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Ludvig Lorenz

A Nineteenth-Century Theoretical Physicist

By Helge Kragh

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Correspondence
Manuscripts are to be sent to

The Editor
Det Kongelige Danske Videnskabernes Selskab
H. C. Andersens Boulevard 35
DK-1553 Copenhagen V, Denmark.
Tel: +45 33 43 53 00
E-mail: kdvs@royalacademy.dk.
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Abstract

Ludvig Valentin Lorenz (1829-1891) was Denmark's first theoretical physicist of international recognition. Although generally considered a secondary figure in the history of science, he contributed importantly to a wide range of subjects ranging from materials science to fundamental theories of optics and electrodynamics. Apart from his theoretical work he was also a brilliant experimenter who felt as much at home in his laboratory as behind his study desk. Today his name is eponymously associated with terms such as the Lorenz gauge, the Lorenz-Lorentz formula, the Lorenz-Mie theory and the Lorenz number. In this biography Lorenz's life and science is described comprehensively and contextually, in part relying on Danish archival sources and other unpublished material. The focus of the book is on his contributions to physics, and to a lesser extent mathematics, which are discussed with consideration to how they were received in his own country and by the international community of physicists. In addition to a detailed discussion of Lorenz's scientific work the book also evaluates his position in nineteenth-century physics generally and his relations to much better-known physicists such as G. R. Kirchhoff, J. C. Maxwell, B. Riemann and H. A. Lorentz. Moreover, it includes translations into English of published and unpublished sources which until now have existed in Danish only.

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Preface

> The nice thing about writing a biography is that it forces the historian to focus on his subject and does not permit him to wander in the social swamp. Thus relevance is rather precisely defined for him; it is everything that can be discovered that impinges upon his subject.
>
> Williams (1991), p. 204.

In a book of 2016 published by the Royal Danish Academy of Sciences and Letters I presented the life and work of the Danish chemist Julius Thomsen in the form of a comprehensive biography. The present volume is in the same genre and has more than a little similarity with the earlier one. Ludvig Valentin Lorenz, the subject of this biographical study, was another Danish nineteenth-century scientist of eminence, a contemporary of Thomsen but a physicist rather than a chemist. Although the two scientists interacted in various ways they worked in different fields and also in other respects, such as regard their personalities and their career patterns, their lives were quite different. They had in common, though, that they came from humble backgrounds and ended as highly respected members of the Danish scientific community. Moreover, today neither of them is well known and in international history of science they are considered somewhat peripheral figures.

Lorenz was a mathematically inclined theoretical physicist, the first of his kind in Denmark. Internationally oriented, he made important contributions to optical theory, electromagnetism and the scattering of light on small spherical bodies. Apart from his theoretical work he also made precision experiments on optical refractivity and determined the specific thermal and electrical resistance of metals. To the extent that he was known internationally at the time of his death in 1891, it was primarily due to a clever experimental method he devised to measure the absolute value of the ohm resistance unit. Today, many physicists will consider this a relatively unimportant or even dull work and instead recall Lorenz for his theoretical innovations in electrodynamics. He pioneered the use of retarded potentials in electromagnetism and the so-called "Lorenz gauge condition" known to all physicists is named after him.

It is hard to distinguish Lorenz's life from his science as he, to a large extent, identified himself with his work in physics and mathematics. But of course he also had a life outside his study desk and laboratory. This personal part of his life is covered in the biography so far as extant sources allow it, which unfortunately means that there is not very much to tell. Lorenz was a private and unassuming person who apparently cared little about social circles, human relationships and professional recognition. Only from his diary notes written while a young man do we get a glimpse into his personal thoughts, and then a rather surprising glimpse. Contrary to Thomsen he was not very ambitious and largely stayed out of the public limelight. Despite his scientific brilliance Lorenz spent most of his career as a physics teacher at the Royal Military High School and he never made it to a position in the Danish academic world, meaning the University of Copenhagen or the Polytechnic College. He never became a professor and never wrote a doctoral dissertation.

My occupation and to some degree fascination with Lorenz is not of new date. In part inspired by my former teacher, the physics professor Mogens Pihl, I wrote many years ago a couple of papers on Lorenz's scientific work, one on his contributions to optics and another on his unpublished theory of the transmission of telephone currents. I also spent much time collecting and cataloguing as many sources as possible, the result of my efforts being a guide to the material written in Danish, cp. http://milne.ruc.dk/imfufatekster/pdf/209.pdf. However, at the time nothing came out of my intention of writing a full biography. As far as the archival material is concerned, regrettably a large part of what existed in the 1930s seems to have disappeared or, at least, cannot be localised any longer. To mention but one example, an article by Kirstine Meyer from 1938 refers to letters exchanged between Lorenz and his wife in 1882, when Lorenz stayed in Paris. These letters would be of great interest, but unfortunately they no longer exist. A conservative estimate is that about one third of Lorenz's letters, notes and manuscripts are no longer extant. Most of what does exist is located in three different deposits, namely the archive of the Royal Danish Academy of Sciences and Letters (RAS), the Niels Bohr Archive (NBA), and the archive of the Danish Museum of Science and Technology (DTM; Danmarks Tekniske Museum). In addition there are a few sources from foreign archives and universities.

The focus of the book is unavoidably on Lorenz's science rather than his life. After all, in this case the first is more interesting than the latter (which was fairly uneventful) and the story of his science is also better documented than the story of his life. Nonetheless, in the first chapter I cover Lorenz's life and career contextually and as detailed as possibly, starting with his family background and ending with the publication of his collected works about a decade after his death. While a young man Lorenz kept a diary which he filled with reflections of a philosophical and speculative nature so different in style and substance from his scientific work in later life. It is a fascinating source and I quote extensively from it.

Chapters 2 and 3 deal with Lorenz's important works on optics and electricity, respectively, the two areas of science to which Lorenz made lasting contributions of international significance and on which his scientific reputation rests. The chapters cover topics such as the phenomenological wave equation of light, the Lorenz-Lorentz formula relating transparent bodies' refractivity and density, the electrical theory of light propagation, the Wiedemann-Franz-Lorenz law of the ratio of electrical to thermal conductivity, and the determination of the unit of resistance in absolute measure. As seen from a modern perspective, his scattering theory of light on small spheres, or what is today widely known as the "Lorenz-Mie theory," is arguably the most important of Lorenz's optical works.

While the mentioned topics all belong to so-called pure science, in Chapter 4 the focus shifts to Lorenz's less well-known contributions to applied physics and technology. These contributions, which largely concerned the design of a practical dynamo and a failed attempt to construct a telephone cable for long-distance communication, picture Lorenz as much more than just an ivory tower physicist. Mathematical physicist as he was, on occasions he applied his skills to problems of a very practical and potentially economically rewarding nature. In this respect he was not exceptional, for in the late nineteenth century there was a growing interest in applying scientific theory to technological innovations.

Although Lorenz's scientific legacy rests predominantly on his contributions to optics and electrodynamics, he also applied his talents to other branches of the physical sciences and to areas of pure and applied mathematics. As described in Chapter 5, on one occasion he took up a problem of geophysics but characteristically he published his investigation in a local mathematics journal and hence it went unnoticed among Danish geologists. The chapter also deals with Lorenz's contributions to thermal and molecular physics and considers his ambivalent attitude to the kinetic theory of gases. Although not a trained mathematician Lorenz was a master of mathematical manipulations and, in addition, he dealt seriously with problems in number theory and other branches of pure mathematics. However, his work in this area was not much appreciated by Danish professional mathematicians.

Lorenz was basically uninterested in social and political issues and yet at a few occasions he wrote on subjects which may be classified as political science or were at least related to politics. These writings were not very important, but they are of some value by adding an extra dimension to the portrait of the Danish physicist. At the end of Chapter 5 follows a brief summary of Lorenz's position in nineteenth-century physics from a national and international perspective. His extensive works in optics and electricity were well known internationally and yet they exerted curiously little impact on mainstream physics in the period. Why was he effectively forgotten at the end of the century? What is or should be his proper status in the history of science? The final Chapter 6 is an appendix containing Lorenz's time-line and translated transcriptions of two unpublished sources which offer some insight in his life and in his general view of the methods and aims of science. The chapter also contains a translation of a Danish paper on the electrical theory of light which Lorenz wrote in 1867.

Ludvig Lorenz had a predilection for difficult problems which could be handled by means of complex mathematical analysis and clever approximation formulae. His publications are generally difficult to read and even more difficult to understand, not only because of the heavy doses of mathematics but also because the reasoning is at places obscure. It goes without saying that his biography cannot eschew mathematical formulae, for then there would be little left. On the other hand, I have tried to present Lorenz's ideas by paying more attention to their physical content than to their mathematical formulations. Technical anachronisms cannot be completely avoided but I have kept them to a minimum.

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Helge Kragh

CHAPTER I

Life: The troubled career of a physicist

According to the Danish physicist and historian of physics Mogens Pihl, "Along with H. C. Ørsted, L. V. Lorenz (1829-91) was without any doubt the greatest Danish physicist of the nineteenth century. " Moreover, "Without exaggerating, even from an international perspective he belonged among the greatest physicists of the century."[1] The American physicist Philip Wyatt, a specialist in laser optics, agreed, calling Lorenz "a brilliant Danish physicist whose skills and achievements in both experimental and theoretical physics distinguish him as one of the greatest scientists of the 19th century."[2] Despite this high evaluation, Lorenz is little known in his own country and even less so internationally, where his name, when it occurs in physics contexts, is often thought to be a misspelling of the surname of the eminent Dutch theoretical physicist H. A. Lorentz.

All the same, Lorenz is still today eponymously associated with four different areas of physics (see also Table 2.2). His name is known from the "Lorenz gauge" in electrodynamics which goes back to an electrical theory of light he presented in 1867. Two years later he established the relationship between the refractive index of a transparent substance and its density known as the "Lorenz-Lorentz law." In 1881 he extended the Wiedemann-Franz law on the ratio of a metal's electrical conductivity and its thermal conductivity to include also the temperature dependence. The names "Wiedemann-Franz-Lorenz law" and "Lorenz number" derive from this work. Finally, in his last contribution to physics, dating from 1890, Lorenz developed on a non-electromagnetic basis a comprehensive theory of the scattering of light by a sphere. The theory was largely equivalent to the celebrated theory of Gustav Mie dating from 1908. Hence the term "Lorenz-Mie theory" entered the physicists' vocabulary. These and other contributions will be covered in later chapters.

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1. Pihl (1983), p. 380 and p. 383.
2. Wyatt (1974).

In this chapter we look at Lorenz's life and career from his birth in 1829 to about a decade after his death when his collected scientific papers were published in a French translation sponsored by the Carlsberg Foundation. Lorenz was one of many Danish scientists who did not graduate from the University of Copenhagen but from the city's Polytechnic College. While this was far from unusual, it was most unusual that a leading physicist graduated from the College's class of chemical engineering and not from its so-called mechanical class. Although appreciated by his contemporaries as a gifted mathematician and physicist, Lorenz never obtained a position at either the University or the Polytechnic College. He spent the major part of his active life in the more humble position as physics teacher at the Royal Military High School. From 1887 to his early death four years later, he lived as an independent researcher generously supported by the Carlsberg Foundation.

1.1 Youth and early education

The physicist Ludvig Valentin Lorenz was indeed Danish, but with family roots in Germany and France.[3] His maternal grandfather Carl Gustav Scherfin was born on the island of Rügen in Northern Germany from where he went to Elsinore (Helsingør) in Denmark to establish himself as a baker. The Scherfin family was of French origin, descending from the Huguenots which in the late seventeenth century were persecuted because of their Protestant religion and forced to leave France in large numbers. The surname Scherfin was a German version of the original French name Chervin.

Ludvig Lorenz's father was another German émigré and, coincidentally, from the same area of Germany. Johann Gottfried Lorenz (1795-1849) was born in Stralsund at the Baltic coast, just a short distance from Rügen. Trained in the trading business he went to Denmark, at the time a country with only 1.1 million inhabitants

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[3] What little information we have about Lorenz's early and personal life, apart from the extant archival material, rely on the obituaries written by C. Christiansen (1891a; 1896) and K. Meyer (1938). Christiansen (1891a) is anonymous but there is no question about its authorship.

disregarding the duchies. J. G. Lorenz ended up in Elsinore where he became a partner in Scherfin's bakery and later its owner. He was also a respected lieutenant in the city's civic artillery defence. More importantly, in about 1820 he married the baker's daughter Charlotte Christine Scherfin (1796-1885). The marriage resulted in seven children, one of which was Ludvig Valentin who was born on 18 January 1829 and baptized in St. Olai Church on 24 April the same year.

Six years later and for unknown reasons the Lorenz family moved southwards to Maribo on the island of Lolland, where Lorenz senior established himself as a grocer. From a census of 1840 we know that the family, apart from the father and the mother, consisted of six children, three girls and three boys: Sophie Dorthea Christine (*1820), Gottfried Alexander (*1823), Caroline Amalie (*1825), Edvard Gustav (*1826), Charlotte Amalie (*1828), and Ludvig Valentin (*1829).[4] Very little is known about the Lorenz family and almost nothing about Ludvig's relations to his siblings. The oldest of them, Sophie Dorthea, was married in 1845 to a physician practicing in Maribo. Edvard Gustav (1826-1867), Ludvig's older brother by three years, took his student's exam from Nykøbing Cathedral School in 1845 and later became a medical doctor; in 1863 he married Charlotte S. J. Horneman, a daughter of the popular Danish composer J. O. Emil Horneman.[5]

Ludvig spent most of his childhood and adolescence on Lolland and the neighbouring island of Falster, at that time (and still today, to some extent) sparsely populated and relatively undeveloped parts of the kingdom. Instead of joining one of the state schools, until the age of 14 he was taught at a private school in Maribo. A neatly written notebook of 1842 gives a fascinating insight in what Ludvig mastered at the time (Fig. 1.1). Not only was he acquainted with Latin and Greek, he was also well versed in geography, history

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[4] Frederikke Amalie Lorenz, born in Helsingør 1833, is not included in the census, probably because she did not survive her first years.

[5] Carøe (1904). Emil Horneman (1809-1870) was highly appreciated in Danish national and cultural circles and the most popular composer of his time.

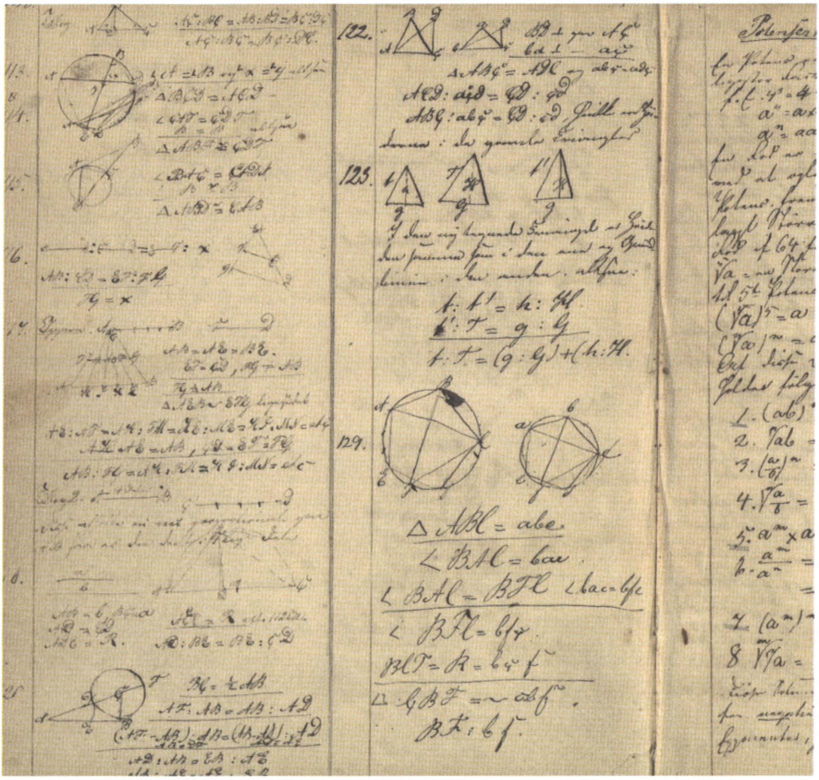

Figure 1.1: Lorenz's first meeting with mathematics. Notebook from 1842, shortly before he entered Nykøbing Cathedral School. Lorenz Papers (DTM).

and mathematics.[6] Some of the elementary education was undertaken by his father who had an interest in philosophical, mathematical and mechanical subjects. But Ludvig also read widely by himself, including fairly advanced books intended for the middle education such as Georg Ursin's textbook in mathematics and Hans Bjørn's textbook in geometry.[7]

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[6] The notebook includes long passages from classical authors such as Homer, Horace and Virgil, some of them in Greek and Latin (Lorenz Papers, DTM).

[7] According to Christiansen (1891a). The two books were Hans Outzen Bjørn, *Lærebog i Geometrien* (1820) and Georg F. K. Ursin, *Lærebog i den Rene Mathematik* (1824). For

As Lorenz told in an autobiographical sketch of 1877, latest at the age of 13 he had committed himself to the study of mathematics and physics:

> Very early on I showed an interest in calculations and mathematics in which fields my father gave me the first guidance; I studied them myself, not so much by reading textbooks as by thinking about mathematical problems which I found in books or posed to myself. Moreover, an evening lecture in physics aroused my interest in this field at an early date (I was twelve years old) and I soon realised that I wanted to make the study of mathematics and physics my calling. This goal was constantly in my mind.[8]

The evening lecture that exerted such an impact on Ludvig's receptive mind was part of a lecture series sponsored by the Danish Society for the Dissemination of Natural Science (Selskabet for Naturlærens Udbredelse), an important organisation founded by H. C. Ørsted in 1824. On the request of Maribo city council, Ørsted asked one of his former students at the Polytechnic College, Carl Carlsen, to give public lectures in Maribo. Ludvig Lorenz, most likely the youngest in the audience, was not the only one who found them to be interesting, for the lectures in the winter term 1841-1842 were so successful that they had to be moved to the city's town hall.[9]

Although preoccupied with mathematics and now also physics, Ludvig was also exposed to philosophical, historical and religious ideas. His youthful interest in these very different areas, or at least his knowledge of them, was indebted to discussions in his home among a small group of people. According to the physicist Christian Christiansen, who had many conversations with Lorenz in his later life, the group included his maternal grandfather, "which he

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a critical review of the state of Danish mathematics at the time and the textbooks used at the gymnasium schools, see Steen (1873).

[8] Lorenz, autobiographical sketch of 1877, draft version in Lorenz Papers (DTM). See Appendix A for the complete translation. I have used the transcription provided by Kirstine Meyer (Lorenz Papers, NBA).

[9] Harding (1924), p. 80 and p. 86. Carlsen (1813-1899) graduated as a mechanical engineer in 1839 and subsequently specialised in hydraulic engineering and the constructions of harbours and canals, in which areas he was a Danish pioneer.

speaks about as a wonderful and highly spirited man, with much knowledge in history, for which reason he, in Lolland, became a confidant and companion to the learned and loveable bishop Jens Møller."[10]

In 1843 Ludvig was sent to Nykøbing Cathedral School, a gymnasium or "sixth form" some twenty miles east of Maribo, to undergo the three-year preliminary higher education that was required for entering academic studies. Founded in 1646, the school was one of the oldest in the country and like most other gymnasium schools its teaching focused on classical languages and Latin in particular. There had earlier in the century been elementary courses in physics and other sciences at the school, but when Lorenz enrolled, physics and chemistry had disappeared and the fields of science were represented only by natural history meaning botany and zoology. The school owned a small library including some textbooks in physics, but these were all from before 1810 and hence quite outdated. They were hardly of much interest to students with a thirst of knowledge such as Ludvig from Maribo.[11]

Lorenz (as I shall call him from hereon) seems not to have liked or benefitted much from his years in Nykøbing. He learned Latin - which he never used except at one occasion- and spent much of his time studying mathematics and related subjects on his own. He had by then developed a firm taste for independent studies which would remain with him throughout his life. However, during his gymnasium education at the cathedral school Lorenz forced himself to give his mathematical interests a lower priority. As a counterweight he engaged in studies of a very different kind, such as aesthetics, philosophy and religion. "I deliberately shelved my mathematical speculations," he recalled in his notes of 1877. "This was in part because I wanted to complete my education at the learned school as quickly as possible, and in part because I felt that my health deteriorated due to my too eager occupation with mathematical problems."

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[10] Christiansen (1891a), p. 1. However, there was no bishop with the name Jens Møller at the time. It was probably Rasmus Møller (1763-1842), bishop of Lolland-Falster diocese since 1831, with whom the Lorenz family was acquainted.

[11] Riis Larsen (1991), pp. 141-143.

Lorenz's decision to switch from mathematical to philosophical reflections, or to supplement the former with the latter, was not only meant as therapy. Apparently the emotional teenager was genuinely interested in existentialist questions and for several years philosophy and religion seem to have challenged mathematics with regard to priority in his mind. In the period from 1845 to about 1854 he wrote a number of small and partly introspective essays on philosophical questions and filled his diary with reflections of this kind. Remarkably, nowhere in the diary is there any mention of the tumultuous political and military events which so upset the nation during the years 1848-1851. Nor do scientific questions related to theoretical physics and mathematics appear to any significant extent.

It is fascinating to follow Lorenz's streams of thought during his later years of study at the Polytechnic College, where he obviously was occupied with much else than would be expected from a diligent engineering student. Thus, in a note of 18 May 1850 he reflects on philosophical questions in a manner which is highly speculative and nebulous, much closer to the spirit of Friedrich Schelling's *Naturphilosophie* than to the period's positivist science. Here is an example:

> Space turns into body by uniting with its own opposite. The forces are spiritual and they result in the individualisation of space. The bodies become organic bodies by this individualisation of unification which is growing in the external world. It is now separated from the individual; it influences it and forces thereby an action in God which again goes outwards from the inner. From the external actions going inwards one can reason and deduce the concept of God: the recognition of the infinite spirit in the infinite corporeal but not necessarily the personal spirit. With the conscious individual follows the separation of soul and body; however, the immortality of soul does not follow as a necessary inference.[12]

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[12] Lorenz notebook, translated from Meyer's transcription (NBA). Of course, the notes were only meant for Lorenz's own consumption.

Among the titles of Lorenz's diary essays were "On Faith," "On Death," "How Do We Comprehend the Infinite?" and "The Difference between Organic and Inorganic Beings." He also contemplated how women's mental capacities compared to those of men. Some of Lorenz's speculations were clearly inspired by Søren Kierkegaard, the great existentialist philosopher and theological thinker who died in 1855. Relying heavily on Kierkegaard's *Philosophical Fragments* (Philosophiske Smuler) from 1844, Lorenz concludes in the first of the mentioned essays dated 27 October 1850, that neither experience, nor reason nor emotion is sufficient to justify belief in Jesus Christ. "In this sense," he writes, "I admit that I have no faith."

Lorenz also dealt with subjects of natural philosophy which he wrote about in a style that reflected the romantic philosophy of nature which was still alive in Denmark but rejected elsewhere in Europe. A longer essay on "The Nature of Heat" from 1849 is of some interest because it shows the inspiration from Ørsted's philosophy of nature as published in two volumes in *Aanden i Naturen* (The Soul in Nature) from 1849-1850. The essay is also of interest in the light of Lorenz's later scientific work on heat and thermal physics (sections 3.3 and 5.2). But at the time he was searching in the dark, mixing philosophical ideas with Cauchy's theory of heat as ether waves and Carnot's theory of heat as a conserved quantity. He had not yet converted to the law of energy conservation or perhaps he had not yet understood the law properly.

Another indication of Lorenz's occupation with Ørsted's philosophy of nature appears in a note related to Ørsted's argument that the laws of nature are immutable "laws of reason" and that superstition means a denial of the universal and necessary effects of the laws.[13] Lorenz objected in his diary that a superstitious person might well accept the laws of nature but postulate that these are deeper or more comprehensive than those known presently. There may be hitherto unknown laws that govern the so-called supernatural events. Only if it is claimed that these deeper laws have no observable effects in the ordinary sense, can one speak of superstition. The question considered by Lorenz was not only the fundamental

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[13] Ørsted (1852), pp. 56-61.

distinction between science and non-science but also about the foundation of scientific knowledge. Is there an ultimate law of science or will we always be able to dig deeper into nature's secrets? These are still questions without an answer and with which physicists and philosophers occupy themselves.

Despite Lorenz's sincere wish to broaden his intellectual interests, in the long run he was unable to escape what he felt was the magical attraction of mathematics and physics. After much soul-searching he reached the conclusion that it was an either-or question in the sense of Kierkegaard. He decided to concentrate fully on mathematics and theoretical physics even though this implied a kind of one-sidedness he had otherwise sought to resist. He convinced himself however, that one-sidedness was not so bad after all, indeed that it was necessary as well as acceptable. In a diary essay from 1852 titled "One-Sidedness" he wrote: "When I thus will turn to the one-sided, let me do it with so much power that the delusion becomes as insignificant as possible. Even better, if one-sidedness takes possession of me and I am unable to escape it, then: It is preferable if the necessities appear to me so forceful and real that they tie down my inner thought and fear that this or that is perhaps one-sided."[14] From this time onward Lorenz lived for his chosen fields of scientific study.

I.2 Polytechnic studies

Having graduated from Nykøbing Cathedral School, Lorenz went to Copenhagen, the country's capital and the only centre for higher academic education. Denmark possessed at the time two universities of which the old university in Copenhagen was by far the most important. The other university was located in Kiel in Holstein, but it was small and not very attractive to students outside the Schleswig-Holstein region. For Lorenz the natural choice was Copenhagen which not only housed the old university but also the new Polytechnic College (which is now the Danish Technical University). Compared to Maribo and Nykøbing, the royal city was a metropole at

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[14] Quoted in Meyer (1938), p. 465.

least in a Scandinavian context. When he arrived in Copenhagen in 1846 the population numbered about 125.000 inhabitants and that number was rapidly growing. By Lorenz's death in 1891 it had tripled. In the mid-nineteenth century scientific research and education was not highly rated in an academic environment dominated by theology, law, medicine and classical languages. Nonetheless, the status of the sciences was slowly improving.[15]

Lorenz's years of study coincided with the three-year's war (1848-1851) also known as the first Danish-Prussian war in which Danish troops fought against German-supported rebels from the duchies in the southernmost parts of Denmark. One would expect that Lorenz, a man in his prime years, was drafted for the army or otherwise did military service. If he did, which is quite possible, we know nothing of it except that it can be safely assumed that he was not actively involved in combat.

In part as a consequence of the war and the pressing military and political needs, scientific and educational reforms were halted. At the University, the sciences were still parts of either the philosophical or the medical faculty and there was only one professor in each of the scientific fields. Not least due to the persistent efforts of Ørsted, in 1850 the Faculty of Philosophy was split into two, one of them being a new Faculty of Science which also comprised mathematics. At its establishment the faculty consisted of seven full professorial chairs, namely physics (H. C. Ørsted), chemistry (E. A. Scharling), geology (J. G. Forchhammer), mathematics (C. Ramus), astronomy (C. F. R. Olufsen), botany (J. F. Schouw) and zoology (J. Steenstrup). Several of the professors also held positions at the Polytechnic College founded in 1829 with Ørsted as its founder and director. Thus, in 1846 Ørsted taught physics at the College, Forchhammer taught chemistry and Ramus mathematics.

"I passed the so-called 'second exam' at the University and sub-

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[15] For a summary account of science in Denmark in the mid-nineteenth century, see Kragh (2016), pp. 25-38. A broader and more comprehensive history is provided in Kragh et al. (2008). The development of physics is briefly described in Pais (1991), pp. 92-97, who notes that Lorenz was "the first Danish theoretical physicist of prominence." Probably following Pihl (1972b), Pais misspells Lorenz's first name "Ludwig."

sequently I studied at the Polytechnic College, where I chose the chemical class, believing that I would benefit in particular by practical work in the laboratory."[16] This is how Lorenz some thirty years later recalled his academic studies. After matriculation to the University he passed in the winter semester 1847 the second exam also known as the *examen philosophicum*, a propaedeutic exam required for further studies. It was divided into two exams, one comprising Latin, Greek, Hebrew, history and mathematics, and another philosophy, physics and astronomy.[17] Well prepared from the school in Nykøbing, Lorenz passed the exams with the highest grade (*laudabilis*). However, his encounter with the University was brief as he decided to continue his studies at the Polytechnic College and not at the University. At the time the Polytechnic College comprised two classes of candidates, one specialising in "mechanics" and the other in "applied science." The latter term mainly referred to technical chemistry or, slightly anachronistically, chemical engineering. The mechanical class on the other hand focused on mathematics, mechanical physics and machine construction. In none of them did the classical languages play any role at all.

At the time when Lorenz studied at the Polytechnic College, a complete course for engineering students was scheduled for three years. However, the average of the actual study time was close to five years. Table 1.1 gives the distribution of weekly lectures for the semesters of the chemical class in about 1850. These were the lectures that Lorenz was offered, if not necessarily those he attended. From his left notebooks we know that in 1848-1849 he followed Ørsted's lectures on chemical physics, Ramus' on analytical geometry, Scharling's on organic technical chemistry, Julius Wilkens' on technology, and Forchhammer's on geology and crystallography. Other of his detailed notes from this period deal with subjects he apparently followed at the University, namely the zoology lectures of Japetus Steenstrup and the lectures in pathology of Ole Bang, a professor of medicine. Lorenz's interests were wide.

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[16] Lorenz, autobiographical sketch. See Appendix A.

[17] According to Christiansen (1896), Lorenz chose Hebrew among his subjects because he briefly contemplated to study theology.

Semester	*1*	*2*	*3*	*4*	*5*	*6*	*total*
Mathematics	4	4	4	4	-	-	16
Chemistry	5	10	5	-	6	-	26
Physics	10	4	5	-	-	-	19
Technology	-	-	-	10	9	-	10
Geology	2	2	2	-	-	-	6
Natural history	-	-	4	4	-	-	8

Table 1.1. Weekly lectures at the Polytechnic College, the chemical class. Chemistry comprised lectures in general, organic, inorganic, and technical chemistry; physics included chemical physics, optics and physics of the Earth. The sixth semester was devoted to laboratory experiments and machine designs, without any lectures. The table is based on data in Steen (1879).

At first Lorenz enrolled as a student in the mechanical class, where the first semester included 11 weekly lectures in mathematics, 10 in physics and 5 in chemistry. He attended Christian Ramus's lectures on calculus and rational mechanics but intensely disliked the professor's presentation of the subjects. This was one reason, if not the only one, why he decided to switch from the mechanical to the chemical class.[18] Ramus was an accomplished mathematician, professor at the University and the Polytechnic College and since 1831 he had taught mathematics at the latter institution.

It is uncertain and to some extent surprising why (and if) Lorenz disliked Ramus's lectures on rational mechanics. In a textbook on "analytical mechanics" published a few years later Ramus presented a modern and comprehensive exposition of the subject, stressing the crucial role of mathematical reasoning in mechanics and theoretical physics generally. The message was clearly stated in the preface:

> It is only by its appearance as a mathematical science that mechanics, as well as the other sciences, has reached perfection in form and become based on evidence in a definite manner; and this is the necessary condition for any certain prediction of phenomena such as governed

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[18] Christiansen (1891a) and (1896), who most likely had it from Lorenz's mouth.

> by the given forces. ... What forever will be hidden from experience and spiritual recognition becomes recognisable by means of mathematical analysis in so far that it follows with necessity from previously known laws.[19]

Ramus's view of theoretical physics and the role of mathematical analysis may not have been shared by Lorenz in the late 1840s, but it agreed perfectly with the philosophy that he settled upon a few years later.

Also students of the chemical class had a course in mathematics, albeit more elementary and limited than the one taught by Ramus. Since 1849 the chemical mathematics course was taught by Adolph Steen, who was later appointed university professor and became a central figure in the Danish mathematical community. The course was initially, in the years 1847 and 1848, taught by Carl Valentin Holten, a 30-year-old physicist and Ørsted protégé with only a limited knowledge of mathematics.

Lorenz must have followed at least some of Steen's lectures and possibly also some of Holten's, especially after Holten in 1850 was appointed lecturer in "mathematical physics." Two years later Holten was appointed Ørsted's successor as ordinary professor at the University. In the physics course Lorenz almost certainly encountered Ørsted's *Naturlærens Mechaniske Deel* (The Mechanical Part of Science) from 1844, a work which the famous physicist and natural philosopher primarily wrote as a textbook for the polytechnic chemistry class.

At the time Ørsted's textbook and more generally his view of mathematics and its role in physics gave rise to a series of minor controversies which cannot have escaped Lorenz's attention.[20] The mentioned textbook was elementary, semi-popular, unsystematic and without quantitative details. It almost completely avoided

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[19] Ramus (1852), p. iv. The reference to "spiritual recognition" was probably a critical allusion to Ørsted's philosophy of nature.

[20] See Ørsted (1920), vol. 3, pp. clv-clxii, Pedersen (1988), and Christensen (2013), pp. 600-607 for the controversies relating to Ørsted's teaching and his view on mathematics.

mathematics except of the most elementary kind. Nor was it exactly modern from a physics point of view - it did not refer to Newton's laws of motion, for example. The campaign against Ørsted, as it unfolded in pamphlets and newspapers such as *Kjøbenhavnerposten* (Copenhagen Post) and *Fædrelandet* (The Fatherland), had many roots and one of them was dissatisfaction with Ørsted's lack of appreciation of the mathematical sciences.

The powerful Ørsted had been able to install his assistant Holten in the position of mathematics teacher, which caused more than a few raised eyebrows among the professional mathematicians. Steen, who had applied for the position in 1847 but was rejected in favour of Holten, sharply attacked Ørsted and what he claimed was the "anti-mathematical" board of directors at the Polytechnic College. His target was the misery of mathematics teaching at the Polytechnic College and more specifically the mathematically incompetent Holten whom he attacked personally. "Mathematical physics figures as a discipline in the curriculum of the institution," Steen pointed out. "At any examination students get marks in it, and yet it has to be said that mathematical physics is not taught at the Polytechnic College." And with regard to Ørsted's textbook in physics: "[It] cannot stand the critique of a mathematician. ... All those concepts which physics should provide become unclear and uncertain due to the lack of mathematical precision and accurate expression."[21]

Ørsted chose not to reply, but in a long letter in *Fædrelandet* of 1847 he made it clear that his view of mathematics in the physical sciences was totally at odds with the one favoured by Steen:

> There are such mathematicians who think that physics should only be treated in a mathematical way. On the other hand, I have myself tried to maintain throughout my scientific career, and the more the further I advanced in it, a treatment based on the very nature of physics in which mathematics yields as far as possible to an experimental treatment. Apart from this I have always stated how important it is for physics that its truths are also presented in a mathematical form, and I encourage those in the audience who wish to go further to follow

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[21] Steen (1847) pp. 15-16. Steen's critical attitude was shared by Ramus, whose textbook of 1852 was implicitly a rejection of the views held by Ørsted and Holten.

Figure 1.2: Carl Valentin Holten (1818-1886), professor of physics in the post-Ørsted era 1852-1886. Royal Library, Copenhagen, Picture Collection.

> courses in mathematics. But I cannot advise anyone to start in mathematics in order to become a physicist.[22]

For Ørsted, a mathematical presentation of physics would only divorce it further from natural philosophy, make it increasingly foreign to the Danish people, and greatly diminish its value as "spiritual education." His strongest argument for preferring Holten over Steen was precisely that the former was *not* a specialist in mathematics.

And how did young Lorenz, the polytechnic student from Maribo, feel about the controversy? Unfortunately there is no documentary evidence which throws light on his attitude to the dispute concerning mathematics and physics at the Polytechnic College, but it

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[22] Quoted in Pedersen (1988), p. 148.

is most unlikely that he was not preoccupied by it. After all, this was a topic about which he had strong feelings and which would colour much of his later work. On the one hand, as shown by his diary notes he was still influenced by philosophical views with a fairly strong affinity to Ørsted's natural philosophy. On the other hand, he was struggling to liberate himself from philosophical and spiritual reflections and instead to focus fully on questions of mathematics and physics. His increasing obsession with mathematics might have made him sympathetic to Steen's arguments and hostile to Ørsted's view that physics should be predominantly experimental and with only a minimum of mathematics. But this is nothing but a speculation. We do not know and it is possible, as indicated by his dissatisfaction with the mathematical lectures that he also disagreed with the anti-Ørsted views expounded by Steen and Ramus.

The critique of Ørsted was not the only one that affected the Polytechnic College in the mid-nineteenth century. There was at the time a series of political discussions concerning the aim of the College and its future, including its relation to the Royal Military High School (Den Kongelige Militære Højskole). This institution, where Lorenz would later spend a large part of his career, was founded in 1830 and its teaching subjects overlapped to a considerable degree with those of the polytechnic school. The extensive debate was primarily concerned with the aim, quality and level of the teaching. Should the polytechnic school be scientifically oriented or should it rather be aimed at the more practical needs of public services and the growing Danish business sector?[23] While these and related questions were much discussed in the early 1850s, Lorenz seems to have ignored them.

Incidentally, the level of mathematics and physics at the Military High School was generally higher than it was at the Polytechnic College during its first decades. In a massive textbook on "technical mechanics" from 1833, Johan Arndt Dyssel confronted the military students with a heavy dose of calculus and argued, much like Steen did fourteen years later, that higher mathematics was indispensable

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[23] See Kragh (2016), pp. 36-38, 97-98 for aspects of the debate.

for the true understanding of physics and mechanics.[24] As late as 1855 the Ministry of Finance declared that "the Military High School should be the institution responsible for the higher teaching of mathematics and natural sciences in the country."[25]

First of all, Lorenz was an independent mind who did not regularly attend the formal education but preferred to study by his own. This may not only have been the result of his ingrained taste for independency but also of his self-chosen isolation. According to a reliable source, young Lorenz was "morbidly shy."[26] Had he wished to, he could have joined what little social life there was around the Polytechnic College such as the Polytechnic Association (Polyteknisk Forening) founded in 1846 by a group of students and other people. It was as much a social club for students and teachers as it was a professional organization for science and engineering. Many of the polytechnic students joined the club, valuing the opportunity it gave to establish contacts and listen to general lectures, and they continued being members of the club after graduation. Lorenz was never a member.

Apart from being exposed, to some degree, to the teaching of Ørsted, Steen and Holten, in chemistry - which was after all his main subject - he must have received laboratory training from Julius Thomsen, three years his senior, who from 1846 to 1853 worked as an assistant to the professors Edvard Scharling and Georg Forchhammer. While Thomsen graduated in 1846 with the highest mark and soon embarked on a glorious career in chemistry, when Lorenz graduated in 1852 he received only a second grade. His average grade was 6.29, while Thomsen's was 7.08.[27] In short, Lorenz was not a very good student. Though a graduate in chemical engineer-

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[24] Dyssel (1833). For few years J. A. Dyssel (1798-1846) also taught technology and mechanics at the Polytechnic College. According to Nielsen (1910), p. 177, "There is no doubt that during the first twenty-five years of its existence the High School offered the best teaching in mathematics in the country."

[25] Steen (1879), p. 44.

[26] Christiansen (1891a).

[27] The grading system was based on a scale invented by Ørsted in 1833, where the top grade was 8 and the lowest - 23. For a complete list of graduates from the Polytechnic College 1829-1890 including their final grades, see Voigt (1890).

ing, he was not seriously interested in either chemistry or engineering, and he had no ambitions of using his exam for a living or for anything else. The courses were not wasted though, for they introduced Lorenz to practical laboratory work which he would benefit from later in life.

1.3 Years of uncertainty

Lorenz's studies had been financed by his parents and with his father's death in 1849 the money dried up and he had to find other means of living. One of them was to offer coaching in mathematics, which he did in 1850. Right after graduation in 1852, he may have worked briefly at the Polytechnic College, as an assistant for Holten.[28] The same year he "took on the job as private tutor in the house of Count Moltke-Hvidtfeldt," as he reported in 1877. The nobleman in question was possibly Count Adam Gottlob Moltke-Hvitfeldt (1798-1876), a high-ranking diplomat, politician and owner of the manor Glorup on the island of Fyn (Funen). For most of a year Lorenz stayed in Leipzig with some or all of the count's family. After his return in 1853 he took on occasional teaching jobs at middle and gymnasium schools in Copenhagen, where he taught physics and chemistry. For a time he taught at Efterslægtselskabets Skole (literally: The School for Future Generations) founded in 1786 by a philanthropic society and also at the private Von Westen Institute, both located in central Copenhagen. Established in 1799 the latter school was a gymnasium whose rector during the years 1844-1873 was Henrik Georg Bohr, the grandfather of the great physicist Niels Bohr.[29]

Lorenz was far from the only promising scientist who was forced to spend a period as school teacher. Among German physics and mathematics graduates it was quite common to take up temporary teaching positions at secondary schools until they could get their

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[28] According to Hansen (1932) and also to Pihl (1983), p. 380, who says that Lorenz and Holten were not on good terms, an evaluation he may have taken from Appel (1891). I have been unable to verify that Lorenz actually had a temporary position at the Polytechnic College. The position may have been unofficial.

[29] Pais (1991), pp. 34-35.

Figure 1.3: The Von Westen Institute ca. 1880 where Lorenz worked as a physics teacher. Royal Library, Copenhagen, Picture Collection.

first positions as assistant professors. Nor was this kind of career pattern foreign to Danish graduates. Thus, in both a national and an international perspective there was nothing unusual about Lorenz's time as a teacher except that for him it was not a short period during which he waited to enter an academic institution. He never got that far. It was all a matter of the size of the country and its academic traditions. While there were many universities and technical colleges in Germany, there was only one of each in Denmark.

Continuing his private studies of still more advanced topics in theoretical physics, in June 1854 Lorenz responded to a University prize competition in mathematics the subject of which was the geometrical properties of Fresnel wavefronts. On behalf of the Faculty the professors in mathematics and physics, C. Ramus and C. Holten, concluded: "The author has presented a most satisfactory treatment of the complicated question concerning the construction of the wavefront's tangent planes; it demonstrates that the author is well acquainted with the principles of science and in particular with the methods of analytical geometry which he knows how to apply successfully. For this reason we judge the author to be worthy of the prize."[30] In addition to the University's gold medal Lorenz was awarded a modest sum of money.

At about the same time Lorenz also worked privately on problems in hydrodynamics, trying to establish the equilibrium positions of rotating fluids. As a result of his gold medal he received free residence in Borchs Kollegium, a students' dormitory established 1691 and named after its founder, the Danish seventeenth-century chemist, philologist and natural philosopher Ole Borch or, in its Latin version, Olaus Borrichius. He lived there from February 1854 to September 1858.[31] Moreover, during the period 1857-1862 he was awarded a recently established private grant called the Smith Stipend which for seven years supported him with a total of 2,550 rixdollars.[32] For a time Lorenz thus was secured economically, if only

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[30] A. Linde, *Meddelelser Angaaende Kjøbenhavns Universitet 1849-1856*, vol. 2 (Copenhagen, 1857), p. 89.

[31] See Mondrup (1943).

[32] A. Linde, *Meddelelser Angaaende Kjøbenhavns Universitet og den Polytekniske Læreanstalt*, vol. 3 (Copenhagen, 1886), p. 577. The grant was based on the will of Johannes L. Smith (1789-1849), a Danish civil servant who wanted to support students and candidates "who promise in due course to achieve results beyond the ordinary." Lorenz proved a worthy recipient.

barely so, but he had no permanent position and was wholly outside the Danish academic and scientific environment.

The organisation called the Society of Scandinavian Natural Scientists (De Skandinaviske Naturforskeres Selskab) was founded in 1839, its main purpose being to establish and run a series of conventions for scientists and physicians in the Scandinavian countries and thereby further contact between them. Modelled on the national institutions in Germany and England (Gesellschaft Deutscher Naturforscher und Ärtzte, 1822; British Association for the Advancement of Science, 1831), the Scandinavian meetings became very important for the presentation and exchange of scientific communications among scientists in the Nordic countries. In the early period the meetings provided an opportunity for aspiring scientists to make themselves known to their peers and to establish useful social contacts. The proceedings of the Scandinavian meetings were published in Swedish, Danish or Norwegian, though they appeared somewhat irregularly and often with great delay. Nonetheless, communications given at the meetings and published in the proceedings were an important part of the infrastructure of Scandinavian science in the period from about 1840 to 1880.[33]

With Ørsted as the central and celebrated figure the fifth meeting was held in Copenhagen in the summer of 1847, at a time when eighteen-year-old Lorenz had not yet entered the Polytechnic College. After graduation he decided to attend the seventh meeting which took place in Christiania (Oslo) in Norway in July 1856, the first of his few scientific meetings. The Christiania meeting included 246 participants, among them fifty from Denmark. One of them was the physics professor Carl Holten. Another participant was Julius Thomsen who presented a paper on the thermal effects of electrochemical processes, a subject which presumably interested Lorenz. Shy, introvert and isolated as he was, Lorenz did not himself present a paper.[34] On the other hand, he most likely used the occasion to

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[33] On the Scandinavian science conventions, see Eriksson (1991) and Christensen (2013), pp. 517-533.

[34] *Forhandlinger ved de Skandinaviske Naturforskeres Syvende Möde* (Christiania: C. C. Werner, 1857). In the list of participants Lorenz was misspelled "Lorentz."

make himself visible to the Danish scientists gathering in Christiania. Contrary to Thomsen, later in life Lorenz only attended the Scandinavian meetings on rare occasions. One of them was the 1860 meeting in Copenhagen, where he gave an important paper on optical theory (Section 2.1).

One way for an outsider to increase his recognition and enter the small world of Danish science was to write an essay for one of the University's prize competitions, as Lorenz had successfully done. Another way went through the Royal Danish Academy of Sciences and Letters. The prestigious and elitist society established in 1742 accepted manuscripts for publication in its transactions (*Skrifter*) submitted by non-members. In fact it happened quite regularly that papers in *Skrifter* were written by young authors outside the Academy. During the period 1847-1880 twenty-nine memoirs were published by twenty different authors who were not members of the Academy.[35] Of course, such submissions were critically evaluated by relevant members of the Academy. To mention but one example of this kind of authorship, in 1850 the polytechnic candidate Ludvig August Colding had his important work on the conservation of energy or "force" published in *Skrifter*. Six years later he became a member of the Academy.[36]

Lorenz may have thought that what was possible for Colding would also be possible for him. In late 1857 while staying in Maribo with his mother, he submitted to the Royal Danish Academy a manuscript entitled "Contributions to the Physical Theory of Heat," requesting that if it were accepted it would be published in *Skrifter*.[37] As mentioned, the nature of heat was a subject that Lorenz had pondered about as early as 1849 and apparently it was close to his heart.

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[35] Lomholt (1942-1973), vol. 2, p. 37.

[36] L. A. Colding (1815-1888) graduated from the Polytechnic College in 1841 as a candidate from its mechanical class. His paper of 1850 on energy conservation was endorsed by Ørsted, who was secretary of the Royal Danish Academy. Somewhat similar to Lorenz, Colding's youthful speculations about the immutability of forces were rooted in philosophical and religious ideas, but by 1857 he was thoroughly acquainted with and fully accepted the mechanical theory of heat. See Dahl (1972).

[37] Lorenz to the Royal Danish Academy of Sciences and Letters, 30 December 1857 (Lorenz Papers, RAS).

But Lorenz's attempt to win early recognition as a physicist failed miserably.

The Academy appointed two competent members to review the manuscript, one of them Colding and the other Johan C. Hoffmann, an artillery officer and former teacher of physics and chemistry at the Military High School. The review procedure was unusually slow, resulting in a final recommendation only in March 1859. According to the report of Colding and Hoffmann, Lorenz defended the view that heat is a property of the ether whereas he did not accept the modern view of heat as molecular motion. He further argued that the ether was the connecting link between light and heat and a concept which promised to unify all the physical forces. Understandably, the two reviewers were not happy with the submitted paper, which they found was immature, speculative and difficult to understand. They consequently rejected it, if politely: "We cannot recommend that the paper in its present form is published in *Skrifter*; perhaps the author, in a subsequent paper such that he indicates, will be able to present his thoughts with greater clarity and understanding."[38]

It was a turning point in Lorenz's scientific life when he, in 1858, was able to travel to Paris to improve his knowledge of theoretical physics. The study tour, which lasted for about a year, was made possible by government and university grants, an indication that after all his talents were not quite overlooked in Denmark. Staying in a rented room in Quai Saint- Michel in the French capital, at the time an important centre of mathematics and physics, Lorenz was for the first time confronted with modern and advanced mathematical physics. He may also have appreciated the city for its architecture, parks and rich cultural life, but that we know nothing about. At the Science Faculty of the famous Sorbonne University he attended lectures by illustrious scientists such as, in mathematics, Michel Chasles, Joseph Bertrand, Jean-Marie Duhamel and Joseph Liouville; and in physics he followed lecture courses offered by Ga-

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[38] The Royal Academy of Sciences and Letters to Lorenz, 14 March 1859 (Lorenz Papers, RAS).

briel Lamé, Paul Desains and Henri Regnault.[39] Of these Lamé was an authority on the theory of elasticity, a branch of physics closely related to optics and one that appealed to Lorenz's mathematical mind. He wrote an examination paper on elasticity theory which was reviewed by Lamé, Bertrand and Liouville and later published in a revised version.[40]

After his return to Copenhagen, a matured Lorenz was ready to become a theoretical physicist in the international tradition; he had now abandoned his earlier speculations never to return to them. From a more practical view, not much had changed though and Lorenz continued to live as a part-time school teacher. He continued his previous work as assistant teacher at the Von Westen Institute and now also at the private Blaagaard Teacher's College established in 1859 and located in the Nørrebro district of Copenhagen.

Although increasingly preoccupied with theoretical physics and research in optics in particular, Lorenz took his job as a teacher seriously, such as reflected in the elementary textbooks he wrote in the mid-1860s.[41] Whether he was a good teacher or not is another question. We have only one testimony from a student taught physics and chemistry by Lorenz at the Von Westen Institute, and it is not flattering: "When Lorenz sat at his teacher's desk reeling off his learning, he might as well have spoken Chinese, for I understood nothing; and when he demonstrated an experiment to show how practice followed theory he might just as well have acted as a magician. I understood absolutely nothing."[42]

The textbooks supplied the author with an additional income, but it is unclear where they were used and to what extent. The two small mathematical textbooks of 1864, one in arithmetic and the other in algebra, were precise and rather abstract yet not without

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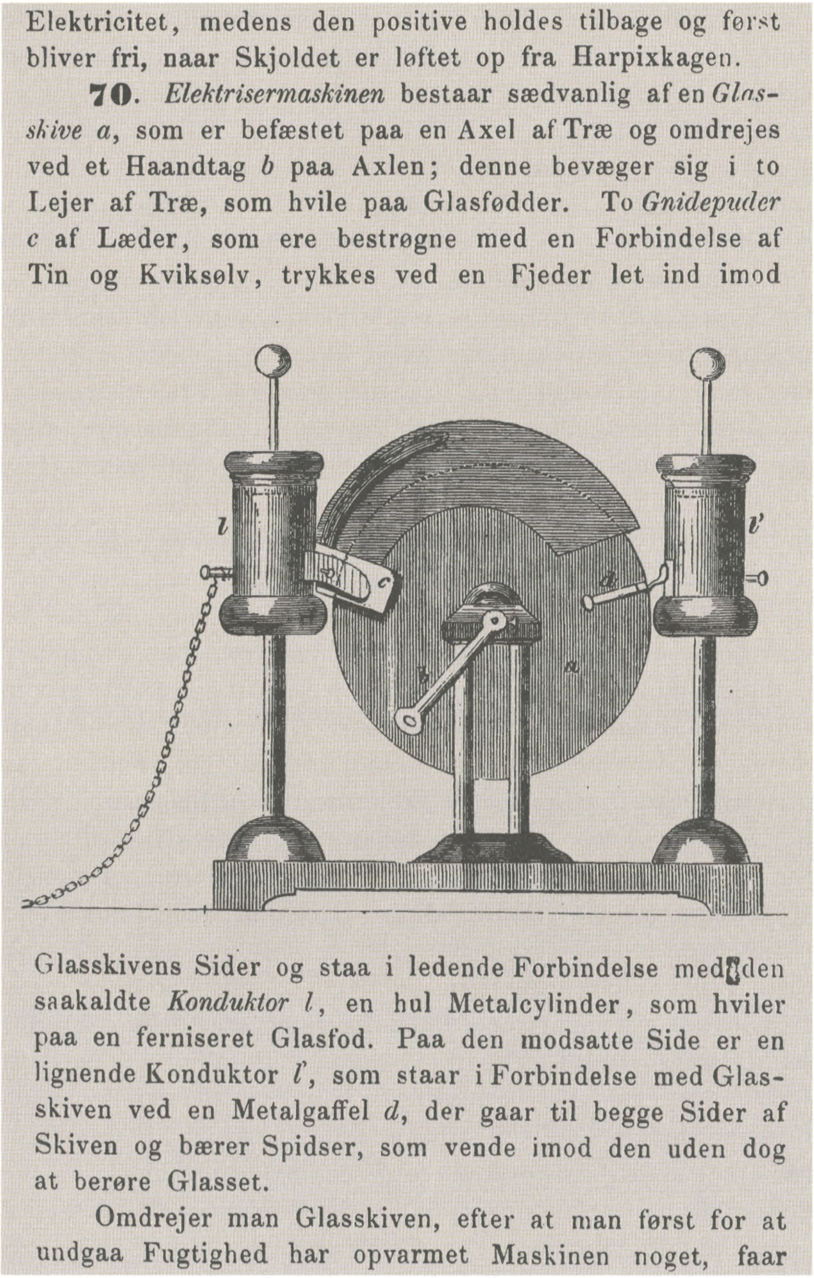
[39] Lorenz's annotated list of lecture courses at Sorbonne University (Lorenz Papers, NBA).

[40] Document from Institut Impérial de France, Académie des Sciences, 31 January 1860 (Lorenz Papers, NBA). See also Section 2.1.

[41] Lorenz (1864b); Lorenz (1864c); Lorenz (1865c).

[42] Berendsen (1918), p. 33. Later a journalist, Nathan J. Berendsen (1849-1920) was a student at the Von Westen Institute in the mid-1870s. By own admission he was a poor student with no interest at all in science and mathematics.



Elektricitet, medens den positive holdes tilbage og først bliver fri, naar Skjoldet er løftet op fra Harpixkagen.

70. *Elektrisermaskinen* bestaar sædvanlig af en *Glasskive a*, som er befæstet paa en Axel af Træ og omdrejes ved et Haandtag *b* paa Axlen; denne bevæger sig i to Lejer af Træ, som hvile paa Glasfødder. To *Gnidepuder c* af Læder, som ere bestrøgne med en Forbindelse af Tin og Kviksølv, trykkes ved en Fjeder let ind imod

Glasskivens Sider og staa i ledende Forbindelse med den saakaldte *Konduktor l*, en hul Metalcylinder, som hviler paa en ferniseret Glasfod. Paa den modsatte Side er en lignende Konduktor *l'*, som staar i Forbindelse med Glasskiven ved en Metalgaffel *d*, der gaar til begge Sider af Skiven og bærer Spidser, som vende imod den uden dog at berøre Glasset.

Omdrejer man Glasskiven, efter at man først for at undgaa Fugtighed har opvarmet Maskinen noget, faar

Figure 1.4: A page from Lorenz's Kortfattet Naturlære (A Brief Study of Physics), showing the electrifying machine for the production of static electricity.

pedagogical qualities. They included subjects such as logarithms, finite and infinite series, quadratic equations, prime numbers and calculation with numbers raised to the power of an arbitrarily large rational number.

Lorenz's elementary physics textbook intended for middle school and gymnasium students was widely used, witness that it appeared in five editions, the first published in 1865 and the last in 1887. The text of *Kortfattet Naturlære* (A Brief Study of Physics) was condensed, systematic and very informative; many students have probably found it demanding. Without using mathematics it covered mechanics, sound, light, heat, magnetism and electricity, all of the subjects being illustrated with experiments and machines such as the air-pump, the steam engine and the telegraph system (Fig. 1.4). The students were introduced to Newton's second law of motion in the very beginning of the book, if in words only. With regard to heat they were told that it is "an activity the real nature of which we are still unaware." Moreover, "we only know that light must be regarded as a wave motion of a kind different from that of sound."[43]

Still without a secure position and hence without a secure economy, on 12 August 1862 Lorenz married 31-year-old Agathe Fogtmann, the daughter of a grocer from Jutland. The couple spent their first years in Lorenz's small apartment in the Blaagaard Teacher's College. "Those who have visited him in these days will recall the narrow room he used as a study and where there was scarcely space enough for a visitor."[44] The marriage between Ludvig and Agathe seems to have been happy but was childless. Agathe Lorenz died in 1922 at the age of 91 and surviving her husband by 31 years.

Life as an assistant teacher and independent scientist was difficult but did not prevent Lorenz from being scientifically productive. In 1860 he participated in the meeting of Scandinavian scientists in Copenhagen together with 450 other attendees of which 310 were Danish. On this occasion - contrary to the earlier one in Christiania - he delivered a paper. The subject of his presentation was the theory of polarized light vibrations. During the next few years he wrote sev-

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[43] Lorenz (1865c), third edition of 1878, p. 74 and p. 58.

[44] Christiansen (1891a), who wrote from his personal experience.

Figure 1.5: Agathe Lorenz, née Fogtmann (1831-1922). Royal Library, Copenhagen, Picture Collection.

eral papers on optics which appeared in leading physics journals such as *Annalen der Physik und Chemie* and *Philosophical Magazine*. One of his papers, based on his examination paper in Paris, was in French and published in *Crelles Journal* devoted to mathematics and mathematical physics. In addition, he published several papers in Danish, some on physics but most of them on mathematical problems.

Given that Lorenz was an amateur in the true sense of the term and with no association to a research institution, his productivity was particularly remarkable in its quality. Physics in Denmark was at the time at a modest and rather provincial level, with by far most publications written in Danish and aimed at a local audience. Lorenz, on the other hand, wrote his memoirs on optics and other subjects primarily for the international physics community. Table 1.2 presents a comparison of the literary output of Lorenz and three other notable nineteenth-century Danish physicists, L. Colding, C. Holten and C. Christiansen.

	Birth/ death	*Books*	*Papers (Danish)*	*Papers (Foreign)*	*Profession by 1870*
L. Colding	1815-1888	0	14	5	City Engineer
C. Holten	1818-1886	2	19	0	University Professor
L. Lorenz	1829-1891	4	20	17	Teacher, Military High School
C. Christiansen	1843-1917	0	9	9	Lecturer, Polytechnic College

Table 1.2. Publications of four leading Danish physicists based on the lists in J. C. Poggendorff's *Biographisch-Literarisches Handwörterbuch*, Vol. 3 (Leipzig: J. A. Barth, 1898) covering the period 1858-1883. The lists are not complete, but I have used the same source for reason of comparison. Danish publications translated into a foreign language appear in both the fourth and the fifth column.

1.4 The Military High School and what followed

Lorenz's fortunes changed for the better only in 1866, when he obtained for the first time a permanent position and, no less important, was elected a member of the Royal Danish Academy. The two events meant a new and happier chapter in his life.

The position as physics teacher at the Royal Military High School had since 1860 been occupied by the chemist Julius Thomsen who in 1866 was appointed full professor at the University and consequently left the High School. Lorenz was appointed his successor and stayed in this position for twenty-one years, unable to obtain a position at either the University or the Polytechnic College. In 1868 the Military High School was restructured and incorporated into the Army's Military Academy (Hærens Officersskole), and as a result it moved from the crowded central Copenhagen to the peaceful Frederiksberg Castle outside the city. With the change followed better laboratories but otherwise it did not affect Lorenz's position and working conditions significantly (to avoid confusion I shall continue to refer to the institution as the Military High School). Nor did it change the High School's academic standing,

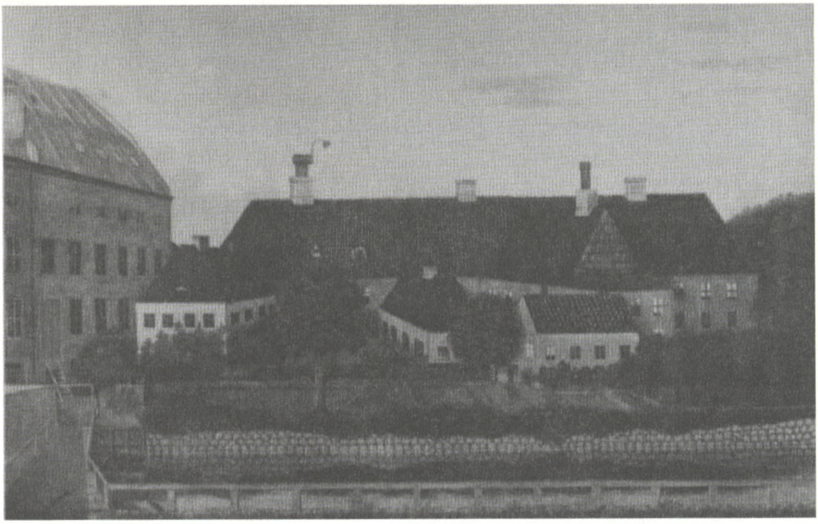

Figure 1.6: The Military High School in the centre of Copenhagen shortly after it inauguration in 1829. Royal Library, Copenhagen, Picture Collection.

for contrary to the University and the Polytechnic College it was not, strictly speaking, an academic institution as it had no right to confer academic degrees to its students.

At about the time when the Military High School changed to the Army's Military Academy teaching at the school was organised in four consecutive classes starting with the youngest and ending with the oldest students. While mathematical subjects were taught for all classes, physics was a subject only for the last three classes where it entered with about 90 hours per year. Chemistry was restricted to the two oldest classes and technology for the oldest. The distribution of courses in scientific and technical subjects is summarised in Table 1.3.

Class	1	2	3	4
Mathematics	200	330	250	100
Physics	-	90	90	100
Chemistry	-	-	150	200
Descriptive geometry, topography	-	-	200	140
Technical mechanics, machines, technology	-	-	-	350

Table 1.3. Teaching at the Military High School in the 1870s as given by the number of hours per class or approximately hours per year. The column for the oldest class refers to cadets of the artillery line. The course in rational mechanics was classified as mathematics and not physics. Source: *Undervisningsplan for Officeerskolen* (Copenhagen: J. H. Schulz, 1870).

Lorenz largely worked alone at the High School but collaborated to a certain extent with the school's chemistry teacher Haldor F. A. Topsøe, a specialist in crystallography (see also Section 3.4). Another teacher at the Military High School who Lorenz knew and with whom he may have interacted scientifically was Julius Petersen, who taught mathematics from 1881 to 1887 and later became one of Denmark's most distinguished and internationally recognised mathematicians. The careers of Lorenz and Petersen had crossed earlier, as the latter worked as an assistant teacher at the Von Westen Institute during the period 1859-1871.[45] As Lorenz had for a period been supported by the Smith Stipend, so was it the case with Petersen in the 1860s. Moreover, they were both members of the Mathematical Society (Matematisk Forening) founded in 1873.

What mattered most to Lorenz was that he now had at his disposal an excellent laboratory and consequently could engage in experimental research and not only theoretical investigations. He greatly improved the High School's physical laboratory and that to such an extent that, in the words of the University's physics professor, "under the direction of Lorenz it became the country's largest and best equipped collection [of instruments] of its kind."[46]

Of course, Lorenz's primary duty was to teach physics to the

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45. See Berendsen (1918), p. 33.

