Chapter 2. J.J. Thomson's early work in Cambridge: a continuous physics and a reductionist science

1. In Cambridge, as a Graduate

J.J. Thomson's aim was to stay in science, and in the years immediately after his graduation, he worked intensely to complement his training and to build up a curriculum that could secure him a position in academia. An obvious choice for a high wrangler was to try to stay in Trinity College with a fellowship, a possibility that eventuated after the summer of 1880. Another option arose in 1881, in the form of a vacant professorship of applied mathematics in his very own Owens College, but his application was turned down in favour of Schuster, who was more experienced and already teaching there. The fascinating thing about the period 1880-1884 is not only the amount of work he did, both theoretical and experimental, but also the scope of it: we shall find him developing a dynamical theory of physics and chemistry for his Trinity College fellowship dissertation, and a dynamical theory of matter with which he won the prestigious Adams Prize of 1882; we shall find him in the Cavendish, first trying to acquire the experimental skills for which any MT student had no time, and later undertaking some basic experiments that Maxwell had devised in the *Treatise*; and we shall also find him developing some basic theoretical work on electrodynamics. All this work finally granted him a university lectureship in Cambridge in 1883 and fellowship of the Royal Society in the spring of 1884, even before he was appointed professor of Experimental Philosophy and director of the Cavendish Laboratory.

Competition for college fellowships in Cambridge was fierce, and candidates were allowed to apply up to three times, in the first, second and third summers after graduation. It was rather unusual to make use of the first attempt since, besides sitting for an examination, candidates had to write an original piece of research work and possibly compete with people in their second or third year after graduation; but Thomson applied for it and, contrary to the expectations of his college tutor, he was elected to a fellowship. In retrospect, J.J. would explain that the reason for his success was, partly, that his research topic had been in his mind, only waiting to be formally developed, since his days at Owens College with Balfour Stewart: the reduction of all forms of energy to manifestations of kinetic energy. Parts of this dissertation were later published in two papers in the *Philosophical Transactions*,[1] and formed the core of his first book, *Applications of Dynamics to Physics and Chemistry*,[2] which was published in 1888. This work linked his early ideas from Owens with the new methods that he had learnt in Cambridge. As Topper explained, "Since the essence of Maxwell's theory had been the identity of mechanical and electrical energies, Thomson carried this to what he considered to be its logical conclusion—to reduce all forms of energy (potential, electromagnetic, chemical, thermal, and so forth) to kinetic energy".[3]

This first project shows a characteristic that I want to emphasise in this chapter: Thomson had an all-embracing view of what he called the *Physical Sciences*, a concept that included all branches of physics and some areas of chemistry. In the preface to *Applications of Dynamics to Physics and Chemistry*, Thomson argued that, since the issues in the book "relate to phenomena which belong to the borderland between two departments of Physics [meaning physics and chemistry], and which are generally either entirely neglected or but briefly noticed in treatises upon either, I have thought that it might be of service to students of Physics to publish them in a more complete form".[4] The first few chapters of the book introduced the reader to the mathematical methods of Lagrange and Hamilton as presented both by Maxwell and by Routh,[5] which enable the scientist to deal with physical phenomena without knowing the intrinsic properties of the system or the nature of the mechanism to be investigated. The phenomena to which these methods were applied in the following chapters of the book include temperature, electromotive forces, elasticity, evaporation, absorption of gases by liquids, surface tension of solutions, chemical dissociation, chemical equilibrium and the connection between chemical change and electromotive forces.

Thomson had such a strong faith in the power of the mathematical methods that he had learnt in the MT that he thought they were also valid to explain many chemical phenomena. To give just a few examples, when talking about chemical dissociation, Thomson claimed that "this phenomenon has some analogy with that of evaporation", since one could imagine that in the same way there is an equilibrium between liquid and gas that allows for condensation and evaporation between the two states of matter, "so in dissociation, we have also equilibrium between portions of the same substance in two different conditions, both in the gaseous state, the molecules in the one condition being more complex than those in the other, and matter being able to pass from one condition into the other by the more complex molecules splitting up, 'dissociating' as it is called into the simpler ones, while on the other hand some of the simpler ones combine and form the more complex molecules".[6] In a later chapter on electrolysis, the continuity between physics and chemistry is again made clear: "the principle that when a system is in equilibrium the Hamiltonian function is stationary can be applied to determine the connexion between the electromotive force of a battery and the nature of the chemical combination which takes place when an electric current flows through it".[7]

Thus, Thomson's work embodied both a metaphysical and a methodological reductionism. He not only thought that chemical processes were ontologically reducible to physico-dynamical processes, but also that the tools to study chemical processes were the mathematical methods he had learnt in the Mathematical Tripos. However, I differ with the explanation of Crowther who, in his history of the Cavendish laboratory, said that this 1888 book "made [Thomson] one of the founders of the new science of theoretical physical chemistry. If he had continued in it, he might have become as distinguished in physical chemistry as he became in physics".[8] Thomson's reductionist programme was quite different to the later discipline of physical chemistry. According to most historians of science, physical chemistry was the outcome of chemists interested in the physical foundations of chemical phenomena. The fathers of physical chemistry, the Swede Svante Arrhenius, the German Wilhelm Ostwald, and the Dutch J. H. van't Hoff, were chemists—mainly organic chemists—willing to develop a new discipline.[9] Thomson, on the other hand, was mainly a physicist trying to extend the methods of physics into the chemical realm, in a more all-embracing way. That may explain why Thomson was not in contact with the new physical chemists until well into the twentieth century, after Thomson's corpuscle became the electron of the physical chemists.[10]

2. Early Experimental Work at the Cavendish

Although not completely ignorant of experimental matters, thanks to his time at Owens College, the two and a half years in the MT had almost prevented him from even visiting the Cavendish, only a few steps down the road from Trinity College, except for short visits to his Mancunian friends Poynting and Schuster. Had he spent there some time, he would have met Maxwell in person, the first director of the Cavendish, who died prematurely in 1879. The laboratory was now under the direction of Lord Rayleigh. Thomson's first re-encounter with an experimental laboratory was not totally successful: he failed to do anything significant in trying "to detect the existence of some effects which I thought would follow from Maxwell's theory that changes in electric forces in a dielectric produced magnetic forces".[11] Rayleigh suggested that Thomson should start with easier, more standard, experiments that would allow him to acquire a close familiarity with basic experimental techniques in electromagnetism. In a few months, his results materialised in his first experimental paper, published in the *Philosophical Magazine*.[12]

The experiments proved that "a coil acts in many cases like a condenser, and possesses appreciable electrostatic capacity", something that had been incidentally mentioned by Helmholtz and others, and about which "I am not aware, however, of any experiments in which this property produced any very marked effect".[13] Thomson's aim was, basically, not so much to prove this effect, but to measure its intensity. From a practical point of view, the experimental setting was nothing new, but "closely resemble[d] some used by Lord Rayleigh in some electromagnetic experiments" ten years earlier, in 1870: two wires were wound side by side on a bobbin, one being the primary and the other the secondary circuit, on which the current was induced. The goal of the experimental setting was to measure the condenser capacity of the secondary coil by trying to find induced currents in it when the current in the primary coil was broken. In order to do so, this second coil was cut into sections that would be linked by small spirals through which the expected induced current passed. A small magnetised needle inside this spirals would measure the intensity of such induced currents and, indirectly, the condenser capacity of the coil. This work was relatively simple and mechanical, but helped Thomson familiarise with the Cavendish regime of experimental work in electromagnetism that had been initiated by Maxwell and which Lord Rayleigh was continuing. I shall comment on that later in this chapter.

