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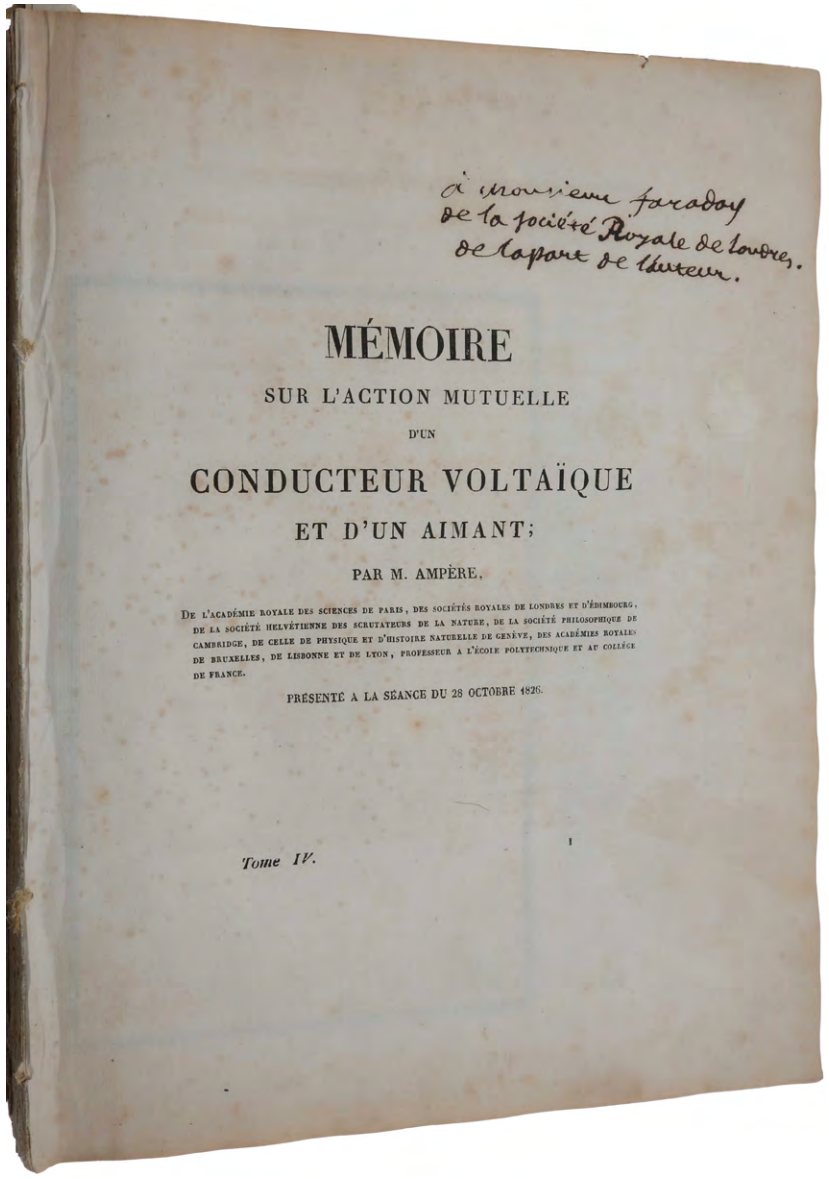
INSCRIBED BY AMPÈRE TO FARADAY

AMPÈRE, André-Marie. *Mémoire sur l'action mutuelle d'un conducteur voltaïque et d'un aimant. Offprint from Nouveaux Mémoires de l'Académie royale des sciences et belles-lettres de Bruxelles, tome IV, 1827. Bound with 18 other pamphlets (listed below).* [Colophon:] Brussels: Hayez, Imprimeur de l'Académie Royale, 1827.

$38,000

4to (265 x 205 mm). Contemporary quarter-cloth and plain boards (very worn and broken, with most of the spine missing), entirely unrestored. Preserved in a custom cloth box.

First edition of the very rare offprint, with the most desirable imaginable provenance: this copy is **inscribed by Ampère to Michael Faraday**. It thus links the two great founders of electromagnetism, following its discovery by Hans Christian Oersted (1777-1851) in April 1820. The discovery by Ampère (1775-1836), late in the same year, of the force acting between current-carrying conductors was followed a year later by Faraday's (1791-1867) first great discovery, that of electromagnetic rotation, the first conversion of electrical into mechanical energy. This development was a challenge to Ampère's mathematically formulated explanation of electromagnetism as a manifestation of currents of electrical fluids surrounding 'electrodynamic' molecules; indeed, Faraday directly criticised Ampère's theory, preferring his own explanation in terms of 'lines of force' (which had to wait for James Clerk Maxwell (1831-79) for a precise mathematical formulation). Faraday's criticism led Ampère to modify his theory, and to carry out his own experiments on electromagnetic rotation which actually went beyond what Faraday had accomplished. The present work, which



"brought to a close his main sequence of research publications on electricity and magnetism" (Grattan-Guinness, p. 960), showed that Ampère's theory was able to reproduce the results of the rival theories of Biot and Poisson. It immediately precedes Ampère's summary work *Théorie mathématique des phénomènes électro-dynamiques uniquement déduite de l'expérience*, which refers to it on p. 196: "je l'ai d'ailleurs développée dans un autre écrit [the present work] où j'ai discutée même temps, sous ce nouveau point de vue, tout ce qui est relative à l'action mutuelle d'un aimant et d'un conducteur voltaïque." This offprint is distinguished by the presence of the colophon 'Bruxelles, M. Hayez, Imprimeur de l'Académie Royale' at the foot of the last page of text, which is not present in the journal issue. The present pamphlet volume is almost certainly from Faraday's library (four of the pamphlets in the volume are inscribed to him). It is also possible that the binding itself, which must be described as functional rather than decorative, was executed by Faraday: before becoming Humphry Davy's (1778-1829) assistant in 1813, the appointment which ignited his great scientific career, Faraday had been an apprentice bookbinder. It is known that he bound his laboratory notebooks, and some of his other books, himself (many are still held by The Royal Institution in London). A volume of pamphlets such as ours would have been a prime candidate on which Faraday could exercise his old skills.

Provenance: Michael Faraday (inscription on title in Ampère's hand: 'à Monsieur Faraday de la Société Royale de Londres de la part de l'auteur').

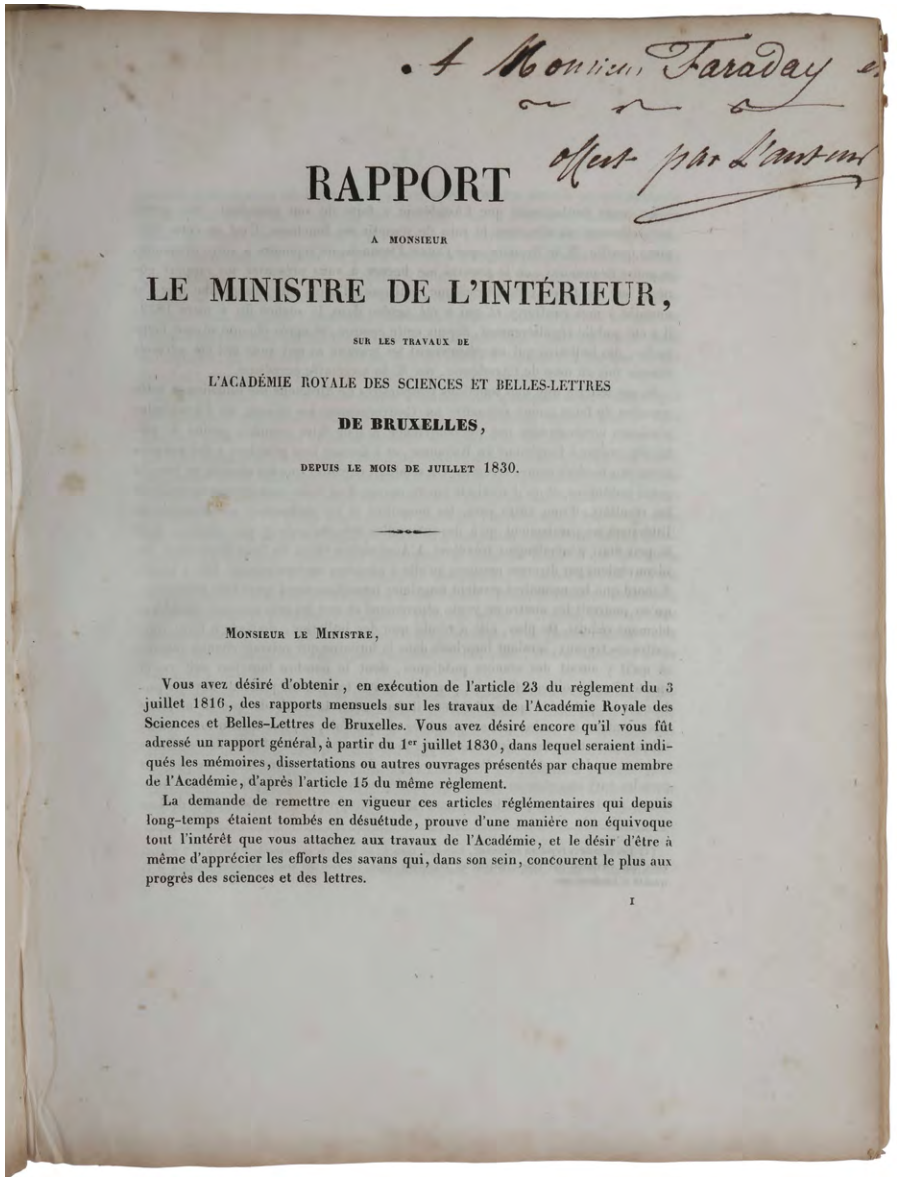
"[The] reconciliation of different physical models was of great importance to Ampère at this time … In a long paper on this matter presented to the Belgium Academy in October 1826 and published by them as [the present work], he extended the discussion into a general appraisal of three hypotheses for electromagnetism. 'The first consists in admitting the existence of two fluids called austral and boreal', as thought Poisson (for example); 'But some vague and general insights should not suffice, for the explanation of the facts which comprise this new branch of Physics'. 'The second is that by which I have given an account of the observed phenomena', where the magnet was treated as a solenoid. 'Finally the third hypothesis', supported by Biot, assumed 'a primitive elementary action', in which magnet and pole tended to turn each other, thus 'forming what Mr. Poinsot has called a couple'; while for Ampère this was apparently 'directly contrary to the first principles of Dynamics', it needed examination. In order to further his comparison he presented again his surface-circuit theorem, and related the expressions both to Poisson's formula for magnetic action and to his own for element/solenoid action. In the rest of the paper, he considered various cases of magnet/circuit action, noting all hypotheses but keeping his preference clearly evident" (Grattan-Guinness, p. 960).

According to Ampère's first theory of electrodynamic phenomena, magnetic forces were the result of the motion of two electric fluids; permanent magnets contained these currents running in circles concentric to the axis of the magnet and in a plane perpendicular to this axis. By implication, the earth also contained currents which gave rise to its magnetism. It was not long, however, before Auguste Fresnel (1788-1827) pointed out to his friend Ampère that his theory had several difficulties, notably the fact that the supposed currents in magnets should have a heating effect that was not observed. Fresnel suggested instead that the electric currents circulated around each molecule, rather than around the axis of the magnet. In January 1821 Ampère publicly accepted Fresnel's idea. Between 1821 and 1822, Gaspard de la Rive (1770-1834), Albert van Beek (1787-1856) and Faraday performed some experiments showing that the poles of a cylindrical magnet are not located exactly at the extremities of the magnet, as was predicted by Ampère's theory. These experiments forced Ampère to modify his conception of microscopic currents: due to the collective interactions between the small current-carrying loops, the planes of these molecular currents should, he said, no

longer remain perpendicular to its magnetic axis.

For both Jean-Baptiste Biot (1774-1862) and Siméon Denis Poisson (1781-1840), on the other hand, electrical phenomena were to be explained in terms of magnetism, not the other way round. Together with a protégé, Félix Savart (1791-1841), Biot performed experiments late in 1820 which led them to the 'Biot-Savart law' for the force exerted on a magnet by a current-carrying wire. Biot claimed to have deduced this law from "a mathematical expression for forces assumed to act upon magnetic fluid particles from each tiny section of a conducting wire, [but] Biot considered this conclusion to be only a preliminary step toward a full explanation based on an understanding of 'a true molecular magnetization impressed upon the particles of metallic bodies by the voltaic current that traverses them.' For Biot, the genuine fundamental forces were assumed to act between magnetic fluid particles in the wire and in the magnet" (Hofmann, p. 234). "Biot claimed that the central issue to be resolved was 'how each infinitely small molecule of the connecting wire contributes to the total action of the section to which it belongs' and thereby brings about the 'molecular magnetism' of the wire … From Biot's point of view, explanation of all the new phenomena ultimately had to be traced to a specific distribution of magnetic fluid particles" (*ibid*., p. 281).

"Biot's Laplacian colleague Poisson took an even more conservative approach. Rather than try to account for the new electromagnetic and electrodynamic phenomena, he developed a purely magnetic theory in strict accordance with Laplacian standards. In parallel with his two electricity memoirs of a decade earlier, during 1824 Poisson developed an analogous imponderable fluid theory of magnetism in two lengthy memoirs he eventually published in 1826. Unimpressed by developments inspired by Oersted's discovery in 1820, Poisson staunchly preserved the Laplacian tradition by treating magnetism as a domain entirely distinct from that of electricity. With a single introductory remark, he cavalierly



à Monsieur Faraday

offert par l'auteur

RAPPORT

A MONSIEUR

LE MINISTRE DE L'INTÉRIEUR,

SUR LES TRAVAUX DE

L'ACADÉMIE ROYALE DES SCIENCES ET BELLES-LETTRES

DE BRUXELLES,

DEPUIS LE MOIS DE JUILLET 1830.

MONSIEUR LE MINISTRE,

Vous avez désiré d'obtenir, en exécution de l'article 23 du réglement du 3 juillet 1816, des rapports mensuels sur les travaux de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles. Vous avez désiré encore qu'il vous fût adressé un rapport général, à partir du 1er juillet 1830, dans lequel seraient indiqués les mémoires, dissertations ou autres ouvrages présentés par chaque membre de l'Académie, d'après l'article 15 du même réglement.

La demande de remettre en vigueur ces articles réglémentaires qui depuis long-temps étaient tombés en désuétude, prouve d'une manière non équivoque tout l'intérêt que vous attachez aux travaux de l'Académie, et le désir d'être à même d'apprécier les efforts des savans qui, dans son sein, concourent le plus aux progrès des sciences et des lettres.

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dismissed the issue that so preoccupied Ampère. 'The identity of the magnetic fluid and the electric fluid does not necessarily result from the important facts that have been discovered recently. Fortunately, the solution of this question is not at all relevant to the object of this memoir; our analysis is independent of the particular nature of the boreal and austral fluids; our goal is simply to determine the resultants of their attractions and repulsions, and, if it is possible, how they are distributed within magnetized bodies' … [Poisson's] most fundamental physical assumption was that magnetic effects are produced by two magnetic fluids that act on each other through fundamental forces that vary as the inverse square of the distance between their particles. Poisson also assumed that the austral and boreal fluids are confined by some unknown force to the interior of small 'magnetic elements'" (ibid., pp. 323-4).

Ampère's response was to show that the structure assumed in Poisson's model, and the forces he applied to it, generated accurate results simply because this was an alternative way of describing the action of circuits and electrodynamic forces. This approach required detailed analysis of Poisson's model and a demonstration of how it could be translated into an equivalent description of the magnetic state in terms of microscopic electric circuits. Ampère "tackled the problem of magnetic surfaces and their equivalence to electric circuits in a long memoir he later claimed to have completed for the most part early in 1826; the published version appeared in the *Mémoires* of the Belgian Académie Royale in 1827 [offered here]. His primary result was a mathematical theorem that became his new foundation for the transformation of surface integrals into contour integrals … Ampère immediately applied his theorem to the geometry of Poisson's model to calculate the magnetic force between a magnetic surface and an exterior magnetic molecule … Ampère concluded that he had discovered a general identity between magnetic surfaces and electric circuits, "from which it follows that the action on a magnetic molecule by the surface in question covered by magnetic elements is identical to that which a conducting wire would exert on the same molecule if it was substituted for the closed contour which circumscribes this surface. It is in this way that one accounts for this law in the first hypothesis in which everything should be reduced to the mutual action of austral and boreal magnetic molecules" [p. 21 of the present work].

"Ampère's own view was of course that electromagnetic interactions are all due to more basic interactions between electric currents. Nevertheless, by showing that the Biot-Savart force could be reduced to interactions between magnetic elements, Ampère could argue that there was no need to follow Biot and postulate a transverse force between current elements and magnetic molecules; he then could use his demonstration of the equivalence of magnetic elements to tiny electric circuits to complete the reduction of the Biot-Savart force to his own electrodynamic forces" (ibid., pp. 333-6).

The contents of the volume are as follows (in the order in which they are bound; all items are 4to):

AVOGADRO, Amadeo. [Drop-head title:] Mémoire sur la force élastique de la vapeur du mercure à différentes températures. Offprint from Memorie della Reale Accademia delle Scienze di Torino, Tom. XXXVI. Turin: de l'Imprimerie Royale, 1833. Pp. [ii], 3-73 with one plate.

AMPÈRE, André-Marie. [Drop-head title:] Mémoire sur l'action mutuelle d'un conducteur voltaïque et d'un aimant. Offprint from Nouveaux Mémoires de l'Académie royale des sciences et belles-lettres de Bruxelles, tome IV, 1827. [Colophon:] Brussels: Hayez, Imprimeur de l'Académie Royale, [1827]. Pp. [2], 3-88, with three folding engraved plates. **Inscribed by Ampère to Michael Faraday**.

HACHETTE, Jean-Nicolas-Pierre. [Drop-head title:] Notice sur la mésure des

effets dynamiques et description d'un modèle exécuté aux frais de la Société d'Encouragement, sur lequel on a réuni le dynamomètre à coussinets mobiles de M. Welter, et le dynamomètre à frein de M. de Prony. Offprint from Bulletin de la Société d'Encouragement pour l'Industrie nationale, No. CCCXX, February 1831. [Colophon:] [Paris]: Huzard, 1831. Pp. [1], 2-11 with one folding plate.

HACHETTE, Jean-Nicolas-Pierre. [Drop-head title:] Mémoire sur les divers modes de numérotage employés dans les filatures et dans les tréfileries. Offprint from Bulletin de la Société d'Encouragement pour l'Industrie nationale, No. CCXLVII, December, 1824. [Colophon:] [Paris]: Huzard, 1824. Pp. [1], 2-13.

THENARD, Louis-Jacques & CHEVREUL, Michel-Eugène. [Drop-head title:] Rapport … sur un mémoire de M. Serullas ayant pour titre: De l'action de l'acide sulfurique sur l'alcohol et des produits qui en résultent. Offprint from Institut Royale de France, Académie Royale des Sciences (séance de 19 Janvier 1829). [Colophon:] [Paris]: Firmin Didot, 1829. Pp. [1], 2-15.

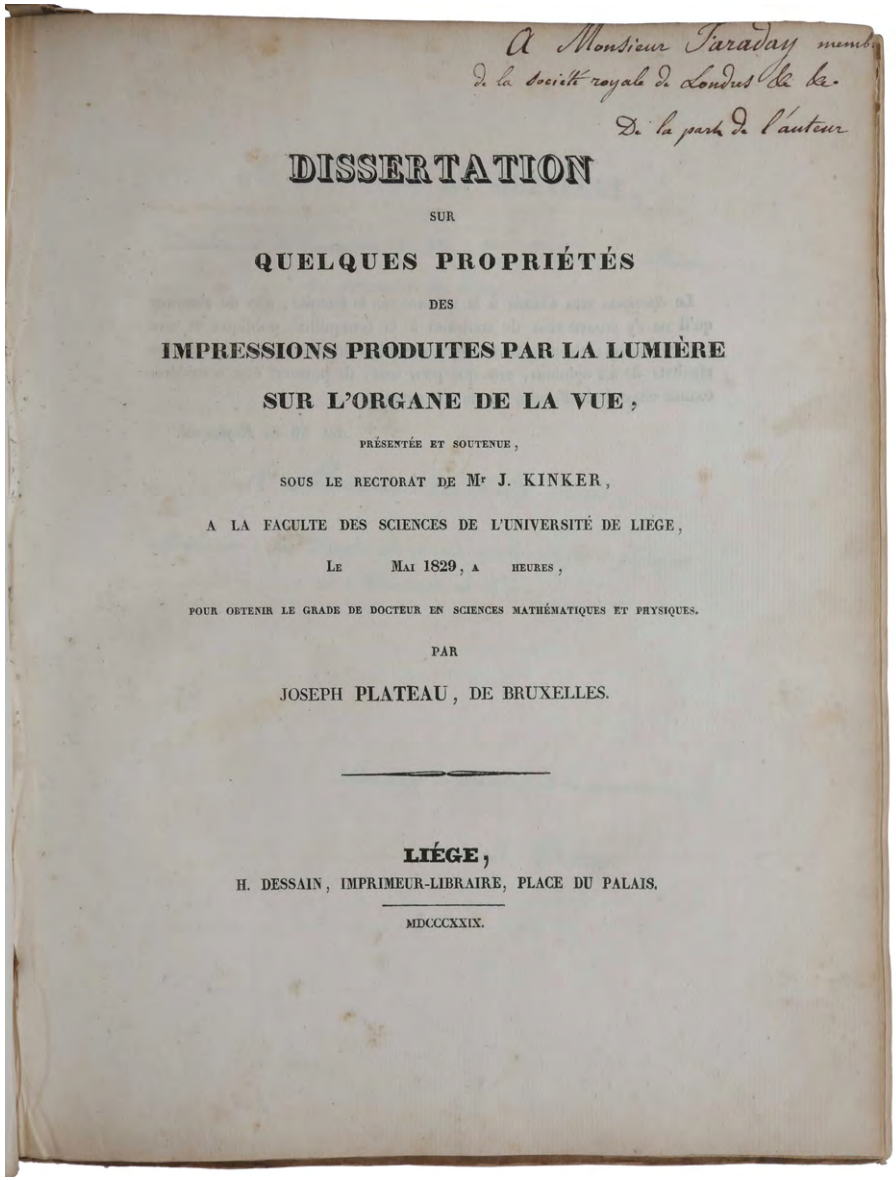
CHEVREUL, Michel-Eugène. [Drop-head title:] Rapport sur le bouillon de la Compagnie Hollandaise. Offprint from Institut Royale de France, Académie Royale des Sciences (19 Mars 1832). [Colophon:] [Paris]: Firmin Didot, 1832. Pp. [ii], 42.

QUETELET, Adolphe. Recherches sur l'intensité magnétique de différents lieux de l'Allemagne et des Pays-Bas. Offprint from Mémoires de l'Académie Royale de Bruxelles, Tom. VI. Pp. [ii], [1], 2-18. Brussels: Hayez, 1830.

QUETELET, Adolphe. [Drop-head title:] Rapport à monsieur le minister de l'intérieur, sur les travaux de l'Académie Royale des Sciences et des Belles-Lettres de Bruxelles, depuis le mois de Juillet 1830. No imprint. Pp. [1], 2-12. **Inscribed by Quetelet to Michael Faraday**.

PLATEAU, Joseph Antoine Ferdinand. Dissertation sur quelques propriétés des impressions produits par la lumière sur l'organe de la vue. Liège: Dessain, 1829. Pp. [5], 6-32 with one folding plate. **Inscribed by Plateau to Michael Faraday.**

DE LA RIVE, Auguste. Recherches sur une propriété particulière des conducteurs


A Monsieur Faraday membre
de la société royale de Londres &c &c.
De la part de l'auteur

DISSERTATION

SUR

QUELQUES PROPRIÉTÉS

DES

IMPRESSIONS PRODUITES PAR LA LUMIÈRE

SUR L'ORGANE DE LA VUE,

PRÉSENTÉE ET SOUTENUE,

SOUS LE RECTORAT DE Mr J. KINKER,

A LA FACULTE DES SCIENCES DE L'UNIVERSITÉ DE LIEGE,

Le Mai 1829, a heures,

POUR OBTENIR LE GRADE DE DOCTEUR EN SCIENCES MATHÉMATIQUES ET PHYSIQUES.

PAR

JOSEPH PLATEAU, DE BRUXELLES.

LIÉGE,

H. DESSAIN, IMPRIMEUR-LIBRAIRE, PLACE DU PALAIS.

MDCCCXXIX.

métalliques de l'électricité. [With:] MARCET, F. Note sur l'analyse de quelques substances végétales. Offprint from Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, Tome III. Geneva: Heirs of J. J. Paschoud, 1827. Pp. [ii], [1], 2-24.

MACAIRE-PRINCEP, Jean-François & MARCET, François. [Drop-head title:] Analyse de la neige rouge du pole. Offprint from Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, Tome IV, 1828. No imprint. Pp. [1], 2-4.

MACAIRE-PRINCEP, Jean-François & MARCET, François. [Drop-head title:] Note sur l'action mutuelle de l'ammoniaque et du phosphore. Offprint from Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, Tome IV. [Colophon:] Geneva: Barbezat, 1828. Pp. [1], 2-4.

MACAIRE-PRINCEP, Jean-François. [Drop-head title:] Mémoire pour servir à l'histoire des assolemens. Offprint from Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, Tome V. No imprint. Pp. [1], 2-16 with two folding tables.

CADET-DE-GASSICOURT, Charles Louis. De l'extinction de la chaux. Paris: Colas, 1812. Pp. [5], 6-20.

PRONY, Gaspard de. [Drop-head title:] Réclamation … contre une interprétation erronée donnée à un passage de son Traité de Mécanique Analytique (Paris, 1815) par M. Kater … dans un mémoire ayant pour titre: An account of experiments for determining the length of the pendulum, etc. (London, 1818). [Colophon:] [Paris]: Firmin Didot. Pp. [1], 2-4.

DELAROCHE, François Étienne. Expériences sur les effets qu'une forte chaleur produit dans l'économie animale. Paris: Didot Jeune, 1806. Pp. [4], 5-90.

WECKENBACH, Wilhelm. Disputatio mathematica inauguralis de pontium lapideorum forma et mensuris ex aequilibrii doctrina determinandis. Amsterdam: Sulpke, 1830. Pp. [vii], [1], 2-116, [2], with two folding plates. **Inscribed by Weckenbach to Michael Faraday**.

CUVIER, Georges. [Drop-head title:] Analyse des travaux de l'Académie Royale des Sciences pendant l'année 1826. Partie Physique. [Paris]: Firmin Didot, 1827. Pp. [ii], [1], 2-71.

FOURIER, Jean-Baptiste Joseph. [Drop-head title:] Analyse des travaux de l'Académie Royale des Sciences pendant l'année 1826. Partie Mathématique. [Paris]: Firmin Didot, 1827. Pp. [ii], [1], 2-94.

Of the works other than Ampère's in the present volume, perhaps the most interesting is the dissertation of Joseph Plateau (1801-83), who was one of the first to demonstrate the illusion of a moving image. This copy is inscribed to Faraday, who, on 10 December 1830, had presented a paper at the Royal Institution entitled 'On a Peculiar Class of Optical Deceptions' about the optical illusions that could be found in rotating wheels. As Faraday publicly acknowledged, much of his paper was similar to what Plateau had published in his thesis. "Plateau's early work was in the field of physiological optics. The basis of much of this work was his observation that an image takes an appreciable time to form on, and to disappear from, the retina. In his dissertation (1829) [offered here] Plateau showed, among other things, that the total length of an impression, from the time it acquires all its force until it is scarcely sensible, is approximately a third of a second. He applied his results to the study of the principles of the color mixture produced by the rapid succession of colors. This led to the formulation of the law (now known as the Talbot-Plateau law) that the effect of a color briefly presented to the eye is proportional both to the intensity of the light and the time of presentation. Plateau also studied various optical illusions that result from the persistence of the image on the retina" (DSB). In his thesis Plateau also first described the anorthoscope, a device which demonstrates an optical illusion that turns an anamorphic picture on a disc into a normal image when the disc is spun fast and seen through the four radial slits of a counter-rotating black disc. "Plateau revisited this concept several times in the *Correspondance Mathématique et Physique* and by January 1836 finally

decided to have the device itself published. He sent a box with the instrument to Michael Faraday on 8 January 1836 … Faraday had previously inspired Plateau to use a mirror with revolving discs … Faraday thought the anorthoscope was a beautiful machine with an exceedingly curious and good effect, and mentioned: "It has wonderfully surprized many to whom I have showed it and they all refuse to believe their own eyes and cannot admit that the forms seen are the things looked at" [letter to Quetelet, 12 May 1836]" (Wikipedia, accessed 16 July 2017).

Faraday appears to have been on terms of close friendship with Adolphe Quetelet (1796-1874), author of two of the pamphlets in the present volume, one of which is inscribed to Faraday. Best known for his introduction of statistical methods into the social sciences, Quetelet was a polymath whose work on atmospheric electricity was much admired by Faraday.

Grattan-Guinness, *Convolutions in French Mathematics, 1800–1840*, 1990. Hoffman, *André-Marie Ampère*, 1995.



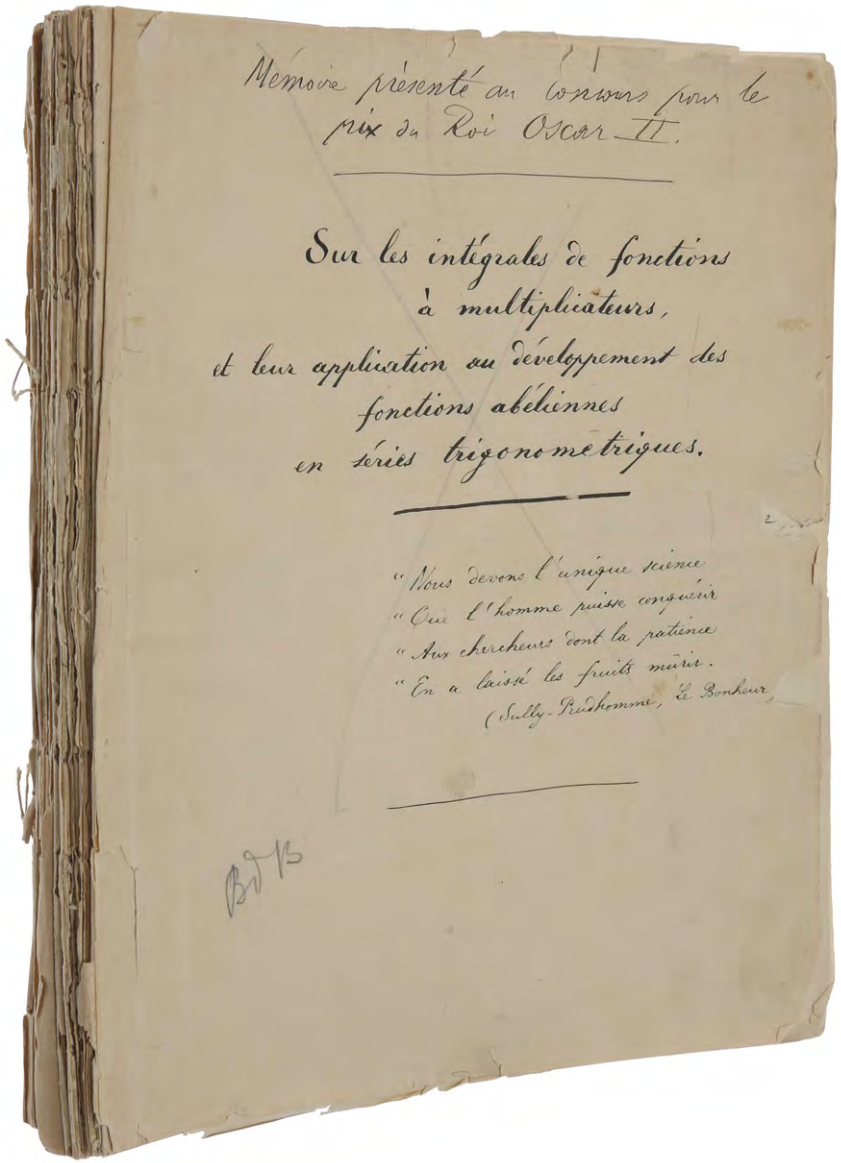
THE ORIGINAL MANUSCRIPT FOR ONE OF HIS MOST NOTABLE WORKS

APPELL, Paul Émile. *Mémoire présenté au concours pour le prix du Roi Oscar II. Sur les intégrales de fonctions à multiplicateurs et leur application au développement des fonctions abéliennes en séries trigonométriques. Autograph manuscript, 278 leaves written on rectos only. Undated, but first published in Acta Mathematica 13 (1890), pp. 4-174.*

$9,500

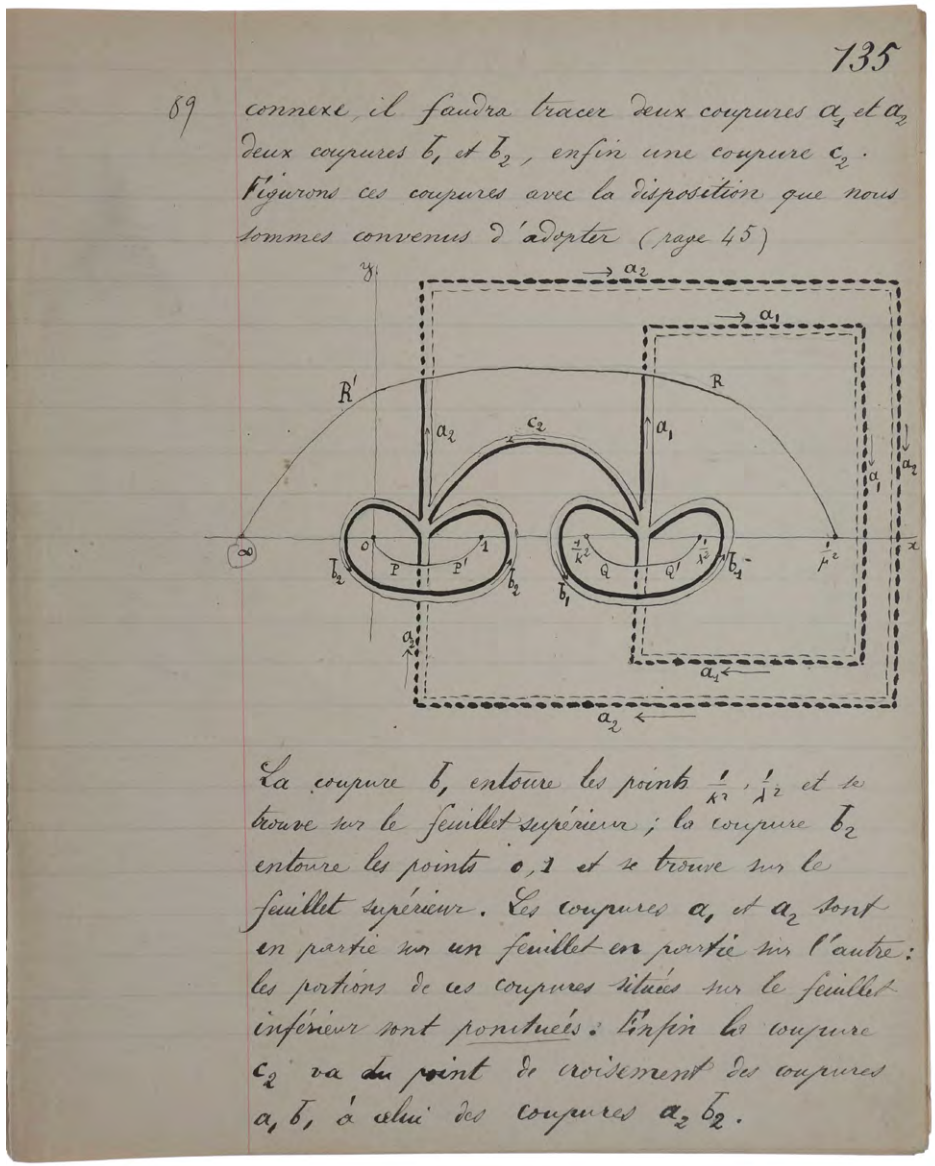
Autograph manuscript in 8vo (220 x 175 mm), 278 leaves written on recto page, consisting of title (1 leaf), device leaf (1 leaf), Table des matières (1 leaf) and headtext leaf 1-210 (including 4bis, 156 a-d, 171 bis) + Supplément title (1 leaf) and headtext 1-60 and Table des Matières de supplément (4 leaves). Illustrations in text. Partly worn, some marginal tears. Custom cloth box.

Important autograph manuscript in which Appell generalises the theory of Abelian functions, due principally to Abel, Jacobi, Riemann and Weierstrass, to a class of functions he terms 'fonctions à multiplicateurs', and investigates their integrals and Fourier expansions. "His scientific work consists of a series of brilliant solutions of particular problems, some of the greatest difficulty. He was a technician who used the classical methods of his time to answer open questions, work out details, and make natural extensions in the mainstream of the late nineteenth century" (DSB), which cites the present paper as one of his most notable works. Appell's memoir won honorary second prize in the competition mentioned on the title, the first prize going to Henri Poincaré for his great work



'Sur le problème de trois corps et les équations de la dynamique.'
In his statement on behalf of the prize committee, Charles Hermite wrote about Appell's contribution: "Le mémoire de M. Appell, sur les intégrales de fonctions à multiplicateurs et leur application au développement des fonctions abéliennes en séries trigonométriques, est également digne du plus haut intérêt. M. Appell a ouvert un champ nouveau dans la théorie des fonctions d'une variable, en donnant l'origine d'une catégorie de transcendants, douées de propriétés extrêmement remarquables, dont il a fait une étude approfondie et qui sont appelées à jouer un grand rôle. C'est, à notre époque, un des plus importants résultats de l'Analyse que ces découvertes de nouvelles fonctions auxquelles s'attachent les noms illustres de'Abel et de Jacobi, de Göpel, de Rosenhaim, de Weierstrass et Riemann. M. Appell s'est surtout inspiré de Riemann; son beau Mémoire, ceux de M. Poincaré sur les fonctions fuchsiennes, d'autres travaux français continuent l'oeuvre de ces grands géomètres." No manuscripts by Appell are listed on ABPC.

"In 1885 Gösta Mittag-Leffler could look back at a successful start on his career as the first professor in mathematics of the newly founded *Stockholms högskola* (later to become Stockholm University). After his education and doctor's degree from Uppsala, he had spent three years in Paris and Berlin while he forged firm bonds with many leading European mathematicians. He then turned to Helsinki where he broadened his horizons as a dedicated and strong-willed professor. Four years later, in 1880, he returned to his native Stockholm. The new principles on which *Stockholms högskola* was founded gave Mittag-Leffler the opportunity to work out his ideas of how mathematical education and research should be organized and promoted. He attracted several talented postgraduates, and managed to engage Sofia Kovalevskaya as lecturer and later full professor at the department. The journal *Acta Mathematica* which he founded in 1882 was right from the beginning received as a leading international publication. It established itself as an effective transmitter of new mathematical ideas between France and Germany


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89 connexe, il faudra tracer deux coupures a_1 et a_2 deux coupures b_1 et b_2, enfin une coupure c_2. Figurons ces coupures avec la disposition que nous sommes convenus d'adopter (page 45)

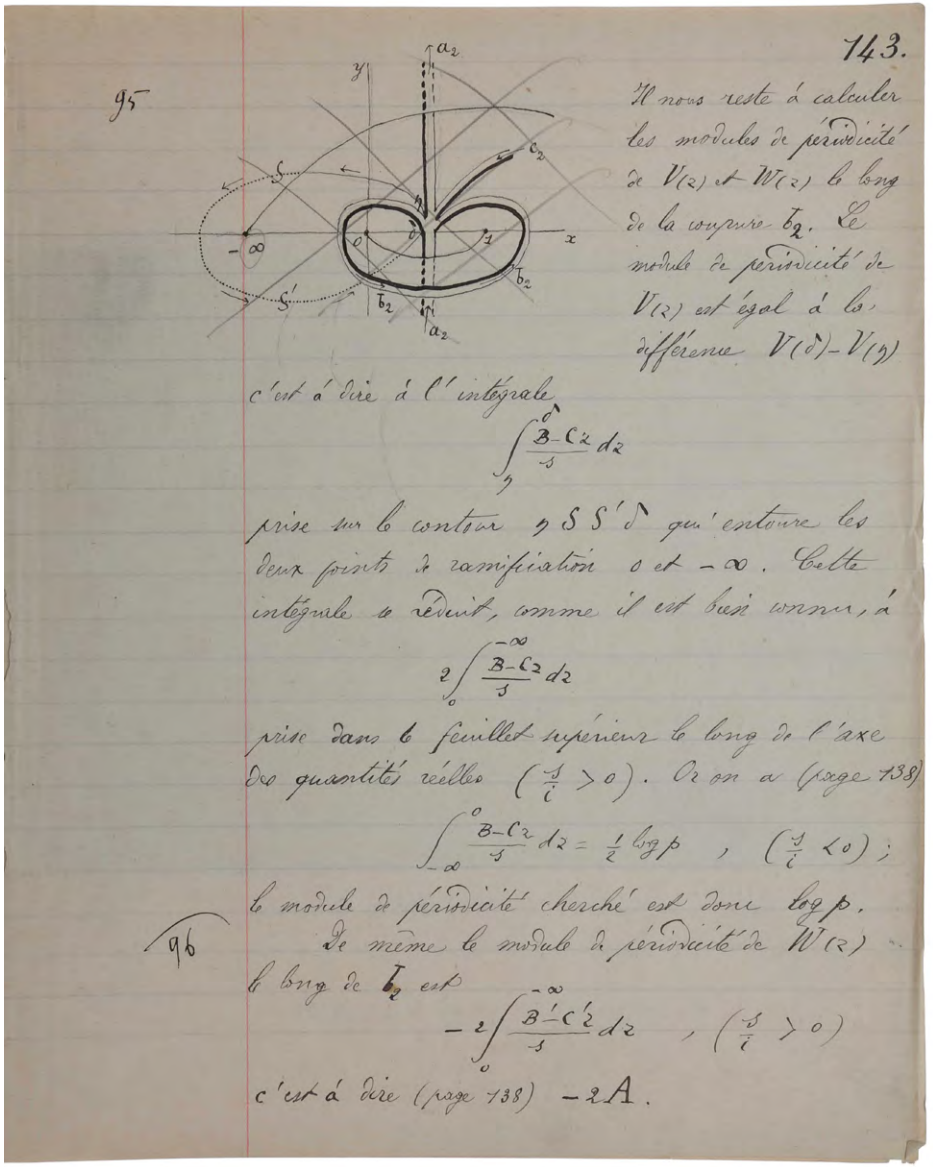
La coupure b_1 entoure les points $\frac{1}{k^2}$, $\frac{1}{\lambda^2}$ et se trouve sur le feuillet supérieur; la coupure b_2 entoure les points 0, 1 et se trouve sur le feuillet supérieur. Les coupures a_1 et a_2 sont en partie sur un feuillet en partie sur l'autre: les portions de ces coupures situées sur le feuillet inférieur sont ponctuées: Enfin la coupure c_2 va du point de croisement des coupures $a_1 b_1$ à celui des coupures $a_2 b_2$.

in the aftermath of the Franco-Prussian War (1870-71).

"One of Mittag-Leffler's inventive ideas in promoting *Acta Mathematica* was to turn to King Oscar II of Sweden and Norway, both for financial support of the project and as the first enlisted subscriber. In 1884 another grand idea from Mittag-Leffler had matured. Again involving King Oscar, he now wanted to arrange an international prize competition in mathematics honouring the 60th birthday of the king. It seems that Mittag-Leffler first revealed his plan to Kovalevskaya. We find a short reference in a letter to her from 4 May 1884. A month later it is clear from another letter by Mittag-Leffler that she had by then discussed the prize with Karl Weierstrass. In July Mittag-Leffler for the first time briefed Weierstrass on the plans.

"It was first decided to form a small prize jury consisting only of three members: Mittag-Leffler himself, acting as administrative and coordinative liaison with his mentors and friends Karl Weierstrass in Berlin and Charles Hermite in Paris. They were not only the two dominant mathematicians of the older generation, but there was also a special sympathy between them. This would be a prize awarded not for past contributions, but for a solution to an unsolved problem specified by the committee. In order to attract the best mathematicians from different branches of mathematical analysis they agreed on four questions. The first three seem to have been proposed by Weierstrass and the fourth by Hermite. It took a year of intense discussions to settle all the details, which were then officially announced in Volume 7 of *Acta Mathematica* in the middle of 1885.

"The special features of this competition was the international and ambitious appeal, and the connection not to an academy or institution, but to the journal Acta Mathematica, where the winning entry finally was to be published. The prize consisted of a gold medal and 2,500 Swedish kronor. (As a comparison, Mittag-

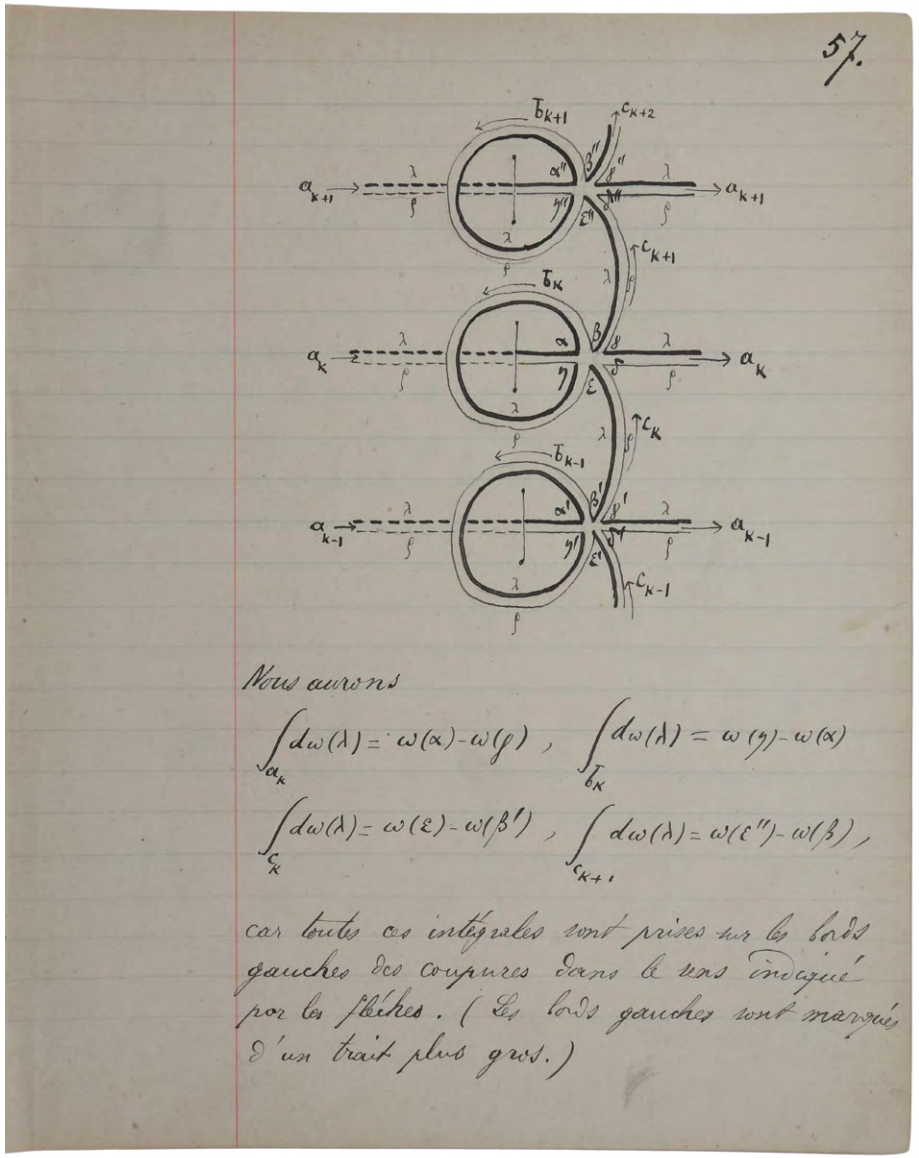


Leffler's annual salary as professor was 7,000 kronor.) The memoirs should be submitted before 1 June 1888 (almost three years after the announcement), with anonymity maintained through a motto on an enclosed sealed envelope containing the name of the author.

"The first question was a formulation of the classical *n*-body problem in celestial mechanics, and it attracted the most attention: "For a system of arbitrarily many mass points that attract each other according to Newton's law, assuming that no two points ever collide, find a series expansion of the coordinates of each point in known functions of time converging uniformly for any period of time." Weierstrass nourished great hopes that there would be a method of solution based on a simple fundamental idea, and that such a solution could help conclude whether the system was stable. In applying this formulation of the *n*-body problem as a model of the solar system, could it be excluded that the planetary orbits might alter radically over a long time, or even that one planet be thrown out of the system? …

"A list of all the twelve manuscripts received by June 1888 was published in Volume 11 of *Acta* with their identifying epigraphs. It turned out that five of the authors had attempted the prestigious Question 1, one had tried Question 3, while six treated a subject of their own, which remained a secondary option. Only four of the authors have been identified: in addition to Poincaré also Paul Appell, Guy de Longchamps and Jean Escary … Although no entry had actually solved any of the questions, Mittag-Leffler and his jury were soon of the preliminary opinion that Poincaré was in a class of his own, that Appell should be awarded a second honorary prize, and that no other entries needed much further examination" (Rågstedt).

In 1873, Appell (1855-1930) entered the École Normale, from which he graduated first in the class of 1876, three months after earning his doctorate. From this time



on, Appell was highly active in teaching, research, editing, and public service. He typically held several teaching posts at the same time, including the chair of mechanics at the Sorbonne from 1885. He was elected to the Académie des Sciences in 1892. He served as dean of the Faculty of Science of the University of Paris from 1903 to 1920 and as rector from 1920 to 1925. In various government posts, including membership in the Conseil Supérieure d'Instruction Publique, he was an exponent of educational reform and initiator of numerous large-scale projects, including the Cité Universitaire. He vigorously supported the movement for women's rights, carried from Alsace his brother's reports destined for the French War Office, defended his fellow Alsatian Dreyfus and served on an expert commission whose ruling played a key role in his final rehabilitation. During World War I he founded and led the Secours National, a semi-official organization uniting all religious and political groups to aid civilian victims. He described the return of the tricolour to Alsace as the fulfilment of his "lifelong goal" and felt that Germany had been treated too easily. He served as secretary-general of the French Association for the League of Nations.

