, as the next prime number. The labour, great as it would be, would not bear comparison with that of the calculation of π to 600 places, and it would certainly be much more useful.
>
> Besides, it is probable that modern methods of analysis may enable us (by a single 'happy thought' as it were) to avoid the larger part of the labour. It is in matters like this that we have the true 'raison d'être' of mathematicians. (Tait 1884a, 97.)

As it turned out, though perhaps not by a "single happy thought," help was soon on its way. Tait's advertisement reached a broad public when it was published in the January 1884 issue of the *Philosophical Magazine*, and at least one of its readers decided to respond.

The tabulating tradition

§ 40. The Reverend Thomas Penyngton Kirkman, Rector of Croft, Lancashire, was already well versed in combinatorial and graph-theoretical problems when he became interested in knot classification in 1884, probably motivated by his reading of Tait's address to the Edinburgh Mathematical Society.[132] In the 1850's, Kirkman had studied circuits in polyhedral graphs, a famous example being the circuits along the edges of a dodecahedron which Hamilton had made popular by his "Icosian Game."[133] Accordingly, Kirkman viewed knot and link diagrams as four-valent "polyhedral" graphs. In fact, he was only interested in knot projections and explicitly decided not to consider the question as to which projections, when viewed as alternating or non-alternating knot diagrams, corresponded to equivalent knots. Kirkman even argued over this point with Tait, trying to convince the latter that it was not physically reasonable to consider operations which twisted the threads of a knot or link.[134]

Kirkman's crucial idea, which eventually enabled him to enumerate all knot projections of up to 11 crossings, was to start the enumeration by considering only such four-valent graphs (knot projections) which formed what he called a "solid knot." The term derived from viewing a knot projection as the net of a polyhedron in space; the condition of solidity meant that no closed curve in the plane intersected the given graph in just one or two of its vertices or in two points on different edges. Any reduced projection of a prime knot could be further transformed into a "solid knot" by employing what

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[132] This is at least what Tait claimed (Tait 1885, 346).

[133] See (Biggs, Lloyd and Wilson 1976, 28 ff.) for more information and reprints of sources.

[134] See (Kirkman 1884, §§ 5, 6). Kirkman liked controversy: "Whatever be the decision of the reader, I am highly delighted, while attempting to write on a theme so dry and tiresome, that we have, at the outset, such a pretty little quarrel as it stands wherewith to allure his attention." In particular, Kirkman was far from dealing with something like the twisting conjecture. Only in his next paper, Kirkman conceded that it might be reasonable to consider "the curious transformations and reductions by twisting of Listing and Tait" (Kirkman 1885).

Kirkman called a removal of "flaps," a "flap" being a part of the diagram as illustrated in Fig. 18a. Since the removal of flaps could lead to link projections even if one started from a knot projection, Kirkman had to consider "plurifilar" graphs of $n-2$ four-valent vertices (crossings) for enumerating "unifilars" of n crossings. The basis for his approach was, therefore, the enumeration of "solid plurifilars." Using his own kind of symbolic notation and a partition method similar to Tait's second approach, Kirkman succeeded in producing a list of knot projections of up to nine crossings. He sent his first results to Tait in May 1884.

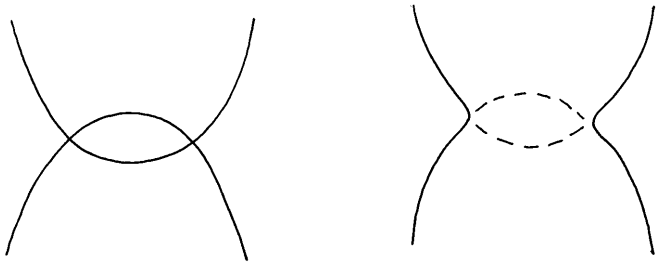


Fig. 18. (**a**) A flap (**b**) Removal of flaps

It remained to determine which knot diagrams obtained from Kirkman's list corresponded to the equivalent knots. Restricting to the alternating case, Tait completed this task within a few weeks after receiving Kirkman's paper. Tait's main tool in looking for knot equivalences was the consideration of twists, as he himself pointed out. However, Tait still did not formulate the twisting conjecture or an equivalent statement. During the process, a few errors in Kirkman's list were corrected and the final tables, both of Kirkman's graphs and of Tait's alternating knots of orders three to nine, were communicated to the R.S.E. (see Fig. 1). But Tait also felt that his table was not completely satisfactory from a mathematical point of view. Both Kirkman's and his own methods had "the disadvantage of being to a greater or less extent tentative. Not that the rules laid down [...] leave any room for mere guessing, but they are too complex to be always completely kept in view. Thus we cannot be absolutely certain that by means of such processes we have obtained all the essentially different forms which the definition we employ comprehends." (Tait 1884c, § 1.) This reservation notwithstanding, Tait was finally satisfied with the number of knots obtained: "Reverting to the main object of my former paper, we now see that the distinctive forms of less than 10-fold knottiness are together more than sufficient (with their perversions, &c.) for the known elements, as on the Vortex Atom Theory." (L.c., § 5.)

Besides the table, Tait's paper gave a new discussion of beknottedness, introducing an idea suggested by links like the Borromean rings (see Fig. 19). In such links, each component was both unknotted and unlinked with every other individual component, but still at least one crossing had to be changed in order to disentangle the system. By analogy, Tait argued that this phenomenon of "locking" between the various parts of a knot was the real difficulty in relating the beknottedness to the twist number of a knot. The idea of distinguishing between linking and locking had already come up in the summer of 1877 in a brief exchange with Maxwell, who showed Tait how to