His next experimental work, also suggested by Lord Rayleigh, was published two years later, in 1883, time by which he had been able to move towards more complicated experiments. One of the most dramatic consequences of Maxwell's work was the suggestion that light was an electromagnetic disturbance propagated in the same medium through which other electromagnetic actions are transmitted. This relationship between electromagnetism and light would eventually be explicitly observed in the 1888 experiments of Heinrich Hertz. At the time of writing the *Treatise*, Maxwell thought that this relationship could be inferred from, on the one hand, the value for the speed of light in air *V*, and, on the other hand, by measuring "*v*", the ratio between the electrostatic and electromagnetic units of electric charge, which had dimensions of a speed, and which physically accounted for the speed of the transmission of electromagnetic waves. Since the ways to obtain both values were totally independent, "the agreement or disagreement of the values of *V* and of *v* furnishes a test of the electromagnetic theory of light".[14] The *Treatise* also gave a number of possible experimental settings that would provide with a value of *v*.

By 1880 there had been up to six attempts to measure *v*, all of which gave results in the same order of magnitude to the speed of light. This latter value was, also, only known in first approximation. Neither value could "be said to be determined as yet with such a degree of accuracy as to enable us to assert that the one is greater or less than the other. It is to be hoped that, by further experiments, the relation between the magnitudes of the two quantities may be more accurately determined".[15] And this was the fundamental task to which Thomson was about to contribute in his first serious experimental paper.[16] J.J.'s work followed one of the methods suggested by Maxwell to determine *v*: a modified Wheatstone bridge, with a condenser in one of its arms, allowed for simultaneous measurement of the capacitance of the condenser (an electrostatic unit) and the resistance of the different arms of the bridge (an electrodynamics unit). In his biography of Thomson, Rayleigh (son of the other Lord Rayleigh) pointed at two facts about these experiments worth mentioning. First, that the then director of the Cavendish "had already designed some of the apparatus to be used, and had contemplated taking part in the work himself … but, as he mentioned to me many years later, 'Thomson rather ran away with it', a natural result of energy, enthusiasm and self-reliance".[17] Second, that Thomson seems to have been "over sanguine" in spite of his "rather limited experience in experiments", overlooking possible sources of error "without applying the test of using alternative methods".[18]

The results he obtained, 2.963×10^{10} cm per second, were the closest to the speed of light known at the moment (around 2.998×10^{10}), but they later proved to have an accumulated error of 1%. In 1890, he repeated the experiments with G.F.C. Searle, identifying some of the sources of error, and obtained a value of 2.995×10^{10}.[19] As we shall see, by that time, J.J. had almost finished preparing the revised third edition to Maxwell's *Treatise*.

3. The Origins of the Electromagnetic Theory of Matter

Warwick describes Thomson as one of the most prominent Maxwellians of the second generation, by which he means the group of Cambridge graduates, including Thomson, J.H. Poynting, R. Glazebrook and J. Larmor, who "received a systematic introduction to Maxwell's work in the coaching room [with Routh] and in W.D. Niven's lectures".[20] Thomson mastered the content, the ethos and the limitations of the *Treatise* to such an extent that he could "fashion himself as the University's leading expert on Maxwell's theory".[21] The training in the Mathematical Tripos was such that only a few months after graduating, and while preparing his dissertation for the Trinity College fellowship, J.J. could publish a paper in the *Philosophical Magazine* in which he went beyond the *Treatise* in describing the behaviour of light as an electromagnetic wave.

The object of this paper was to generalise Maxwell's equations of light "by taking into account the motion of the medium through which light is passing",[22] something that might help validate Maxwell's theory of light with particular experimental results. In particular, Thomson's generalised equations of light helped him to derive mathematical formulae for the reflection and refraction of a ray of light at the surface of a transparent medium, as well as the change in speed for light in a moving medium. As Warwick pointed out, "where Maxwell had attempted to develop a complicated dynamical account of the interaction of electromagnetic waves with transparent media, Thomson assumed only that the rapidly oscillating electromagnetic fields of a light ray were subject to the same boundary conditions at the surface of a dielectric as were static electric and magnetic fields".[23] The results Thomson obtained were in accordance with the ones given by the optical theory of light. J.J. was here embodying the ethos of a Maxwellian of the second generation: on the one hand, he was focusing on the central question of the *Treatise*, i.e., the electromagnetic nature of light, as he had also done in his first serious experimental work; on the other hand, he was addressing the problem with the use of Lagrangians and Hamiltonians, and not with the use of some intricate mechanism for the behaviour of the light ray, as Maxwell himself had unsuccessfully tried in the 1860's. Furthermore, by applying the continuity of boundary conditions to the case of rapidly oscillating fields, Thomson was basically putting together two different sections from the *Treatise*. This is the “kind of information that a member of the first generation [of Maxwelians] might have passed on to a keen student like Thomson”.[24]

More relevant for the history of electrodynamics was his paper “On the Electric and Magnetic Effects produced by the Motion of Electrified Bodies”, published in April 1881, and normally mentioned as the foundational stone of the so-called electromagnetic theory of matter. Again, the paper aimed at developing Maxwell’s *Treatise* further, but now the trigger were some recent experiments on cathode rays by William Crookes and by Eugen Goldstein who, at that time, thought that these rays consisted of charged particles at high speed.[25] Thomson was, at this stage, interested in cathode rays only as a way to apply its experimental results to the ratification and further development of Maxwell’s *Treatise*. His paper consisted of a mathematical analysis of the behaviour of a charged particle in an electric and magnetic field. In particular, he studied three aspects: “the force existing between two moving electrified bodies, what is the magnetic force produced by such a moving body, and in what way the body is affected by a magnet”.[26] The particular way Thomson dealt with the mathematical formalism in this paper is, yet again, a very good example of his training in the Mathematical Tripos in the late 1870s: far from making new and complex mechanical suppositions, Maxwell’s equations were treated as a given. “When this requirement broke down, Thomson clearly thought it appropriate to proceed by making the simplest possible mathematical adjustment to the troublesome expression such that Maxwell would be satisfied”,[27] something that was in tune with the problem-solving mentality of a recently-graduated high wrangler.

The starting point for J.J. was the case of a charged sphere moving in a medium of a certain inductive capacity. Following Maxwell, Thomson argued that the moving charged sphere would induce a variation of the electric displacement at every point of the medium. “Now, according to Maxwell’s theory, a variation in the electric displacement produces the same effect as an electric current; and a field in which electric currents exist is a seat of energy; hence the motion of the charged sphere has developed energy, and consequently the charged sphere must experience a resistance as it moves through the dielectric".[28] This resistance could be mathematically treated as an increase of mass of the moving particle. Thus, the mass of the charged particle would grow by its own movement, like any solid moving in a perfect fluid. To understand better this idea, we can look at his own explanation of the effect later, at the end of his life: "It is interesting to compare this result with that for a sphere moving through water. When the sphere moves it sets the water around it in motion. The necessity of doing this makes the sphere behave as if its mass were increased by a mass equal to half the mass of a sphere of water of the same volume as the sphere itself. This additional mass is not in the sphere but in the space around it".[29]

The idea that a moving charged particle generated an increase of mass due to its own movement was picked up a decade later by his closest rival in the MT, Joseph Larmor, as well as others, both in England and in the Continent, to develop a theory of matter in which the origin of *all* mass was supposed to be of electromagnetic origin.[30] As we shall see in chapter 4, in the first years of the new century, J.J. Thomson himself would toy with the idea of electromagnetic mass as the source for the mass of his corpuscle and, since he was hoping for all matter to be composed only of corpuscles, for the mass of all matter. The idea was then short-lived; but in 1881 it was totally gone away from J.J.'s picture. As Jammer remarked many years ago,[31] the example Thomson picked to illustrate the increase of mass in his 1881 paper illustrates how far he was from seriously considering any sort of an electromagnetic theory of mass. "To form some idea of what the increase of mass could amount to in the most favourable case—he says—let us suppose the earth electrified to the highest potential possible without discharge, and calculate the consequent increase in mass".[32] The calculations for this "most favourable case" gave an increase of 650 tons for the earth, which is totally negligible compared to the whole mass of the earth. This case was certainly the "most favourable" because this extra mass depended directly on the radius of the sphere, so the bigger the sphere the higher the increase in mass. With this, Thomson implied that the induced mass, although mathematically interesting, was actually irrelevant for ordinary matter.