"Appell's first paper (1876) was his thesis on projective geometry in the tradition of Chasles, but at the suggestion of his teachers he turned to algebraic functions, differential equations, and complex analysis. He generalized many classical results (e. g., the theories of elliptic and of hypergeometric functions) to the case of two or more variables. From the first his work was close to physical ideas. For example, in 1878 he noted the physical significance of the imaginary period of elliptic functions in the solution of the pendulum problem, and thus showed that double periodicity follows from physical considerations. In 1880 he wrote on a sequence of functions (now called the Appell polynomials) satisfying the condition that the derivative of the nth function is n times the previous one.

"In 1885 Appell was awarded half the Bordin Prize for solving the problem of "cutting and filling" (*deblais et remblais*) originally posed by Monge: To move a given region into another of equal volume so as to minimize the integral of the element of volume times the distance between its old and new positions. In 1889 he won second place (after Poincaré) in a competition sponsored by King Oscar II of Sweden: To find an effective method of calculating the Fourier coefficients in the expansion of quadruply periodic functions of two complex variables.

"The flow of papers continued, augmented by treatises, textbooks, and popularizations and seemingly unaffected by other responsibilities. Although Appell never lost his interest in "pure" analysis and geometry, his activity continued to shift toward mechanics, and in 1893 Volume 1 of the monumental *Traité de mécanique rationnelle* appeared. Volume V (1921) included the mathematics required for relativity, but the treatise is essentially an exposition of classical mechanics of the late nineteenth century. It contains many of Appell's contributions, including his equations of motion valid for both holonomic and non-holonomic systems, which have not displaced the classical Lagrangian system in spite of undoubted advantages" (DSB).

Rågstedt, 'From order to chaos: the prize competition in honour of King Oscar II,' mittag-leffler.se/library/prize-competition.

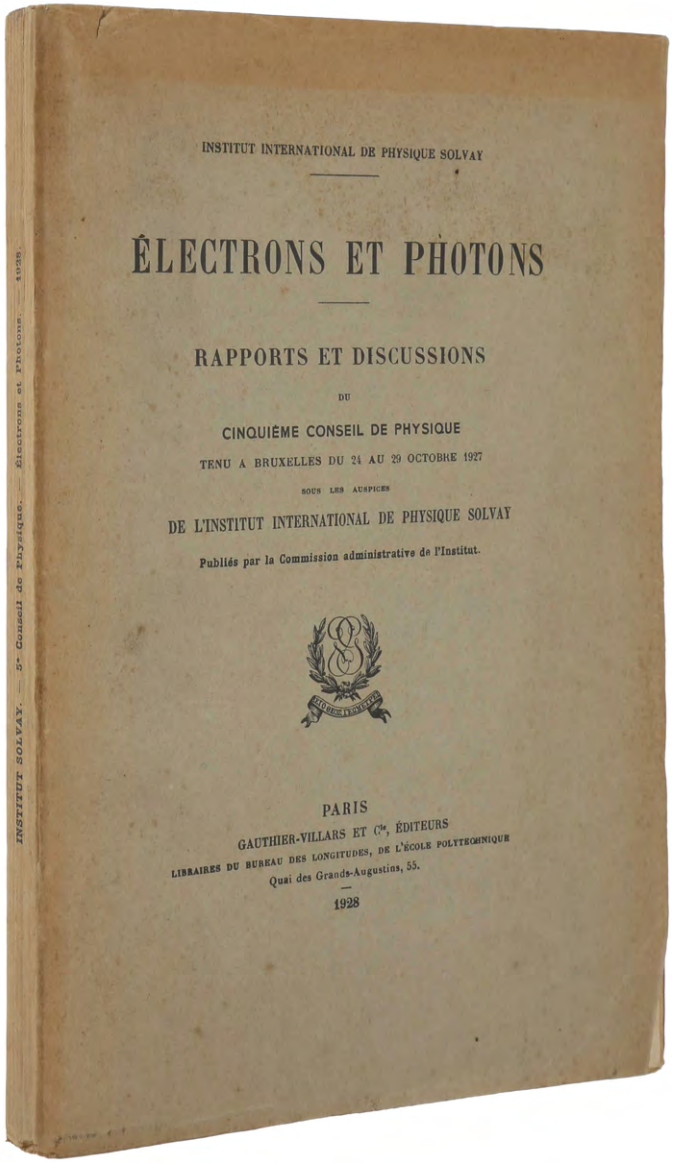
THE BOHR-EINSTEIN DEBATE

[BOHR, Niels; BORN, Max; BRAGG, William Lawrence; COMPTON, Arthur; DE BROGLIE, Louis; DIRAC, Paul; EINSTEIN, Albert; HEISENBERG, Werner; LORENTZ, Hendrik; SCHRÖDINGER, Erwin; et al.] *Électrons et Photons. Rapports et Discussions du Cinquieme Conseil de Physique tenu a Bruxelles du 24 au 29 Octobre 1927 sous les Auspices de L'Institut International de Physique de Solvay.* Paris: Gauthier Villars, 1928.

$5,000

8vo (255 x 165 mm), pp. viii, 289, with frontispiece portrait of Lorentz. Uncut and unopened. Original printed wrappers, some light sunning to wrappers, small closed tear to front wrapper. Otherwise very fresh and clean in its original state.

First edition, rare in the original printed wrappers, of the proceedings of the fifth Solvay Congress, where the debate between Bohr and Einstein on the consistency and completeness of quantum mechanics began. It was at this, the most famous of the Solvay conferences, that Einstein, disenchanted with Heisenberg's uncertainly principle, made his famous remark that "God does not play dice," to which Niels Bohr replied, "Einstein, stop telling God what to do!" Seventeen of the twenty-nine attendees, which included nearly all the principal architects of the old and the new quantum theory, were or became Nobel Prize winners. "The three and a half years since the fourth Solvay Conference … were marked by enormous progress in quantum physics. Partly based on discoveries and ideas that had been available already before 1924 – such as the Compton effect and matter waves – the new atomic theory had arisen, which did more than throw new light on the difficulties discussed at the 1924 conference: quantum or wave mechanics went right to the heart of the problems posed by atomic phenomena. The two subjects



put programmatically into the title of the fifth Solvay Conference – electrons and photons – designated the crucial points of interest, because 'electrons' also stood for the smallest, massive constituents of matter, and they now became associated with waves, and 'photons' (a name given only recently, in October 1926 by the physical chemist Gilbert N. Lewis, to Einstein's light-quanta) characterized the quantum-theoretical aspect of electromagnetic radiation. It was the declared intention of the Scientific Committee of the *Institut International de Physique Solvay* to contribute by scientific reports and discussions about them to the clarification of the scientific concepts in the physics of the day. In retrospect, one may indeed attribute an important success to the 1927 Solvay Conference in marking the completion of the ideas that had first been discussed in the international physics community sixteen years previously at the first Solvay Conference of 1911" (Mehra & Rechenberg, pp. 233-4). The present volume contains the following reports, and discussions about them by the participants: 'The Intensity of the Reflection of X-rays,' by Bragg; 'Disagreement between Experience and the Electromagnetic Theory of Radiation,' by Compton; 'The New Dynamics of Quanta,' by de Broglie;'The Mechanics of Quanta,' by Born and Heisenberg;'The Mechanics of Waves,' by Schrödinger;'The Quantum Postulate and the New Development of Atomic Theory,' by Bohr. No copies located in auction records.

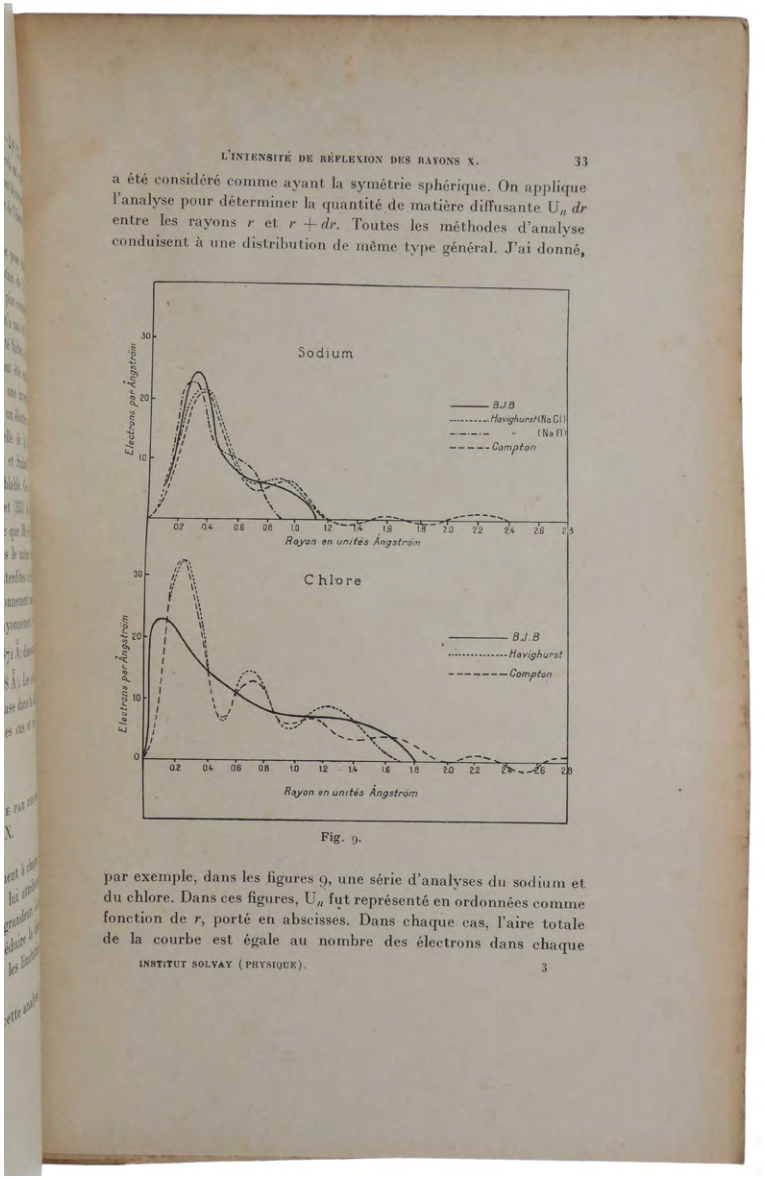
In 1911 the Belgian industrialist Ernest Solvay invited a group of the world's most prominent physicists, including Einstein, Planck, Lorentz, Sommerfeld, Rutherford and Marie Curie, to participate in a scientific conference on the difficultiesof reconciling classical physics with quantum theory. The conference "set the style for a new type of scientific meetings, in which a select group of the most well informed experts in a given field would meet to discuss the problems at its frontiers, and would seek to define the steps for their solution" (Mehra, *Solvay Conferences*, p. xv). The first Solvay Conference—widely considered a turning point in the history of modern physics—was so successful that in the following year Solvay established a foundation, now known as the International Solvay Institutes for Physics and Chemistry, "to encourage the researches which would extend and deepen the knowledge of natural phenomena" (*ibid.*) and to sponsor further conferences. The next two Solvay Conferences met in 1913 and 1921; subsequent conferences have been held every three years except during wartime.

"From amongst the members of the Scientific Committee [of the 1927 Congress], two had already played a leading role in 1911: the Chairman Hendrik Lorentz and Albert Einstein; the latter had presented then the most revolutionary report [on the light quantum]. In spring 1926, in the early stage of preparing for the new conference, Lorentz again requested Einstein to write a report. The latter answered promptly: 'If you wish that I take over the report on quantum statistics, I shall do so with pleasure; because, without being in great difficulty, I shall never say "no" to you' (Einstein to Lorentz, 1 May 1926) … On 17 June 1927, Einstein wrote to Lorentz: 'I recall having committed myself to you to give a report on quantum statistics at the Solvay [Conference]. After much reflection back and forth, I came to the conclusion that I am not competent for giving such a report in a way which really corresponds to the state of the thing. The reason is that I have not been able to participate as intensively in the modern development of quantum theory as would be necessary for that purpose. This is in part because I have on the whole too little receptive talent for fully following the stormy developments, in part also because I do not approve of the purely statistical way of thinking on which the new theory is founded …' As a substitute speaker for the topic assigned to him, he proposed either Enrico Fermi from Italy or Paul Langevin from France. Ultimately, however, neither of them gave the report on Einstein's subject. Instead, Niels Bohr agreed to contribute a report on a different topic: namely, on his latest considerations on the problem of the interpretation of quantum mechanics. "The rapporteurs at the fifth Solvay Conference fell into three groups: the

experimentalists Bragg and Compton; the theoreticians advocating the Gottingen-Cambridge-Copenhagen versions of quantum mechanics – Bohr, Born, and Heisenberg; and those of the wave-mechanical camp – de Broglie and Schrodinger.

"The selection of Arthur Holly Compton seemed to be most appropriate, because the Compton effect – discovered in late 1922 – had been one of the crucial results triggering the entire development which ended with the new atomic theory by providing Einstein's light-quantum hypothesis of 1905 a firm experimental foundation. Since its discovery, and even more so after the refutation of the Bohr-Kramers-Slater theory of radiation … Einstein's fundamental light-quantum conception … became a physical reality. Compton's report dwelt on the conceptual consequences rather than on experimental details. In particular, he addressed the questions of the aether and of electromagnetic waves, on the one hand, and the phenomena contradicting the (classical) wave concepts, such as the photoelectric effect, X-ray diffraction, certain electron-recoil effects (observed by C. T. R. Wilson and W. Bothe in 1923), and the individual interaction between radiation-quanta and electrons (i.e., the Compton effect). Compton showed also in some detail how the Bohr-Kramers-Slater theory failed to account for these observations.

"The report of William Lawrence Bragg, a regular participant in the Solvay Conferences since 1913, appeared to address, on first inspection, less central points. However, from his presentation of the material on reflection of X-rays, one easily recognizes the strategy of the Scientific Committee of the Conference: Bragg had to take over the task of stressing those radiation phenomena that could be described by the wave theory, namely, the diffraction of X-rays by crystal lattices. Consequently, he gave the story from Laue's discovery in 1912, over the subsequent work of his father William Henry Bragg and himself, to the later



L'INTENSITÉ DE RÉFLEXION DES RAYONS X. 33

a été considéré comme ayant la symétrie sphérique. On applique l'analyse pour déterminer la quantité de matière diffusante $U_n\,dr$ entre les rayons r et $r+dr$. Toutes les méthodes d'analyse conduisent à une distribution de même type général. J'ai donné,

Fig. 9.

par exemple, dans les figures 9, une série d'analyses du sodium et du chlore. Dans ces figures, U_n fut représenté en ordonnées comme fonction de r, porté en abscisses. Dans chaque cas, l'aire totale de la courbe est égale au nombre des électrons dans chaque

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investigations of Paul Ewald, William Duane, and others. Bragg demonstrated in detail how the old and the new wave theories worked to describe the phenomena of diffraction and refraction of X-rays. In the discussion of Bragg's report, Hendrik Kramers presented at some length the recent development of the dispersion theory by himself and Ralph Kronig.

"Both experimental reports served as a firm basis for the discussion of the theoretical concepts, which provided the central theme of the conference. This significance was shown by the discussions immediately following them. Compton's talk especially gave rise to a lively exchange of ideas and arguments, in which, besides the experimentalists (e.g., Bragg, Madame Curie, O. W. Richardson, and C. T. R. Wilson), almost all of the theoretical experts present participated, i.e., Bohr, Born, Debye, Dirac, Ehrenfest, Lorentz, Pauli, and Schrödinger – with one important exception: according to the published proceedings of the fifth Solvay Conference [offered here] Einstein *remained silent* after the presentations of Compton and Bragg …

"The presentation of the theoretical reports at the Solvay Conference proceeded by following the historical order in which the ideas had been published between 1923 and 1926: thus, de Broglie's talk came first, then Born and Heisenberg's, followed by Schrödinger's, and finally Bohr's …

"In the course of the year 1927, the Copenhagen physicists, Heisenberg among them, clarified their ideas on the interpretation of atomic phenomena. Although they admitted the existence (and persistence) of statistical relations in quantum mechanics, they searched for – and succeeded in – formulating principles that, in their opinion, at least, provided the deeper reason for these statistical features: the uncertainty relations and the complementarity principle. In spite of this difference in attitude toward what they regarded as truly fundamental and actually derived, Heisenberg felt no difficulty in preparing a joint Solvay report together with his former teacher Max Born. Their report provided a view of the work performed in Göttingen and Cambridge in establishing quantum mechanics, with chapters on matrix mechanics and its transformation into wave mechanics (I), the physical interpretation of the theory (II), and the uncertainty principle (III). The balance in representing the main interests of the two authors was achieved insofar as Section II dealt with Born's statistical interpretation and Section III with Heisenberg's limitation on measurements in quantum mechanics. Moreover, the Born-Heisenberg report also signaled the agreement reached by Heisenberg and Niels Bohr during the summer of 1927 …

"Louis de Broglie entitled his report 'The new dynamics of quanta'; he covered the story from his first ideas on matter waves (in 1923-24) to the advent of Schrödinger's equation (in 1926) and on to the new pilot-wave theory (in 1927). He further applied the pilot-wave theory to the problem of the hydrogen atom and claimed that the treatment yielded an easier understanding of the actual situation; finally, he spoke about the experimental verification of matter waves obtained recently in the experiments of Clinton Joseph Davisson and Lester Halbert Germer, and George Paget Thomson and Alexander Reid … Erwin Schrödinger, on the other hand, concentrated on the mathematical aspects of his wave-mechanical scheme, the time-independent as well as the time-dependent equations, the formal equivalence of wave mechanics to the Born-Heisenberg-Jordan matrix scheme, and the relativistic wave equation. At the end of the conference, Niels Bohr presented a modified version of his Como lecture under the title 'The Quantum Postulate and the New Development of Atomic Theory' …

"Although the Born-Heisenberg and Schrödinger reports provoked only technical questions, that of de Broglie and especially the one of Bohr stimulated some conceptual discussion. Thus, Lorentz asked de Broglie how the old Sommerfeld

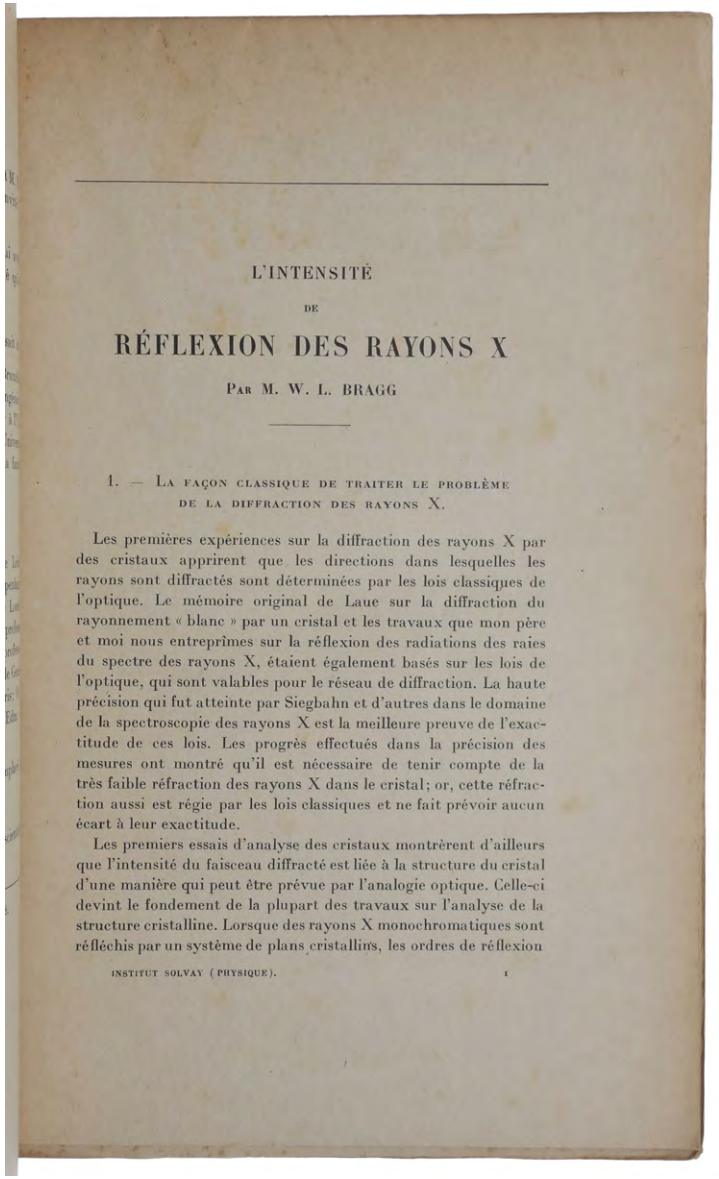
quantum conditions could be obtained from the new matter-wave ideas, and Pauli provided an appropriate calculation using the conservation law for the relativistic electric current; also, Leon Brillouin illustrated some 'optical' applications of matter waves. Finally, Bohr's report at the end provided the proper start for a very excited 'General Discussion of the New Ideas Put Forward.'

"Upon an opening reflection of Hendrik Lorentz – who expressed some reservation with respect to the new pictures of electrons in quantum and wave-mechanics – and a technical illustration of Max Born for dealing with many-electron systems in the probability scheme, Einstein addressed an elementary problem in the physical interpretation of the theory. He suggested, in particular, to consider an electron passing through a slit in a screen and to discuss the diffraction phenomena obtained. He claimed that 'with respect to [quantum mechanics] one can take two standpoints regarding its validity,' namely:

Interpretation I: The de Broglie-Schrodinger waves do not correspond to a single electron, but to an electron cloud, extended in space. The theory does not give [then] any information about an ensemble of an infinity of elementary processes.

Interpretation II: The theory claims to be a complete theory of individual processes. Each particle which moves towards the screen, as far as one can determine from its position and velocity, is described by a de Broglie-Schrödinger wave packet of small length and small aperture. This wave packet is diffracted and, after diffraction, arrives partly at the film [where it is registered in a resolved state] (Einstein, in the present work, pp. 254-5).

"Evidently, *Interpretation II* went beyond *I* and even included the latter; it also implied that the conservation laws (especially for momentum) were valid for individual atomic processes, thus explaining the Bothe-Geiger experiment as well



L'INTENSITÉ

DE

RÉFLEXION DES RAYONS X

PAR M. W. L. BRAGG

1. — LA FAÇON CLASSIQUE DE TRAITER LE PROBLÈME DE LA DIFFRACTION DES RAYONS X.

Les premières expériences sur la diffraction des rayons X par des cristaux apprirent que les directions dans lesquelles les rayons sont diffractés sont déterminées par les lois classiques de l'optique. Le mémoire original de Laue sur la diffraction du rayonnement « blanc » par un cristal et les travaux que mon père et moi nous entreprîmes sur la réflexion des radiations des raies du spectre des rayons X, étaient également basés sur les lois de l'optique, qui sont valables pour le réseau de diffraction. La haute précision qui fut atteinte par Siegbahn et d'autres dans le domaine de la spectroscopie des rayons X est la meilleure preuve de l'exactitude de ces lois. Les progrès effectués dans la précision des mesures ont montré qu'il est nécessaire de tenir compte de la très faible réfraction des rayons X dans le cristal; or, cette réfraction aussi est régie par les lois classiques et ne fait prévoir aucun écart à leur exactitude.

Les premiers essais d'analyse des cristaux montrèrent d'ailleurs que l'intensité du faisceau diffracté est liée à la structure du cristal d'une manière qui peut être prévue par l'analogie optique. Celle-ci devint le fondement de la plupart des travaux sur l'analyse de la structure cristalline. Lorsque des rayons X monochromatiques sont réfléchis par un système de plans cristallins, les ordres de réflexion

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as other experiments. Still, Einstein also objected to this interpretation, because: 'If $|\Psi|^2$ [where Ψ is the wave function] were simply considered as the probability for a particle to be at a place at the definite instant, it might happen that *one and the same* elementary process would cause an action at *two or more* places on the screen,' which would imply an action-at-a-distance, hence, a violation of the relativity postulate. The only way out of this difficulty had to be sought with de Broglie in further attempts to localize the microscopic particle. Einstein claimed further that the multidimensional phase space assumed for many-particle systems in quantum or wave mechanics and the corresponding permutation properties contradicted the new statistical results.

"Although Lorentz tried to illuminate the statistical argument further, Wolfgang Pauli contradicted Einstein by referring to the recent work of Paul Dirac, Pascual Jordan, and Oskar Klein on field quantization. He also refuted another argument of Einstein's, that the range of forces in quantum mechanics might create problems, by pointing to the work of Walter Heitler and Fritz London on molecular binding. Dirac, at first, supported Pauli's plea; then, he stated his 'opinion about determinism and the significance of numbers which occur in the calculus of quantum theory,' notably: 'In the classical theory one starts from certain numbers which completely specify the initial state of the system, and one deduces certain numbers which specify the final state. This determination applies only to an isolated system' (Dirac, in the present work, p. 261).

"Now, according to Bohr, isolated systems are by definition unobservable, because any observation must disturb the system; as a result, already 'the classical deterministic theory cannot be defended.' Furthermore: 'In the quantum theory, one starts from certain numbers from which one deduces certain [other] numbers … The perturbations, which an observer inflicts on a system in order to observe it, are directly subject to his control and are acts of his free will. *It is exclusively the numbers, which describe these acts of free choice, that can be taken as initial numbers for a calculation in the quantum theory*. Other numbers specifying the initial state of the system are fundamentally unobservable and do not appear in the quantum-theoretical treatment' (Dirac, *loc. cit.*) …

"After Dirac had illustrated his interpretation of the quantum-mechanical process and its observation in the case of a sample collision experiment, Heisenberg remarked that he did 'not agree' with Dirac, saying 'that in the experiment described nature makes a choice,' because: 'Even if you place yourself very far from your scattering material, and if you measure after a very long time, you can obtain interference by taking two mirrors. If nature were to make a choice, it would be difficult to imagine how the interference can be produced. Evidently, we can say that nature's choice can never be known until the decisive experiment has been done; for this reason we cannot make any real objection to this choice, because the expression "nature makes a choice" does not have any physical consequences. I would say, as I have done in my latest paper, that the *observer himself* makes the choice, because it is not until the moment when the observation is made that the "choice" becomes a physical reality and that the phase relation in the waves, i.e., the ability to interfere, is destroyed' (Heisenberg, in the present work, pp. 264-5) …

"The differences in interpretation among the main pioneers of quantum mechanics that showed in this exchange, notably, between Bohr (and Heisenberg) and Dirac, would become more pronounced in the future …

"In the recollections of some participants of the fifth Solvay Conference, the exchange between Bohr and Einstein on fundamental questions concerning the interpretation of quantum mechanics stands out vividly. Thus, Bohr, after more than twenty years, wrote a detailed account of his 'Discussions with Einstein on

Epistemological Problems in Atomic Theory,' where he introduced the important part dealing with the 1927 exchange by saying: 'At the general discussions in Como, we all missed the presence of Einstein, but soon after, in October 1927, I had the opportunity to meet him in Brussels at the Fifth Physical Conference of the Solvay Institute … At the Solvay meetings, Einstein had from their beginning been a most prominent figure, and several of us came to the Conference with great anticipations to learn his reaction to the latest stage of the development which, to our view, went far in clarifying the problems which he had himself from the outset elicited so ingeniously. During the discussions, where the whole subject was reviewed by contributions from many sides, Einstein expressed, however, a deep concern over the extent to which causal account in space and time was abandoned in quantum mechanics.'

"The official discussions referred to above throw light on some of the exchanges on the questions that did interest Einstein, although Bohr's participation in them does not seem to have been so active. For example, no answer from Bohr to Einstein's analysis of the electron's passage through a slit or screen was recorded. Bohr just made some notes, which are to be found in his files, and, as Louis de Broglie recalled: '[Also] Einstein said hardly anything beyond presenting a very simple objection to the probability interpretation. Then he fell silent'. However, Heisenberg took away quite a different impression from the conference, and decades later, he wrote enthusiastically: 'The discussions were soon focused upon a duel between Einstein and Bohr on the question as to what extent atomic theory in its present form could be considered to be the final solution of the difficulties which had been discussed for several decades. We generally met already at breakfast in the hotel, and Einstein began to describe an ideal [*Gedanken*] experiment in which he thought the inner contradictions of the Copenhagen interpretation were especially clearly visible. Einstein, Bohr and I walked together from the hotel to the conference building, and I listened to the lively discussion between those two people whose philosophical attitudes were so different, and from time to time I added a remark on the structure of the mathematical formalism. During the meeting and particularly in the pauses we younger people, mostly Pauli and I, tried to analyze Einstein's experiment, and at lunch time the discussions continued between Bohr and the others from Copenhagen. Bohr had usually finished the complete analysis of the ideal experiment by late afternoon and would show it to Einstein at the supper table. Einstein had no good objection to this analysis, but in his heart he was not convinced. Bohr's friend Ehrenfest, who was also a close friend of Einstein, said to him, 'I am ashamed of you, Einstein! You put yourself here just in the same position as your opponents in their futile attempts to refute your relativity theory."

"Thus, by piecing together the contemporary documents (of 1927) with the later recollections of the participants, a fairly consistent historical picture of the great epistemological debate between Bohr and Einstein has arisen. The fifth Solvay Conference would not end this debate, however. Both participants returned to the problems involved again and again, especially at the sixth Solvay Conference in 1930 and, a few years later, in 1935. Still, quantum mechanics had already scored the main points in its favour. 'The most important success of the Brussels meeting was that we could see that against any objections, against any attempts to disprove the theory, we could get along with it,' Heisenberg summarized the result in an interview in 1963, and added: 'At that time [in 1927] it was practically Bohr, Pauli and myself, perhaps just the three of us. That very soon spread out'" (Mehra & Rechenberg, *The Historical Development of Quantum Theory*, vol. 6, pp. 232-256).

For an English translation and detailed analysis of the conference reports, see Bacciagaluppi & Valentini, *Quantum Theory at the Crossroads. Reconsidering the 1927 Solvay Conference*, Cambridge, 2009.

BOHR'S DISCUSSIONS WITH EINSTEIN

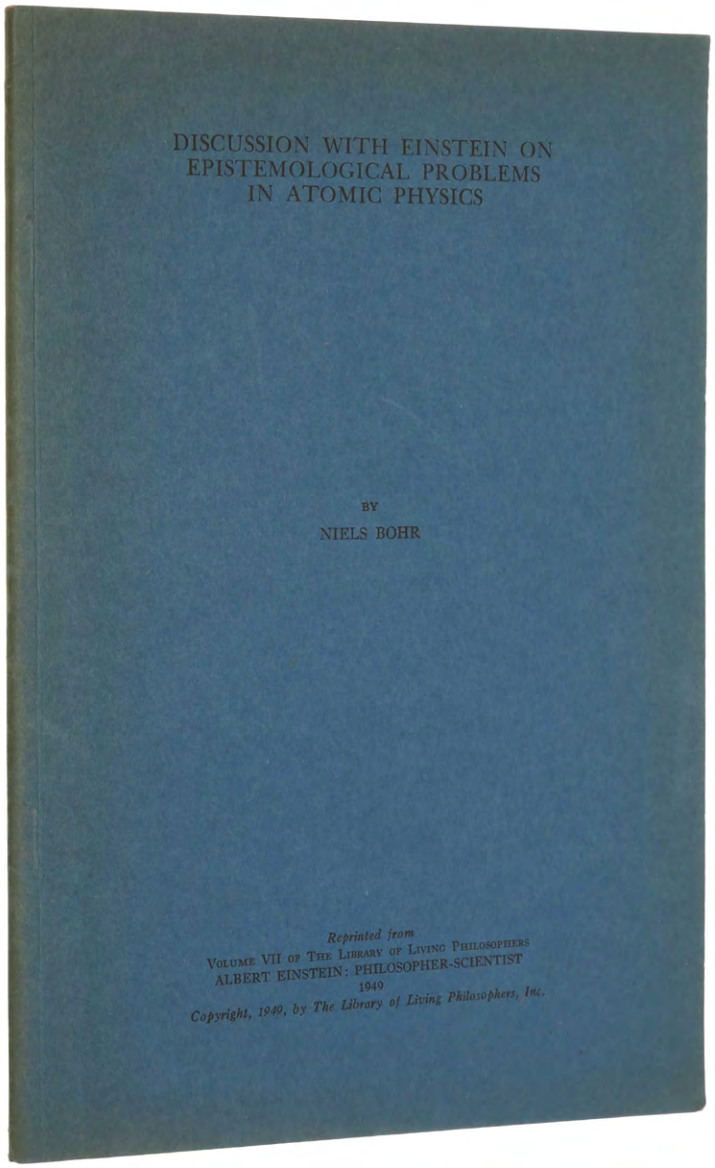
BOHR, Niels. *'Discussion with Einstein on Epistemological Problems in Atomic Physics.' Offprint from Albert Einstein: Philosopher-Scientist, Paul A. Schilpp (ed.).* Evanston, IL: The Library of Living Philosophers, 1949.

$1,500

8vo (232 x 155 mm), pp. 201-241. Original printed wrappers. Some very light sunning to wrappers, otherwise a very fine copy.

First edition, rare offprint issue, of Bohr's famous account of his decades-long debate with Einstein on the foundations of quantum physics, "one of the great scientific debates in the history of physics, comparable, perhaps, only to the Newton-Leibniz controversy of the early eighteenth century … In both cases it was a clash of diametrically opposed philosophical views … in both cases it was a clash between two of the greatest minds of their time" (Jammer, *Philosophy of Quantum Mechanics*, p. 120). "Bohr's account of his discussion with Einstein has been called 'one of the great masterpieces of modern scientific reporting.' According to Abraham Pais 'nowhere in the literature can a better access to [Bohr's] thinking be found'" (Jammer, p. 136). "It is generally agreed that Bohr's article of 1949 is his finest exposé on complementarity (Pais, *Niels Bohr's Times*, p. 428). The highlight of the paper, and of the Bohr-Einstein debate itself, was Bohr's refutation of Einstein's famous 'photon in a box' Gedankenexperiment.

The Bohr-Einstein debate began in 1920, when Bohr met Einstein in Berlin, and continued in 1927 at the fifth Solvay conference, where Einstein presented arguments to show that quantum mechanics was an inconsistent theory, arguments

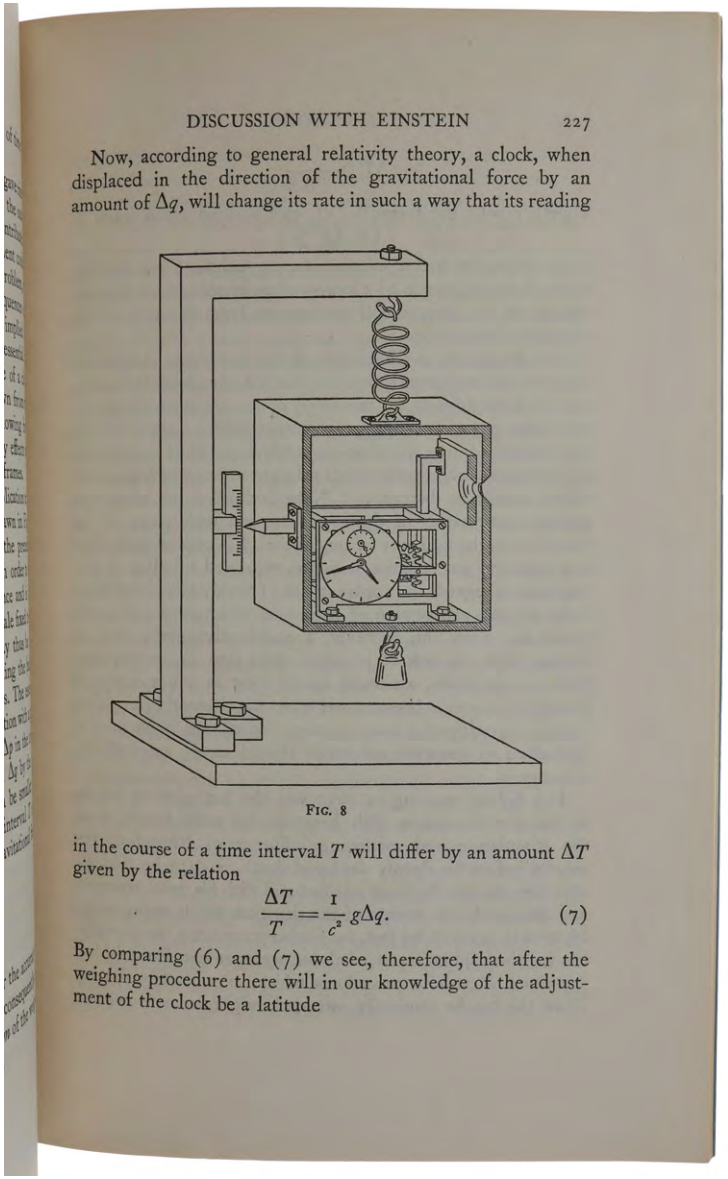


which Bohr refuted. At their next encounter, in 1930 at the following Solvay meeting in Brussels, Einstein informed Bohr that he had found a counterexample to the uncertainty relation between energy and time.

"The argument was quite ingenious. Consider a box having in one of its walls a hole that can be opened or closed by a shutter controlled by a clock inside the box. The box is filled with radiation. Weigh the box. Open the shutter for a brief interval during which a single photon escapes. Weigh the box again, some time later. From the weight difference one can deduce the energy of the photon by using $E = mc^2$. Thus (in principle) one has found to arbitrary accuracy both the photon energy and its time of passage, in conflict with the energy-time uncertainty relation.

"Rosenfeld who was in Brussels at that time has recalled: 'It was quite a shock for Bohr … he did not see the solution at once. During the whole evening he was extremely unhappy, going from one to the other and trying to persuade them that it couldn't be true, that it would be the end of physics if Einstein were right; but he couldn't produce any refutation. I shall never forget the sight of the two antagonists leaving [the Fondation Universitaire], Einstein a tall majestic figure, walking quietly, with a somewhat ironic smile, and Bohr trotting near him, very excited … The next morning came Bohr's triumph.'

"The flaw Bohr had found was that Einstein had not taken into account that the weighing process amounts to observing the displacement of the box in a gravitational field ... The imprecision in the displacement of the box generates an uncertainty in the determination of the mass, and hence of the energy, of the photon. When the box is displaced then so is the clock inside which therefore ticks in a gravitational field slightly different from the one in the initial box position. Now Bohr noted the well-known fact that the rate of a clock depends in a specific way on its position in a gravitational field. Correspondingly the imprecision in



DISCUSSION WITH EINSTEIN 227

Now, according to general relativity theory, a clock, when displaced in the direction of the gravitational force by an amount of Δq, will change its rate in such a way that its reading

FIG. 8

in the course of a time interval T will differ by an amount ΔT given by the relation

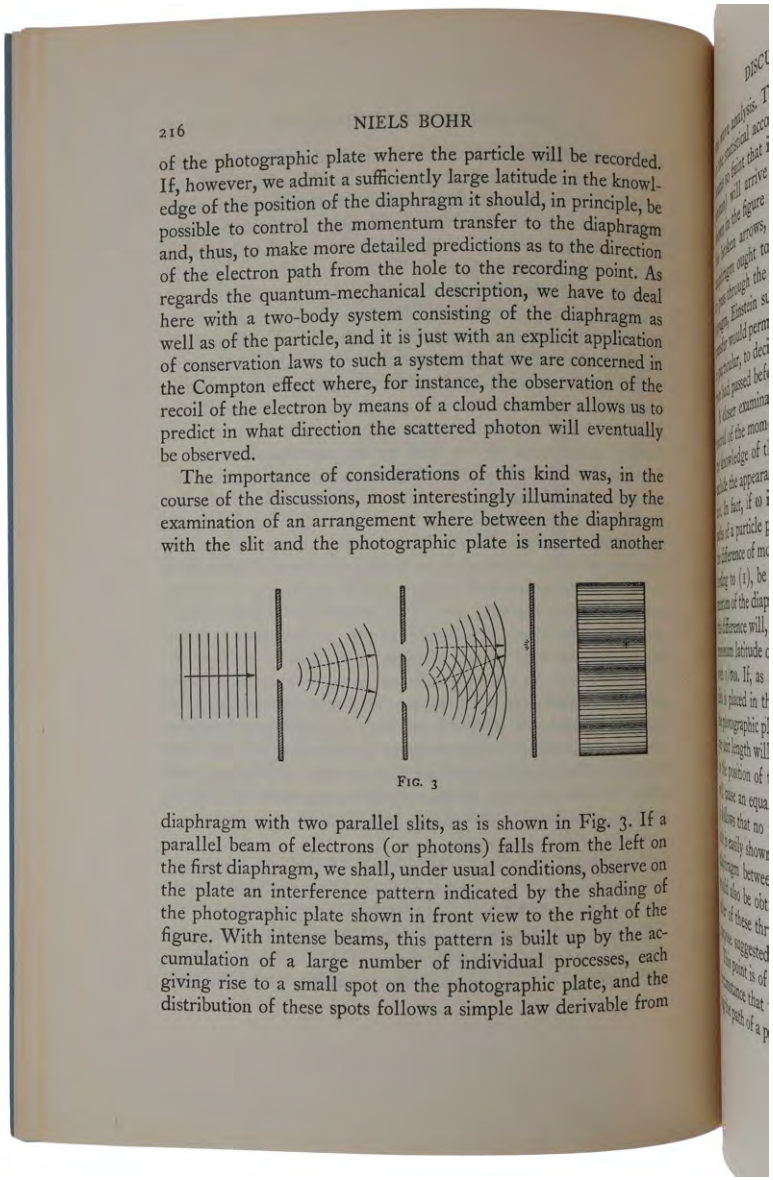
$$\frac{\Delta T}{T} = \frac{1}{c^2} g \Delta q. \qquad (7)$$

By comparing (6) and (7) we see, therefore, that after the weighing procedure there will in our knowledge of the adjustment of the clock be a latitude

the box displacement generates an uncertainty in the determination of time. Using the position-momentum uncertainty relation as input, Bohr could show that the uncertainties in energy and in time are just linked by the energy-time uncertainty relation. All was well.

"Bohr's version of the clock-in-the-box experiment was not published until 1949 when it appeared in a volume planned to be presented to Einstein on his 70th birthday" (Pais, pp. 427-8).

"This episode was one of the highlights of the Bohr-Einstein debate – and this not only because of the dramatic features involved. It was also a turning point in Einstein's attitude toward quantum mechanics. Accepting Bohr's counterargument – for what could have been nearer to his heart than his own red-shift formula – he gave up any hope of refuting the quantum theory on the grounds of an internal inconsistency. Instead … after the 1930 Solvay Congress Einstein concentrated on demonstrating the *incompleteness*, rather than the *inconsistency*, of quantum mechanics" (Jammer, p. 136).



216 NIELS BOHR

of the photographic plate where the particle will be recorded. If, however, we admit a sufficiently large latitude in the knowledge of the position of the diaphragm it should, in principle, be possible to control the momentum transfer to the diaphragm and, thus, to make more detailed predictions as to the direction of the electron path from the hole to the recording point. As regards the quantum-mechanical description, we have to deal here with a two-body system consisting of the diaphragm as well as of the particle, and it is just with an explicit application of conservation laws to such a system that we are concerned in the Compton effect where, for instance, the observation of the recoil of the electron by means of a cloud chamber allows us to predict in what direction the scattered photon will eventually be observed.

The importance of considerations of this kind was, in the course of the discussions, most interestingly illuminated by the examination of an arrangement where between the diaphragm with the slit and the photographic plate is inserted another

FIG. 3

diaphragm with two parallel slits, as is shown in Fig. 3. If a parallel beam of electrons (or photons) falls from the left on the first diaphragm, we shall, under usual conditions, observe on the plate an interference pattern indicated by the shading of the photographic plate shown in front view to the right of the figure. With intense beams, this pattern is built up by the accumulation of a large number of individual processes, each giving rise to a small spot on the photographic plate, and the distribution of these spots follows a simple law derivable from

BOOLEAN ALGEBRA

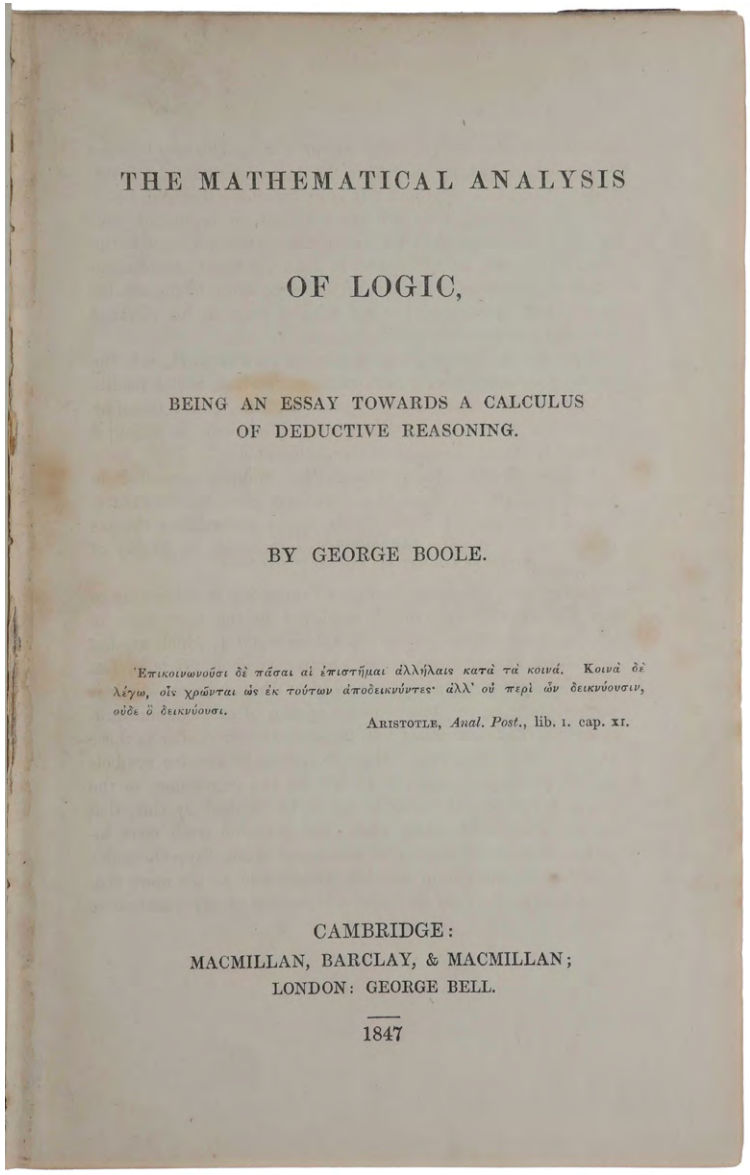
Landmark Writings in Western Mathematics, 36.

BOOLE, George. *The mathematical analysis of logic, being an essay towards a calculus of deductive reasoning.* Cambridge: Macmillan, Barclay & Macmillan, 1847.

$48,000

8vo (212 x 135 mm), pp. [ii], [1-2], 3-82, errata slip tipped onto title verso, interleaved with blanks throughout. Later nineteenth-century half-calf and marbled boards, spine ruled and lettered in gilt (spine and corners worn, lower 15 mm of spine lacking).

First edition, very rare in commerce, of Boole's first book, the birth of modern symbolic logic and the first presentation of 'Boolean algebra' – this is the copy of the great economist John Maynard Keynes (1883-1946). "Boole's work also contains what Bertrand Russell called the greatest discovery of the nineteenth century: the nature of pure mathematics" (OOC). "Self-taught mathematician George Boole (1815–1864) published a pamphlet in 1847 – *The Mathematical Analysis of Logic* – that launched him into history as one of the nineteenth century's most original thinkers" (Introduction to the CUP reprint). This copy is interleaved throughout and has many pencil annotations, both in the text itself and on the inserted blanks. We have not been able to determine who was responsible for the annotations, but they may derive, at least in part, from annotations made by Boole who prepared at least two interleaved copies himself (see below). "*The Mathematical Analysis of Logic* marks the beginning of symbolic logic in the modern sense. Boole showed that classical logic was actually a branch of mathematics which gave rise to a



THE MATHEMATICAL ANALYSIS

OF LOGIC,

BEING AN ESSAY TOWARDS A CALCULUS
OF DEDUCTIVE REASONING.

BY GEORGE BOOLE.

Ἐπικοινωνοῦσι δὲ πᾶσαι αἱ ἐπιστῆμαι ἀλλήλαις κατὰ τὰ κοινά. Κοινὰ δὲ λέγω, οἷς χρῶνται ὡς ἐκ τούτων ἀποδεικνύντες· ἀλλ' οὐ περὶ ὧν δεικνύουσιν, οὐδὲ ὃ δεικνύουσι.

ARISTOTLE, *Anal. Post.*, lib. I. cap. XI.

CAMBRIDGE:
MACMILLAN, BARCLAY, & MACMILLAN;
LONDON: GEORGE BELL.

1847

hitherto unconsidered type of algebra. Boole's book however went considerably further. It threw a great deal of light on the nature of pure mathematics; it opened up possibilities of an extension of the subject into totally new and unexpected areas – classical mathematics had concentrated on the notions of shape and number and even when symbols were employed, they were generally interpreted in terms of number. Boole had now introduced the notion of interpreting symbols as classes or sets of objects, a concept breathtaking in its scope because it meant that the study of all well-defined sets of objects now came under the realm of mathematics … By enlarging the horizons of mathematics so enormously, Boole unwittingly (but perhaps subconsciously, wittingly) highlighted a topic that has come to influence virtually every aspect of present-day life – the storage and processing of information, which in turn has led to the development of computer science. Not only is Boole's algebra the 'correct' and most economical tool for handling information, but the electronic machines which now do the work actually operate according to principles determined by that self-same algebra. Boole has been called the 'Father of Symbolic Logic' and the 'Founder of Pure Mathematics', but he is just as deserving of the title, 'Father of Computer Science'" (MacHale, p. 82). ABPC/RBH list only two copies since Honeyman: the OOC copy, Christie's 2005, $10,800, and Bonham's 2013, £25,000 = $38,000. Both of these copies were in modern bindings (the OOC copy with the original front wrapper bound in).