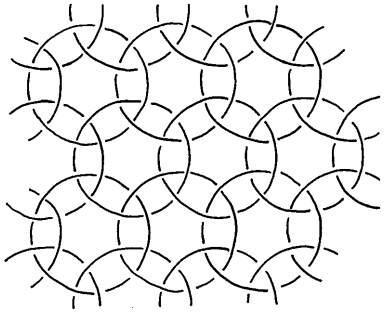


Fig. 19. Maxwell's infinite configuration of "locked" circles

extend the Borromean link to an infinite configuration of mutually unlinked circles.[135] In January 1885, Kirkman sent Tait his next list of "polyhedral data." This time, they included all knot projections of ten crossings. As before, Tait set out to reduce Kirkman's list by looking for equivalent alternating diagrams. Even more than with the previous table, Tait felt that he could not be sure to have succeeded in producing a complete and correct list of alternating knots. Nevertheless, he presented Kirkman's new paper and the results of his own "somewhat protracted" work to the R.S.E. in June and July 1885, including some remarks on amphicheiral knots of up to ten crossings (Tait 1885). Just before the publication of the table of alternating "tenfolds," Tait was surprised to receive a duplication of his own work, a table of alternating ten-crossing knots compiled by Charles N. Little, a mathematician and civil engineer of Nebraska State University who spent some time studying mathematics in Göttingen about this time.[136] As was to be expected, the two tables were not in complete agreement. Checking the differences, Tait detected what turned out to be his only error before his own tables were printed in September 1885.

Somewhat earlier, Tait had also received a list of 1581 knot projections of eleven crossings from the tireless Kirkman. Tait's last published word on knots signalled his permanent retirement from the field: "The number of forms is so great, and the time I can spare for the work so limited, that I cannot promise [to undertake a census of 11-folds] at an early date." (Tait 1885, 347.) He never did, but in April 1889 Tait suggested to Little that he might wish to pursue this work further.

§ 41. Little's attention had originally been drawn to the subject of knot tables by Tait's and Kirkman's earlier papers. The method used in his first paper was a modification of Tait's second classification stragegy, relying on the graphical formulae of knots. After receiving Little's paper, Tait opened a correspondence with him, and Little, in turn, deepened his interest in knot tables. In the course of the next fifteen years, three further substantial contributions by Little were published in the publications of the R.S.E.

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[135] Tait to Maxwell, 30 June 1877; Maxwell to Tait, 13 July 1877.

[136] On Little's career, see *Who was who in America*, 3rd printing, Chicago: The A.N. Marquis Company, 1943, vol. 1: 1897–1942. It is not clear to me whether Little was motivated to write his paper (Little 1885) as a result of his stay in Göttingen.

In July 1889, Little's second paper was read by Tait to the R.S.E. For the first time in the project of knot tabulation, Little took up a new issue: the classification of *non-alternating knots*, i.e. knots which do not possess an alternating diagram. While Tait had recognized the existence of such knots from the outset, he had decided not to include them into his enterprise (cf. Tait 1877g, § 4, § 13). Indeed, his work clearly shows that he was guided by the alternating case throughout his involvement with knot investigations. For various reasons, non-alternating knots were harder to classify than alternating ones. In the former case, one could again start from the list of knot projections. But now for every possible choice of over- and undercrossings it had to be decided whether the resulting knot admitted a diagram with fewer crossings, or other diagrams with the same number of crossings. Moreover, while something like the twisting conjecture was implicit and crucial in the classification of alternating knots, no similar transformation process for non-alternating diagrams had been discussed by the earlier writers. Thus in producing his table of various diagrams of the 11 prime, non-alternating knots of up to nine crossings which formed the core of his paper, Little had to rely on extensive experimentation. It is thus quite understandable why he ultimately expressed reservations as to the correctness of his results: "In deriving the knots from the knot forms [i.e. knot projections] the conditions to be observed are so many that a single worker cannot be *absolutely* certain that all the groups of forms obtained are really distinct knots." (Little 1889, § 7.) One must keep in mind here that Little still lacked any calculable invariants which he could have used to prove rigorously that two given knots were inequivalent. But such reservations did not keep him from continuing his work. As mentioned above, in early 1889 Tait had suggested to Little to use Kirkman's "polyhedral data" of eleven crossing knot projections for a tabulation of alternating knots of order 11. About a year later, Little had accomplished the task. After making a series of corrections to Kirkman's list, he produced a table of 357 prime alternating knots, with a total of 1595 reduced alternating diagrams (Little 1890).[137]

In the years to follow, Little tried to extend his tabulation of non-alternating knots. In the fall of 1893, Little thought he had completed a full list of non-alternating knot diagrams with crossing number ten, whithout yet having considered the question of equivalence. According to his own words, "the matter was then laid aside and taken up anew in the spring of '99."[138] In July 1899, his results were communicated to the R.S.E. by Tait, including a table of 43 supposedly distinct non-alternating prime knots of order ten together with more than five hundred different diagrams of these knots.

On the technical level, Little's work had been grounded in his belief in the general invariance of the twist number of a knot diagram. He even claimed to be able to give an exceedingly simple "proof" of this invariance. This proof is worth quoting since it reveals the method which Little used in his tabulations:

> Theorem. – The total twist of a reduced knot is constant for all forms in which the knot can be projected.

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[137] The table was for the first time checked by J. H. Conway in 1967. He found 1 duplication and 11 ommissions (Conway 1970, 329).

[138] (Little 1900, § 1.) One often reads in modern texts the rather misleading remark that Little needed 6 years to complete his tables, see e.g. (Conway 1970, 329).

> The proof is very simple. The twist of the crossings is not altered by any of the transformations permissible to alternate forms, since these consist of rotations of a portion of the knot through an angle of π about an axis in the plane of the knot projection. [...] In the changes peculiar to non-alternate forms the thread is shifted from one portion of the knot to another, so as to alter the position of two consecutive overs (or unders). (Little 1900, § 8.)

This seems to be the first explicit statement of the twisting conjecture. The additional operations used for non-alternating diagrams – termed "two-passes" by modern authors[139] – may be illustrated by Fig. 20.