46. Christiansen (1891a).

army cadets, something he took seriously and which materialised in three textbooks on general physics, optics, and heat, respectively. To some extent these books were also read and used outside the High School. Thus, a book of 1870 titled *Lectures on Physics* (Forelæsninger over Naturlære) gave a solid introduction to mechanics, sound, electricity and magnetism. It was studied by the University's professor in philosophy, Rasmus Nielsen, who in a book on natural philosophy published three years later quoted extensively from it. Nielsen aimed at unifying naturalism with the older tradition of *Naturphilosophie* and apparently found Lorenz's expositions of mechanics, electricity and magnetism to be suitable for this purpose.[47] The philosopher also studied Lorenz's textbook on heat, a subject which interested him and which he wanted to know more about.[48]

Lectures on Physics did not include either heat or optics, but these were topics covered in the textbooks for the High School's advanced class. The two books were on an intermediate level but of a higher, more professional standard than the corresponding books used at the University. One of Lorenz's textbooks, the one on optics, was translated into German as *Die Lehre vom Licht* and published by the recognised Leipzig publisher B. G. Teubner.[49] According to a later source, Lorenz "was very popular with cadets, although he was a rather exacting teacher."[50]

Although generally satisfied with his position at the Military High School, the salary was modest and Lorenz felt forced to supplement his income by continuing his teaching of physics at the Blaagaard Teacher's College. There are very few testimonies of Lorenz's teaching at the Blaagaard College or of the students he taught there. One of those who listened to and apparently enjoyed his lectures was 19-year-old Sophus Tromholt who later became a leading

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[47] Lorenz (1870b); Nielsen (1873), especially pp. 276-282. R. Nielsen (1809-1884) also quoted from other Danish scientists, including J. G. Forchhammer, C. Holten and J. Thomsen.

[48] In a letter of 19 December 1877 Nielsen asked Lorenz to explicate various physical and mathematical questions related to the theory of heat (Lorenz Papers, DTM).

[49] Lorenz (1876); Lorenz (1877). B. G. Teubner to Lorenz, 13 and 29 January 1877 (Lorenz Papers, DTM).

[50] Hansen (1932), who offers no evidence.

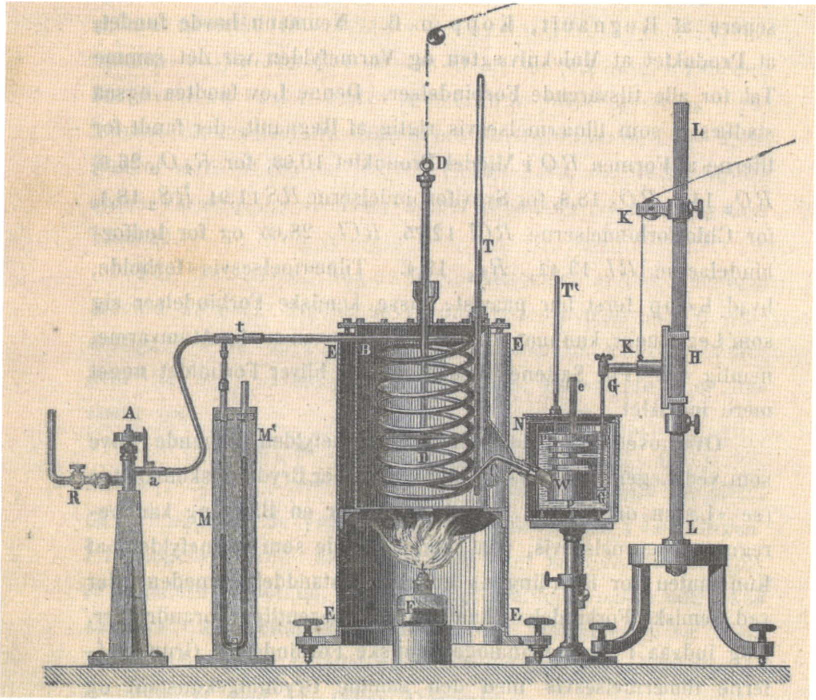

Figure 1.7: Illustration from Lorenz's textbook on heat showing the experimental setup by means of which the French physicist Henri-Victor Regnault measured the heat capacity of gases. Source: Lorenz (1877), p. 82.

explorer of polar light. Having completed his teacher's training in 1871 Tromholt wanted to improve his knowledge of physics and astronomy, which was his reason for addressing his former teacher. He recalled with gratitude "the obliging and kind attitude with which you always met me during my stay at the teacher's college." Tromholt had got interested in polar light and reported some of his observations to Lorenz from whom he obviously wanted support for his plan for further studies.[51]

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51. Tromholt to Lorenz, 15 September 1872 (Lorenz papers, DTM). His positive recollection of Lorenz as a teacher is confirmed in Appel (1891), who spoke with several former students from the Blaagaard College. Tromholt worked as a teacher in Jutland for a few years after which he went to Norway. Although he published widely on

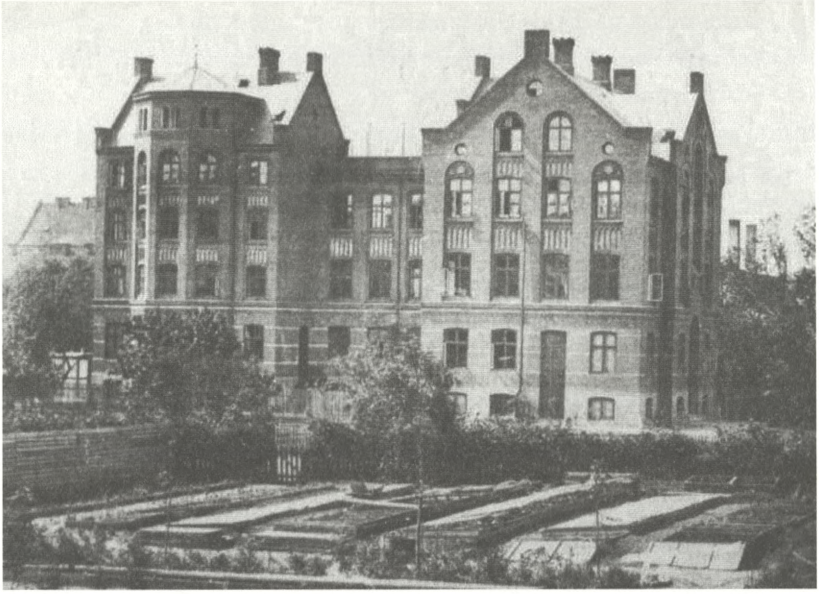

Figure 1.8: Blaagaard Teacher's College, photograph of 1867. Royal Library, Copenhagen, Picture Collection.

Lorenz was a modest man and personally (if not scientifically) unambitious; when it came to the external and material aspects of life he was unassuming. He fought a constant struggle to finance his experimental research and spent a good deal of time writing applications for grants, many of them to the Royal Danish Academy of Science, the Carlsberg Foundation, or the Ministry of Church and Education (Ministeriet for Kirke- og Undervisningsvæsenet). Economic support was necessary, he complained in an application of 1873, "if my time and resources shall not be fragmented in the fight for daily bread."[52] In many if not all cases he received the support that he requested.

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polar light and other subjects he remained outside or on the fringe of the Danish scientific community. See Moss and Stauning (2012).

[52] Lorenz to the Ministry of Church and Education, 2 August 1873. Lorenz to the Royal Danish Academy, 14 February 1869, seeking money for an apparatus to measure the optical refraction of vapours; and 27 October 1865, for experiments on refraction at different temperatures (Lorenz Papers, RAS).

In the same year that Lorenz was appointed physics teacher at the Military High School he entered as a member of the Royal Danish Academy of Sciences and Letters, the ultimate recognition of him as one of the country's leading scientists. The Academy was quite an exclusive club with only about twenty members of its scientific class at the time of his election. He was proposed by the mathematician Adolph Steen, who highlighted Lorenz's mathematical mind: "Mr. Lorenz deserves considerable credit for his work on the mathematical theory of light which has won wide recognition; moreover, he is a bright and penetrating mathematician such as witnessed by several minor works."[53] The recommendation, to which was attached a list of Lorenz's publications, was supported by J. Thomsen and other members of the learned society.

Although not particularly active in the Royal Danish Academy, Lorenz appreciated its meetings in the stately Prinsens Palæ (The Prince's Mansion) near the National Museum. For a period he attended the meetings as often as possible. The historian of the Academy reports that during the years 1860-1870, only thirteen of its members were present at more than two-thirds of its meetings and that Lorenz was one of them.[54] During his later years he only attended the meetings infrequently, the last time being on 20 March 1891, three months before he passed away. In several cases he did his duty as reviewer of incoming manuscripts and applications for support (see Section 4.1 for an example).

Many of the members of the Academy's science class were active as lecturers for the Society for the Dissemination of Natural Science, such as was the case with C. Holten, S. M. Jørgensen, J. Thomsen and C. Christiansen. With Lorenz's membership in 1866 he may have felt it his duty to contribute to the activities of the Society. After all, it was the Society lecture in Maribo some twenty-five years earlier which had first awakened his interest in physics; and besides, with his experience as an assistant school teacher he was able to explain difficult physics at a popular level. Lorenz's involvement with the Society for the Dissemination of Natural Science was lim-

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[53] Letter of 6 November 1866 (Lorenz Papers, RAS).

[54] Lomholt (1942-1973), vol. 2, p. 462.

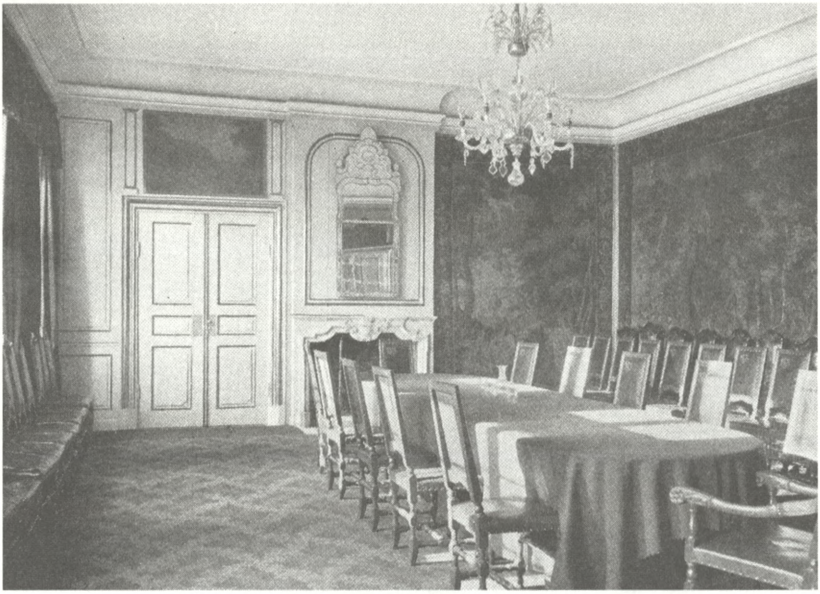

Figure 1.9: The meeting room of the Royal Academy of Sciences and Letters during the time when Lorenz was a member. Until 1899 the Academy was housed in a set of rooms in Prinsens Palæ (The Prince's Mansion) near the National Museum in central Copenhagen. Reproduced from Lomholt (1942-1973), Vol. 2, plate IX.

ited to two public lectures, in 1867-1868 ("Sound") and 1870-1871 ("Recent Progress in Science"), and one lecture in 1868 for students at a middle school ("Michael Faraday's Discoveries").[55]

As Lorenz eventually received recognition from the Danish scientific community so he received public and social recognition. In 1869 he was awarded the Order of the Dannebrog, to be extended in 1883 to the more prestigious Cross of Honour of the Order of Dannebrog. Four years later he was appointed a Councillor of State (Etatsråd), a purely honorary title. Lorenz never became a professor and never wrote a doctoral dissertation, and yet he was often addressed as a professor and doctor. The professorship assigned him in 1876 was of a titular and purely formal kind, a title of honour with no academic implications whatsoever. It was common at the

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[55] Harding (1924), pp. 147-149. Lorenz to the Society for Dissemination of Natural Science, 12 November 1867 (SNU Archive, NBA).

Figure 1.10: Ludvig Lorenz at the time he was elected a member of the Royal Danish Academy. Source: Hald (2005), p. 19.

time to appoint honourable scientists, authors and artists titular professors, and Lorenz was just one of many. To mention but one example, Henrik G. Bohr, the rector of the Von Westen Institute and Lorenz's former employer, was also appointed a titular professor. In 1884 Lorenz could add to his honours membership of the prestigious Academia Leopoldina, a Saxon academy of science with roots back to 1652.[56]

Lorenz possibly aimed for more than just a titular professorship, for in 1874, when there was a vacant position at the University of Christiania in Norway he expressed interest in it. He may have applied for the position, but in that case unsuccessfully. The chair went to a 29-year-old Norwegian, Oscar Emil Schiøtz, who had published almost nothing and from a professional point of view was

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[56] C. H. Knoblauch to Lorenz, 9 August 1884 (Lorenz Papers, RAS). Carl Hermann Knoblauch (1820-1895), a professor of physics at the University of Halle, was at the time president of Academia Leopoldina.

clearly inferior to Lorenz.[57] As to the University of Copenhagen, with Holten's death in 1886 a new physics professor had to be found. One could have imagined that the highly qualified Lorenz would have applied for the position, but apparently he did not. The fourteen years younger Christiansen, who came from a position as lecturer at the Polytechnic College and was a friend of Lorenz, was chosen as the best or perhaps the only candidate. Only with him did modern theoretical physics enter Danish university teaching.

Two years older than the University of Copenhagen, the University of Uppsala, Sweden, was founded in 1477 as the first one in Scandinavia. Like its Danish counterpart it was originally a Catholic institution. In 1877 the Swedes celebrated with much festivity its 400-year's anniversary and in this connection conferred honorary doctoral degrees on several Scandinavian and other scientists. Lorenz was one of them and he much appreciated the recognition that followed with the title.[58] Together with other Danish scientists and scholars he went to Uppsala in September to be conferred the distinction as *doctor honoris causa* of philosophy (dr.phil. h.c.). The Danish chemist Christen T. Barfoed, director of the Chemical Laboratory of the Agricultural College, was also appointed honorary professor but in his case in medicine. In what at the time was Sweden's academic capital Lorenz had the opportunity to meet other prominent Scandinavian scientists such as the Norwegian physicist and mathematician Carl Anton Bjerknes and his compatriot, the mathematician and chemist Cato Maximilian Guldberg.[59]

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[57] Lorenz wrote a draft application formally addressed to the king of Norway to which he added a brief curriculum vitae (Lorenz Papers, DTM). However, I have been unable to confirm that he actually applied for the position. O. E. Schiøtz (1846-1925) replaced Hartvig Caspar Christie (1826-1873), another undistinguished physicist. Schiøtz had been on a study tour to Germany but after his return to Norway he focused on teaching and local problems, such as geology and meteorology, without publishing in internationally recognised journals.

[58] Lorenz to C. R. Nyblom, 25 March 1877 (Lorenz Papers, RAS).

[59] See Uppsala (1879), where Lorenz is briefly portrayed on p. 365. One would imagine that Lorenz was interested in the work of Bjerknes in particular. C. A. Bjerknes (1825-1903) was a pupil of G. Lamé and worked primarily in hydrodynamics which he developed into forms which he claimed could explain the laws of electrodynamics and even gravitation. Although Lorenz would not have accepted

Lorenz's titles meant an increased status in the social hierarchy but nothing more than that. His honorary doctoral title too belonged to this category, although in this case it related to the scientific and not the social hierarchy. Reflecting his increased social recognition Ludvig and Agathe Lorenz moved to a larger and more appropriate residence in Frederiksberg Allé 13, not far from Frederiksberg Castle and the Military High School.

1.5 Late life and legacy

The Carlsberg Foundation was established in September 1876, economically based on the wealthy brewer Jacob C. Jacobsen's donation of a capital sum of 1 million kroner which in 1881 was increased to 2.2 million kroner. This provided an annual interest of 110,000 kroner, at the time a very large amount of money. According to the statutes of the Foundation it should be managed by a board of five directors appointed by the Royal Danish Academy of Science from its own ranks. By 1887 the chairman of the board was the chemist Christen T. Barfoed and the other members were the historian Edvard Holm, the philologist Johan L. Ussing, the zoologist Japetus Steenstrup, and the chemist Sophus M. Jørgensen. The statutes of 1876 listed the different purposes that the Foundation could support. One of them was "Salaries for life, or for a limited period, for distinguished men who are able fruitfully to work as 'free scientists' untrammelled by public duties."[60] During Jacobsen's lifetime – he died on 30 April 1887 – the possibility of life-long salary stated in paragraph 9 was only used on a single occasion and it has never since been used.

On 24 February 1887 Lorenz received what was undoubtedly a most welcome letter from the Foundation's board of directors:

Bjerknes' physical theory, the research interests of the two Scandinavian physicists had much in common.

[60] Pedersen (1992), p. 226. For the history of the Carlsberg Foundation, see also Glamann (2003).

> Acting in the service of science the board of directors wants to offer you all its assistance to consider a position in which you can work as a "free scientist." With this in mind it allows its chairman to announce that it has agreed to offer you a life-long salary of 4000 kroner per year from the day that you resign from the public position which you occupy at the present and thus satisfy the statutes of the foundation § IX concerning the condition for granting a "life-long salary." In the hope that you will accept this grant in order to use all your strength on your own work, the board of directors expresses its recognition of how much science owes to you.[61]

Most likely, had it not been for his membership of the Royal Danish Academy and the contacts it provided, Lorenz would not have received, at the age of 58, this unique and most generous offer from the Carlsberg Foundation to pay him as an independent researcher for the rest of his life. Although Lorenz then had to quit his position at the Military High School and therefore also his experimental investigations, he happily accepted the offer. In his letter of reply he wrote to the Carlsberg Foundation:

> It will be my task for the future to prove that I am worthy of the confidence that the board of directors has shown me by this offer. From the members of the Royal Academy of Science, so highly respected by the board of directors, I will gather courage and desire to continue my work in the service of science. ... [As a result of] the economic independence and release from any interrupting work I will be able to take an optimistic view of the future and concentrate my efforts on larger problems to the best of my ability. I consequently accept the board's honourable offer in the hope that my work, in agreement with the purpose stated by the originator of the Carlsberg Foundation, will also contribute to the progress of science. [62]

High on his agenda of "larger problems" was the project he had recently started on the scattering of plane waves by spherical bodies.

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[61] Letter to Lorenz of 24 February 1887, signed by C. T. Barfoed, E. Holm, S. M. Jørgensen, J. Steenstrup and J. L. Ussing (Lorenz Papers, RAS).

[62] Draft of letter from Lorenz to the Carlsberg Foundation, 26 February 1887 (Lorenz Papers, RAS).

This important work, which until 1898 existed in Danish only, was his last publication in physics. When he introduced it to the members of the Royal Danish Academy of Science in October 1889, he said that "The work which I have the honour to present to the Academy is concerned with a simple problem in formal optics, namely, a transparent sphere illuminated by parallel rays of light."[63] But the theory he presented was far from simple - it was frighteningly complex (see Section 2.4). At the end of his life Lorenz was not only concerned with the difficult optical scattering theory, for he also found time to look at the effect of the system of constituency on the representation of minority parties in the Danish Parliament. This, a subject of an entirely different nature, was of potential political importance but to Lorenz it was primarily a problem inviting considerations of the mathematical theory of probability (Section 5.4).

Lorenz spent his last year in mathematical research, in part completing a comprehensive paper on prime numbers and in part, even more ambitiously, contemplating a future work on the famous three-body problem. In April 1891 he wrote to a Swedish colleague, the astronomer and mathematician Hugo Gyldén:

> I expect, in a few weeks from now, to send you a small memoir I have written on the prime numbers. Following this work, which has taken me nearly a year to complete I now look around for other problems of a mainly mathematical character since I no longer have at my disposal a physical collection. I also feel most in the mood to tackle a larger problem in applied mathematics. Only the future will tell whether it will be the "three-body problem" or something else, but I will, at any rate, acquaint myself with the present status of this problem.[64]

The problem which Lorenz referred to was a classic in mathematical physics and at the time in the centre of a heated mathematical discussion. It concerns how to determine the motion of three bodies of

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63. Lorenz Papers, RAS.

64. Lorenz to Gyldén, 19 April 1891 (Archive of the Royal Swedish Academy of Sciences, Stockholm). The Finnish-born J. A. Hugo Gyldén (1841-1896) was director of the Stockholm Observatory and a leading specialist in celestial mechanics.

known masses on the basis of the laws of classical mechanics and the initial conditions of the bodies' positions and velocities. The three bodies may be the Sun, the Earth and the Moon. Going back to Newton's *Principia* of 1687, the problem was investigated by dozens of mathematicians and astronomers, culminating with a series of works by the brilliant French mathematician and physicist Henri Poincaré. Lorenz knew that Poincaré had recently been awarded a prize named after the Swedish king Oscar II for a memoir on the problem and that Gyldén was centrally involved in the scandal that followed.[65]

The three-body problem was well known also in Danish scientific circles. For example, in 1889 the Royal Danish Academy announced an international gold medal competition relating to the astronomical aspects of the problem.[66] Lorenz was more interested in its mathematical aspects. He began studying Poincaré's memoir and other relevant publications, but he did not come far in his studies of the intricate three-body problem. His position as an independent physicist paid by the Carlsberg Foundation ended abruptly less than two months after his letter to Gyldén. On the ninth of June 1891 he died unexpectedly of a heart attack, at the age of 62. He was buried in the Assistens Cemetery, Copenhagen, located in the neighbourhood where he had spent most of his life.

As a respected citizen and scientist, Lorenz's untimely death was duly noted in Danish newspapers and magazines. *Berlingske Tidende*, the country's oldest newspaper dating from 1749, included on 10 June 1891 an obituary on the physicist and his unusual career. In an anonymous obituary in *Illustreret Tidende* (Illustrated Magazine), a popular and widely read bourgeois journal founded in 1877, Christiansen paid homage to his friend and colleague in theoretical physics. Referring to Newton, Ampère, William Thomson, Kirchhoff and Maxwell, he deplored that "with the death of Lorenz, our fa-

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[65] For the scandal relating to Poincaré's memoir and Gyldén's involvement in it, see Barrow-Green (1997).

[66] *Videnskabernes Selskab, Oversigt* (1891), pp. 22-26. The evaluation committee consisting of T. N. Thiele, J. P. Gram and H. Valentiner awarded the gold medal to the Austrian astronomer Eduard von Haerdtl (1861-1897).

therland has recently lost a man who can in any way be compared with them as regards mathematical brilliance, experimental acuteness and tireless diligence."[67] Having known Lorenz since the 1860s and being close to him both personally and scientifically, Christensen's insightful obituary is a valuable historical document.

Jacob Appel, a polytechnic candidate and later a leading Folk High School teacher and prominent politician, portrayed Lorenz in an article in which he emphasised his "unusually independent" way of working as well as his somewhat isolated status in the Danish academic environment. He wrote about Lorenz's "world-famous memoir" from 1867, which was of course a grossly exaggerated label, and regretted that Lorenz never obtained a position at the University.[68] The physicist Peter Kristian Prytz, a former collaborator of Lorenz and at the time associate professor at the Polytechnic College, described Lorenz in the pages of *Tidsskrift for Physik og Chemi* (Journal of Physics and Chemistry). This journal was established in 1862 by Julius Thomsen and his brother Carl August Thomsen, and Lorenz had contributed a paper on the theory of light to its very first volume (Section 3.1). Among Lorenz's qualities as a physicist Prytz highlighted how his best works relied on an intimate connection between experiment and theory. He further noted, correctly, that "Lorenz was not a popular author" and that the major part of his scientific work was "inaccessible to a broader public."[69]

Indeed, although Lorenz wrote elementary and pedagogical textbooks he was foreign to the genre of popular science in the ordinary sense. While it was common for leading Danish scientists to write for a broad audience or to appeal to the literary-cultural sector of society, this was not Lorenz's style. In 1870 he was addressed by the editor of a new Danish cultural magazine with the request of contributing with an article to the magazine's first issue: "It will be easy for you, with your great amount of knowledge, to give a pres-

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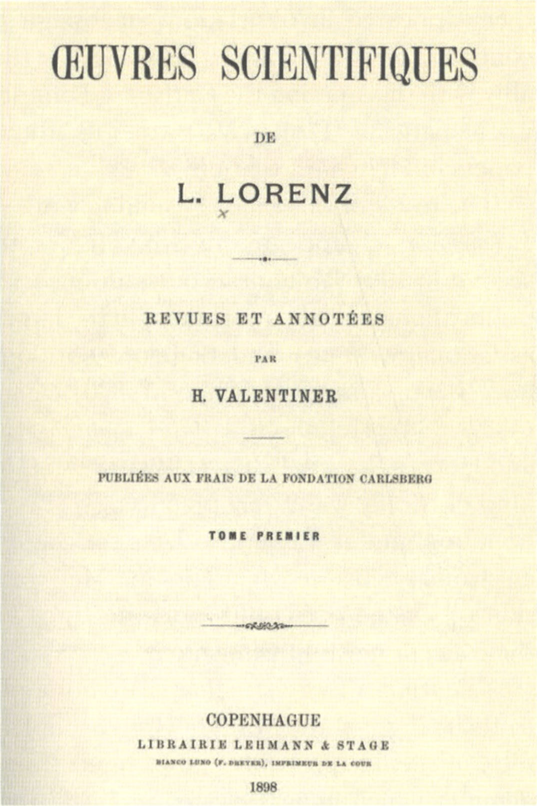
[67] Christiansen (1891). Together with portraits of other Danish nineteenth-century physicists (Ørsted, Holten and Colding), Christiansen published in 1886 a concise description of Lorenz's contributions to physics. See Hansen (1886), p. 763.

[68] Appel (1891). J. Appel (1866-1931) had met Lorenz, but only casually and without knowing him personally.

[69] Prytz (1891).



ŒUVRES SCIENTIFIQUES

DE

L. LORENZ

REVUES ET ANNOTÉES

PAR

H. VALENTINER

PUBLIÉES AUX FRAIS DE LA FONDATION CARLSBERG

TOME PREMIER

COPENHAGUE
LIBRAIRIE LEHMANN & STAGE
BIANCO LUNO (F. DREYER), IMPRIMEUR DE LA COUR
1898

Figure 1.11: The first volume of Herman Valentiner's edition of Lorenz's collected works.

entation of some important law of nature, some significant progress in physics or something of that kind. I would like to have a general account aimed at cultured readers of how physics has developed historically, what its method is and where it stands presently."[70] Lorenz ignored the request.

While the memory of Lorenz soon faded, the Carlsberg Foundation wanted to honour the physicist which it had so generously supported but who had unfortunately passed away before he could deliver the scientific results expected from him. Some of Lorenz's major contributions to physics were published in Danish in the transactions of the Royal Danish Academy of Science and not all of

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[70] Vilhelm Møller (1846-1904), an author and editor of *Nyt Dansk Maanedsskrift* (New Danish Monthly) to Lorenz, 14 December 1870 (Lorenz Papers, DTM).

them appeared in translations in foreign journals. Generally his contributions were not well known at the turn of the century. Probably with this in mind the Carlsberg Foundation commissioned the mathematician Herman Valentiner to edit a collection of Lorenz's papers in French translation.

The result of this time-consuming work was *Oeuvres Scientifiques de L. Lorenz* which appeared in two volumes, the first in 1898 and the second in 1904. To honour outstanding scientists with this kind of publications was not uncommon in the larger scientific nations such as Germany, France and England. Random examples are *The Collected Works of James MacCullagh* (1880), Cauchy's *Oeuvres Complètes* (1882-1938), Helmholtz's *Wissenschaftliche Abhandlungen* (1882-1895), and Hertz's *Gesammelte Werke* (1894-1895). However, in Denmark this kind of tribute was rarely practiced. Not even the great Ørsted was honoured in this way until 1920 - the centenary of the discovery of electromagnetism - when Kirstine Meyer edited a splendid collection of Ørsted's scientific works in three volumes. Unfortunately the collection was in Danish only.

Valentiner was a mathematician, not a physicist, who from 1887 to 1900 taught at the Military High School and also at the Polytechnic College. He probably knew Lorenz from the Royal Danish Academy to which he was elected in 1888. Apart from a biographical sketch in the second volume *Oeuvres Scientifiques* consisted of annotated translations of thirty of Lorenz's papers, leaving out only some of his minor mathematical communications. Valentiner's annotations were almost purely concerned with mathematical details which he expounded critically and at great length. However, although a mathematician he also had an amateur interest in physics. When he presented the first part of volume 1 to the Royal Danish Academy at a meeting of 30 October 1896 he emphasised the difference between Lorenz's and Maxwell's electromagnetic theories of light rather than their similarities, and he further supported Lorenz's scepticism regarding theories based on the "purely hypothetical" molecular forces.[71] We return to Lorenz's scepticism in Section 5.2.

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[71] Valentiner (1896) and Valentiner (1898-1904), vol. 1, pp. 204-210. For the electromagnetic theory of light, see also Valentiner (1897). *Oeuvres Scientifiques de L. Lorenz* was

Oeuvres Scientifiques was known to the physics community but attracted little attention and was rarely cited. After all, from the perspective of early twentieth-century physics there was no reason to re-study the works of the Danish physicist which seemingly belonged to a tradition no longer of great relevance. The first volume was positively reviewed in *Nature*, where the anonymous reviewer focused on Lorenz's contributions to optical refraction and to the absolute determination of the unit of electrical resistance. "The author's name is well known as one who has worked at optical theory, and has carried out experiments of great importance with a view to the verification of crucial points in that theory."[72] The *Nature* review did not mention Lorenz's work of 1867 on the electromagnetic theory of light.

Physical chemists were made aware of Lorenz and his works in a review paper written by the famous Wilhelm Ostwald, a pioneer of physical chemistry and a future Nobel Prize laureate. Rather than dealing with Lorenz's work and its current significance in physical chemistry and related areas Ostwald used the review to present a summary biography of the Danish physicist based on Valentiner's account.[73] Ostwald was keenly interested in the history of science and had in 1889 initiated the important series of reprints known as *Klassiker der Exacten Naturwissenschaften* (Classics of the Exact Sciences). He apparently saw Lorenz as merely a historical figure on par with other great scientists of the past.

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reprinted in 1965 by Johnson Reprint Corporation, New York, and is available on the internet as https://archive.org/details/oeuvresscientifi01lore.

[72] *Nature* **61** (1900): 465-466.

[73] *Zeitschrift für Physikalische Chemie* **50** (1905): 380-381.

CHAPTER 2

Theories of light and elasticity

Lorenz's first and lasting love was optics. This was the subject of his very first scientific paper from early 1860, and it was the subject of a long and complex communication he published thirty years later, the year before his premature death.[1] Initially Lorenz conceived light as transverse waves propagating in the ether as described by the theory of elasticity, which was in accordance with the generally accepted view. But he soon decided that a more abstract, mathematical theory with no physical imagery and only a minimum of interpretation was the way ahead. In 1867 he proposed an electrical theory of light (Section 3.2) in accordance with his earlier, phenomenological description but still differing from it.

Apart from formulating the fundamental equations of light waves and their propagation in space, Lorenz used his theory to derive formulae for reflection, refraction, dispersion and other optical phenomena. His extensive work on refraction, which included elaborate experiment as well as theory, was of particular importance as it led to a law of refractivity with applications to physical chemistry, molecular physics and other areas of science. At the end of his life Lorenz completed a thorough analysis of light scattering by spherical bodies which anticipated what became known as Mie scattering some two decades later. The legacy of Lorenz in modern optical theory is demonstrated by names such as Lorenz-Mie scattering and the Lorenz-Lorentz formula.

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1. Kragh (1991) is a general survey of Lorenz's work in optics. For a comprehensive and more detailed modern exposition of Lorenz's works in optics, see Keller (2002) on which source I rely in part. Another equally detailed and equally technical (but less modern) exposition is given in Pihl (1939), pp. 16-75. For other details and mathematical elaborations, see the extensive notes in Valentiner (1898-1904), vol. 1.

2.1 The elastic theory of light

Influenced by his stay in Paris, Lorenz's first scientific works were safely anchored in the elastic or mechanical theory of light which still in the early 1860s was the generally accepted theoretical framework in optics. In his lectures at the Military High School on elasticity theory Lorenz praised the theory as the most important of all and as the one which promised an explanation of phenomena as diverse as sound, light, electricity and magnetism (see Appendix B). It was simply of "enormous importance."

The higher aim of this theory or research programme, as worked out in different forms by Augustin-Jean Fresnel, Augustin-Louis Cauchy, Gabriel Lamé, George Stokes, Franz Neumann and others, was to derive the laws of optics from those of elasticity. The generally shared ontological assumption was that light consisted of transverse waves made up of deformations in an incompressible hypothetical medium.[2] In the mid-nineteenth century the new wave optics was a hot area of physics and an excellent career opportunity for young physicists wanting to enter the international scene. It appealed to mathematically talented students with a flair for and insight in experiments. Lorenz's chosen research area was just the right one as seen from an international perspective, but unfortunately not from the perspective of the more backward Danish physics community.

The medium carrying the vibrations of light was usually identified with the world ether, which was also known as the "luminiferous ether" to emphasise its crucial role as the seat of the oscillations of light. The concept of the ether (or several ethers) goes back to ancient Greek natural philosophy and has played diverse roles through the history of science and ideas. With the emergence of the wave theory of light in the 1820s the ether was generally seen as

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[2] For informative but technically demanding accounts of the subject, see Whittaker (1958), pp. 128-170 and Darrigol (2012), pp. 225-239. A survey of nineteenth-century optics is offered in Buchwald (2013). For the history of classical theories of elasticity, Isaac Todhunter's magisterial work edited by Karl Pearson is still worth consulting. See Todhunter (1886-1893). Glazebrook (1885), a detailed report on nineteenth-century optical theories, is another contemporary source of considerable value.

necessary and associated with optical phenomena in particular. After all, if light consists of waves there must be a medium in which the waves propagate. But there were many shades of opinion among ether theorists and also among the few physicists who doubted or even rejected the existence of the ether.[3]

During his stay in Paris Lorenz had followed Lamé's lectures and eagerly studied his celebrated textbook on the mathematical theory of elasticity. In the last part of the book Lamé eloquently described the ether as a necessary key to understand and unify the diversity of physical phenomena. Concerning this "sole cause of all the facts observable" he wrote that "it is impossible to arrive at a rational and complete explanation of physical nature without interposing this agent, whose presence is inevitable."[4] While Lamé adopted a realist position, arguing that the ether was indispensable and no less real than material bodies, other physicists were instrumentalists who considered the optical ether to be merely a useful hypothesis.

What all agreed upon was that, if the ether existed, painfully little was known about it. As the Irish physicist and mathematician James MacCullagh wrote in 1838, "It is certain, indeed, that light is produced by undulations, propagated, with transversal vibrations, through a highly elastic ether; but the constitution of this ether, and the laws of its connexion … with the particles of bodies, is utterly unknown."[5] Half a century later the elastic ether had been replaced by the electromagnetic ether, but its constitution was still utterly unknown. About 1860, when Lorenz entered the field, the incompressibility of the solid ether and the transverse nature of the vibrations therein were subjects of debate. Lorenz's early work in optics helped to clarify the questions.

Within the elastic paradigm or research tradition there were several problematic areas, one of them being the explanation of Fresnel's formulae for the intensity and polarisation of reflected and

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[3] For the variety of ether theories in the eighteenth and nineteenth centuries, see Cantor and Hodge (1981).

[4] Lamé (1852), pp. 325-327.

[5] Quoted in Darrigol (2010), p. 149.

refracted rays of light. According to Fresnel's laws of 1823, for oscillations normal to the plane of incidence the intensity ratio between the reflected and the incident light was given by

$$\frac{I_r}{I_0} = \left[\frac{\sin(\varphi_i - \varphi_r)}{\sin(\varphi_i + \varphi_r)}\right]^2,$$

where φ_i denotes the angle of incidence and φ_r the angle of refraction. For light polarized parallel to the plane of incidence he found

$$\frac{I_r}{I_0} = \left[\frac{\tan(\varphi_i - \varphi_r)}{\tan(\varphi_i + \varphi_r)}\right]^2$$

Fresnel derived similar formulae for the transmitted light.

According to Fresnel and George Green among others, the vibrations of reflected polarized light took place perpendicularly to the plane of polarization; on the other hand, Neumann and MacCullagh held that the planes of polarization and vibration coincided. While Fresnel's view implied that the density of the ether varied but the elasticity remained constant in the two media, the Neumann-MacCullagh view required a constant density and varying elasticity of the ether. The position of Neumann and MacCullagh was criticized by Stokes and accepted only by a minority of researchers. One of them was the German physicist Carl Holtzmann whose experiments of 1856 seemed to agree with the idea of polarized light vibrations in the plane of polarization and thus disagreed with Stokes' conclusion.

Another problem related to experiments on the reflection of light. In 1848 the French physicist Jules Jamin showed that the reflected wave from a metallic surface often acquired a degree of elliptical polarization and that even if the incident light was linearly polarized. Apparently Jamin's results could be reconciled with Fresnel's laws only by adopting Cauchy's elastic light theory of 1839, according to which the density of the corpuscular ether varied periodically. Cauchy conceived at the time the ether as a kind of atomic or molecular lattice and to account for Jamin's data he was forced to assume longitudinal waves decaying very rapidly with the distance from the interface. However, by 1860 Cauchy's theory was generally rejected as artificial and unconvincing. More importantly, because of its basis in the theory of elasticity it included the existence of longitudinal waves and led to consequences with no support in experiment. For these and other reasons Jamin's findings were considered problematical.

Thirty-year-old Lorenz entered the discussions concerning the proper foundation of the elastic theory of light with three papers of 1860 which were in part based on a presentation he gave to the eighth meeting of Scandinavian scientists held in Copenhagen in July that year. The meeting was attended by 451 scientists and physicians of whom 310 were from Denmark, 99 from Sweden and 37 from Norway.[6] The geologist and director of the Polytechnic College J. G. Forchhammer gave a lecture on his important investigations of the chemical composition of ocean water and the zoologist J. Steenstrup spoke on extinct animals excavated from ancient caves. Lorenz's talk was in a section combining physics, chemistry and mathematics. Other communications in the section were given by the leading Swedish physicist Anders J. Ångström, famous for his pioneering work in spectroscopy, and by the young Norwegian mathematician and chemist Cato M. Guldberg, who a few years later, together with his brother-in-law Peter Waage, would formulate the fundamental law of mass action.

Danish contributors to the 1860 convention included the physics professor Carl Holten and the chemists Julius Thomsen and Christen Barfoed. Yet another talk was given by the 24-year-old Gustavus D. Hinrichs, who the following year migrated to the United States and is today known for his early version of the periodic system of the elements. In a highly speculative address on "Five Laws of Cosmic Physics" Hinrichs suggested that the Kant-Laplace nebular hypothesis might be supported by various numerical relations combining physics and astronomy.[7] If Lorenz listened to Hinrichs' presentation it is almost certain that he disapproved of it, so differ-

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[6] *Forhandlinger ved de Skandinaviske Naturforskeres Ottende Møde i Kiøbenhavn* (Copenhagen: Gyldendal, 1861).

[7] On Hinrichs, a fascinating character in the history of Danish and American science, see Zappffe (1969). See also Kragh (2016), pp. 266-268. His presence at the 1860 Scandinavian meeting has not been previously noticed.

ent it was in style and attitude from his own conception of science. The next year Hinrichs left Denmark for the United States, where he stayed until his death in 1923.

Apart from attending the lectures and scientific parts of the Copenhagen meeting Lorenz also had the opportunity to participate in some of the many dinners, parties and excursions which were an integral and no less important element in the Scandinavian science meetings. The social excursions included visits to the city's museums, art galleries, libraries, laboratories and scientific collections. Lorenz could have joined the tour to the Royal Porcelain Factory or the one to J. C. Jacobsen's innovative Carlsberg Brewery in Valby outside the city. Little could Lorenz know that many years later Jacobsen's brewery would significantly influence his life (Section 1.5).

In his address to the Scandinavian meeting Lorenz discussed in detail the problem of the direction of the ethereal oscillations in polarized light from both a theoretical and an experimental perspective.[8] Reporting a series of experiments of his own made with optical gratings he interpreted the results as support for the view that the oscillations were perpendicular to the plane of polarization. His address received critical comments from Ångström, who suggested that the question could be solved by a new experimental method. Although Lorenz's address was in Danish, in a revised version it quickly appeared in a German translation and it was also summarised in the abstract journal *Fortschritte der Physik* issued by the German Physical Society.

At about the same time Lorenz published a paper on the reflection of light in *Mathematisk Tidsskrift* (Mathematical Journal), a new Danish journal for the mathematical sciences.[9] Not only did translations of both papers appear in the leading German journal *Annalen der Physik und Chemie*, based on the German version they were also translated into French and English, in *Annales de Chimie et de Physique*

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[8] Lorenz (1860a). A German translation appeared in *Annalen der Physik und Chemie* **111** (1860): 315-328.

[9] Lorenz (1860b). *Annalen der Physik und Chemie* **111** (1860): 460-473. For *Mathematisk Tidsskrift*, see Section 5.1.

and *Philosophical Magazine*, respectively.[10] Moreover, they were critically and extensively reviewed in *Fortschritte*, in both cases by the mathematician E. Christoffel. In addition to these translations Lorenz submitted in November 1860 a long paper in French to *Crelles Journal* which appeared the following year (see below). Lorenz's early papers on optics were thus well received and known to the international community of physicists.

The over-all aim of Lorenz's early papers in theoretical optics was to understand the ether vibrations in polarized light. This he did by a careful study of the way light bends in its passage through a small aperture in an opaque screen, assuming that the diffracted waves depend only on the disturbance over the aperture. From a theoretical analysis supported by his own experiments Lorenz concluded that the view of Neumann and MacCullagh was untenable and that the only correct one was Fresnel's. This conclusion was in fact not new since Stokes, using methods different from Lorenz's, had proved the same in works published in the early 1850s. Lorenz based his considerations on a formula for light diffracted the angle β when passing a hole. If α and α_1 denote the angles that the vibrations of the incident and the diffracted rays form with the normal to the plane of the two rays, then Stokes showed that

$$\tan \alpha_1 = \cos \beta \, \tan \alpha$$

Lorenz confirmed Stokes' experimental results as well as their theoretical interpretation. On the other hand, he also argued that Stokes' theory was incomplete and lacking in rigour and hence in need of improvement.

Much later, Stokes commented on Lorenz's investigation and its relation to his own earlier work. Although Stokes thought that the Danish physicist had not gone much further than he had himself and not grasped "the spirit of my method," he granted that "Lo-

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[10] The important German journal *Annalen der Physik* was founded in 1799, but under the editorships of J. C. Poggendorff (1824-1876) and G. H. Wiedemann (1877-1899) its title changed to *Annalen der Physik und Chemie*, to return in 1900 to its original title. I shall sometimes refer to the journal as just *Annalen*.

renz's result like mine were decisively in favour of the supposition that in polarized light the vibrations are *perpendicular* to the plane of polarization."[11] Also the famous William Thomson, at the time Baron Kelvin of Largs, called attention to Lorenz and his confirmation of Stokes' result which he called "one of the surest doctrines through the whole range of natural philosophy."[12]

With regard to the disturbing observations of Jamin and their apparent irreconcilability with Fresnel's laws, Lorenz demonstrated that they could be understood without involving the longitudinal component in the ether vibrations appearing in Cauchy's theory. Nor was the hypothesis of a periodic variation of the ether atoms necessary. To show this he assumed that the two optical media were separated by a boundary or transition layer which he conceived to be divided in an infinite number of infinitesimally thin sheets, each of the sheets having a constant density. A ray of light reflected at one of the interior layers would on emergence be retarded relatively to the ray at the surface. According to Lorenz:

> All calculations ... which have hitherto been made concerning the reflexion and refraction of light, have proceeded on the hypothesis of an instantaneous passage from one medium to the other, and a consequent instantaneous change of the index of refraction. Such a passage is, however, a mere metaphysical abstraction, which cannot possibly exist in nature; and the calculation would be more exact and more satisfactory if a gradual passage were admitted between the two media through a space which might afterwards be assumed to be as small as we please.[13]

As further support for the hypothesis of intermediate transition layers he pointed out that, "It is, moreover, a fact that bodies are really surrounded by an atmosphere which must produce such a gradual

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[11] Stokes (1883), p. 328. Glazebrook (1885, p. 204) defended Stokes' reputation: "Lorenz appears to consider his method as more general than that of Stokes, but this is due to a misconception on his part."

[12] Thomson (1904), p. 323.

[13] Lorenz (1860b), quoted from p. 480 in its English translation titled "On the reflexion of light at the boundary of two isotropic transparent media," *Philosophical Magazine* **21** (1861): 480-491. The English version was based on the German translation in *Annalen*.

change of refraction." Still adhering to the ether hypothesis he thought of an ethereal atmosphere with density and elasticity changing across the layer.

Assuming that Fresnel's formulae were strictly valid for the passage and reflection of light from one sheet to it neighbouring sheet, Lorenz was able to account for Jamin's anomalous results including certain relations between phase difference and refractivity that were inexplicable on Cauchy's theory. The results, he argued, not only lent additional support to the absolute validity of Fresnel's formulae, they also supported the physical hypothesis of intermediate layers. Lorenz estimated that the thickness of the layers was between 1 and 10 per cent of the wavelength of visible light or roughly between 5 nm and 50 nm. He may have been the first to take into account the transition layer, a concept which came to play an important role in later optical theory.[14]

Many years later Lorenz's geometrical theory of reflection was discussed critically by the French physicist Éleuthère Mascart in a textbook on optics, and as late as 1907 interest in it was revived by the young German physicist Reinhard Kynast, a student of Otto Lummer.[15] In his doctoral dissertation Kynast explored Lorenz's old theory which he extended to cover also total reflection, a case which Lorenz had not considered in his 1860 paper. Kynast concluded that although Lorenz's old theory rested on objectionable mathematical arguments, its formulae agreed with experimental data no less precisely than did Paul Drude's optical theory based on the electromagnetic theories of Maxwell and Lorentz.

The early works of Lorenz, later to be expanded with investigations due to the Third Lord Rayleigh (John W. Strutt), Friedrich Eisenlohr and other physicists, proved that the only admissible theory within the solid-elastic paradigm was the one defended by Green. The theories of Neumann, MacCullagh and Cauchy, on the other hand, led to inconsistencies. Lorenz thus helped clarifying the entangled web spun by the many elastic theories of light.

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[14] Volkmann (1891), p. 319 credited Lorenz for the idea of the transition layer and so did Schuster (1909), p. 241.

[15] Mascart (1891), pp. 438-440; Kynast (1907).

The German mathematician and physicist Elwin Bruno Christoffel is today famous for his seminal contributions to differential geometry and the symbols named after him to be found in, for example, Einstein's theory of general relativity. In 1862 Christoffel wrote an unusually long and detailed review in *Fortschritte der Physik* in which he subjected one of Lorenz's 1860 *Annalen* papers to critical analysis. Although he found the investigation of the Danish physicist to be "very competently carried out" he objected to certain parts of the theory and its claimed agreement with experiments.[16] Christoffel also criticized Lorenz's model of a gradual passage of light through a series of infinitesimal layers. Moreover, he argued that most of Lorenz's results regarding the direction of the light vibrations had earlier been established and better justified by Eisenlohr in a work of 1856. Lorenz did not respond to the rather sharp criticism, which may have had the effect of making his work more rather than less known in the international community of theoretical physicists.

Rayleigh was aware of Lorenz's contributions to optical theory. In one of his early papers dealing with the reflection of light Rayleigh derived results which he admitted "have been already given by Lorenz, of Copenhagen ... [but] I cannot agree with him on many important points." It was clearly important to 29-year-old Rayleigh to maintain his priority and to present his own theory as different from Lorenz's. "Those who have done me the honour of reading my papers ... will understand how I anticipated the two polarizing angles by the very different process there employed."[17] Rayleigh also referred to Lorenz's hypothesis of a gradual transition between the boundary of two transparent media, but at the time without accepting it. He objected that the hypothesis had certain experimental consequences that had not in fact been observed and were therefore "fatal" to the hypothesis. In his later work Rayleigh did accept the idea of transitional layers.

With regard to later developments in electrodynamics another of Lorenz's early works in the theory of optics and elasticity deserves

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[16] Lorenz (1860b). *Fortschritte der Physik* **16** (1862): 223-224. Christoffel reviewed Lorenz (1860a) in the same volume of *Fortschritte*, pp. 223-224.

[17] Rayleigh (1871a), who referred to Lorenz (1861b) and Lorenz (1860a).

mention. In a long paper published in *Journal für die Reine und Angewandte Mathematik*, often known as *Crelles Journal* (or just *Crelle*) after its founder August L. Crelle, Lorenz discussed various problems in theories of elasticity and hydrodynamics. In a couple of later papers published in Danish he examined motions in perfect fluids, but these minor contributions to hydrodynamic theory were unrelated to optics and mainly of a mathematical character.[18] The same was the case with an original but unpublished work on the motion of an ellipsoid in a viscous fluid which Christian Christiansen, with Lorenz's permission, later reproduced in his textbook on theoretical physics.[19] Lorenz's hydrodynamic calculations were an orgy of advanced mathematics but with no obvious physical applications.

Based on his general equations of elasticity, Lorenz's paper in *Crelles Journal* dealt with a few physical phenomena, such as optical diffraction and the characteristic sounds produced by air moving in a pipe.[20] The latter phenomenon had been investigated experimentally and Lorenz found that his theory agreed reasonably well with the experimental data. Eleven years earlier Hermann von Helmholtz had published a paper in *Crelles Journal* on the motion of air in an organ pipe, but Lorenz did not cite this important contribution to mathematical physics.[21] In relation to the first phenomenon, diffraction, he presented what was arguably a mathematical proof of the fundamental Huygens principle in wave theory, possibly the first proof of its kind.[22] "We are thus able to resolve," Lorenz stated, "a problem which so far has been solved only inexactly and incompletely by means of Huygens's principle."

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18. Lorenz (1866) and Lorenz (1874).

19. According to Christiansen (1887-1889), preface, "State Councillor Dr. L. Lorenz has kindly allowed me to reproduce his highly elegant solution." The elegant solution appeared on pp. 194-202. See also Pihl (1980).

20. Lorenz (1861a), reviewed and summarised by the mathematician Siegfried Aronhold in *Fortschritte der Physik* **17** (1863): 106-113.

21. Darrigol (2005), p. 148.

22. According to Pihl (1939), pp. 107-110. The full analytical explanation of Huygens's principle is usually ascribed to Kirchhoff in a paper of 1882. See Darrigol (2012), pp. 276-280.

Of greater interest still, Lorenz introduced the concept of retarded action if at the time only in a mathematical and abstract form. Lorenz called attention to the integral

$$P = \int \frac{\varphi(t - r/a)}{r} d\omega$$

where a is a constant and t the time parameter; $d\omega$ is the space element $d\omega = d\alpha d\beta d\gamma$ and $\varphi = (\alpha,\beta,\gamma,t)$ is a function. The quantity r denotes the distance between the space points (x,y,z) and (α,β,γ),

$$r = \sqrt{(x-\alpha)^2 + (y-\beta)^2 + (z-\gamma)^2}$$

If the quantity φ is interpreted as an electrical charge, we have in this integral what later became known as the retarded scalar potential. However, in Lorenz's work of 1861 it only appeared as a mathematical quantity stripped of physical interpretation. Whatever its interpretation, Lorenz pointed out that P satisfied the d'Alembert differential equation, writing it as

$$a^2\nabla^2 P - \frac{\partial^2 P}{\partial t^2} = -4\pi a^2 \varphi$$

(Lorenz used the symbol Δ^2 for the Laplace operator).While in 1861 the equations were only mathematical curiosities within the tradition of the elastic theory of light, six years later they were to occupy an important position in Lorenz's new approach to an electrodynamic theory of light based on retarded potentials.

In a paper submitted to *Annalen* on 28 June 1861, Lorenz introduced an abstract "light vector" to describe the ether vibrations in the reflection and refraction of light, but he still phrased his work in the language of the elastic ether theory. The result of his investigation, he wrote, was this:

> As to the ether, I have not assumed that it has no absorption or lack of elasticity. … It is possible that the density of the ether is throughout constant. From this we infer that the ether's elasticity constant μ is the same in all transparent, non-crystalline bodies and in empty space (on the other hand, we know nothing about the proper com-

> pressibility constant λ). Moreover, it follows that the oscillations of the light ether are perpendicular to the plane of polarization.[23]

Within a year Lorenz changed to a more mathematical-instrumentalist formulation in which the concept of the light vector was not coupled to vibrations in the ethereal medium.

2.2 Optics based on mathematical phenomenology

As mentioned, Lorenz's first attempt to establish a theoretical basis for Fresnel's formulae and indeed for all of ray optics was founded on the well-established theory of elasticity. However, after less than two years of work within the mechanical-elastic paradigm he realised that the laws of optics could not possibly be fully deduced from the theory of elasticity. The presence of a longitudinal component followed from the elastic theory but was foreign to optical phenomena and hence had to be eliminated; this could be done only by introducing artificial assumptions. Lorenz had worked hard to deduce Fresnel's boundary conditions directly from the theory of elasticity, that is, to find the boundary conditions and the corresponding wave equation leading to them. But he had failed.

Latest by the beginning of 1862 Lorenz reached the conclusion that the fundamental theory of optics must be stripped of all mechanical features, to rest solely on abstract conceptions in agreement with the observed phenomena of optics. He made Newton's famous phrase *hypotheses non fingo* his own. The result was a dismissal of the French-inspired mechanical models of light and the adoption of a purely phenomenological theory of light which he would largely maintain throughout his life even when conceiving light as electrical oscillations.[24]

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23. Lorenz (1861b), p. 249.

24. Largely in agreement with Pihl (1939), Rosenfeld (1979) and Keller (2002) I use the term "phenomenology" for the view that knowledge claims about physical objects and theories are rooted in empirical phenomena and perhaps nothing but abstract representations of such phenomena. The term should not be confounded with the philosophical discipline known as phenomenology (due to Edmund Husserl and others) and also not with the more technical sense in which particle physicists use the

The phenomenological approach to the study of optical phenomena was not, of course, Lorenz's invention but he may have been the first to use it explicitly.[25] In mid-nineteenth century physics an approach of this kind was advocated by Kirchhoff and some other German physicists who favoured a more mathematical and positivist approach than the one followed by many of their colleagues in France and England. In the case of Lorenz, as in the case of Kirchhoff, the rejection of the Laplacian mechanical world view was more a matter of practical methodology than ontological commitment. In fact, ontology was disregarded as much as possible.

Unlike the later Ostwald-Mach-Duhem energetics school of a markedly positivist orientation, Lorenz did not deny the existence of atoms and he never expressed any interest in the energetics programme or something like it (see also Section 5.2). But then, energetics in the more strict sense only developed with Wilhelm Ostwald's publications from 1891, the year when Lorenz passed away. Lorenz's view of science was anti-metaphysical but not anti-hypothetical.[26] At the same time as he was pursuing a phenomenological program in optical theory, he was quite willing to combine it with aspects of the atomic or molecular hypothesis, as discussed in later sections. In a presentation given at the ninth meeting of Scandinavian scientists in Stockholm in 1863, Lorenz stressed that the modern optical theory had led to knowledge about the internal structure of matter and "in its consequences leads to the same result as found in chemistry."[27] The result Lorenz referred to was that material bodies consist of transparent molecules or atoms separated by vacuum.

Lorenz was emphatic in his rejection of the mechanistic approach which he associated with Laplace and Cauchy, that is, the approach based on the assumption that matter (and perhaps space too) is made up of atoms interacting through short-range central

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term as an approach including theory as well as experiment. Lorenz did not himself characterise his view as phenomenological.

[25] According to Wangerin (1909), p. 88, Lorenz was "the first who, if not from the beginning, consciously adopted a phenomenological attitude" in optical theory.

[26] On this aspect of Lorenz's philosophical attitude, see Rosenfeld (1979).

[27] Lorenz (1865a).

forces. Thus, in a popular article of 1862 he wrote that he did not try to establish a physical theory of light but, on the contrary, to describe mathematically the behaviour of light so that a theory would be unnecessary. "I have attempted ... to elevate science above a limited conception and to find the general laws that, they alone, are necessary for the mathematical deduction," he proudly declared. He went on to extoll the virtues of mathematics, which in the form of differential equations "has its own way of expressing the general laws of nature."[28]

In an important paper published in *Annalen*, Lorenz spelled out his methodological turn. "I have tried," he wrote,

> ... to develop the theory of light with the smallest possible number of hypothetical assumptions, whether in regard to the nature of light itself, to that of the luminiferous medium, or to that of material bodies; and it will appear ... that an essential part of the ordinary physical hypotheses are not needed for the explanation of the phenomena of light, inasmuch as the theory is capable of being carried through in a manner different from that which has been hitherto followed in the investigation of this subject, and consisting in the further development of the *formal* side of the theory.[29]

With regard to the nature of the molecular forces Lorenz argued that it was futile and counter-productive to speculate about such physical hypotheses since physicists would in all likelihood never know whether they were true or not: "The science of our day takes a totally different direction, and seeks to free itself from all such conceptions, which are only *ignes fatui*, and perhaps no better guides than the conceptions of Bacon's time were in their day."[30]

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[28] Lorenz (1862), p. 193 and p. 197.

[29] Lorenz (1863), quoted from p. 87 of the translation in *Philosophical Magazine* **26** (1863): 87-126 and **28** (1864): 409-425. A brief review appeared in *Fortschritte* **20** (1867): 144.

[30] Lorenz (1864a), p. 412 in the English translation in *Philosophical Magazine*. An *ignis fatuus* is an illusion or something that deludes instead of revealing the truth. To Francis Bacon, *ignes fatui* were idols that direct people from the true nature of things. In Lorenz (1865a) he used the Danish term *lygtemænd* (will-o-the-wisps) for the untrustworthy physical hypotheses.

In perfect accordance with Lorenz's methodological turn, the concept of the ether, a cornerstone in the British tradition in optics in particular, became less and less important to him. In this respect he merely followed a trend, for at the time many papers in optics largely disregarded ether dynamics and instead focussed on the wave theory's mathematical principles and their applications. The ether did indeed figure in Lorenz's works of 1862 to 1865 but now merely as a name, a somewhat unnecessary substitute for the medium of light propagation. Contrary to his colleagues in Great Britain, he was unwilling to assign to it any physical quantities or to speak of it as, for example, a perfect fluid. But it took two more years until he explicitly denied the existence of the ether.

Methodological rhetoric apart, what were the essence and main results of Lorenz's phenomenological theory of light? His aim was to base the theory solely on directly observable quantities by establishing three differential equations expressing the variation of the intensity and direction of the plane of polarization with the space and time coordinates. "From these equations," he immodestly wrote, "it should be possible to deduce all the phenomena of light."[31] To work out the ambitious programme Lorenz introduced an abstract light vector **u** signifying the ethereal vibrations propagating with a phase velocity v. In accordance with his predilection for phenomenology he paid no attention to the physical meaning of **u** and v, but merely stated that v was a function of the space coordinates. In a homogeneous medium v related to the observed intensity I by

$$v^2 I = |\mathbf{u}|^2$$

In 1863 Lorenz wrote the fundamental equations for the light vector $\mathbf{u} = (u_x, u_y, u_z)$ in the form

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[31] Lorenz (1865a), p. 230.

$$\frac{\partial}{\partial y}\left(\frac{\partial u_x}{\partial y}-\frac{\partial u_y}{\partial x}\right)-\frac{\partial}{\partial z}\left(\frac{\partial u_z}{\partial x}-\frac{\partial u_x}{\partial z}\right)=\frac{1}{v^2}\frac{\partial^2 u_x}{\partial t^2}$$

$$\frac{\partial}{\partial z}\left(\frac{\partial u_y}{\partial z}-\frac{\partial u_z}{\partial y}\right)-\frac{\partial}{\partial x}\left(\frac{\partial u_x}{\partial y}-\frac{\partial u_y}{\partial x}\right)=\frac{1}{v^2}\frac{\partial^2 u_y}{\partial t^2}$$

$$\frac{\partial}{\partial x}\left(\frac{\partial u_z}{\partial x}-\frac{\partial u_x}{\partial z}\right)-\frac{\partial}{\partial y}\left(\frac{\partial u_y}{\partial z}-\frac{\partial u_z}{\partial y}\right)=\frac{1}{v^2}\frac{\partial^2 u_z}{\partial t^2}$$

The three equations can in modern and condensed notation be written as

$$-\nabla \times (\nabla \times \mathbf{u}) = \frac{1}{v^2}\frac{\partial^2 \mathbf{u}}{\partial t^2}$$

Under the condition that there are no longitudinal waves, **u** satisfies

$$\nabla \cdot \mathbf{u} = 0$$

Lorenz noted that, since mathematically

$$\nabla^2 \mathbf{u} = \nabla(\nabla \cdot \mathbf{u}) - \nabla \times (\nabla \times \mathbf{u})$$

the optical equation was equivalent to

$$\nabla^2 \mathbf{u} = \frac{1}{v^2}\frac{\partial^2 \mathbf{u}}{\partial t^2}$$

As a consequence of and, at the same time, a justification of this equation, Lorenz deduced four boundary conditions that expressed the continuity of the tangential components of light passing between two media (1) and (2):

$$\mathbf{u}_{tan}^{(1)} = \mathbf{u}_{tan}^{(2)}$$

and

$$\left[\nabla \times \mathbf{u}^{(1)}\right]_{tan} = \left[\nabla \times \mathbf{u}^{(2)}\right]_{tan}$$

The four equations matched exactly Fresnel's boundary conditions, whereas six equations were required according to the elastic theory.

Lorenz's method of deducing the boundary conditions from a fundamental wave equation, instead of grafting them on as foreign elements, was an important advance in optical theory. However, it was not quite new since MacCullagh in a work published posthumously in 1848 had derived a wave equation similar to Lorenz's from which the boundary conditions followed.[32] On the other hand, MacCullagh derived his result from a new kind of elastic solid ether and thus, while his theory was formally equivalent to Lorenz's it was quite different from it in a physical sense. Lorenz did not refer to the work of the Irish physicist, different as it was in style and spirit from his own work. In retrospect, the wave equation found by MacCullagh and Lorenz can be seen as an anticipation of Maxwell's later electromagnetic wave equation. In Lorenz's case, the equation re-emerged in his 1867 theory of electric waves to be dealt with in detail in Section 3.2.

Lorenz had great confidence in his wave equation and general theory of light which led to Fresnel's formulae. Moreover, he emphasised its broadness in explanatory power:

> We perceive that our object - namely to deduce all the phenomena of light, which do not depend upon unknown, electrical or chemical forces, from our fundamental equations - is now attained; for the explanation of Double Refraction, of Circular Polarization, of Chromatic Dispersion, of Reflexion, and of Refraction results from them as simple consequences.[33]

To these virtues he added that the theory also promised to lead to an understanding of photo-elasticity - changes in a material's optical

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[32] On MacCullagh's theory, see Whittaker (1958), pp. 142-144 and, for a full and penetrating analysis, Darrigol (2010). According to the latter source, Lorenz's wave equation was "a legitimate anticipation of the electromagnetic equation." Keller (2003) similarly concludes that Lorenz's equation was "identical to that obtained from the Maxwell equations." See also Volkmann (1891), p. 431 for Lorenz's priority in deriving the boundary conditions.

[33] Lorenz (1864a), quoted from the English translation in *Philosophical Magazine*, p. 419.

properties under mechanical deformation - and the absorption of light in matter. Moreover, he vaguely suggested that his equations might be extended to cover thermal phenomena as well and that they thus heralded a unification of the theories of light and heat.