Provenance: H. A. Routledge (signature on front free endpaper, dated June 1955); John Maynard Keynes (1883-1946) (note in Routledge's hand on front free endpaper: 'From Lord Keynes' Collection'). We understand from the previous owner that Keynes inherited the book from his father John Neville Keynes (1852-1949). Boole collected ideas for the improvement of his *Mathematical analysis of logic* on interleaved copies of the work; these notes provided a source for his later book *Laws of thought* (1854). Two such copies are known: one is in the library of the Royal Society of London with annotations entirely in Boole's hand; the other is in possession of Dr. J. Hinton (a great-great grandson of Boole), which has annotations partly in Boole's hand and partly in the hand of an unidentified amanuensis (see Smith, pp. 27-8). The annotations in our interleaved copy are not in Boole's hand (nor are they in Keynes' hand), but it is possible that they derive from Boole as do some of the annotations in the Hinton copy.

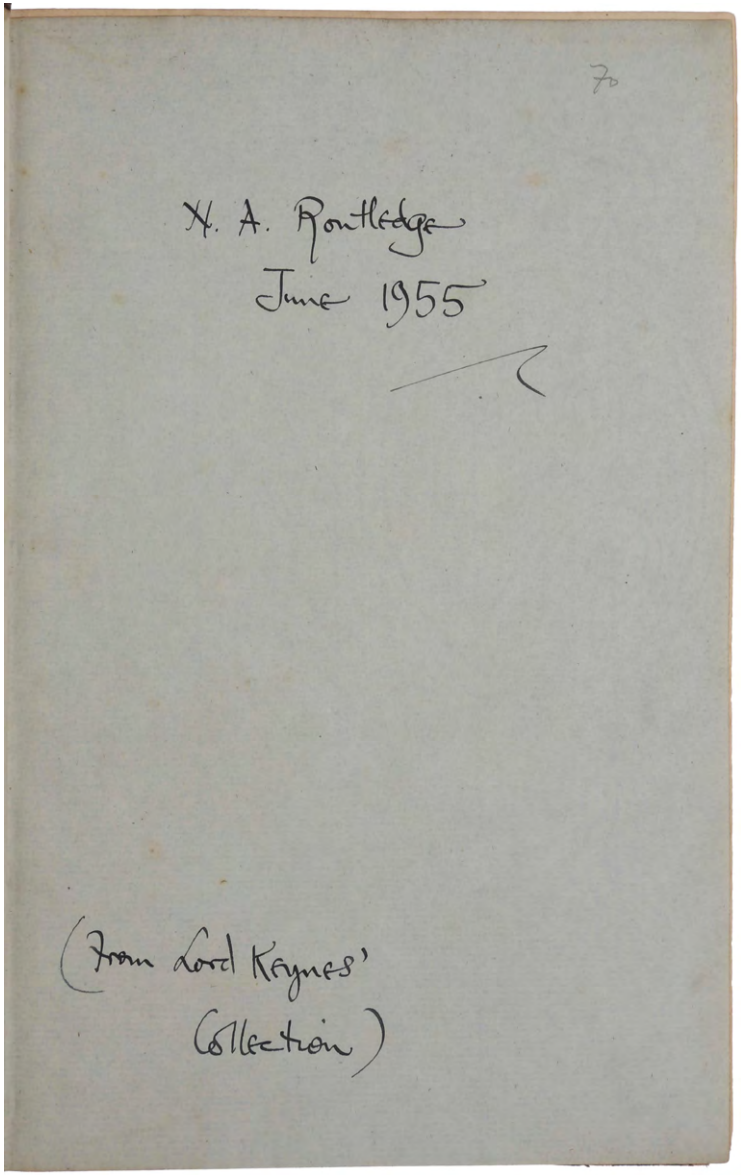
"Boole's contribution to logics made possible the works of subsequent logicians including Turing and Von Neumann … Even Babbage depended a great deal on Boole's ideas for his understanding of what mathematical operations really are … Since Boole showed that logics can be reduced to very simple algebraic systems – known today as Boolean Algebras – it was possible for Babbage and his successors to design organs for a computer that could perform the necessary logical tasks. Thus our debt to this simple, quiet man, George Boole, is extraordinarily great … His remark about a 'special law to which the symbols of quantity are not subject' is very important: this law in effect is that $x^2 = x$ for every x in his system. Now in numerical terms this equation or law has as its only solution 0 and 1. This is why the binary system plays so vital a role in modern computers: their logical parts in effect carrying out binary operations. In Boole's system 1 denotes the entire realm of discourse, the set of all objects being discussed, and 0 the empty set. There are two operations in this system which we may call + and ×; or we may say *or* and *and*. It is most fortunate for us that all logics can be comprehended in so simple a system, since otherwise the automation of computation would probably not have occurred – or at least not when it did" (Goldstine, pp. 37-38).

"Early in the spring of 1847, Boole's long-dormant interest in the connections between mathematics and logic was dramatically reawakened. At this time, a furious controversy was raging between the supporters of de Morgan and those of Sir William Hamilton, the Scottish philosopher and metaphysician (not to

be confused with Sir William Rowan Hamilton). Hamilton was a logician who distrusted mathematics, but he was an innovator in logic and had, about this time, introduced the notion of 'quantification of the predicate' which was to lead to a widening of the scope of logic. Classical logic had concentrated on the 'four forms' of statement — all A are B, no A are B, some A are B, some A are not B. In Hamilton's approach, the predicate, or second term B, is quantified by considering statements of the type: all A are all B, any A is not some B, and so on. De Morgan too was at this time working on a more mathematical theory of logic which included a notion equivalent to quantification of the predicate. Hamilton at once accused de Morgan of plagiarism, despite the fact that the notion in question was not original to either of them nor, as it transpired, of any great significance *per se* in the development of logic. Hamilton's charges were unjust, even absurd, but controversy raged for many years and attracted a great deal of attention …

"From his neutral position, Boole was able to judge the merits and defects of the approaches of both Hamilton and de Morgan. Though Hamilton disliked mathematics and even poured scorn on the subject, yet his approach seemed to suggest that logic should concentrate on 'equations' connecting 'collections of objects or classes'. De Morgan's approach, on the other hand, seemed to concentrate on a purely symbolic representation of logical processes, yet his notation was cumbersome and unwieldy. Why not, thought Boole, synthesise the two approaches by representing each class of objects by a single symbol and allow relations between classes to be expressed by algebraic equations between the symbols? This devastatingly simple but ingenious notion intrigued and excited him and he set to work at once on a book expounding a new mathematical theory of logic" (MacHale, p. 79-80).

"The priority dispute triggered Boole to write his first book; but its content was much influenced by his researches on differential operators. Partly drawing



upon the work of his friend the Cambridge mathematician Duncan Gregory, he produced a long paper on these methods which he submitted to the Royal Society in 1844. After wondering about rejecting the manuscript they published it ['On a general method in analysis', *Philosophical Transactions*, Vol. 134, pp. 225–282], and then awarded him a Gold Medal for his achievement!

"This theory was one of the early algebras in which the 'objects' were neither numbers nor geometrical magnitudes; and it had met controversy in its algebraic laws, such as identifying powers with orders (that is, D^2 for D on D, not D times D). Aware of the mystery, Boole (and before him Gregory) tried to bring light by highlighting the principal desirable properties … and using them in solving various differential equations" (*Landmark Writings*, p. 472).

"Boole wrote his book at a furious pace and it was ready in a matter of months. He entitled it *The Mathematical Analysis of Logic, being an Essay Towards a Calculus of Deductive Reasoning* [*MAL*] … The book was then published by Macmillan, Barclay and Macmillan of Cambridge, with a preface dated 29 October 1847" (MacHale, p. 81).

"The Introduction chapter starts with Boole reviewing the symbolical method. The second chapter, First Principles, lets the symbol 1 represent the universe which "comprehends every conceivable class of objects, whether existing or not." Capital letters *X, Y, Z,* … denoted classes. Then, no doubt heavily influenced by his very successful work using algebraic techniques on differential operators, and consistent with De Morgan's 1839 assertion that algebraists preferred interpreting symbols as operators, Boole introduced the elective symbol x corresponding to the class X, the elective symbol $\underline{y}$ corresponding to Y, etc. The *elective symbols* denoted election operators—for example the election operator red when applied to a class would elect (select) the red items in the class …

"Then Boole introduced the first operation, the multiplication xy of elective symbols. The standard notation xy for multiplication also had a standard meaning for operators (for example, differential operators), namely one applied y to an object and then x is applied to the result. (In modern terminology, this is the composition of the two operators) …

"The first law in *MAL* was the distributive law $x(u+v) = xu + xv$, where Boole said that $u+v$ corresponded to dividing a class into two parts. This was the first mention of addition. On p. 17 Boole added the commutative law $xy = yx$ and the idempotent law $x^2 = x$ (which Boole called the index law). Once these two laws were secured, Boole believed he was entitled to fully employ the ordinary algebra of his time, and indeed one sees Taylor series and Lagrange multipliers in *MAL*. The law of idempotent class symbols, $x^2 = x$, was different from the two fundamental laws of symbolical algebra—it only applied to the individual elective symbols, not in general to compound terms that one could build from these symbols. For example, one does not in general have $(x+y)^2 = x+y$ in Boole's system since, by ordinary algebra with idempotent class symbols, this would imply $2xy = 0$, and then $xy = 0$, which would force x and y to represent disjoint classes. But it is not the case that every pair of classes is disjoint.

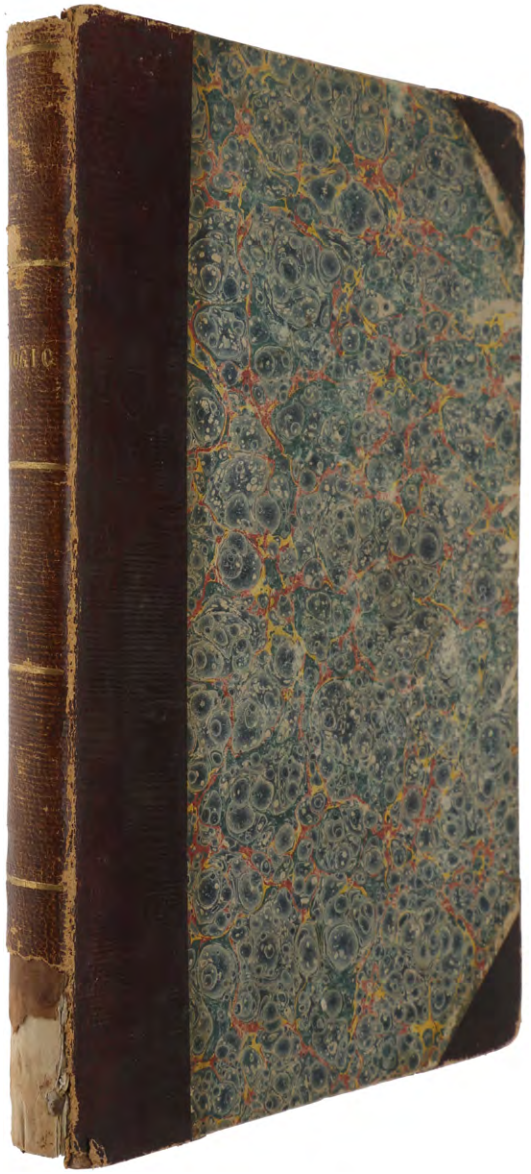
"Boole focused on Aristotelian logic in *MAL*, with its 4 types of categorical propositions and an open-ended collection of hypothetical propositions. In the chapter 'Of Expression and Interpretation,' Boole said that necessarily the class not-X is expressed by $1-x$. This is the first appearance of *subtraction*. Then he gave equations to express the categorical propositions. The first to be expressed was All X is Y, for which he used $xy = x$, which he then converted into $x(1-y) = 0$. This was the first appearance of 0 in *MAL*—it was not introduced as the symbol for the empty class. Indeed the empty class did not appear in *MAL* …

"Beginning with the chapter Properties of Elective Functions, Boole developed general theorems for working with equations in his algebra of logic—the Expansion Theorem and the properties of constituents are discussed in this chapter. Up to this point his sole focus was to show that Aristotelian logic could be handled by simple algebraic methods, mainly through the use of an elimination theorem borrowed from ordinary algebra.

"It was natural for Boole to want to solve equations in his algebra of logic since this had been a main goal of ordinary algebra, and had led to many difficult questions (e.g., how to solve a 5th degree equation). Fortunately for Boole, the situation in his algebra of logic was much simpler—he could always solve an equation, and finding the solution was important to applications of his system, to derive conclusions in logic. An equation was solved in part by using expansion after performing division. This method of solution was the result of which he was the most proud—it described how to solve an elective equation for one of its symbols in terms of the others, and it is this that Boole claimed (in the Introduction chapter of *MAL*) would offer "the means of a perfect analysis of any conceivable set of propositions"" (*Stanford Encyclopedia of Philosophy*).

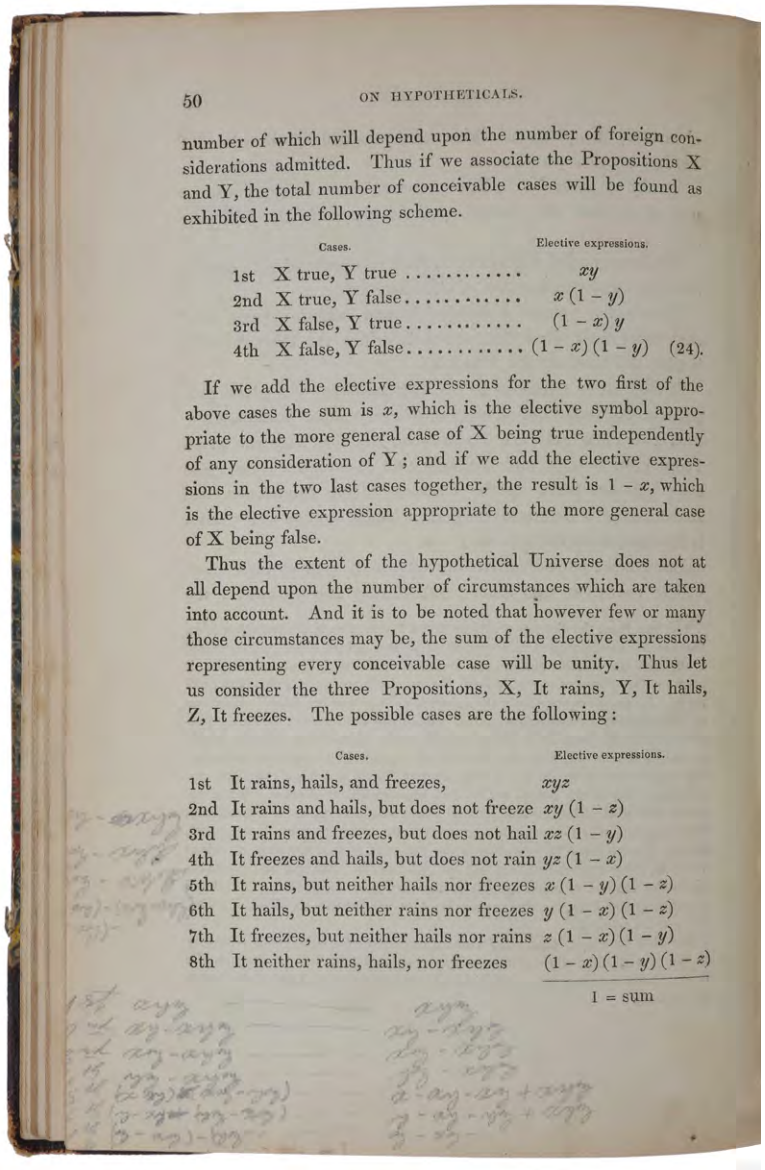
Seven years after publishing the present work, Boole gave a more elaborate treatment of Boolean algebra in *An Investigation of the Laws of Thought*. His aim was partly to address criticisms of the earlier work by de Morgan and others; he also considered the application of his theory to probability, a topic not treated in the earlier work.

"The range and depth of the achievements of George Boole are especially remarkable when one notes not only the shortness of his life but also the disadvantageous circumstances of his background. He was born to an intelligent tradesman who however was so poor that George had to become the main



breadwinner in his 20th year when he opened a school. Nevertheless, he found time to teach himself advanced mathematics, and also Greek, Latin, French and German, especially in order to read important works. His research papers began to appear in the early 1840s, and his principal interest soon turned to an English specialty: the 'calculus of operations', now called 'differential operators', where differentiation was represented by the letter 'D', higher-order differentiation by 'D^2, D^3, . . .', integration by 'D^{-1}', and so on. This tradition had developed under the influence of the algebraised calculus propounded by J. L. Lagrange, initially by some French mathematicians; but from the 1810s this algebra and related topics were prosecuted in England by Charles Babbage and John Herschel as part of the revival of research mathematics there. Boole was to become a major figure in this movement in the next generation; as we have seen, it was to affect his work on logic" (*Landmark Writings*, pp. 470-471).

Goldstine, *The Computer from Pascal to von Neumann*, 1972. MacHale, *The Life and Work of George Boole. A Prelude to the Digital Age*, 2014. *Landmark Writings in Western Mathematics 1640-1940*, 2005 (Chapter 36). Smith, 'Boole's Annotations on 'The Mathematical Analysis of Logic'', *History and Philosophy of Logic 4* (1983), 27-39.



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number of which will depend upon the number of foreign considerations admitted. Thus if we associate the Propositions X and Y, the total number of conceivable cases will be found as exhibited in the following scheme.

	Cases.	Elective expressions.
1st	X true, Y true	xy
2nd	X true, Y false	$x(1-y)$
3rd	X false, Y true	$(1-x)y$
4th	X false, Y false	$(1-x)(1-y)$ (24).

If we add the elective expressions for the two first of the above cases the sum is x, which is the elective symbol appropriate to the more general case of X being true independently of any consideration of Y; and if we add the elective expressions in the two last cases together, the result is $1-x$, which is the elective expression appropriate to the more general case of X being false.

Thus the extent of the hypothetical Universe does not at all depend upon the number of circumstances which are taken into account. And it is to be noted that however few or many those circumstances may be, the sum of the elective expressions representing every conceivable case will be unity. Thus let us consider the three Propositions, X, It rains, Y, It hails, Z, It freezes. The possible cases are the following:

	Cases.	Elective expressions.
1st	It rains, hails, and freezes,	xyz
2nd	It rains and hails, but does not freeze	$xy(1-z)$
3rd	It rains and freezes, but does not hail	$xz(1-y)$
4th	It freezes and hails, but does not rain	$yz(1-x)$
5th	It rains, but neither hails nor freezes	$x(1-y)(1-z)$
6th	It hails, but neither rains nor freezes	$y(1-x)(1-z)$
7th	It freezes, but neither hails nor rains	$z(1-x)(1-y)$
8th	It neither rains, hails, nor freezes	$(1-x)(1-y)(1-z)$
		1 = sum

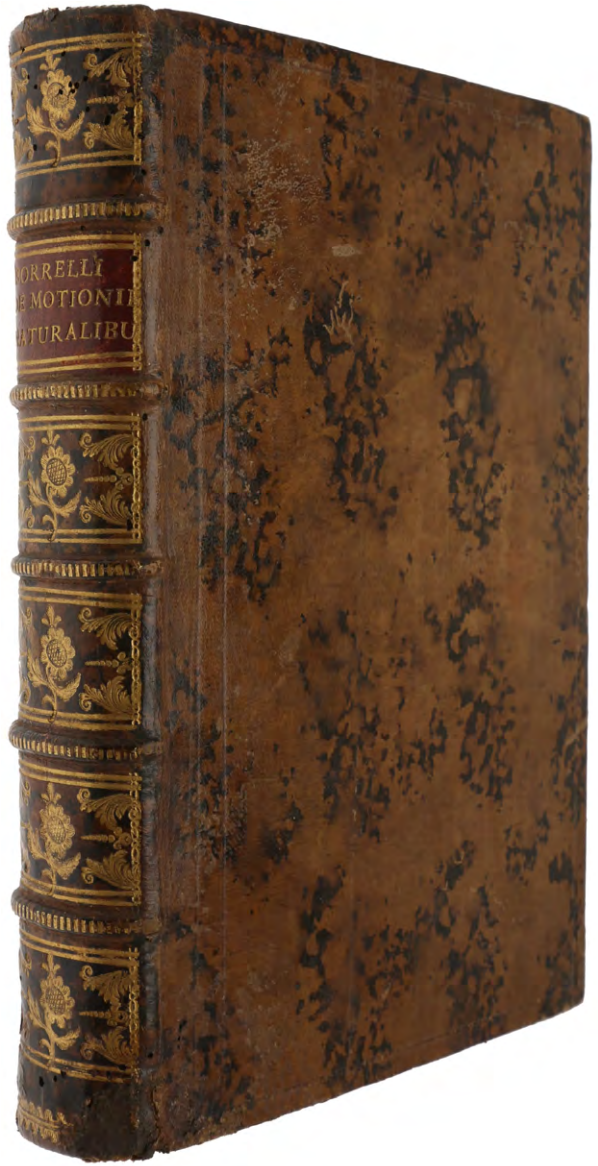
THE FIRST TREATISE ON CAPILLARITY

BORELLI, Giovanni Alfonso. *De motionibus naturalibus a gravitate pendentibus.* Bologna: Dominici Ferri, 1670.

$8,500

4to (228 x 154 mm). Half-title, woodcut device on title, woodcut diagrams in text. A few minor stains, some light finger soiling, occasional small marginal tears, Tt1 with two paper flaws outside of the text, 18th-century mottled calf, spine gilt (small wormholes in spine, foot of spine lightly rubbed).

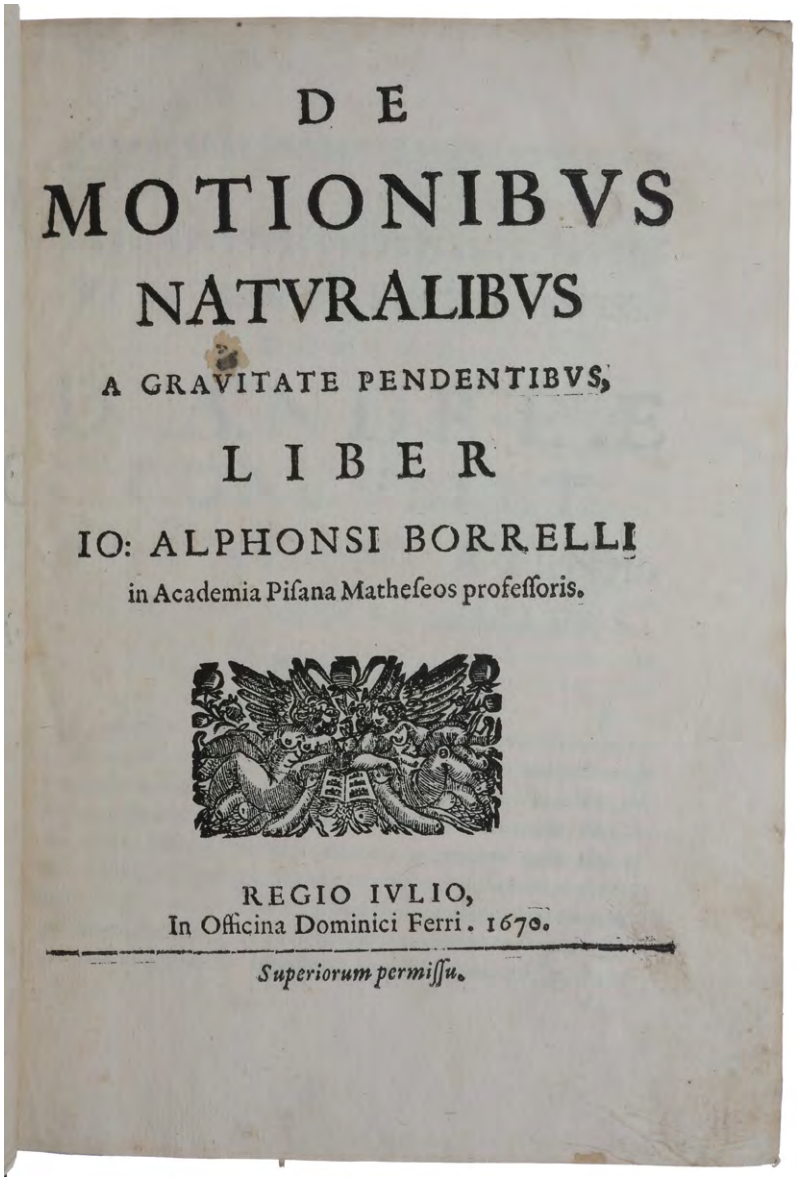
First edition and a fine copy, of one of the most important early treatises on fluid mechanics. In this work Borelli "argues against positive levity, discusses the Torricellian experiment, takes up siphons, pumps, and the nature of fluidity, tries to understand the expansion of water while freezing, and deals with fermentation and other chemical processes" (DSB). "This work is important as the first treatise on capillarity, and for containing important investigations on the action of capillary tubes, in which the author, *inter alia*, formulates the law that the height of the ascent of liquids in capillary tubes is inversely proportional to their diameters. His investigations also led him to the conclusion that the phenomenon of capillarity is independent of the pressure of air" (Zeitlinger in Sotheran, *First supplement*, 3060). "[*De motionibus naturalibus*] was well known among Borelli's contemporaries and is quoted by Varignon in his *Projet d'une nouvelle mechanique*. It is reviewed in the first volume of *Philosophical Transactions Abridged*, where it is praised for its thoroughness, for the discussion of Torricelli's experiments and for Borelli's stand against Descartes on the nature of fluidity" (Roberts & Trent). Borelli regarded this work, together with his *De vi percussionis* (1667),



as necessary preparation for his masterpiece, *De motum animalium* (1680-81), on which he had worked since the early 1660s. The work appeared in three subsequent editions, testifying to its influence.

"While *On the Force of Percussion* laid out some foundational propositions relating to motion and colliding bodies, the subsequent work, *On the Natural Motions*, was intended to be far more wide reaching … In the concluding propositions to *On the Force of Percussion*, Borelli dismisses Aristotle's views on falling bodies. Borelli begins *On the Natural Motions* in that same pose. He attacks Aristotle for supposing that elements possess either heaviness or lightness. According to Aristotle, positive levity (referring to the supposed intrinsic lightness of bodies that allows them to rise rather than fall in a natural state) is a quality of air and fire. Earth and water, on the other hand, contain a gravity that impels them towards their natural place at the centre of the earth. Borelli makes it clear from the outset of this second book that he is determined to dispel these views" (Boschiero).

"Giovanni Alfonso Borelli, while embracing the doctrine of corpuscles in motion …, uniquely sought to constrain it by means of mathematics. *In De motionibus naturalibus a gravitate pendentibus* he was concerned 'to show that all events in the world of body are up for explanation by the force of heaviness only, if only one attributes fitting shapes to the particles of bodies, so that the force of heaviness, directed toward the center of the Earth, compels them according to mechanical laws [i.e., those of statics] to move in other directions, too, depending on circumstance. He therefore regards the corpuscles as machines, which are moved by the force yet determined as to direction by their construction. Since the only driving force is thus the pressure of bodies upon others and themselves, he conceives of the totality of movements on Earth as a problem in hydrostatics'" (Cohen, pp. 393-4).

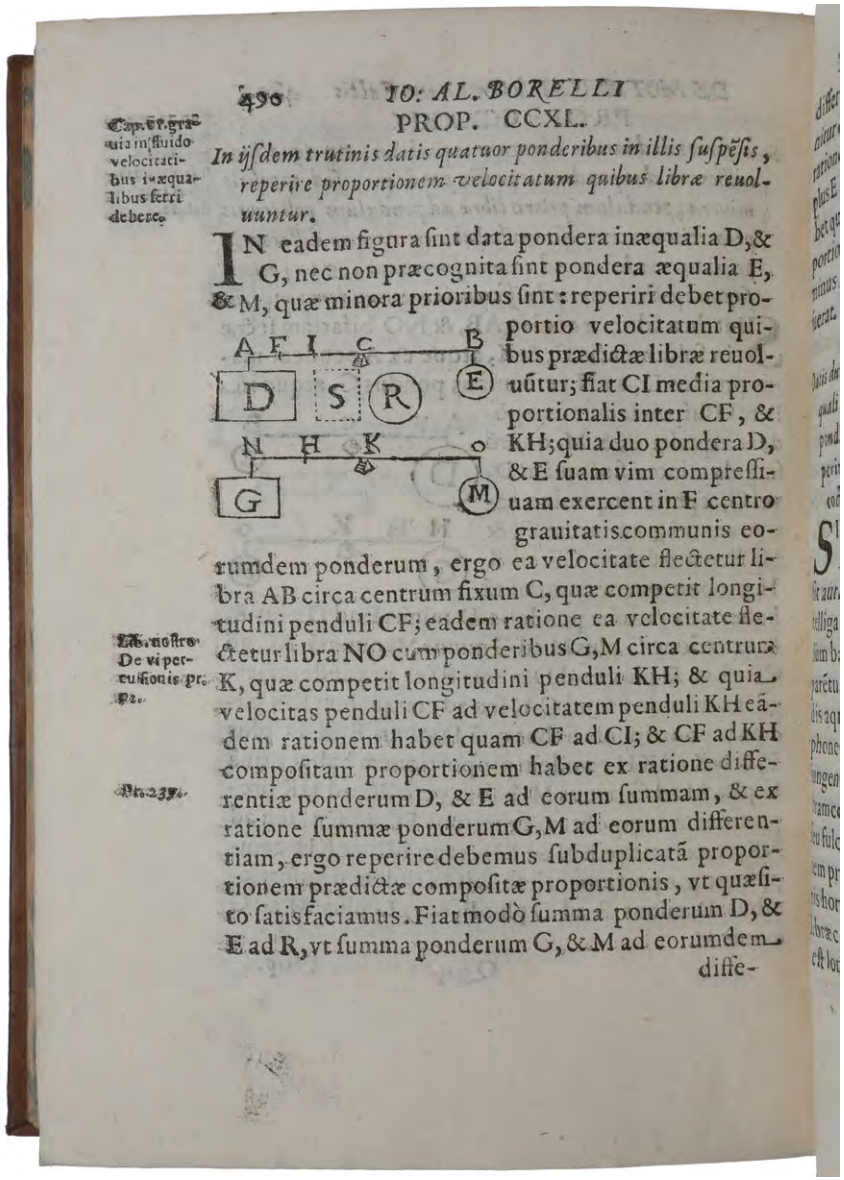

DE
MOTIONIBVS
NATVRALIBVS
A GRAVITATE PENDENTIBVS,
LIBER
IO: ALPHONSI BORRELLI
in Academia Pisana Matheseos professoris.

REGIO IVLIO,
In Officina Dominici Ferri. 1670.
Superiorum permissu.

"Borelli's analysis of the collision of elastic bodies relies on a notion of the spring-like and machine-like properties of some parts of nature. For example, air particles impact upon each other, transmitting a motive virtue one upon the other that is maintained through the compressive properties of these corpuscles. In other words, each corpuscle of air, when impacted, compresses, moves as a result of its attained motive virtue and recoils in order to re-acquire its lost space before impacting onto another particle. In such an environment, less dense particles of fire, with their own shapes and properties inconsistent with those of air, are pushed upwards. Thus, Borelli debunks Aristotle's theory of positive levity, which attributes motion only to intrinsic qualities of nature rather than extrinsic forces proportional to elastic properties of corpuscles. To illustrate his point, on several occasions Borelli discusses the 'structure of air': *It is . . . reasonable to assign to the particles of air a structure which forces them to unfold when they are constricted against their natural requirement. We understand this if we conceive that the substance of air consists of countless juxtaposed small machines. Then we clearly perceive that an elastic virtue can be found in this aggregate since the small machines attempt at dilating after being compressed* (Prop. XCCIII).

"After covering further ground with the description of theories and experiments to do with the vacuum, the Torricellian tube and the freezing process of liquids, Borelli reaches his conclusions reminding his readers of the final destination of this project: *This is enough on the motions resulting from the innate force of gravity. We indeed do not consider extending further these preliminaries nor delaying the edition of the main argument on the movements of animals longer* (Prop. CCLXXVII)" (Boschiero).

Born in Naples, Giovanni Borelli (1608-79) studied mathematics at Rome under Benedetto Castelli. Sometime before 1640 he was appointed professor of mathematics at Messina. In the early 1640s, he met Galileo in Florence. In 1656



490 IO: AL. BORELLI

PROP. CCXL.

In ijſdem trutinis datis quatuor ponderibus in illis ſuſpẽſis, reperire proportionem velocitatum quibus libræ reuoluuntur.

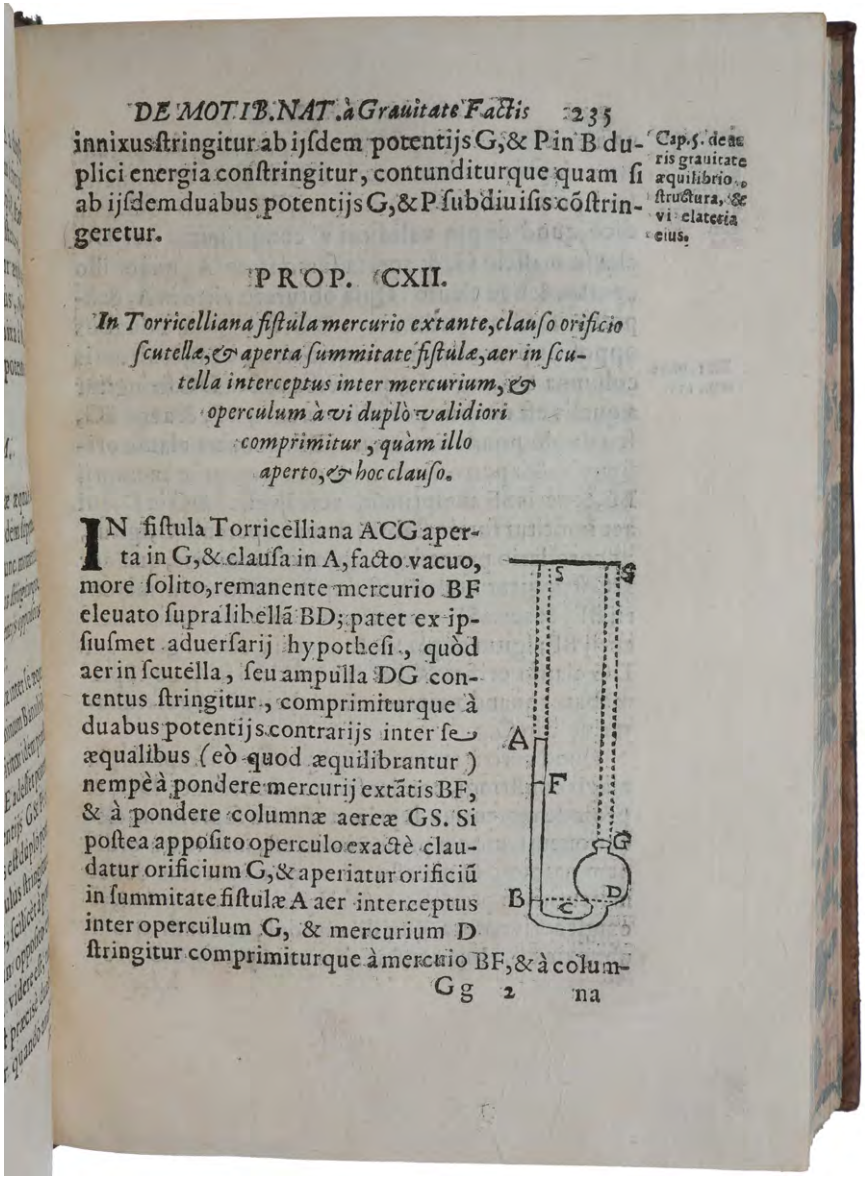
IN eadem figura ſint data pondera inæqualia D, & G, nec non præcognita ſint pondera æqualia E, & M, quæ minora prioribus ſint: reperiri debet proportio velocitatum quibus prædictæ libræ reuoluũtur; fiat CI media proportionalis inter CF, & KH; quia duo pondera D, & E ſuam vim compreſſiuam exercent in F centro grauitatis communis eorumdem ponderum, ergo ea velocitate flectetur libra AB circa centrum fixum C, quæ competit longitudini penduli CF; eadem ratione ea velocitate flectetur libra NO cum ponderibus G, M circa centrum K, quæ competit longitudini penduli KH; & quia velocitas penduli CF ad velocitatem penduli KH eãdem rationem habet quam CF ad CI; & CF ad KH compoſitam proportionem habet ex ratione differentiæ ponderum D, & E ad eorum ſummam, & ex ratione ſummæ ponderum G, M ad eorum differentiam, ergo reperire debemus ſubduplicatã proportionem prædictæ compoſitæ proportionis, vt quæſito ſatisfaciamus. Fiat modò ſumma ponderum D, & E ad R, vt ſumma ponderum G, & M ad eorumdem

diffe-

Borelli was appointed to the chair of mathematics at the University of Pisa, a post previously held by Galileo. It was in Pisa that Borelli met the Italian anatomist Marcello Malpighi; the two men became founder members of the short-lived *Accademia del Cimento*. Motivated by Malpighi's own studies, Borelli began his first investigations into the science of animal movement. This began an interest that would continue for the rest of his life, eventually earning him the title of the Father of Biomechanics.

In May 1665, Cardinal Michelangelo Ricci, Roman correspondent and adviser to the Tuscan Court, wrote to Prince Leopoldo de Medici, the patron of the Cimento, encouraging Borelli to apply himself to the composition of a treatise on motion. Borelli's initial response was that he was instead concentrating on a treatise on anatomy, but, seemingly at Ricci's insistence, Borelli eventually agreed, the result being the two works *De vi percussionis and De motionibus naturalibus*. Both works were composed, or at least drafted, before his departure from the Medici court in 1667, when he returned to Messina. At this point the Cimento effectively ceased to function, even though it apparently was not formally dissolved, and even though Prince, now Cardinal, Leopold continued to direct some experimental work until he died in 1675.

"On the way [to Messina, Borelli] passed through Rome and stopped for the summer in Naples. While there he was the guest of the Investiganti for whom he repeated many of the experiments he had performed at the Cimento. And he also repeated for his own edification some work that the Investiganti had accomplished independently. As a result of this visit, Concublet provided for the publication of *De motionibus naturalibus*, for which Borelli reciprocated by writing a warm dedication to him" (DSB). The *Accademia degli Investiganti* was founded in 1663 by Leonardo di Capua, under the patronage of Andrea Concublet, Marquis of Arena.



DE MOT. IB. NAT. à Grauitate Factis 235

innixus ſtringitur ab ijſdem potentijs G, & P in B duplici energia conſtringitur, contunditurque quam ſi ab ijſdem duabus potentijs G, & P ſubdiuiſis cõſtringeretur.

Cap. 5. de æris grauitate æquilibrio, ſtructura, & vi elaterea eius.

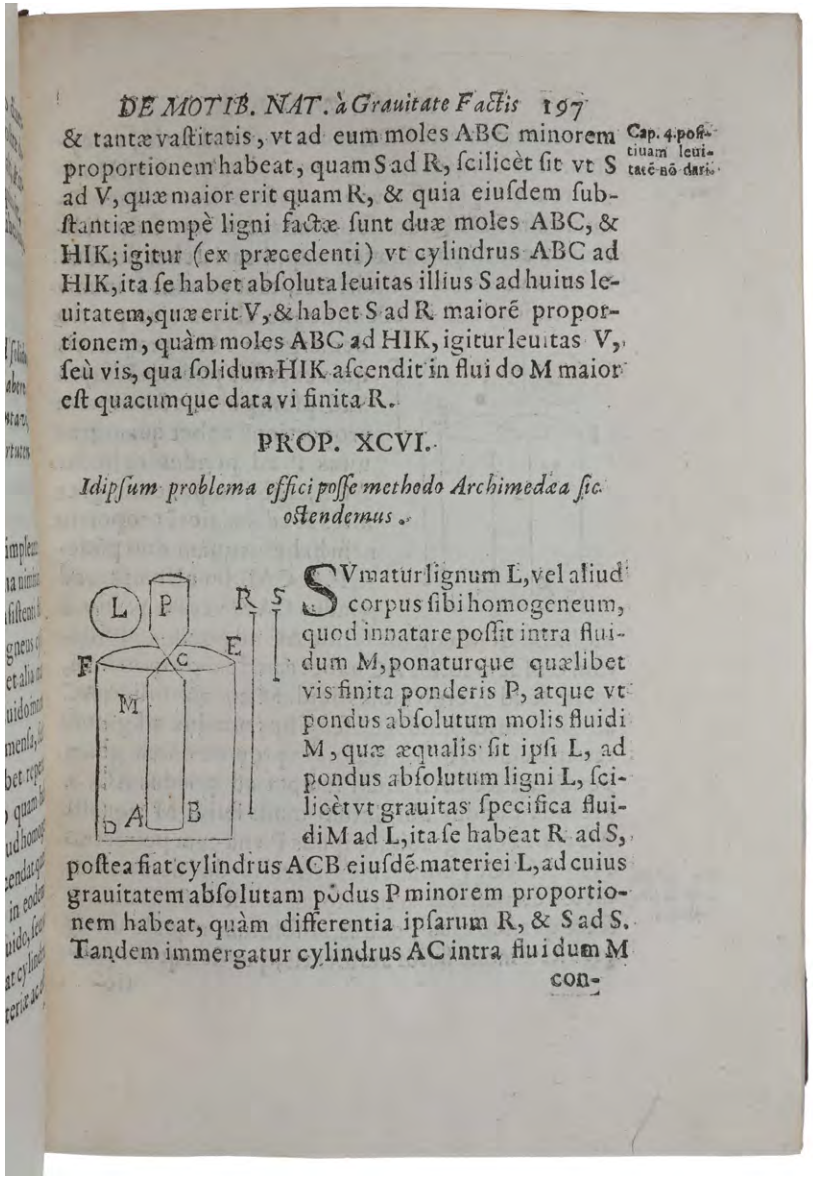
PROP. CXII.

In Torricelliana fiſtula mercurio extante, clauſo orificio ſcutellæ, & aperta ſummitate fiſtulæ, aer in ſcutella interceptus inter mercurium, & operculum à vi duplò validiori comprimitur, quàm illo aperto, & hoc clauſo.

IN fiſtula Torricelliana ACG aperta in G, & clauſa in A, facto vacuo, more ſolito, remanente mercurio BF eleuato ſupra libellã BD; patet ex ipſiuſmet aduerſarij hypotheſi, quòd aer in ſcutella, ſeu ampulla DG contentus ſtringitur, comprimiturque à duabus potentijs contrarijs inter ſe æqualibus (eò quod æquilibrantur) nempè à pondere mercurij extãtis BF, & à pondere columnæ aereæ GS. Si poſtea appoſito operculo exactè claudatur orificium G, & aperiatur orificiũ in ſummitate fiſtulæ A aer interceptus inter operculum G, & mercurium D ſtringitur comprimiturque à mercurio BF, & à colum-

Gg 2 na

DSB II.306-14; Carli-Favaro 78 (329); Cinti 291 (147); De Caro 54; Krivatsy 1577; Riccardi I.159-60; Roberts and Trent p. 42; Wolf, *History of science, technology, and philosophy I* pp. 58-60. L. Boschiero, *Introduction to Borelli's On the Movement of Animals – On the Natural Motions Resulting from Gravity*, P. Maquet (tr.), 2015; H. F. Cohen, *How Modern Science Came into the World: Four Civilizations, One 17th Century Breakthrough*, 2014.



DE MOTIB. NAT. à Grauitate Factis 197

& tantæ vastitatis, vt ad eum moles ABC minorem proportionem habeat, quam S ad R, scilicèt sit vt S ad V, quæ maior erit quam R, & quia eiusdem substantiæ nempè ligni factæ sunt duæ moles ABC, & HIK; igitur (ex præcedenti) vt cylindrus ABC ad HIK, ita se habet absoluta leuitas illius S ad huius leuitatem, quæ erit V, & habet S ad R maiorē proportionem, quàm moles ABC ad HIK, igitur leuitas V, seù vis, qua solidum HIK ascendit in fluido M maior est quacumque data vi finita R.

Cap. 4. positiuam leuitatē nō dari.

PROP. XCVI.

Idipsum problema effici posse methodo Archimedæa sic ostendemus.

SVmatur lignum L, vel aliud corpus sibi homogeneum, quod innatare possit intra fluidum M, ponaturque quælibet vis finita ponderis P, atque vt pondus absolutum molis fluidi M, quæ æqualis sit ipsi L, ad pondus absolutum ligni L, scilicèt vt grauitas specifica fluidi M ad L, ita se habeat R ad S, postea fiat cylindrus ACB eiusdē materiei L, ad cuius grauitatem absolutam pōdus P minorem proportionem habeat, quàm differentia ipsarum R, & S ad S. Tandem immergatur cylindrus AC intra fluidum M con-

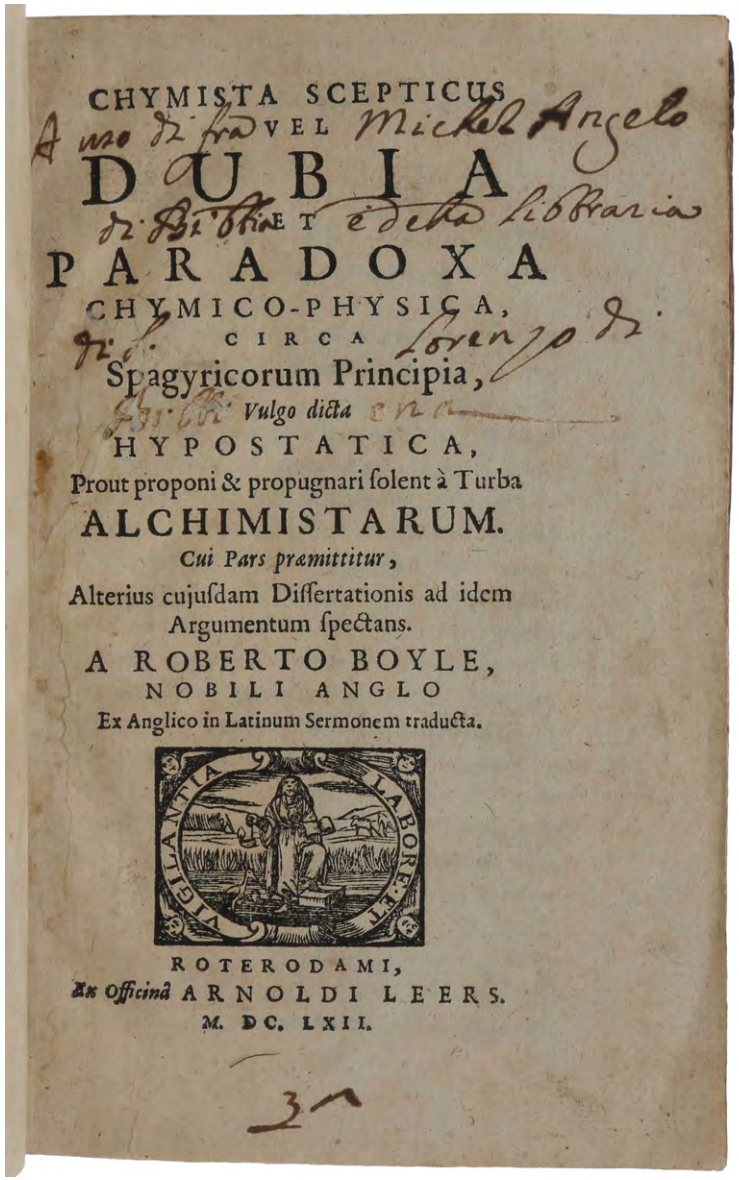
THE SCEPTICAL CHYMIST

BOYLE, Robert. *Chymista scepticus vel dubia et paradoxa chymico-physica, circa spagyricorum principa, vulgo dicta hypostatica, Prout proponi & propugnari solent à Turba Alchimistarum. Cui pars premittitur, alterius cujusdem dissertationis ad idem argumentum spectans.* Rotterdam: A. Leers, 1662.

$17,500

Two parts in one vol., 8vo (165 x 102 mm), pp. [18], 24 [Considerationes physicae]; [ii], 294 [Chymista scepticus], woodcut printer's device on title. Contemporary worn but unrestored calf. Toning throughout, but less than usually found in this work. Early inscriptions to title, small repair with Japanese paper due to ink corrosion.

First Latin edition, one of two issues, each published one year after the first English edition (the other was published at London, no priority established), of this milestone in the history of chemistry. "His most important work [where he] set down his corpuscular theory of the constitution of matter, which finally freed chemistry from the restrictions of the Greek concept of the four elements, and was the forerunner of Dalton's atomic theory" (Sparrow). "Boyle's most celebrated book is his *Sceptical Chymist* … It contains the germs of many ideas elaborated by Boyle in his later publications" (Partington II, p. 496). This Latin edition is the second edition overall; both issues are very rare: we have been unable to locate any copy of either issue in auction records. OCLC lists six copies of the Rotterdam issue and four of the London issue in the US. The first English edition now commands a very high price – the last complete copy at auction sold for £362,500 in 2015 – so that this first Latin edition is the earliest form of Boyle's greatest work that is accessible to the majority of collectors.



CHYMISTA SCEPTICUS
VEL
DUBIA
ET
PARADOXA
CHYMICO-PHYSICA,
CIRCA
Spagyricorum Principia,
Vulgo dicta
HYPOSTATICA,
Prout proponi & propugnari ſolent à Turba
ALCHIMISTARUM.
Cui Pars præmittitur,
Alterius cujuſdam Diſſertationis ad idem
Argumentum ſpectans.
A ROBERTO BOYLE,
NOBILI ANGLO
Ex Anglico in Latinum Sermonem traducta.