To finish with this paper, let us go back to Thomson's motivation for it; i.e., to shed some light on discussions about the nature of the phenomena of discharge in vacuum tubes that Crookes and others had been studying. In particular, Crookes had started a controversy with German scientists on the nature of cathode rays, between corpuscular and undulatory explanations of this phenomenon of discharge. Furthermore, Arthur Schuster had been working in the Cavendish on discharge-tubes-related phenomena, on which he would eventually become an expert in Owens College. As we shall see, this area of study would also become one of the cornerstones of J.J.'s scientific career. Now, in 1881, however, he only used it as a source of information useful to develop some theoretical aspects of Maxwell's *Treatise*, but also as a sufficiently new area in which to make some original contribution, also following ideas from the *Treatise*. Sparking from his study on the behaviour of electrified moving bodies, Thomson suggested a mechanism to explain one aspect of the discharge of cathode rays, i.e., "the green phosphorescence observed in vacuum-tubes at places where the molecular streams strike the glass".[33] He assumed that the collision of an electrified molecule on the glass of the tube would involve a reversal of the velocity of this charged body, therefore a very large variation in the vector potential generated by it and, as a consequence, the glass would be subjected to a rapidly varying electromagnetic force. But this, "if Maxwell's electromagnetic theory of light be true, is exactly what it is subjected to when a beam of light falls upon it, which we know is the ordinary method of exciting phosphorescence".[34] This last statement again illustrates something we have already found: Thomson's early work after graduation was typical of a Cambridge Maxwellian of the second generation in that, while taking the Treatise as the basic electromagnetic theory to be developed, it was also aware of its basic limitation, i.e., the need for a clear confirmatory experimental proof of the relationship between light and electromagnetic field.

4. The Vortex Ring Theory of the Atom

But perhaps the most revealing work of Thomson in this early period is his essay "On Vortex Rings", with which he won the prestigious Adams Prize of 1882, a prize that had been established in 1848 to commemorate the discovery of Neptune by John Couch Adams, and which was, at the time, open to Cambridge graduates only. The subject for that year was “an investigation of the action of two vortex rings on each other,” and it was typical of the Cambridge of the day and fashionable among many British mathematicians and physicists.[35] The question of vortex rings had been present since 1867, when William Thomson had suggested an atomic model in which atoms could be thought of as vortex rings in the ether. Besides its unifying character, this theory of a plenum was interesting since it maintained the immutability of primordial atoms. Helmholtz had shown, in a previous paper, that vortex filaments in a perfect fluid would not dissipate or be destroyed. Since the ether was mainly understood as a fluid, these results served to account for the indestructibility of atoms, and, at the same time, to treat them as special manifestations of the ether. This conception had considerable impact among scientists, and in the 70s and 80s “British physicists became increasingly attracted to this simple picture of atomic matter involving a concentration of ether spinning like a smoke ring in air”.[36]

William Thomson, as the father of the vortex atom model, felt that the theory was consistent with two of his key philosophical prejudices: his enthusiasm for dynamical models and his profound dislike for atomism, understood as Lucretius or Newton did, i.e. “the monstrous assumption of infinitely strong and infinitely rigid pieces of matter”.[37] This model gave him, for some time, the possibility of explaining those atoms in terms of a more fundamental continuous fluid. Thomson disliked the atomic theory because he felt that “Lucretius’ atom does not explain any of the properties of matter without attributing them to the atom itself. … Every … property of matter has … required an assumption of specific forces pertaining to the atom”.[38] In other words, W. Thomson felt that the nature of matter was not fully explained by atomic theories: atoms could explain the organization and some of the properties of bodies, but neglected the explanation of what matter actually is. In the years up to 1880, W. Thomson worked at this hypothesis and tried to explain with it many physical phenomena, including gravitation, the kinetic theory of gases, the dissipation of energy and the wave motion in solids and liquids.[39] Although, by 1882, W. Thomson and P.G. Tait had given up this cosmological idea, the topic remained, however, of interest to mathematicians. This explains why most of the papers on vortex theory were published in journals of mathematics, not physical journals, since it was regarded as a most interesting mathematical problem. Not only hydrodynamics was involved, but also the new area of topology of knots.[40]

In this milieu, the 1882 Adams Prize was intended mainly as an exercise with a purely mathematical interest, but J.J. Thomson managed to broaden the question once again and to turn the problem of the stability of two vortex rings into an all-embracing theory of matter, thus reviving the theory of vortex atoms as a true theory of matter. In his words, an atomic theory based on the behaviour of vortex rings "has *á priori* very strong recommendations in its favour. For the vortex ring obviously possesses many of the qualities which a molecule that is to form the basis of a dynamical theory of gases must possess. It is indestructible and indivisible; the strength of the vortex ring and the volume of liquid composing it remain for ever unaltered; and if any vortex ring be knotted, or if two vortex rings be linked together in any way, they will retain for ever the same kind of be-knottedness or linking. These properties seem to furnish us with good materials for explaining the durable qualities of the molecule".[41] Moreover, many other properties of the molecules, either independently or in relation to other molecules, could be accounted for using this model. The theory could explain atoms as secondary structures of a primary entity, the ether, and so it could be regarded as a more fundamental explanation of matter, since "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 stated".[42] The argument of simplicity was a very powerful one; the only fundamental entity of nature would be the all-present fluid and everything could be represented by the energy in every point. Many years later, in his autobiography, Thomson looked back to this theory with some nostalgia to say that "there was a spartan simplicity about it. The material of the universe was an incompressible perfect fluid and all the properties of matter were due to the motion of this fluid".[43]

Thomson's essay "On Vortex Rings", besides being a typical example of Cambridge mathematical work with its over 50 pages of Lagrangians and differential equations, reveals in its last section that all these calculations "would enable us to work out a complete dynamical theory of gases",[44] by which he meant both the physical and chemical properties of material substances. In the essay, he also studied the possibility of permanent combinations of elementary vortex rings, and he concluded that there could be stable combinations in systems consisting of up to six such rings. This was quite in agreement with the possible valences of most elements, and he was led to speculate as follows: "The atoms of the different chemical elements are made up of vortex rings all of the same strength, but some of these elements consist of only one of these rings, others of two of the rings linked together, others of three, and so on"; thus, in this case, "each vortex ring in the atom would correspond to a unit of affinity in the chemical theory of quantivalence".[45] In this model, the mass of the atoms was no longer their fundamental characteristic, and their chemical affinity assumed such a role, bringing fundamental chemical properties into the picture, which shows us again, from a different angle, that J.J. was very much interested in chemical combinations of elements and substances as a means towards a better understanding of the foundations and the constitution of matter.