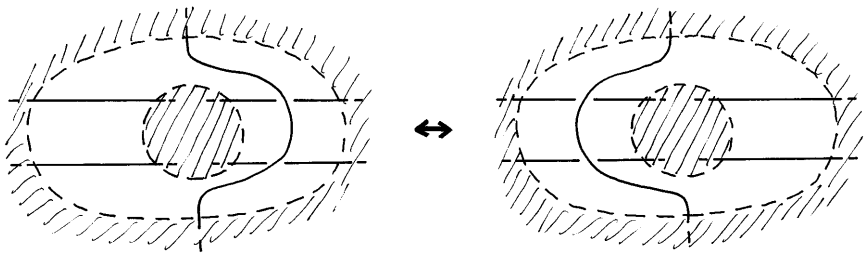


Fig. 20. Little's two-passes

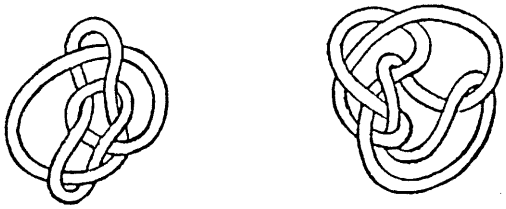
Evidently, a two pass does not alter the twist number, whatever the orientation of the arcs involved may be. We may take it for granted that, given Kirkman's and Tait's earlier lists of knot projections, Little's tabulations were based on a systematic test of possible twists and two-passes, aided by the previous determination of the twist number of diagrams. Unfortunately, Little's claim that twists and two-passes exhaust the possible transformations of reduced knot diagrams was erroneous. In the case of non-alternating ten-crossing knots, there was one pair of equivalent knot diagrams which was not related by a sequence of twists and two-passes; in fact these diagrams even had different twist numbers. The error was first recognized in 1974.[140]

§ 42. Around the turn of the century, the techniques of modern topology became gradually available, and thus the study of knots could finally be based on a rigorous footing, an effort begun by Wilhelm Wirtinger, Heinrich Tietze, and Max Dehn, and pursued

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[139] See e.g. (Thistlethwaite 1985, 21).

[140] See (Perko 1974). Perko found that the following two knot diagrams in Little's list were actually equivalent, reducing the number of non-alternating knots of order 10 to 42:



after the First World War by Kurt Reidemeister and James W. Alexander. Until this time, the 19th-century knot tabulations seem to have been viewed with some reservation by mathematicians, due to their apparent irrelevance for the core subjects of mathematical research as well as the obvious lack of rigor in the techniques used to produce these results.[141] Real appreciation for the work of Tait and his followers emerged only in the 1920's when the knot tables, at least those of lower orders, could be checked using new homological invariants and were found to be correct. In particular, the torsion numbers of cyclic coverings of the knot exterior and Alexander's polynomial invariant were used to verify Tait's tables by (Alexander and Briggs 1927) and (Alexander 1928).

Nevertheless, the tabulating tradition founded by Tait also survived into the twentieth century. In 1917, Mary Gertrude Haseman finished her Ph.D. dissertation at Bryn Mawr College with a paper entitled *On knots: With a census of the amphicheirals with twelve crossings*. She published this work in the natural place: the *Transactions* of the R.S.E. Not much is known about the background of her work, but the short *curriculum vitae* joined to her thesis shows that she had been in touch with two mathematicians who had brought the fruits of their mathematical education from Cambridge, England, to the United States in the late 1880's: Frank Morley at Johns Hopkins, and Charlotte Angas Scott, Haseman's principal thesis advisor at Bryn Mawr.[142] Haseman's thesis was based on certain rules for the construction of amphicheirals which Tait had described thirty years earlier (Tait 1885, §§ 4, 5). She followed these precepts in order to construct alternating amphicheirals of twelve crossings. While she knew of and used the twisting conjecture – among other things, she introduced the name of "tangles" for the four-ended parts of a diagram involved in a twist and gave rules how twists changed the scheme of tangles – she did not systematically check her list for duplications. Also, Haseman did not address the question as to whether there existed amphicheiral knots not obtainable from Tait's construction rules.[143] Haseman's thesis advisors apparently did nothing to help her in learning the new methods of modern topology, and her thesis did not mention any of the recent topological texts. The work closest to her own would have been Dehn's paper on the inequivalence of the two trefoil knots (Dehn 1914): this paper gave the first proof satisfying modern standards that there exist non-amphicheiral knots at all.

§ 43. Summing up, we find that the tradition of knot tabulations was a cooperative enterprise, inspired and guided by Tait's initiative. Kirkman did the hard work of compiling tables of knot projections. Tait then determined the equivalences of alternating forms of up to ten crossings, whereas Little did the same for eleven crossings on

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[141] This is my conclusion from the fact that virtually no mathematician of some standing was inspired by Tait's work to take up the subject of knots; compare the survey in (Dehn and Heegaard 1907, 207-215). The main steps in the passage to the modern theory of knots have been described in my *Branch points of algebraic functions and the beginning of knot theory* (Epple 1995). As the title indicates, the most important motivation for the formation of modern knot theory came from algebraic function theory and not from the tabulating tradition.

[142] On Scott's and Morley's roles for American mathematics, see (Parshall and Rowe 1994, 241 ff. and 432 ff.).

[143] This still seems to be an open question. In the 1970's, Perko showed that Tait's methods suffice for amphicheiral knots of up to 10 crossings; see (Thistlethwaite 1985, 19).

Tait's suggestion. Moreover, Little completed the tabulations of prime knots of up to ten crossings by studying non-alternating forms (based on his "theorem"). By checking each other's work where it overlapped, their tables achieved an impressive level of completeness and correctness. The central role Tait played in coordinating the tabulations is also documented by the fact that, with the only exception of Little's first paper, all contributions were read before the R.S.E and published in its journals.