VIGILANTIA LABORE ET

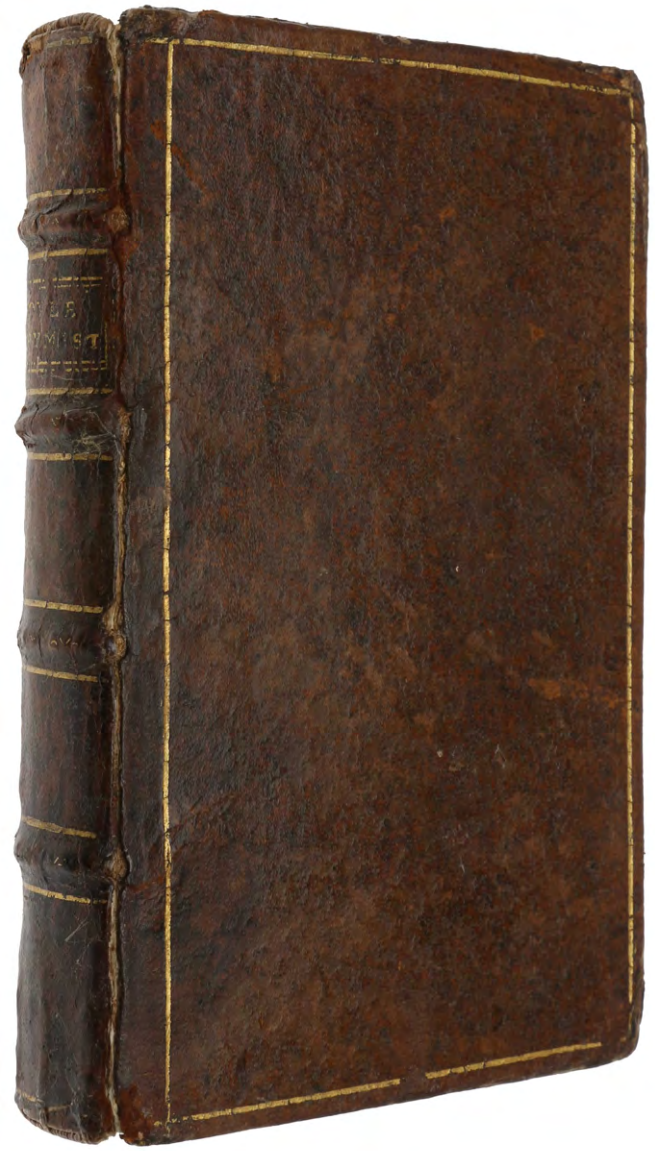
ROTERODAMI,
Ex Officinâ ARNOLDI LEERS.
M. DC. LXII.

Provenance: Michel Angelo di Biblia (inscription on title).

"The 'Sceptical Chymist' is one of the great books in the history of scientific thought, for it not only marks the transition from alchemy to modern chemistry but is a plea, couched in most modern terms, for the adoption of the experimental method. Boyle inveighed against the inaccurate terminology of the 'vulgar spagyrists' and the 'hermetick philosophers,' as he termed the alchemists who refused to define their terms … He predicted that many more [elements] existed than had been described, but insisted that many substances, then thought to be elemental, were, in fact, chemical compounds. He set forth the modern distinction between a compound and a mixture, pointing out that a true chemical compound possessed properties entirely different from either of its constituents" (Fulton).

"The importance of Boyle's book must be sought in his combination of chemistry with physics. His corpuscular theory, and Newton's modification of it, gradually led chemists towards an atomic view of matter ... Boyle distinguished between mixtures and compounds and tried to understand the latter in terms of the simpler chemical entities from which they could be constructed. His argument was designed to lead chemists away from the pure empiricism of his predecessors and to stress the theoretical, experimental and mechanistic elements of chemical science. The *Sceptical Chymist* is concerned with the relations between chemical substances rather than with transmuting one metal into another or the manufacture of drugs. In this sense the book must be considered as one of the most significant milestones on the way to the chemical revolution of Lavoisier in the late eighteenth century" (PMM).

"Boyle has ben called the founder of modern chemistry, for three reasons: (1) he realized that chemistry is worthy of study for its own sake and not merely as an aid to medicine or alchemy – although he believed in the possibility of the latter;



(2) he introduced a rigorous experimental method into chemistry; (3) he gave a clear definition of an element and showed by experiment that the four elements of Aristotle and the three principles of the alchemists (mercury, sulphur and salt) did not deserve to be called elements or principles at all, since none of them could be extracted from bodies" (Partington II, p. 495).

The Sceptical Chymist takes the form of a dialogue, clearly modeled on Galileo's Dialogo, involving four participants. The Aristotelian Themistius and the Paracelsian Philoponus state their positions briefly, but soon fall silent. A wide-ranging discussion ensues between the sceptical Carneades (Boyle himself) and Eleutherius, the open-minded enquirer. Carneades argues – citing many experimental examples – that the Aristotelian four-element system and the Paracelsian three-principle model give equally inadequate explanations of what happens when complex substances are attacked by fire, or by powerful solvents. He shows that these processes often generate new compounds, rather than the promised 'primitive and simple, or perfectly unmingled bodies', which remain stubbornly elusive. His second proposal is more speculative – and theologically more dangerous. Boyle believed, and hoped to prove in time, that the ultimate constituents of bodies were minute atoms, differing only in 'bulk, figure, texture and motion'. This idea was first suggested by the ancient Greek natural philosophers Leucippus and Democritus. Their successor, Epicurus, incorporated it into a godless materialistic world-view that was universally condemned by Christian theologians. Consequently, atomistic theories were suppressed for centuries. By the mid-17th century the works of the classical Greek atomists had been printed, translated and commented upon by scholars such as Pierre Gassendi, though there was still considerable hostility to them from clergy of all persuasions. But Boyle – a devout (though somewhat unorthodox) Christian who funded translations of the Gospels into many languages, including Gaelic and Turkish – saw no reason why a benign deity could not have chosen to create an atomic universe.

Dibner, *Heralds* 39; Grolier/Horblit 14; Norman 299; Partington II, pp. 495-8; PMM 141; Sparrow, *Milestones* 27 (all for the first edition in English). Fulton 37.

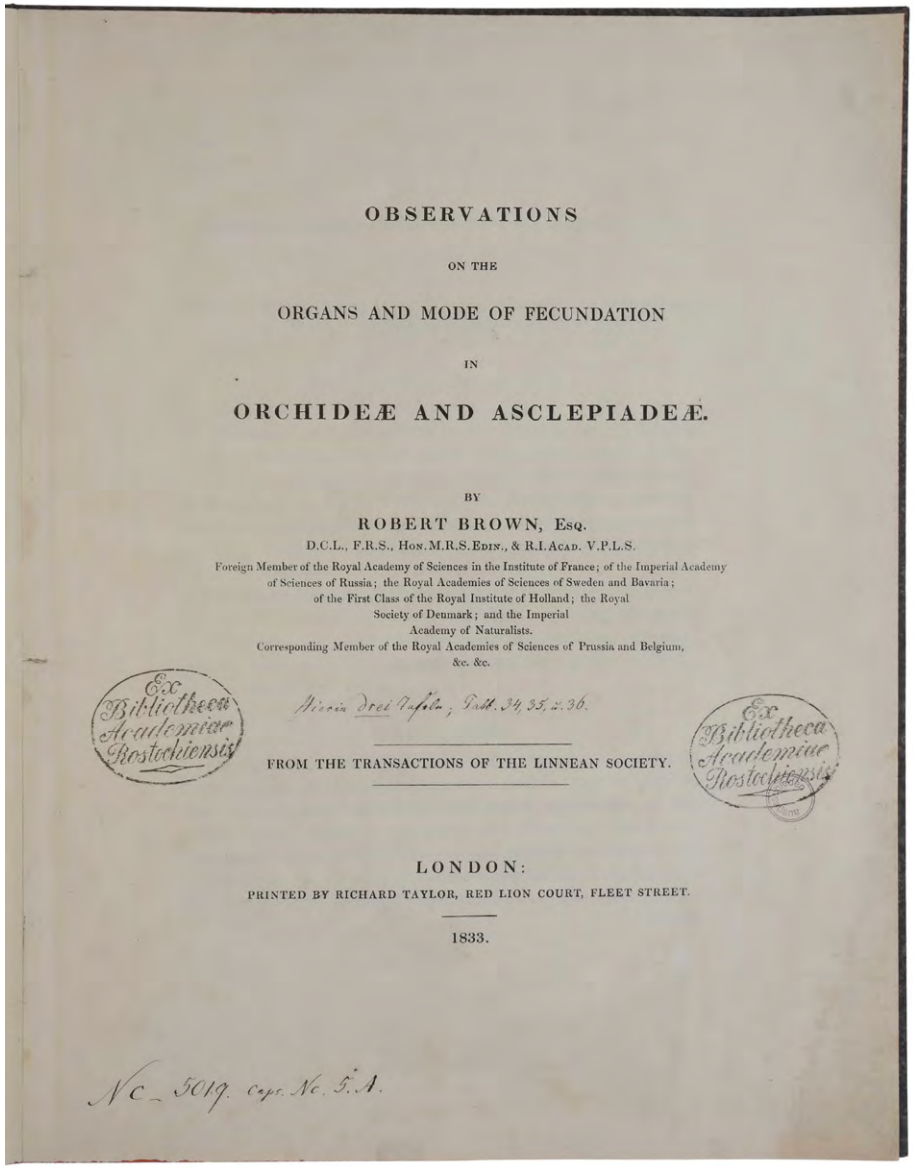
DISCOVERY OF THE CELL NUCLEUS PRESENTATION COPY

BROWN, Robert. *Observations on the Organs and mode of Fecundation in Orchidae and Asclepiadeae.* London: printed by Richard Taylor, Red Lion Court, Fleet Street, 1833.

$3,500

4to (282 x 221 mm), pp. [ii], 685-745, with three lithographed plates (with some damp staining). 20th century speckled boards with red lettering-piece on spine, contemporary plain wrappers bound in.

Presentation offprint of the discovery of the cell nucleus, **inscribed by Robert Brown**, famous for his discovery of 'Brownian motion', widely regarded as one of the most important discoveries of 19th century physics. "In 1833, Brown observed and described the nucleus, the first organelle" (Mullins, p. 2). "Brown first expressed his observations in an 1831 speech, and published them in a paper two years later. His statements suggested the nucleus played a key role in fertilization and development of the embryo in plants. Thus, Brown not only named the nucleus, but created the possibility that it was at the center of cellular creation" (science-of-aging.com). Brown's paper was read to the Linnaean Society on 1 November and 15 November 1831. He was at the time custodian of the botanical collections of the British Museum and later became president of the Linnaean Society. No copy on ABPC/RBH is the last 30 years.



OBSERVATIONS

ON THE

ORGANS AND MODE OF FECUNDATION

IN

ORCHIDEÆ AND ASCLEPIADEÆ.

BY

ROBERT BROWN, Esq.

D.C.L., F.R.S., Hon. M.R.S. Edin., & R.I. Acad. V.P.L.S.

Foreign Member of the Royal Academy of Sciences in the Institute of France; of the Imperial Academy of Sciences of Russia; the Royal Academies of Sciences of Sweden and Bavaria; of the First Class of the Royal Institute of Holland; the Royal Society of Denmark; and the Imperial Academy of Naturalists.

Corresponding Member of the Royal Academies of Sciences of Prussia and Belgium, &c. &c.

Ex Bibliotheca Academiae Rostochiensis

FROM THE TRANSACTIONS OF THE LINNEAN SOCIETY.

Ex Bibliotheca Academiae Rostochiensis

LONDON:

PRINTED BY RICHARD TAYLOR, RED LION COURT, FLEET STREET.

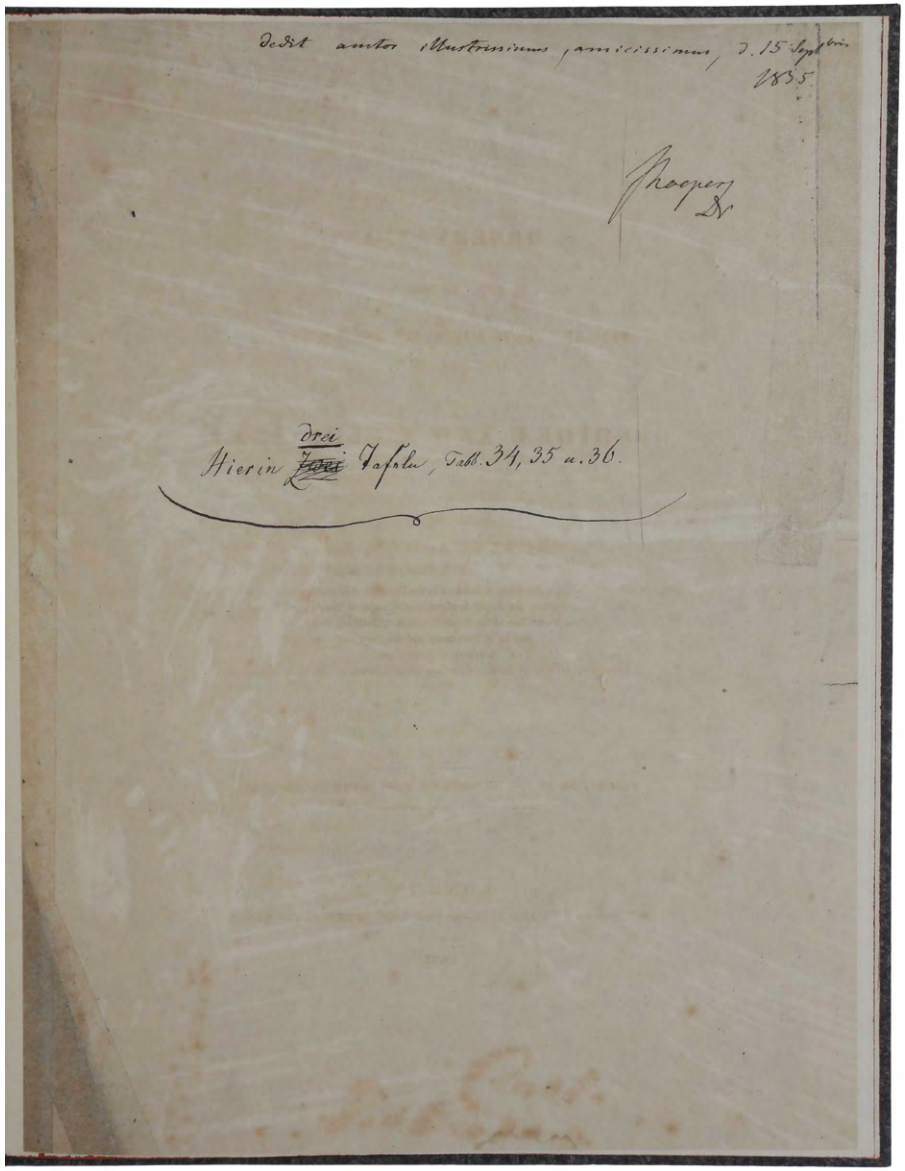
1833.

Nc 5019 cop. No. 5. A.

Provenance: Presentation inscription "Dedict autor illustrissimus, amicissimus, d. 15 Sept. 1835, Dr. Shaeper (?) on upper wrapper. Ex Bibliotheca Academiae Rostockiensis (two ink stamps on title and one on upper wrapper).

Brown (1773-1858) was asked by Joseph Banks to serve as botanist on Matthew Flinder's 1801 expedition to answer the question whether New Holland was one island or several. For three and a half years Brown did intensive botanical research in what is now Western Australia, collecting about 3400 species, of which about 2000 were previously unknown. After his return to England in 1805, Brown spent much of the rest of his life working on the specimens he had collected. His discovery of 'Brownian motion' was a by-product of his botanical work, as he first noted it by observing the motion of pollen grains suspended in water.

The present paper "contains his observations that the pollen of orchids, when placed upon the stigma, emits pollen tubes traceable into the ovary. Embedded in this paper is a discovery highly relevant to the cell theory, and thus to the development of cytology. Regarding the leaves of orchids, Brown stated: "In each cell of the epidermis of a great part of this family, especially of those with membranous leaves, a single circular areola, generally somewhat more opake than the membrane of the cell, is observable… only one areola belongs to each cell…. This areola, or nucleus of the cell as perhaps it might be termed, is not confined to the epidermis, being also found not only in the pubescence of the surface particularly when jointed, as in Cypripedium, but in many cases in the parenchyma or internal cells of the tissue…. The nucleus of the cell is not confined to the Orchideae but is equally manifest in many other Monocotyledonous families; and I have even found it, hitherto however in very few cases, in the epidermis of Dicotyledonous plants" [pp. 710-712]. A few earlier botanists evidently had observed the presence of this nucleus in some cells, as Brown himself points out, but he was the first specially to demonstrate its general occurrence in living cells and to give it the name "nucleus"" (DSB).



A pamphlet with the same title as the present offprint was published in 1831 – this is extremely rare (we have traced no copy in auction records, and there is no copy on OCLC). The journal article in the *Transactions of the Linnaean Society* (1833), of which the offered work is an offprint (and to which it is of course textually identical), is textually different, and more extensive, than the 1831 pamphlet – in fact, the two are textually identical up to p. 696 of the journal article, corresponding to p. 10 of the pamphlet, but are quite different subsequently. Interestingly the offprint has the same title as the separate printing, which differs slightly from the title of the journal article ('On the organs and manner of fecundation in orchideae and asclepiadeae'). Both the DSB and Garrison & Morton cite the journal issue in *Transactions of the Linnean Society* as the first published edition to use the word 'nucleus.'

For a digitized version of the 1831 pamphlet, see https://archive.org/stream/b22285003#page/12/mode/2up/search/areola

Garrison-Morton 109 (journal issue). Mullins, *The Biogenesis of Cellular Organelles*, 2007.



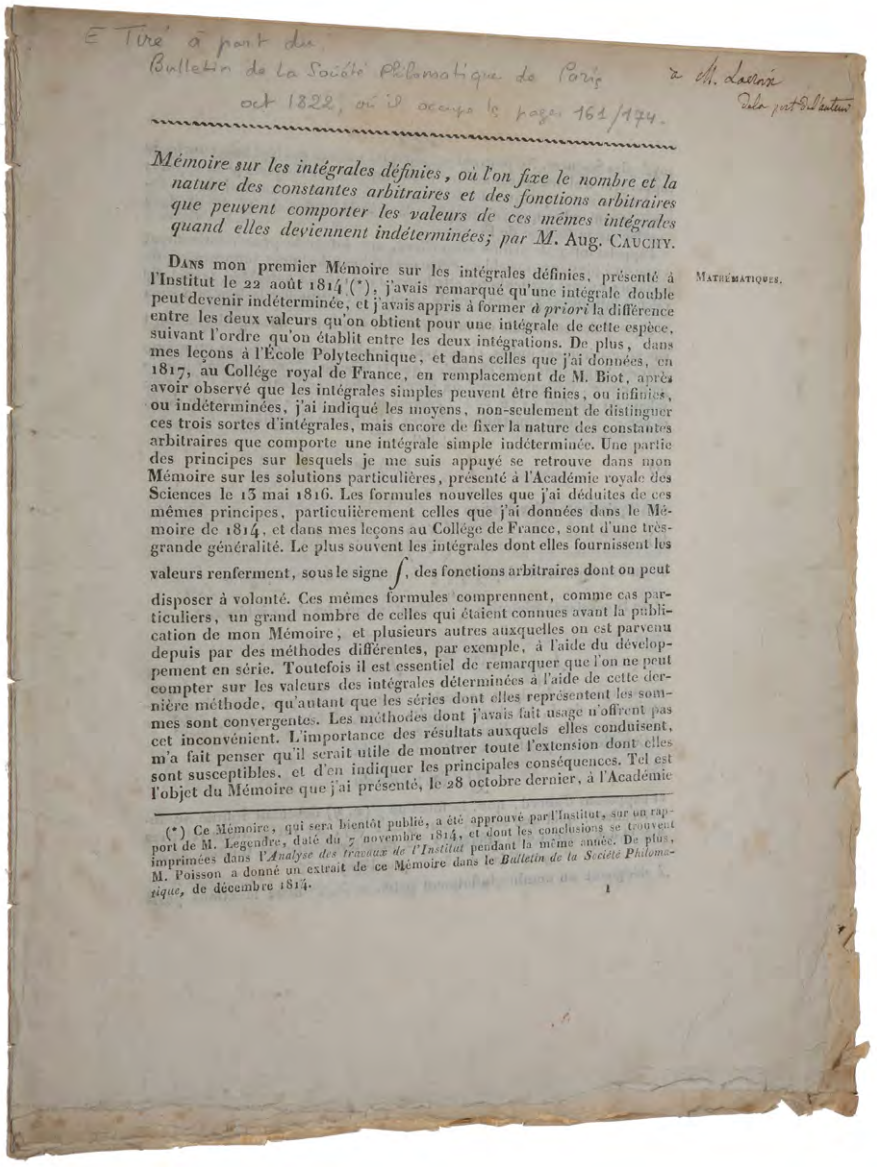
INSCRIBED BY CAUCHY TO LACROIX

CAUCHY, Augustin-Louis. *Mémoire sur les intégrales définies, où l'on fixe le nombre et la nature des constantes arbitraires et des fonctions arbitraires que peuvent comporter les valeurs de ces mêmes intégrales quand elles deviennent indéterminées. Offprint from Bulletin des Sciences par la Société Philomathique de Paris, Année 1822.* [Paris: Plassan, 1822].

$4,500

4to (265 x 210 mm), pp. [1] 2-14. Seven uncut loose leaves, at some point extracted from a binding. Light spotting, edges slightly frayed.

First edition, extremely rare separately-paginated offprint (journal pagination 161-174), inscribed by Cauchy (1789-1857) to his senior colleague Sylvestre François Lacroix (1765-1843). In this paper Cauchy published for the first time many of the most important results of his great work *Mémoire sur les intégrales définies*, submitted to the Académie des Sciences on 22 August 1814 but not published until 1827, which laid the foundations of complex function theory (the delay was possibly due to its length of almost 200 pages). "This remarkable paper [i.e., the 1814 paper] is a major seed for 19th century mathematics, for in it Cauchy inaugurated the theory of complex variable functions and their integration" (Grattan-Guinness, p. 656). "The discoveries with which Cauchy's name is most firmly associated in the minds of both pure and applied mathematicians are without doubt his fundamental theorems on complex functions" (DSB). In this 1822 work we find, in particular, the first published statement of 'Cauchy's integral formula' (p. 10) and the 'residue theorem' (on p. 9) – these are the central results of complex function theory. On p. 11 of the present paper, Cauchy also argues for the first time that the definite integral should be defined in terms of 'partial sums,'



Tiré à part du
Bulletin de la Société Philomatique de Paris
oct 1822, où il occupe les pages 161/174.

Mémoire sur les intégrales définies, où l'on fixe le nombre et la nature des constantes arbitraires et des fonctions arbitraires que peuvent comporter les valeurs de ces mêmes intégrales quand elles deviennent indéterminées; par M. Aug. Cauchy.

MATHÉMATIQUES.

Dans mon premier Mémoire sur les intégrales définies, présenté à l'Institut le 22 août 1814 (*), j'avais remarqué qu'une intégrale double peut devenir indéterminée, et j'avais appris à former *à priori* la différence entre les deux valeurs qu'on obtient pour une intégrale de cette espèce, suivant l'ordre qu'on établit entre les deux intégrations. De plus, dans mes leçons à l'École Polytechnique, et dans celles que j'ai données, en 1817, au Collége royal de France, en remplacement de M. Biot, après avoir observé que les intégrales simples peuvent être finies, ou infinies, ou indéterminées, j'ai indiqué les moyens, non-seulement de distinguer ces trois sortes d'intégrales, mais encore de fixer la nature des constantes arbitraires que comporte une intégrale simple indéterminée. Une partie des principes sur lesquels je me suis appuyé se retrouve dans mon Mémoire sur les solutions particulières, présenté à l'Académie royale des Sciences le 13 mai 1816. Les formules nouvelles que j'ai déduites de ces mêmes principes, particulièrement celles que j'ai données dans le Mémoire de 1814, et dans mes leçons au Collége de France, sont d'une très-grande généralité. Le plus souvent les intégrales dont elles fournissent les valeurs renferment, sous le signe $\int$, des fonctions arbitraires dont on peut disposer à volonté. Ces mêmes formules comprennent, comme cas particuliers, un grand nombre de celles qui étaient connues avant la publication de mon Mémoire, et plusieurs autres auxquelles on est parvenu depuis par des méthodes différentes, par exemple, à l'aide du développement en série. Toutefois il est essentiel de remarquer que l'on ne peut compter sur les valeurs des intégrales déterminées à l'aide de cette dernière méthode, qu'autant que les séries dont elles représentent les sommes sont convergentes. Les méthodes dont j'avais fait usage n'offrent pas cet inconvénient. L'importance des résultats auxquels elles conduisent, m'a fait penser qu'il serait utile de montrer toute l'extension dont elles sont susceptibles, et d'en indiquer les principales conséquences. Tel est l'objet du Mémoire que j'ai présenté, le 28 octobre dernier, à l'Académie

(*) Ce Mémoire, qui sera bientôt publié, a été approuvé par l'Institut, sur un rapport de M. Legendre, daté du 7 novembre 1814, et dont les conclusions se trouvent imprimées dans l'*Analyse des travaux de l'Institut* pendant la même année. De plus, M. Poisson a donné un extrait de ce Mémoire dans le *Bulletin de la Société Philomatique*, de décembre 1814.

1

an approach he developed in detail in his *Résumé des Leçons* (1823). This was an essential step towards the theory of complex integration he presented in *Mémoire sur les intégrales définies, prises entre des limites imaginaires* (1825), and was the basis for all rigorous approaches to integration until the twentieth century. We can find no record of this offprint in auction records, nor of any offprint inscribed by Cauchy. Not on OCLC.

Provenance: Sylvestre François Lacroix, inscription on title page "à M. Lacroix de la part de l'auteur". Although Lacroix's mathematical work contained little that was absolutely new and original, he was a great teacher and writer of textbooks. His monumental three-volume treatise on calculus, *Traité du Calcul Différentiel et du Calcul Intégrale*, incorporated the various advances made since the middle of the eighteenth century, and synthesised the works of Euler, Lagrange, Laplace, Monge, Legendre, Poisson, Gauss, and Cauchy, whose writings are followed up to the year 1819. As a student at the École Polytechnique (from which he graduated in 1807), Cauchy studied analysis under Lacroix. Several years later, Lacroix was one of the examiners of Cauchy's 1814 memoir (the other was Legendre), and its publication was recommended by their report to the Institut de France.

One of Cauchy's motivations in writing the 1814 paper was to justify the use of complex variables to evaluate real definite integrals. "The practice had begun with Euler and continued with Laplace, Poisson, and Legendre, in his *Exercises de Calcul Intégrale* (1811). In the Mémoire on this topic that he presented in 1814 Cauchy commented that many of the integrals had been evaluated for the first time "by means of a kind of induction" based on "the passage from the real to the imaginary", and that no less a figure than Laplace had remarked that the method "however carefully employed, leaves something to be desired in the proofs of the results". Cauchy accordingly set himself the task of finding a "direct and rigorous analysis" of this dubious passage" (Gray, pp. 59-60). Cauchy's defence of this technique led, among other things, to the Cauchy-Riemann equations. However, his chief interest in the 1814 paper was in double integrals.

"Mathematicians of the period, with their somewhat formal attitude to the processes of the calculus, not only tended to regard a double integral as a nested pair of single integrals but to be indifferent to the order of integration. This formal approach precluded interpreting double integrals in terms of line integrals along the boundaries of a rectangle in the plane of complex numbers; they were strictly about integrals on a rectangle in the real plane. However, it was known that in some cases this attitude was too naive and led to conflicting results, and it was this problem that Cauchy addressed in the second part of the *Mémoire* …

"Cauchy showed varying the order of integration of a double integral can yield two distinct definite values when the function becomes infinite or indeterminate for certain values of the variables within the domain of integration. To investigate the matter further, he introduced what he called singular integrals ('intégrales singulières'), which he defined as limits as the end points of the integral tend to the same fixed value "without the integrals being zero." Singular integrals are a profound idea. They were to crop up repeatedly in real analysis and, in an intriguing different way, in Cauchy's later work on complex integrals" (Gray, p. 61).

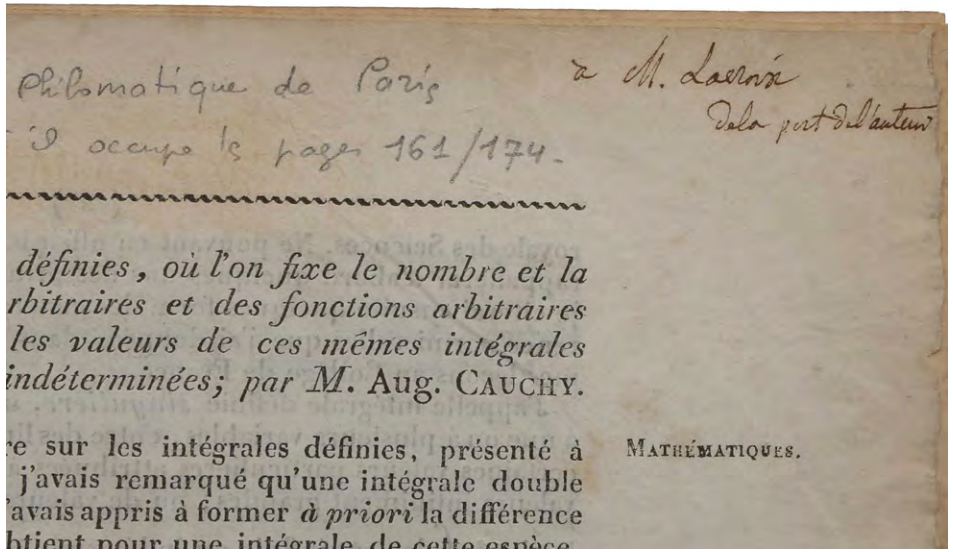
In the 1822 paper, Cauchy began by recalling his 1814 definition of a singular integral and then drew upon results from the same paper on the evaluation of functions of a complex variable over real-valued limits. This led to the formula at the top of p. 169, the first published version of the residue theorem. "This pretty form of a residue theorem was put through some of its paces in the rest of Cauchy's paper [i.e., the offered paper]. For example, he found an integral formula for [the *n*th derivative], and two pages later he obtained a couple of evaluations

of which Poisson had given special cases … in a postscript he reproved some of those results using Parseval's theorem" (Grattan-Guinness, p. 740).

"At this point [p. 11, second paragraph] Cauchy made an interesting remark that helps to explain his motivation for introducing the concept of definite integral the way he did in his *Résumé*. He said that he considered each definite integral, taken between real limits, 'as nothing other than the sum of the values of the differentials corresponding to the various values of the variable between the given limits. This way of thinking of a definite integral, it seems to me, must be preferred because it leads equally to all cases, even those in which one docs not know at all how to pass generally from the function placed under the integral sign to the primitive function [i.e., the function whose derivative is the function to be integrated].' Thus, his research work on definite integrals, including in particular his theory of singular integrals, apparently persuaded him to abandon the prevailing view at the time that the definite integral should be defined via the primitive function and, instead, to define the definite integral as limits of partial sums. In addition, Cauchy stated, this view 'has the advantage' of always providing real values for integrals of real functions, as well as allowing one to separate 'easily' any imaginary equation into its real and imaginary parts. This will not be the case when considering a definite integral as the difference of the values taken by a (discontinuous) primitive function at the limits of integration, or when allowing the variable to take imaginary values" (Gray & Bottazzini, pp. 121-122). "… although there is hardly any doubt that by the early 1820s Cauchy was in possession of a wealth of results that, in an incoherent way, prefigured his later achievements, a satisfactory theory of complex functions was still lacking. An essential step towards such a theory was his decision to get rid of the 'old' definition of definite integrals based on primitive functions, and instead to define them as limits of partial sums" (*ibid*., p. 128).

"Cauchy showed understandable signs of impatience in [the present] paper at the non-appearance of his 1814 paper on complex integration. Not only did he announce – falsely, it turned out – that it 'will soon be published' [p. 1, footnote], but he also mentioned other texts which in the end were never published and seem now to be lost … [The present paper] appeared from the *Société Philomatique* in their November *Bulletin*. At 14 pages it was rather long for them … so they accepted his offer to pay for the printing of eight of the pages" (Grattan-Guinness, p. 738).

Grattan-Guinness, *Convolutions in French Mathematics 1800-1840*, Section 11.4.4; Gray, *The Real and the Complex: A History of Analysis in the 19th Century*, 2015; Gray & Bottazzini, *Hidden Harmony – Geometric Fantasies. The Rise of Complex Function Theory*, 2013; Smithies, *Cauchy and the Creation of Complex Function Theory*, Sections 3.5-3.7.


Philomatique de Paris
il occupe les pages 161/174.
à M. Lacroix
De la part de l'auteur
définies, où l'on fixe le nombre et la
rbitraires et des fonctions arbitraires
les valeurs de ces mêmes intégrales
indéterminées; par M. Aug. CAUCHY.
re sur les intégrales définies, présenté à
j'avais remarqué qu'une intégrale double
'avais appris à former *à priori* la différence
MATHÉMATIQUES.

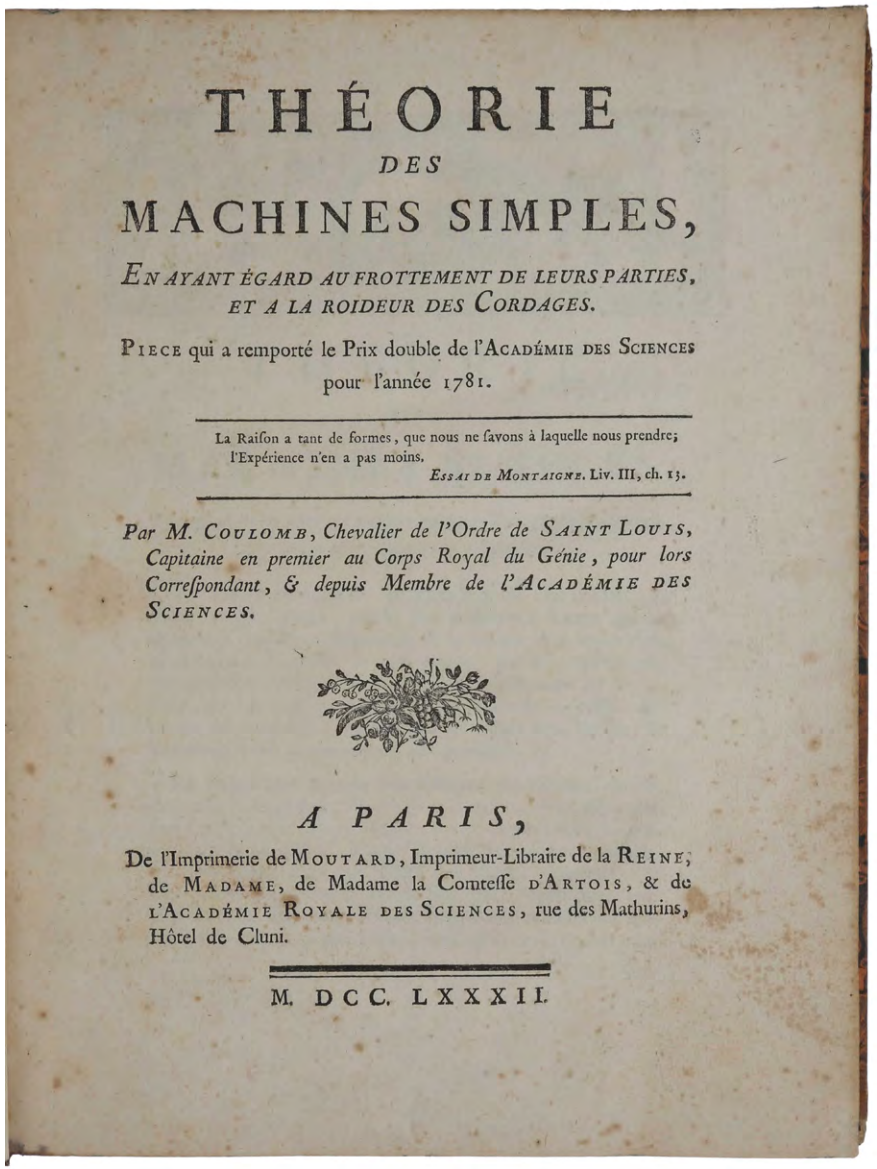
CREATED THE SCIENCE OF FRICTION

COULOMB, Charles Augustin. *Théorie des Machines Simples, en ayant égard au frottement de leurs parties, et a la roideur des Cordages. Piece qui a remporté le Prix double de l'Académie des Sciences pour l'année 1781.* Paris: Moutard, 1782.

$13,500

4to (249 x 190 mm), [1-3] 4-172 pp., 5 plates numbered ii-vi (fully complete), contemporary half calf over marbled boards, front and rear boards with some surface wear and scratches, lower capital chipped, spine with moderate wear, very light spotting to a few leaves. A very nice and completly unrestored copy.

Extremely rare offprint, with imprint three years before publication in journal form, of this important memoir in which Coulomb created the science of friction. "Coulomb's most celebrated study, one that brought him immediate acclaim, was "Théorie des machines simples," his prize-winning friction study of 1781. He investigated both static and dynamic friction of sliding surfaces and friction in bending of cords and in rolling. From examination of many physical parameters, he developed a series of two-term equations, the first term a constant and the second term varying with time, normal force, velocity, or other parameters. In agreement with Amontons's work of 1699, Coulomb showed that in general there is an approximately linear relationship between friction and normal force; but he extended the investigation considerably to show complex effects due to difference in load, materials, time of repose, lubrication, velocity, and other considerations. Coulomb's work in friction remained a standard of theory and experiment for a century and a half, until the advent of molecular studies of friction in the twentieth century. To quote Kragelsky and Schedrov's recent



THÉORIE
DES
MACHINES SIMPLES,
EN AYANT ÉGARD AU FROTTEMENT DE LEURS PARTIES, ET A LA ROIDEUR DES CORDAGES.
PIECE qui a remporté le Prix double de l'ACADÉMIE DES SCIENCES pour l'année 1781.

La Raiſon a tant de formes, que nous ne ſavons à laquelle nous prendre; l'Expérience n'en a pas moins.
ESSAI DE MONTAIGNE. Liv. III, ch. 13.

Par M. COULOMB, Chevalier de l'Ordre de SAINT LOUIS, Capitaine en premier au Corps Royal du Génie, pour lors Correſpondant, & depuis Membre de l'ACADÉMIE DES SCIENCES.

A PARIS,
De l'Imprimerie de MOUTARD, Imprimeur-Libraire de la REINE, de MADAME, de Madame la Comteſſe D'ARTOIS, & de l'ACADÉMIE ROYALE DES SCIENCES, rue des Mathurins, Hôtel de Cluni.

M. DCC. LXXXII.

monograph ([*Development of the Science of Friction - Dry Friction* (1956)], p. 52) on the history of friction: "Coulomb's contributions to the science of friction were exceptionally great. Without exaggeration, one can say that he created this science" (Gillmor in DSB III.442). The *Théorie* was delivered orally in 1781 and it won an important prize of the Academy of Sciences, a fact advertised on the title: 'Piece qui a remporté le Prix double de l'Académie des Sciences pour l'Année 1781.' It was published in 1785 in Tom. X of the *Mémoires de Mathématique et de Physique Présentés à l'Académie Royale des Sciences, Par Divers Savans*, pp. 161-332. The completely different pagination (and signatures) establish the present work as a separate publication, and it was almost certainly intended for limited distribution (Academy members, etc.). OCLC lists three copies of this offprint worldwide: Harvard, Université Louis Pasteur, Strasbourg (2 copies or a duplicate record?). No copies located in auction records.

"During the period 1720-1793 the Academy offered numerous prizes in essay contests. Capturing one of the handsome awards was not always a guarantee of entrance into the Academy, but those who did win found themselves among an impressive group which included Pierre Bouguer, three Bernoullis (Johann, Johann II and Daniel), Euler, Lagrange, Bossut and Bailly. The contests fell into two main categories, astronomy and maritime …

"The Academy had proposed for the prize for 1779 (and again for 1781), the solution of problems of friction for sliding and rolling surfaces, the resistance to bending in cords and the application of these solutions to simple machines used in the navy. There existed so many friction theories and experimentally determined constants that it was specifically indicated that the contestants must examine "the effects resulting from the stiffness of ropes, being determined after *new experiments made on the full scale*; it is required also, that the experiments be applicable to *machines utilized* in the Navy, such as the pulley, the capstan, and the inclined plane." The solutions of some of these problems were of the highest priority. For example, the data on inclined planes would be used in constructing arrangements for ship launchings. Coulomb noted that often when ships were launched by sliding down ways they would stick halfway down, the ways would catch fire from the generated heat or the ship would fall over and possibly suffer major damage ….

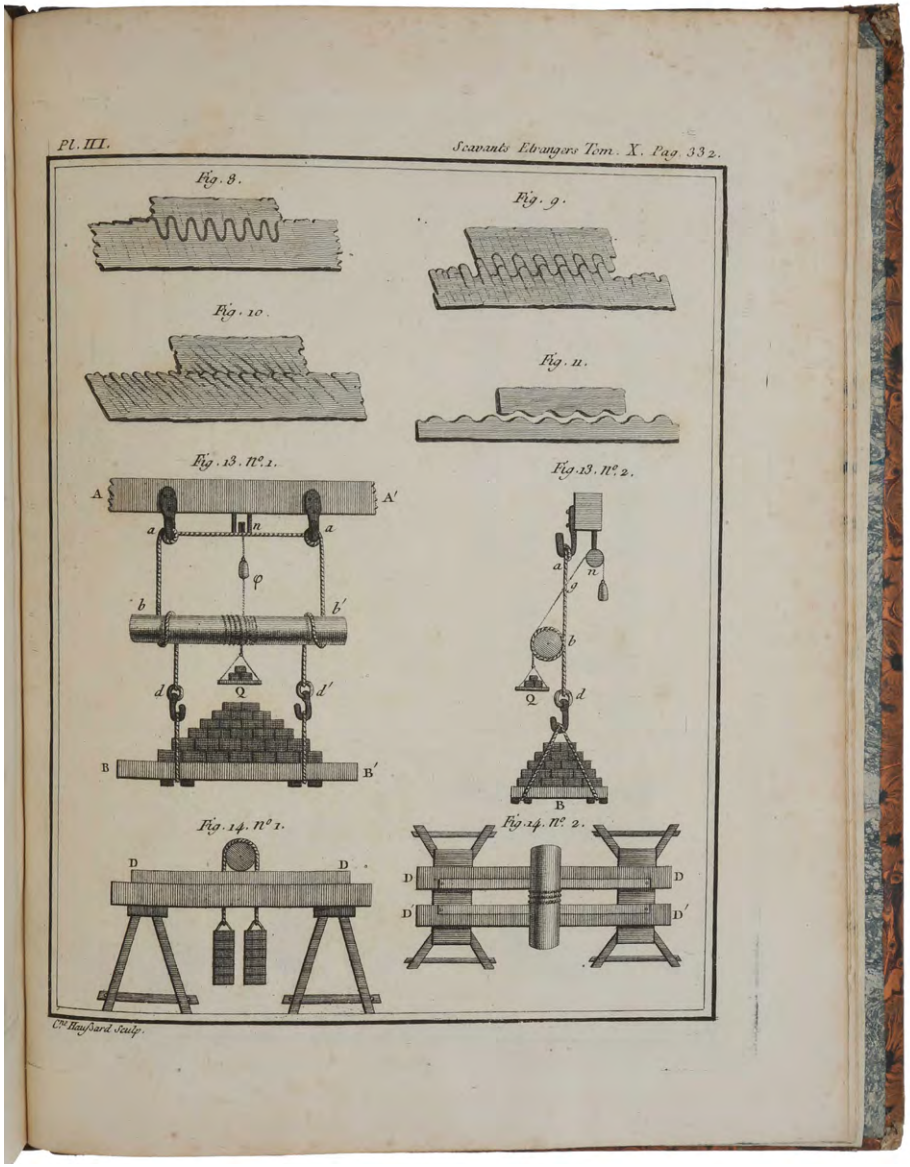
"In the preface to "Theory of Simple Machines" Coulomb discussed the existing work on friction – the original hypothesis of Amontons and the experimental variances found by Musschenbroek, Camus and Desaguliers. He criticized the neglect of deformation considerations in previous work. Camus and Desaguliers had noted that friction varied with the time that the two static surfaces had remained together before motion, but neither had investigated this relationship. As in previous memoirs, Coulomb acknowledged his debt to Bossut.

"The "Theory of Simple Machines" was composed in two parts: 1) the friction of sliding surfaces and 2) the bending of cords and movements of rotation. In each of these sections Coulomb gave numerous figures depicting his apparatus. He carefully described his materials and the reasons for employing each particular piece. For instance he explained that in the investigations on sliding surfaces he disregarded the friction of a small pulley in his friction table apparatus. He did so because in separate experiments he examined the friction in the pulley and found it to be less than 1/150th of the total value in the experiments with the table. In other cases he noted the exact temperature of the air, and examined rope that had been weathered by sun, rain, or salt water. He found correction factors for these variables. In another case he considered the humidity of the air. He left this factor out of his published results only after it was found to have no differential effect on the various experiments. Thus it had been assumed that if a pulley or a plane surface were polished to a high sheen then it could be considered smooth

and as frictionless as possible. Desaguliers had stated that such surfaces would produce higher friction values due to cohesive forces. Coulomb discovered that one cannot determine directly by any human sense whether a plane surface, for instance, is perfectly smooth. Nevertheless the friction coefficient can be decreased by "running-in" the object. For a plane surface this would entail passing a very heavy plane object back and forth over its surface. A pulley could be "run-in" by drawing a rope over it for a number of hours. In each case the friction coefficient would be reduced after a period of time, even if the surface had formerly appeared perfectly smooth.

"This patient, inquisitive, wide-ranging type of experimentation was unique in the history of friction studies. Coulomb's method produced physical answers expressed in analytical terms that could never arise out of the purely rational approach of an Euler … And oppositely, Coulomb's use of analysis to frame his experimental laws and to guide him in searching for patterns of development could bring a myriad of facts into account. This was never accomplished by the rambling experimentation of a Bélidor or a Desaguliers … Coulomb used analytical methods evolved by Parent, Euler, Daniel Bernoulli, and Bossut. He extended and codified the experiments of Amontons, Desaguliers, and others. It was just this fruitful combination of analysis and directed experiment that led him to his results.

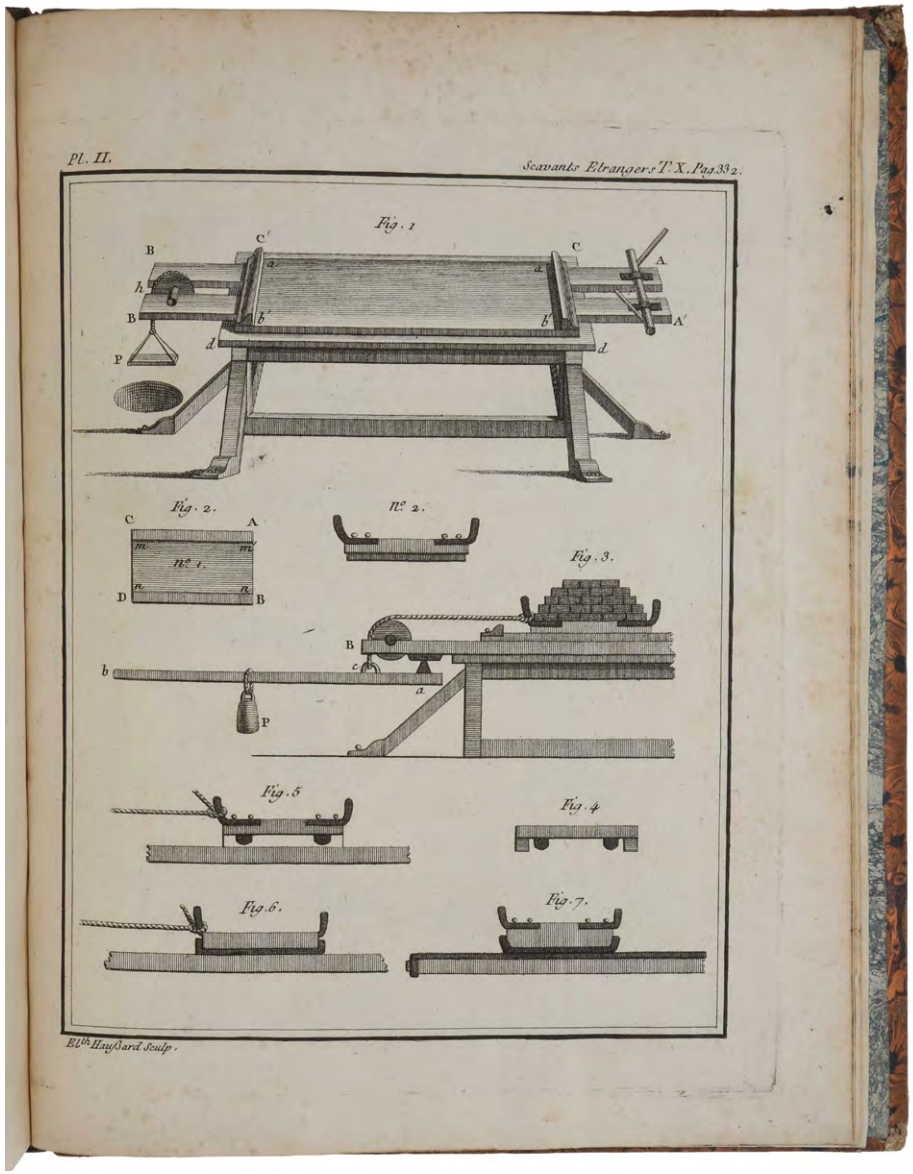
"In nearly all of Coulomb's formulas for calculating friction effects he employed two-term equations. These contained a constant term and a term varying with time, normal force, velocity, or some other parameter. These formulas accounted for the small effect due to cohesion or surface films and the larger effect due to mechanical interaction … In articles III and IV at the beginning of his memoir he noted the five most important parameters in static and dynamic friction:



1. the nature of materials and their surface coatings
2. the extent of surface area
3. normal force
4. the length of time that the surfaces remain in contact before motion begins (time of repose)
5. the relative velocity of the contact surfaces.

