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A. J. Kox
Editor

The Scientific Correspondence of H. A. Lorentz

VOLUME I

Springer

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The Scientific Correspondence of H.A. Lorentz, Volume 1

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ISBN: 978-0-387-77939-3 e-ISBN: 978-0-387-77940-9
DOI: 10.1007/978-0-387-77940-9

Library of Congress Control Number: 2008935908

Mathematics Subject Classification (2000): 01A75, 76A02, 76D07, 81V10, 81-03

Printed on acid-free paper

9 8 7 6 5 4 3 2 1

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INTRODUCTION

This volume presents a selection of 434 letters from and to the Dutch physicist and Nobel Prize winner Hendrik Antoon Lorentz (1853–1928), covering the period from 1883 until a few months before his death in February 1928. The sheer size of the available correspondence (approximately 6000 letters from and to Lorentz) preclude a full publication.

The letters included in this volume have been selected according to various criteria, the most important of which is scientific importance. A second criterion has been the availability of letters both from and to Lorentz, so that the reader can follow the exchange between Lorentz and his correspondent. Within such correspondences a few unimportant items, dealing with routine administrative or organizational matters, have been omitted. An exception to the scientific criterion is the exchange of letters between Lorentz and Albert Einstein, Max Planck, Woldemar Voigt, and Wilhelm Wien during World War I: these letters have been included because they shed important light on the disruption of the scientific relations during the war and on the political views of these correspondents as well as of Lorentz. similar reasons the letters exchanged with Einstein and Planck on post-war political issues have been included.

Biographical sketch

Hendrik Antoon Lorentz was born on July 18, 1853 in the Dutch town of Arnhem. He was the son of a relatively well-to-do owner of a nursery. After a brilliant secondary school career, in which he excelled in all subjects, Lorentz started his studies at Leiden University in 1870. In 1875 he defended his doctoral dissertation, prepared under the direction of P.L. Rijke, who at the time was the only professor of physics in Leiden (and who had also supervised Johannes D. Van der Waals's dissertation).

In 1877 it was decided to split Rijke's chair into two chairs, one for experimental and one for mathematical (or theoretical) physics. Rijke would concentrate on experimental physics; for the new chair Van der Waals was the first candidate. After the latter's refusal — he accepted an offer of the newly created University of Amsterdam — Lorentz was asked. The call came as a surprise to Lorentz: he was in the middle of a job application as secondary-school teacher in Leiden. He accepted immediately, thus justifying his earlier decision to refuse a call to Utrecht as professor of mathematics.

Four years after his appointment, in 1881, Lorentz married Aletta Catharina Kaiser, the niece of Frederik Kaiser, a well-known Leiden astronomer. Lorentz had met his future wife a few years earlier, while he was riding a merry-go-round, as was later reported by one of Aletta's brothers. Lorentz must have been a cheerful young man, judging by the words of the same brother, who writes about Lorentz's "cheerfully sparkling, coal-black eyes behind his shiny eye-glasses." Lorentz and his wife had four children, one of which died eleven months after his birth. Of the other ones, two daughters and a son, the elder daughter, Geertruida Luberta also became a physicist. She married the well-known physicist Wander Johannes de Haas.

During the first twenty years of his career Lorentz led a fairly secluded life. He traveled little and concentrated on his research and his heavy teaching-load. As far as we know, the first foreign colleague Lorentz visited was Woldemar Voigt, whom Lorentz visited in 1897 during a vacation trip in Germany. But it was not until the next year, when he was 45 years old, that Lorentz attended his first scientific meeting outside of the Netherlands. It was the 70th *Naturforscherversammlung*, the yearly meeting of the Gesellschaft Deutscher Naturforscher und Ärzte, held in Düsseldorf. He attended the meeting at the invitation of Ludwig Boltzmann, with whom he had already been corresponding for several years. At the meeting Lorentz met many important physicists, such as Max Planck, Wilhelm Wien, and Ludwig Boltzmann. As his daughter recounts, the meeting was a great success and constituted a turning-point in Lorentz's life: from then on he once and for all exchanged the seclusion of his study for a lively interest in the international scientific community. Almost every year he traveled abroad to attend a conference or to give lectures. Special mention in this respect deserve the five Solvay Conferences that he helped organize and chaired.

In 1902 Lorentz and his Amsterdam colleague (and former Leiden assistant) Pieter Zeeman shared the Nobel Prize for Physics for their work in the field of magneto-optics, in particular the discovery and explanation of the Zeeman Effect. It was one of the first of a long series of honors, honorary degrees and honorary memberships that Lorentz would receive.