This leads back to the question of how important Tait's original motivation, vortex chemistry, has been for the tabulating tradition. For Kirkman and Little, the purely combinatorial or intuitive fascination of the topic may well have been the main reason for taking up Tait's lead. But they were at least aware of its potential physical applications. Little documented his awareness of the application of knots to ether vortices for instance in the introductory passages of his last paper (Little 1900, § 3). Kirkman, on the other hand, only mentioned electricity and magnetism in this respect. His first paper on knots ended with the remark: "This may suffice on solid knots until their value in electricity and magnetism is so enhanced as to call for a formal treatise on the whole subject." (Kirkman 1884, Postscript, 1 September 1884.) Kirkman's silence on vortex atoms comes close to an implicit criticism. It might well be explained by his determined anti-materialist beliefs or his sympathy for Boscovich's theory of atoms as force centres.[144] In Tait's case, however, the physical motivation was clearly dominant. In several passages of his writings, he expressed his wish to leave the laborious mathematical project, and he actually did so once he found that the tables had become sufficiently extended to serve as a universe of forms for "the known elements." For Tait, the task of knot tabulation, as a preliminary to broaden the scientific content of Thomson's theory of vortex atoms, now seemed complete. Also, one should not forget that during the late 1880's, Thomson himself became more and more sceptical about his theory. This may well be another reason why Tait abandoned the quest, leaving it to Little to carry on the project of knot tabulation.[145]

A chemist's interest in topology: Crum Brown

§ 44. While Tait was still working on his knot tables, the Edinburgh chemist Alexander Crum Brown, married to a sister of Tait's wife, also turned to topology for a short

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[144] This sympathy was for instance expressed in a letter to Maxwell, dated 8 November 1878, in which Kirkman criticised the notion of mass presented in Thomson's and Tait's *Treatise on Natural Philosophy*. The letter is in Cambridge University Library, Add 7655/II, No. 167.

[145] During his work on knots, Tait tried to keep Thomson informed of what he was doing, although Thomson apparently never took a serious interest in Tait's enterprise. Shortly after sending his paper on *Listing's Topologie* to the *Philosophical Magazine*, Tait wrote to Thomson: "I am going to *smash* Vortex-atoms at R.S.E. (Jan^{y} 7) so I bid you to hearken." (Tait to Thomson, 20 December 1883; Kelvin Papers Cambridge, T 33.) This is not the final criticism of Thomson's theory, however, but a technical idea about knots, involving the cutting up of crossings. In November 1884, Tait had a little exchange with Thomson who did not see that for knot diagrams with 6 or more crossings, non-alternating choices of crossing orientations were possible (Tait to Thomson, 1 and 4 November 1884, Kelvin Papers Cambridge, T 36 and T 37).

period.[146] In several respects, Crum Brown, a theoretically oriented chemist not very fond of work in the laboratory, was in a good position to appreciate Tait's tabulation enterprise.[147] He had witnessed Thomson's vortex atom speculations from their very beginnings. As mentioned earlier, he had assisted Tait both in making smoke-ring experiments and in producing drawings and wire models of knots and links for the meeting of the R.S.E. in early 1867 at which Thomson first presented his new ideas. Being a fellow of the R.S.E. and a regular guest in Tait's house, Crum Brown must also have been aware of the latters involvement with knots and in particular of the fact that Tait began to use an analogue of Brown's own graphical notation in his work.

Among the reasons why Crum Brown had designed this notation were the possibilities it offered not only for explaining isomerism, but also the common structure of various series of organic substances with similar parts like the hydrocarbons or the alcohols.[148] Even before Cayley turned to the systematic application of graph theory to explore chemical structure, Crum Brown himself tried to construct a mathematical calculus expressing the possibilities of combinations of (especially organic) radicals. Roughly speaking, he conceived chemical molecules as "operands" and possible substitutions of radicals as "operators" of his calculus; his graphical formulae thus encoded the ways in which operators might act on the operands (Brown 1867). The paper which presented this calculus was actually read to the R.S.E. at the same meeting which saw Tait's presentation of his remarkable smoke-ring experiments and Thomson's first paper on vortices.

A crucial question for the utilization of graphical notation to explain chemical compounds was, as Brown put it in one of his lectures, whether or not "the form of the formulae in any way resembles the form of the molecules these formulae represent."[149] Brown clearly thought this to be the case, as his extended discussion of the various proposed forms of benzene (including Ladenburg's three-dimensional arrangement) in these same lectures showed. On the other hand it was quite unclear how this structural similarity between graphical formula and molecule should be understood, and several chemists seem to have been highly sceptical about the idea. It was Tait who proposed to solve this puzzle by conceiving the notation as giving a *topologically*, though not a *spatially*, correct picture of chemical bonding. In his 1883 address on *Listing's Topologie*, Tait argued that "Crum Brown's chemical *Graphic Formulae* [. . .], of course, do not

[146] Unfortunately, not many of Brown's papers seem to have been preserved in Edinburgh libraries. There are some correspondence and lecture notes taken by students in Edinburgh University Library, but these do not provide much evidence for the topic of interest here.

[147] In a contemporary biographical sketch, we read: "As an analytical or practical chemist Dr. Crum Brown cannot be said to have made a reputation. He has been heard to wish that ⟨some one would invent a machine for doing those tiresome analyses.⟩" Brown's wider interests are said to include mathematics, philology, Russian, Chinese, and church history. And: "Perhaps he believes more than most of us in a region where paradoxes are not only soluble but solved." See *Quasi Cursores. Portraits of the High Officers and Professors of The University of Edinburgh at its Tercentenary Festival*, 1884, 282–232; Edinburgh University Library, JY 1202.

[148] See (Biggs, Lloyd and Wilson 1976, 60). These features were also responsible for the final success of Brown's notation.

[149] Notes of lectures on *Advanced Chemistry* by Prof. Crum Brown, Summer Session 1884, by James Walker; Edinburgh University Library, Gen. 47 D, p. 8.

pretend to represent the actual positions of the constituents of a compound molecule, but merely their relative connection" in the sense of the new science of topology (Tait 1884, 85). Most probably, this way of looking at the matter was completely acceptable to Brown, and the prominence Tait's address gave to his formulae aroused his interest in the new mathematical field.