The "circular polarization" mentioned in the quotation is today better known as optical activity. The rotation of the plane of polarization by certain transparent crystals and liquids was discovered in the early 1810s, before the advent of the wave theory of light. The remarkable phenomenon was later studied theoretically by Fresnel, MacCullagh and other physicists on the basis of the elastic wave theory. Although Lorenz was not the first to offer an explanation of optical activity, he was satisfied that the phenomenon could be explained by his general theory without special assumptions. At the time it was realised that somehow optical activity was connected to the material's molecular structure and might perhaps be due to a helical arrangement in the molecules. However, the exact relation became clear only in 1874 with the discovery, made independently by J. H. van't Hoff and J. A. Le Bel, of the asymmetric carbon atom as the cause for stereoisomerism in organic compounds such as, for example, $CHBrCH_3C_2H_5$.

Although Lorenz understood that optical activity required a kind of symmetry breaking on the molecular level, he was unable to go further. In his 1863 paper he wrote about the phenomenon:

> In nature, circular polarization appears as an exceptional case, while in the mathematical treatment of the subject it appears as the most general case. ... Circular polarization therefore presupposes a want of symmetry, which in the calculation is the more general, in nature the rarer case.[34]

One aspect of physical optics which did not follow from Lorenz's theory was the puzzling line spectra as revealed by the spectroscope recently introduced by Kirchhoff and Robert Bunsen. The vortex theory of atoms and ether developed by William Thomson and other British physicists in the late 1860s suggested an explanation of

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[34] Lorenz (1863), English version p. 218.

the spectra produced by chemical elements, but the very foundation of the vortex theory contrasted with Lorenz's belief of what a physical theory should be like. Not only did he ignore the idea of atoms as vortex structures in the ether, he also largely ignored the new science of spectroscopy except using the spectroscope in some of his optical experiments when he needed monochromatic light of a particular wavelength.

But of course Lorenz was acquainted with spectroscopy and its use in spectral analysis, a subject he covered in his textbook in optics for students at the Military High School. "By means of spectral analysis," Lorenz wrote, "it has been possible to detect traces of different metals in chemical compounds and five new metals have been discovered by this method."[35] In another work he emphasised that the spectral lines were characteristics of a particular substance and unaffected by its temperature and density, which he saw as evidence for the immutable nature of atoms (or "molecules" as he generally preferred to call the building stones of matter).[36] While some scientists in the period associated the numerous spectral lines with the possibility of atoms being composite bodies, Lorenz never entertained this kind of fruitful speculations.

This digression apart, the range of Lorenz's phenomenological light theory was impressive. On the other hand, while the theory could explain almost all known optical phenomena (if sometimes only in principle), its explanatory power was not matched by a corresponding predictive power. Indeed, the theory did not result in any novel predictions. As Lorenz further pointed out in his 1863 *Annalen* paper, since his fundamental formulae were "deduced solely from experiment," then "equal weight must be given to any other formulae which lead to equally concordant results." In other words, experimental data did not lead to a unique set of equations and consequently his theory could not be uniquely verified or falsified by comparing its consequences with experiments. In philosophy of

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[35] Lorenz (1876), pp. 171-183. The metals identified by means of spectroscopy were cesium, rubidium, thallium, indium and gallium.

[36] Lorenz (1875a), p. 494. Like many contemporary scientists, Lorenz did not distinguish sharply between the terms "atom" and "molecule."

science this problem is related to what is called the Duhem-Quine thesis, so named after Pierre Duhem and Willard Quine. It is also known as the problem of empirical underdetermination. Any phenomenon can be explained by many different hypotheses or sets of equations, and so empirical data alone are insufficient to prove or disprove a theory. However, the underdetermination thesis is controversial and philosophers disagree about its meaning and validity.[37]

2.3 Studies in refraction and dispersion

In 1866, slightly before he was elected a member of the Royal Danish Academy, Lorenz received a grant of 200 Rd (rix-dollars) from the Academy to construct "an apparatus for the determination of the refractive index of water at various temperatures, independent of colour dispersion." Three years later he was awarded 150 Rd to an "apparatus for experiments on the refractive index, colour dispersion and density of water and vapours of other liquids."[38] Lorenz had at the time become seriously interested in comparing precise measurements of the refractive index of transparent bodies and the predictions of his optical theory. As he wrote in his first paper on the subject, "The absence of a theory susceptible of demonstrating the relationship between the refractive index of substances and their molecular state is much to be regretted."[39]

Between 1869 and 1880 Lorenz published three important papers on the subject, which he had previously considered in his communication to the 1863 meeting of Scandinavian scientists. Referring to tables of the refractive index n of different substances he suggested on this occasion that n always decreased with the density d and that this was "a rule which has not previously been noticed." Lorenz argued that the rule could be explained from his optical

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[37] The underdetermination thesis can be exemplified by the different nineteenth-century theories of electrodynamics, including Lorenz's in relation to Weber's and Maxwell's. On this issue, see Pietsch (2012).

[38] Lorenz Papers (RAS). See also Lomholt (1942-1973), vol. 2, p. 352.

[39] Lorenz (1869b), p. 206.

theory based on the assumption that the substances consisted of transparent molecules separated by empty space; whereas the speed of light remained constant in the intermolecular vacuum regions ($v = c$), it was allowed to vary inside the molecules ($v < c$). Based on such assumptions he suggested a rule of the form

$$n^2 - 1 \sim d$$

He realised that the rule was only a rough approximation, but "to find the exact law would be very complicated."[40] Contrary to what Lorenz thought, the relationship had been noticed much earlier, first by Newton in the revised edition of *Opticks* from 1718. Based on experiments with a wide variety of substances the celebrated natural philosopher suggested a "refractive power" of the form mentioned by Lorenz (Fig. 2.1). Almost a century later Laplace, in his famous *Mécanique Céleste*, derived on the basis of the corpuscular theory of light that

$$\frac{n^2 - 1}{d} = \text{constant}$$

Laplace emphasised that the constant should be independent of pressure and temperature. Although the Newton-Laplace rule agreed well with experiments for gases, it soon turned out that it failed for most liquids and solid bodies.

At about 1860 experiments suggested that there was a definite relationship between a body's refractive index and its density but not the exact form of the relationship. According to the British scientists John Hall Gladstone and Thomas Dale, the two quantities were related by the rule

$$(n - 1)v = (n - 1)\frac{1}{d} = \text{constant}\,,$$

The quantity $v = 1/d$ is known as the body's specific volume. This relation was widely used for analyses of solutions, glasses and crystals, and determinations of the "Gladstone-Dale constant" are still

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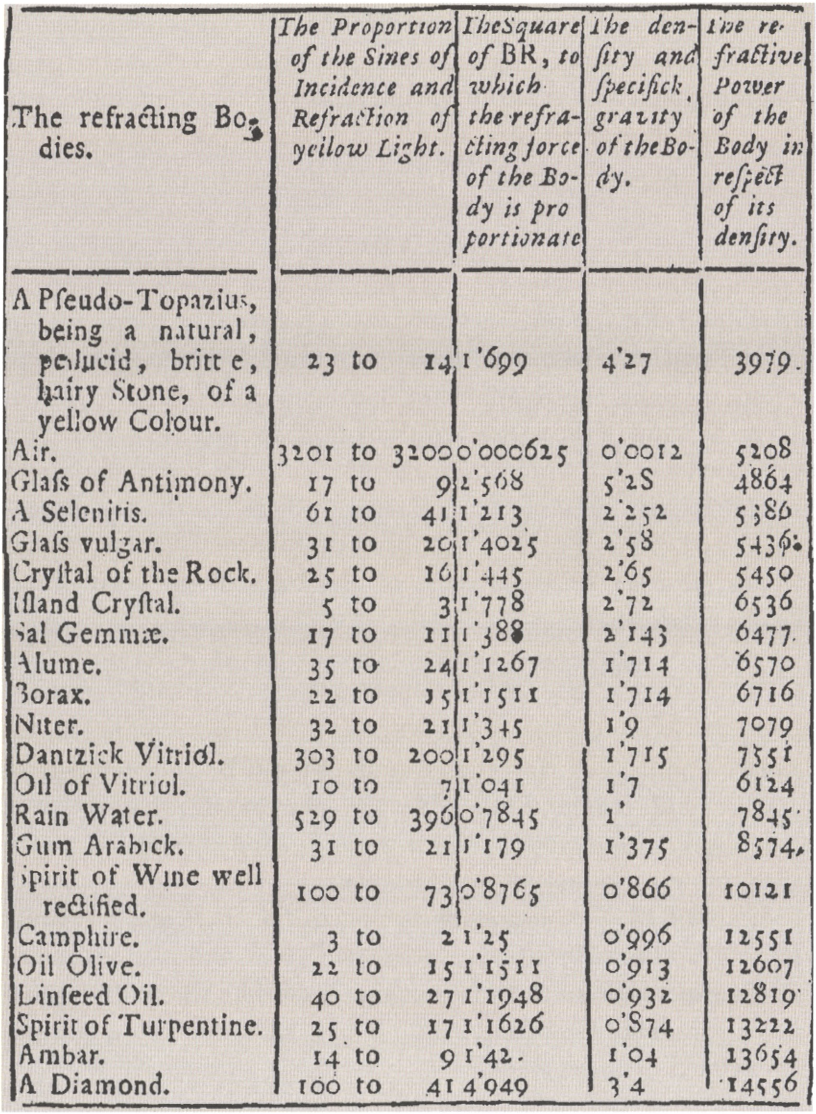
[40] Lorenz (1865a).



The refracting Bodies.	The Proportion of the Sines of Incidence and Refraction of yellow Light.	The Square of BR, to which the refracting force of the Body is proportionate	The denſity and ſpecifick gravity of the Body.	The refractive Power of the Body in reſpect of its denſity.
A Pſeudo-Topazius, being a natural, pellucid, brittle, hairy Stone, of a yellow Colour.	23 to 14	1'699	4'27	3979.
Air.	3201 to 3200	0'000625	0'0012	5208
Glaſs of Antimony.	17 to 9	2'568	5'28	4864
A Selenitis.	61 to 41	1'213	2'252	5386
Glaſs vulgar.	31 to 20	1'4025	2'58	5436.
Cryſtal of the Rock.	25 to 16	1'445	2'65	5450
Iſland Cryſtal.	5 to 3	1'778	2'72	6536
Sal Gemmæ.	17 to 11	1'388	2'143	6477.
Alume.	35 to 24	1'1267	1'714	6570
Borax.	22 to 15	1'1511	1'714	6716
Niter.	32 to 21	1'345	1'9	7079
Dantzick Vitriol.	303 to 200	1'295	1'715	7551
Oil of Vitriol.	10 to 7	1'041	1'7	6124
Rain Water.	529 to 396	0'7845	1'	7845.
Gum Arabick.	31 to 21	1'179	1'375	8574.
Spirit of Wine well rectified.	100 to 73	0'8765	0'866	10121
Camphire.	3 to 2	1'25	0'996	12551
Oil Olive.	22 to 15	1'1511	0'913	12607
Linſeed Oil.	40 to 27	1'1948	0'932	12819
Spirit of Turpentine.	25 to 17	1'1626	0'874	13222
Ambar.	14 to 9	1'42.	1'04	13654
A Diamond.	100 to 41	4'949	3'4	14556

Figure 2.1: Newton's attempt of 1718 to establish an approximate relationship between refractivity and density for a variety of substances. Columns 2 and 3 give the refractive index n and the quantity $(n^2 - 1)$, respectively; column 4 gives the density d relative to water, and in column 5 Newton lists the values of $(n^2 - 1)/d$.

part of modern materials science. However, the Gladstone-Dale rule and other rules proposed in the period were basically empirical relations lacking the theoretical foundation that Lorenz and many other physicists aimed at.[41] Moreover, the Gladstone-Dale constant was not a characteristic parameter of the refractive substance as it varied somewhat with its physical state.

In a lengthy memoir published by the Royal Danish Academy in 1869, Lorenz obtained better data and the required theoretical justification.

Working at the Military High School Lorenz relied in his experiments on assistance by other Danish physicists and chemists. One of them was Haldor Topsøe, a young chemist who was given the task of purifying the substances used for the refractivity measurements. Topsøe served at the time as an assistant at the Polytechnic College under the chemistry professor Julius Thomsen and in 1876 he became Lorenz's colleague at the Military Academy, a step toward a later career in pure and industrial chemistry. From his elaborate experiments in 1869 Lorenz established a number of empirical formulae, to be illustrated by his result based on measuring the refractive index for the yellow sodium light passing water at different temperatures t. In the interval between 0 °C and 30 °C he found that

$$\frac{dn(t)}{dt} = [0.076 - 5.606t + 0.0640t^2] \times 10^{-6}$$

or, in integrated form,

$$n(t) = n(0) + [0.076t - 2.803t^2 + 0.002134t^3] \times 10^{-6}$$

Thus, at a change in temperature of 10°C the observed change in refractivity was found to be of the order of only 0.01 per cent. Measurements of this kind had earlier been reported by Jamin in 1856, but Lorenz's data were more precise and in better agreement with later data. For his experimental method, see Fig. 2.2.

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[41] Gladstone and Dale (1864). See Partington (1953), pp. 6-12 for details and references.

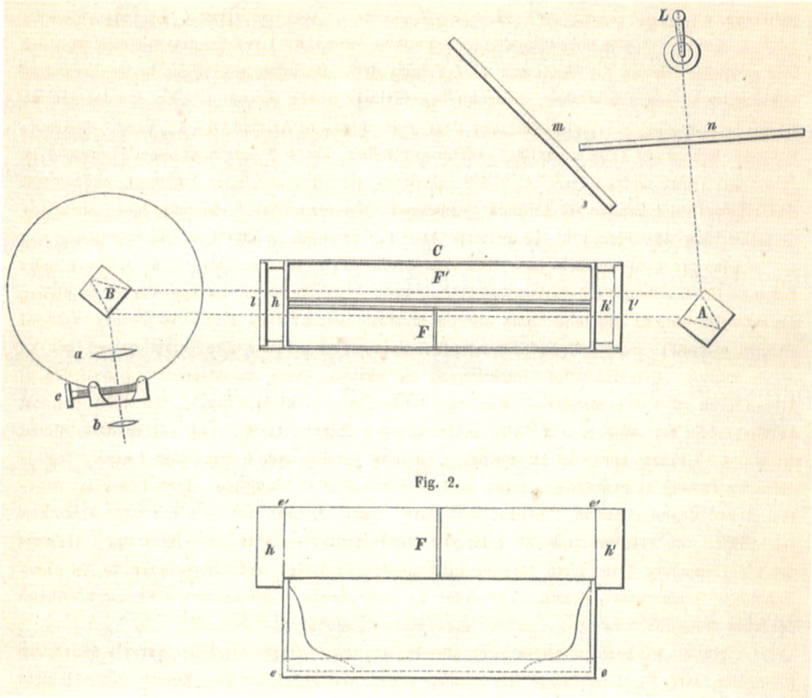

Figure 2.2: Lorenz's apparatus for the determination of the relationship between the refractivity and density of liquids. In the tank C a thin tube with the liquid is enclosed between two mirror glasses l and l'. The two parts of the tank F and F' and the two small containers h and h' are filled with distilled water. The tank is mounted between two Jamin mirrors B and A formed as cubes. One of the light rays passes the tube while the other ray passes the water in the tank with the result that the interference lines are displaced. By measuring the number of displaced lines and the weight of the liquid Lorenz could relate the refractivity of the liquid to its density. Source: Lorenz (1869b), p. 211.

The refractive index depends on the wavelength and according to Cauchy's semi-empirical dispersion formula of 1836 the dependency can be represented as

$$n(\lambda) = m + \frac{a_1}{\lambda^2} + \frac{a_2}{\lambda^4} + \frac{a_3}{\lambda^6} \dots$$

The quantity m thus denotes the refractive index reduced to an infinite wavelength or zero frequency, $n(\lambda) \to m$ for $\lambda \to \infty$. If only the two first terms on the right hand are used, we have

$$n(\lambda) = m + \frac{a_1}{\lambda^2}$$

Then m can be calculated from measurements of two values of n corresponding to two wavelengths λ_1 and λ_2 with the result that

$$m = \frac{\lambda_1^2 n_1 - \lambda_2^2 n_2}{\lambda_1^2 - \lambda_2^2}$$

However, Cauchy's formula did not really explain dispersion as the a_n quantities were unknown and assumed to depend on the masses of the ether molecules and the force between them. Lorenz introduced in his 1869 paper the notion of a "dispersion curve," with which term he referred to a graph where the refractive index was plotted against the inverse of the square of the wavelength, $n = n\ (\lambda^{-2})$. This curve must always be convex, he argued. Based on this conclusion he considered an early dispersion formula based on Cauchy's dispersion theory which E. Christoffel had proposed in a paper of 1862.[42] According to Christoffel's formula, well known at the time, the $n\ (\lambda)$ function was given by

$$n = \frac{n_0\sqrt{2}}{\sqrt{1 + \frac{\lambda_0}{\lambda}} + \sqrt{1 - \frac{\lambda_0}{\lambda}}},$$

where n_0 and λ_0 are two positive constants. Lorenz proved that the corresponding dispersion curve was concave, from which he inferred that Christoffel's formula was wrong and did not deserve further attention.

Having discussed his own data and those reported by other scientists such as Adolph Wüllner, Hans Heinrich Landolt and Joseph Fraunhofer, Lorenz concluded that m only depends on the density and that the temperature merely enters indirectly, namely by chang-

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[42] Christoffel (1862). Recall that Christoffel in the same year sharply criticised Lorenz's optical theory (Section 2.1).

ing the volume and hence the density. He ended up with the following expression for water:

$$m(t) = 1.3219 + [21.05t - 2.759t^2 + 0.02134t^3] \times 10^{-6}$$

Although Lorenz's experimental work was of unsurpassed precision, it did not differ essentially from similar measurements made in German and French laboratories. What distinguished his work from investigations made elsewhere was its connection to theory, which he covered in the second part of his treatise.

Proceeding from his fundamental wave equation Lorenz deduced in 1869 that the quantity $(m^2 - 1)v/(m^2 + 2)$ was given by a certain function that only depended on the distribution in space of the refractive substance.[43] Since it was known from the Gladstone-Dale rule that $(m - 1)v$ was approximately constant, Lorenz concluded that the correct law of refractivity was given by what he called the "refraction constant," namely

$$\frac{m^2 - 1}{m^2 + 2} v = \text{constant } (= R_{\text{LL}})$$

For reasons of simplicity he assumed the refractive medium to be composed of optically homogeneous spherical molecules with m_i being their internal refractive index. He stated that "The assumptions that the molecules are spherical and that $[m_i]$ is constant are made only to facilitate the calculation." With v_i being the specific proper volume of the molecules Lorenz could then write the law as

$$\frac{m^2 - 1}{m^2 + 2} v = \frac{m_i^2 - 1}{m_i^2 + 2} v_i$$

He further argued that the reduced refractive index was approximately constant and for a mixture consisting of k components could be expressed as

$$\frac{m^2 - 1}{m^2 + 2} v = \sum_{j=1}^{k} \frac{m_j^2 - 1}{m_j^2 + 2} v_j$$

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[43] See Keller (2002), pp. 273-280 for details on Lorenz's derivations.

The observation turned out to have significant consequences for chemical investigations. For an isotropic substance consisting of only one kind of molecule he deduced the approximate relation

$$\frac{m^2-1}{m^2+2}v = P\left(1-\frac{k^2}{v^2}\right)$$

Here P and k are two constants that depend on the molecular structure of the substance but not on its volume or temperature. Lorenz admitted that his formulae depended on certain hypotheses concerning the spatial arrangement of the molecules. To make his theory "more intuitive" he assumed that the molecules were placed in the corners of cubes of equal size.

For a gas, where v is large and m only slightly larger than 1,

$$m^2-1 \cong 2(m-1) \text{ and } m^2+2 \cong 3$$

Lorenz noted that the expression above approximates to

$$(n-1)v = \frac{3}{2}P$$

in agreement with the Gladstone-Dale formula. Moreover, the Lorenz expression also accommodates the Newton-Laplace rule since

$$\frac{n^2-1}{d} = R_{\mathrm{LL}}(n^2+2) \cong 3R_{\mathrm{LL}}$$

Only after a period of six years did Lorenz return to his studies of refraction, this time in a predominantly experimental paper where he reported precision measurements on oxygen, hydrogen, water vapour, ethanol, ether and other volatile liquids.[44] In his 1875 paper Lorenz also worked out refraction formulae for any wavelength. Adopting the same model he applied in 1869, that is, a substance composed of spherical and optical homogeneous molecules, he obtained

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[44] Lorenz (1875a).

$$\frac{n^2-1}{n^2+2}v = \frac{n_i^2-1}{n_i^2+2}v_i(1+\delta),$$

With β being a measure of the molecular radius, he stated the δ quantity as

$$\delta = \frac{16}{5}\pi^2\frac{n_i^2-1}{n_i^2+2}\frac{\beta^2}{\lambda^2}$$

Lorenz used this result for two purposes. Firstly, he pointed out that since $\delta = \delta(\lambda^{-2})$, the expression explained dispersion, if only qualitatively, without relying on special assumptions about molecular forces or the structure of the ether. This stood in contrast to Cauchy's earlier theory of dispersion which relied on such assumptions and also was unable to explain why dispersion does not take place in void space. In Lorenz's very different theory, dispersion was a property of the heterogeneity of a substance and thus excluded dispersion in a vacuum. This he had pointed out as early as 1863, when he wrote about his optical theory that, "chromatic dispersion appears on this theory as a property of material bodies, essentially dependent on their periodic heterogeneity, whereas, on Cauchy's theory, the absence of chromatic dispersion in a vacuum can only be explained by new hypotheses."[45] The second use Lorenz made of his result was to estimate a lower limit to the size of molecules, a subject which will be considered in Section 5.2.

Lorenz's refractivity-density law, derived as a theoretical consequence of his theory of light, received solid confirmation in 1880, when Peter Kristian Prytz, a 29-year-old university-trained physicist, published extensive measurements on the refractive constants of a variety of liquids and vapours. His experiments were undertaken on the instigation of Lorenz, who also supplied Prytz with the necessary optical apparatus and arranged economic support from the Royal Danish Academy of Science. In 1878 Lorenz strongly recommended Prytz's request to the Academy. The project, he wrote,

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[45] Lorenz (1863), p. 134.

> … is concerned with the determination of the refraction and chromatic dispersion of vapours by means of the method I have used in earlier works. I obviously consider such determinations to be of scientific importance; and so do other scientists, such as shown by Mascart's recent investigations on the same subject albeit these make use of a quite different experimental method.[46]

Prytz's measurements showed convincingly that Lorenz's law was superior to the Gladstone-Dale rule. "The result of my experiments is a further confirmation of the view concerning the refractive constant to which Lorenz has been led by theoretical considerations and which his own experiments have verified," Prytz wrote.[47] The paper was rewarded with a silver medal by the Royal Danish Academy. Lorenz, together with his physics colleagues C. Holten and C. Christiansen, concluded that "the work is of considerable scientific importance for it is only in this way … that the law that connects the refraction of light and the density of bodies can be established."[48] In 1887 Prytz followed Lorenz as physics teacher at the Military Academy and seven years later he was appointed professor at the Polytechnic College.

Prytz's 1880 paper in *Annalen der Physik und Chemie* was preceded by a paper in which Lorenz presented a detailed summary of his two communications on optical refraction originally published in two sequels in the transactions of the Royal Danish Academy.[49] It was only on this occasion that the international community of physicists became aware of his extensive work on the refractivity-density law. Since his memoirs of 1869 and 1875 were written in Danish, they were known only by scientists in Scandinavia.

In the *Annalen* paper Lorenz based his theory on the physical assumption that "The only really homogeneous medium is the 'free' space; on the other hand, all bodies of a molecular constitution only

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[46] Lorenz to the Royal Academy, 30 October 1878; Prytz to Lorenz, 23 September 1879 (Lorenz Papers, RAS).

[47] Prytz (1880).

[48] Letter of 15 April 1880 (Lorenz Papers, RAS). Lomholt (1942-1973), vol. 1, p. 81.

[49] Lorenz (1880).

appear to be homogeneous, in the sense that the function ω which can be considered an expression for the speed of light in any point of the body, enters as a rapidly periodic function of the coordinates x, y and z in the interior of the homogeneous body."[50] He assumed that the disturbance given by the light vector **u** could be written as

$$\mathbf{u} = (\mathbf{u}_0 + \mathbf{u}_2)C + \mathbf{u}_1 S\,,$$

where the functions C and S stand for

$$C = \cos(kt - lx - my - nz - \beta)$$

$$S = \sin(kt - lx - my - nz - \beta)$$

The three constants $\mathbf{u}_1$, $\mathbf{u}_2$ and β were chosen in such a way that the average values of $\mathbf{u}_1$ and $\mathbf{u}_2$ over a space containing a large number of molecules were zero. The new and simpler approach led to the same expression for the relation between refractivity and density as in his earlier theory, namely a constant value of the ratio $(n^2 - 1)/d(n^2 + 2)$.

Lorenz's refraction law is today referred to as the Lorenz-Lorentz law, or more commonly the Lorentz-Lorenz law, because the later so famous Dutch physicist Hendrik Antoon Lorentz derived the same result in 1878. Just the year before, he had been appointed professor of theoretical physics at the University of Leiden, at the tender age of 24. Contrary to the Danish physicist, Lorentz obtained the result by combining the Clausius-Mossotti formula with the electromagnetic theory of light, although not Maxwell's theory but an alternative action-at-a-distance theory proposed by Helmholtz.[51] Lorentz appreciated Maxwell's field theory but at the time he thought that it depended too much on unconfirmed hypotheses.

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[50] Characteristically, when Glazebrook (1885, p. 182) reviewed Lorenz's paper he translated Lorenz's "free space" as "ether."

[51] Lorentz (1880). For an English translation of his 1878 memoir, see Lorentz (1936), pp. 1-119. See Carazza and Robotti (1996) for a historical perspective on the Lorentz theory and its associated molecular model, and Hirosige (1969) for its foundation in Helmholtz's electrodynamics.

Yet he accepted without any doubt what he called the main principle of Maxwell's theory, namely "the hypothesis that vibrations of light are movements of the same character as electric currents."[52]

What is known as the Clausius-Mossotti formula was first proposed, if only implicitly, by the Italian physicist Ottaviano Mossotti in 1847 and subsequently by Clausius in 1879 in an attempt to explain the dielectric properties of insulators on an atomistic basis. From a historical point of view the order of names is of course unfortunate, but Mossotti-Clausius is rarely used. With ε_r the material's dielectric constant (or relative permittivity $\varepsilon/\varepsilon_0$) and α the polarizability of the molecule, what may be called the Clausius-Mossotti-Lorentz formula for a unit volume with N molecules is

$$\frac{\varepsilon_r - 1}{\varepsilon_r + 2} = \frac{4\pi}{3} N\alpha$$

In modern literature this expression is often used synonymously for the Lorenz-Lorentz formula. Modern physicists occasionally use the rather cumbersome name Clausius-Mossotti-Lorenz-Lorentz formula, abbreviated CMLL.

Interestingly, in unpublished notes the Danish Lorenz proved something similar, namely that the law of refractivity could be derived on the basis of a version of the electromagnetic theory of light.[53] Contrary to his Danish near-namesake, Lorentz considered a molecular or atomic model in connection with his theory, namely that a molecule consists of an electric charge harmonically bound to the rest of the molecule and characterised by its electric polarizability. Also contrary to the Danish physicist, Lorentz pictured material molecules as being embedded in an all-pervading ether which he regarded as a dielectric substance. He emphasised the necessity of assuming inter-molecular space being filled with ether. Lorentz ended up with the following expression:

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[52] Lorentz (1936), p. 1. The hypothesis was literally the same as the one Lorenz proposed in his theory of 1867.

[53] See Section 3.2 and Pihl (1939), pp. 59-61, which gives a summary of Lorenz's calculations.

$$\frac{n^2-1}{(n^2+2)d}=\frac{\frac{4\pi}{3}\rho^3(3+4\pi\varepsilon_0)-4\pi\varepsilon_0\frac{\rho^3}{\kappa}}{M(3+8\pi\varepsilon_0)\frac{\rho^3}{\kappa}-8\pi\varepsilon_0}$$

Here ε_0 denotes the dielectric constant of the ether, M is the mass of a molecule, d the density of the body, and κ is the ether's specific resistance according to Helmholtz's theory. The quantity on the right side of the equation is thus a constant for a particular transparent body. In the last part of his extensive 1878 memoir Lorentz compared his theoretical refraction law with available experimental data from the literature. However, unlike Lorenz he did not perform experiments of his own.

Since Lorentz originally published his paper in Dutch, and Lorenz published his in Danish, the Lorenz-Lorentz formula became generally known only when abridged and revised versions of their papers appeared in German in 1880. Both papers were published in *Annalen* but in two different issues and with Lorentz's as the first.[54] Apparently the two authors were at the time unaware of each other's work. In the case of Lorenz, he summarised and discussed the two Danish articles he had written in 1869 and 1875 whereas Lorentz's German paper was a substantially reduced and revised version of his 1878 memoir published in Dutch in the proceedings of the Amsterdam Academy.

When receiving the physics Nobel Prize in 1902, sharing it with his compatriot Pieter Zeeman, Lorentz referred to the theoretical discovery made more than twenty years earlier: "When I drew up these formulae I did not know that Lorenz at Copenhagen had arrived at exactly the same result, even though he started from different viewpoints, independent of the electromagnetic theory of light. The equation has therefore often been referred to as the formula of Lorenz and Lorentz."[55] Four years later, in a series of lectures deliv-

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[54] Gustav Wiedemann, the editor of *Annalen*, had originally planned to have the two papers published consecutively, but for some reason this did not happen. Wiedemann to Lorenz, 7 May 1880 (Lorenz Papers, DTM).

[55] Lorentz, "The theory of electrons and the propagation of light." Online as https://www.nobelprize.org/nobel_prizes/physics/laureates/1902/lorentz-lecture.html.

ered at Columbia University in New York he called the double discovery "a curious case of coincidence."[56] The Danish Lorenz never referred to the work of the Dutch Lorentz.

Since Lorenz came to his result as early as 1869, the curious coincidence does not constitute a case of a truly simultaneous discovery. Although Robert Merton and other sociologists of science have long ago noted that simultaneous and independent discoveries - what are sometimes called "multiples" - are frequent in the history of science, the case of Lorenz and Lorentz does not belong to the category.[57] As far as priority is concerned, the law should undoubtedly be called the Lorenz law, possibly the Lorenz-Lorentz law but not the Lorentz-Lorenz law.[58]

The order Lorenz-Lorentz can be found in the literature in the 1890s, but with the rising fame of the Dutch physicist the order was soon reversed. In an obituary article on Lorentz, Max Planck referred to the formula relating refractivity and density, "which by accident had been established at the same time by his namesake, the Danish physicist Ludvig Valentin Lorenz, and for this reason has been assigned the curious double name Lorentz-Lorenz."[59] Much later we find the same usage in the authoritative textbook on optics written by Max Born and Emil Wolf: "By a remarkable coincidence, the relation was discovered independently and practically at the same time by two scientists of almost identical names, Lorentz and Lorenz, and is accordingly called the Lorentz-Lorenz formula."[60]

In its modern formulation the Lorenz-Lorentz law is stated as a

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[56] Lorentz (1909), p. 145, and similarly in Schuster (1909), p. 284.

[57] Merton (1973), pp. 343-370. In an early study Ogburn and Thomas (1922) listed 148 simultaneous discoveries and inventions, one of them being the Lorenz-Lorentz law.

[58] The case for "Lorenz-Lorentz" instead of "Lorentz-Lorenz" was briefly argued in Sihvola (1991).

[59] Planck (1928), p. 551, and also in Schuster (1909), p. 284.

[60] Born and Wolf (1970), fourth edition, p. 87. The book contains many references to Lorenz, in all cases related to the Lorenz-Lorentz formula. Google Scholar gives ca. 3,770 results for "Lorenz-Lorentz" and ca. 15,100 for "Lorentz-Lorenz." See Table 2.2. According to the Web of Science, the corresponding number for papers with the terms in the title is 7 and 90.

relation between the refractive index of a substance, a macroscopic quantity, and its polarizability α, a microscopic quantity:

$$\frac{n^2-1}{n^2+2}=\frac{4\pi}{3}N\alpha,$$

When the polarizability is small, the equation reduces to

$$n^2-1 \cong 4\pi N\alpha \quad \text{or} \quad n-1 \cong 2\pi N\alpha$$

In agreement with the Gladstone-Dale formula, this expression is valid for gases at normal pressure. It follows from the Lorenz-Lorentz theory that the polarization of a molecule in a solid body under the influence of an external electric field is not only determined by the strength of the field and the number of molecules per volume. There is also an effect due to the polarized neighbour molecules which produce an additional force. This force was in the earlier literature sometimes called the "Lorentz-Lorenz force," a name which should not be confused with the well-known Lorentz force $q\boldsymbol{v} \times \boldsymbol{B}$ acting on an electrical charge moving in a magnetic field.[61]

After the refraction studies of Lorenz and Lorentz had become widely known they spurred a large number of further experiments and theoretical investigations. While Lorenz's theory presupposed transparent substances, in 1904 the Leipzig physicist Ferdinand Kirchner showed in his doctoral dissertation that a slightly modified form of the Lorenz-Lorentz formula was valid also for metallic emulsions.[62] To incorporate absorption into Lorenz's theory he made use of the refractive index in the complex form

$$n' = n(1-ik)$$

Here the real part n is the ordinary refractive index and the quantity k in the imaginary part is a coefficient of extinction or absorption.

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[61] *Handbuch der Physik*, vol. 20 (Berlin: J. Springer, 1928), pp. 503-505.

[62] Kirchner (1904). The idea of a complex refractive index was originally introduced by Cauchy in the context of his 1836 theory of dispersion. See Whittaker (1958), pp. 162-166.

Kirchner referred to Lorenz's *Annalen* paper of 1880 but not to the simultaneous paper of Lorentz, and he used the term Lorenz-Lorentz rather than the reverse combination.

As a result of numerous experiments on molecular refractivity under various conditions it soon turned out that the Lorenz-Lorentz law agreed far better with experimental data than competing formulae. The subject of molecular refractivity belonged as much to chemistry as to physics and became an important tool in the new discipline of physical chemistry that emerged during the 1880s.[63] When Lorenz figures in books on the history of chemistry, and not only in those on the history of physics, it is solely because of his role in the Lorenz-Lorentz formula.

The leading Swiss chemist Hans Heinrich Landolt and his German colleagues Wilhelm Ostwald and Julius Wilhelm Brühl were among those who applied Lorenz's formula to calculate the so-called molecular refractivity or refractive power of a particular substance. They defined the molecular refractivity as the product of the specific refractivity R_{LL} and the molecular weight M, that is, with n determined at a particular wavelength,

$$MR_{\mathrm{LL}} = \frac{n^2 - 1}{n^2 + 2}\frac{M}{d}$$

It turned out that in many cases Lorenz's summation rule for mixtures could be carried over to chemical compounds. If a compound consists of $q_1, q_2, \ldots$ elements with atomic weights $\mu_1, \mu_2, \ldots$, then the molecular weight is

$$M = q_1\mu_1 + q_2\mu_2 + \cdots.$$

According to the summation rule the molecular refractivity $r = R_{\mathrm{LL}}$ is simply the weighted sum of the individual atomic refractivities given by

$$r_i = \frac{n_i^2 - 1}{n_i^2 + 2}\frac{\mu_i}{d_i}$$

---

[63] Muir (1884), p. 291; Partington (1953), pp. 78-101.

It follows that

$$\frac{n^2-1}{n^2+2}\frac{M}{d}=q_1\mu_1r_1+q_2\mu_2r_2+\cdots$$

However, experiments showed that although the rule was approximately correct for many compounds it was not universally true. In several cases the molecular refractivity differed substantially from the sum of the constituent atomic refractivities, thus indicating that the first quantity is influenced by the constitution of the molecule as given by the arrangement of atoms and the presence of double and triple bonds.

By applying the Lorenz-Lorentz formula and related optical methods the chemists obtained valuable information concerning the bonds in chemical compounds, their constitution, and the difference between isomeric compounds.[64] For example, molecular refractivity studies were used to clarify the vexed and much-discussed question of the constitution of benzene with the stoichiometric formula C_6H_6. According to Brühl, A. Kekulé's model with three single and three double bonds agreed well with benzene's molecular refractivity, whereas alternative formulae suggested by H. Armstrong, J. Thomsen and others did not. Brühl's conclusion led to a dispute with Thomsen in Copenhagen who from reasons of thermochemistry believed that the Kekulé model was wrong.[65]

Although Lorenz was trained as a chemist, he took little interest in these chemical aspects derived from his work. On the other hand, at the end of his 1880 paper he dealt with a number of chemical reactions during which the refractivity constant changed, from which he suggested that the change in refractivity might constitute a measure of the chemical affinity. Lorenz suspected that exothermic processes were generally followed by a decrease in refractivity and endothermic processes by an increase. However, he admitted that the case of ammonia

$$N_2+3H_2\rightarrow 2NH_3+Q$$

---

[64] See the detailed review in Smiles (1910).

[65] For the dispute between Thomsen and Brühl, see Kragh (2016), pp. 198-201.

was an exception to the rule. The molecular refractivity of NH_3 was known to be 0.3266 and Lorenz's measurements of a mixture of N_2 and H_2 in the mass ratio $\mu_N{:}3\mu_H = 14{:}3$ resulted in 0.3116.

The Lorenz-Lorentz law was also used to obtain information about the size of molecules and the range of the unknown molecular forces. Franz Exner, a physicist at the University of Vienna, showed in 1885 that the law could be used to measure the magnitudes of molecules and that it was more reliable than methods based on condensation and diffusion experiments.[66] For the diameter of gas molecules he suggested the formula

$$D = C\frac{n^2 - 1}{n^2 + 2}\ell ,$$

where C is an empirical constant and ℓ the mean free path of the molecules. Exner found $D \sim 10^{-8}$ cm for simple molecules such as H_2O and CO_2.

In a review paper of 1888 the British physicist Arthur William Rücker referred to the works of L. Lorenz and H. A. Lorentz as well as to Prytz's experimental confirmation of the law named after them. Rücker found it of particular interest that the measurements of Lorenz and Prytz indicated that the value of $(n^2 - 1)/d(n^2 + 2)$ did not depend on whether the substance was in a liquid or a vaporous state (see Table 2.1). Having reviewed the experimental data Rücker concluded that "The results, on the whole, confirm the accuracy of the physical meaning of the expression $(n^2 - 1)/(n^2 + 2)$, and tend to show that the diameter of the molecule is the same in the liquid and gaseous state."[67]

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[66] Exner (1885).

[67] Rücker (1888), p. 252.

Substance	*Formula*	*Work*	*Liquid*	*Vapour*
Ethyl ether	$(C_2H_5)_2O$	Lorenz (1875)	0.30264	0.3068
Ethanol	C_2H_5OH	Lorenz (1875)	0.28042	0.2825
Water	H_2O	Lorenz (1875)	0.20615	0.2068
Methanol	CH_3OH	Prytz (1880)	0.2567	0.2559
Methyl acetate	$(CH_3)_2COO$	Prytz (1880)	0.2375	0.2399
Ethyl formate	C_2H_5COOH	Prytz (1880)	0.2437	0.2419

Table 2.1. Data for the quantity $(n^2 - 1)/d(n^2 + 2)$ obtained by Lorenz and Prytz. Source: Rücker (1888).

Lorenz had considered chromatic dispersion in his works since the early 1860s but without dealing systematically with the subject. This he did in a paper of 1883 in which he developed his refraction theory into a theory of dispersion. He prefaced his paper as follows:

> We know the laws for the propagation of light in a perfectly homogeneous, transparent body and also the laws for the transition from such a medium to another and similar one. It should therefore be possible, without any hypotheses at all, to calculate the propagation of light in the interior of a non-homogeneous, transparent medium; we only need to know the velocity of light, which is the only relevant quantity, in any point of the medium. I shall here attempt to make the calculation with the purpose of deriving the law for the chromatic dispersion of an isotropic, transparent body in so far that it depends on the wavelength and the body's density.[68]

Lorenz considered a collection of randomly ordered point atoms, which he imagined to be surrounded by concentric shells. Each shell was characterised by a constant refractive index, which diminished with its distance from the atom, corresponding to a variation of the effective velocity of light. Moreover, Fresnel's boundary conditions were assumed to hold true for the passage of light between any two neighbouring shells. Based on this model and his fundamental wave equation, Lorenz obtained the dispersion relation

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[68] Lorenz (1883), with German translation in *Annalen der Physik und Chemie* **20** (1883): 1-21. Review in *Fortschritte* **39** (1885): 15-17.

$$D = \frac{n^2 - m^2}{n^2 + 2m^2}v = \frac{p}{\lambda^2} + \frac{q}{\lambda^4} + \frac{r}{\lambda^6} + \cdots$$

He calculated the dispersion constants D and compared the formula with experiments of his own. For example, using diethyl ether as a liquid at 10 °C he found $D = 636$, and in the state of a gas at 100 °C he obtained $D = 623$; for chloroform the results were 413 and 409, respectively. He took the approximate agreement of the dispersion constants in the two different states to be support of his theory.

We can form an impression of Lorenz's elaborate work on optical dispersion from a letter to Prytz in which he requested the younger physicist to join him, but apparently Prytz declined the request:

> Dear Mr. Prytz! I have now finished my work on the dispersion theory and have found this dispersion constant,
>
> $$\frac{n^2 - A^2}{n^2 + 2A^2}v = D$$
>
> where $n^2 = A^2 + \frac{B}{\lambda^2} + \cdots$. The refraction constant is
>
> $$\frac{A^2 - 1}{A^2 + 2}v = R$$
>
> I have made some loose calculations and by using $\lambda_{\mathrm{Li}}/\lambda_{\mathrm{Na}} = 1.138953$ or
>
> $$\frac{\lambda_{\mathrm{Li}}^2/\lambda_{\mathrm{Na}}^2}{\lambda_{\mathrm{Li}}^2/\lambda_{\mathrm{Na}}^2 - 1} = 4.365$$
>
> I get for the liquids
>
> $$D = 4.365\frac{n_{\mathrm{Na}}^2 - n_{\mathrm{Li}}^2}{3n_{\mathrm{Na}}^2 - 2 \times 4.365(n_{\mathrm{Na}}^2 - n_{\mathrm{Li}}^2)}$$
>
> So I first calculate $4.365(n_{\mathrm{Na}}^2 - n_{\mathrm{Li}}^2) = L$ and then
>
> $$D = 4.365\frac{n_{\mathrm{Na}}^2 - n_{\mathrm{Li}}^2}{3n_{\mathrm{Na}}^2 - 2 \times 4.365(n_{\mathrm{Na}}^2 - n_{\mathrm{Li}}^2)}$$

> where *d* is the density. … I consider the calculation of the mentioned dispersion constant to be rather interesting as I believe that it gives more precise results than the experiments themselves, where the errors can accumulate to several per cents. … .[69]

Lorenz's dispersion theory was just one of many such theories in the period, where those proposed by Wolfgang Sellmeier in 1872, Helmholtz in 1875 and Lorentz in 1878 were seen as more attractive. A major reason why Lorenz's theory failed to win acceptance was that it, contrary to the other theories, fell short in explaining so-called anomalous dispersion, where the usual order of colours in the spectrum is inverted (Fig. 2.3).[70] The peculiar optical phenomenon was first described by the French physicist François Le Roux in experiments of 1862 using prisms filled with iodine vapour. However, it attracted wide attention only after 27-year-old Christian Christiansen in Copenhagen independently rediscovered it eight years later in experiments with a solution of fuchsine.

Sellmeier, a former student of Franz Neumann, was the first to come up with a dynamical explanation based on the hypothesis of atoms oscillating with amplitudes much smaller than the one of the ether particles. He assumed that the propagation of the distortional ether waves acted directly on the material atoms. By such reasoning Sellmeier derived in 1872 a formula of the form

$$n^2 - 1 = \sum_{i=1}^{n} \alpha_i \frac{\omega_i^2}{\omega_i^2 - \omega^2}$$

Here the α_i symbols refer to material constants and the ω_i symbols to selective or natural absorption frequencies; ω is the frequency of light.[71] In 1878 Lorentz derived an expression of a similar form which accounted for the anomalous dispersion as a result of electri-

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[69] Lorenz to Prytz, 14 April 1883 (Royal Library Archive, Copenhagen).

[70] Darrigol (2012), pp. 250-252; Glazebrook (1885), pp. 216-220. Lorenz (1883) was aware of Helmholtz's dispersion theory to which he referred.

[71] Whittaker (1958), p. 265 refers to the "Maxwell-Sellmeier formula for dispersion" because a similar expression was suggested by Maxwell in a Mathematical Tripos examination paper of 1869. See Harman (1995), pp. 420-421, 864-865.

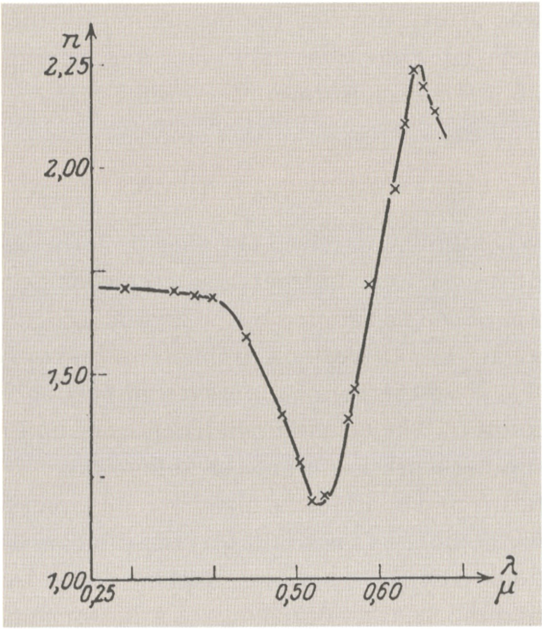

Figure 2.3: Anomalous dispersion as measured in 1895 with a prism of solid cyanine. The phenomenon was outside the range of Lorenz's dispersion theory.

cal charges oscillating with a characteristic frequency. Contrariwise, in Lorenz's theory there were neither vibrating ether particles nor electrical charges.

Christiansen was close to Lorenz and the elder physicist was of course aware of the importance of anomalous dispersion. In his 1876 textbook on optics he referred to "Christiansen … who found in investigations of a solution of fuchsine or red aniline a most remarkable anomalous chromatic dispersion with absorption of the green rays."[72] Seven years later he ended his memoir with a brief and somewhat despairing reference to the phenomenon: "If the law of anomalous dispersion is to be derived theoretically, it will be necessary to extend the calculations to cover a system of atoms corresponding to composite bodies or mixtures."

To put it briefly, Lorenz's dispersion theory was unsuccessful, belonging to the past and not to the future. In retrospect its value

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[72] Lorenz (1876), p. 189.

was that it provided the inspiration for the methods used in Lorenz's later theory of light scattering, perhaps the most remarkable of his many contributions to optics.

2.4 Light scattering by spheres

When Lorenz received the offer from the Carlsberg Foundation to be funded as an independent researcher, he happily accepted it. He was of course flattered by the unique recognition and the possibility it gave him to work on problems of his own choice. In the letter of thanks to the Foundation cited in Section 1.5, he wrote that, as a result of "the economic independence and release from any interrupting work I will now be able to take an optimistic view of the future and concentrate my efforts on larger problems to the best of my ability."[73] One of the larger problems he had in mind was undoubtedly the scattering of plane waves by transparent spherical bodies. He had worked on this topic for some time, starting in 1885 and completing it in late 1889. On 18 October he presented his forthcoming memoir to the Royal Danish Academy. Present at the meeting were twenty-two members including J. Thomsen (the president), C. Christiansen, J. Petersen, T. N. Thiele and H. Valentiner, all of them members of the scientific class. About a year later, on 14 November 1890, he discussed his work in more mathematical detail in connection with another communication to the Academy the main subject of which was his new theory of prime numbers (see Section 5.1).[74]

During the period Lorenz developed his scattering theory, he used Christian Christiansen, since 1886 professor of physics at Copenhagen University, as a sounding board and to check his involved calculations. Here is an example, indicating the cordial relations between the two physicists:

> Dear Christiansen! I have just this early morning finished the computation of the diffuse light intensity of the sphere, first for an infinitely

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[73] Lorenz to the Carlsberg Foundation, 26 February 1887 (Lorenz Papers, NBA).

[74] *Videnskabernes Selskab, Oversigt* (1889), p. 51 and (1890), p. 60.

> small refraction. As you will see, the summations can in this case – and probably also if one continues along this way – be performed exactly. This is rather interesting and seems to indicate that the general solution to the problem is not entirely impossible, although the final result is not very satisfactory; it puzzles me that the intensity of light is not proportional to the second power of the radius, but instead to the fourth power. In all probability there is some error of calculation. I have only gone through all the calculations once, but I send them to you nonetheless as in any case the calculation shows that the problem is solvable. We travel to Bornholm on Thursday evening; with many regards and the wish from both of us to you and your wife of a very pleasant journey. Yours ever, L. Lorenz.[75]

The correspondence concerning Lorenz's light scattering theory and other matters, most of them of a technical and mathematical kind, continued over the next three years. For example, in a letter of January 1888: "This early morning I discovered that I made an error in the proof for the boundary conditions of light diffraction that I sent you yesterday. By taking a second look at the original proof I found that the expression $[du/dx + dv/dy + dw/dz]$ is much too general. The proof assumes the boundary conditions $[u] = 0$, $[v] = 0$, $[w] = 0$, but … ." And half a year later: "It is cold and rainy today, so I can find no better things to do than converse with you about matters of science. After having sent you the first epistle I became aware of an error in my calculations, but I told myself that you probably would notice it yourself. … At the same time I realised that in one of my former messages concerning the boundary conditions for an arbitrary boundary interface I erroneously believed that the four boundary conditions corresponding to $[\mu P] = 0$, $[drT/dr] = 0$, $[T] = 0$, $[J] = 0$ were sufficient; but they are not, for … ."[76]

The mathematical problem of analysing the behaviour of a plane light wave traversing a collection of homogeneous, transparent and isotropic spheres was primarily to integrate the wave equation of light given certain boundary conditions. A satisfactory result only appeared in 1890 with the publication in the transactions of the

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[75] Lorenz to Christiansen, 26 June 1886 (Lorenz Papers, NBA).

[76] Lorenz to Christiansen, 7 January and 15 June 1888 (Lorenz Papers, RAS).

Figure 2.4: Christian Christiansen (1843-1917) in 1908, at a time when he taught physics to young Niels Bohr. Christiansen was Lorenz's confidant and involved in his calculations of light scattering by spheres. Courtesy: Niels Bohr Archive, Copenhagen.

Royal Danish Academy of a major, 62-page analysis of a sphere or a collection of spheres illuminated by plane waves of light.[77] The paper was a mathematical *tour de force* with significant physical applications. In the estimation of the American physicist Nelson Logan, who was instrumental in calling attention to Lorenz's forgotten paper:

> This was truly one of the most remarkable memoirs to be published in the 19th century. The history of scattering theory has been greatly enriched by the existence of this paper. This was Lorenz's last major memoir, but it alone should have been sufficient to have made him

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[77] Lorenz (1890b) and Valentiner (1898-1904), vol. 1, pp. 403-502. The Danish version Lorenz (1890b) can conveniently be found on http://people.compute.dtu.dk/jerf/Lorenz_Danish.pdf. For analysis of and comments on Lorenz's paper, see Pihl (1939), Logan (1965), Kragh (1991) and Keller (2002). For an as yet incomplete English translation by Jeppe R. Frisvad and the author see http://people.compute.dtu.dk/jerf/Lorenz_English.pdf.

> one of the famous figures of the last century. However, this crowning achievement, which represented years of painstaking work, has gone almost completely unnoticed by the generation of scientists who have followed Lorenz. Every serious "student" of diffraction theory is urged to turn the pages of this memoir to witness for himself the almost unbelievable contents of Lorenz's memoir on the sphere.[78]

Aspects of the problem of scattering of waves by a sphere had previously been investigated by other physicists, including Stokes and Rayleigh in England and in particular by the German mathematician Rudolf F. Adolf Clebsch. Although Lorenz was probably familiar with Rayleigh's important contributions, he only cited his early, non-Maxwellian paper of 1871. Altogether Lorenz's lengthy memoir contained no more than seven references to other authors. Apart from Clebsch and Rayleigh, he also cited G. Airy, G. Stokes, P. Gilbert, M. Boitel and E. Mascart.

It was clearly the work of Clebsch which served as his main inspiration. Three years younger than Lorenz, Adolf Clebsch started his brilliant but short career (he died in 1872) with works in hydrodynamics and elasticity theory. While assistant professor of theoretical mechanics at Karlsruhe Polytechnic College he presented a profound 68-page memoir in *Crelles Journal* on the reflection of elastic waves by a sphere. In this memoir he introduced several important mathematical innovations to solve the wave equations governing the problem. [79]

Working within the framework of the elastic solid wave theory, Clebsch took into account both transverse and longitudinal waves. This resulted in interesting mathematics but also in difficulties with respect to the physical interpretation of the mathematical results. He was forced to admit that he was unable to fully deduce the laws

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[78] Logan (1965), p. 777. In Logan (1962) he briefly referred to "the unjust neglect of Lorenz's paper." The same year Wait (1962) referred to the "Lorenz-Mie theory … first given in explicit form by Lorenz and some time later by Mie."

[79] Clebsch (1863). See Logan (1965) and, for a summary, Todhunter (1886-1893), pp. 168-180. In 1868 Clebsch co-founded with Carl Neumann the journal *Mathematische Annalen*. The Clebsch-Gordan coefficients used extensively in quantum mechanics and elementary particle physics are named after Clebsch and another nineteen-century German mathematician, Paul Gordan (1837-1912).

of reflection from the equations of motion of the elastic solid. Only in the special case of a very small and perfectly rigid sphere did he obtain physically useful results. Clebsch's pre-Maxwell memoir is today recognised as a remarkable contribution to mathematical physics, but it exerted almost no influence at all on the physics community. In fact, during the late nineteenth century Lorenz was alone in recognising its importance, such as he did in his presentation speech to the Royal Danish Academy of Science:

> I need to mention a single attempt to determine the reflection of light from a perfectly bright spherical surface, which is essentially a simpler task than the one dealt with here, which is based on the differential equations of light. The attempt has been made by Clebsch but with the deterrent results, as he puts it in the explanations to his great work (Crelles Journal of 1863), that the results of the entire investigation are terribly complex; moreover, for the optically important case of a very small wavelength it seems difficult to present the results in a suitable form. On the other hand, he has succeeded in the case of very small reflecting spheres. The mentioned author proceeds from the differential equations of elasticity theory, but this is of minor importance as in the case under consideration the results would be correct even with this less correct point of departure. I have myself based the theory on different equations, the same which I proposed for approximately 25 years ago.[80]

Although Lorenz was evidently inspired by Clebsch's memoir, the method of his 1890 paper was original and relied to a large extent on his own previous works. The scattering theory was yet another application of his phenomenological theory of light dating from 1862 if with important modifications. As Lorenz phrased it in his address to the Royal Danish Academy:

> What characterise my equations and distinguish them from those of others is that they contain all the fundamental conditions for the passage from one body to another; a single function depending on the space coordinates enters into them and this is nothing but the veloc-

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[80] Handwritten notes (Lorenz Papers, RAS).

> ity of light. The passage from one body to another is characterised by this velocity only. In an earlier work some five years ago I used the same equations to determine the motion of light in a system of very small spheres. I have continued this work as I could use the general formulae found then; they were valid also for a single refracting sphere of arbitrary size. And yet, during my computations I have transformed the entire and most complicated material into a simpler and more natural form and have continued to develop formulae without relying on my earlier work. The formulation of the basic equations and the derivation of the general series, which in a formal way solve the problem, occupy but a minor part of the memoir. As also pointed out by Clebsch, the difficulties only enter when the results are applied to large spheres.

While Rayleigh had made use of Maxwell's electromagnetic theory of light in his scattering theory of 1881, Lorenz did not refer to Maxwell's theory and also not to his own electrodynamic light theory of 1867. Words such as "electrical" and "magnetic" simply did not appear significantly in his memoir. As in his previous publications, he preferred to present the theory in a formal way that did not depend on the physical nature of light. In the beginning of the paper he emphasised the mathematical aspect:

> The general fundamental law of light propagation is like the laws for transmission of electricity and elastic forces of a simple form, since it can be expressed by three concurrent partial differential equations of the second order in which the three oscillatory components are the dependent variables while the space and time coordinates are the independent variables. All problems in formal optics must be subject to integration of these equations. ... [They] differ from the theory of elasticity by the fact that they rule out the possibility of longitudinal oscillations; since they are valid for every point in any transparent heterogeneous medium, the boundary conditions at the transition from one body to another can be derived from the differential equations themselves.[81]

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[81] Lorenz (1890b), pp. 3-4.

Referring to the three differential equations for the propagation of light in Cartesian coordinates, he wrote:

> The present task is to integrate these equations under the assumption that ω [the velocity of light] has a constant value inside the surface of a given sphere and a different constant value outside it and with a discontinuous transition in the spherical surface itself. This discontinuous transition can be considered to be produced by a surface layer of finite thickness and continuous change of ω … which passes over to become a layer of thickness zero. At this transition the oscillatory components must stay finite here as everywhere whereas their differential coefficients with respect to r [the distance from the sphere's centre] might become infinite.

That is, from a physical point of view Lorenz conceived the discontinuity of the sphere as a limiting case of a continuum of infinitesimally thin spherical layers, the same approach that he had applied in several of his earlier papers. Changing to spherical coordinates (φ,ψ,r), he showed that the boundary conditions required the following quantities to vary continuously across the spherical interface:

$$u_{\varphi}, \quad u_{\psi}, \quad \frac{\partial(ru_{\varphi})}{\partial r}-\frac{\partial u_r}{\partial \varphi}, \quad \text{and} \quad \frac{\partial(ru_{\varphi})}{\partial r}-\frac{1}{\sin\varphi}\frac{\partial u_r}{\partial \varphi}$$

He then assumed that the incident light wave $\mathbf{u}^0$ was propagating with velocity ω/k, where the wave number $k = 2\pi/\lambda$, in such a way that

$$\mathbf{u}^0 = \left(u_r^0,\, u_{\varphi}^0, u_{\psi}^0\right) = [0, \exp i(\omega t - kx)\,, 0]$$

Inside the sphere the wave was given by the light vector $\mathbf{u}^{\mathrm{i}}$, while the total light wave outside the sphere was expressed as

$$\mathbf{u} = \mathbf{u}^0 + \mathbf{u}^{\mathrm{a}}$$

With these preliminaries Lorenz could state his problem, namely, to determine $\mathbf{u}^{\mathrm{a}}$ and $\mathbf{u}^{\mathrm{i}}$ from $\mathbf{u}^0$ and the boundary conditions.

After an elaborate mathematical analysis he managed to find general expressions for the light vector inside and outside the sphere and then proceeded to consider separately the two cases, $R << \lambda$ and $R >> \lambda$, R being the radius of the sphere and λ the wavelength. For the case of large spheres he used the results to calculate the intensities of the various (primary and secondary) rainbows. As an example allowing comparison with observations, he stated: "Based on calculation the second rainbow, which occurs after two internal reflections, has an apparent brightness 7.864 times less than the first rainbow, of course on the assumption that they are formed under the same conditions."[82] Lorenz's explanation of the primary and secondary rainbow from fundamental principles of physics was only mentioned rather *en passant*, and, as it was written in Danish, it attracted no attention then or later. He prepared a more detailed manuscript on the rainbow, but it was never completed and only some of his mathematical notes on the problem have survived (Fig. 2.5).[83]

In a letter to Christiansen of January 1889, Lorenz expressed his early interest in the problem: "I could wish to get hold on some of the literature on the rainbow and would therefore be obliged if you could inform me where to find the two memoirs, which you referred to, and what recent literature there is about the problem. As far as I understand, the secondary rainbows are just focal lines which disappear at infinity."[84] At the time the rainbow problem was attacked by several eminent physicists and meteorologists, among them G. Stokes and Rayleigh in England, E. Mascart in France, and J. Pernter in Austria. What was known as the "Airy-Stokes-Mascart theory" enjoyed general acceptance. Lorenz was probably aware of at least some of the literature, but he chose to follow his own ideas.[85]

In his treatment of small spheres of molecular dimensions $R < \lambda$ Lorenz dealt effectively with the optical properties of what today are known as nanoparticles. He first considered a single sphere and then a large number of identical but randomly ordered spheres of refractive index N. For the light outside a sphere of radius R he de-

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[82] Lorenz (1890b), p. 42.

[83] Note on "Regnbueproblemets Løsning" (The Solution of the Rainbow Problem). Lorenz Papers (RAS).

[84] Lorenz to Christiansen, 27 January 1889 (Lorenz Papers, RAS).

[85] See Boyer (1987) for a comprehensive history of the rainbow and attempts to understand the phenomenon in terms of mathematics and optical physics.

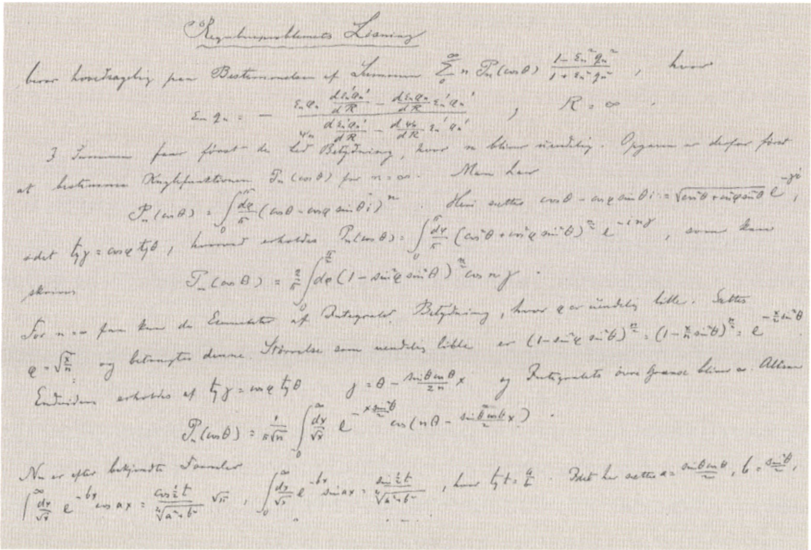

Figure 2.5: Lorenz's calculations from 1889 on "The Solution of the Rainbow Problem."

duced that it only depended on the quantity

$$\frac{N^2 - 1}{N^2 + 2} R^3$$

Lorenz next imagined that the radii of the spheres increased until R_1 (still $< \lambda$) and that the refractive index changed from N to N_1. Assuming the validity of the Lorenz-Lorentz formula he concluded that the motion of light remained uninfluenced by the change. He further proved that the intensity or square of the amplitude scattered by a single sphere relative to the intensity of the incident light was

$$L = \frac{128\pi^5 R^6}{3 \lambda^4} \left(\frac{N^2 - 1}{N^2 + 2} \right)^2$$

Since the refractive index for air and other transparent bodies only varies slightly in the wavelength range of visible light, the expression for L is dominated by the λ^{-4} term. As Lorenz pointed out, this result agreed with what Rayleigh had found in 1871.[86] Indeed, for N close to 1 it gives approximately the same expression as found by Rayleigh, namely

$$L = \frac{512\pi^5 R^6}{27\ \lambda^4}(N-1)^2$$

While Lorenz cited what he called "Rayleigh's law," as mentioned he did not refer to Rayleigh's electromagnetic derivation of 1881 and neither did he refer to other work published by the British authority in scattering theory.

