Stellingen bij het proefschrift van A.J.Q. Alkemade

1. Het leggen van verbanden tussen passages uit Lucretius' *De Rerum Natura* en de turbulentietheorie zoals gedaan door Serres is onzinnig.

   M. Serres, *La naissance de la physique dans le texte de Lucrèce; fleuves et turbulence.*
   Paris, 1977.

2. Een gebrek aan historische kennis bij beoefenaren van een bepaalde tak van wetenschap uit zich in de naamgeving van vergelijkingen en theorema's.

3. Bij het schrijven van een numeriek programma moet men niet alleen letten op de convergentie van het rekenschema zelf, maar ook op de convergentie van het schrijven van dit programma.

4. De pogingen van Lumley en Kline om een relatie te leggen tussen Kuhns opvattingen over wetenschappelijke revoluties en de huidige crisis in het turbulentie-onderzoek zijn zwak en houden ten onrechte geen rekening met de kritiek zoals deze bijvoorbeeld door Laudan op Kuhns ideeën zijn geuit.

   J.L. Lumley (ed.), *Whither Turbulence? Turbulence at the Crossroads.*
   Berlin, etc., 1990.

   L. Laudan, *Progress and its Problems; Towards a Theory of Scientific Growth.*
   Berkeley, etc., 1977.

5. Er is geen goede rechtvaardiging te vinden voor het gebruik van een Gaussische verdeling van de vorticiteit in de kern van een wervelring bij modellering daarvan.

6. Het schrijven van een historisch overzicht van de ontwikkeling van het onderwerp waarop men promoveert is geen verspilling van tijd en moeite.

7. Ondanks een eerbiedwaarige geschiedenis is het verschijnsel 'wervel' (*vortex*) nog altijd onvoldoende eenduidig gedefinieerd in de wetenschappelijke literatuur. Deze situatie hindert de vooruitgang van het onderzoek op het gebied van werveldynamica en haar raakvlakken met turbulente stromingen.

8. De invoering van het begrip *inviscid dissipation* van Aksman *et al.* draagt niet bij tot een verklaring voor het niet behouden zijn van de zgn. interactie-energie bij numerieke simulaties met vortonen.

   M.J. Aksman, E.A. Novikov, S.A. Orszag, "Vorton method in three-dimensional hydrodynamics".
   Phys. Rev. Lett. 55 (1985) 2510.

9. Het bestaan van onderzoekscholen op een vakgebied is een noodzakelijke noch voldoende voorwaarde voor 'toponderzoek'.

10. Het is niet goed dat men in discussies bij voorbaat of zonder goede reden van zijn overtuiging afwijkt of deze niet naar voren brengt. Het is echter ook niet goed, zoals Burgers lijkt te stellen, dat men in zijn meningen geen enkele toegeeflijkheid betuigt jegens andersdenkenden.

    J.M. Burgers, *Het Atoommodel van Rutherford-Bohr.*
    Proefschrift. Haarlem, 1918.

On Vortex Atoms and Vortons

Proefschrift

ter verkrijging van de graad van doctor
aan de Technische Universiteit Delft,
op gezag van de Rector Magnificus,
prof.ir. K.F. Wakker,
in het openbaar te verdedigen
ten overstaan van een commissie
aangewezen door het College van Dekanen
op dinsdag 5 april 1994 om 16.00 uur
door

Alfons Johannes Quirinus Alkemade,

werktuigkundig ingenieur,
geboren te Gouda.

Dit proefschrift is goedgekeurd door de promotoren:

- prof.dr.ir. E.W.C. van Groesen
- prof.dr.ir. F.T.M. Nieuwstadt
- prof.dr. H.A.M. Snelders

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Aan haar zonder wier ...

Toute l'âme résumée
Quand lente nous l'expirons
Dans plusieurs ronds de fumée
Abolis en autres ronds

Atteste quelque cigare
Brûlant savamment pour peu
Que la cendre se sépare
De son clair baiser de feu

Ainsi le choeur des romances
A ta lèvre vole-t-il
Exclus-en si tu commences
Le réel parce que vil

Le sens trop précis rature
Ta vague littérature

Stéphane Mallarmé

Het promotie-onderzoek waarvan dit proefschrift de weerslag vormt, werd mogelijk gemaakt door financiële ondersteuning vanuit het Stimuleringsgebied Stroming en Warmte van de Stichting voor Fundamenteel Onderzoek der Materie. De auteur is FOM hiervoor zeer erkentelijk.

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Contents

Prologue

The history of the development, whether normal or abnormal, of ideas is of all subjects that in which we, as thinking men, take the deepest interest.
J. Clerk Maxwell

This thesis deals with two topics which are related to the concept of vorticity. Therefore, it consists of two parts. The "vortex-atom-part" shows the development of a theory of matter, introduced by the English scientist Lord Kelvin in 1867, which would attract the attention of several 19th century scientists up to the beginning of our century. Kelvin's "vortex atom theory" can be put into the context of several developments in 19th century physics, especially those with regard to theories of matter and the still developing theory of rotational flow or vorticity. Therefore, the vortex-atom-part not only tries to sketch the actual development of the vortex atom itself, it also provides a historical background for this development.

The second part, the "vorton-part", is an account of the theoretical foundation and the application to numerical simulations of the vorton method. This is one of the many vortex methods, applied nowadays to the (numerical) study of flow phenomena. Vortex methods are based on the fact that vortices play important roles in fluid flows and can be regarded as important applications of the knowledge on vortex motion which has been gathered in the past centuries and of the surging use of numerical techniques in fluid mechanics. The vorton method will be investigated by means of numerical simulation of several test cases. Most of these were already studied by the scientists who occupied themselves with the elaboration of the vortex atom model or who were just incited to research on vortex motion by this model. However, their investigations were largely hindered by mathematical difficulties. Today, the use of vortex methods as computational tools may provide more insight into the kinematics and dynamics of vortex structures.

In an "interlude" between these two parts, I will try to indicate how several of the issues which will be regarded in the vorton-part have a past going back to the period of the flowering of the vortex atom (and even to earlier times) [1]. This part of the thesis may be of interest to the historically interested reader who wants to become informed of results from research of more than a century ago still have a bearing on modern research. It may also be of interest for the "physical" reader [2], who wants to know about the roots of concepts he is using today. In the Interlude I will also take the opportunity to introduce some of the recent and essential developments in vorticity theory and how several topics related to vorticity and vortex motion play important roles in modern fluid mechanics, including turbulence.

In the Epilogue I will look back on the two parts of this thesis from a broader point of view, i.e. on the development of scientific theories and the use of models. Especially, attention

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[1] Nice illustrations of the continuing traditions in vortex dynamics can be found in Saffman's recent book [205].

[2] I.e. the reader interested in current scientifically obtained results, e.g. those presented in the vorton-part of this thesis.

is given to the possible lessons that one may learn from the development of the vortex atom with regard to the present modelling of turbulent flows.

During the research on both parts, I became convinced that their combination was more interesting than I had initially realized. In the field of fluid mechanics, and probably in many others, the appreciation of history is still largely absent or at most only commencing. I also realized that it would be nonsensical to try to show any direct relationships between the two parts. However, it should be possible to let the "physical" reader realize that modern science has a past and, more importantly, is influenced by this past. Still later, I started to realize that the development of the vortex atom might even teach modern scientists the use of models. Both parts may be read separately but I have taken the opportunity to refer in the vorton-part to equations and theorems which have been introduced in the vortex-atom-part.

The writing of both parts has required a different approach. In an historical account the writer has to be careful in studying and treating the original research, which, consciously or subconsciously, he is constantly comparing to present knowledge. He should try to treat the developments which he describes from the point of the view which scientists had at the time corcerned. Therefore, he has to take account of the *status quo* of science during the time he is writing about, and usually also of the developments which led to this state.

In a "physical" account of a theory, the writer is usually not bothered with the origin of the results he applies. First of all, he may not be aware of the historical development, secondly his striving for clarity and a logical presentation of the different parts of his story does not allow treatment of the sometimes confusing and obscure historical developments of the concepts that he uses.

The style of writing is also be different between the two parts. As a historian, an author is telling a story. He tries to indicate how the subject of his story could arise, how it has developed, and, as in the case of the vortex atom, how it declined. In the treatment of his subject he is also trying to convince the reader of some issue, e.g. of his opinion on the reasons why a decline could happen. This view on the philosophy of science is implicitly valid in the story he writes and the choices he makes. For him, the story is not just an historical account.

As a physicist, an author is trying to show whether a method or theory or, more generally, an approach is suitable or unsuitable for certain applications. Usually, he gives some background and details on the approach itself, but the most important part of his presentation are the conclusions, in which he may refute other approaches and give suggestions for further research. The "physical author" will (and should) try to provide the facts and to draw his conclusions objectively; he does not intent to present a story, but an account.

Prologue to the Vortex-Atom-Part

The development of a scientific concept is the result of both scientific discussion *and* of personal circumstances, friendships, quarrels. A researcher writing on some specific area of the history of science has to investigate all relevant information: not only the material published in journals and books, but also the, mostly unpublished, letters, notebooks, etc. For the writing of the history of the vortex atom, I have only investigated the published material that I could find and consult, and, for reasons of time and financial support, made no attempt to search in the archives; therefore, I will not claim that this story is complete by any means [3]. However, I am

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[3] I could trace only two papers dealing especially with the vortex atom: the paper by Silliman [215] and the M.A. Thesis by Pauly [173].

convinced that the most relevant information is presented here. In the presentation, I have tried to take care that the story doesn't become a summation of facts and opinions. In that case, the reader would soon find himself lost and the "physical" reader would be confirmed in his opinion on the relevance of historical surveys.

The vortex-atom-part tells the story of the rise, elaboration, and fall of a scientific theory (or model; see the Epilogue). This story consists of six chapters of which the first three chapters can be regarded as an exposition of the prehistory of the vortex atom and the road towards its birth. In Chapter 1 the development of fluid mechanics will be shortly described, and more specifically the rise of the concept of vorticity and its theory. This chapter ends with the appearance, in 1858, of Helmholtz's fundamental paper on vorticity theory which, because of its special importance in the development of the theory, is treated separately in Chapter 2. To understand the introduction of the vortex atom by Kelvin, in Chapter 3 I roughly sketch his scientific development. Besides, this chapter contains an account of the direct incentive which led to Kelvin's proposal of the vortex atom, i.e. Tait's experiment with smoke rings. In 1867 Kelvin not only introduced the vortex atom in his paper "On Vortex Atoms", he also started work on "vortex motion" in general, leading to a seminal paper in 1869. Both will be discussed in Chapter 4. In Chapter 5, I will treat the reception and development of the vortex atom theory, both in and outside Britain. Not only fundamental criticism started to rise, elaboration of the model also showed its inability to comply with several essential requirements related to its status as a theory of matter. In Chapter 6 the decline of the vortex atom is treated. Despite the general awareness of its weakness, the theory had drawn the attention of scientists building models of the ether, one of the most haunting riddles in physics at that time. However, the vortex ethers appeared as difficult to elaborate as the vortex atom model and they couldn't solve this riddle. Meanwhile, even Kelvin himself had lost faith in the vortex atom. The rising consciousness of the importance of "electricity" in the atom, accompanied by the discovery of the electron, gave the vortex atom its final deathblow.

Prologue to the Vorton-Part

The study of vorticity and vortical flows has remained a constant topic of active research in fluid mechanics after the fall of the vortex atom. Though a direct correlation may be hard to prove, the last few decades have seen an extra stimulus in research on vortex motion, due to the growing interest in the role of so-called coherent structures in turbulent fluid flows. These structures are generally thought to be of vortical nature and understanding of their behaviour and interaction may be essential to a solution of the turbulence problem.

Though nowadays it seems that turbulent flows can be simulated numerically by completely solving the governing equations, available computational power still restricts the range of turbulent flows (e.g. expressed by means of the so-called Reynolds number). Vortex methods, tools to simulate only the vortical parts of flows, may be a means to circumvent these restrictions and, as a result, may increase our insight into turbulence. If we may represent a coherent structure as an elementary vortex configuration, e.g. as a vortex ring, vortex methods are the ideal means to study its behaviour. In a full "phenomenological modelling" of e.g. turbulent boundary layers one may then restrict the simulation to one or a few of these vortical structures.

Chapter 7 contains a general review of vortex methods. One of the vortex methods which have been introduced during the last few decades is the vorton method, whose merits and capabilities are still relatively unexplored. In Chapter 8 the general characteristics of the vorton method will be presented. The vorton fields and the equations describing the displacement and deformation of vortons will be derived. Application of the vorton method means the representation of continuous vortex structures by the discrete vortons and the numerical solution of the vorton equations. This gives the behaviour of the vortex structures.

To gain insight into the possibilities and limitations of (the application of) the vorton method, I have simulated six basic configurations or test cases, all of them involving the vorton equivalent of the vortex ring, i.e. the vorton ring. In Chapter 9 I defend the choice of these cases and present the diagnostics used in their investigation. In Chapter 10, for each of the six test cases the vorton simulation results are presented in combination with recent results from literature. However, experimental data are scarce, especially quantitative data. This has somewhat limited the possibility to investigate the value of the vorton method presented here. Nevertheless, in the general discussion of the simulation results in Chapter 11, I have been able to formulate several conclusions; besides, some suggestions for future research are put forward.

Special attention is drawn to §10.6. The original aim of my work has been to regard the question: "can the vorton method be applied to the study of coherent structures in the turbulent boundary layer?". This layer is characterized by a shear flow profile and a no-slip boundary condition at the wall. Since indications exist which point out an active role of vortex rings in boundary layers, in §10.6.2 a vortex ring has been taken as basic ingredient to investigate the above question.

In writing the vorton-part, I have assumed that the reader is familiar with the basics of fluid mechanics, i.e. with the generally used equations and symbols. Besides, he is supposed to be familiar with the results of vorticity theory mentioned in Chapter 2 and §4.2 [4] and of the physical concepts introduced in the Interlude.

Regarding the numerical simulations, I want to remark that I only used the computing facilities available at the Laboratory of Aero- and Hydrodynamics (a HP-minicomputer). Obviously, a larger computer would have fastened the computations or would have made more extensive computations possible. At this stage I found no need to resort to this.

Furthermore, I have not tried to optimize the numerical scheme on purpose such that calculations ran as fast as possible; my attention has principally been devoted to getting a correct vortex method, not the fastest one.

Further Remarks

- Throughout this thesis, I have tried to use the same symbols for all physical quantities involved. This meant that I had to adapt several of the symbols used in the older literature. The reader could object that this attitude obstructs insight into the development of notation in this part of physics and may deceive the unsuspecting reader, who will believe that regarding symbols fluid mechanics did not develop or reached agreement immediately after introduction. I have chosen for convenience and clearness and can only encourage the reader to read the original literature himself.

  For convenience a list of symbols is provided at the end of this thesis.

- For the same reason, vector notation is generally used for reasons of convenience. Note, however, that in 19th century literature, e.g. the works of Helmholtz and Kelvin, this was not yet common practice.

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[4] For a more thorough and mathematical introduction to vorticity theory, consult [115], [268], [210], and [205].

- Unless otherwise stated, the theorems and equations used in this thesis are only valid for:
    - Incompressible flows: flows for which the velocity field satisfies $\boldsymbol{\nabla} \cdot \boldsymbol{v} = 0$, i.e. the velocity field is divergencefree. Actually, most results in this thesis are valid for a wider class of flows, i.e. barotropic flows: flows for which the the pressure is only a function of density ρ and/or time t, i.e. $f(p, \rho, t) = 0$ for some function f.
    - Inviscid or perfect flows: flows in which viscosity plays no role.
    - Flows under the action of conservative body forces, i.e. forces which can be represented as a gradient of a force potential (e.g. the gravitational force).
- Regarding terminology, I have to remark that the terms vortex motion, vorticity theory, vortex dynamics, and some others are not used according to any strict rules.

Chapter 1

Vorticity before 1858

The concept of the vortex atom, the subject of the first part of this thesis, could only arise after the mathematical and physical basis of the theory of vorticity and vortex motion [1] had been laid. This had happened mainly during the second half of the 18th and the first half of the 19th century. The theory became definitely established as a serious branch in fluid mechanics when the German scientist Helmholtz published a fundamental paper in 1858. The development of fluid mechanics itself can be traced back to classical Greece, but has only been treated as a serious part of mechanics since Newton. Since then it has constantly occupied and fascinated many of the most highly esteemed scientists.

In §1.1 a concise survey is given of the development of fluid mechanics up to the middle of the 19th century. It mainly serves as a means of setting vorticity theory in an historical order and background and as a source of references on this development. In §1.2 the history of man's fascination with vortices and the eventual introduction of vorticity into fluid mechanics is treated.

1.1 A Short Survey of the Development of Fluid Mechanics

Despite mankind's long-standing interest in fluid mechanics, so far few, if any, serious comprehensive studies on its history have been published. A thorough account of scientific research on fluid motion up to the works of Lagrange was published in an extensive "Prologue" to the volume of Euler's *Opera Omnia* devoted to fluid mechanics [267]. Naturally, one can find accounts of the history of fluid mechanics in works on the history of mechanics in general, such as [50] and [226]. Furthermore, two works on the history on fluid mechanics, though rather global and superficial, have been published: those by Rouse & Ince [203] and by Tokaty [265]. An older "historical sketch" (particularly useful for the development of aerodynamics) can be found in [64]. For a description of the developments in our century, we only have concise surveys like [67].

A table showing essential parts of the development of fluid mechanics up to the Second World War is given by table 1.1. This development is divided in four periods which can roughly be characterized both by fields of research and by the various scientists who have contributed to the development of these fields. In the last column some references are given [2].

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[1] Vortex motion is usually called rotational motion nowadays [268, §29]. However, we will keep to this term in honor of Kelvin's paper treated in §4.1.

[2] For general biographical accounts of the scientists mentioned, we refer to the several volumes of the *Dictionary of Scientific Biography* [65].

hydrostatics		
	Archimedes (187 B.C.-212 B.C.)	
	Stevin (1548-1620)	[226]
	Pascal (1623-1662)	[226]
classical/mechanistic fluid mechanics:	fluid mechanics as part of (rational) mechanics	
- fundamentals	Newton (1642-1727)	[267] [226]
	D. Bernoulli (1700-1782)	
	Euler (1707-1783)	[267] [226]
- ballistics	Euler	[267] [226]
- potential flow theory	D'Alembert (1717-1783)	[267] [226]
	Euler	[267]
mathematical fluid mechanics:	mathematical treatment of physical flows	
- viscous flows	Navier (1785-1836)	[226]
	Poisson (1781-1840)	[226]
	De Saint-Venant (1797-1886)	[226]
	Stokes (1819-1903)	[281]
- vorticity theory	Helmholtz (1821-1894)	[110]
	Kelvin (1824-1907)	[218]
- gas dynamics	Riemann (1826-1866)	[226]
	Hugoniot (1851-1887)	
	De Saint-Venant	[226]
modern fluid mechanics:	towards (engineering) applications	
- turbulence	Reynolds (1842-1912)	[150]
	Prandtl (1875-1953)	[202]
	Taylor (1886-1975)	[20]
	Burgers (1895-1981)	
	Von Kármán (1881-1963)	[100]
- aerodynamics	Lanchester (1868-1946)	[104]
	Von Kármán	
	Zhukovsky (1847-1921)	[223]
	Prandtl	[202]
- rheology	Maxwell (1831-1879)	
	Burgers	
- geophysical fluid mechanics	Richardson (1881-1953)	[17]
	Bjerknes (1862-1957)	[55]
	Rossby (1898-1957)	

Table 1.1: Survey of the development of fluid mechanics.

Some remarks have to be made about this table. First, the sequence of fields and names is not meant to indicate any rate of importance. Second, the classification of periods and their subdivions into areas of research is an oversimplification and only superficial. The names of the periods and the short explicative slogans are our own interpretations. The representatives mentioned here are generally regarded as some of the most famous ones, but several others should certainly be included in any serious extension of this table. For the references only the most relevant and readily available for each representative have been chosen. Third, one should not get the impression that e.g. the development of hydrostatics ended with the works of Pascal or that today the theory of potential flows has become useless or neglected after the rise of the theory of viscous flows.

Although many entries of this timetable will not be relevant to the understanding of the rise of vorticity theory, they are given here to enable the reader to place the development of our topic in a historical context. In the next section, it will be shown how the parts which *are* important (mainly the insuffiency of potential flow theory), have made their contributions, eventually leading to Helmholtz's results.

1.2 History of Vorticity up to 1858

One of the first pronouncements on the role of vortices is by the Greek Democritus (400 B.C.), who based his theory on that of Leucippus. In the 5th century B.C. Leucippus had supposed that the collisions of atoms in random motion would give rise to a vortex. In Diogenes Laertius' account of Democritus's philosophy, we read:

> All things come into being by necessity, the cause of the coming into being of all things being the vortex, which he [Democritus] calls necessity.

The meaning of this statement remains obscure [3] and Democritus is better remembered for his theory of matter, which has furnished him with the name of the Atomist.

For the followers of Democritus, the constitution of matter rested on the existence of an infinite number of indivisible and impenetrable particles. These had weight and hardness but didn't exercise any force on each other. Where there was no "Being" (matter), there was "Not-Being" which could be called vacuum or empty space. Other important characteristics of the Democritean atoms were their invariability (no change of shape) and complete equality in quality. According to the Atomistic viewpoint, the motion of bodies *per se* could be explained in terms of the motion of the atoms. The difference between specific bodies were attributed to the shape, position, configuration and kind of motion of the atoms; hence, mechanical concepts played a fundamental role in this atomic model.

One of the interesting aspects of the Democritean theory concerned the origin of so-called worlds: the atoms have been moving for ever in the infinite, empty space. Through interaction they will form whirling conglomerates, which expand into the worlds, which can be regarded as complexes of atoms. The worlds are born in an infinite number next and after each other, but in course of time they will desintegrate again into their constituents. One of them is our world. However, this process doesn't take place randomly, but it happens by necessity. Maybe, we have to regard Democritus' words given above in this context of the worlds.

---

[3] According to [224], a rich source of original texts on atoms, the whirling motion is actually a kind of shuffling motion. For a survey of the role of vorticity in Greek antiquity, see the only general work on vortex motion, by Lugt [134, Ch 1].

The word *vortex* in its present meaning probably appeared for the first time in the discussion of meteorological phenomena in *De rerum natura*, a didactic poem by the Roman poet Lucretius (50 B.C.). Lucretius can be seen as a follower of the Democritean tradition, since his poem treats the doctrine of the Greek philosopher Epicurus who in his turn was strongly influenced by Democritus' doctrine of nature.

Besides meteorology, Lucretius discussed matter. Everything was built up by an infinite number of atoms and voids. The atoms were as indivisible, eternal and invariable as the Democritean ones. However, whereas the motion of the Democritean atoms was completely indeterministic, the Epicurean atoms had an additional "degree of freedom" to which Lucretius attached the name of *clinamen*[4]: the atoms' motion along straight lines due to free fall could be disturbed causing a small, spontaneous, deviation from these straight motions and a collision and accumulation of the atoms. By this phenomenon, Lucretius explained the birth of the "All", i.e. of all beings.

Though the fascination with vortices must have been constantly present during the ages, up to Descartes we only have some drawings of whirls as observed by Leonardo da Vinci (1452-1519) [5].

For Descartes (1596-1650) physical science, that is to say his theory of matter and motion, rested on the basic assumption that matter equals extensiveness (*extension*) in space. This led him to reject the concept of *actio in distans*, which by that time had already been a serious point of discussion in the explanantion of e.g. the working of gravity. The existence of indivisible parts, the vacuum, and absolute motion also didn't fit in Descartes' picture. Consequently, he rejected the Democritean and Lucretian theory of matter.