In spite of several offers from other universities, Lorentz remained connected with Leiden during his whole career. The most tempting of these offers came in 1905, when he received a call to Munich to occupy the chair that had been left by Boltzmann in 1895 and that had been vacant since. But the promise by the University of the establishment of a new physics chair to relieve Lorentz's duties made him decide to stay. Around 1910, however, Lorentz decided to leave Leiden. His still heavy teaching load and his many administrative duties left him too little time for his scientific work. In the summer of 1912 he moved to Haarlem, a small provincial town close to Amsterdam, and became *Curator* of the physics laboratory of Teyler's Foundation. This position also gave him the opportunity to do experimental work, something that he had wished but never been able to do in Leiden. In this work he was assisted by a *Conservator*.

Lorentz did not sever all his ties with Leiden: he remained there, first as extraordinary professor, and after having reached the legal retirement age of 70 years, as "special professor" ("bijzonder hoogleraar") on a chair specifically created for him; until his death he lectured once a week, on Monday mornings, on recent developments in physics.

Lorentz's first choice as his successor in Leiden was Albert Einstein, who declined because he had just accepted a call to the ETH in Zurich. Instead, the then still relatively unknown Austrian Paul Ehrenfest was appointed. Between Lorentz and Ehrenfest a close and complex relationship developed.

In particular after his departure from Leiden, Lorentz's role in the international scientific community became very prominent. The outbreak of the First World War in August 1914 increased the importance of this role. As a member of a neutral country, Lorentz tried to become an intermediary between his colleagues from the countries at war. As becomes clear from the correspondence in this volume, he did his best to limit the damage caused by the disruption of the international scientific contacts. Lorentz continued his efforts after the war, in particular as a member of the Committee on International Intellectual Cooperation of the newly-formed League of Nations.

Until the end of his life, Lorentz remained active, both in science and outside of it. In addition to his work for the League of Nations, he was a member of various Dutch government committees. One of these was the committee that advised the Dutch government on the building of a dike to close off the Zuiderzee, an estuary of the North Sea. As chairman of this committee Lorentz single-handedly created the theoretical framework for the calculations needed to determine the ideal position and height of the dike and its influence on the tides.

In 1925 Lorentz's golden doctorate was celebrated in grand style. A number of prominent scientists and representatives of various governments honored him in speeches; he received an honorary doctorate, an important decoration, and a fund was established in his name to promote activities in the field of theoretical physics. This Lorentz Fund still exists, as well as the Lorentz Medal, a prize that was instituted for outstanding achievements in theoretical physics. In 1927 Max Planck received the first Lorentz Medal out of Lorentz's hands.

Hendrik Antoon Lorentz died on February 4, 1928, at the age of 74, after a brief illness. Official mourning was declared in the Netherlands and the funeral was attended by a great number of people, including Dutch dignitaries and many foreign scientists. As a mark of honor for the deceased, national telegraph service was suspended for several minutes during the funeral.

It is impossible to assess in a few words the importance of Lorentz's work, and perhaps his papers should speak for themselves. But it seems indisputable that he was one of the last great representatives of classical, nineteenth-century science, and that his fundamental ideas, to paraphrase a statement by Albert Einstein, have been absorbed so completely into physics, that it is sometimes difficult to see how bold they were, and how crucial for the development of physics.

What do the letters in this volume add to our knowledge of Lorentz's science and his personality? In the first place they provide evidence of the breadth and depth of his knowledge of physics. The topics on which he corresponds with authority range from the theory of elasticity to hydrodynamics, from electrodynamics to wave mechanics. In several instances his correspondents think they have found a mistake in his work, but in all cases but one it is they who are in error, as is ever so gently pointed out to them. (The one exception is in his work on hydrodynamics; see Letters 73 and 75). In addition, it becomes clear that Lorentz was chronically overburdened with work: his articles for the *Encyklopädie der mathematischen Wissenschaften*, for instance, suffered delays of many years and letters often remained unanswered for months. The correspondence also gives an impression of Lorentz's well-balanced, perhaps even serene personality, and of his warm interest in other people. This side of Lorentz shows itself not only in his very personal letters to his German colleagues at the beginning of the First World War, but also in his unperturbed and conciliatory responses to several angry and excited letters from Johannes Stark (see, for instance, Letter 274). The correspondence presented here thus confirms the general picture of Lorentz given in most biographical accounts: that of a uniquely gifted scientist and an admirable personality.

Presentation

The letters are presented in their original language (which for this volume is mainly German); a few Dutch drafts are accompanied by English translations. The editorial apparatus is in English. A planned second volume will contain the complete correspondence between Lorentz and his successor Paul Ehrenfest, as well as some supplementary material. The Lorentz-Ehrenfest correspondence contains many Dutch letters; for these items English translations will be provided in addition to the original texts.

In the transcriptions the original text and layout of the letters has been followed as much as possible. Exceptions are interlineated words or lines, which are silently included in the text, and missing punctuation, which is provided without further comment. Where relevant, deleted text is indicated by < >; missing text is denoted by [...], and illegible text by [---]. Each letter is preceded by physical information on the original (letter, postcard, handwritten, typed etc.) and information on its provenance. Where no provenance is given, the letter is in the Lorentz Archive at the Noord-Hollands Archief in Haarlem, the Netherlands.