To treat a gas of molecules Lorenz assumed simple additivity for the intensities, in which case a unit volume with A spheres would scatter the intensity AL. "This quantity," he wrote without further explanation, "is the absorption coefficient of the system." To see this, consider a beam of light of cross section 1 cm^2 passing a gas containing A spheres per cubic cm. Having traversed the distance x, the intensity of light diffracted by the sphere in the infinitesimal sheet of thickness dx will be

$$dI(x) = -ALI(x)dx$$

or, with $h = AL$ the absorption or attenuation coefficient,

$$I(x) = I_0 \exp(-hx)$$

This is essentially the Lambert-Beer absorption law first established in the eighteenth century. Lorenz's aim was to relate the measurable quantity h to other observables and then to deduce the value of A. For this purpose he needed to replace N with some measurable quantity, the refractive index N_i for the gas itself. He imagined each molecular or atomic sphere to be surrounded by a much larger sphere of radius R_i so that the total volume of the substance was filled with these spheres, which implies that

$$A = \frac{3}{4\pi}\frac{1}{R_i^3}$$

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86. Rayleigh (1871b).

Inserting $v = 4/3\pi R^3$ and $v_i = 4/3\pi R_i^3$ in the Lorenz-Lorentz refraction law then transformed the expression for the intensity into

$$A = \frac{24\pi^3}{h\lambda^4}\frac{N_i^2 - 1}{N_i^2 + 2}$$

The radii of the molecular spheres can be expressed as

$$R^3 = \frac{h\lambda^4}{32\pi^4}\frac{(N_i^2 + 2)(N^2 + 2)}{(N_i^2 - 1)(N^2 - 1)}$$

Since $(N^2 + 2)/(N^2 - 1) > 1$ it follows that

$$R^3 = \frac{h\lambda^4}{32\pi^4}\frac{(N_i^2 + 2)(N^2 + 2)}{(N_i^2 - 1)(N^2 - 1)}$$

Lorenz illustrated his theory with a concrete example: "Let us consider as an example the refractive index and the absorption coefficient for atmospheric air at normal pressure, namely $N_i = 0.00029$ and, with 10^{-6} mm as a unit of length, $h\lambda^4 = 0.0017$. With this coefficient 11.3 per cent of light at wavelength 580 will be absorbed over a distance of 8 km; for $\lambda = 480$ the amount will be doubled." From his formulae and using light of wavelength $\lambda = 580$ nm Lorenz derived a value for what was effectively Avogadro's number - or at the time known as Loschmidt's number - and also a minimum value for the size of molecules in air. For this application to molecular physics, see Section 5.2.

At the end of his memoir Lorenz pointed out that whereas the intensity of light emitted from the spheres would be very small, in a particular case it would be of the same order as the intensity of the incoming light. For this case he deduced that absorption lines or narrow bands of measurable widths would appear in the spectrum. "The line width," he concluded, "is always proportional to the square root of the traversed distance and also to the square root of the number of spheres in the space unit." From the system's refractive index and the width of the absorption lines it would be possible to "determine all the constants of the system, namely the number of spheres in a volume unit, the size of the spheres and their refractive index." But lacking experimental data he did not provide numerical answers based on this method. Considering a system of transparent spheres, he said in his oral presentation of October 1889: "In this case a continuous spectrum will be split up by a large number of dark lines. At certain wavelengths a kind of resonance will occur. The individual sphere is so small that it can be regarded a point and it disturbs the motion of light until the distance of one wavelength. For a collection of spheres the result will be total absorption."

There is in the sociology of science a concept called "sleeping beauties," a term that relates to scientific papers which are at first ignored but subsequently and often many decades later become recognised as important contributions with a correspondingly high citation rate. They hibernate for a long period followed by a rather sudden spike of popularity.[87] Such sleeping beauties are not uncommon in science and Lorenz's 1890 memoir may be considered an example.

His great work was only published in Danish in the not widely circulated transactions (*Skrifter*) of the Royal Danish Academy without being listed in abstract journals such as the widely read *Beiblätter zu den Annalen der Physik und Chemie*. It thus remained unknown to the international community of physicists. It is puzzling indeed that Lorenz, an internationally oriented physicist used to publishing in foreign journals, did not prepare a version of his scattering article in either German or French. He must have known that few Scandinavian colleagues were able to fully understand the work and appreciate its significance, and that the language alone would effectively block its dissemination. Yet he seems to have rested content with having solved the difficult scattering problem, without worrying about the impact of his work. There is the possibility, though, that Lorenz contemplated the possibility to have it translated into French but that his death put an end to the publication of the planned translation. In fact, he not only contemplated a French translation but actually had one made.[88] Given his previous publica-

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[87] See Ke (2015).

[88] According to Valentiner, who later translated and edited Lorenz's works, a certain Mr. Frisch made a French translation which was approved by Lorenz. See Valentiner (1898-1904), vol. 1, p. 503. The translation presented in this work was the one authorised by Lorenz in either 1890 or 1891.

tions it is a little strange that he chose a French translation rather than one in German.

The language was not the only reason why Lorenz's theory was practically unknown amongst contemporary and later physicists. By the turn of the century it was available in French translation in Valentiner's edition of *Oeuvres Scientifiques de L. Lorenz* but without making Lorenz's scattering theory more widely known. At that time Maxwell's theory of electrodynamics reigned supreme and had been fully embraced also by German physicists. The Göttingen physicist Paul Drude published in 1894 a comprehensive text on electricity, magnetism and optics significantly titled *Physik des Aethers auf Elektromagnetischen Grundlage* (Physics of the Ether Based on Electromagnetism). It was solidly based on Maxwell's theory and did not refer to Lorenz's works in either optics or electrodynamics. Few physicists would see any reason to study the difficult 1890 memoir based on a very different, non-Maxwellian framework which did not even mention electromagnetic fields.

By the 1890s Lorenz's theories were generally seen as outdated or uninteresting because they did not take into account the electromagnetic ether. One of the few references to Lorenz's scattering theory appeared in a book of 1902 by the Cambridge mathematical physicist Hector M. Macdonald, who cited the Danish 1890 memoir.[89] Two years later his compatriot J. C. Maxwell Garnett pointed out in a paper on the colour of metals that a result obtained by Rayleigh could be "proved directly by adapting the analysis given by L. Lorenz ('Vidensk. Selsk. Skr.,' Copenhagen) to the electromagnetic theory."[90] In a book of 1915 the mathematician Harry Bateman referred to both *Oeuvres Scientifiques* and to the Danish version of the paper.[91]

To the limited extent that Lorenz's work was known in the early part of the twentieth century it was more because of its mathematical than its physical content. It was Lorenz's innovative analysis of Bessel functions which attracted the interest of John Nicholson, a

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[89] Mcdonald (1902), p. 187.

[90] Maxwell Garnett (1904), p. 387.

[91] Bateman (1915), p. 79 and p. 90.

British mathematical physicist who in a series of papers of 1906-1910 studied exact solutions of how waves are scattered by a sphere. Acknowledging his indebtedness to Lorenz's memoir, Nicholson found a number of asymptotic formulae for the Bessel functions.[92] It seems to have been Nicholson's papers which belatedly made Rayleigh aware of Lorenz's theory to which he referred in a couple of papers in the autumn of his life. As he noted in 1918, Lorenz had in 1890 given "an equivalent formula" for the scattering of light. Rayleigh merely cited some of Lorenz's mathematical results without paying attention to Lorenz's memoir as a physical theory anticipating his own work published nine years later.[93]

Given that Lorenz was aware of Rayleigh's lifelong occupation with the theory of light scattering and that Rayleigh was acquainted with some of Lorenz's works, one might have expected the Danish physicist to communicate his results to his colleague in England. But if he did, nothing came out of it. There is no trace of communication between the two physicists either in the archival material kept in Denmark or in the Rayleigh Archive in Bedford, Massachusetts, except that Rayleigh may have received some of Lorenz's papers.[94] As a consequence, when Rayleigh published his important treatise on light scattering by spheres in 1899, he was unaware of Lorenz's memoir. In this celebrated work Rayleigh deduced many of the results which Lorenz had found nine years earlier.[95] Referring to the theory of scattering by a sphere, in 1969 Milton Kerker, a distinguished colloid scientist and expert in light scattering, wrote:

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[92] Nicholson (1907) and (1910). Nicholson (1881-1955) is best known for the atomic model he proposed in 1911 and which included not only an atomic nucleus (independent of Rutherford) but also Planck's constant. For a short period of time Nicholson's model of the atom was a competitor to Bohr's quantum model.

[93] Rayleigh (1918).

[94] Rayleigh appeared in a long list of physicists to whom Lorenz sent one or more offprints of his papers, but we do not know which papers he sent and when (Lorenz Papers, DTM).

[95] Rayleigh (1899). For Rayleigh's theory of scattering by small dielectric spheres, see Kerker (1969), pp. 31-39, who calls attention to Lorenz's 1890 theory and its similarity to Rayleigh's.

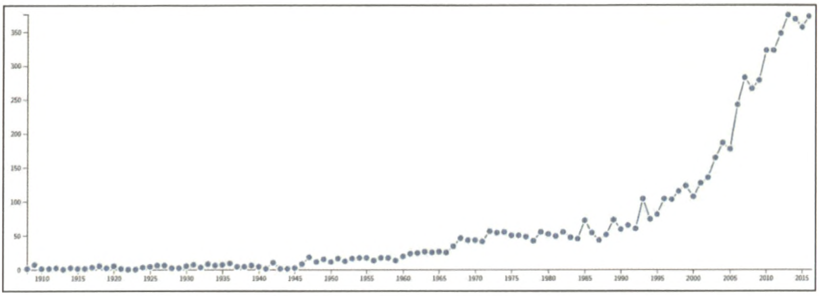

Figure 2.6: Mie's sleeping beauty. Number of citations to G. Mie's 1908 paper as a function of time according to Web of Science.

"Certainly, if this theory is to be associated with the name or names of individuals, at least that of Lorenz, in whose paper are to be found the practical formulas so commonly used today, should not be omitted."[96]

If Lorenz's memoir of 1890 were a sleeping beauty, so was it the case with Clebsch's earlier 1863 memoir, and later with Gustav Mie's seminal paper of 1908 on electromagnetic scattering of dielectric spheres (Fig. 2.6).[97] Mie, at the time professor at the University of Greifswald in northern Germany, was an expert in Maxwellian electrodynamics and a leading figure in the attempts to establish this theory as the foundation of all of physics, including mechanics and perhaps even the enigmatic gravity. Whereas his ambitious theory of ether and matter built on electromagnetic fields did not survive the Einsteinian revolution in physics (which Mie opposed), his more limited scattering theory published in *Annalen* is today considered a brilliant and far-reaching classic of physics.[98] But originally it did not attract much attention and in England it was ignored for decades. It is only since the 1960s that it has it been recognised as an exceptionally important and fertile paper, the foundation of a whole

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[96] Kerker (1969), p. 59.

[97] Mie (1908). The paper was in 1976 translated into English and more recently also into some other languages, see Wriedt (2012), p. 55.

[98] For Mie's life and science, see Hergert (2012). His scattering theory and its many modern applications are reviewed in Wriedt (2012). Both sources contain references to Lorenz.

class of optical sciences going well beyond its original and more modest aim of explaining the colour effects of gold and other metallic colloids. Today Mie theory is of interest not only to physicists, but also to meteorologists, engineers, astronomers and biologists.

What matters in the present context is that Lorenz's paper, just like Mie's, contained the exact solution of the scattering problem for a spherical body. For this reason the two theories lead to the same empirical predictions, at least implicitly. In other words, they are formally equivalent or even identical, although in a physical sense they are of course quite different. And yet there was no historical connection between the two papers. Mie actually cited an older publication of Lorenz, namely his 1880 *Annalen* paper on refractivity and density, but he did not cite the 1890 treatise first published by the Royal Danish Academy of Science. The same was the case when Peter Debye in 1909, as part of his doctoral dissertation supervised by Arnold Sommerfeld, wrote another lengthy and important paper on the scattering problem and the pressure of light on a sphere. Contrary to Mie, Debye referred to and made use of Clebsch's old paper but he was probably unaware of Lorenz's memoir, to which he did not refer.[99]

The fact that Mie did not formally cite Lorenz's paper on scattering by a sphere does not necessarily mean that he was unaware of it. He referred several times to "the Lorenz theory" in relation to the work by Maxwell Garnett which quoted Lorenz's 1890 paper. The way Mie wrote about the Lorenz theory suggests that what he had in mind was this paper and not the one of 1880 (which Maxwell Garnett did not refer to). For example, Mie referred to a collection of very small spheres or particles where the light from each particle is influenced by the diffuse scattered light from all the other particles. He wrote that the effect on the irradiation from a particle "has been investigated theoretically by L. Lorenz, and J. C. Maxwell-Garnett has developed mathematically the consequences of Lor-

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[99] Debye (1909). Logan (1965) notes that some of Debyes's mathematical results were first given by Lorenz, and Wyatt (1974) goes as far as to claim, exaggeratedly, that "the more elegant derivation and formulation of Debye ... is an exact copy of Lorenz's work."

enz's theory for the optics of colloidal solutions of metals."[100] The remark makes more sense if it refers to the 1890 theory than if it refers to Lorenz's paper on refraction dating from 1880. So my suggestion is that Mie possibly knew of Lorenz's scattering theory although he may not have studied it in any detail.

Only a handful of physicists in the electromagnetic tradition cited Lorenz's treatise, which effectively laid dormant for another half a century. By and large, it was only in the 1960s that physicists slowly began to realise the close connection between Lorenz's theory and Mie's. The French physicist Gérard Gouesbet, discussing the case within the philosophical context of underdetermination, says about the relationship between the two theories:

> Mie's and Lorenz' theories lead to the same experimental predictions, that is to say, they are empirically equivalent. The work of Lorenz has been overlooked, certainly in part because it has been written in Danish but, most importantly, certainly as another reason, it happens that the theoretical framework used by Lorenz was not that of Maxwell, but another one, of a mechanical nature, relying on the existence of ether. Therefore, although the two theories are empirically equivalent, they rely on two different visions of the world, i.e. two different ontologies.[101]

However, although the theories of Mie and Lorenz were indeed ontologically different, Gouesbet misunderstands the role of the ether in the two theories. It was Mie's electromagnetic theory, not Lorenz's, which relied on the ether, and it did so because it was based on Maxwell's ether theory. The ether was absent in Lorenz's scattering theory.

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[100] Mie (1908), p. 414 and a similar remark on p. 377. Mie also referred to Kirchner (1904), a paper citing Lorenz (1880) in relation to the optical properties of metallic emulsions.

[101] Gouesbet (2012), p. 74. Also Horvath (2009) misrepresents Lorenz's solution of the scattering problem as relying on the ether theory.

Name	*Year*	*Lorenz*	*Lorentz*
Lorenz gauge	1867	3,410	12,300
Lorenz-Lorenz formula	1869	3,770	15,100
Wiedemann-Franz-Lorenz law	1872	386	85
Lorenz number	1872	7,700	3,370
Lorenz-Mie theory	1890	4,670	720

Table 2.2. Lorenz eponymies. Names in physics associated with L. Lorenz and the year of his works relevant to the names. Column 3 gives the number of references in Google Scholar to the names in column 1, whereas column 4 refers to H. A. Lorentz, namely the alternative names Lorentz gauge, Lorentz-Lorenz formula, Wiedemann-Franz-Lorentz law, Lorentz number, and Lorentz-Mie theory.

Today the theory of the Danish physicist is increasingly considered an earlier but physically different version of Mie's theory. Instead of speaking of the Mie theory, some physicists prefer the name Lorenz-Mie theory or, occasionally, Lorenz-Mie-Debye theory.[102] The term Lorenz-Mie theory first turned up in the early 1960s and is today frequently used, whereas the reverse combination Mie-Lorenz theory is rare. It is more unfortunate to find in the physics literature references to the "Lorentz-Mie" theory, which is obviously wrong since H. A. Lorentz was not involved in the scattering theory at all. But it was Lorenz's destiny to have a name which could too easily be confused with that of the great Lorentz, a towering figure whose name is known to all physicists (Table 2.2). It was also his destiny to have his name associated with a method (the Lorenz-Mie-Debye solution) for analysing the Maxwell equations which he intentionally ignored.

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[102] E.g. Keller (2002), p. 286.

CHAPTER 3

Electricity, ether and electrodynamics

Independently of Maxwell, in 1867 Lorenz argued in a remarkable paper that light can be described by the equations of electrodynamics and that electricity propagates in free or almost free space with the velocity of light. In the same paper he introduced what many years later became known as the Lorenz gauge condition for the vector potential. His route to the important insight relating light to electrical vibrations was optical, in so far that it relied on his earlier works in optics, and it was also inspired to a large extent by Kirchhoff's theories of electricity.

Lorenz's electrodynamic theory of light attracted the interest of Maxwell, but at the time of Lorenz's death - or even that of Maxwell twelve years earlier - it had fallen into oblivion. One might expect that after 1867 Lorenz would defend and further develop his theory, but strangely he did not. When he turned to other areas of the electrical sciences it was as if he had forgotten about his brilliant contribution to electrodynamics. As a background for appreciating Lorenz's theory we introduce this chapter with a summary account of electromagnetic theories in the period around the mid-nineteenth century.

The other areas of electricity to which Lorenz contributed significantly included a series of elaborate experiments on the thermal and electrical conductivity of metals, illustrating that he mastered the art of experiment no less than he mastered the art of abstract theory. His work resulted in an extension of the Wiedemann-Franz law into what is occasionally known as the Wiedemann-Franz-Lorenz law. The so-called "Lorenz number" still plays a role in modern solid-state physics. Lorenz's work on conductivity led him to investigate how to determine electrical resistance most precisely and in absolute units. His construction in 1873 of a cleverly designed induction apparatus for this purpose brought him into the centre of the contemporary discussion concerning the definition of the ohm unit. According to Rayleigh and other experts, Lorenz's method was not only "ingenious" and "beautiful," it was also the one which offered the best prospects for solving the problem of determining an absolute measure for the ohm unit. To physicists and engineers in the early twentieth century, "Lorenz of Copenhagen" was known primarily for his work in this area of applied or engineering physics and not for his electrodynamic theory of light or other contributions to theoretical physics.[1]

3.1 Theories of electricity and magnetism

Following Alessandro Volta's sensational discovery of the battery or "voltaic pile" in 1800, scientific interest in electricity experienced a boom both in theory and experiment. Magnetism was eagerly explored too, but during the first two decades electricity was conceived as essentially different from magnetism. A new chapter – in the form of electromagnetism – was opened with H. C. Ørsted's famous discovery in 1820 of the magnetic action of a current passing a metallic conductor. The discovery was quickly exploited and extended by other physicists and in particular by André-Marie Ampère in France (who coined the word "electrodynamics"). In a book of 1826 he formulated an inverse-square law for the force acting between current elements, not between moving charges. Not only did the law account for the effect observed by Ørsted, it also accounted for several other phenomena of electricity and magnetism. In its simplest form Ampère's celebrated law of force states that

$$\mathbf{d}^2\mathbf{F} = \frac{ii'}{r^3}[(\mathbf{ds}\cdot\mathbf{r})\mathbf{ds}' + (\mathbf{ds}'\cdot\mathbf{r})\mathbf{ds} - (\mathbf{ds}\cdot\mathbf{ds}')\mathbf{r}]$$

Here $\mathbf{ds}$ and $\mathbf{ds}'$ are two current elements with currents i and i' and $\mathbf{r}$ is the line joining the elements. Thus, according to Ampère's law the force depends not only on the distance r between the interacting current elements but also on the angles between them. Having es-

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[1] In his commemorative essay of 1932 the physicist H. M. Hansen singled out Lorenz's method as his "chief achievement," considering it to be more important than his electrodynamic theory of light, the Lorenz-Lorentz refraction formula, and the Wiedemann-Franz-Lorenz law. See Hansen (1932).

tablished the basic laws of the electrical forces, Ampère interpreted magnetism in terms of molecular currents. With its basis in the Newtonian-Laplacian tradition Ampère's research programme exerted massive influence on continental physicists aiming at formulating the laws of electromagnetism in a coherent mathematical theory. Maxwell generously referred to Ampère as "the Newton of electricity."[2]

Another of the giants of nineteenth-century physics, Michael Faraday, received inspiration from the discoveries of Ørsted and Ampère, but his dynamical and experimentally based ideas about electricity and magnetism were entirely original and quite different from those of the French physicist. Perhaps Faraday's most important work was his experiment of 1831 that led to one of the laws named after him, the electromagnetic induction law describing how a magnet in motion generates electricity. To Faraday, space was neither a vacuum nor filled with inert ether but a dynamical seat of constant activity. What others conceived as actions at a distance he thought of as contiguous actions permeating and deforming an intervening medium.

While imagery was plentiful in Faraday, mathematics was sorely absent which contributed to the reserved reception even in England. Nonetheless, by introducing the all-important concepts of the electromagnetic field and its associated lines of force he established a powerful foundation for the theories which were subsequently formulated in mathematical terms by William Thomson, James Clerk Maxwell and others. Still by the mid-nineteenth century the Faraday approach to electromagnetism was little known on the Continent and when known, it was unappreciated. The continental, pre-Maxwellian approach did not consider fields but was largely based on forces acting on hypothetical charged particles. Magnetism was seen as an effect of the electrical forces.

The leading figure in the predominant research tradition was

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[2] Maxwell (1954), vol. 2, p. 175. Detailed histories of electricity and magnetism in the nineteenth century include Whittaker (1958), Kaiser (1981) and Darrigol (2000). These three books and also Wise (1981) and Harman (1982) include sections on Lorenz's electrodynamic theory of light.

Wilhelm Eduard Weber, a collaborator of Gauss and for periods a professor at the universities of Leipzig and Göttingen. Weber's higher aim was to unify the phenomena of electricity and magnetism at a fundamental level. To do so he violated one of the dogmas of the French school, namely that only central and velocity-independent forces operated in the world of electricity. In an important work of 1846 Weber proposed a law of force that he claimed was valid for all electrical particles (no such particles existed in Faraday's view). The Weberian force between two particles depended on the distance, of course, but not in the simple Newtonian way of varying inversely with the square of the distance. Nor did the force propagate continuously, for according to Weber it acted instantaneously. For two particles of charge q_1 and q_2 separated by the distance r, Weber's law stated a force F_w that depended on the relative motion of the particles, namely as

$$F_{\mathrm{w}} = \frac{q_1 q_2}{r^2}\left[1 - \frac{1}{C^2}\left(\frac{dr}{dt}\right)^2 + \frac{2r}{C^2}\frac{d^2 r}{dt^2}\right]$$

The velocity term referred to Ampère's law and the acceleration term to phenomena related to electromagnetic induction. If there is no motion between the particles, the derivatives are zero and the law reduces to Coulomb's electrostatic law $F = q_1 q_2/r^2$. The quantity C was an undetermined constant, which Weber later related to the velocity of light c by $C = c\sqrt{2}$ (see Section 3.2). In Weber's model the current was given by the passage of positive charges in one direction and the passage of negative charges at the same uniform velocity in the opposite direction. This assumption was first stated in 1845 by Gustav Fechner, a close friend of Weber.

Weber's theory included the ethereal medium, which he perceived as electrical in nature, for example composed of equal numbers of negative and positive particles. In his later work he speculated that also matter atoms were made up of particles of the same numerical charge that revolved around each other and possibly vibrated. Somewhat similar speculations were entertained by Fechner. Although theoretically impressive Weber's theory was also conspicuously speculative as it rested on the hypothesis of microscopically small charged particles that lacked solid experimental evidence. It was left for the future to demonstrate that such particles or something like them actually exist. They were then called electrons, a name which was introduced by the Irish physicist G. Johnstone Stoney in 1891 and which for a period referred to particles of positive as well as negative unit charge.

Weber's theory was held in high esteem in the 1850s, when it was not yet realised that it only applied to closed circuits but failed for problems involving open circuits. It was far from the only mathematical theory of electricity proposed by the industrious German physicists. Other physicists contributing notably to the German tradition in electrodynamics were Franz Neumann, his son Carl Neumann, Robert Kirchhoff, Rudolf Clausius, and Bernhard Riemann. Their theories differed in various ways from Weber's and completely from Faraday's. In an important work from 1870 Hermann von Helmholtz argued that the velocity-dependent terms in Weber's electrodynamic force law led to absurdities such that an electric particle might be accelerated to an infinite velocity within a finite period of time. Already in 1847 he had objected that a velocity-dependent force violated energy conservation. According to Helmholtz, electrodynamics could not possibly be based on Weber's law and nor could it be based on Carl Neumann's generalised law.[3] Some of Helmholtz's criticism of Weber's theory was echoed by Maxwell.

To simplify, through a large part of the nineteenth century electrodynamics was dominated by two very different, competing approaches or research programmes. One of them was the French-German approach built on electrical point-like particles distributed in an empty space (or inert ether) and interacting at a distance. The other approach was the British field theory in which there were no electrical particles and in which electromagnetic phenomena, including charges and currents, were properties of the ethereal medium. Remarkably, the two antagonistic research programmes co-existed for several decades during which period both were progressive. Maxwell acutely remarked that "The comparison, from a philo-

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[3] Helmholtz (1870). His work led to disputes with Weber and Neumann. See Kaiser (1993) and Schlote (2006).

sophical point of view, of the results of the two methods so completely opposed in their first principles must lead to valuable data for the study of scientific speculation."[4]

3.2 An electrodynamic theory of light

As mentioned in Section 2.2, Lorenz paid little attention to the ether and its nature in his optical works of the 1860s, tending to regard it as a medium with no physical characteristics. Yet it was only in two papers of 1867 that he unequivocally rejected the ether as an unnecessary concept and one which was flawed from a methodological point of view. In a popular Danish paper he wrote:

> The assumption of an ether would be unreasonable; because, it is a new non-substantial medium which has been thought of only because light was conceived in the same manner as sound and it [the ether] hence had to be a medium of exceedingly large elasticity and small density in order to explain the large velocity of light. ... If we need to assume some medium for light between the celestial globes, we do not have to conceive it as different from the known gases. On the whole it is most unscientific to fabricate a new substance when its existence is not revealed in a much more definite way.[5]

Without referring to Maxwell (and probably without thinking of him), Lorenz implicitly suggested that the very basis of Maxwell's electromagnetic theory was "unscientific." For according to the Scottish physicist it was imperative that light propagated in what he called an "aethereal medium filling space and permeating bodies."[6] To his mind, not only light but all electromagnetic phenomena were effects of the dynamics of this medium without which energy could not be transmitted.

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[4] Maxwell (1954), vol. 2, p. 158. As leading contributors to theories of the action at a distance type he mentioned "Gauss, Weber, F. E. Neumann, Riemann, Betti, C. Neumann, Lorenz and others."

[5] Lorenz (1867a). See Appendix C for an English translation of the paper, which also appears as http://arxiv.org/abs/1803.06371.

[6] Maxwell (1965), Part 1, p. 528.

The context of Lorenz's dismissal of the imponderable light ether was a new theory in which the vibrations of light were considered to be electrical currents. He presented the theory to the Royal Danish Academy at a meeting of 25 January 1867 in the presence of only 14 members, among them Julius Thomsen and Ludvig Colding. The announcement of the new "discovery" attracted some public attention and was described by the newspaper *Dagbladet* (The Daily) as a proof that light is electrical in nature. Although the newspaper admitted that so far the discovery was purely theoretical and without experimental proof, it suggested that practical applications might well appear in the future: "In so far that all practice is based on scientific knowledge this [discovery] will not be without practical importance, at least indirectly. After all, in 1820 the significance of Ørsted's discovery of electromagnetism was only dimly perceived and could anyone then have foreseen its impact on practical life?"[7]

Lorenz's full paper was entitled "On the Identity of the Vibrations of Light with Electrical Currents," published in the Academy's proceedings (*Oversigt*) with an extensive summary in French published separately.[8] The same year the paper was translated into German in *Annalen* with the German version serving as the basis for an English translation in *Philosophical Magazine*. Although "the present general opinion regards light as consisting of backward and forward motions of particles of aether," according to Lorenz, "light cannot ... consist of vibrations of the kind hitherto assumed." In his new electrical theory there was "scarcely any reason for adhering to the hypothesis of an aether, for it may well be assumed that in the so-called vacuum there is sufficient matter to form an adequate substratum for the motion."[9]

Lorenz's phenomenological attitude to the problems of light and electricity differed from that of his German colleagues by incorporating aspects of the dynamical thinking that characterised such

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[7] *Dagbladet*, 17 February 1867.

[8] *Sur l'Identité des Vibrations de la Lumière et des Courants Électriques* (Copenhagen, 1867), 7 pp.

[9] Lorenz (1867b), quoted from the translation in *Philosophical Magazine*.

natural philosophers as Faraday and Ørsted. In most respects the mathematically inclined Lorenz differed a great deal from these two thinkers, but he was probably influenced by Ørsted in his search for analogies that might reveal the deep-lying unity of the natural forces. As early as 1816 Ørsted thought of light as "a series of immeasurably small electrical sparks," an idea which appealed to him because "it does not presuppose any force or matter whose existence has not been experimentally proved."[10] The influence from Ørsted was instrumental in Lorenz's 1867 identification of light with electric disturbances. Almost paraphrasing Ørsted he explained his guiding principle as follows:

> The endeavours to search for connections between the various forces have been a significant reason for the progress of recent science; the idea that the various forces in nature are merely different manifestations of the one and same force has proved itself more fertile than all physical theories. It turned out that only one further step along the already established road had to be made, and this step leads to the remarkable result that *the vibrations of light are electrical currents*.[11]

In Lorenz's more technical paper of 1867, the one which was translated into German and English, he referred specifically to Ørsted's discovery of electromagnetism as a partial confirmation of the unity of forces. As to his own electrical theory of light, he said that "[it] manifestly lead[s] us a step further towards developing the idea of the unity of forces, and opens up a fresh field for future inquiries."

Lorenz introduced his paper with critical comments of a methodological nature. While acknowledging the idea of the unity of force as a valuable guiding theme, he argued that it had not been proved experimentally and lacked a theoretical foundation. With regard to the theories of electricity based on electrical fluids and

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[10] See Ørsted (1998), pp. 398-399. In a communication of 1830 Ørsted spoke of electricity and magnetism as oscillations in the ether on par with optical oscillations, yet at the same time he expressed doubts that light really consisted in ether oscillations (ibid., p. 541).

[11] Lorenz (1867a). For technical discussions of Lorenz' theory, see Keller (2002), pp. 254-272 and Pihl (1939), pp. 46-53. See also Kaiser (1981), pp. 157-162.

those of optics based on the ether, he wrote: "Yet these physical hypotheses are scarcely reconcilable with the idea of the unity of force, and … have only been useful inasmuch as they furnish a basis for our imagination." His alternative was to adopt a positivistic strategy:

> Hence it would probably be best to admit that in the present state of science we can form no conception of the physical reason of forces and of their working in the interior of bodies; and therefore (at present, at all events) we must choose another way, free from all physical hypotheses, in order, if possible, to develop theory step by step in such a manner that the further progress of a future time will not nullify the results obtained.

As he saw it, the electrical theory of light demonstrated the usefulness of the chosen strategy.

Methodology apart, Lorenz started out from Kirchhoff's electrodynamic theory of 1857 concerned with the propagation of electricity in metallic wires.[12] The theory was based on a set of local equations comprising Ohm's law and the law of induction which in modern notation and slightly anachronistically can be summarised in the vector equation

$$\mathbf{j} = -\sigma\left(\nabla\varphi + \frac{\partial \mathbf{A}}{\partial t}\right)$$

Here **j** denotes the current density vector, σ the specific electric conductivity, φ the scalar potential and **A** the vector potential. In Kirchhoff's theory the two potentials were expressed in terms of the charge density ρ and the current density **j** as

$$\varphi(\mathbf{x}, t) = \int \frac{\rho(\mathbf{x}', t)}{|\mathbf{x} - \mathbf{x}'|} d\mathbf{x}'$$

and

$$\mathbf{A}(\mathbf{x}, t) = \int \frac{\mathbf{j}(\mathbf{x}', t)}{|\mathbf{x} - \mathbf{x}'|} d\mathbf{x}'$$

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[12] Kirchhoff (1857); Kirchhoff (1891), pp. 183-201.

Kirchhoff's potentials satisfied the continuity equation or equation of charge conservation. With (*u*, *v*, *w*) being the components of the current density he stated the result as

$$\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}=-\frac{1}{2}\frac{\partial \rho}{\partial t},$$

which corresponds to Lorenz's equation

$$\frac{1}{2}\frac{\partial \rho}{\partial t}+\nabla\cdot\mathbf{j}=0$$

The factor of ½ reflected the contemporary view that electrical currents are composed of negative as well as positive charges moving in opposite directions. Neither Kirchhoff nor Lorenz operated with the electric field strength **E**, but many of their equations involving the current density **j** can be transformed into field equations simply by the substitution

$$\mathbf{j}=\sigma\mathbf{E}$$

Lorenz noted that although Kirchhoff's equations were "deduced in a purely empirical manner," they were only known to be true to a degree corresponding to the accuracy of ordinary experiments. Hence it would be admissible to introduce very small terms the effect of which would not turn up experimentally. Moreover, such a modification was preferable for methodological reasons as it would lead to a more general theory of non-instantaneous interaction. Lorenz argued that the most general assumption would be to assume a finite velocity for the propagation of the electrical action. This assumption he expressed by introducing retarded potentials defined in the same way that he had done in 1861 in the context of his elastic theory of light. That is, he wrote the scalar potential as

$$\varphi(\mathbf{x},t)=\int\frac{\rho(\mathbf{x}',t-r/a)}{|\mathbf{x}-\mathbf{x}'|}d\mathbf{x}'$$

and the vector potential as

$$\mathbf{A}(\mathbf{x},t) = \int \frac{\mathbf{j}(\mathbf{x}',t-r/a)}{|\mathbf{x}-\mathbf{x}'|}\,d\mathbf{x}'$$

Thus, “the entire action between the free electricity and the electrical currents *requires time to propagate itself*... The action in the point xyz at the moment t does not depend on the simultaneous condition $x'y'z'$, but on the condition in which it was at the moment $t - r/a$; that is, so much time in advance as is required to traverse the distance r with the constant velocity a.” Lorenz proved by a series expansion that the retarded potentials give the instantaneous potentials in the limit $r/a \to 0$. Clearly, he understood that the physical interpretation of the new potentials φ and $\mathbf{A}$ is that at a given point $\mathbf{x}$ and a given time t, the potentials are determined by the charge and current that existed at other points in space $\mathbf{x}'$ at an earlier time. He did not comment on the mathematical possibility of an advanced potential involving $f(t + r/a)$ instead of the retarded form $f(t - r/a)$.

Lorenz did not actually refer to φ and $\mathbf{A}$ as potentials, a name which does not appear in his paper. In agreement with Kirchhoff he called the two functions “two components of electromotive force - one arising from the inducing action of free electricity, the other from the inducing action of the variable intensities of the current.” Contrary to Maxwell, neither Kirchhoff nor Lorenz associated the vector potential with magnetism but regarded both potentials to be electrical quantities. For this reason the symbol for the vector potential in Lorenz’s theory does not stand for quite the same as the vector potential appearing in Maxwell’s equations.[13]

As to the value of the velocity constant a Lorenz noted that it was not given by theory but that it must be very great, say of the order of the velocity of light in vacuum. Weber had earlier used in his force law the constant c_w as the ratio between an electrical charge measured in electromagnetic and electrostatic units, respectively. In this way he had found that

$$c_{\mathrm{W}} \cong \sqrt{2}c_0\,,$$

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[13] Whittaker (1958), p. 270; Roche (1990).

where c_0 denotes the experimentally determined velocity of light in vacuum or air.[14] At the time it was known that c_0 was close to 3×10^8 m s^{-1}, and experiments by Weber and Rudolf Kohlrausch from 1856 resulted in $c_w = 4.4 \times 10^8$ m s^{-1} or $c_w / \sqrt{2} = 3.1 \times 10^8$ m s^{-1}.

In his studies of the propagation of currents in various conducting media, Kirchhoff concluded in 1857 that the propagation velocity was independent of the nature of the conductor and approximately equal to the velocity of light. Although Kirchhoff considered the Weber or Weber-Kohlrausch relationship to be a remarkable coincidence and more than just a numerical one, he refrained from drawing conclusions with respect to the nature of light. Weber's attitude was bolder and more in agreement with Lorenz's view. In 1864 Weber commented that "If this close agreement of the propagation velocity of electric waves with the velocity of light could be regarded as an indication of a deep connection between the two sciences, then this agreement would captivate our attention, considering the high importance of the search for such a connection." And yet Weber drew back from identifying light and electrical disturbances: "It is obvious that the true meaning of this velocity [Kirchhoff's] with respect to electricity must be considered, and this meaning is not of a kind that would allow great expectations."[15]

Lorenz disagreed. Citing the results of Weber and Kirchhoff, he suggested that they were valid also for the propagation of electric waves in free space. "We have therefore some reason for taking $a = c/\sqrt{2}$," he cautiously wrote.[16] He was pleased to note that with this value his electrodynamic equations "assume now a very simple form, and lead to exactly the same differential equations as those which I formerly deduced for the vibrations of light." In his seminal paper of 1861-1862 "On Physical Lines of Force" Maxwell had reached what was essentially the same conclusion. He argued as follows:

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[14] See Assis (2003) for references and details. This source also contains an English translation of the 1856 Weber-Kohlrausch paper.

[15] Quoted in Darrigol (2000), p. 73.

[16] Lorenz (1867b), who used the symbol *c* for and not for the velocity of light.

> The velocity of transverse undulations in our hypothetical medium, calculated from the electro-magnetic experiments of MM. Kohlrausch and Weber, agrees so exactly with the velocity of light calculated from the optical experiments of M. Fizeau, that we can scarcely avoid the inference that light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena.[17]

However, Maxwell's "inference" was in reality a claim without proper justification. He was not as yet in possession of an electromagnetic theory of light from which the claim could be derived or at least draw support.

To deduce the physical consequences of his new approach, Lorenz adapted his optical results obtained in the early 1860s to the electrical case. With Δ being the Laplace operator - for which Lorenz used the symbol Δ^2 - he stated the wave equation in the general form

$$\left(\Delta - \frac{1}{a^2}\frac{\partial^2}{\partial t^2}\right)\int \frac{\rho(\mathbf{x}', t - r/a)}{|\mathbf{x} - \mathbf{x}'|}d\mathbf{x}' = -4\pi\varphi(\mathbf{x}, t)$$

The operator in front of the integral is the four-dimensional d'Alembert operator often represented by the symbol □. Instead of providing a proof of the equation Lorenz referred to his 1861 paper in *Crelles Journal*, where the same equation occurred in a non-electrical context and a proof was given.

After some further calculations Lorenz obtained for the variation of the current density $\mathbf{j} = (u, v, w)$ three partial differential equations, which he wrote as

$$\frac{\partial}{\partial y}\left(\frac{\partial u}{\partial y} - \frac{\partial v}{\partial x}\right) - \frac{\partial}{\partial z}\left(\frac{\partial w}{\partial x} - \frac{\partial u}{\partial z}\right) = \frac{1}{a^2}\frac{\partial^2 u}{\partial t^2} + \frac{16\pi}{a^2}\sigma\frac{\partial u}{\partial t}$$

$$\frac{\partial}{\partial z}\left(\frac{\partial v}{\partial z} - \frac{\partial w}{\partial y}\right) - \frac{\partial}{\partial x}\left(\frac{\partial u}{\partial y} - \frac{\partial v}{\partial x}\right) = \frac{1}{a^2}\frac{\partial^2 v}{\partial t^2} + \frac{16\pi}{a^2}\sigma\frac{\partial v}{\partial t}$$

$$\frac{\partial}{\partial x}\left(\frac{\partial w}{\partial x} - \frac{\partial u}{\partial z}\right) - \frac{\partial}{\partial y}\left(\frac{\partial v}{\partial z} - \frac{\partial w}{\partial y}\right) = \frac{1}{a^2}\frac{\partial^2 w}{\partial t^2} + \frac{16\pi}{a^2}\sigma\frac{\partial w}{\partial t}$$

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[17] Maxwell (1965), Part 1, p. 500. The reference is to the French physicist Hippolyte Fizeau (1819-1896), who in 1849 determined the speed of light in air to $c = 313{,}300 \mathrm{km\ s^{-1}}$

"These equations for the components of the electrical current," he commented, "agree fully with those I have already found for the components of light up to the last member, into which the electrical conductivity [σ] enters." The wave equation for the variation of **j** can in condensed form be expressed as

$$-\nabla \times (\nabla \times \mathbf{j}) = \frac{1}{a^2}\frac{\partial^2 \mathbf{j}}{\partial t^2} + \frac{16\pi}{a^2}\sigma\frac{\partial \mathbf{j}}{\partial t}$$

Lorenz identified the last term with the absorption of light which would increase with the electrical conductivity. In agreement with experiments it showed that all good conductors absorb light to a great extent and that the opposite is the case for poor conductors. For example, it was known that carbon in the form of the opaque graphite is a good conductor, whereas a transparent diamond is a non-conducting form of carbon.

Given the similarity of his phenomenologically based optical wave equation as stated in Section 2.2 and the one of electrical currents, Lorenz could transfer most of his old results to the new electrodynamic theory of light. In the course of deriving his equations, Lorenz argued that the retarded potentials had to satisfy a certain condition. Largely following Kirchhoff's notation he symbolised the scalar potential by $\bar{\Omega}$ and the vector potential by (α,β,γ). With this notation he wrote the constraint as

$$\frac{\partial \bar{\Omega}}{\partial t} = -2\left(\frac{\partial \alpha}{\partial x} + \frac{\partial \beta}{\partial x} + \frac{\partial \gamma}{\partial x}\right)$$

In modern notation and Gaussian units the factor 2 disappears and the Lorenz constraint becomes

$$\nabla \cdot \mathbf{A} + \frac{1}{c}\frac{\partial \varphi}{\partial t} = 0$$

Without emphasising the significance of this formal innovation, a particular fixing of the divergence of **A**, he thus introduced the constraint of the vector potential known today as the Lorenz gauge condition.[18]

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[18] See Jackson and Okun (2001) for a history of the gauge concept in classical and quantum electrodynamics.

In the ordinary Maxwell formulation of electrodynamics the fields and potentials are related through the equations

$$\mathbf{E} = -\nabla\varphi - \frac{\partial \mathbf{A}}{\partial t} \quad \text{and} \quad \mathbf{B} = \nabla \times \mathbf{A}$$

While the field quantities **E** and **B** are unique, this is not the case for the potentials φ and **A**. If they are replaced by

$$\varphi' = \varphi + \frac{\partial \beta}{\partial t} \quad \text{and} \quad \mathbf{A}' = \mathbf{A} - \nabla\beta\,,$$

where β is an arbitrary differentiable scalar, it leaves the **E** and **B** fields unchanged. With the full development of the Maxwell equations it was understood that the potentials have to be defined and that this can be done in different ways without affecting the physical meaning of the fields. The freedom corresponds to different "gauges."

The constraint stated by Lorenz was and is still often attributed to H. A. Lorentz, who used it in his important work on electromagnetism in the early twentieth century and spelled out in clear language the idea of the arbitrariness of the potentials. But paternity to the constraint used by Lorentz undoubtedly belongs to his Danish near-namesake. As far as priority is concerned, it is one of many cases where a discovery made by some scientist is or has been named after another and better-known scientist.[19]

Maxwell generally worked in what is known as the Coulomb or radiation gauge, according to which

$$\nabla\cdot\mathbf{A} = 0$$

Despite its name, it owes nothing to Charles Augustin Coulomb, the famous French eighteen-century physicist, but was first stated

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[19] Jackson (2008) speaks of the "zeroth theorem in the history of science," a name introduced in *New Scientist* **196** (2007), p. 61 with reference to the Lorenz-Lorentz case. Apart from the Lorenz gauge, other examples of the zeroth law are the Dirac delta function (Heaviside), Avogadro's number (Loschmidt), Hubble expansion (Lemaître), and Olbers' paradox (Chéseaux). This theorem, principle or rule was stated many years earlier in the form of "Stigler's law of eponymy." See Stigler (1980).

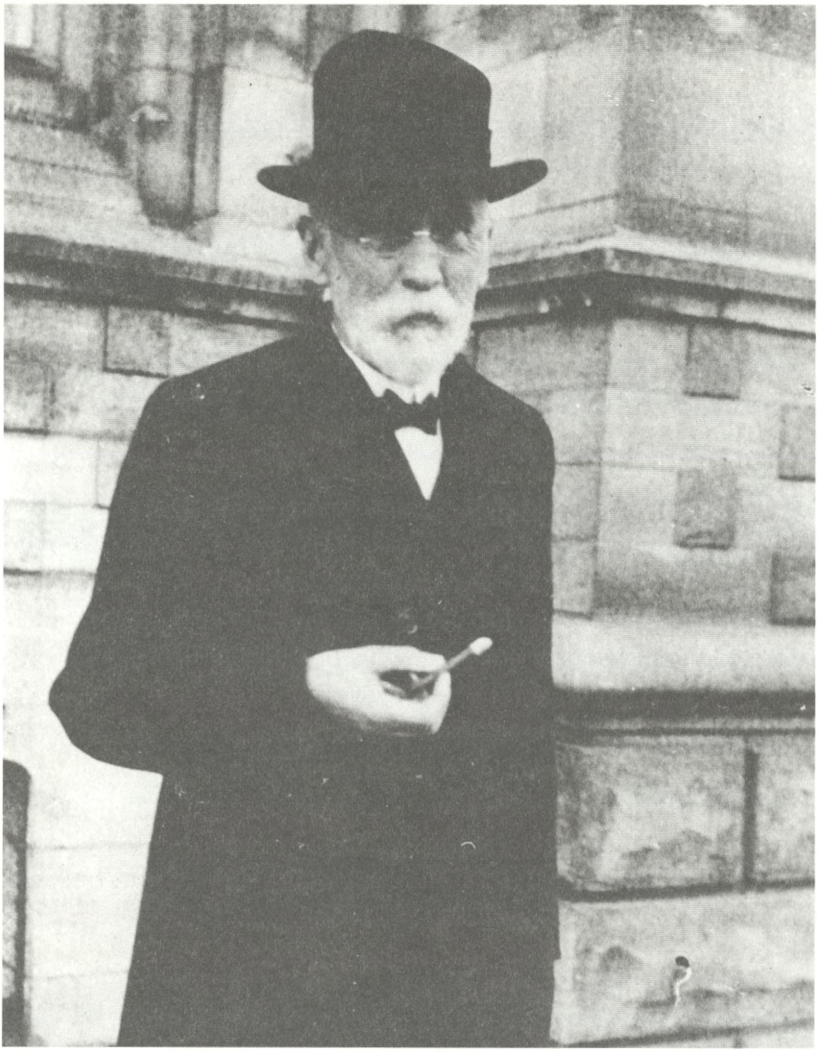

Figure 3.1: The Dutch physicist H. A. Lorentz (1853-1928) who is often credited with the paternity of the Lorenz gauge condition in electrodynamics. His important work also intersected with Lorenz's in other areas of physics. Photograph from 1927, with permission of the Niels Bohr Archive, Copenhagen.

by Maxwell in 1861 using Faraday's name "electrotonic state" for the vector potential. Maxwell insisted that $\nabla \cdot \mathbf{A} = 0$ was not an arbitrary choice but that it was demanded for physical and not merely mathematical reasons.[20] However, this gauge is not suited for dynamic fields where it causes the scalar field to adjust instantaneously to changes in electrical charges, corresponding to an infinite speed of propagation and implying problems of causality. In terms of potentials Maxwell formulated his equations in free space as

$$\frac{\partial^2}{\partial t^2}\nabla \cdot \mathbf{A} + \frac{\partial}{\partial t}\nabla^2 \varphi = 0$$

He commented that "**J** [= $\nabla \cdot \mathbf{A}$] must be a linear function of t, or a constant, or zero, and we may therefore leave **J** and Ψ [= φ] out of account for periodic disturbances." As was discussed later in the century, the Coulomb gauge $\nabla \cdot \mathbf{A} = 0$ has the consequence that $\nabla^2\varphi$ is independent of time and hence, apparently, that the scalar potential cannot be propagated in time.[21] The problem worried the Irish physicist Gerald FitzGerald who latest by 1890 reached the conclusion that Maxwell's quantities **J** and Ψ were not independent but related in the form of the Lorenz gauge, meaning that

$$\mathbf{J} = -\partial\Psi/\partial t$$

FitzGerald was at the time unaware of Lorenz's paper.[22]

Although first stated many years before the advent of relativity theory, the Lorenz gauge condition is, contrary to the Coulomb gauge, manifestly Lorentz invariant. It can be written in terms of the four-potential $\mathbf{A}^\mu = (\varphi/c, \mathbf{A})$ as

$$\partial_\mu \mathbf{A}^\mu = 0$$

The first example of a particular gauge, a relation between the two electromagnetic potentials, can retrospectively be found in Kirchhoff's 1857 paper, where it appears in a form corresponding to

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[20] Maxwell (1965), Part 1, p. 476. See also Roche (1990).

[21] Maxwell (1954), vol. 2, p. 434. It appears that Maxwell (1965, Part 2, p. 581) was close to discovering the Lorenz gauge. See also the comments in Hunt (1991), p. 117 and Buchwald (1985), p. 277.

[22] On FitzGerald's worries, see Hunt (1991), pp. 117-118.

$$\nabla \cdot \mathbf{A} - \frac{1}{c}\frac{\partial \varphi}{\partial t} = 0$$

The little-used "Kirchhoff gauge" differs from Lorenz's condition only by a change in sign. Of more interest, in a lecture delivered in Göttingen in 1861 Riemann showed that the electrodynamic (vector) potential $\mathbf{u} = (u_1, u_2, u_3)$ could be chosen to satisfy a relation to the electrostatic scalar potential V which he wrote in the form

$$\frac{\partial V}{\partial t} = \frac{\partial u_1}{\partial x} + \frac{\partial u_2}{\partial y} + \frac{\partial u_3}{\partial z}$$

Riemann interpreted V as the density of the ether and $\mathbf{u}$ as the ether's flux or current intensity.[23] With modern symbols the relation can be expressed as

$$\nabla \cdot \mathbf{A} - \frac{\partial \varphi}{\partial t} = 0$$

Although Riemann's lectures were only published in 1875, in a sense he anticipated the Lorenz gauge. For the sake of completeness it should be mentioned that Helmholtz, in his important paper of 1870, made use of an equation corresponding to

$$\nabla \cdot \mathbf{A} + \frac{k}{c}\frac{\partial \varphi}{\partial t} = 0\,,$$

where k is an arbitrary number.[24] The Helmholtz gauge gives the Kirchhoff gauge for $k = -1$, the Coulomb gauge for $k = 0$, and the Lorenz gauge for $k = +1$.

What was the physical mode of action responsible for the generation of light? Lorenz found that it could be expressed in several ways but dismissed the question as unimportant because there was no reason, as far as he could tell, to prefer one physical hypothesis over another. As he wrote, "After careful investigation of this point, I have completely given up the idea of getting any good from physical hypotheses." Nevertheless, his paper did include such a physi-

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[23] Riemann (1875), p. 330. See also Wise (1981), p. 290.

[24] Helmholtz (1870), who did not cite Lorenz's paper.

cal hypothesis, namely that the mode of motion of light might be rotational rather than vibrational. This is what he had suggested four years earlier, in the context of his phenomenological theory of light, and he now returned to it:

> If we suppose light to consist of *rotating* vibrations in the interior of bodies, about axes which, according to the theory of electricity, we regard as directions of vibration, the electrical current is no translatory motion, but a rotation continued in one direction, and the axis of rotation becomes then the direction of the current. This rotation will only be continuous in good conductors, and the motion travel there in the direction of the axis, whereas it becomes periodical in bad conductors, and is propagated by what in electricity we call induction, in a direction at right angles to the axis of rotation.[25]

That is, Lorenz conceived a steady current to be a steady rotation continued along its own axis, whereas oscillatory currents would be propagated by electromagnetic induction perpendicular to the axis of the current and in this way constitute light.

While Lorenz's reasoning was not generally guided by physical models or analogies, it was guided by formal analogies. Since he had derived electrical equations identical to those he had earlier found for light, he inferred that light was made up of electrical oscillations. In his 1867 paper in the Danish journal *Tidsskrift for Physik og Chemi* the analogical form of reasoning appeared explicitly:

> Where, then, should we look for these periodic currents, propagating more easily the more poorly they are conducted in the body and only in a direction perpendicular to the current, if not in *the ray of light*? After all, the vibrations of light are periodical and perpendicular to the direction of light; moreover, light can only pass through extremely poor conductors.[26]

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[25] Lorenz (1867b), p. 301. For his earlier allusion to light being rotational, which was also an idea entertained by Ørsted, see Lorenz (1863).

[26] Lorenz (1867a), p. 5; Appendix C.

Of course, such an inference from analogy to identity is not logically justified; it rather amounts to a new hypothesis in disagreement to Lorenz's expressed desire of avoiding physical hypotheses. He seems not to have noticed or admitted the inherent conflict between his theory and his professed methodology. On the contrary, he claimed that the identity of light and electrical oscillations "indubitably follows" from the analogous forms of the involved equations.

Lorenz furthermore used a plenitude argument to the effect that, since propagation of electrical currents followed from the equations and hence was possible it had to correspond to a real phenomenon in nature. Lorenz thus formulated what in the history of ideas is known as the principle of plenitude: "What turns out to be possible in calculations deduced from really existing laws and conditions will always turn out to correspond to reality."[27] In his popular paper of 1867 Lorenz briefly discussed if the claimed identity of light and electrical vibrations could be confirmed by means of experiments. Given the high frequency of the vibrations a direct confirmation would scarcely be possible, but he vaguely suggested that photoelectric effects of some kind might provide indirect confirmation (see Appendix C).

In the conclusion of his 1867 paper Lorenz summarised the new theory and pointed out its incompatibility with the class of action-at-a-distance theories favoured by most German physicists: "Electrical forces require time to travel, and ... every action of electricity and electrical currents does in fact only depend on the electrical conditions of the *immediately surrounding* elements." He claimed no priority for this qualitative insight, for "This is well known to be an idea indicated by Ampère, and which several physicists, more particularly Faraday, have defended." Lorenz thought that retarded ac-

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[27] Lorenz (1867a), p. 5. The so-called principle of plenitude goes back to Leibniz and can be found even earlier. It states essentially that what is allowed according to the laws of nature and hence can potentially exist also has a real existence. Whether expressed explicitly or just used implicitly this metaphysical principle has played an important heuristic role in a wide range of sciences, including chemistry, astrophysics, cosmology and modern elementary particle physics. Sometimes it has worked, sometimes not.

tion might be valid for any kind of physical interaction and not only for electricity: "What Rømer taught us about light 200 years ago is valid also for the electrical forces … [and] for other forces such as the gravitational attraction."[28]

With respect to the novel feature of retarded potentials in Lorenz's theory it is worth pointing out that he was preceded by Riemann, who introduced the idea in a paper submitted to the Göttingen Academy (Königliche Gesellschaft der Wissenschaften zu Göttingen) on 10 February 1858. In fact, in a letter to Weber of 1845 Gauss had suggested that the potential at some point and at some time was due to the distribution of electricity elsewhere not at that instant but at an earlier time depending on the distance. He thus anticipated that the propagation of electrical action might take time, but neither Gauss nor Weber developed the idea into a scientific theory.[29]

Although Riemann was convinced that he had found the long-sought connection between electricity and light, he retracted the paper shortly after its submission. For this reason it was only published posthumously nine years later and, as it happened, in the very same issue of *Annalen* containing Lorenz's paper, which followed immediately after Riemann's. One can reasonably assume that Johann Christian Poggendorff, the editor of the journal, deliberately arranged the papers consecutively.

Much like Lorenz, Riemann based his work on "the assumption that the action of one electrical mass on the rest of them is not instantaneous, but is propagated to them with a constant velocity which, within the limits of error of observation, is equal to that of light."[30] The assumption implied that the electrical charge $q(t)$ in

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[28] The casual reference to the propagation of the gravitational force appeared in Lorenz (1867a) but not in Lorenz (1867b).

[29] See Kaiser (1981), p. 109. Gauss' letter was well known at the time and influenced Maxwell, among others.

[30] Riemann (1867), p. 237. The reason for his retraction of the paper may have been a rather elementary mathematical error, such as pointed out by Clausius in 1868, but it may also have been his failure of deriving Weber's force law, which is suggested in Goenner (2017). See also Rosenfeld (1979), first published in *Nuovo Cimento* in 1956, and Jungnickel and McCormmach (1986), pp. 179-181.

Figure 3.2: G. F. Bernhard Riemann (1826-1866), brilliant German mathematician and physicist. https://commons.wikimedia.org/wiki/File:Georg_Friedrich_Bernhard_Riemann.jpeg.

the Coulomb force had to be replaced by $q(t - r/c)$. Riemann found that the differential equation for the propagation of electricity was the same as the equation governing the propagation of light and possibly also of radiant heat. Also like Lorenz, he replaced Poisson's equation for the electrostatic potential with an equation containing the second-order time derivative of the potential. But contrary to Lorenz - who was of course unaware of Riemann's work prior to 1867 - he considered only the retarded form of the scalar potential φ and not that of the vector potential **A**. Moreover, he did not state the retarded scalar potential for a charge distribution in integral form, but only for a point charge, namely as

$$\varphi(\mathbf{x}) = \frac{f(t - |\mathbf{x} - \mathbf{x}'|/c)}{|\mathbf{x} - \mathbf{x}'|}$$

From 1858 until his premature death eight years later Riemann often lectured on electricity and magnetism, but without returning to the connection with the propagation of light.

Despite its somewhat sketchy character, Riemann's posthumous paper was well known and much discussed. It attracted the critical attention of, for example, Enrico Betti in Italy and Rudolf Clausius and Carl Neumann in Germany.[31] The latter took over from Riemann the idea of retarded potentials, but without drawing the conclusion that light was electrical in nature. Neumann tended to believe that the analogy between optics and electrodynamics was superficial and not of great physical interest. To Clausius, the analogy was not only superficial but non-existing. In a paper of 1868 he denied any connection between Neumann's theory and the propagation of light.[32] As far as Lorenz is concerned, when he became aware of Riemann's work in 1867 he ignored it and he never commented on it.

In what some authors call the Riemann-Lorenz formulation of classical electrodynamics, the scalar and vector potentials are the basic quantities from which all experimental facts can be deduced.[33] This is contrary to the usual formulation of the Maxwell equations, where the electric and magnetic fields **E** and **B** are the basic quantities. This formulation was not Maxwell's but was due to later physicists such as H. Hertz and O. Heaviside who believed that the potentials were unnecessary and hence avoided them.[34] In works from the turn of the century the Riemann-Lorenz approach was developed into the so-called Liénard-Wiechert retarded potentials found independently by Alfred-Marie Liénard in 1898 and Emil Wiechert in 1900. These potentials describe the Maxwell equations of arbitrarily moving point charges (electrons) in the Lorenz gauge. Wiechert referred in his derivation to Riemann, but not to Lorenz; and Liénard referred to neither Riemann nor Lorenz.

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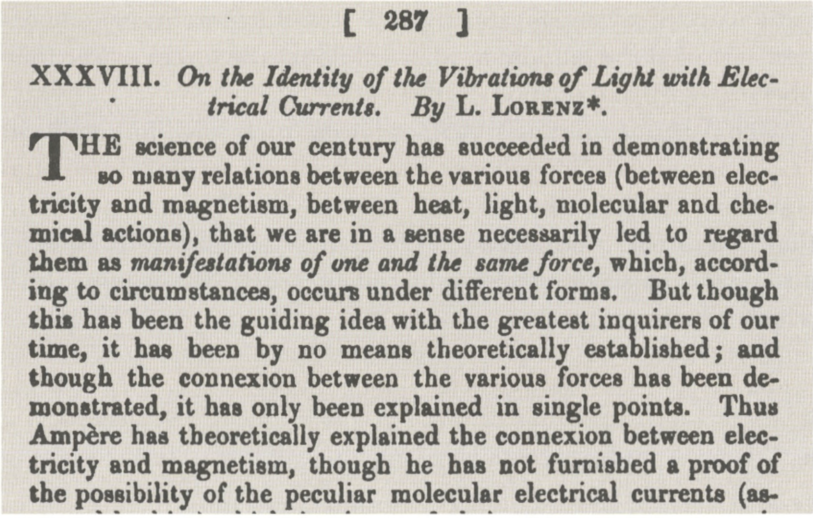
[31] Clausius (1868). See Kaiser (1978) and Kaiser (1981) for an account of Riemann's, Lorenz's and others' use of retarded potentials in electromagnetic theory.

[32] For Neumann's theory and the Neumann-Clausius dispute concerning the propagation of electromagnetic potentials, see Archibald (1986). In a letter to Thomson of 1868 Maxwell pointed out that "those who supposed that Neumanns [*sic*] potential travelled like light were greatly mistaken." Harman (1995), p. 500.

[33] Rosseaux (2005). See also Moon and Spencer (1954) who credit Riemann for the idea of retarded electrical action and also refer to Lorenz's paper of 1867.

[34] In classical electrodynamics the potentials φ and **A** could be treated as just mathematical constructs which did not, contrary to the field strengths, have direct physical effects. With the development of quantum mechanics the ontological status of the potentials changed. In particular, the Aharonov-Bohm effect illustrates that the potentials are physically real. For an argument that the Lorenz vector potential is physically meaningful even in the classical domain, see Giulani (2010).



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XXXVIII. *On the Identity of the Vibrations of Light with Electrical Currents.* By L. Lorenz*.

THE science of our century has succeeded in demonstrating so many relations between the various forces (between electricity and magnetism, between heat, light, molecular and chemical actions), that we are in a sense necessarily led to regard them as *manifestations of one and the same force,* which, according to circumstances, occurs under different forms. But though this has been the guiding idea with the greatest inquirers of our time, it has been by no means theoretically established; and though the connexion between the various forces has been demonstrated, it has only been explained in single points. Thus Ampère has theoretically explained the connexion between electricity and magnetism, though he has not furnished a proof of the possibility of the peculiar molecular electrical currents (as-

Figure 3.3: Lorenz's paper on the electrodynamic theory of light as translated from the German version in *Philosophical Magazine*.

Lorenz's paternity to the retarded potentials was not generally recognised and soon forgotten. All that the prominent German electron theorist Max Abraham had to say about the matter in his influential textbook *Theorie der Elektrizität* (Theory of Electricity) from 1905 was that the retarded potentials "have been employed by H. Poincaré, E. Beltrami, V. Volterra, H. A. Lorentz and others." And Heaviside, obviously unaware of the continental tradition, ascribed what he called the "progressive potentials" to FitzGerald.[35]

Indeed, FitzGerald became a leading proponent of retarded potentials which he first applied to the case of electrodynamics in a paper of 1883. He took over the concept from Rayleigh, who in his *Theory of Sound* from 1877 had used it to describe the propagation of mechanical waves. Although FitzGerald was probably unaware of Lorenz's work in 1883, he later became very interested in it. In a letter to Joseph Larmor dating from 1897 he wrote: "Have you seen the other (Copenhagen?) Lorenz's simultaneous-with-Maxwell's-

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[35] Abraham (1905), vol. 2, p. 59; Heaviside (1912), p. 452. For other erroneous statements about the priority of retarded potentials, see O'Rahilly (1965), p. 184.

work with these $f(t - r/c)$ functiwons? It is quite interesting. He entirely escaped the muddle in Maxwell about the forces at one time obeying $\Delta^2 = 0$ and at another $\Delta^2 = a^2 d^2/dt^2$."[36]

The American physicist A. Gordon Webster gave full credit to "L. V. Lorenz of Copenhagen" in a book on partial differential equations first published in 1927. Not only did Webster point out that Lorenz had introduced retarded potentials as early as 1861, he also devoted a section to what he called "Lorenz's equation," with which term he referred to

$$\Delta\varphi - \frac{1}{a^2}\frac{\partial^2\varphi}{\partial t^2} = -4\pi\rho(t)$$

This equation, he wrote, "was dealt with successively by L. Lorenz, Lord Rayleigh, H. A. Lorentz, Beltrami, and Poincaré, and has in the last twenty years become of great importance in connection with the theory of electricity, for both the scalar and vector potentials are propagated in accordance with it."[37] Elsewhere in his book Webster used the name "Lorenz-Beltrami equation" for the equation first stated by Lorenz in his 1861 paper on the elastic theory of light.

