The Vortex Atom:
A Victorian Theory of Everything

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> The particles embedded in the Ether are not independent of it, they are closely connected with it, it is probable that they are formed out of it: they are not like grains of sand suspended in water, they seem more like minute crystals formed in a mother liquor . . . [The ether] is the primary instrument of Mind, the vehicle of Soul, the habitation of Spirit. Truly it may be called the living garment of God. (Lodge 1925, p. 39).

1. Introduction

During the long history of conceptions of matter, the tension between continuum and corpuscular theories has been a persistent theme. It forms a pair of opposing or complementary concepts, an invariant idea that can be followed since Greek antiquity (Holton 1988; Kragh 1987, pp. 83–87). Matter seems to be corpuscular and solid, but may it not be possible to explain the individuality of elementary particles as manifestations of an all-pervading cosmic fluid? If such a fluid is assumed, it may itself be regarded as a macroscopic effect of minute particles in rapid motion. Alternatively, one may ascribe ontological priority to the continuous fluid, that is, regard it as an ultimate and irreducible quality of nature. The latter assumption formed the basis of the vortex theory of matter that was widely popular in Great Britain during the Victorian era. According to this theory, the material world was

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constituted of atoms that were but particular kinetic manifestations of an all-pervading, homogeneous and perfect fluid. The vortex theory was a *hydrokinetic* theory of matter. The fluid was often identified with the luminiferous ether, but the theory did not rest on any such identity. In fact, in the early version of the theory the ether played no explicit role.

The vortex atom theory has often been seen as part of a Cartesian tradition in European thought, which is justified in a general sense but not when it comes to details. In spite of the similarities, there are marked differences between the Victorian theory and Descartes's conception of matter. Thus, although Descartes's plenum was indefinitely divisible, his ethereal vortices nonetheless consisted of tiny particles in whirling motion. It was non-atomistic, yet particulate. Moreover, the French philosopher assumed three different species of matter, corresponding to emission, transmission, and reflection of light (luminous, ‘‘subtle’’, and material particles). The vortex theory, on the other hand, was strictly a unitary continuum theory. As indicated by the contemporary literature, the vortex atom theory had a most prominent position in late-nineteenth-century physics, and especially so within British natural philosophy.[1] The theory of vortex motion in fluids originated in a work by the brilliant German physicist Hermann von Helmholtz and was investigated also by Continental scientists, but only very rarely did these relate their work to the vortex atom theory. This theory was very much a British one, originating in Scotland and soon diffused by mathematical physicists trained in the Cambridge tradition of natural philosophy. The most prominent of the vortex atom physicists were William Thomson, William Hicks, and J. J. Thomson. The only other country, where the vortex atom was favourably received, was the United States of America.

Aspects of the vortex theory of atoms have often been the subjects of historical analysis, but typically in relation to specific problems only. No historical account exists that covers all or even most aspects of this multifaceted theory. Moreover, when the vortex theory has attracted historical attention, more often than not it has been in connection with the development of ideas of electromagnetism, that is, with the electromagnetic ether associated with Maxwellian electrodynamics (e.g., Whittaker 1958, Schaffner 1972, and Stein 1981). The vortex atom was however primarily conceived as a solution to the age-old problem of the constitution of matter, and not of ether, and in this regard it has never received the more full attention that it might well deserve.[2] The object of the present essay is to present such a fuller exposition and to uncover some of the less obvious implications of the vortex theory. It seems not to be generally recognised that William Thomson's theory of vortex atoms was a serious and ambitious attempt of launching a reformed atomic theory, and that this theory, whatever its fate, should therefore occupy a prominent place also in histories of theories of matter.[3] That it was indeed such a theory will be fully documented in what follows.

The present essay starts with the origin of the theory in the 1850s and 1860s, and covers various areas in which it was used or expected to be usable. These include spectra, gas theory, gravitation, and the constitution of the ether. I pay particular attention to the promising but short-lived programme of vortex atom chemistry that was mostly associated with the works of J. J. Thomson. At about 1890, the theory had run out of steam and was abandoned by most researchers, including its founder William Thomson. Yet, at the very same time it experienced, if only indirectly, a kind of revival in the shape of the electron theory of matter. The similarity of the vortex atom programme and the electron, or electromagnetic, research programme is pointed out. I also place the subject within the context of philosophy of science, mainly by looking at the way in which the vortex atom theory was considered by contemporary and slightly later scientists and philosophers. As a theory with a fairly definite life-time, from 1867 to about 1898, it invites questions relating to the dynamics of theories. Why did the theory – patently wrong, after all – survive for so long? How did it compare with other theories? And, how did it fare with respect to experimental tests? I believe that the story of the vortex atom is not only of considerable historical value, as a contribution to the history of the physical sciences, but also that it may serve as an important case in discussions of theory change. However, this is not the place for a systematic exploration of these aspects. At the end of the essay, I briefly consider the heritage of the vortex atom, that is, certain traces of or similarities to it that can still be found in modern physics.

2. Inventing the vortex atom

Until 1867, William Thomson had not been much occupied with the constitution of matter in terms of atoms and molecules. But although he did not publish on this subject, privately it did concern him. The roots of his later ideas of a vortex constitution of matter atoms may possibly be found in a paper of 1856 in which he analysed magneto-optical phenomena. In this connection, he speculated "whether all matter is continuous, and molecular heterogeneousness consists in finite vortical or other relative motions of contiguous parts of a body" (Thomson 1856, reprinted in Thomson 1904, pp. 569–577, on p. 571). This was precisely the question he would answer affirmatively eleven years later, but at the time he did not further develop the suggestion.

Thomson's introduction of vortex atoms rested on two pillars, his fascination by hydrodynamics and his critical attitude to atomism. As to the first pillar, in a letter to Gabriel Stokes of December 20, 1857, he wrote, "Now I think hydrodynamics is to be the root of all physical science, and is at present second to none in the beauty of its mathematics." Three days later he expanded his remark in a long letter on the hydrodynamics of a perfect fluid. This concept, "which I first learned from you, is something that I have always valued as one of the great things of science, simple as it is, and I now see more than ever its importance." Thomson continued: "One conclusion from it is that instability, or a tendency to run to eddies, or any kind of dissipation of energy, is impossible in a perfect liquid (a fluid with neither viscosity nor compressibility)" (Wilson 1990, vol. 1, pp. 227–230). The notion of a perfect fluid was at the heart of the significant change that occurred in British thinking about hydrodynamics around 1850, when the science became based on abstract rather than real fluids (Yamalidou 1998). In this conceptual shift, Stokes was a key figure. The fluids that the new generation of hydrodynamicists studied were mathematical constructs with no particular microscopic constitution, just the kind of fluid that Thomson would need for his vortex atoms.

In his notebook of early 1859, Thomson rejected the Newtonian or Daltonian view of matter as being made up of indivisible, point-like atoms interacting at a distance through a vacuum by means of short-range forces.[4] Thomson was far more attracted to a continuum theory – "the doctrine of the Universal Plenum" – and speculated that the properties of gross matter might be explained by substituting for the discrete particles strains in an elastic fluid ether; or, as he phrased it, that matter could be conceived as manifestations of "motions or eddies in a fluid." However, at that time he saw no possibility of doing without particles, and therefore temporarily shelved the project. Thomson's dislike of Newtonian atomism may also be glimpsed from a somewhat cryptic remark he made in 1862 about the size of atoms. In this context, he added that the size should be understood as referring to molecular structures, for "I do not believe in atoms."[5] His friend Peter Guthrie Tait did not share Thomson's antipathy to atoms. In a letter of 1861 Tait objected that he was unaware of any continuum theory that could account for the corpuscular properties of matter. Interestingly, he mentioned specifically Helmholtz's theory of vortex motion, the very theory that six years later would serve precisely this purpose (Smith and Wise 1989, p. 379).

Helmholtz's theory of 1858, a pioneering work in mathematical hydrodynamics, originated in his acoustical research.[6] The German physicist and medical doctor was led to study the use of Green's theorem in problems of hydro- and aerodynamics and to determine the forms of motion that friction produces in fluids. This he did by analysing three separate kinds of motion of an indefinitely small volume of the fluid, namely, translation, expansion or contraction, and a rotation around some axis. Helmholtz defined what he called a vortex line, directed along the axis of rotation, and also a vortex filament. The latter was the tube formed by the vortex lines through every point of the boundary of an infinitely small closed curve. From these definitions, and pure mathematics, Helmholtz showed that vortex filaments or tubes in a frictionless fluid form closed rings or, if the fluid-filled space is bounded, may terminate in the bounding surface. In either case they are permanent in the sense that they cannot be either created or annihilated. Moreover, he demonstrated mathematically that vortex rings have invariable strengths, that is, the product of the cross section of a filament and its angular velocity remains constant. Not only does the strength remain constant during the motion of the vortex tube, it is also the same at all cross sections. Although Helmholtz's celebrated theory was a piece of mathematical physics, he did point out that it might relate to physical phenomena, such as in electricity and magnetism. But he did not suggest that it had anything to do with the ultimate constitution of matter.

Although Thomson had read Helmholtz's memoir on *Ring-Wirbelfäden* already in the spring of 1859, at that time he did not see it as relevant to his speculations about matter theory.[7] Yet, in a general way he was predisposed towards the idea of a vortex atom, namely, that the discreteness of matter could be accounted for within a continuum theory. This can be illustrated by a letter he wrote to Stokes on October 13, 1866, a few months before he invented the model of the vortex atom. Thomson objected to the view that matter is either atomistic or continuous and homogeneous. According to him, the dichotomy could be removed if only the requirement of homogeneity was abandoned:

> The only views that have ever appeared to me as true or natural as to the constitution of matter are those that suppose all space to be full but the properties of known bodies to be due to or necessarily associated with molecular structure or of a sponge or other organic tissue or brick work, i.e. that there are vast variations of density from point to point within spaces of dimensions some small fraction of a wave length (though not inappreciably small) (Wilson 1990, vol. 2, p. 330).

In the vortex theory, suggested a few months later, the density variations were replaced by variations in vorticity (angular velocity) in a fluid with constant density.

Thomson's early interest in vortices, as in his 1856 interpretation of magnetism, was in part inspired by the ideas of William Rankine, the Scottish engineer and natural philosopher.[8] According to Rankine's model, developed around 1850, the atom consisted of a small nucleus surrounded by an elastic atmosphere of innumerable vortices, with light and radiant heat originating from vibrations of the nucleus. Rankine applied his vortex atomic model to a range of phenomena, including phase transitions and thermodynamics. However, his vortex model was very different from the hydrodynamic vortex atom that Thomson suggested in 1867. Not only was Rankine's atom not an ethereal structure, his ether was also corpuscular and thus in contrast to Thomson's continuous ether. The origin of the true vortex atom seems to have been independent of Rankine's earlier speculations, although in a general sense these undoubtedly helped stimulating Thomson's interest in vortical motion. Ideas of a kind somewhat similar to Rankine's – to conceive matter particles as vibrational, pulsating, or vortical manifestations of a fluid – were common at the time, both in Britain and abroad. For example, the chemist Thomas Graham suggested in 1864 a kind of vortex atom. As he wrote, "A special rate of vibration or pulsation originally imparted to a portion of the fluid medium [the ether] enlivens that portion of matter with an individual existence and constitutes it a distinct substance or element."[9]

Thomson's recognition of the potential of Helmholtz's theory, as a framework for building up a theory of matter, came through Tait in early 1867. Tait was much impressed by Helmholtz's work and immediately translated it into English for his own sake.[10] In January 1867, Tait demonstrated to Thomson and others a way of producing smoke rings by means of a simple apparatus. Such smoke ring demonstrations became popular among British physicists, and often appeared in textbooks as an illustration of vortex atoms (figure 1).[11] But it was only the first demonstration, of January 1867, that had a major effect on theoretical physics. Thomson was much impressed and he now realised – "with the lightning rapidity of thought," as his first biographer has it (Thompson 1910, p. 512) – how the smoke rings illustrated Helmholtz's vortex motions and a possible model of atoms. His enthusiasm is apparent from a long letter he wrote to Helmholtz on January 22, 1867 (Thompson 1910, pp. 513–516). "Just now," he wrote, "Wirbelbewegungen have displaced everything else, since a few days ago Tait showed me in Edinburgh a magnificent way of producing them." After having described Tait's smoke ring apparatus in some detail, Thomson revealed the cause of his fascination, namely the connection to Helmholtz's theory of vortical motion:

> The absolute permanence of the rotation, and the unchangeable relation you have proved between it and the portion of the fluid once acquiring such motion in a perfect fluid, shows that if there is a perfect fluid all through space, constituting the substance of all matter, a vortex-ring would be as permanent as the solid hard atoms assumed by Lucretius and his followers (and predecessors) to account for the permanent properties of bodies (as gold, lead, etc.) and the differences of their characters. Thus, if two vortex-rings were once created in a perfect fluid, passing through one another like links of a chain, they never could come into collision, or break one another, they would form an indestructible atom; every variety of combinations might exist.

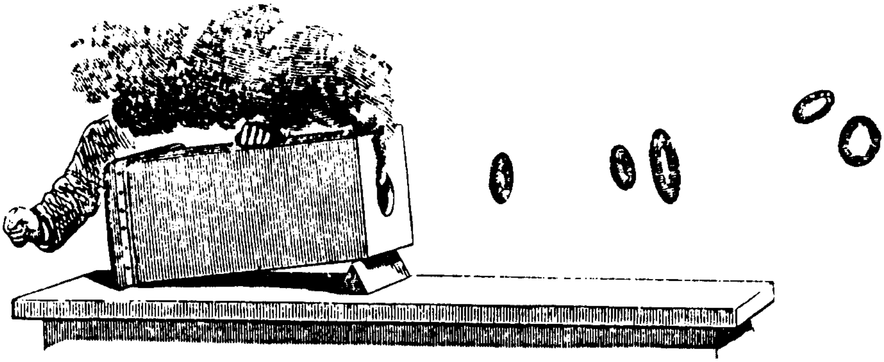

Fig. 1. Tait's smoke ring apparatus. Source: Tait 1876, p. 292.

After a correspondence with Tait, Thomson publicly announced his visionary hypothesis of vortex atoms on February 18, 1867, in a lecture before the Royal Society of Edinburgh. Here, he enthusiastically elaborated on the points mentioned in his letter to Helmholtz.[12] He strongly criticised "the monstrous assumption of infinitely strong and infinitely rigid pieces of matter" that even "the greatest modern chemists" took for granted. As he pointed out, this classical conception of the atom was able to explain the properties of matter only by attributing them to the atom itself. To the mind of Thomson, this was not an explanation at all. As a much preferable alternative he considered that "the only true atoms" were the vortex rings and filaments described by Helmholtz's theory. Among the properties of the vortices that appealed to him were their definite modes of vibration, which not only presented "an intensely interesting problem of pure mathematics" but also suggested an explanation of the spectra produced by chemical elements. Such an explanation could not be provided by the standard theory of atoms: "It would be necessary that the molecule of sodium, for instance, should be not an atom, but a group of atoms with void space between them. Such a molecule could not be strong and durable, and thus it loses the one recommendation which has given it the degree of acceptance it has among philosophers."[13] Not only did the vortex theory suggest an explanation in principle, Thomson even thought it might be applied to definite spectra, such as the yellow double line of the sodium spectrum: "It seems, therefore, probable that the sodium atom may . . . consist of two approximately equal vortex rings passing through one another like two links of a chain."

The vortex atom hypothesis was not merely of great mathematical and physical interest, it was also considered attractive because of its congruence with the religious feelings of many Victorian scientists (a theme that will be taken up in section 9). In his 1867 address, Thomson referred to the permanence of the vortex atoms and was happy to point out that they could only have come into existence through "an act of creative power," that is, they must have been created by God. The same point was made by Tait in an address to the British Association of 1871. "Our President's [Thomson's] splendid suggestion of Vortex-atoms, if it be correct, will enable us thoroughly to understand matter, and mathematically to investigate all its properties," he said. And he then added: "Yet its very basis implies the *absolute necessity* of an intervention of Creative Power to form or destroy one atom even of dead matter" (Tait 1871, p. 6). On this question, Thomson and Tait spoke with one voice.

During the months following February 1867, Thomson worked intensely on the mathematics of vortex atoms. His efforts led to a long paper in which he analysed mathematically both rotational and irrotational motion in a perfect fluid. (In rotational motion, any infinitely small element turns around an axis of its own, which is not the case in irrotational motion.) The most important of Thomson's results was the introduction of the concept of circulation, a measure of the average rate of turning of a closed loop of fluid particles. He proved that Helmholtz's theorems could be compressed into the single theorem that the circulation remains the same throughout the motion. As Thomson stated in his introduction, the work had "been performed to illustrate the hypothesis, that space is continuously occupied by an incompressible frictionless liquid acted on by no force, and that *material* phenomena of every kind depend solely on motions created in this liquid."[14] If Thomson and Tait were vortex atom apologists, the theory seems not to have appealed to Helmholtz, who chose to ignore what was the brainchild of his own theory of vortex hydrodynamics. Helmholtz's Faraday lecture of 1881, in which he dealt with chemical atomic theory and electrical atomic particles, might have been a natural occasion to mention the vortex atom theory (Helmholtz 1881). But he did not.

Maxwell, who probably read Helmholtz's paper a little later than Thomson, commented on it in a note added to the second instalment of his important memoir of 1861, "On physical lines of force." It was not the vortex hydrodynamics that interested him most, but rather Helmholtz's cautious remarks about an analogy between hydro- and electrodynamics. Helmholtz, he wrote, "has pointed out that the lines of fluid motion are arranged according to the same laws as the lines of magnetic force, the path of an electric current corresponding to a line of axes of those particles of the fluid which are in a state of rotation."[15] After Thomson had launched the vortex atom, Maxwell followed the development with keen interest, but also, it seems, with measured scepticism. In a letter to Tait of 1867 he mused that "Thomson has set himself to spin the chains of destiny out of a fluid plenum as M. Scott sets an eminent person to spin ropes from the sea sand."[16] Yet Maxwell recognised the power of the theory and was far from immune to its magic. Only he found the vortex theory interesting mainly within the framework of electromagnetism rather than matter theory. In his *Treatise on Electricity and Magnetism* of 1873, he reviewed Helmholtz's theory of vortex motion and applied it to his provisional hypothesis of molecular vortices. These were however not vortex atoms, but small vortices of the field or light-bearing medium (Maxwell 1954, pp. 461–467).

Maxwell first referred publicly to the vortex atom in 1870, in an address he gave as president of the mathematical and physical section of the British Association. As he would do in greater detail five years later, he extolled Thomson's theory because of its methodological virtues. Maxwell much preferred the vortex atom model over "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." The vortex theory, on the other hand, was free of ad hoc features, for it rested on "nothing but matter and motion" and included "no central forces or occult properties of any kind" (Maxwell 1965, part II, p. 223). Three years later, he dealt briefly in a review with "that remarkable extension of the science of hydrokinetics which was begun by Helmholtz and so aptly followed up by Thomson himself." Yet nearly six years had passed since Thomson's breakthrough, and Maxwell bemoaned the slow progress of vortex physics: "It is to be hoped that the latter [Thomson] will soon complete his papers on *Vortex Motion* and give them to the world. But why does no one else work in the same field? Has the multiplication of symbols put a stop to the development of ideas?"[17] Indeed, why not? It may have been Thomson's prestige and perceived "ownership" of the vortex atom that prevented a more rapid diffusion. At any rate, by 1880 the field had taken firm root within British mathematical physics. The "multiplication of symbols" was not a problem; on the contrary, the mathematical challenges served as a stimulus for young physicists and mathematicians to take up the subject.