The level of annotation to the letters is determined by the following considerations. The starting point has been that the written text should speak for itself as much as possible. Notes are only provided in so far as additional information is needed for the intended readership — which is taken to consist of professional historians of science as well as interested scientists — to understand the contents or the context of the letter, both concerning scientific matters and non-scientific ones. This means that bibliographic information is provided wherever necessary, that the meaning of non-obvious formulas and symbols is explained, and that historical context is provided for discussions on physics. No attempt has been made to be exhaustive in providing references to the secondary literature in the annotation. Brief biographical information is given for important persons mentioned in the letters; more detailed biographical background on the correspondents included in this volume is given in a separate introductory biographical section.

This volume has two separate bibliographies: one containing all items referred to in the editorial apparatus, and one giving an annotated bibliography of Lorentz's writings. The latter bibliography supersedes the one given in Volume 9 of Lorentz's *Collected Papers.*

Some of the letters presented here can also be found in other collections. The most important of these are: *The Collected Papers of Albert Einstein*, 10 volumes to date (Princeton: Princeton University Press, 1987–); *Arnold Sommerfeld: Wissenschaftlicher Briefwechsel*, 2 volumes (Berlin [etc.]: GNT Verlag, 2000, 2004); *La correspondance entre Henri Poincaré et les physiciens, chimistes et ingénieurs* (Basel [etc.]: Birkhäuser, 2007).

Biographical notes on the correspondents

Bjerknes, Vilhelm (1862–1951) became Professor of Applied Mechanics and Mathematical Physics at the University of Stockholm in 1895. From 1907 to 1912 he held a chair at the University of Kristiania (Oslo); in 1912 he was appointed Professor of Geophysics in Leipzig. In 1917 he returned to Norway, where he founded the Bergen Geophysical Institute.

Boltzmann, Ludwig (1844–1906). After completing his study of physics in Vienna, Boltzmann became Professor of Mathematical Physics in Graz in 1869. From 1890 to 1894 he was Professor of Theoretical Physics in Munich; he then moved to Vienna (1894–1900), Leipzig (1900–1902), and back to Vienna.

Einstein, Albert (1879–1955). While holding a modest position at the Swiss Patent Office in Bern, in 1905 he published the papers (on special relativity, Brownian motion, and the light quantum) that would make him famous. From 1909 to 1911 he was Professor of Theoretical Physics at the University of Zurich. He then moved to Prague, and then back to Zurich, now at the Eidgenössische Technische Hochschule (1912). In 1914 he moved to Berlin, where he became Professor of Theoretical Physics and salaried member of the Prussian Academy of Sciences. In 1933 he fled the Nazi regime and settled at the Institute of Advanced Study in Princeton, where he remained until his death. In 1922 he received the Nobel Prize for 1921. Einstein and Lorentz had a very special relationship of friendship and mutual admiration (see *Kox 1993* for an analysis).

FitzGerald, George Francis (1851–1901) was Fellow and later (1881) Professor of Natural Philosophy at Trinity College in Dublin from 1877 until his death.

Planck, Max (1858–1947). His first professorship was in Kiel (1885–1892). He then became Professor of Theoretical Physics at the University of Berlin until his retirement in 1927. In 1919 he received the Nobel Prize.

Poincaré, Henri (1854–1912) worked in Caen before taking up a position as lecturer at the University of Paris (Sorbonne) in 1881. In 1886 he became

Professor of Mathematical Physics at that university and in 1896 he was appointed to the chair of Mathematical Astronomy and Celestial Mechanics. Poincaré made important contributions to mathematics as well as physics; he was also influential as a philosopher of science.

Lord Rayleigh (John William Strutt; 1842–1919) was Cavendish Professor of Physics at the University of Cambridge from 1879 to 1884. He then became a private scholar; from 1887 to 1905 he was also Professor of Natural Philosophy at the Royal Institution in London. He received the Nobel Prize in 1904.

Schrödinger, Erwin (1887–1961) became Professor of Physics in Breslau (now Wroclaw) in 1921. In 1922 he moved to the University of Zurich, where he stayed until 1927, when he was appointed at the University of Berlin. In 1933 he left Germany because of the Nazi regime. After stays in Oxford and Princeton he eventually was appointed at the University of Graz in 1936. After its German annexation in 1938 he left Austria and eventually ended up at the Dublin Institute for Advanced Studies in 1940. In 1956 he went back to Vienna. He received the Nobel Prize in 1933.

Sommerfeld, Arnold (1868–1951) became *Assistent* with Felix Klein in Göttingen in 1894 and became *Privatdozent* there the following year. In 1897 he was appointed Professor of Mathematics at the Bergakademie in Clausthal; in 1900 he became Professor of Technical Mechanics in Aachen and in 1906 Professor of Theoretical Physics in Munich.