Lorenz sent a copy of his Danish 1867 article to Poggendorff in Berlin in order to get a German version of it published in *Annalen*. It would then be accessible to an international audience of physicists and supposedly attract their attention. If Lorenz had hoped that his theory would in this way arouse interest abroad, as he probably did, he must have been disappointed. His electrical theory of light was presumably well known to contemporary physicists, but only very few of them actually referred to it. Even worse, no one - including himself - sought to develop it. Somewhat remarkably, neither Clausius nor Neumann, nor for that matter Betti, mentioned Lorenz's work in their papers of 1868, although they almost certainly had read it. Neumann undoubtedly had, for in a long letter to Lorenz he

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[36] FitzGerald to Larmor, 21 November 1897, quoted in Hunt (1991), p. 42. FitzGerald's Δ^2 is the Laplace operator usually written as Δ or ∇^2.

[37] Webster and Plimpton (1947), p. 217. The book was originally published in German under Webster's name alone.

explained his own view concerning what he called the "progressive propagation of the potentials."[38] Nor did Helmholtz, in his important series of writings on electrodynamics, pay explicit attention to the Danish physicist.

On the other hand, young Hendrik A. Lorentz was aware of Lorenz's theory. In his doctoral dissertation from 1875, a detailed investigation of physical optics based on the electromagnetic theories of Maxwell and Helmholtz, he referred on the very first page to these two famous physicists and also pointed out with reference to the 1867 paper that "independently of Maxwell but following a different route the same results have been obtained by Lorenz."[39] Much later, in his Nobel Lecture delivered on 11 December 1902, Lorentz again referred to the Danish physicist. Perhaps to please his audience in Stockholm he dealt at some length with the nineteenth-century Swedish physicist and meteorologist Erik Edlund, who in the 1870s proposed an electrical theory based on the hypothesis of a single ethereal fluid consisting of what he called "molecules of ether."[40] According to Lorentz:

> Edlund went as far as to identify the electric fluid with the ether, ascribing to a positively charged body an excess of ether and to a negatively charged one a deficiency of ether. ... The way pioneered by Edlund, in which the distinction between ether and electricity was completely swept aside, was incapable of leading to a satisfactory synthesis of optical and electrical phenomena. Lorenz at Copenhagen came nearer to the goal. You know, however, that the true founders of our present views on this subject were Clerk Maxwell and Hertz.[41]

As far as Edlund's rather speculative theory is concerned, it differed in most respects drastically from Lorenz's. Not only did it presup-

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[38] C. Neumann to Lorenz, 4 September 1868 (Lorenz Papers, DTM).

[39] Lorentz's dissertation *De Theorie der Terugkaatsing en Breking van het Licht* (The Theory of the Reflection and Refraction of Light) is obtainable online as https://www.lorentz.leidenuniv.nl/history/proefschriften/sources/Lorentz_1875.pdf.

[40] See Edlund (1872). The idea of a molecular but non-electrical ether was popular in the earlier optical theories of Cauchy and others.

[41] Nobel Lecture, accessible online as https://www.nobelprize.org/nobel_prizes/physics/laureates/1902/lorentz-lecture.html.

pose the ether as a real substance, it also likened the free ether to a perfect electrical conductor. On the other hand, Edlund's theory shared with Lorenz's the general idea of retarded electrical actions, something which Lorentz highlighted in his Nobel Lecture albeit without acknowledging Lorenz's priority.

What was possibly the first reference to Lorenz's theory came from none other than Maxwell, who in a paper of 1868 briefly commented on it. "The propagation of attraction through space forms part of this hypothesis also," Maxwell wrote, "though the medium is not explicitly recognised."[42] Claiming that the continental theories were inconsistent with energy conservation and Newton's third law, he criticised Riemann and Lorenz for ignoring the action of the medium through which the electric disturbances propagated: "From the assumptions of both these papers we may draw the conclusion, first, that action and reaction are not always equal and opposite, and second, that apparatus may be constructed to generate any amount of work from its resources." In a letter to his friend Peter G. Tait, a mathematical physicist, he phrased the critique in different wording. The theories of Riemann and Lorenz, Maxwell wrote, would lead to the absurd consequence of "a locomotive engine fit to carry you through space with continually increasing velocity."[43] To stress the absurdity, he compared the consequence with "Gulliver's Travels in Laputa."[44]

At the 1870 meeting of the British Association for the Advancement of Science in Liverpool, Maxwell commented on the theories of electricity favoured by Weber and other scientists in Germany.

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[42] Maxwell (1868), reprinted in Maxwell (1965), Part 2, pp. 125-143, on p. 137. Lorenz was aware of Maxwell's critical remarks which he summarised in one of his undated notebooks but without commenting on them (Lorenz Papers, DTM).

[43] The letter of 12 March 1868 is reproduced in Harman (1995), pp. 353-355. Jackson and Okun (2001) point out that the criticism is unjustified as it ignores the electromagnetic momentum. They also make the unconvincing suggestion that Maxwell's criticism was a major reason why Lorenz's theory was soon forgotten (see below).

[44] In Jonathan Swift's *Gulliver's Travels* from 1726, Laputa is a flying island populated by people mastering the art of magnetic levitation - obviously a figment of imagination.

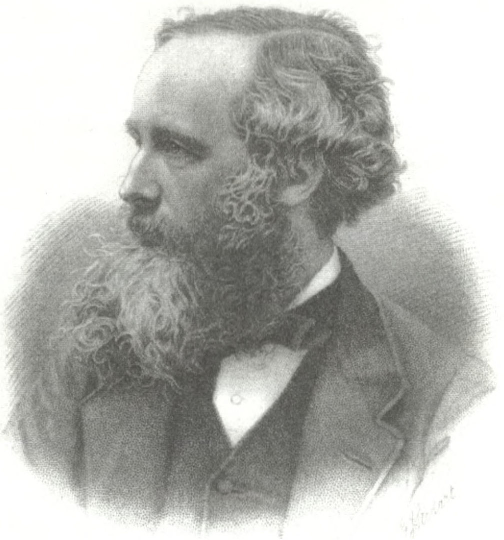

Figure 3.4: James Clerk Maxwell (1831-1879). Source: Front page, Maxwell (1965), first edition 1890.

Referring to the force between electrical particles he said, "according to a theory hinted at by Gauss, and developed by Riemann, Lorenz, and Neumann, [it] acts not instantaneously, but after a time depending on the distance."[45] Although Maxwell found the theory of "these eminent men" to be valuable and deserving of careful study, he much preferred his own field theory based on disturbances in the ethereal medium. Considering the two classes of theory to be temporarily equivalent, he philosophised:

> Both these theories are found to explain not only the phenomena by the aid of which they were originally constructed, but other phenomena, which were not thought of or perhaps not known at the time; and both have independently arrived at the same numerical result, which gives the absolute velocity of light in terms of electrical quantities. That theories apparently so fundamentally opposed should have so large a field of truth common to both is a fact the philosophical importance of which we cannot fully appreciate till we have reached a scientific altitude from which the true relation between hypotheses so different can be seen.

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[45] Maxwell (1965), Part 2, p. 228. For Gauss' anticipation of the retardation of electrical action, see his letter to Weber of 1845 as cited in Kaiser (1978).

Maxwell clearly realised that the situation in electrodynamics was underdetermined by the available empirical evidence (see also Section 2.2). Two very different theories may both agree with known phenomena within their common domain and also predict new phenomena, and yet they cannot be distinguished by these empirical factors alone. This is indeed a matter of "philosophical importance."[46] Three years later, in his classic *Treatise on Electricity and Magnetism*, Maxwell returned to Lorenz, who had found

> ... from Kirchhoff's equations of electric currents, by the addition of certain terms which do not affect any experimental result, a new set of equations, indicating that the distribution of force in the electromagnetic field may be conceived as arising from the mutual action of contiguous elements, and that waves, consisting of transverse electric currents, may be propagated, with a velocity comparable to that of light, in non-conducting media. He therefore regards the disturbances which constitute light as identical with these electric currents.[47]

Interestingly, Maxwell referred in both cases to Lorenz's theory as published in *Annalen* and not to the English translation in *Philosophical Magazine*. It was also the German translation which was carefully reviewed in *Fortschritte* by Gustav Radicke, a physicist from Bonn. Radicke seems to have been unconvinced of Lorenz's conclusion, which he realised built on a somewhat shaky foundation: "It appears to him [Lorenz] that he has *proved* (!) that the oscillations of light and electric currents are *identical*; and moreover, that the result has been achieved without assuming any physical hypothesis."[48]

Although Lorenz's theory of electrodynamics was coolly received, it was not ignored. Contrary to the silence of Neumann,

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[46] A philosophical analysis of underdetermination in classical electromagnetic theory is given in Pietsch (2012). The two different versions of the Lorenz-Lorentz formula, one optical and the other electromagnetic (Section 2.3), provide another example.

[47] Maxwell (1954), vol. 2, pp. 450. For Maxwell's objections to Lorenz's electrodynamics, see further McDonald (2016) and O'Rahilly (1965), pp. 182-185.

[48] *Fortschritte der Physik* **23** (1870): 197-200. The exclamation mark and the italics are Radicke's.

Clausius and Helmholtz, the prominent Leipzig physicist and astronomer Carl Friedrich Zöllner referred to the theory in some detail. A staunch advocate of Weber's force law Zöllner developed it into an atomistic theory according to which a neutral body consisted of an equal amount of positive and negative unit charges $\pm e$. With m denoting the mass of the hypothetical particle (and assuming $m_+ = m_-$) he derived in 1882 a remarkable relation which in a sense explained Newton's gravitational constant G in terms of electrical particles:

$$\frac{e^2}{Gm^2} \cong 3 \times 10^{40}$$

In a memoir of 1876 Zöllner reviewed Lorenz's *Annalen* paper, quoting extensively and approvingly from it. As he noted, "L. Lorenz in Copenhagen ... proceeds in his work essentially mathematically and denies explicitly all physical hypotheses." Zöllner apparently considered Lorenz's theory to provide support for his own view that "the so-called light ether is nothing but an aggregate of electrical molecular currents consisting of a very small number of electrical particles, at least of two combined into a doublet."[49]

Lorenz's theory also appeared in a major French textbook of 1882 on electricity and magnetism based on lectures given at Collège de France. The two distinguished authors, E. Mascart and J. Joubert, noted that Lorenz's theory was based on the notion of retarded actions and that it led to the consequence that electrical vibrations propagate in space with the velocity of light. "It thus leads to results which are completely similar to those which Maxwell has deduced from an entirely different theory."[50] Two years later, in a lecture series given at the University of Kiel, also Heinrich Hertz referred to the results that Lorenz had achieved "without any guidance." According to Hertz, "By advancing more in the direction of

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[49] Zöllner (1876), available online as Google Book. Because of his polemical and often unorthodox publications, some of them on spiritualist phenomena and others attacking Helmholtz, Zöllner was a controversial figure in German science. See Kragh (2012).

[50] Mascart and Joubert (1882), p. 688.

Riemann than that of Maxwell yet unaware of their results, he too realised that one can conceive of electrical oscillations as existing in the ether; and further, that if they exist they must propagate in accordance with the laws of optics. ... Those who later entered the field followed in the footsteps of these [three physicists] - the field had been discovered."[51]

In a paper from the same year Hertz commented on Lorenz's 1867 theory in relation to what he conceived as Maxwell's superior field theory:

> Similar laws for the propagation of potentials were proposed by Riemann in 1858 and by Lorenz in 1867, wanting to unify optical and electrical phenomena within the same framework. These investigators recognised that the laws involve the addition of new terms to the forces which actually occur in electrodynamics; and they justify this by pointing out that these new terms are too small to be experimentally observable. But we see that the addition of these terms is far from needing any apology as their absence would necessarily involve contradiction of generally accepted principles.[52]

Yet, to Hertz, who disliked the electromagnetic potentials and was convinced of the truth of Maxwell's theory, the Riemann-Lorenz approach was of historical interest only.

The situation was different in the late 1860s when continental physicists sometimes associated the electromagnetic theory of light with the names of Lorenz and Riemann rather than Maxwell. When the American genius Josiah Willard Gibbs during his tour to Europe 1866-1869 first encountered advanced electromagnetic theory, he studied the works of Kirchhoff, Riemann and Lorenz, but not those of Maxwell. According to Ole Knudsen, a historian of physics, "Gibbs' first encounter with the electromagnetic theory of light was almost certainly through Lorenz's paper. ... It is tempting to

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[51] Hertz (1999), p. 88.

[52] Hertz (1884). See also Kaiser (1978), p. 307. Hertz (1887) referred to Lorenz (1879a).

infer that Lorenz made a lasting impression on him."[53] Perhaps so, but the impression cannot have been very deep as Gibbs never referred to the Danish physicist in any of his scientific works.

On the whole, the electrical oscillation theory of light proposed by Lorenz attracted only moderate attention and latest by 1890 it had largely fallen into oblivion. Consider the influential and systematic report on electromagnetic theories that J. J. Thomson gave in 1885 to the British Association for the Advancement of Science.[54] Thomson covered in some detail the theories of, for example, Weber, Riemann, Clausius and C. Neumann, and also those of less well-known scientists such as H. Grassmann, J. Stefan and D. Korteweg. He paid particular attention to Helmholtz's theory and its relation to Maxwell's. But although his report was aimed to be comprehensive, for some reason he did not refer to Lorenz's memoir of 1867. At the time of Thomson's report Lorenz was well known to British physicists for his work on the determination of the electrical resistance unit (Section 3.4), but not for his earlier contribution to electrodynamics. The report probably had the effect of further marginalising his electrical theory of light in the British physics community.

The theory was not completely marginalised, though. Thus the German physicist Paul Volkmann, a professor at the University of Königsberg, was aware of Lorenz's theory to which he referred in a textbook on optical theory from 1891. Published at a time when electrodynamics was not yet equated with Maxwell's field equations, at least not in Germany, the book gives a fascinating insight in this transitional phase of optics. Referring to the recent development, Volkmann wrote that it demonstrated "the possibility of interpreting light as an electrical or respectively magnetic state of oscillation, that is, to conceive of a theory of light based on electricity and magnetism. This was the way in which the *electrodynamic theory of light* arose due to [the works] of Maxwell and L. Lorenz."[55] To Volk-

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[53] Knudsen (1987), p. 277.

[54] Thomson (1885).

[55] Volkmann (1891), p. 5, who referred to Lorenz's 1867 paper and also to some of his earlier papers on optics. On Volkmann's view on the foundation of optics and his

mann, apparently, Lorenz's theory was comparable to Maxwell's. Also another textbook of 1891, written by the Viennese physics professor Viktor von Lang, included Lorenz's theory alongside Maxwell's.[56]

But a few years later the situation had changed as Maxwell's electromagnetic ether came to dominate the field. "The study of electricity, magnetism, light and radiant heat is comprised as a whole by the physics of the ether," declared Paul Drude three years after Lorenz's death.[57] What Drude called the physics of the ether was just another name for Maxwellian electrodynamics. He consequently chose to disregard Lorenz's 1867 theory.

While it is understandable that Lorenz's alternative theory was forgotten after about 1890, it is more of a mystery why the theory attracted so little attention also at an earlier date. According to the American physicist J. David Jackson, a specialist in electromagnetism and its history, Maxwell's objections of 1868 were a major factor.[58] However, the suggestion is implausible given that at the time Maxwell's views held no particular authority and were neither well known nor much appreciated among German physicists. Maxwell's theory was generally thought to be very difficult and almost impenetrable. A German translation of *Treatise on Electricity and Magnetism* appeared in 1883, made by the physicist Max Weinstein,[59] but it was only with the publication of textbooks by Ludwig Boltzmann, Paul Drude and August Föppl in the 1890s that Maxwell's theory came to dominate the field. Not even in England did Maxwell's theory enjoy wide support until the 1880s. As is well known, William Thomson never accepted it.

In his own country Lorenz's electrodynamic theory was known through Valentiner's edition of *Oeuvres Scientifiques*, but even there, under the impact of the success of Maxwell's theory it was ignored or relegated to the past. For example, in a review of the history of

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role in German physics generally, see Jungnickel and McCormmach (1986), vol. 2, pp. 104, 144-148.

[56] Lang (1891), pp. 478-480.

[57] Drude (1894), p. 9.

[58] Jackson and Okun (2001), p. 671

[59] Maxwell (1883).

the electromagnetic theory of light dating from 1909, the author, a Danish physics teacher, found no place for Lorenz.[60] The physics professor Christian Christiansen knew Lorenz well and was influenced by him. In a textbook of 1897 he referred several times to Lorenz's works but, with regard to the electromagnetic theory of light he merely mentioned that "Maxwell and Lorenz showed that electrical oscillations may also exist in the dielectric."[61] In later editions of Christiansen's textbooks, the theory of light was dealt with exclusively from the Maxwellian point of view.

But as mentioned, Lorenz's electrodynamic theory was not completely forgotten in the early period following his death. We have referred to Volkmann and FitzGerald, and there were a couple of other significant exceptions to the silence. In a biographical essay of 1901 the prominent Maxwellian physicist Oliver Lodge referred to the modern idea of scalar and vector potentials propagating from the sources. "The very same scheme," Lodge commented, "had been proposed as early as 1867 by L. Lorenz ... [in] equations which would equally well represent the facts of electrodynamics for bodies at rest so far as at that time known, while it would also include an electric theory of light consistent with existing optical knowledge."[62] Lodge considered Lorenz's theory a "brilliant and powerful attempt at generalization ... precisely parallel analytically with the form of Maxwell's theory" and yet he concluded that it was self-contradictory from a physical point of view. His objection is worth recording *in extenso*:

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[60] Filskov (1909). "Today, no physicist has the slightest doubt ... that light consists of waves in the ether," he wrote, apparently unaware of Einstein's hypothesis of light quanta.

[61] Christiansen (1897), p. 228, a translation of a book published in German in 1894. Confusingly, in the English version the name of H. A. Lorentz is on one occasion (p. 265) misspelled "H. A. Lorenz." In Christiansen (1915), p. 350, a textbook widely used in Denmark, Lorenz was briefly presented as the one who had turned Ørsted's vision of a connection between light and electricity into a scientific theory.

[62] Larmor (1902), pp. ixx-lxiv, on p. lii. Lodge's biographical essay on FitzGerald was first published in *The Electrician* **46** (1901): 701-702.

> All true current was taken to be current of conduction, dielectric polarization not being contemplated; thus even in free space it was necessary to have conductivity, otherwise true current could never become established there. Yet if a distribution of current could be supposed thus established, it would travel according to the same laws as the light-vibration, as a result of the initial postulate that all the effects are propagated in time with a common velocity - provided however that the conductivity is small enough to be neglected in this latter connexion. But on the other hand, if the conductivity is indefinitely small it could never give rise to any electric flux to be so propagated; thus the hypothesis of free propagation in time is inconsistent with conduction.

Lodge suggested that if Lorenz had added a dielectric constant for matter, this would "at once bring his theory into conformity with Maxwell's, at the stage in which the latter was left by its author."

In 1902 the French physicist, chemist and polymath Pierre Duhem published a critical study of Maxwell's theory in which he included a detailed summary of Lorenz's electrodynamic theory. Duhem disliked Maxwell's electrodynamics which to his mind was plagued by inconsistencies and relied much too heavily on mechanical models and analogies. It was unsatisfactory from a logical and methodological point of view and yet Duhem admitted that it could not be ruled out on empirical grounds. He further objected in general terms that "in the theories of the propagation of electric actions proposed by B. Riemann, L. Lorenz, and Carl Neumann there is not a reality that travels through the space, but a fiction, a mathematical symbol, such as the potential function." As to Lorenz's theory, Duhem found it to be "certainly seductive" but he also argued that it faced great difficulties. His most serious objection was not unlike the one raised by Lodge:

> According to the previous theory [Lorenz's], in any very poor conductive medium, transverse electric currents always propagate with a speed equal to the speed of light in a vacuum. On the contrary, in a transparent medium, light travels with a speed that characterizes this medium and which is less than the speed of light in a vacuum; and we

> see no easy way to change the hypothesis of the previous theory so that this contradiction disappears.[63]

Based on this objection Duhem summarily concluded that one must "condemn irrevocably the electromagnetic theory of light proposed by L. Lorenz."

Maxwell published his electromagnetic theory of light in *Philosophical Transactions* in 1865, but it took several years until it attracted serious attention among physicists on the other side of the Channel. Certainly, in 1867 Lorenz was unaware of Maxwell's theory. While this is not remarkable, it is remarkable that he never referred in public to the electromagnetic theory of the Scottish genius. In fact, he only referred to Maxwell at two occasions and then it was in connection with the kinetic theory of gases.[64] His silence was not rooted in ignorance, though, for some of Lorenz's unpublished notes document that he had studied Maxwell's electromagnetic theory. He presumably realised that although his own equations for electricity corresponded to those in Maxwell's theory, the two theories differed greatly in form, content and physical interpretation.[65]

Key elements in Maxwellian electrodynamics such as dielectric polarization and the displacement current were absent in Lorenz's theory, which, although a theory of propagation of electrical signals, did not employ the notion of fields in the sense of Faraday and Maxwell. To Lorenz, the propagation of light was a result of conduction currents and therefore limited to a medium with some degree of uniform conductivity. The conductivity could be very small, but not zero. Contrariwise, Maxwell's theory applied to a perfectly non-conducting medium, a perfect dielectric, through which the oscillating fields propagated. Famously, the theory made Maxwell to express the velocity of light purely in terms of electromagnetic constants. With ε_0 denoting the vacuum permittivity and μ_0 the vacuum

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[63] Duhem (2015), pp. 150-155.

[64] Lorenz (1875b) and Lorenz (1877), p. 191.

[65] The relationship between Lorenz's and Maxwell's electromagnetic theories of light was examined in Valentiner (1897), which is a slightly expanded version of his comment in Valentiner (1898-1904), vol. 1, pp. 204-210.

permeability - where "vacuum" should be understood as free ether - he derived that

$$c = \frac{1}{\sqrt{\varepsilon_0 \mu_0}}$$

In unpublished notes of 1887 Lorenz derived the relationship between the dielectric constant and the refractive index that Maxwell had found on the basis of his field theory (Fig. 3.5).[66] By using his own theory of electrodynamics he derived an expression for the refractive index *n* and its dependence on the size of the molecular current elements. After having derived a similar expression for the dielectric constant in terms of the vacuum permittivity he concluded that the relative permittivity was given by

$$\varepsilon_r = \varepsilon / \varepsilon_0 = n^2$$

This is the very same relationship as the one obtained from Maxwell's theory, but Lorenz's method was much more cumbersome.

What the theories of Maxwell and Lorenz had in common was that they both denied the existence of action-at-distance forces of the kind favoured by many German physicists in the tradition of Weber. Lorenz argued that light was a manifestation of electrical currents, but he did not justify his theory in terms of its experimental consequences; nor did he suggest or predict that there might exist electromagnetic waves at other than optical wavelengths. In this respect Maxwell was a bit more specific, as he concluded in 1865 that "light itself (including radiant heat, and other radiations if any) is an electromagnetic disturbance in the form of waves propagating through the electromagnetic field according to electromagnetic laws."[67]

It was this prediction which Hertz verified in his series of bril-

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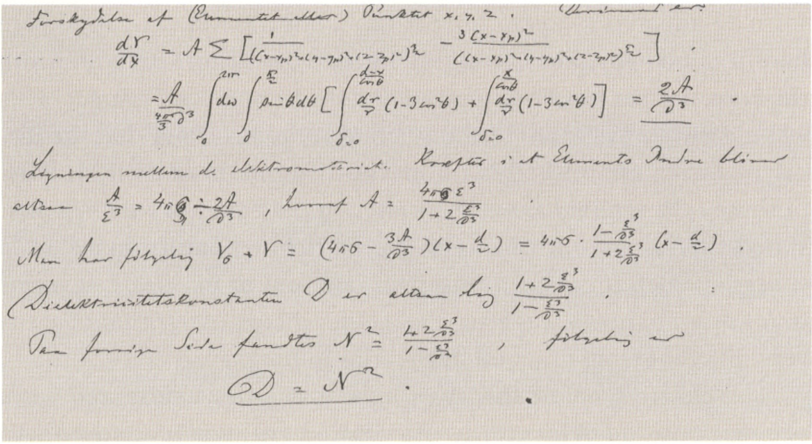
[66] "Elektricitetens Bevægelse i et System af Ledende, ved Tomt Rum Adskilte Smaadele" (The Motion of Electricity in a System of Conducting Elements Separated by Empty Space), 9 pp., dated 1 June 1887 (Lorenz Papers, RAS).

[67] Maxwell (1965), Part 1, p. 535.



Forskydelse af (Elementet eller) Punktet x, y, z. [illegible]

$$\frac{dr}{dx} = A\sum\left[\frac{1}{((x-x_n)^2+(y-y_n)^2+(z-z_n)^2)^{\frac{3}{2}}} - \frac{3(x-x_n)^2}{((x-x_n)^2+(y-y_n)^2+(z-z_n)^2)^{\frac{5}{2}}}\right].$$

$$= \frac{A}{\frac{4\pi}{3}\partial^3}\int_0^{2\pi} d\omega \int_0^{\frac{\pi}{2}} \sin\theta\, d\theta \left[\int_{\delta=0}^{\frac{d-x}{\cos\theta}} \frac{dr}{r}(1-3\cos^2\theta) + \int_{\delta=0}^{\frac{x}{\cos\theta}} \frac{dr}{r}(1-3\cos^2\theta)\right] = \underline{\frac{2A}{\partial^3}}.$$

Ligningen mellem de elektromotoriske Kræfter i et Elements Indre bliver altsaa $\frac{A}{\varepsilon^3} = 4\pi\sigma - \frac{2A}{\partial^3}$, hvoraf $A = \frac{4\pi\sigma\varepsilon^3}{1+2\frac{\varepsilon^3}{\partial^3}}$.

Man har følgelig $V_0 + V = (4\pi\sigma - \frac{3A}{\partial^3})(x-\frac{d}{2}) = 4\pi\sigma\cdot\frac{1-\frac{\varepsilon^3}{\partial^3}}{1+2\frac{\varepsilon^3}{\partial^3}}(x-\frac{d}{2})$.

(Dielektricitetskonstanten D er altsaa lig $\frac{1+2\frac{\varepsilon^3}{\partial^3}}{1-\frac{\varepsilon^3}{\partial^3}}$.

Paa forrige Side fandtes $N^2 = \frac{1+2\frac{\varepsilon^3}{\partial^3}}{1-\frac{\varepsilon^3}{\partial^3}}$, følgelig er

$$\underline{D = N^2}.$$

Figure 3.5: Lorenz's unpublished derivation of the relationship between the dielectric constant (*D*) and the specific refractivity (*N*).

liant experiments nearly a quarter of a century later and which were instrumental in turning Maxwell's theory into a standard theory of electromagnetism. Maxwell did not himself speculate any more than Lorenz did about the possibility of actually observing the propagation of electromagnetic waves. Another of Maxwell's theoretical predictions, dating from his *Treatise* of 1873, was that light should exert a tiny pressure on a macroscopic body which for strong sunlight he calculated to be 8.8×10^{-8} lb per square foot or 4.4×10^{-6} Pa. Maxwell did not consider the predicted effect to be measurable and it was only confirmed experimentally in the early years of the new century, principally by the Russian physicist Pyotr Lebedev. Lorenz's paper contained no similar prediction.

Lorenz's electrodynamic theory of light was in several ways remarkable, not least because it explicitly denounced the concept of the ether as a carrier of light as "unreasonable." It may in this respect appear very modern, even Einsteinian. However, although Lorenz rhetorically dismissed the ether as superfluous, in a sense he re-introduced it in his own way, as he filled the vacuum with conduction currents instead of conceiving it as an absolute void. As a kind of substitute for the ether he assumed at the end of in his 1867 paper that, "in the so-called vacuum there is sufficient matter to form an adequate substratum for the motion [of electricity]."[68] He had in mind some kind of highly rarefied gas "[not] different from the known gases," as he expressed it in his Danish 1867 paper.[69] This was clearly a physical hypothesis, contrary to his claim that the theory had been established "without the assumption of a physical hypothesis." Revealingly, two years later, in the paper in which he first stated the Lorenz-Lorentz formula, he defined empty space as "a space in which there is no *recognizable* amount of matter."[70] It seems that Lorenz's theory was after all an ether theory of a sort, if of a sort very different from other ether theories in the period.

The electrodynamic theory of light that Lorenz proposed has a peculiar position in his corpus of scientific writings. It was a work of genius, but an isolated one which he did not follow up upon or refer to in his later works. Perhaps characteristically, in his autobiographical sketch of 1877 he mentioned some of his papers in mathematics and physics, but among them were not his electrical theory of light published eight years earlier (see Appendix A). Lorenz's apparent lack of interest in his own theory is puzzling. In a perceptive study of the history of electrodynamics, the physicist Léon Rosenfeld argued in 1956 that Lorenz's identification of light with oscillating electrical currents was "by no means an isolated incident, a stroke of luck in Lorenz's career; it is the first, and most successful, stage in the quest of very wide scope pursued with remarkable singleness of purpose."[71] But although Lorenz's theory of 1867 was not a stroke of luck, neither was it a first stage of a planned research programme.

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[68] Lorenz (1867b), *Philosophical Magazine*, p. 301. Darrigol (2000), p. 213 points out that Lorenz "identified the optical ether with a bad conductor" and Wise (1981) similarly calls Lorenz's assumption "just another ether hypothesis, one in which ether and normal matter differ only in aggregation and not in substance."

[69] Lorenz (1867a), translated in Appendix C. Conceptions of the ether as a rarefied gas were known at the time and later in the century. For example, Edlund (1872) described the electrical ether like an ordinary gas, see Section 2.2. For other examples, see Kragh (1989).

[70] Lorenz (1869b), p. 23, emphasis added.

[71] Rosenfeld (1979), p. 151. Rosenfeld was a leading quantum theorist and a close associate of Niels Bohr. He was also a Marxist and his political-philosophical views shine through in some of the passages of the paper.

It was the product of his earlier phenomenological wave theory of light, but in so far as it was an electrodynamic theory it was singular, the first stage as well as the last stage.

Two years later Lorenz published his important work on refractivity leading to the Lorenz-Lorentz law (Section 2.3), but at the time without using his electrical light theory or referring to it. Also in his textbooks from the 1870s he did not touch on the subject. Only in a paper of 1879 on electrical oscillations did he briefly refer to the connection between light and electrical vibrations that he had investigated so thoroughly twelve years earlier.[72]

During the first half of the twentieth century attempts to revive interest in Lorenz's theory of electromagnetism were made by the mathematician Edmund Whittaker in his monumental *History of the Theories of Aether and Electricity* and later by the Irish physicist Alfred O'Rahilly in his *Electromagnetic Theory*. According to Whittaker, "the theory of L. Lorenz is practically equivalent to that of Maxwell, so far as concerns the propagation of electromagnetic disturbances through free aether." Yet he also pointed out that Lorenz's "theory lacks the rich physical suggestiveness of Maxwell's."[73]

O'Rahilly, in an ambitious but failed attempt to reform electrodynamics, agreed: "Starting with the Lorenz-Riemann generalisation from electrodynamics, we immediately deduce Maxwell's equations for vacuum - without so much as mentioning the so-called displacement currents." But to obtain the Maxwell equations from Lorenz's theory, O'Rahilly had to omit "irrelevant additions, such as his assumption of conducting matter distributed throughout space." In fact, this assumption was essential and far from irrelevant. Citing large passages from Lorenz's 1867 memoir, O'Rahilly argued that the theory of the Danish physicist was preferable to that of Maxwell on grounds of logic and simplicity. He obviously rated Lorenz's theory highly:

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[72] Lorenz (1879a), p. 187 in Valentiner (1898-1904), vol. 2, p. 187. See also the equally brief reference in Lorenz (1883).

[73] Whittaker (1958), p. 268. The book was first published in 1910.

> As a matter of fact, the view of Lorenz is accepted universally to-day, while there appears to be little or no realisation of the elementary inference that Maxwell's displacement-current is thereby rendered unnecessary. Everyone now uses the retarded potentials introduced by L. Lorenz.[74]

However, O'Rahilly's unorthodox attempt to establish electromagnetism on a non-Maxwellian basis in partial accordance with the views of Lorenz fell on deaf ears. A more critical and perhaps also more reasonable evaluation of Lorenz's theory was offered by Wilhelm Wien, a recipient of the 1911 Nobel Prize in physics. In a survey article on electromagnetic theories of light published in the multi-volume *Encyclopädie der Mathematischen Wissenschaften* he included a section on Lorenz's theory. Although Wien found it to be interesting and close to Maxwell's theory, he objected that the theory of 1867 was not really an electro-*magnetic* theory:

> It is inferior [to Maxwell's theory] in so far that it only takes into consideration the electrical and not the magnetic forces. In Lorenz's theory there is no appreciation of the important fact that, apart from the electrical force a magnetic force also exists which in transverse waves is orthogonal to the former. In this way one of the most important advantages of Maxwell's theory is lost, namely that the latter unifies the two complementary systems of the older theory, whose vectors too had to be assumed to be orthogonal.[75]

In fact, Lorenz consistently wrote of light as *electrical* vibrations and referred nowhere in his paper to magnetism or magnetic vibrations. The words simply did not appear in his 1867 paper and nor did terms such as "electromagnetic" and "electromagnetism." As mentioned, nor did the term "potential" appear.

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[74] O'Rahilly (1965), p. 189, first published in 1938. According to R. King, in a review of Pihl (1939) in *Isis* **40** (1949): 64-66, "If Maxwell had never lived, modern electromagnetism could have been developed from the work of Lorenz." However, this counterfactual scenario is highly unlikely. As pointed out by Chalmers (1973), Lorenz "was far from proposing a serious rival to Maxwell's theory."

[75] Wien (1909), p. 104.

3.3 The Lorenz number

In 1819, the French scientists Pierre-Louis Dulong and Alexis-Thérèse Petit concluded from a series of experiments that when the atomic weight of an element was multiplied by its specific heat, the number obtained was approximately the same for all elements. As Franz Neumann showed in 1831, the rule can be extended to include chemical compounds. If the specific heat capacity is denoted c and the atomic or molecular weight A, the empirical Dulong-Petit law states that

$$c \times A \cong 6 \text{ cal degree}^{-1}$$

However, useful as the law is, it was realised at an early date that it is approximate only and not valid for all elements and compounds.[76]

Another and even more interesting empirical law, this time relating the thermal conductivity κ of a metal to its electrical conductivity σ, was established by the Berlin physicists Gustav Wiedemann and Rudolph Franz in 1853. They found that at room temperature, "the conductivities of metals for electricity and heat are very closely related and are probably both functions of the same quantity."[77] That is, according to their measurements

$$\kappa/\sigma \cong \text{constant}$$

Suggesting some deep-lying connection between electricity and thermal phenomena the Wiedemann-Franz law attracted much attention. Although the law was confirmed by F. Neumann in 1862, for a while its validity was questioned. In 1880 Heinrich F. Weber raised serious objections but in a careful investigation published the following year Kirchhoff and his collaborator Gustav Hannemann proved beyond any reasonable doubt that the Wiedemann-Franz

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[76] See Fitzgerel and Verhoek (1960). Lorenz (1877), p. 80 noted that the mean value of the "atomic heat" was 6.38 with deviations up to 8 per cent or, in the case of boron and carbon even more.

[77] Wiedemann and Franz (1853), p. 531.

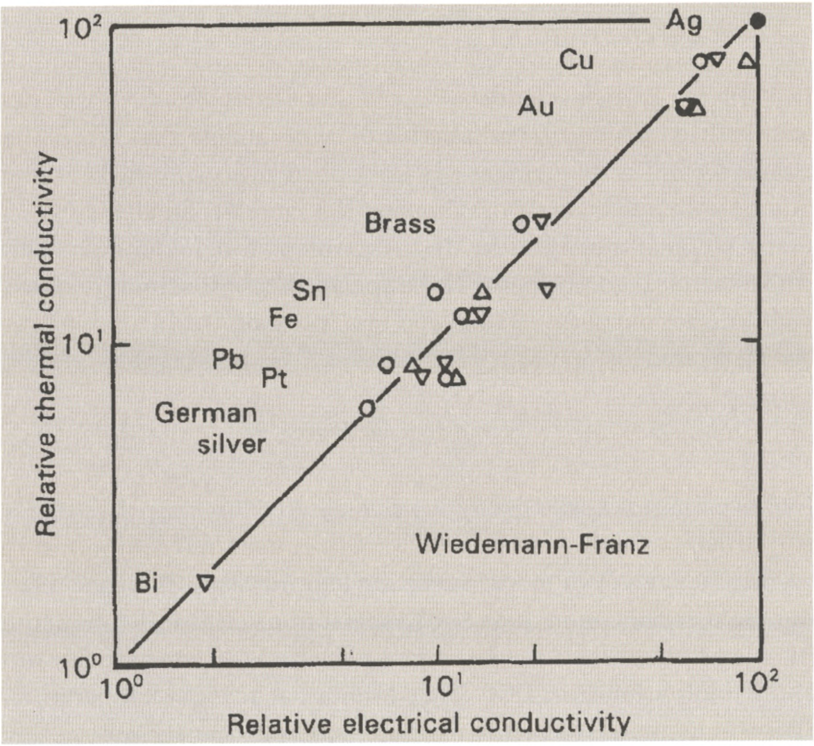

Figure 3.6: The Wiedemann-Franz law showing the linear relationship between the thermal conductivities of metals and their electrical conductivities as measured by early workers. Data from Wiedemann and Franz (1853).

law was correct.[78] Yet, although the empirical validity was thus confirmed, the law escaped theoretical explanation.

Lorenz shared the interest in the law, which he examined in papers of 1872 and 1881 which belong to what would later be called solid-state or condensed matter physics. The main result of Lorenz's work was an extension of the Wiedemann-Franz law to cover temperature variations. Briefly, Lorenz found that the constant in the law was proportional to the absolute temperature *T*. What is sometimes known as the Wiedemann-Franz-Lorenz law[79] states that

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[78] Kirchhoff and Hannemann (1881).

[79] The term "Wiedemann-Franz-Lorenz law" was first used in the 1910s but only

$$\kappa/\sigma = LT$$

The ratio $L = \kappa/\sigma T$ is today called the Lorenz number. It is almost but not quite independent of the temperature, as illustrated by silver where L at 100 °C is 1.03 times the value at 0°C. For $T \rightarrow 0$ K the Lorenz number has the numerical value

$$L = 2.44 \times 10^{-8}\ \mathrm{W\Omega K^{-2}}$$

While the term "Lorenz number" has won general recognition, in many textbooks and papers of both earlier and more recent dates the ratio $\kappa/\sigma T$ is referred to as the "Lorentz number."[80] This may be a reference to H. A. Lorentz's later work on the subject to be mentioned below, but it may also indicate that the author knows about the famous Dutch physicist and has never heard of the more anonymous Dane. In any case and priority apart, it is confusing with two names for the same quantity and especially so if the two different names appear in the same publication.[81]

Not unlike the motivation for his 1867 electrodynamic theory of light, Lorenz was in 1872 motivated by his wish to elucidate "the connection between various forces independently of all physical hypotheses." An important means for doing so would be to find absolute measures for heat and temperature, and to relate these to the absolute units which Gauss and Weber had introduced in the study of electricity and magnetism. Hitherto the degree of heat had only been determined arbitrarily, with the result that "the thread has been broken which connects heat with the other physical forces." Lorenz wanted to re-establish and strengthen the connection. "The object of the present investigation," he wrote, "is to establish a definition of the absolute degree of heat in a purely empirical manner,

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became more common in the 1960s. Most physicists use "Wiedemann-Franz law" for the extended version that includes Lorenz's temperature dependence.

[80] For example, Morse (1964), p. 372. Google Scholar gives a total of about 7,700 references to "Lorenz number" and 3,370 references to "Lorentz number."

[81] In Tritt (2004) some of the chapters use "Lorenz number" and others "Lorentz number."

and by introducing it into science to illustrate more clearly the relation in which heat and electricity stand to each other."[82]

To find a natural or absolute unit of heat he first examined relevant experiments concerning heat capacity and electrolysis, namely the data expressed in the Dulong-Petit law for gases at constant pressure and Faraday's electrolytic laws for simple electrolytes. In both cases he interpreted them in terms of the number of atoms rather than in stoichiometric terms. Since the absolute number of atoms was not known, he used what corresponds to the unit one-thousandth of a mole, for example the number of "atoms" (molecules) in 16 mg of oxygen or 1 mg of hydrogen.

For the absolute measure of a degree of heat Lorenz suggested as a definition that it should be "that increase in temperature which the unit of work produces, in being completely and exclusively changed into heat, in the same number of atoms of the element which the unit of electricity liberates from an electrolyte under normal circumstances." Having reviewed experimental data on the electrical and thermal conductivity of pure metals he noted that whereas the first quantity varied approximately inversely with the absolute temperature, the second did not vary significantly with the temperature. Without referring to either Wiedemann or Franz, in 1872 Lorenz stated his extension of the law named after them: "The conducting-power of a pure metal for heat and for electricity is proportional to the temperature calculated from absolute zero."

On the basis of his proposed absolute measures of heat and temperature units Lorenz found that the relationship could be expressed by defining the absolute temperature as

$$T' \equiv \kappa / \sigma$$

Moreover, Lorenz pointed out that in this way a "remarkable analogy" was established between the propagation of electricity and thermal energy. With *V* denoting the voltage, the heat and electrici-

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[82] Lorenz (1872a), p. 61, presented to the Royal Danish Academy on 8 March 1872. The paper appeared in a German translation the same year (*Annalen*) and the following year in English (*Philosophical Magazine*). See Pihl (1939), pp. 76-96 for details.

ty passing a unit area perpendicular to the *x*-axis could be written in the same forms, respectively as

$$-\sigma T'\frac{dT'}{dx} \quad \text{and} \quad -\sigma V\frac{dV}{dx}$$

In his textbook of heat from 1877 Lorenz suggested that "the heat measure and the electrical voltage thus become analogous quantities and are measured in the same units."[83]

While Lorenz's early work on thermal and electrical conductivity relied on experimental data from other physicists, in an extensive paper from 1881 he returned to the subject which he now based on careful experiments of his own. Although primarily a mathematical physicist, Lorenz was completely at home in the art of experiment and his skill as an experimenter is nowhere shown better than in the series of elaborate experiments he conducted throughout 1880 on the thermal and electrical conductivity of metals. This *tour de force* rested on innovative experimental methods combined with detailed and complex calculations of the involved experimental uncertainties.[84]

For the determination of the electrical conductivity Lorenz applied a method he had invented some years earlier to measure the conductivity of a mercury column with great precision (see Section 3.4). The method was particularly suited to measure very small resistances. But he was mostly concerned with determining the thermal conductivity of various metals at different temperatures. For this purpose he devised an apparatus in which thin metallic wires were placed in a thermostatic block in such a way that their resistance could be measured by means of thermoelectric methods.[85] In addition to the conductivity measurements Lorenz also made precision experiments on the specific heat capacity of the chosen metals. He singled out twelve metals, some of them chemical elements and

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[83] Lorenz (1877), p. 186.

[84] Lorenz (1881). Abridged German translation in *Annalen* **13** (1881): 422-447, 582-606 and reviewed in *Fortschritte* **37**, parts 1-2 (1882): 840-845. Lorenz's paper in *Annalen* followed immediately after the Kirchhoff-Hannemann paper.

[85] Many years later Lorenz's method was described in Christiansen's textbook in physics. See Christiansen (1915), pp. 195-197.

others alloys such as brass and so-called German or nickel silver (which is a Cu-Ni-Zn alloy in the mass ratio 3:1:1). For both the thermal and the electric conductivity he made measurements at 0 °C and 100 °C. Table 3.1 shows some of Lorenz's data in cgs units. The symbol c_0 stands for the specific heat capacity at 0 °C.

	κ_0	κ_{100}	κ_0/σ_0	$\frac{\kappa_{100}}{\sigma_{100}}:\frac{\kappa_0}{\sigma_0}$	c_0
Copper	0.7198	0.7226	1574	1.358	0.08970
Aluminium	0.3435	0.3619	1529	1.367	0.2043
Magnesium	0.3760	0.3760	1537	1.398	0.2438
Iron	0.1665	0.1627	1605	1.530	0.1050
Tin	0.1528	0.1423	1635	1.334	0.05360
Lead	0.0836	0.0764	1627	1.304	0.03077
Bismuth	0.0177	0.0164	1900	1.372	0.03014
Nickel silver	0.0700	0.0887	1858	1.314	0.09141
Red brass	0.2460	0.2827	1562	1.360	0.09005

Table 3.1. Lorenz's measurements of thermal and electrical conductivities of metals.

As it appears from the table, the ratio $(\kappa_{100}/\sigma_{100}):(\kappa_0/\sigma_0)$ was largely the same for all metals except iron. Lorenz assumed that the temperature dependence followed the linear relation

$$\kappa = K\sigma T\,,$$

where T is the absolute temperature and K a constant.

Much of Lorenz's long 1881 paper was concerned with the heat emitted from hot metals to the surrounding air.[86] In 1817 Dulong and Petit had presented an empirical law for the rate of cooling, relating it to the pressure of the air p and the temperature difference between the metal and the air. With T denoting the temperature of the metal and T_0 the temperature of the air they stated the law as

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[86] See Besson (2012) for an interesting study of the history of cooling laws mentioning Lorenz's contribution to the subject.

$$v = -\frac{dT}{dt} \sim p^a (T - T_0)^b ,$$

where a and b are constants to be determined experimentally. In a series of experiments with the cooling of metals in various gases, Lorenz investigated the heat loss from a heated vertical plate of height H freely exposed to air or other gases. Using various idealisations and approximations he derived a formula for the heat loss per unit area and time, which he expressed as

$$L = 0.548 \left[\frac{g c_p \rho^2 \kappa^3}{\eta H T} \right]^{1/4} \vartheta_0^{5/4}$$

Here g is the acceleration of gravity, c_p the heat capacity of the gas, ρ is density, κ its heat conductivity, and η its viscosity coefficient; T refers to the absolute temperature of the gas far from the plate and $T + \vartheta_0$ to the gas temperature very close to the plate.

In an important paper of 1915 Wilhelm Nusselt, a professor of engineering at the Technical University in Dresden, examined problems in natural and forced convective heat transfer by means of dimensional analysis.[87] It was in this paper that he introduced what came to be known as the Nusselt number for the ratio of convective to conductive heat transfer across a boundary. Nusselt referred to "Lorenz's well-known formula" and arrived at results that generalised and extended it. A more realistic version of Lorenz's heat conduction formula was tested by two physicists at the Technical University in Danzig (now Gdańsk in Poland) who found a satisfactory agreement between theory and experiment.[88] Lorenz's work is still cited in the scientific and engineering literature as a pioneer paper in the study of natural convective heat transfer.[89]

Lorenz determined the constant b in the Dulong-Petit formula to be 1.25 and the a constant to approximately 0.5 but depending on the nature of the gas. To get an improved law for the heat lost by the metal to the air due to natural convection he found it necessary to

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[87] Nusselt (1915). See also Eckert (1981).

[88] Schmidt and Beckmann (1930).

[89] E.g., Kacac, Yener and Pramuanjaroenkij (2014), p. 378.

make a correction accommodating recent results obtained by Josef Stefan in a critical examination of the Dulong-Petit cooling formula. In terms of the absolute temperature T Lorenz stated his improved cooling law as

$$-\frac{dT}{dt}=\frac{q\alpha}{mc}(T^4-T_0^4)+\frac{q\beta}{mc}(T-T_0)^{5/4}$$

Here m is the mass of the metallic body, q its surface area and c its heat capacity; α and β are two constants. For small values of the difference in temperature the expression yields Newton's law of cooling dating from 1701. According to this law the rate of heat loss of a hot body is proportional to the temperature difference between the body and its surroundings:

$$\frac{dT}{dt}=-k(T-T_0)$$

Lorenz's experiments showed the new law to be satisfied to a high degree of accuracy. For example, for a small cylinder with mercury in air heated to $T-T_0=6.9$ °C experiments gave a cooling time of 1207 s, while the formula predicted 1208 s. In a later investigation of the heat loss from incandescent filaments the cooling formula was confirmed over the different and much wider temperature range of approximately 600 to 1700 °C.[90]

We find in Lorenz's paper an early use of the method of similarity based on dimensional analysis. This important method was later applied in a more sophisticated form by a number of other physicists, first by Joseph Boussinesq in his classical text *Théorie Analytique de la Chaleur* published in 1901-1903. Like Lorenz, Boussinesq found that the net heat flux per unit surface of a solid body varied with the excess temperature as $(T-T_0)^{5/4}$.

The experiments conducted by Lorenz in 1881 offered support for the first term in the cooling formula, which he took over from Stefan's empirical T^4 law for heat radiation proposed in 1879. This law is today known as the Stefan-Boltzmann law, stating that the energy radiated by a black body per unit area and time is propor-

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[90] Hartman (1916).

tional to T^4. It originated as an empirical cooling law and only changed into a fundamental law of black bodies after Boltzmann in 1884 derived it on the basis of the second law of thermodynamics and Maxwell's theory of electromagnetism.[91] With the emergence of quantum theory it turned out that the constant γ in the Stefan-Boltzmann law

$$j = \gamma T^4$$

can be expressed in terms of Planck's constant h, the Boltzmann constant k_B and the velocity of light in vacuum c namely as

$$\gamma = \frac{2\pi^5 k_B^4}{15c^2h^3}$$

Lorenz was aware of Kirchhoff's fundamental studies of heat radiation and the definition of a perfect "black body" as one that absorbs all radiation falling upon it. He referred to blackbody radiation in his textbooks in physics, but of course he did not foresee and could not have foreseen what eventually would emerge from the study of this subject.[92] It was only after Lorenz's death that attempts to understand blackbody radiation in terms of fundamental physics became a hot research topic leading to Max Planck's celebrated law of 1900, the beginning of the quantum revolution. In this context it is not irrelevant to mention that Christiansen, Lorenz's younger friend and physics colleague, in 1884 made a significant contribution to blackbody physics by demonstrating that radiation trapped by a cavity has the same properties as blackbody radiation.

Lorenz's 1881 paper was primarily a contribution to solid-state physics, but as mentioned it also contained new insight in the theory of convective heat transfer. Moreover and perhaps more surprisingly, it has been assigned a minor place in the history of fluid dynamics. In his theoretical analysis of the experiments conducted in

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[91] See Brush (1986), pp. 478-523 for the history of the Stefan-Boltzmann law, the Dulong-Petit law and other works on cooling. Brush mentions Lorenz's cooling law on p. 517. For this law see also Pihl (1939), pp. 77-93.

[92] Lorenz (1876), p. 176; Lorenz (1877), p. 166.

Copenhagen, Lorenz engaged in hydrodynamic calculations which have been seen as an anticipation of the later concept of a boundary or transition layer. According to this idea there is a thin layer of the fluid in the immediate vicinity of a bounding surface where the effects of viscosity are significant. Frictional effects occur only in this layer and can be neglected elsewhere. The crucially important idea of a viscous boundary layer is usually attributed to the famous German physicist Ludwig Prandtl who introduced it in a talk of 1904 to a mathematical audience. At the time Prandtl was not aware of Lorenz's paper, but he later studied it and generously - perhaps too generously - referred to it as "the first paper on free heat convection and at the same time the first on boundary layers!"[93]

However, it is questionable if Lorenz's work of 1881 really contains the idea of a boundary layer, which nowhere appears explicitly in the paper. Although some experts believe that Prandtl's assessment is justified in so far that the paper "contains the prototype of the boundary-layer concept," other experts disagree and refer to other and more likely precursors such as William Rankine and Boussinesq.[94]

While Lorenz had proposed his extension of the Wiedemann-Franz law with some reservation in 1872, in his paper nine years later he was much more confident. He now felt justified to refer to it as a new empirical law. Empirical it was, but Lorenz wanted to understand it. At the end of his paper he suggested a theoretical interpretation in terms of thermoelectric forces and what he called "molecular potential differences" across double layers. Speculating that metals have a discontinuous structure he argued that when a current passes from one layer to another heat is generated or lost. In 1834 the French physicist Jean-Charles Peltier had discovered that in a circuit consisting of two different conductors one of the junctions is cooled and the other heated. Lorenz may have had in mind a kind of microscopic Peltier effect. However, his interpretation was

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93. Tani (1977); Van Dyke (1994). See also Darrigol (2005), pp. 282-292 for Prandtl's work and the history of the boundary-layer approach in fluid dynamics.

94. Tani (1977), p. 91; O'Malley (2014), p. 9.

neither well known nor generally accepted (although Niels Bohr found it to be "very interesting," see below).

About two decades after Lorenz's death, the new electron theory of metals promised a microscopic explanation of the WFL (Wiedemann-Franz-Lorenz) law, just as Boltzmann in 1877 had explained the Dulong-Petit law on the basis of statistical mechanics. In an important paper of 1900 the Leipzig physicist Paul Drude combined the kinetic theory of gases and the electron hypothesis to explain metallic conduction and derive the constant of the WFL formula.[95] His formula was widely acclaimed and received experimental support from measurements made at Berlin's Physikalisch-Technische Reichsanstalt. Four years later H. A. Lorentz attacked the same problem on the basis of a more sophisticated theory assuming only negative electrons as the carriers of electric conduction (Drude's electrons were positive as well as negative). With e denoting the elementary charge and k_B the Boltzmann constant, the two expressions for the κ/σ ratio were

$$\left(\frac{\kappa}{\sigma}\right)_{\text{Drude}} = \frac{4}{3}\left(\frac{3k_B}{2e}\right)^2 T \quad \text{and} \quad \left(\frac{\kappa}{\sigma}\right)_{\text{Lorentz}} = \frac{8}{9}\left(\frac{3k_B}{2e}\right)^2 T$$

Strangely, Lorentz's more comprehensive and rigorously derived theory resulted in a coefficient that agreed less precisely with measurements than Drude's theory. For the term $3k_B/2e$ in cgs units the results of the two physicists were

$$\text{Drude} = 4.42 \times 10^{-7}, \;\; \text{Lorentz} = 5.39 \times 10^{-7}$$

The modern value is 4.3×10^{-7}. By 1910 the WFL law had not yet been satisfactorily explained and thus constituted an empirical anomaly relative to existing theory.

The puzzle was recognised by 26-year old Niels Bohr in his doctoral dissertation on the electron theory of metals, written under the

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[95] For the early electron theory of metals, see Wiederkehr (2010) and Eckert et al. (1992). Lorentz (1909), pp. 64-67 summarised the situation at a time when quantum theory was known but not yet applied to the problem.

supervision of Christiansen, in which he carefully discussed the theoretical predictions of Eduard Riecke, Drude, Lorentz and other electron theorists. In an earlier examination paper of 1909 for the master's degree, he listed the experimental data from Lorenz's 1881 paper, which he had evidently studied with care. Bohr also cited a later work by Lord Rayleigh, noting that "Lorenz, by the way, has carried out exactly the same calculations as Lord Rayleigh. However, he [Lorenz] was not particularly concerned with alloys, but used his calculations to give a very interesting justification for the Lorenz law for metals in general."[96] In lectures on the electron theory of metals in the spring of 1914, Bohr dealt in detail with "Lorenz's law" and the different results of the Lorenz number obtained by Drude and Lorentz.[97]

The WFL problem was a contributing factor to the crisis in classical electron theory and the recognition that somehow the new and strange quantum theory had to be taken into account. Only with Arnold Sommerfeld's celebrated quantum-mechanical theory of metals from 1927 was the problem solved, at least by and large.[98] By replacing the Maxwell-Boltzmann statistics with the new Fermi-Dirac quantum statistics, the German physicist derived a substantially better expression for the κ/σ ratio than previous physicists, namely

$$\frac{\kappa}{\sigma} = \frac{\pi^2}{3}\left(\frac{k_{\mathrm{B}}}{e}\right)^2 T = 1.46\left(\frac{3k_{\mathrm{B}}}{2e}\right)^2 T$$

He thus derived for the Lorenz number the theoretical value

$$L = \frac{\pi^2}{3}\left(\frac{k_{\mathrm{B}}}{e}\right)^2$$

As was customary at the time and is still customary, Sommerfeld referred to the formula as the Wiedemann-Franz law without men-

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[96] For an English version of Bohr's examination paper, see Bohr (1972), pp. 131-160. Bohr also referred to Lorenz's work in his dissertation of 1911. Rayleigh (1896) did not mention Lorenz's 1881 paper of which he was most likely unaware, possibly because it was not translated into English.

[97] Bohr (1972), pp. 445-456.

[98] Sommerfeld (1927).

tioning Lorenz. "The statement of the Wiedemann-Franz law," he wrote, "is that κ/σ is universal and proportional to the absolute temperature." Later investigations showed that the Lorenz number $\kappa/\sigma T$ departs slightly from the Sommerfeld value.

3.4 Absolute electrical resistance

The need for a standard unit of electrical resistance was felt as early as the 1840s, when physicists suggested the first albeit hopelessly inadequate definitions.[99] In 1860 the German engineer and industrialist Werner Siemens advocated the use of a mercury column 1 m long and 1 mm^2 in cross section at 0 °C, the so-called Siemens unit which was popular in Germany in particular (and should not be confounded with the present SI unit for electrical conductance). The question was also taken seriously in Great Britain, where a Committee on Standards of Electrical Resistance was formed in 1861 under the auspices of the British Association for the Advancement of Science. Among the members of the original committee were Maxwell and William Thomson, and later members included Rayleigh, Oliver Lodge and J. J. Thomson. In his ground-breaking *Treatise on Electricity and Magnetism* from 1873 Maxwell dealt in some detail with the ohm unit. He characterised the determination of a conductor's electrical resistance as no less than "the cardinal operation in electricity, in the same sense that the determination of weight is the cardinal operation in chemistry."[100]

The work of the British physicists resulted in the "BA unit" which in its material form consisted of reproducible standard coils made of platinum-silver wire. The unit proposed in 1861 was *defined* to be 10^9cm s^{-1} or 10^9 cgs units, but experiments gave values 1-2 per cent smaller. Although the BA ohm was adopted by the big British cable companies and thus was commercially successful, the discrepancy puzzled the physicists. Later on the British Association committee realised that its original standard unit was imprecise and

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[99] For a historical review of the unit of electrical resistance, see Brooks (1931). Rayleigh's important work in this area is treated in Schaffer (1994) and Davis (2017).

[100] Maxwell (1954), vol. 1, p. 465.

turned to the rival mercury standard. By the early 1880s there were basically two resistance units related as

$$1 \text{ Siemens unit} = 0.9540 \text{ BA unit}$$

Whether one unit or the other, it could be expressed in absolute electromagnetic (cgs) units, but there was no international agreement of whether or not the absolute system should be adopted as the official one. Incidentally, the well-known symbol Ω for the ohm unit was first suggested in 1867, based on the phonetic resemblance of the words ohm and omega.

In October 1882 Lorenz stayed in Paris, the city in which he had done post-graduate studies in theoretical physics more than two decades earlier. The occasion was a meeting of an international commission on electrical units, a result of the Electrical Congress held in connection with the 1881 Paris Electrical Exhibition (see also Section 4.1). The congress had decided to keep the names ohm and volt for the units of resistance and electromotive force, and to adopt the name ampere for the unit of electrical current. As far as the ohm unit was concerned, it was generally agreed to follow Siemens' idea of defining it in terms of a mercury column at 0 °C and of cross section 1 mm^2. However, the exact length of the column was a matter of discussion and so was the relation of Siemens' unit to the BA unit. A major aim of the 1882 International Conference was to establish a reproducible standard of the ohm which could be expressed in the absolute electromagnetic system. More specifically, the aim was to determine the length of the mercury column to have a resistance of 1 ohm or 10^9 absolute cgs units.

The International Conference, arranged by the French Foreign Ministry, was attended by 47 delegates from many European and a few non-European nations including some of the period's most eminent physicists, chemists and engineers.[101] Among them were William Thomson from the United Kingdom and Jean-Baptiste Dumas from France. Henry Rowland participated from the United States and the German delegation counted luminaries such as Siemens,

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[101] Cochery (1882); *Science* **1** (1883): 87-89.

Helmholtz, Wiedemann and Kohlrausch. Rayleigh had attended the electrical congress in 1881, but because of sickness he was prevented from joining the 1882 meeting (and thus also from meeting Lorenz). According to Simon Schaffer, a historian of science,

> In hotel lobbies and around marble-topped tables in nearby restaurants, the physicists energetically touted the rival virtues of the absolute system whose values Rayleigh had established and the conventional, mercury, system which Siemens' networks used. … Thomson and his allies denied that the mercury standard was either easy to understand or sufficiently accurate to serve the purposes of energy physics. … The international community developed a compromise which embodied the varying interests of the laboratories, engineering works and rival states.[102]

At the end of the conference the members of the commission were received by the president of France at the Palais d'Elysées followed by a lavish reception.

Denmark sent two delegates, Lorenz and Niels H. Hoffmeyer, the latter a former army captain who at the time served as director of the Danish Meteorological Institute.[103] While Hoffmeyer did not participate in the discussions, Lorenz was a central figure to whose work the German, French, Italian and British delegates made frequent references. Siemens and Wiedemann reported on what the latter called "modifications of the beautiful method due to M. Lorenz." According to Éleuthère Mascart, a noted French physicist, "among the methods using induction, Lorenz's has the greatest

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102. Schaffer (1994), p. 282.

103. The Meteorological Institute was founded in 1872 on the initiative of Hoffmeyer (1836-1884), who was also its first director and upon his death was succeeded by the physicist Adam Paulsen (1833-1907). The institute was originally placed under the Ministry of Naval Affairs. Hoffmeyer introduced telegraphic service in Danish meteorology, made useful weather charts and participated in several international conferences. He was a prominent member of the International Meteorological Organization founded in 1873 and when the organization met in Copenhagen in 1882 to establish the first International Polar Year, he presided over the meeting. However, Hoffmeyer had no competence in electrical science and technology.

theoretical simplicity and also offers the greatest guarantees." Other of the commission members shared the interest in Lorenz's work on the determination of the ohm unit, on which he gave an address at the Paris meeting.[104]

The October 1882 conference was followed by another conference in Paris two years later, again including Thomson, Helmholtz and other leading physicists. As a compromise it was decided to adopt 106 cm for the length of the mercury column of resistance 1 ohm. Lorenz was invited to the 1884 conference but for unknown reasons he was unable to come. He did however communicate with the commission in the form of letters and also reported on his recent experiments made in Copenhagen with a new apparatus. The following year he published a full account of the experiments in the transactions of the Royal Danish Academy which also appeared in German in *Annalen der Physik und Chemie*.[105] What by the early 1880s had become the celebrated Lorenz method dates from a paper he published in 1873 on the electrical resistance of a mercury column expressed in absolute measure.[106]

As he had first done in his 1872 paper on the determination of specific heats, Lorenz pointed out that existing determinations of the ohm unit were all based on the use of varying electrical currents and that this might be a reason for the discordant results. He thought, erroneously, that the discordances were due to the influence on the resistance from the variability of the currents. A constant electromotive force producing no current would be an advantage, he suggested. Moreover, the fact that the dimension of resistance was that of a speed (cm/sec) in the absolute system indicated how to design an apparatus by means of which the resistance could be expressed by mechanical quantities.