Thomson managed to make it his own and to bring the study of vortex rings further into the realm of ultimate explanations of physical phenomena, so much so that, some months after he wrote the essay, he applied some of its results in an attempt to explain the conduction of electricity in gases.[46] Furthermore, the essay "On Vortex Rings" is also revealing about the way in which Thomson approached chemistry: he was trying to explain chemical processes in terms of dynamical physics, thus abandoning the idea of affinities as some sort of force that was different from mechanical forces. The fact that Thomson was trying to reduce chemistry to physics was also evident to those who read the essay in Cambridge. For example, G.H. Darwin, the professor of astronomy, congratulated Thomson for winning the Adams Prize in the following terms: "The problems you have solved are of amazing difficulty, and the results of the greatest interest. May you go on and discover a true dynamical theory of chemistry".[47] Nevertheless this aim was not exclusive to Thomson, and indeed, in 1885, G.F. FitzGerald would correspond with Thomson while trying to develop a model of the electromagnetic ether, saying: "I thought it possible that electrical forces might be explained by these general effects of vortices &c. and that chemical forces might be due partly to these and partly to actions produced by the distortions of the vortices. For though chemical and electrical forces are due to like causes nevertheless chemical action is of a much higher order of complexity than simpler electrical actions".[48] From his very early years as a researcher, Thomson proved that he had a deep interest in formulating a dynamical theory of chemistry, which would incorporate chemistry within the deductive Physical Sciences; and his work was seen in this way by many of his peers.

In his study of the *status quo* of the vortex atom theory in Victorian science, Helge Kragh gave it a very definite lifetime: from 1867, the year of W. Thomson's suggestion, to 1898, when William Hicks, a professor in Sheffield, gave up the project. J.J. Thomson worked on it until 1891, when he moved to the theory of Faraday tubes. The new theory, which we shall encounter in the next chapter, was less cosmological, for its principal goal was to explain the interaction between electricity and matter, the key idea of his long research project studying the discharge in tubes. Eventually, that new theoretical framework led him to the discovery of the corpuscle. The new theory would, nonetheless, keep one of the main features of the vortex atom theory, i.e., the assumption that the fundamental entity in nature is the ether and that atoms (and, later, corpuscles), as well as the Faraday tubes, are an epiphenomenon of the ether.

5. Director at the Cavendish Laboratory

In 1874, the Cavendish Laboratory was inaugurated in the New Museums Site, a piece of land that had previously hosted the Botanic Garden. Its main purpose was to serve as a teaching laboratory for the study of heat, electricity and magnetism. These three subjects had been introduced into the Mathematical Tripos in 1868 and, as a result of this decision, the new laboratory of physics and a new chair of experimental physics were created. The first professor of experimental physics was James Clerk Maxwell, who was capable of both highly sophisticated theoretical work and first-class experimental research. These two qualities were essential for a first Cavendish Professor in Cambridge, since it contented both traditional Cambridge dons, who thought that physics was necessarily mathematical, and also the reformers, who believed in a closer connection between the university and the practical, industrial and technological needs of the nation.[49] The main role of the Cavendish was to serve as a laboratory devoted to teaching purposes. Research, as was usually the case in Cambridge, was a private activity for a professor in early and mid-Victorian Cambridge. As a matter of fact, Cambridge was relatively late among the leading universities in incorporating a laboratory for physics into the landscape; so much so that the Cavendish was not part of the first generation of institutions in the so-called 'laboratory revolution' in Britain.[50]

In 1884, the chair of Experimental Physics in Cambridge was vacant. Lord Rayleigh had accepted the professorship after Maxwell's untimely death in 1879 but, after serving for over four years, he resigned, and the university had to start the search for a new professor. The candidates for the chair were Arthur Schuster (Manchester), Osborne Reynolds (Manchester), Richard T. Glazebrook (demonstrator at the Cavendish with Rayleigh), Joseph Larmor (first wrangler in 1880, now in Galway) and J.J. Thomson (Trinity College, Cambridge). The powerful Trinity College was eager to dominate the election, in spite of the fact that Rayleigh's favourite was Glazebrook, who had been at the Cavendish since 1876, and had thus worked with both Maxwell and Rayleigh. But precisely because of that, it is quite likely that some of the electors wanted a complete change at the Cavendish, for Maxwell and Rayleigh had connected it too closely to the electrical industry, a characteristic which was not appealing to the Cambridge establishment.[51]

There has been much speculation about the reasons why the relatively inexperienced J.J. Thomson was appointed to the chair of experimental physics. Thomson said that his election as Lord Rayleigh's successor was a complete surprise to himself as well as to others.[52] To take one example, when Schuster complained that a junior person had been appointed, his mentor in Manchester, Roscoe, tried to calm him down by pointing at the secrecy of any election in Cambridge: "I do not at all agree with you that any slight is thrown upon the work of the Senior Candidates by the choice of a junior. The election is a very complicated function of X. And this X is certain to remain an unknown quantity (…) as the Electors swore a dreadful oath not to reveal anything whatever about his election".[53] However, it was not very unusual to appoint brilliant young students to professorships in Cambridge and the influence of Trinity College was strong enough to make the appointment of a Trinity man quite foreseeable. "Well, no one can say that the appointment is a bad one. Only (…) it might perhaps have been a better one"[54] Roscoe said to the two Manchester contestants, Schuster and Reynolds, the day after the election. After his election, the vice-chancellor of the university, Norman MacLeod Ferrers, made the following statement: "Professor J.J. Thomson combines a great amount of mathematical knowledge and power with, as I am assured, an experimental skill which promises to make him in the long tenure of office to which I trust he may look forward a worthy successor of the two distinguished men by whom the Cavendish

Professorship has been occupied".[55] It is interesting that Ferrers regarded the experimental skills of J.J. Thomson as *promising*, as something that could be improved in the near future. It seems that the short time he had spent at the Cavendish between 1881 and 1884 was sufficient proof of his ability to become a good experimentalist, which, together with his mathematical skills, made him a promising replacement for Maxwell.[56]

A few of the first steps that Thomson took after his appointment are quite significant, for they also showed that he thought some aspects of chemistry relevant to the work of a laboratory of experimental physics. Glazebrook and William Napier Shaw, demonstrators at the Cavendish under Rayleigh, stayed in the laboratory. This was essential, to keep a certain continuity in the institution, especially in terms of teaching obligations, but they did not collaborate with Thomson in their research. A few months after his election, Thomson appointed Richard Threlfall, who had recently graduated in the NST and had later spent some time in Fittig's chemical laboratory in Strasbourg, as a demonstrator in physics. With him, he started a series of experiments on the chemical composition of gases. The first paper that they published was "Some Experiments on the Production of Ozone", which is clearly a chemical topic, and it was followed by a series of similar joint papers. That these experiments can be classified as chemical is not an anachronism, for Thomson himself spoke of them in such terms: "I am at present", he wrote to Schuster in early 1885, "experimenting on a chemical thing, viz. the proportion of ozone formed at different pressures. I worked out the thing theoretically and am now trying whether the theory is right or not".[57]

Threlfall's collaboration helps us to illustrate two aspects of Thomson's early days in his chair. On the one hand, he was clearly lacking in experimental skills and needed the help of people with more experience in the laboratory; on the other hand, he was determined to make the Cavendish a centre for *all* the Physical Sciences, chemistry included, thus abandoning the almost exclusively electromagnetic orientation which the Cavendish had had in the previous decade,[58] which had turned the laboratory into what Simon Schaffer called a "manufactory of Ohms".[59] In this respect, it is not wholly justifiable to assert that Thomson's lack of a research project was the reason a wide range of apparently disconnected research projects were carried out at the Cavendish in the 1880's and early 1890's.[60] If Thomson's idea of physics was all-embracing and included traditionally chemical topics, then all these researches had their place in the Cavendish. Threlfall was mainly an experimental chemist, and when he left for Australia the following year, Thomson's experiments ran into trouble for lack of experimental know-how. He therefore often turned to people from the Chemistry Department for advice.[61] His letters to Threlfall speak about the advice he received from the professor of chemistry, George Liveing,[62] and some of his published papers acknowledge the help of the praelector in chemistry at Gonville and Caius College, Matthew M. Pattison Muir.