Stark, Johannes (1874–1957) was first *Privatdozent* in Hannover (1906–1907). He then held chairs at the Universities of Greifswald (1907–1909), Aachen (1909–1917), again Greifswald (1917–1920) and Würzburg (1920–1922). From 1933 to 1939 he led the Physikalisch-Technische Reichsanstalt in Berlin. He was one of the most important proponents of the so-called “German physics” during the Nazi regime. He was awarded the Nobel Prize in 1919.

Voigt, Woldemar (1850–1919) was Extraordinary Professor at the University of Königsberg (1875–1883) before he became Professor of Theoretical Physics in Göttingen, where he stayed until his death. In addition to his work in physics he published musicological works, in particular on the compositions of Johann Sebastian Bach.

Wiechert, Emil (1861–1928) became *Assistent* in physics at the University of Göttingen in 1890. He was appointed Professor of Geophysics there in 1898.

Wien, Wilhelm (1864–1928) was *Privatdozent* in Aachen (1896–1900), then Professor of Physics in Giessen (1899–1900), Würzburg (1900–1920), and Munich (1920–1928). He received the Nobel Prize in 1911.

Acknowledgments

I am indebted to many people and organizations for help and support during the work on this edition. Mrs. L. van den Beld-Smit, Mrs. P. Uythoven-Tol, and Mrs. S. Schatz-Kopuit were of invaluable assistance in transcribing the letters. My colleagues at the Institute of Theoretical Physics of the University of Amsterdam created a stimulating atmosphere for my work. I am in particular grateful to Leo van den Horn and Leendert Suttorp for invaluable moral support and unfailing interest. B.E. Yavelow provided essential information on Lorentz's Russian publications. I am also grateful to Jed Buchwald and Diana Kormos Buchwald for their trust and support and to my fellow editors at the Einstein Papers Project, first at Boston University and now at the California Institute of Technology, for their interest.

Three people deserve special mention. The first is my late teacher Sybren R. de Groot, who has had a decisive influence on my life and career, and who has never let me down. The late Hendrik B.G. Casimir gave me much needed support at crucial moments, for which I am extremely grateful. Over the years I have benefited enormously from conversations with Martin J. Klein. He has taught me many things that are essential for a historian of science.

The initiative to this edition was taken by the Royal Dutch Society of Sciences of Haarlem, the Netherlands. I am grateful to this venerable learned society, in particular to Mr. J.G. de Bruijn, for support throughout the years. The Netherlands Organization for Scientific Research (N.W.O.) has generously financed my work, first through a regular grant, and later through a Constantijn and Christiaan Huygens Fellowship. I am especially indebted to Dr. Hette Weijma of N.W.O. for his unfailing help in matters concerning finance and planning. The Lorentz Fonds (Leiden) has awarded me several travel grants.

Finally: this book would never have been completed without the trust and love of my wife Henriëtte. Words fail to express my gratitude to her and to our daughter Laura. I dedicate this volume to them.

LIST OF LETTERS

1. To Woldemar Voigt, 7 March 1883

Handwritten letter (Deutsches Museum, Munich).

Leiden, 7 März 1883

Hochgeehrter Herr,

Mit besonderm Interesse las ich vor einigen Jahren Ihre Abhandlung über die Elasticität des Steinsalzes,[1] und ich wünschte jetzt bei meinen Vorträgen über mathematische Physik an der hiesigen Universität die Resultate derselben mitzutheilen. Ich hatte schon behauptet, dass Ihre Untersuchung den besten Beweis lieferte für die Ungelässigkeit der Poisson'schen Theorie, doch bei näherer Ueberlegung erhob sich bei mir ein Bedenken gegen die von Ihnen angewandte Berechnungsweise. Wenn man nämlich nicht die Cauchy'sche sondern die in jüngster Zeit noch von Ihnen selbst begründete de-Saint-Venant'sche Formel für die Torsion benutzt,[2] so werden dadurch die Resultate Ihrer Versuche so geändert, dass gerade jene Behauptung hinfällig wird. Da Sie vielleicht — wenigstens so ging es mir zuerst — einen so grossen Einfluss der veränderten Formel nicht erwarten, erlaube ich mir, Ihnen meine Berechnung Ihrer Versuche kurz mitzutheilen.

Nach der Formel von de Saint-Venant ist zunächst für Stäbchen normal zur Würfelfläche[3]

$$M = \varepsilon\tau\frac{bd^3}{l}\left[\frac{1}{3} - \frac{1}{16}\left(\frac{4}{\pi}\right)^5\frac{d}{b}\sum_{k=0}^{k=\infty}\frac{1}{(2k+1)^5}\frac{e^{(2k+1)\frac{\pi b}{d}}-1}{e^{(2k+1)\frac{\pi b}{d}}+1}\right],$$

wo für die von Ihnen benutzten Stäbchen mit hinreichender Genauigkeit der letzte Bruch = 1 gesetzt werden darf, wodurch

$$M = \frac{1}{3}\varepsilon\tau\frac{bd^3}{l}\left[1 - 0{,}630249\frac{d}{b}\right]$$

oder

$$\tau = \frac{3Ml}{\varepsilon bd^3}\frac{1}{1 - 0{,}630249\frac{d}{b}}$$

wird.