We won't go into the details of Descartes's ingeneous doctrines. We just mention his original world system, which he thought able to describe every single motion on earth and in heaven. One of the most intriguing parts of his system was the omnipresence of vortices (*tourbillons*). Without going into any details of this vortex theory [6], we only remark that according to Descartes push, pull, and (vortical) motion of material bodies could explain all phenomena in nature.

Newton's severe criticism of Descartes' doctrines, which had appeared of little heuristic value, initiated a new era in physical science at the end of the 17th century, the beginning of the period in which the use of force and mathematical analysis became dominant. In the 18th century the trend towards mathematization of different aspects of physics was continued. One of its promotors was Euler whose contributions can be found in almost every branch of science.

In fluid mechanics, we owe to Euler the so-called velocity potential which can be traced to the years 1752-1755 [268, §36]. If a velocity field $\boldsymbol{v}$ satisfies the condition

$$\boldsymbol{\nabla} \times \boldsymbol{v} = \boldsymbol{0} \, ,$$

it follows that $\boldsymbol{v}$ can be written as the gradient of some scalar: $\boldsymbol{v} = \boldsymbol{\nabla}\Phi$. A flow for which the above condition is satisfied, is called irrotational flow, a term which will become clear below.

---

[4] This term could be translated by "swerve" or "clash". Although it appears only once in the whole text of the *De Rerum Natura*, it has become closely associated with the Lucretian doctrine, partly, I think, because of its intriguing nature. Serres [210] has taken the occurrence of the *clinamen* in the poem as evidence for Lucretius' role as founder of modern physics, as the first to recognize the difference between "laminar" and "turbulent" motions, and as the first who recognized the importance of vortical flow.

[5] For a review of Leonardo's work on fluid mechanics see e.g. [269].

[6] For a more extensive description of Descartes's theory see e.g. [47, Ch IV] and [4].

The vector $\boldsymbol{\nabla} \times \boldsymbol{v}$ will be replaced by the vector $\boldsymbol{w}$, i.e.

$$\boldsymbol{w} \equiv \boldsymbol{\nabla} \times \boldsymbol{v} . \tag{1.1}$$

Its components can thus be written as:

$$\begin{aligned} w_1 &= \frac{\partial w}{\partial y} - \frac{\partial v}{\partial z} \\ w_2 &= \frac{\partial u}{\partial z} - \frac{\partial w}{\partial x} \\ w_3 &= \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y} \end{aligned}$$

where u, v, w are the components of the velocity vector $\boldsymbol{v}$ and x, y, z are the components of the spatial location vector $\boldsymbol{x}$, i.e. the vector which determines the position in space of a fluid particle in a fixed frame of reference.

Despite the still small number of mathematical tools available at that time, the study of irrotational flows appeared feasible since they were completely described by the velocity-potential Φ. However, it soon became apparent that the theory of these so-called potential flows hardly provided any flows relevant to the real world. Today, we know that most fluid motions in nature and technology are rotational. Even in nearly irrotational flows, the relatively small amount of vorticity present may be of central importance in determining major flow characteristics.

The vector field determined by $\boldsymbol{w}$, which is equal to zero for irrotational flows as we have seen, has become known as the **vorticity** field [7]. Although the mathematical concept of vorticity cannot be found literally in 18th century works on fluid mechanics, these works undisputably contained the first notions of the importance of rotational flows, i.e. flows in which $\boldsymbol{w} \neq 0$ in some parts of the flow. In the writings by D'Alembert and Euler two of the most prolific writers on fluid mechanics during the 18th century, formulations of an important equation in vorticity theory can be found, which has been called the **D'Alembert-Euler vorticity equation** [268, §94]:

$$\frac{D\boldsymbol{w}}{Dt} = (\boldsymbol{w} \cdot \boldsymbol{\nabla})\boldsymbol{v} - \boldsymbol{w}(\boldsymbol{\nabla} \cdot \boldsymbol{v}) . \tag{1.2}$$

The derivative D/Dt is the so-called material derivative:

$$\frac{D}{Dt} \equiv \frac{\partial}{\partial t} + \boldsymbol{v} \cdot \boldsymbol{\nabla} , \tag{1.3}$$

which is the rate of change when a material particle is followed during its displacement.

An important change that took place at the turn of the 18th century was the institutionalisation of scientific research and specialisation of scientists, brought about by the rise of scientific institutions.

For France, the most important institution became the École Polytechnique in Paris, where Laplace (1749-1827) and Poisson (1781-1840) propagated the *physique mathématique* as the way of describing all sorts of physical phenomena. For them, the physical reductions, i.e. the

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[7] For the introduction of the term "vorticity" itself we refer to [268, §29]. For convenience, we will use this term from this instance on, though from a historical viewpoint this is incorrect.

making of models to represent physical phenomena, were equally important as the mathematical deductions, and consequently required the same rigorous methods. For Laplace these were algebraic methods.

One example of the mathematical treatment of a physical phenomenon was the theory of heat which Fourier (1768-1830) brought into the area of rational mechanics, at the same time drawing attention to a distinction between a mathematical and a physical representation. Another example is Fresnel who constructed a physical model of light, thereby abandoning the corpuscular theory of light initiated by Newton and giving a stimulus to a new, mechanical, view of the ether. Like *actio in distans* the ether had been haunting scientists ever since Newton had proposed it as the medium for the action of gravity and light (hence its description as "luminiferous medium").

Cauchy (1789-1857), another *Polytechnicien*, was one of the first, together with Lagrange (1736-1813), to introduce symbols to stand for the vorticity components. However, their early work, which appeared after that of d'Alembert and Euler, was still purely formal and somewhat mystifying.

The kinematical significance of the vorticity vector did not begin to be recognized until around 1840 the Irish scientist MacCullagh and Cauchy himself proved that the components of the curl-operator (see (1.1)) satisfied the vectorial law of transformation [268, Ch III]. By that time Cauchy had also provided a complete and explicit description of the convection of vorticity. One of his results, the **Cauchy Vorticity Formula** [268, §94], is given by:

$$\boldsymbol{w} = (\boldsymbol{w}_0 \cdot \boldsymbol{\nabla}_X)\boldsymbol{x} \tag{1.4}$$

or:

$$w_i = (w_0)_j \frac{\partial x_i}{\partial X_j}$$

where the scalars X_i are the components of the material location vector $\boldsymbol{X}$, i.e. the location of a fluid particle at time $t = 0$ which can be regarded as the labels of the particle. This expression, a general solution to the D'Alembert-Euler vorticity equation (1.2), has the following physical interpretation, as illustrated in fig. 1.1: a cube, initially of sides X_1, X_2, and X_3, is deformed in time; the vector from one corner to the opposite, which represents the local vorticity vector, is thus stretched and rotated. These two important aspects of vortex dynamics will be called **vortex deformation**.

Cauchy also reformulated a result which had already been present in Lagrange's works. This **Lagrange-Cauchy Theorem** [8] says that inviscid flows, i.e. flows in which viscosity plays no role, which are irrotational at a certain moment, have been so ever before, and will remain so for ever.

The physical meaning of the vorticity vector only became clear in a paper of 1847 when George Stokes, longtime Lucasian professor in Cambridge, discovered that at each point of a velocity field the vector $\boldsymbol{\nabla} \times \boldsymbol{v}$ may be regarded as twice the angular velocity of a small element of the continuum [222, §2]. The same paper has appeared to be a treasure of many other important contributions to fluid mechanics. One of his results was a fundamental theorem on the kinematics of continua, nowadays called the **Cauchy-Stokes Decomposition Theorem**: an arbitrary instantaneous state of motion may be resolved at each point into a uniform

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[8] See [268, §104] for the controversies which have surrounded this theorem during the 19th century.

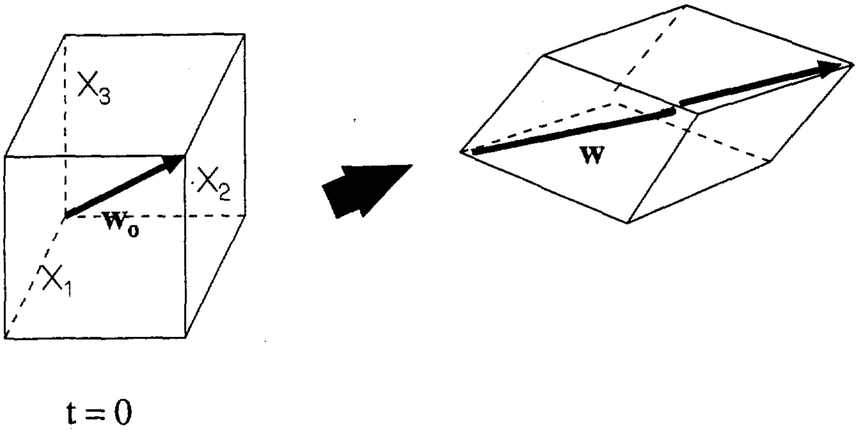

Figure 1.1: Illustration of vortex deformation as expressed by the Cauchy Vorticity Formula (1.4). The arrow indicates time development.

translation, a dilatation along three mutually perpendicular axes, and a rigid rotation of these axes [222, §2] [9].

This result would again be discovered ten years later by the German scientist Hermann von Helmholtz and led to the sound foundation of the theory of vorticity, as will be discussed in the next chapter.

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[9] See also [268, §34]. A more general and abstract version of this theorem has become known as the Hodge Theorem in mathematics.

Chapter 2

Helmholtz's Contribution to Vorticity Theory (1858)

In Germany, towards the end of the 18th century and at the beginning of the 19th century, science had been strongly influenced by the romantic *Naturphilosophie* in which a speculative approach towards natural phenomena was advocated. All natural phenomena, both organic and inorganic, both the microcosmos and the macrocosmos, had to be united into one model. This conviction stimulated the use of analogies and interest in electricity and magnetism, not without success. It also led to a rejection of mechanistic explanations and of the existence of atoms. Newton's mechanistic approach of nature had to be superseded by a dynamical view of the world which was considered as one living whole.

In their search for knowledge, several German scientists were most of all guided by their intuition. The deductive method and the use of experimental data were almost completely absent. This strongly frustrated the German scientist Helmholtz, whose attitude, though certainly influenced by the Naturphilosophical doctrines, was different [1]. He was one of the first to treat all the phenomena which had seemed so different from each other in the 18th century (heat, light, electricity, and magnetism) as different manifestations of a new concept: energy. In 1847, this resulted in his mathematical formulation of the principle of conservation of energy.

Around 1857, Helmholtz, who had been trained as a physician and had become professor of physiology in 1849 in Konigsberg, was working on physiological topics and became involved in related areas like optics and acoustics. His study of the physiology of the ear incited his study of the application of Green's integrals to hydrodynamics, leading to a paper titled "Über Integrale der hydrodynamischen Gleichungen, welche den Wirbelbewegungen entsprechen", written in 1857 and published in 1858 [75].

Though, as discussed in Chapter 1, others before him had become aware of the fundamental importance of vorticity in fluid flows, Helmholtz can be given the honour of being the first to construct a rather complete set of theorems and equations describing the kinematics and dynamics of vortex motion. His achievement, an impressive demonstration of mathematical skill and physical insight, has become a classic and up to this day has been cited frequently and respectfully. Below, some important results due to Helmholtz are presented.

After some introductory remarks and citing Lagrange and Euler as predecesors, Helmholtz came to an analysis of the general movement of a small fluid particle (in old German: *Wassertheilchen*). He noticed that part of this movement is described by the vector $\boldsymbol{\omega}$, given by:

$$\boldsymbol{\omega} = \frac{1}{2}\boldsymbol{\nabla} \times \boldsymbol{v} \tag{2.1}$$

which could be regarded as the rotation of the particle [2]. Comparing with definition (1.1), we

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[1] For a full treatment of Helmholtz's scientific achievements, see [110].

[2] Compare the Cauchy-Stokes Decomposition Theorem mentioned at the end of §1.2.

see that twice this rotation vector is the vorticity vector $\boldsymbol{w}$, as Stokes had already noticed (see §1.2).

Furthermore, Helmholtz proposed some definitions of vortex structures still used today:

vortex line: *A curve which at each point in the fluid is tangent to the local vorticity vector* $\boldsymbol{w}$.

vortex tube (see fig.2.1): *The surface formed by vortex lines passing through some closed contour is called a vortex tube.*

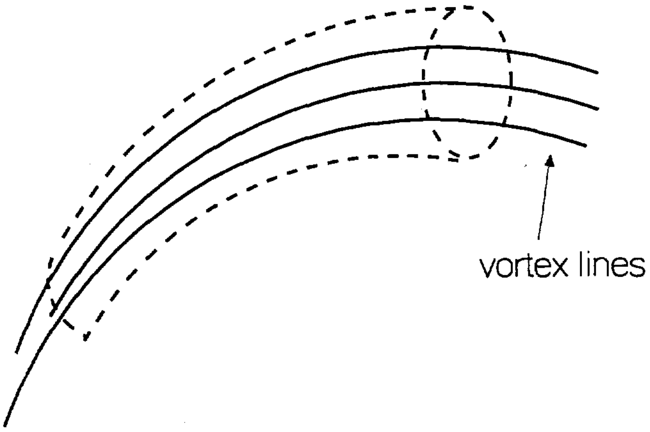

Figure 2.1: A vortex tube.

vortex filament: *A vortex tube (usually with an infinitesimally small cross-section) surrounded by irrotational fluid.*

In the paper one can find three important vorticity theorems which are still called after Helmholtz. We should remark that he only regarded perfect (inviscid) fluids and did not take into account the diffusion of vorticity. However, the first vorticity theorem is general and also valid for real, i.e. viscous, fluids. In order to get a clear understanding of this theorem, we have to introduce the following definition:

strength of a vortex tube: *The strength of a vortex tube at a certain cross-section A is defined as the surface integral*

$$\int_A \boldsymbol{w} \cdot \boldsymbol{n} \,.$$

We can now formulate

Helmholtz's First Theorem: *The strength of a vortex tube at a single time is the same at all cross-sections.*

From the First Theorem, Helmholtz correctly concluded that vortex tubes cannot end inside a fluid but must be closed or end at a boundary [3].

The other two theorems [4] are only valid for inviscid flows.
In order to state the second vorticity theorem we have to introduce the concept of

> **material lines**: *A line in a vector field is material if it constantly consists of the same material particles.*

We can now formulate:

> **Helmholtz's Second Theorem**: *Vortex lines are material lines.*

This means that, for the conditions mentioned above, vorticity can neither be generated nor destroyed. Vorticity is a property attached to the fluid particles and is transported by them.
Finally, we can state

> **Helmholtz's Third Theorem**: *The strength of a vortex tube remains constant as the tube moves with the fluid.*

In §1.2, we saw that in the works of D'Alembert and Euler a vorticity equation can be found as given by (1.2). Helmholtz rediscovered this equation, though for incompressible flows only and his result is still called the **Helmholtz (vorticity) equation** [5]:

$$\frac{D\boldsymbol{w}}{Dt} = (\boldsymbol{w} \cdot \boldsymbol{\nabla})\boldsymbol{v} \ . \tag{2.2}$$

This equation describes the vortex deformation phenomenon, already illustrated in fig.1.1 since the Cauchy vorticity formula is a solution of the Helmholtz equation [6].

Another important discovery by Helmholtz was the analogy between parts of the (older) electromagnetic theory and vorticity theory. Helmholtz's equation from which the velocity field $\boldsymbol{v}$ can be calculated, once the vorticity field $\boldsymbol{w}$ is given, is usually called the **rule of Biot-Savart** after its electro-magnetic counterpart.
The equation reads:

$$\boldsymbol{v}(\boldsymbol{x}) = \boldsymbol{\nabla} \times \frac{1}{4\pi}\int_{V'} \frac{\boldsymbol{w}(\boldsymbol{x}')}{r} = \frac{1}{4\pi}\int_{V'} \frac{\boldsymbol{w}(\boldsymbol{x}') \times \boldsymbol{r}}{r^3} \tag{2.3}$$

where V' is the vorticity-containing volume and $\boldsymbol{r} \equiv \boldsymbol{x} - \boldsymbol{x}'$; see fig.2.2.

In the last section of his paper, Helmholtz treated circular vortex filaments or infinitesimally thin vortex rings; see fig.2.3.
Showing his impressive mathematical skill, Helmholtz derived expressions for the velocity field induced by one vortex ring on another one and extended his result to an arbitrary set

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[3] This conclusion is not true of vortex lines, as has sometimes been claimed. See [268, §10] for a discussion and for the proof that vector lines of any solenoidal field cannot possess any special properties.

[4] Helmholtz's own proof of the Second and Third Theorem are not completely rigorous [268, §46].

[5] Lamb, in his famous text-book *Hydrodynamics* (see §5.1), has pointed out a flaw in Helmholtz's derivation, which may, however, be corrected [115, §146].

[6] Helmholtz does not seem to have been aware of this fact.

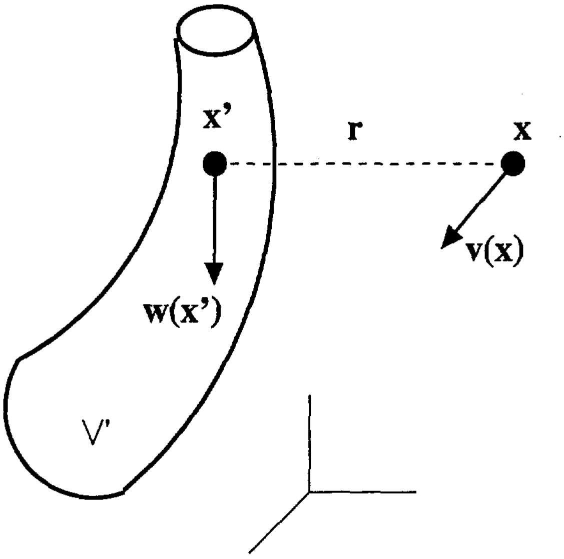

Figure 2.2: Illustration of the rule of Biot-Savart: the velocity at location **x** is determined by the vorticity **w** in vorticity-containing volume V'.

of coaxial [7] rings. From this he derived results for a single ring of infinitesimally small core radius and in infinite space. The ring's radius and velocity appeared to remain constant. At its center the fluid had a constant velocity along the ring's axis in the direction in which the ring moved, i.e. fluid flowed through the ring's aperture.

Helmholtz predicted that two coaxial rings, moving in the same direction behind each other, would show what has since been called the leap-frog effect: the ring at the back will approach the ring at the front; meanwhile the latter's radius is increasing and its velocity consequently is decreasing; at a certain moment the lagging ring will overtake the leading ring and the initial situation will emerge again though with rings exchanged. This procedure, Helmholtz thought, will repeat itself indefinitely.

Another situation discussed was that of two coaxial rings approaching each other (i.e. having opposite direction of vorticity). They would grow in size and approach each other at decreasing speed. Helmholtz remarked that this situation, if completely symmetrical, could also be obtained by letting a single ring approach a fixed wall perpendicularly.

After publication of this paper, Helmholtz again directed his broad mind towards acoustics and optics, in which areas he published several fundamental publications. Though occasionally he still worked on hydrodynamical topics, this research mostly grew out of his other fields of research [232, p.529]. Besides, he may have realized that further mathematical elaboration of e.g. the interaction of vortex rings would be very difficult, as would be discovered by those elaborating the vortex atom theory, introduced by Kelvin ten years after Helmholtz's seminal paper.

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[7] Coaxial vortex rings: parallel rings having a common center axis line.

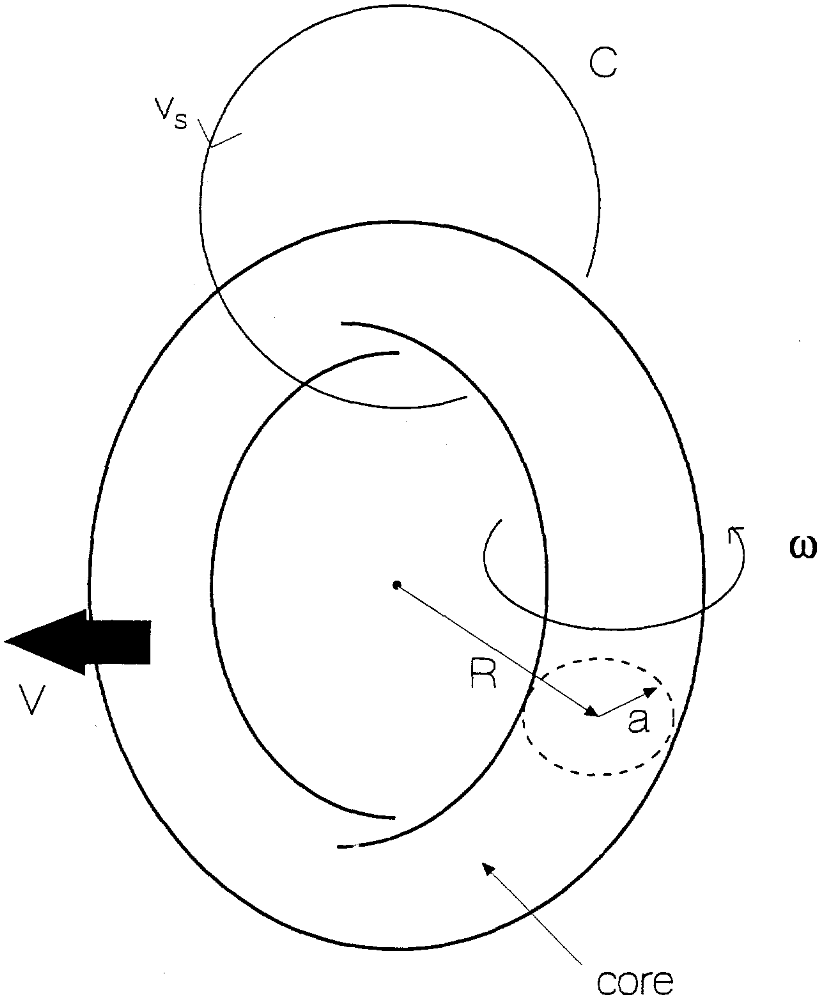

Figure 2.3: A vortex ring: R = ring radius, a = core radius, ω = angular velocity, V = ring velocity. Curve C is related to the definition of circulation (see §4.1).

Chapter 3

Kelvin and the Road towards the Vortex Atom

Physics in the 19th century can be characterized by unification and mathematization. In the 18th century "unification" had only meant the interpretation of phenomena other than mechanical as so-called imponderable (i.e. electric, magnetic, and caloric) fluids. In the 19th century, the unification became much broader: an explanation of nonmechanical phenomena was tried by means of the methods which had been developed in mechanics. We have seen that mechanical concepts had already played an important role in Descartes' approach (see §1.2). In the last century, this approach revived and the formulation of mechanical (or mechanistic) models [1] became a popular activity, especially in Britain. The new "mechanical view of nature" saw matter in motion as the basis and explanation of all physical phenomena. It made possible a mathematical formulation of physics and freed it from ad hoc interpretations such as the imponderable fluids had been.

In Britain important developments started to take place at the beginning of the 19th century. The pursuit of scientific research became a serious profession and universities expanded their science faculties. In Cambridge, not only analytical mathematics was introduced from the Continent, also Cauchy's work on ether became known. The quest for an ether theory (see §1.2) still occupied the minds of many scientists in the 19th century. Stokes, as one of the first English scientists, began to elaborate the mechanical theory of the so-called elastic solid ether. In order to avoid any speculation about the molecular structure of the ether, Stokes stressed its physical structure. The same physical interest had become clear in his important paper of 1845 (mentioned in §1.2) in which not only theory of viscous flows but also the theory of "elastic solids" had been firmly established [222].