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104. Cochery (1882), pp. 25-28, which is an abridged version of Lorenz (1882).

105. Cochery (1884), pp. 34-36, entitled "Resumé des Expériences sur la Détermination de l'Ohm." For the full exposition, see Lorenz (1885). Parts of Lorenz's letters of 25 and 27 April 1884 are reproduced in Cochery (1884).

106. Lorenz (1873), reviewed in *Fortschritte der Physik* (1873), pp. 730-732 by F. Kohlrausch, who judged Lorenz's method to be "new and very simple and elegant." For a description of the method, see also Christiansen (1897), pp. 207-208 and Larsen (1915).

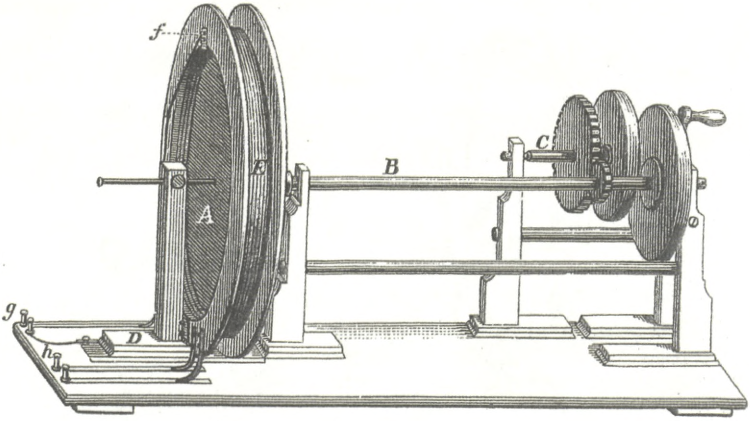

Figure 3.7: Lorenz's first apparatus for measuring the ohm unit as described in Lorenz (1873).

To turn his design into a reality, Lorenz made Jürgensen's workshop in Copenhagen construct an apparatus in which the central part was a rotating copper disc placed in a magnetic field. In the Lorenz apparatus, the field was produced by a battery-generated current circulating through a large, flat coil placed coaxially with the disc (Fig. 3.7). The mercury resistance that was to be measured was inserted into the circuit. By *n* uniform rotations of the copper disc a voltage *E* is induced in the disc given by

$$E = M \times i \times n\,,$$

where *i* denotes the current through the coil and the mercury resistance. *M* is the mutual inductance between the coil and the disc, and can be calculated from their dimensions alone, such as Lorenz did in a series of complex calculations. By varying the speed of rotation a condition can be reached where there is no current flowing, meaning that the electromotive force is equal but opposite to the fall of voltage in the resistance. At this stage the resistance is found to be

$$R = n \times M$$

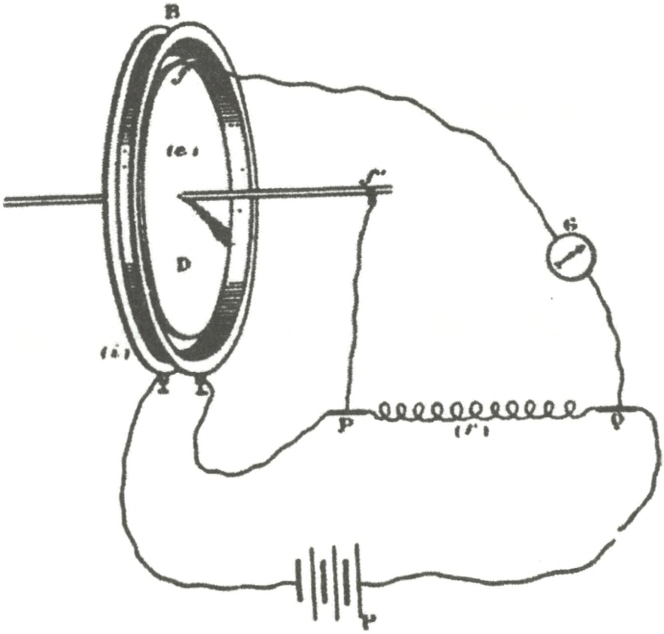

Figure 3.8: The Lorenz method for resistance measurements. The electromotive force generated across the rotating copper disc D is balanced against that produced by the current through the resistance PQ, which carries the same current as that through the coils B. Source: G. Lippmann, *Unités Electriques Absolues: Lecons Professées a la Sorbonne* (Paris: Carré et Naud, 1899), p. 154.

Lorenz used a mercury column of standard dimensions encapsulated in a glass pipe. The result of his experiments with the new apparatus was that the ohm unit corresponded to a length of 107.1 cm, somewhat longer than found by British and German physicists. Differently phrased and in his own words, "As the mean value of five experiments it turns out that 1 Q.E. [Siemens units] = 0.9337 O.E. [ohm], or that the mercury unit is equal to 0.9337×10^{10} absolute units."

The esteemed chemist Julius Thomsen, professor and newly appointed director of the Polytechnic College, shared Lorenz's interest in electrical and thermal properties of metals. Thomsen examined in the early 1880s the mechanical equivalent of heat as measured by the electromotive force of various voltaic cells, a line of work which naturally related to Lorenz's resistance measurements. In a letter to Thomsen of late 1882 Lorenz reported on his latest results:

> My measurements have drawn out longer than I expected; the reason is that I have applied two new methods and has used the opportunity to study them closely. I contemplate to submit a communication to Wiedemann's Annalen. I dare say that I have obtained a very high precision in the measurements. In particular, the method by means of which I have determined the changes in the resistance at various values of the current is so sensitive that I can easily observe a change of 1/100 per mille. The results have surprised me greatly, for the increased resistance turned out to be much less than I expected. ... I have found the resistance of the platinum wire in water at 18 °C to be 1.5519 in the British Association unit. The standard [which I used] is from Elliot Brothers, verified by Lord Rayleigh in Cambridge and has a very small temperature coefficient which has been determined by Rayleigh. According to the best and most recent experiments by H. Weber in Braunschweig and Lord Rayleigh, 1 Brit. Assoc. unit = 0.9865 ohm. The resistance is thus 1.5309 ohm or, with gram, centimetre and second as absolute units, 1.5309×10^9 cm/sec. The resistance increases with 0.184 per cent for every degree. As shown by the small temperature coefficient the wire cannot be made of pure platinum, for like all pure metals it has a temperature coefficient which is nearly twice as large.[107]

In his letter Lorenz compared his measurements of the electrically determined mechanical equivalent of heat with the value found by Thomsen. Lorenz reported a best value of 436.1 gram-meter (or 4.27 joule) against Thomsen's 429.13 gram-meter (or 4.21 joule). He was confident that his own value was superior, although in fact it was not.

At the request of the International Congress the Danish government allocated the substantial sum of 5,000 kroner to Lorenz to make an improved version of his apparatus and use it for precise

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[107] Lorenz to Thomsen, 17 December 1882 (Uppsala University Library). See also Kragh (2016), pp. 251-252.

Figure 3.9: The original Lorenz machine in its improved form kept at the Danish Museum of Science and Technology in Elsinore. The brass cylinder of length 1 m and diameter 0.3 m is wound with a single layer of thin copper wire. The cylinder was rotated by an electric motor with a frequency of 250 per minute and connected directly to the shaft of the cylinder. Published with permission of the Danish Museum of Science and Technology.

resistance measurements.[108] The work was done at the chemistry laboratory of the Military High School and supported by the laboratory's director Haldor Topsøe, who provided the chemically pure mercury. The new apparatus was larger and more sophisticated than the earlier one if also less transparent (Fig. 3.9). Whereas Lorenz had turned his original apparatus by hand, the new one used an electromotor for the rotation of the copper disc and a much larger, cylindrically formed coil.

As Lorenz reported to the commission in Paris, his first measurements from the spring of 1884 resulted in a length of the ohm mercury column of 106.19 cm. His final result was a little less, namely

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[108] Lorenz (1885). See Larsen (1915) for an excellent overview of Lorenz's apparatuses and his work on determining the unit of electrical resistance.

105.93 cm. Other early measurements with the Lorenz method made in Cambridge, Baltimore and St. Petersburg gave largely concordant results. It later turned out that Lorenz's value was slightly inaccurate because he did not sufficiently take into account that the materials in his apparatus must be absolutely non-magnetic.

Name(s)	*Year*	*Resistance BA units*	*Mercury length (cm) of 1 ohm*	*Country*
Lorenz	1873	--	107.10	Denmark
Sidgwick & Rayleigh	1883	0.95412	106.23	England
Rowland & Kimball	1883	0.95388	106.32	USA
Mascart, Nerville & Benoit	1884	0.95374	106.33	France
Lorenz	1884	--	106.19	Denmark
Lenz	1884	--	106.13	Russia
Strecker	1885	0.95334	--	Germany
Lorenz	1885	--	105.93	Denmark
Kohlrausch	1888	0.95331	106.32	Germany
Glazebrook & Fitzpatrick	1888	0.95352	106.29	England

Table 3.2. Measurements of the unit of electrical resistance, 1873-1888.

While Lorenz did not concern himself with resistance measurements after 1885, his method quickly won acceptance abroad. Rayleigh had in his early experiments used a rotating coil apparatus originally conceived by Maxwell, and with this method he found 1 BA unit = 0.98651 ohm. Having acquainted himself with Lorenz's method, Rayleigh found it to be "ingenious." In a systematic comparison from the fall of 1882 of the different methods for determining the ohm unit, he wrote as follows:

> This method [Lorenz's], which, with the introduction of certain modifications not affecting its essential character, I am disposed to consider the best of all, was proposed and executed by Lorenz, of Copenhagen, in 1873. ... On the whole, I am of the opinion that if it is desirable at the present time to construct apparatus of the most favourable scale, so as to reach the highest attainable accuracy, the modification of Lorenz's method ... is the one which offers the best prospect of success.[109]

In his careful examination of Lorenz's method, Rayleigh pointed out various difficulties, such as the influence of terrestrial magnetism and thermoelectric effects due to the sliding contacts. He further noticed that in Lorenz's construction, the ratio of the coil and the disc was not optimal. Rayleigh consequently proposed a different arrangement with two induction coils and the disc placed between them.

Neither in his communications to the International Conference nor in his 1885 paper did Lorenz comment on Rayleigh's constructive criticism, although he was undoubtedly aware of it. Lorenz's apparatus completed in 1884 did not take into account any of the improvements suggested by Rayleigh and he also did not contact the British physicist by means of letters (see also Section 2.4 for Lorenz's curious reticence with regard to Rayleigh - and vice versa). On the other hand, he was in contact with a few other physicists involved in research concerning absolute resistance measurements. One of them was the younger Italian physicist Antonio Roiti, of the University of Florence, whom he had met at the 1882 conference in Paris and with whom he corresponded.[110] Roiti originally favoured a different experimental method but came to admire Lorenz's alternative and his work in physics generally.

Two years after Lorenz's death an international congress met in Chicago under the presidency of Helmholtz. Representatives from

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[109] Rayleigh (1882), pp. 338 and p. 346; Sidgwick and Rayleigh (1883). See Davis (2017) for further details.

[110] Letters from Roiti to Lorenz of 5 January and 21 January 1885 (Lorenz Papers, RAS). Roiti (1843-1921) was internationally oriented and an outstanding teacher, one of his students being the mathematician Vito Volterra.

the major scientific nations decided to define an "international ohm" as follows: "As a unit of resistance, the *international ohm*, which is based upon the ohm equal to 10^9 c.g.s units of resistance of the system of electro-magnetic units, and is represented by the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice, 14.4521 grammes in mass of a constant cross-sectional area and of a length of 106.3 centimetres."[111] Instead of specifying the cross section of the tube containing the mercury column, the new definition referred to the mass of mercury which was more precisely reproducible. The mercury column standard was maintained until 1948, when the ohm was redefined in absolute terms.

Experiments with the modifications proposed by Rayleigh and made by British physicists resulted in improvements that promised an accuracy of the order 1 to 10,000. As a result of these experiments, Frederick Smith and collaborators at the National Physical Laboratory, an institution founded in 1898, engaged in constructing a large-scale modified Lorenz apparatus. With this advanced and costly machine, determinations of resistances could be made with the same accuracy as the accuracy involved in direct measurements of the mercury ohm standard. In his report of 1914, Smith concluded: "The instrument ... may be used for absolute measurements of resistance with a precision satisfying all present demands whether purely scientific or technical. ... The results justify Lord Rayleigh's belief that the ohm, as defined in absolute measure, can be realised with a precision comparable with that of the international ohm."[112]

At the time Rayleigh was still alive, whereas the long-deceased Lorenz was a mere shadow from the past. And yet the development that led to the machine of the National Physical Laboratory started modestly in 1873 with the work of the Danish physicist. And in this case, his name could not and was not confounded with that of Lorentz.

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111. Osterberg (1894), p. 17.

112. Smith (1914), p. 108. See also Larsen (1915).

CHAPTER 4

From physics to technology

Although Lorenz was very much a theoretical and mathematical physicist, and was in this respect unique in the Danish physics community, he was not foreign to applications. His predilection for fundamental and theoretical physics did not prevent him from developing an interest also in the experimental and technical aspects of his chosen fields of research. After all, he was trained as a polytechnic engineer and lived at a time when technological applications of science were rated highly and seen, not without reason, as heralding a transformation of society. It was an era during which the marvels from the infant electric industry attracted massive public, commercial and cultural attention. One indication of Lorenz's interest in the practical and more mundane aspects of physics is his extensive work on a method to determine an absolute measure of the ohm unit for resistance. This work, which was considered in Section 3.4, aroused much interest in the electric industry and was as much of a technological as of a scientific nature. Also on a few other occasions Lorenz engaged in instrument design. Thus, in 1878 he described to the Royal Danish Academy of Science a new construction of a hygrometer.[1]

As an expert in the electrical sciences it is no wonder that Lorenz showed a somewhat sporadic interest in technological issues focused on electric and electromagnetic devices, which he on and off investigated from a scientific basis during the 1880s. The interest led him to design a new type of electric dynamo and an innovative cable for the transmission of telephone currents. These innovations were meant to enter the commercial market and Lorenz may have hoped in this way to earn a substantial income from his patents or royal privileges as they were called in Denmark.[2] However, Lorenz

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[1] *Videnskabernes Selskab, Oversigt* (1878), p. 22. His presentation to the Royal Academy on 22 March 1878 was oral and he did not publish a paper on the new hygrometer.

[2] A patent law in accordance with the international patent system used in Germany, France and most other countries were only introduced in Denmark in 1894.

was not much of an entrepreneur. From a commercial point of view his involvement in the business of electrical technology was largely unsuccessful and after a decade's work or so he seems to have abandoned his hope of making money from science. During the last years of his life he retreated to what he liked most, after all, namely mathematics and problems of theoretical physics.

4.1 A Danish dynamo

Until the late 1870s Lorenz dealt with technical problems only at a few occasions, either as a consultant or by evaluating projects of a technological nature for the Royal Danish Academy or military authorities. For example, in 1875 he examined together with Julius Thomsen an invention for multiplex telegraphy made by the 26-year-old physicist and inventor Poul la Cour, who was at the time employed at Denmark's new Meteorological Institute founded in 1872. La Cour's invention, a so-called phono-telegraph, was a clever telegraphic device based on tuning forks which permitted several telegraph messages to be sent simultaneously on the same wire. Lorenz and Thomsen judged that la Cour's work was "based on an entirely new idea" and that it demonstrated the author's "superb talent for the practical applications of science."[3] They consequently recommended awarding the Academy's prestigious gold medal for the invention.

Although la Cour had at the time obtained a British patent, in the end his phono-telegraph - sometimes considered a precursor to the telephone - failed in making the transition to a commercial product. For a time the ambitious la Cour entered a partnership with the Copenhagen master mechanic and instrument maker Christopher Peter Jürgensen, who had interest and experience in mechanical inventions. For example, in 1870 Jürgensen was the first to produce the so-called "writing ball" (*skrivekugle*), an early typewriter invented by the Danish inventor Rasmus Malling-Hansen five years earlier. Unfortunately Jürgensen proved to be a better mechanic than a business man, such as Malling-Hansen, la Cour, and

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[3] Royal Academy of Sciences and Letters, Archive, 15 February 1875.

also Lorenz would experience. Neither in the case of la Cour nor that of Lorenz did the partnership with Jürgensen develop to their satisfaction. Later in life la Cour turned to other inventions and today he is best known as an innovative Folk High School teacher and a pioneer of electric power generated by windmills.[4]

By the 1870s it was realised that the coming age of electricity would rest crucially on the construction of electromagnetic dynamos or generators for the production of electric power either locally or integrated into larger networks. Although the invention of the first dynamos goes back to the early 1830s, it was only some forty years later that the first useful direct-current dynamos saw the light of day and soon became the backbone of the electric revolution. In Denmark, the versatile and autodidact inventor Søren Hjorth experimented at an early time with a self-magnetising dynamo, but although he was granted a patent (meaning a royal privilege) in 1854 and his machine actually produced, nothing practical came out of his inventiveness.[5] The era of the modern dynamo machine essentially started with Werner von Siemens' production of dynamos in the early 1870s based on the dynamo-electric principle. The first dynamos used in Denmark were of the design of the Belgian inventor Zénobe Gramme but most of the later machines were bought from Siemens' company in Germany.

In about 1880 Lorenz became seriously interested in the optimal design of a dynamo, a problem he characteristically attacked by means of detailed calculations of the involved electromagnetic processes. To transform his theoretical design into a real machine he joined forces with C. P. Jürgensen. In 1882 the two titular professors entered a formal business partnership to exploit the Danish patent of the new dynamo they had obtained the same year. Lorenz left it to his partner to look after the further economic aspects: "I, Jür-

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4. La Cour's life and inventions, including his work on the phono-telegraph, are described in Hansen (1985). Before he received the gold medal in 1875, Poul la Cour (1846-1908) was honoured with the Academy's silver medal in 1872 for a new method of measuring the height of clouds.

5. See Smith (1912) for an attempt to rehabilitate Hjorth in the history of electrical technology. A survey of the early history of dynamos and electrical motors is given in Atherton (1984), pp. 147-165.

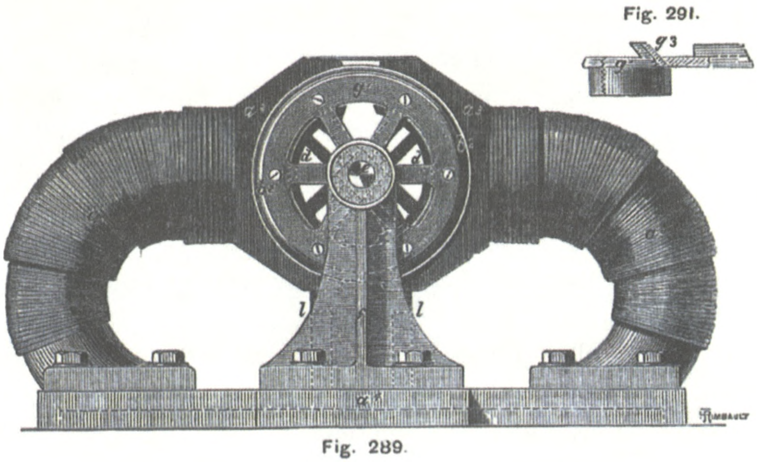

Figure 4.1: The Jürgensen-Lorenz dynamo. Source: Dredge (1882), p. 301.

gensen, have the right to sell on my own the patents to all countries except Denmark on the condition that I, Lorenz, then receive one third ... of the income from the sale."[6]

Over the next few years the Jürgensen-Lorenz patent was extended to Sweden, Norway, England, France and possibly some other countries. It is unknown how much money, if any, Lorenz earned from the invention but it cannot have been much. His involvement in the dynamo business ended in 1885, when Jürgensen bought his patent rights. Although the Jürgensen-Lorenz machine for a period of time attracted much interest and found use in parts of Danish light industry and possibly elsewhere, in the end it was not a commercial success.

The Danish-built dynamo was essentially a new version of the Gramme machine but with several innovative features that made it attractive at least from a theoretical point of view (Fig. 4.1). Among the attractive features was that it was a light and compact dynamo and yet with a remarkably high electromotive power. To put it in a nutshell, the machine had a field magnet of horseshoe form and a central stationary electromagnet acting as an interior field magnet.

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[6] Business agreement of 4 May 1882 (Lorenz Papers, RAS).

This interior electromagnet was composed of four radial arms placed inside a revolving annular armature made up of a series of narrow rings. The ventilation of the armature was secured by the rotation of a disk fastened to it and acting as a fan. One of the advantages of the construction was that it facilitated repairs, as the inner magnet as well as the rotating armature could separately be taken out without removing the magnets.[7] Like other dynamos in the period the Jürgensen-Lorenz machine generated direct and not alternating current.

The technically sophisticated machine was presented at the First International Exposition of Electricity held in Paris in 1881, an event at which the European public could for the first time experience the magic of Thomas Edison's amazing electric light bulbs. The Danish dynamo also appeared at the Vienna Electric Exhibition two years later. At both occasions it attracted positive attention, which at the Paris exhibition resulted in a gold medal awarded to the invention. Various dynamos were tested and compared by a commission which reported its results to the French Academy of Science. The commission was favourably impressed by the Danish dynamo:

> Of the many dynamos subjected to these tests, the Jürgensen dynamo-machine takes a predominant place with respect to total efficiency, as 97 per cent of the horse-power, given by the engine to the dynamo is given out electrically by the dynamo. ... An efficiency as great as that given by the Jürgensen dynamo-electric machine has not even approximately been obtained by any other transformation of force.[8]

Whereas Jürgensen participated in the Paris exhibition, Lorenz did not. When Lorenz went to Paris the following year it was not in connection with the dynamo, but to participate in a commission aimed at standardising electrical units such as recounted in Section 3.4.

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[7] For contemporary technical descriptions of the Jürgensen-Lorenz dynamo, see Prytz (1881), Dredge (1882), pp. 301-303, and Edsberg (1884). Lorenz never published on the subject.

[8] Quoted in Edsberg (1884). The efficiency of the other dynamos tested by the commission ranged between 85 and 95 per cent.

According to one report from the Paris exhibition, the Jürgensen-Lorenz machine - or what was generally called just the Jürgensen-machine - was "probably the most curious looking generator at the Exhibition."[9] Whatever the curious look, the Danish dynamo attracted considerable interest. In England, it was tested by two of the country's leading electrical engineers, the professors William Ayrton and John Perry at the Finsbury Technical College. In Denmark, a large Jürgensen-Lorenz dynamo was manufactured for use at the Copenhagen naval ports and compared with a French-produced Gramme dynamo. In both cases the dynamos were fed with mechanical energy from a 14 horse-power steam engine. The comparative tests reported by the naval captain V. Edsberg were favourable to the locally produced machine.

The competition market in the area of electric dynamos was tough and the initial success of the Danish machine was not followed up by a corresponding success on either the national or the international commercial market. Perhaps Jürgensen's limited ability as a business man was to blame, or perhaps the machine was just too advanced and specialised to be widely used in industry. The verdict of a German specialist in electric technology was this: "The design of this machine may be considered perfect from a theoretical point of view, but its practical construction involves mechanical difficulties in the manufacture of machines of considerable size."[10]

In the period from about 1878 to 1886 Lorenz occupied himself with several other aspects of electrical technology, some of them related to dynamos and others to the new and exciting electrical illumination. In 1879 he worked as a consultant for the Danish Army, investigating electric arc light produced by dynamos of the Gramme construction. A few years later he made extensive measurements of incandescent light bulbs of the types invented by the pioneers Edison, Joseph Swan and Hiram Maxim. Moreover, and of more interest, he also seems to have experimented with using direct-current dynamos for the manufacture of aluminium based on electrolytic decomposition of cryolite (chemical formula Na_3AlF_6), the Green-

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[9] Heap (1884), p. 65.

[10] Schellen (1884), pp. 368-369.

landic mineral which at the time was of great industrial importance in Denmark. Experiments of this kind were not new but resulted in a yield of aluminium too low to make the method commercially valuable. Apparently Lorenz's experiments fared no better. Nothing practical came out of his experiments and Lorenz never referred to them in his publications.[11]

4.2 Transmission of telephone currents

Alexander Graham Bell's sensational invention of the telephone was initially received with some scepticism in scientific circles. Many physicists and electricians were disappointed to learn that the invention did neither rest on a new scientific discovery nor on a mechanically advanced concept. In a paper in *Nature* of 1878, Maxwell recalled that when he and his physicist colleagues had first heard of the telephone two years earlier, they had expected something sophisticated and innovative from a scientific and technological point of view. But the apparatus was surprisingly unsophisticated. According to Maxwell, "When at last this little instrument appeared, consisting, as it does, of parts, everyone of which is familiar to us, and capable of being put together by an amateur, the disappointment arising from its humble appearance was only partially relieved on finding that it was really able to talk."[12]

The telephone aroused Lorenz's interest at an early date. The first telephones in Denmark were bought in 1877 from the Bell Company's German competitor Siemens & Halske, but it took two more years until a group of private investors and technology enthusiasts established the country's first telephone company. In 1882 the company was transformed into the Copenhagen Telephone Company which over the following years expanded to a nationally dominating telephone business (today it has metamorphosed into TDC

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[11] Notes and laboratory diaries kept in Lorenz Papers (DTM). According to Christiansen (1896), Lorenz experimented with electrotyping to produce light metals. The Danish cryolite industry was based on an invention by the chemist Julius Thomsen. For details and references to early attempts at producing aluminium from cryolite by electrolytic methods, see Kragh (2016), pp. 51-85.

[12] Maxwell (1965), Part 2, p. 751.

A/S). But in the beginning telephony was for the few and mainly used for in-house communication or over short distances within the city. In 1883 there were less than 500 subscribers, all in them of Copenhagen and mostly companies rather than private citizens.

Lorenz was not among them. His first interest in the telephone was not as a means for communication but as a measuring device in electrical experiments. A few physicists, including notables such as Helmholtz, Rayleigh and Boltzmann, had earlier used the telephone for acoustical studies and in alternating-current bridge systems to measure feeble currents. In an investigation of 1879 Lorenz adopted the technique to measure the self-inductance L and capacitance C of various kinds of wires.[13] He conducted some of the elaborate experiments in the church of Frederiksberg Castle, which was part of the Military High School and close to its laboratory. It was important to avoid disturbances from the environment, and "I succeeded [by making the experiment] in the present church of Frederiksberg Castle; here the wires could be stretched between two choir stalls at a height of 4.8 m above the ground."

The main purpose of Lorenz's investigation of 1879 was to verify the theory of electrical oscillations in a circuit including a capacitor and an induction coil with negligible resistance. As early as 1853, William Thomson had derived a formula for the period T of such oscillations, namely that

$$T = 2\pi\sqrt{LC}$$

In a series of experiments starting in 1857 the German amateur physicist Berend Feddersen studied the periodic discharges from a Leyden jar by means of a rotating mirror. He largely but not completely confirmed Thomson's formula with respect to the capacitance. Lorenz was the first who showed convincingly that the period varies with the square root of the self-inductance. He was able to explain the deviations between theory and experiment, and thus to maintain the validity of the formula, by referring to a wrongly calculated capacitance. It was in connection with the experimental de-

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[13] Lorenz (1879a). For this work, see also Section 3.2.

termination of the L factor in a LC circuit that he made use of the telephone.

For the current oscillations in experiments of the Feddersen type Lorenz derived the expression

$$I(t) = A\cos\left(\frac{t}{T}\pi\right) + B\sin\left(\frac{t}{T}\pi\right)$$

and found that the period of oscillation was given by

$$T = \frac{\pi}{a}\sqrt{CK}$$

K is what he called the "electrodynamic constant," a quantity consisting of two terms, an induction constant and a term depending on the wire's cross section. The constant a was given by the system of units (3×10^8 m) and the capacitance C could be determined experimentally. In his experiments Lorenz found "a most satisfactory agreement between observation and theory." The agreement was satisfactory indeed, as shown by his two series of experiments:

$$T_{\text{theory}} = 6.38 \times 10^{-6}\ \text{s}, \quad T_{\text{exp}} = 6.32 \times 10^{-6}\ \text{s}$$
$$T_{\text{theory}} = 8.42 \times 10^{-6}\ \text{s}, \quad T_{\text{exp}} = 8.16 \times 10^{-6}\ \text{s}$$

The 1879 paper on the propagation of electricity was well known at the time and continued being cited both before and after Lorenz's death. For example, in Heinrich Hertz's famous investigation of 1887 on the detection of electromagnetic waves, he referred to Lorenz's paper.[14] According to two American physicists, writing at about the same time, "Lorenz, by his repetition of Feddersen's results, and by his mathematical analysis of them, apparently gave subsequent observers a solid basis for calculation."[15]

Lorenz also considered the effect of the L (or K) and C quantities for telegraphic transmission. He concluded that the self-inductance of a suspended iron wire would cause the signals to move slower and become more blurred, whereas it would have the beneficial ef-

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[14] Hertz (1887).

[15] Trowbridge and Sabine (1890), p. 110.

fect of reducing the attenuation. At the time Lorenz did not attempt to develop his work into a theory of telegraphy and he disregarded the transmission of telephone currents, using the telephone solely as a measuring instrument. On the other hand, he did consider telegraph lines in his 1879 paper, arguing that in the case of lines made of iron the induced magnetism must be taken into regard. For an aerial telegraph wire of radius α and height above the ground h Lorenz showed that the electrodynamic constant per length was given by

$$K = 2\log\frac{h}{\alpha} + 2\alpha k$$

The quantity k is a "magnetisation function" which for non-magnetic wires has the value $k = 0$. Lorenz pointed out that a larger K resulted in a greater speed of the telegraph signals and that the "magnetic inertia" was generally a harmful effect. He also made complex calculations of the self-inductance of circular and other forms of solenoids.[16]

After a telephone service had been established in Copenhagen, Lorenz invented a new type of closed coil for the telephone's transformer. The coil was tested by Johan L. W. V. Jensen, a mathematician and chief engineer at the Copenhagen Telephone Company, who found it to work excellently and therefore recommended that "the mentioned coil be patented in all countries of significance as soon as possible."[17] Indeed, in 1888 Lorenz was granted a Belgian patent and the following year a Danish patent. The coil was further tested by the Copenhagen Telephone Company, but it is unknown if it was actually used as a transformer in practical telephony.

In any case, it was not so much the telephone apparatus itself as the transmission of telephone currents in a metallic conductor which attracted Lorenz's interest and on which problem he focused in the mid-1880s. Given the novelty and importance of this theo-

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[16] Lorenz (1879a), pp. 170-171. Some of the results of Lorenz's calculations were reproduced in Christiansen (1915), p. 724.

[17] Jensen to Lorenz, 2 September 1888 (Lorenz Papers, DTM). Belgian patent, no. 1,587 of 1888, and Danish patent, no. 83,720 of 1889.

retical research it is remarkable that he never published his work in this area. Consequently the results were known only to a few of his contemporaries and have survived only in the form of notes, letters and an undated manuscript probably completed in late 1884.[18] To understand the background of Lorenz's theory of telephone currents we need to recall the state of art of telegraph and telephone transmission in the early days of telephony.

When telephony entered the scene of communication technology it was widely understood as a form of telegraphy. Indeed, the telephone was often referred to as a "speaking telegraph." But for reasons that were not very clear at the time, it turned out that the range of telephone conversations was limited to a few hundred kilometres - in marked contrast to telegraph messages which could easily be transmitted over 1,000 km or more. Not only did the speech signals become so weak that they were barely audible, the received signals were also distorted, making it even harder to understand the transmitted message. This obviously posed a problem for the infant telephone industry in its competition with the mighty telegraph system. Nonetheless, practical engineers assumed that the propagation of telephone currents was analogous to the transmission of telegraph signals and consequently that, with some modifications, the theory and practical experiences of telegraphy could be used also for telephonic communication.

More specifically, engineers and electricians adopted William Thomson's celebrated theory of propagation of telegraph signals to the new art of telephony. According to this theory dating from 1855 the voltage V over a long perfectly insulated cable would obey the diffusion equation

$$\frac{\partial^2 V}{\partial x^2} = RC\frac{\partial V}{\partial t},$$

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[18] Lorenz, "The Theory of Telephone Currents," a detailed manuscript in Danish kept at the archive of the Royal Danish Academy. The manuscript, which is not in Lorenz's handwriting, is probably based on his notes but prepared by another person, possibly the later physics professor Christian Christiansen. Lorenz's theory is examined in Kragh (1992) which also contains information of its sources and suggestions of why Lorenz decided not to publish it. Much of the material in sections 4.2 and 4.3 is based on this paper.

where R and C refer to the resistance and capacitance per unit length. It follows from the equation that the current will be retarded in such a way that it reaches a maximum at a time proportional to RCx^2. In agreement with Thomson's theory telephone engineers generally concluded that to reduce the attenuation, and thereby to increase the speaking distance, the product of the line's capacitance and resistance per length should be minimised.

Apart from these two parameters there was, however, a third parameter characterising an electric circuit, namely self-inductance, which plays no significant role in telegraphy but is important in the propagation of the rapidly oscillating speech currents. Many engineers at the time did not understand self-induction very well and therefore chose to ignore it. Even worse, when self-inductance was taken into regard it was often considered to act in the same way as resistance, that is, to increase the attenuation.[19] Engineers generally associated self-induction with the impedance in an alternating-current circuit given by, for example,

$$Z = \sqrt{R^2 + (\omega L)^2}\ ,$$

where ω is the cyclic frequency of the current. From this point of view, self-induction entered as just another version of the ohmic resistance and hence had to be reduced as much as possible. As William Preece, a leading engineer in the British Post Office expressed it, "self-induction is a serious hindrance to rapid telegraphy and to long-distance speaking on telephones."[20]

What matters is that early telephone cables and wire systems were constructed in such a way that all three electric parameters – capacitance, resistance and self-inductance – were as small as possible. This was the essence of what telegraph engineers sometimes called "Preece's law," the semi-empirical rule that for any telephone line the maximum length for intelligible speech x was given by

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[19] For the problem of long-distance telephony in the late nineteenth century, see Jordan (1982), Wasserman (1985) and Kragh (2009).

[20] Preece (1887), p. 373.

$$RCx^2 = A$$

Here A is a constant depending on the type of line. Although the self-inductance does not enter explicitly, it is part of the A constant which depends on the metal used for the wire.

In order to reduce what was thought to be the harmful effect of self-induction it was suggested to use copper instead of iron for the lines. For example, the 498-km Paris-London line that opened for traffic in 1891 was considered a major technological achievement and a triumph of engineering methods in telephony; the line used copper conductors of heavy gauge and the submarine Channel cable was supplied with heavy layers of gutta-percha. This traditional and somewhat unimaginative solution to the long-distance problem was based on the experiences of telegraph engineers, whereas it did not involve scientific analysis of the propagation of speech currents. To repeat, about 1885 the propagation of telephone currents was seen as a special instance of telegraph transmission theory rather than a field requiring its own scientific basis. Most engineers and physicists considered the performance of a telephone line to be given by the line's capacitance and resistance, whereas the self-inductance was seen as either irrelevant or detrimental. This is where Lorenz entered the game (Fig. 4.2).

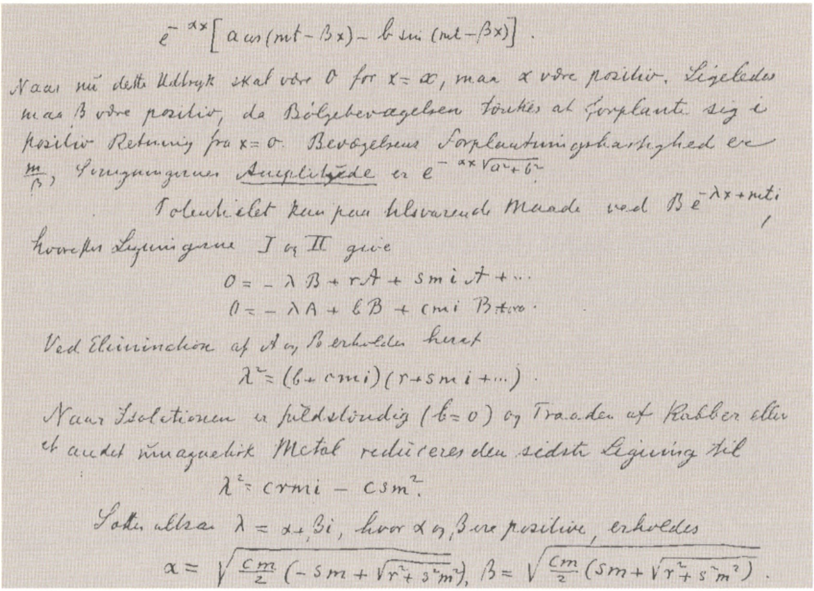
Lorenz first established from the general electrical theory of circuits the differential equations governing the propagation of an arbitrary signal in a long conductor. Following Kirchhoff he wrote the equations for the current and voltage gradients as

$$-\frac{\partial I}{\partial x} = GV + C\frac{\partial V}{\partial t}$$

and

$$-\frac{\partial V}{\partial x} = RI + L\frac{\partial I}{\partial t}$$

The symbols G, C, R and L refer to leakage, capacitance, resistance and self-inductance, respectively, all per unit length. The first equation is a continuity equation expressing the conservation of electrical charge, and the second one is a generalised version of Ohm's



$$e^{-\alpha x}\left[a\cos(mt-\beta x) - b\sin(mt-\beta x)\right].$$

Naar nu dette Udtryk skal være 0 for $x = \infty$, maa α være positiv. Ligeledes maa β være positiv, da Bølgebevægelsen tænkes at forplante sig i positiv Retning fra $x = 0$. Bevægelsens Forplantningshastighed er $\frac{m}{\beta}$, Svingningernes <u>Amplitude</u> er $e^{-\alpha x}\sqrt{a^2+b^2}$.

Telefonkablet kan paa tilsvarende Maade ved $Be^{-\lambda x+mti}$, hvorefter Ligningerne I og II give

$$0 = -\lambda B + rA + smiA + \cdots$$
$$0 = -\lambda A + bB + cmiB + \cdots.$$

Ved Elimination af A og B erholdes heraf

$$\lambda^2 = (b + cmi)(r + smi + \cdots).$$

Naar Isolationen er fuldstændig ($b = 0$) og Traaden af Kobber eller et andet umagnetisk Metal reduceres den sidste Ligning til

$$\lambda^2 = crmi - csm^2.$$

Sættes altsaa $\lambda = \alpha + \beta i$, hvor α og β ere positive, erholdes

$$\alpha = \sqrt{\frac{cm}{2}\left(-sm + \sqrt{r^2 + s^2m^2}\right)},\quad \beta = \sqrt{\frac{cm}{2}\left(sm + \sqrt{r^2 + s^2m^2}\right)}.$$

Figure 4.2: Excerpt of Lorenz's manuscript on the transmission of telephone currents. At the bottom are the formulae for attenuation and distortion of a telephone cable or wire expressed by its resistance, capacitance and self-inductance.

law. Lorenz then pointed out that if the input signal was a speech current it could be Fourier resolved into a sum of harmonic waves. For a single cyclic frequency ω he used a trial function the real part of which he wrote as

$$I(x,t) = e^{-\beta x}[a\cos(\omega t - \alpha x) - b\sin(\omega t - \alpha x)]$$

Here I is the current and the constants a and b refer to its initial amplitude. As Lorenz mentioned, the expression shows that the electric wave propagates with velocity and amplitude given by

$$v = \frac{\omega}{\alpha} \quad \text{and} \quad I = I_0 e^{-\beta x}$$

He next expressed the quantities a and b in terms of the primary constants R, L and C (while he disregarded G). Assuming the conductor to be perfectly insulated he obtained in this way

$$(\beta + i\alpha)^2 = i\omega C(R + i\omega L)$$

It is noteworthy that Lorenz made use of the mathematics of complex numbers, a method he was familiar with from his works in optical theory but at the time was rarely used in electrical science and engineering. Finally, by separation of the real and imaginary parts he arrived at values for the line constants α and β:

$$\alpha = \left[\frac{\omega C}{2}\left(\sqrt{R^2 + L^2\omega^2} + L\omega\right)\right]^{1/2}$$

and

$$\beta = \left[\frac{\omega C}{2}\left(\sqrt{R^2 + L^2\omega^2} - L\omega\right)\right]^{1/2}$$

Now a speech signal consists of a superposition of many harmonic waves. Consequently it will be distorted because the different waves propagate with different velocities, as given by α, thus causing a change in the original composition of the signal. Moreover, the attenuation of the amplitudes of the waves given by β will differ according to their frequencies.

In manuscript notes from the same period Lorenz generalised his theory to cover also leaking cables with a leakage per unit length given by G, the reciprocal of the insulation resistance. He further showed that under conditions valid for cables with an appreciable self-inductance the equation for the attenuation constant can be approximated to

$$\beta \cong \frac{R}{2}\sqrt{\frac{C}{L}} + \frac{G}{2}\sqrt{\frac{L}{C}}$$

The α -equation can similarly be written

$$\alpha \cong \omega\sqrt{CL}$$

It follows that

$$v = \frac{\omega}{\alpha} \cong \frac{1}{\sqrt{C/L}}$$

Since now neither v nor β depends on ω, the transmission will be approximately without distortion. Moreover, for conductors with negligible leakage, we have the expression

$$\beta \cong \frac{R}{2}\sqrt{\frac{C}{L}}$$

The result indicates that a very small self-inductance will result in a reduced speaking distance and not in the desired extension of the distance. Lorenz finally gave the condition for a cable completely without distortion, that is, where all frequencies are attenuated in the same proportion. He found this to be the case if

$$RC = LG$$

Concerning the leakage factor, he noticed that, "in general, lack of insulation on long lines will increase the articulation but diminish the intensity of the sound." Lorenz discussed systematically the effects of varying the four line parameters on the quality of transmitted speech, that is, the attenuation and distortion. In one of his notes he observed:

> For $G = 0$, $d\beta/d\omega$ is proportional to C, which implies that not only the intensity but also the sound of speech diminish with increasing capacitance. As is well known, this is a defect of all cables the capacitances of which must necessarily be large compared with the capacitance of aerial wires; it will therefore be of importance to examine if the constant L can be altered in a practical way, and if such a change will improve the transmission.[21]

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[21] Madsen (1922). The Danish telegraph engineer Søren P. Madsen gave in 1920 an address on Lorenz's theory in which he cited long passages from sources which are no longer extant.

Evidently, Lorenz was not only interested in the telephone transmission problem for academic reasons but also because his scientific analysis might guide the construction of a new technology for producing telephone cables.

As mentioned, Lorenz never published his theory and his conclusions regarding cable construction were thus unknown to almost all contemporary physicists and engineers. Basically the same conclusions were published slightly later, first in 1887 by the brilliant and eccentric British physicist Oliver Heaviside. It is tempting to ask if there were any connections between the two physicists. Heaviside left school at the age of 16 and never received a formal education in physics and mathematics. His only salaried position was as a telegraph operator and technical expert in Newcastle, where he worked for the Danish-owned Great Northern Telegraph Company. He spent more than a year in Fredericia in Denmark, where Great Northern's telegraphists were trained, and he even taught himself Danish.[22] After his early retirement in 1874 Heaviside became an independent researcher - unfortunately with no independent means. However, despite his connection to Denmark, in all likelihood he was unacquainted with Lorenz and Lorenz with him.

Heaviside might have known about Lorenz's electrodynamic theory of 1867, but if so, being committed to the ether and Maxwell's field theory he would have found no merit in it. As far as Lorenz was concerned, he never referred to Heaviside's work.

Heaviside's masterful analysis failed to lead to immediate consequences for long-distance telephony.[23] With the plus sign corresponding to α and the minus sign to β, his formulae from 1887 can be summarised as

$$\alpha,\beta = \left[\frac{1}{2}\left(\sqrt{(R^2+L^2\omega^2)(G^2+C^2\omega^2)} \pm (RG-\omega^2 LC)\right)\right]^{1/2}$$

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[22] Nahin (1988), p. 20.

[23] Heaviside published his theory in a series of papers in the journal *The Electrician* which were later collected in his *Electrical Papers* published in two volumes in 1892 and 1894. See Nahin (1988) and Yavetz (1995) for expositions of Heaviside's theory and the author's troubles with getting it recognised by the British telephone authorities.

Although the theories of Lorenz and Heaviside were in many ways formally equivalent, there was the important difference that Heaviside was a devoted Maxwellian who based his work entirely on the field theory of electromagnetism. For him, the electromagnetic energy was conveyed by changes in the ether, whereas what went on within the wire was less essential. Lorenz, on the other hand, ignored Maxwell's theory and expressed his equations in a purely phenomenological way, without making use of either field quantities, the ether concept or Maxwell's displacement current. He replaced physical interpretation by mathematics.

For the sake of completeness it should be added that also Aimé Vaschy in France independently developed and published the telephone transmission theory at about the same time as Heaviside and without relying to any extent on Maxwell's theory.[24] Lorenz was aware of Vaschy's theory and also of the contributions to telephony of other continental physicists and engineers, such as shown by a scrap paper dated 1886 and titled "Literature on Telephony." His list of literature did not include references to Heavisides's work.

Given that priority depends on the chronology of published works there is no doubt that priority to the theory of the inductively loaded cable belongs to Heaviside or perhaps to Heaviside and the lesser known Vaschy jointly. From this point of view it is irrelevant that Danish physicist Lorenz had the idea at a slightly earlier date.

4.3 The Lorenz-Krarup cable

The road from understanding scientifically how telephone currents propagate to the implementation of the knowledge in real telephone lines turned out to be long and bumpy. Heaviside's recommendation of adding induction coils was eventually transformed into an engineering theory with direct connection to practice, but it took more than a decade until the first loaded test cables were constructed and then proved their worth. Unfortunately, Lorenz did not live to witness the transition from theory to practice, but he worked in his own way to make it possible.

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[24] For Vaschy's work on long-distance telephony, see Atten (1993).

It was important for Lorenz to have his theory supported or at least tested by measurements on very long telephone lines, and he found such preliminary support in field experiments made in the United States by the Belgian engineer and inventor François Van Rysselberghe.[25] "The agreement between theory and experience," Lorenz wrote, "is of particular importance in that one could not beforehand be certain to obtain such an agreement." Van Rysselberghe concluded that long-distance telephony did not depend on the particular telephones and microphones used, but solely on the kind of wire or cable. He found that speech could be clearly perceived only over 400 km of iron wire, but that copper wires allowed for more than doubling the distance. Moreover, the experiments of the Belgian researcher questioned the engineers' belief that a line's self-inductance was either irrelevant or detrimental to transmission.

To Lorenz, Van Rysselberghe's data promised to transform his abstract theory into a technologically useful prescription of cable design. The data indicated that a 20-fold reduction in amplitude made transmission just possible on the long American lines. Noticing that

$$1/20 \cong e^{-3},$$

Lorenz suggested as a general rule that the practical maximum speaking distance was given by the expression

$$x_{\max} \cong \frac{3}{\beta}$$

Since in general the attenuation constant β would depend on the parameters R, C and L, contrary to the engineering belief encapsulated in Preece's law there could be no simple relationship between speaking distance and the RC factor.

Lorenz was clearly interested in turning his theoretical discovery into a practical invention. His theory assumed a non-magnetic conductor in the form of copper and to increase the self-inductance he

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[25] On Van Rysselberghe and his experiments, see Tucker (1978).

suggested covering the copper wire continuously with either a thin envelope of soft iron or a finely wound iron wire. This he thought "should be of the greatest importance for long-distance telephony." Using the rule stated above and a cyclic frequency ω = 7,000 Hz he calculated a maximum speaking range of $x_{\max}$ = 324 km for a 2 mm copper wire; if provided with a 0.2 mm iron layer, the range would increase to 432 km. As another example he considered a 5 mm copper wire covered with 0.5 mm soft iron, for which he calculated a maximum speaking distance no less than 6,500 km. Although Lorenz realised that such distances were probably unrealistic, he was confident that his proposed method of increasing the speaking distance was more than just the fancy of a theoretical physicist. The method would eventually be known as "uniform inductive loading."

Eager to turn his theoretical insight into a commercially useful invention Lorenz knew that as a first step he had to convince a cable manufacturer or a telephone company about its potential usefulness. At the time there were no manufacturers of telegraph or telephone cables or wires in Denmark and the only telephone company of some importance, the Copenhagen Telephone Company, operated lines no longer than about 80 km. Lorenz nonetheless contacted the company's chief engineer Johan Jensen, explaining his work and asking for a practical evaluation of it, but apparently nothing came out of the contact.

In late 1886 Lorenz then addressed one of Europe's largest cable manufacturers, the German company Felten & Guillaume in Mühlheim near Cologne, which was the chief supplier of wires and cables to Denmark. He requested the company to test his design of an inductively loaded cable. Mentioning that he had arrived at his idea "already several years ago," he pointed out that "the usual fallacy that self-induction is a harmful agent to the propagation of telephone currents is a misconception [and] if not corrected, for a long time it will prevent the technicians from experimenting with copper wire covered with iron."[26]

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[26] Letter of 11 December 1886, quoted in Danish translation in Madsen (1922). The original but no longer extant letter contained several figures and graphical illustra-

In his long and detailed letter to the Felten & Guillaume company, Lorenz wrote that he intended to publish his theoretical work on telephone currents, but as mentioned this did not happen. Having summarised the main conclusions of his theory, he illustrated it with a concrete example supposedly to make it more inviting to the German company from an economic point of view. He emphasised that to obtain the same speaking distance without inductive loading, one would have to increase the amount of the costly copper greatly. The amount of copper saved per 100 km line would be about 2 tons, he estimated, and thus result in a considerable saving on long lines. As to the construction of the proposed "armoured wire" - soon to be called a loaded wire - he recommended a 0.1 mm thick iron envelope wound around a copper conductor of radius 0.9 mm. However, the exact dimensions could only be established by experiments made on real wire systems for which test cables had to be made. Moreover:

> I want to emphasise that the advantage of using my wire will only appear at very long distances (more than 100 km). Experiments done at less distances will not yield adequate results since accidental circumstances, and in particular the dimensions of the microphone's induction coil, will greatly influence the result. In my design I have had in mind exclusively long distances.

Unfortunately the response of Felten & Guilleaume to Lorenz's request has not survived, but there is no doubt that it was negative. Most likely, as seen from the point of view of the German cable company it would be a risky investment to produce the necessary long test line whose transmission quality largely rested on calculations only. It may also have influenced the company's decision that Lorenz's proposal was solely concerned with aerial wires or cables whereas he ignored the possibility of underground and submarine cables.

In 1887, at the time he left the Military High School to become a privately funded physicist, Lorenz abandoned his work on telephone transmission never to return to it. It is more than a little puzzling

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tions of Lorenz's proposed design.

that he did nothing to publish his theory, which he could easily have done either in a foreign journal, in the local *Tidsskrift for Mathematik* (Journal of Mathematics), or, even more easily, in one of the publication series of the Royal Danish Academy. Nor did he give an oral presentation of his telephone theory to members of the Academy. It is unknown why he decided to keep the theory for himself, but one may hypothesise that it was for patent reasons. Recall that he also did not publish on his design of the dynamo manufactured by Jürgensen. Another possible explanation is that after Heaviside's paper of June 1887 had appeared, Lorenz may have concluded that there was no point in publishing a theory which was essentially the same.

Despite Lorenz's decision to abandon further work on telephone currents, it was not quite the end of his adventure into wire and cable design, for after his death the idea of inductive loading was reconsidered by a few Danish physicists and engineers. More importantly, Heaviside's somewhat similar idea was eventually transformed into a powerful engineering theory that in the early years of the new century resulted in a minor revolution in long-distance telephony. Contrary to Lorenz, Heaviside suggested that the best way to add self-inductance was in the form of coils inserted discretely in the line. On the other hand, he also considered continuous inductive loading and his technological recommendations thus had a broader basis than those proposed by Lorenz.

Historians of science and technology have generally assumed that the theory of inductive loading and its later technological spin-off was a direct consequence of Maxwellian field theory. Not only that "loading was based on an understanding of the Maxwell-Heaviside electromagnetic theory,"[27] but also that this theory "was a necessary condition for the conception and development of the innovation."[28] Clearly, inductive loading technology was a science-based innovation that would hardly have become a reality in the absence of advanced electromagnetic theory. But as illustrated by the cases of Lorenz and Vaschy, there was no necessary connection between Maxwell's theory and the proposal of adding self-induct-

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[27] Brittain (1970), p. 57.

[28] Wasserman (1985), p. 8.

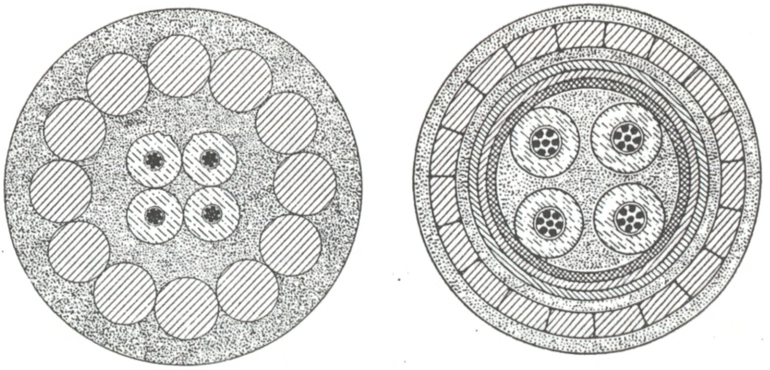

Figure 4.3: The first Krarup cables. To the left, the Elsinore-Helsingborg cable of 1902 insulated with gutta-percha and with conductors wound with 0.2 mm iron wire. To the right, the 1903 Fehmarn-Lolland cable insulated with impregnated paper and a 0.3 mm iron winding. Source: P. O. Pedersen, *Telefonledningernes Teori* (Copenhagen: Gjellerup, 1914), p. 113.

ance to a telephone line. In the history of science-based technologies there are several other examples of two quite different theories which have led to the same or approximately the same recommendation for a new technology.

The method of discrete loading was independently developed into a practical technology by the American pioneers Michael Pupin at Princeton University and George Campbell at the Bell System corporation.[29] After the Bell System had acquired Pupin's patent things went fast. The method of discrete loading turned out to be very successful as it extended the speaking range very considerably, meaning to distances longer than 1,000 km. Due to a combination of loading coils and tube amplifiers, in 1914 the first transcontinental telephone line was completed in the United states over a distance of 7,600 km.

At a Scandinavian meeting for engineers in Stockholm in 1897, Johan Jensen revived interest in Lorenz's old and half-forgotten theory.[30] Essentially following Lorenz's design but choosing differ-

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[29] See Britain (1970) and Wasserman (1985).

[30] Jensen (1898).

ent dimensions, Jensen proposed to wind a copper wire of radius 0.8 mm with a thin iron tape or wire of thickness 0.07 mm. He addressed Felten & Guilleaume but was no more successful than Lorenz had been eleven years earlier. Only with the work of Carl Emil Krarup, a young physicist and engineer trained at the Polytechnic College, did the uniformly loaded cable become a reality in long-distance and submarine telephony.[31] Krarup was familiar with the theories of Vaschy and Heaviside, and he knew about Lorenz's unpublished work through Jensen's account of 1897 although it did not inspire him directly. Whatever his sources, by 1901 he proposed a cable construction in which the copper core was narrowly wound with very thin wires of soft iron. The following year he published his design specifically aimed for submarine cables in *Elektrotechnische Zeitschrift* (Journal of Electrical Technology) and after extensive testing Felten & Guilleaume started large-scale production of continuously loaded cables or what came to be known as Krarup cables.

In November 1902 the first Krarup cable of length 5.3 km was laid between Elsinore in Denmark and Helsingborg in Sweden. A few months later it was followed by a 19.3 km German-Danish cable between Fehmarn and Lolland which was still in use after fifty years of operation (Fig. 4.3). The Elsinore-Helsingborg cable had self-induction $L = 2.65$ mHy km^{-1} and an attenuation factor $\beta = 2.74$ km^{-1}. For the 1903 Fehmarn-Lolland cable the values were $L = 2.50$ mHy km^{-1} and $\beta = 0.99$ km^{-1}. Also in 1903 a 75 km Krarup cable connected the island Heligoland with Cuxhaven in mainland Germany. Eight years later the French Telegraph Company laid a cable of the same kind across the Channel.

By that time more than fifty telephone cables of the design first suggested by Lorenz were in operation. Although the connection between Lorenz's telephone transmission theory of the mid-1880s and Krarup's later series of submarine cables is weak indeed, perhaps it is not inappropriate to speak of the latter cables as a delayed realisation of Lorenz's theory.

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[31] See Kragh (1994) for a detailed account of Krarup's invention and the technology of uniformly loaded telephone cables.

CHAPTER 5

Mathematical and other works

Although Lorenz's scientific legacy rests predominantly on his contributions to optics and electrodynamics, he also did interesting research in other branches of the physical and mathematical sciences. He had a deep interest in the physics of heat phenomena and was known internationally for his experimental work on heat conduction. Moreover, Lorenz followed closely the new and exciting development in the mechanical or kinetic theory of heat, but in this case with some reservation and without contributing to the development. Still, in connection with his research in electricity and optics he was able to make early estimates of the size of molecules and the number of molecules in a unit volume of a gas, works which were recognised as important contributions to molecular physics. This kind of work was as much of chemical as of physical relevance, but in spite of his background in chemical engineering Lorenz largely ignored the chemical implications of his work.

Throughout his life Lorenz had a burning interest in mathematics whether pure or applied. His lack of formal training in higher mathematics did not prevent him from entering the Danish mathematical scene and writing widely on subjects of a mathematical nature. Many of these were minor and often polemical contributions, several of them criticising works of Danish mathematicians. Although undoubtedly a talented mathematician, his contributions were not always highly regarded in the small Danish mathematical community and they were largely unknown outside the country. Only in few cases did Lorenz write on subjects beyond physics and mathematics, one of the exceptions being an interesting but unnoticed work on geophysics.

Lorenz seems to have been uninterested in social and political issues, at least in the ordinary sense, and yet he did write a couple of pieces which may count as either political polemic or political science. The first of these works, a pamphlet dating from 1865, was political in the traditional sense whereas an article of 1890 on the election system in Denmark's new democratic order was in reality an exercise in mathematical probability theory.

5.1 Mathematics, more or less pure

Lorenz was part of the small but growing Danish mathematical community, but he was not a central actor any more than he was a central actor in the even smaller Danish physical community. The first mathematical journal in Denmark was established in 1859, first as *Mathematisk Tidsskrift* (Mathematical Journal) and since 1865 published under the title *Tidsskrift for Mathematik* (Journal for Mathematics). The scope and content of the new journal was broad, as it included a variety of articles and notes ranging from elementary school mathematics to advanced research contributions. Much of the content was oriented toward the educational sector and not a few of the articles were polemical discussions between Danish mathematical authors. Three years after the establishment of the mathematics journal a somewhat similar journal for chemistry and physics was founded by the brothers August and Julius Thomsen, titled *Tidsskrift for Physik og Chemie* (Journal of Physics and Chemistry). While Lorenz only published two papers in this journal, in 1862 and 1867, he was a frequent contributor to the Danish mathematics journal in which he published his first paper in 1860 and the last one in 1890.

A forum for Danish mathematicians broadly conceived was established in 1873 on the initiative of Thorvald N. Thiele, a 35-year-old astronomer, actuary and mathematician.[1] Lorenz was among the first members of Matematisk Forening (Mathematical Society) which at its foundation included 65 members. Six years later a Danish Chemical Society was founded and only in 1908 did the corresponding Physical Society see the day of light. Although not very active, at one occasion in 1874 Lorenz gave a talk to the Mathematical Society on the subject of mathematical physics.

In 1876 the Society decided to announce two prize problems and

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[1] On the early history of the Danish Mathematical Society, see Crone (1923).

Lorenz proposed that one of the problems should be on the motion of heat in a cylinder of constant thermal conductivity. However, the proposal was rejected by the Society's board consisting of Thiele, J. Petersen and H. G. Zeuthen who instead chose problems of a more purely mathematical nature. As a result, Lorenz resigned his membership of the Mathematical Society. Thiele begged Lorenz to reconsider his resignation, pointing out that it would be wrong to "punish" the Society for a decision for which only the board was responsible. "You will understand," he wrote, "that by demonstrating your dissatisfaction with what happened and leaving the Society, you make it impossible that your point of view will win recognition." In a generally friendly letter of reply Lorenz assured Thiele that he nourished no bad feelings against him or the Society. And yet, "my resignation was indeed caused by the Society's rejection of the problem I proposed."[2] In the view of Lorenz, the Mathematical Society was not seriously interested in mathematical physics or other areas of applied mathematics and for this reason he ceased his membership.

While *Tidsskrift for Mathematik* was a local Danish journal with very little visibility outside the country, in 1882 an international but Scandinavian mathematics journal was founded as *Acta Mathematica* by the leading Swedish mathematician Gösta Mittag-Leffler, a professor at the Stockholm University College.[3] To secure a high international standard he appointed an editorial board consisting of eminent mathematicians from the Scandinavian countries. Mittag-Leffler was aware of Lorenz's mathematical contributions and seems to have appreciated them. Consequently, he asked Lorenz to enter the editorial board of *Acta Mathematica* together with the professional mathematicians J. Petersen and H. G. Zeuthen.[4] The members of the editorial board were supposed to contribute to the journal with

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[2] Thiele to Lorenz, 27 October 1876; Lorenz to Thiele, draft of 28 October 1876 (Lorenz Papers, DTM).

[3] Domar (1982).

[4] Mittag-Leffler to Lorenz, 18 April 1882 and also 3 July 1877, where Mittag-Leffler invited Lorenz to visit him in Sweden (Lorenz Papers, DTM). The first editorial board consisted of fourteen members of which six were from Sweden, four from Norway, three from Denmark, and one from Finland.

works of their own, but Lorenz never did so. Nor did he contribute to the major journal *Mathematische Annalen*, which was founded in 1868 and for which Carl Neumann, one of the editors, requested Lorenz's support. In an interesting letter of August 1868 Neumann described his negotiations with the Leipzig publisher B. G. Teubner, the background for the new journal project and his dissatisfaction with the German journals in mathematics.[5]

With one exception, namely his 1861 paper in *Crelles Journal* (Section 2.1), Lorenz published all his mathematical works in *Tidsskrift for Mathematik* and thus in Danish. The more important of these appeared in French translation in Valentiner's collection from 1904, but until then they were known only to Scandinavian mathematicians. Apart from several minor notes, Lorenz published 22 articles in the Danish journal. Some of these articles were brief and polemical, while others of them were of a more substantial and sometimes original character. The mathematician Valentiner judged that "the majority of these [mathematical] works offer nothing of much interest, but among them there are a few remarkable results."[6] Generally Lorenz was not popular or highly respected among Danish mathematicians. Christiansen wrote diplomatically about a contrast between Lorenz and the professional mathematicians' focus on pure mathematics.[7]

As an early example of the polemical genre, in 1861 Lorenz wrote a critical review of a textbook on optics recently published by the physics professor C. Holten. Although assuring that he did not want to criticise the book, this is just what he did, feeling it his duty to "correct the errors, especially because they might be disseminated to an entire generation of students."[8] And Lorenz found many errors of both a mathematical and a conceptual kind. Without say-

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5. Neumann to Lorenz, 19 August 1868 (Lorenz Papers, DTM). At the time Neumann and his co-editor A. Clebsch apparently thought of a different and more elaborate title for the journal, namely *Journal für Mathematik und Mathematische Theile der Naturwissenschaft* (Journal of Mathematics and the Mathematical Parts of Science).