We have seen that the two years following Tait's address mark a second period of intensive work on knots. Toward the end of that period, in December 1885 and in January 1886, Brown read two short papers to the R.S.E., entitled *On a case of interlacing surfaces* and *On the simplest form of half-twist surface*. The second paper contained a discussion of Möbius band-like surfaces, while the first described a configuration of three infinite surfaces pierced with circular holes, the boundaries of which were linked locally like the Borromean rings. In fact Brown showed – without mentioning Maxwell's name – that the three sets of unlinked circles making up the infinite "Borromean" pattern which Maxwell had communicated to Tait in 1877 (see Fig. 19 above) bound three infinite surfaces which may be embedded in space without mutual intersections. It seems very likely that Tait had told Brown of Maxwell's pattern. From Brown's papers it remains unclear whether he had any chemical reasons for looking at Möbius bands or interlacing surfaces. Admittedly, Brown never took up knots or links in relation to issues of chemical structure. Nevertheless, his *interest* in topology would appear to derive from an awareness of potential connections between chemistry and the new discipline. After Tait had reinterpreted the earlier work on isomerism and graphs in terms of Listing's new science, it was only natural that Crum Brown wanted to familiarize himself with some of its elements.

Aware of a new discipline: The British reception of Listing's work

§ 45. On several occasions in this study, we have had reason to mention the British reception of Johann Benedikt Listing's papers on topology. While Listing's name was not unknown in Britain before the developments described – in particular, his contributions to physiological optics had found some recognition – from the 1870's onward, Listing's *Vorstudien zur Topologie* and his *Census räumlicher Complexe* became standard references whenever British natural philosophers dealt with topological problems. The interest which Maxwell, Tait and others showed in these writings is perhaps the most telling indication of the growing awareness of the emergence of this new mathematical field in British scientific circles. For this reason, Maxwell's and Tait's reception of Listing's essays must be summarized with a view to their perception of the disciplinary role of topology.

We have seen that it was Maxwell who, in early 1869, first called attention to Listing's *Census*, as both Thomson and Tait plainly acknowledged in their publications. Beyond the question set for the Cambridge Mathematical Tripos in January 1869, Maxwell's first public reference to Listing was made in a short talk to the London Mathematical Society which summarized the contents of Listing's paper.[150] In this talk, Maxwell emphasized

[150] See § 26 above and (Maxwell 1995, 466–471).

the use of topological considerations for integration in space regions. He noted that if such a region could be continuously contracted to a system of p points joined by l lines, then it necessarily had $l - p + 1$ "cycles," and "any other path must be compounded of these." Moreover, Maxwell explained that path integrals joining two fixed points of the region along varying curves would give infinitely many values, the differences of which were sums of integer multiples of the integrals along "cycles." As an example, Maxwell mentioned Gauss's linking integral in which one of the two curves involved was considered as fixed while the other represented a variable path.

The first reference Maxwell made to Listing's *Vorstudien* of 1847 appears to be in the context of the preparation of his *Treatise* in an attempt to establish a convention for the orientation of space. After corresponding with Tait about this issue, Maxwell put the problem before the London Mathematical Society in May 1871, pointing out that different writers used different conventions. The quaternionists and Listing, in his *Vorstudien*, were among those who chose the orientation "x to South, y to West, and z upwards" as the positive one, while Thomson and Tait in their *Treatise on Natural Philosophy* had adopted the opposite convention.[151] The issue of orientation had indeed been of central importance in the *Vorstudien*, and Listing had called the relation between the two orientation systems of space that of *perversion*, a term both Maxwell and later Tait adopted. Maxwell finally established a consensus within the London Mathematical Society for adopting the convention of Thomson and Tait, making reference to astronomy, screw-making, and botany.[152]

When Maxwell finally published his *Treatise on Electricity and Magnetism* in 1873, Listing's terminology was given a prominent place in the introductory paragraphs of the text.[153] Again, the main interest was to provide a language suited for describing the topological issues which had to be dealt with in integration theory. According to Listing's conventions, the number of internal boundary components of a space region with one external boundary surface was called its "periphractic number;" what we would today call its first Betti number was termed the "cyclomatic number." The technique of contraction of regions to one-dimensional diagrams that Maxwell had taken from Listing's *Census* and described in his talk to the London Mathematical Society was repeated and used to introduce the crucial lemma which stated that the cyclomatic number of a space region equals that of its complement. Then, the relation between the cyclomatic number of a space region and the cyclomatic numbers of its various bounding surfaces, which had been so intensively discussed between Maxwell and Thomson, was restated in the new terminology and followed by remarks on line- and surface-integrals. This time, Maxwell also pointed out that such considerations were related to a disciplinary development: "We are here led to considerations belonging to the Geometry of Position, a subject which, though its importance was pointed out by Leibnitz and illustrated by Gauss, has been little studied. The most complete treatment of this subject has been given by J. B. Listing." (Maxwell 1873, § 18.)

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[151] See Maxwell to Tait, 8 and 11 May 1871, Tait to Maxwell, 9 and 13 May 1871. For the question to the London Mathematical Society, see (Maxwell 1995, 641–643).

[152] L.c., see also the corresponding footnote to § 23 of Maxwell's *Treatise*.

[153] See especially (Maxwell 1873, §§ 16–26).

In the body of the *Treatise*, Maxwell included a detailed discussion of Thomson's extension of Green's theorem for multiply connected regions (§ 96), and an even longer discussion of both an electromagnetic and a geometrical interpretation of Gauss's linking integral (§§ 409–423). The crucial point, i.e. the proof that the value of this double integral was an integral multiple of 4π, was taken from an insight due to Ampère: the magnetic field induced by a closed current could be replaced by the field generated by a "magnetic shell," i.e. a "uniformly magnetized" surface bounded by the current's path. The work done on a magnetic particle when moved along a closed curve through the field was then proportional to the number of intersections of this curve with the "magnetic shell."[154] Maxwell also presented his earlier example, illustrating that the linking integral might vanish even if the two curves involved were themselves linked. Today, this example is usually called the "Whitehead link."[155] Once more, Maxwell referred to the disciplinary development involved, joining the growing group of scientists calling for a development of topology: "It was the discovery by Gauss of this very integral [...] that led him to lament the small progress made in the Geometry of Position since the time of Leibnitz, Euler and Vandermonde. We have now, however, some progress to report, chiefly due to Riemann, Helmholtz, and Listing." (Maxwell 1873, § 421.) The appearance of the names of Helmholtz and Listing in this update of a famous earlier remark by Gauss aptly summarizes, in a nutshell, the weave of events described in the previous two sections.