Berechne ich hiermit $\frac{1}{\varepsilon}$ so finde ich[4]

aus	W_I	$10^6\frac{1}{\varepsilon}$ =	0,7899,
	W_I (Torsion um die Axe)		0,7891,
	W_{II}		0,7936,
	W_{III}		0,8003.

Im Mittel

$$\frac{1}{\varepsilon} = \frac{0{,}793}{10^6}$$

A.J. Kox (ed.), *The Scientific Corrrespondence of H.A. Lorentz*,
doi: 10.1007/978-0-387-77940-9_1,

Die Biegung hat ergeben

$$\frac{1}{\varepsilon} - \frac{2}{A-B} = \frac{0{,}206}{10^6}$$

und wir haben also

$$\frac{2}{A-B} = \frac{0{,}587}{10^6}.$$

— — — —

Für die Stäbchen der Gattung *GA* lautet die Formel von de Saint-Venant

$$\tau = \frac{3Ml}{\varepsilon b d^3} \frac{1}{1 - 0{,}630249\sqrt{\frac{2\varepsilon}{A-B}}\frac{d}{b}}.$$

Wenn ich nun für

$$\sqrt{\frac{2\varepsilon}{A-B}}$$

den aus den obigen Resultaten folgenden Werth setze kann ich aus den beobachteten Werthen von τ jedesmal ε berechnen. Ich finde dann

aus *GA* (I)	$10^6 \frac{1}{\varepsilon} = 0{,}794,$
" *GA* (II)	0,786,

was gut mit dem bereits gefundenen stimmt.

In gleicher Weise lassen die Beobachtungen an den Stäbchen *GB* zu $\frac{2}{A-B}$ zu berechnen. Für diese Stäbchen ist nämlich

$$\tau = \frac{3Ml}{\frac{1}{2}(A-B)bd^3} \frac{1}{1 - 0{,}630249\sqrt{\frac{A-B}{2\varepsilon}}\frac{d}{b}},$$

und ich nehme wieder für $\sqrt{\frac{A-B}{2\varepsilon}}$ den bereits gefundenen Werth. Ich erhalte dann aus

GB V		$10^6 \frac{2}{A-B} = 0{,}591,$
"	(Torsion um Axe)	0,586,

in befriedigender Uebereinstimmung mit dem vorher gefundenen. Die beiden Stäbchen *GB* II und *GB* III geben dagegen für $10^6 \frac{2}{A-B}$ die Werthe 0,613 und 0,466. Es sind dies die Stäbchen, die Sie von der Berechnung ausgeschlossen haben.

— — — —

Setze ich nun

$$\frac{1}{\varepsilon} = \frac{0{,}793}{10^6}, \qquad \frac{2}{A-B} = \frac{0{,}587}{10^6}$$

und nach den Biegungsversuchen

$$\frac{1}{\varepsilon} - \frac{2B}{(A-B)(A+2B)} = \frac{0{,}693}{10^6},$$

so wird

$$\frac{2}{A-B}\frac{B}{A+2B} = \frac{0{,}100}{10^6}, \qquad (\alpha)$$

$$\frac{A+2B}{B} = 5{,}87,$$

$$\frac{A-B}{B} = 2{,}87 \quad \text{(nebenbei } \frac{A}{B} = 3{,}87\text{)},$$

$$\frac{1}{B} = \frac{0{,}842}{10^6}.$$

Nach der Theorie von Poisson sollte $\varepsilon = B$ sein.[5] Die Rechnung giebt zwar einen Unterschied zwischen $\frac{1}{\varepsilon}$ und $\frac{1}{B}$ doch liesse sich derselbe durch Beobachtungsfehler erklären. Wenn nämlich der Werth (α) um etwa 0,003 zu klein wäre, was sehr gut möglich ist, so wäre wirklich $\frac{1}{B} = \frac{1}{\varepsilon}$.

Es scheinen mir also Ihre Versuche *innerhalb der Grenzen der Beobachtungsfehler* das Poisson'sche Gesetz zu bestätigen.

Vielleicht haben Sie selbst schon diese Folgerung gezogen; wenn das der Fall ist bitte ich freundlichst um Entschuldigung für dieses nutzlose Schreiben. Anderenfalls wäre es mir sehr angenehm zu erfahren, ob Sie mit meiner Berechnung einverstanden sind.

Beiläufig will ich noch bemerken, dass die Theorie mir für die Torsion um eine Kante den nämlichen Werth gegeben hat wie für die Drillung um die Axe, eine Bestätigung Ihrer experimentellen Resultate.

Mit vorzüglicher Hochachtung verbleibe ich
Ihr ergebener

H.A. Lorentz

[1]*Voigt 1876*, which summarizes Voigt's doctoral dissertation.