6. Valentiner (1898-1904), vol. 2, p. xx.

7. Christiansen (1896).

8. Lorenz (1861c), p. 166. The book was C. Holten, *Lysets Naturlære* (Copenhagen, 1861).

ing so he suggested that, when it came to the science of optics, the country's only professor in physics was incompetent. Holten wisely refrained from answering the critique which presumably made Lorenz unpopular among some Danish physicists. If there were tensions between the two physicists, such as suggested by several sources, Lorenz's review did nothing to ease them.

Lorenz also got involved in a lengthy polemic over mathematical questions with one of his colleagues at the Military High School, the army officer and geodesist Georg Zachariae. The exchange of opinions in *Tidsskrift for Mathematik* concerned the theory of observational errors on which topic Lorenz published a lengthy treatise.[9] Of greater interest still is a mathematical dispute between Lorenz and Frederik M. Bing, an esteemed Danish mathematician who served as chief actuary at the State Life Insurance Company. Like the ten years older Lorenz, Bing was a chemistry candidate from the Polytechnic College and self-taught as a mathematician. Together with his friend Julius Petersen Bing published in the 1870s a couple of remarkable works on political economy from the perspective of mathematics. One of the papers was later internationally recognised as an important contribution to neoclassical distribution theory.[10]

In 1879 Bing published a paper in *Tidsskrift for Mathematik* in which he critically examined the concept of posterior probability and demonstrated by means of examples that in some cases Bayes' theorem, so named after the British eighteenth-century philosopher Thomas Bayes, leads to contradictory or paradoxical results.[11] In modern mathematical literature one might still find references to "Bing's paradox."[12] According to Bing, in cases where there is no

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[9] Lorenz (1872b), presented to the Royal Academy of Science at meeting of 12 January 1872.

[10] See Whitaker (1982).

[11] Bing (1879).

[12] Dale (1999), pp. 430-435, where Lorenz's objections are also considered. "Bing's paradox" may first have attracted international attention when it was reviewed in a paper of 1920 by the British mathematician and physicist Edmund T. Whittaker. See Whittaker (1915-1921), pp. 170-171, whose explanation of the paradox basically agrees with the one offered by Lorenz.

prior knowledge of acting causes there can be no posterior probabilities, thus rendering Bayes' theorem useless. In one of several examples he considered 100 individuals of age 40 of which we know that none of them died within a year. From Bayes' theorem and some supposedly reasonable assumptions he then inferred that all the individuals of this age must die before the end of the year! Lorenz, defending the Bayesian view, argued that some of Bing's assumptions were wrong and that his so-called paradox merely illustrated the unreasonableness of the basic assumptions.[13] He agreed with much of Bing's paper but disagreed with its implicit claim that Bayes' theorem was just wrong. Without saying so, he implied that Bing was not fully competent in his own field of expertise, namely mortality statistics.

Unconvinced of Lorenz's arguments Bing replied, accusing his opponent of having misread his paper. His reply caused a detailed rejoinder from Lorenz, after which he considered the matter as closed. However, Bing continued the polemics in yet another paper. The Bing-Lorenz controversy was heated but restricted to scientific disagreements that on both sides involved advanced mathematical arguments. If nothing else it showed that Lorenz was fully at home in the mathematical theory of probability. Many years later the Danish philosopher Kristian Kroman re-examined in great detail the debate concerning posterior probability, concluding that Lorenz's objections and his defence of Bayes' theorem were justified.[14] Kroman's critique of Bing's arguments was no less sharp than Lorenz's.

Another of Lorenz's mathematical papers deserves not to sink into oblivion and is indeed still cited in the modern literature.[15] In 1871 Lorenz investigated a problem in number theory, a branch of pure mathematics in which he had a deep and abiding interest. He considered the equation

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13. Lorenz (1879b).

14. Kroman (1908). The Danish-American statistician Arne Fisher referred in an influential textbook to Bing's paper, which "caused a sharp, and often heated, discussion among the older and younger mathematicians at that time." He mentioned Thiele and Kroman but not Lorenz, Bing's main antagonist. See Fisher (1922), p. 56.

15. Lorenz (1871).

$$m^2 + cn^2 = N\,,$$

where m and n are integers, c is a given number and N a positive integer. The number N can be represented by different combinations of m and n, but how and in how many ways? Distinguishing between what he called the synthetic and analytical method of solving the problem, Lorenz chose the latter. He based the method on transformations of series which he wrote as

$$\sum_{m=-\infty}^{+\infty} \sum_{n=-\infty}^{+\infty} q^{m^2+cn^2}, \quad q < 1$$

After a series of involved calculations Lorenz arrived at general rules for the representations of the number N. Thus, for $m^2 - n^2 = N$ he found that the number of representations was "twice the number of divisors of N or of $N/4$, depending on whether N is odd or can be divided by 4; if N can only be divided by 2, the equation cannot be decomposed." Lorenz noted that one of his rules was new, namely the one concerning the number of representations of

$$m^2 + 3n^2 = N$$

As he wrote:

> If a number N contains prime factors $p_1^{\alpha_1}, p_2^{\alpha_2}, \ldots$ of the form $3m + 1$, and if the prime factors of the form $3m + 2$ only appear in even powers, then the number of solutions of the equation $m^2 + 3n^2 = N$ is given by
>
> $$\rho_N = 2(\alpha_1 + 1)(\alpha_2 + 1)\ldots$$
>
> if N is odd, and by
>
> $$\rho_N = 6(\alpha_1 + 1)(\alpha_2 + 1)\ldots$$
>
> if N is even. If, on the contrary, N contains a prime factor of the form $3m + 2$ to an odd power, the result is $\rho_N = 0$.[16]

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[16] Lorenz (1871), p. 109.

Lorenz stated the various numbers of representations in a form that can be translated into the identity

$$\sum_{m=-\infty}^{\infty}\sum_{n=-\infty}^{\infty} q^{m^2+mn+n^2} = 1 + 6\sum_{n\geq 0}\left(\frac{q^{3n+1}}{1-q^{3n+1}} - \frac{q^{3n+2}}{1-q^{3n+2}}\right)$$

He did not bother to prove the identity, but a proof can be found in Valentiner's edition of Lorenz's collected papers. Lorenz's identity laid effectively buried in the pages of *Mathematisk Tidsskrift* until 1923, when the American mathematician Leonard Dickson called attention to it in an encyclopaedic work on the history of number theory.[17] Much later Michael Hirschhorn pointed out that Lorenz's formula is an important contribution to the theory of numbers.[18] Interestingly, the same identity was stated in a letter that the famous Indian mathematician Srinivasa Ramanujan wrote in about 1917, of course without knowing of Lorenz's paper. Hirschhorn consequently refers to the result as the "Lorenz-Ramanujan identity" and the term "Lorenz identity" can also be found in the mathematical literature.[19] Lorenz suggested in his 1871 paper that some of his results in number theory might be of use in mathematical physics, but without saying in which areas. He apparently prepared a separate paper on this topic intended for "Borchardt's Journal." However, this paper never appeared.[20]

The famous Riemann hypothesis or conjecture dating from 1859 implies a certain result concerning the number of primes less than a given number *x*. Lorenz had previously dealt with the theory of prime numbers and in 1891 he published a detailed and complex investigation on the number of primes in the transactions of the Royal Danish Academy of Science.[21] This was his last contribution to science. He was in contact with another member of the Academy,

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[17] Dickson (1923), p. 29.

[18] See Hirschhorn (2001) and Hirschhorn (2017), pp. 179-184.

[19] Varouchas (1998).

[20] Lorenz (1871), p. 111. The German mathematician Carl Wilhelm Borchardt (1817-1880) served from 1856 to 1880 as editor of *Journal für die Reine und Angewandte Mathematik* founded by A. L. Crelle and previously known as *Crelles Journal.*

[21] Lorenz (1878); Lorenz (1891).

the mathematician and actuary Jørgen P. Gram, who in a paper of 1884 had examined the same problem and derived a formula directly relating Riemann's zeta function $\xi(s)$ to the number of primes. At the time the number of primes had been computed up to $x = 10^9$ and compared to Riemann's formula.

Lorenz's aim was to determine analytically the number of primes for any finite value of x. But at the end of his paper, after numerous complex calculations, he had to admit that his efforts had not resulted in an exact solution. Nonetheless, he thought his work was a step in the right direction. While Gram's works on prime numbers and the Riemann hypothesis attracted wide attention, Lorenz's paper did not. The ambitious paper was rarely cited and seems to have exerted no impact at all on the mathematical community. Valentiner's French translation of 1904 changed nothing.

As we have seen in previous chapters, some of Lorenz's works on optical theory contained results that were as much of mathematical as of physical interest. Thus, in his 1883 theory of optical dispersion Lorenz made use of and analysed a transformed Bessel equation which here, for the first time, entered mathematical physics.[22] With n being an integer and μ a quantity relating to the velocity of light, the equation was of the form

$$\frac{d^2f}{dr^2} - \frac{2n}{r}\frac{df}{dr} - \mu f = 0$$

In his monumental treatise on Bessel functions the Cambridge mathematician George N. Watson called attention to Lorenz's innovative work in applied mathematics, referring in particular to his 1890 theory on the scattering of light by a sphere. In this paper (Section 2.4) Lorenz derived a formula for the asymptotic expansion of products of Bessel functions of the form $J_\alpha^2 + Y_\alpha^2$, where the two symbols refer to Bessel functions of the first and the second kind. The formula had not previously been stated in the mathematical literature and Watson consequently credited Lorenz with the "discovery" of the formula.[23]

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[22] Lorenz (1883), p. 172.

[23] Watson (1922), p. 224 and p. 229. Watson relied on Valentiner's French edition of

Finally, with the advent of digital computers a few mathematically minded physicists re-discovered certain angular functions that Lorenz, in his 1890 theory, had defined by a differential equation. As they noted, these angular functions might be generated more simply and conveniently by means of what at the time were advanced computer programs.[24]

5.2 Molecular physics

Together with electricity and magnetism, during the last quarter of the nineteenth century the most popular research areas of physics were optics, thermodynamics and so-called molecular physics including the mechanical theory of gases.[25] Lorenz did not publish on theoretical thermodynamics except that he dealt comprehensively with the subject in his 1877 textbook on heat. The book included a brief reference to the notorious "heat death" apparently following from the different formulations of the second law of thermodynamics due to Clausius and William Thomson. According to Lorenz,

> It is impossible … continually and for ever to produce work by transforming heat to work. … It follows that, for the entire universe, it will not be possible to regain continually the loss of work due to the mechanical action and its accompaniment in the form of heat; consequently, at one time in the future, it [the mechanical action] will diminish and finally cease, that is, converge toward zero.[26]

Although Lorenz did not make use of Clausius' neologism "entropy," he followed Clausius by stating the second law in terms of the quantity corresponding to the entropy difference between two states *A* and *B*, namely

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Lorenz's papers.

[24] Wyatt (1974), containing a photograph of Lorenz.

[25] See Brush (1986) for a full history of the kinetic theory of gases and related fields of physics.

[26] Lorenz (1877), p. 50. The heat death scenario had been introduced to the Danish public in a popular article by Julius Thomsen in 1855. See Kragh (2016), p. 108.

$$\Delta S = \int_A^B \frac{dQ}{T}$$

Later in the book he discussed the validity of the second law and its relation to the kinetic theory of gases, a question of great significance in the period. Not unlike what Maxwell had done in his famous thought experiment colloquially known as "Maxwell's demon," Lorenz imagined a creature which could filter molecules moving fast from those moving more slowly. His creature was not a demon or an intelligent being but a bird:

> Concerning the hypothesis of continually acting internal motions one can object that it leads to the possibility of transforming them into external work even though mechanical equilibrium and temperature equilibrium are present. Thus, let us imagine the wings of a bird to be constructed in such a way that individual air particles could more easily pass through the wings from above and downwards than from below and upwards. The internal motion of the air would then suffice to drive the bird upwards as soon as it stretched out it wings. Sure, this is nothing but a thought experiment. And yet it shows that if internal motions exist [in the air] Carnot's theorem cannot be considered a universally valid principle in the same manner as, for example, the principle of energy conservation.[27]

While Lorenz relegated thermodynamics to his textbook he contributed actively to research in molecular physics and also had an interest in gas theory and its association to heat phenomena. In a paper of 1870 he turned toward one of the central questions of molecular physics, namely to estimate the value of Avogadro's number or, in his and his contemporaries' formulation, to estimate the number A of molecules in a unit of water, which he took to be 1 ml or 1

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[27] Lorenz (1877), p. 190. Maxwell invented his demon (which he called a "finite being") in a letter to P. G. Tait of 1867 and published it four years later in his *Theory of Heat*. The name "demon" was introduced by William Thomson, but Maxwell disliked it.

mg. This he did by a new method based on thermal and electrochemical data for the electrolysis of water.[28]

From Faraday's law of electrolysis and Weber's measurements expressed in electromagnetic units Lorenz stated that the electrical charge necessary to decompose 1 mg of water was

$$Ae = 107,$$

where the symbol e refers to the quantity of electricity needed to decompose a single molecule. Somewhat arbitrarily assuming that the water molecules were closely packed in a tetrahedral form gave him a relation between A and the distance β between two adjacent molecules of water,

$$A = \frac{\sqrt{2}}{\beta^3}$$

Lorenz further argued from energy considerations that

$$\frac{e}{\beta} < \frac{E}{c\sqrt{2}},$$

where E is the electromotive force. Using data for a Daniell copper-zinc element he found in this way an upper limit of

$$A < 1.36 \times 10^{21}\ \mathrm{ml}^{-1}$$

For the molecular distance or approximately the diameter of a water molecule he got

$$\beta < 10^{-10}\ \mathrm{m}$$

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[28] Lorenz (1870a), German translation in *Annalen der Physik und Chemie* **160** (1870): 644-647 and an abridged English translation in *Philosophical Magazine* **40** (1870): 390-392. Review in *Fortschritte* (1870), 669-671 by the Göttingen physicist Eduard Riecke. See also Pihl (1939), pp. 102-104. Like in Section 2.4 I use *A* for Loschmidt's number and not *N*, which is also used as the symbol for the refractivity index.

Lorenz compared his estimates to those of the French physicist Athanase Dupré, a professor at the University of Rennes.[29] Using different methods based on the mechanical theory of heat, Dupré had found four years earlier that

$$A > 1.25 \times 10^{20} \quad \text{and} \quad \beta < 2 \times 10^{-10}\ \text{m}$$

Furthermore and as mentioned below, Lorenz also referred to recent estimates made by William Thomson. Like other scientists working in the field, Lorenz was fully aware that his and others' results were only rough estimates and that their mutual discrepancies did not necessarily mean that they were in contradiction.

In his 1875 study of refraction Lorenz had derived a formula which related the refractive index in the form $(n^2 - 1)/(n^2 + 2)$ to the size of the molecules making up the refractive substance. The relevant term in the formula given in Section 2.3 is

$$\frac{16}{5}\pi^2\frac{n_i^2 - 1}{n_i^2 + 2}\frac{\beta^2}{\lambda^2}$$

Without further explanation or giving any references Lorenz stated that the quantity could be estimated experimentally and that, with λ being the wavelength of sodium light (589.3 nm), it had the value ca. 0.22. The symbol β refers here to the molecular radius or what Lorenz more cautiously called "the radius of the molecular sphere of action, meaning the sphere surrounding a molecule within which there is an appreciable effect of the molecule's influence on the velocity of light propagation."[30] This quantity is greater than the actual radius of the molecule. Since

$$\frac{n_i^2 - 1}{n_i^2 + 2} < 1$$

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[29] Dupré (1866).

[30] Lorenz (1875a), s. 493. Keller (2002), p. 281, finds it "amazing" that Lorenz distinguished between the molecule's particle size and its optical size. But the distinction was common at the time when the "sphere of sensible molecular action" (Cauchy's term) was often associated with the wavelength of light.

Lorenz was able to conclude that

$$\beta > 15 \times 10^{-9}\ \mathrm{m}$$

He was pleased to note that the German physicist Georg Hermann Quincke from recent measurements of viscosity and capillarity had found molecular radii agreeing with the limit inferred from the optical method.[31]

Lorenz returned to the question of molecular size in his last major work, the investigation of light scattering by small spheres (Section 2.4). In this work he had derived a formula for the lower limit of the quantity R and by using $\lambda = 580$ nm and absorption data for atmospheric air he found[32]

$$R \geq 1.41 \times 10^{-10}\ \mathrm{m} \quad \text{and} \quad A = 1.63\ \times 10^{19}\ \text{molecules ml}^{-1}$$

Recall that Lorenz's R was not really a measure of the molecular radius but for a sphere of action considerably greater. Somewhat strangely, it his work of 1890 Lorenz did not refer to his earlier molecular estimates and he also did not refer to values found by other researchers, although at the time there were several of them.

In his scattering paper of 1899 Rayleigh derived the expression

$$A = \frac{32\pi^3}{3h\lambda^4}(N_i - 1)^2\,,$$

which is the same as Lorenz's expression given in Section 2.4 if N_i is only slightly larger than one. Rayleigh argued that the attenuation coefficient h of air was too uncertain to allow an optical determination of A. On the other hand, he believed that Maxwell's value of Loschmidt's number deduced in 1873 from diffusion experiments and other data was close to the mark (see below).

Lorenz's estimate of the size of molecules in 1870 was early, but not the earliest one.[33] Priority belongs to the Austrian chemist and

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[31] Quincke (1869).

[32] Lorenz (1890b), p. 59.

[33] For historical reviews of and references to the early literature, see Hawthorne

physicist Josef Loschmidt, at the time an assistant professor of physical chemistry at the University of Vienna. In a paper of 1865 published in the proceedings of the Vienna Academy, Loschmidt reasoned on the basis of the kinetic theory of gases that the diameter of an air molecule is, in round numbers, 10^{-6} mm. The corresponding figure for the number of air molecules in a volume of 1 cm^3 would be $N_L = 1.83 \times 10^{18}$, but Loschmidt did not state this or any other number for what subsequently became known as the Loschmidt number. The present value of this number is

$$N_\text{L} = 2.688 \times 10^{19}$$

Today Avogadro's number, referring to the number of molecules in a mole, is better known and of course N_A and N_L are easily convertible. For temperature 0 °C the two constants relate as

$$N_\text{A} = 6.022\ \text{mole}^{-1} \cong 22.4 \times 10^3 N_\text{L}$$

Early contributors to molecular physics were primarily interested in the size of molecules and in many cases avoided to state explicitly their values of N_L. Whereas Avogadro's law was well known by 1870, references to the corresponding number only appeared some three decades later.

While Lorenz was probably unaware of Loschmidt's 1865 memoir, he knew about a recent paper in *Nature* in which William Thomson had used four different methods to determine the diameter of molecules. Thomson ended up with an estimate between 10^{-8} cm and 2×10^{-9} cm and cited 6×10^{-21} as an upper limit for N_L. As mentioned, also Maxwell was much interested in the subject. In a paper of 1873 he reported as his best estimates that the diameter of a hydrogen gas molecule was 5×10^{-10} m and its mass 4.6×10^{-24} g. For Loschmidt's number he obtained $N_\text{L} = 1.9 \times 10^{19}$, in fair agreement with the modern value.[34]

The kinetic-molecular theory of gases, pioneered by John J. Wa-

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(1970) and Brush (1986), pp. 75-78.

[34] Maxwell (1965), Part 2, pp. 343-350.

terston and August K. Krönig and developed into a mature form by R. Clausius and J. C. Maxwell, and later again by L. Boltzmann, was one of the great scientific achievements of the nineteenth century. Lorenz followed the theory with interest but also with considerable reservation. The sceptical attitude was not unusual at the time when there was much debate about the status of the kinetic theory and its failure in explaining satisfactorily the structure of molecules.

As it appears from Lorenz's left notes and from various passages in his publications, he remained sceptical with regard to the dynamical foundation of the kinetic theory. Thus, in his 1867 paper on the electrical theory of light he grouped the hypothesis of "heat as motions of the molecules of bodies" together with the questionable hypotheses of electrical fluids and light as ether vibrations.[35] Many of the results derived from the kinetic theory, such as concerned viscosity and heat conduction, relied on assumptions of short-range, repulsive forces between molecules. Maxwell proposed a force varying with the distance r as r^{-5}, but Lorenz objected that this and similar hypotheses failed to guarantee a continual motion of the molecules. He also singled out for criticism an idea suggested by the Austrian physicist Josef Stefan according to which the molecular repulsion arose from a dense cloud of ether surrounding the molecule's hard spherical core. Without providing any calculations he stated that, "With the assumed forces a motionless equilibrium would be possible."[36]

Lorenz used his 1877 textbook on heat to spell out some of his conceptual objections to the Clausius-Maxwell-Boltzmann gas theory. Among these objections were that, to his mind, the theory in its present state seemed irreconcilable with the second law of thermodynamics. Lorenz explicitly criticised the basic assumption that heat and molecular motion were identical, and at the end of the book he addressed Boltzmann's treatment of polyatomic molecules. Molecules such as CO_2 and NH_3 were supposed to be of finite size

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[35] Lorenz (1867b), *Philosophical Magazine*, p. 287. He admitted that the molecular hypothesis, contrary to the two other hypotheses, had exerted "a signal influence on science."

[36] Lorenz (1875a), pp. 494-495. For nineteenth-century views about intermolecular forces, see Brush (1986) and Rowlinson (2002), pp. 162-174.

and provided with a rotational degree of freedom. But Lorenz objected that "if the atoms really are united by means of forces into a molecule, then one might also conceive the atoms to consist of an arbitrary number of mass points." His critique of the physical assumptions of gas theory was brief, heterodox and qualitative. At the very end of the book he wrote: "The assumption of the identity of heat and internal motions explains nothing; it has no support in gas theory and also not in the mechanical theory of heat, and it thus has no foundation at all in the theory of heat."[37] In an autobiographical note of 1877 (see Appendix A) he stated his opposition in no uncertain terms: "I have no confidence in the customary views and physical theories such as, for example, the now generally accepted theory of heat as molecular motion."

It has been suggested that Lorenz's critical attitude indicates that he doubted the existence of atoms and molecules as real physical bodies such as did several physicists and chemists at the time.[38] However, the suggestion is hard to reconcile with his often stated view that solid matter does consists of molecules. The very goal of his studies of refraction and dispersion, Lorenz said, was "to gain more precise knowledge of the bodies' internal molecular constitution."[39] He explicitly assumed that transparent bodies consist of separate molecules between which light propagates with the same velocity as in empty space. Moreover, in his physics textbooks he confidently dealt with chemical molecules as consisting of combinations of atoms. In his 1883 memoir on dispersion Lorenz expressed his belief in the existence of atoms as well as his scepticism with regard to the kinetic theory of heat:

> There can be no reasonable doubt that material bodies consist of separate atoms or molecules... Even though I do not consider it proved at all that heat is identical to inner molecular motion, I do not doubt the existence of such motion.[40]

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[37] Lorenz (1877), pp. 195-197.

[38] According to Pihl (1939), p. 102, Lorenz "was not quite convinced of the reality of molecules."

[39] Lorenz (1869b), p. 205 and similarly in Lorenz (1875a), p. 483.

[40] Lorenz (1883), pp. 167-168.

Again, he ended his 1890 memoir on scattering theory by stating that "the purpose has in part been to demonstrate the possibility of gaining insight about the elements by means of the system's optical properties, the elements themselves being too small to be directly observable." He clearly had atoms and molecules in mind for what he rather confusingly referred to as "elements."[41]

Lorenz's dissatisfaction with the existing molecular gas theory appears indirectly in a mathematical paper he published in Danish in 1875.[42] The work was a gas theory of a sort but entirely different from the theories in the Clausius-Maxwell tradition. It was an attempt to derive the fundamental equations of continuum physics on the basis of a "purely kinematical" theory involving only mass points in motion. Indeed, the term "molecule" appears nowhere in the paper. Lorenz stressed repeatedly that he made no physical assumptions except that the total mass of the system was conserved. There were neither molecules nor forces in his abstract theory. Although he claimed that it was superior to the molecular gas theory, he provided no physical applications and he did not refer to either names or sources. Nonetheless, the work can be seen as related to Maxwell's equations for transport processes as discussed in his great paper titled "On the Dynamical Theory of Gases."[43] Whether or not Lorenz saw it in this light, the work made not impact at all. Lorenz probably realised that it was out of tune with current developments in physics and might be of mathematical interest only.

5.3 Excursions into geophysics

In the late summer of 1869, The International Congress of Anthropology and Prehistoric Archaeology convened in Copenhagen, one of the first international scientific congresses ever in the country. It

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[41] Lorenz (1890b), p. 62. The Danish term "element" generally means a building bloc of something, whereas a (chemical) element is usually called a *grundstof*, literally meaning a basic substance.

[42] Lorenz (1875a), with French translation in Valentiner (1898-1904), vol. 2, pp. 483-492.

[43] See Pihl (1939), which offers a reconstruction of the theory and suggests that it was published as an alternative to Maxwell's gas theory.

was the fourth of a series of archaeological congresses which had started in Neufchatel, Switzerland, three years earlier. With 340 participants from 17 different countries it was a big event, including not only lectures and discussions but also a Royal Banquet at Christiansborg Castle followed by an arrangement in the Tivoli Garden. The president of the congress was Jens J. A. Worsaae, a prominent Danish historian and archaeologist, and among the participants were most of the country's scientific elite.[44] Although archaeology and anthropology were fields far from Lorenz's research interests, he too participated. According to Lorenz, it was a talk given by the German-Swiss geologist Édouard Desor on the changing features of the Earth's surface that inspired him to publish the same year his one and only paper on geophysics.

While a student at the Polytechnic College, Lorenz had followed courses in geology, mineralogy and crystallography by the geology professor Johan G. Forchhammer, and he thus had some background in the earth sciences. In his 1869 paper in *Tidsskrift for Mathematik* he criticised the conventional view that the Earth's sea level or ground level has remained constant though its history.[45] Rather than accepting this unproved hypothesis Lorenz found it more likely that the Earth experienced long-time vertical changes of both a global and a local nature. He thought that these changes might be caused by relocations in the distribution of mass in the interior of the Earth but without commenting on the physical mechanism behind the process. Lorenz was aware that the shape of the Earth could best be approximated by an oblate spheroid, but for reasons of simplicity he took the shape to be spherical and the Earth to consist of concentric, homogeneous layers.

Let a segment of the Earth be defined by a cone with its vertex in the centre and at a constant angle θ relative to the central axis. For a layer with radii between r and $(r + dr)$ he imagined the density to increase with the quantity σ at the expense of a lower density elsewhere. He assumed the total mass M of the Earth to be constant. Lorenz then showed mathematically that the change in mass distri-

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[44] See Wiell (1999).

[45] Lorenz (1869a).

bution during a certain period would result in a changed radius R as given by the increase

$$\delta R = dr\frac{R^3\sigma}{M}2\pi\theta$$

With a mean value for the density of the Earth of $\rho = 5.6$ g cm^{-3}, expressing the angle θ in degrees, and inserting

$$M = \frac{4}{3}\pi R^3\rho$$

he wrote the increment as

$$\delta R = 0.00467\delta\rho\sigma\theta$$

Using as an example $\sigma = 0.1$ and $\theta = 1$ degree he showed that these values would lead to an appreciable increase in the Earth's radius, say of the order of 1 metre. The result, he concluded, "shows quite clearly that over long periods of time changes in the mass distribution of the inner Earth may well produce an appreciable change in the ground level; this is the case whether one thinks of motions in the liquid interior of the Earth or of melting and solidification at the border of the Earth's crust."[46] He further argued that the mechanism could account also for local elevations and depressions on the surface of the Earth. Notice that Lorenz apparently assumed the interior Earth to be in a liquid state. The idea of a hot liquid interior surrounded by a relatively thin crust was popular in the first half of the nineteenth century but later on it was opposed by William Thomson and others, who favoured a completely solid Earth. At the time of Lorenz's paper the dispute between "fluidists" and "solidists" was still unsettled.[47]

In his textbook on heat for the students at the Royal Military High School, Lorenz returned briefly to problems of a geophysical

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[46] Lorenz (1869a), p. 154.

[47] Twenty years later the doctrine that the Earth is solid throughout had won general acceptance. See Brush (1996), pp. 150-174 for different views concerning the physical state of the interior of the Earth.

nature. From the general theory of heat propagation he considered a one-dimensional rod with a temperature at $x = 0$ varying periodically as

$$T(t) = T_0 + A\cos(\alpha a t)$$

T_0 and a are constants and the quantity α is given by

$$\alpha = aq/2\kappa\,,$$

where κ is the thermal conductivity; the quantity $q = c\rho$ is the product of the heat capacity and the density of the material of the rod. The temperature variation in space and time could then be expressed by

$$T(x,t) = T_0 + A\exp(-\alpha x)\cos\alpha(at - x)$$

The heat will propagate as a wave with velocity α, wavelength $\lambda = 2\pi/\alpha$, and an attenuating amplitude. In the distance λ from the boundary plane at $x = 0$, the amplitude will be reduced from the value A to $A\exp(-2\pi)$.

The surface of the Earth is subject to periodical changes in temperature and due to these changes heat waves will propagate towards its inner parts. Based on field measurements Lorenz estimated that the maximum of the heat wave caused by the annual temperature variation would occur at a depth of approximately 11 m half a year later than at the depth of 1 m. The corresponding wavelength is then about 20 m, which, with the year as a unit of time, is the same as $\alpha = 2\pi/20$. "Thus, if the minimum and maximum of the temperature is known at the depth of 1 m … and also the moment for the arrival of the maximum, from which the time can be found, one can approximately calculate the temperature in any depth x and to any time t."[48]

Assuming the Earth to be thermally homogeneous Lorenz considered the question of the thermal conductivity κ of the layers of the Earth. Using the units m and 10^3 kg, clay soil has density 1.8 and

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[48] Lorenz (1877), p. 176.

heat capacity 0.44, from which $q = 0.8$; moreover, from $a = 20$ and the formula $aq = 2\kappa\alpha$ it follows that κ is approximately 25. From observations it is known that the temperature gradient is about 1 degree per 30 m. From this Lorenz inferred: "The amount of internal heat which the Earth loses corresponds to a heat increase of 1 ° in a layer of water of height 0.8 m covering the entire Earth. As a result of this loss of heat the Earth as a whole would cool no more than 1 ° in the course of a few million years." He did not consider the influence of solar heat on the Earth's heat balance.

In his textbook Lorenz also considered the problem of two different bodies in contact over a plane surface.[49] The temperature of one of the bodies is T and that of the other body T'. When brought into contact, what is the temperature T_0 at the bounding surface? Assuming the surface to be infinitely thin and the bodies in contact to be infinitely large, Lorenz found that the answer was approximately given by

$$T_0 = \frac{T\sqrt{\kappa c \rho} + T'\sqrt{\kappa' c' \rho'}}{\sqrt{\kappa c \rho} + \sqrt{\kappa' c' \rho'}}$$

Thus, for given T and T' the surface temperature is constant and not as one might expect proportional to the thermal conductivities of the bodies. It follows from the equation that

$$\frac{T - T_0}{T_0 - T'} = \sqrt{\frac{\kappa' c' \rho'}{\kappa c \rho}}$$

If the body at temperature T' refers to air and the other body to a solid, $\rho \gg \rho'$ whereas $c \sim c'$ and $\kappa > \kappa'$. Hence

$$T - T' \gg T - T_0 ,$$

meaning that a warmer solid body in contact with air will only be cooled insignificantly. He commented that "This is a major reason why organic substances filled with air, such as wool and feather, are good thermal insulators."

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[49] Lorenz (1877), pp. 177-178; Christiansen (1897), pp. 317-318.

Lorenz's few and scattered writings related to geophysics were not original research and not meant to be so. They probably reflected what he knew from the literature of earlier and contemporary ideas. In his fundamental work on heat conduction from 1822 Joseph Fourier had estimated the effect on the solid Earth of periodic temperature variations due to solar heating. He also estimated the loss of the Earth's internal heat and its consequence in the form of surface cooling but only to conclude that the cooling was negligible. Lorenz had undoubtedly studied Fourier's famous *Théorie Analytique de la Chaleur* in detail.

Later in the century the question was reconsidered in the light of the new thermodynamics and taken up by William Thomson, in particular, in his controversial arguments for a young Earth.[50] From detailed calculations Thomson concluded in 1865 that the heat conducted out of the Earth was far from negligible and that it would limit the age of the Earth to at most 100 million years. For the average temperature gradient he estimated 1/28 °C per metre of depth, close to the value cited by Lorenz. There was in the period a great deal of interest in the cooling Earth, a subject which on a popular level was also described in the Danish literature, and it is possible that the interest is reflected in Lorenz's textbook.[51]

Although of no international significance, Lorenz's unassuming paper of 1869 was the first mathematical work in geophysics in the history of Danish science. At the time geophysics scarcely existed as a discipline in Denmark, where interest in the earth sciences was largely limited to classical geology, terrestrial magnetism, meteorology and oceanography. No one at either the University or the new Meteorological Institute, when it was founded in 1872, seems to have noticed Lorenz's paper hidden in a local mathematical journal.[52] And yet it deserves to be known as an interesting contribution to the earliest phase of Danish geophysics.

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[50] See Brush (1996) and Burchfield (1975).

[51] For an example from the popular literature, see Fogh (1855) written by a geologist. Lorenz (1865c) dealt briefly with the heat balance of the Earth, citing the temperature increase of the surface of the solid Earth to be about 1 °C per 100 feet or 31 m of depth.

[52] Lorenz's paper was not included in Valentiner's French edition of his collected

An unpublished note on the process of ice formation can with some good will be considered as belonging to the field of mathematical geophysics. While the note is no longer extant, its essence is known from C. Christiansen's textbook in theoretical physics published in 1887.[53] Consider a lake covered by a sheet of ice of thickness δ and having the temperature $-\theta_0$ at its uppermost surface. The temperature θ of the mass of ice is a function of the time t and the distance x from the surface. Then the equation

$$\frac{\partial\theta}{\partial t} = \kappa^2 \frac{\partial^2\theta}{\partial x^2}$$

is valid everywhere within the ice sheet. New ice will be formed continually at the boundary between ice and the water. By formulating the problem mathematically Lorenz found an expression connecting θ and L, where the latter quantity denotes the specific heat of fusion of ice or the energy required to melt a gram of solid ice to a liquid. With c the specific heat and $\delta(t)$ denoting the varying thickness of ice, he showed that the connection is given by

$$-\frac{c\theta}{L} = \frac{1}{2!}\frac{1}{\kappa^2}\frac{d\delta^2}{dt} + \frac{1}{4!}\frac{1}{\kappa^4}\frac{d^2\delta^2}{dt^2} + \cdots$$

If the thickness of the ice sheet increases linearly as $\delta = q\kappa t$, where q is a constant, it follows that

$$c\theta_0/L = \exp(q^2 t) - 1$$

Lorenz considered his analysis to be a nice exercise in mathematical physics, but there is nothing which indicates that he applied it to the real conditions of freezing lakes or otherwise saw it as a contribution to theoretical meteorology or glaciology.

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papers. It is not mentioned in Egedal et al. (1945), a survey of the history of geophysics in Denmark, and also not in other sources.

[53] Christiansen (1897), p. 307, a translation of Christiansen (1886-1887), where the problem is treated in vol. 2, pp. 16-18. According to Christiansen, "the solution was communicated to the author by L. Lorenz." See also Pihl (1980) and Christiansen (1891b), where Christiansen reported experiments on the formation of ice and compared them with the theory.

Figure 5.1: The Austrian physicist Josef Stefan (1835-1893) known eponymously from terms such as Stefan flow, Stefan problem, and Stefan-Boltzmann law. Lorenz worked in some of the same research areas as Stefan. https://en.wikipedia.org/wiki/Josef_Stefan.

On the other hand, the problem of the formation and growth of ice was not new and at the time it attracted increased interest. In 1891 Stefan developed a theory somewhat similar to the one of Lorenz and Christiansen but with less emphasis on mathematics and more on experimental evidence. In his paper on "Theorie der Eisbildung" (Theory of Ice Formation) published in *Annalen* he compared the theory with measurements made in the arctic regions. While Stefan's theory attracted interest among geophysicists and oceanographers, Lorenz's analysis remained unknown in the community of earth scientists. One may still find references in the modern literature to the "Stefan problem" and to what somewhat confusingly is called "Stefan's law," a law or equation which is essentially the same which Lorenz derived without publishing it.[54]

As Lorenz's unpublished work on ice formation was of indirect relevance to the earth sciences, so was it the case with his unpub-

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[54] Stefan (1891); Leppäranta (1993). For a historical note on the Stefan problem, see http://ta.twi.tudelft.nl/nw/users/vuik/papers/Vui93e.pdf. "Stefan's law" usually refers to the law of blackbody radiation, cp. Section 3.3.

lished work on the hydrodynamics of an ellipsoid in motion referred to in Section 2.1. At least, Christiansen - or possibly Lorenz - suggested that the complex calculations might be used to determine the velocity of raindrops formed in the clouds and thus potentially to contribute to theoretical meteorology.[55]

5.4 Political writings

Lorenz was not a political animal and he generally kept out of discussions that were not narrowly related to questions of mathematics and physics. And yet, at two occasions he did intervene in the political debate going on in Denmark, an indication that he was, after all, concerned with more than just equations and experimental data. These publications had no impact whether political or otherwise, but they belong to Lorenz's literary heritage as much as do his minor contributions to physics and mathematics. In 1865 he published anonymously or rather semi-anonymously (he wrote under the name "L. L.") a pamphlet in which he discussed one of the period's most burning questions, namely the revision of the country's constitution after the devastating defeat to the Prussian forces in the war of 1864.[56]

On 5 June 1849 king Frederik VII signed Denmark's first democratic constitution which ended the nearly 200-year long era of absolute monarchy. The new Parliament consisted of two chambers, the *Folketing* (Lower House) and the conservatively oriented *Landsting* (Upper House) with different rules for election to the two chambers. A provisional reform of 1855 was intended to be valid for the Unitary State (Helstaten) including Schleswig-Holstein, but in the end it excluded German-speaking Holstein. A revised constitution of 18 November 1863 separated off Holstein and confirmed that the constitution was valid only for Denmark and Schleswig. This move caused national as well as international problems not least because it was a clear violation of the London Agreement of 1852 according to which Denmark had agreed to treat the two duchies on an equal

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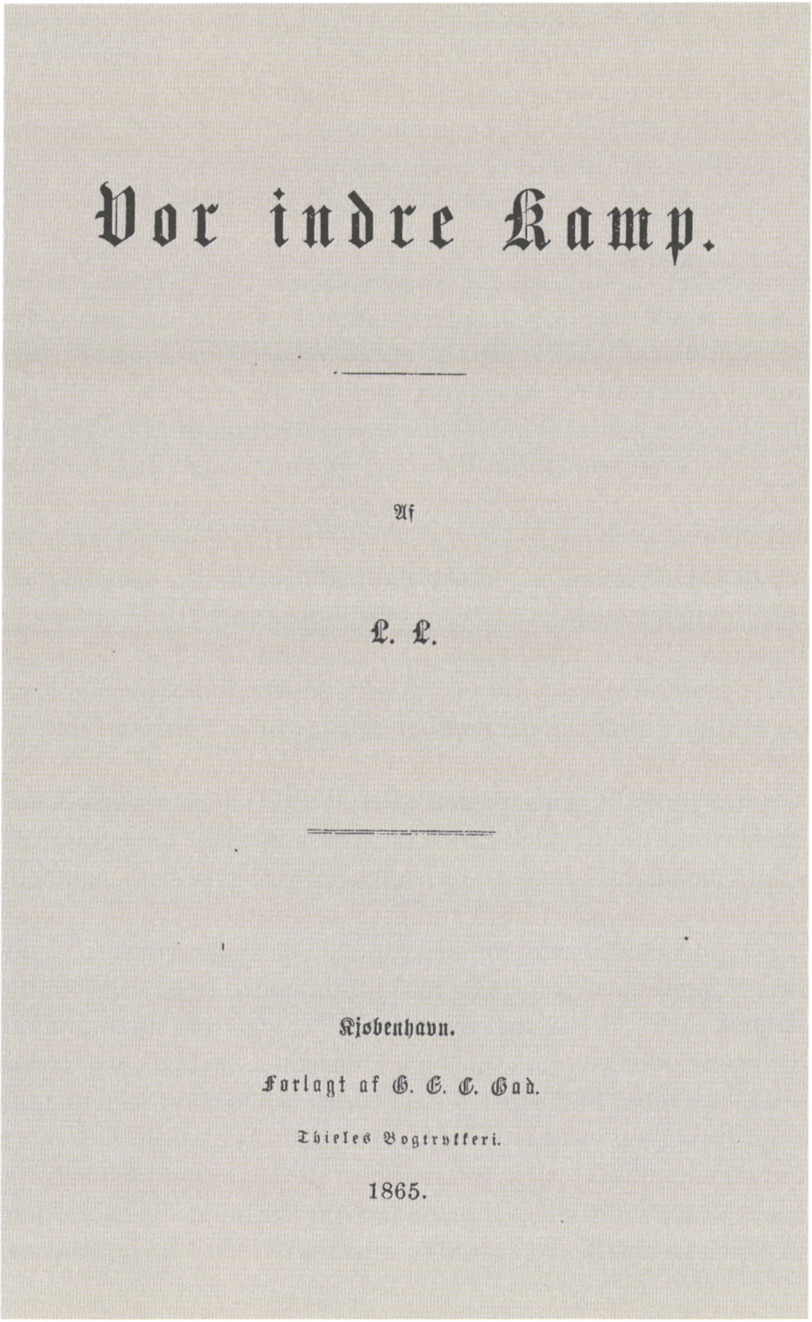
[55] Christiansen (1887-1889), vol. 1, p. 202.

[56] For the political situation, see Jespersen (2004), p. 58-66.



Vor indre Kamp.

Af

L. L.

Kjøbenhavn.
Forlagt af G. E. C. Gad.
Thieles Bogtrykkeri.
1865.

Figure 5.2: Lorenz's semi-anonymous political pamphlet of 1865.

footing. It was a major reason for the attack of Prussian and Austrian troops the following year.

For a period of time after the disastrous war there was a great deal of political confusion with in reality two constitutions and two corresponding legal assemblies, one of June 1849 and the other of November 1863. The National-Liberal Party was in favour of the latter constitution which it wanted to be the common one for all parts of the Unitary State, while the Farmers' Party (Bondevennerne) wanted to retain the 1849 constitution. In May 1865 an election was called on the delicate issue and after much debate and many negotiations, in July 1866 a compromise was agreed upon by the three major parties, the National-Liberals, the Farmers' Party and a conservative group of influential landowners. The result was essentially a revision of the original constitution but with the transformation of the *Landsting* into a stronger and more conservative political force.

Lorenz's pamphlet with the title *Our Inner Fight* (Vor Indre Kamp) was clearly a polemic aimed at the forthcoming election (Fig. 5.2).[57] It was a passionate call for retaining the 1849 constitution in an unaltered form. In agreement with the view of the Farmers' Party Lorenz stated that, "as long as the people is not demoralised, the constitution should be considered as sacred by the people." He criticised the National-Liberal Party, which he called the Centre Party, for being reactionary, out of tune with the wishes of the people, and unable to agree upon a new constitution. As he saw it, the key question was simple, namely a choice between the original constitution and no constitution at all - presumably meaning a return to absolute monarchy. Perhaps with an allusion to Kierkegaard, he summarised his message with the words "either-or." Indeed, the pamphlet's style, its florid metaphoric language and its emotional content were reminiscent of the young Lorenz and his early interest in wide-ranging philosophical and ethical issues. His excursion into the world of politics had nothing in common with the analytical and scientific papers he wrote at the time.

If Lorenz's work of 1865 was a political manifesto, his later and

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[57] Lorenz (1865b).

quite different work may be classified as an abstract contribution to political science. In connection with a discussion in Danish newspapers concerning an election to the Parliament, Lorenz decided in 1890 to analyse how the chosen constituency system and the number of constituencies affected the representation of minority parties in the Parliament.[58] He found the question to be interesting, not so much for political reasons as because it could be neatly analysed by means of mathematical probability theory. It was not the first time that a Danish scientist applied his mathematical skills to the rules of the new democratic order. The military officer, mathematician and geodesist Carl C. G. Andræ proposed in 1855 a so-called transferable vote system for elections designed to achieve proportional representation. Andræ's innovative system was first used in 1856 and later adapted for elections to the second chamber (the *Landsting*).[59] The system also attracted attention outside the country.[60]

In his paper in *Nyt Tidsskrift for Matematik* (New Journal of Mathematics) Lorenz made a number of assumptions, including that there was the same number of votes in each constituency, that the candidates were chosen by the majority party or group, and that the total number of votes was distributed equally among the constituencies. After a heavy dose of mathematical analysis involving complicated integrals and series expansions he felt justified to make a few general conclusions. For example, if a minority party had at least 30 per cent of the votes and there were more than ten constituencies, then the probability for its representation in the Parliament would not depend on the number of constituencies.

Lorenz's paper was severely criticized by Thorvald Thiele, who was an expert in statistics and actuary mathematics. Thiele was not so much concerned with Lorenz's mathematics as with his unrealistic and arbitrary assumptions, which led to conclusions of academic

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58. Lorenz (1890a).

59. Andræ (1812-1893) taught topography, geodesy and mathematics at the Military High School. Although he published little, he was highly regarded as a scientist and administrator. He was a member of the Royal Danish Academy of Science since 1853 and also had a long and distinguished political career.

60. See Andrae (1926), written by Carl Andræ's son in part to defend his father's priority to the election method.

mathematical interest only. Using more realistic and sophisticated assumptions Thiele's conclusions regarding the representation of minorities were quite different from Lorenz's. As Thiele pointed out, "Probability calculus differs from mathematics by its essentially practical character ... It is far from inconsequential and with regard to its practical applications even dangerous if, as in this case, an excellent mathematical treatment turns into an exercise in probability theory based on assumptions the connections of which to the real world the author does not even consider."[61] Lorenz did not respond to Thiele's scathing objections.

5.5 Lorenz and the physics community

During Lorenz's lifetime theoretical physics developed into a distinct and potent branch of the physical sciences, on the one hand with a strong foundation in mathematics and on the other hand closely linked to experiment. At his death in 1891 the German-inspired theoretical physics had made tremendous progress and was recognised as perhaps the most fundamental of all natural science. And yet this was only the beginning, for with increased epistemic status and institutional support its greatest successes were still in the future. According to Helmholtz the new research field offered a profound understanding of the laws of nature or what he eloquently called an "intellectual mastery over nature."[62]

The development through the second half of the nineteenth century differed from country to country and was generally dominated by the major European powers with Germany in the front. In the United Kingdom, theoretical or mathematical physics prospered too, but within a different tradition and often under the older labels of "natural philosophy" or "mixed mathematics." In the case of Denmark and several other small nations theoretical physics barely existed at the end of the century or was represented by only one or two physicists. H. A. Lorentz's appointment in 1877 to a new chair of theoretical physics at the University of Leiden was extraordinary

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[61] Thiele (1890).

[62] Jungnickel and McCormmach (1986).

and possibly the first such chair outside Germany.

Ludvig Valentin Lorenz was not only Denmark's first theoretical physicist in the new European tradition, for a period he was also the only physicist in the country who made noteworthy contributions to the international scene of physics. The next theoretical physicist of importance was Niels Bohr, who belonged to a different era and whose only and indirect connection to Lorenz was through Christiansen, Lorenz's friend and Bohr's teacher. Appropriately, when Bohr was awarded his doctoral degree in 1911, Christiansen placed the young physicist in a broken tradition going back to Ørsted and Lorenz. According to a report in the Copenhagen newspaper *Dagbladet* (The Daily):

> Professor Christiansen recalled that following H. C. Ørsted, among the Danes Lorenz was the one most acquainted with the concerned scientific subject area. The professor always used to consult Lorenz when he needed answers to questions of this kind. Since the days of Lorenz we have had no real competence in the field referred to [theoretical physics] and for this reason the opponent [Christiansen] expressed his delight that now Niels Bohr had met this need.[63]

Thirteen years later, on the occasion of the award of the Ørsted Medal to Bohr, the physics professor Martin Knudsen again alluded to the nineteenth-century tradition in Danish physics. Referring to the modern version of Maxwell's electromagnetic theory of light with light waves emitted by electrons in motion, Knudsen said: "Such conceptions, already dimly conceived by Ørsted, were to take firmer shape in the hands of our countryman L. Lorenz only a few years after Maxwell and independently of him."[64]

Perhaps it was an advantage that Lorenz was not a member of the small group of physicists who held positions at the University and the Polytechnic College and who to some extent carried on the

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[63] Bohr (1972), p. 99.

[64] Bohr (1984), p. 137. The Ørsted Medal was established by the Society for the Dissemination of Natural Science for which Martin Knudsen (1871-1949) served as chairman from 1900 to 1939.

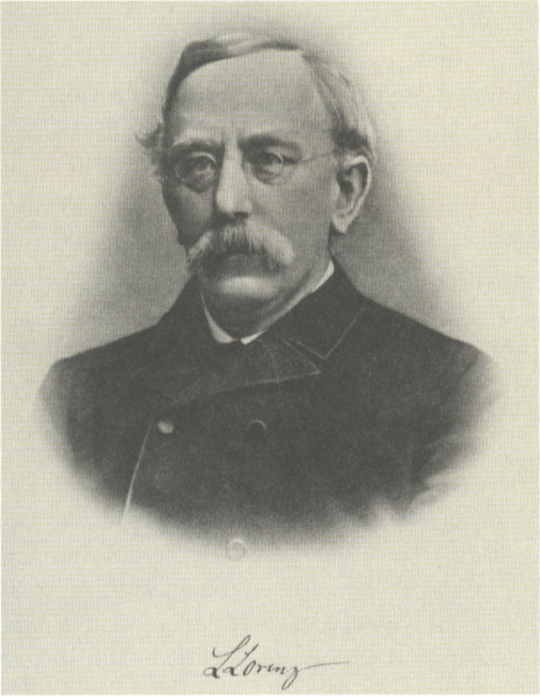

Figure 5.3: Ludvig Valentin Lorenz (1829-1891). Royal Library, Copenhagen, Picture Collection.

heritage of Ørsted. As an independent scientist he was forced to look abroad and publish some of his major works in German physics journals. The highly qualified Lorenz would have been an obvious candidate for one of the very few academic physics positions in Denmark. The reason that he never obtained such a position was not resistance from the academic establishment but simply that there were no vacancies in the 1850s and 1860s. Recall that at the time there was only a single physics professorship in the country. Instead Lorenz became a teacher at the Royal Military High School and its successor institution, the Army's Military Academy. In this position he thrived and seems to have been quite satisfied despite a heavy teaching load. The School comprised a good library and an excellent laboratory - what more could a physicist want? Not only was Lorenz not career-minded, he also seems to have been content with working in problem areas for the sole reason that they interested him. If others failed to share his interest, he did not care very much.

Although in some ways an outsider, Lorenz was recognised by his Danish peers as a brilliant physicist and mathematician, such as demonstrated by his election to the Royal Danish Academy of Science at the relatively young age of 37 and even more so by the lifelong grant he was later awarded by the Carlsberg Foundation. We have another indication of his recognition in a testimony of 1873 from German-born Heinrich Louis d'Arrest, professor of astronomy and director of the Copenhagen Observatory:

> The higher mathematical physics, which Associate Professor Lorenz is at present the only one to cultivate in our country, is not a speciality of my own. ... Nonetheless, I do not hesitate to point out to the high Ministry that Associate Professor Lorenz is one of the first and internationally respected scientists within this field. His extremely penetrating investigations belong to the most difficult and remarkable accomplishments of our time. Among mathematical physicists there is no second opinion about this. These investigations are concerned with the so far little known relations between light and electricity and with theoretical deductions regarding the molecular constitution of certain bodies; others are contributions to the absolute determinations of fundamental quantities in the theory of heat.[65]

On the other hand, the smallness of the Danish physics community and the absence of competence in advanced theoretical physics meant that much of Lorenz's work was not properly appreciated if appreciated at all. The only exception to Lorenz's professional isolation was the younger Christian Christiansen with whom he formed a useful and relatively close friendship. Lorenz was a loner, an autodidact who neither had the opportunity nor the desire to build up a school of physics. Although he had connections to a few Danish physicists, in particular Christiansen and Prytz, he had no pupils or collaborators. All of Lorenz's publications appeared with him as the sole author.

As mentioned, compared to other Danish physicists Lorenz was internationally oriented and for several decades the only one with a

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[65] Letter of recommendation appended to an application of Lorenz to the Ministry of Church and Education, 8 April 1873 (Lorenz Papers, DTM).

solid reputation abroad. He was probably better known and more appreciated by physicists in German-speaking Europe than in his own country. Not only did he write in leading physics journals such as *Annalen* and *Crelles Journal*, he also corresponded with several of Europe's premier physicists. As an indication of his international position he was the only Scandinavian invited to cosponsor the festschrift in honour of Helmholtz's 70th birthday.[66] However, he did not live to contribute to the festschrift. Among his correspondents were distinguished scientists including Ludwig Boltzmann, Carl Neumann, Friedrich Kohlrausch, Heinrich F. Weber, August Kundt and, in France, the later Nobel Prize laureate Gabriel Lippmann. In Scandinavia he corresponded with Carl A. Bjerknes, Peter Waage, Gösta Mittag-Leffler and Hugo Gyldén, among others. "I have been happy to use this opportunity to enter a correspondence with you," Boltzmann wrote in 1879. "Allow me to assure you about my deepest respect and to request that you send me your memoirs also in the future."[67]

On the other hand, although well connected Lorenz showed little interest in communicating and explaining his research to foreign scientists even in cases where it might have been natural. Thus there are no extant letters in which he discussed his works in optics and electrical theory to any extent. As noted previously (Section 2.4), Lorenz had no contact with Rayleigh concerning the scattering of light on spheres even though this was a topic of common interest to the two physicists. The same holds true with respect to the optical theory of refractivity, where there was no personal contact between H. A. Lorenz in Leiden and L. Lorenz in Copenhagen. He kept a notebook with scientists to whom he sent offprints of his papers, and the list contains the names of many of Europe's most distinguished physicists.[68] To mention but one example, he sent offprints of his 1879 *Annalen* paper on the propagation of electricity to

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[66] *Ansprachen und Reden Gehalten bei der am 2. November 1891 zu Ehren von Hermann von Helmholtz Veranstalteten Feier* (Berlin: Hirschwald, 1892).

[67] Boltzmann to Lorenz, 28 August 1879 (Lorenz Papers, DTM).

[68] Lorenz Papers (DTM). Other names on his list of recipients were O. Chwolson, A. Ångström, M. Brillouin, H. Gyldén, E. Mascart, Lord Rayleigh, S. Thompson, H. Poincaré, J. Rydberg and G. Quincke.

F. Kohlrausch, E. Ketteler, C. F. Zöllner, W. Weber, A. Kundt, C. Neumann, R. Clausius, H. Helmholtz, L. Boltzmann, J. Stefan, W. C. Röntgen, and H. A. Lorentz.

With regard to his electrodynamic theory of 1867, apparently Lorenz did nothing to make it better known or to contact physicists working on related theories. He undoubtedly knew that Maxwell had commented critically on the theory (Section 3.2) and yet it did not occur to him to establish contact with the physicist in Cambridge. Nor is there any known correspondence with Clausius who in 1868 entered the question of electrodynamics based on retarded potentials. Finally, given that E. Christoffel had dealt seriously and critically with Lorenz's early works in optics in the abstract journal *Fortschritte* (Section 2.1), one might assume that Lorenz would respond to the German mathematician. But as far as we know, he did not.

Thus, in spite of his international orientation and command of German and French, Lorenz had no close contacts abroad. Travels brought him a few times to Paris and also to Leipzig and probably other German cities, but most of his time he lived a somewhat isolated life in peripheral Copenhagen. In 1845 the oldest organisation for physicists was formed as the Physical Society of Berlin, the precursor of the mighty German Physical Society (Deutsche Physikalische Gesellschaft). The trend-setting Berlin society included foreign members, but Lorenz was not a member and he never participated in any of the meetings in Berlin or elsewhere.

After Lorenz's death and Valentiner's publication of his collected works, the memory of Lorenz faded in his native country as well as abroad. Internationally he appeared in some of the few histories of physics written in the first half of the twentieth century, but only as one name among many other names. His contributions to physics entered Whittaker's *History of Aether and Electricity* from 1910 and also Edmund Hoppe's *Geschichte der Physik* (History of Physics) published in 1926. The latter work, limited to the history until about 1910, mentioned Lorenz in connection with optical polarization, electrical oscillations, the ohm unit, and electrodynamics based on retarded potentials.[69] His electrodynamic theory of light as an alter-

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[69] Whittaker (1958); Hoppe (1926). Apart from five references to Lorenz, Hoppe's

native to Maxwell's also appeared in Humphrey Pledge's *Science Since 1500* published in 1939.[70]

The centenary of Lorenz's birth in 1929 was ignored by Danish physicists, but three years later Hans Marius Hansen, a professor of physics at Copenhagen University, portrayed Lorenz in a book commemorating "Danish men of science ... [who] have broken new trail by some pioneering work in their special study, or ... have been standard-bearers in the progress of science."[71] Of the 45 deceased Danish "men of science" (and they were all men) included in the volume only two were physicists, the other being the unavoidable H. C. Ørsted.

Only late in the 1930s did the physicist and able historian of science Kirstine Meyer take up a serious study of the life and work of Lorenz based on archival studies. Her extensive work resulted in a valuable but brief article in a Danish biographical dictionary.[72] It was primarily a younger colleague of Meyer, the physicist Mogens Pihl, who undertook the difficult task to understand and analyse in detail Lorenz's works in physics on which topic he wrote his doctoral dissertation in 1939.[73] In his later years Pihl wrote several papers on Lorenz and also the distinguished Belgian-Danish physicist Léon Rosenfeld, a close collaborator of Niels Bohr, contributed with a paper on Lorenz's theory of light as electrical oscillations.[74]

All the same, Lorenz largely remained unknown to physicists as well as historians. When Ronold King, an American applied physicist, reviewed Pihl's book in the history of science journal *Isis*, he wondered how many knew about Lorenz and his achievements. "The reviewer requested a well-known library research service to look up the life and work of L. V. Lorenz. The answer was: 'A careful investigation of available library sources failed to yield any information in English on this physicist.' Probably few men have

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book also referred to Ørsted (nine times), Christiansen (four times) and Colding (three times).

[70] Pledge (1939), p. 148.

[71] Meisen (1933), preface.

[72] Meyer (1938).

[73] Pihl (1939).

[74] Pihl (1972a); Pihl (1972b); Pihl (1980); Rosenfeld (1979).

been given so little credit as has L. V. Lorenz."[75] King further reflected on the reasons why the Danish physicist was so undeservedly forgotten:

> The reasons are several. Among the most important is the fact that practically all of Lorenz's principal contributions were discovered independently at about the same time by other investigators who were not scientifically so isolated. In always every case it was they who ultimately received exclusive credit. ... Another reason for Lorenz's obscurity, cited as the principal one by Mr Pihl, is the complicated and difficult-to-follow nature of Lorenz's publications.

While there is some truth in this assessment, it is not the whole truth. Moreover, Lorenz was not quite as unknown and his work not quite as obscure and unappreciated as King supposed.

The situation lamented by King changed to some extent when Nelson Logan and others in the 1960s rediscovered Lorenz's 1890 treatise on the scattering of light by spheres.[76] With the rise of interest in Mie's theory followed an increased attention to Lorenz's role as precursor to what eventually became the Mie-Lorenz or Lorenz-Mie theory (Section 2.4). The year 1991 marked the centenary of Lorenz's death, but with a single exception the event went unnoticed.[77] From a more historical and contextual perspective H. Kragh called attention to Lorenz's contributions in a couple of papers,[78] and about a decade later the Danish physicist Ole Keller published what may well be the definitive study, at least in the eyes of physicists, of Lorenz's extensive work in optics.[79] Another important study from the same time was David Jackson's and Lev Okun's work on the origin of the gauge concept highlighting the Lorenz and not the Lorentz gauge.[80]

This is more or less the historical lineage leading up to the present biography.

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[75] R. King, review of Pihl (1939) in *Isis* **40** (1949): 64-66.

[76] Logan (1965). See Section 2.4.

[77] Kragh (1990).

[78] Kragh (1991); Kragh (1992).

[79] Keller (2002).

[80] Jackson and Okun (2001).

Appendices

Appendix A: Autobiographical sketch

The following is a translation of a brief autobiography which Lorenz wrote in 1877 in connection with his reward as honorary doctor at the University of Uppsala this year. On this occasion Lorenz submitted a biographical text and although much shorter than the one below, some of the phrases are the same.

> I, L. V. Lorenz, am the son of grocer J. G. Lorenz († 1849) and Charlotte Lorenz, born Scherfin. My father was of German stock and born in Stralsund. My mother's father was born on Rügen but came, many generations ago, from French ancestors who were forced to leave their native country because of religious persecution.
>
> When I was six years old my family moved to Maribo on Lolland, where I received education in a private school until the age of fourteen. Very early on I showed an interest in calculations and mathematics in which fields my father gave me the first guidance; I studied them myself, not so much by reading textbooks as by thinking about mathematical problems which I found in books or posed to myself. Moreover, an evening lecture in physics aroused my interest in this field at an early date (I was twelve years old) and I soon realised that I wanted to make the study of mathematics and physics my calling. This goal was constantly in my mind.
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> However, I deliberately shelved my mathematical speculations when I, at the age of fourteen, started at Nykøbing School on Falster. This was in part because I wanted to complete my education at the learned school as quickly as possible, and in part because I felt that my health deteriorated due to my too eager occupation with mathematical problems. When I graduated as a student (in 1846) I continued putting a restraint on myself with regard to mathematical studies. To develop myself in a philosophical sense I shunned over-specialisation.

I passed the so-called "second exam" at the university and subsequently I studied at the Polytechnic College, where I chose the chemical class, believing that I would benefit in particular by practical work in the laboratory. Only after having passed the exam (in 1852) did I seriously engage in the advanced study of mathematics. At the time I could no longer count on support from my home and consequently I took on the job as private tutor in the house of Count Moltke-Hvidtfeldt. After about a year, part of which I spent in Dresden, I returned to Copenhagen, where I was employed as an assistant at the Physical Collection and as a school teacher in physics and chemistry.

In 1854 I answered a university prize competition with an essay on the geometrical properties of Fresnel wavefronts. In this way I received free lodging at Borch's Student Residence and, for a period of seven years, the large Smith Stipend. As a result I was able to cultivate my studies without bothering too much about teaching at the school.

In 1856 I participated in the Meeting of Scandinavian Scientists in Christiania. The following year, after having occupied myself too eagerly with a mathematical problem – the equilibrium positions of rotating liquid bodies – I fell ill due to overwork and became hospitalised with some sort of typhus. For a long time thereafter I was once more forced to keep away from mathematical studies. On the other hand, while a student I had taken up with interest physics and in particular physical theories such as the theory of heat and the molecular relations of bodies.