§ 46. Tait did even more to promote Listing's achievements in British science. We have seen how he exploited several of the ideas in Listing's *Vorstudien*, like the twisting operation and the type symbol. Tait regarded the latter's contributions so highly that he proposed that Listing be elected an honorary fellow of the R.S.E. in 1879.[156] After Listing's death, Tait wrote a very appreciative obituary for *Nature* (Tait 1883). In its first sentence, Tait pointed out the role Listing had played in the network of scientific communication: "One of the few remaining links that still continued to connect our time with that in which Gauss had made Göttingen one of the chief intellectual centres of the civilised world has just been broken by the death of Listing." (Tait 1883, 81.) Only a short paragraph of the obituary was devoted to Listing, the physiological optician (in this respect, he was compared to Helmholtz); the remainder of the text described Listing, the topologist. Tait sketched the contents of the *Vorstudien* and referred the reader to Maxwell's *Treatise* for obtaining "a fair idea of the nature of [the] contents" of the *Census* (l.c.).

Listing's Topologie also received prominent attention in the address Tait delivered to the Edinburgh Mathematical Society on 9 November 1883, a talk which probably called Kirkman's attention to knots and links and inspired Crum Brown to write topological notes as we have seen earlier. In it, Tait presented Listing's ideas to a public ranging from university professors to college teachers and mathematical amateurs. The point of

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[154] See (Maxwell 1873, § 421). These passages, together with Schering's decision to place Gauss's fragment among his writings on electromagnetism, were responsible for the general opinion that Gauss was led to the linking integral by electromagnetic considerations. See note 48 above.

[155] See the figure above, § 16.

[156] The proposal was made at the meeting of the R.S.E. on 3 February 1879, and Listing was elected on 3 March. See the minutes of the R.S.E., National Library of Scotland, Acc. 10000, no. 7.

the address was precisely to acknowledge the existence of a new science. Tait began his talk by apologizing for the barely understandable title he had chosen. In order to illustrate his theme he mentioned some basic problems like, in the first place, the knot problem, or relations "among the numbers of corners, edges, faces, and volumes of a complex solid figure," or, tellingly, "Crum Brown's chemical *Graphic Formulae*." (Tait 1884a, 85.) Then he added:

> For this branch of science, there is at present no definitely recognized title except that suggested by Listing, which I have therefore been obliged to adopt. [...] The subject is one of very great importance, and has been recognized as such by many of the greatest investigators, including Gauss and others; but each, before and after Listing's time, has made his separate contributions to it without any attempt at establishing a connected account of it as an independent branch of science. It is time that a distinctive and unobjectionable name were found for it; and once that is secured, there will soon be a crop of *Treatises*. (Tait 1884a, 85 f.)

It is important to note that Tait did not speak as a *mathematician* here, i.e. as one who sought to promote a new branch of mathematics. It was rather the *physicist* Tait who claimed recognition for topology, a field of mathematics which might be of physical relevance just like Tait's other favourite mathematical subject, quaternions.

> In what follows I shall not confine my illustrations to those given by Listing [...]; but I shall also introduce such as have more prominently forced themselves on my own mind in connexion mainly with pure physical subjects. It is nearly a quarter of a century since I ceased to be a Professor of Mathematics;[157] and the branches of that great science which I have since cultivated are especially those which have immediate bearing on Physics. But the subject before us is so extensive that, even with this restriction, there would be ample material, in my own regarding, for a whole series of elementary lectures. (Tait 1884, 86.)

Tait then went on to explain that Listing's articles were still far too unknown in British circles and that they would merit "an English dress" more than many other scientific papers which had been translated into English. Mentioning his own initial lack of familiarity with Listing's work, Tait explained: "I was altogether ignorant of the existence of the *Vorstudien* till it was pointed out to me by Clerk-Maxwell, after I had sent him one of my earlier papers on *Knots*; and I had to seek, in the Cambridge University Library, what was perhaps the only then accessible copy." (L.c.) Tait proceeded to give a detailed survey over the contents of the *Vorstudien*, adding further illustrations and problems taken from graphs like Hamilton's Icosian game and the problem of map colouring to which Tait claimed to know a solution.[158] When he came to knots, Tait described his own involvement with these studies, again emphasizing the physical background:

> As I have already said, the subject of knots affords one of the most typical applications of our science. I had been working at it for some time, in consequence

[157] Tait speaks of 1860, the year he came to the chair of Natural Philosophy in Edinburgh. Before that he had been at Queen's College, Belfast.

[158] See note 130 above.

> of Thomson's admirable idea of Vortex-atoms, before Clerk-Maxwell referred me to Listing's Essay [...]. Listing's remarks on this fascinating branch of the subject are, unfortunately, very brief, and it is here especially, I hope, that we shall learn much from his posthumous papers [...]. My first object was to *classify* the simpler forms of knots, so as to find to what degree of complexity of knotting we should have to go to obtain a special form of knotted vortex for each of the known elements.[159]

At the end of this talk, Tait referred to Listing's *Census* and the generalization of Euler's theorem about polyhedra it contained. This paper, too, Tait remarked, would merit an English translation and a better reception.[160]

Although Tait's prediction that there would soon be a "crop of *Treatises*" on the new science proved premature, his address clearly documents both his insight into the disciplinary development of topology as well as his conscious effort to promote it. It is also evident that the "science of situation" advocated by Tait was a physicist's topology: like Thomson and Maxwell, he was only interested in understanding those topological problems that were tied to configurations of matter and motion in actual space.[161]

The topology of matter: Concluding remarks

§ 47. The thesis which the two previous sections of this study were intended to advance is twofold. On the one hand, I have tried to show, by describing the weave of events connecting Helmholtz's seminal paper on vortex motion in 1858 with the reception of Listing's work in the 1880's, that British natural philosophers became conscious of and seriously interested in the emergence of what they perceived as a new scientific discipline, topology, well before this discipline reached what Kuhn would have called its paradigmatic phase. On the other hand, I have tried to make clear that this interest was anything but passive, as in particular Tait's knot tabulations dramatically demonstrate. The physicists felt a *need* for topology, and in order to satisfy this need, they started to produce mathematical knowledge which pure mathematicians had not yet to offer.