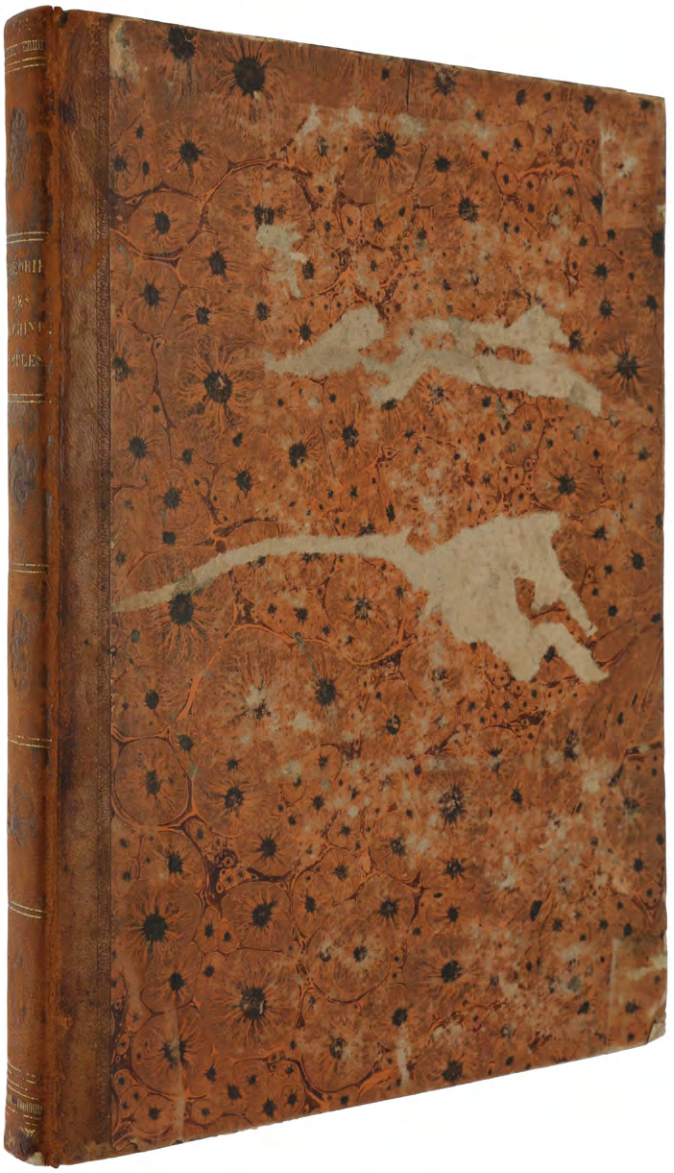
Coulomb then proceeded immediately to discuss the two major hypotheses for the cause of friction phenomena: first, the engagement or enmeshing of surface asperities; and second, the cohesion of the surface molecules. Those, as Desaguliers, who held that friction depended on the area of surface contact, employed the cohesion theory to explain friction. Most others accounted for it by the use of Amontons' mechanical surface-asperity theory … Coulomb said that Amontons' Law held for most practical cases of friction phenomena. By this he meant that there was an approximately linear relationship between friction and normal forces but that this did not remain constant from one material to another …

"Coulomb became aware that Amontons' Law did not accurately reflect the situation in friction problems. Abbé Bossut had been the first to indicate clearly the difference between static and dynamic friction but he had not sufficiently examined the problem. Camus and Desaguliers had noted that the friction of a body shocked or shaken was less than that of a body started from rest but they had not tried to determine the relation that exists between these two kinds of friction. Bossut had noted that the longer two surfaces remained in static contact the harder it was to start them in motion. Coulomb … advanced the suggestion that the contacting surfaces might be covered with a small surface layer or film.



"Of greater import than the small cohesion contributions was the factor of normal force. According to Amontons' Law, the friction was approximately proportional only to the normal force exerted by the upper body. Amontons further added that the friction force was equal to about one-third of the normal force. Amontons had expressed both surface-asperity and surface-spring hypotheses to explain the behaviour of friction. However, he considered springs to deform instantaneously. Coulomb was faced with the following experimental facts: metals seemed to have approximately the same coefficient of friction for both static and dynamic conditions; friction in fibrous materials, on the other hand, varied depending upon the length of time the surfaces had remained in contact and upon the velocity of surface motion. Adapting the "brush bristle" analogy of Musschenbroek, Coulomb explained the variation of friction with the time of repose and with velocity in fibrous matter in the following way: if wood fibers are considered as little springs capable of deformation, then as two substances remained pressed together, their asperities would interlock or enmesh more and more with time of repose. There was a definite time needed for this deformation to occur. After increasing for a time the static friction seemed to reach a limit value. Coulomb assumed that this limit indicated the occurrence of full deformation. In dynamic friction, the surface asperities did not have time enough to enmesh, thus, the surface asperities could be regarded as almost rigid and the friction would be proportional to normal force alone.

"Under "enormous" pressures the surface asperities or cavities became bent, and with increasing velocity the asperities enmeshed less and less. This would explain the relative decrease of friction with increasing velocity at high pressures. Metals did not seem to share this property. There was no effect of time of repose, nor of lessening friction with velocity. This was easily explained by Coulomb. Wood was thought to be composed of flexible, elastic, elongated fibers. Metal, however, was composed of "angular, globular, hard and inflexible parts, so that no degree of



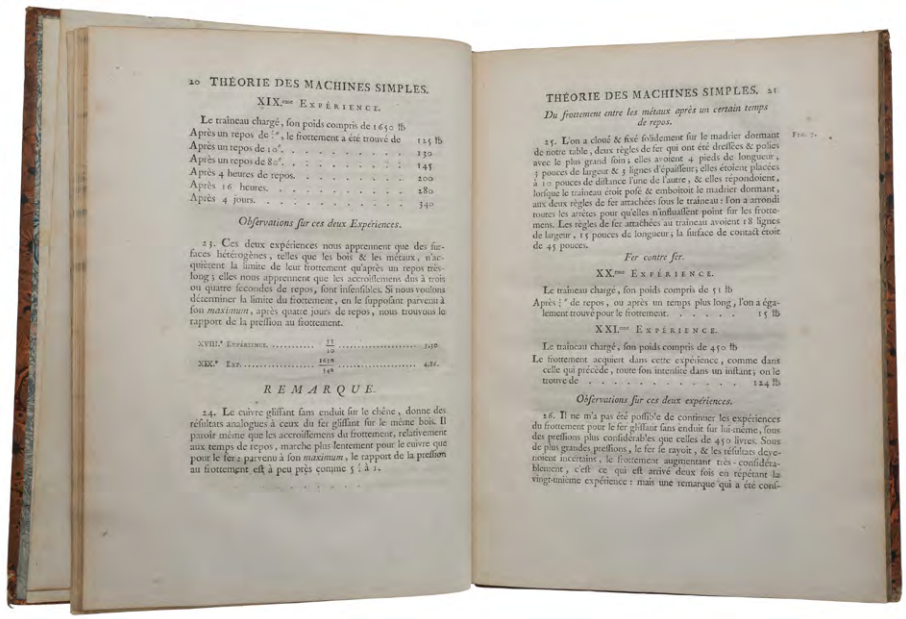
pressure nor of tension can change the shape of the parts which cover the surface of metals" …

"In sum, Coulomb's hybrid two-term formulas and his composite theory gave a very good account of known frictional behaviour" (Gillmor, pp. 127-136).

The present work presents a bibliographical puzzle. The leading American scholar of Coulomb, C.S. Gillmor of Wesleyan University, does not list this 1782 publication of the *Théorie des Machines* in the DSB article or his monograph, *Coulomb and the evolution of Physics and Engineering in 18th-century France* (1972). According to Gillmor, the first published version of the present work appeared in 1785 on pages 161-332 of vol. X of the *Mémoires de Mathématique et de Physique Présentés à l'Académie Royale des Sciences, Par Divers Savans*. The Norman catalogue also cites the 1785 journal appearance as being the first. The plates in the present edition (figures 1-27 on plates numbered II-VI) are integral with the text, and appear to be the same as those in the journal publication (the 'missing' plate I belongs to a different article in the journal). Each plate contains the printed title 'Scavants Etrangers T.X. Pag. 332' within the platemark. This links the plates to the journal, since the plates in such publications are generally bound at the end of the article and the page number is precisely the last numbered page of the work in the journal publication (332). But, the pagination in the present edition is completely different, pp. [1]–172, and the date of the imprint is three years earlier. This anomaly may be explained by the fact that the volumes of the *Mémoires* at this time were published about once every five years – the previous Tom. IX was published in 1780. The text and plates of each volume were probably printed over a period of years, so that the present work may have been printed in 1782 even though the complete volume of the *Mémoires* was not completed until three tears later.

Separate printings of Coulomb's other Academy memoirs are known, for example his *Mémoires sur l'Électricité et le Magnétism* (Horblit 31b; Dibner 58; Norman 527). Those printings, however, were probably assembled after their appearance in journal form and issued by the printer as collections, with new title pages, from leftover sheets from the journal publication as they are not separately-paginated and retain the signatures of the journal issue. The offered item is thus a highly unusual case of a true offprint preceding the journal issue by several years.
The present work was first published in book form in 1809, and again in 1821.

Norman 526 (journal issue); Roberts & Trent, p. 82 (1821 book edition). Gillmor, *Coulomb and the evolution of Physics and Engineering in 18th-century France*, 1972.


20 THÉORIE DES MACHINES SIMPLES.

XIX^me Expérience.

Le traîneau chargé, son poids compris de 1650 lb
Après un repos de 1', le frottement a été trouvé de 125 lb
Après un repos de 10'. 130
Après un repos de 80'. 145
Après 4 heures de repos. 200
Après 16 heures. 280
Après 4 jours. 340

Observations sur ces deux Expériences.

23. Ces deux expériences nous apprennent que des surfaces hétérogènes, telles que les bois & les métaux, n'acquièrent la limite de leur frottement qu'après un repos très-long; elles nous apprennent que les accroissemens des à trois ou quatre secondes de repos, sont insensibles. Si nous voulons déterminer la limite du frottement, en le supposant parvenu à son *maximum*, après quatre jours de repos, nous trouvons le rapport de la pression au frottement.

XVIII.^e Expérience. 11/20 550
XIX.^e Exp. ..... 140/348 ..... 435.

REMARQUE.

24. Le cuivre glissant sans enduit sur le chêne, donne des résultats analogues à ceux du fer glissant sur le même bois. Il paroît même que les accroissemens du frottement, relativement aux temps de repos, marche plus lentement pour le cuivre que pour le fer; parvenu à son *maximum*, le rapport de la pression au frottement est à peu près comme 5 : 2.

THÉORIE DES MACHINES SIMPLES. 21

Du frottement entre les métaux après un certain temps de repos.

Fig. 7.

25. L'on a cloué & fixé solidement sur le madrier dormant de notre table, deux règles de fer qui ont été dressées & polies avec le plus grand soin; elles avoient 4 pieds de longueur, 3 pouces de largeur & 3 lignes d'épaisseur; elles étoient placées à 10 pouces de distance l'une de l'autre, & elles répondoient, lorsque le traîneau étoit posé & emboîté le madrier dormant, aux deux règles de fer attachées sous le traîneau: l'on a arrondi toutes les arêtes pour qu'elles n'influassent point sur les frottemens. Les règles de fer attachées au traîneau avoient 18 lignes de largeur, 15 pouces de longueur; la surface de contact étoit de 45 pouces.

Fer contre fer.

XX.^me Expérience.

Le traîneau chargé, son poids compris de 51 lb
Après ; ' de repos, ou après un temps plus long, l'on a également trouvé pour le frottement. . . . . 15 lb

XXI.^me Expérience.

Le traîneau chargé, son poids compris de 450 lb
Le frottement acquiert dans cette expérience, comme dans celle qui précède, toute son intensité dans un instant; on le trouve de . . . . . 124 lb

Observations sur ces deux expériences.

26. Il ne m'a pas été possible de continuer les expériences du frottement pour le fer glissant sans enduit sur lui-même, sous des pressions plus considérables que celles de 450 livres. Sous de plus grandes pressions, le fer le rayoit, & les résultats devenoient incertains, le frottement augmentant très-considérablement, c'est ce qui est arrivé deux fois en répétant la vingt-unième expérience: mais une remarque qui a été com-

THE DISCOVERY OF WORMHOLES

EINSTEIN, Albert & ROSEN, Nathan. *The Particle Problem in the General Theory of Relativity.* Offprint from *Physical Review*, vol. 48, no. 1, 1 July, 1935. Lancaster, PA: American Physical Society, 1935. [Offered with:] **FLAMM, Ludwig.** *Beiträge zur Einsteinschen Gravitationstheorie.* Offprint from *Physikalische Zeitschrift*, 17 Band, 1916. Leipzig: S. Hirzel, 1916.

$3,500

[Einstein/Rosen:] 4to (266 x 201 mm), pp. 73-77. Original printed wrappers (very lightly sunned and some tiny spots). [Flamm:] 4to (276 x 196 mm), pp. [1] 2-6. Original printed wrappers, a little fraying to the bottom of wrappers, slight crease from having been folded for postage.



First edition, very rare offprints, of the theoretical discovery of the concept of a 'wormhole,' "a hypothetical 'tunnel' connecting two different points in space-time in such a way that a trip through the wormhole could take much less time than a journey between the same starting and ending points in normal space. The ends of a wormhole could, in theory, be *intra-universe* (i.e. both exist in the same universe) or *inter-universe* (exist in different universes, and thus serve as a connecting passage between the two) … The theory of wormholes goes back to 1916, shortly after Einstein published his general theory [of relativity], when Ludwig Flamm, an obscure Austrian physicist, looked at the simplest possible solution of Einstein's field equations, known as the Schwarzschild solution (or Schwarzschild metric). This describes the gravitational field around a spherically-symmetric non-rotating mass. If the mass is sufficiently compact, the solution describes a particular form of the phenomenon now called a black hole – the

Schwarzschild black hole. Flamm realized that Einstein's equations allowed a second solution, now known as a white hole, and that the two solutions, describing two different regions of (flat) space-time were connected (mathematically) by a space-time conduit" (daviddarling.info/encyclopedia/W/wormhole.html). Flamm's paper (offered here) made little impact until 1935 when a similar idea was explored by Einstein and his student Nathan Rosen, who studied "the theory of intra- or inter-universe connections in a paper whose actual purpose was to try to explain fundamental particles, such as electrons, in terms of space-time tunnels threaded by electric lines of force. Their work gave rise to the formal name 'Einstein-Rosen bridge' for what the physicist John Wheeler would later call a 'wormhole'" (*ibid.*). No copies of either offprint listed on ABPC/RBH.

Provenance: This copy of the Einstein/Rosen offprint was acquired as part of the offprint collection of the Austro-Dutch physicist Paul Ehrenfest (1880-1933), one of Einstein's closest friends and colleagues. Einstein continued to send offprints to Ehrenfest's widow after his death two years before the present paper was published. Ehrenfest met Einstein for the first time in 1912 in Prague, where Einstein spent a year as full professor, and the two men remained close friends thereafter. In 1922, Einstein and Ehrenfest published a joint paper in *Zeitschrift für Physik* which attempted to explain the Stern-Gerlach experiment, the results of which had been published just weeks earlier. Their paper can be considered the first significant contribution to the quantum measurement problem.

"In the last decades of his life, Albert Einstein tried endlessly to unify electromagnetism with his own theory of gravity, general relativity. These efforts are mostly now regarded as quixotic, but a short proposal written in 1935 with a colleague has survived in unlikely fashion as the source of science-fiction ideas for speeding across the universe by means of "wormholes" through spacetime. From the modern perspective, the paper also illustrates how general relativity posed mathematical and conceptual difficulties that foxed even its creator.

"Einstein and Nathan Rosen, both at the Institute for Advanced Study in Princeton, wanted to rid physics of singularities–points where mathematical quantities become infinite or otherwise ill-defined–such as the concept of a particle that has all its mass concentrated into an infinitely small geometrical point. In general relativity, a point mass curves spacetime around it in a way that was calculated by Karl Schwarzschild in 1916. The Schwarzschild solution has mathematical singularities both at zero and at the so-called Schwarzschild radius.

"Reinterpretation of the Schwarzschild solution avoids these singularities, Einstein and Rosen argued in their 1935 *Phys. Rev*. paper [offered here]. They imagined a path tracing radially inward. Instead of trying to cross the imaginary spherical shell at the singular radius and proceeding down to the center, Einstein and Rosen showed how to match the path onto another track that emerges outward again–but into a separate section of spacetime. Imagine funnel shapes pulled out of two adjacent rubber sheets and connected at their necks, providing a continuous, tube-shaped path from one surface to the other. This construction makes a smooth connection or bridge between two distinct pieces of spacetime. "Viewed from afar, either part of this solution represents the gravitational effect of a mass because spacetime is strongly curved, but no physical body is present. Einstein and Rosen added an electromagnetic field to their solution, so that it could also represent a charged body. They hoped their construction would offer a starting point for a unified theory of gravity and electromagnetism based purely on fields, avoiding point particles and the singularities that came with them.

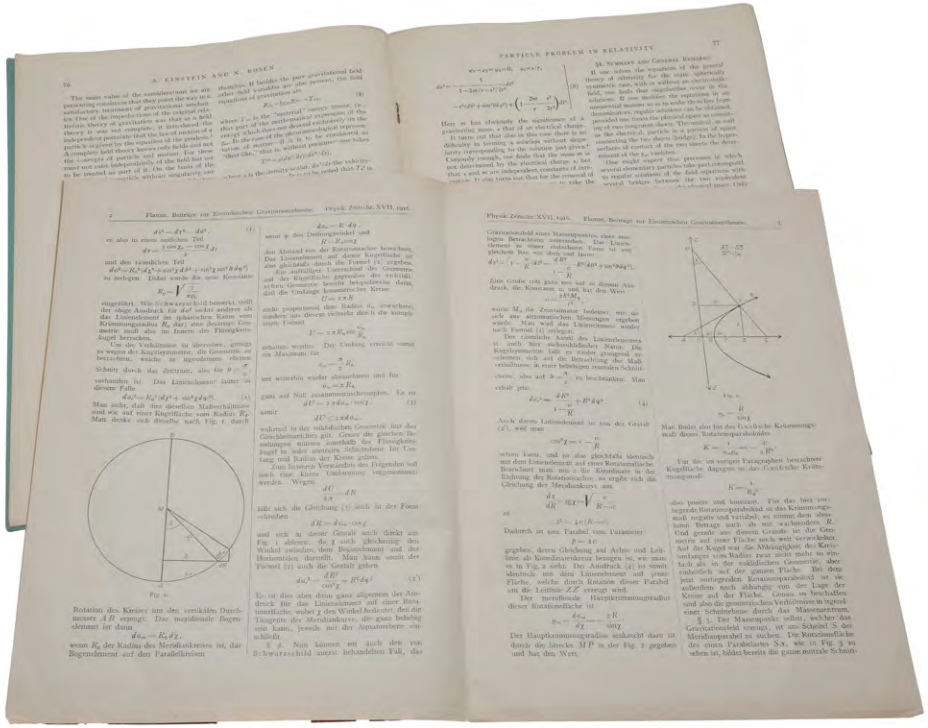
"Not until 1939 was the modern idea of a black hole broached, and only later were the subtleties of the Schwarzschild solution fully understood. The singular radius that Einstein and Rosen worked hard to avoid became the black hole's

event horizon. Although it is a one-way surface–light can pass across it going inward, but cannot come out–all physical quantities remain well defined at the event horizon. No true singularities arise there.

"Further theoretical work showed that the Einstein-Rosen "wormhole" is not, contrary to outward appearances, a stable structure. For an observer trying to pass through, the wormhole opens up and closes too quickly for even a photon to get through. Later work suggested that exotic forms of energy threaded through a wormhole might keep it open but it remains unclear whether such arrangements are physically feasible.

"Although a 1916 paper by Ludwig Flamm from the University of Vienna [the second offered paper] is sometimes cited as giving the first hint of a wormhole, "you definitely need hindsight to detect it," says Matt Visser of Victoria University in Wellington, New Zealand. Einstein and Rosen were the first to take the idea seriously and to try to accomplish some physics with it, he adds. The original goal may have faded, but the Einstein-Rosen bridge still pops up occasionally as a handy solution to the pesky problem of intergalactic travel" (*American Physical Society, Focus: The Birth of Wormholes*).

Weil 196.



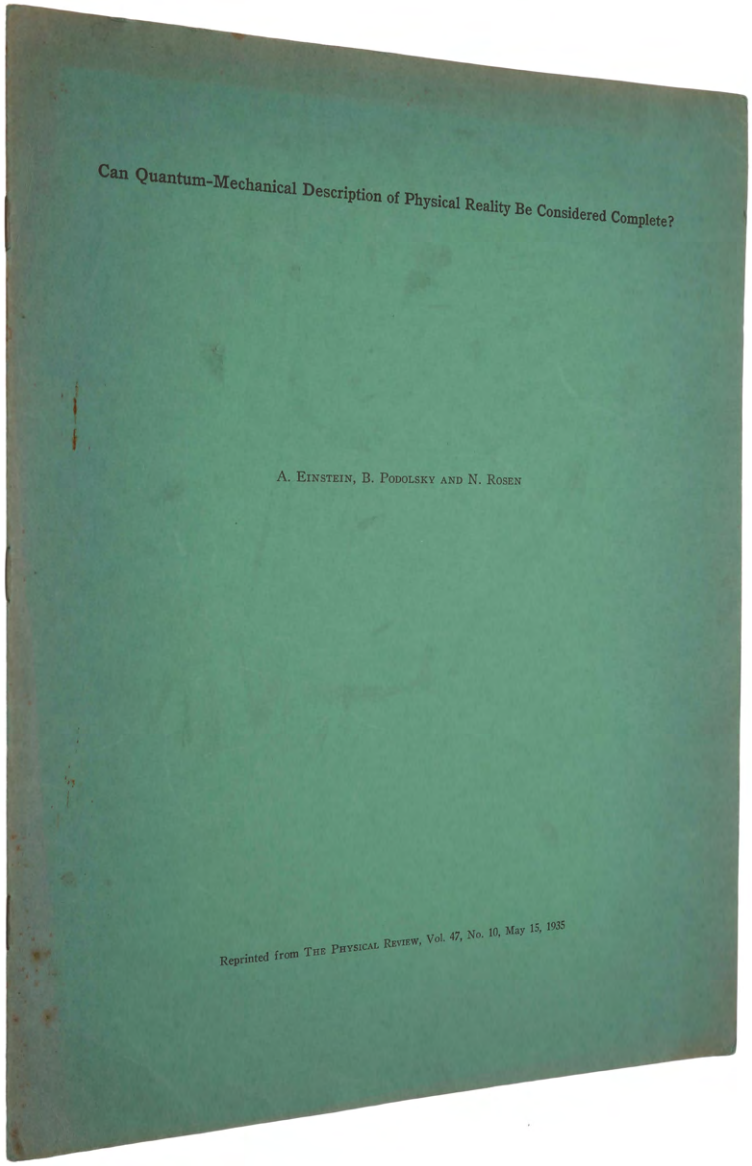
THE EPR-EXPERIMENT

EINSTEIN, Albert, PODOLSKY, Boris & ROSEN, Nathan. *Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?* Offprint from *Physical Review,* vol. 47, no. 10, May 15, 1935. Lancaster, PA: APS, 1935.

$18,500

4to (267 x 200 mm), pp. [777] 778-780. Original printed wrappers, slightly sunned and some very small rust stains from staples.

First edition, very rare offprint, of the famous 'EPR paper', one of the most discussed and debated papers of modern physics, and the foundation for the new fields of quantum computing and cryptography. "In the May 15, 1935 issue of *Physical Review* Albert Einstein co-authored a paper with his two postdoctoral research associates at the Institute for Advanced Study, Boris Podolsky and Nathan Rosen. The article was entitled 'Can Quantum Mechanical Description of Physical Reality Be Considered Complete?'. Generally referred to as 'EPR', this paper quickly became a centrepiece in the debate over the interpretation of the quantum theory, a debate that continues today. The paper features a striking case where two quantum systems interact in such a way as to link both their spatial coordinates in a certain direction and also their linear momenta (in the same direction). As a result of this 'entanglement', determining either position or momentum for one system would fix (respectively) the position or the momentum of the other. EPR use this case to argue that one cannot maintain both an intuitive condition of local action and the completeness of the quantum description by means of the wave function." (*Stanford Encyclopedia of Philosophy*). "The EPR paradox inspired many authors afterwards; in particular, discussion emerged on the revival of the hidden parameter idea by David Bohm and others after the early 1950's. John



Bell's analysis of the situation in the 1960's showed that hidden variables resulted in an inequality (for the 'local condition') which could be tested by experiment and found not to be satisfied" (Pais, *Twentieth Century Physics*, I, p. 229). ABPC/RBH record only the Plotnick copy (Christie's, 4 October 2002, $4183).

Provenance: This copy was acquired as part of the offprint collection of the Austro-Dutch physicist Paul Ehrenfest (1880-1933), one of Einstein's closest friends and colleagues. Einstein continued to send offprints to Ehrenfest's widow after his death two years before the present paper was published. Ehrenfest met Einstein for the first time in 1912 in Prague, where Einstein spent a year as full professor, and the two men remained close friends thereafter. In 1922, Einstein and Ehrenfest published a joint paper in *Zeitschrift für Physik* which attempted to explain the Stern-Gerlach experiment, the results of which had been published just weeks earlier. Their paper can be considered the first significant contribution to the quantum measurement problem and a precursor to the EPR paper.

"By the 1920s, it had become clear to most physicists that classical mechanics could not fully describe the world of atoms, especially the notion of "quanta" first proposed by Planck and further developed by Albert Einstein to explain the photoelectric effect. Physics had to be rebuilt, leading to the emergence of quantum theory.

"Werner Heisenberg, Niels Bohr and others who helped create the theory insisted that there was no meaningful way in which to discuss certain details of an atom's behavior: for example, one could never predict the precise moment when an atom would emit a quantum of light. But Einstein could never fully accept this innate uncertainty, once famously declaring, "God does not play dice." He wasn't alone in his discomfort: Erwin Schrödinger, inventor of the wave function, once declared of quantum mechanics, "I don't like it, and I'm sorry I ever had anything to do with it."

"In a 1935 paper, Einstein, Boris Podolsky and Nathan Rosen introduced a thought experiment to argue that quantum mechanics was not a complete physical theory. Known today as the "EPR paradox," the thought experiment was meant to demonstrate the innate conceptual difficulties of quantum theory. It said that the result of a measurement on one particle of an entangled quantum system can have an instantaneous effect on another particle, regardless of the distance of the two parts.

"One of the principal features of quantum mechanics is the notion of uncertainty: not all the classical physical observable properties of a system can be simultaneously determined with exact precision, even in principle. Instead, there may be several sets of observable properties–position and momentum, for example–that cannot both be known at the same time. Another peculiar property of quantum mechanics is entanglement: if two photons, for example, become entangled –that is, they are allowed to interact initially so that they will subsequently be defined by a single wave function–then once they are separated, they will still share a wave function. So measuring one will determine the state of the other: for example, with a spin-zero entagled state, if one particle is measured to be in a spin-up state, the other is instantly forced to be in a spin-down state. "This is known as "nonlocal behavior;" Einstein dubbed it "spooky action at a distance." It appears to violate one of the central tenets of relativity: information can't be transmitted faster than the speed of light, because this would violate causality.

"It's worth noting that Einstein wasn't attempting to disprove quantum mechanics; he acknowledged that it could, indeed, predict the outcomes of various experiments. He was merely troubled by the philosophical interpretations of the theory, and argued that, because of the EPR paradox, quantum mechanics could not be considered a complete theory of nature. Einstein postulated the existence of hidden variables: as yet unknown local properties of the system which should account for the discrepancy, so that no instantaneous spooky action would be

necessary. Bohr disagreed vehemently with this view and defended the far stricter Copenhagen interpretation of quantum mechanics. The two men often argued passionately about the subject, especially at the Solvay Conferences of 1927 and 1930; neither ever conceded defeat.

"There have been numerous theoretical and experimental developments since Einstein and his colleagues published their original EPR paper, and most physicists today regard the so-called "paradox" more as an illustration of how quantum mechanics violates classical physics, rather than as evidence that quantum theory itself is fundamentally flawed, as Einstein had originally intended.

"But the paper did help deepen our understanding of quantum mechanics by exposing the fundamentally non-classical characteristics of the measurement process. Before that paper, most physicists viewed a measurement as a physical disturbance inflicted directly on the measured system: one shines light onto an electron to determine its position, but this disturbs the electron and produces uncertainties. The EPR paradox shows that a "measurement" can be performed on a particle without disturbing it directly, by performing a measurement on a distant entangled particle.

"Today, quantum entanglement forms the basis of several cutting-edge technologies. In quantum cryptography, entangled particles are used to transmit signals that cannot be intercepted by an eavesdropper without leaving a trace. The first viable quantum cryptography systems are already being used by several banks. And the burgeoning field of quantum computation uses entangled quantum states to perform computational calculations in parallel, so that some types of calculations can be done much more quickly than could ever be possible using classical computers" (*American Physical Society, This Month in Physics History: Einstein and the EPR Paradox* - https://www.aps.org/publications/apsnews/200511/history.cfm).

Weil *195.



MAY 15, 1935 PHYSICAL REVIEW VOLUME 47
Printed in U. S. A.

Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?

A. EINSTEIN, B. PODOLSKY AND N. ROSEN, *Institute for Advanced Study, Princeton, New Jersey*
(Received March 25, 1935)

In a complete theory there is an element corresponding to each element of reality. A sufficient condition for the reality of a physical quantity is the possibility of predicting it with certainty, without disturbing the system. In quantum mechanics in the case of two physical quantities described by non-commuting operators, the knowledge of one precludes the knowledge of the other. Then either (1) the description of reality given by the wave function in quantum mechanics is not complete or (2) these two quantities cannot have simultaneous reality. Consideration of the problem of making predictions concerning a system on the basis of measurements made on another system that had previously interacted with it leads to the result that if (1) is false then (2) is also false. One is thus led to conclude that the description of reality as given by a wave function is not complete.

1.

ANY serious consideration of a physical theory must take into account the distinction between the objective reality, which is independent of any theory, and the physical concepts with which the theory operates. These concepts are intended to correspond with the objective reality, and by means of these concepts we picture this reality to ourselves.

In attempting to judge the success of a physical theory, we may ask ourselves two questions: (1) "Is the theory correct?" and (2) "Is the description given by the theory complete?" It is only in the case in which positive answers may be given to both of these questions, that the concepts of the theory may be said to be satisfactory. The correctness of the theory is judged by the degree of agreement between the conclusions of the theory and human experience. This experience, which alone enables us to make inferences about reality, in physics takes the form of experiment and measurement. It is the second question that we wish to consider here, as applied to quantum mechanics.

Whatever the meaning assigned to the term *complete*, the following requirement for a complete theory seems to be a necessary one: *every element of the physical reality must have a counterpart in the physical theory*. We shall call this the condition of completeness. The second question is thus easily answered, as soon as we are able to decide what are the elements of the physical reality.

The elements of the physical reality cannot be determined by *a priori* philosophical considerations, but must be found by an appeal to results of experiments and measurements. A comprehensive definition of reality is, however, unnecessary for our purpose. We shall be satisfied with the following criterion, which we regard as reasonable. *If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.* It seems to us that this criterion, while far from exhausting all possible ways of recognizing a physical reality, at least provides us with one

EINSTEIN'S TURBULENT PRIVATE LIFE

EINSTEIN, Albert. *ALS in German, one leaf with 31 lines on recto plus eight lines and signature on verso, signed 'Dein Albert', Le Coq (Belgium), 19. VII. [19]33.*

$28,500

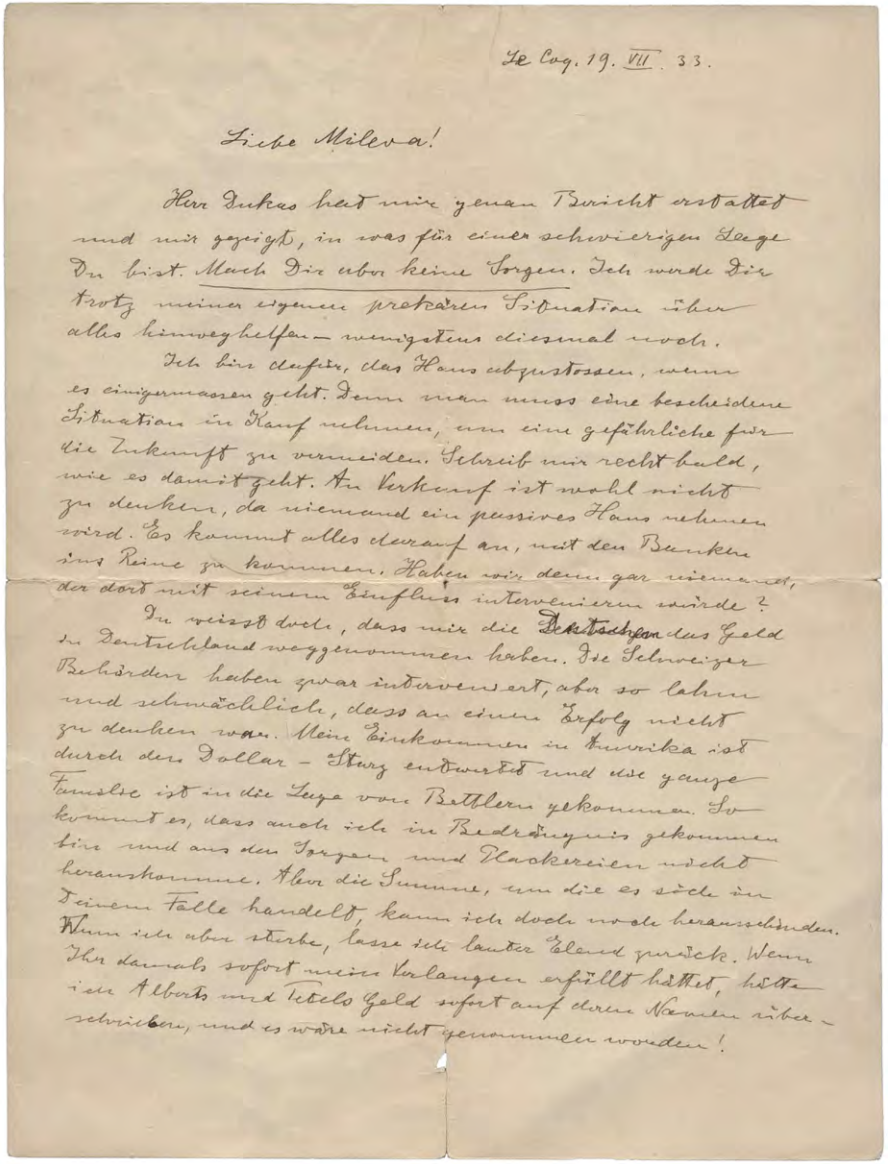
One leaf (270 x 205 mm), previously folded with slight wear at creases and small hole not affecting text, slightly soiled on verso.

A fascinating, if somewhat depressing letter showing the dire financial straits in which Einstein's ex-wife Mileva, and their two sons, found themselves in as the Nazi's came to power in Germany. Einstein had already moved to the United States with his new wife Else, but had returned to Europe in 1933 for a visit, during which time this letter was written. Einstein ends with his best wishes for their youngest son, Eduard ("Tetel"), who had been institutionalized in 1922 after developing schizophrenia. Sadly, Eduard's release from the institution to which Einstein refers was only temporary.

Dear Milena!

Mr Dukas gave me an exact report and showed me what a difficult situation you're in. But don't worry. In spite of my own precarious situation, I'll tide you over everything – at least this time.

I opt for getting rid of the house if that's feasible somehow. Because one must make do with a poor situation in order to avoid a dangerous one in the future. Please write soon and let me know how things go with that. Selling it is hardly imaginable



Le Coq, 19. VII. 33.

Liebe Mileva!

Herr Dukas hat mir genau Bericht erstattet und mir gezeigt, in was für einer schwierigen Lage Du bist. Mach Dir aber keine Sorgen. Ich werde Dir trotz meiner eigenen prekären Situation über alles hinweghelfen – wenigstens diesmal noch.

Ich bin dafür, das Haus abzustossen, wenn es einigermassen geht. Denn man muss eine beschiedene Situation in Kauf nehmen, um eine gefährliche für die Zukunft zu vermeiden. Schreib mir recht bald, wie es damit geht. An Verkauf ist wohl nicht zu denken, da niemand ein passives Haus nehmen wird. Es kommt alles darauf an, mit den Banken ins Reine zu kommen. Haben wir denn gar niemand, der dort mit seinem Einfluss intervenieren würde?

Du weisst doch, dass mir die Deutschen das Geld in Deutschland weggenommen haben. Die Schweizer Behörden haben zwar interveniert, aber so lahm und schmächtlich, dass an einen Erfolg nicht zu denken war. Mein Einkommen in Amerika ist durch den Dollar-Sturz entwertet und die ganze Familie ist in die Lage von Bettlern gekommen. So kommt es, dass auch ich in Bedrängnis gekommen bin und aus den Sorgen und Plackereien nicht herauskomme. Aber die Summe, um die es sich in diesem Falle handelt, kann ich doch wohl herausschinden. Wenn ich aber sterbe, lasse ich lauter Elend zurück. Wenn Ihr damals sofort meinem Verlangen erfüllt hättet, hätte ich Alberts und Tetels Geld sofort auf deren Namen überschrieben, und es wäre nicht genommen worden!

because nobody will take a house burdened with debt. Everything depends on making settlements with the banks. Don't we have anybody influential who would intervene there?

You know how the Germans took my money in Germany. The Swiss authorities did intervene, but in such a lame and weak manner that success was hardly to be expected. My revenues in America have been devalued by the dollar crash, and the whole family are now in a position of beggars. That's how I've ended in hardship, too, and cannot escape the worries and drudgery. Nevertheless, I can elicit the lump sum we're talking about in your case. But if I die, all I'll leave behind is misery. If you had fulfilled my request back then, I would have transferred Albert and Tetel's money in their name immediately, and it wouldn't have been taken away!

[Page 2]

I am very happy to hear that Tetel will be temporarily released from the institution on a trial basis. If this turns out a success, it would be great bliss. Please send my warmest regards to him!

All the best, and chin up!

Sincerely, Albert.

Please write to me as soon as things clear up, so I can make quick decisions. You never know what comes next!

Albert Einstein (1879-1955) had a turbulent private life. While studying at the ETH in Zurich, he fell in love with Mileva Marić (1875-1948), a fellow student who came from Serbia. She was one of the first women to study physics there. These first years of their relationship were warm and passionate. Mileva gave birth to their daughter, Lieserl, in 1902, yet because neither of them had a secure income at the time, they did not marry until a year later. All traces of Lieserl after the age of two seem to have been lost. There has been some speculation as to her fate – she may have been put up for adoption or she may have died at a young age. Albert and Mileva went on to have two sons, Hans Albert (1904-73) and Eduard (1910-65).

During the years in which Einstein developed his early revolutionary theories, Mileva functioned as a sounding board for his ideas. Yet, there is no evidence that she participated in his scientific work. Apparently, Mileva never got over the loss of her daughter and increasingly suffered from depression. By around 1909 their relationship began to deteriorate. They became increasingly estranged and in 1912 Albert became involved with his cousin, Elsa Loewenthal. Upon Einstein's move to Berlin in 1914, he and Mileva separated. They were divorced in 1919, and soon afterwards he married Else, who had two daughters, Margot and Ilse, from a previous marriage. Albert's sons remained with Mileva in Zurich, yet he continued to be in contact with them.

After the couple's separation, Mileva suffered a nervous breakdown in spring 1916 and spent six months in recovery in a sanatorium while a housekeeper and friends cared for the boys. The following year she complained of heart problems, and at other times she suffered from back and spinal pain, headaches, glandular swelling, the flu, a neck and jaw infection, and scrofula, a form of tuberculosis affecting the lymph nodes. She was born with a luxated hip that caused a lifelong limp. And she continued to suffer bouts of depression. Throughout her life as a mother, she also had the burden of being primary carer for Eduard, a frail and sensitive boy who was diagnosed with schizophrenia at the age of twenty when he was a medical student. Accounts from various contemporaries paint a picture of

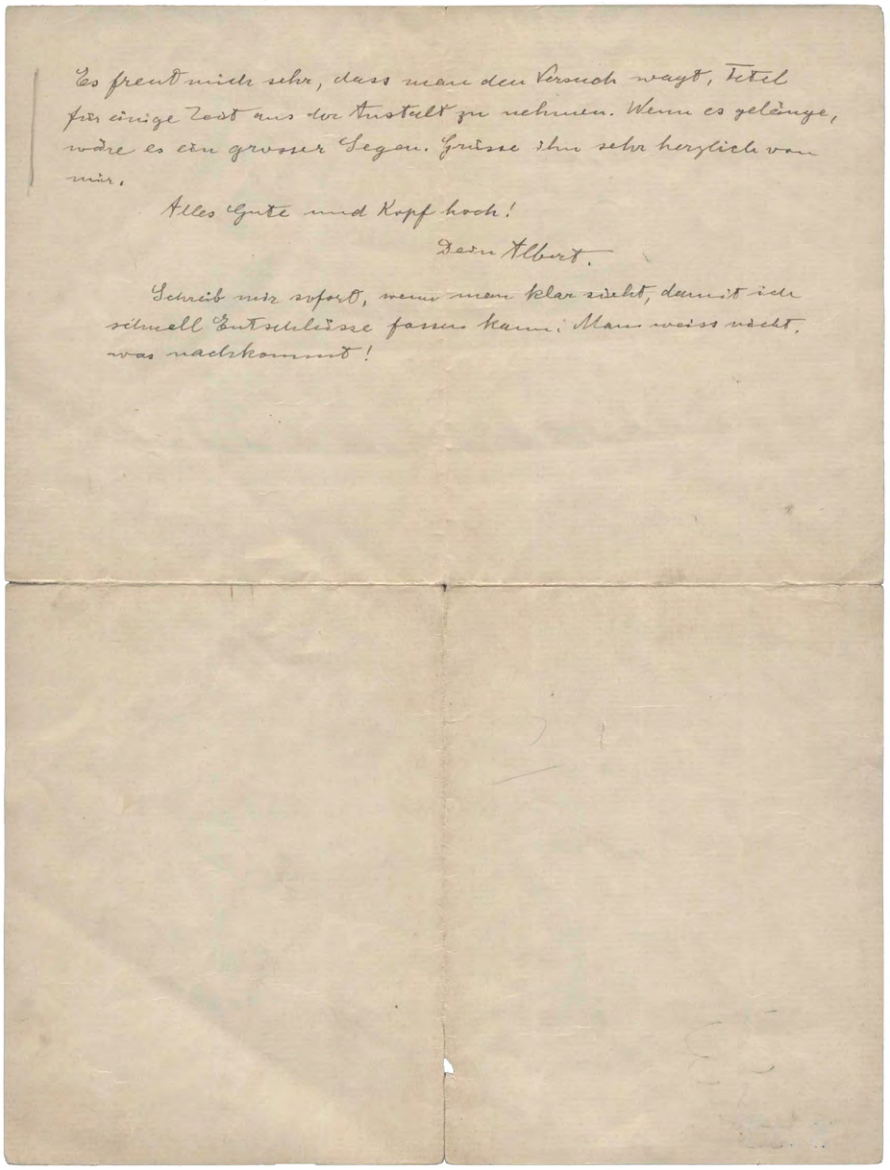
an embittered woman, though her relationship with Einstein achieved a kind of equilibrium of mutual respect.

In 1922, Einstein gave Mileva the yield from his Nobel Prize money, in accordance with their divorce settlement, controlling but not keeping the principal. (By 1919 he had already been nominated for the Prize several times so it was assumed he would eventually receive the honour.) Mileva bought income property for herself and the boys – including a five-unit apartment house in Zurich – and Einstein also continued to pay the family's considerable medical bills.

Einstein had not been entirely surprised when his son, "Tetel" ("little") was diagnosed with schizophrenia. He recognized that there was a family disposition, on the maternal side, toward mental illness; Mileva's sister, Zorka, also suffered from schizophrenia. "Tetel" had wanted to be a psychiatrist and was in medical school when afflicted with his first breakdown in 1930 – triggered, apparently, by an unhappy love affair with an older woman. By 1932 he was committed to "Burgholzli," the University of Zurich psychiatric hospital, and so began the pattern of institutionalization which would constitute the sad story of his life.

Einstein left Europe on December 14, 1932 with his secretary Helen Dukas and they arrived in Los Angeles on January 9, 1933. He remained in Pasadena until early March 1933, when he decided to return to Europe, despite warnings for his safety after Adolph Hitler came to power on January 30, 1933. In May, Albert visited Eduard at Burghölzli. Albert played his violin for his son but received no response at all. It was the last time they saw each other.

Calaprice et al, *An Einstein Encyclopedia*, 2015. Highfield & Carter, *The Private Lives of Albert Einstein*, 1993. Milentijević, *Mileva Marić Einstein: Life with Albert Einstein*, 2015.


Es freut mich sehr, dass man den Versuch wagt, Tetel
für einige Zeit aus der Anstalt zu nehmen. Wenn es gelänge,
wäre es ein grosser Segen. Grüsse ihn sehr herzlich von
mir.

Alles Gute und Kopf hoch!

Dein Albert.

Schreib mir sofort, wenn man klar sieht, damit ich
schnell Entschlüsse fassen kann. Man weiss nicht,
was nachkommt!

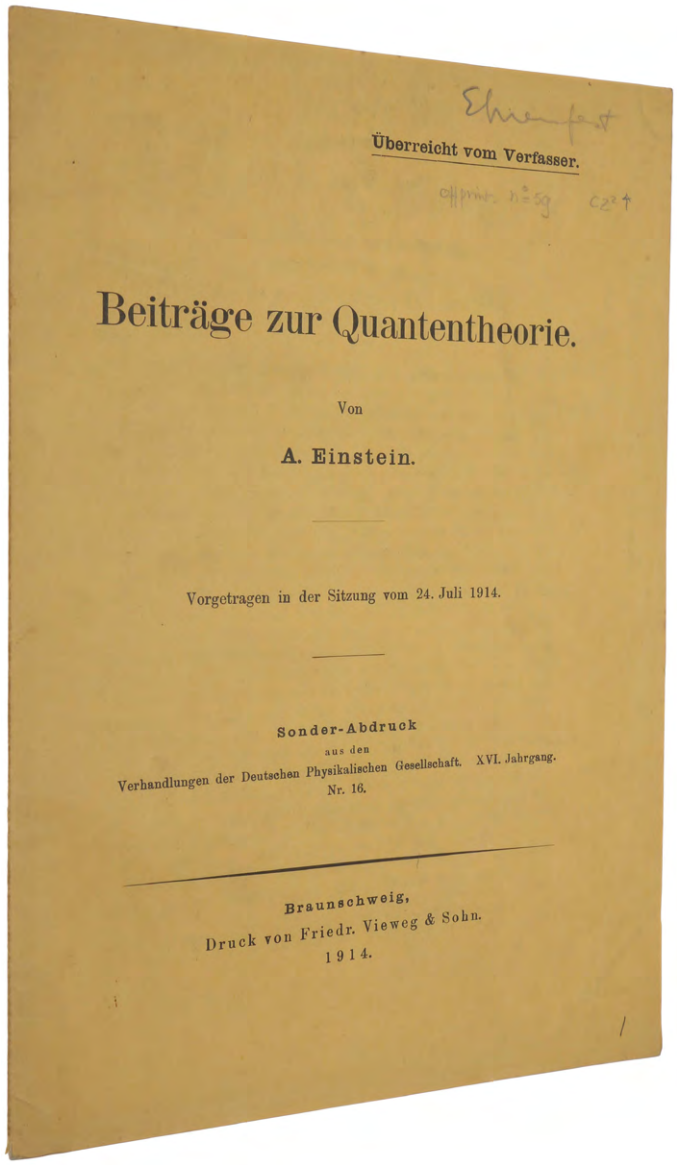
AUTHOR'S PRESENTATION OFFPRINT ANNOTATED BY PAUL EHRENFEST

EINSTEIN, Albert. *'Beiträge zur Quantentheorie.' Offprint from Verhandlungen der Deutschen Physikalischen Gesellschaft, 16. Band, 1914.* Braunschweig: Vieweg & Sohn, 1914.

$18,500

8vo (230 x 155 mm), pp. [1:blank] 820-828. Original printed wrappers. A very fine and fresh copy.

First edition, extremely rare author's presentation offprint (with 'Überreicht vom Verfasser' (Presented by the Author) printed on front wrapper), and signed and annotated by Einstein's close friend and colleague Paul Ehrenfest, of this crucial transitional paper in which Einstein uses the light quantum hypothesis to give new derivations of Planck's radiation law and Nernst's third law of thermodynamics (Einstein points out that the alleged 'proofs' which try to derive the theorem of Nernst from the mere fact that the specific heat of all substances goes to zero at absolute zero temperature, are not genuine). Einstein had first put forward the idea of light quanta in 1905, but in later years he came to doubt the validity of the hypothesis, despite its earlier success in explaining the photoelectric effect. Einstein's success in the present paper in deriving two of the most important achievements of quantum theory using the light quantum hypothesis re-established his confidence in that hypothesis, and he began to think again about the interaction between radiation and matter, resulting two years later in his great papers on the quantum theory of radiation. The only other copy we have located in auction records is that in Einstein's own reference collection of offprints


Ehrenfest

Überreicht vom Verfasser.

Beiträge zur Quantentheorie.

Von

A. Einstein.

Vorgetragen in der Sitzung vom 24. Juli 1914.

Sonder-Abdruck
aus den
Verhandlungen der Deutschen Physikalischen Gesellschaft. XVI. Jahrgang.
Nr. 16.

Braunschweig,
Druck von Friedr. Vieweg & Sohn.
1914.

formerly in the Richard Green Library (sold Christie's New York, 17 June 2008, lot 100). OCLC lists three copies in US (American Philosophical Society, Florida, Princeton), one in UK and one in Switzerland.