Stokes was a representative of a fairly large group of scientists, that became responsible for the revitalization of British science: the Scottish and the Irish. Whereas in the South of England (e.g. Cambridge) industry was despised and physics was mostly left to amateurs, in the North a rather opposite attitude could be found. The Scottish were much more nonconformists who turned to trade and, especially, technology. Other famous men of science from the North, who would become representatives of the new Cambridge school of physics, were William Thomson (1824-1907) (later Lord Kelvin) [2], Peter Guthrie Tait (1837-1901), James Clerk Maxwell (1831-1879), and Joseph John Thomson (1856-1940). All these men have played an important role in the story of the vortex atom.

Helmholtz's 1858 paper, a fine example of "applied mathematics", was received with great sympathy by British physicists. Among Helmholtz's greatest admirers was Kelvin, who had become professor of Natural Philosophy at Glasgow University in 1846 and would keep this

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[1] Discussion on the meaning of the term "model" will be postponed to the Epilogue.

[2] In this thesis only the name Kelvin will be used in order to avoid confusion with another English scientist who will also be playing an important role in the development of the vortex atom, J.J. Thomson. One should realize that this is not really correct, as by the time W. Thomson wrote on the vortex atom, he hadn't been raised to the peerage yet.

position for 53 years. In 1855 both men, who had deep respect for each other, met for the first time, when Kelvin invited Helmholtz to attend a meeting of the British Association [110, I,p.252]. Kelvin's biographer, Thompson [232], holds that Kelvin had read Helmholtz's 1858 paper already in its year of publication. However, according to the recent, very extensive, biography by Smith & Wise [218], it was only in 1862, that Kelvin did become aware of Helmholtz's paper, when Tait drew his attention to it. Anyhow, in 1863 Kelvin told Helmholtz about his views on the power of vortices to explain the rigidity of matter, during a visit Helmholtz brought to Glasgow.

To get a better understanding of the birth of the vortex atom, we have to regard the road leading towards Kelvin's paper "On vortex atoms" of 1867. Apart from the influence of Helmholtz's 1858 paper, several aspects related to Kelvin's scientific development have to be taken into account, which will be treated in §3.1. The most direct incentive towards the vortex atom, Tait's 1867 experiment with vortex rings, is treated separately in §3.2.

3.1 Kelvin's Scientific Development towards the Vortex Atom

Up to 1847 Kelvin and others had succeeded in translating the observed analogies between the theories of heat, electricity, magnetic forces, and hydrodynamics into a single mathematical form, though each area still constituted a separate physical interpretation of the basic mathematical form. There was unity of language rather than of phenomena. The new dynamical approach of heat brought about an alteration of this state: the "mechanical effect" (energy) lost by gross bodies had to re-emerge in mechanical states of the ether and of constitutive elements which, for convenience, were referred to as molecules.

At the end of the 1840s, his work on the foundation of thermodynamics would also incite Kelvin to speculations on the molecular structure of matter. For the convertibility of heat and work he stated the hypothesis that heat is a form of energy, consisting of molecular motions. In this way, he violated one of the fundamental doctrines of his prior work: the opposition against physical hypotheses. The Scottish anti-hypothetical tradition had always limited natural philosophy to the sensible motions of bodies, without regarding molecular motions. All the same, this shift to the molecular level determined his further work.

During the 1840s, Kelvin also sought an explanation for the dynamics of force distributions. He stated important new concepts (e.g. energy), and transformed his kinematics of fields into field dynamics. One important source of inspiration in this regard was the work by Faraday (1791-1867). In his description of the forces between magnetic bodies, Faraday had introduced in 1845 the term "magnetic field", a concept that became of fundamental importance in the further development of 19th century physics. Faraday regarded the field as an intervening ether, but his representations couldn't explain the mechanism by which forces were propagated. According to Faraday, the transmission of forces takes place along the so-called "lines of force".

Kelvin rejected Faraday's view that all phenomena could be explained as "force" and also the view of the German romantic *Naturphilosophen* (mentioned in Chapter 2) that matter was a state of dynamic equilibrium between opposing "forces". In both views all attempts at mechanical reduction, such as Kelvin would like to formulate, had been despised. However, Faraday's results did stimulate Kelvin in attempting to find other, *in casu* mechanical, theories of the ether, which *could* explain the propagation of forces. Furthermore, around 1850 he came to a mathematical theory of magnetism in which Faraday's concept of lines of force was the central issue. Faraday's experiments on the influence of magnetism on polarized light, also stimulated Kelvin's first thoughts on "vortical" structures: he explained magneto-optic rotation as an elastic reaction in the ether to innate spiral structures that are also in orbital rotation.

During the years 1847-1851, Kelvin still maintained his purely macroscopic ideal of describing physical phenomena by a mathematical theory. However, this had become threatened by increasingly pressing demands for a physical conception of molecular reality. The mathematical analogy was powerful, but also showed weakness: related physical theories were set in parallel without relating them physically. To avoid this problem, Maxwell had proposed a less restrictive use of mathematical analogy. Kelvin could not agree with this approach and began to use the molecular theory and started his life-long pursuit for a theory of matter that would unify all physical forces.

Though not directly apparent, Kelvin's views on the ether must have influenced his view on matter, since he was convinced that there was no dichotomy between ether and matter. His opinion on the structure of ether would change fundamentally during these years. Initially, he had regarded ether as air, later he thought the ether to be much finer-grained than air. It was no longer air, it was like air [281, Ch.7]. The year 1851 showed an important transition for Kelvin in his constant search for a consistent theory of ether and matter [218, Ch.12]. He proposed the "aer" to indicate a unity of ether and matter. However, Kelvin's proposal to treat matter and ether as structures of the same kind in an underlying continuous fluid was regarded sceptically.

During this period, a typical example of a mechanical model was proposed by the Scottish physicist Rankine. In Rankine's theory of molecular vortices each atom of matter consisted of a nucleus surrounded by an elastic atmosphere. The quantity of heat was the kinetic energy of the revolutions or oscillations among the particles of the atmospheres, which Rankine supposed to constitute vortices about the nuclei [3].

With his theory, Rankine became one of the first to regard the mathematical consequences of the vortex hypothesis. Besides, he set the view on the ether as consisting of nuclei of atoms, vibrating independently (or nearly so) of their atmospheres. The model also impressed Kelvin, who in his paper introducing the vortex atom in 1867 (see §4.2 below) would remark that Rankine had showed the "possibility of founding a theory of elastic solids and liquids on the dynamics of ... closely-packed vortex atoms". To Kelvin this was "a most suggestive step in physical theory" [243, p.3].

Summarizing his views on the relation between matter and ether in a paper of 1856 [242], Kelvin mentioned three possible conceptions. Besides Rankine's notion of matter permeating the spaces between the ether's nuclei and the mechanical view in which matter and ether consisted of particles, he suggested an alternative model, showing his slow movement into the direction of the theory of vortex atoms. In the late 1850s he came to be convinced that the ether should be regarded as a fluid. The vortical motions in the perfectly elastic ethereal continuum were the cause of the molecular structure and the solidity and impenetrability of bodies. But an important question remained: how could his speculations provide a physical explanation if vortices in a plenum did not seem to possess the property of indestructibility?

Beside the requirement of indestructibility, another requirement showed up. The kinetic theory of gases, developed in the 1850s and 1860s, had encouraged the notion that vibrating molecules, supposed to consist of hard bodies, were the sources of spectral radiation. These motions were transmitted through the ether as vibrations of definite wavelengths. This view

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[3] A fuller treatment of this theory, and its role in the development of thermodynamics, is given in [218, Ch.10].

had led to the requirement of flexibility and elasticity of the atom. Among the British physicists who tried to develop dynamical molecular models to explain results of spectroscopy, Kelvin would become very ambitious. Years after his first work in this field he would mention the fact that it was Stokes who, in 1852, had taught him the requirement that "the ultimate constitution of simple bodies should have one or more fundamental periods of vibration" [243, p.3].

Except elasticity and thermodynamics, electricity and magnetism started to play a role in Kelvin's formulation of a theory of molecular structures. In 1847 he had suggested to consider the propagation of electrical and magnetic forces in terms of the linear and rotational strain of an elastic solid. His starting point was Stokes' already mentioned 1847 paper [222] in which, for the first time in a clear manner, rotation and strain in continuous media had been treated mathematically. Kelvin regarded motions of electrical fluid as vortex motions and considered thermo-electric rotations [218, p.405-].

Stokes would also stimulate Kelvin's interest in hydrodynamics [4]. In 1857, Kelvin wrote Stokes on his attempts to find a theory of rotating "motes" in a perfect fluid. The stress which he had begun to put on rotational motion not only arised from his work in heat and magnetic theory, but he also thought that the repulsion caused by the rotating motes would lead to a stiff, stable structure required for e.g. luminiferous vibrations [218, p.409].

Kelvin's attempts to implement rotational motion led in 1858 to a very speculative thought on "eddies" in an universal fluid, which might explain gravity and inertia in the solar system. He had started work on the hydrodynamics of the motes, parts of the molecules, and their interactions and stability. In his correspondance with Stokes, we find the latter's criticial remarks, but Kelvin did not reply [218, p.411].

It seems that Kelvin would only start again on rotational motion after his acquaintance with Helmholtz's paper in the early 1860s [218, p.412], as will be discussed in Ch. 4.

3.2 Kelvin and Tait

Besides Kelvin's general convergence towards vortex motion as discussed above, a more direct incentive in the development of the vortex atom theory has to be mentioned. Kelvin's inspirator in this respect was Peter Tait, professor of natural philosophy in Edinburgh.

Like Kelvin, Tait admired Helmholtz and had made a personal translation of the 1858 paper directly after its publication [109, p. 127]. In a short epilogue to this translation, which appeared only in 1867 [75], Tait spoke of "one of the most important recent investigations in mathematical physics". We can only guess why this translation was published ten years after the original had been published, taking into account Tait's remark that he had made it "long ago". Tait mentioned Helmholtz's personal revision of the translation, though without an indication when this had taken place.

Both men became close cooperators in 1861 when Kelvin proposed Tait to join in writing a textbook [218, Ch. 11]. In 1866 and 1867 their collaboration was at its peak as they worked on what would become one of the most important and influential 19th century references in physics, the *Treatise on Natural Philosophy*. In the book they attempted to propagate the use of 'dynamics' rather than simply 'mechanics'. The significance of this choice in favour

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[4] In a letter to Stokes of 1857, Kelvin wrote: "Now I think hydrodynamics is to be the root of all physical science, and is at present second to none in beauty of its mathematics." As Smith [217, p.400] correctly remarks, it would be absurd to regard these words as a "key to unlocking the mysteries of Thomson's inner thoughts"; they are a sign of his enthusiasm for this rising branch of physics.

of dynamical explanation was fundamental: instead of an abstract, purely analytical, mathematical treatment of motions, they chose a physical approach based on the assumption of Newton's laws of motion, highlighting the importance of the concept of force. Instead of only kinematical considerations, they put emphasize on the dynamical aspects. This program of dynamical theories implied replacing forces acting at a distance by matter in motion. All physical phenomena were dynamical, also those which appeared to be statical.

Because of the co-operation on the *Treatise on Natural Philosophy*, Kelvin visited Tait regularly. During one of those visits, in January 1867, Kelvin witnessed a simple experiment performed by Tait in his study, which can be regarded as the most direct incentive for the vortex atom model [5].

A description of Tait's experiment can be found in a letter Kelvin wrote to Helmholtz a few days after the experiment:

> Just now, however, *Wirbelbewegungen* have displaced everything else, since a few days ago Tait showed me in Edinburgh a magnificent way of producing them. Take one side (or the lid) off a box ... and cut a large hole in the opposite side [see sketch in fig.3.1]. Stop the open side AB loosely with a piece of cloth, and strike the middle of the cloth with your hand. If you leave anything smoking in the box, you will see a magnificent ring shot out by every blow. ... 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 accuracy of the circular form of the whole ring, and the fineness and roundness of the section, are beautifully seen. ... 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 ... 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. I am, as yet, a good deal puzzled as to what two vortex-rings through one another would do (how each would move, and how its shape would be influenced by the other). By experiment I find that a single vortex-ring is immediately broken up and destroyed in air by enclosing it in a ring made by one's fingers and cutting it through. But a single finger held before it as it approaches very often does not cut it and break it up, but merely causes an

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[5] Experiments on vortex rings had been few up to that time. Helmholtz, in his 1858 paper, had suggested a way to produce ring-like structure by means of a spoon, pulled through the surface of a water tank. However, it seems that he never undertook any serious experiments on vortex rings.

Early experimental work had been done by Rogers [197] in 1858 and by Reusch [187] in 1860.

> indentation as it passes the obstacles, and a few vibrations after it is clear. [232, p.513]

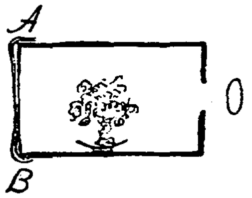

Figure 3.1: Kelvin's sketch of Tait's experiment. From [232].

From Kelvin's paper "On vortex atoms" [243] (to be discussed in §4.2) we learn that both men also experimented with two smoke boxes and studied the behaviour of two vortex rings approaching each other under different angles. Unfortunately, we only have a very concise, quantitative description of their observations [243, p.11-12]. In one of his popular lectures, first published in 1876 [229], Tait would perform the same experiments before an audience, where he also showed vibrations of rings obtained by using an elliptical or square hole in the box.

Clearly, the experiment with vortex rings caused the final convergence of Kelvin's views on vortex motion and only three weeks later, in February 1867, his theory of vortex atoms was exposed in a lecture before the Royal Society of Edinburgh.

Chapter 4

Kelvin and the Birth of the Vortex Atom

In the 17th century, the time of the Renaissance and revaluation of the classical Greek philosophy, the Democritean/Lucretian atom theory (see §1.2) had encountered a renewed interest. Not only physicists, but also chemists, looking for models able to explain chemical phenomena, came to favour a corpuscular-theoretical model [47, Ch III].

By the 1860s the Lucretian theory of matter was still generally accepted as a way to regard matter. At about the same time, the "hard bodies" had been taken up by the promotors of the kinetic theory of gases. One of them was Maxwell and the theory had quickly gained much support. It was a mechanistic-physical theory in which observable macroscopic properties of gases were deduced to the movements of molecules or atoms, hard particles (or bodies) which behaved according to Newton's laws. Its success strengthened the general belief in the reality of atoms [1].

Kelvin, however, did not favour the Lucretian theory of matter, and hence the kinetic gas model. The vortex ring, and the results in vorticity theory discovered by Helmholtz in 1858, not only led to his own theory of matter but also to some new fundamental results on vortex motion.

4.1 Kelvin's Contribution to Vorticity Theory

The year 1867 could be named Kelvin's *annus mirabilis* with respect to his work on vorticity, resulting in one short notice and two extensive papers: "On vortex atoms" [243] and "On vortex motion" [245]. These three publications can be read completely independently, though in the last we can detect how his vortex atom model [2] influenced the kind of research on vortex motion he thought necessary for the development of the theory.

Kelvin's short notice appeared as an appendix to Tait's translation of Helmholtz's 1858 paper [244] (see §3.2), having been sent as a letter to Tait shortly before the translation was published in the *Philosophical Magazine* [75]. It contained only one result, but this expression for the "translatory velocity of a circular vortex ring" has become one of the classical results in vorticity theory and it has since often been referred to and applied. It is given by [3]:

$$V = \frac{\omega a^2}{2R}(\log \frac{8R}{a} - \frac{1}{4}) \tag{4.1}$$

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[1] See e.g. [31] for a general treatment of kinetic gas theories.

[2] The terms "vortex atom model" and "vortex atom theory" will be used without distinction. For a discussion of the vortex atom as a model, we refer to the Epilogue.

[3] This result is only valid for the vortex ring which we will call the Kelvin-ring. See §A.2 of the Interlude for its definition. In absence of further remarks, vortex rings in the discussion of 19th century research will mean rings with core size small compared to their radius and of uniform distribution of vorticity, as Kelvin assumed. Nowadays it is usual to add an order term to the expression for V. Kelvin realized the existence of lower order terms but could neglect them due to his assumptions.

where V is the ring velocity, ω is the the approximate "angular velocity of the molecular rotation" in the core (see relation (2.1)), a the radius of the ring's core, and R the ring's radius; see fig.2.3.

Unfortunately, we get no indication about the way Kelvin derived his expression, but most probably he based it on the analysis of vortex rings which had appeared in Helmholtz's 1858 paper [4].

Though read in 1867, the paper "On vortex motion" would only be published in 1869, after it had been "recast and augmented" in 1868 and 1869. In Kelvin's notebooks of 1867-9 we find several calculations and drawings of vortices in preparation of this paper [218, p.422].

Contrary to the expectations raised by the title, the first part of the paper is devoted to "the hypothesis, that space is continuously occupied by an incompressible frictionless liquid acted on by no force, and the material phenomena of every kind depend solely on motions created in this liquid". Though it contains some references to vortex motion [5], we only mention his treatment of the topological concept of multiply continuous spaces, i.e. spaces of which the bounding surface is such that there are irreconcilable paths between any two points in it. A picture was presented of several knots showing a variety of knotted and knitted "wires"; see fig.4.1. Presumably, he needed this result to defend the existence of a large variety of vortex atoms (see §4.2).

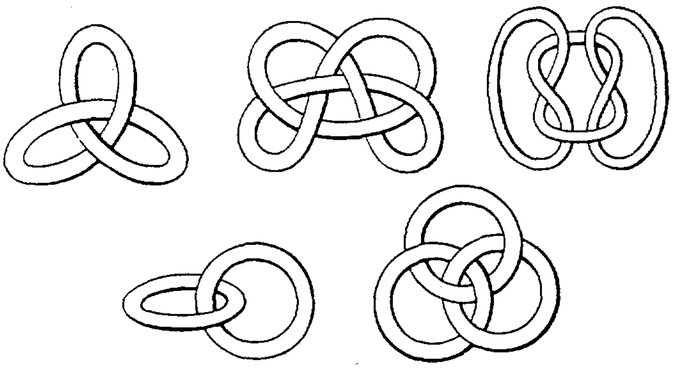

Figure 4.1: Knotted and knitted wires. From [245].

In the "instalment" of 1869, the second part of the paper, we find Kelvin's greatest contribution to vorticity theory, i.e. the introduction of the concept of circulation:

Circulation: *The circulation* Γ *around a closed curve, say* C*, in some velocity field is defined in the following way:*

$$\Gamma \equiv \oint_C v_s \tag{4.2}$$

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[4] Rott & Cantwell [201] remark that Helmholtz's derivation included an erroneous factor of 2, but this did not influence his final result as he used only a low order approximation. Kelvin derived the result for higher accuracy and apparently corrected Helmholtz's mistake.

See [212] for a recent discussion on Kelvin's probable source of inspiration.

[5] Kelvin's definition of a vortex [245, §20] seems to be the first attempt in history, but has become only one of many. A proper definition is still lacking.

where v_s is the component of $\boldsymbol{v}$ tangential to the curve C which is a loop enclosing any part of the ring's torus; see fig.2.3. We can regard Γ as the mean value of the tangential velocity, multiplied by the length of the circuit [6].

It is Kelvin's merit that, due to the introduction of circulation, Helmholtz's Third Theorem and the Lagrange-Cauchy Theorem (see §1.2) could be proved elegantly and more rigorously. Kelvin's reformulation of the Helmholtz's Third Theorem is now called

> **Kelvin's Circulation Theorem:** *An inviscid flow is circulation-preserving, i.e. the circulation in any closed line moving with the fluid remains constant through all time.*

Kelvin's reformulation of the Lagrange-Cauchy Theorem (see §1.2), i.e.:

> **Lagrange-Cauchy Theorem (Kelvin's version):** *A motion is irrotational if and only if the circulation about every circuit equals zero,*

confirmed his belief in determinism in the mechanical philosophy which he and Tait had proposed in their *Treatise on Natural Philosophy* (see §3.2). However, he also recognized that it was only valid for simply-connected regions in which all curves are reducible, i.e. can be shrinked indefinitely. This may explain his fascination for multiply-connected regions and the knotted and knitted wires mentioned above.

4.2 "On vortex atoms" (1867)

Kelvin's lecture "On Vortex Atoms" [7] [243] was delivered before the Royal Society of Edinburgh in February 1867, only three weeks after Tait's smoke ring experiment. Kelvin started with some severely critical remarks on the "Lucretian atom" [8]. To him justification for the "monstrous assumption of infinitely strong and infinitely rigid pieces of matter" [243, p.1] [9] could only be found in the fact that it allowed an explanation of the "unalterable distinguishing qualities of different kinds of matter" [243, p.1]. Furthermore, the Lucretian model didn't "explain any of the properties of matter without attributing them to the atom itself" [243, p.1]. Therefore, "the Lucretius atom has no prima facie advantage over the Helmholtz atom" [243, p.2], i.e. the vortex ring which Helmholtz had introduced in his 1858 paper.

Kelvin realized that a "gas" consisting of his vortex atoms would have to obey several requirements if it wanted to challenge the kinetic theory of gases. Referring to the experiment in Tait's lecture-room, he remarked that the elasticity of the rings was at least as good an explanation for the elasticity of gases as the "clash" of the Lucretian atoms [243, p.2], which to him could be equalled to the atoms of the kinetic gas theory. Contrary to investigators in this

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[6] Having defined circulation, we can rewrite equation (4.1) in its commonly used form (assuming a uniform vorticity distribution in the core, as Kelvin had done):

$$V = \frac{\Gamma}{4\pi R}(\log \frac{8R}{a} - \frac{1}{4}) \tag{4.3}$$

where Γ is the circulation of the ring.

[7] It will be referred to as the Vortex Atom paper in the rest of this thesis.

[8] The role of Lucretius in Victorian Britain is discussed in [270], where Kelvin is cited: "I have been reading Lucretius ... and trying hard on my own account to make something out of the clash [= clinamen] of atoms, but with little success" [270, p.348] (see §1.2).

[9] Wherever possible, for Kelvin's work page numbering of the *Mathematical and Physical Papers* (referred to as *MPP* in the Bibliography) will be used.

field, Kelvin remarked that for his theory of matter he only had to assume the rings' "inertia and incompressible occupation of space" [243, p.2].

He must have realized that if one wanted to investigate the properties of a gas of vortex atoms, their interactions would have to be calculated and that this would be very hard. Nevertheless, with some confidence Kelvin stated that "a full mathematical investigation of the mutual action between two vortex rings of any given magnitudes and velocities passing one another in any two lines, so directed that they never come nearer to one another than a large multiple of the diameter of either, is a perfectly solvable mathematical problem". And adding with complete confidence: "Its solution will become the foundation of the proposed new kinetic theory of gases" [243, p.2].

With regard to the requirement of the explanation of the variety of atoms, Kelvin showed diagrams and wire models "to illustrate knotted or knitted vortex atoms, the endless variety of which is infinitely more than sufficient to explain the varieties [...] of known simple bodies and their mutual affinities" (see §4.1 and fig.4.1). Helmholtz's First Theorem gave him confidence that the atom would be indestructible: "a closed line of vortex core is literally indivisible by any action resulting from vortex motion" [243, p.3].

With regard to the requirement of the capability of vibration and elasticity of the atoms and the related requirement on the explanation of spectral properties, Kelvin remarked that in case of the Lucretian model one had to assume that molecules cannot be single atoms, but are groups of atoms "with void space between them" [243, p.3]. However, "such a molecule could not be strong and durable", whereas vortex atoms had by nature "perfectly definite fundamental modes of vibration, depending solely on that motion the existence of which constitutes it" [243, p.4].