Maxwell's friendly-critical attitude to the vortex atom theory is best known from his famous article on atoms for the 1875 edition of *Encyclopaedia Britannica*. In this masterpiece of an essay, written in 1874, he praised Thomson's theory from a methodological point of view. "The disciple of Lucretius may cut and carve his solid atoms in the hope of getting them to combine into worlds; the follower of Boscovich may imagine new laws of force to meet the requirements of each new phenomenon," he wrote, adding that, "but he who dares to plant his feet in the path opened up by Helmholtz and Thomson has no such resources" (Maxwell 1965, part II, pp. 471–472). What mostly impressed Maxwell was precisely the closed nature of the theory, the absence of any arbitrary element:

> But the greatest recommendation of this theory, from a philosophical point of view, is that its success in explaining phenomena does not depend on the ingenuity with which its contrivers 'save appearances', by introducing first one hypothetical force and then another. When the vortex atom is once set in motion, all its properties are absolutely fixed and determined by the laws of motion of the primitive fluid, which are fully expressed in the fundamental equations . . . [Thomson's] primitive fluid has no other properties than inertia, invariable density, and perfect mobility, and the method by which the motion of this fluid is to be traced is pure mathematical analysis. The difficulties of this method are enormous, but the glory of surmounting them would be unique.

With these words, Maxwell elegantly characterised the strengths and weaknesses of the vortex atom theory, which he recognised was still in an immature state and perhaps more a research programme than a full theory that could be tested experimentally. His intuition proved right.

3. Between mathematics and physics

The "discovery" of the vortex atom, itself a product of mathematical hydrodynamics, triggered a series of mathematical investigations directly or indirectly related to Thomson's theory. Over the next three decades, the original vortex theory mutated into a large number of mathematical models that, in many cases, had only little connection to claims of physical reality.[18] Although interest in vortex motion was not limited to Great Britain, it was only in this country that researchers sought to develop the theory into a vortex model of ether and matter, or both.[19] During the last quarter of the century, more than a dozen, mainly Cambridge-trained British mathematicians and physicists were busy with developing the insights originally obtained by Helmholtz and Thomson. The growing vortex programme, backed up by the immense authority of Thomson, became a career possibility for many British physicists (Pauly 1975). The most active scientists in the field, apart from Thomson himself, were William Hicks and Joseph J. Thomson, but they were not alone. To the same research tradition in vortex hydrodynamics, or topics related to it, must be counted Augustus Love, Micaih Hill, Horace Lamb, Thomas Lewis, Charles Chree, Alfred Greenhill, Henry Pocklington, C. V. Coates, Arthur Leahy, Alfred Basset, Richard Hargreaves, and Horatio Carslaw, and possibly a few others. As to Tait, Lodge and FitzGerald, they stood somewhat outside this group. The few contributions from the United

States were limited to general comments or reviews, not always of a kind that revealed the author's understanding of the theory.

Thomson himself followed up on his 1867 theory with works on the stability of vortices and the energy of vortex motion within a space bounded externally by a closed surface (Thomson 1880a [MPP 4, pp. 115–128]; Thomson 1880b). He also investigated the vibrations of columnar vortices, concluding that the periods are functions of the radius of the vortex column (Thomson 1880c [MPP 4, pp. 152–165]). These works were mathematical in nature and did not explicitly refer to the vortex atom view of matter. Only in 1881, in a brief non-mathematical note, did he consider "a gas composed of vortex atoms," which he argued would behave in the same way as "is given by the ordinary kinetic theory, which regards the atoms as hard elastic particles" (Thomson 1881 [MPP 4, p. 188]). Also the important investigation of 1883, made by another and younger Thomson, was highly mathematical. J. J. Thomson, then 25 years old, won the Adams Prize for his essay on the subject for the year 1882, namely "A general investigation of the action upon each other of two closed vortices in a perfect incompressible fluid" (J. J. Thomson 1883). J. J. Thomson investigated the dynamics of a liquid containing vortex rings and derived an expression for the velocity of translation that differed slightly from that found by William Thomson. He further discussed in mathematical detail the mutual action of two vortices, and also the motion of linked vortices of either equal or unequal strength. It was this part of J. J. Thomson's work that was most original. As we shall see below (section 6), he used it to suggest a vortex theory of gases as well as a theory of the constitution of chemical elements.

As was soon realised, vortices in fluids may have many forms besides being shaped as rings or columns. Thus, Micaih Hill, a young mathematician at Mason's College, Birmingham, generalised the equations of vortex filaments. In 1885, after being appointed professor of mathematics at University College, London, he developed a theory of cylindrical vortices in an infinite fluid, and ten years later he went on to analyse spherical vortices.[20] In the latter work, he found that a spherical mass of fluid in vortical motion would move through the surrounding fluid as if it was a rigid sphere. The spherical vortices or vortex atoms could be understood as limiting cases of highly distorted ring vortices.

Still other forms of vortex motion were investigated by William Mitchinson Hicks, a mathematical physicist who had studied under Maxwell and in 1883 became professor at Firth College in Sheffield. Fascinated by the theory of vortex motion, Hicks found the same year that hollow vortex rings could exist. He studied the motion and vibrations of such objects, as well as vortices with cores of varying density, in several papers.[21] He also discovered spiral-shaped vortex filaments, and based on these objects he sketched a new version of the vortex atom that we shall consider below (Hicks 1883; Hicks 1885b; Hicks 1898). As late as 1899, FitzGerald studied similar vortex objects, but not as possible constituents of matter. His system of spiral vortices was intended as a model of the ether as a turbulent fluid.[22]

The background for Hicks's revision of the vortex picture lay in a simple problem: if atoms are made of ether – assumed to be of exceedingly low density – how can the much greater density of ordinary matter be explained? Hicks felt 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." However, the case he considered in 1883 was rather the opposite one, namely, "when there is no internal layer and no rotational motion in the fluid at all; merely the cyclic motion about a ring-shaped hollow."[23] Hicks found his hollow-vortex theory to be of particular value in the field of spectral lines. A hollow vortex atom, he showed, could vibrate in many different ways and thereby reproduce almost any conceivable feature to be found in spectral investigations. For example, if one mode of vibration was assumed, the frequency would depend on the energy of the vortex; if another mode was assumed, it would not.

William Thomson endorsed Hicks's work on vortices with vacuous cores, which he saw as a possible way to revitalise the vortex atom theory. In a letter to FitzGerald, he expressed his hope that Hicks's theory "will be the beginning of the vortex theory of ether and matter, if it is ever to be a theory" (Thomson 1889–91 [MPP 4, pp. 202–204, on p. 202]). Thomson's endorsement is understandable, for as early as 1872–73 he had himself considered such objects. In a letter to Stokes of January 1, 1873, he discussed vortices with hollow cores, and wrote: "Void-core ring vortices, with varieties of knots, serve well for atoms, *being expansible* yet rigorously permanent & stable" (Wilson 1990, p. 383). Hicks continued to investigate vortex structures many years after the vortex atom had been abandoned. As late as 1923, at a time when the quantum theory of Niels Bohr and Arnold Sommerfeld governed atomic research, he examined the subject in much the same way as he had done forty years earlier (Hicks 1923). Hicks's work exemplifies how the mathematical framework of the vortex atom theory came to live its own life, independent of the physical hypothesis that had once stimulated it.

It should be noted that Hicks's introduction of vortex objects with hollow cores, or with cores of varying density, marked a significant conceptual change in the vortex programme. In the original vortex picture, the ether was of constant density, atoms being solely kinetic configurations. A partially evacuated ether was a strange animal, a trade-off between contradictory ideals that left behind the theory's original appeal to monism. Moreover, if there were vacuous parts in the ethereal fluid, or if its density was assumed to vary in space, it would not be homogeneous and hence not perfect. Yet these conceptual problems seemed not to worry either Hicks or Thomson. Although Hicks admitted that "the simplicity of the theory is to some extent lost by having two elementary matters in the place of one" (Hicks 1883, p. 305), his interest was in the mathematical aspects and he was undisturbed by the loss in simplicity. So long that the vortex objects could be described mathematically and serve to illuminate physical phenomena, he was satisfied. Without further ado, in the 1880s hollow-cored and varying-density vortex atoms entered the theory and turned it into a more complicated, perhaps philosophically less appealing picture.

Most of the mentioned works had their roots in, or were partly motivated by, the vortex atom theory, yet their emphasis was on the mathematical methods rather than physical aspects. Many of them appeared in the Cambridge mathematical journals *Messenger of Mathematics* and *Quarterly Journal of Pure and Applied Mathematics*. A few were published in the *American Journal of Mathematics* whose editor, Thomas Craig, was interested in vortex theory. Occasionally the physicists in this hydrodynamic tradition referred to vortex atoms, but only rarely did they connect their ideas with measurable properties of matter or otherwise spell out the connection between mathematics and physics. As a typical, and not particularly noteworthy, example we may consider a paper that Augustus Love, lecturer in mathematics at Cambridge University, wrote in 1894 on the motion of two vortex rings travelling in the same direction. Introducing his mathematical analysis of the problem, he wrote: "In application of the vortex-atom theory to problems of radiation and chemical combination, it is conceivable that this . . . type of motion may play an important part."[24] This was as close as he came to empirical physics. Love and his fellow mathematicians found the vortex atom interesting, but mainly because of what Thomson in 1867 had called the "intensely interesting problem of pure mathematics" associated with it.

The vortex atom gave impulse not only to advances in mathematical hydrodynamics, but also to a new branch of topology, the theory of knots. Although knot theory can be traced back to a work of 1847, by the German mathematician Johann Listing, it was only with Tait's contributions that the field became recognised as an interesting branch of mathematics. Originally inspired by Helmholtz's paper on vortex motion and its perceived relevance for quarternion analysis, Tait started about 1870 to think seriously about topology. In this work, that soon led him to the study of knots, the theory of vortex atoms served as a strong impulse and became, in his mind, integrally linked with a topology of matter.[25] In his 1869 paper on vortex motion, Thomson considered various forms of knots (figure 2), which served as the immediate background for Tait's work. Whereas it became increasingly difficult to justify the vortex atom both physically and mathematically, Tait developed fruitfully the topological ideas associated with Thomson's theory. Vortex atoms might have no physical existence, but, as Tait and Balfour Stewart phrased it, they might nonetheless be "very valuable from one point at least, viz. the extension and improvement of mathematical methods" (Stewart and Tait 1881, p. 140). One of these methods was the theory of knots that

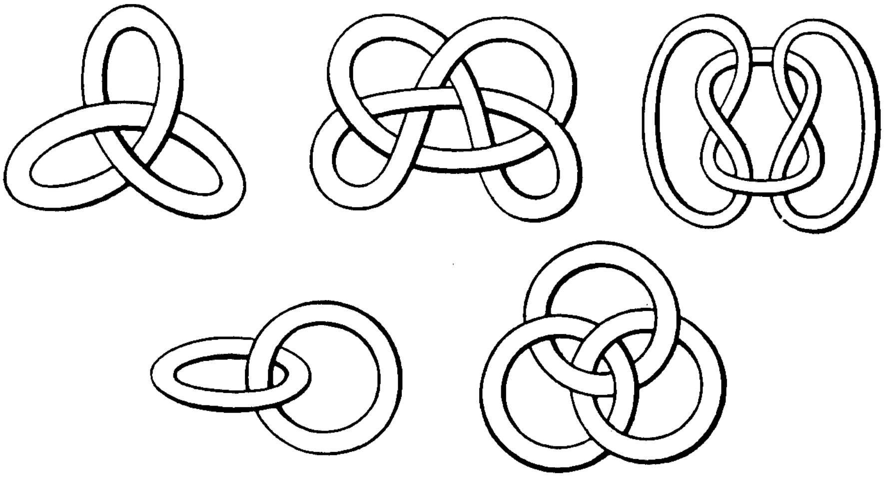

Fig. 2. Some of Thomson's knots of 1869. Source: Thomson 1882–1911, vol. 4, p. 46.

Tait began to develop in 1876. In his effort to classify and understand knots, he was motivated by the vortex atom. In a communication to the 1876 annual meeting of the British Association, he implied that the vortex atom might be seen as a special case of knot theory.[26] And in his first major work on the topic he admitted that "I was led to the consideration of the form of knots by Sir W. Thomson's theory of vortex atoms." This theory, he continued, could not possibly lead to an explanation of the spectra if vortex atoms were simple objects:

> For though there is, of course, an infinite number of possible modes of vibration for every vortex, the number of modes whose period is within a few octaves of the fundamental mode is small unless the form of the atom be very complex. Hence the difficulty, which may be stated as follows (assuming, of course, that the visible rays emitted by a vortex atom belong to the graver periods): "What has become of all the simpler vortex atoms?" or "Why have we not a much greater number of elements than those already known to us?" (Tait 1877 [Tait 1898–1900, vol. 1, pp. 273–317, on pp. 273–274]).

The line spectra of elements were generally taken as evidence for internally structured atoms, and often for the vortex atom theory (McGucken 1969). Several British scientists followed Norman Lockyer in his belief that atoms were not only structured, but could also be dissociated into smaller units. However, according to Tait, the vortex atom excluded the dissociation hypothesis, presumably because knotted vortices were permanent structures. In 1881, he contrasted Lockyer's dissociation hypothesis with "our belief in the existence of *essentially different* elementary atoms, which is at the basis of the beautiful Vortex Theory." Tait argued that astro-spectroscopic data did not constitute compelling evidence for the dissociation of atoms, and that one could even assume "the existence of exactly equal vibration-periods in two perfectly distinct vortex-atoms" (Tait 1881 [Tait 1898–1900, vol. 1, pp. 454–456]).

In order to make the connection between knots and vortices, a multitude of knot types had to exist, which caused Tait to study more complicated cases of "knottiness." Apart from using knot theory to illuminate possible mechanisms of light emission from atoms, Tait also attempted to represent the chemical elements by means of a suitable classification of knots and links. He, and a few mathematically inclined chemists, including the Edinburgh professor of chemistry Alexander Crum Brown, further speculated that the study of knots might throw light on the problem of chemical structure and provide new graphical formulae (Brown 1867).

Tait's attempt to use knot theory in atomic chemistry was not the only attempt among Victorian scientists to apply abstract mathematics to problems of structural chemistry and chemical classification. At about the same time, Arthur Cayley, James Sylvester, and William Clifford investigated the possible uses of graph and invariant theory as means of illuminating the atomic theory of chemistry (Parshall 1997). These works were attempts to accelerate the progress of chemistry through mathematics and, in this sense, they belonged to the same tradition as that briefly pursued by Tait. Although they were welcomed by a few chemists, they failed to establish a substantial connection between pure mathematics and chemistry. Whether based on knot, graph, or invariant theory, these attempts of mathematization were short-lived. It should be noted that they differed from the vortex atom programme in chemistry (see section 6) in that they explored formal analogies rather than attempted to reduce chemistry to an atomic science governed by laws of physics. In fact, mathematical chemistry in the style of Tait or Sylvester did not necessarily rest on an acceptance of atoms as real particles. As shown by the ''calculus of chemical operations'' proposed by Benjamin Brodie, mathematics might just as well serve the cause of anti-atomism (Brock 1967).

Just like the work in mathematical vortex hydrodynamics of Hicks and others, knot theory had its roots in the vortex atom theory but soon developed into an independent mathematical study where Thomson's theory dropped out of sight. Although Tait's approach to knot theory was physical and intuitive, the vortex atom served more as an entry to the field than an area of application. Characteristically, the second and third parts of Tait's trilogy ''On knots'', published in 1884–1885, contained no reference to vortex atoms.

4. Vortex sponges and ether squirts

By the 1870s, the luminiferous ether was firmly established in British physics as the medium through which light and other signals propagated. According to a widely held view, the world consisted of material atoms embedded in an all-pervading continuous ether. This was, for example, the view of James

Challis, Plumian professor of astronomy in Cambridge, according to whom the ether “is an extremely rare and elastic fluid [which] fills all space not occupied by the atoms.”[27] Challis was an unrestrained ether unificationist who, like some vortex theorists, sought to explain all physical forces in terms of the hydrodynamics of the ether. But his theory of everything was rather less ambitious: the material atoms were not part of it. At any rate, the relationship between matter and ether could be conceived in many other ways. Instead of a dualistic ontology, ether could be thought of as matter-like or, conversely, ponderable matter as structures in the ether. Of course, a few scientists denied the existence of the ether altogether, but they were few and definitely out of tune with the British tradition. Given the fluidity between the concepts of matter and ether, a historical account of the vortex atom of matter cannot ignore theories of the ether, the other side of the coin, as many saw it.

In most of the early writings on the vortex atom, the vortical motions were supposed to occur in a hypothetical fluid that was only specified by its elastic properties. Thomson’s theory was an attempt to explain matter in terms of structures in this fluid, and he did not originally apply it to the ether. Nor did he explicitly identify the fluid with the ether. On the other hand, there is little doubt that he thought that the ether, too, must be vortical, in which case he would be able to account for its elasticity.[28] He did not express this view very clearly, but in some cases he spoke of ether and matter as were they merely two different manifestations of the same substratum. In late September 1884, he asserted that “We have no knowledge that the luminiferous ether is gravitationally attracted by masses such as the earth or the sun; or that there is mutual attraction between different parts of the ether itself” (Thomson 1891, p. 336). Yet only two weeks later, in his sixteenth Baltimore Lecture, he gave voice to a rather different view: “We have not the slightest reason to believe the luminiferous ether to be imponderable; it is just as likely to be attracted to the sun as air is . . . [T]he onus of proof rests with those who assert that it is imponderable.” Perhaps, he speculated, ether was merely a more fine-grained version of matter. One day it might be possible to “understand the luminiferous ether as differing from glass and water and metals in being very much more finely grained in its structure” (Thomson 1904, p. 266 and p. 12). In a note of 1899 added to the later statement, Thomson wrote: “I now see that we have the strongest possible reason to believe that the ether is imponderable.”

Whatever the precise nature of Thomson's views about ether and matter, by the mid-1870s he began to investigate vortex models of the ether essentially along the line of the vortex atom theory. However, only in 1887 did he publish a fully articulated vortex theory of the ether, namely, "an attempt to construct, by giving vortex motion to an incompressible inviscid fluid, a medium which shall transmit waves of laminar motion as the luminiferous æther transmits waves of light." Thomson demonstrated that such a medium could be constructed, but his model did not quite satisfy him. He ended his paper with these words: "I am thus driven to admit, in conclusion, that the most favourable verdict I can ask for the propagation of laminar waves through a turbulently moving inviscid liquid is the Scottish verdict of *not proven*."[29]

Thomson's theory of 1887 belonged to a class of ether theories that for a period attracted much attention, so-called vortex sponge theories. The general idea of a vortex sponge was introduced by Thomson in 1880, but at that time not as a model of the ether. According to Thomson, a vortex sponge was "a mixture homogeneous on a large scale, but consisting of portions of rotational and irrotational fluid, more and more finely mixed together as time advances" (Thomson 1880b, p. 474). Five years later, Hicks published the first vortex sponge ether theory, a model in which waves could be transmitted through an incompressible fluid with tiny vortex rings closely packed together (Hicks 1885b). On a larger scale the medium would act as a fluid, hence allow also vortex atoms in addition to the much smaller vortex rings that were responsible for the propagation of transverse vibrations. At the same time, George Francis FitzGerald took up the problem in which he engaged himself wholeheartedly for several years. In early 1885 he reported to Oliver Lodge that he was working hard on "a theory of the ether that requires it to be a perfect liquid full of vortices."[30] His theory, presented to the Physical Society in London on March 28, 1885, appeared in print later the same year.

FitzGerald was principally concerned with the luminiferous ether, not with matter, but he realised that the vortex sponge model had consequences also for vortex atomic theory: "The supposition that the ether is a vortex sponge in a perfect liquid, does not diminish the number of possible hypotheses as to the constitution of matter: on the contrary, it very much increases the possible modes of action of matter."[31] The same message was part of an address he delivered before the British Association in 1888 and in which he suggested that the vortex sponge theory might be able to explain chemical actions. Such actions did not differ in principle from electromagnetic phenomena, only were they limited to distances comparable with the size of atomic vortices and therefore presumably much more complex. FitzGerald had no confidence in the simple theory of ring vortices flowing around in a perfect fluid, but he felt that the multiplicity of objects and motions of a generalised vortex theory might offer a solution: "With the innumerable possibilities of fluid motion it seems almost impossible but that an explanation of the properties of the universe will be found in this conception." To explain the properties of the universe was no small feat, but there was even more to recommend the vortex theory: "There are metaphysical grounds, too, for reducing matter to motion and potential to kinetic energy" (FitzGerald 1888, pp. 561–562).