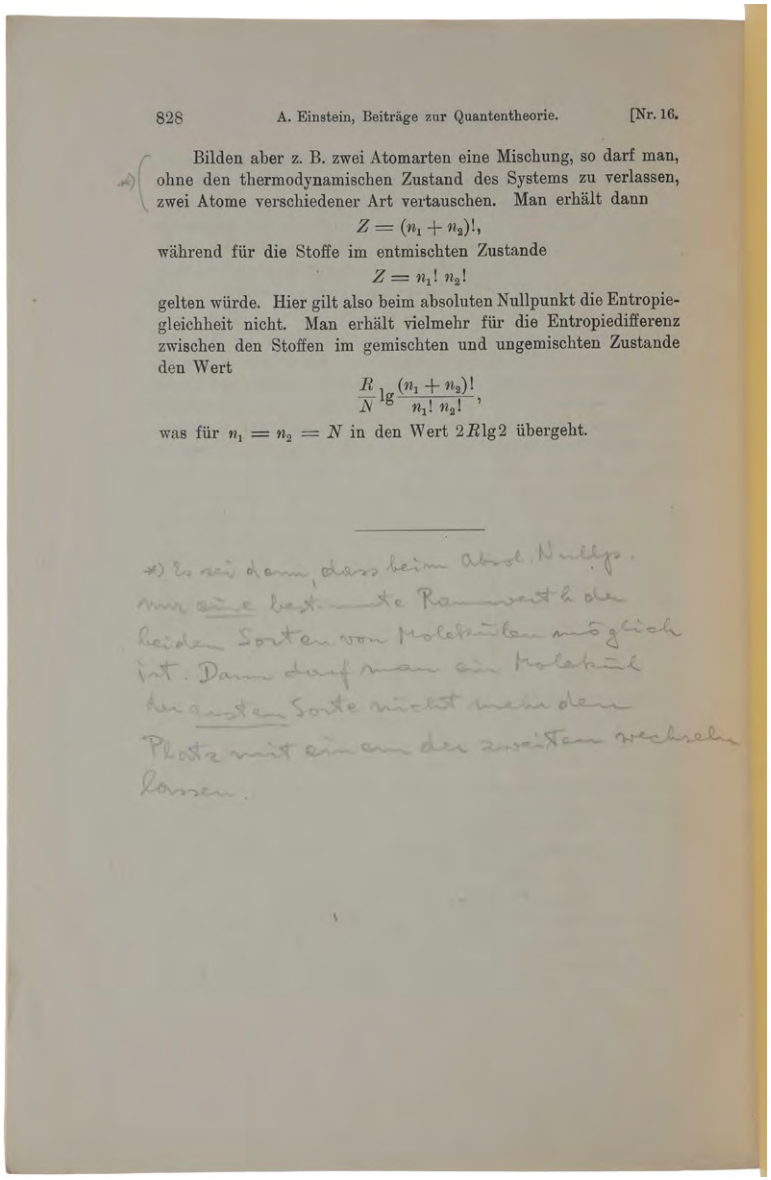
Provenance: The Austro-Dutch physicist Paul Ehrenfest (1880-1933) ('Ehrenfest' written in his hand on upper wrapper; seven lines in his hand written in pencil on last page of text, commenting on the first three lines of text on the page – see below). Ehrenfest met Einstein for the first time in 1912 in Prague, where Einstein spent a year as full professor, and the two men remained close friends thereafter. When Einstein returned to Zürich in July 1912, he recommended Ehrenfest to succeed him in his position in Prague, but this was prevented by Ehrenfest's declaration that he was an atheist. However, just at this time Lorentz resigned his position as professor at the University of Leiden, and on his advice Ehrenfest was appointed as his successor. He remained there for the rest of his career. Einstein said of him: "He was not merely the best teacher in our profession whom I have ever known; he was also passionately preoccupied with the development and destiny of men, especially his students. To understand others, to gain their friendship and trust, to aid anyone embroiled in outer or inner struggles, to encourage youthful talent — all this was his real element, almost more than his immersion in scientific problems" … On his invitation Einstein accepted in 1920 an appointment as extraordinary professor at the University of Leiden. This arrangement allowed Einstein to visit Leiden for a few weeks every year. At these occasions Einstein would stay at Ehrenfest's home. In 1923 Einstein stayed there for six weeks, after German ultra-nationalists in Berlin had made threats against his life. On the occasion of the 50th anniversary of Lorentz' doctorate (December 1925) Ehrenfest invited both Bohr and Einstein over to Leiden, in an attempt to reconcile their scientific differences about the emerging quantum theory. These discussions were continued at the 1927 Solvay Conference, where Ehrenfest much to his dismay had to side with Bohr's position in this great debate" (Wikipedia, accessed 29 May 2017). In 1922, Einstein and Ehrenfest published a joint paper in *Zeitschrift für Physik* which attempted to explain the Stern-Gerlach experiment, the results of which had been published just weeks earlier. Their paper can be considered the first significant contribution to the quantum measurement problem.

In 1909 Einstein wrote Lorentz that he had never believed in independent, localized light quanta, because, among other reasons, this concept was incompatible with the division of rays during refraction. In July 1910 he wrote to Sommerfeld: "To me the basic question is: 'Is there a way to unify the energy quanta and Huygens's principle!' Appearances are against it, but the good Lord has found the trick." He soon retreated from this quantum view and examined another revolutionary possibility: "At present," he wrote to Laub in November, "I am very hopeful to solve the radiation problem, without *light quanta*. I am exceedingly curious to see how the thing evolves. Even the energy principle in its present form would have to be given up." Perhaps he had in mind a virtual, wavelike radiation field correlating quantum jumps in distant molecules, as in the later theory of Bohr, Kramers, and Slater. A week later, Einstein renounced this new attempt: "Again, the solution of the radiation problem has come to naught. The devil has indulged in a rotten trick with me." Six months later, Einstein confided to Michel Besso his doubts on the existence of quanta in general: "I no longer ask myself if these quanta really exist. And I do not try any more to construct them because I now know that my brain is unable to do it. But I still explore the consequences as carefully as I can to learn the range of validity of this idea." In February 1912 he wrote to Hopf: "Quanta certainly do what they ought to, but they do not exist, like the immovable ether. At the moment, the latter is turning diligently in its grave intending to come to life again — poor fellow." In 1913, in a more pronounced retreat from quantum discontinuity, Einstein and Otto Stern derived Planck's law without quantization at all.

"Einstein forcefully reasserted the reality of quanta in a publication of 1914 [the offered paper]. Perhaps Niels Bohr's new theory of spectra encouraged him to do so, although there is no trace in his writings of any reflections on this theory before 1916. Perhaps he realized that the zero-point energy failed to solve quantum difficulties that involved other entities than resonators. In any case, he now lent so much reality to the quantum states of a micro-entity as to compare them with different chemical species. This view induced the profound comment: "The concepts of physical and chemical change seem to lose their fundamental difference" [the present paper, pp. 822-3]. For instance, a quantum jump in a resonator and the dissociation of a molecule are comparable processes, for they are both caused by the absorption of an energy quantum.

"Einstein immediately applied this analogy to a new, non-statistical derivation of Planck's formula for the average energy of a harmonic oscillator at temperature T. If the various energy levels nhv of a resonator are identified with different chemical species, a thermalized set of resonators is comparable to a chemical mixture in equilibrium. According to the laws of chemical equilibrium, the free energy of the mixture must be a minimum, which implies that the concentration of the species nhv should be proportional to e-nhv/kT. Consequently, the average energy of the resonators must be $U = 1/(e^{h\nu/kT} - 1)$, in conformity with Planck's result of 1900.

"Through this reasoning, Einstein confirmed the quantum-theoretical version of Gibbs's canonical law, according to which the probability of the discrete energy value E_n is proportional to $e^{-En/kT}$ for a (non-degenerate) system in contact with a thermostat at temperature T. He still did not know how to proceed from the quantized resonators to the black-body law … A new, quantum-theoretical picture of the interaction between matter and radiation was needed. Einstein found it in the summer of 1916, after the completion of his new theory of gravitation left



828 A. Einstein, Beiträge zur Quantentheorie. [Nr. 16.

Bilden aber z. B. zwei Atomarten eine Mischung, so darf man, ohne den thermodynamischen Zustand des Systems zu verlassen, zwei Atome verschiedener Art vertauschen. Man erhält dann

$$Z = (n_1 + n_2)!,$$

während für die Stoffe im entmischten Zustande

$$Z = n_1!\, n_2!$$

gelten würde. Hier gilt also beim absoluten Nullpunkt die Entropiegleichheit nicht. Man erhält vielmehr für die Entropiedifferenz zwischen den Stoffen im gemischten und ungemischten Zustande den Wert

$$\frac{R}{N} \lg \frac{(n_1 + n_2)!}{n_1!\, n_2!},$$

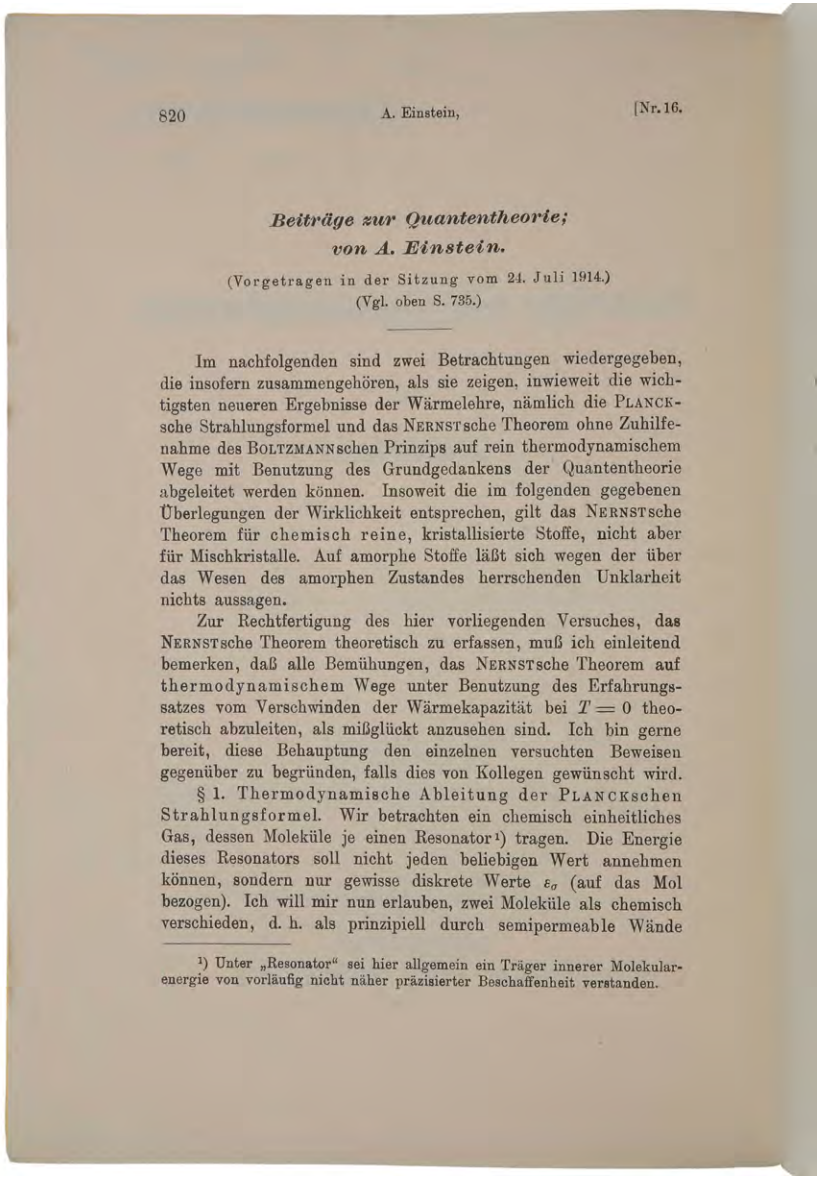
was für $n_1 = n_2 = N$ in den Wert $2R\lg 2$ übergeht.

*) Es sei dann, dass beim Abs. Nullp. nur eine bestimmte Raumverteilung der beiden Sorten von Molekülen möglich ist. Dann darf man ein Molekül der ersten Sorte nicht mehr dem Platz mit einem der zweiten wechseln lassen.

him more time for quantum meditation" (*Cambridge Companion to Einstein*, pp. 133-4).

Ehrenfest's annotations on p. 828 refer to Einstein's statement at the top of that page, according to which the thermodynamic state of the system will be unchanged when two different kinds of molecules are interchanged. Ehrenfest remarks that one cannot interchange a molecule of the first kind with one of the second if the spatial separation between the two is too large.

Weil, *Albert Einstein Bibliography*, 67.



820 A. Einstein, [Nr. 16.

Beiträge zur Quantentheorie;
von A. Einstein.

(Vorgetragen in der Sitzung vom 24. Juli 1914.)
(Vgl. oben S. 735.)

Im nachfolgenden sind zwei Betrachtungen wiedergegeben, die insofern zusammengehören, als sie zeigen, inwieweit die wichtigsten neueren Ergebnisse der Wärmelehre, nämlich die PLANCKsche Strahlungsformel und das NERNSTsche Theorem ohne Zuhilfenahme des BOLTZMANNschen Prinzips auf rein thermodynamischem Wege mit Benutzung des Grundgedankens der Quantentheorie abgeleitet werden können. Insoweit die im folgenden gegebenen Überlegungen der Wirklichkeit entsprechen, gilt das NERNSTsche Theorem für chemisch reine, kristallisierte Stoffe, nicht aber für Mischkristalle. Auf amorphe Stoffe läßt sich wegen der über das Wesen des amorphen Zustandes herrschenden Unklarheit nichts aussagen.

Zur Rechtfertigung des hier vorliegenden Versuches, das NERNSTsche Theorem theoretisch zu erfassen, muß ich einleitend bemerken, daß alle Bemühungen, das NERNSTsche Theorem auf thermodynamischem Wege unter Benutzung des Erfahrungssatzes vom Verschwinden der Wärmekapazität bei $T = 0$ theoretisch abzuleiten, als mißglückt anzusehen sind. Ich bin gerne bereit, diese Behauptung den einzelnen versuchten Beweisen gegenüber zu begründen, falls dies von Kollegen gewünscht wird.

§ 1. Thermodynamische Ableitung der PLANCKschen Strahlungsformel. Wir betrachten ein chemisch einheitliches Gas, dessen Moleküle je einen Resonator[1]) tragen. Die Energie dieses Resonators soll nicht jeden beliebigen Wert annehmen können, sondern nur gewisse diskrete Werte ε_σ (auf das Mol bezogen). Ich will mir nun erlauben, zwei Moleküle als chemisch verschieden, d. h. als prinzipiell durch semipermeable Wände

[1]) Unter „Resonator" sei hier allgemein ein Träger innerer Molekularenergie von vorläufig nicht näher präzisierter Beschaffenheit verstanden.

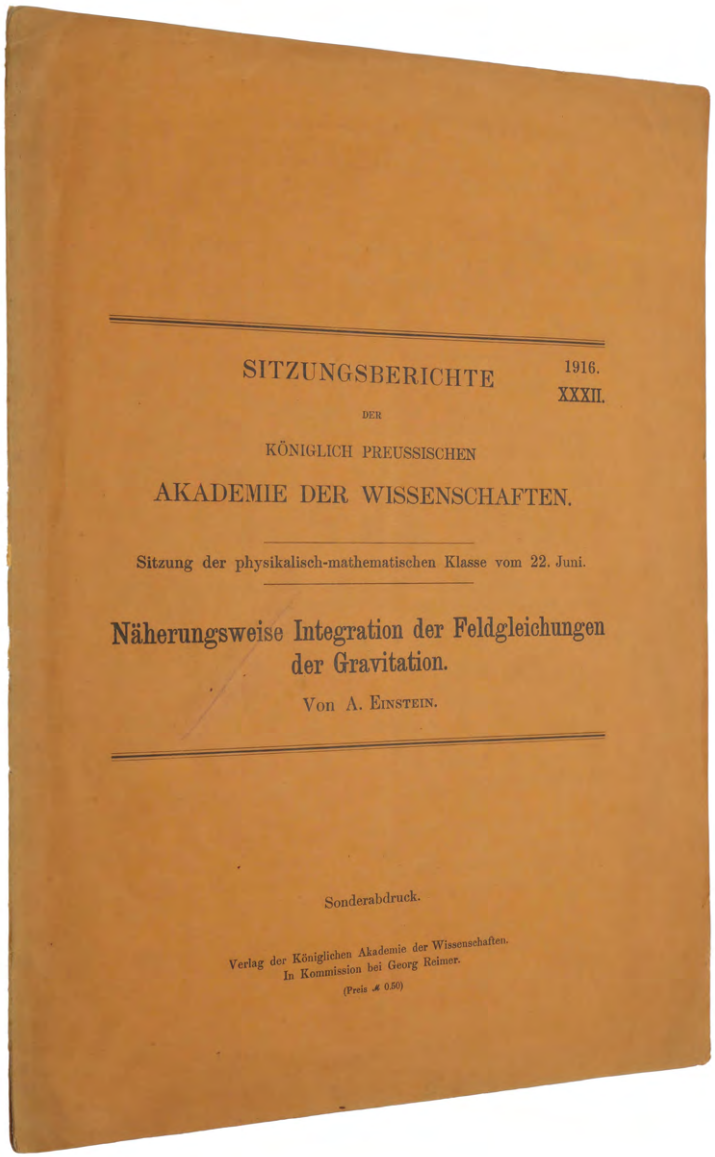
EINSTEIN'S PREDICTION OF GRAVITATIONAL WAVES

EINSTEIN, Albert. *Näherungsweise Integration der Feldgleichungen der Gravitation.* Offprint from Sitzungsberichte der Königlich preussischen Akademie der Wissenschaften, XXXII. Berlin: Königlich Akademie der Wissenschaften, 1916.

$6,500

8vo (252 x 179 mm), pp. 688-696. Original printed wrappers, 5 cm pencil line across front wrapper, very mild sunning to wrappers, otherwise fine.

First edition, offprint issue, in the original printed wrappers, of the paper in which Einstein first proposed the existence of gravitational waves on the basis of general relativity, which he had completed just eight months earlier. "In June 1916 [Einstein] published a follow-up paper [the offered paper] to his recently formulated theory of the gravitational field in which he predicted the existence of gravitational waves traveling at the speed of light, in analogy with electromagnetic radiation (i.e. light, radio waves, etc.). In it he derived a formula for the emission of gravitational waves" (Blum, Lalli & Renn). Notwithstanding this paper, some physicists, including Einstein himself, later came to doubt the existence of gravitational waves (see below). These doubts were finally laid to rest when, on 14 September 2015, gravitational waves formed by the collision of two black holes 1.3 billion light years away were detected by both LIGO (Laser Interferometer Gravitational-wave Observatory) detectors in Louisiana and Washington State in the US. Regarding this discovery, Stephen Hawking wrote: "The discovery of gravitational waves by the LIGO team is an incredible achievement. It is the first


SITZUNGSBERICHTE 1916. XXXII.

DER

KÖNIGLICH PREUSSISCHEN

AKADEMIE DER WISSENSCHAFTEN.

Sitzung der physikalisch-mathematischen Klasse vom 22. Juni.

Näherungsweise Integration der Feldgleichungen der Gravitation.

Von A. Einstein.

Sonderabdruck.

Verlag der Königlichen Akademie der Wissenschaften.
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(Preis ℳ 0.50)

observation of gravitational waves as predicted by Einstein and will allow us new insights into our universe."

"Even before relativity, [Hendrik Antoon] Lorenz had conjectured in 1900 that gravitation 'can be attributed to actions which do not propagate with a velocity larger than that of light.' The term *gravitational wave* (onde gravifique) appeared for the first time in 1905, when [Henri] Poincaré discussed the extension of Lorenz invariance to gravitation. In June 1916 [in the offered paper], Einstein became the first to cast these qualitative ideas into explicit form" (Pais, p. 279). Remarkably, Einstein's work on gravitational waves seems to have led to his realization that a quantum theory of gravity would be necessary, and then to new discoveries in quantum theory itself. "He also pointed out [in the same paper] that the existence of radiationless stable interatomic orbits is equally mysterious from the electromagnetic as from the gravitational point of view! 'It seems that the quantum theory will have to modify not only Maxwell's electrodynamics but also the new gravitational theory.' Perhaps this renewed concern with quantum physics spurred him, a few months later, to make one of his great contributions to quantum electrodynamics: in the fall of 1916 he introduced the concepts of spontaneous and induced transitions and gave a new derivation of Planck's radiation law" (*ibid*., p. 280).

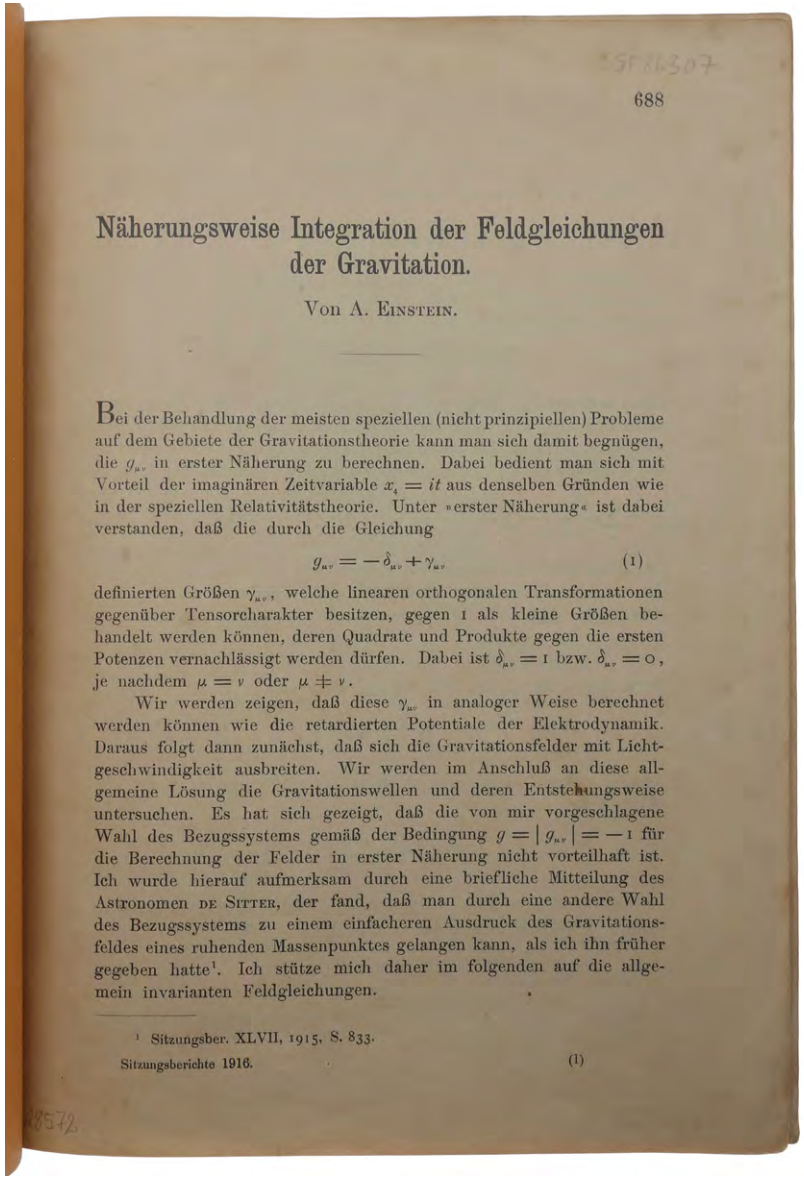
"Einstein's earliest calculations in general relativity were based on the theory's Newtonian approximation. This was a natural choice, since it was vital to the theory's acceptability to most physicists that it should be able to reproduce, in some plausible, if approximate, sense, the enormously successful predictions of Newton's gravitational theory. In addition, this approximation scheme could then prove useful in identifying physical scenarios in which higher-order terms in the approximation scheme predicted measurable departures from the Newtonian prediction, the most famous case of this being the Mercury perihelion advance. Einstein's early emphasis on the Newtonian approximation scheme may explain his comment to Karl Schwarzschild, in a letter of February 19, 1916, that gravitational waves did not exist within his new theory. Unfortunately, Einstein's letter does not state the grounds for this belief …

"However, within a few months of this letter, Einstein was to change his mind, and not for the last time, on the existence of waves in his theory. Probably the occasion for this change of heart came about in his investigation of a new method of approximation, comparing his theory to Maxwellian electrodynamics … Following a suggestion of the astronomer Willem de Sitter (1872-1934), Einstein adopted a linearized approximation scheme of his equations, with a coordinate choice which cast the resulting linearized field equations in a form analogous to a familiar version of the Maxwell equations. Once this maneuver had been accomplished, it was trivial to solve the resulting wave equation, using techniques developed in the study of Maxwell's equations. In this way one could demonstrate the existence of a solution of the linearized Einstein equations representing plane gravitational waves. The paper published by Einstein on this linearized approximation in 1916 [the offered paper] thus became the first mathematical theory of gravitational waves, and the approach it pioneered is still fundamental to the design of such existing gravitational wave detectors as the LIGO, VIRGO, and GEO600 detectors operating (respectively) in the United States, Italy, and Germany" (Kennefick, pp. 270-3).

Efforts to detect gravitational waves began in earnest in the 1960s. In June 1969, after almost a decade's work, Joseph Weber reported that he had detected gravitational waves using a 'Weber bar,' a large, solid bar of metal isolated from outside vibrations. Weber found residual vibrations of the bar which he attributed to the presence of gravitational waves, but his results were challenged by other scientists partly because the intensity of the gravitational waves necessary to

produce Weber's results was much larger than expected. New evidence came from observations over three decades of the Hulse-Taylor binary, a pair of stars, one of which is a pulsar, which orbit each other rapidly at very short distance, which should cause them to radiate significant amounts of energy in the form of gravitational waves. This in turn should cause their period of rotation to increase over time; the observations of this slowing turned out to agree with Einstein's predictions to within 0.2%. Russell Hulse and Joe Taylor were awarded the 1993 Nobel Prize in Physics for this work, the first indirect evidence of the existence of gravitational waves. The first *direct* observation of gravitational waves was made by the LIGO team in 2015.

Weil 86. Blum, Lalli & Renn, 'One hundred years of gravitational waves: the long road from prediction to observation,' Research Topic 43, Max Planck Institute for the History of Science, 11 February 2016; Kennefick, 'Einstein, gravitational waves, and the theoretician's regress,' pp. 270-280 in *The Cambridge Companion to Einstein*, 2014; Pais, *Subtle is the Lord*, 1982.



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Näherungsweise Integration der Feldgleichungen der Gravitation.

Von A. Einstein.

Bei der Behandlung der meisten speziellen (nicht prinzipiellen) Probleme auf dem Gebiete der Gravitationstheorie kann man sich damit begnügen, die $g_{\mu\nu}$ in erster Näherung zu berechnen. Dabei bedient man sich mit Vorteil der imaginären Zeitvariable $x_4 = it$ aus denselben Gründen wie in der speziellen Relativitätstheorie. Unter »erster Näherung« ist dabei verstanden, daß die durch die Gleichung

$$g_{\mu\nu} = -\delta_{\mu\nu} + \gamma_{\mu\nu} \qquad (1)$$

definierten Größen $\gamma_{\mu\nu}$, welche linearen orthogonalen Transformationen gegenüber Tensorcharakter besitzen, gegen 1 als kleine Größen behandelt werden können, deren Quadrate und Produkte gegen die ersten Potenzen vernachlässigt werden dürfen. Dabei ist $\delta_{\mu\nu} = 1$ bzw. $\delta_{\mu\nu} = 0$, je nachdem $\mu = \nu$ oder $\mu \neq \nu$.

Wir werden zeigen, daß diese $\gamma_{\mu\nu}$ in analoger Weise berechnet werden können wie die retardierten Potentiale der Elektrodynamik. Daraus folgt dann zunächst, daß sich die Gravitationsfelder mit Lichtgeschwindigkeit ausbreiten. Wir werden im Anschluß an diese allgemeine Lösung die Gravitationswellen und deren Entstehungsweise untersuchen. Es hat sich gezeigt, daß die von mir vorgeschlagene Wahl des Bezugssystems gemäß der Bedingung $g = |g_{\mu\nu}| = -1$ für die Berechnung der Felder in erster Näherung nicht vorteilhaft ist. Ich wurde hierauf aufmerksam durch eine briefliche Mitteilung des Astronomen de Sitter, der fand, daß man durch eine andere Wahl des Bezugssystems zu einem einfacheren Ausdruck des Gravitationsfeldes eines ruhenden Massenpunktes gelangen kann, als ich ihn früher gegeben hatte[1]. Ich stütze mich daher im folgenden auf die allgemein invarianten Feldgleichungen.

[1] Sitzungsber. XLVII, 1915, S. 833.

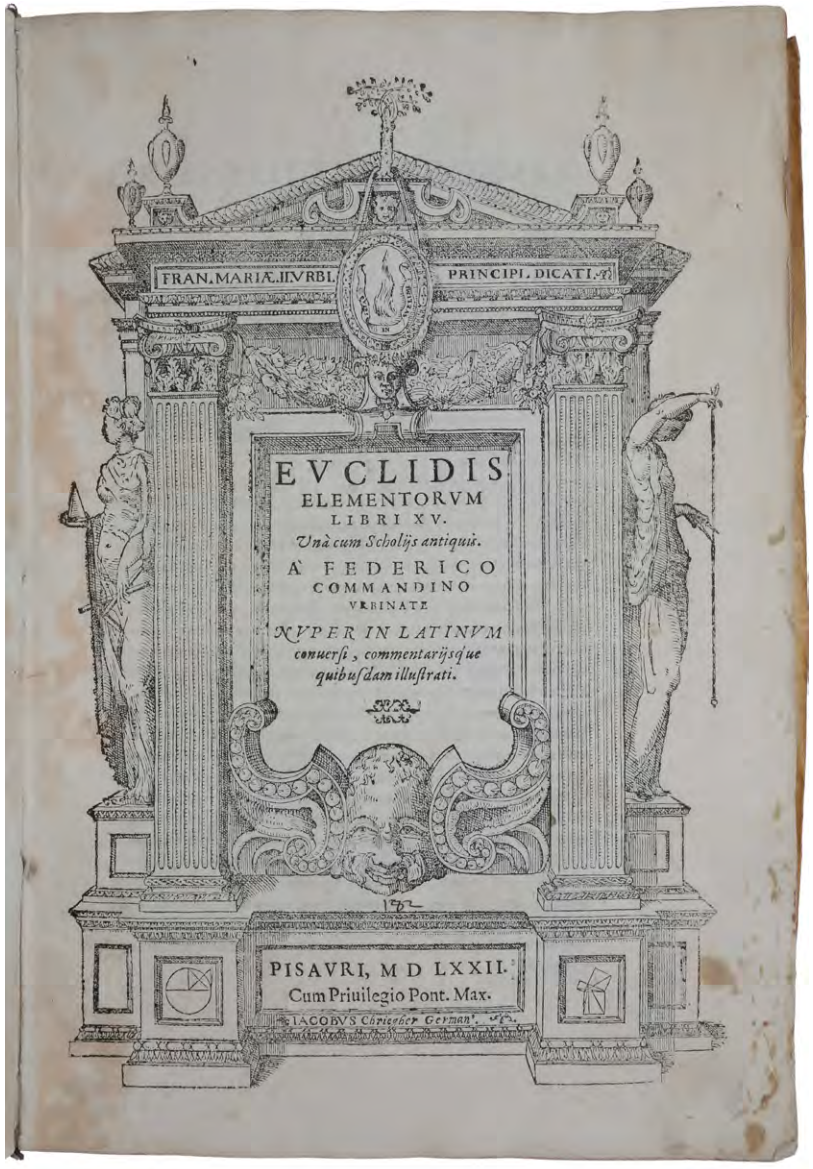
THE MOST IMPORTANT LATIN TRANSLATION OF THE 'ELEMENTS'

EUCLID. *Elementorum libri XV una cum scholiis antiquiis. A Federico Commandino Urbinate nuper in latinum conversi, commentariisque quibusdam illustrati.* [Colophon:] Pesaro: Camillo Franceschini, 1572.

$9,500

Folio (318 x 218 mm), ff. [xii], 255, [1], with hundreds of woodcut geometrical diagrams in text, historiated initials and printed marginal notes, text in roman and italic type. Title-page with woodcut architectural border signed Iacobus Chriegher German[us]. Contemporary limp vellum, remains of ties, old paper repairs to inner hinges, otherwise fine and unrestored. A very large and fine copy.

First edition of "the most important Latin translation [of Euclid] ... it was the foundation of most translations which followed it up to the time of Peyrard [1814]" (Heath, *The Thirteen Books of Euclid's Elements*, Vol. 1, p. 104). Euclid's *Elements* is the "oldest mathematical textbook still in common use today" (PMM). The *Elements* "exercised an influence upon the human mind greater than that of any other work except the Bible" (DSB). Federico Commandino was the most prolific Renaissance translator of Greek mathematical works, as well as the most mathematically competent. His translation was the first to be based upon a tolerably critical Greek original. Although thus far only the chief book-producing centers like Venice had been involved in printing Euclid, this new translation into Latin of the fifteen books of the *Elements* was produced in Pesaro, a provincial seaport on the Adriatic near Urbino.



The first printed Latin translations of Euclid were the medieval translation from the Arabic by Campanus of Novara (Venice, 1482), followed by a Latin translation from the Greek by Bartolomeo Zamberti (Venice, 1505). These two translations, together with the *editio princeps* of the Greek (Basel, 1533), were the basis for all subsequent Latin editions for more than half a century, and for many translations after that. "However, the better part of this influence was interrupted suddenly and decisively by the fourth major version: the publication at Pesaro in 1572 of the Latin translation by Federico Commandino of Urbino. Commandino—who, in addition to the place he holds in the history of physics deriving from his *Liber de centro gravitatis* (Bologna, 1565), prepared exacting Latin versions of many other Greek mathematical works—was clearly the most competent mathematician of all Renaissance editors of Euclid. He was also most astute in his scholarship, for we know that in addition to the 1533 *editio princeps*, he employed at least one other Greek manuscript in establishing the text for his translation. For the first time, save for the anonymous translation in the twelfth century, we now have a version (no matter what language) of the *Elements* that is solidly based on a tolerably critical Greek original. It even includes, also for the first time, a rendering of numerous Greek scholia. Aware, but critical, of the efforts of his predecessors, Commandino leaves no doubt of the advantage of staying closer to the Greek sources so many of them had minimized, if not ignored. The result of his labors may prove to be of less fascination than other versions, since it so closely follows the Greek we already know, but the importance it held for the subsequent modern history of the *Elements* is immeasurable. It came to serve, in sum, as the base of almost all other proper translations before Peyrard's discovery of the "pristine" Euclid in the early nineteenth century. Thus, to cite only the most notable cases in point, Greek texts of the *Elements* with accompanying Latin translation frequently based the latter on Commandino: for example, Henry Briggs's *Elementorum Euclidis libri VI priores* (London, 1620) and even David Gregory's 1703 Oxford edition of Euclid's

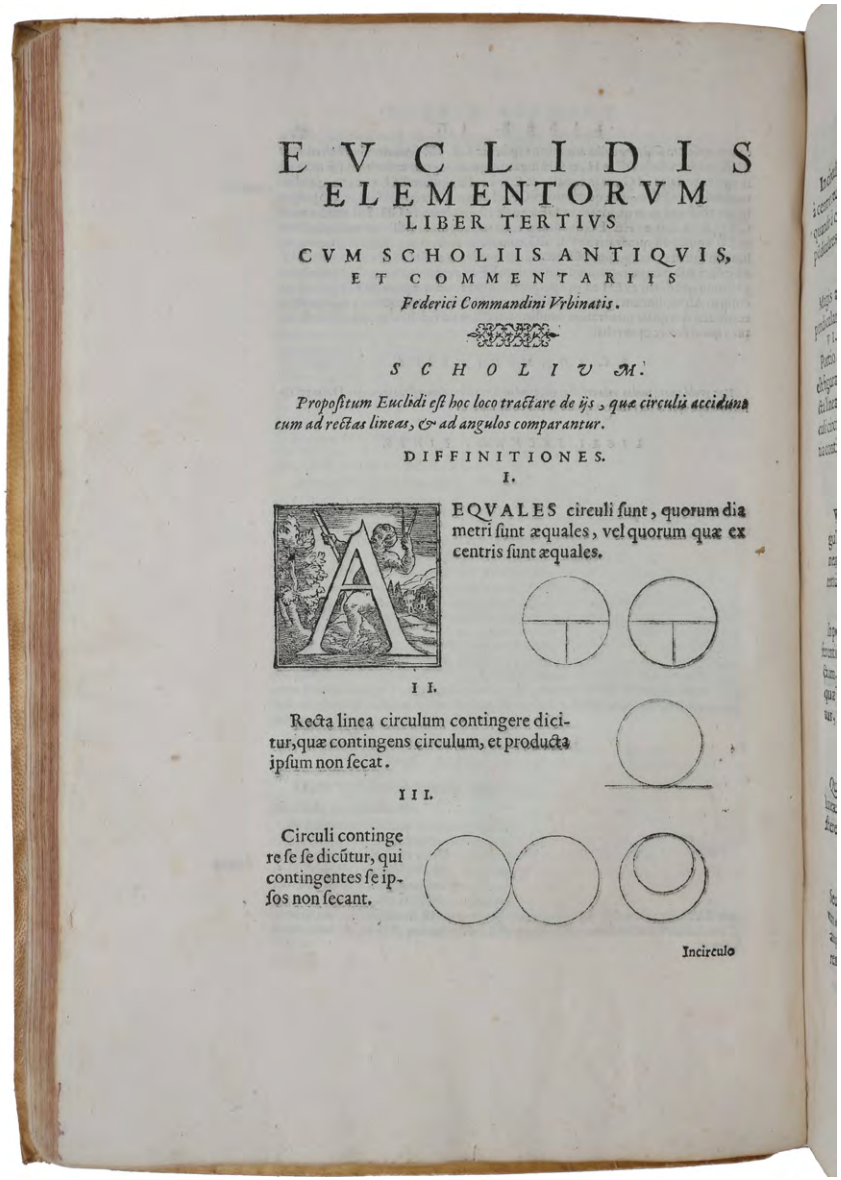


Opera omnia (which was the standard, pre-nineteenth-century source for the Greek text). Commandino was also followed in later strictly Latin versions: that of Robert Simson, simultaneously issued in English at Glasgow in 1756; and even that of Samuel Horsley, appearing at London in 1802. Vernacular translations often followed a similar course, beginning with the Italian translation, revised by Commandino himself, appearing at Urbino in 1575 and extending to and beyond the English version by John Keill, Savilian professor of astronomy at Oxford, in 1708" (DSB, under Euclid).

"Born ca. 300 BC in Alexandria, Egypt, "Euclid compiled his *Elements* from a number of works of earlier men. Among these are Hippocrates of Chios (flourished c. 440 BC), not to be confused with the physician Hippocrates of Cos (c. 460–375 BC). The latest compiler before Euclid was Theudius, whose textbook was used in the Academy and was probably the one used by Aristotle (384–322 BC). The older elements were at once superseded by Euclid's and then forgotten. For his subject matter Euclid doubtless drew upon all his predecessors, but it is clear that the whole design of his work was his own …

"Euclid understood that building a logical and rigorous geometry depends on the foundation—a foundation that Euclid began in Book I with 23 definitions (such as "a point is that which has no part" and "a line is a length without breadth"), five unproved assumptions that Euclid called postulates (now known as axioms), and five further unproved assumptions that he called common notions. Book I then proves elementary theorems about triangles and parallelograms and ends with the Pythagorean theorem …

"The subject of Book II has been called geometric algebra because it states algebraic identities as theorems about equivalent geometric figures. Book II contains a construction of "the section," the division of a line into two parts



EVCLIDIS
ELEMENTORVM
LIBER TERTIVS
CVM SCHOLIIS ANTIQVIS,
ET COMMENTARIIS
Federici Commandini Vrbinatis.

SCHOLIVM.

Propoſitum Euclidi eſt hoc loco tractare de ijs, quæ circulis accidunt cum ad rectas lineas, & ad angulos comparantur.

DIFFINITIONES.
I.

EQVALES circuli ſunt, quorum diametri ſunt æquales, vel quorum quæ ex centris ſunt æquales.

II.

Recta linea circulum contingere dicitur, quæ contingens circulum, et producta ipſum non ſecat.

III.

Circuli contingere ſe ſe dicũtur, qui contingentes ſe ipſos non ſecant.

Incirculo

such that the ratio of the larger to the smaller segment is equal to the ratio of the original line to the larger segment. (This division was renamed the golden section in the Renaissance after artists and architects rediscovered its pleasing proportions.) Book II also generalizes the Pythagorean theorem to arbitrary triangles, a result that is equivalent to the law of cosines. Book III deals with properties of circles and Book IV with the construction of regular polygons, in particular the pentagon.

"Book V shifts from plane geometry to expound a general theory of ratios and proportions that is attributed by Proclus (along with Book XII) to Eudoxus of Cnidus (c. 395/390–342/337 BC). While Book V can be read independently of the rest of the *Elements*, its solution to the problem of incommensurables (irrational numbers) is essential to later books. In addition, it formed the foundation for a geometric theory of numbers until an analytic theory developed in the late 19th century. Book VI applies this theory of ratios to plane geometry, mainly triangles and parallelograms, culminating in the "application of areas," a procedure for solving quadratic problems by geometric means.

"Books VII–IX contain elements of number theory, where number (arithmos) means positive integers greater than 1. Beginning with 22 new definitions—such as unity, even, odd, and prime—these books develop various properties of the positive integers. For instance, Book VII describes a method, antanaresis (now known as the Euclidean algorithm), for finding the greatest common divisor of two or more numbers; Book VIII examines numbers in continued proportions, now known as geometric sequences (such as ax, ax^2, ax^3, ax^4, …); and Book IX proves that there are an infinite number of primes.

"According to Proclus, Books X and XIII incorporate the work of the Pythagorean Thaetetus (c. 417–369 BC). Book X, which comprises roughly one-fourth of the *Elements*, seems disproportionate to the importance of its classification of incommensurable lines and areas (although study of this book would inspire Johannes Kepler [1571–1630] in his search for a cosmological model).

"Books XI–XIII examine three-dimensional figures, in Greek *stereometria*. Book XI concerns the intersections of planes, lines, and parallelepipeds (solids with parallel parallelograms as opposite faces). Book XII applies Eudoxus's method of exhaustion to prove that the areas of circles are to one another as the squares of their diameters and that the volumes of spheres are to one another as the cubes of their diameters. Book XIII culminates with the construction of the five regular Platonic solids (pyramid, cube, octahedron, dodecahedron, icosahedron) in a given sphere" (Britannica).

Rose (p. 185) emphasizes the importance of Commandino to the mathematical renaissance of the sixteenth century: "Perhaps the clearest perception of the mathematical renaissance is to be found in the writings of the Urbino school. Not only did Commandino, Guidobaldo dal Monte and Bernardino Baldi (1533-1617) pursue the revival of Greek mathematics and the restoration of mathematical certainty, but in their thought there also emerged a strong sense of the historical development of mathematics. The idea of a mathematical renaissance is especially evident in the tributes paid to the founder of the Urbino school by his two important pupils. Guidobaldo writes in 1577:

'Yet in the midst of that darkness (though there were also some other famous names) Federico Commandino shone like the sun. He by his many learned studies not only restored the lost heritage of mathematics, but actually increased and enhanced it. For that great man was so well endowed with mathematical talent that in him there seem to have lived again Archytas, Eudoxus, Hero, Euclid, Theon, Aristarchus, Diophantus, Theodosius, Ptolemy, Apollonius, Serenus,

Pappus and even Archimedes himself, for his commentaries on Archimedes smell of the mathematician's own lamp. And lo! just as he had been suddenly thrust from the darkness and prison of the body (as we believe) into the light and liberty of mathematics, so at the most opportune time he left mathematics bereft of its fine and noble father and left us so prostrate that we scarcely seem able even by a long discourse to console ourselves for his loss.'

And Baldi:

'Commandino with the greatest diligence and insight restored to light, to dignity and to splendour the works of nearly all the principal writers of the age in which mathematics had flourished.'"

Adams E 984; Censimento 16 CNCE 18358; Honeyman Coll. 985; Riccardi I, 362; Steck III.83; Thomas-Stanford 18. Rose, *The Italian Renaissance of Mathematics: Studies on Humanists and Mathematicians from Petrarch to Galileo*, 1975.



EVCLID. ELEMENT.

4. primi.

item, aequales perpendiculares ipſae. quare trianguli MOP duo latera MP PO aequalia ſunt duobus lateribus MR RN trianguli MNR: & angulus MPO eſt aequalis angulo MRN. baſis igitur OM. eſt aequalis baſi MN. & ita demonſtrabitur baſis ON ipſi NM aequalis. ex quibus conſtat triangulum MNO aequiangulum eſſe. ergo ſi reliquis dodecaedri angulis triangula aequilatera eodem modo ſubtendantur, deſcriptum erit icoſaedrum. ſunt enim omnes anguli ipſius dodecaedri numero viginti, quot ſunt icoſaedri triangula. In dato igitur dodecaedro icoſaedrum deſcriptum eſt. quod facere oportebat.

EUCLIDIS ELEMENTORVM FINIS.

PISAVRI

CVM LICENTIA SVPERIORVM.

APVD CAMILLVM FRANCISCHINVM.

M. D. LXXII.

Bart. Zabi
Copia in sede ste

A LANDMARK WORK IN MATHEMATICS

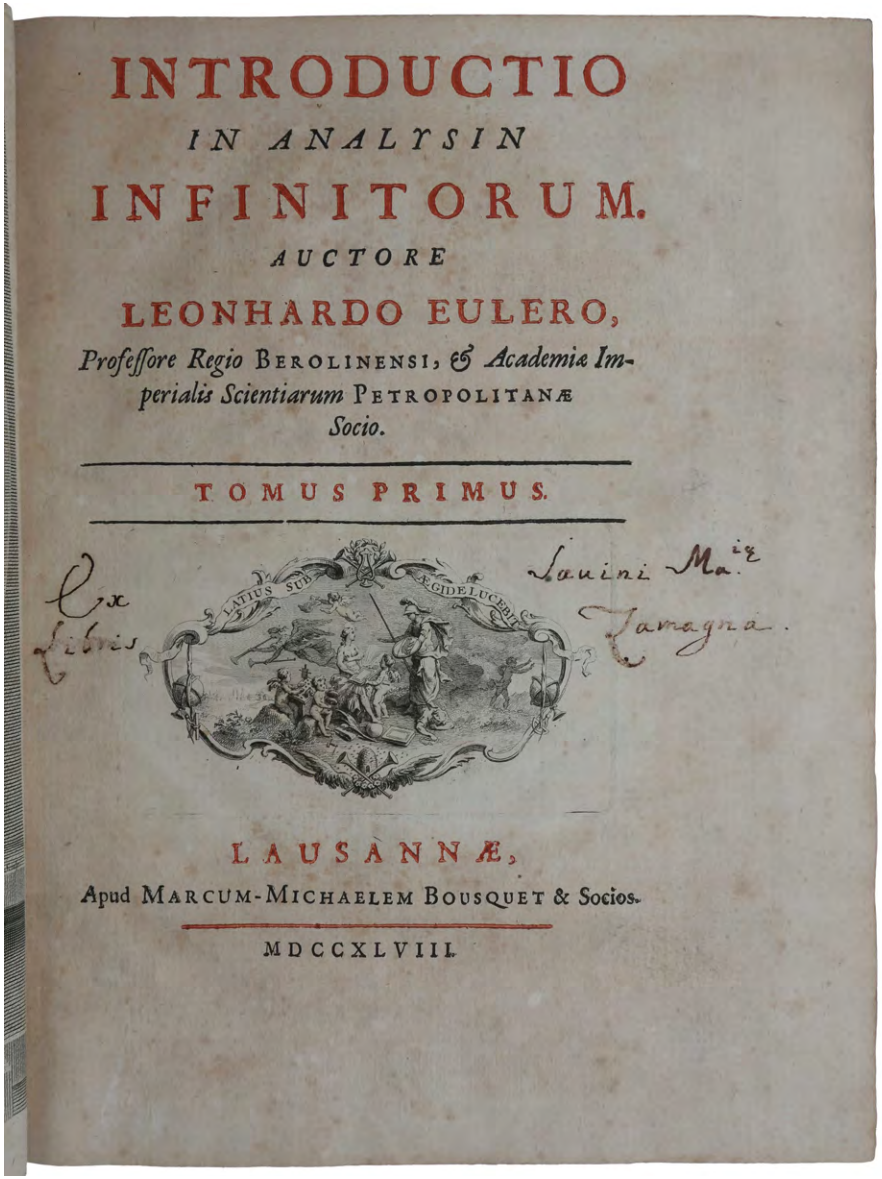
[PMM 196; Norman 732]

EULER, Leonhard. *Introductio in analysin infinitorum.* Lausanne: M.-M. Bousquet, 1748.

$15,000

2 vols. 4to (245 x 192 mm), pp. [2] [i-ii], iii-xvi, [3] 4-320, folding printed table after p. 224; [iv] [3] 4-398 [2], titles printed in red and black with engraved title vignette, engraved frontispiece by Soubeyran after De la Monce, engraved folding portrait of dedicatee Jean Jacques Dortous de Mairan (1678-1771) by Etienne Frequet (1719-94) after J. Tocquet, and 40 folding engraved plates. Contemporary calf, spine gilt, capitals with old but well-done leather restoration, occasional light spotting, some plates a little browned. Overall a very good copy.