Kelvin admitted that elaboration of this last statement would be another difficult analytical problem, "but certainly far from insuperable in the present state of mathematical science" [243, p.4]. He pointed out that that this result could only apply to a vortex ring for which the core radius was much smaller than the ring radius. However, he tried to defend his model by remarking that the "lowest fundamental modes of the two kinds of transverse vibrations of a ring" were more serious than the deformation of the core [243, p.4].

To show the fruitfulness of his new model, he regarded sodium. Since the sodium atom had appeared to have two fundamental modes of vibration of slightly different periods, Kelvin concluded that the "sodium atom may not consist of a single vortex line, but might very probably consist of two approximately equal vortex rings passing through one another like two links of a chain" and this model would certainly "fulfil the 'spectrum test' for sodium" [243, p.5] [10]. Furthermore, Kelvin proposed to study the influence of temperature on the fundamental modes of the vortex atoms.

Clearly, Kelvin did realize that his new theory of matter had to satisfy several severe requirements: it had to compete with the kinetic theory of gases, it had to explain the variety and indestructibility of the atoms, and it had to show spectral properties. He also realized that to this end an analytical elaboration of the properties of both single rings and of the interaction of several rings was needed. However, to him the elaboration of these issues would only be a question of time and (great) mathematical effort.

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[10] In a footnote, added afterwards, Kelvin remarked that the sodium atom might after all be explained by a single vortex ring.

Chapter 5

The Development of the Vortex Atom

In the introduction of Chapter 3 we mentioned the popularity of mechanical models among British scientists. We especially treated Rankine's molecular vortices, a typical example of these models. The vortex atom model, however, was a different kind of model. It didn't involve "mechanical" concepts like springs (as Kelvin would indeed frequently apply for his models), but was solely based on hydrodynamical concepts.

This "hydrodynamical" model meant a fundamental difference with the existing views on matter. This was clearly expressed by the English scientist Pearson [1] in 1889: "The old view endowed its atoms with certain inherent forces, and having done so, more or less completely ignored the existence of the medium; the new view endows its atoms with no inherent forces, but with motion - it looks to the action of this motion on the medium to explain the action of one atom on another. The old view saw everywhere in the universe force, the new view finds everywhere motion - that is a gross way of putting the difference" [174, p.71].

However, one soon started to realize, Kelvin himself not in the least, that many properties which had been attached to the vortex atoms in the Vortex Atom paper and other properties which might still be unknown, had to be fully elaborated before the value of the new theory of matter could be properly judged. Before discussing (in §5.3) the treatment of some of the problems related to the properties of the vortex atoms, both by Kelvin and others, we first look at the reception of the vortex atom in Britain (§5.1) and on the Continent (§5.2), *in casu* in Germany and France.

5.1 The Reception of the Vortex Atom in Britain

Initially, Kelvin found little interest for his theory of matter. For one part, this can be explained by the circumstance that he had opposed the Lucretian atom and thus challenged the foundations of the successful kinetic theory of gases. Kelvin's theory was based on continuous space-filling entities, a theory which could not find many adherents at that time. Another reason is the fact that his Vortex Atom paper remained the sole publication on this topic for several years after its publication. Only in December 1871, Kelvin spoke again to the Edinburgh Society on the continuation of his Vortex Motion paper. Unfortunately, only an abstract of this lecture has been published [247].

As for Tait, from whom one would be inclined to expect at least some support for Kelvin's theory, his first reaction was only published in 1875, anonymously in a book titled *The Unseen Universe* [221]. It was a typical example of the discussion on "natural theology" still prevalent at that time in Britain. Tait, together with his colleague Stewart, tried to defend the modern physics of their time against accusations of being too materialistic and hostile to religion. Tait's

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[1] In 1885, Pearson would propose an atom model resembling the vortex atom model in a paper from which we cite here. It is exemplary for the research the vortex atom theory would set into motion in the 1870s and 1880s.

chapter on the vortex atom theory was mainly used for treatment of mind-matter dualism and did not contribute to a serious discussion on the vortex atom, since it was neither in favour of the new theory nor against it.

Though Tait considered the vortex atom theory a subject worthy for one of his "Lectures on some recent advances in physical science", first published in 1876 and still in print in 1885, his final words in his lecture on vortex motion sound very reserved:

> With a little further development the theory may perhaps be said to have passed its first trials at all events, and, being admitted as a possibility, left to time and the mathematicians to settle whether, really, it will account for everything already experimentally found. If it does so, and if it, in addition, enables us to predict other phenomena, which, in their turn, shall be found to be experimentally verified, it will have secured all the possible claim on our belief that any physical theory can ever have. [229, p.304-5]

The first real, and strongly needed, support for Kelvin's theory from the physical community was provided by Maxwell. Though around 1867 Maxwell was still an ardent supporter of the kinetic gas theory (see the introduction to Chapter 4), he actually had preceded the vortex atom model in 1861 by introducing an ether model involving so-called molecular vortices (see e.g. [66, pp.146-]). Initially, he had reacted rather critically to the new theory of matter in a letter to Tait of November 1867 [109, p.106]. However, in his address to the British Association for the Advancement of Science [2] in 1870 [145], he appeared to have changed his sceptical attitude, though his treatment of the vortex atom was mainly intended to show how illustrative methods or expositions could help to represent physical phenomena.

He recognized that when a full mathematical elaboration of the model could be achieved, the vortex atom would "stand in a very different scientific position from those theories of molecular action which are formed by investing the molecule with an arbitrary system of central forces invented expressly to account for the observed phenomena" [145, p.223]. These theories he referred to were the then prevalent atom models which had earlier been proposed by Boscovich and Dalton. Boscovich had proposed his point-atom model in 1745. He suggested that matter consisted of points without extensiveness which could be regarded as centers of force. Interaction took place through "fields" of force around the centers. During the 19th century, Boscovich's point-atomism was still in favour, although in modified versions, especially with the French physicists, and with certain philosophically minded German physicists. In Britain opinions were divided. It was strongly criticized for its artificial character by, among others, Kelvin [3]. The theory of matter proposed by the English physicist and chemist John Dalton (1766-1844) had been intended to link the theory of atoms to Lavoisier's theory of the elements. Though most chemists in the 19th century were sceptical about the existence of atoms, they recognized that this concept could be helpful in acquiring new insights in chemical phenomena [4].

Maxwell also realized that this model could not "fail to disturb the commonly received opinion that a molecule, in order to be permanent, must be a very hard body" [145, p.224], i.e. the opinion of those adhering the kinetic gas theory. According to Maxwell's molecular theory

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[2] To be called the British Association furtheron.

[3] A short description of Boscovich's theory can be found in [65].

[4] This situation is well illustrated by a lecture by the English chemist Williamson [280] and the sequent discussion [29] from 1869.

of spectra, spectral lines were due to collisions of these hard bodies in diatomic molecules. However, experiments showed that mercury vapor consisted of monatomic molecules and *did* emit a spectrum. Maxwell realized that the vortex atom was a better model with regard to this issue which is reflected in his most important supportive contribution to the vortex atom theory, the "Atom" article in the 1874 edition of the Encyclopaedia Britannica [147]. There, he explicitly asserted that neither the Lucretian nor the Boscovichian atoms could account for spectra and that the vortex atom alone was capable of internal motions or vibrations.

As the greatest advantage of the vortex atom, Maxwell mentioned the fact that no "hypothetical forces" are introduced to "save appearances" [147, p.471]. He remarked that a theory of matter had to explain mass and gravitation, but apparently thought that the model would comply with this requirement. However, only three years later, Maxwell's expectations had faded away. In a review of a text-book on the kinetic theory of gases in *Nature* [144], he expected that the vortex atoms "would soon convert all their energy of agitation into internal energy, and the specific heat of a substance composed of them would be infinite" [144, p.245].

By 1877 the vortex atom (and vortex motion in general) had drawn general attention and Maxwell's criticism does not seem to have influenced opinions. Several highly esteemed mathematicians and physicists [5] started research in the area. Beginning the same year two Cambridge mathematical journals, the *Quarterly Journal of Pure and Applied Mathematics* and the *The Messenger of Mathematics*, began to be filled regularly with papers by undergraduates and younger fellows on vortex motion and the interactions of variously shaped bodies moving in perfect fluids. However, the papers were mostly fragmentary and contained little explanation on their motivation or their purposes. Besides, the Helmholtz's theory of vorticity still didn't seem to have been grasped by all scientists. An amazing lack of proper understanding of the theory can be found, for example, in a reaction on Kelvin's theory published in 1883 by the British geologist Croll [43](1883). Croll wondered about the force that counterbalances "the centrifugal force of the rotating material of the vortex-atom" and argued that it cannot be the exterior incompressible liquid surrounding the vortex atom, since it offers no resistance to the motion of the vortex atom. There is no cohesive force, Croll thought, so "What then prevents the revolving material from being dissipated by the centrifugal force of rotation?". Apparently, he didn't realize that vorticity was a property of the flow field itself.

The vortex atom even became an issue outside the physical community. In literature, the vortex atom was alluded to in George Eliot's novel *Middlemarch* [35]. S.T. Preston, private scholar and prolific writer in physics, tried to introduce the vortex atom into a lively discussion on the physical basis of the "phenomena of thought" which took place in Nature around 1880. According to him, these "phenomena" were influenced by "changes or permutations of which the molecules of matter were capable". The old "hard" (Democritean) atoms could only move, but the vortex atom had more kinds of permutations; its number of permutations was even infinite, as thought was itself [183].

The theory of vorticity and vortex rings also became a common part of the text-books on hydrodynamics which started to appear in the second half of the 19th century. The one which would become famous for more than half a century, and is even referred to today, was *Hydrodynamics* by Horace Lamb [6]. Originally, it had appeared under a different title already in 1879 [115], originating from the lectures Lamb had started somewhat earlier when still a

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[5] Love published an important review of "English researches in vortex motion" in 1887 [133].

[6] See [91] for a history of this text-book.

fellow at Cambridge. That edition already contained a chapter on vortex motion (treating e.g. the leap-frog of two vortex rings), but the subject of vortex atom was not treated. In the 1895 edition [115], Lamb remarked that "the method of experimental illustration by means of smoke-rings is too well-known to need description here", referring to Tait's lectures [229]. On the vortex atom Lamb remarked that "this lies outside our province, but it has given rise to a great number of interesting investigations ..." [115, §166] but he recognized that the impulse to some of the investigations on vortex motion mentioned in his book were suggested by Kelvin's vortex atom theory.

The changing attitude of the Cambridge community towards hydrodynamics was stimulated once again when in 1873 Maxwell presided over the reformation of the famous Tripos examination at Cambridge and managed to have hydrodynamics included. The following year, Kelvin and Tait acted as examiners and introduced questions on motion of perfect flows and Helmholtz's theory of vortex motion.

Maxwell had only expressed his intuitive opinion on the vortex atom, but what was really needed was a mathematical elaboration, followed by a physical interpretation, of the properties of vortex rings, both of single ones and of several interacting ones. Only in this way the physical community could be convinced of the consistency and usefulness of the model.

Around 1880, Joseph John Thomson had come to Cambridge on scholarship, where he was completely dependent on University support for maintaining himself as a scientific worker. Therefore, he sought a branch of physics which could earn him some esteem and a stable position [239].

In 1881 the topic for the prestigious Adams Prize, to be awarded in 1883, was announced: "A general investigation of the action upon each other of two closed vortices in a perfect incompressible fluid. In particular it is suggested that the case of two linked vortices should be fully discussed, with the view of determining (1) whether any steady motion is possible, and (2) whether any motion can occur in which there are periodical changes in the forms and dimensions of the vortices." Kelvin probably had some influence in the choice of this topic as it was a logical problem arising from his Vortex Atom paper.

Thomson, who had already published two papers on vortex motion, submitted an essay entitled *A treatise on the motion of vortex rings* [234] [7] and won. It was the first comprehensive attempt to get an analytical picture of interacting vortex rings, after the first attempts by Helmholtz who, however, had only treated coaxial rings (see Chapter 2).

Thomson's faith in the vortex atom theory as a theory of matter seems to have been weak. In his Introduction, he had to remark that the vortex atom theory "cannot be said to explain what matter is, since it postulates the existence of a fluid possessing inertia", and his claim that it was "evidently of a very much more fundamental character than any theory hitherto started" [234, §1] sounds as a hollow phrase, expressed out of politeness. To him, the most important aspect of the new theory seems to have been the fact that the vortex atom theory allowed investigation into the mechanisms of intermolecular forces. Thus, it enabled one to form "much the clearest representation of what goes on when one atom influences another" [234, §1]. Not surprisingly, Thomson chose to restrict his activities to an elaboration of a gas consisting of vortex atoms, interacting with each other.

Therefore, he first needed a strong fundament with regard to the behaviour and interaction of vortex rings. In the first part of the *Treatise*, Thomson derived, from the equations which

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[7] To be referred to as the *Treatise* in this thesis.

had been found by Helmholtz, quantities like momentum and kinetic energy for rotational flows. Furthermore, he calculated the velocity and stability of vortex rings. The topic of the second part was the interaction of two rings and one ring in the neighbourhood of a solid, i.c. a flat plate and a sphere. However, to enable mathematical elaboration he had to assume that the rings did not approach closer than several times their radii. The third part was on linked or knotted rings, whose interest had been roused by Kelvin's Vortex Motion paper (see §4.1). As Thomson assumed that the shortest distance was small compared to ring's radius, the tubes could be regarded as straight cylindrical vortex columns, which again facilitated the elaboration enormously.

In the final part, exposing his vortex theory of gases, Thomson had to admit that the theory was much to complicated to be treated in general, but he mentioned he had tried to derive results which could show some of the properties of a gas consisting of vortex atoms, such as chemical affinity. This part, not surprisingly, was most speculative and after publication met with several critical remarks. It induced Thomson to investigate several other aspects of gases, mainly published in the *Philosophical Magazine*, of whose board Kelvin became a member in 1871. However, in a paper shortly published after the *Treatise* [235] on electric discharge in gases, he clearly recognized that his gas model was not suited at all for his purposes. By then, his faith had largely faded away.

Thomson's *Treatise* did not remain unnoticed, which was partly due to the prestige of the Adams Prize. However, though his work was generally prized for its impressive mathematical achievements, criticism arose soon after its publication. In a review which appeared in *Nature* in 1883, Osborne Reynolds, professor of engineering in Manchester [8], found one inconsistency in Thomson's derivations and one, apparently fatal, flaw in the vortex atom theory itself [190].

In the final part of his *Treatise*, Thomson had attempted a derivation of Boyle's law. For that purpose, he had assumed that the velocities at the solid boundaries were small. Reynolds remarked that these boundaries also existed of vortex atoms and that no reason existed to make this supposition. The flaw was concerned with Thomson's proposal of a test experiment on so-called thermal effusion. According to the current theories of gas at that time, the pressures on the two sides of a diaphragm which were of unequal temperature had to be proportional to the square root of absolute temperature. From his vortex gas theory, Thomson had derived that the pressure will be proportional to the temperature raised to a power greater than one. This experiment would be crucial for the theory, but he realized that it was hard to perform. Reynolds remarked that he had already performed such an experiment around 1879 and that Thomson had probably meant the phenomenon of "thermal transpiration", since effusion, he remarked, was only a theoretical idea.

Reynolds proposed a more suitable experiment, which he supposed to be crucial. According to his view, the velocity of sound must be limited by the mean velocity of the vortex atom. As Thomson had shown that this mean velocity diminished with temperature, and as experimentally it had been found that the velocity of sound increased as the square root of temperature, Reynolds concluded "that the verdict must be against the vortex atom theory. However the vortex atoms are very slippery things; and we should like to hear Mr. Thomson's opinion before adopting one of our own" [190, p.195]. However, Thomson never seems to have reacted to Reynolds's criticism, but, regarding the reputation of *Nature* and of Reynolds himself, it must have been a severe blow to the status of the *Treatise*.

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[8] Reynolds's fascination with vortex motion is evident from a lecture given in 1877 [189], in which he treated some experiments he had performed with smoke rings around 1876 [188].

Despite this seemingly fatal attack, Reynolds, and presumably many with him, didn't dare to really challenge a theory introduced by someone of such high esteem as Kelvin. Or they didn't think it necessary. Instead of criticizing the vortex atom, however, there was the possibility of attempting to adapt the original vortex atom in order to let it meet certain requirements. The most important of these attempts was made by Hicks, one of the scientists whom the 1881 Adams Prize had stimulated to take up research in vortex motion.

Hicks had found two properties of matter which were hard to explain by the vortex atom theory: gravity and the different densities of the elements. The first, he thought, could be solved by considering the theory of pulsating spheres in a fluid, a phenomenon which had been initiated by the Norwegian physicist and founder of meteorology Bjerknes and which Hicks had already considered in a paper of 1879 [78]. He had found that gravitation could be explained if the circulations of the vortex atom exceeded a certain amount; this "cyclic irrotational motion, connected with the vortex, may be so large as to produce [a vacuum]" and hence he suggested to consider hollow vortices, i.e. vortex tubes whose vorticity was concentrated on their surfaces.

Regarding the different densities of the elements, Hicks had to invoke the ether. In an abstract [80] to the first of a series of three extensive papers on vortex motion ([81], [82], and [87]), he remarked:

> When the exceedingly small density of the ether compared with what we call ordinary matter is considered, it is clear that the supposition that matter is composed of vortices of the same density as the ether is surrounded with great difficulties, and we are driven to the conclusion that, if a vortex ring theory be the true one, the cores of the vortices must be formed of a denser material than the surrounding ether, and that probably this core has rotational motion. [80, p.305]

For Hicks it was evident that for an explanation of the different masses, the original vortex atom theory could not remain as elegant as it had been proposed by Kelvin. In the first two of three papers mentioned above, written in 1884-85, Hicks fully treated the hollow vortex rings and studied their steady motion and vibrations. Under his assumptions, he claimed, problems with Kelvin's vortex atom could be solved to any order of approximation.

Hicks's introduction of the hollow ring must have impressed the physical community for its ingenuity, but whether it could save the vortex atom theory remained unclear. However, Hicks himself soon left any attempts to extend his hollow-vortex theory, though in 1885 he received the Hopkins Prize, given for the best original discovery by an alumnus of Cambridge in mathematical physics in the previous three years. His faith to the vortex atom remained. In his address to the British Association of 1895 [85], he tried to explain static electricity by means of vortex atoms and in a lecture read in 1898 on "a kind of gyrostatic aggregate", which "has brought to light an entirely new system of spiral vortices" [86, p.332], he still wondered whether his new theory could throw any light on a vortex atom theory. However, at the end on this lecture he remarked to have found no point in pursuing his new results as he realized that it was "wild speculation" and that attention would be low.

We have to conclude that though outwardly the vortex atom seemed to be received with sympathy, a closer look reveals that severe and fundamental criticism appeared. However, even worse was the lack of appropriate attempts to defend the new theory of matter by elaborating its characteristics. Besides, it is difficult to judge whether the esteem for the vortex atom was due to the (supposed) scientific value of the theory, or to the status Kelvin had acquired, or just to the attractive experiments with rings.

5.2 The Reception of the Vortex Atom outside Britain

For reasons mentioned before, Kelvin's vortex atom was most heartily received in Great Britain itself, at least in the first instance. Reception elsewhere, which seems to have started slowly, largely differed among countries and among scientists. The treatment in this section of some of these reactions is especially meant to show distinctive nationalistic tendencies in physics.

Generally, on the Continent reception was hostile or, to say the least, indifferent. In both Germany and France philosophical objections hindered scientists, especially those who could be regarded as most able to do so, to contribute to the vortex atom theory. However, since no really convincing *physical* arguments were raised against the theory, Kelvin and his followers must not have been really disturbed by the reactions of their foreign colleagues (supposed they knew them).

Our survey of reactions surely is incomplete and will be restricted to Germany and France [9]. As for Russia, for example, we have evidence that the theory was known in some circles, but inaccessibility of Russian literature hinders a sketch of its reception there. Reception in the United States seems to have been passive only.

5.2.1 Germany

As remarked in Chapter 2, in the beginning of the 19th century German science had been largely influenced by the romantic *Naturphilosophie*, characterized by a search for underlying unitary principles and conservation laws and by an absence of the, typically British, empirical approach. However, by the time of Kelvin's introduction of the vortex atom, the romantic *Naturphilosophie* had largely lost its influence and several German physicists had developed a, sometimes extreme, desire for empirical evidence.

One of them was Helmholtz. After his 1858 paper, Helmholtz had returned to his research in acoustics and optics. In 1870, he wrote Kelvin that he was still working on vorticity theory, though only occasionally [232, p.529] and apparently not on the the vortex ring. Soon after his 1858 paper, he became more and more convinced that agreement between theory and experiment could only be acquired if viscosity was taken into account [111, p.23] and he realized that this would be fatal to the vortex atom theory.

Published comments by Helmholtz on Kelvin's vortex atom are scarce. In a funeral oration of 1871, he remarked:

> Ueber die Atome in der theoretischen Physik sagt Sir W. Thomson sehr bezeichnend, dass ihre Annahme keine Eigenschaft der Körper erklären kann, die man nicht vorher den Atomen selbst beigelegt hat [a remark from the Vortex Atom paper; see §4.2]. Ich will mich, indem ich diesem Ausspruch beipflichte, hiermit keineswegs gegen die Existenz der Atome erklären, sondern nur gegen das Streben aus rein hypothetischen Annahmen über Atombau der Naturkörper die Grundlagen der theoretischen Physik herzuleiten. ... Man hat begriffen dass auch die mathematische Physik eine reine Erfahrungswissenschaft ist; dass sie keine andere Principien zu befolgen hat, als die experimentelle Physik. [76, vol.III,p.13]

The only direct reference to the vortex atoms can be found in Helmholtz's preface to Hertz's famous book *Prinzipien der Mechanik* of 1894 in which we find a more fundamental reason for Helmholtz's passive attitude towards, Kelvin's theory:

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[9] For a view on the reception of the vortex atom in The Netherlands, we refer to papers by the physicists Lorentz [132], W.H. Julius [97], and V.A. Julius [96], and especially to the thesis by Quint [184].

> Englische Physiker, wie Lord Kelvin in seiner Theorie der Wirbelatome ... haben sich offenbar durch ähnliche Erklärungen [i.e. deriving all known physical laws from certain fundamental principles, e.g. from Newton's laws] besser befriedigt gefühlt, als durch die blosse allgemeinste Darstellung der Thatsachen und ihrer Gesetze, wie sie durch die Systeme der Differentialgleichungen der Physik gegeben wird. Ich muss gestehen, dass ich selbst bisher an dieser letzteren Art der Darstellung festgehalten, und mich dadurch am besten gesichert fühlte; doch möchte ich gegen den Weg, den so hervorragende Physiker, ... eingeschlagen haben, keine prinzipiellen Einwendungen erheben. [77, p.xxi]

Hertz's approach, as Helmholtz remarked, had been similar to Kelvin's. Not surprisingly, then, Hertz regarded Kelvin's theory as a firm support for his own hypothetical approach:

> Ich erinnere ... an die Wirbeltheorie der Atome von Lord Kelvin, welche uns ein Bild des materiellen Weltganzen vorfuhrt, wie es mit den Prinzipien unserer Mechanik in vollem Einklange ist. [77, vol.III,p.44]

Somewhat surprisingly, the vortex atom got a relatively important place in Oskar Meyer's well-known and influential text-book on the "rivalling" kinetic gas theory [155], whose first edition was published in 1877. For Meyer the vortex atom was "die glücklichste Hypothese" which could satisfy the requirements of a theory of matter. However, Meyer's arguments for his enthousiasm seem doubtful and highly speculative. Regarding the chemical aspects, he admitted that he couldn't mention many results which had been derived from the vortex atom theory. Apparently, however, the vortex atom was more than a *Hypothese* to him as he supposed that the ringlike form of the vortex atom could represent the "abgeplatte oder auch langgestreckte Form" of most molecules. Even in the 1899 edition of his book, by which time the vortex atom had become almost completely obsolete, a section on the vortex atom was inserted in which Meyer suggested that electricity might be included in the model.