Lodge was a receptive correspondent, for he held views that to a large extent agreed with those of FitzGerald. In particular, he shared the view that all nature was emergent from the ether. What he called the modern view of the ether, was this: "One continuous substance filling all space: which can vibrate as light; which can be sheared into positive and negative electricity; which in whirls constitutes matter; and which transmits by continuity, and not by impact, every action and reaction of which matter is capable" (Lodge 1883, p. 330). FitzGerald's vortex ideas, including his "metaphysical grounds" for believing in them, were to some extent influenced by the views of George Johnstone Stoney. Although Stoney did not contribute to the vortex atom theory, his ideas about the goal of physics and the composition of the world were consistent with it. In a lecture read before the Royal Dublin Society, he made it clear that he held the theories of Thomson and FitzGerald in high regard: "Though we have as yet only a glimmering of this great subject, it is pretty certain that either these hypotheses, or something like them, are the true ultimate account of material Nature" (Stoney 1890, p. 476; Hunt 1991, p. 99–102). Stoney distinguished between an "elemental" and structureless ether, and a luminiferous ether that was structured like ponderable matter. In full agreement with the vortex philosophy, he considered material density to be just an epiphenomenona of the motions in a particular portion of space. His strong emphasis on the ether as a plenum made him reject the vacuous coreless vortices as parts of real nature.

The vortex sponge theories of FitzGerald, Hicks and Thomson aroused great enthusiasm as possible candidates for the ultimate mechanical theory of the universe. Although designed as a theory of the ether, the vortex sponge model was often taken to represent material atoms as well. After all, matter and ether were supposed not to differ in essence, only in degree. Evidence that such a view was endorsed by FitzGerald appears in a lecture he read before the Royal Institution in 1890. The subject was Hertz's new waves, but at the end FitzGerald,

> stated that what seemed a possible theory of ether and matter was that space was full of such [straight and hollow] infinite vortices in every direction, and that among them closed vortex rings represented matter threading its way through the ether. This hypothesis explains the differences in Nature as differences of motion. If it be true, ether, matter, gold, air, wood, brains, are but different motions (FitzGerald 1970, p. 25).

In spite of the high expectations of FitzGerald, Hicks, Lodge, and some other physicists, the vortex sponge model did not survive for long. Already when FitzGerald gave his Royal Institution lecture, the theory was in deep trouble, in part as a result of the objections that Thomson raised against it. Yet grand and aesthetically pleasing theories die hard, and as late as 1895 the indefatigable Hicks spoke favourably of the theory, which he continued to find promising. At this occasion, he introduced yet another vortex model of the ether, what he called a cell theory. He described the cell as a tiny portion of the fluid ''in which the motion is a complete system in itself'' and whose dimension was smaller than the wavelength of light. Hicks operated with two kinds of ether, the primary medium and the secondary, light-transmitting medium built up of the first. ''Whether an atom of matter is to be considered as a vortical mass of the primary or secondary medium is a matter to be left open in the present state of theory.''[32]

The late Victorian period witnessed a bewildering variety of views concerning the nature of the ether and the relationship between ether and matter. In Britain, the favoured view was a continuous ether that might possibly include atoms as particular structures, as in the vortex atom theory. Yet, if this was the view of the majority it did not stand alone. It is, in fact, difficult to think of a conception that was not advocated, or at least tried out, by one physicist or other. Thus, the physicist and telegraph engineer Samuel Tolver Preston agreed that there was no essential difference between ether and matter, but he also argued that the quasi-material ether was discrete, consisting of very small particles.[33] Ideas, often highly speculative, of corpuscular ethers were common on the Continent but not among British mathematical physicists.[34] Preston's advocacy of a corpuscular ether did not prevent him from praising the vortex atom theory, which he found to be "a practical working hypothesis" far superior to any other conception of the atom. Preston stressed the physical and philosophical advantages of the vortex theory, and was particularly impressed by the explanation of complex spectra that the theory offered. Not a mathematical physicist, his understanding of the vortex theory was limited, as was that of the Scottish geologist James Croll. In a paper of 1883, Croll objected to the theory that it disagreed with Newton's first law of motion. He found it difficult to understand why the centrifugal force did not dissipate the fluid material of the vortex atom (Croll 1883).

Although not a vortex atom theory, Karl Pearson's ether theory of matter deserves mention in the present context. The theory is barely known today and even at the time it was put forward it attracted almost no interest.[35] Yet this ambitious theory shared many of the methods and goals of the vortex atom theory, and it was fully consistent with the spirit that permeated Victorian physicists within the vortex atom tradition.

Pearson, a monist and a positivist, rejected the dualistic conception of two primary substances, ether and matter atoms. As a young man he was attracted to the vortex theory, such as shown by one of his earliest papers, an attempt to reduce the elastic solid ether to vortex motion (Pearson 1883). In a paper of 1885 – this was a great year for atomic hypotheses – he praised the vortex atom as an "extremely beautiful hypothesis" but suggested that it was not the only possibility of a monistic theory of matter. As an alternative, he proposed that the ultimate atom might be a differentiated spherical part of the ether, or perhaps a vacuum within the ether, pulsating with a natural frequency. His ether or fluid was incompressible, but differed from that of the vortex atomists by being irrotational and not necessarily perfect. He found the conception of spherical ether atoms, when worked out mathematically, to be promising with regard to the understanding of a wide range of natural phenomena. For example, he deduced that the interatomic force must vary with the distance between atoms as the inverse cube, and in general prophesied: "The question of chemical combination and decomposition would become one of calculation were we able to observe the period of free pulsation of every elementary atom and to tabulate chemical intensities and chemical coefficients or the equivalent chemical affinities."[36]

In a subsequent paper (Pearson 1888–89), he criticised the spring-shell atom model that Thomson had proposed in 1884 and that four years later was elaborated by the German mathematician Ferdinand Lindemann

(Thomson 1904, p. 118; Lindemann 1889). Thomson's "dispersion molecule" consisted of a number of concentric shells arranged around a massive core and connected by stretched springs; the massive shells had their proper periods of vibration, and the whole system was embedded in the ether, represented by the outer, mass-less shell. Although Thomson made it clear that he did not mean this mechanical model to be literally true, but only to be useful, Pearson apparently found the Thomson-Lindemann model to be a retrograde step relative to the vortex atom. Or, as he wrote in a paper of 1891, "The Thomson-Lindemann atoms and molecules thus show us so far only complex mechanisms, and raise the not unnatural repugnance of the philosophical mind to a dualistic theory of the universe" (Pearson 1891, p. 311).

In Pearson's modified theory of 1891, he sought to combine the merits of the extended vortex atom and the Boscovichian point atom. This he did by reducing the atomic sphere to a point from which ether continuously flows in all directions of space, or what he called an ether squirt. He later described sn his point atom as "something like a tap turned on under water, except that the machinery of the tap is dispensed with in the case of the squirt" (Pearson 1900, p. 267). Elsewhere in the world there were counterparts of the squirt atoms, sinks that absorbed ether and acted like negative matter.[37] As to the question of from where the ether flowed, and to where it returned, he preferred to leave it to the metaphysicians. And yet the un-metaphysical Pearson did not refrain from briefly speculating about "a space of higher dimensions" as a possibility.[38] Pearson developed his hydrodynamic and monistic theory in considerable mathematical detail, and endeavoured to turn it into a model that could illuminate concrete problems of physics and chemistry. But, like so many other theories of this class, it did not deliver what it promised. Considering that Pearson strongly favoured a positivist methodology, and tended to regard both ether and atoms as nothing but mental constructs, it is remarkable that he spent such an effort in building up an atomic theory based on the ether as the sole medium of the universe.[39]

5. The enigma of gravitation

With the extension to the ether, as in the vortex sponge model, it seemed possible, at least in principle, to account for the propagation of light within the vortex atom programme; and, even more in principle, perhaps also to incorporate electrical and magnetic phenomena. This might be wishful thinking, but if so it was considered to be realistic wishful thinking. The old riddle of explaining gravitation was a much harder problem, for here even crude explanation sketches were missing. During most of the lifetime of the vortex atom programme, it was recognised that the theory, in order to be judged credible, must ultimately be able to give some explanation of gravity. A truly fundamental theory – and this was what the vortex atom theory was considered to be – just could not exclude gravitation. "It may be hard to say of an infant theory that it is bound to explain gravitation," Maxwell wrote in 1875, yet this was precisely what the vortex atom theory was expected to do (Maxwell 1965, part II, p. 473). For example, in 1876 Tait wrote that, "The theory of vortex-atoms must be rejected at once if it can be shown to be incapable of explaining this grand law of nature" (Tait 1876, p. 298). Seven years later, Oliver Lodge, another vortex atom enthusiast, stated the same verdict. In order to be accepted, the appealing theory of vortices "must account for gravitation . . . Vortex atoms must be shown to gravitate" (Lodge 1883, p. 329).

The trouble was that no such theory came forward or even seemed to be within reach.[40] The first attempt to give a quantitative explanation of gravitation on a vortex basis, and possibly the only theory that deserves to be labelled a vortex atom theory of gravitation, was suggested by Hicks in a series of papers between 1879 and 1883 (Hicks 1879; Hicks 1880; Hicks 1880–83. See also Roseveare 1982, pp. 102–104). Hicks assumed hollow vortex atoms to pulsate in the ether in such a way that they would act on each other by an inverse square law. He showed that in order that gravity be attractive, the pulsations must not depend on the energy. In this way he obtained a complicated force law that, in general, implied not only ordinary matter, but also "negative" matter that would repel ordinary matter (but attract other negative matter). According to Love's review, written only a few years later, "when it is remembered how small is the force of gravitation compared with electric and magnetic forces and the molecular forces of cohesion &c., it will be felt as a possibility that the gravitation of masses compacted as vortex-atoms may be a small residual effect, of which our approximate mathematical work has so far failed to take account" (Love 1887, p. 337). Hicks admitted that his theory of gravitation was not quite satisfactory, but he continued to feel that "the least unsatisfactory [theory] is that depending on the vortex atom theory of matter, which attributes it to pulsations of hollow vortex atoms'' (Hicks 1895, p. 605). A somewhat similar ethereal pulsation theory was proposed by Arthur Leahy (1889), who however adopted an elastic solid ether and obtained an expression of gravitation that in some respects differed from that found by Hicks. In a paper of 1891, Rouse Ball conjectured that the visible universe was connected with a four-dimensional ethereal space, and on this speculative basis he succeeded in deriving an inverse-square law of attraction between particles (Ball 1891; Ball 1905, pp. 371–372). Although Ball's hypothesis was not based on the vortex atom theory, it was consistent with it.

William Thomson was of course aware of the problem of gravitation, but did not address it directly. In 1872, he published a partly historical study of the collision theory of gravitation that George-Louis Lesage had suggested back in 1782. The Geneva natural philosopher had assumed the existence of a myriad of tiny ''ultramundane corpuscles'' moving at high speed in all directions, and explained in this way gravitation as a screening effect. That is, he showed that the effect of the corpuscles' impact on matter particles would be to make any two of these behave as if attracted by an inverse square force. However, Lesage's theory presupposed a particular (cage-like) structure of matter and also that the fine corpuscles were inelastic, which disagreed with the later principle of energy conservation. Thomson now saw a way to revive Lesage's theory by replacing the hard ultramundane particles with his perfectly elastic vortex atoms. With this modification, he concluded that ''The corpuscular theory of gravity is no more difficult in allowance of its fundamental assumptions than the kinetic theory of gases as at present conceived'' (Thomson 1873 [MPP 4, pp. 64–76, on p. 75]; Smith and Wise 1989, pp. 425–430). Maxwell, in his 1875 article on ''Atom'', dealt with Lesage's theory in some detail and also mentioned Thomson's identification of the ultramundane corpuscles with vortex atoms. Although Maxwell admitted the theory to be ingenious, he doubted that it was capable of explaining the moderate temperature of bodies under a constant bombardment by corpuscles (Maxwell 1965, p. 476).

In his paper on Lesage's theory, Thomson did not mention the vortex atom directly, but there is no doubt that this was just what he thought his ''perfectly elastic atom'' to be. This is confirmed by a Royal Institution lecture of 1881 in which he dealt with elasticity. In the style of Tait, Thomson demonstrated before his audience the smoke rings, and then went on:

> May not the elasticity of every ultimate atom be thus explained? But this kinetic theory of matter is a dream, and can be nothing else, until it can explain chemical affinity, electricity, magnetism, gravitation, and the inertia of masses (that is, crowds of vortices). Le Sage's theory might give an explanation of gravity and of its relation to inertia of masses, on the vortex theory, were it not for the essential æolotropy 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.[41]

At that time, the only justification of the vortex atom theory that Thomson could come up with was that "no other theory of matter is possible."

Although Lesage's theory was much discussed during the last quarter of the nineteenth century, the Lesage-Thomson vortex variant attracted only modest interest. George Forbes, professor of astronomy in Glasgow, found it to be an appealing working hypothesis and described it in the following, dramatic way: "Sir William Thomson supposes ultramundane corpuscles to be vortex rings with no hole in the centre and elongated, like a serpent rushing forwards and always turning inside out, spitting its inwards out at its mouth, and absorbing its skin at the other end" (Forbes 1878, p. 499). Forbes speculated that the interaction between matter vortex atoms and the finer vortex corpuscles would lead to the consequence that hot bodies must emit heat radiation to be absorbed by cold bodies, and that this might point forward to a new theory of light.

Thomson's sketch of a kinetic theory of gravitation relied on a vortex interpretation of gas theory, such as he had already hinted at in his first paper on vortex atoms: "A full mathematical investigation of the mutual action between two vortex rings . . . will become the foundation of the proposed new kinetic theory of gases" (Thomson 1867 [MPP 4, p. 2]). He convinced himself that a vortex gas theory was in better agreement with the fundamental laws of statistical mechanics (such as the equipartition theorem) than the Maxwell-Boltzmann theory based on collisions between solid molecules. Moreover, he claimed that it followed from the latter interpretation that all kinetic energy in a gas would eventually dissipate and be converted into vibrational energy.[42] In 1884, he discussed at length a model vortex gas – "composed of either Helmholtz cored vortex rings or of coreless vortices" – that would avoid the, to him, unsatisfactory assumption of collisions between solid molecules:

> Whether, however, when the vortex theory of gases is thoroughly worked out, it will or will not be found to fail in a manner analogous to the failure which I have already

> pointed out in connection with the kinetic theory of gases composed of little elastic solid molecules, I cannot at present undertake to speak with certainty. It seems to me most probable that the vortex theory cannot fail in any such way [as in the Maxwell-Boltzmann theory], because all I have been able to find out hitherto regarding the vibration of vortices, whether cored or coreless, does not seem to imply the liability of translational or impulsive energies of the individual vortices becoming lost in the energy of smaller and smaller vibrations. (Thomson 1884 [Thomson 1891, pp. 225–259, on p. 258]).

About that time, the specific heats anomaly was often used as an argument against the kinetic theory of gases. According to the kinetic theory, the ratio between the specific heats at constant pressure and volume should be 1.33 for diatomic molecules, whereas experiments gave the value 1.41.[43] Moreover, absorption and emission of spectral lines seemed to require molecules with a very large number of internal degrees of freedom, which the molecules of the kinetic theory could not provide. The latter problem was easily solved by the vortex gas theory, where the molecules have an almost infinite number of vibrational modes, but then the specific heats problem remained, indeed became much graver. For, if the equipartition theorem were assumed to be valid, it would lead to a ratio close to one. In a review of 1877, Maxwell pointed out the problem and argued that a vortex theory of gases would not work:

> It will not do to take a body formed of continuous matter endowed with elastic properties, and to increase the coefficients of elasticity without limit till the body becomes practically rigid. For such a body, though apparently rigid, is in reality capable of internal vibrations, and these of an infinite variety of types, so that the body has an infinite number of degrees of freedom. The same objection applies to all atoms constructed of continuous, non-rigid matter, such as the vortex-atoms of Thomson. Such 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.[44]

In short, the mechanism that made vortex molecules suitable for an understanding of spectra, led to disaster in the area of gas theory. Thomson's view with regard to gas theory was not widely accepted and rarely referred to by experts in the field. It did, however, find strong support in J. J. Thomson, who in works of 1883 and 1885 developed it into a quantitative theory of gases (J. J. Thomson 1883, pp. 109–113; J. J. Thomson 1885). His answer to the problem of specific heats pointed out by Maxwell was to deny the validity of the equipartition theorem. In accordance with the elder Thomson, he argued that molecular collisions were rare and not very important. Gas pressure, he asserted, was produced as vortex rings slowed down and expanded upon approaching a surface, and then recoiled at slow speed. He found a striking difference between the Maxwell-Boltzmann theory and the alternative vortex theory of gases: whereas, according to the former theory, the average velocity of molecules increases with the temperature, in the alternative vortex theory the velocity decreases. J. J. Thomson suggested that experiments on thermal effusion might decide between the two views, but apparently such experiments were never made. He further found a correction to the gas law, namely that $pV = \text{const} \times T$ had to be replaced by $pV = \text{const} \times T - b$, where b is a small term. This, he wrote,

> agrees with the results of Regnault's experiments; thus the vortex atom theory explains the deviation of gases from Boyle's law. In this respect it compares favourably with the ordinary theories, for if we assume the molecules to be elastic spheres we cannot explain any deviation from Boyle's law, while if we assume that the atoms repel one another with a force varying inversely as the fifth power of the distance, the deviation ought to be the other way, i.e. *pv* ought to be greater than the value given by Boyle's law, which is contrary to experimental results.[45]

With J. J. Thomson's works, the vortex theory of gases was developed into a serious alternative to the ordinary collision theory of Maxwell and Boltzmann. However, the alternative does not seem to have attracted much interest or played any significant role in the discussions of gas theory that took place in the period.[46] For example, at the meeting of the British Association in 1885, the kinetic gas theory was discussed by, among others, Thomson, Hicks, Crum Brown, Reynolds, and J. J. Thomson (*Nature 32* [1885], pp. 352, 533–535). Apparently none of them suggested introducing the vortex gas theory as an alternative to the problematic Maxwell-Boltzmann theory. Nor was the alternative mentioned by another vortex atom sympathiser, Tait, in his detailed studies of gas theory between 1886 and 1892 (Tait 1898–1900, vol. 2, pp. 124–211). After 1885, nothing more was heard of the vortex theory of gases.

If the vortex atom theory failed to explain gravitation, so did other theories of a mechanical kind. And there were a multitude of such theories, which only had in common that they were short-lived and, most of them, decidedly speculative and arbitrary (Zenneck 1903, pp. 53–65; Rosenberger 1886–90, vol. 3, pp. 579–600; Roseveare 1982, pp. 101–112). Several of these theories relied on the ether (or several ethers), typically by assuming ether atoms that would act as Lesage's ultramundane corpuscles, but none of them were based on a continuous ether filled with elastic vortex atoms. Closest, perhaps, came hydrodynamic pulsation theories of the kind that the Norwegian physicist Carl Anton Bjerknes proposed from the early 1870s onwards and that caught the interest of several British scientists, including Hicks, Leahy, and Pearson. According to Bjerknes, theory as well as experiment showed that two spherical bodies immersed in an incompressible fluid pulsating in phase would attract each other in accordance with an inverse square law.[47] He did not identify his fluid with the ether, nor his spherical bodies with vortex atoms, but in Britain, Hicks and Leahy were inspired by his work to do just that.

Yet, gravitational theories of the Bjerknes type only remained sketches. And although they may be said to be consistent with the vortex atom programme, they were independent of it. On the whole, and to paraphrase Thomson, the verdict of the vortex atom theory with regard to gravitation could only be *guilty*.