First edition of Euler's great textbook on analysis, the only work of Euler listed in *Printing and the Mind of Man*. "In his 'Introduction to Analysis' Euler did for modern analysis what Euclid had done for ancient geometry" (PMM). The editors of the first volume of the *Introductio* in *Opera Omnia* (Ser. 1, Vol. 8) emphasize that the work "still today deserves to be not only read, but studied with devotion. No mathematician will put it aside without immense benefit," and that the *Introductio* "marks the beginning of a new epoch and that this work has become influential for the whole development of the mathematical science by virtue of not only its content, but also its language." The eminent historian of mathematics, Carl Boyer, in his address to the International Congress of Mathematicians in 1950, called it the greatest modern textbook in mathematics. Boyer cited Euclid's Geometry as the greatest mathematical textbook of the classical period, perhaps


INTRODUCTIO
IN ANALYSIN
INFINITORUM.
AUCTORE
LEONHARDO EULERO,
Professore Regio Berolinensi, & Academiæ Imperialis Scientiarum Petropolitanæ Socio.
TOMUS PRIMUS.
LAUSANNÆ,
Apud Marcum-Michaelem Bousquet & Socios.
MDCCXLVIII.

of all time, appearing in over one thousand editions. For the medieval period, he chose the less well-known Al-Khowarizmi, largely devoted to algebra. But for "modern" times, Boyer made the case for Euler's *Introductio* as the greatest modern textbook—and, appropriately, this time a text in analysis. Gauss said, "The study of Euler's works will remain the best school for the various fields of mathematics, and nothing can replace it." In 1979 André Weil remarked that he was trying to convince "the mathematical community that our students of mathematics would profit much more from a study of Euler's *Introductio in analysis infinitorum*, rather than of the available modern textbooks" (Alexanderson, p. 635).

Provenance: Sauini Zamagna (?) (inscription on title).

"Leonhard Euler (1707-83), the great Swiss mathematician, was born at Basel where he became the pupil of Johann Bernoulli, whose sons persuaded Catherine the Great to invite him to St. Petersburg. In 1747 he was invited by Frederick the Great to Berlin. In 1766 he became blind, but this did not interfere with his colossal output of work. Altogether he produced thirty-two books in Latin, German and French, and approximately nine hundred memoirs (making him the most productive mathematician who ever lived). He covered practically the entire field of pure and applied mathematics, but some of his best and most enduring work was done in analysis, which he established as an independent science.

"In his 'Introduction to Analysis' Euler did for modern analysis what Euclid had done for ancient geometry. It contains an exposition of algebra, trigonometry and analytical geometry, both plane and solid, a definition of logarithms as exponents, and important contributions to the theory of equations. He evolved the modern exponential treatment of logarithms, including the fact that each number has an infinity of natural logarithms. In the early chapters there appears for the first time the definition of mathematical function, one of the fundamental concepts of modern mathematics. From Euler's time mathematics and physics tended to be treated algebraically, and many of his principles are still used in teaching mathematics" (PMM).

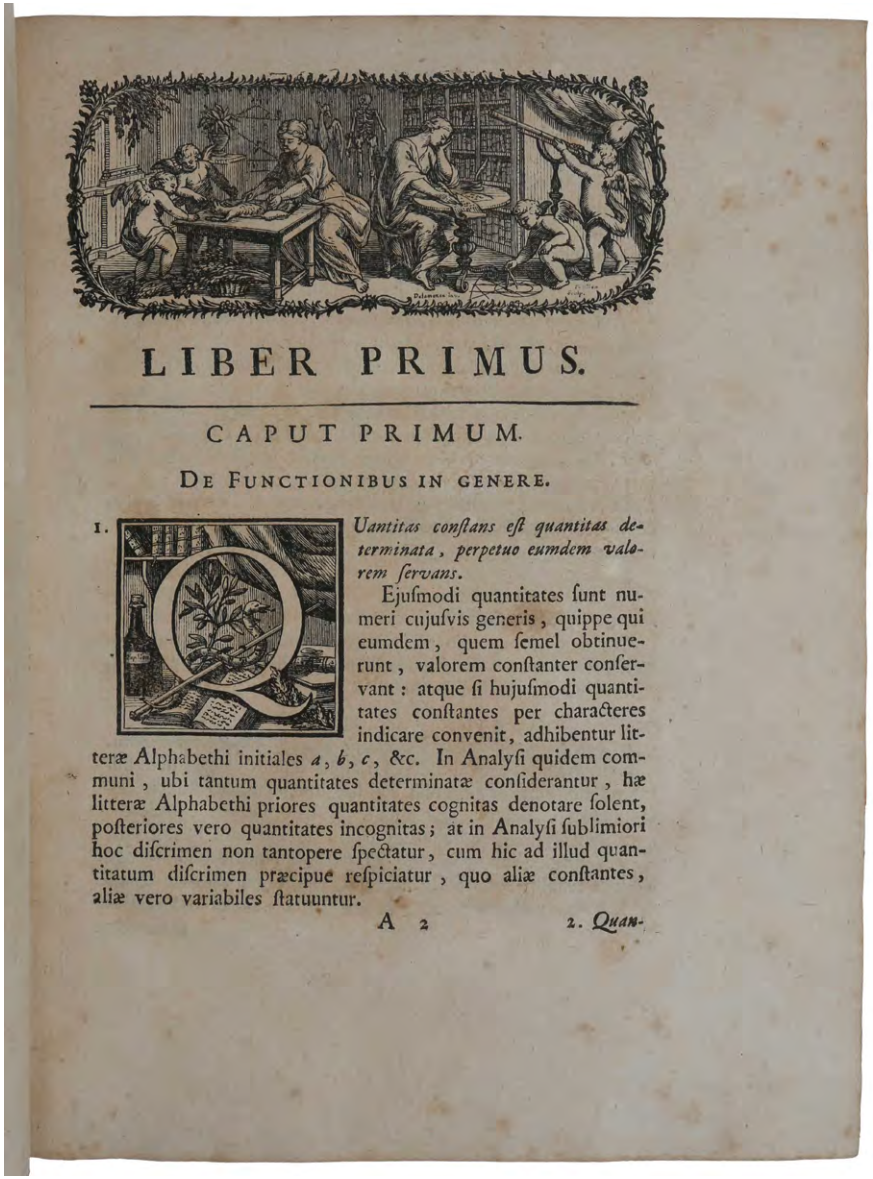
"The topics in the *Introductio* include: the definition of a function and the distinction between single-valued and many-valued "functions", or odd and even functions. There are questions of polynomial factorization related to the Fundamental Theorem of Algebra; substitutions, infinite series, exponentials and logarithms, and expansions into partial fractions; multiple angle formulas; evaluations of $\zeta(2k)$, for k a positive integer (the famous so-called Basel problem); infinite series-infinite product formulas; and partitions of integers. Much of the latter part of volume 1 is devoted to generating functions and continued fractions. Volume 2 is largely concerned with classification of curves and the study of the conics, and higher degree curves and surfaces. D. J. Struik referred to it as the first textbook in analytic geometry.

"What makes this remarkable as a textbook is that the later topics are closely related to Euler's research, and in some cases their appearance here, and sometimes in earlier papers, opened up whole new fields of research. We cannot say that of most "elementary" modern textbooks. Whether an idea appeared first in the *Introductio* is not very important. To be sure, Leibniz and Johann Bernoulli had come close to our modern definition of function, but it was here that Euler spelled it out clearly …

"This is one of the first mathematics books that "looks modern". Except for "xx" to denote "x^2" and a few other conventions carried over from earlier times, it introduced modern notation: $f(x)$ for function, e for the base of the natural logarithm (appearing in print here for the first time but used earlier by Euler in a manuscript not published until 1862 in his *Opera posthuma mathematica*

et physica), and π (though it had been used by Christian Goldbach two years earlier). Euler proved that e is irrational in the *Introductio*, though the fact could be deduced from work in a paper of 1737. The proof in the *Introductio* using continued fractions was not, however, to everyone's satisfaction. In 1748 Euler still used $\sqrt{-1}$ and did not introduce "i" for this number until 1777. Other problems covered in the *Introductio* had occurred earlier in Euler's reports to the St. Petersburg Academy and in published papers. The genius exhibited in the *Introductio* was in the way he formulated the problems and the methods he introduced. In the problem of partitions he developed powerful methods using generating functions. Again, using infinite series and products he came up with the relationships that inspired Riemann's work on the ζ-function. It is often assumed that the formula usually called "Euler's formula", relating the exponential and the trigonometric functions, appeared first in the *Introductio*, but the formula was probably observed earlier by Roger Cotes. As was often the case, however, Euler did more with it. He was a master expositor, so when he took a subject he molded it into something that people could understand, thereby prompting subsequent development.

"Beyond the beautiful mathematics contained in the book, however, there is the beauty of the original edition of this set of volumes as physical objects. Most of Euler's books were published by the press of the Academy in St. Petersburg. Those are rather sober, straightforward products of the printer's art. When Euler wrote the *Introductio*—probably around 1745—he was working in Berlin. Unfortunately the Berlin Academy published journals but not books, and the St. Petersburg Academy was having financial problems. So Euler had to look for a publisher elsewhere. In 1744 his *Methodus inveniendi lineas curvas* had been published by the firm of Marc-Michel Bousquet, located in Lausanne and Geneva. So he approached them about publishing the *Introductio*. This firm had also published in 1742 a beautifully decorated set of four volumes, the *Opera Omnia* of Jean



LIBER PRIMUS.

CAPUT PRIMUM.

De Functionibus in genere.

1. *Uantitas conſtans eſt quantitas determinata, perpetuo eumdem valorem ſervans.*

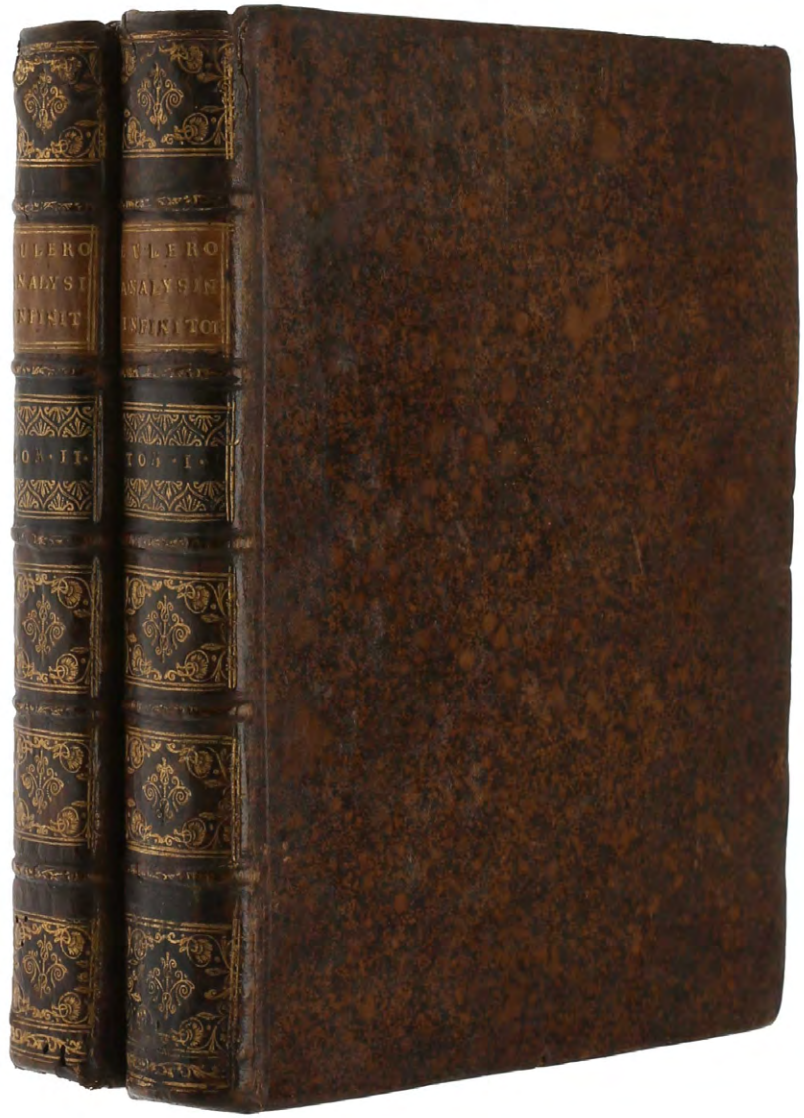
Ejuſmodi quantitates ſunt numeri cujuſvis generis, quippe qui eumdem, quem ſemel obtinuerunt, valorem conſtanter conſervant: atque ſi hujuſmodi quantitates conſtantes per characteres indicare convenit, adhibentur litteræ Alphabethi initiales *a*, *b*, *c*, &c. In Analyſi quidem communi, ubi tantum quantitates determinatæ conſiderantur, hæ litteræ Alphabethi priores quantitates cognitas denotare ſolent, poſteriores vero quantitates incognitas; at in Analyſi ſublimiori hoc diſcrimen non tantopere ſpectatur, cum hic ad illud quantitatum diſcrimen præcipue reſpiciatur, quo aliæ conſtantes, aliæ vero variabiles ſtatuuntur.

A 2 2. *Quan-*

Bernoulli. Handsome though these earlier volumes were, fortunately for us, the publisher seems to have given this set of two volumes an even more generous budget, so the book is decorated with an elegant engraving for a frontispiece, a title page in two colors, a fold-out, engraved dedication portrait of the physicist-astronomer-geologist Jean Jacques Dortous de Mairan, who was a patron of the publisher, secretary of the Académie Royale des Sciences and member of various other scientific societies.

"The engraved frontispiece is the work of Pierre Soubeyran, based on the design of a member of the de La Monce family of portrait painters. It shows two classically attired women under an arch flanked by columns. Hovering overhead is a putto. One woman is gesticulating while the other is writing mathematical formula in a book and the putto is offering inspiration. There are mathematical instruments on the floor and a book open to pages with "Calcul Differentiel" and "Table des Sinus". The inscription at the top of the frontispiece gives the title of the *Introductio* in French, *Analyse des infiniments petits*, which, curiously, happens to be the name of L'Hôpital's famous text of 1696. The title of the first French translation of the *Introductio* in 1796, on the other hand, was *Introduction de l'analyse infinitesimale*" (Alexanderson, pp. 636-7).

PMM 196; Norman 732. G. J. Alexanderson, 'Euler's Introductio in analysis infinitorum,' *Bulletin of the American Mathematical Society* 44 (2007), pp. 635-8.

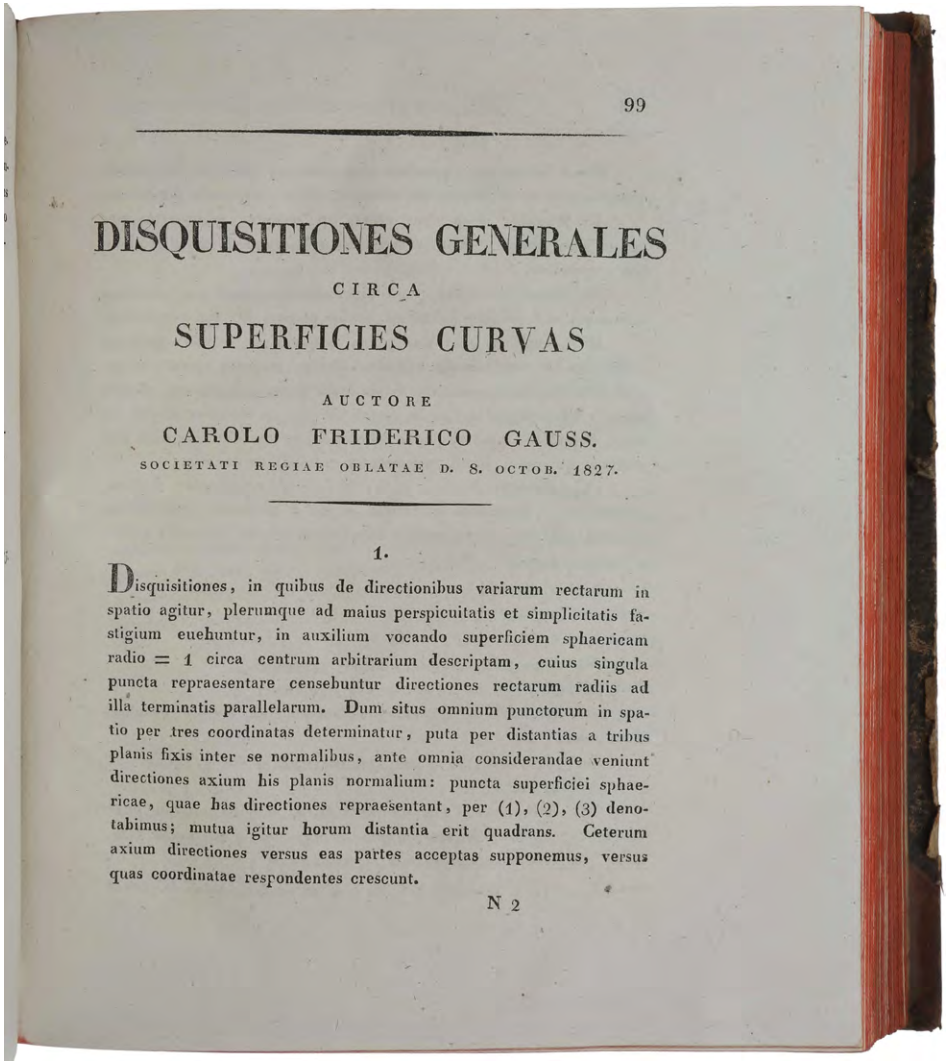


THIRTEEN PAPERS BY GAUSS

GAUSS, Carl Friedrich. [Contained in five volumes of *Commentationes societatis regiae scientiarum Gottingensis*]: *1. Disquisitio de elementis ellipticis palladis ex oppositionibus annorum...; 2, Summatio quarumdam serierum singularium; 3. Disquisitiones generales circa seriem infinitam etc.; 4. Theoria attractionis corporum sphaeroidicorum ellipticorum homogeneorum methodo noua tractata; 5. Observationes cometae secundi a. 1813 in observatorio Gottingensi factae ad nonnullis adnotationibus circa calculum orbitarum parabolicarum; 6. Methodus nova integralium valores per approximationem inveniendi; 7. Demonstratio nova altera theorematis omnem functionem algebraicam rationalem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse; 8. Theorematis de resolubilitate functionum algebraicarum integrarum in factores reales demonstratio tertia. Supplementum commentationis praecedentis; 9. Theorematis fundamentalis in doctrina de residuis quadraticis demonstrationes et ampliationes novae; 10. Determinatio attractionis, quam in punctum quodvis positionis datae execeret planeta, si eius massa per totam orbitam, ratione temporis, quo singulae partes describuntur, uniformiter esset dispertita; 11. Theoria residuorum biquadraticorum, Commentatio prima; 12. Supplementum theoriae combinationis observationum erroribus minimis obnoxiae; 13. Disquisitiones generales circa superficies curvas.* Göttingen: Dieterich, 1811, 1813, 1816, 1820, 1828.

$9,500

4to (238 x 188 mm), pp. xx, 564 with 17 folding plates (Vol. I); xxiv, 565 with 11 folding plates (Vol. II); pp. xxiv, 564 with 16 folding plates (Vol. III); xxiv, 512 with 18 folding plates (Vol. IV); xxiv, 579 with 24 folding plates (Vol. VI). In vols. I & II, each article is separately-paginated; in vols. III-VI, each section is separately-paginated. Vol. I in contemporary full calf, upper compartment of spine missing,



99

DISQUISITIONES GENERALES
CIRCA
SUPERFICIES CURVAS

AUCTORE
CAROLO FRIDERICO GAUSS.
SOCIETATI REGIAE OBLATAE D. 8. OCTOB. 1827.

1.

Disquisitiones, in quibus de directionibus variarum rectarum in spatio agitur, plerumque ad maius perspicuitatis et simplicitatis fastigium euehuntur, in auxilium vocando superficiem sphaericam radio = 1 circa centrum arbitrarium descriptam, cuius singula puncta repraesentare censebuntur directiones rectarum radiis ad illa terminatis parallelarum. Dum situs omnium punctorum in spatio per tres coordinatas determinatur, puta per distantias a tribus planis fixis inter se normalibus, ante omnia considerandae veniunt directiones axium his planis normalium: puncta superficiei sphaericae, quae has directiones repraesentant, per (1), (2), (3) denotabimus; mutua igitur horum distantia erit quadrans. Ceterum axium directiones versus eas partes acceptas supponemus, versus quas coordinatae respondentes crescunt.

N 2

Vols. II, III, IV, and VI in uniform contemporary half-calf. All volumes with old ex-library rubber stamps.

First editions, journal issues, of thirteen important papers by Gauss, including works on the fundamental theorem of algebra, number theory, hypergeometric functions, approximation theory, differential geometry, gravitation, and celestial mechanics. "Gauss ranks, together with Archimedes and Newton, as one of the greatest geniuses in the history of mathematics" (*Printing & the Mind of Man*, p. 155). "In his first years at Göttingen, Gauss experienced a second upsurge of ideas and publications in various fields of mathematics. Among the latter were several notable papers inspired by his work on the tiny planet Pallas, perturbed by Jupiter [including *Disquisitio de elementis ellipticis palladis*]; *Disquisitlones generates circa seriem infinitam*, an early rigorous treatment of series and the introduction of the hypergeometric functions, ancestors of the "special functions" of physics; *Methodus nova integralium valores per approximationem invenlendi*, an important contribution to approximate integration; … and *Determinatio attractionis quam in punctum quodvis positionis datae exerceret planeta si eius massa per totam orbitam ratione temporis quo singulae partes describuntur uniformiter esset dispertita*, which showed that the perturbation caused by a planet is the same as that of an equal mass distributed along its orbit in proportion to the time spent on an arc. At the same time Gauss continued thinking about unsolved mathematical problems … By 1817 Gauss was ready to move toward geodesy … the period of preoccupation with geodesy was in fact one of the most scientifically creative of Gauss's long career. Already in 1813 geodesic problems had inspired his *Theoria attractionis corporum sphaeroidicorum ellipticorum homogeneorum methodus nova tractata*, a significant early work on potential theory … Surveying problems also motivated Gauss to develop his ideas on least squares and more general problems of what is now called mathematical statistics. The result was the definitive exposition of his mature ideas in the *Theoria combinationis obseruationum erroribus minimis obnoxiae* (with supplement) [only the supplement present in the offered volumes] … However, the crowning contribution of the period, and his last breakthrough in a major new direction of mathematical research, was *Disquisitiones generates circa superficies curvas*, which grew out of his geodesic meditations of three decades and was the seed of more than a century of work on differential geometry" (DSB).

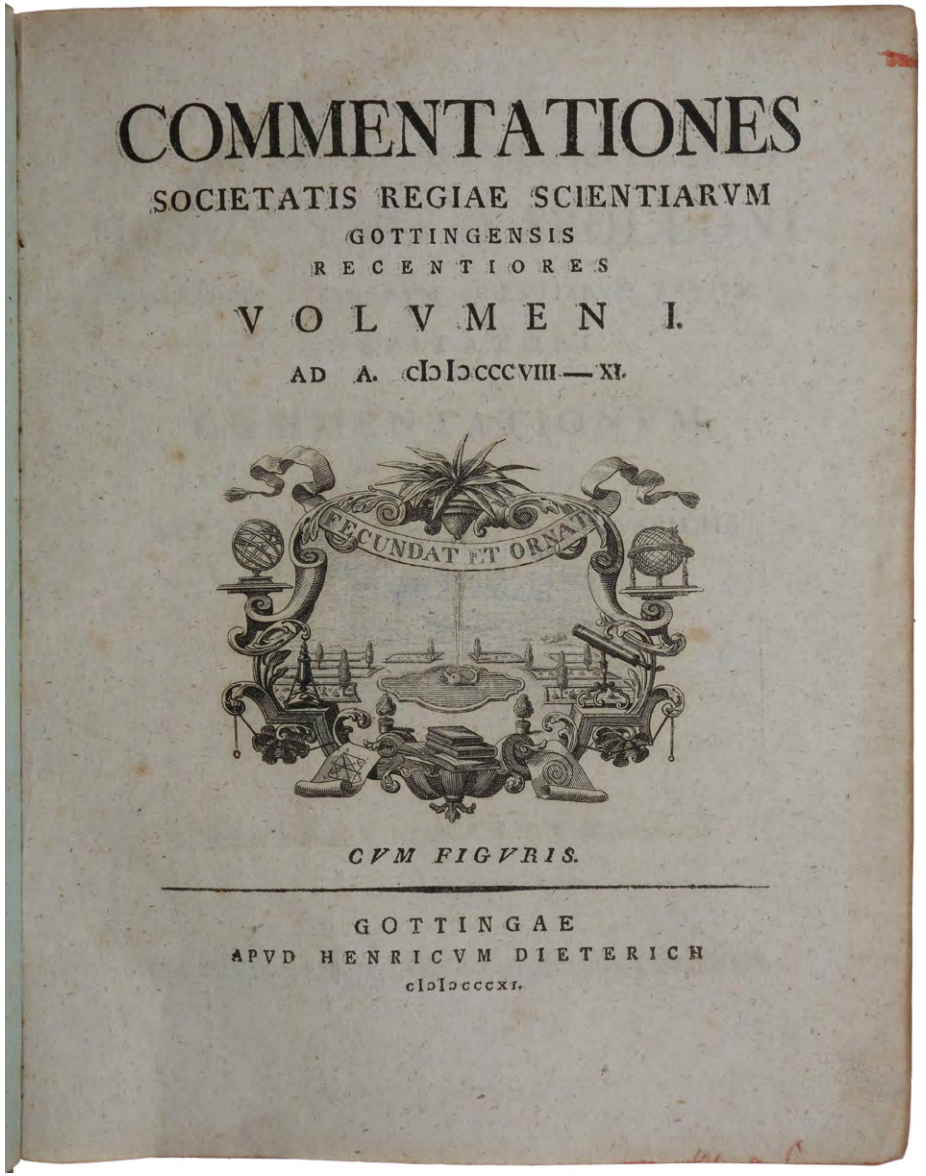
Gauss's first great work was his doctoral thesis, *Demonstratio Nova Theorematis Omnem Functionem Algebraicam Rationalem Integram unius Variabilis in Factores Reales Primi vel Secundi Gradus resolvi posse* (1799), in which he gave the first rigorous proof of the fundamental theorem of algebra. This theorem, which states that every algebraic equation in one unknown has a root, was stated by Albert Girard, Descartes, Newton, and Maclaurin. Attempts at a proof were made by d'Alembert, Euler, and Lagrange, but Gauss was the first to furnish a rigorous demonstration. However, Gauss's proof of 1799, which used geometric notions, was itself not completely rigorous (its gaps were not completely filled until 1920), and Gauss himself was not satisfied with it. In 1816 he published two further proofs, *Demonstratio nova altera theorematis omnem functionem algebraicam rationalem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse and Theorematis de resolubilitate functionum algebraicarum integrarum in factores reales demonstratio tertia*. The first of these is almost strictly algebraic, but it is not really complete as it presupposes that every real equation of odd degree has a real root and that every quadratic equation with complex coefficients has two complex roots. The third proof is easier but uses calculus.

Two of the papers in these volumes deal with analysis (i.e., calculus). *Disquisitlones generales circa seriem* "has several claims to fame: it considers x as a complex variable; and it contains the earliest rigorous argument for the convergence of a power series and a study of the behaviour of the function at a point on the boundary of the circle of convergence, as well as a thorough examination of

continued fraction expansions for certain quotients of hypergeometric functions" (Gray, *Linear Differential Equations and Group Theory from Riemann to Poincaré* (2010), p. 5).

In *Methodus nova integralium valores per approximationem inveniendi*, Gauss introduced new quadrature (i.e., integration) formulas with a degree of accuracy considerably improved as compared with the Newton-Cotes formula. The latter formula gives an approximate value for the integral of a function over an interval in terms of the values of the function at the intermediate points obtained by dividing the interval into n equal parts. Gauss asked what could be accomplished if these intermediate points were available as parameters to be chosen? He answered this question completely, leading to the so-called Gauss quadrature formula. He showed that if these division points are taken to be the roots of a Legendre polynomial of degree n, the quadrature formula is exact for the integration of polynomials of degree up to $2n - 1$. In fact, Gauss did not use Legendre polynomials in his proof, he used his hypergeometric function instead (which contain the Legendre polynomials as a special case). "Gauss's method is not only elegant but it differs from the more modern approaches in that he arrives at his result from first principles, and not, as is now done, by showing how the result follows from properties of Legendre's polynomials" (Goldstine, *A History of Numerical Analysis* (1977), p. 225).

"Three of the papers deal with gravitation and celestial mechanics. "Gauss used the method of least squares to determine the orbit of Ceres in 1801 just after the discovery of the planet by Piazzi. When a second minor planet, Pallas, was later discovered by Olbers, Gauss again set about determining its precise orbit, and he explained his approach in a memoir dated 1810 [*Disquisitio de elementis ellipticis palladis*]. Starting from six observations made when the planet was in opposition — and so close to the Earth — Gauss obtained 12 equations between


COMMENTATIONES

SOCIETATIS REGIAE SCIENTIARVM

GOTTINGENSIS

RECENTIORES

VOLVMEN I.

AD A. CIↃIↃCCCVIII — XI.

FECUNDAT ET ORNAT

CVM FIGVRIS.

GOTTINGAE

APVD HENRICVM DIETERICH

CIↃIↃCCCXI.

6 unknowns (the mean anomaly, the mean diurnal movement, the longitude of the perihelion, the eccentricity, the longitude of the node, the inclination). After obtaining an approximate solution, he determined 12 linear equations that must satisfy corrections to be made to the 6 unknowns. Rejecting the tenth because it was too imprecise on account of the observation, he used 11 equations from which he derived 6 normal equations and finally 6 corrections. In order to make it easier to deal with the algebraic solution of the system formed by the normal equations, Gauss made certain remarks at the end of the T*heory of the Movement of Celestial Bodies* (1809) which he developed in his Memoir on the minor planet Pallas (1810), and then in the second part of the *Theory of the Combination of Observations* which appeared in 1823 [the penultimate paper offered here]. These remarks remind us of what is now called, in linear algebra, Gaussian elimination, or the Gauss pivot method, and the transformation of a quadratic into quadratic form, that is into a sum of squares, which produces a diagonal matrix" (Chabert, *A History of Algorithms* (1999), p. 291). "In 1810, in his *Disquisitio de Elementis ellipticis Palladis*, Gauss sets out to determine details ('elliptical elements') about the orbit of Pallas, the second-largest asteroid of the solar system. He obtains a system of linear equations in six unknowns, where not all equations can be satisfied simultaneously. Hence he needs to determine values for the unknowns that will minimize the total squared error. Instead of merely solving the problem at hand, Gauss digresses and introduces a method for dealing with such systems of linear equations in general" (Althoen & McLaughlin, 'Gauss-Jordan reduction: a brief history,' *The American Mathematical Monthly* 94 (1987), p. 134).

In *Determinatio attractionis*, Gauss's astronomical work led to different mathematical ideas. In this paper he showed that the gravitational attraction due to a planet is the same as that of an equal mass distributed along its orbit in proportion to the time spent on an arc, and that the attraction caused by such a 'Gaussian ring' could be expressed in terms of elliptic integrals. He related the evaluation of these integrals to the 'arithmetic-geometric mean'. This was the only work he published on elliptic integrals (although much more remained in manuscript and was published after his death).

Theoria attractionis corporum sphaeroidicorum ellipticorum homogeneorum methodus nova tractate, which is arguably the first work in potential theory, contains the first indication of what was later called the 'divergence theorem', which relates integrals over a volume in three-dimensional space with integrals over its bounding surface. "The contributions Gauss's paper makes are twofold: first, the idea of the surface of a body and its general local infinitesimal area, and secondly, the process of lines entering and exiting from the surface. These two concepts remained long after the rest of the paper had been forgotten" (Harman, *Wranglers and Physicists* (1985), pp. 114-5). The divergence theorem was proved in complete generality by George Green in 1828.

The number-theoretic papers in these volumes all relate to the law of quadratic reciprocity, which Gauss privately referred to as the *Aureum Theorema* (Golden Theorem). This was discovered by Euler in 1783 but the first proof was given by Gauss in his *Disquisitiones arithmeticae* (1801). In *Theorematis fundamentalis in doctrina de residuis quadraticis demonstrationes et ampliationes novae* Gauss provided his fifth and sixth proofs which are particularly important since, as Gauss explains in the introduction to this work, the techniques they employ ('Gauss's lemma' and 'Gauss sums,' respectively) can be applied to the study of cubic and biquadratic reciprocity. Gauss sums were studied in his *Summatio quarumdam serierum singularium*.

"Gauss' fifth proof of the quadratic reciprocity is Gauss' most elementary proof of the quadratic reciprocity law… for which Gauss has given altogether eight proofs, six of which were published, and two more were found in his papers after

his death. The fifth proof was published in 1818, together with the sixth proof, under the title *Theorematis Fundamentalis in Doctrina de Residuis Quadraticis Demonstrationes et Ampliationes Novae*. But Gauss found it probably already between 1807 and 1808… [it is] based on Gauss' lemma" (Laudal & Piene, *The legacy of Niels Henrik Abel* (2004), p. 276).

"The sixth and last of Gauss' published proofs of the law of quadratic reciprocity was published in 1818… He mentions in the introduction to this paper that for years he had searched for a method that would generalize to the cubic and biquadratic case and that finally his untiring efforts were crowned with success… The purpose of publishing this sixth proof, he states, was to bring to a close that part of the higher arithmetic dealing with quadratic residues and to say, in a sense, farewell…" (Ireland & Rosen, *A classical introduction to modern number theory* (1998), p. 76).

The attempt to generalize quadratic reciprocity for powers higher than the second was one of the main goals that led nineteenth century mathematicians, including Gauss, Dedekind, Dirichlet, Eisenstein, Hilbert, Jacobi and Kummer to the study of general algebraic number fields; notably, Kummer invented 'ideal numbers' in order to state and prove higher reciprocity laws. The ninth in the list of 23 unsolved problems which David Hilbert proposed to the International Congress of Mathematicians in 1900 asked for the "Proof of the most general reciprocity law for an arbitrary number field." The first steps were taken by Gauss himself in *Theoria residuorum biquadraticorum* [of which the first part appears in this collection – a second part appeared in Vol. VII]. In this paper Gauss stated (but did not prove) the law of biquadratic [i.e., quartic] reciprocity.

The last paper in the collection is the most important, *Disquisitiones generales circa superficies curvas*. The most important result of this masterpiece in the mathematical literature is the *Theorema egregium*" (Zeidler, *Quantum Field theory III. Gauge Theory* (2013), p. 15). "A decisive influence on the entire course of development of differential geometry was exerted by the publication of [this] paper of Gauss… It was this paper, carefully polished and containing a wealth of new ideas, that gave this area of geometry more or less its present form and opened a large circle of new and important problems whose development provided work for geometers for many decades" (Kolmogorov & Yushkevitch, *Mathematics of the 19th century: Geometry. Analytic Function Theory* (1996), p. 7).

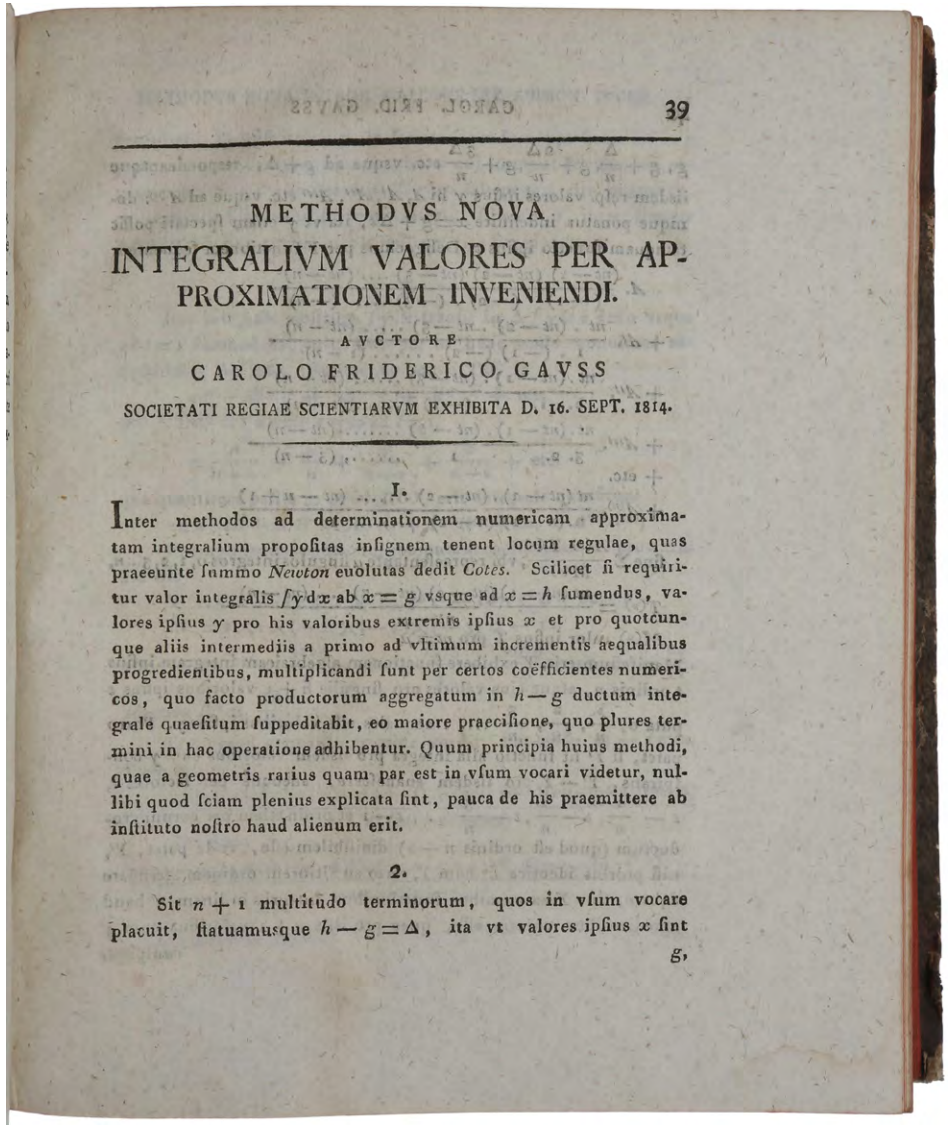
The *Theorema egregium* shows that a certain measure of how curved a surface is (now called its Gaussian curvature) is 'intrinsic,' i.e. can be determined by measurements made only on the surface and without reference to the ambient three-dimensional space. "Gauss' *Theorema egregium* had an enormous impact on the development of modern differential geometry and modern physics culminating in the principle 'force equals curvature'. This principle is basic for both Einstein's theory of general relativity on gravitation and the Standard Model in elementary particle physics" (Zeidler, p. 16). Gauss also shows that certain other features of a surface, such as its geodesic lines (the shortest paths), are intrinsic.

A second major result contained in the present paper, which perhaps had even greater ramifications in mathematics than the *Theorema egregium*, is a version of what is now known as the Gauss-Bonnet theorem. Gauss showed that the total Gaussian curvature of the interior of a triangle on a surface with geodesic sides is the amount by which the sum of the angles of the triangle exceeds its Euclidean value π. This result was generalized by Pierre-Ossian Bonnet in 1848 and by many later mathematicians, and served as the prototype of results linking the local geometry of a space (e.g. its curvature) with its 'topology' (its overall shape), a theme which pervades much of 20th century mathematics. Remarkably, Gauss describes at the end of the present work some measurements he has made

to verify the Gauss-Bonnet theorem. He tells us that he has measured the angles of a triangle, the greatest side of which was more than 15 miles, and found that the sum of the angles of the triangle is greater than two right-angles by almost 15 seconds of arc, in agreement with the theorem as it applies to the surface of the Earth.

The discovery that some important geometrical properties of a surface are intrinsic suggested that a surface should be treated as a space with its own geometry; this is the idea that was taken up and generalized to higher dimensions by Riemann in his Habilitationsschrift. “In his approach to differential geometry, Riemann used ideas from Carl Friedrich Gauss’s theory of surfaces, but liberated them from the restriction of being embedded in (three-dimensional) Euclidean space. He started from a determination of the length of a line element as a positive-definite quadratic differential form to derive further notions depending on metrics, in particular that of the geodesic line. Moreover, he introduced the sectional curvature of an infinitely small surface element, derived from the Gaussian curvature of the associated finite surface inside the manifold, which is generated by all geodesic lines starting in the surface element” (*Companion Encyclopedia*, p. 928).

Many, and possibly all, of these papers were also issued in offprint form, although most of them are extremely rare. While in many cases offprints were published later than the journal, in this case the offprints probably take precedence. For example, *Methodus nova integralium valores per approximationem inveniendi* appeared as an offprint in 1815 but in the journal in 1816, and *Determinatio attractionis* appeared as an offprint in 1818 but in the journal in 1820.



39

METHODVS NOVA
INTEGRALIVM VALORES PER AP-
PROXIMATIONEM INVENIENDI.

AVCTORE
CAROLO FRIDERICO GAVSS

SOCIETATI REGIAE SCIENTIARVM EXHIBITA D. 16. SEPT. 1814.

I.

Inter methodos ad determinationem numericam approximatam integralium propositas insignem tenent locum regulae, quas praeeunte summo *Newton* evolutas dedit *Cotes*. Scilicet si requiritur valor integralis $\int y\,dx$ ab $x = g$ vsque ad $x = h$ sumendus, valores ipsius y pro his valoribus extremis ipsius x et pro quotcunque aliis intermediis a primo ad vltimum incrementis aequalibus progredientibus, multiplicandi sunt per certos coëfficientes numericos, quo facto productorum aggregatum in $h - g$ ductum integrale quaesitum suppeditabit, eo maiore praecisione, quo plures termini in hac operatione adhibentur. Quum principia huius methodi, quae a geometris rarius quam par est in vsum vocari videtur, nullibi quod sciam plenius explicata sint, pauca de his praemittere ab instituto nostro haud alienum erit.

2.

Sit $n + 1$ multitudo terminorum, quos in vsum vocare placuit, statuamusque $h - g = \Delta$, ita vt valores ipsius x sint

g,

AN EXCEPTIONALLY FINE COPY

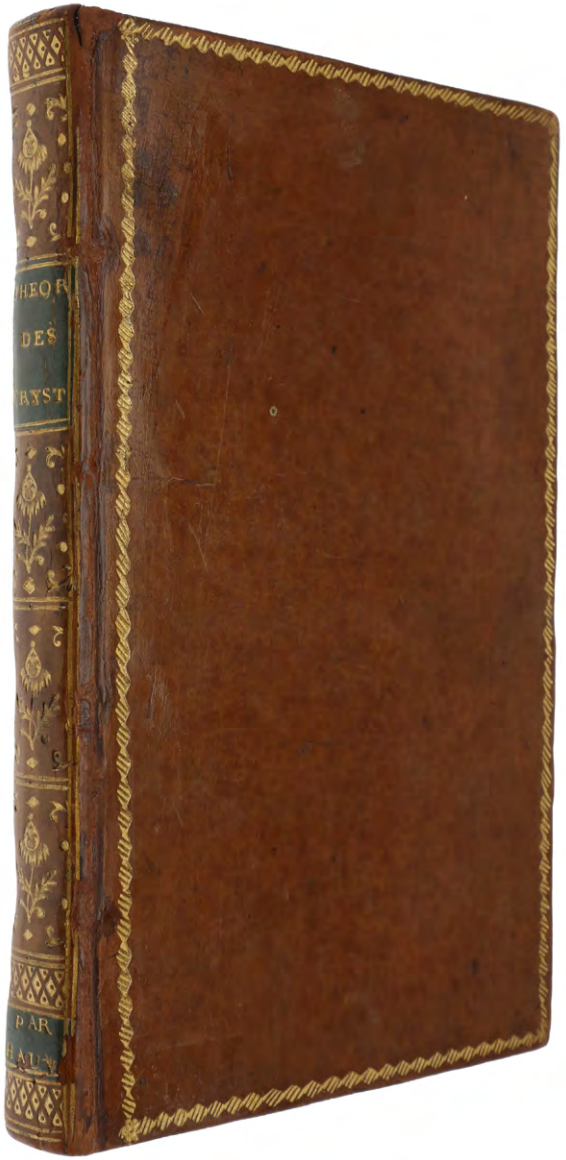
[Dibner 92; *En Français dans le Texte* 176; Norman 1021; Sparrow 94]

HAÜY, René-Just. *Essai d'une Théorie sur la Structure des Crystaux, appliquée a plusieurs Genres de Substances crystallisées.* Paris: de la Rochelle, 1784.

$8,500

8vo (192 x 128 mm), pp. [viii], [1], 2-236, with eight folding engraved plates by Sellier after Fossier. Contemporary French polished calf, covers with gilt border, spine richly gilt with floral decoration and two black lettering-pieces (joints slightly rubbed). A very fine copy.

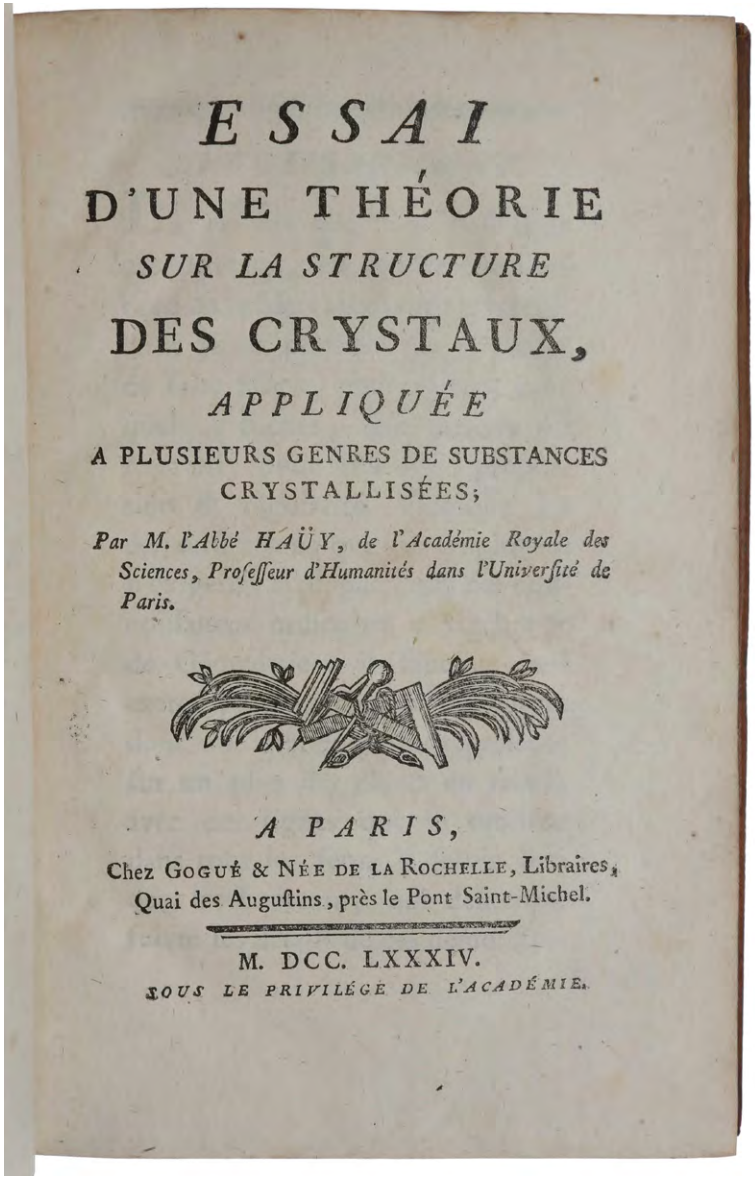
First edition of Haüy's first printed book, very rare in such fine condition. "The foundation of the mathematical theory of crystal structure. Haüy proposed six types of primary crystal forms (parallelpiped, rhombic dodecahedron, hexagonal dipyramid, right hexagonal prism, octahedron and tetrahedron), and argued that the primitive form or 'nucleus' of a crystal species results from the cleavage of all secondary forms, although for him this 'nucleus' was a mathematical concept rather than a physical reality. He put forward the idea of the crystal molecule, and recognized the discontinuity principle; i.e., that not all angles and inclinations of crystal faces are possible, and therefore the varieties of a crystal species are limited. He derived secondary forms theoretically by stacking layers of molecules on the faces of the nucleus, enunciating the laws of decretement (reduction of each successive layer) and symmetry that regulate their growth. His law of decretement led his successors directly to the law of rational indices (sometimes called 'Haüy's law') (Norman). "Romé de l'Isle had deduced the various forms of the same crystal species by truncating the edges or the solid



angles of the rather arbitrarily selected primitive form. Haüy established a more rigorous mathematical relationship between primary and secondary forms of the same species, and his choice of the primary form was founded on more physical grounds" (DSB). "*L'Essai d'une Théorie sur la Structure des Crystaux* est la premier grand ouvrage d'Haüy. C'est un modèle de logique dans le raisonnement et de clarté dans l'expression" (*En Français dans le Texte*).

Haüy's first works were two memoirs submitted to the Académie Royale des Sciences in 1781. In these papers Haüy developed a lamellar theory of crystal structure, following Torbern Bergmann, although Haüy's approach was much more rigorous and elaborate. "With his 1784 *Essai d'une Théorie sur la Structure des Crystaux, appliquée a plusieurs Genres de Substances crystallisées*, Haüy breaks away radically from his own and Bergmann's lamellar theory and introduces his molecular theory: in the case of calcite, for example, 'the nucleus, or primitive form, has as constituting molecules small rhombohedra, rather than lamellae'. The constituting molecules ('molécules constituantes') are those which were floating in the fluid where the crystal was dissolved, which were mutually attracted and became agglomerated during crystallization. The shape of these constituting molecules can be found with the help of the mechanical division of the crystal, or the striations on the surface. The cleavage takes place between two molecules and can be done at any place in the crystal; if the mechanical division is pushed to its ultimate limit, therefore, it leads necessarily to identical and uniform molecules. Their arrangement, which constitutes the 'structure' of the crystal, spreads regularly throughout the crystal. This is a crucial point, since the notion of three-dimensional periodicity of crystals is here clearly introduced for the first time.