With the decline of the Naturphilosophie, new schools of thought on the development of science arose in Germany. Lasswitz, a Gymnasium teacher of mathematics and physics and prolific essayist on epistemology, was a follower of the so-called neo-Kantian school whose principles are evident in his discussion of the vortex atom theory in a paper of 1879 [120]. Though initially Lasswitz praised the vortex atom theory and thought that eventually one could even explain "die Gesetze der Wärme und die Thatsachen der Chemie aus der Energie und der Form der Wirbelatome", he also thought it was still missing an essential element.

For him, a theory could only be of "wissenschaftliche Bedeutung" if it was "nicht bloss in irgend einem Theile der Physik von praktischem Vortheil", but also satisfied "das Erkenntnissbedürfniss des Geistes". "Der Bau der Wissenschaften muss ein einheitlicher sein." To him, a theory had to be extended "bis eine einfache Anschaulichkeit gewonnen ist; sie muss uns nachweisen, wie durch das Zusammenwirken unserer Sinnen und unseres Denkens fundamentale Begriffe unseres physikalischen Erfahrung erzeugt werden, bei welchen der ganzen Natur unserer Organisation nach eine weitere Frage nach Erklärung nicht mehr auftreten kann" [120, p.279]. This required an investigation of the concept of matter in a manner which Lasswitz called "erkenntniss-theoretisch".

Lasswitz concluded that the vortex atom was not an acceptable model. Kelvin had been able to propose his theory because "er und die Vertheidiger seiner Theorie immer noch die Atome als real-transcendente Dinge ansehen, nicht als Erzeugnisse unserer Erkenntnissthätigkeit bei unserer Orientierung in der Welt." Kelvin's vortex atom moved problems from the macrostructure of nature to its microstructure since it was still a moving and changing object; therefore, Lasswitz argued, the hard (Lucretian) atoms were preferable.

Though his arguments must have appeared vague and irrelevant to most physicists, especially the British, the rising popularity of the neo-Kantian attitude may be an important explanation for the lack of enthousiasm, and interest, for Kelvin's theory in Germany.

5.2.2 France

The strong bond between experimental and mathematical research in French science (see §1.2) had led to a flourishing scientific community and a spreading influence in Europe at the beginning of the 19th century. However, around 1830 this influence had started to decline and Britain and Germany were taking its place. Part of this shift can be attributed to the rapid adaption of education to the changed scientific attitude in these latter countries.

Recognition of the importance of vorticity had began early in France thanks to the results found by Cauchy (see §1.2), who in 1827 had introduced the concept of "rotations moyennes" (see e.g. [125]). However, the admiration of this preceding work, together with an even more chauvinistic admiration of the Cartesian heritage, hindered the French in a full appraisal of foreign vortex theories [10]. This is exemplified by Wurtz's book on atomism of 1873 [286], in which the author remarked that the vortex atom idea was not new, but essentially Descartes's theory: "l'esprit humain semble tourner dans un cercle". However, Wurtz admitted that Kelvin had used more rational scientific arguments.

In an early French reaction of 1870, Bertrand, professor at the *École Polytechnique*, argued that the existence of vortex atoms was not consistent with the equations of fluid mechanics. However, his arguments in the *Journal des Savants* of which Bertrand himself was the editor, remain unclear to us [25] (see also [268, §29]). Apart from short references, and even some appraisals, of the vortex atom theory (see e.g. [125] and [63]), around 1890 French interest in Kelvin's work seems to have been completely absent. Poincaré had chosen the vortex theory for his 1891-1892 lectures at the Faculté des Sciences in Paris [180], and the published version of the lecture notes can be regarded as the first text-book on this theory, which he considered the greatest achievement of fluid mechanics at that time. However, he only shortly mentioned Kelvin's vortex-atom theory, and did not even refer to Kelvin in his introduction of the circulation concept.

The French attitude may be explained by the general rejection of the manner in which physics in Britain was exercised and which the French saw exemplified by Kelvin's work. One of the most important critics in this regard, together with e.g. Poincaré, was Pierre Duhem, a highly prolific scholar on the history and philosophy of science and a respected physicist. He reproached the British scientists lack of order, method, and concern for logic and experimental results. Furthermore, he criticized the provisional character of the various models they had introduced and the incompatibility of these models.

Taking notice of Duhem's strong opposition to atomistic theories, it is not surprising that he was especially critical of the vortex atom. An example of his sometimes furious, and not completely objective, treatment of Kelvin's model can be found in his *L'evolution de la mécanique*, where Duhem remarked that "cette hypothese de W. Thomson nous presente le plus haut degré de simplification auquel puisse parvenir l'explication des phénomènes naturels" [52, Ch XIV]. Though Duhem admitted that it contained advantages such as the absence of

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[10] Even in the 20th century, in France highly speculative books could appear (like those by Parenty [172] and Varin [271]) in which Descartes's vortex theory forms the basis of broad physical theories.

"force reelle", and the possibility to explain diversity of the elements, he warned that the vortex atom hypothesis "s'enfonce si profondement au-dessous des apparences sensibles, qu'il devient bien malaise de remonter jusqu'à celles-ci et de fournir l'explication des faits que nous constatons chaque jour."

Duhem's opinions were broadly propagated by himself and his own scientific work testifies that indeed he had chosen for a completely different approach of physics. His views on the differences between British science and French (and German) science must have been upheld by many of his contemporaries and has been a main reason for the lack of interest in the vortex atom on the Continent.

5.3 Issues surrounding the Vortex Atom

In the Vortex Atom paper, Kelvin had tried to show that his theory of matter possessed several of the properties which at that time were generally imposed on such a theory:

- the indestructible and impenetrable nature of the vortex atoms meant satisfaction of the requirement of conservation of matter;
- their elasticity and vibrations could explain the spectra;
- their many possible configurations could provide all elements with a signature of their own;
- no need existed for an artificial mechanism to keep several atomic rings together in a molecule; this explained chemical affinity.

The lack of any arbitrary parameters to be fixed made the model even more attractive. Besides, it was recognized that the vortex atom had an external kinetic energy, due to its self-induced velocity, and an internal form of energy, its vibration. For Kelvin, the only problem seemed to be the mathematical elaboration.

However, Kelvin must soon have started to realize that several problems were not only of mathematical nature. In the 1871 continuation of his Vortex Atom paper [247], mentioned in §5.1, he gave the description of three topics on which he intended to make further investigations: a system of vortex atoms as a kinetic theory of gases "without the assumption of elastic atoms"; the "realisation by vortex atoms of Le Sage's 'gravific' fluid consisting of an innumerable multitude of 'ultramundane corpuscles'"; and the "propagation of waves along a row of vortex columns alternately positive and negative". Still highly optimistic, Kelvin concluded that "the difficulties of forming a complete theory of the elasticity of gases, liquids, and solids, with no other ultimate properties of matter than perfect fluidity and incompressibility are noticed, and shown to be, in all probability, only dependent on the weakness of mathematics" [247, p.576-7].

As evidenced by the papers on vortex motion which would follow the next years, we conclude that by 1871 Kelvin already had planned a rather complete, and ambitious, research program in order to strengthen the position of his theory. In the next sections we discuss some of these fundamental issues, Kelvin's contribution to these, and reactions by others. The issues can be divided into two groups. The proof of steadiness and stability was a general problem related to vortex motion. Problems more directly related the vortex atom model were the comparison with properties exhibited by kinetic gases and the explanation of gravity and spectra.

5.3.1 Stability and Steadiness

Fundamental to the viability of the vortex atom model was the stability and steadiness of the vortex rings. The first issue was related to the question: is a vortex configuration conserved under changes of its level of energy? For the second, the problem read: are all (stable) states of a vortex configuration similar at a constant level of energy?

For Kelvin, the question of stability was familiar. Before the introduction of the vortex atom, he and Stokes had already discussed the stability of vortical motes (see §3.1). During the years 1872-1876, they discussed the topic again but this time it was concerned with a two-dimensional vortex (a cross section of a straight vortex tube [11]) in a circular boundary [12]. Both men could not agree. Kelvin argued that it was stable or at least quasi-stable and according to Stokes it was instable [218, p.433-].

Kelvin's picture of the stability of vortex atoms was of the same kind as that of the 2-D vortex. To tackle the issue, he regarded the change of the vortex from a stable maximum energy state to stable minimum energy state. The intermediate states, corresponding to different stages of vortex atom during its interactions with other vortex atoms, were called "maximum-minimim" states. To Kelvin these latter states meant that stability was uncertain, while Stokes argued that they would not be stable at all, except under special conditions.

Kelvin was also convinced that any finite number of vortices would always equilibrate, as it would gradually dissipate its energy and reach a state of minimum energy, i.e. state of maximal stability. The vortex atoms would then still be real vortices and energy in the universe would still be conserved. Stokes, who had clearly lost faith in the vortex atom model, tried to proof that the rotational motion of vortex atom could be annihilated, but to Kelvin nothing that God created could be destroyed [218, p.437].

In the Vortex Atom paper, Kelvin had tacitly assumed that "Helmholtz's rings" were stable and steady. Only in 1875, he seriously regarded these issues in a paper entitled "Vortex statics" [248]. His aim was formulated as "to investigate general conditions for the fulfilment of this proviso [i.e. steady motion], and to investigate, further, the conditions of stability of distributions of vortex motion satisfying the conditions of steadiness" [248, p.115]. His "general analytical condition for steadiness of vortex motion" had already been developed in the discussion with Stokes, as discussed above:

> If, with ... vorticity and "impulse" given, the kinetic energy is a maximum or a minimum, it is obvious that the motion is not only steady, but stable. If, with same conditions, the energy is a maximum-minimum, the motion is clearly steady, but it may be either instable or stable. [248, §4]

Though Kelvin realized that the energy of the vortex ring was a maximum-minimum, he presented it as a case of stable steady motion. For more complicated vortex structures, such as the "toroidal helix" [13], he could only speculate on their steadiness and stability.

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[11] Kelvin's interest in this vortex may be due to the vortex which had arised in the work based on a centrifugal pump designed by his brother James [218, p.412-].

[12] In general, Stokes's reaction to Kelvin's ideas had become unfavourable. On Kelvin's theories of ether-matter interactions he remarked: "It is easy to frame plausible hypotheses which would account for the results, but it is quite another matter to establish a theory which will admit of, and which will sustain, cross-questioning in such a variety of ways that we become convinced of its truth" [282, p.xxxix].

[13] The nomenclature was mostly invented by Kelvin himself. For a description of this "toroidal helix" we refer to Kelvin's paper.

Kelvin realized that his results were rather unconvincing. "Hitherto, I have not indeed succeeded in rigorously demonstrating the stability of the Helmholtz ring in any case" [248, §19]. From a simple and purely intuitional consideration of the ring, he concluded that "from the maximum-minimum problem we cannot derive proof of stability" [248, §19]. For rings with "ordinary proportions of diameters of core to diameter of aperture", he could only rely on "natural history", i.e. the observation of his own smoke rings.

Kelvin also realized that as important as the stability of the single vortex ring would be the stability (and steadiness) of configurations consisting of several vortices. One of the questions which he wanted to solve in this respect was the greatest number of columnar vortices or rectilinear vortex tubes that could be put in a "vortex mouse-mill", a regular polygon with vortices at the corners. He supposed this to be a proper simplification of the much too complicated case of interacting vortex rings.

In 1878, he discussed results of an experiment by the American Mayer [249], who had put floating bar magnets vertically in a basin of water and had found their positions of stable equilibrium to be at the vertices of regular polygons with one in the middle. Since infinitely thin straight vortex columns interacted in the same way as the magnets, this experiment could solve Kelvin's question, which to him was "of vital importance in the theory of vortex atoms" [249, p.140]. However, though Kelvin could partly confirm Mayer's results by mathematical calculation, he didn't conclude on the stability of his atoms [14].

The number of investigations of the stability of vortex motion by others seem to have been small. In his *Treatise*, Thomson remarked that Kelvin (possibly referring to [248]) had "proved that [the vortex ring] is stable for all small alternations in the shape of its transverse section". Thomson himself concluded "that it is stable for all small displacements" [234, §13]. Investigating the steadiness and stability of two linked vortices, he found that this configuration was steady only when the rings were close together. Hicks, in his extensive papers on vortex motion [81] and [82], investigated steadiness and stability of his hollow vortex ring. He found stability for the ordinary hollow ring, but could only find (conditional) stability if he extended his ring model with additional circulation and density differences (see Chapter 4). In his final discussion of the vortex atom in 1895 [85], he still warned that this issue had not been solved.

The issue of stability and steadiness remained unsettled, mainly due to a lack of appropriate mathematical techniques. However, for Kelvin and his followers, the existence of smoke rings seemed enough "evidence", though they may have felt somewhat uneasy with the situation.

5.3.2 Compatibility with Kinetic Gas Theory

As mentioned in the introduction of Chapter 4, the popularity of the kinetic gas theory had largely increased by the time Kelvin had introduced his vortex atom. Kelvin himself strongly rejected the kinetic gas theory of elastic-solid molecules colliding by actual contact since he supposed that all kinetic energy would be converted into vibrations and rotations. However,

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[14] See [219] for a full account of Mayer's experiments and also for J.J. Thomson's reference to Mayer's results in his speculation on a possible arrangement of electrons in atoms.

In his *Treatise*, Thomson treated the case of the "mouse-mill" analytically and showed that the motion was stable for the number of vortices $n < 7$ and instable for $n \geq 7$ [234, §54]. Only in 1931, Havelock showed that the case $n = 7$ is neutrally stable. Re-examination 50 years afterwards by Dritschel [49] (numerically for finite-core-sized vortices) and by Dhanak [46] (analytically) showed that $n = 7$ is stable; Dritschel pointed out the mistakes in Thomson's calculations.

Kelvin realized that to show the superiority of his own vortex atom model, he had to show that it possesed the same characteristics as kinetic gases. This would require a complete determination of the interactions of large numbers of vortex atoms, and a statistical approach was necessary as was common in kinetic gas theories. However, he had always despised such an approach as it introduced indefiniteness.

His only paper in this regard concerned one of the main results of the kinetic gas theory, i.e. the partitioning of energy: any concentration of energy within a gas had to spread throughout the whole gas, giving a specific randomized distribution. In "On the average pressure due to impulse of vortex-rings on a solid" of 1881 [252], he argued, without any proofs, that the pressure exerted by a cloud of vortex atoms (the "gas" which had been regarded by Thomson; see §5.1) was the same as that shown by the kinetic gas theory.

However, Kelvin also correctly realized that the integral of the pressure on the wall would be zero and thus the "gas" was not able to transmit momentum as any kinetic gas could do. Unconvincingly, he tried to show that the integral was nonzero if the flat plate was replaced by a finite tube [15].

Pressure was also regarded in Thomson's *Treatise* which would remain *the* attempt to settle the theory of a vortex atom gas. In his attempt to derive Boyle's law, Thomson found an additional term from which he concluded that "the vortex atom theory explains the deviation of gases from Boyle's law", adding the remark that other models were not able to show this result [234, §56].

However, Thomson realized that his derivation in the *Treatise* had been somewhat obscure. Besides, he might have been incited by Reynolds's fundamental criticism (see §5.1). In a paper of 1885 [237], he tried to apply a statistical method to derive again an expression for the pressure that a system of vortex atom exerts in a vessel. First of all he warned that the problem of the distribution of velocities of ordinary solid particles as founded by Maxwell and Boltzmann (the Maxwell-Boltzmann equipartition theorem) had been based on identical particles, whereas for the vortex atom model the sizes could differ. Nevertheless, after a long investigation on the distribution of vortex atoms, he indeed derived Boyle's law. However, this time one quantity remained undetermined, for which he couldn't indicate how to determine it.

The investigation of the relation between temperature and velocity of the rings, which had already been proposed for investigation in Kelvin's Vortex Atom paper [243, p.11], led to Thomson's discovery of a remarkable property of the vortex atom. For increasing temperature, i.e. kinetic energy, of a vortex ring its radius increased and consequently, according to expression (4.3) [16], its velocity decreased. For a kinetic gas, however, it had been shown that the velocity of the particles increased. Thomson tried to weaken this last result by remarking that it was based on monatomic gases; for diatomic gases, he thought, the velocity could indeed decrease [234, §57].

Despite Thomson's attempt, this property of the vortex atom would become much criticized. The only one who seems to have tried to remove the opposition on this issue was Hicks. However, his arguments only appeared in 1895 [85]), by which time the vortex atom theory had largely been abandoned.

Meanwhile, Kelvin realized that the only way to avoid more criticism was to intensify his attack on the kinetic gas theory. In a lecture before the British Association in 1884 [254],

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[15] This result indeed seems to be in error, as has been already recognized by Quint in his thesis of 1888 [184] (see note 9) and recently by Saffman [205, §5.2].

[16] For which Thomson had, erroneously, found a factor 1 instead of $\frac{1}{4}$ [234, §13].

he tried to convince his audience that by his vortex atom model he had shown that inelastic bodies could behave as elastic bodies by their motion. He accused the kinetic theory of gases of being of no use on the atomic or molecular level and, surprisingly, claimed that the problem of equipartition of energy was deadly for any kinetic theory like the kinetic gas theory. Besides, he repeated his old argument that kinetic models had to assume elasticity, whereas the vortex atom model had not.

Unfortunately, reactions by the defenders of the kinetic gas theory are unknown, but we may suppose that the attempts by Kelvin and his followers were not taken very seriously, as their argumentation had been weak and unconvincing.

5.3.3 Gravity and Inertia

Ever since Newton's days, gravity had attracted the attention of scientists and, not surprisingly, many theories had been formulated [17]. The question how to include gravity in his vortex atom model must have bothered Kelvin soon after the appearance of the Vortex Atom paper. In 1868, he had started a correspondence with Fleeming Jenkin [18] who had immediately raised several questions regarding the properties of the vortex atom model. One of these concerned gravity, an issue Kelvin had not mentioned in his Vortex Atom theory.

A related issue, also raised by Jenkin [217], was inertia. In the Vortex Atom paper Kelvin had remarked that the only properties required of the vortex atoms were "inertia and incompressible occupation of space" [243, p.2]. Jenkin asked Kelvin how the fluid, possessing inertia, "can leave a free passage to aggregate vortices called gross matter." He could not understand "how the inertia of the medium in a given space can be increased or diminished by motion". Kelvin seems to have replied by explaining the difference between "what he saw as apparent inertia and primeval inertia, the latter only being inherent" but, unfortunately, details are lacking.

Also in Maxwell's contribution to the Encyclopaedia Britannica [147] (see §5.1) the need for an explanation of mass had been recognized. To Maxwell it seemed that Kelvin had proposed that only by the motion of the rings we can define matter in the primitive fluid, i.e. matter as a mode of motion. However, Maxwell remarked, the inertias of this mode of motion had to be explained, because "inertia is a property of matter, not of modes of motion" [147, p.472]

Thus, Kelvin could not avoid an attempt to comprehend gravity and inertia within the vortex atom theory if it were ever to achieve completeness. He tried to formulate such an explanation by means of the 18th century theory of gravity proposed by Le Sage, which had been introduced to him by Jenkin during their discussion on gravity. Le Sage's theory of gravitational action essentially depends on the bombardment of so-called ultra-mundane corpuscles on ponderable bodies [19].

In "On the ultramundane corpuscles of Le Sage" of 1872 [246], Kelvin extensively discussed Le Sage's theory of matter and added his own adaption to the vortex atom model. He noticed that the postulate of hard atoms in a void underlying both the kinetic theory of gases (which he had much criticized, as we have seen) and Le Sage's theory, was open to doubt and he tried to show that the specific problems he pointed out for Le Sage's theory could be resolved by replacing the hard atoms by his vortex atoms. The weakness of Kelvin's attempt to incorporate Le Sage's model was soon recognized. Nevertheless, Maxwell, in his enthousiastic

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[17] See [137] for an extensive survey of conceptions of gravity in the 18th and 19th centuries.

[18] Jenkin, an engineer who had collaborated with Kelvin in submarine telegraphy, had been the first to treat the vortex atom theory after its introduction, in a paper on the atomic theory of Lucretius [94].

[19] For a full account of Le Sage's theory, see [137, p.111-].

Encyclopaedia Britannica paper [147], didn't dare to criticize the vortex atom model on this point despite his description of a fundamental flaw in Le Sage's theory.

Kelvin himself, recognized the weak position of his theory and explained its failure to provide support to his vortex atom in 1881 [253]:

> Le Sage's theory might give an explanation of gravity and its relation *to inertia of masses*, on the vortex theory, were it not for the essential aeolotropy [= non-isotropy] of crystals, and the seemingly perfect isotropy of gravity. No finger-post pointing towards a way that can possibly lead to a surmounting of this difficulty, or a turning of its flank, has been discovered, or imagined as discoverable. Belief that no other theory of matter is possible is the only ground for anticipating that there is in store for the world another beautiful book to be called *Elasticity, a Mode of Motion.* [253, p.473]

An alternative model to explain gravity came from Hicks, whose research on pulsating spheres has already been mentioned in §5.1. Hicks [78] tried to show how vortex atoms could show attractive and repulsive forces by a pulsative change of their volumes. However, he had to conclude that even for an incompressible fluid, for large collections of vortex atoms gravity "would take time for its full effect to travel any distance" [78, p.284]. This obviously meant an important flaw of his model and Hicks's proposal gained little support [137, p.285]. In a paper of 1883 [80], Hicks had become much more cautious. Though he could show that pulsating rings would also attract or repel each other, he made no remarks on gravity anymore.

5.3.4 Spectra

In §3.1. we mentioned that the origin and nature of line spectra of elements had been established by the middle of the 19th century and were found to originate in the motions of the molecules or atoms which were transmitted to the ether as vibrations of definite wavelengths. This led to the requirement of elasticity of the atom. Besides, different materials showed different spectra and any theory of matter should be able to show this. Kelvin realized that if he would indeed be able to show correct spectral properties of his atoms, this would mean an important step in the challenge of the kinetic gas theory, which, as remarked in §5.1, was insufficient on this issue.

In his Vortex Atom paper, Kelvin had tried to convince his audience, by means of his experiments with smoke rings, that "the vortex atom has perfectly definite fundamental modes of vibration, depending solely on that motion the existence of which constitutes it [i.e. vortex motion]" [243, p.4]. In "Vortex Statics" of 1875 [232] he derived the first four of these modes from an anlogy with the deformation of an elastic wire; see fig.5.1.

However, he realized that mathematical treatment would, again, be complicated and he proposed to consider the modes of vibration of an infinitely long, straight, cylindrical vortex. He added that "these results are, of course, applicable to the Helmholtz ring when the diameter of the approximately circular section is small in comparison with the diameter of the ring ... "[243, p.4]. Kelvin's results on the vibrational properties of this "columnar vortex" were only published in 1880 [250]. He did indeed find the dispersion relation for long bending waves on a rectilinear vortex with a constant-vorticity core, i.e. helical disturbances, but didn't dare to draw any further conclusions regarding the vortex atom model.