6. Vortex chemistry

The vortex theory's failure in accounting for gravitation was to a large extent compensated by what was widely conceived as its success in providing some sort of explanation of chemical phenomena at the microscopic level. As mentioned, Tait's knot theory, an outgrowth of the vortex atom, had to a limited extent been related to the study of chemical phenomena. British chemists were from an early date aware of the vortex atom, but most of them did not consider ideas of atomic structure to be of chemical interest. After all, it was still a matter of debate if atoms existed in the first place. In a paper of 1869, Alexander Williamson, professor of chemistry at University College, London, spoke for the majority of chemists when he said about atoms: "They may be vortices, such as Thomson has spoken of; they may be little hard indivisible particles of regular or irregular form. I know nothing of it" (Williamson 1869, p. 365). By implication, he did not care. The possibility of using the vortex atom hypothesis for chemical purposes had its origin in an experiment that Kelvin (William Thomson) learned about in 1878.[48]

On April 15, 1878, Kelvin read a lecture to the Royal Society of Edinburgh, "On vortex vibrations, and on instability of vortex motions." The lecture was never published, but it made Kelvin perceptive to a brief paper that appeared three days later in *Nature* and in which was described an experiment made by an American physicist.[49] Alfred Marshall Mayer, professor at Stevens Institute of Technology in Hoboken, New Jersey, studied the configurations of equally magnetised needles floating on water. Apart from the mutual repulsion, they were attracted to the centre by the horizontal component of the force from a large magnet of opposite polarity. Placing up to twenty needles in the water, he observed how they vibrated and arranged themselves in stable configurations such as triangles and squares (figure 3). Mayer mainly used the experiment didactically, to illustrate to his students phenomena such as allotropy and isomerism, but for Kelvin it had a deeper significance. As soon as he became aware of the experiment, he repeated it

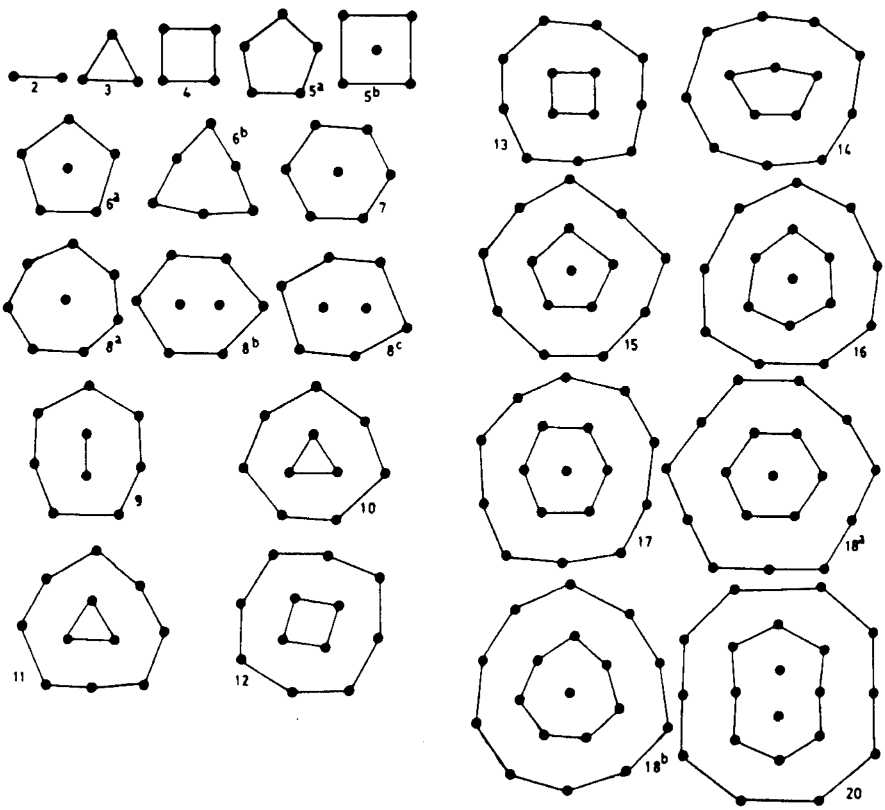

Fig. 3. Mayer's configurations of floating magnets. Source: Mayer 1879, pp. 100–101.

with the purpose of illustrating "the kinetic equilibrium of groups of columnar vortices revolving in circles round their common centre of gravity" (Thomson 1878 [MPP 4, pp. 135–140]). As he noted, there was a nearly perfect analogy between the forces structuring the magnets and those governing a system of vortices. He could therefore use the experiment to find equilibrium configurations for vortices that could only be found mathematically after lengthy calculations, if at all. For example, he had not calculated whether five vortices of equal strength would remain stable in a pentagonal arrangement, "But Mr. Mayer's experiment, showing it to be stable for the magnets, is an experimental proof that it must be stable for the vortices."

Mayer's experiment soon became popular, both for didactic and scientific reasons. It was developed into a variety of new versions, for example with electrified rods replacing the magnetised needles (Monckman 1889; Peirce 1878; Mayer 1879). For a larger number of needle magnets, the configurations found by Mayer exhibited a peculiar periodicity, with certain groups occurring in several of the systems of inscribed polygons. This feature was eventually taken as an illustration of the periodic system, but in the early days neither Mayer, Kelvin nor other scientists occupied with the magnet experiment pointed out the similarity between the patterns of magnetised needles and the structure of the periodic system. This may appear surprising, but possibly it merely reflects that Mendeleev's system was not well known at the time and rarely mentioned even in the chemical literature (Brush 1996). J. J. Thomson seems to have been the first scientist to make the connection, namely, in 1890: "If we examine [the figures of equilibrium] we see that as the number of molecules increase there is a tendency for certain peculiarities to occur . . . Thus, if we regard the elements as made up of one substance and increasing atomic weight to indicate a [?new] number of atoms of this primordial element then as the number of atoms is continually increased certain peculiarities in their structure will recur which would probably be accompanied by a recurrence of certain properties."[50]

Thomson wrote some of his first scientific papers on the vortex atom theory[51] and in 1882 fully investigated the connection between Mayer's experiment and atomic structure. Late in his life, he recalled that as a young man he was greatly interested in vortex rings, a subject that attracted him by its "Spartan simplicity." The main result of his interest was the Adams Prize essay, which "involved long and complicated mathematical analysis and took a long time" (J. J. Thomson 1936, pp. 94–95). *A Treatise on the Motion of*

Vortex Rings was indeed a mathematical tour de force, even by the high standards of Thomson's fellow vortex theorists. In the introduction, he sang the praise of the vortex atom in tunes very similar to those of Maxwell and Kelvin:

> This theory cannot be said to explain what matter is, since it postulates the existence of a fluid possessing inertia; but it proposes to explain by means of the laws of Hydrodynamics all the properties of bodies as consequences of the motion of this fluid. It is thus evidently of a very much more fundamental character than any theory hitherto started . . . Since this theory is the only one that attempts to give any account of the mechanism of the intermolecular forces, it enables us to form much the clearest mental representation of what goes on when one atom influences another.[52]

In his analysis of several interacting vortex rings, the most original part of the work, Thomson examined the stability of cylindrical vortices arranged at equal intervals round the circumference of a circle, that is, the same problem that Kelvin had used Mayer's experiment to illustrate. Using standard perturbation theory, adopted from celestial mechanics, he found after lengthy calculations a general formula that expressed the conditions of stability. His general method was to express the perturbed coordinates as $\exp(pt)$ and then determine the p-coefficients. If the coefficients were imaginary, the equilibrium system would have periods of vibration and be stable; for real coefficients, a disturbance would lead to instability. Thomson found in this way that configurations with $n=2$, 3, 4, 5 and 6 vortices would be stable, but that seven vortices on the same ring could not form a stable system. For larger n, where analytic calculations would be next to impossible, he referred to Mayer's magnet experiment; and he noted that his calculations for smaller n were in approximate agreement with these results. However, Thomson's calculations proved the hexagon to be stable, which contradicted the answer found experimentally by Mayer.

For simple systems, Thomson considered both vortices of equal strengths linked together and links of unequal vortices. He realised that there was no reason why the vortices should be equal, but for reasons of simplicity assumed that "the atoms of the different chemical elements are made up of vortex rings of the same strengths." The assumption facilitated the calculations, and it also agreed with Thomson's monistic inclination toward a unified theory of matter. At the time implicitly, and soon also explicitly, Thomson adhered to the unitary view of matter associated with William Prout's hypothesis. According to this hypothesis, dating back to 1815, all chemical elements consist of subatomic units, which Prout originally took to be hydrogen atoms. In a wider sense, the Proutean or neo-Proutean hypothesis postulates the existence of some form of ‘‘protyle’’, possibly smaller than the hydrogen atom, as the basic building block of all matter.

The novelty of Thomson’s work did not so much lie in the complexity of its calculations as in the author’s serious attempt, in the last part of the essay, to establish the vortex atom hypothesis as an empirically useful theory with applications both in physics and chemistry. Apart from the vortex gas theory, mentioned above, Thomson endeavoured to demonstrate how chemical valency and actions could be understood on a vortex basis (Sinclair 1987). He pictured the combination of gaseous bodies as an association of two vortex rings, when one overtakes the other. If so, they would not separate but continue to circulate in and out of one another. ‘‘We may suppose,’’ Thomson wrote, ‘‘that the union or pairing in this way of two vortex rings of different kinds is what takes place when two elements of which these vortex rings are atoms combine chemically; while, if the vortex rings are of the same kind, this process is what occurs when the atoms combine to form molecules.’’[53] If two paired vortex rings were disturbed by neighbouring rings, their radii would be changed and they would briefly separate. ‘‘We are thus led to take the view of chemical combination put forward by Clausius and Williamson, according to which the molecules of a compound gas are supposed not to always consist of the same atoms of the elementary gases, but that these atoms are continually changing partners.’’[54]

Thomson explained that an important parameter in the process was the ratio of the time the atoms remained together and the time they stayed free, which would determine whether chemical combination occur or not. Thus, as he noted in a subsequent paper, ‘‘The value of this ratio would afford a very convenient measure for the affinity of the constituents of a compound for each other’’ (J. J. Thomson 1884, p. 235). He further suggested that this line of reasoning would explain that the dissociation temperature is higher than the formation temperature; and also that it indicated that chemical combination could only occur in a certain temperature interval. Thomson introduced his lengthy paper of 1884 with long quotes from his *Treatise*, but in fact his equations did not depend specifically on the vortex atom theory. The paper outlined a theory of thermal dissociation of gases, in which the main cause was not the increased number of collisions, but in some cases, rather the separation between two vortex rings that would automatically take place even if they remained undisturbed. Thus, dissociation would occur even without collisions. In his analytical treatment of simple cases, Thomson derived expressions that could be compared with experimental data. For example, in the case of iodine ($I_2 \rightleftharpoons 2I$), he found good agreement with data on vapour pressures and argued that, in this case, the paired time was approximately nine times that of the free time. It was however unclear to which extent the agreement was based on the assumption of vortex atoms. "I shall take the vortex-atom theory of gases as the basis of the following remarks," he wrote in a paper of 1883; and then he added, significantly, "though much of the reasoning will hold whichever theory of gases be assumed" (J. J. Thomson 1883c, p. 427).

Thomson's attempt at a vortex theory of valency was based on the assumption that "each vortex ring in the atom would correspond to a unit of affinity in the chemical theory of quantivalence." He took the valency of an element to be given by the ratio of the number of links in the atom to those in the hydrogen atom. As far as gases were concerned, he concluded that if the vortex rings were linked together in the most symmetrical way, "then no element could have an atom consisting of more than six vortex rings at the most, so that no single atom would be capable of uniting with more than six atoms of another element so as to form a stable compound." This prediction he found to agree nicely with chemical knowledge, as there were no examples of gaseous compounds of the type AB_n with $n>6$; and only one case of $n=6$ was known, namely, tungsten chloride (WCl_6). Thomson further applied his vortex notion of valency to other elements and got, if only with difficulty and by making some arbitrary assumptions, a reasonable agreement with known data. However, he was led to place nitrogen and phosphorus as monovalent elements, which obviously disagreed with the properties of these elements. At the end of the essay, he summarised as follows:

> According to the view we have taken, atomicity corresponds to complexity of atomic arrangement; and the elements of high atomicity consist of more vortex rings than those whose atomicity is low; thus high atomicity corresponds to complicated atomic arrangement, and we should expect to find the spectra of bodies of low atomicity much simpler than those of high.

Again he found his expectation to be matched by experience.

Thomson's explanation of valency and chemical combination was later reviewed by FitzGerald. Although the Irish physicist was fully aware of its weaknesses, he felt that there was "something very striking in the numerical coincidence between the number of bonds required for chemical combinations and the number of vortices that can be absorbed into a single system of this kind" (FitzGerald 1896b [FitzGerald 1902, p. 349]). FitzGerald attempted to extend the theory by sketching a possible vortex-atom explanation of the optical asymmetry of certain organic molecules, but admitted that his attempt provided no more than a crude analogy. He also considered another classical problem of structural chemistry, the benzene molecule, and with the same unsatisfactory result. Yet he believed that such problems were not outside the reach of a future vortex theory: "Too little, however, is known of the possible combinations of vortex filaments to be at all sure whether six filaments, each with an attendant satellite, could not very well circulate round one another in a stable group."[55]

Thomson also developed a qualitative theory of electric discharges in gases, a topic that would eventually lead him to the peak of his career, the discovery of the electron. His early understanding of electric discharges was solidly based on his vortex theory of gases, supplied with Maxwell's notion of the electric field as a velocity distribution in the ether. On Thomson's theory, the energy of two separate vortex rings was higher than that of the vortex molecule, and so energy would be absorbed from the ether in dissociating the molecule; when the ring atoms recombined, energy would be emitted in the form of heat. According to Thomson, chemical decomposition was not an accidental attendant of the electrical discharge, but a necessary cause for it. Contrary to other researchers, who considered the vacuum to be a perfect conductor, Thomson was led to the view that it had an infinite electric strength, that is, was an insulator. His vortex-based theory of discharge led to critical comments from the Manchester physicist Arthur Schuster and an exchange of letters between the two professors.[56]

Kelvin welcomed Thomson's *Treatise*. In a letter to George Darwin of December 30, 1884, he wrote, "I am becoming hot on vortex motion through having . . . J. J. T's book at hand" (Sharlin 1979, p. 212). It was reviewed by Osborne Reynolds, who had much praise for the work, but also expressed some scepticism toward "the vortex atoms [which] are very slippery things." Reynolds's interest in vortices was mainly restricted to practical aspects of hydrodynamics and did not include their role as possible constituents of matter. In a Royal Institution lecture of 1877, he mentioned the vortex atom, but only briefly (Reynolds 1970). Reynolds focused his critical comments on Thomson's theory of vortex gases, for if the vortex theory failed to explain the phenomena of gases, this "would appear to be crucial as regards its unfitness as an atomic theory." He suggested that the velocity of sound would afford a "crucial test" and that the theory did not pass the test. For, he argued, it followed from the vortex atom theory that the velocity of sound was limited by the mean velocity of the vortex atoms. "And since Mr. Thomson has shown that this mean velocity diminishes with the temperature, while experimentally it is found that the velocity of sound increases as the square root of the temperature, it appears that the verdict must be against the vortex atom theory" (Reynolds 1883). Reynolds's criticism received support from FitzGerald, who further found that the vortex atom theory, in J. J. Thomson's version, raised difficulties as to the supposed ether drag caused by the motion of the earth. "Simple ring vortices in a perfect liquid can hardly be an adequate theory," he concluded.[57] FitzGerald was not against the vortex theory, but he found the original version of Kelvin and its elaboration by Thomson to be insufficient.

The vortex-chemical theory never made much of an impact on chemistry, but in Britain it was not completely ignored. Thus, the Cambridge chemist Matthew M. Pattison Muir responded favourably to Thomson's theory, which he included in his textbook in theoretical chemistry.[58] Also another Cambridge scientist, the chemist and spectroscopist George Liveing, found Thomson's view appealing and the vortex atom a possible progress compared to the currently accepted notion of atoms. In an 1882 address to the British Association, he referred to the well known difficulties of the rigid atom; "but now the vortex atom, whether we think it probable or not, at least gives us a standing ground for the assertion that the supposed impenetrability of matter, and the curious compound of nucleus and atmosphere which had been invented to account of elasticity, are not necessary assumptions."[59] Two years later, the eminent Manchester chemist Henry Roscoe followed up on Liveing's theme, the increased value of physical theory for chemistry. As a subject of interest to the chemists he mentioned "the vortex-ring constitution of matter thrown out by Sir William Thomson, and lately worked out from a chemical point of view by J. J. Thomson of Cambridge" (Roscoe 1884, p. 666). Roscoe summarised the essence of the theory and added cautiously that it yet had to be seen if it agreed with chemical facts.

In the late 1880s, J. J. Thomson seems to have lost interest in his theory of vortex chemistry, as indirectly shown by his 1888 monograph on theoretical chemistry (J. J. Thomson 1888. See also Sinclair 1987 and Chayut 1991). In this work, he gave a thorough account of dissociation and chemical equilibrium, partly along the line that he had followed four years earlier. But now the vortices had disappeared, and the book just did not mention vortex atoms. At about this time he became involved in a minor controversy with Wilhelm Ostwald, the famous German chemist who was about to establish the new cross-disciplinary science of physical chemistry. In his massive textbook in general chemistry, Ostwald objected to Thomson's vortex-based theory of gas reactions. His criticism concerned Thomson's concept of the ratio between paired and free times, which he found to be obscure, not the vortex atom itself. In reply, an insulted Thomson restated his idea in popular language and attacked Ostwald's account of gas reactions for being sloppy. In the reply, he mentioned that "one of the reasons for undertaking the investigation was, that an eminent spectroscopist had mentioned to me that there was spectroscopic evidence to show that the molecules got split up independently of the collisions, and . . . I wished to see if I could get any evidence of this from the phenomena of dissociation."[60]

In the United States, a few chemists referred to the vortex atom theory, and that even at a time when it had long been abandoned by its British founders. Harry Clary Jones, a Leipzig-trained physical chemist at Johns Hopkins, referred positively to the theory in a textbook of 1902; and as late as 1904, Francis Venable, another American chemist, praised the vortex atom as a possible foundation for a theory of matter: "It would seem to be the culmination of centuries of work, not fancy, and to embody the explanation of all facts known – chemical, physical and mathematical."[61] In Russia, Dmitri Mendeleev was not a friend of Victorian ideas of primary matter and the complexity of chemical elements, such as he made clear in his 1889 Faraday Lecture (Mendeleev 1889). He may have had included the vortex atom in what he called utopian speculations. At any rate, he was well aware of the theory, which he chose to include in one of the lengthy footnotes of his *Principles of Chemistry*. Mendeleev seems not to have been entirely unsympathetic to "the oft-revived vortex hypothesis," which he traced back to Descartes. After a brief review of the theory and the smoke ring experiments associated with it, he offered his opinion, which was based on a partly incorrect understanding of the nature of the vortex atom:

> The vortex hypothesis has been established in our times, but it has not been fully developed; its application to chemical phenomena is not clear, although not impossible; it does not satisfy a doubt in respect to the nature of the space existing between the rings (just as it is not clear what exists between atoms, and between the planets), neither does it tell us what is the nature of the moving substance of the ring, and therefore for the present it only presents the germ of an hypothetical conception of the constitution of matter, consequently, I consider that it would be superfluous to speak of it in greater detail. (Mendeleev 1891, vol. 1, p. 217).

Mendeleev came to conceive the world ether as corpuscular and of the nature of a gaseous chemical element with atomic weight much less than hydrogen's. In the essay where he introduced this unorthodox idea, he referred briefly to "the fact that the atoms of modern science have often been explained by vortex rings."[62] But Mendeleev remained hostile to monism and objected to the conception of ether (or ether vortex atoms) as a primary substance.

Thomson had assumed vortex rings as the basis for his theory, but there were other ways in which chemical combination could be illustrated by means of vortex atoms. Thus, in 1895 Hicks indicated that a similar explanation might be obtained on the basis of Hill's spherical vortices, an idea that greatly appealed to him. Hicks found that one spherical vortex might swallow up another and retain it inside in an equilibrium state. This mechanism, he suggested, "seems to open up another mode of chemical combination," i.e., an alternative to Thomson's theory (Hicks 1895a, p. 600; Hicks 1895b). Three years later, Hicks developed his ideas into a new theory of spiral vortices, in which the variation of a certain parameter led to systems of "families" of vortex aggregates exhibiting a periodicity. The results, he wrote, "irresistibly suggests curves connected with the physical properties of the elements." In particular, he found that his analysis might illuminate "how the fusibilities of the elements alter periodically with the atomic weights" (Hicks 1898, p. 336). Among Hicks's conclusions were that metals and non-metals were distinguished by different vortex aggregates, and also a vortex atom explanation of the different fusibilities, or degrees of electropositivity, of the elements. Contrary to Thomson's vortex chemistry, Hicks's theory was completely ignored by the chemists.