6. Third Edition of Maxwell's Treatise

Thomson's own experimental work transcended the traditional boundaries between physics and chemistry. Shortly after his election in 1884, Thomson started a long-term project on the study of electrical discharges in tubes filled with gases. In the *Treatise*, Maxwell had said that "these, and many other phenomena of electrical discharge, are exceedingly important, and when they are better understood they will probably throw great light on the nature of electricity as well as on the nature of gases and the pervading space. At present, however, they must be considered as outside the domain of the mathematical theory of electricity".[63] Thomson agreed that the study of discharge in gases promised to give insight into the nature of electricity (a physical problem) and the composition of matter (a chemical issue); but he thought that the time had now come to try to give a dynamical account of these phenomena. Dealing with discharge in tubes meant not only dealing with electricity, but also working with different gases, the preparation of which was clearly a task for chemists.[64] Following the early theories of Crookes and Schuster, electrolysis was the model that he used to account for the phenomena that he observed in the discharge tubes, emphasising that molecules in the gas split to make the transfer of charge possible. The importance given to electrolysis can also be traced back to another suggestion of Maxwell in his *Treatise*, where he stated that "of all electrical phenomena electrolysis appears the most likely to furnish us with a real insight into the true nature of the electric current, because we find currents of ordinary matter and currents of electricity forming essential parts of the same phenomenon".[65] Thus, Thomson's actual research project in the 1880's shows that he thought that the time had come for chemistry to be given the status of a grown-up science and thus to become a full member of the Physical Sciences. He believed that it was time for scientists to develop physical—dynamical—theories to account for the chemical problems of electrolysis, gas tubes, the constitution of matter and the composition of gases.

Maxwell, the first Cavendish Professor, also had his own views on the boundaries between physics and chemistry. Whereas Whewell had made it clear that chemistry was not yet a proper physical science, Maxwell admitted that some areas of chemistry could be regarded as physics. In his classification, “What is commonly called Physical Science occupies a position intermediate between the abstract sciences of arithmetic algebra and geometry and the morphological and biological sciences”, where the morphological sciences “are rich in facts, and will be well occupied for ages to come in the coordination of these facts, though their cultivators may be cheered in the mean time by the hope of the discovery of laws like those of the more abstract sciences, and may indulge their fancy in the contemplation of a state of scientific knowledge when maxims cast in the same mould as those which apply to our present ideas of dead matter will regulate all our thoughts about living things”.[66]

The case of chemistry was, for Maxwell, rather odd. Being a Physical Science, chemistry incorporated dynamical explanations but, at the same time, ran away from them with its constant accumulation of new facts and data. In his words, “though [Physical] Dynamical Science is continually reclaiming large tracts of good ground from the one side of Chemistry, Chemistry is extending with still greater rapidity on the other side, into regions where the dynamics of the present day must put her hand upon her mouth. But chemistry is a Physical Science, and that of very high rank. I do not, however, pretend to be able to go over its possessions and to show strangers the boundaries”.[67]

Here, I emphasise the link between Thomson’s and Maxwell’s ideas because, at this point, J.J. was not only a prominent member of the second generation of Maxwellians but, in a way, he shaped himself as the natural continuator of the great physicist, his successor in the chair of Experimental Physics and the one who would bring Maxwell’s project to its ultimate fulfilment. That partly explains his constant reference to the *Treatise* in most of the projects he undertook. This also gives us the context of one of his early tasks in the directorship of the Cavendish: to prepare a third edition of the *Treatise*. The original but complicated two-volume opus had already seen a second edition in 1881. Maxwell himself had begun the task of correcting, changing and clarifying many aspects of the Treatise. By his death, he had only had time to revise, correct and, at times, totally re-write the first nine chapters. The rest had been only slightly corrected in some obvious mistakes by W.D. Niven, with the assistance of his brother Charles and J.J. Thomson.[68] By the end of the decade of the 1880s, a new edition that incorporated clarifications and the latest developments in electromagnetism was needed, and J.J. agreed to undertake this task. In the preface to this third edition of 1891, Thomson explained his original idea for this undertaking: "When I began to revise this Edition it was my intention to give in foot-notes some account of the advances made since the publication of the first edition, not only because I thought it might be of service to the students of Electricity, but also because all recent investigations have tended to confirm in the most remarkable way the views advanced by Maxwell. I soon found, however, that the progress made in the science had been so great that it was impossible to carry out this intention without disfiguring the book by a disproportionate quantity of foot-notes".[69] The solution he chose: to leave the Treatise as it was and to "complement" it with a whole new book that included the latest developments in electromagnetism and which is referred to throughout the third edition a the "Supplementary Volume". We will discuss this book at the beginning of the next chapter. Now, let us go back to the institutional *tour de force* that Thomson undertook with the other scientific departments in Cambridge at the beginning of his tenure.

7. Mapping the Domains of the Physical Sciences

From an institutional point of view, physics and chemistry were two quite separate worlds in Cambridge in the 1870's. Physics was mainly part of the MT and chemistry was one of the central disciplines in the NST. This state of affairs was not in the least unique. According to John W. Servos, by the middle of the century, chemists and physicists in many of the leading European universities worked in different institutes, used different instruments, and measured different properties. They even spoke different languages, since, whereas the chemist needed only arithmetic to express weight relations, the student of physics was becoming ever more dependent upon the higher mathematics.[70] Looking at the institutional development of the departments of physics, chemistry and, later, engineering, it could be said that Cambridge was just one particular case of this general description. However, looking at the work and the interests of some of the people who were influential in the university, one can argue that the process of specialisation took place alongside a parallel—but unsuccessful—effort to unify of the sciences, with mathematical physics as the exemplary "science". As we have seen, this was the view of J.J. Thomson, but it was also that of the professor of chemistry, George Downing Liveing.

Liveing was the professor of chemistry in Cambridge since 1861. His formation made him capable of teaching the natural sciences in the Cantabrigian style. He graduated as 11th wrangler in 1850 and enrolled in the first ever NST course, after which he was awarded a lectureship at St. John's College. The college soon realized the need for new facilities for teaching demonstrations, as well as for independent research in chemistry, and, therefore, a new laboratory was built. This laboratory became the first place in Cambridge where practical experimental tuition was part of the training of students.[71] There were also some meagre university facilities assigned to the chair of chemistry: an office, which was to be shared with the professors of botany and the Jacksonian professor, and two small empty rooms. These rooms and an extension to them provided a small chemistry laboratory for the purpose of demonstration lectures and research; but the number of students and the amount of research increased without any corresponding growth in the facilities. That is the reason why Liveing preferred to use the laboratory of St John's for teaching purposes, in the first years of his tenure, while he kept the university facilities for his own research.[72]

Liveing is a relatively unknown figure in the history of science. The impact of his work in Cambridge has more to do with the large amount of administrative work that he did than with his research. From his point of view, chemistry was the central experimental science and physics was the model towards which chemistry had to be directed. To give an example, in 1874 he wrote in the Student's Guide to the University of Cambridge that "chemistry teaches laws of nature which are universal, and which find their applications whenever the structure of natural objects is under consideration". In his Presidential Address to the chemistry section of the British Association for the Advancement of Science of 1882, he claimed that chemistry was not only a descriptive science but was about to become a predictive science based on mechanical principles, as adult sciences were supposed to be from a Whewellian point of view.[73] The most relevant advance in chemistry in recent years, said Liveing, "was in the attempt to place the dynamics of chemistry on a satisfactory basis, to render an account of the various phenomena of chemical action on the same mechanical principles as are acknowledged to be true in other branches of physics. I cannot say that chemistry can yet be reckoned amongst what are called the exact sciences (…), but that some noteworthy advances have in recent years been made, which seem to bring such a solution of chemical problems more nearly within our reach".[74] This optimistic statement was followed by a fragment of the *Philosophy of the Inductive Sciences*, in which Whewell regretted that the chemical attraction known as affinity had not been reduced to the mechanical laws of attraction. Liveing thought that this moment was close at hand, especially if chemists considered the principle of conservation of energy, which was admitted both in physics and chemistry, as the cornerstone in the explanation of this property.