Though Kelvin's elaboration of the vibration of the column was recognized as a fine piece of analytical performance, many doubted the possibilities of the vortex atom model regarding

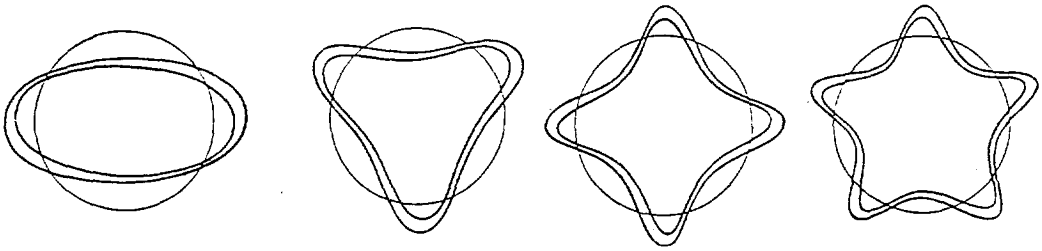

Figure 5.1: Steady modes of a vortex ring according to Kelvin. From [248].

spectra. In Maxwell's final deadly blow on the vortex atom theory in his critical 1877 paper [144] (see §5.1), he remarked that spectral requirements only allowed a finite number of degrees of freedom, whereas the vortex atom had an infinite number.

Hicks [80], on the other hand, seriously tried to elaborate the explanation of spectral lines by means of his hollow vortex model. However, his calculations did not show any concrete results and he found himself forced to remark that it would be "venturesome" to draw conclusions with respect to properties of the vortex atom, or to find analogies with kinetic theory of gases. One of the reasons for his reserve was related to the fact that, contrary to the interaction of hard bodies in kinetic gases, the interaction of vortex atoms depended on the mode of approach. Furthermore, his evaluation of the dependence of the time of vibration on energy showed disagreement with the result found by Thomson in his *Treatise*. This dependence of spectra on energy seemed fatal to the vortex atom.

Other, more detailed, critical remarks on Kelvin's attempt to derive spectral results appeared and were even more destructive. W.H. Julius (see note 9) remarked that a statistical approach was necessary for a real comparison of the properties of a gas of vortex atoms. One had to determine whether such a gas would give a single value of the spectrum if the number of "collisions" was reduced i.e. if the temperature was lowered. From a numerical example for carbonic acid, he showed that for a correct spectral property, the vortex atoms should not approach more than 30 times their diameter; evidently, there was no reason why this should be the case in a "gas" of vortex atoms [97, p.132-6]. Another Dutchman, Quint (see also note 9), remarked that Kelvin's suppositions in his treatment of the oscillations of the vortex column were in violation of the law of continuity [184, p.70]. Furthermore, he argued that Kelvin had just supposed that vortex atoms were elastic. His only proof seemed to lie in Tait's experiment, but Quint wondered whether this argument was not a hypothesis itself. The experiment had been carried out in a viscous, compressible fluid, so translation to ideal fluids was questionable [184, p.129].

The spectral properties of the atoms remained obscure and the results found showed severe inconsistencies. Not surprisingly, in the discovery of the spectral series formulae, by Balmer and Rydberg around 1885 (who were presumably only slightly familiar with Kelvin's theory), the vortex atom would play no role at all [151, p.172-]. Up to 1890 progress in the explanation of spectra was slow. By then the electromagnetic "viewpoint" was quickly gaining influence (see §6.3). Though still new models were proposed to reconcile older models like the vortex atom with the developing electromagnetic view [20], it had become clear that the search for explanation of spectra by models like that of the vortex atoms had been in vain.

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[20] One of these was the model by Stoney in 1891, who suggested a connection between electrical charges associated with each bond in a chemical atom called "electrons" and vortex atoms [151, p.188-9].

Chapter 6

The Decline of the Vortex Atom

In 1884 Kelvin was invited for a series of lectures in the United States at John Hopkins University, which have become known as the Baltimore Lectures [241] [1]. These lectures, devoted to the propagation of light and its interaction with matter, still showed a seemingly fully-resistent optimism regarding explanation by means of mechanical models: e.g. Kelvin presented a mechanical model of a molecule consisting of spherical shells connected by springs (called gyrostats). The vortex atom model, which we have called "hydrodynamical" to contrast it with the mechanical models (see the introduction of Chapter 4), was not even mentioned once [2]. In general, after 1884 the vortex atom would be absent in Kelvin's still steadily produced papers on all kinds of physical subjects.

Nevertheless, the vortex atom theory still played some role in physics, a role which Kelvin himself might never have expected and to which he didn't contribute very much himself. It formed the source of inspiration for a new direction in the modelling of the ether: vortex ethers. This is the subject of §6.1. As we have seen in Chapter 5, reception of the vortex atom had been unfavourable to Kelvin. Despite his efforts to elaborate the several issues mentioned in §5.3, and due to the flaws which these efforts had shown in the vortex atom model, the fame of his theory steadily declined. In §6.2, we will discuss Kelvin's reaction. Besides the insufficiency of the model itself, its decline can be related to the general conceptual changes which took place in physics in the 1890s. These changes, which made the fall of any theory like that of the vortex atom inevitable, will be treated in §6.3.

6.1 Vortex Ethers

The second (1904) edition of the Baltimore Lectures shows the fast developments which had been taking place at the end of the last century. It is extended with several appendices, containing some of Kelvin's lectures and papers which had appeared after the first edition. One of them was Kelvin's 1900 paper on two "nineteenth century clouds over the dynamical theory of heat and light" [259]. The first of these clouds concerned the relative motion of the still mystical interaction of ether and ponderable bodies which had become known as the ether drift: is the ether taken along with bodies (such as the earth) or is it always at rest; or: does ether drag exist or not?

Around 1885, the answer to this question was still lacking and the elaboration of ether-models had become a serious business. The elastic-solid ether model (see the introduction of Chapter 3) seemed useful indeed but also showed difficulties in connection with reflection and refraction. MacCullagh had proposed a rotationally elastic ether, but it could not be translated into a physical conception of its mechanism. Kelvin himself, in his Baltimore Lectures, had proposed a gyrostatic model which was based on MacCullagh's proposal.

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[1] The first edition appeared the same year. A modern annoted edition is [99].

[2] Recall Duhem's criticism of Kelvin's easy shifting between various models (see §5.2.2).

Because of its characteristic of moving freely through the ether, the original vortex atom appeared to be a popular alternative in the 1880s to both Fresnel's ether drift with partial ether drag and Stokes's total ether drag. Since it was generally assumed that planetary bodies had to be able to move unresistedly through the ether, one realized that many of its properties had to be those of a perfect fluid. On the other hand, it was realized that in an ether model also properties of solids had to be included to make the ether capable of transmitting waves of light. Consequently, the question was raised whether a configuration of vortex atoms could indeed transmit any kind of "waves".

The relation of the vortex atom and the ether has a complicated history. Initially, in his Vortex Atom paper, Kelvin had not mentioned the ether at all. However, the problem must have started to bother him after one of the first letters he received from Jenkin (see §5.3) in which the question was put whether the ether also consisted of some kind of vortex rings. Unfortunately, we do not have Kelvin's response but we have found no indications that the perfect fluid in which the motion of the vortex atom took place was not the ether itself, as some historians have suggested (e.g. Wilson [281, Ch.7]). Both on account of his old (ether is aerial) and new (ether is air-like) vision on the substance of ether (see §3.1), it is probable that indeed Kelvin would think the ether to consist of vortex rings.

Some followers of Kelvin's theory of matter did identify the "perfect liquid" in which the vortex atoms existed with the ether, as is evidenced by their papers. In Hicks's contribution to the vortex atom theory, for example, a question had been raised concerning the explanation of the large density of ponderable matter as compared with ether (see §5.1).

If, on the other hand, the "perfect liquid" could not be equalled to the ether, the unpleasant situation of "a dualistic physical conception" arose, as was remarked by Pearson in his 1885 paper [174] (from which we quoted in the introduction to Chapter 5). If an atom is not a difference of motion in the ether, "we are compelled to suppose two primary substances, ether-substance and atom-substance". The problem is that "we should be explaining our atoms by means of an ether which would in itself be atomic" [174, p.119].

For Kelvin, Pearson's dualistic view seems to have been out of the question. He realized that one of the ways to settle the question would be to show the possibility of the transmission of waves by a "vortex ether". The only work which seems to have been intended for this purpose is [255], presented at the 1887 meeting of the British Association in Manchester. Though his lecture was titled "On the vortex-theory of the lumniferous ether (On the propagation of laminar motion through a turbulently moving inviscid liquid)" [256], the main title may just have been meant to draw the audience's attention, since in reality the paper is an attempt to "investigate turbulent motion of water between two fixed planes". The paper does not give the impression of a man believing to have proposed another promising ether model. On the contrary, it showed the signs of a man full of doubts, who at the end of the paper had to conclude with the "Scottish verdict of *not proven*" because he was still doubtful about the stability on the arrays of vortex rings (see fig.6.1) which he supposed to transmit the "waves of laminar motion".

Somewhat earlier, in 1885, another strong adherent to the vortex ether [3] had proposed his model. In 1880, FitzGerald, professor at Trinity College in Dublin, had already developed a

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[3] The vortex ether has also become known as the vortex sponge. Though the introduction of this term has generally been attributed to Kelvin, he does not seem to have done so in the context of his ether model. We have only traced "vortex sponge" once, in a general paper on the stability of vortex motion [251].

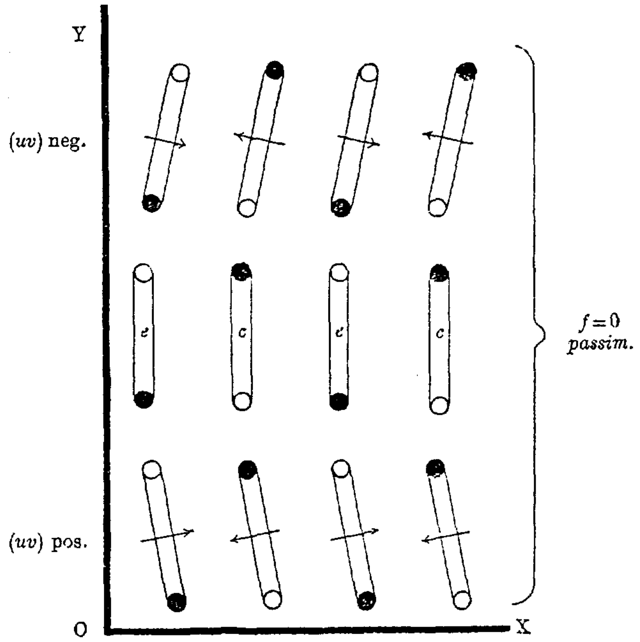

Figure 6.1: Kelvin's model of a vortex ether. From [256].

theory of the ethereal plenum to incorporate Maxwell's theory of light, based on pure electromagnetic properties. However, Kelvin's papers on vortex motion had changed his mind towards a theory in which ether and matter were represented by vortex motions in a universal plenum [4].

In "On a model illustrating properties of ether" [57] of the same year, FitzGerald proposed his new ether model. He stressed the point that regarding the nature of matter he would not make any supposition, the 'sponge' was just a model of the ether. In the rest of the paper he regarded a possible explanation of polarization, but drew no conclusions.

FitzGerald's paper does not seem to have roused much reaction. The only other paper related to this topic seems to have been a letter in *Nature* in 1889 [59] in which FitzGerald reacted to Kelvin's ether model, treated above [256]. His reaction consisted of a similar "electromagnetic interpretation of turbulent liquid motion" as in Kelvin's paper. To which he added that " a natural hypothesis would be that matter consisted of free vortex rings." [59, p.34].

Kelvin in his turn reacted to FitzGerald's remarks by regarding the "stability and small oscillation of a perfect liquid full of nearly straight coreless vortices" [257]. The most remarkable aspect of this paper is Kelvin's apparent conversion towards Hicks's hollow vortex core (see §5.1). The rotational vortex cores had to be discarded absolutely, and "we must have nothing but irrotational revolution and vacuous cores" [257, p.202]. He concluded that Hicks's work on the hollow vortex together with his own paper on the columnar vortex [250] (see §5.1) "will be the beginning of the Vortex Theory of ether and matter, if it is ever to be a theory" [257, p.202]. Apparently, his faith was declining.

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[4] Despite his enthusiasm for the vortex ether, regarding the vortex atom, FitzGerald was very critical. In a paper of 1885 [56], he called the vortex atom "hardly ... an adequate theory ... It certainly is not sufficient to explain luminous and electrical and magnetic phenomena, to suppose the ether to be simply a perfect liquid at rest" [56, p.340].

After the introduction of a new vortex ether model, consisting of a regular pattern of straight hollow vortex tubes, Kelvin concluded:

> I have been anxiously considering the effect of free vortex rings with vacuous cores among the vortex columns of this tensile vortex ether, as suggested for cored vortices at the end of your communication ... [i.e. FitzGerald's paper [59]]. It will be an exceedingly interesting dynamical question; though it seems to promise at present but little towards explaining universal gravitation or any other property of matter; so you may imagine I do not see much hope for chemistry and electro-magnetism. [257, p.204]

These remarks seem to have ended the life of the vortex ether on both FitzGerald's and Kelvin's side, though in his address at the 1888 meeting of the British Association, the former still suggested that the problems could be overcome [58, p.562]. Ironically, nowadays FitzGerald is best known for the contraction theory named after him and Lorentz, which at the end of the 19th century was one of the signals that the ether would definitely disappear as a topic in physics.

In the meantime, a crucial experiment had been performed by Michelson and Morley in 1887 which showed the incorrectness of any stationary ether hypothesis and actually meant the death of the ether hypothesis. The properties of ether had suddenly become self-contradictory [5].

The Michelson-Morley experiment did not immediately put an end to model-building. On the contrary, Hertz's discovery of wireless waves, an experimental demonstration of Maxwell's electromagnetic theory, must have stimulated new research, as that by Hicks and Lodge.

In 1885, Hicks had discussed the possibility of transmitting waves through a medium consisting of an incompressible fluid of closely-packed small vortex rings [83] and in 1888 had proposed "a vortex analogue of static electricity" [84]. He still discussed the possibilities of a vortex 'sponge' model in his address to the Section of Mathematical and Physical Science of the British Association in 1895. However, Hicks had to conclude that "we can make little further progress until we know something of the arrangement of the small motion which confer the quasi-rigidity [of the ether]" [85, p.601], but he nevertheless considered some possibilities. He even tried to show how the explanation of "the magnetic rotation of the plane of polarisation of light" could be explained from vortex rings and how gravity could be explained from the vortex ether. "In all cases, whether a fluid ether is an actual fact or not, the results obtained will be of special interest as types of fluid motion" [85, p.606].

Still in 1893, Lodge [6], then professor of physics in Liverpool, argued that all phenomena and all experiments, except that by Michelson and Morley, could be explained in terms of the vortex atom. Probably, he suggested, even this last result could be "explained away", though

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[5] Still, in a series of lectures given in 1899, Michelson [156] stated that "the 'ether vortex theory' which, if true, has the merit of introducing nothing new into the hypotheses already made, but only of specifying the particular form of motion required." After an explanation of his experiment with Morley, he treated the characteristics of vortex rings: "In fact, there are so many analogies that we are tempted to think that the vortex ring is in reality an enlarged image of the atom." Apparently, at that time Michelson still strongly believed in the unification which the ether could provide to physics though deep inside he must have had different opinions.

[6] In 1885 Lodge had done some really fundamental work to determine the value of the vortex atom [130]. To approach the difficult subject of interaction vortex rings, he had calculated and drawn their streamlines, a much more "experimental" approach than had been done in other papers before by anyone else. In the same paper he mentioned his experiments with smoke-rings using a "powerful intermittent induction-coil Leyden-jar discharge" [130, p.70].

on the other hand the vortex atom model might have to be adapted [131] [7].

Clearly, the adherents of the vortex ether were unable to put up a really satisfactory model. Their papers remained full of suggestions and lacked concrete results. Consequently, the role of vortex models never became generally accepted.

Not surprisingly, Kelvin turned his attention towards new ether models. His final opinion on the ether was published shortly before his death in 1907 and shows his transition towards the new "electric particle" approach (see §6.3 below). Looking back, he concluded:

> I do not propose to enter on any atomic theory of ether. It seems to me indeed most probable that in reality ether is structureless. ... We sometimes hear the "luminiferous ether" spoken of as a fluid. More than thirty [8] years ago I abandoned, for reasons which seem to me thoroughly cogent, the idea that ether is a fluid presenting appareances of elasticity due to motion, as in collisions between Helmholtz vortex rings. Abandoning this idea, we are driven to the conclusion that ether is an elastic solid. [262, p.236]

However, by 1907 the ether had already become an obsolete concept in physics due to the introduction of Einstein's theory of relativity.

6.2 Kelvin's Reaction to the Decline of the Vortex Atom

From the discussion in §5.3 of Kelvin's elaboration of the several issues surrounding his vortex atom theory, it appeared that his mind was still directed towards its problems at least up to his 1884 lecture before the British Association (see §5.3.2). Thereafter, as discussed in §6.1, Kelvin shortly directed his attention towards the vortex ether, though with an apparent lack of enthusiasm and hope.

In 1883 he still lectured on the vortex atom in Newcastle, where he received a copy of Thomson's *Treatise* [232, p.1046]. Not surprisingly, this support for his theory pleased him very much; to one of his colleagues he wrote: "I am becoming hot on vortex motion through having ... J.J. Thomson's book at hand" [213, p.212]. Though the same year, Tait wrote Kelvin that he had found a means for destroying the vortex atom for good, apparently Kelvin was not dissuaded by Tait's proof [213, p.214].

However, the still rising popularity of the vortex atom at that time and the attempts to apply the vortex atom theory to physical phenomena had also had its negative consequences: the weakness of the theory became clear. One of Kelvin's biggest worries must have been his inability to proof the stability of the vortex ring (see §5.3.1). However, in a footnote to a paper of 1905, we learn that Kelvin's doubts on this point had only become a real conviction after writing a paper entitled "On the stability of steady and of periodic fluid motion" published in 1887 [255]:

> It now seems to me certain that if any motion be given within a finite portion of an infinite incompressible liquid originally at rest, its fate is necessarily dissipation to infinite distances with infinitely small velocities everywhere; while the total kinetic energy remains constant. After many years of failure to prove that the original

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[7] Actually, Lodge performed an experiment of the same rank as that of Michelson and Morley which, by its confusing result, aided to the declining faith in the ether [65].

[8] From this remark a problem of chronology arises, as by 1877 Kelvin was still favourably inclined towards the idea of elasticity resulting from vortex motion. He probably meant twenty years, as by 1887 his faith in the stability of the vortex ring had definitely been lost (see §5.3.1; see also [281, p.178]).

> Helmholtz circular ring is stable, I come to the conclusion that it is essentially unstable, and that its fate must be to become dissipated ... . [261, p.370]

Regarding the fate of the vortex atom theory with regard to other issues - gravity, spectra, and the compatibility with the kinetic gas theory - we only have some scattered remarks by Kelvin. In 1886, he told Merz, author of an excellent survey of 19th century science, that the vortex atom did not realize his expectations, inasmuch as it did not explain inertia or gravity. [232, p.1046] In 1898 he wrote Holman, professor at M.I.T. and surveyor of physics at the end of the 19th century: "I am afraid it is not possible to explain all the properties of matter by the Vortex-Atom theory alone, that is to say, merely by motion of an incompressible fluid; and I have not found it helpful in respect to crystalline configurations, or electrical, chemical, or gravitational forces. ... With great regret I abandon the idea that a mere configuration of motion suffices" [232, p.1047].

Kelvin's recognition of the failure of the vortex atom model led to a short refuge in the Boscovichean theory of matter (see §5.1). This seems remarkable as in the early 1860s he had completely rejected this theory. However, he must also have realized that the space-filling force of Boscovich's theory was not so very much different from the space-filling vortex ether theory. While in his 1884 Baltimore Lectures he had put the Boscovich model behind the vortex atom, in the 1890s he was readily employing Boscovichian force curves (see e.g. [281, Ch.9]). While he retained a speculative belief in the sufficiency of explanation by means of models like that of the vortex atom, he had shifted, as a practical matter, to the more positivistic approach of Boscovich. However, he still insisted that atoms had to be considered as having finite dimensions and structure, properties which the Boscovichean atom was lacking.

Finally, however, as for the vortex atom, Kelvin recognized that the Boscovichean atom could provide no sufficient explanation for matter. Kelvin's final atom model of 1901 [260] was a static arrangement, which contained electric particles which he called electrions.

6.3 The Rise of a New Physics

British physics after 1880 showed a complex interaction between Maxwell's electromagnetic theory, vortex ether theories, new insights into the nature of the electric charge, and modified vortex atom conceptions of matter. This "struggle" would eventually be lost by the vortex atom and all related models that had failed to incorporate the electromagnetic theory of light. This is apparent by the change in Kelvin's opinion on matter as we already indicated at the end of §6.2.

Several incentives towards the new development in physics, i.e. the shift towards "electric" models, can be mentioned.

One incentive was related to the second cloud over the dynamical theory of heat and light, which Kelvin had treated in [259] (see §6.1): the Maxwell-Boltzmann equipartition theorem (see §5.3.2). While Kelvin had tried to dispel its meaning and to show its failure, in the 1890s other British physicists incorporated the electric charge into vortex atom conception in an attempt to reconcile the difficulties related to the equipartition theorem.

Another incentive came from the still ongoing attempts to adapt the vortex atom to new developments in physics. Larmor, then lecturer in Cambridge and eventual successor at Stokes's position, attempted to resolve the problems of constructing an ether theory that would represent all optical and electromagnetic phenomena. In the first part of his extensive paper "A dynamical theory of the electric and luminiferous medium" of 1893-1895 [116], we encounter a man still fighting with the heritage of the declining vortex atom and the vortex ether models. However, Larmor came to the conclusion that some other bond for the atoms of a molecule had to be found, in addition to the hydrodynamic one. This he found in the attractions of the electric charges of the atoms. Thus, while the first part of his paper had been a last attempt to reconcile the vortex theory with the newly emerging concept of electrons, the second part would be completely devoted to the electrons.

Not surprisingly, Larmor's theory encountered the same attacks as the vortex atom theory: it was too complex and far-fetched to be real. Nevertheless, Larmor's work found some support in a paper by Pocklington [179](1895) who discovered that the missing energy in the model could be found in the electric charge of the vortex.

Though British physicists like Larmor were still trying to integrate Maxwell's theory of electromagnetic fields into Kelvin's ethereal continuum, others just formulated field equations without any involvement of (mechanical) ether models. The development of ether and field theories more and more challenged the hegemony of the view of nature expressed in e.g. the vortex atom theory.

The final blow to the vortex atom was given by J.J. Thomson, former defender of the vortex atom (see §5.1).

The *Treatise* had given Thomson some reputation. In 1884, at 28, he was chosen as Cavendish Professor of Experimental Physics at Cambridge University (Maxwell and Rayleigh had been his predecessors), above men like FitzGerald and Reynolds. Recalling the failures he had met in his elaboration of the vortex atom theory (as treated in §5.3), it is not surprising that after 1885 Thomson had growing doubts on the vortex atom and acquired a positivistic viewpoint on matter. After 1886 he stopped research on vortices and went over to experiments on rarified gases to verify his ideas on chemical combinations. Nevertheless his work was still based on his investigations on the vortex atom. His research with cathode rays showed that their long path lengths could only be explained by assuming a very small particle, very much smaller than an atom, which would be inconsistent with the vortex atom model. Even if Larmor's model would be used, Thomson realized, no explanation could be found for this phenomenon. However, the vortex theory still guided his search [266]. A theory of cathode rays based on linking and unlinking vortex rings led Thomson to think that the phenomena would be clearer in a higher vacuum. The final result of these experiments was the discovery of the electron in 1897 [151, p.173-]

Thomson's discovery meant a definite justification for the newly arising "electric" atom models. A new physics had been born. As Pearson accounted in 1900:

> The end of the nineteenth century ... marks the advent of experimental knowledge requiring an entire revision of the hypotheses and theories as to the constitution of matter. ... Whereas through the greater part of the nineteenth century, "matter" was the concept which was looked upon as fundamental in physical science, of which there was a curious accidental property called electricity, it now appears that electricity must be more fundamental than matter, in the sense that our once elementary matter must now be conceived as a manisfestation of extremely complex electrical phenomena. [176, p.356-]

Related to this shift in "fundamental concept", i.e. from mechanical towards electrical, the end of the 19th century saw the decline of devising mechanical and hydrodynamical models (see the introduction of Chapter 5). If the supposed mechanical explanation provided no new insight, and if it led to no further progress, in what sense did it provide an explanation? Planck's attack on the kinetic gas theory, for example, in the 1890s, can be seen as the growing challenge of the whole "mechanical programme". Even the position of matter was attacked. If all matter is made of elements which are of an electrical nature, is not electricity instead of matter the fundamental physical reality? It was the fate of the vortex atom model to introduce the theory of fields into Britain, which would result in field theory of the atom in which the vortex atom was no longer needed.