7. From vortex atom to electron

By around 1890, J. J. Thomson lost confidence in the vortex atom as a realistic theory of matter, but he did not abandon it completely. First, the vortex atom theory continued to function heuristically, as an exemplar both in a methodological and an ontological sense. Second, the theory provided him with powerful mathematical and conceptual tools that he would utilise in his later theory of the electron atom. Third, and perhaps most important, as Thomson became increasingly involved with electrolysis and electrical discharges in gases, he continued to use the vortex picture to form a conception of the electromagnetic field that included the non-Maxwellian concept of discrete electrical charges. In short, in the process that led to the discovery of the electron, the vortex atom played an indispensable role (Kragh 2001).

This role is testified in Thomson's autobiography, where he wrote: "One thing that appealed to me was the analogy between the properties of vortex filaments and those of the lines of electric force introduced by Faraday to represent the electric field . . . In fact, it seemed that even if the vorticity did not suffice to represent matter it might yet give a very useful representation of the electric field" (J. J. Thomson 1936, pp. 94–95). Thomson developed his idea of a vortex tube model of electromagnetism in a paper of 1891 as well as in his book on electromagnetic theory published two years later (J. J. Thomson 1891; J. J. Thomson 1893). The basic entity was the unit tube of force with a strength equal to the electrolytic unit of charge; the tubes either formed closed loops or terminated on material atoms. In the latter case, the atom from where a tube started would be positively charged and the atom on which it ended would have a negative charge. "In this respect," he noted, "the tubes resemble lines of vorticity in hydrodynamics, as these lines must either be closed, or have their extremities on a boundary of the fluid" (J. J. Thomson 1891, p. 150; similarly in J. J. Thomson 1893, pp. 3–4). Thomson had briefly suggested a picture like this as early as 1883, in his *Treatise*, (pp. xi, 13), and his elaboration of it in the early 1890s was indebted to works by John Henry Poynting, Arthur Schuster, and William Hicks. In a paper of 1888, Hicks had examined a vortex model of static electricity in which the lines of force between oppositely charged bodies were represented by vortex filaments (Hicks 1888). Thomson did not refer to Hicks's work, but there is little doubt that his analogy between unit tubes and vortex filaments relied on it. In a work of 1895, Thomson regarded "a Faraday tube as a bundle of vortex filaments" and illustrated the interaction between charged particles by means of hydrodynamic calculations copied from the vortex theory of atoms (J. J. Thomson 1895, p. 520). His "gyrostatic" model, not meant to be more than an analogy, included the Proutean idea that atoms are composite and that their energy and charge are determined by the number and configurations of the components. In 1895 the components were subatomic gyrostats, as they had earlier been vortex rings; and two years later they would become electrons (or corpuscles, as Thomson named his elementary particles).

In his famous work of 1897, soon to be celebrated as the discovery of the electron, Thomson did not refer to the vortex atom. Yet he most likely had the model in his mind, especially in his sketch of a theory of atomic constitution based on equilibrium states of a large number of corpuscles. As he noted, "the problem of finding the configurations of stable equilibrium for a number of equal particles acting on each other according to some law of force . . . is of great interest in connexion with the relation between the properties of an element and its atomic weight" (J. J. Thomson 1897, p. 313). This was precisely the problem that he had considered earlier within the framework of the vortex atom. As he had done in his 1883 essay, he now referred to Mayer's experiment as a substitute for the abstruse calculations and cited Mayer's polygonal arrangements for up to forty-two magnets as a striking analogy to the periodic system. In his mature atomic model of 1904, he performed detailed stability calculations that were essentially modified versions of those he had published in his *Treatise* twenty-one years earlier. For more than eight electrons, he devised a new approximation method by means of which he found the stability configurations of electron atoms with up to sixty electrons (J. J. Thomson 1904).

Joseph Larmor was another British physicist who contributed very importantly to the discovery of the electron, a term he borrowed from Stoney and used as early as 1894 to signify a singularity in the electromagnetic ether. Although Larmor's electrons were introduced to explain electromagnetic and optical phenomena and not primarily as constituents of matter, did he not ignore their role as building blocks of chemical atoms. And although they emerged on the ruins of the vortex atoms, so to speak, the two concepts had much in common.

Contrary to many of his fellow British mathematical physicists, Larmor did not contribute to the vortex atom research tradition, yet since the early 1880s he was thoroughly familiar with the theory and found it to be appealing.[63] The complex road that led him to the electron started with his insight that it might be possible to connect "three fundamental theories," namely, "Maxwell's theory of electric phenomena, . . . Lord Kelvin's vortex atom theory of matter, and the purely dynamical theories of light and radiation that have been proposed by Green, MacCullagh, and other authors" (Larmor

1893 [Larmor 1929, vol. 1, pp. 389–413, on p. 411]). Larmor used the vortex atoms to discuss the relation between ether and matter in areas such as refraction, dispersion, and the optics of moving bodies. In his great paper of 1894, he suggested a vortex-like conception of the atom, namely this:

> An atom . . . would be mathematically a singular point in the fluid medium of rotational elastic quality. Such a point may be a centre of fluid circulation, and may have elastic twist converging on it, but it cannot have any other special property besides these; in other words this conception of an atom is not an additional assumption, but is the unique conception that is necessarily involved in the hypothesis of a single rotationally elastic aether. (Larmor 1894 [Larmor 1929, vol. 1, pp. 414–535, on p. 474]).

However, he soon encountered troubles with the vortex atoms. For example, he had to add electrical charge to the vortices to account for molecular properties, and there were problems with explaining diamagnetism. Consequently, he was forced to admit that the vortex analogy failed for magnetism, ‘‘And when we consider individual molecules, the question is also mixed up with the unsolved problem of the nature of the inertia of a vortex molecule’’ (p. 506). Partly as a result of FitzGerald’s objections, he decided to abandon the vortex atom approach and start on a fresh, now with charged primordial atoms or ‘‘monads’’, that is, electrons. In a lengthy postscript added August 13, 1894, Larmor reflected on the relationship between the old vortex atom theory and the new electron theory. With the exception of mathematical investigations of stability, ‘‘The original vortex-atom theory of matter has scarcely had a beginning made of its development,’’ he wrote. ‘‘How far a theory like the present can take the place of or supplement the vortex theory, is therefore a very indefinite question.’’ He spelled out the difference between the two theories as follows:

> A guiding principle in this discussion has been to clearly separate off the material energy involving motions of matter and heat, from the electric energy involving radiation and chemical combination, which alone is in direct relation to the aether. The precise relation of tangible matter, with its inertia and its gravitation, to the aether is unknown, being a question of the structure of molecules (p. 530).

For Larmor, the vortex atom played an important if only temporary role in guiding him to the electron theory. By 1895 he no longer considered the vortex theory to be a candidate for the physics of the real world. It was praiseworthy from a methodological point of view, but merely as a "vivid illustration" as he expressed it in 1900 before the British Association (Larmor 1900a, p. 625). In the same year, he published his important monograph *Aether and Matter*, like Thomson's *Treatise* based on an essay awarded the Adams Prize. Larmor gave a comprehensive account of his electron theory in which there was only little trace of the vortices in which the theory had originated. Yet he indicated that the electron theory could be viewed as a more satisfactory version of the vortex atom theory, "which has exercised much fascination over high authorities on molecular physics." His electron theory of matter was superior to the vortex theory, but essentially of the same type: "The atom of matter possesses all the dynamical properties of a vortex ring in a frictionless fluid, so that everything that can be done in the domain of vortex-ring illustration is implicitly attached to the present scheme" (Larmor 1900b, pp. 161 and 165–166). The similarity was one of spirit, rather than substance. Larmor distinguished between two types of fundamental theories, a dualistic and a monistic one. According to the dualistic view, which he rejected, ether and matter were separate entities or ether was identified with a species of matter. Far more satisfactory was "thoroughgoing constitutive theories of the aether, like the vortex-atom theory and the one above sketched, . . . [according to which] the aether is fundamental, and its properties must be adapted to be consistent, by themselves alone, with the whole range of physics" (p. 336).

Aether and Matter was reviewed by FitzGerald, who pointed out one remarkable difference between Larmor's electron theory and the vortex atom theory. If matter consists of clusters of electrons in orbital motion, "it certainly makes it probable that the transmutation of elements is a possible development of chemistry, while a structure such as that of knotted vortices would make it improbable that we would ever be able to untie them and thus transmute one atom into another" (FitzGerald 1900, p. 265). Three years earlier, in a comment on J. J. Thomson's conception of electron atoms, FitzGerald had similarly warned against the alchemy that seemed to follow from such a model of the atom (FitzGerald 1897, p. 104).

8. The end of a research programme

Still in the early years of the twentieth century, scientific writers referred occasionally to the vortex atom theory. But references were few and it was obvious that, as a research field, the theory was dead at the time. In fact, by then the theory had been nearly dead for more than a decade. As we have seen, since its origin in 1867, it was developed and advocated by three physicists in particular. Of these, Kelvin and Thomson abandoned the theory about 1890, while Hicks continued to defend and develop it for another decade.

Lord Kelvin, who as William Thomson had originally pioneered the vortex atom theory, vacillated over the years in his support of it. At times he advocated it enthusiastically, at other times he ignored it or rather emphasised its difficulties. He considered it to be a very attractive model, but nonetheless a model only, and in accordance with his general style of physics he had no problem in changing between models of atoms or the ether. Kelvin realised that the vortex model was probably unable to explain gravitation, but still in 1884 – possibly as a result of the revived interest in the theory caused by Thomson's work – he had confidence in the theory. At the meeting of the British Association this year, he discussed the kinetic theory of gases. "We cannot," he said, "avoid the question of impacts, and of vibrations and rotations of the molecules resulting from impacts and we must look distinctly on each molecule as being either a little elastic solid, or a configuration of motion in a continuous all-pervading liquid."[64] In Kelvin's steps toward a kinetic theory of matter, the vortex atom formed a crucial link. At the end of his address, as quoted earlier (section 5), he expressed as his belief that the vortex atom was the best offer of an atomic theory of matter.

It is difficult to say exactly when Kelvin lost confidence in the vortex atoms, but it probably occurred gradually during the last half of the 1880s. Towards the end of his life, he confided that, "More than thirty years ago I abandoned the idea that the ether is a fluid presenting appearances of elasticity due to motion, as in collisions between Helmholtz vortex rings" (Thomson 1907 [MPP 6, pp. 235–243, on p. 236]). This would mean that he abandoned the theory latest by 1877, which does not agree with the historical data. It is plausible, as suggested by David Wilson, that Kelvin meant twenty rather than thirty years (Wilson 1987, p. 178).

Wilson's suggestion is corroborated by a paper Kelvin read to the Royal Society of Edinburgh in 1904 on waves in deep waters. In a footnote he added:

> After many years of failure to prove that the motion in the ordinary Helmholtz circular ring is stable, I came to the conclusion that it is essentially unstable, and

> that its fate must be to become dissipated as now described. I came to this conclusion by extensions not hitherto published of the considerations described in a short paper entitled "On the stability of steady and of periodic fluid motion" in the Phil. Mag. for May 1887.[65]

There exists some further if not very precise evidence of Kelvin's reasons for abandoning the vortex atom. First, in reply to a request from the American physicist Silas Holman, he wrote as follows:

> 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 . . . I wish I could say a great deal more on the subject which has never ceased to interest me. We may expect that the time will come when we shall understand the nature of an atom. With great regret I abandon the idea that a mere configuration of motion suffices. (Holman 1898, p. 226; Merz 1965, vol. 2, p. 182).

Second and last, in 1899 Kelvin wrote to the Dutch physicist Willem Julius that, "In respect to all these Ether Theories, my own Vortex-Atom included, I must unhappily rank with Mephistopheles, 'der Geist der stets verneint' . . . I cannot feel any happiness in any ether-theory which does not account for electro-static force and ordinary magnetic attraction . . ." (Smith and Wise 1989, p. 489). It thus seems that Kelvin gave up the vortex theory of atoms about 1887, partly because he reached the conclusion that the vortex ring was unstable and partly because the theory seemed unable to account for phenomena such as magnetism and gravitation.[66]

Tait, another of the vortex poineers, stopped referring to the vortex atom in the mid 1880s. Although he still found the theory attractive (Tait 1885, pp. 19–21), he seems to have lost confidence in it. "I am going to *smash* Vortex-atoms at R.S.E. (Jan. 7) so I bid you to hearken," he wrote to Kelvin on December 20, 1883 (Kelvin Papers, Correspondence, T33, Cambridge University Library). Tait's paper to the Royal Society of Edinburgh was not published, but according to the abstract it "contained a discussion of the consequences of the *assumption* of continuity of motion throughout a perfect fluid . . . on which W. Thomson founded hits theory of vortex-atoms." Tait appearently found the vortex atom theory to be untenable because it "involves action applied simultaneously to all parts of the fluid mass, not to the rotating portion alone" (Tait 1898–1900, vol. 2, p. 103).

The case of J. J. Thomson is somewhat different. Latest by 1890, he seems to have quietly left the vortex atom, possibly under the impact of its failure as the foundation of a new gas theory. Yet, as we have seen, the vortex atom, and more generally the vortex picture, continued to inspire him in the research that in 1897 led to the discovery of the electron. Moreover, even after having suggested the electron atom, he made no secret of his fascination by the vortex model. As far as he was concerned, the theory was not really proved wrong, only was the vortex atom no longer a very useful concept in explaining physical phenomena. However, heuristically and as a mental picture it remained as strong as ever. His reply to Holman was markedly more optimistic than that given by Kelvin:

> With reference to the Vortex-atom Theory, I do not know of any phenomenon which is manifestly incapable of being explained by it; and personally I generally endeavour (often without success) to picture to myself some kind of vortex-ring mechanism to account for the phenomenon with which I am dealing. In lectures and papers, however, I generally content myself with an illustration which, though it has no claim to the fundamental character of one based on vortex motion, is easily conceived as handled by the mind, and so is more adapted as a guide to research. I regard, however, the vortex-atom explanation as the goal at which to aim, though I am afraid we know enough about the properties of molecules to feel sure that the distribution of vortex motion concerned is very complex. (Holman 1898, p. 226).

It is noteworthy that whereas Kelvin came to reject the vortex atom theory because it did not provide an explanation of various physical phenomena, Thomson defended its virtues by the argument that it was not "manifestly incapable" of doing so. He presumably meant that in principle, if not (yet) in practice, the theory could provide the wanted explanations.

Again, in his comprehensive 1907 account of the electron atom, *The Corpuscular Theory of Matter*, Thomson did not simply present his new atomic model as a definite progress with respect to the older model. A progress it was, but not unquestionably so. As Thomson admitted, his new atomic theory "is not nearly so fundamental as the vortex theory of matter, . . . [where] the difference between matter and non-matter and between one kind of matter and another is a difference between the kinds of motion in the incompressible liquid at various places, matter being those portions of the liquid in which there is vortex motion." Still, although Thomson was greatly attracted by fundamental theories of everything, he was also a pragmatist: "The simplicity of the assumptions of the vortex atom theory are, however, somewhat dearly purchased at the cost of the mathematical difficulties which are met with in its development; and for many purposes a theory whose consequences are easily followed is preferable to one which is more fundamental but also more unwieldy'' (J. J. Thomson 1907, p. 2).

In the same book, Thomson sketched a modified version of his atomic model that, to his own satisfaction, would explain the large mass of the positive sphere in which the electrons moved. (Originally, he had taken the sphere to be massless.) Thomson illustrated this model with ''an example taken from vortex motion through a fluid,'' because it ''may make this idea clearer.'' From the illustration he concluded that ''the system of the positive and negative units of electricity is analogous to a large sphere connected with vortex filaments with a very small one, the larger sphere corresponding to the positive electrification, the small one to the negative'' (p. 151). There is ample evidence that the vortex model continued to appeal to Thomson throughout his life, either as a model of matter or of electricity, or of both. If a revolution in theoretical physics occurred between 1900 and 1930, it did not much affect Thomson's thinking. ''I agree with you about the close connection between electricity and vortex motion,'' he wrote to Lodge. ''I have always pictured a line of electric force as a vortex filament'' (Davis and Falconer 1997, p. 6). The year was 1931.

The same year, Thomson, now aged 74, wrote one of his last research papers in which he aimed to show that ''the Field Equations for a liquid full of vortex filaments are in general of exactly the same type as Maxwell's Equations of the electromagnetic field, and, in special cases, as Schrödinger's Equation.''[67] This he did on a familiar hydrokinetic basis, using formulae and arguments that were very much in the vortex atom style that he had cultivated sixty years earlier. Clearly, he had not forgotten his mechanical heritage. Vortex atoms might exist or not. What mattered to the elder Thomson, was that they were made of ether and that the ether itself was vortical. His devotion to the vortex picture was inextricably linked to his belief in the ether. In 1937, three years before his death, he wrote to a correspondent:

> Again, I differ from you about the value of the conception of an ether, the more I think of it the more I value it. I regard the ether as the working system of the universe. I think all mass momentum and energy are seated there and that its mass momentum and energy are constant, so that Newtonian mechanics apply. I regard the lines of force as linking up what we call matter with ether, that these lines like Vortex rings in air or water carry with them a volume of surrounding fluid much greater than their own volume, so that a part of the mass of the ether is linked up to the body from which the lines start and constitute its mass; this mass has, however,

> been taken from the ether so that the sum of the mass of matter and free ether is constant. (Rayleigh 1943, p. 203).

As an active advocate of the vortex atom, William Hicks held out longer than Kelvin and Thomson. For him, who was perhaps more a mathematician than a physicist, the theory's disappointing record with regard to empirical physics did not count all that highly. Presiding over the British Association at its 1894 meeting, Lord Salisbury had described the ether as a "half-discovered entity" and professed that the nature of the atom – "whether it is a movement, or a thing, or a vortex, or a point having inertia" – remained a complete mystery (Salisbury 1894, p. 8). Hicks begged to disagree. The following year, serving as president of the section of mathematics and physics under the British Association, he used his entire address to review in an optimistic tone the theories of vortex atoms and vortex sponges. He started his address with sketching out his idea of the ultimate laws of physics, what a theory of everything should look like. Hicks's formulation deserves a close reading, which justifies a lengthy quotation:

> The ultimate aim of pure science is to be able to explain the most complicated phenomena of nature as flowing by the fewest possible laws from the simplest fundamental data. A statement of a law is either a confession of ignorance or a mnemonic convenience. It is the latter if it is deducible by logical reasoning from other laws. It is the former when it is only discovered as a fact to be a law. While, on the one hand, the end of scientific investigation is the discovery of laws, on the other, science will have reached its highest goal when it shall have reduced ultimate laws to one or two, the necessity of which lies outside the sphere of our cognition. These ultimate laws – in the domain of physical science at least – will be the dynamical laws of the relations of matter to number, space, and time. The ultimate data will be number, matter, space, and time themselves. When these relations shall be known, all physical phenomena will be a branch of pure mathematics. We shall have done away with the necessity of the conception of potential energy, even if it may still be convenient to retain it; and – if it should be found that all phenomena are manifestations of motion of one single continuous medium – the idea of force will be banished also, and the study of dynamics replaced by the study of the equation of continuity. (Hicks 1895, p. 595).

Hicks's undaunted enthusiasm for the vortex atom model did not mean that he just accepted it as a correct physical theory or that he was not aware of its problems. Thus, as a major problem he mentioned "the difficulty of explaining the masses of the elements on the vortex atom hypothesis" (p. 596). He also realised that only very little progress had been made over the years in the mathematical development of the theory. However, this potential problem he deftly turned into a defence of the theory, for until such progress was made, “we cannot test them [the two vortex theories, of matter and ether] as to their powers of adequately explaining physical phenomena” (p. 597). On the whole, Hicks painted the future of the vortex theory in rosy colours. Although it was probably not an entirely correct theory, in empirical terms, yet it “shows very promising signs” as a candidate for the mythical ultimate theory (p. 605). Certainly, it did not in its present form explain gravitation, but then neither did other theories. Besides, Hicks was hopeful that the theory might still be developed to account for gravitation, either in the version of hollow vortex atoms or his own idea of a vortex cell theory of the ether. In any case, the road toward progress would be to develop still more advanced mathematical methods. “It is at present a subject in which the mathematicians must lead the attack,” he ended his address (p. 606).