Later in this discourse, Liveing regretted that the basic training in chemistry was still far from embodying this new approach. "We still find chemical combinations described as if they were statical phenomena (…). We still find change of valency described as a suppression of 'bonds of affinity' (…). We still find saturated compounds spoken of as if the stability of a compound were independent of circumstances, and chemical combination no function of temperature and pressure (…). They present something easily grasped by the infant mind, and schoolmasters are fond of them, but only those who have each year to combat a fresh crop of misconceptions, and false mechanical notions engendered by them, can be aware how much they hinder, I won't say the advance, but the spread of real chemical science".[75] The rhetoric of this fragment, with its allusions to childish and adult science, perhaps shows the influence of Whewell's philosophy, but more significantly, this report helps us to link his ideas to those of the young Thomson. Indeed, Liveing recognised that the vortex theory was not one of those "false mechanical notions" but that it provided a "standing ground" for the fundamental explanation of matter.[76] It is significant that, at the time when Liveing was preparing his speech, Thomson was working on the dynamics of vortex rings, with which he tried to bring about an invasion of the field of chemistry by the "adult" science of physics.

In 1885, just one year after J.J. Thomson's appointment, Liveing summarized his speculations in his only book, *Chemical Equilibrium, the Result of the Dissipation of Energy*, in which he set out his ideas in favour of basing chemical phenomena on mechanical principles. In the preface to the book, he manifested an even stronger support for the vortex theory of atoms on the grounds that it gave "more definite ideas of the manner in which dissipation of energy results in equilibrium", and because it helped the acceptance that chemical action could be "founded on sound mechanical principles".[77] This step forward towards a foundation on "sound mechanical principles" meant that chemistry could not be satisfied with the concept of affinity, "whatever that may mean", but that it needed to consider chemical bonding as consisting "in a harmony of the motions of the combined atoms in virtue of which they move and vibrate together, and that such harmony is brought about by the general force in nature which compels to an equal distribution of energy throughout the universe".[78]

Liveing further advocated for the introduction of mechanical principles in chemistry using the following analogy: "The view here advanced (…) of chemical elements is that they need not differ in substance one from another but only in the magnitude and the form of structure of their atoms. Hydrogen and oxygen may be compared to different bells which though made of the same metal ring out different tones. The possibility of the chemical combination of two atoms will depend on the possibility of complete harmony of their motions".[79] It is interesting to note that Liveing is being fully consistent with Thomson's views on the pre-eminence of kinetic energy, which had been the core of his Trinity fellowship dissertation and the central idea of his 1888 book *Applications of Dynamics to Physics and Chemistry*. Even though there is no extant correspondence between Liveing and Thomson, it is quite likely that they discussed such ideas in their meetings in Cambridge.[80] Nevertheless, in 1891, there were complaints that in the Department of Chemistry "there is too much of the Physical Side and very little Organic teaching",[81] a defect that had to be corrected. The programmatic understanding that existed between Liveing and Thomson was not exciting for everyone in Cambridge.

Pattison Muir, the incumbent chemist at Gonville and Caius College, Cambridge, is also relevant in this chapter. It is not surprising that he came to Thomson's aid for chemical and experimental advice after Threlfall left Cambridge, since they knew each other very well, and Thomson may have had particular confidence in him. Pattison Muir had been a demonstrator and assistant lecturer in Owens College, Manchester, from 1873 to 1877, in the years when Thomson attended that school. In his 1888 book, Thomson quoted profusely from Pattison Muir's book, a *Treatise on the Principles of Chemistry*. This work is also a good example of the reductionist approach to chemistry of some Cambridge scientists, which was consistent with Thomson's mindset. In close parallel to Liveing's statements, Pattison Muir declared his enthusiasm for the recent progress of chemistry: "of late many chemists have resumed the investigation of the general conditions of chemical action, and have obtained results which give good grounds for hoping that this study will throw light on the masses of facts already accumulated concerning compounds, and groups of compounds, and taken along with that method of investigation which is based on a study of the composition of compounds, will lead to the establishment of chemistry as a branch of the science of dynamics".[82]

Neither Thomson, nor Liveing, nor Pattison Muir thought that chemistry was about to disappear. A grown-up chemistry could be subsumed into a general category of Physical Sciences, in the same way that electricity and heat were. Pattison Muir would try to set the boundaries between physics and chemistry by saying that "Chemistry deals with those reactions between bodies wherein profound modifications in the properties of the bodies occur" and by stating that "Chemistry furnishes problems for the solution of which physical and dynamical methods are applicable. Chemical science is ever tending toward abstract truths, i.e. truths involved in many phenomena although actually seen in none".[83] Thus, chemistry had its own methods and aims, as far as empirical data was concerned; but, in terms of explanations, physics was the model. Even more, the approach of chemistry and physics needed to take place from both sides. Not only did chemists need to approach physics in search of explanations, but also physicists were expected to broaden the scope of their explanatory methods to include the realm of chemical phenomena. As an example, Pattison Muir would talk about the molecular hypothesis as "one of the lines along which dynamical science pursues its advance into the sphere of chemistry. The study of chemical phenomena is also brought within the pale of dynamical methods by the application to these phenomena of the general principles of the conservation and degradation of energy".[84]

In addition to Liveing, there was a second professor in Cambridge who devoted himself to chemistry. James Dewar was Jacksonian Professor of Natural Experimental Philosophy, a chair that had been created in 1783 and that had been held, at times, by people working in engineering, as well as in other applied sciences such as medicine, metallurgy, chemistry and mechanics. Dewar was educated in Edinburgh and had his first appointments there, but he was elected Jacksonian Professor in 1875. However, he decided to make this position compatible with the Fullerian Professorship of Chemistry at the Royal Institution of Great Britain, to which he was appointed two years later. This meant that he spent more time in London, where he had better facilities for his experiments.[85] That Dewar was suitable for a position as a professor of experimental science in Cambridge can be inferred, among other factors, from the report that Maxwell wrote about him in 1874 for a position at the University of Glasgow. It is likely that, only one year later, Maxwell would have used similar arguments: "I consider it of great importance in the present state of science that the Chairs of Chemistry in our universities should be filled by men who are able not only to extend our knowledge of the combinations of matter, but also to take part in working out the right views of combination of bodies, and who must therefore possess a thorough knowledge of physical as well as of chemical theories, and a mastery of the most accurate methods of research. The researches of Mr. Dewar in physical chemistry relate to properties of bodies which are among the most fundamental of those lying within our present range of observation, his methods of experimentation sound and well devised, and the largeness of the field which he has already exploited afford every reason to expect that he will continue to make important contributions both to the extension of chemical science and to its establishment on a firm physical basis".[86]