Larmor, who had seen the decline of his own model, tried to formulate the merits of the vortex atom model in his 1900 presidential address for the British Association, which can be regarded as a summary of the state of matter and ether theories at the turn of the century:

> The vortex-atom theory has been a main source of physical suggestion, because it presents, on a simple basis, a dynamical picture of an ideal material system, atomically constituted, which could go on automatically without extraneous support. The value of such a picture may be held to lie, not in any supposition that this is the mechanism of the actual world laid bare, but in the vivid illustration it affords of the fundamental postulate of physical science, that mechanical phenomena are not parts of the scheme too involved for us to explore, but rather present themselves in definite and consistent correlations, which we are able to disentangle and apprehend with continuously increasing precision. [117, p.625]

Kelvin not only had to accept that his theory of "matter as motion" had failed, it must also have bothered him that the new generation of physicists had stopped using his kind of models and had shifted towards analytical models [9]. Kelvin's methodology would die with him and with his vortex atom.

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[9] See the Epilogue.

Interlude: Between Vortex Atom and Vorton

As we have seen, the introduction of the vortex atom by Kelvin meant a new impulse to research on vortex motion. The impressive works by Thomson, Hicks, and several others (see §5.1) marked a transition towards a serious, mathematical treatment of vortex motion. Though the decline of the vortex atom may have caused a temporary stagnation in the development of vorticity theory , since Kelvin's days the subject of vorticity and vortices has been steadily enriched and and is still actively explored. Moreover, it has become generally recognized that vorticity is an essential part of most fluid flows. Several new topics in research on vortex motion have been introduced of which we mention, without further exposition, vortex-breakdown (see e.g. [205, §14.4]), geophysical vortex flows, vortices in wakes of solid bodies, vortex shedding, vortex buckling, vortex sound, and vortex merging [10].

As has become clear from §5.2, in the 1880s and 1890s research on vortex motion hardly spread from Britain to other parts of the world, and seems to have remained a British speciality for some time after the decline of the vortex atom and vortex ether. Only in the first decade of this century, a growing interest can be detected on the Continent.

In the beginning of the 20th century new areas in fluid mechanics developed, like the theory of boundary layer flow and the theory of airfoils [11]. Their popularity temporarily hindered recognition of interesting and important results which were still discovered in vorticity theory [12]. At the same time, however, discoveries in these fields showed new and unsuspected aspects of the role of vorticity in fluid flows.

In 1904, the German Prandtl proposed the boundary layer, a thin layer near the surface of a body in which vorticity is generated due to the so-called no-slip boundary condition [13]. Due to the realization that the viscous boundary layer could be regarded separately from the inviscid flow above it, adoption of his theory permitted mathematical simplifications of the hydrodynamical equations which resulted in the solution of some long-standing viscous flow problems in fluid mechanics (e.g. the drag met by bodies in fluid flows). The early developments in the design of airplanes gave an important stimulus to the field of aerodynamics. The new theory of airfoils, formulated by the Russian Zhukovsky and others, showed the important connection between the lift of a wing and Kelvin's (still underrated) concept of circulation. Besides, this new field stimulated the study of compressible (vortical) flows [134, §4.7].

In another major field of 20th century fluid mechanics, turbulence, developments at the beginning of the 20th century were still slow and few [14]. Only in the 1930s vorticity became involved (again) in turbulence research, as will be discussed in §B.

Here, we will not review all developments in vorticity theory up to our days [15]. The choice

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[10] The only available general survey of vortex flows, both in nature and technology, is the book by Lugt [134]. A survey of present-day topics related to vortex motion is [13].

[11] We refer to [67] for a concise survey of research in fluid mechanics in the first part of the 20th century.

[12] A nice example in this context is the publication in 1906 by the Italian scientist Da Rios of the dynamical equations for the global behaviour of vortex filaments. This paper fell into complete oblivion, to be only rediscovered in the 1960s [192].

[13] See [230] for a history of boundary-layer theory.

[14] Lamb inserted one (short) section on turbulent motion in the second edition (1895) of his *Hydrodynamics* (see §5.1). This was kept unaltered up to the last, 6th, edition of 1932.

[15] Unfortunately, no (comprehensive) review seems to exist on the development of vorticity theory in the 20th

of the topics presented in this Interlude has been guided by the subjects to be treated in the vorton-part of this thesis. In §A developments in experimental and analytical treatment of vortex rings will be discussed. The last three sections contain an introduction into the role of vorticity in modern fluid mechanics: §B on turbulence, §C on topological fluid mechanics, and §D on vortex methods.

A Vortex Rings

In this section we will present some of the 20th century developments in the experimental and analytical treatment of vortex rings. Although their use in theories of matter or ether had become almost completely abandoned by 1900, their recognition as the most fundamental (closed) vortex structure caused a continuing interest in their characteristics.

A.1 Experiments

At the end of the 19th and the beginning of the 20th century, the essentials of experimental work on vortex rings were largely based on the first achievements by Tait [16] though the execution of the experiments became more sophisticated. During the 1870s, Ball, professor of applied mathematics and mechanics at the Irish Royal College of Science, had received a grant from the Royal Irish Academy to develop a machine for producing smoke rings. In his highly impressive experiment he studied the retardation of vortex rings due to viscous diffusion (see e.g. [19]).

In 1901, Wood, then instructor at the Physical Laboratory of the University of Wisconsin, published several experiments with smoke rings [285] and one of his pictures (see fig.a) showed the "fusion of two rings moving side by side into a single large ring. ... At the moment of union the form of the vortex is very unstable, being an extreme case of the vibrating elliptical ring. It at once springs from a horizontal dumb-bell into a vertical dumb-bell ... and then slowly oscillates into the circular form ...". This result seems to be the first description of an important aspect of vortex dynamics, which has only recently got more serious attention: vortex reconnection (see §C below).

In 1911, similar results though from a more sophisticated experiment on vortex rings were published by Northrup, then at Princeton University (see [166] for full details and [167] for a summary of his results). Introducing his investigations, he remarked:

> It seems strange ... that though the laws of vortex motion were exhaustively examined by the ablest mathematicians of the time, few if any experiments were made to study vortex motions in air and fluids, beyond the first experiments with smoke rings. ... The experiments which are about to be described would, if made earlier, have possibly had a greater interest as bearing upon Lord Kelvin's ingenious theory of the vortex atom. [166, p.213]

Northrup had constructed an ingenious "gun" with which he could make vortex rings in almost any initial configuration he wanted. He was even able to make beautiful stereoscopic

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century. To get an impression of the development in this field, one could consult bibliographies like the *Royal Society of London Catalogue of Scientific Papers 1800-1900 (Cambridge, 1909)* and the annually published *Annalen der Physik und Chemie* and *Die Fortschritte der Physik*. For developments in the last three decades we can refer to the reviews which have been published in the *Annual Review of Fluid Mechanics*.

[16] An alternative method to produce rings was suggested by J.J. Thomson. In 1885 he presented results of experiments he had done together with Newall at Cambridge. They noticed that a drop of ink became unstable when it fell into water and secondary smaller rings developed [240]. This work on rings formed from liquid drops was only reconsidered in 1966 [34].

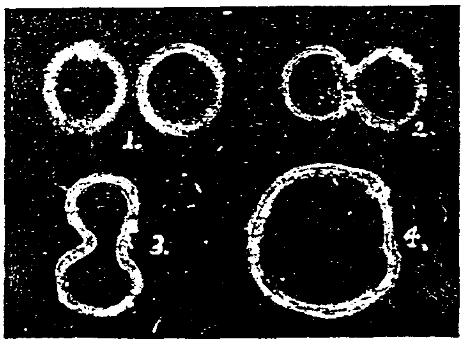

Figure a: Illustration of reconnecting vortex rings from Wood's 1901 experiment [285]. The rings are moving away from the reader.

photographs of the interaction of these rings. Besides experiments on elliptical rings and their instability, Northrup repeated Wood's experiment on the interaction and subsequent reconnection of two initially parallel rings. His sketch of the process (see fig.b) was accompanied by the following comment:

> In Fig.[b] *a* shows two rings a few centimeters from the gun. They are shown in side view, moving in the direction indicated by the arrow, *m*. The velocity of the fluid *in reference to the rings* is indicated at three points by the arrows 1, 2, 2^1. Since this velocity is greater at 1 than at 2 and 2^1, the points on the circumferences of the rings which are adjacent lag behind the points which are opposite, and consequently the planes of the two rings begin to tilt forward in the manner indicated.
>
> Furthermore, as the velocity of the fluid is greater at 1 than at other points equally distant from the filaments of the rings, here will be acting a pressure, according to Bernoulli's principle, which will tend to force together adjacent points of the circumferences of the two rings. Hence, a moment later the rings will assume a position indicated at *b*. The rings should now be viewed from behind, when their form will be (at a very brief instant later than shown in *b*) as indicated in full line at *c*, or in dotted line at *d*, which is a side view. The high velocity of the fluid at the point of contact has caused the two rings to unite and assume the form of a figure 8, with its upper and lower ends greatly tilted forward in the direction of motion of the now single ring.
>
> As this distorted single ring has everywhere an equal tension along its filamentary line, it tends to assume a circular form and lie in a plane normal to the forward direction of motion. But in changing its form to assume the circular plane ring form it overshoots this equilibrium position and assumes, as seen from behind, the form shown at *d* in heavy line. This double oscillation about the form of equilibrium now continues, and the ring advances ... [166, p.366-8]

It seems that only in 1939 the next extensive experiments on vortex rings were performed, in Germany by Krutzsch [112]. Krutzsch discovered that instability of rings led to a pattern of

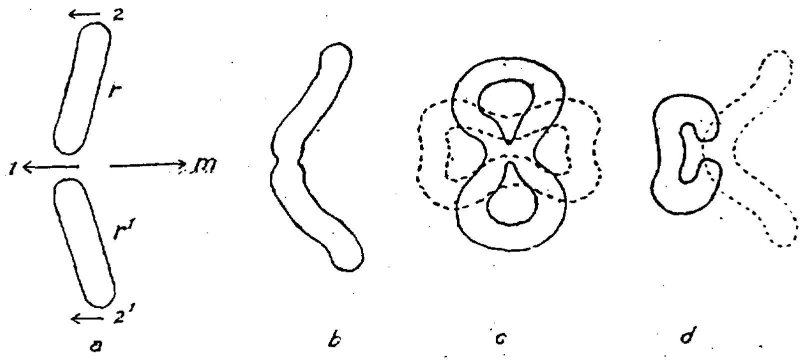

Figure b: A sketch of reconnecting vortex rings, drawn by Northrup in 1911 to illustrate his experimental results [166] (see text).

sinusoidal variations of the ring's radius, called azimuthal instability; see fig.c. The following war period seems to have prevented continuation of his work. Not before the 1970s experiments

Figure c: Instability of a vortex ring in the form of an azimuthal wave disturbance as found by Krutzsch in his 1939 experiment [112].

on vortex rings seem to have been resumed. Although the equipment had become even more sophisticated, researchers were still mainly concerned with the classical configurations: the head-on collision of two vortex rings (as Helmholtz had already discussed; see Chapter 2), the reconnection of two initially parallel vortex rings (as had been done in the experiments by Wood and Northrup), and the leap-frog effect. The latter, which had already been predicted by Helmholtz in 1858, was first demonstrated experimentally in 1978 [287] [17].

A.2 Analytical Treatment

The analytical work on vortex rings by Kelvin and others in the last three decades of the 19th century had been impressive and their results were generally regarded as physically well-

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[17] Discussion of more recent experimental results will be postponed to Chapter 10, where the simulation of six vorton configurations is treated.

founded for several cases. However, in their derivations the following properties of the rings had been assumed (compare fig.2.3):

- uniform vorticity distribution in the core and no vorticity outside the core;
- circular core;
- core radius a small compared to the ring radius R: $a/R \ll 1$.

A vortex ring satisfying these conditions will be called a **Kelvin-ring** in this thesis. One started to realize that in most circumstances these "ideal" rings would be only very approximate representations of real vortex rings [18]. However, mathematical techniques were lacking for fuller treatment and the investigation of vortex rings whose conditions differed from those of the Kelvin-ring only started in the 1970s (see e.g. [276] and [205] for a survey).

One of the most elementary results on the vortex ring had been its velocity, for which Kelvin's expression (4.3) became a landmark. Hicks, who had been one of the most dedicated followers of the vortex atom theory and the main propagator of the hollow vortex ring (see §5.1), had confirmed the correctness of expression (4.3). However, he also showed that for his hollow ring (see §5.1) the factor $\frac{1}{4}$ had to be replaced by $\frac{1}{2}$, thereby providing a direct proof of the influence of the vorticity distribution in the core [82]. Further analytical confirmation of Kelvin's result appeared in an impressive paper of 1893 by Dyson, communicated by J.J. Thomson [54]. Dyson extended the expression to rings of non-circular cross-sections and found a higher order error estimation of expression (4.3). In 1914 Gray again confirmed Kelvin's result [69], but only in 1970 the velocity of a ring of small cross section with arbitrary distribution of vorticity within the core was derived for the first time (see [276] and [205] for details). For an arbitrary distribution of vorticity in the core (but still a small and circular core), it was found that the ring velocity could be written as:

$$V = \frac{\Gamma}{4\pi R}(\log\frac{8R}{a} + A - \frac{1}{2})$$

where the factor A depends on the vorticity distribution only. For uniform vorticity $A = \frac{1}{4}$, confirming Kelvin's result [19].

The attempts by Thomson in his *Treatise* of 1883 (see §5.1) had shown the limitations of analytical treatment of the interaction of vortex rings [20]. Subsequent analytical research had necessarily been limited to the relatively simple cases of head-on collision and leap-frogging of coaxial rings. Thomson's followers also realized that only configurations of coaxial vortex rings

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[18] In 1888, Chree [38] had already shown that cores of vortex tubes may not remain circular.

[19] J.J. Thomson in his *Treatise* (see §5.1) had proposed a factor 1 [234], but Hicks convinced him that he was wrong [82]. Experimental confirmation of the above equation for V was only tried by Sullivan *et al.* in 1973 (see [276]); they found differences within 20% of the theoretical values. In the last few decades expressions have also been found for unsteady rings, for compressible rings, viscous rings, and rings with swirl; see for a discussion [205, §10.3].

[20] Roberts [193] took up J.J. Thomson's analytical work on interacting vortex rings in 1972. He remarked that the interaction of rings are "strongly reminiscent of those given in standard texts for scattering under central forces", which suggested to him that it could be described from the standpoint of classical Hamiltonian dynamics of interacting particles. However, Roberts noticed that rings cannot be compared to elastic particles since a ring has an infinite number of degrees of freedom (compare Maxwell's remark mentioned in §5.3.4). However, if the separation of the rings is large compared to their diameter, collision is elastic and for this case Roberts presented a Hamiltonian formulation. He showed that J.J. Thomson had made an error in his derivation, which did destroy the Hamiltonian character of his final results.

would be feasible for analytical treatment. Dyson's paper (see above) contained fundamental results on the leap-frog interaction and on the head-on collission of two Kelvin-rings. For the latter, he (like Helmholtz in 1858) recognized that it was equal to the interaction of a single ring with a "fixed plane"; in this case the ring interacted with its "mirrored" image on the other side of the plane. By means of impressive calculations Dyson was able to provide an exact analytical expression for the trajectory of the core center of the ring approaching the plane. Besides, he derived an equation for the rate of change of radius R when the ring had approached the plane closely:

$$\dot{R} = \frac{\Gamma}{2\pi R}$$

where Γ is the circulation. For the relation between R and the distance d between the ring and the plane, he found the curve shown in fig.d. For leap-frogging rings he derived conditions for the unbounded continuation of the process. In 1922, Hicks [88] would also investigate the leap-frog interaction and head-on collision. However, his work once again demonstrated the strong restrictions imposed by the available analytical techniques and the need for new methods to handle vortex interaction. Only the advent of numerical techniques would make an important progress on this point possible (see §D below).

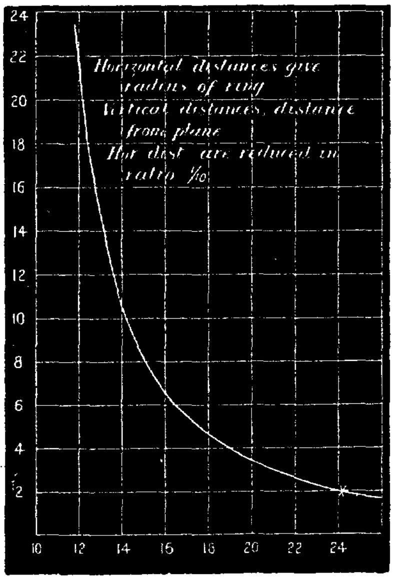

Figure d: Dyson's figure showing the relation between the radius R (*horizontal*) and distance d (*vertical*) of a vortex ring approaching a "plane". From [54].

A.3 Steadiness and Stability

As discussed in §5.3.1, proof for the steadiness and stability of the vortex atom became an important issue in the discussion of the model. Regarding steadiness, only attempts by Kelvin himself have been traced and his results on this issue, as on other ones, lack sufficient rigour. Apparently, a real proof for the existence of steady Kelvin-rings was only given by Maruhn in 1957 and Fraenkel in 1970, though the question remains a subject of study (see e.g. [7]).

Regarding stability, in 1875 [248] Kelvin was still convinced that the Kelvin-ring was a steady and stable configuration. By 1905, however, he had changed his opinion and realized that it was "essentially unstable" (see §6.2). His main attempt with regard to this issue had been his 1880 paper [250] on the stability of "columnar vortices" by means of a dispersion relation for linearized perturbations [21]. However, he had to admit that he was unable to give a proof [22]. Thomson had tried to show that up to $O(\log a/R)$ all modes of oscillation are stable and that each mode had a definite natural frequency [234, §13].

Others also came to recognize that no convincing proof existed on the stability of vortex rings. In 1885, Lodge [130] analytically investigated a ring moving in a "oblique direction" and found "a good deal of vibration, both of the ring as a whole and of its cross section; and it looks as though a very little would suffice to break it up altogether." In his lectures on vortex theory of 1893 (see §5.2), Poincaré showed that there were no sufficient guarantees for the stability of the vortex atom [180].

As for vortex ring experiments, new investigations on their stability only reappeared in the 1970s. Maxworthy [149] has showed experimentally that a laminar ring is only stable at low Reynolds number [23] though stability of the ring configuration seems to set in again when the core becomes fully turbulent. The instability of the laminar ring takes the form of bending waves around the perimeter, and these waves grow in amplitude as time proceeds. Some of these "steady modes" had already been drawn by Kelvin in 1875 (see fig.5.1) and had been observed in the experiment by Krutzsch (see fig.c). Maxworthy attributed this instability to the method of generation of the rings and did not regard it as an intrinsic property.

However, the analytical work by Widnall and co-workers on the stability of vortex rings (or locally curved vortex filaments in general) showed that even for inviscid rings instability can arise regardless of their generation (see [277], [278], [279]). They showed that the proper treatment of the internal structure of the flow within the core as influenced by bending waves is crucial to stability analysis. This was something which both Kelvin in his investigation of the columnar vortex [250] and Thomson [234] had not considered [24].

Notice, however, that Widnall's analysis is based on the Kelvin-ring. Studies on the stability of more general vortex rings representations, e.g. with nonuniform core distributions, are still lacking and may only be possible by means of numerical methods (compare §10.1.2 below).

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[21] It appears that Kelvin's work was experimentally investigated only in 1989 by Vatistas [272]. He found unstable transitional regions between equilibria, which Kelvin had not noticed.

The perturbations studied by Kelvin are nowadays called Kelvin waves [205, §11.3]. The variational principle proposed by Kelvin in [248] has again been applied in the 1980s [205, §14.2].

[22] See [194] for a recent review of Kelvin's approach.

[23] The Reynolds number can be defined by:

$$Re \equiv \frac{UD}{\nu}$$

where U is a characteristic velocity, D is a characteristic dimension, and ν is the kinematic viscosity. For its history, see [200]. For vortex structures, another definition of Re can be found: Re $= \Gamma/\nu$, where Γ is the circulation.

[24] Ironically, Kelvin's analysis for the neutral waves can be used as one of the ingredients to a simplified demonstration of the instability mechanism [212].

Recently, Lifschitz in [33] investigated stability by another method. His geometrical optics approach allows to describe short wavelength instabilities, which "play an important role in many situations". Whereas Widnall's theory was only applicable to thin rings with constant vorticity in the circular core, Lifschitz could study thicker rings. He showed that all laminar rings are unstable.

B Vorticity and Turbulence

Without doubt, at the time of the birth of the vortex atom (1867) the phenomenon of turbulence had already been encountered in many flow configurations, but it certainly had not become an object of study. In 1883, Reynolds [191] had been the first to investigate by experiment the transition from laminar to turbulent flow and his work meant an important stimulus to research on turbulent motion [25]. Kelvin's 1887 paper on a possible model of the vortex ether [256] (see §6.1) had originally been entitled "On the propagation of laminar motion through a turbulently moving inviscid liquid" and had been an attempt to investigate "turbulent motion of water between two fixed planes" [26]. In 1889, FitzGerald [59] reacted to Kelvin's paper by an investigation of turbulent motion and possible analogies with electro-magnetic equations. He remarked that desintegration by diffusion of turbulent flow (as represented by Kelvin's model shown in fig.6.1) could be avoided "by supposing the turbulent liquid to consist of interlocked vortex rings, or of infinite intercrossing lines ..." [59, p.34] [27]. However, Kelvin's crude vortex ring model of a turbulent motion was not based on any physical knowledge of turbulence, which around 1887 was still very limited and mostly experimental.

This connection between turbulent motion and vortex motion followed the fate of the vortex ether itself and dropped from the general attention at the beginning of this century. Subsequent attempts to study turbulence from the general equations of fluid mechanics showed that the mathematics needed for full (analytic) treatment of turbulent flow was too difficult to handle, and approximations had to be made. This necessitated the use of physical insight. Prandtl's mixing-length model, introduced in the 1920s, was based on an analogy with the kinetic gas theory [28]. However, the prewar period was mainly a period of mathematical modelling and of experiments. Only in 1932, Taylor [231, Vol.II,§24] developed a theory in which the dynamics of turbulent motion was regarded as an effect of diffusion of vorticity. He showed that his suppositions led to better agreement with experimental results than the diffusion of momentum theories popular at that time [29]. In 1938 [231, Vol.II,§41], he showed the importance of the stretching of vortex filaments in turbulence.