During the early years of the new century, Hicks became involved in a brief dispute concerning the Michelson-Morley experiment and its alleged solution by means of the FitzGerald-Lorentz contraction hypothesis. In a careful analysis he argued that this hypothesis, with its basis in the electron theory of Lorentz and Larmor, did not explain the null-result of the ether drift experiments (Hicks 1902; Warwick 1995). Of interest in the present context is that Hicks’s arguments were based in the thought-style that he had acquired during his long occupation with the vortex atom theory. In a letter to Larmor of January 7, 1902, he admitted that, “I have accustomed my mind to hydrodynamical concrete images.” What these images were, in relation to the motion of the earth through the ether, he expressed as follows: “My concept is one in which when a solid moves through the ether, its vortex atoms set themselves on the whole to move thus – the solid is merely a swarm which goes on because its single atoms are moving, and is not itself a thing which carries the atoms. It seems to me likely that in this case the vortex atoms would contract perpendicular to the line of motion, or expand along it” (Warwick 1995, p. 308). Although Hicks probably no longer considered the vortex atoms to be real entities, his “concrete image” of matter was still framed in the language of the vortex theory with which he had worked for more than twenty years.

Silas Holman, professor emeritus of physics at the Massachusetts Institute of Technology, was one of the last physicists who sang the praise of the vortex atom theory as a fundamental theory of physics. In a book of 1898, he devoted nearly forty pages to a review of the theory, which he clearly believed was more than just a beautiful dream, a useful mental picture, or an ingenious romance.[68] Holman chose to focus on the theory's positive merits and tended to disregard the lack of progress during more than a decade. Even in the case of gravitation, a problem for this as well as other theories, he remained optimistic. Holman convinced himself that Kelvin's sketch of a vortex version of Lesage's theory was in "a condition far from hopeless" (Holman 1898, p. 216). For, as he argued, "it is clearly in advance of all predecessors, and has not encountered disproof" (p. 222). Holman summarised his opinion of the vortex theory as follows: "The theory has not yet, it is true, been found capable of satisfactorily accounting for several important classes of phenomena, . . . but this constitutes no disproof . . . [T]he theory must be judged by what it has accomplished, not by what we have not yet succeeded in doing with it. And when thus tested, the theory still remains preeminent" (p. 225). Holman's book was reviewed by Charles Sanders Peirce, who thoroughly disliked it. He included in his criticism the author's confidence in the vortex atom, a theory he branded as "a priori metaphysics."[69]

Holman was not the only American physicist who, about the turn of the century, continued to find the vortex atom theory attractive. Albert Michelson, of ether-drift fame, not only judged the theory to be "most promising," he was also "tempted to think that the [smoke] vortex ring is in reality an enlarged image of the atom." He believed that the vortex theory "ought to be true even if it is not." Like Hicks and Holman, he saw it, or some related ether-based theory, as the best bid for a truly fundamental theory of all physical phenomena. When, hopefully in the near future, the problems of the ether vortices had been solved,

> Then the nature of the atoms, and the forces called into play in their chemical union; the interaction between these atoms and the non-differentiated ether as manifested in the phenomena of light and electricity; the structures of the molecules and molecular systems of which the atoms are the units; the explanation of cohesion, elasticity, and gravitation – all these will be marshaled into a single compact and consistent body of scientific knowledge. (Michelson 1903, pp. 161–163).

The theory's conspicuous lack of progress did not worry him any more than it worried Holman: "The mathematics of the subject is unfortunately very difficult, and this seems to be one of the principal reasons for the slow progress in the theory."

To the extent that the vortex atom theory was still discussed at the turn of the century, it was mostly in a historical and methodological context. To my knowledge, after Hicks's work of 1898 no one attempted to develop the theory scientifically. A representative example of the theory's status may be found in FitzGerald's biographical sketch of Kelvin on the occasion of his jubilee as a professor. FitzGerald included among Kelvin's many remarkable scientific achievements "the suggestive hypothesis of the vortex atom," about which he concluded that, "this hypothesis is the most far-reaching of any that have been proposed as a ultimate structure of matter."[70] Similarly, in a lecture of 1908, on the occasion of Kelvin's death, Lodge paid more attention to the vortex atom theory than to the Scottish physicist's pioneering works in electromagnetism and thermodynamics. The theory of vortex atoms, he said, "would constitute Kelvin's greatest title to fame" (Rowlands 1990, p. 241).

Although Lodge was a chief exponent of the new theory of electrons, he continued to illustrate the constitution of matter by means of vortex images. In another lecture of 1908, at the Royal Institution, he illustrated the immateriality of the ether as follows: "If you tie a knot on a bit of string, the knot is composed of string, but the string is not composed of knots. If you have a smoke or vortex-ring in the air, the vortex-ring is made of air, but the atmosphere is not a vortex ring." He went on asking how it is possible for matter to be composed of ether. The properties of matter, he answered, "can be imitated by a fluid in motion; a statement which we can make with confidence as the result of a great part of Lord Kelvin's work" (Lodge 1970, p. 292). The new developments in atomic physics, such as quantum theory and the proton-electron model of the atom, did not cause Lodge to change his view. In this respect, as in others, he was Thomson's brother in spirit. He conceived the proton as a positive electron, and kept to the vortex imagery. As late as 1925, he wrote about the electron: "Can it be a special kind of whirl, or is it a knot or a strain or a bubble, a hollow or an extra condensation, or what?" (Lodge 1925, p. 175).

The rhetorical strategies associated with the vortex programme did not change much over time. As we have seen, the theory was primarily justified on methodological and aesthetic grounds rather than its ability to explain and predict physical phenomena. From its start in 1867 to its end about 1900, British and American physicists evaluated it in terms such as "suggestive", "far-reaching", "ingenious", and "promising", rather than, say, "correct" or "credible". In his 1883 review of ether physics, Oliver Lodge described the vortex atom theory as "beautiful" and added, just like Michelson would do twenty years later, that it was "a theory about which one may almost dare to say that it deserves to be true" (Lodge 1883, p. 329).

Another persistent theme in the history of the theory was the hope that progress would be effected through mathematics. Given that the theory was immensely complicated from a mathematical point of view, it could always be argued that it was not *yet* understood sufficiently to be physically useful. For example, as early as 1872 Kelvin argued that the difficulties "are . . ., in all probability, only dependent on the weakness of mathematics" (Smith and Wise 1989, p. 425). Tait, too, emphasised the problem of mathematics. Consider the complete analysis of a collision between two vortex rings in the general case where no symmetric motion is assumed. The investigation would, he wrote, "employ perhaps the lifetimes for the next two or three generations of the best mathematicians in Europe." He admitted this was a formidable difficulty, yet not one that should lead to despair. For, "it is the only one which seems for the moment to attach to the development of this extremely beautiful speculation; and it is the business of mathematicians to get over difficulties of that kind" (Tait 1876, p. 298). Much the same message was included in Hicks's 1895 address.

As mentioned, outside Britain and the United States, the vortex theory was practically ignored. Among the few exceptions was the French physicist Marcel Brillouin, an admirer of Kelvin. In an 1887 review of hydrodynamics, Brillouin included a detailed account of "Sir William Thomson's celebrated hypothesis of vortex atoms" and its applications to spectroscopy, gravitation, and gas phenomena (Brillouin 1887, pp. 33–40). Without endorsing the theory, he found it "seductive" and worth a close study. Like many of his British colleagues, he was impressed by its methodological qualities and, and the same time, fully aware of its empirical weaknesses: "A hypothesis is nothing but a means to induce the mind to renounce its habits; whether it is more or less good, is less important; to be useful, it has to be original and susceptible for precision. I think that we cannot deny either the one or the other of these qualities to hold for the atom vortex hypothesis" (p. 40). Six years later another, most eminent French scientist referred critically to the vortex atom. Henri Poincaré gave in 1891–92 a series of lectures on vortex hydrodynamics that were published in 1893 and in which he re-examined Helmholtz's theory within a more general mathematical framework. He concluded that the infinite permanence deduced by Helmholtz was no longer guaranteed and that under certain circumstances the theorem would only be approximately valid. Poincaré was aware of the connection to the vortex atom: ‘‘One has even attempted to find the mechanical explanation of the universe in the existence of these vortical motions. Instead of representing space as occupied by atoms separated by immense distances as compared to their dimensions, Sir William Thomson holds that matter is continuous, but that some portions of it are animated by vortical motions which, as a consequence of Helmholtz’s theorem, must retain their individuality’’ (Poincaré 1893, as quoted in Belloni 1980, p. 64). Apparently he thought that his mathematical analysis had refuted the vortex atom theory. However, Poincaré’s detailed exposition of vortex hydrodynamics did not include further references to this theory.

As early as 1880, Tolver Preston complained that German physicists paid no attention to the theory or, if they did, failed to appreciate its true nature.[71] However, there was at least one exception, the German physicist Oskar Emil Meyer, a leading expert in the theory of gases and an early supporter of Maxwell’s theory of molecular collisions. Meyer’s 1877 textbook on gas theory included a section on Kelvin’s theory of vortex atoms, which ‘‘avoids the philosophical objections which can justly be raised to the assumption of atoms.’’ Meyer did not make scientific use of the theory either in the physics of gases or in other areas, but he left no doubt that he found it promising:

> The multiplicity of these states [of motion] has given rise to a multiplicity of kinds of vortex atoms, which, in spite of their multiplicity, were all formed by the same substance and in accordance with the same laws, and which must bear witness to these laws for all time by the regularity of their properties. The conformity to law exhibited by the properties of atoms, and especially the law of periodicity of these properties, will then find explanation by this theory.[72]

Although Wilhelm Ostwald was aware of the vortex atom theory, which he knew from his polemic with J. J. Thomson, he did not to refer to it in either his scientific work or his advocacy of energetics as an alternative to atomism. In a highly critical comment on Ostwald’s programme of energetics, FitzGerald claimed that the German chemist had not only misunderstood the mechanical ethers but also erred in identifying mechanicism with materialism. ‘‘Prof. Ostwald ignores such theories at that of the vortex atom, which postulates only a continuous liquid in motion,’’ he complained.[73] The vortex programme rested on field physics and a continuum view of matter that were antagonistic to the action-at-a-distance theories favoured on the Continent. For example, the German astronomer and physicist Friedrich Zöllner fiercely opposed continuum theories and flatly denied any scientific legitimacy to Kelvin's vortex atoms.[74] It is hard to say to what extent Zöllner's hostility was shared by other German physicists. After all, silence indicates neither approval nor dismissal, and there may well have been physicists who privately sympathised with the vortex programme, or parts of it, but chose not to say so in public. As a young man, German-born Arthur Schuster worked with Kirchhoff in 1872–73, and he later recalled that the great German theorist admired Kelvin for his vortex theory of matter. "'I like it,' he remarked, 'because it excludes everything else,' and he added with a sigh: 'If only it could explain gravitation'" (Schuster 1932, p. 219; Schuster 1911, p. 34). Kirchhoff may have admired the vortex atom, but if so he did not express his admiration in his writings. He worked extensively with the theory of continuous media, and in his lectures on mathematical physics he covered vortex motion, including vortex tubes and rings (Kirchhoff 1876, pp. 251–272). Yet, like Helmholtz, he did not mention the vortex atom.

The position of Heinrich Hertz was unrepresentative, but then, as noted by Helmholtz, his style of physics was closer to Kelvin's than that of other German physicists. In a general sense, Hertz agreed with British theorists of the vortex school, namely, in his inclination "to ask whether all things have not been fashioned out of the ether," as he expressed it in 1889.[75] Although he did not advocate any specific model of matter or ether, he seems to have considered the vortex atom with some sympathy. His reconstruction of mechanics drew apparently on inspiration from the vortex atom theory, which he considered to have features in common with his own theory. Hertz referred approvingly to "Kelvin's vortex theory of atoms, which presents to us an image of the material universe that is in complete accord with the principles of our mechanics" (Hertz 1894, p. 44).

In the 1890s, when Maxwellian (or post-Maxwellian) field electrodynamics had become widely accepted also among Continental physicists, the vortex theory was in rapid decline. Because of its mechanical foundation, it must have appeared unappealing in an environment increasingly hostile to materialism and the mechanical world-view. Now it is a matter of debate to which extent the vortex atom and its associated ether can be said to belong to such a world-view. During the period here dealt with, the last third of the nineteenth century, the British ether became increasingly dematerialised, but without losing its connection to mechanical theories of matter.[76] In 1878, FitzGerald recommended "to emancipate our minds from the thraldom of a material ether," yet, although his view marked a step towards dematerialisation, he does not seem to have conceived the ether as entirely different from matter.[77] The British ether of the vortex programme was mechanical in the sense that it possessed inertia as an irreducible property, and non-material only in the sense that it was continuous and not derivable from matter. Although vortex physicists agreed with Larmor's statement, that "Matter may be and likely is a structure in the aether, but certainly aether is not a structure made of matter," they did not understand it in an anti-mechanical sense (Larmor 1900, p. vi). FitzGerald agreed with Ostwald's anti-materialism, but, referring to the vortex atom, denied that it implied anti-mechanicism.

According to the vortex theory, as understood in the 1880s, atoms were composed of the same fluid ether that was the carrier of electromagnetic interactions. Conceived as parts of a unitary "theory of everything" the vortex matter atoms required an associated vortex-filled ether. However, this did not amount to a fully monistic view, for there were still differences between the etherial vortex atoms and the surrounding luminiferous ether. Electricity and magnetism were part of the latter, not the former, which were governed only by the laws of mechanics (or rather hydrodynamics). In J. J. Thomson's 1883 *Treatise*, the high point of vortex atomism, electromagnetism played no role at all. And in most applications of the vortex atom theory, as to chemistry and gas phenomena, the ether between the atoms played no role either. In short, the new electron physics of Lorentz, Larmor, Wilhelm Wien and others insisted on a thoroughly electromagnetic ether that contrasted with, or was widely conceived to contrast with, the more or less mechanical ether on which the vortex atom theory built. With the victory of the electromagnetic world-view, the vortex atom theory became obsolete and was soon effectively forgotten.

9. Ideological uses and philosophical responses

The theory of vortex atoms was a scientific hypothesis, developed in mathematical details and devised to solve fundamental problems of physics. But there was another side to it, and perhaps not a less important one. The theory was also an important part of the world picture of late-Victorian Britain, and as such it served purposes that must be characterised as ideological. It resonated eminently with values dear to the Victorian mind, such as unity, continuity, and non-materialism. During the second half of the nineteenth century, the established harmony between science and religion came under attack from agnostics or "scientific naturalists" who argued that the material and spiritual world were entirely separate spheres. All there was to experience, they claimed, could be comprehended in terms of matter and energy.

Exponents of scientific naturalism often used the atomic theory to argue their more or less materialist cause. According to them, the material world was composed of solid atoms of the billiard-ball type, which was however a view that physicists increasingly considered to be obsolete. The vortex atom was entirely different from the Daltonian atom, and it was not connected with the materialism and determinism that since the days of Democritus had been associated with atomism. In short, the vortex atom theory did not agree very well with the agenda of scientific naturalists such as Herbert Spencer, John Tyndall, Thomas Huxley, and William Clifford. Perhaps as a consequence of the non-materialist nature of the vortex atom, the scientific naturalists paid scant attention to the theory and preferred the old-fashioned solid atoms.[78] Not only did the vortex atom abolish the traditional association between atomism and materialism, it also founded atomic theory upon the ether, this non- or quasi-material medium that was so dear to Victorian scientists. It was through the ether that vortex atoms sometimes entered as a scientific background for spiritual thinking and a revived natural theology. Whether the ether was vortical or not, it came increasingly to be seen as dematerialised – "suprasensual" as Larmor expressed it. To some physicists, most notably Lodge, the ether became of deep spiritual significance, a psychic realm scarcely distinguishable from the mind.[79]

The extra-scientific, ideological use of the vortex atom was first made clear in 1868, in an essay in which the telegraph engineer Fleeming Jenkin argued that free will was fully consistent with the modern conception of atoms and energy. Jenkin took his departure in Lucretius's modification of Greek atomism (in his *De rerum natura*) and extended his review to later atomic theories of matter. He treated at some length the vortex atom, and suggested that Newton's atomism – properly interpreted – could be considered an anticipation of Kelvin's ideas. Jenkin found the vortex atom to be most promising. For example, its vibratory modes "would correspond to the special waves of light which the chemical atom of each elementary substance is capable of exciting or receiving." And this was not all, for "nor need we despair even of explaining light and gravitation with the same machinery."[80] Kelvin was most pleased with Jenkin's essay, not least its appraisal of the vortex atom theory. In his 1871 British Association presidential address, he quoted from the "admirable paper" and reiterated his belief that the vortex theory "may possibly lead to a full understanding of the properties of atoms" (Thomson 1871, p. xciv).

In *The Unseen Universe*, published anonymously by Balfour Stewart and Tait in 1875, vortex atoms played a no less important role. The general message of this important and popular book was that science was in intimate harmony with religion. The authors argued in great detail that although the visible universe must come to an end, there must exist an eternal "unseen universe" which is the seat of spiritual forces and in contact with the material world: "We are led by scientific logic to an unseen, and by scientific analogy to the spirituality of this unseen. In fine, our conclusion is, that the visible universe has been developed by an intelligence resident in the Unseen."[81] The arguments of Stewart and Tait presupposed an ethereal world consistent with the vortex theory, but they did not rely specifically on this hypothesis, which they used "for purposes of illustration." Although they considered the vortex atom theory to be very promising, they also realised that only very simple cases of vortex ring structures had been investigated. "Hence we are at present altogether unable to decide or even to guess whether this idea will or will not pass with credit some of the most elementary examinations to which a theory of the ultimate nature of matter must of course be subjected." Placing the vortex atom theory in a cosmological context, they asked from where the ether vortices had originally come. It must be "an act impressed upon the universe from without, . . . for if the antecedent of the visible universe be nothing but a perfect fluid, can we imagine it capable of originating such a development in virtue of its own inherent properties, and without some external act implying a breach of continuity? – we think most assuredly not." Although Stewart and Tait had great sympathy for Kelvin's theory, they found that it collided with the sacrosanct "principle of unbroken continuity" and were therefore driven to postulate a not-so-perfect fluid in the unseen world out of which vortices could develop without divine intervention. Thus, whereas in 1867 Kelvin noted as a satisfactory feature that vortex atoms could only be created by a divine act, in 1875 Stewart and Tait, in their argument for Christian belief, came to the opposite conclusion.

The Unseen Universe caused great debate and was criticised by scientific naturalists for being a return to natural theology. Of interest in the present context is the response of William Kingdon Clifford, the brilliant mathematician. In an extensive and highly critical review, Clifford dealt in detail with the vortex atom hypothesis and the way in which Stewart and Tait used it.[82] This way he dismissed as ideological, as a way to refashion supernatural marvels. He generally praised Kelvin's results, which "if they are not the foundation of the final theory of matter, are at least imperishable stones in the tower of dynamical science." However, he also criticised the theory for offering no true explanation of physical phenomena; for its basis, the idea of a perfect fluid, was not a known entity but just a mathematical fiction. Clifford expressed a kind of Machian sensationalism and denied that matter and energy were more than "complex mental images." As far as matter theory was concerned, he preferred his own alternative of space curvature, which "hints at a possibility of describing matter and motion in terms of extension only."[83]

The main conceptual advantage of the vortex atom was that it combined atomism with a continuum view of nature. But was the theory really satisfactory from a conceptual point of view? Several philosophers concerned with foundational problems of physics argued that it was not. In the late-nineteenth century debate over atomism, which mainly took place in Germany and France, the vortex atom theory received some philosophical attention. The first, and to this day most detailed, philosophical account appeared as early as 1879, written by the German philosopher and historian of atomism Kurd Lasswitz, who was an advocate of rigid atoms in motion.[84] In a work published in 1878, Lasswitz had briefly rejected the vortex theory on philosophical grounds, apparently because he found it to be unvisualizable and of mathematical interest only (Lasswitz 1878, p. 105). In his essay of the following year, he claimed that Kelvin's theory was explanatory empty because it left unexplained the motion of the individual "particles of the vortex atom." Moreover, it was doubly obscure because it presupposed *two* miracles, a supranatural creation of the ethereal fluid and of the vortical motions in it. Lasswitz's conceptual criticism of the theory was however based on the misunderstanding that Kelvin's ether was of the same kind as Descartes's, that is, was composed of tiny, subatomic particles.