All this was for a long period of time in my mind. I have to say that I only reached a negative result: This true workplace of nature was like a fortress which could not be conquered by storm but only by a slow siege in the form of following methodically the rules of science. This view which I arrived at runs through all my scientific writings, from my memoir on the theory of light to my last work on "The Science of Light." The nihilistic view would be a disaster for science and is quite foreign to me. But I have no confidence in the customary views and physical theories such as, for example, the now generally accepted theory of heat as molecular motion. I try to avoid any physical theory that does not follow by necessity from the phenomena. In this way one avoids much of what is considered uncertain even today. I defend what may be called a realistic position.

Supported by means from the government and the University I stayed for about a year (1858-1859) in Paris. I attended mathematical lectures by Bertrand, Lamé and Duhamel, and those on physics given by Regnault, Becquerel (father and son), Despretz, Dessain and others. My first scientific papers were of a purely theoretical nature, such as Mem. sur l'elasticité (Crelles J. 58, 1860). After I was appointed teacher at the Military High School in 1866, its physical collection offered me a welcome opportunity to deal with practical physics. I made determinations of the refractive properties of bodies accompanied by theoretical investigations. My mathematical treatises can be found in nearly all the fifteen volumes of the mathematical journal.

In 1866 I was elected a member of d. K. D. V. S. [Royal Danish Academy of Sciences and Letters] and three years later I was honoured with the Knight Cross of the Order of Dannebrog and, last year, with the title of professor. When the Military High School ceased to exist and was replaced with the Army's Military Academy I was transferred to the latter school as a teacher, a position I still hold. In 1862 I married Agathe Lorenz, born Fogtmann.

Appendix B: Introductory lecture in elasticity theory

Undated draft of lecture to students at the Royal Military High School (Lorenz Papers, NBA).

The Importance of Mathematical Physics for Science

Mathematics enters where the senses are unable to reach and that is exactly where the centre of gravity is for science as a whole. It will lead us to an explanation of light, heat, electricity and magnetism and, more generally, to the completely hidden essence of the physical bodies and their actions. This crucial problem has so far resisted any kind of [explanation from] induction; we know nothing and none of the proposed hypotheses have led us to the goal. The reason must be that time is not ripe for hypotheses and inductions applied to a field which is not as yet sufficiently cultivated. First of all, it needs to be further and much more thoroughly cultivated by pure mathematics. As far as the theory of elasticity is concerned our task is primarily to establish how far we can proceed on the basis of the concept alone and without the use of physical hypotheses. Only then will it be possible to infer from the phenomena which hypothesis is inevitably necessary and which is merely an appendage.

In the case of light we recognise that the theory explains very much; and yet something remains which cannot be explained. It is therefore just a first approximation, but we understand what the next approximation has to explain. I believe that the theory of elasticity is presently the branch of science which has the greatest importance. I believe that it will not only lead us to the explanation of sound and light but also of heat, electricity and magnetism. Belief, even in the scientific sense, is not directly important to science in so far that it is not science; but it is important to the scientist in so far that without belief he would not be a scientist. It is the shining star showing the way, it is his guidance and without it nature cannot be investigated scientifically. It is from this that his drive and sense of what is essential comes from, and these faculties are what make up a scientist.

The purpose of these lectures is to arouse your interest in this branch of science which is of such an enormous importance. But let me make a remark, so you will not be disappointed at a later time. The lectures are in general a more or less original summary of the most important contributions made by earlier scientists. However, I have reached a standpoint from which science, as I see it, has to a large extent changed its appearance. I have succeeded in finding methods which transform many problems and solve many others which hitherto remained unsolved. A large part of the scientific works on elasticity rests on hypotheses which have turned out to be either wrong or unnecessary; moreover, they are concerned with issues of no relevance for our true purpose. The purpose is the study of nature, not the study of others' works. Nevertheless, I will not pass over the historical aspects of science. At the end of each lecture I will consistently point out how the lecture relates to earlier works.

You will discover that science is not a completed whole to which nothing can be added and nothing changed; contrary to some other fields of knowledge you may be acquainted with, it is not a Chinese Wall which you cannot surpass. No, there is in fact very much left to do and I would wish that you would start with the intention of proceeding as fast as possible. Indeed, one of my aims with these lectures is to win collaborators in this rich area which is much too complex for an individual.

Appendix C: Translation of Lorenz (1867a)

(The notes are added and not part of Lorenz's paper).

On Light

What is light? To get a closer insight into this natural agency will surely be of interest to everyone with a sense for its grand meaning; and also because it is the most important intermediator between us and the outer world and the only messenger from the distant world globes, indeed for everything living and moving in nature. If the question concerns only the path of light and its swift motion through space and bodies, and how to find the laws for the motion, then science has a perfectly satisfying answer. In this case, where the question can be approached mathematically, it turns out that the phenomena of light are in close agreement with the exact and ideal results derived from mathematical thinking. But if we want to know more about the true nature of light - the very foundation of its actions - we have to admit that we are far from an adequate answer. The reason is especially that we know very little or even nothing of what happens in the deep interior of the bodies which is inaccessible to our senses and yet is the ultimate source of all physical action.

It was once assumed that light consist of tiny particles expelled from luminous bodies, an assumption which still in our century was defended with great skill. However, as more became known about the properties of light one was forced to assume that *light is a motion of waves*, meaning that the action of light consists in periodic motions or *vibrations propagated in forward direction*. Thus, it is not something of a material nature emitted from the luminous body but a motion in propagation. To this picture was associated a *medium* for the motion, for there cannot be empty space where a ray of light passes. This theory must be considered perfectly scientifically justified and for this reason be irrefutable at any time. Moreover, we can determine with great accuracy the enormous speed of the motion and the number of oscillations in any given time; and that notwithstanding that one needs millions of years to count the number of oscillations that a ray of light transmits to the eye in a second. And yet, if we proceed with the analogy between light and other forms of wave motion the results become more dubious. People have imagined that the medium for the motion of light was a special substance which combined extraordinary tension with imperceptible weight; that light was oscillations in this "ether" in analogy to sound consisting of oscillations in the material bodies. But this idea became increasingly doubtful, for light vibrations are different. They do not move in the direction of the light ray, as one might expect, but only perpendicular to it.

In an earlier paper in this journal's first volume[81] I accounted for my view concerning the theory of light. I have shown that one must disregard all physical hypotheses about the nature of light and base its laws solely on facts. Having completed this part of the theory of light (in Pogg. Ann., vol. 121),[82] the next step must

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[81] Lorenz (1862).

[82] Lorenz (1864a).

be to establish a connection between these laws and the laws for other forces. The endeavours to search for connections between the various forces have been a significant reason for the progress of recent science; the idea that the various forces in nature are merely different manifestations of the one and same force has proved itself more fertile than all physical theories. It turned out that only one further step along the already established road had to be made, and this step leads to the remarkable result that *the vibrations of light are electrical currents*.

To prove this one must know how the electrical currents propagate through bodies. In any point the current depends on the electricity of the surrounding parts of bodies, whether it is in motion as an electrical current or is at rest as static electricity. In the latter case it produces by "distribution" a separation of the two kinds of electricity in all the points of the body, and the electrical currents act in the same way if they decline or increase in strength. By distribution and "induction" the two kinds of electricity are separated and an electrical current is generated. After the corresponding laws had been established, Kirchhoff could finally present it all in a mathematical form and from his equations the problem could be solved.[83]

Regarding the plausibility of these equations it should be noticed, on the one hand, that they could be considered the correct expression for experimental results in so far that they match the accuracy of the experiments; on the other hand, they lack the theoretical foundation without which they would not be the exact expression for the law. To understand this, consider a body which in a point A suddenly receives some amount of electricity. As a result, in other points of the body a separation of the two kinds of electricity will be produced by polarization. The corresponding "electromotive" force will in part be proportional to the amount of electricity received in the point A and in part be inversely proportional to the square of the distance from this point. The law is exact in so far that it is simple and in agreement with the laws for other forces acting at a distance.

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[83] Kirchhoff (1857).

But the question is if the time for this [electrical] action is precisely determined. Does the action occur instantaneously in all parts of the body whether they are close to or far from the source point? Or does it take some time for the action to propagate so that it first arrives to the closer parts of the body and only later on to the points more far away? This question is closely connected to another one, namely if these forces really act at a distance or propagate from point to point through the intervening space such as suggested by Faraday. If the latter is the case it must take some time however small for the electrical actions to be transmitted from one place to another. If the speed of propagation is very high, say of the order of that of light, we would be unable to detect it experimentally and yet it does not follow that we can ignore it. As soon as we formulate this either-or question[84] the choice leaves no doubt, for it is a choice between the particular and the general encompassing the particular. In such a situation one must of course choose the general. In other words, one must assume that the electrical action does not propagate instantaneously but *propagates with a certain velocity*; then one can always return to the particular case by assuming the velocity to be infinite or the action at any distance to occur instantaneously. As far as experiments are concerned they merely show that the velocity must be very great but so far undetermined. Experiments can never prove that it is infinite and that the action appears instantaneously. But really, this is the assumption behind the laws that Kirchhoff and others before him have stated for the electrical actions and it can only be justified by a much deeper knowledge of the nature of electricity than we possess at present.

To assume that Kirchhoff's formulae are incomplete and yet approximately correct is the same as regarding them as *the first term in a series expansion*. They seem indeed to have the character of such a series expansion. I have assumed that the electrical actions need a very short time to propagate and tried to generalise the formulae accordingly. In this way I have arrived at different

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[84] This is possibly an allusion to Søren Kierkegaard's "either-or" philosophy as expounded in his *Philosophiske Smuler* (Philosophical Fragments) from 1844.

formulae which are somewhat simpler than Kirchhoff's and, when expanded in a series, coincide with Kirchhoff's in their first term. The next terms in the series include a very small quantity raised in increasing powers and they are insignificant in ordinary experiments with electrical currents; they only become of some importance when they current change significantly in an extremely small time interval. In this modified form the equations show that in poor conductors electricity can exist as periodic currents vibrating in both directions but propagating in none of these directions as they propagate in a direction perpendicular to them. What turns out to be *possible* in calculations deduced from really existing laws and conditions will always turn out to correspond to reality. Where, then, should we look for these periodic currents, propagating more easily the more poorly they are conducted in the body and only in a direction perpendicular to the current, if not in *the ray of light*? After all, the vibrations of light are periodical and perpendicular to the direction of light; moreover, light can only pass through extremely poor conductors.

It turns out that the velocity of propagation in poor conductors is just the same as the velocity of light and that it is possible to determine the velocity of light in air solely from Weber's experiment with an accuracy no less than the one determined in different ways. Moreover, it turns out that the equations for the electrical currents can be transformed in such a way that they, apart from one term, *agree completely with the equations I have previously found for the vibrations of light* and from which the entire theory of light can be deduced.[85] The disagreement due to the mentioned term merely serves to confirm the correctness of the theory since the term becomes significant only for good conductors such as the metals; it shows that they must absorb light in accordance with experience, whereas the term disappears for very poor conductors. From this we infer that even the least degree of transparency indicates that the body is a very poor conductor compared to the metals. On the other hand, the reverse is not necessarily

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[85] The reference is to Lorenz (1864a).

the case, for opaqueness can be due to other causes of which lack of homogeneity is one example.

The theory thus cast a new light on several facts such as the poor electrical conductivity of transparent bodies, the opaqueness of good electrical conductors, and a certain agreement between the velocity of propagation of light and that of electricity; these facts suggest a connection between light and electricity which has long been suspected but an explanation of which has been missing. It would be extremely difficult to demonstrate by means of direct experiments that light vibrations are electrical currents, the reason being that the electrical currents in a ray of light vibrate billions of times per second. And yet there are known facts indicating that vibrations of light might be transformed to electrical currents, namely when light hits upon the interface between two different metals. The interface acts like a kind of electrical valve favouring the propagation in one direction and not the other.[86] Still, the real causes are not revealed by these facts, for they depend on the molecular constitution of the bodies.

Having demonstrated that oscillations of light are electrical currents we could further ask, what is an electrical current? On the assumption that an oscillation of light is the same as oscillating parts of the ether, the electrical current is nothing but a progressive motion of the ether - a real and material current of some sort of liquid. But although this is an often-held opinion it is completely untenable. Sure, the assumption may lead to the correct equations for the oscillations of light, but only by regarding them as infinitesimally small displacements of ethereal parts. As soon as we deal with the finite displacements constituting the electrical currents the equations will appear in quite a different form. The identity of the equations for electrical currents and those of light vibrations demonstrates that we always have to do

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86. Lorenz might have thought of the photovoltaic effect discovered by A. E. Becquerel (1820-1891) in 1839 or possibly of the thermoelectric Seebeck effect named after the German physicist Thomas Seebeck (1770-1831) who discovered or rediscovered it in 1821.

with small relative motions, both when the electrical currents are vibrating back and forth and when they are propagating forwards. Neither in the case of the electrical current nor in that of the ray of light is there anything of a material kind which moves. We are dealing only with propagating *molecular motions*. What these motions are, more exactly, is a question we cannot yet answer with any degree of certainty. I once proposed the hypothesis that the oscillations of light were rotating oscillations of material parts and that the direction of the oscillations is along the axis of rotation.

On this hypothesis one must consider the electrical current to be a continual rotation of the material parts and the direction of the axis of rotation becomes the direction of the current. But other kinds of molecular motion are conceivable and it is unlikely that the problem can be easily solved. Besides, to add to the assumption of molecular motion the assumption of an ether would be unreasonable; because, it is a new non-substantial medium which has been thought of only because light was conceived in the same manner as sound and it hence had to be a medium of exceedingly large elasticity and small density in order to explain the large velocity of light. However, the hypothesis is superfluous if the velocity does not depend on elastic forces but on quite different molecular forces. What we know about the huge power of the molecular forces makes it understandable that they are able to generate motions with such a great velocity of propagation. If we need to assume some medium for light between the celestial globes, we do not have to conceive it as different from the known gases. On the whole it is most unscientific to fabricate a new substance when its existence is not revealed in a much more definite way.

Among the certain results of this investigation is that it takes time for the actions of electricity to propagate from one place to another. What Rømer taught us about light 200 years ago is valid also for the electrical forces. We might well add that it is valid also for other forces such as the gravitational attraction and assume that in general no action can propagate instantaneously and thus be all over in space at the same time. The basic assumption is that the forces or at least the electrical forces propagate successively from one point to another in the bodies. They appear to act at a distance, but in reality they do not reach farther than to the nearest surrounding molecules.

Appendix D: Time-line

1829	Born 18 January in Elsinore (Helsingør)
1835	The Lorenz family moves from Elsinore to Maribo
1842	Lorenz listens to an evening lecture on physics in Maribo
1843	Enters Nykøbing Cathedral School; graduates 1846
1847	Preliminary studies at the University of Copenhagen
1848	Studies at the Polytechnic College in Copenhagen; graduates from the chemical class in 1852
1852-53	Private tutor in the house of Count Adam Gottlob Moltke-Hvitfeldt; stay in Leipzig
1854	University gold medal on essay on Fresnel waves
1854-58	Alumni at Borch's Kollegium
1856	Meeting of Scandinavian Scientists in Christiania, Norway
1857-62	Supported by Smith Stipend
1858	Submits paper on heat to the Royal Danish Academy of Science; the paper is rejected
1858-59	Studies at the University of Paris (Sorbonne)
1860-82	Assistant teacher at Blaagaard Teacher's College
1860	Meeting of Scandinavian Scientists in Copenhagen; presents paper on optical theory
1860	First publications on theoretical optics
1862	Marriage to Agathe Fogtmann, 12 August
1863	Phenomenological theory of light
	Meeting of Scandinavian Scientists in Stockholm

1865	Popular physics book (*Kortfattet Naturlære*)
	Pamphlet on Denmark's political constitution
1866	Teacher at the Royal Military High School
	Member of the Royal Danish Academy of Sciences and Letters
1867	Paper on the electrodynamic theory of light
1867-68	Lectures for the Society for the Dissemination of Natural Science
1868	Maxwell responds critically to Lorenz's electrical theory
1869	Order of Dannebrog
	First paper on the refractivity of transparent bodies (Lorenz-Lorentz law). Danish paper on geophysics
1870	Paper on molecular dimensions and the Loschmidt number
1871	Mathematical paper on number theory
1872	First paper on thermal and electrical conductivity of metals
1873	Member of the Mathematical Society
1876	Titular professor
	Resigns in protest membership of the Mathematical Society
1877	Honorary doctor of philosophy, University of Uppsala
1879	Dispute with F. M. Bing concerning Bayes' theorem
1880	Lorenz-Lorentz law on refractivity, experimental and theoretical
1881	Wiedemann-Franz-Lorenz law; Lorenz number
	Invention of Jürgensen-Lorenz dynamo
1882	Method of absolute determination of the unit of electrical resistance. Paris conference on the ohm unit
1883	Appointed Councilor of State (Etatsråd)
	Theory of chromatic dispersion
1884	Unpublished theory of the propagation of telephone currents. Member of Academia Leopoldina
1885	Improved Lorenz apparatus for electrical resistance measurement

1886	Failed attempt to produce inductively loaded telephone cable
1887	Life-long grant from the Carlsberg Foundation
1890	Theory of scattering of light by spheres (Lorenz-Mie theory)
1891	Last paper, on the theory of prime numbers
	Death on 9 June by heart attack
1898-1904	*Oeuvres Scientifiques de L. Lorenz* (by H. Valentiner)

Bibliography

The following abbreviations are used:

AC	*Annales de Chimie*
AP	*Annalen der Physik und Chemie*
OS	*Oeuvres Scientifiques de L. Lorenz*, 2 vols. (Valentiner, 1898-1904)
PM	*Philosophical Magazine*
RSP	*Rayleigh Scientific Papers* (Rayleigh, 1964)

Abraham, Max (1905). *Theorie der Elektrizität*. Leipzig: B. G. Teubner.

Andrae, Poul (1926). *Andrae and his Invention: The Proportional Representation Method*. Philadelphia: Andræ.

Appel, Jacob (1891). "Ludvig Valentin Lorenz." *Danskeren: Tidsskrift for Land og By* **6**: 363-375.

Archibald, Thomas (1986). "Carl Neumann versus Rudolf Clausius on the propagation of electrodynamic potentials." *American Journal of Physics* **54**: 786-790.

Assis, Andre K. T. (2003). "On the first electromagnetic measurements of the velocity of light by Wilhelm Weber and Rudolf Kohlrausch." In *Volta and the History of Electricity*, eds. F. Bevilacqua and E. Giannetto, pp. 267-286. Milan: Hoepli.

Atherton, W. A. (1984). *From Compass to Computer*. San Fransisco: San Fransisco Press.

Atten, Michel (1993). "L'oeuf ou la poule: Science et technique de l'électricité en France 1850-1900." *Resaux* no. **61**: 113-123.

Barrow-Green, June (1997). *Poincaré and the Three-Body Problem*. Providence, RI: American Mathematical Society.

Bateman, Harry (1915). *The Mathematical Analysis of Electrical and Optical Wave Motion on the Basis of Maxwell's Equations*. Cambridge: Cambridge University Press.

Berendsen, Nathan J. (1918). *Af en Gammel Journalists Erindringer*. Copenhagen: Povl Branner.

Besson, Ugo (2012). "The history of the cooling law: When the search for simplicity can be an obstacle." *Science and Education* **21**: 1085-110.

Bing, Frederik M. (1879). "Om aposteriorisk sandsynlighed." *Tidsskrift for Mathematik* **3**: 1-22.

Bohr, Niels (1972). *Niels Bohr. Collected Works*, Vol. 1, ed. J. Rud Nielsen. Amsterdam: North-Holland.

Bohr, Niels (1984). *Niels Bohr. Collected Works*, Vol. 5, ed. Klaus Stolzenburg. Amsterdam: North-Holland.

Born, Max and Emil Wolf (1970). *Principles of Optics: Electromagnetic Theory of Propagation, Interference and Diffraction of Light*. Oxford: Pergamon Press.

Boyer, Charles B. (1987). *The Rainbow: From Myth to Mathematics*. Princeton: Princeton University Press.

Brittain, J. E. (1970). "The introduction of the loading coil: George A. Campbell and Michael I. Pupin." *Technology and Culture* **11**: 36-57.

Brooks, H. B. (1931). "The unit of electrical resistance: Past history and impending change." *Transactions of the American Institute of Electrical Engineers* **50**: 1318-1324.

Brush, Stephen G. (1986). *The Kind of Motion We Call Heat*. Amsterdam: North-Holland.

Brush, Stephen G. (1996). *Nebulous Earth: The Origin of the Solar System and the Core of the Earth from Laplace to Jeffreys*. Cambridge: Cambridge University Press.

Buchwald, Jed Z. (1985). *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century*. Chicago: University of Chicago Press.

Buchwald, Jed Z. (2013). "Optics in the nineteenth century." In *Oxford Handbook of the History of Physics*, eds. J. Z. Buchwald and R. Fox, pp. 445-472. Oxford: Oxford University Press.

Burchfield, Joe D. (1975). *Lord Kelvin and the Age of the Earth*. Chicago: University of Chicago Press.

Cantor, Geoffrey N. and Michael J. S. Hodge, eds. (1981). *Conceptions of Ether: Studies in the History of Ether Theories 1740-1900*. Cambridge: Cambridge University Press.

Carazza, Bruno and Nadia Robotti (1996). "The first molecular model for an electromagnetic theory of dispersion and some aspects of physics at the end of the nineteenth century." *Annals of Science* **53**: 587-607.

Carøe, Kristian (1904). *Den Danske Lægestand, Supplementsbind*. Copenhagen: Gyldendal.

Chalmers, Alan F. (1973). "Maxwell's methodology and his application of it to electromagnetism." *Studies in History and Philosophy of Science* **4**: 107-164.

Christensen, Dan Ch. (2013). *Hans Christian Ørsted: Reading Nature's Mind*. Oxford: Oxford University Press.

Christiansen, Christian (1887-1889). *Indledning til den Mathematiske Fysik*, 2 vols. Copenhagen: J. H. Schulz.

Christiansen, Christian (1891a). "Ludvig Valentin Lorenz." *Illustreret Tidende* **32** (21 June): 1-2.

Christiansen, Christian (1891b). "Om betingelserne for isdannelse." *Kgl. Da. Videnskabernes Selskab, Oversigt*: 44-53.

Christiansen, Christian (1896). "Lorenz, Ludvig Valentin." *Brickas Dansk Biografisk Lexicon* **10**: 376-381.

Christiansen, Christian (1897). *Elements of Theoretical Physics*, trans. W. F. Magie. London: Macmillan & Co.

Christiansen, Christian (1915). *Lærebog i Fysik*, 4th edition revised by M. Knudsen. Copenhagen: Gyldendal.

Christoffel, Elwin B. (1862). "Ueber die Dispersion des Lichtes." *Annalen der Physik und Chemie* **117**: 27-45.

Clausius, Rudolf (1868). "Ueber die von Gauss angeregte neue Auffassung der elektrodynamischen Erscheinungen." *Annalen der Physik und Chemie* **135**: 606-621.

Clebsch, Rudolf F. A. (1863). "Über die Reflexion an einer Kugelfläche." *Journal für die Reine und Angewandte Mathematik* **61**: 195-262.

Cochery, Adolphe, ed. (1882). *Conférence Internationale pour la Détermination des Unités Électriques*. Paris: Imprimerie Nationale.

Cochery, Adolphe, ed. (1884). *Conférence Internationale pour la Détermination des Unités Électriques: Deuxième Session*. Paris: Imprimerie Nationale.

Crone, C. (1923). *Matematisk Forening Gennem 50 Aar*. Copenhagen: J. Gjellerup.

Dahl, Per F. (1972). *Ludvig Colding and the Conservation of the Energy Principle*. New York: Johnson Reprint Corporation.

Dale, Andrew I. (1999). *A History of Inverse Probability: From Thomas Bayes to Karl Pearson*. New York: Springer.

Darrigol, Olivier (2000). *Electrodynamics from Ampère to Einstein*. Oxford: Oxford University Press.

Darrigol, Olivier (2005). *Worlds of Flow: A History of Hydrodynamics from the Bernoullis to Prandtl*. Oxford: Oxford University Press.

Darrigol, Olivier (2010). "James MacCullagh's ether: An optical route to Maxwell's equations?" *European Physical Journal* **35**: 133-172.

Darrigol, Olivier (2012). *A History of Optics: From Greek Antiquity to the Nineteenth Century*. Oxford: Oxford University Press.

Davis, Edward A. (2017). "Lord Rayleigh's determination of the ohm." In *Precise Dimensions: A History of Units from 1791-2018*, eds. Malcolm C. Cooper and Jim G. Grozier, pp. 3.1-3.15. London: IOP Publishing.

Debye, Peter (1909). "Der Lichtdruck auf Kugeln von beliebigem Material." *Annalen der Physik* **30**: 57-136.

Dickson, Leonard E. (1923). *History of the Theory of Numbers*, Vol. 3. Washington D.C.: Carnegie Institution.

Domar, Yngve (1982). "On the foundation of Acta Mathematica." *Acta Mathematica* **148**: 3-8.

Dredge, James (1882). *Electric Illumination*, Vol. 1. London: Engineering.

Drude, Paul (1894). *Physik des Aethers auf Elektromagnetischer Grundlage*. Stuttgart: F. Enke.

Duhem, Pierre (2015). *The Electric Theories of J. Clerk Maxwell: A Historical and Critical Study*, trans. A. Aversa. Heidelberg: Springer.

Dupré, Athanase (1866). "Mémoire sur la théorie mécanique de la chaleur." *Annales de Chimie et de Physique* **7**: 236-282.

Dyssel, J. Arndt (1833). *Forelæsninger over den Tekniske Mechanik*. Copenhagen: Den Militære Højskole.

Eckert, E. R. G. (1981). "Pioneering contributions to our knowledge of convective heat transfer." *Journal of Heat Transfer* **103**: 409-414.

Eckert, Michael et al. (1992). "The roots of solid-state physics before quantum mechanics." In *Out of the Crystal Maze: Chapters from the History of Solid-State Physics*, eds. Lillian Hoddeson et al., pp. 3-87. New York: Oxford University Press.

Edlund, Erik (1872). "On the nature of electricity." *Philosophical Magazine* **44**: 81-100, 174-188.

Edsberg, V. (1884). *Notice sur des Expériences Exécutées avec la Machine Dynamo-Électrique et avec la Lampe á Arc de M. Jürgensen*. Copenhagen: F. Bagge.

Egedal, J. et al. (1945). *Geofysiken i Danmark*. Copenhagen: Munksgaard.

Eriksson, Nils (1991). *"I Andens Kraft, på Sannings Stråt...": De Skandinaviska Naturforskarmötene*. Gothenburg: Acta Universitatis Gothenburgensis.

Exner, Franz (1885). "Über eine neue Methode zur Bestimmung der Grösse der Moleküle." *Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien* **91**: 850-880.

Filskov, Niels (1909). "Et resumé af den elektromagnetiske lysteoris historie." *Fysisk Tidsskrift* **8**: 182-187.

Fisher, Arne (1922). *The Mathematical Theory of Probabilities*. New York: Macmillan.

Fitzgerel, Robert K. and Frank H. Verhoek (1960). "The law of Dulong and Petit." *Journal of Chemical Education* **37**: 545-549.

Fogh, Carl (1855). "Om den indre jordvarme." *Tidsskrift for Populære Fremstillinger af Naturvidenskaben* **1**: 32-52.

Giulani, Giuseppe (2010). "Vector potential, electromagnetic induction and 'physical meaning'." *European Journal of Physics* **31**: 871-880.

Gladstone, John H. and Thomas Dale (1864). "Researches on the refraction, dispersion and sensitiveness of liquids." *Philosophical Transactions of the Royal Society* **153**: 317-343.

Glazebrook, Richard T. (1885). "Report on optical theories." *British Association for the Advancement of Science, Report*: 157-261.

Glamann, Kristof (2003). *The Carlsberg Foundation: The Early Years*. Copenhagen: Carlsberg Foundation.

Goenner, Hubert (2017). “Some remarks on ‘A Contribution to Electrodynamics’ by Bernhard Riemann.” In *From Riemann to Differential Geometry*, eds. L. Ji, A. Papadopoulos and S. Yamada, pp. 111-123. Berlin: Springer.

Gouesbet, Gérard (2012). “From theories by Lorenz and Mie to ontological underdetermination of theories by experiments.” In *The Mie Theory*, eds. W. Hergert and T. Wriedt, pp. 73-100. Berlin: Springer-Verlag.

Hald, Anders (2005). ”Nogle danske statistikeres liv og deres værker.” *Matematisk-Fysiske Meddelelser* **51** (Copenhagen: Royal Danish Academy of Sciences and Letters).

Hansen, Hans C. (1985). *Poul la Cour: Grundtvigianer, Opfinder og Folkeoplyser*. Askov: Askov Højskoles Forlag.

Hansen, H. C. Peter (1886). *Illustreret Dansk Litteraturhistorie*. Copenhagen: P. G. Philipsens Forlag.

Hansen, Hans M. (1932). ”Ludvig Valentin Lorenz.” In *Prominent Danish Scientists Through the Ages*, ed. Vilhelm Meisen, pp. 148-151. Copenhagen: Levin & Munksgaard.

Harding, Marius C. (1924). *Selskabet for Naturlærens Udbredelse*. Copenhagen: Gjellerups Forlag.

Harman, Peter M. (1982). *Energy, Force, and Matter: The Conceptual Development of Nineteenth-Century Physics*. Cambridge: Cambridge University Press.

Harman, Peter M., ed. (1995). *The Scientific Letters and Papers of James Clerk Maxwell*, Vol. 2. Cambridge: Cambridge University Press.

Hartman, L. W. (1916). “The heat losses from incandescent filaments in air.” *Physical Review* **7**: 431-441.

Hawthorne, Robert M. (1970). “Avogadro’s number: Early values by Loschmidt and others.” *Journal of Chemical Education* **47**: 751-755.

Heap, David P. (1884). *Report on the International Exposition of Electricity in Paris*. Washington, D.C.: Government Printing Office.

Heaviside, Oliver (1912). *Electromagnetic Theory*, Vol. 3. London: The Electrician.

Helmholtz, Hermann von (1870). “Über die Bewegungsgleichungen der Elektricität für ruhende leitende Körper.” *Journal für die Reine und Angewandte Mathematik* **72**: 57-129.

Hergert, Wolfram (2012). “Gustav Mie: From electromagnetic scattering to an electromagnetic view of matter.” In *The Mie Theory*, eds. W. Hergert and T. Wriedt, pp. 1-51. Berlin: Springer-Verlag.

Hertz, Heinrich (1884). “Über die Beziehungen zwischen den Maxwell'schen elektrodynamischen Grundgleichungen und den Grundgleichungen der gegnerischen Elektrodynamik.“ *Annalen der Physik und Chemie* **23**: 84-103.

Hertz, Heinrich (1887). “Über sehr schnelle elektrische Schwingungen.“ *Annalen der Physik und Chemie* **31**: 421-448.

Hertz, Heinrich (1999). *Die Constitution der Materie: Eine Vorlesung über die Grundlagen der Physik aus dem Jahre 1884*, ed. A. Fölsing. Berlin: Springer.

Hirosige, Tetu (1969). "Origins of Lorentz's theory of electrons and the concept of the electromagnetic field." *Historical Studies in the Physical Sciences* **1**: 151-209.

Hirschhorn, Michael D. (2001). "Three classical results on representations of a number." In *The Andrews Festschrift: Seventeen Papers on Classical Number Theory and Combinatorics*, eds. D. Foata and G. N. Han, pp. 159-165. Berlin: Springer.

Hirschhorn, Michael D. (2017). *The Power of q: A Personal Journey*. Cham, Switzerland: Springer.

Hoppe, Edmund (1926). *Geschichte der Physik*. Braunschweig: Vieweg & Sohn.

Horvath, Helmuth (2009). "Gustav Mie and the scattering and absorption of light by particles: Historic developments and basics." *Journal of Quantitative Spectroscopy & Radiative Transfer* **110**: 787-799.

Hunt, Bruce J. (1991). *The Maxwellians*. Ithaca, NY: Cornell University Press.

Jackson, J. David (2008). "Examples of the zeroth theorem of the history of science." *American Journal of Physics* **76**: 704-719.

Jackson, J. David and Lev B. Okun (2001). "Historical roots of gauge invariance." *Reviews of Modern Physics* **73**: 663-680.

Jensen, Johan L. V. (1898). "Lovene for vexelstrømmes bevægelse i lange ledninger." In *Förhandlingar vid Nordiska Teknikermötet i Stockholm 1897*, pp. 116-118. Stockholm.

Jespersen, Knud (2004). *A History of Denmark*. Houndmills, Hampshire: Palgrave.

Jordan, D. W. (1982). "The adoption of self-induction by telephony, 1886-1889." *Annals of Science* **39**: 433-461.

Jungnickel, Christa and Russell McCormmach (1986). *Intellectual Mastery of Nature: The Torch of Mathematics 1800-1870*. Chicago: University of Chicago Press.

Ke, Quing et al. (2015). "Defining and identifying sleeping beauties in science." *Proceedings of the National Academy of Sciences* **112**: 7426-7431.

Kacac, S., Y. Yener and A. Pramuanjaroenkij (2014). *Convective Heat Transfer*. London: CRC Press.

Kaiser, Walter (1978). "Die zeitliche Ausbreitung von Potentialen in der Elektrodynamik." *Gesnerus* **35**: 297-317.

Kaiser, Walter (1981). *Theorien der Elektrodynamik im 19. Jahrhundert*. Hildesheim: Gerstenberg Verlag.

Kaiser, Walter (1993). "Helmholtz's instrumental role in the formation of classical electrodynamics." In *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*, ed. David Cahan, pp. 374-402. Berkeley: University of California Press.

Keller, Ole (2002). "Optical works of L. V. Lorenz." In *Progress in Optics* **43**, ed. Emil Wolf, pp. 195-294. Amsterdam: Elsevier Science.

Kerker, Milton (1969). *The Scattering of Light and Other Electromagnetic Radiation*. New York: Academic Press.

Kirchhoff, Gustav R. (1857). "Über die Bewegung der Elektricität in Drähten." *Annalen der Physik und Chemie* **100**: 193-217.

Kirchhoff, Gustav R. (1891). *Vorlesungen über Electricität und Magnetismus*, ed. Max Planck. Leipzig: B. G. Teubner.

Kirchhoff, Gustav R. and Gustav Hannemann (1881). "Über die Leitungsfähigkeit der Metalle für Wärme und Elektricität." *Annalen der Physik und Chemie* **13**: 406-422.

Kirchner, Ferdinand (1904). "Über die optischen Eigenschaften entwickelter Lippmannscher Emulsionen." *Annalen der Physik* **13**: 239-270.

Knudsen, Ole (1987). "The influence of Gibbs's European studies on his later work." In *From Ancient Omens to Statistical Mechanics*, eds. J. L. Berggren and B. R. Goldstein, pp. 271-280. Copenhagen: University Library.

Kragh, Helge (1989). "The aether in late nineteenth century chemistry." *Ambix* **36**: 49-65.

Kragh, Helge (1990). "1990-91: Ludvig Lorenz centennial years." *Uppsala Newsletter, History of Science* no. **14**: 4-5.

Kragh, Helge (1991). "Ludvig Lorenz and nineteenth century optical theory: The work of a great Danish scientist." *Applied Optics* **30**: 4688-4695.

Kragh, Helge (1992). "Ludvig Lorenz and the early theory of long-distance telephony." *Centaurus* **35**: 305-324.

Kragh, Helge (1994). "The Krarup cable: Invention and early development." *Technology and Culture* **35**: 129-157.

Kragh, Helge (2009). "Styles of science and engineering: The case of early long-distance telephony." *Centaurus* **51**: 175-188.

Kragh, Helge (2012). "Zöllner's universe." *Physics in Perspective* **14**: 392-420.

Kragh, Helge (2016). *Julius Thomsen: A Life in Chemistry and Beyond*. Copenhagen: The Royal Danish Academy of Sciences and Letters.

Kragh, Helge et al. (2008). *Science in Denmark: A Thousand-Year History*. Aarhus: Aarhus University Press.

Kroman, Kristian (1908). "Den aposterioriske sandsynlighed." *Kgl. Da. Videnskabernes Selskab, Oversigt*: 133-166.

Kynast, Reinhard (1907). "Über die bei der Reflexion an isotropen, durchsichtigen Medien auftretenden Phasenverschiebungen." *Annalen der Physik* **327**: 726-748.

Lamé, Gabriel (1852). *Leçons sur la Théorie Mathématique de l'Élasticité des Corps Solides*. Paris: Gauthiers-Villar.

Lang, Viktor von (1891). *Einleitung in die Theoretische Physik*. Braunschweig: Vieweg und Sohn.

Larmor, Joseph, ed. (1902). *The Scientific Writings of the Late George Francis FitzGerald*. London: Longmans, Green & Co.

Larsen, Absalon (1915). "Den danske fysiker Ludvig Lorenz og vore elektriske enheder." *Fysisk Tidsskrift* **13**: 129-148.

Leppäranta, Matti (1993). "A review of analytical models of sea-ice growth." *Atmosphere-Ocean* **31**: 123-138.

Logan, Nelson A. (1962). "Early history of the Mie solution." *Journal of the Optical Society of America* **52**: 342.

Logan, Nelson A. (1965). "Survey of some early studies of the scattering of plane waves by a sphere." *Proceedings IEEE* **53**: 773-785.

Lomholt, Asger (1942-1973). *Det Kongelige Danske Videnskabernes Selskab 1742-1942*, vols. 1-5. Copenhagen: Munksgaard.

Lorentz, Hendrik A. (1880). "Ueber die Beziehung zwischen der Fortplanzungsgeschwindigkeit des Lichtes und der Körperdichte." *Annalen der Physik und Chemie* **9**: 641-665.

Lorentz, Hendrik A. (1909). *The Theory of Electrons*. New York: Dover Publications.

Lorentz, Hendrik A. (1936). *H. A. Lorentz: Collected Papers*, Vol. 2. The Hague: Martinus Nijhoff.

Lorenz, Ludvig V. (1860a). "Om retningen af ætherens svingninger i det polariserede lys." In *Forhandlinger ved de Skandinaviske Naturforskeres Ottende Møde*, pp. 473-487. Copenhagen: Gyldendal. [AP 111 (1860): 315-328; PM 21 (1861): 321-331; AC 41 (1861): 227-232; OS1: 3-18]

Lorenz, Ludvig V. (1860b). "Om lysets tilbagekastning fra plane, gjennemsigtige, isotrope legemer." *Tidsskrift for Mathematik* **2**: 116-129. [AP 111 (1860): 460-473; PM 21 (1861):480-491; OS1: 29-46]

Lorenz, Ludvig V. (1861a). "Mémoire sur la théorie de l'élasticité des corps homogène a élasticité constante." *Journal für die Reine und Angewandte Mathematik* **58**: 329-351. [OS2: 3-36]

Lorenz, Ludvig V. (1861b). "Bestimmung der Schwingungsrichtung des Lichtäthers durch die Reflexion und Brechung des Lichtes." *Annalen der Physik und Chemie* **114**: 238-250.

Lorenz, Ludvig V. (1861c). "Nogle bemærkninger og kritiske anmærkninger." *Mathematisk Tidsskrift* **3**: 161-169.

Lorenz, Ludvig V. (1862). "Om lysets theori." *Tidsskrift for Physik og Chemi* **1**: 193-203.

Lorenz, Ludvig V. (1863). "Die Theorie des Lichtes, I." *Annalen der Physik und Chemie* **118**: 111-145. [PM 26 (1863): 81-93, 205-219; OS1: 87-126]

Lorenz, Ludvig V. (1864a). "Die Theorie des Lichtes, II." *Annalen der Physik und Chemie* **121**: 579-600. [PM 28 (1864): 409-425; OS1: 139-165]

Lorenz, Ludvig V. (1864b). *Elementær Arithmetik*. Copenhagen: Gad.

Lorenz, Ludvig V. (1864c). *Elementær Algebra*. Copenhagen: Gad.

Lorenz, Ludvig V. (1865a). "Om lysets theori." In *De Skandinaviska Naturforskarnes Nionde Möte i Stockholm i 1863*, pp. 225-240. Stockholm.

Lorenz, Ludvig V. (1865b). *Vor Indre Kamp*. Copenhagen: N. P.

Lorenz, Ludvig V. (1865c). *Kortfattet Naturlære*. Copenhagen: Gad.

Lorenz, Ludvig V. (1866). "Om den permanente bevægelse af vædsker." *Tidsskrift for Mathematik* **2**: 65-74. [OS2: 367-381]

Lorenz, Ludvig V. (1867a). "Om lyset." *Tidsskrift for Physik og Chemi* **6**: 1-9.

Lorenz, Ludvig V. (1867b). "Om identiteten af lyssvingninger og elektriske strømme." *Kgl. Da. Videnskabernes Selskab, Oversigt*: 26-48. [AP 131 (1867): 243-263; PM 34 (1867): 287-301; OS1: 173-196]

Lorenz, Ludvig V. (1869a). "Om hævninger og sænkninger." *Tidsskrift for Mathematik* **5**: 149-154.

Lorenz, Ludvig V. (1869b). "Eksperimentale og theoretiske undersøgelser af legemernes brydningsforhold, I." *Kgl. Da. Videnskabernes Selskab, Skrifter* **8**: 205-248. [OS2: 211-286; AP 11 (1880): 70-103]

Lorenz, Ludvig V. (1870a). "Om antallet af de i et milligram vand indeholdte moleculer." *Kgl. Da. Videnskabernes Selskab, Oversigt*: 40-43. [AP 160 (1870): 644-647; PM 40 (1870): 390-392; OS2: 49-53]

Lorenz, Ludvig V. (1870b). *Forelæsninger over Naturlære for Officersskolens Næstældste Klasse*. Copenhagen: C. A. Reitzel.

Lorenz, Ludvig V. (1871). "Bidrag til tallenes theori." *Tidsskrift for Mathematik* **1**: 97-114. [OS2: 400-429]

Lorenz, Ludvig V. (1872a). "Bestemmelse af varmegrader i absolut maal." *Kgl. Da. Videnskabernes Selskab, Oversigt*: 1-21. [AP 162 (1872): 429-452; PM 46 (1872): 62-77; OS2: 57-83]

Lorenz, Ludvig V. (1872b). "Udjævning af iagttagelsesfejl." *Tidsskrift for Mathematik* **2**: 1-20, 125-135.

Lorenz, Ludvig V. (1873). "Kviksølvets elektriske ledningsmodstand i absolut maal." *Kgl. Da. Videnskabernes Selskab, Oversigt*: 67-84. [AP 149 (1873): 251-269; OS2: 85-108]

Lorenz, Ludvig V. (1874). "To sætninger fra læren om vædskernes sammenhængskraft." *Tidsskrift for Mathematik* **4**: 1-2.

Lorenz, Ludvig V. (1875a). "Eksperimentale og theoretiske undersøgelser af legemernes brydningsforhold, II." *Kgl. Da. Videnskabernes Selskab, Skrifter* **10**: 483-518. [OS1: 301-356; AP 11 (1880): 70-103]

Lorenz, Ludvig V. (1875b). "Kinetiske grundligninger for et system af punkter." *Tidsskrift for Mathematik* **5**: 81-86 [OS2: 483-492].

Lorenz, Ludvig V. (1876). *Læren om Lyset: Forelæsninger for Officersskolens Ældste Klasse*. Copenhagen: C. A. Reitzel.

Lorenz, Ludvig V. (1877). *Læren om Varmen: Forelæsninger for Officersskolens Ældste Klasse*. Copenhagen: C. A. Reitzel.

Lorenz, Ludvig V. (1878). "Om primtalrækken." *Tidsskrift for Mathematik* **2**: 1-3. [OS2: 521-530]

Lorenz, Ludvig V. (1879a). "Om elektricitetens forplantning." *Kgl. Da. Videnskabernes Selskab, Oversigt*: 41-72. [AP 7 (1879): 161-193; OS2: 185-224]

Lorenz, Ludvig V. (1879b). "Bemærkninger til Hr. Bings afhandling 'Om Aposteorisk Sandsynlighed'." *Tidsskrift for Mathematik* **3**: 57-66.

Lorenz, Ludvig V. (1880). "Ueber die Refractionsconstante." *Annalen der Physik und Chemie* **11**: 70-103.

Lorenz, Ludvig V. (1881). "Om metallernes ledningsevne for varme og elektricitet." *Kgl. Da. Videnskabernes Selskab, Skrifter* **2**: 37-76. [AP 13 (1881): 422-447, 582-606; OS2: 243-307]

Lorenz, Ludvig V. (1882). "Sur les méthodes à employer pour la détermination de l'ohm." *Journal de Physique* **1**: 477-483. [OS2: 120-127]

Lorenz, Ludvig V. (1883). "Farvespredningens teori." *Kgl. Da. Videnskabernes Selskab, Skrifter* **6**: 165-182. [AP 20 (1883): 1-21; OS1: 371-396]

Lorenz, Ludvig V. (1885). "Bestemmelse af kviksølvsøjlers elektriske ledningsmodstande i absolut elektromagnetisk maal." *Kgl. Da. Videnskabernes Selskab, Skrifter* **2**: 310-333. [AP 25 (1885): 1-31; OS2: 131-172]

Lorenz, Ludvig V. (1890a). "Valgkredssystemet og minoriteten." *Nyt Tidsskrift for Matematik* **1** (B): 40-47.

Lorenz, Ludvig V. (1890b). "Lysbevægelsen i og uden for en af plane lysbølger belyst kugle." *Kgl. Da. Videnskabernes Selskab, Skrifter* **6**: 1-62. [OS1: 403-502]

Lorenz, Ludvig V. (1891). "Analytiske undersøgelser over primtalsmængderne." *Kgl. Da. Videnskabernes Selskab, Skrifter* **6**: 427-450 [OS2: 531-569].

Macdonald, Hector M. (1902). *Electric Waves*. Cambridge: Cambridge University Press.

Madsen, Søren P. (1922). "Danske bidrag til telefonkablers teori og konstruktion." In *Første Nordiske Elektroteknikermøde i København 1920*, pp. 91-96. Copenhagen: Ivar Jantzen.

Mascart, Éleuthère (1891). *Traité d'Optique*, Vol. 2. Paris: Gauthier-Villars.

Mascart, Éleuthère and Jules Joubert (1882). *Leçons sur l'Électricité et le Magnétisme*, Vol. 1. Paris: G. Masson.

Maxwell, James C. (1868). "On a method of making a direct comparison of electrostatic with electromagnetic force; with a note on the electromagnetic theory of light." *Philosophical Transactions of the Royal Society* **158**: 643-657.

Maxwell, James C. (1883). *Lehrbuch der Electricität und des Magnetismus*, 2 vols., trans. M. B. Weinstein. Berlin: J. Springer.

Maxwell, James C. (1954). *A Treatise on Electricity and Magnetism*, Vol. 2. New York: Dover Publications.

Maxwell, James C. (1965). *The Scientific Papers of James Clerk Maxwell*, ed. W. D. Niven. New York: Dover Publications.

Maxwell Garnett, James C. (1904). "Colours in metal glasses and in metallic films." *Philosophical Transactions of the Royal Society* **203**: 385-420.

McDonald, Kirk T. (2016). "Maxwell's objection to Lorenz's retarded potentials." http://www.physics.princeton.edu/~mcdonald/examples/maxwell.pdf

Meisen, Vilhelm, ed. (1933). *Prominent Danish Scientists Through the Ages*. Copenhagen: Levin & Munksgaard.

Merton, Robert K. (1973). *The Sociology of Science*. Chicago: University of Chicago Press.

Meyer, Kirstine (1938). "Lorenz, Ludvig Valentin." In *Dansk Biografisk Leksikon* **14**, pp. 464-469. Copenhagen: Schultz.

Mie, Gustav (1908). "Beiträge zur Optik trüber Medien, speziell kolloidaler Metallösungen." *Annalen der Physik* **25**: 377-442.

Mondrup, Georg (1943). *Borchs Kollegiums Historie 1825-1912*. Copenhagen: Gad.

Moon, Parry and Domina E. Spencer (1954). "Electromagnetism without magnetism: An historical sketch." *American Journal of Physics* **22**: 120-124.

Morse, Philip (1964). *Thermal Physics*. New York: W. A. Benjamin.

Moss, Kira and Peter Stauning (2012). *Sophus Tromholt*. Copenhagen: Epsilon.

Muir, M. M. Pattison (1884). *A Treatise on the Principles of Chemistry*. Cambridge: Cambridge University Press.

Nahin, Paul J. (1988). *Oliver Heaviside: Sage in Solitude*. New York: IEEE Press.

Nicholson, John W. (1907). "The asymptotic expansion of Bessel functions of high order." *Philosophical Magazine* **14**: 697-707.

Nicholson, John W. (1910). "The asymptotic expansion of Bessel functions." *Philosophical Magazine* **19**: 516-537.

Nielsen, Niels (1910). *Matematiken i Danmark 1801-1908*. Copenhagen: Gyldendal.

Nielsen, Rasmus (1873). *Natur og Aand: Bidrag til en med Physiken Stemmende Naturphilosophie*. Copenhagen: J. H. Schubothe.

Nusselt, Wilhelm (1915). "Das Grundgesetz des Wärmeüberganges." *Gesundheits-Ingenieur* **38**: 477-482, 490-496.

Ørsted, Hans Christian (1852). *The Soul in Nature*. London: Henry G. Bohn.

Ørsted, Hans Christian (1920). *H. C. Ørsted: Naturvidenskabelige Skrifter*, 3 vols., ed. Kirstine Meyer. Copenhagen: Høst & Søn.

Ørsted, Hans Christian (1998). *Selected Scientific Works of Hans Christian Ørsted*, eds. Karen Jelved, Andrew D. Jackson and Ole Knudsen. Princeton: Princeton University Press.

Ogburn, William F. and Dorothy Thomas (1922). "Are inventions inevitable? A note on social evolution." *Political Science Quarterly* **37**: 83-98.

O'Malley, Robert E. (2014). *Historical Developments in Singular Perturbations*. Cham, Switzerland: Springer.

O'Rahilly, Alfred (1965). *Electromagnetic Theory: A Critical Examination of Fundamentals*. New York: Dover Publications.

Osterberg, Max, ed. (1894). *Proceedings of the International Electrical Congress Held in the City of Chicago*. New York: American Institute of Electrical Engineers.

Pais, Abraham (1991). *Niels Bohr's Times, in Physics, Philosophy, and Polity*. Oxford: Oxford University Press.

Partington, James R. (1953). *An Advanced Treatise on Physical Chemistry*, Vol. 4. London: Longmans, Green and Co.

Pedersen, Olaf (1988). "Newton versus Ørsted: The delayed introduction of Newtonian physics into Denmark." In *Newton and the New Direction in Science*, eds. G. V. Coyne, M. Heller and J. Zycinski, pp. 135-153. Vatican State: Specola Vaticana.

Pedersen, Olaf (1992). *Lovers of Learning: A History of the Royal Danish Academy of Sciences and Letters 1742-1992*. Copenhagen: Munksgaard.

Pietsch, Wolfgang (2012). "Hidden underdetermination: A case study in classical electrodynamics." *International Studies in the Philosophy of Science* **26**: 125-151.

Pihl, Mogens (1939). *Der Physiker L. V. Lorenz: Eine Kritische Untersuchung*. Copenhagen: Munksgaard.

Pihl, Mogens (1972a). "The scientific achievements of L. V. Lorenz." *Centaurus* **17**: 83-94.

Pihl, Mogens (1972b). "Lorenz, Ludwig Valentin." In *Dictionary of Scientific Biography* **8**, ed. C. Gillispie, pp. 501-502. New York: Charles Scribner's Sons.

Pihl, Mogens (1980). "Two contributions of L. V. Lorenz to mathematical physics." *Centaurus* **24**: 361-368.

Pihl, Mogens (1983). "Fysik." In *Københavns Universitet 1479-1979*, Vol. 12, ed. M. Pihl, pp. 365-426. Copenhagen: Gads Forlag.

Planck, Max (1928). "Hendrik Antoon Lorentz." *Naturwissenschaften* **16**: 549-555.

Pledge, Humphrey (1939). *Science Since 1500: A Short History of Mathematics, Physics, Chemistry, Biology*. London: His Majesty's Stationary Office.

Preece, William (1887). "On copper wire." *The Electrician* **19**: 372-374.

Prytz, Peter K. (1880). "Experimentelle Untersuchungen über die Refractionsconstante." *Annalen der Physik und Chemie* **11**: 104-120.

Prytz, Peter K. (1881). "Den Lorenz-Jürgensen'ske dynamoelektriske maskine." *Tidsskrift for Physik og Chemi* **2**: 289-292.

Prytz, Peter K. (1891). "Ludvig Valentin Lorenz." *Tidsskrift for Physik og Chemi* **30**: 286-288.

Quincke, Georg H. (1869). "Ueber die Entfernung in welcher die Molekülarkräfte der Capillarität noch wirksam sind." *Annalen der Physik und Chemie* **137**: 402-414.

Ramus, Christian (1852). *Analytisk Mechanik*. Copenhagen: C. A. Reitzel.

Rayleigh, Lord (1871a). "On the reflection of light from transparent matter." *Philosophical Magazine* **42**: 81-97. [RSP1: 120-134]

Rayleigh, Lord (1871b). "On the light from the sky, its polarization and colour." *Philosophical Magazine* **41**: 107-120, 274-279. [RSP1: 87-95]

Rayleigh, Lord (1882). "Comparison of methods for the determination of resistances in absolute measure." *Philosophical Magazine* **14**: 329-346. [RSP2: 134-150]

Rayleigh, Lord (1896). "The electrical resistance of alloys." *Nature* **54**: 154-155. [RSP4: 232-234]

Rayleigh, Lord (1899). "On the transmission of light through an atmosphere containing small particles in suspension, and on the origin of the blue of the sky." *Philosophical Magazine* **47**: 375-384. [RSP4: 397-405]

Rayleigh, Lord (1918). "On the scattering of light by a cloud of similar small particles of any shape and oriented at random." *Philosophical Magazine* **35**: 373-381. [RSP6: 540-546]

Rayleigh, Lord (1964). *Scientific Papers*, 6 vols. New York: Dover Corporation.

Riemann, Bernhard (1867). "Ein Beitrag zur Elektrodynamik." *Annalen der Physik und Chemie* **131**: 239-242.

Riemann, Bernhard (1875). *Schwere, Electricität und Magnetismus*, ed. Karl Hattendorff. Hannover: Carl Rümpler.

Riis Larsen, Børge (1991). *Naturvidenskab og Dannelse: Studier i Fysik- og Kemiundervisningens Historie i den Højere Skole indtil Midten af 1800-Tallet*. Copenhagen: Dansk Selskab for Historisk Kemi.

Roche, John J. (1990). "A critical study of the vector potential." In *Physicists Look Back: Studies in the History of Physics*, ed. J. J. Roche, pp. 144-168. Bristol: Adam Hilger.

Rosenfeld, Léon (1979). "The velocity of light and the evolution of electrodynamics." In *Selected Papers of Léon Rosenfeld*, eds. Robert S. Cohen and John J. Stachel, pp. 134-177. Dordrecht: Reidel.

Rosseaux, Germain (2005). "Lorenz or Coulomb in Galilean electromagnetism?" *Europhysics Letters* **71**: 15-20.

Rowlinson, John S. (2002). *Cohesion: A Scientific History of Intermolecular Forces*. Cambridge: Cambridge University Press.

Rücker, Arthur W. (1888). "On the range of molecular forces." *Journal of the Chemical Society, Transactions* **53**: 222-262.

Schaffer, Simon (1994). "Rayleigh and the establishment of electrical standards." *European Journal of Physics* **15**: 277-285.

Schellen, H. (1884). *Magneto-Electric and Dynamo-Electric Machines*, Vol. 1. New York: Van Nostrand.

Schlote, Karl-Heinz (2006). "Carl Neumann's contributions to electrodynamics." *Physics in Perspective* **6**: 252-270.

Schmidt, Ernst and Wilhelm Beckmann (1930). "Das Temperatur- und Geschwindigkeitsfeld vor einer Wärme abgebenden senkrechten Platte bei natürlicher Konvektion." *Technische Mechanik und Thermodynamik* **1**: 341-349, 391-406.

Schuster, Arthur (1909). *An Introduction to the Theory of Optics*. London: Edward Arnold.

Sidgwick, Eleanor M. and Lord Rayleigh (1883). "Experiments, by the method of Lorenz, for the further determination of the absolute value of the British Association unit of resistance." *Philosophical Transactions A* **174**: 295-322.

Sihvola, Ari (1991). "Lorenz-Lorentz or Lorentz-Lorenz?" *IEEE Antennas and Propagation Magazine* **33**: 21.

Smiles, Samuel (1910). *The Relations between Chemical Constitution and some Physical Properties*. London: Longmans, Green and Co.

Smith, F. E. (1914). "Absolute measurements of a resistance by a method based on that of Lorenz." *Philosophical Transactions A* **214**: 27-108.

Smith, Sigurd (1912). *Søren Hjorth, Inventor of the Dynamo-Electric Principle*. Copenhagen: Elektroteknisk Forening.

Sommerfeld, Arnold (1927). "Zur Elektronentheorie der Metalle." *Naturwissenschaften* **15**: 825-832.

Steen, Adolph (1847). *Mathematiken som Læregjenstand ved Høiskoler*. Copenhagen: N.p.

Steen, Adolph (1873). "Det mathematiske studiums fremgang i Danmark i dette hundredaar." *Tidsskrift for Mathematik* **3**: 161-174.

Steen, Adolph (1879). *Den Polytekniske Læreanstalts Første Halvhundrede Aar*. Copenhagen: B. Luno.

Stefan, Josef (1891). "Ueber die Theorie der Eisbildung, insbesondere über die Eisbildung im Polarmeere." *Annalen der Physik und Chemie* **42**: 269-286.

Stigler, Stephen M. (1980). "Stigler's law of eponymy." In *Science and Social Structure*, ed. T. F. Gieryn, pp. 147-158. New York: New York Academy of Sciences.

Stokes, George G. (1883). *Mathematical and Physical Papers*, Vol. 2. Cambridge: Cambridge University Press.

Tani, Itiro (1977). "History of boundary-layer theory." *Annual Review of Fluid Mechanics* **9**: 87-111.

Thiele, Thorvald N. (1890). "Om kredsvalgproblemet." *Nyt Tidsskrift for Matematik* **1** (B): 56-63.

Thomson, Joseph J. (1885). "Report on electrical theories." *British Association for the Advancement of Science, Report*: 97-155.

Thomson, William (1904). *Baltimore Lectures on Molecular Dynamics and the Wave Theory of Light*. London: C. J. Clay and Sons.

Todhunter, Isaac (1886-1893). *The Theory of Elasticity and of the Strength of Materials*, 2 vols. Cambridge: Cambridge University Press.

Tritt, Terry M., ed. (2004). *Thermal Conductivity: Theory, Properties and Applications*. New York: Kluwer Academic.

Trowbridge, John and W. C. Sabine (1890). "Electrical oscillations in air." *Proceedings of the American Academy of Arts and Sciences* **25**: 109-123.

Tucker, D. G. (1978). "François Van Rysselberghe: Pioneer of long-distance telephony." *Technology and Culture* **19**: 650-674.

[Uppsala] (1879). *Upsala Universitets Fyrahundraårs Jubelfest*. Stockholm: Kongl. Bocktryckeriet.

Valentiner, Herman (1896). "Remarques sur les mémoires ... de L. Lorenz." *Kgl. Da. Videnskabernes Selskab, Oversigt*: 440-445.

Valentiner, Herman (1897). "Om grundlaget for Maxwell's og Lorenz's elektromagnetiske lysteorier." *Nyt Tidsskrift for Fysik og Kemi* **2**: 14-21.

Valentiner, Herman, ed. (1898-1904). *Oeuvres Scientifiques de L. Lorenz*, vols. 1-2. Copenhagen: Lehman & Stage.

Van Dyke, Milton (1994). "Nineteenth-century roots of the boundary-layer idea." *SIAM Review* **36**: 415-424.

Varouchas, Jean (1998). "Démonstration élementaire d'une identité de Lorenz." *Ramanujan Journal* **2**: 495-498.

Voigt, Julius (1890). *Den Polytekniske Læreanstalts Kandidater*. Copenhagen: J. H. Schultz.

Volkmann, Paul (1891). *Vorlesungen über die Theorie des Lichtes*. Leipzig: B. G. Teubner.

Wait, James R. (1962). "Electromagnetic scattering from a radially inhomogeneous sphere." *Applied Scientific Research*, B, **10**: 441-450.

Wangerin, Albert (1909). "Optik. Ältere Theorie." In *Enyclopädie der Mathematischen Wissenschaften*, Vol. V.3, ed. A. Sommerfeld, pp. 1-94. Leipzig: B. G. Teubner.

Wasserman, Neil H. (1985). *From Invention to Innovation: Long-Distance Telephone Transmission at the Turn of the Century*. Baltimore: Johns Hopkins University Press.

Webster, A. Gordon and Samuel J. Plimpton (1947). *Partial Differential Equations of Mathematical Physics*. New York: Hafner Publishing Company.

Whitaker, John K. (1982). "A neglected classic in the theory of distribution." *Journal of Political Economy* **90**: 333-355.

Whittaker, Edmund (1915-1921). "On some disputed questions of probability." *Transactions of the Faculty of Actuaries* **8**: 163-206.

Whittaker, Edmund (1958). *A History of the Theories of Aether and Electricity*, Vol. 1. London: Thomas Nelson.

Wiedemann, Gustav and Rudolph Franz (1853). "Über die Wärme-Leitungsfähigkeit der Metalle." *Annalen der Physik und Chemie* **89**: 497-531.

Wiederkehr, Karl H. (2010). "Über Vorstellungen von der elektrischen Leitung einer Elektronentheorie der Metalle und der Beginn einer Festkörperphysik." *Sudhoffs Archiv* **94**: 57-72.

Wiell, Stine (1999). "The congress of anthropology and archaeology in Copenhagen in 1869 - behind the stage." *Antiquity* **73**: 136-142.

Wien, Wilhelm (1909). "Elektromagnetische Lichttheorie." In *Enyclopädie der Mathematischen Wissenschaften*, Vol. V.3, ed. A. Sommerfeld, pp. 95-198. Leipzig: B. G. Teubner.

Williams, L. Pearce (1991). "The life of science and scientific lives." *Physis* **28**: 199-213.

Wise, M. Norton (1981). "German concepts of force, energy, and the electromagnetic ether: 1845-1880." In *Conceptions of Ether: Studies in the History of Ether Theories 1740-1900*, eds. G. Cantor and M. Hodge, pp. 269-308. Cambridge: Cambridge University Press.

Wriedt, Thomas (2012). "Mie theory: A review." In *The Mie Theory*, eds. W. Hergert and T. Wriedt, pp. 53-71. Berlin: Springer-Verlag.

Wyatt, Philip J. (1974). "How Lorenz might have viewed the computation of the angular functions occurring in his theory." *Applied Optics* **13**: 2751-2752.

Yavetz, Ido (1995). *From Obscurity to Enigma: The Work of Oliver Heaviside, 1872-1889*. Basel: Birkhäuser Verlag.

Zappffe, C. A. (1969). "Gustavus Hinrichs, precursor of Mendeleev." *Isis* **60**: 461-476.

Zöllner, J. C. Friedrich (1876). "Über die physikalischen Beziehungen zwischen hydrodynamischen und elektrodynamischen Erscheinungen." *Berichten d. Königl. Sächs. Gesellschaft der Wissenschaften zu Leipzig*.

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