Through Thomson's topological theory of atoms, Maxwell's dynamical treatment of electricity and magnetism (mathematically related to the former by Helmholtz's hydrodynamic-electromagnetical analogy), knot tabulations and work on graphs representing chemical structure, the new field was increasingly recognized to be of importance for natural philosophy, at least in scientific circles under the hegemonial influence

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[159] (Tait 1884, 95 ff.). Little, who probably returned to Germany a second time in the late 1890's, reported that Klein and Stäckel had enabled him to see Listing's (and Gauss's) *Nachlaß*. The only interesting document he found was a sketch of the transformations of the four-crossing knot into its "perverse;" see (Little 1900, § 4, note).

[160] See (Tait 1884, 98). Among the few who had read the *Census* was also Cayley, who "contributed an elementary statement of its contents to the *Messenger of Mathematics* for 1873." (L.c.)

[161] This, however, does not necessarily imply that he was only interested in understanding the topology of *Euclidean* space. See Section IV for the position of the British natural philosophers on this issue.

of Thomson, Maxwell, and Tait. Two reasons can be discerned which account for this recognition. The first is that the general policy of striving for a dynamical understanding of matter and physical phenomena made it unavoidable to deal with those mathematical aspects of the dynamics of continuous media which came to be classified as topological for systematic reasons. These included, in particular, the relations between vector fields, path integrals, and the topology of spatial domains. The second reason is that the option for atomism in chemistry and physics called for a study of combinatorical aspects underlying the possible groupings of atoms in molecules, leading to graph-theoretical work on hydrocarbons, isomerism etc. Both factors combined in Thomson's vortex atom theory which brought with it a need for new topological results. Here, it was the topological combinatorics of linked and knotted vortices which required attention and which was soon found to present challenging problems, the strictly mathematical solutions of which went far beyond the capacities of the tabulators. Whatever one may think of the physical views Thomson, Tait, and Maxwell attempted to elaborate, and independent of the inadequacies of vortex atomism, the role consciously assigned to topology in this episode of scientific practice must be considered as a significant causal factor in the pre-disciplinary period of this new mathematical field.

Although the Scottish natural philosophers felt compelled to make original contributions to topology themselves, their results were couched in the semi-intuitive, often physical styles of argument available to them. This was a consequence of the fact that the new "science" could hardly be said to exist as such in the usual sense of the word. There were still no "topologists," or treatises on topology comparable to Maxwell's *Treatise on Electricity and Magnetism* or Thomson and Tait's *Treatise on Natural Philosophy*, and even the mathematicians still had trouble in getting beyond intuitive arguments in this early period of topology. Given this background, it is also understandable that the results obtained by the natural philosophers fell into categories corresponding to the main physical ideas that guided their work. Relating to the connections between topology and vector fields, the culminating point was Thomson's "proof" that the number of linearly independent irrotational flows in a space region with given boundary conditions equals the first Betti number of the domain (its "cyclomatic number," as Maxwell put it). The other outstanding achievement of this tradition was the knot tables of Tait, Kirkman, Little, and Haseman. Certainly, these tables were received as, and remain today, a somewhat obscure and marginal scientific contribution, an inevitable consequence of the circumstance that they were based on rather tentative methods and that the intended application to vortex chemistry never came to fruition. Nevertheless, the labour invested in the production of these tables was not entirely in vain. If nothing else, they demonstrated that the problem of classifying knots and links was by no means a simple matter, and in this way they furnished a legitimation for topologists in early twentieth century to take up the problem again by employing the new methods offered by the modern discipline of topology.

The fact that the argumentative style in the topological work of Maxwell, Thomson and Tait was strongly based on intuitive and physical arguments deserves special emphasis. Some of the arguments described above (such as those intended to show that knot complements are doubly connected or those relating to twists of knot diagrams) provide beautiful historical examples of what Georg Polya called plausible or physical reasoning in mathematics. Moreover, they show that the lack of rigorous proof, well known to all

involved, did not deter the physicists from venturing into the field, nor did it prevent them from achieving sound results. Indeed, the style of their findings reveals close affinities with the topological arguments of "pure mathematicians" during the same period. Riemann, Betti, and Klein all had to rely on intuitive and highly informal techniques when trying to establish or explicate a topological insight.

In all areas of topology to which the Scottish natural philosophers made contributions, we find a keen interest in *numerical invariants*. The "cyclomatic" and "periphractic" numbers connected with spatial domains and graphs, the linking, twisting, and crossing numbers and the elusive beknottedness, all provide instances documenting this viewpoint. In this respect, the physicists were again not far from their mathematical colleagues, as becomes clear for instance by reference to the discussions on dimension, genus of surfaces and algebraic curves, or Betti's generalizations of Riemann's connectivity number.[162]

§ 48. Both aspects of the argument I have tried to make may be condensed into the assertion that the physical interests motivating Maxwell's, Thomson's and Tait's engagement with topological matters led to a *heteronomous* development of mathematical knowledge. The distinction between heteronomous and autonomous mathematical work was proposed by Erhard Scholz in order to overcome the limitations of the standard categories of pure and applied mathematics, which often convey rather problematic connotations and pictures of 19th-century scientific activity.[163] Certainly, these conventional categories are inadequate for describing the use and extension of topological ideas in the context of the Scottish tradition in natural philosophy. From a modern perspective, topological concepts and results belong to a very pure domain of mathematics. Yet it is clear from the foregoing narrative that this perception was shared neither by Maxwell nor Tait. While Tait never tired of emphasizing that physical interests had drawn him to the subject, Maxwell viewed topology as linked to natural philosophy for conceptual reasons. For him, and certainly also for Thomson and Tait, topology was the science investigating the properties of *physical* continuity in actual space.[164] On the other hand, their mathematical contributions cannot be said to have been applications of pre-existing pure mathematics. For this reason, new categories of historical analysis are needed that enable us to describe the kinds of *legitimation* justifying mathematical work such as that considered here. This is what Scholz's distinction tries to capture. Accordingly, an *autonomous* development in mathematics is one in which the social *and* cognitive legitimacy of a particular episode of research can be drawn from a socially and cognitively pre-established system of disciplined mathematical practice, for example, the gradual extension of number theory in the 19th century from Gauss to Hilbert. The legitimacy of this research was provided from *within* mathematics as an evolving discipline or system of disciplines. A *heteronomous* development, by contrast, is one in which the legiti-

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[162] This interest in numerical invariants is characteristic for much of the 19th-century work on topological questions. Only when Poincaré introduced homology and the fundamental group, the situation was effectively changed.