According to the German-born American positivist Johann Stallo, atomic hypotheses were merely aids to the mental faculty and did not warrant any belief in the reality of atoms. In his 1882 book *The Concepts and Theories of Modern Physics*, he claimed that atoms were impossible objects both from a metaphysical and conceptual point of view. To account for gas phenomena and spectra, atoms must be elastic bodies, yet this is impossible, for elasticity presupposes that the body consists of parts that can change their position. Now the vortex atom was perfectly elastic without consisting of discrete parts, which might seem to solve the old conundrum. But Stallo found the vortex solution to be illusory and denied that the vortex theory differed essentially from what he called the atomo-mechanical view. "It seems to be evident," he wrote, "that motion in a perfectly homogeneous, incompressible and therefore continuous fluid is not sensible motion."[85] This time-honoured objection, long known in the history of matter, had been countered by Maxwell in 1878, when he pointed out that elasticity and continuity are not incompatible. Maxwell referred to the vortex atom as an example: "A medium, . . . though homogeneous and continuous as regards its density, may be rendered heterogeneous by its motion, as in Sir W. Thomson's hypothesis of vortex-molecules in a perfect liquid" (Maxwell 1965, part I, p. 774). Or, as Lodge later put it in plain language: "There is no real difficulty: fish move freely in the depths of the ocean."[86] In a review essay in *Mind* (vol. 8, pp. 276–284), the British physicist Donald MacAlister subjected Stallo's book to severe criticism. MacAlister was particularly concerned about Stallo's dismissal of the vortex theory, which the American author ("not a mathematician") had got quite wrong. He argued at length that the vortex motion in a perfect fluid was as "sensible" as any kind of motion and that this was accepted knowledge among experts. MacAlister evidently held the theory in high esteem and considered it his duty to defend it. Like several other advocates of the theory, he appealed to the theory's mathematical complexity as both a problem and – mostly – a hope:

> The work of deduction [from theory to phenomena] is so difficult and intricate that it will be long before the resources of the theory are exhausted. The mathematician in working it out acquires the feeling that, although there are still some facts like gravitation and inertia to be explained by it, the still unexamined consequences may well include these facts and others still unknown . . . The Vortex-theory is still in its infancy. We must give it a little time (p. 279).

Stallo remained unimpressed. In the second edition of his book, published in 1884, he replied that allegiance to the vortex atom theory was just "characteristic of the confusion of modern theorists, who insist upon reducing all physical action to impact'' (p. 19).

The philosophical objections of Lasswitz and Stallo appeared at a time when the vortex atom theory was still an area of active research, but they do not seem to have made any impact on the British physicists. Objections of a somewhat similar kind appeared in the anti-atomistic treatise of the French philosopher Arthur Hannequin and, briefly, in Émile Meyerson's *Identité et Réalité*.[87] To another French anti-atomist, Pierre Duhem, Kelvin's vortex atom theory was just one more example of ''the ample but weak mind of the English physicist.''[88] I have found almost no trace of the vortex atom theory in the German controversy over atomism versus energetics that culminated in 1895 and in which Ostwald, Boltzmann, and Georg Helm were the leading figures.[89] Boltzmann ignored the vortex gas theory and may have considered the vortex atom, as well as other models of internally structured atoms, to be speculative and ad hoc. At least, this is a possible interpretation of a passage included in an address he delivered in 1899. Referring to such models as ''extravagancies,'' Boltzmann said, ''Every second-best [physicist] felt himself called upon to devise his own special combination of atoms and vortices, and fancied, having done so, that he had pried out the ultimate secrets of the Creator.''[90]

10. Vortex atoms and superstrings

The vortex atom was a foundational theory of physics, and as such it was difficult to subject to experimental tests. As we have seen, throughout its lifetime it was faced with serious difficulties of both an empirical and a conceptual nature, yet in the eyes of vortex advocates none of these amounted to a definite refutation. Although the theory was abandoned, it was not really refuted and certainly never falsified. In the attempts to keep the theory alive as at least a research programme worth to develop, physicists attracted to it used various strategies of arguments. For example, was it not possible that the difficulties would disappear in some future development of the theory? – or if some other vortex object was assumed instead of the simple ring and tube structures? One could always cling to the hope that future work in mathematics would change the situation, as Hicks argued in 1895. For a theory so appealing that it deserved to be true, one could find many ways to avoid the unwelcome and prosaic conclusion that it was just wrong.

The whole situation with respect to the theory's viability is well illustrated by the Helmholtz lecture that FitzGerald gave in 1896 to the London Chemical Society and in which he included a substantive review of the vortex atom theory.[91] According to FitzGerald, the theory was in serious but not inescapable troubles, and he pointed out a number of ways in which the problems could be surmounted. Thus, he reminded his audience that the more energy a vortex ring is given, the greater its inertia and the slower it moves. On the other hand, experiments strongly indicated that the ratio between inertial and gravitational mass was unaffected by temperature. Therefore, if the inertia of a body increased with temperature, "it would lead to very serious discrepancies in the astronomical theory of the motions of the various members of the solar system." But FitzGerald eyed a possible (if perhaps not very plausible) way to save the phenomena. There were no reliable measurements of how the gravitational mass varied with the temperature, and so it was possible to assume that it increased in just the same way as the inertial mass. In that case, the problem would disappear. He also mentioned the problem of the velocity of sound that back in 1883 had been raised by Reynolds as an argument against J. J. Thomson's vortex theory of gases, and on which FitzGerald had earlier commented. Now he stated the problem as follows: "Either the molecules are not thin vortex rings in an otherwise simple liquid, or else when we give heat to a gas we are in some mysterious way taking more energy from it than we give to it." This was a real problem, but again not a fatal one, for it rested on the assumption of the extra-atomic ether being a simple fluid. And FitzGerald had reasons to believe that it was a very complicated structure: "I prefer in our ignorance the horn of the dilemma, that holds that atoms are not simple vortex rings in an otherwise unmoving liquid."

FitzGerald used part of his review to examine the chemical consequences of the vortex theory, such as they had been derived by J. J. Thomson in the early 1880s. For example, he found that on the ring vortex hypothesis a mercury ring atom must have a radius 14 times as great as a hydrogen ring atom in order to agree with the known atomic weights (mercury's being about 200 times that of hydrogen). From this followed that "the volume of a mercury atom would be something like 2800 times as great as that of a hydrogen atom," a ratio that disagreed with what was known experimentally.[92] Here was indeed another difficulty, but: "This difficulty would be largely surmounted if we . . . suppose that massive rings had thick cores with a slow rotation, so as to have the same strength but a greater momentum than the inner rings.'' Or, alternatively: ''The theory of nearly spherical and worm-like vortices would lead to a somewhat similar solution.'' FitzGerald concluded that Thomson's theory of valency based on the association of thin vortex rings disagreed with chemical facts. Once again, this recognition did not lead him to reject the vortex atom theory, but to suggest alternatives within the theory:

> This should induce study of other forms of vortex motion – study of thick rings and of spherical and worm vortices. There are several ways in which these latter are not subject to the same objections as thin ring vortices. They in some cases increase in velocity when energy is given to them, so that the objections depending on the velocity of sound increasing with temperature would not apply . . . They could apparently swallow one another up, so that something analogous to chemical combination could exist . . .

To put it in a nutshell, ''the'' vortex atom theory was so rich and flexible, and so undetermined, that it was practically beyond falsification. Of course, for the very same reason it was also unverifiable. The mathematical richness of the vortex theory might be considered a blessing, but it was a curse as well. It made FitzGerald believe that it was ''almost impossible'' that the universe would not be explainable in vortex terms (see section 4). The generalised vortex theory that he dreamt of could in principle explain everything, and therefore also the properties of the one and only universe. But could it also explain why the numerous other conceivable states of the universe, all of them describable within the theory's framework, do *not* exist? The theory could (again in principle) explain the mass of an atom of chlorine, but had chlorine had any other atomic weight the theory could account for that as well. In short, the theory explained too much – and therefore too little.

Much of the magic of the vortex atom programme rested on its claim of being a unified and all-encompassing theory, indeed a theory of everything. Never, since the days of Descartes, had there been such an ambitious and fundamental theory of physics. It is scarcely surprising that much of the unification rhetoric of the vortex programme can be found also in later theories of a similar grand scope. The successor, in a sense, of the vortex atom theory was the generalised electron theory or so-called electromagnetic world picture based on the theories of Larmor, Lorentz, Max Abraham and others.

The new theory, as it culminated in the work of Gustav Mie, was no less grand than the old vortex theory but also no more successful when it came to explaining concrete physical phenomena. In 1911, Mie described elementary particles as knots in the ether, and wrote, ‘‘The entire diversity of the sensible world, at first glance only a brightly coloured and disordered show, evidently reduces to processes that take place in a single world substance – the ether’’ (Kragh 1999, p. 117).

Vortex imagery can be found even in modern fundamental physics, as well as in speculative and unorthodox ideas that claim to integrate physics and spiritual life.[93] On a more serious level, parts of modern particle and field physics have returned to some of the notions that so captivated Victorian vorticists. For example, the quantum theory of superfluidity, as first developed by Richard Feynman in 1955, entails quantised vortex rings that – quantisation apart – many late-nineteenth British physicist would have appreciated (Lane 1962, pp. 105–139). Moreover, not only have modern physics certain similarities with the Victorian vortex theory, in a few cases theories have been influenced by it. The British physicist Tony Skyrme, who around 1960 introduced solitons into particle physics in the form of so-called ‘‘skyrmions’’, admitted inspiration from Kelvin’s picture of the vortex atom.[94] Also the important work of the Russian theorist Ludwig Faddeev in non-linear field theory has been influenced by the nineteenth-century vortex and knot background. A research paper of 1997, including references to works of Kelvin and Tait, starts with the words: ‘‘In 1867, Lord Kelvin proposed that atoms – then considered to be elementary particles – could be described as knotted vortex tubes in ether. For almost two decades, this idea motivated an extensive study of the mathematical properties of knots, and the results obtained at that time by Tait remain central to mathematical knot theory’’ (Faddeev and Niemi 1997, p. 58). Today, knot theory is a major area of physics, with applications ranging from quantum field theory over biophysics to chaos theory (Kauffman 2000).

Yet, although traces of the Victorian past can be found in modern physics, of course we do not live in a world made up of ether vortices. We live in a world composed of leptons and quarks, or perhaps of superstrings. Modern unified theories have nothing substantial in common with the vortex past, but they do have something in common with it on the methodological and rhetorical levels.[95] One of them is the governing role of mathematics, and the belief that, when the final theory has been found, ‘‘all physical phenomena will be a branch of pure mathematics," as Hicks said in his 1895 address. Another obvious similarity is the problem of the testability of such a high-level theory, of making contact with experimental data. The vortex atom theorists were unable to calculate the properties of a hydrogen atom, just like the string theorists have been unable to calculate the properties of electrons and protons.

Many theoretical physicists believe today that some version of superstring theory may accomplish what the vortex theory could not in the past – and much more. In an interview of 1998, the leading string theorist Edward Witten said,

> I feel that we are so close with string theory that – in my moments of greatest optimism – I imagine that any day, the final form of the theory might drop out of the sky and land in someone's lap. But more realistically, I feel that we are now in the process of constructing a much deeper theory than anything we have had before and that well into the twenty-first century, when I am too old to have any useful thoughts on the subject, younger physicists will have to decide whether we have in fact found the final theory! (Greene 2000, p. 373).

Substitute "vortex" for "string" and "twentieth" for "twenty-first", and we have pretty much the hopes and feelings of Hicks, FitzGerald and other late-Victorian vortex atom theorists.

11. Concluding remarks

In many ways, the vortex atom theory marked the zenith of the mechanical world-view. This highly – if not wildly – ambitious theory was a serious attempt to understand all of physics on the basis of vortices in a fluid ether. From an ontological point of view, it was a paragon of simplicity, but to make physical sense of it was anything but simple. Indeed, it soon turned into a mathematical monster. It is most remarkable that the theory was developed only by British scientists and that it was mainly in Great Britain that it was considered attractive. The reasons for this are not entirely clear, although William Thomson's invention of the vortex atom is certainly part of the explanation. The mathematical framework of the theory fitted well with the Cambridge style of mathematics and the approach to hydrodynamics that characterised British physicists and applied mathematicians. In addition, the theory had extra-scientific ramifications that were in deep harmony with the zeitgeist of Victorian Britain.

In this essay, the entire life-span of the vortex atom theory has been followed, a period of less than forty years. I have paid particular attention to how it disappeared from the scene of physics, and pointed out that it was never unambiguously proved wrong by experiments. It was abandoned, not primarily because it disagreed with empirical data but rather because of its lack of progress. If a fundamental theory at its early phase is judged promising, and yet persistently fails to deliver what it promised, scientists will usually lose interest in it. The longer the theory lives on without much hope of progress and increased contact with experiments, the less interest in attempts to develop it as a physical (rather than mathematical) theory. In the case of the vortex atom theory, one may speak of *abandonment by exhaustion*.

I have referred to the vortex atom as a theory, a model, or a hypothesis, without discriminating between the terms (thereby following the usage of the Victorian vortex physicists). Perhaps it might be more appropriately called a research programme, say in the meaning of Imre Lakatos and his followers.[96] Research programmes are not verified or falsified in any direct sense, but rather evaluated by their ability to produce still more empirically fertile theories. According to Lakatos, a progressive research programme predicts novel facts, and some of these are corroborated by experiments. If this is not the case, and if it remains so for a longer period, the research programme is said to degenerate. There can be little doubt that the vortex atom programme started to degenerate shortly after its birth. Yet it continued to live on for some thirty years, which illustrates that there is more to theory survival than empirical tests. A truly fundamental theory, such as the vortex atom theory, is not judged solely on its empirical merits. Present string theory provides another illustration.

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NOTES

1. Contributions to the vortex atom theory can be found in Royal Society 1909–1912, which lists 154 papers in the categories "Vortex motion. Vortex atoms" and "Vortex theories". Perhaps a third of these dealt with the vortex atom hypothesis. It is evident from the list that almost all the papers came from British physicists or mathematicians. As a supplement, I have also looked at the literature listed under the entries "Hydrodynamics" and "Molecular physics" in *Jahrbuch über die Fortschritte der Mathematik* between 1868 and 1900, and in *Beiblätter zu den Annalen der Physik und Chemie*, 1877–1905. My best estimate is that the vortex atom population, defined as those who contributed to or advocated the theory, amounted to about twenty-five scientists.
2. The pioneering work is Silliman 1963, which focuses on the origin and early years of the vortex atom theory. Siegel 1981, Sinclair 1987, and Epple 1999, pp. 94–130, are valuable supplements. The mathematical background is stressed in Archibald 1989, and Doran 1975 calls attention to the theory as a precursor of the electron theories of the 1890s. The most complete account, Pauly 1975, is unpublished. I am most grateful to Philip Pauly for having provided me with a copy of his work.
3. Already Silliman, in his 1963 study, noted this neglect. It has not changed much since then. For example, the most recent comprehensive history of atomism (Pullman 1998) does not even mention the theory.
4. See Knudsen 1971, which includes a transcription of Thomson's text in his notebook of January 6, 1859.
5. Letter to James Joule, published in Thomson 1872, pp. 317–318. See also Smith and Wise 1989, pp. 409–411. J. J. Thomson similarly recalled a conversation from the 1880s in which "Kelvin said he did not believe in atoms but only in molecules" (J. J. Thomson 1936, p. 50).
6. Helmholtz 1858. Koenigsberger 1902–03, pp. 307–320. The enduring status of Helmholtz's theory may be illustrated by Arnold Sommerfeld's textbook in the physics of deformable bodies. The very first word in the 1964 edition is "Helmholtz", to be followed by a summary account of his 1858 vortex dynamics, which in a later chapter is described in detail (Sommerfeld 1964, pp. 115–150).
7. In a letter to Helmholtz of May 12, 1859, reproduced in Thompson 1910, pp. 401–402, Thomson wrote that he had read with interest "your paper on rotatory motion in fluids you were so good to send me." Thus it is wrong when Smith and Wise (1989, p. 417) state that Thomson did not know of the paper prior to 1862. In Silliman 1963, p. 464, Thomson's reading of Helmholtz's paper is dated the fall of 1858.
8. Smith and Wise 1989, pp. 400–408. For Rankine's model, see Daub 1967. Smith 1980 and Doran 1975 disagree with regard to "influence" in general and to Rankine's in particular. According to Bellone 1980, p. 56 and p. 189, the genesis of the vortex atom

is "incomprehensible" without taking into account Thomson's interest in Riemann's generalised geometry of metric spaces. I have found no evidence for any such influence, or for Thomson's interest in Riemann's work.

9. Graham 1864, p. 83. I am not suggesting that Thomson was inspired by Graham. Much later, in his *Aether and Matter*, Joseph Larmor quoted Graham's words which he, not entirely correctly, interpreted as "Graham's view that an atom is a vortex in the aether." See Larmor 1900, pp. xxiv and 320.
10. In 1867, Tait's translation appeared in print, as Helmholtz 1867. Tait's early interest in Helmholtz's vortex theory seems to have been motivated by his work with quarternions, where he found some of Helmholtz's mathematical results to be relevant. See Tait's letter to Hamilton, of December 7, 1858, as reproduced in Knott 1911, pp. 126–127, and also Helmholtz to Tait, 1867, as excerpted in Koenigsberger 1902–03, vol. 1, p. 311. For more details, see Epple 1998, pp. 316–319.
11. E.g., Ball 1868; Tait 1876, pp. 290–300; Tait 1885, pp. 19–21. Robert Ball, professor at the Royal Irish Academy, determined experimentally the velocity of smoke rings and the degree to which they deviated from the idealised case of a perfect fluid.
12. Thomson 1867, a stenographically reported paper, reprinted in Thomson 1882–1911, vol. 4, pp. 1–12. In what follows, Thomson 1882–1911 is abbreviated MMP. See also the report in *The Scotsman*, February 19, 1867, as reproduced in Thompson 1910, pp. 517–518. For the background, see Epple 1998, p. 323.
13. Thomson 1867, p. 3. Note that "atom" here means an indivisible particle, and "molecule" the smallest part of a chemical element, what is usually called an atom.
14. Thomson 1869 (MPP 4, pp. 13–66), based on a paper read on April 29, 1867. Emphasis added. Thomson still thought of vortices as constituents of ponderable matter, not of ether.
15. Maxwell 1965, part I, p. 488. Maxwell's continued interest in Helmholtz's theory is documented in Maxwell 1995, vol. 2, esp. pp. 399–407.
16. Letter of November 13, 1867, reproduced in Knott 1911, p. 106 and in Maxwell 1995, p. 321, where Peter Harman explains the reference to "M. Scott"'. See also Pauly 1975, p. 42. On Maxwell's work in knot and vortex theory, see further Epple 1998, pp. 325–328 and Harman 1998, pp. 154–158. Maxwell constructed in 1868 a simple apparatus, a zoetrope, to illustrate visually vortex rings threading through each other (Maxwell 1995, vol. 2, p. 446).
17. Maxwell 1965, part II, pp. 301–307, on p. 307. This was a review of Thomson 1872 that appeared in *Nature* 7 (1873), pp. 218–221.
18. In theory of science, and especially the sociology of scientific knowledge, "mutants" denote rival theories with a common foundation in some background theory. The idea of mutation of scientific theories, or at least the name, seems to have its origin in Pickering 1984, see pp. 290–300.
19. See note 1. Contemporary surveys of and references to vortex theory can be found in Lamb 1879, from its second to sixth revised edition (1895 to 1932) entitled *Hydrodynamics*, and in Hicks 1881, Love 1887, and Love 1901. For a Continental review, see Brillouin 1887. Marcel Brillouin, who greatly valued Thomson's work, dealt in some detail with the vortex atom theory, but without endorsing it as a realistic hypothesis (see also section 8).
20. Hill 1880; Hill 1885; Hill 1895. On Hill, see the obituary notice in *Proceedings of the Royal Society A* (1929), pp. i–iv.
21. Hicks 1883, which developed earlier ideas in Hicks 1879. On Hicks, see Milner 1935.