[2]See *Voigt 1882a*; for the work of Cauchy and De Saint-Venant, see, e.g., *Love 1927* (in particular its "Historical Introduction").

[3]In the equation M is the moment exerted on the bar, b, d, and l are its dimensions, and τ is the torsional angle. For the crystals studied by Voigt the elasticity coefficients ε, A, and B used in this letter are defined (following Franz Neumann) by $\sigma_{ii} = (A-B)\varepsilon_{ii} + B\mathrm{Tr}\sigma$ and $\sigma_{ij} = 2\varepsilon\varepsilon_{ij}$ $(i \neq j)$, with σ_{ij} the stress tensor and ε_{ij} the strain tensor.

[4]Lorentz uses Voigt's designation for the various bars that were studied. See *Voigt 1876*, p. 196, for more details.

[5]See, e.g., *Love 1927*, "Historical Introduction," for more on Poisson's theory. For Voigt's equations to conform to Poisson's theory for non-crystalline bodies the additional relation $A = 3B$ should be satisfied.

2. From Woldemar Voigt, 21 March 1883

Handwritten letter in German script.

Königsberg i/Pr. d. 21/3 83.

Hochgeehrter Herr!

Für Ihr freundliches Schreiben[1] sage ich meinen besten Dank. Ich habe bereits im vorigen Sommer bei Publication meiner Formeln für Krystallelasticität[2] meine Steinsalz-Beobachtungen neu zu berechnen begonnen u. den grossen Einfluss bemerkt, den die genauere Formel auf die Endresultate hat. (Als ich die Beobachtungen 1873 machte, war ich noch Student und ganz unter Neumann's Einfluss,[3] der S. Venant wenig kennt, dies erklärt dass ich seine Formeln nicht benutzte.) Die Folgerung, welche Sie gezogen haben, hatte ich aber nicht gemacht, zumal weil verschiedene Erfahrungen bei späteren Beobachtungen mir eine *Wiederholung* der Torsionsbeobachtungen räthlich erscheinen liessen, u. ich die definitive Berechnung bis nach deren Vollendung verschieben wollte. Ihr Resultat ist von grosser Wichtigkeit und lässt sehr wahrscheinlich erscheinen, dass die Abweichungen von der Poisson'schen Theorie, die mehrfach bei *unkrystallinischen* Studien beobachtet worden sind, von irgendwelchen *Neben*umständen abhängen. Dass nach meinen Beobachtungen Glas in *entgegengesetztem* Sinne von der Theorie abweicht, wie die Metalle spricht ebenfalls dafür.[4]

Lieb wäre mir, wenn Sie auf die Publication Ihrer Berechnung freundlichst verzichteten; ich gedenke im Sommer neue Beobachtungen anzustellen u. würde dann bei der Veröffentlichung Ihre Resultate mittheilen.[5]

Hochachtungsvollst

W. Voigt.

[1]Letter 1.
[2]See *Voigt 1882b.*
[3]Voigt was then working on his doctoral dissertation in Königsberg under Franz Neumann (1798–1895).
[4]Voigt refers here in particular to the discrepancy between the measured values of Poisson's ratio and its theoretical value of 0.25. For glass Voigt had found values smaller than 0.25 (see *Voigt 1888a*). See also *Love 1927*, "Historical Introduction."
[5]Voigt's new results were published as *Voigt 1884*; see also *Voigt 1883*, in which he comments on Lorentz's remarks (pp. 971–972).

3. From Ludwig Boltzmann, 11 December 1886

Handwritten letter.

Graz, Halbärtgasse 1 11/12 1886.

Hochgeehrter Herr!

Mit grossem Interesse habe ich das mir gefälligst zugesandte Manuscript gelesen.[1] Ich freue mich sehr, dass sich in Ihnen jemand gefunden hat, welcher an dem Weiterbau meiner Ideen über Gastheorie arbeitet. In Deutschland ist fast niemand, welcher die Sache ordentlich verstünde. Auch Ihre ältere Abhandlung betreffend die Ableitung der hydrodynamischen Gleichungen aus der Gastheorie[2] habe ich mit grösstem Interesse gelesen und danke Ihnen für die Zusendung. Den Separatabdruck Ihrer Abhandlung über das Hallphänomen[3] habe ich leider nicht erhalten. Wenn Sie noch genügend viele besitzen, würde ich um Zusendung eines neuen an meine obige genaue Adresse bitten.