[163] The distinction is presented in (Scholz 1989, chapter III).

[164] In this sense, I am tempted to say the Scottish physicists adhered to an *Aristotelian* view of topology.

macy of an episode of mathematical research derives from ends that originate outside of the established disciplinary structure of mathematics. One of Scholz's examples is the crystallographical literature of the early 19th century. His study shows that within this field a significant body of mathematical knowledge had accumulated, knowledge which after the advent of group theory could immediately be translated into pure mathematics. Scholz's other example, graphical statics, was a subject connected with the events described in this study through Maxwell's contributions; we have seen that, among other things, Maxwell brought his topological ideas to bear on that particular topic. Both heteronomous and autonomous developments can produce knowledge which we might classify as pure or applied.

The mathematical contributions of Maxwell, Thomson and Tait to topological problems clearly belong to the category of heteronomous mathematical work. Without the motivation in dynamical theories of matter and electromagnetism, this work certainly would not have been undertaken. Moreover, these efforts were located outside the established system of disciplinary mathematics at the time in Britain. This is clear not only from the Scottish natural philosophers' perception of the subject matter of topology, but also from their position within the British scientific community. While all three had been educated in the Cambridge system – both Thomson and Maxwell had been Second Wranglers, and Tait had even emerged as Senior Wrangler from the Mathematical Tripos, and all of them received a Smith's Prize – their later careers increasingly estranged them from an outlook on pure and mixed mathematics that a Cambridge mathematician like Cayley represented. Oriented toward a unified "mental representation" of physical phenomena and also practical issues of technology, they developed something between an ironical distance and a marked disdain for mathematicians such as Cayley and Sylvester who pursued subjects like higher analytic geometry or the algebraic theory of invariants just for their own sake.[165] Nevertheless, when these natural philosophers encountered, in the pursuit of their proper scientific aims, problems like those decribed here, they were prepared to do serious mathematical work in the ways open to them and without the blessings of pure mathematicians. Somebody like Cayley, on the other hand, became interested in the kind of mathematical work described here only when problems were touched upon which were familiar to him for other reasons. In fact, Cayley's work on the enumeration of hydrocarbons in the 1870's is a good example of *applied* mathematics in the literal sense: he had studied trees some fifteen years earlier in the context of differential calculus.

The causal role of heteronomous mathematics both for the development of science in general and the development of mathematics in particular should not be underestimated. On the one hand, the autonomy of the modern system of mathematical (sub-) disciplines has always been dependent on successful interactions with other domains

[165] For Thomson, this has been described convincingly by (Smith and Wise 1989, 168–192). Tait's views on pure mathematics can be seen for instance from his 1871 address to the British Association for the Advancement of Science (Tait 1871) and his exchange with Cayley on the use of quaternions (Tait 1889); Maxwell's position – involving the idea of mental pictures or representations – may e.g. be gathered from his introductory remarks on Lagrange's dynamical formalism in §§ 553, 554 of the *Treatise.*

of scientific practice. Heteronomous mathematics, as well as successful applications in the strict sense, provided a way to achieve such interactive stability, if mathematicians could show that some of their contributions, motivated by aims external to established mathematics but pursued within mathematics, helped scientists or other practicioners in their respective practices. On the other hand, heteronomous mathematics often provided mathematicians with new and substantial problems and these affected the inner architecture of the discipline of mathematics itself. In particular, this happened whenever a new domain of problems was *mathematized*, i.e. represented in a way admitting mathematical treatment. In this sense, heteronomous mathematical developments could in some cases be historical preconditions of later autonomous work; the transition then resulted from an integration of the newly mathematized problems or problem fields into the stock of mathematics and from the elimination of the original motivating contexts.

The topological work in the context of physics described here gives an instance of the second aspect rather than the first. Yet, there were no mathematicians who could have profited from taking up the topological problems connected with fluid motion or the classification of knots and links, solving the physicist's problems with methods they knew beforehand. Nevertheless, the growing need for topological knowledge that Thomson, Maxwell and Tait felt and sought to satisfy by their own, sometimes insufficient contributions forms one piece in the mosaic which will have to be put together in order to understand the emergence of topology after the turn to the 20th century.

Appendix:

From the Minutes of the Royal Society of Edinburgh

This appendix documents the role of the Royal Society of Edinburgh as a forum for activities related to vortex atoms and knots. The following chronicle lists all papers on vortex atoms and on knots and links, as well as some papers on related topics, which were presented at meetings of the society between 1867 and 1918. If the minutes indicate that a paper was read by a Fellow of the R.S.E. different from the author, this is mentioned. Not all of the papers were abstracted or printed in the *Proceedings* or *Transactions* of the R.S.E. Other areas of physics that received particular interest at the meetings of the R.S.E. during the period considered include: techniques of spectrum analysis (mainly 60's and 70's); thermoelectricity (mainly 70's and early 80's); the telephone (late 70's and 80's).

From the Minutes of the R.S.E.	
18/2-1867	Thomson: *On vortex atoms*
	Brown: *On an application of mathematics to chemistry*
29/4-67	Thomson: *On vortex motion*
5/4-69	Brown: *On chemical structure*
31/5-69	Rankine: *On the thermal energy of molecular vortices*
21/3-70	Tait: *On the steady motion of incompressible perfect fluids in two dimensions*
	Tait: *On the most general motions of incompressible perfect fluids*
16/5-70	Tait: *On Green's and other allied theorems*

From the Minutes of the R.S.E., *cont.*	
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5/2-77	Tait: *On knots*
19/2-77	Tait: *On links*
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