A less complete theory of hollow vortices was developed by Thomson (1880c), who considered hollow columnar vortices, and also by the American physicist Henry Rowland (1880). Both works were mathematical and they did not refer to vortex atoms.

22. FitzGerald, ''On a hydrodynamical hypothesis as to electromagnetic action'' (1899), in FitzGerald 1902, pp. 472–477.
23. Hicks 1883, p. 305. Communications to the Royal Society often appeared in two versions: after a preliminary ''abstract'' in the *Proceedings*, with an emphasis on the physical ideas, the mathematical details followed in an often very lengthy paper in the *Transactions* (in this case Hicks 1885a).
24. Love 1894. In 1898, Love was appointed professor of Natural Philosophy in Oxford. Between 1887 and 1895, he contributed several papers on vortex motion in fluids. See Milne 1941.
25. The origin of knot theory and its connection with the vortex atom is detailed in Epple 1998 and Epple 1999, pp. 109–160. As shown by Epple, also Maxwell contributed to the early phase of knot theory, although he did not publish on the subject.
26. P. G. Tait, ''Some elementary properties of closed plane curves,'' pp. 270–272 in Tait 1898–1900, vol. 1.
27. Challis 1873, p. 14. Maxwell disagreed. See his anonymous review of Challis's book, in Maxwell 1965, part II, pp. 338–342. For an evaluation of Challis's work and philosophy of nature, including its theological aspects, see Scheuer 1997, pp. 257–286.
28. On Thomson's changing views of the ether, see Wilson 1987, pp. 155–180.
29. Thomson 1887, p. 486 and p. 494. See also the analysis in Whittaker 1958, pp. 296–300. Thomson's paper was also published in *Philosophical Magazine* 24 (1887), pp. 342–353, but with a much less revealing title: ''On the propagation of laminar motion through a turbulently moving inviscid liquid.''
30. Quoted in Hunt 1991, p. 96, which includes an account of the vortex sponge model and full references to FitzGerald's works.
31. FitzGerald, ''On a model illustrating some properties of the ether'' (1885), in FitzGerald 1902, pp. 142–156, p. 155. See also the abstract in *Nature* 31 (1885), pp. 498–499.
32. Hicks 1895, p. 601. Olivier Darrigol has aptly characterised the vortex sponge as ''the string theory of those days: its basis was attractively simple, it could not be refuted, but it could not be developed far enough to be verified.'' Darrigol 2000, p. 189.
33. Preston 1881. Maxwell about Preston: ''He is by no means a paradoxer though a fierce speculator, and what is rare among such folk he improves and amends his errors.'' Letter to Tait of December 12, 1877, reproduced in Garber, Brush and Everitt 1995, p. 272.
34. For Continental theories of corpuscular ether, see Rosenberger 1886–90, vol. 3, pp. 592–613. Rosenberger gave but a brief account of the vortex atom theory and wondered if Thomson really believed that such atoms existed (p. 612). For the variety of views of the ether, see also Cantor and Hodge 1981 and Kragh 1989.
35. Pearson's ideas were not completely ignored though. They were discussed in Ball 1905, pp. 363–364, alongside with the vortex atom theory and other ideas of matter and space. Ball's book, first published in 1892, went through several editions. Although very much has been written about Pearson, the historical literature has focused almost exclusively on his work in statistics and biometrics.
36. Pearson 1885, p. 104. According to Pearson 1888–89, p. 38, the paper was written in 1883.
37. In 1898, Arthur Schuster, in a note that may have been intended as a parody of specu-

lations à la Pearson, coined the names anti-atom and anti-matter for the hypothetical material made up of ether sinks. Schuster 1898.

38. Pearson 1891, p. 313. Higher dimensions were a popular pastime for Victorian physicists and mathematicians. In many of these speculations, the ether served an important role or was suggested somehow to have its seat in hyperspace. For reviews, see Bork 1964 and Beichler 1988. As far as I know, none of the ideas about higher dimensions related specifically to the vortex atom.
39. Pearson included both the vortex atom and his own ether squirt theory in the second edition of his influential book on philosophy of science, Pearson 1900, pp. 265–268. In the early 1890s, he criticised William Thomson for entertaining a realist view of atoms and the ether. According to Pearson, conceptual models of atoms ‘‘have not necessarily equivalents in the material universe,’’ and the ether was ‘‘only an intellectual mode of briefly summarizing certain wide groups of sensations.’’ See Todhunter and Pearson 1893, p. 477 and p. 453. Compare with Pearson 1900, p. 179: ‘‘I . . . speak of the ether as a scientific concept on the same footing as geometrical surface and atom.’’
40. There was no shortage of non-vortex ether theories of gravitation. For one such theory, which claimed to explain gravitation in terms of ether waves, see Challis 1873.
41. Thomson 1891, pp. 152–153. Æolotropy=anisotropy. Compare also with Tait's account: the new version of Lesage's corpuscles ‘‘must, of course, be smaller vortices’’ (Tait 1876, p. 300).
42. See Thomson's letter to Stokes of November 2, 1875, reproduced in Wilson 1990, pp. 408–409.
43. The ratio is given by $\gamma = c_p/c_v = (n+2)/n$, where n is the number of degrees of freedom of the gas molecule. If n is very large, $\gamma \cong 1$. See Brush 1976, pp. 353–356 and also Clark 1976.
44. See *Nature* 16 (1877), pp. 242–246, a review of Henry W. Watson, *A Treatise on the Kinetic Theory of Gases* (Oxford: Clarendon, 1876), reprinted in Garber, Brush and Everitt 1995, pp. 156–166, on p. 165. See also Garber 1978, pp. 277–278. The individual specific heats follow the expressions $c_p = 5 + r + 2v$ and $c_v = 3 + r + 2v$, where r and v are the numbers of degrees of freedom of, respectively, rotation and vibration. If v is infinite, so become c_p and c_v.
45. J. J. Thomson 1883, p. 112. Effusion denotes the process whereby molecules emerge through a small hole in a container. On Regnault's experiments and the gas law, see Brush 1976, pp. 399–401.
46. This is indirectly shown by Meyer 1899, which included a section on vortex atoms but without referring to the work of J. J. Thomson. In the German edition of 1877, Meyer wrote, ‘‘I agree with William Thomson in his conviction that his hypothesis of the atom vortex forms the beginning of a future development of the kinetic theory’’ (p. 246). In the 1899 edition, the comment was deleted. On Oskar Meyer, see also below.
47. Unfortunately, there is no good historical work on Bjerknes's theories, most of which were originally published in Norwegian. Only in 1900 did his works become generally available, through his son, the meteorologist Vilhelm Bjerknes. See Bjerknes 1900.
48. William Thomson became Lord Kelvin, Baron of Largs, in 1892. In order to avoid confusion with J. J. Thomson, a main character in the later phase of vortex history, I use the name Kelvin from now on. Biographical references continue to refer to Thomson.
49. Mayer 1878. The experiment and various uses of it are examined in Snelders 1976. On Mayer and his view of atomic theory, see Moyer 1983, pp. 35–43.
50. Manuscript notes of 1890, as quoted in Davis and Falconer 1997, p. 17. According to

Davis and Falconer, the ''molecules'' in the first sentence meant vortex filaments. The notes were apparently a draft to J. J. Thomson 1892, where the account appears in a somewhat changed form on p. 410. In the published version, Thomson made it clear that the atomic arrangement he thought of was ''on the supposition that the atoms are vortex rings.''

51. J. J. Thomson 1879 and J. J. Thomson 1883a. Kelvin was referee on the latter paper and corresponded with the author about its details. See Wilson 1990, p. 531. Michael Chayut has suggested that Thomson first heard of the vortex atom and Mayer's magnets from Balfour Stewart, who was Thomson's professor at Owens College and one of the authors of *The Unseen Universe*. See Chayut 1991, p. 532. This may be true for the vortex atom, but not for Mayer's experiment that dates from two years after Thomson had left Owens College for Cambridge University.
52. The emphasis on ''mental representation'' was a typical feature of Thomson's methodology, as examined in Topper 1980.
53. J. J. Thomson 1883b, p. 114. If not otherwise mentioned, the following quotations are from the same source, pp. 114–124.
54. According to Alexander Williamson (1852) and Rudolf Clausius (1857), a chemical system in equilibrium was not static, but chemical changes continually occurred in two opposite directions. This idea of a dynamic equilibrium was in 1884 greatly developed by Jacobus van 't Hoff into the modern theory of chemical equilibrium.
55. Ibid. p. 350. FitzGerald's interest in asymmetric molecules was connected with the question of vitalism. See FitzGerald 1898 and, for background, Palladino 1990.
56. See letters in *Nature* 42 (1890), pp. 295, 591–592, and 614. Schuster did not support the vortex atom theory.
57. FitzGerald, ''On currents of gas in the vortex atom theory of gases'' (1884), in FitzGerald 1902, pp. 131–134, p. 133.
58. Muir 1884, pp. 450–451, and also in the second edition of 1889, pp. 387–388, 403–405.
59. Liveing 1882, p. 480. In a book on chemical equilibrium of 1885, Liveing wrote that, ''The theory of the constitution of matter which I give is an outcome of the vortex theory.'' Quoted in Sinclair 1987, p. 98.
60. Ostwald 1887, vol. 2, p. 745. J. J. Thomson 1887, p. 379. Might the eminent spectroscopist have been William Crookes? In 1886, Crookes referred to the vortex atom theory, which he however mixed up with Graham's speculations mentioned in note 8. See Crookes 1886, p. 560.
61. Jones 1902, pp. 37–39. In the third edition of 1907, the vortices had disappeared, now replaced by electrons. Venable 1904, p. 269. For late American approval of the vortex atom, see also Silas Holman, below.
62. Mendeleev 1904, p. 5, which was included in the third English edition of *Principles of Chemistry* (1905. Vol. 2, pp. 509–529). On Mendeleev's ether chemistry, see Kragh 1989.
63. In the preface to his *Treatise*, Thomson thanked Larmor ''for a careful revision of the proofs and for many valuable suggestions.'' J. J. Thomson 1883. On Larmor's road to the electron theory, see Buchwald 1985, pp. 141–173, and Darrigol 1994. FitzGerald's influence is documented in Hunt 1991, pp. 217–222.
64. Thomson 1884 (Thomson 1891, pp. 225–259, on pp. 235–236). Kelvin's discussion was connected with his view of the second law of thermodynamics. He apparently thought that the vortex atom gas theory provided the law with a satisfactory justification. See Smith and Wise 1989, pp. 428–430.
65. Thomson 1905 (MPP 4, pp. 368–418, on p. 371). The mentioned 1887 paper is in MPP

4, pp. 166–185. Since Kelvin never published his conclusion, one may assume that he had no proof of instability, only lack of proof of stability.

66. Bellone 1980, p. 63, locates Kelvin's awareness of the failure of the vortex atom to the mathematical structure of the theory, namely, the vortex atom's lack of complete stability. This was part of Kelvin's reason, but Bellone's suggestion that it was caused by the conclusion Henri Poincaré arrived at in 1893 is unfounded. Kelvin abandoned the theory before 1893, and his reasons had nothing to do with Poincaré's re-examination of Helmholtz's theory, which Kelvin probably did not even know about. See also below.
67. J. J. Thomson 1931, p. 1057. Thomson was not the only physicist of a classical inclination who attempted to formulate wave mechanics in hydrodynamic terms. The first such formulation was presented by Erwin Madelung in 1926, to be followed by Arthur Korn in 1927. See Jammer 1974, pp. 33–36, which includes no reference to Thomson's paper. The derivation given by Thomson differed from those of Madelung and Korn in that it was based on the electromagnetic wave equation and not directly on hydrodynamics. In fact, the derivation was precisely the same as one that Louis de Broglie had presented in 1926.
68. The phrase ‘‘ingenious romance’’ was used by Roger Cotes in his preface to the second (1713) edition of Newton's *Principia* to ridicule Descartes's vortex theory.
69. Quoted in Moyer 1983, p. 131. The original source is *The Nation* 68 (February 1899), pp. 95–96. Another critical review, an essay written by J. G. Macgregor, appeared in *Physical Review* 9 (1899), pp. 59–64.
70. FitzGerald et al. 1899, p. 13. FitzGerald's attraction to the vortex atom theory turned up the oddest places, as in lectures and addresses on quite different subjects. For example, he used part of a lecture on ‘‘Flying’’, given to the Liverpool Physical Society in 1896, to tell about vortex atoms (Rowlands 1990, p. 158). And the major part of his Helmholtz memorial lecture the same year dealt with the theory that Helmholtz's hydrodynamics had initiated but that the German physicist never endorsed nor expressed any interest in. See FitzGerald 1896b and also below.
71. Preston 1880, p. 58. The peculiar limitation of vortex theory to Great Britain was pointed out in Merz 1965, vol. 2, p. 62. On national styles in science, see Reingold 1991.
72. Meyer 1899, pp. 350–351, a revised English translation of Meyer's *Theorie der Gase* of 1877. On Meyer's work in gas theory, see Brush 1976, pp. 435–442.
73. FitzGerald 1896, p. 442, where the British style of physics was explicitly opposed to what FitzGerald claimed was the German style. Ostwald's ideal of an inductive science was ‘‘worthy of a German who plods by habit and instinct,’’ whereas ‘‘A Briton wants emotion – something to raise enthusiasm, something with a human interest.’’
74. See, e.g., Zöllner 1876, p. xxiv. Following Wilhelm Weber, Zöllner advocated a corpuscular theory of matter and ether with electrical particles interacting at a distance.
75. Hertz 1895, p. 354. Hertz's view of the ether is examined in Mulligan 2001. In Helmholtz's preface to the book, he pointed out the similarity of Hertz's approach to that of ‘‘English physicists, like Lord Kelvin when he formulated his theory of vortex atoms, or Maxwell when he imagined the system of cells with contents animated by rotation’’ (p. xxvii).
76. Barbara Doran probably over-emphasises the dematerialisation of the British ether – ‘‘no more mechanical than Einstein's field’’ – and also the influence of Kelvin's vortex speculations – of which ‘‘the modern quantum atom was an immediate descendant.’’ Doran 1975, pp. 210 and 179.

77. FitzGerald, ‘‘On the electromagnetic theory of the reflection and refraction of light’’ (1878), in FitzGerald 1902, pp. 45–73, on p. 173. Stein 1981, p. 319.
78. Turner 1974, p. 26. The ideological use of the ether, including vortex atoms, is dealt with in Wynne 1979. For a representative example of the kind of atomism favoured by the scientific naturalists, see Tyndall 1897, pp. 78–93.
79. Larmor 1900, p. vi. On Lodge and the theological significance of the ether, see Wilson 1971, and Cantor and Hodge, 1981, pp. 135–156. Several of the vortex atom physicists were deeply engaged in spiritualism and psychical research, an area in which the ether played a central role (Oppenheimer 1985, pp. 378–390).
80. Quoted in Smith 1980, p. 412. For the Victorian interest in Lucretian atomism, see also Turner 1973 and, as an example, Masson 1884. Jenkin’s essay was published in the *North British Review* of 1868 and received extensive quotation in Stewart and Tait 1881, pp. 234–235. Jenkin, who was a close friend of Kelvin, corresponded in 1867 with him on matter theory, including the new idea of vortex atoms (Smith 1980). In 1869, he was appointed the first professor of engineering at Edinburgh University.
81. Stewart and Tait 1881, p. 223. For analysis, see Heimann 1972 and Smith and Wise 1989, pp. 630–631. The chapter on ‘‘Matter and Ether‘‘, including the vortex theory, was written by Tait. See Knott 1904, p. 236.
82. Clifford 1879, vol. 1, quotations from pp. 237, 243, 245. The review originally appeared in the *Fortnightly Review* in 1875.
83. According to Clifford’s theory of 1876, anticipating some of the features in Einstein’s general theory of relativity, variations in the curvature of space was the sole cause for what is perceived as matter. For discussion and references, see Farwell and Knee 1990.
84. Lasswitz 1879. Published in a, to British physicists, obscure journal, it is unlikely that Lasswitz’s analysis was known to Kelvin and his vortex colleagues. Much later, Milic Capek agreed with Lasswitz that ‘‘even vortex theories of atoms did not necessarily entail abandoning the discontinuity of matter.’’ Capek 1961, p. 111. However, as pointed out by Doran 1975, p. 190, Capek’s argument suffers from the same fallacy as that of Lasswitz.
85. Stallo 1882, p. 43. For Stallo’s book and the debate it spurred, see Moyer 1983. It was reviewed by Tait in *Nature* 26 (1882), pp. 521–522.
86. Lodge 1925, p. 155. Lodge’s metaphor had old roots. In his *Le Monde* of 1644, Descartes used it to demonstrate the possibility of rotational motion in a world completely filled up with incompressible matter.
87. Hannequin 1895, pp. 98–100 and 128–133, who cited Lasswitz and Stallo. Meyerson 1930 (originally published 1908), pp. 249–250, merely repeated Stallo’s objection: ‘‘How conceive of the rings of Thomson and of Helmholtz or the singular points of Larmor? How can the ether in either of these cases be distinguished from the surrounding ether?’’
88. Duhem 1954 (first published 1906), p. 83. In Duhem 1905, pp. 169–176, he gave a fuller account of *l’atome-tourbillon*, which he dismissed because of what he considered its lack of explanatory power.
89. In a critical comment on Boltzmann’s defence of atomism, the German physicist Paul Volkmann referred briefly to ‘‘W. Thomson’s idea of atoms as ether vortices.’’ Volkmann 1897, p. 202.
90. Boltzmann 1925, p. 218. See also Boltzmann 1895, p. 414, where he discussed a system of molecules that ‘‘move through the ether without loss of energy as rigid bodies, or as Lord Kelvin’s vortex rings move through a frictionless liquid in ordinary hydrodynamics.’’

91. FitzGerald 1896b. The quotations are taken from FitzGerald 1902, pp. 346–353. The ‘‘worm vortices’’ mentioned by FitzGerald were presumably the same as Hicks’s spiral vortices.
92. FitzGerald’s arguments were qualitative and not very clear. The figure 2800 comes out as the cube of 14, but he did not state how he got the latter figure.
93. See, for example, Ash and Hewitt 1990, which promises a vortex metaphysics of matter, energy, life after death, UFOs, spiritual phenomena, and much more.
94. See Filippov 2000, p. 227–229, and Tony Skyrme’s ‘‘The origins of skyrmions’’ reprinted therein on pp. 242–245. Solitons are particle-like, solitary wave pulses.
95. I do not want to make too much out of the similarities, which after all are countered by many dissimilarities. For example, contrary to, say, superstring theory, the vortex atom theory did not rest on or introduce any new principles of physics. In fact, it can be said to be a conservative theory of everything, for it rested solidly on hydrodynamics and the ether; and it contradicted no laws either of mechanics, electrodynamics or thermodynamics. As to flexibility, or number of variants, the more recent versions of string theory are remarkably inflexible. Yet other fundamental theories, such as grand unified theory (GUT) and inflation theory, exist in a number of variants that is no less embarrassing than the number of ether objects found in the vortex atom theory.
96. Lakatos 1978. Pauly 1975 adopts a Lakatosian perspective in his analysis of the vortex research programme. The vortex theory can also be seen as an attempt to establish a research tradition, a concept that is closely related to, but not identical with, Lakatos’s research programme. According to Larry Laudan, a research tradition is neither explanatory, nor predictive, nor directly testable; it is a set of ontological and methodological prescriptions that guide the researcher to study certain problems in certain ways. Laudan 1977, pp. 70–120.