Was nun den Gegenstand Ihres letzten Manuscripts betrifft, so haben Sie unbedingt Recht, dass da meine Deductionen einer wesentlichen Verbesserung bedürfen.[4] Meine Behauptung, dass die von mir aufgestellte Zustandsvertheilung weder durch die Zusammmenstösse noch durch die innere Bewegung der Moleküle verändert wird, sobald sie zu Anfang schon vorhanden war, bleibt richtig; nur ihr Beweis erfährt eine Modification. Denn wenn schon zu Anfang die Wahrscheinlichkeit, dass $\xi_1 \ldots w_r$ zwischen den Grenzen ξ_1 und $\xi_1 + d\xi_1 \ldots w_r$ und $w_r + dw_r$ liegen[5] gleich $Ae^{-h(\chi+\lambda)}d\xi_1 \ldots dw_r$ war, wobei χ die Kraftfunction, λ die lebendige Kraft ist, so ist ja die Bedingung $\varphi(\xi_1 \ldots \xi_r, u_1 \ldots w_r) = \varphi(\xi_1 \ldots \xi_r, -u_1 \ldots -w_r)$ [6] erfüllt und es kann bewiesen werden, dass die betreffende Zustandsvertheilung sich erhält. Dass mein Beweis, dass diese Zustandsvertheilung die einzig mögliche ist, für einatomige Moleküle richtig bleibt, bemerken Sie selbst. Auch für Moleküle bleibt er, wie ich glaube, richtig, die sich wie feste Körper verhalten, die 3 aufeinander senkrechte Symmetrieebenen haben. Dagegen wird mein Beweis, dass diese Zustandsvertheilung die einzig mögliche ist, in allen andern Fällen im allgemeinen wenigstens falsch und ich sehe vorläufig wenigstens nicht, wie man ihn durch einen vollkommen von jeder Hypothese freien ersetzen könnte. Das eine scheint mir wohl, dass wenn $\varphi(\xi_1 \ldots \xi_r, u_1 \ldots w_r)$ nicht $\varphi(\xi_1 \ldots \xi_r, -u_1 \ldots -w_r)$ wäre und doch Gleichgewicht der lebendigen Kraft wäre, man folgende Betrachtung anstellen könnte. Man liesse das Gleichgewicht der lebendigen Kraft eine sehr lange (unendlich lange) Zeit T hindurch fortbestehen. Dann dächte man sich die Richtung der Geschwindigkeit jedes Atoms umgekehrt, ohne die Grösse zu ändern. Es würde hierauf das Gas alle frühern Zustände entgegengesetzt durchlaufen; es wäre gewissermassen bloss die Zeit umgekehrt. Es würde also das Gas jetzt in einer Zustandsvertheilung unendlich lange verharren, in welcher $\varphi(\xi_1 \ldots \xi_r, u_1 \ldots w_r)$ denselben Werth hätte wie früher $\varphi(\xi_1 \ldots \xi_r, -u_1 \ldots -w_r)$. Wenn also ein Wärmegleichgewicht möglich ist, wobei $\varphi(\xi_1 \ldots \xi_r, u_1 \ldots w_r)$ einen werth a hat, welcher vom Werthe b der Function $\varphi(\xi_1 \ldots \xi_r, -u_1 \ldots -w_r)$ verschieden ist, so ist immer noch ein zweites

Wärmegleichgewicht möglich, für welches $\varphi(\xi_1 \ldots \xi_r, -u_1 \ldots -w_r) = a$ und $\varphi(\xi_1 \ldots \xi_r, u_1 \ldots w_r) = b$ ist. Es scheint mir fast als ob auch die Geschwindigkeitsvertheilung, wobei $\varphi(\xi_1 \ldots \xi_r, u_1 \ldots w_r) = ma + nb$ ist, nicht durch die Zusammenstösse gestört werden könnte, wo m und n beliebige Constanten sind; aber dafür gelang mir der Beweis noch nicht. Mit alledem sind aber Ihre Bedenken nicht widerlegt, da kein Beweis geliefert ist, dass nicht zwei verschiedene Wärmegleichgewichte möglich sind.

Wünschen Sie vielleicht, dass ich Ihr Manuscript der Wiener Academie der Wissenschaften vorlege? Dann werde ich diess sogleich thun und es etwa mit einer Bemerkung begleiten, welche meiner Sätze dadurch tangirt werden.[7] Vielleicht gelingt es Ihnen, das was ich oben kurz andeutete, weiter zu entwicklen. Jedesfalls freue ich mich, von Ihnen bald wieder einen Brief zu erhalten.[8] Mit ausgezeichneter Hochachtung

Ihr ergebenster

Ludwig Boltzmann

[1]The manuscript of *Lorentz 1887a*, as becomes clear from the discussion in this and the following letter.

[2]*Lorentz 1880a* or its French translation *Lorentz 1881d*.

[3]*Lorentz 1883a* or its French translation *Lorentz 1884a*.

[4]*Lorentz 1887a* criticizes *Boltzmann 1872*, in which the *H*-theorem is derived on the assumption of 'inverse' collisions taking place with the same probability as the original 'direct' collision (the inverse collision is constructed from the direct one by interchanging the colliding particles immediately after the collision). Lorentz shows that for non-spherical molecules inverse collisions in general do not exist and gives an alternative treatment.