Liveing and Dewar started a joint project on spectroscopic analysis on which many papers were published, beginning in 1878. For that purpose, Liveing bought a spectroscope, paid for at his own expense, since the university would not easily fund scientific research. Spectroscopy could, in retrospect, be considered as a step towards physical chemistry, since it deals with the chemical composition of elements on the basis of their physical behaviour. Institutionally, however, it has been argued that photochemistry did not play an important role in the configuration of the new discipline of physical chemistry at the turn of the century.[87]

While the facilities for experimental physics, i.e., the Cavendish Laboratory, were very good, chemistry lacked a centralized laboratory for research of a high standard. On the other hand, there was more of an experimental tradition in the area of chemistry than there was in physics. The relationship between the Cavendish and the chemistry laboratories can be thought of as that of mutual help, in which neither was superior to the other. Pattison Muir's help, not only in providing advice but also some material help, such as the standardized solutions for Thomson's experiments on ozone of 1885, is a good example. Another specific example of this collaboration is Ebenezer Everett, who became the Cavendish glass blower in 1887, after training in the Chemistry Department. As a matter of fact, Liveing helped personally in this move.[88] The task of blowing glass was crucial for the kind of experiments that Thomson was performing on the discharge of gases in tubes, and Everett proved to be very successful at this job, as Thomson always acknowledged.

Nevertheless, in 1888 a new building for chemistry was built close to the Cavendish laboratory. The new facilities consolidated the institutionalization of chemistry in Cambridge, the recovery of her independence from the Cavendish and a shift towards research in more practical areas of chemistry. The superiority of physics in terms of facilities disappeared and, with this, Thomson's ideal of having chemistry subordinated to physics was jeopardised, although not totally abandoned. In 1894, he would use a metaphor according to which physics, chemistry and engineering appeared to be working on a common project. In his words, "the work of chemists and physicists may be compared to that of two sets of engineers boring a tunnel from opposite ends – they have not yet met, but they have got so near that they can hear the sounds of each other's advances".[89] This quotation brings us the third element, Engineering, for which Thomson had also his own plans.

8. A new Tripos for Engineering

Thomson needed the help of experimental chemists in order to develop his research as Cavendish Professor, and this necessity helped forge a strong relationship between him and people from the department of chemistry. This relationship was not, however, merely contingent, but was also a manifestation of a common philosophical goal: to make use of the explanatory power of physics to account for chemical phenomena. A final example of Thomson's efforts to make physics—or, perhaps better, dynamics—central in the Physical Sciences are his efforts, in the late 1880's and early 1890's, to found a new Tripos that would emphasise even more this centrality of physics in the organisation of the sciences.

In the 1830's and 1840's, several British universities created chairs of engineering to cater for the demand of engineers in an increasingly industrialised country. King's College, London, was the first university to have a professor in engineering, after which Glasgow, University College London, Trinity College, Dublin, and Queen's College, Belfast, followed suit. In Cambridge, discussions on creating a School of Engineering started in 1845, but that project saw opposition from the most conservative dons of the university, for whom a school of engineering was tantamount to having a warehouse giving university degrees. The little engineering one could find in the Cambridge syllabus was traditionally taught under the name of Mechanics by the Jacksonian professor of Natural Experimental Philosophy.

As we saw earlier, in 1875, a chemist—Dewar—was appointed to the Jacksonian professorship and a new chair for "Mechanism and Applied Mechanics" was created. The new professor was to teach "(1) The Principles of Mechanism, (2) The Theory of Structures, (3) The Theory of Machines, and (4) The Steam Engine and other Prime Movers",[90] topics in which a Cambridge student could take a special examination. The new professor was also expected to play a central role in the eventual creation of a school of engineering in the university. But such a school was proving elusive. James Stuart, the first Professor of Mechanism, worked on two fronts in order to consolidate his department. On the one hand, he was keen to obtain a physical space for engineering, for which the University provided an old wooden hut just beside the Cavendish; on the other hand, he was eager to establish a new tripos, different from both the MT and the NST, which would give students in Mechanism a greater prestige than the current "special examination".

The discussions leading to the first proposal for the tripos reveal that the reasons Thomson and others were supporting Stewart had to do with the gap between theoretical and experimental science in Cambridge. The two existing triposes appeared to be so clearly disconnected that both Liveing and Thomson thought it advisable to create a tripos with a broad content of both mathematics and practical science. The proposal regretted "the want of mathematical training in the great number of engineering students, and it was most desirable that the mathematical training should be considerably increased; at the same time these men could not possibly devote three or even two years exclusively to mathematics. (…) The proposal was quite as much for students of Physics as for engineering students. The students of Physics wanted a great deal of Mathematics but a very different kind of Mathematics from that in Part I. of the [Mathematical] Tripos".[91]

Later in the discussions, Thomson made it clear that the reason he was supporting the new tripos was not so much for the sake of engineering but because of the lack of mathematical formation in the NST. That is why he agreed with the suggestion that "if the Mathematical Tripos could be altered that would be the best arrangement". But this would take too long and "the need of mathematical training for these students was very urgent".[92] What was happening was that, due to the difficulty and the apparent uselessness of much of the mathematics that was taught in the MT, students in the NST did not feel encouraged to attend the MT training.

Shaw, echoing Thomson's interest, put it very clearly in the Senate House when he said that "to those observing what students of physics, chemistry, and engineering, in the University actually learnt, it became at once obvious that there was a good deal of ground common to all, and that the examination which covered the common ground and included certain portions of Mathematics would be extremely useful".[93] In his conversations with other engineers, Thomson had also drawn the conclusion that "a knowledge of Physics was absolutely necessary for a scientific training for engineers; indeed one of the most successful Professors said that he thought it more necessary even than practical engineering, for a student could get practical engineering after he had left the University, but he would have no other opportunity of making the simple physical experiments contemplated by the regulations".[94] This helps us to understand the agenda of Thomson to transform physics into the central discipline between a too theoretical MT, and a too experimental NST in which physics played a subsidiary role.

It is quite significant that the final proposal for a Tripos for Mechanics, Physics and Chemistry was signed by Liveing and Dewar, but did not have the consent of Thomson and Glazebrook: physics was considered an optional subject, not the centre of the tripos. This proposal was, nevertheless, turned down, and Stuart resigned in 1889. A new professor was appointed on 12 November 1890. James Alfred Ewing was already Professor of Engineering and Drawing in University College, Dundee, and this experience gave him the authority to work successfully towards the creation of a Department of Engineering in Cambridge.

After the failed attempt to use the Engineering Tripos as a way to consolidate physics as the queen of sciences, Thomson and Glazebrook argued that the optimal situation, as far as physics was concerned, would be that of a Tripos in which students could get almost all the mathematical training acquired in the MT together with the physics and chemistry in the NST, without the need of doing the complete training of the first years of the MT. In a report submitted to the Senate House of the university in 1890, they advocated a "mixed Tripos of Mathematics and Physics".[95] In this case, physics – and not chemistry – would have been compulsory. This time the proposal was signed by Thomson, but not by the professors of chemistry, and it was not approved by the university. Also in the academic year 1891/92, discussions were held in order to split the examination in physics and chemistry into two separate and independent examinations in the NST. The curriculum in chemistry would include a voluntary exercise in fundamental physics, mainly electricity, and the curriculum in physics would also include a voluntary exercise in chemistry. These discussions point at a further separation between both disciplines from a pedagogical and institutional point of view.