Regarding contemporary literature on turbulence, we can conclude that Kelvin's and FitzGerald's original idea of representing turbulent motion by arrays of vortex rings has returned. Already in 1943 a similar attempt at modelling turbulent flow by restricting attention to the influence of vorticity was made by Synge & Lin [225]. They tried to derive the statistical characteristics of turbulence from a model consisting of interacting vortices. Their initial choice for vortex rings, however, had "undesirable features" and the authors turned to a model involving "spherical vortices". Since this attempt, many researchers have been incited to building turbulence models in which the basic concept is some kind of vortical structure [30].

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[25] Unfortunately, as for the theory of vortex motion, no historical survey of the early history of turbulence research has been traced.

[26] Kelvin seems to have actually introduced the term "turbulence" [115, 4th ed.,§366]. Reynolds, in 1883, had spoken of "sinuous flow".

[27] The idea of representing turbulent motion by means of a system of vortex rings has been a topic ever since. One example is Roberts's model [193], mentioned above. More recently, Aref & Zawadzki in [16] wondered whether turbulence can be described as a "gas" of vortex rings.

[28] Notice that this meant a shift in the use of analogy in turbulence research from the vortex atom theory to the rivalling kinetic gas theory.

[29] Nowadays, this diffusion theory is no longer accepted.

[30] One of the latest models is by Lundgren [136], to whom we also refer for a discussion of preceding theories. One of the central issues in the investigation of "vortex models" has been the quest for the correct energy spectrum. The energy cascade at high wave numbers is believed to be independent of viscosity, so the classical

The purpose of the models mentioned above has been to obtain a simplified view on turbulence and to derive some of its essential properties [31]. The vortical elements were not supposed to be physical models of real structures in turbulent flows. Today, however, it has become clear that vortical structures are indeed present in turbulence. To elucidate this development, we will first give a short historical review.

In the 1930s the main treatment of turbulence, regarded as a random fluid motion, had become statistical. However, during the 1940s and 1950s the suggestion arose that besides the random part also a non-random part existed. A growing amount of experimental results, amongst others the observation of the so-called intermittency, led to the idea of the existence of "structures" in turbulent motion. Moreover, these so-called **coherent structures** (CS) were defined as regions of relatively high vorticity. Hence turbulence became envisaged as a number of interacting vortex structures. Especially in transitional flows, vortices were supposed to play an important dynamical role. By the 1970s this view of turbulence had become generally accepted [32]. However, it also became clear that, as Betchov in [60] remarked, "it is not the mere presence of vorticity that characterizes turbulence. It is the complexity of the vorticity field. In a laminar boundary layer, the vortex lines are parallel and stacked near the wall, like uncooked spaghetti. In the turbulent boundary layer the vortex lines are constantly changing and twisting. Near the wall, major entanglements appear, and the vortex lines may develop knots and crossover points. The spaghetti is cooked."

Although nowadays the existence and importance of CS is generally recognized and has been investigated both experimentally and numerically, still consensus is lacking on many aspects [33]. Several different structures have been proposed, but limited quantitative evidence hinders demonstration of the role these structures play. According to Kline & Robinson in [73], three main issues in the present research on CS can be detected: "spatial relationships among the forms of structure; temporal relations in creation, evolution, and decay of structures; a complete model of the important structure(s)".

Even a generally acceptable definition of CS still seems remote and may even be unattainable. Besides, it is still debated whether CS are the remnants of some kind of instability process or whether they are manifestations of some intrinsic universal properties of any turbulent flow. Up to now, research on CS has been done only for transitional or rather low Reynolds number flows and the question has been raised whether CS will survive in "fully developed" turbulence, i.e. at high values of Re. Unfortunately, research on CS is hindered by the difficulties involved in the direct measurement of vorticity in a flow. In numerical research the main problem is a lack of detection methods of CS [196, Ch.9]. However, it is generally agreed that both experimentally and numerically the importance of CS has been established and for the moment we will therefore disregard these problems.

On the close link of CS with vortex dynamics, we quote from the contribution "Whither coherent structures?" by Bridges *et al.* in [135]:

Kolmogorov 5/3-spectrum may be obtained from interaction of inviscid vortex filaments. Kiya & Ishii applied an inviscid vortex method to show how only a few vortex rings, arranged symmetrically on a cube, can produce a Kolmogorov energy spectrum [105]. Moffatt, in [135] and [16], has proposed another model to attain the same results. He suggested that a random distribution of spiral structures (rolled-up vortex sheets) shows a Kolmogorov spectrum.

[31] See the Epilogue for a discussion of models.

[32] See [196, Ch.2] for a historical review of turbulence structure experiments.

[33] The literature is overwhelming and we only mention [92]. A more recent topic in turbulence, related to that of CS, is the study of the structure of vorticity in (isotropic) turbulence; see e.g. [95].

> What troubles us most is our inability to embody information gleaned from the experimental studies of CS into a mathematical framework. We feel that providing a mathematical basis for the CS concept will be the topic of considerable effort for years to come and will bring about a much better *understanding* of turbulence [i.e. explanation, prediction, control]. ... Given the topology of the vortical CS we can say roughly how it will evolve and interact with other CS. This is an advantage not held by other specific definitions of CS. Vortex dynamics gives local, short-term predictability of the dynamically important aspects of the flow. ... If CS are defined by vorticity, their evolution and interactions are directly connected to their topology through vortex dynamics. This is why it is important to categorize CS morphologically. ... Vortex dynamics is the missing mathematical framework for the study of CS. ... Using vorticity to define CS also allows us to predict flow evolution in complicated flow situations using intuition ... instead of having to resort to direct calculation ... . [135]

Although this view on the role of vortex dynamics in research on CS seems reasonable, we should also realize that the deformation and interaction of CS may well be a much more complicated matter than in case of "ordinary" vortex structures like vortex rings. For example, Melander *et al.* in [216] have mentioned five categories of close CS interaction: self-deformation of a single isolated CS, including effects of turbulence it may generate; interaction of a single isolated CS with background potential; interaction of a single isolated CS with turbulent background; isolated interaction of two CS in very close proximity; and isolated interaction of two CS in the presence of a turbulent background. To which they remark: "Only a thorough insight into the dynamics of key vortex interactions can further the present level of understanding of turbulence and the role of CS."

C Vorticity and Topological Fluid Mechanics

Kelvin in his paper "On vortex motion" (see §4.1) was the first to perceive dimly the bridge between mathematical topology and classical fluid mechanics. In this paper he had explained how to avoid the consequences of multiply continuous spaces [34], probably intending to show the possible existence of the many varieties of the vortex atom; see fig.4.1.

Induced by Kelvin's still primitive results, Tait decided to classify and catalogue all knots of increasing order of complexity [227](1876). This catalogue of knots remains as the cornerstone of knot theory, now a well-established branch of topology [35].

Only recently, the link between topology and fluid mechanics has resulted in the field of "topological fluid mechanics", which today seems to be a firm branch of fluid mechanics (see e.g. Moffatt's lecture in [160] and [162]) and has partly been stimulated by the research on CS.

In this new field an important role is played by the so-called helicity field. In the 1960s Moffatt [157] discovered that the topology of vortex structures is closely linked with one of the

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[34] Kelvin adopted terminology introduced by Riemann, known to him through Helmholtz. Although the theory he developed here has never been applied to the vortex atom theory or any related subject, Kelvin's solution of the problem (which had been posed by Helmholtz) of extending Green's theorem to multiply-connected regions was certainly a high mathematical record of this paper. His proof technique is still used today, which shows an unsuspected heritage of this paper.

[35] See Millett in [162] for a historical account of the development of knot theory and Tait's role.

motion-invariants of the Euler equation, i.e. **helicity:**

$$H \equiv \int_V \boldsymbol{w} \cdot \boldsymbol{v}$$

where V is the volume in which the fluid flow under consideration is taking place. Conservation of helicity means that the "topology" of vortex structures

in the flow remains unchanged, something which Kelvin had already recognized in his paper "On vortex motion" of 1869. In viscous flows changes of topology can take place, and H is no longer a motion-invariant. Helicity may also be of profound importance in the non-linear *dynamics* of turbulence, e.g. to characterize organized or coherent motion in turbulent flows. This supposition has been extensively studied and propagated by e.g. Levich [124], but others found opposing results (see e.g. [275]). Lack of experimental results hinders reliable opinions or conclusions [36].

One important aspect of modern research in the topology of vortex structures concerns vortex reconnection.

The experiments performed by Wood and Northrup at the beginning of this century (see §A.1 above) had already shown that vortex rings may become united into a single structure. Today this phenomenon is indicated by the terms cut-and-connect or **reconnection** [37]. However, the importance of vortex reconnection, of which the linking of vortex rings is one example, was not realized until many years later. During the last few decades, as computer technology and numerical possibilities rapidly advanced and new aspects of reconnection were discovered, interest grew quickly [38].

It is now generally accepted that vortex reconnection is only possible when viscous diffusion of vorticity is present [39]. See fig.e for a elementary sketch of the reconnection process and compare with Northrup's sketch in fig.b. In the initial stage of the reconnection, vortex tubes

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[36] We refer to [161] for a review of helicity in fluid mechanics.

[37] We will stick to the use of the term "(vortex) reconnection" throughout this thesis, but recognize that the term "cut-and-connect" sometimes is a better description of the phenomenon discussed.

[38] An interesting revival of the use of analogies between hydrodynamical and electromagnetic phenomena, initiated by Helmholtz and Kelvin, can be detected in the present interest of fluid mechanicists in some parts of research from magnetic field theory, where helicity and reconnection have also become familiar concepts.

Berger and Field [24] showed that magnetic helicity is also closely associated with many aspects of topological structure of the magnetic field. In plasmas with high but finite magnetic Reynolds number, it has been conjectured that reconnection of field lines can alter the field topology, while approximately conserving helicity, as in fluid mechanics. This suggests that helicity is not a good indicator for change in topology.

Greene in [160] has applied mathematical techniques from the theory of magnetic reconnection, "perhaps slightly better understood" than vortex reconnection. However, Hegna & Bhattacharjee, also in [160], notice that for their treatment of magnetostatic equilibria, the analogy between magnetic fields and Euler (i.e. inviscid) flows is only valid up to a certain point.

[39] The theory of viscous vortex motion had already started during the time of the vortex atom, initially because some had argued that the theory was based on inviscid flow, whereas the experiments obviously produced viscous vortex rings.

In 1879 [233] Thomson had treated the vortex equations for viscous fluids and remarked the analogy with the equation for the conduction of heat. He concluded that any vortex motion in an initially irrotational flow must come from the boundary of the fluid: "if vortex motion be set up in any part of a viscous fluid, the motion throughout the fluid immediately becomes rotational". Reynolds [188] had observed that contrary to Kelvin's inviscid picture, the volume of the "vortex ring bubble" continually increased due to entrainment of external irrotational fluid and its velocity decreased because its momentum has to be shared with a greater mass of fluid. Lodge in [130] (see §6.1) had shown analytically that a viscous ring showed a decrease in velocity and an increase in size.

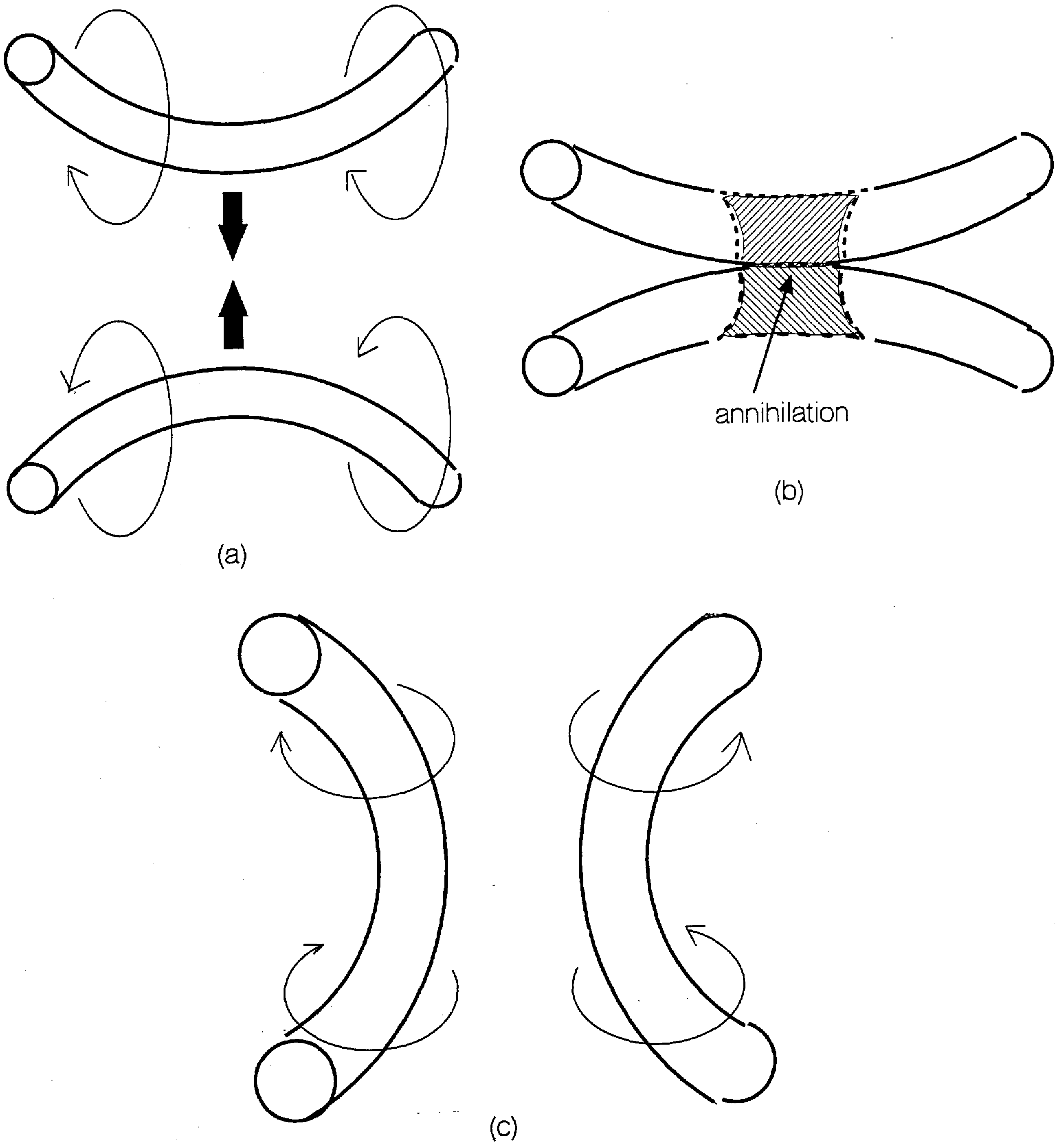

Figure e: Reconnection of two vortex tubes: (a) Alignment of oppositely directed vorticity; (b) Annihilation by viscous diffusion of oppositely directed vorticity in anti-parallel aligned vortex tubes; (c) Formation of new connections (bridging).

of oppositely directed vorticity show **alignment**. Alignment is the tendency of approaching vortex tubes which initially are positioned in a random orientation with regard to each other, to become aligned with the direction of the vorticity vector in both tubes directed anti-parallelly; see fig.f. Then, annihilation of vorticity takes place by diffusion. In the third stage new connections are formed, a process which has been called **bridging**. Two of the main aspects of reconnection which have been generally recognized are the deformation of the core of the vortex structures and the influence of local strain on the reconnection process. Recent research suggests that reconnection results in a rapid local increase of the strain rate and vorticity. Shelley & Meiron in [10] have suggested that core deformation plays an important role in the process. For example, it has been shown that deformation of the core effectively prohibits unbounded growth of vorticity during reconnection.

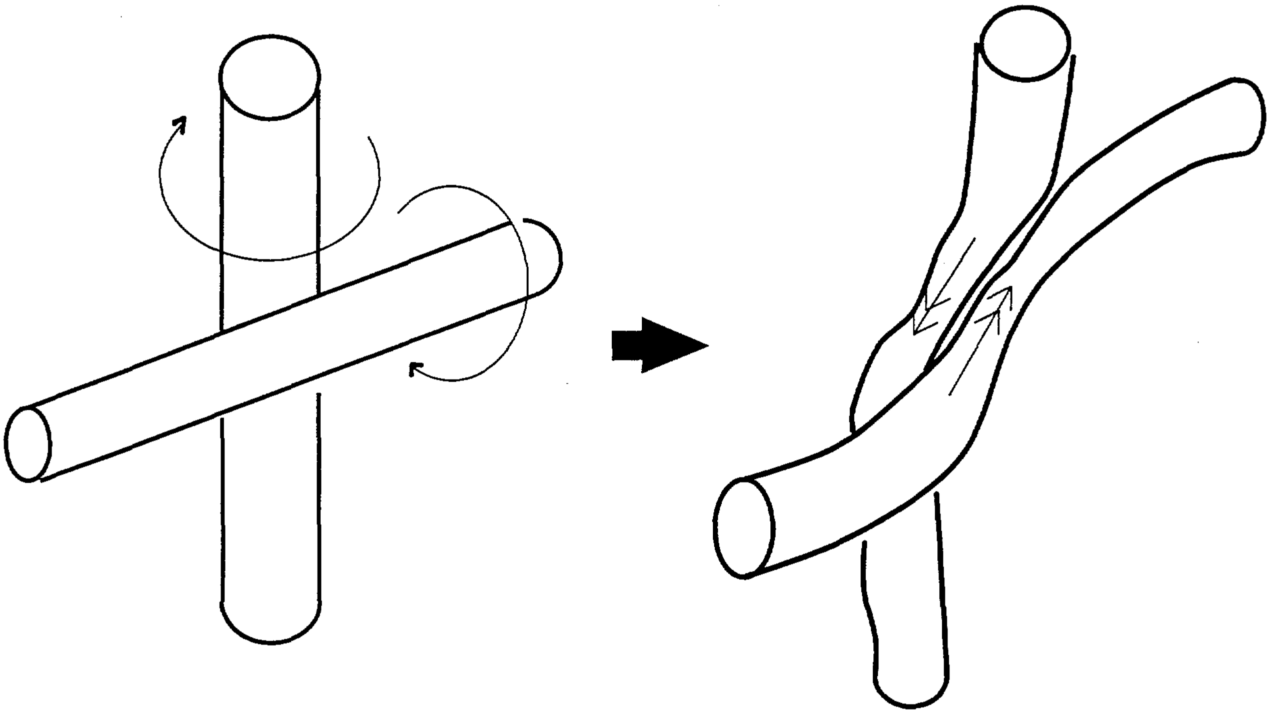

Figure f: Anti-parallel alignment of vortex tubes. Arrow indicates time development. Double arrows indicate direction of vorticity.

However, the exact mechanism of reconnection remains unclear. Most research on this phenomenon has been done by means of numerical simulation of generic test cases: two segments of vortex tubes, placed either orthogonally or anti-parallel (see e.g. [27]).

Analytically, reconnection is hard to treat. The only analytical model known is the one by Saffman [204], who found some agreement with experimental data, but had to admit that

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Due to an early paper by Bobylew of 1873 [26] it was realized that whereas Helmholtz's first theorem was valid for real flows, the second theorem was not and vorticity could both be generated and destroyed by viscous effects, which led to discussions on the origin of vorticity in initially irrotational flows [134, §4.6]. Furthermore, it was realized that in viscous flows circulation was not preserved and the Helmholtz equation (2.2) had to be adapted to take viscous diffusion into account.

The study of viscous vortex rings seems to have been only taken up in the early 1970s, especially by Maxworthy [148].

some large discrepancies exist and that the model contains "questionable choices". Analytical work by Takaki & Hussain (see [13]) showed that reconnection takes place within a time scale of the order of the convective time scale rather than of the diffusive time scale [40]; nevertheless, they concluded that viscosity is necessary in the process.

Some have suggested that vortex reconnection is an important aspect of turbulence. It has been proposed as the mechanism for isotropization, for the production and dissipation of vorticity in the flow, for the so-called energy and enstrophy cascades, and for the production of helicity. However, on this issue opposing views exist. Ashurst & Meiron [18] speculate that reconnection may occur in a turbulent flow whenever two opposite-signed vorticity regions approach each other, whereas Boratav & Zabusky remark in [162] that it has not been proved whether reconnection really takes place in turbulent flow fields at all.

Unfortunately, since reconnection may occur randomly in space and time in a turbulent flow, it is hard to investigate experimentally or by means of direct numerical simulation (DNS). Therefore, our understanding of reconnection in turbulence is still limited and scattered [41].

D Vortex Methods

In one of his *Lectures on some recent advances in physical science*, first published in 1876 [229] (see §5.1), Tait noticed the enormous mathematical difficulties involved in the elaboration of the vortex atom model. As an example, he thought that the interaction of two rings positioned non-symmetrically about an axis "may employ perhaps the lifetimes, for the next two or three generations, of the best mathematicians in Europe; unless, in the meantime, some mathematical method, enormously more powerful than anything we at present have, be devised for the purpose of solving this special problem" [229, p.302]. The attempts by Thomson in his 1883 *Treatise*, and by some others (see §5.1), indeed showed that analytical treatment of this topic could only be achieved if severely restrictive assumptions were made.

One way to avoid unsurmountable mathematical difficulties is a simplification towards two-dimensional (2-D) flow. Kelvin himself had already realized that for the demonstration of the stability of single or interacting vortex atoms, a reduction of the configuration towards infinitely long, straight, and constantly parallel "columnnar vortices" would improve the possibility of analytical treatment (see §5.3.1). In [249] he had discussed regular configurations of such columns, apparently without knowledge of the work that the German Kirchhoff had done some years before [42]. Kirchhoff had shown that the dynamics of these vortices, assuming an infinitesimally small core size, could be described by a Hamiltonian set of equations. In the second half of the 19th century, this subject was taken up by several contemporaries, e.g. by Gröbli [14].

The theory of these essentially parallel vortex filaments, or point-vortices as the 2-D cross-sections of these filaments with a flat plane perpendicular to their axes are called, came to be used in the 1920s and 1930s during the first attempt to solve flow problems by means of discretization of a continuous vortex structure. In 1931, Rosenhead [199] approximated a 2-D vortex sheet [43] by means of point-vortices, and in this way he was able to simulate the roll-up of such sheets. This attempt can be called one of the very first attempts to apply a

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[40] This timescale is related to a typical convective velocity in the flow.

[41] See Caflisch in [32] and Boratav & Zabusky in [162] for a short review.

[42] See for references e.g. [14]. In his 1858 paper Helmholtz had also shortly treated the theory of parallel vortex filaments [75, §5].

[43] A vortex sheet is essentially a surface formed by vortex lines. It had already been described by Helmholtz in his 1858 paper, as Rosenhead remarked.

vortex method, i.e. to simulate the dynamics of vortex structures by means of discrete vortex elements. This enabled the investigation of vortex configurations which experimentally and analytically had been beyond any hope for solution.

However, vortex motion in two dimensions is essentially different from that in three dimensions. As can directly be seen from the Helmholtz vorticity equation (2.2), 2-D vortices do not deform. As it was realized that vortex stretching was an important mechanism in vortex dynamics, interest in the treatment of 3-D vortex motion started to draw attention.

Tait's "enormously more powerful" mathematical method which would finally bring new progress (both for 2-D and 3-D vortex dynamics) was the application of numerical techniques to solve the vorticity equations on computers. The advent of powerful computers in the 1970s made the use of so-called vortex methods a wide-spread tool in fluid mechanics with which the research into interaction of vortex structures (such as vortex rings) could be and indeed was revitalized. Not only did they enable the investigation of finer details of the interaction, also configurations could be investigated which had been inaccessible by experimental means.

The general set-up and a short survey of vortex methods will be the subject of the next chapter.