[5]$\xi_1, \eta_1, \zeta_1, \ldots \xi_r, \eta_r, \zeta_r$ are the center of mass coordinates of the r 'material points' of which a polyatomic molecule is composed; $u_1, v_1, w_1, \ldots u_r, v_r, w_r$ are the corresponding velocity-components. There are only $6r - 3$ independent coordinates: ξ_r, η_r, ζ_r are functions of the other coordinates (through the center of mass condition).

[6]In *Lorentz 1887a* it is shown that this condition for the one-particle distribution function φ (or f in Lorentz's notation) is a sufficient condition for stationarity of the distribution function. Lorentz also finds that it implies the equality $f_1 f_2 = f_1' f_2'$, which Boltzmann had shown to be necessary for stationarity (f_i has the coordinates and velocity of colliding particle i before the collision as its arguments; f_i' refers to the situation after the collision). According to Lorentz, Boltzmann's proof that the above equality implies stationarity was flawed.

[7]The manuscript was published in the *Sitzungsberichte* of the Vienna Academy (see note 1); the paper is immediately followed by *Boltzmann 1887a,* in which the mistake of *Boltzmann 1872* is corrected.

[8]See the discussion in Letters 4–8; see also *Kox 1982*, *1990, 1993a* for historical discussions of the Lorentz–Boltzmann correspondence and Lorentz's work in kinetic gas theory.

4. To Ludwig Boltzmann, 19 December 1886

Handwritten draft. The first page of the ms. is marked: “Aan Boltzmann” (“To Boltzmann”). Above and below the salutation the following addition is written: “Es gereicht mir zur besonderen Freude, dass Sie, dem wir die molekularkinetische Behandlungsweise des Problems der mehratomigen Moleküle verdanken, bereit sind die Schwierigkeiten, auf welche man bei diesem Problem stösst, mit mir in Discussion zu treten.”

Leiden, 19 Dez. 1886.

Hochgeehrter Herr,

Empfangen Sie meinen besten Dank für die so freundliche Weise in welcher Sie meine Bedenken beantwortet haben.[1] Von Ihrer Erlaubnis mache ich recht gern Gebrauch um noch einmal auf die Sache zurückzukommen.

Es scheint mir nämlich, dass nach meinen Betrachtungen nicht nur der Beweis dass die von Ihnen gefundene Zustandsvertheilung die einzig mögliche ist, sondern auch der Beweis, dass dieselbe existiren kann, hinfällig wird, oder wenigstens nicht ohne gewisse Hypothesen stichhaltig bleibt. Wie ich in dem Manuscript, das ich Ihnen zukommen liess, bemerkte, kann der Beweis, dass das Gas sich einem Endzustande nähert, bei welchem $f_1 f_2 = f_1' f_2'$ ist, geliefert werden, sobald man von einem Anfangszustande ausgeht, für welchen die Relation $f(\xi_1 \dots \xi_{r-1}, -u_1 \dots -w_r) = f(\quad)$ besteht,[2] und *annimmt dass diese Relation dann immer bestehen bleibt.* Ist nun der angenommene Anfangszustand derart, dass für denselben schon $f_1 f_2 = f_1' f_2'$ ist, so ist der Anfangszustand selbst der Endzustand, immer wenn man annimmt, dass die Relation $f(\quad) = f(\quad)$ bestehen bleiben muss.

Man kann dies auch so einsehen. Wenn einmal $f(\quad) = f(\quad)$ und gleichzeitig $f_1 f_2 = f_1' f_2'$ ist, so kann man zeigen, dass wenn in einem Zeitelement p Theilchen die Gruppe A verlassen gleich viele Theilchen in die Gruppe $A(-)$ hineingetreten sind;[3] dass also die Gesamtzahl der Theilchen, welche zu den beiden Gruppen gehören, ungeändert geblieben ist. Daraus folgt aber nur dann, dass auch für jede Gruppe einzeln genommen die Molekülzahl ungeändert bleibt, wenn man annimmt, dass nicht eine Verschiedenheit in den Zahlen der Gruppe A und der Gruppe $A(-)$ entstehen kann. Man kann sagen: Besteht einmal der Zustand …, so wird dieser entweder ungeändert bleiben, oder es wird aus demselben ein Zustand entstehen, in welchem nicht mehr … ist.

— — — —

Was zweitens die in Ihrem Schreiben enthaltene Betrachtung betrifft, bei welcher aus einem Zustande des Gases P durch Umkehrung aller Bewegungsrichtungen ein neuer Zustand $P(-)$ hergeleitet wird, so fürchte ich, dass dieser letztere kein stabiler Gleichgewichtszustand sein würde, und demzufolge auch nicht als ein natürlicher Zustand des Gases auftreten könnte; er könnte wie ich glaube nur bestehen, wenn das System der Moleküle in der von Ihnen gedachten Weise sorgfältig präpariert worden ist, und jeden noch so kleinen störenden Einfluss entzogen wäre.

Betrachten wir z.B. die Zusammenstösse