In 1894, the new tripos for engineering was finally created, and Ewing managed to convince the university to build a new building for his Department in the recently purchased site between the Cavendish and the Chemistry Department. He thought it "exceedingly desirable that if, or rather when, such buildings are erected they should be close to the Cavendish and Chemical Laboratories, for much of the work of students of engineering would be done in those places, and proximity of the buildings would greatly facilitate the arrangement of lecture hours, &c."[96] The implementation of the new tripos finally consolidated a split and subsequent specialisation between physics, chemistry and engineering, far from Thomson's dream to situate physics as the queen of the Physical Sciences. In the definitive organisation of the sciences in Cambridge, specialisation won out over Thomson's dreamt of reductionism.

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1 J.J. Thomson, "Some applications of dynamical principles to physical phenomena, I", *Philosophical Transcations*, clxxvi (1885), 307-42; "Some applications of dynamical principles to physical phenomena, II", *Philosophical Transactions*, clxxviii (1887), 471-526.

2 J.J. Thomson, *Applications of dynamics to Physics and Chemistry* (London, 1888).

3 D.R. Topper, "Commitment to mechanism: J.J. Thomson, the early years", *Archive for the History of Exact Sciences*, vii (1971), 393-410.

4 Thomson, *Applications of dynamics* (ref. 27), Preface.

5 In 1876 Routh had won the Adams Prize in an essay in which he introduced "the modified Lagrangian function", which Thomson was now using. Cfr. Warwick, p. 336.

6 Thomson, *Applications of dynamics* (ref. 27), 193.

7 Thomson, *Applications of dynamics* (ref. 27), 265.

8 Crowther, *The Cavendish laboratory* (ref. 40), 107.

9 See J.W. Servos, *Physical chemistry from Ostwald to Pauling : the making of a science in America* (Princeton, 1990); Nye, *From Chemical Philosophy to Theoretical Physics* (ref. 2), chap. 5.

10 On this see Mary Jo Nye, in Histories of the Electron…

11 Recollections, p. 97.

12 J.J. Thomson, "On some electromagnetic experiments with open circuits", *Philosophical Magazine*, xii (1881), 49-60.

13 Ibid, p. 49.

14 Maxwell, Treatise, §786

15 Maxwell, Treatise §787

16 J.J. Thomson, "On the determination of the number of electrostatic units in the electromagnetic unit of electricity," *Philosophical Transactions of the Royal Society*, clxxiv (1883), 707-21.

17 Rayleigh (1942), p. 18

18 Rayleigh (1942), p. 18

19 J.J. Thomson and Searle, "A determination of 'v', the Ratio of the Electromagnetic Unit of Electricity to the Electrostatic Unit", Phil. Trans., 181 (1890): 583-621.

20 Warwick, p. 333

21 Warwick, p. 334

22 Thomson (1880) On Maxwell's theory of light, p. 284.

23 Warwick, p. 337.

24 Warwick, p. 337.

25 While Crookes stuck to the corpuscular theory of cathode rays, Goldstein soon moved to an undulatory explanation. See Darrigol (2000), pp. 274-287.
26 Thomson (1881) On the Electric and Magnetic… p. 229.
27 Warwick, p. 339.
28 Thomson (1881) On the Electric and Magnetic… p. 230.
29 Recollections, p. 93
30 See Jammer 1961, chap 11; McCormmach (1971); Darrigol (2000), chap. 8
31 Jammer, 1961, The Electromagnetic Concept of Mass, p. 137. Chapter 11 of the book is devoted to the electromagnetic theory of mass.
32 Thomson (1881) On the Electric and Magnetic… p. 234.
33 Thomson (1881) On the Electric and Magnetic… p. 237.
34 Thomson (1881) On the Electric and Magnetic… p. 237.
35 See H. Kragh, "The vortex atom: a Victorian theory of everything", *Centaurus*, xliv (2002), 32-126.

36 (Topper 1980, 41) See also Klein (1973) and Kragh (2002).

37 (Thomson 1867, 15)
38 (Thomson 1867, 15-16)
39 Smith and Wise (1989), chap. 12.

40 (Kragh, 2002, 46)
41 (Vortes Rings 1883a, 1)
42 Ibid., #1.

43 Thomson, *Recollections* (ref. 13), 94.

44 J.J. Thomson, *On vortex rings* (Cambridge, 1883), #51.

45 Ibid., #54.

46 J.J. Thomson, "On a theory of the electric discharge in gases", *Philosophical Magazine* xv (1883), 423-34.

47 Darwin to Thomson, 25 January 1883, Cambridge University Library Manuscripts (CUL), Add. 7654, D4.

48 FitzGerald to Thomson, 1 January 1885, CUL, Add. 7654, F15.

49 See Warwick, *Masters* (ref. 9), 264-72, and S. Schaffer, "Late Victorian metrology and its instrumentation", in R. Bud and S.E. Cozzens, eds., *Invisible connections. Instruments, institutions and science* (Washington, 1992), 23-56.

50 Gooday, "Precision measurement…" (ref. 21). Between 1855 and 1872, the following physics laboratories were started in academic institutions: University of Glasgow (1855), University College London (1866), University of Oxford (1866), University of Edinburgh (1868), King's College London (1868), Owens College, Manchester (1870) and Royal School of Mines, London (1872). For an early history of the Cavendish Laboratory see J.J. Thomson et al., eds., *J.J. Thomson and the Cavendish laboratory; a history of the Cavendish laboratory, 1871-1910* (London, 1910); E. Larsen, *The Cavendish*

laboratory, nursery of genius (London, 1962); G.P. Thomson, *J.J. Thomson and the Cavendish laboratory in his day* (London, 1964); J.G. Crowther, *The Cavendish laboratory 1874-1974* (London, 1974); Kim, *Leadership* (ref. 4).

51 See Schaffer, "Late Victorian metrology…" (ref. 39)

52 Thomson, *Recollections* (ref. 13), 98.

53 Roscoe to Schuster, 25 December 1884, Royal Society Archives (RSA), AS/B/163.

54 Roscoe to Schuster, 23, December 1884, RSA, AS/B/162.

55 *Cambridge University Reporter* (CUR), xv (1885), 324.

56 See Warwick, *Masters* (ref. 9), 343.

57 Thomson to Schuster, 1 March 1885, RSA, AS/C/331.

58 In the period 1874-1884, there had been no room in the Cavendish for chemical work, nor was there any need for the Cavendish to deal with the Chemistry Department.

59 See Schaffer, "Late Victorian metrology…" (ref. 39).

60 Kim, in *Leadership* (ref. 4) argues in this way.

61 See Sinclair, "J.J. Thomson and the chemical atom…" (ref. 6), 97.

62 Thomson to Threlfall, 7 Aug. 1886, CUL, Add. 7654, T19: "Then, on Liveing's recommendation I tried acid but this nearly all disappeared when the tube was heated".

63 J.C. Maxwell, *A treatise on electricity and magnetism, vol. 1*, 3rd ed., (Oxford, 1891), 61.

64 To understand this project, Falconer studied the interpretative techniques that were used to account for his experiments. See I. Falconer, "J.J. Thomson and 'Cavendish physics'", in F. James ed., *The development of the laboratory: essays on the place of experiment in industrial civilization*, (London, 1989), 104-17.

65 Maxwell, *A treatise* (ref. 58), 374.

66 J.C. Maxwell, Manuscript for the 9th edition of the Encyclopaedia Britannica, in P.M. Harman, The Scientific Letters and Papers of James Clerk Maxwell, vol. 2 (Cambridge, 1995), 777.

67 Ibid., 782.

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