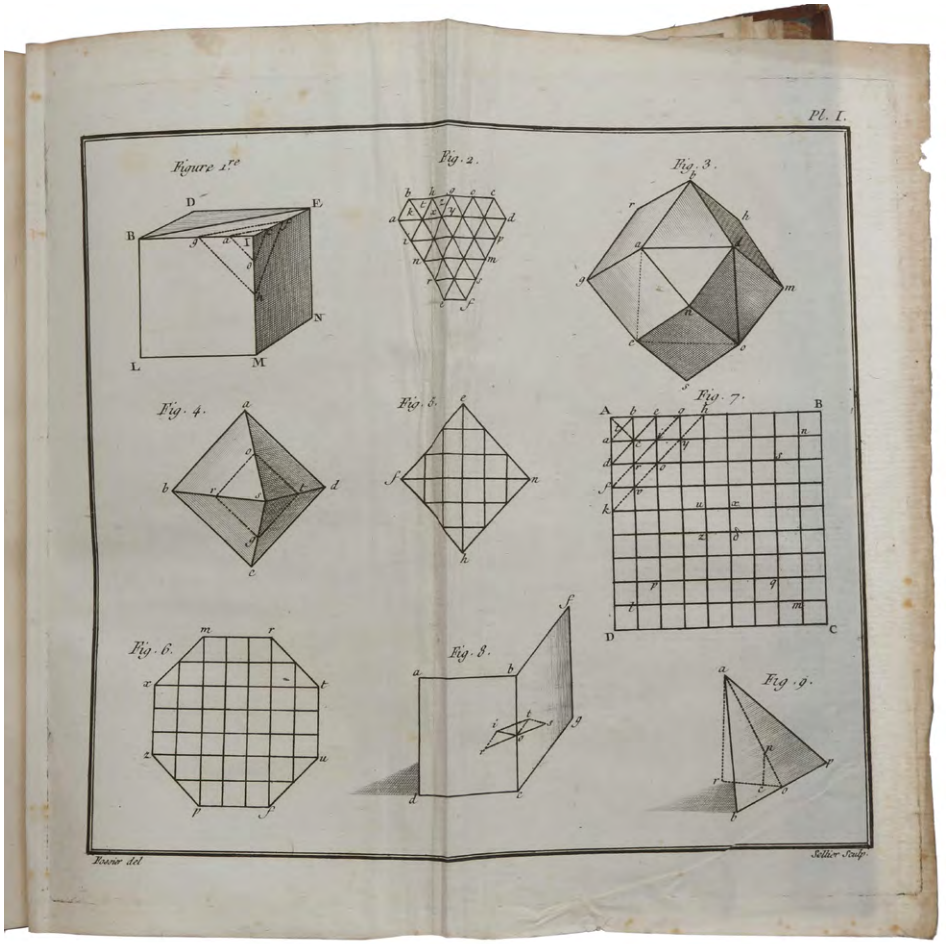
"The secondary forms, which are the external forms actually observed, are obtained by the deposition of layers of constituting molecules. A single layer or group of two or more layers is shiftet with respect to the layers beneath it by


ESSAI
D'UNE THÉORIE
SUR LA STRUCTURE
DES CRYSTAUX,
APPLIQUÉE
A PLUSIEURS GENRES DE SUBSTANCES CRYSTALLISÉES;
Par M. l'Abbé HAÜY, de l'Académie Royale des Sciences, Professeur d'Humanités dans l'Université de Paris.
A PARIS,
Chez Gogué & Née de la Rochelle, Libraires, Quai des Augustins, près le Pont Saint-Michel.
M. DCC. LXXXIV.
SOUS LE PRIVILÉGE DE L'ACADÉMIE.

one, two, three or, rarely, four rows. In theory, the number of possibilities is very large, but in practice the law describing how the secondary form is derived from the primitive form is always simple, it is the *law of decrements, or law of simple rational truncations* ('Haüy's law). The French mineralogist and crystallographer G. Friedel pointed out that the term 'simple' in the wording of the law is essential. Without it, the law would be a simple mathematical statement, impossible to verify, and not a physical law.

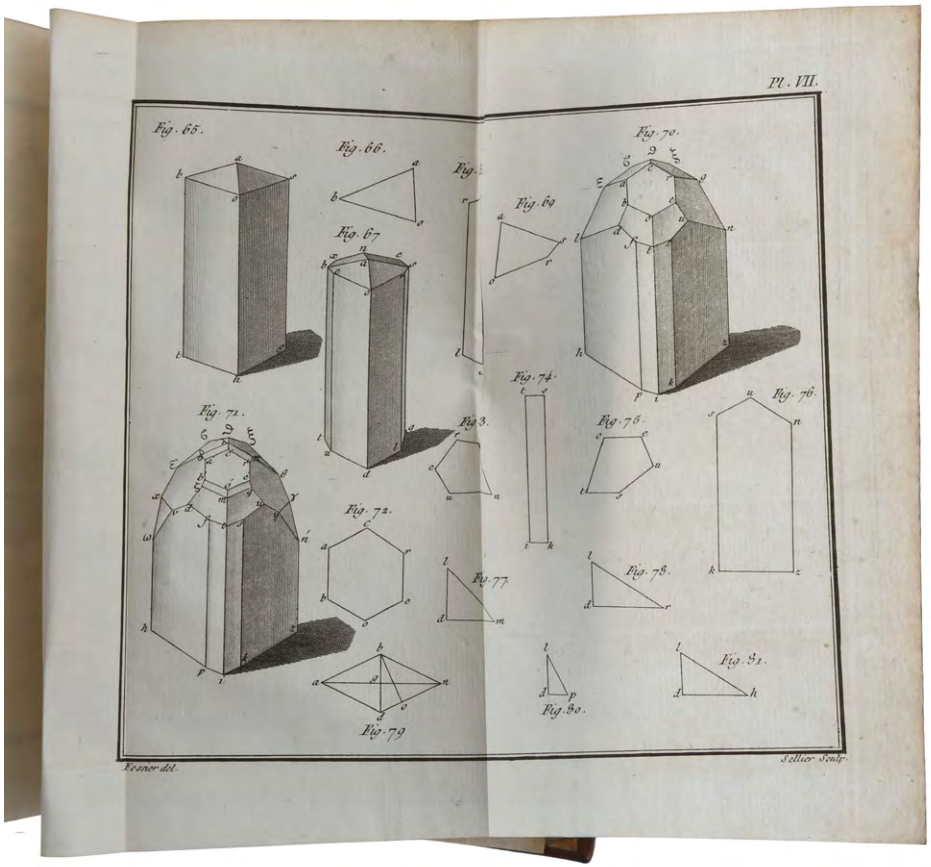
"Haüy observes that the resultant faces of the secondary forms will be stepped, and look like a staircase, but that these steps are so small that they are invisible to the naked eye. Due to the irregularities of the growth, they may in fact sometimes be larger and give rise to the situations one observes at the surface of the crystal. There are no longer half-rhombs along the edges of the layers. The decrements can take place along an edge, around a summit or both (mixed decrements).

"The habit of a particular crystal is the result of the association of various secondary forms which can be interpreted by means of appropriate laws of decrement. After having described the different forms of calcite, the primitive forms of various minerals are determined: the rhombic prism for barite, the octahedron for fluorspar (fluorite), a prism with a rectangular base for gypsum, the rhomb-dodecahedron for garnet, which Haüy recalls is the shape of the honeycomb cell, a rhombic prism for topaz. There is a particular difficulty in the case of fluorspar. A common habit is the cube, and Haüy notes that the smallest crystals, at the start of growth, are little cubes. But the primitive form, obtained by cleavage, is an octahedron. During growth, the layers which agglomerate are parallel to the surface of the cube, but they are not smooth, and are covered with tiny spikes corresponding to the summits of octahedra or tetrahedra" (Authier, *Early Days of X-ray Crystallography* (2013), pp. 322-3).



"As a boy, René-Just (1743-1822) loved music, and his frequent attendance at the services of the local church drew the attention of the prior of an abbey of the Premonstrants. Thanks to his recommendation, Haüy obtained a scholarship at the College de Navarre in Paris around 1755. After completing his studies, he was appointed, in 1764, as regent (master) in the College and in 1770 in the Cardinal Lemoine College where he taught until his retirement in 1784. In 1770, he was ordained priest. He was elected at the Académie Royale des Sciences in 1783 as adjoint in the class of botany and, in 1788, as *associé* in the class of Natural History and Mineralogy. In 1791 he became a member of the commission in charge of elaborating the metric system. During the French Rcvolution, all priests were required to take an oath, which Haüy refused to do, with the result that he was briefly imprisoned in August 1792, freed thanks to the help of his friend, the French naturalist Geoffroy Saint-Hilaire (1772-1844). He was appointed Professor of Physics at the École Normale Supérieure in 1795, when the École was created. His lectures served as the basis of his treatise on physics for high schools (1803), commissioned by Bonaparte; the book was an immediate success and was re-edited several times. In 1795 he was appointed Professor of Mineralogy at the École des Mines and curator of its mineralogy collection, and, in 1802, at the Museum of Natural History. His fame extended beyond the frontiers and attracted many students from every part of Europe. In 1809, he became the first Professor of Mineralogy at Paris University, where he created the still existing Laboratory of Mineralogy. After the Restoration he lost his pension and finished his life in poor conditions" (ibid., p. 319).

Dibner, *Heralds of Science*, 92; *En Français dans le Texte* 176; Horblit 47; Norman 1021-1022; Sparrow, *Milestones of Science*, 94; Ward & Carozzi, *Geology Emerging* (1984), no. 1020; Wilson, *History of Mineral Collecting* (1994), pp. 53-56.



INSCRIBED AUTHOR'S OFFPRINT

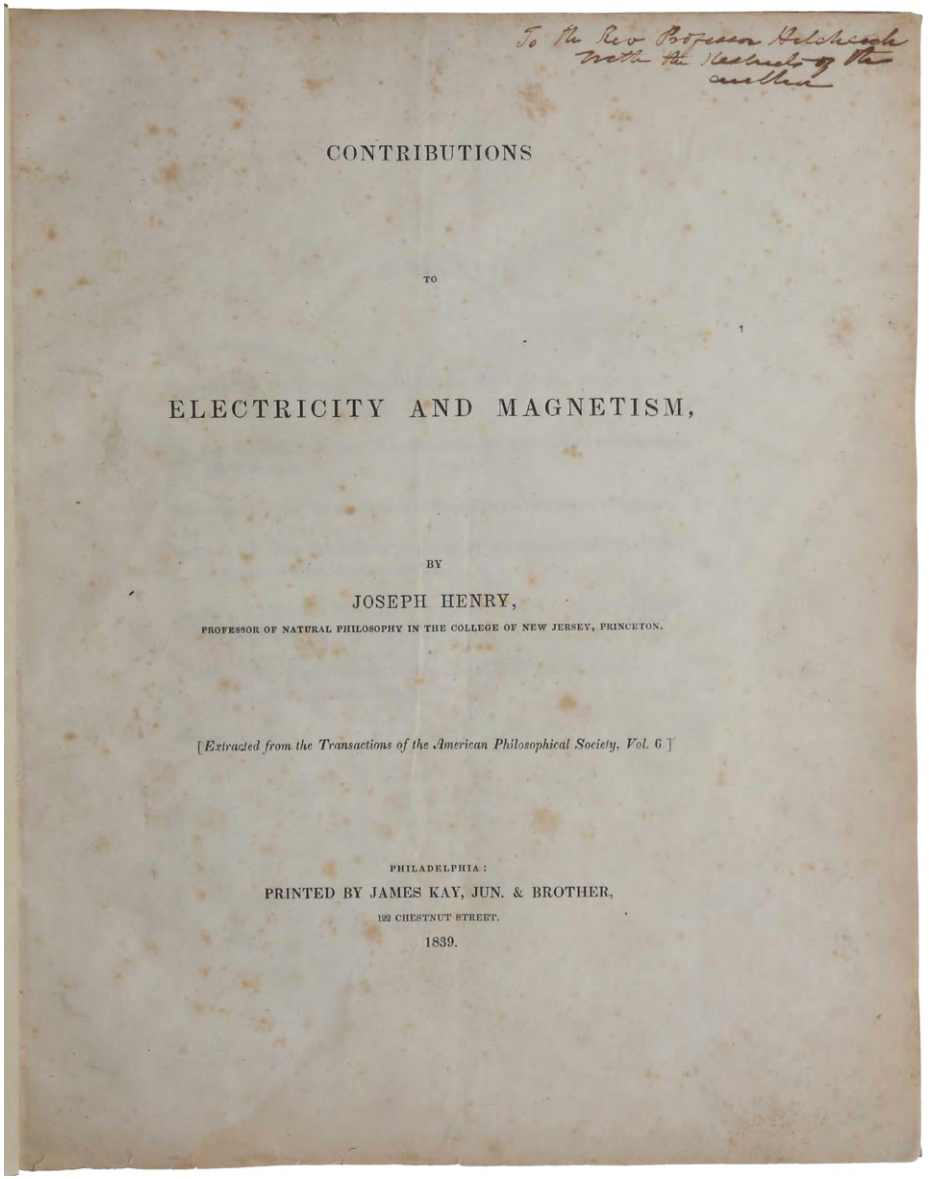
HENRY, Joseph. *Contributions to Electricity and Magnetism. No. III. - On Electro-Dynamic Induction.* Philadelphia: Printed by James Kay, Jun. & Brothers, 1839.

$4,500

4to (278 x 225 mm), pp. [iv], [17] 18-51 [52:blank], illustrations in text. Some very light spotting throughout. Modern paper wrappers, preserved in a quarter-morocco custom clamshell box.

First edition, author's presentation offprint, of the third and most important part of Henry's *Contributions to Electricity and Magnetism*. Following on from his discovery of electromagnetic self-induction in 1832, in the present paper Henry extends his results on 'galvanic' electricity (current produced by batteries) to the inductive effects of static electricity. Along the way, he established the principle of the electrical transformer, which was crucial to later nineteenth-century science and technology. Henry himself considered the findings reported in this paper "the most important I have ever made" (see below). OCLC lists only one copy of this offprint (Yale Medical Library).

Provenance: Presentation inscription in the author's hand on the title page, "To the Rev. Professor Hitchcock with the respects of the author." The recipient is probably Edward Hitchcock (1793-1864), ordained Congregationalist pastor, Professor of Chemistry and Natural History at Amherst College (1825-1845), of Natural Theology and Geology (1845-1864), and third President of the College (1845-1854). Like Henry himself, Hitchcock was a founding member of the American Association for the Advancement of Science.


To the Rev Professor Hitchcock
with the respects of the
author

CONTRIBUTIONS

TO

ELECTRICITY AND MAGNETISM,

BY

JOSEPH HENRY,

PROFESSOR OF NATURAL PHILOSOPHY IN THE COLLEGE OF NEW JERSEY, PRINCETON.

[*Extracted from the Transactions of the American Philosophical Society, Vol. 6.*]

PHILADELPHIA:

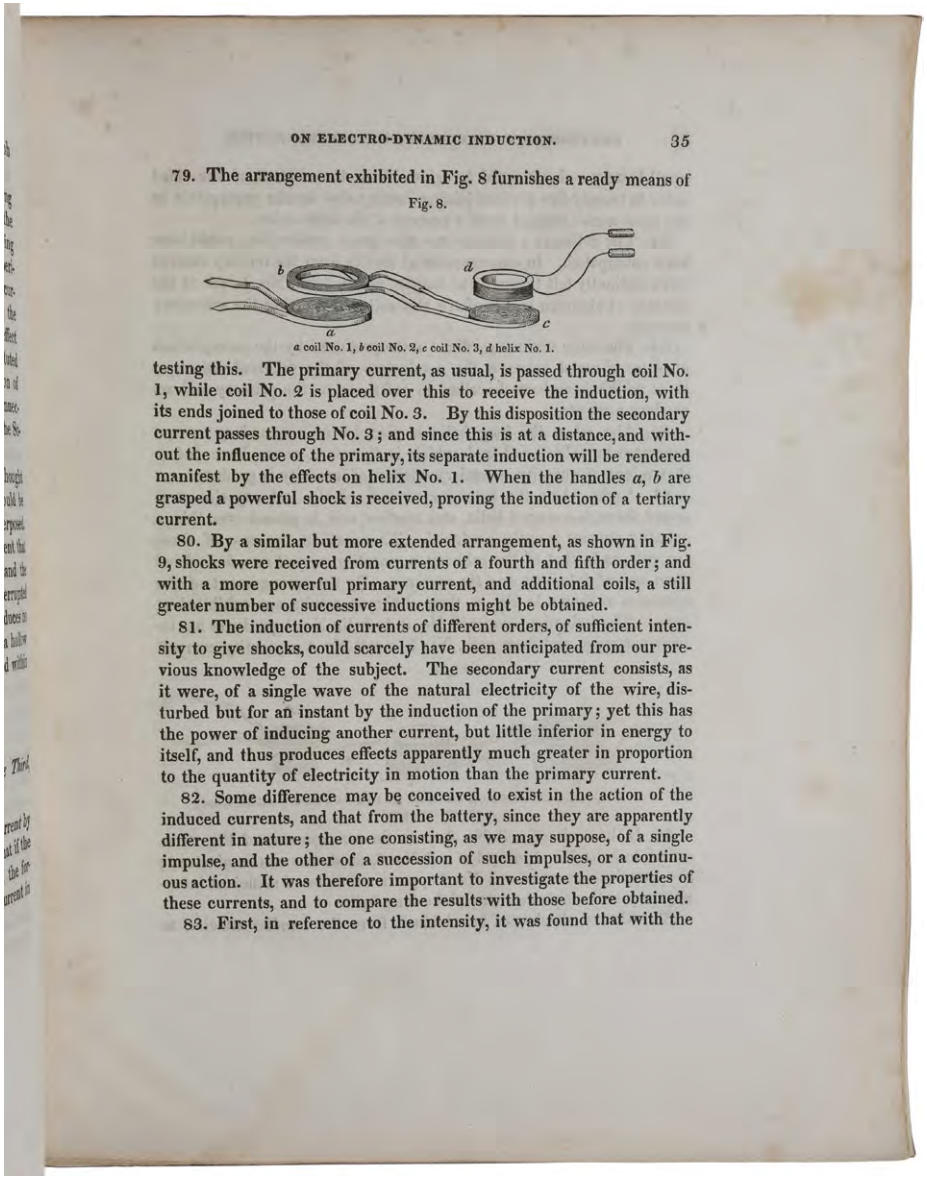
PRINTED BY JAMES KAY, JUN. & BROTHER,

122 CHESTNUT STREET.

1839.

In 1830, when he was an instructor in mathematics at The Albany Academy (New York), Joseph Henry (1797-1878) was the first to observe the phenomena of electromagnetic mutual- and self-induction (the production of a voltage in a wire as a result of a varying current in another, or the same, wire). He failed to publish his results, however, until 1832, after he had seen Faraday's own publication on mutual induction in the first series of his 'Experimental Researches in Electricity.' Henry's paper, 'On the production of currents and sparks of electricity from magnetism' (*American Journal of Science and Arts*, Vol. 22, No. 2, July 1832), established his priority in the discovery of self-induction, although Faraday took the honour of being the first to publish the discovery of mutual induction. "Joseph Henry's discovery of self induction in 1832 was the first appearance of a major US discovery in electricity since Benjamin Franklin" (sparkmuseum.com/book_henry.htm).

In the autumn of 1832 Henry took up a professorship at the College of New Jersey (later to become Princeton University). Initially lacking suitable equipment and with a heavy teaching load, Henry did not resume his experiments on electromagnetism until the late summer of 1834. He resolved to publish his new results in a series of papers in the *Transactions of the American Philosophical Society* entitled 'Contributions to Electricity and Magnetism,' probably modeled on Faraday's series of 'Experimental Researches' published in the *Philosophical Transactions*. The first two 'Contributions' appeared back-to-back in 1835 in Vol. V of the *Transactions*. The first described a new galvanic battery he had constructed in his Princeton laboratory. The second was prompted by Henry's reading, early in 1835, of the ninth series of Faraday's 'Experimental Researches' in which Faraday set out his own conclusions on self-induction. To ensure his priority in the discovery of self-induction, Henry hastily published 'On the influence of a spiral conductor in increasing the intensity of electricity from a



ON ELECTRO-DYNAMIC INDUCTION. 35

79. The arrangement exhibited in Fig. 8 furnishes a ready means of testing this. The primary current, as usual, is passed through coil No. 1, while coil No. 2 is placed over this to receive the induction, with its ends joined to those of coil No. 3. By this disposition the secondary current passes through No. 3; and since this is at a distance, and without the influence of the primary, its separate induction will be rendered manifest by the effects on helix No. 1. When the handles *a*, *b* are grasped a powerful shock is received, proving the induction of a tertiary current.

Fig. 8.

a coil No. 1, *b* coil No. 2, *c* coil No. 3, *d* helix No. 1.

80. By a similar but more extended arrangement, as shown in Fig. 9, shocks were received from currents of a fourth and fifth order; and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained.

81. The induction of currents of different orders, of sufficient intensity to give shocks, could scarcely have been anticipated from our previous knowledge of the subject. The secondary current consists, as it were, of a single wave of the natural electricity of the wire, disturbed but for an instant by the induction of the primary; yet this has the power of inducing another current, but little inferior in energy to itself, and thus produces effects apparently much greater in proportion to the quantity of electricity in motion than the primary current.

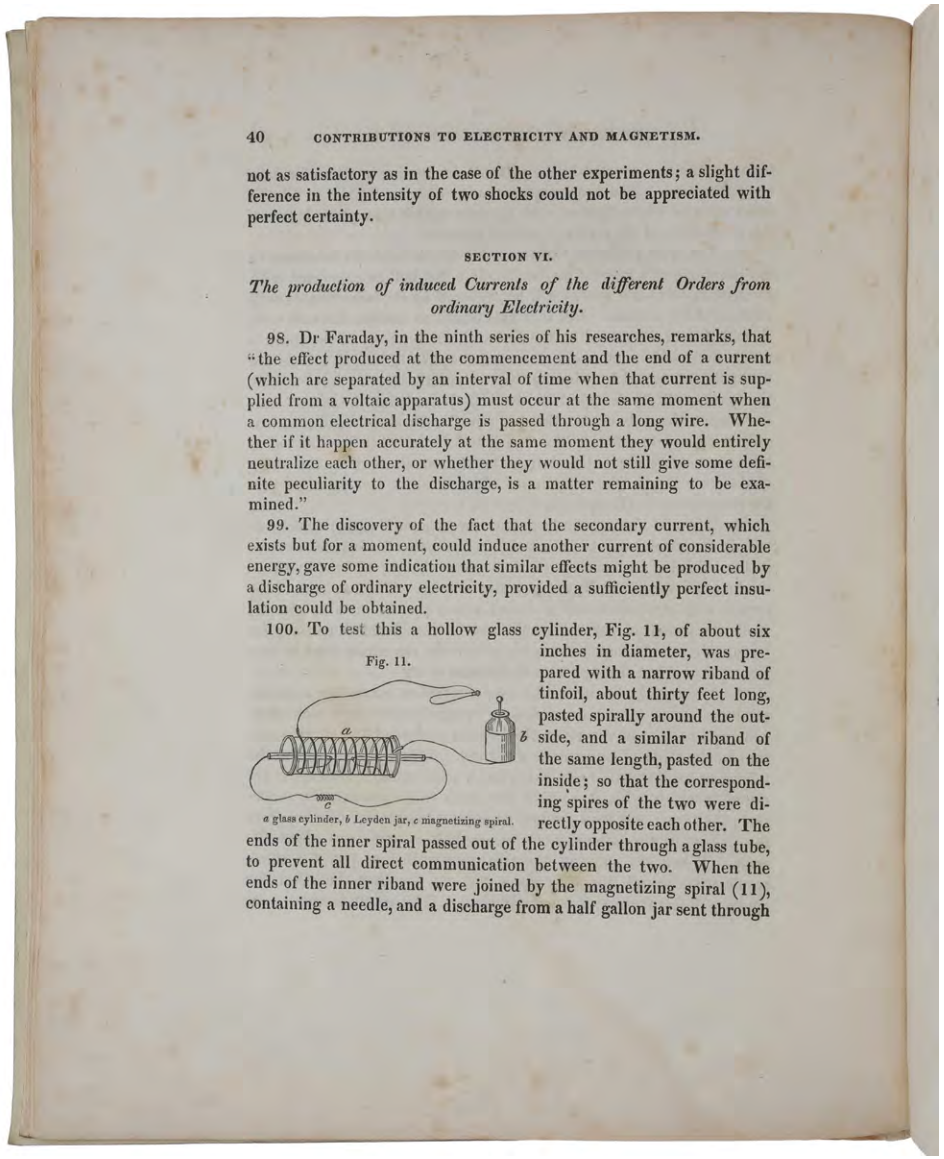
82. Some difference may be conceived to exist in the action of the induced currents, and that from the battery, since they are apparently different in nature; the one consisting, as we may suppose, of a single impulse, and the other of a succession of such impulses, or a continuous action. It was therefore important to investigate the properties of these currents, and to compare the results with those before obtained.

83. First, in reference to the intensity, it was found that with the

galvanic arrangement of a single pair, etc.' (Contribution No. II), which essentially repeated the findings described in his 1832 paper.

After the publication of Contribution No. II, Henry wrote to a colleague that "many new suggestions were presented to my mind which required immediate testing by direct experiment. The result was that the subject grew very rapidly under my investigations and has opened quite a field of research." Henry particularly wished to investigate "the inductive effects of static electricity – specifically, fleeting discharges of Leyden jars into various circuits. These experiments on "common" or "ordinary" electricity complemented his previous investigations involving "galvanic" currents from batteries" (Moyer, *Joseph Henry* (1997), p. 165). But his progress was again delayed – this time by the restoration of Princeton's Philosopher's Hall (which he had instigated) and an 1837 trip to Europe (when he first met Faraday) – and it was not until the spring of 1838 that he was able to resume his research. By early autumn "he wrote to American colleagues that he considered his findings "the most important I have ever made" and projected that his eventual report would contain "a greater number of new generic facts than any I have ever published"…

"Henry published his full findings on static electricity the next year, in 1839, as the third in his APS "Contributions to Electricity and Magnetism." The lengthy, rambling article, which he distributed to a large circle of colleagues at home and abroad, enjoyed a generally positive reception by a wide readership and would be frequently translated and reprinted. The article even would provide incentive for Faraday and Wheatstone to nominate Henry for the most revered scientific prize in Great Britain, if not the world. Though ultimately unsuccessful, Henry would become one of five nominees in 1839 for the Royal Society's Copley Medal [the eventual winner was Robert Brown, of Brownian motion].



40 CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM.

not as satisfactory as in the case of the other experiments; a slight difference in the intensity of two shocks could not be appreciated with perfect certainty.

SECTION VI.

The production of induced Currents of the different Orders from ordinary Electricity.

98. Dr Faraday, in the ninth series of his researches, remarks, that "the effect produced at the commencement and the end of a current (which are separated by an interval of time when that current is supplied from a voltaic apparatus) must occur at the same moment when a common electrical discharge is passed through a long wire. Whether if it happen accurately at the same moment they would entirely neutralize each other, or whether they would not still give some definite peculiarity to the discharge, is a matter remaining to be examined."

99. The discovery of the fact that the secondary current, which exists but for a moment, could induce another current of considerable energy, gave some indication that similar effects might be produced by a discharge of ordinary electricity, provided a sufficiently perfect insulation could be obtained.

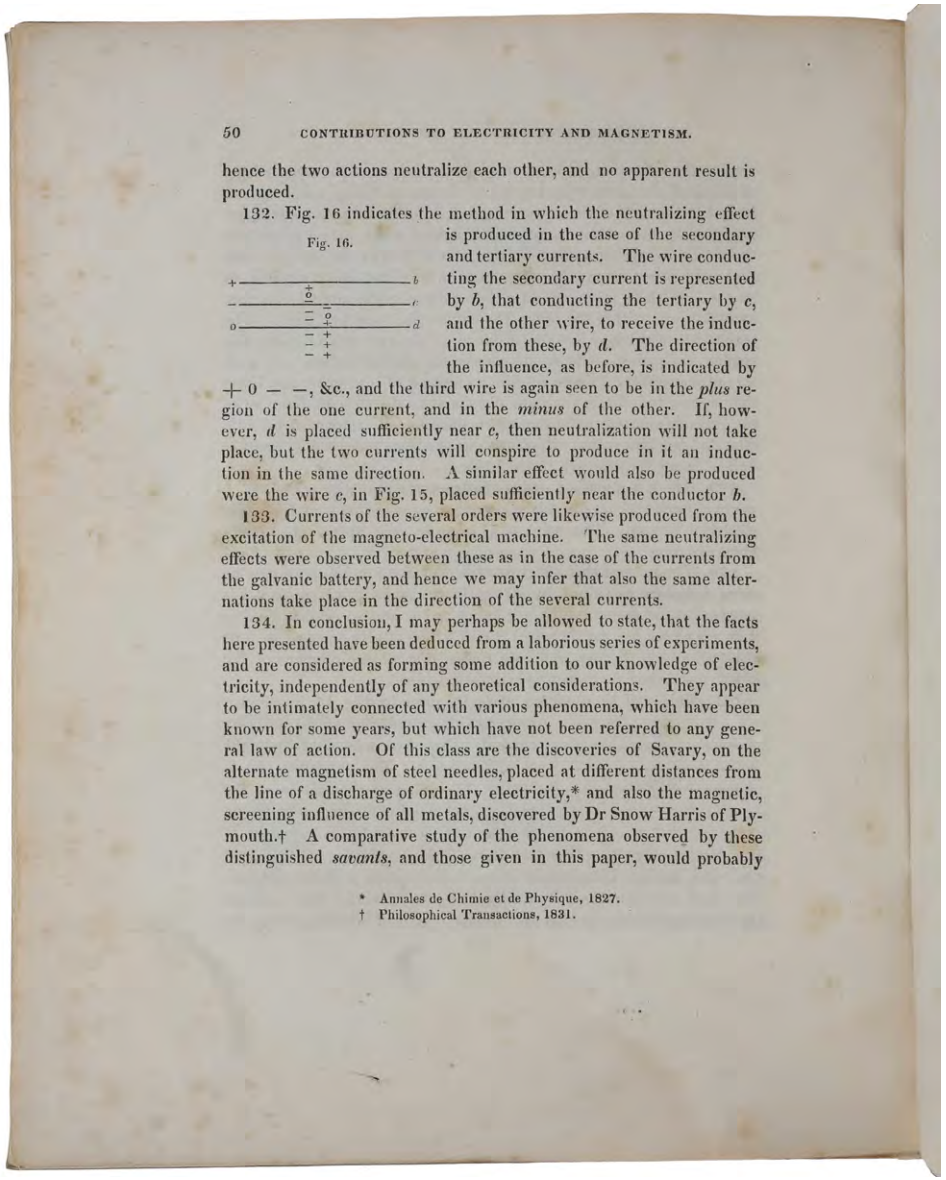
100. To test this a hollow glass cylinder, Fig. 11, of about six inches in diameter, was prepared with a narrow riband of tinfoil, about thirty feet long, pasted spirally around the outside, and a similar riband of the same length, pasted on the inside; so that the corresponding spires of the two were directly opposite each other. The ends of the inner spiral passed out of the cylinder through a glass tube, to prevent all direct communication between the two. When the ends of the inner riband were joined by the magnetizing spiral (11), containing a needle, and a discharge from a half gallon jar sent through

Fig. 11.

a glass cylinder, *b* Leyden jar, *c* magnetizing spiral.

"Early in his new article, Henry summarized the distinction between two types of induction that Faraday had recognized in 1831; he then justified his commitment to exploring, in not only galvanic but also ordinary electricity, the lesser known type. On the one hand, many researchers were developing what he credited as Faraday's (not his own) discovery of the magnetic induction of currents. These researchers were striving particularly to perfect the associated "magneto-electrical machine" – a popular, embryonic electric generator, nicknamed a "magneto," that produced a series of alternating bursts of current. On the other hand, he was alone in extending "the purely electrical part of Dr. Faraday's admirable discovery." By this "purely electrical part" Henry meant Faraday's ancillary finding that a changing galvanic current in a wire induces an oppositely flowing current in a parallel wire. Henry believed that this "Volta-electric induction" helped explain self-induction, the action of a current on itself rather than solely on a parallel wire … Moreover, although he devoted most ink to examples involving galvanic rather than static electricity, he established that Volta-electric induction in galvanic electricity had a counterpart in ordinary electricity. That is, he found that, in analogy to the galvanic case, the discharge of a Leyden jar's ordinary electricity into a wire induced a current in a parallel wire. Since it was common practice to speak of "statical" induction in ordinary electricity and of "dynamic" induction in galvanic currents, Henry would dub this new area of study the "dynamic induction of ordinary electricity." He considered the induction of currents using static electricity an important finding because – as he remarked in his article, and then emphasized to his Princeton students – Faraday had cast doubt on the possibility. Indeed, Henry's results caught Faraday's attention; in the fall of 1839, about when he nominated Henry for the Copley Medal, he mentioned in his diary his intention "to work on Henry's late dynamic induction experiments."

"Henry also showed the existence of induced currents of third, fourth and fifth orders. That is, whereas Faraday had established that a changing galvanic current



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hence the two actions neutralize each other, and no apparent result is produced.

132. Fig. 16 indicates the method in which the neutralizing effect is produced in the case of the secondary and tertiary currents. The wire conducting the secondary current is represented by *b*, that conducting the tertiary by *c*, and the other wire, to receive the induction from these, by *d*. The direction of the influence, as before, is indicated by + 0 − −, &c., and the third wire is again seen to be in the *plus* region of the one current, and in the *minus* of the other. If, however, *d* is placed sufficiently near *c*, then neutralization will not take place, but the two currents will conspire to produce in it an induction in the same direction. A similar effect would also be produced were the wire *c*, in Fig. 15, placed sufficiently near the conductor *b*.

Fig. 16.

133. Currents of the several orders were likewise produced from the excitation of the magneto-electrical machine. The same neutralizing effects were observed between these as in the case of the currents from the galvanic battery, and hence we may infer that also the same alternations take place in the direction of the several currents.

134. In conclusion, I may perhaps be allowed to state, that the facts here presented have been deduced from a laborious series of experiments, and are considered as forming some addition to our knowledge of electricity, independently of any theoretical considerations. They appear to be intimately connected with various phenomena, which have been known for some years, but which have not been referred to any general law of action. Of this class are the discoveries of Savary, on the alternate magnetism of steel needles, placed at different distances from the line of a discharge of ordinary electricity,* and also the magnetic, screening influence of all metals, discovered by Dr Snow Harris of Plymouth.† A comparative study of the phenomena observed by these distinguished *savants*, and those given in this paper, would probably

* Annales de Chimie et de Physique, 1827.
† Philosophical Transactions, 1831.

in one wire induces an opposite flowing current in a separate, parallel wire – a second order effect – Henry used successive, adjacent, freestanding coils of metal ribbon and spools of wire to trace the inductive effect through multiple stages. He did this for both galvanic and ordinary electricity, noticing that the direction of the current flow alternated in each consecutive stage…

"While studying arrays of coils for his third article, Henry also found that by varying the makeup of two adjacent coils, he could modify the character of the original current. In particular, using a primary and secondary coil, he could employ an initial "intensity" current (a weak current associated with what later became known as high potential difference) to induce a "quantity" current (a strong current associated with a low potential difference), and vice versa … Henry's techniques for self-consciously altering currents would contribute to the development of electrical transformers – devices to "step up" and "step down" currents, crucial capabilities in later nineteenth-century science and technology" (*ibid*., pp. 165-7).

This important paper first appeared in the *Transactions of the American Philosophical Society*, New Series, Vol. VI, 1839, pp. 303-337 (the offered paper with separate pagination). It subsequently appeared in Silliman's *American Journal of Science and Arts*, Vol. XXXVIII, January 1840, pp. 209-243; Sturgeon's *Annals of Electricity*, Vol. IV, 1840, pp. 281-310; *Philosophical Magazine*, 3rd Series, Vol. XVI, March 1840, pp. 200-210, 254-265 & 551-562; *Annales de Chimie et de Physique*, 3ième Série, Tom. III, December 1841, pp. 394-407; and Poggendorff's *Annalen der Physik und Chemie*. Supplement Bd. I, 1842, pp. 282-312.

KINETIC THEORY OF GASES

HERMANN, Jacob. *Phoronomia, sive de viribus et motibus corporum solidorum et fluidorum libri duo.* Amsterdam: Apud Rod. & Gerh. Wetstenios, 1716.

$2,800

4to (234 x 190 mm), pp. [xxii], 401, [2], including engraved allegorical frontispiece, with 12 folding engraved plates. Title printed in red and black with engraved vignette, woodcut diagrams in text. Fine contemporary Dutch vellum, red gilt title label to spine. Ex-library stamps of the Royal Society of Edinburgh.

First edition of the first textbook on theoretical mechanics based on the Leibnizian calculus, important for containing the first published discussion of the kinetic theory of gases. "This textbook concerned advanced mechanics in the modern sense and was considered an important work, very favorably reviewed by Leibniz himself in the *Acta Eruditorum*" (Fellmann in DSB). "The beginning of the kinetic theory of gases is usually assigned to the year 1738, when Daniel Bernoulli's 'Hydrodynamica' appeared at Strasbourg. The famous tenth section of this book pursued some of the consequences of the assumption that 'elastic fluids' consist of innumerable tiny particles in rapid motion. [However an] earlier attempt dates from 1716, and is to be found in the 'Phoronomia' of Bernoulli's compatriot Jacob Hermann … For us the importance of Hermann's chapter XXIV is that it is, as far as I know, the first attempt to deal mathematically with the relations between heat and motion. The intuition that the speed of the particles must be squared is especially noteworthy, but perhaps not more so than the assumption that their speeds will not all be the same, so that an average must be taken" (Knowles Middleton). "In 1716, the Swiss mathematician Jakob Hermann (1678-1733)



published his book on theoretical mechanics, *Phoronomia*, in which he proposed the first definite measure of the heat of molecular motion. In Proposition LXXXV he wrote: *Calor, caeteris paribus, est in composite ratione ex densitate corporis calidi, & duplicata ratione agitationis particularum ejusdem* (Heat, in bodies of similar composition, is in the composed ratio of the density of the hot body and the square of the agitation of its particles), in other words, for the first time it was being suggested that the pressure is proportional to the density and the mean square velocity of the molecules" (Wisniak).

Jakob Hermann (1678-1733) received his initial training from Jacob Bernoulli, and graduated with a degree in 1695. He became a member of the Berlin Academy in 1701. He was appointed to a chair in mathematics in Padua in 1707 (on the recommendation of Leibniz), but moved to Frankfurt an der Oder in 1713, and thence to St. Petersburg in 1724. Finally, he returned to Basel in 1731 to take up a chair in ethics and natural law. "While in Padua, Hermann lectured on the standard topics of the day, namely classical geometry, mechanics, optics, hydraulics, and gnomonics. However, his work with other scientists was almost entirely devoted to understanding, developing and applying methods in the infinitesimal calculus. It was while he was in Padua that Hermann did most of the work on his most famous book, the *Phoronomia*, which is a text on mechanics. Although he began writing the book in 1709, it was not published until 1716, three years after he left Italy …

"Hermann held his post in Padua until his contract expired on 28 April 1713. While in Padua he had asked Leibniz's advice about moving to a university in another country and Leibniz had suggested a position in Frankfurt-an-der-Oder, but advised Hermann to see out the six year contract he had signed with Padua. Although he had a very successful career in Italy, Hermann seems to have found the fact that he was a Protestant in a Roman Catholic country somewhat difficult



and, of course, Germany presented a religious environment much more to his liking. After leaving Padua in April, he went to Vicenza where he lived at the home of Giovanni Checozzi (1691-1756) and Sebastiano Checozzi (1693-1719) until June, after which he went first to Basel and subsequently to Frankfurt-an-der-Oder to take up his new appointment. He spent eleven years in Frankfurt-an-der-Oder, again a position which, as we have explained, was largely due to Leibniz's recommendation. But in 1724 Peter the Great called him to Russia to found, together with a number of others, the St. Petersburg Scientific Academy. He went to Russia in 1725, meeting Christian Goldbach in Königsberg while on the journey, and held a chair of higher mathematics in the St. Petersburg Academy until 1731 …

"In 1726, the St. Petersburg Academy began to publish a journal under the title *Commentarii Academiae Petropolitanae*. The journal was launched with an article by Hermann taking pride of place. Leonhard Euler arrived at the Academy in 1727. This was a difficult time since, following the death of Catherine, Peter II had become tsar at the age of twelve. There was political turmoil which had a serious affect on the work of the Academy which was close to collapse. Hermann worked at the Academy through these difficult years until, with the death of Peter II in 1730, the political situation stabilised. He had wanted to return to Basel, however, and had been appointed to the chair of ethics and natural law there in 1727. He had a substitute carry out the duties involved until his contract in St Petersburg came to an end in 1731 at which time he returned to Basel to take up the chair he had formally held for four years. He had hoped that a chair of mathematics would become vacant in Basel before his contract at the St. Petersburg Academy came to an end but this did not happen.

"Hermann worked in mechanics and studied the 'inverse problem' where one has to determine the orbit from a knowledge of the law of force. He proposed

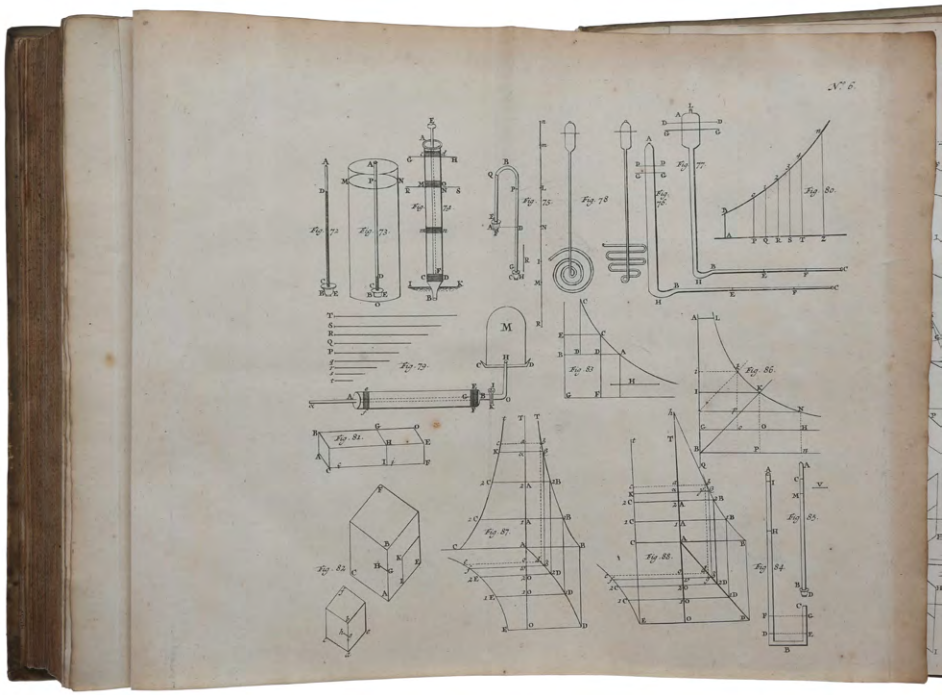

PHORONOMIA,
SIVE
DE VIRIBUS ET MOTIBUS
CORPORUM
SOLIDORUM ET FLUIDORUM
LIBRI DUO,
AUTORE
JACOBO HERMANNO Basil.
antehac in Illustri Patavino Lyceo; nunc vero
in Regio Viadrino Math. Prof. Ord.
& Reg. Scientiarum Societatis, quæ Berolini est, Sodali.
AMSTELÆDAMI,
Apud ROD. & GERH. WETSTENIOS H.FF.
M. D. CCXVI.
ROYAL SOCIETY OF EDINBURGH
1857
1716

the term 'phoronomia' in 1716 for the topic which was sometimes called 'rational mechanics' and is now called 'theoretical mechanics'. This appears in the title of his important book *Phoronomia, sive de viribus et motibus corporum solidorum et fluidorum libri duo* (1716) …

"An example of Hermann's approach is illustrated by looking at how he proved Kepler's area law. This had been proved by Newton in the *Principia* by using an intuitive limiting geometrical process. Hermann, however, gave a proof in the *Phoronomia* in terms of differentials. Although his notation was rather different from modern notation, and not particularly easy to understand, Hermann reworked the same ideas into a notation which is essentially that used today and sent his new version of the proof to John Keill who published it in *Journal litéraire* in 1717" (MacTutor).

"The complexity of the process of mathematization of dynamics is particularly evident in transitional figures, such as Jakob Hermann (1678–1733). He belonged to the Basel school headed by the Bernoullis and, sharing their methodology, made important contributions to the analytical treatment of dynamics. However, he also leaned towards Newton and, on many occasions, preferred to deal with dynamical problems in terms of geometry. His methodology is thus quite eclectic … Hermann's main work is *Phoronomia*, which, written during his Italian period, was published in Amsterdam in 1716. This work is devoted to the dynamics of solid and fluid bodies and covers many problems dealt with by Newton in the first two books of the *Principia*. In the preface, Hermann declares his intention of adhering to geometrical methods, since these seem to him more suitable for beginners [pp. vii–viii]. However, his knowledge of calculus is evident in the way in which he deals with infinitesimals. Hermann's *Phoronomia* is indeed representative of the process of transition that transformed dynamics in the first decades of the 18th century" (Guicciardini).

Not in Norman, Roberts & Trent or Tomash. N Guicciardini. 'An episode in the history of dynamics. Jakob Hermann's proof (1716-1717) of Proposition 1, Book 1, of Newton's Principia,' *Historia Mathematica* 23 (1996), pp. 167-181. W. E. Knowles Middleton, Jacob Hermann and the Kinetic Theory, *British Journal for the History of Science* 2 (1965), pp. 247-250. J. Wisniak, 'Kinetic theory. From Euler to Maxwell,' *Indian Journal of Chemical Technology* 12 (2005), pp. 730-742.

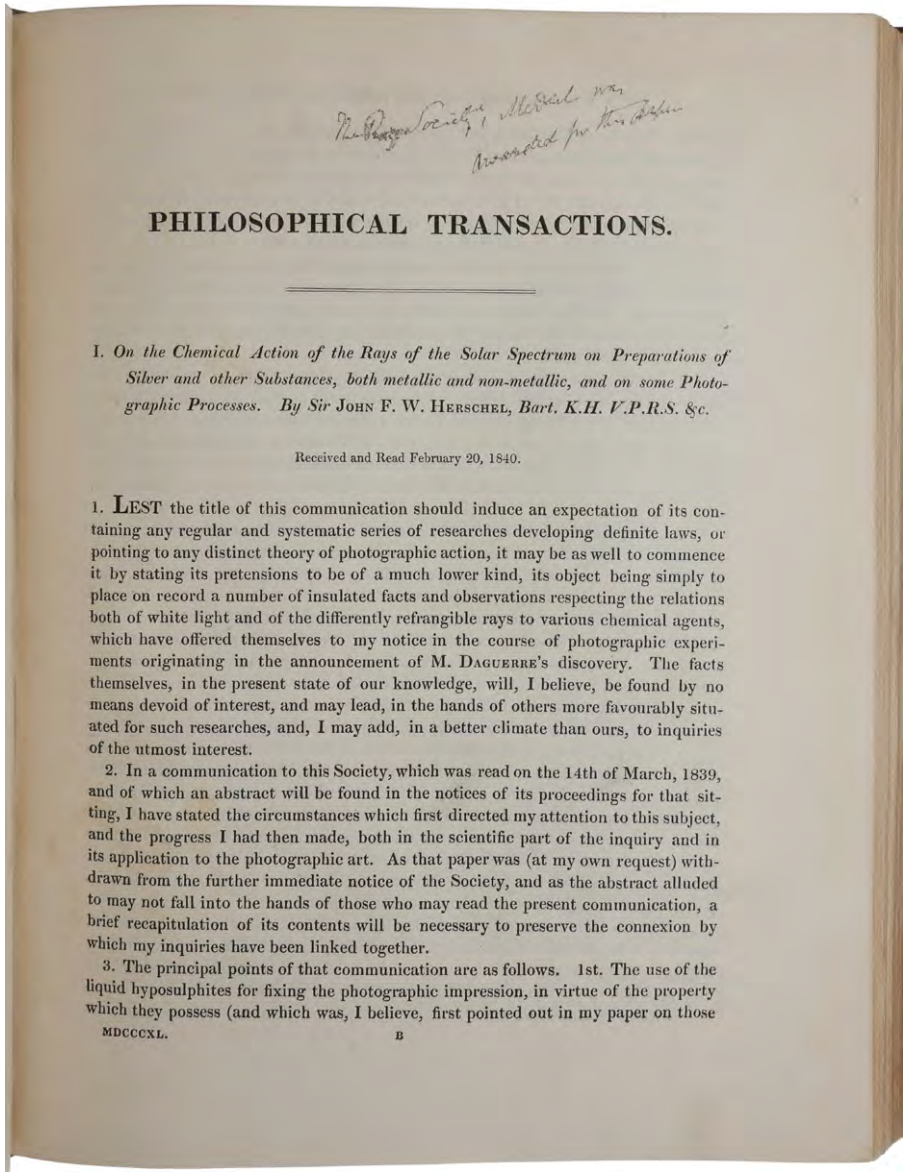


HERSCHEL ON PHOTOGRAPHY: HIS ANNOTATED OFFPRINTS

HERSCHEL, John Frederick William. *On the chemical action of the rays of the solar spectrum on preparations of silver and other substances, both metallic and non-metallic, and on some photographic processes.* [With:] *On the action of the rays of the solar spectrum on vegetable colours, and on some new photographic processes.* [With:] *On certain improvements on photographic processes described in a former communication, and on the parathermic rays of the solar spectrum.* Offprints from the *Philosophical Transactions* for 1840, 1842 & 1843, the first two with authorial annotations. Bound with 66 other offprints, extracts and separate publications by Herschel, many with authorial annotations, on astronomy, mathematics, physics, photography and other subjects, assembled by him and inscribed to his eldest son William James Herschel.

$85,000

Three volumes, thick 4to (278 x 216). Contemporary dark green half-morocco, spines decorated in gilt and with two red lettering-pieces, covers ruled in gilt (slightly rubbed). A hole (4cm x 1.8cm) has been cut into the inner margin of pp. 1-402 of no. 23 (see contents listing below), not affecting text or the title page. An inserted autograph note (probably in W. J. Herschel's hand) indicates that seven diamonds were at one time secreted in this hole, and that they were lost, and then found, in the autumn of 1898 (sadly, the diamonds are no longer present). The inserted note reads: "The "7" Diamonds taken out to go to New Lodge, 24 September Saturday 1898 – and replaced 8 October 1898. I put this note in their place when taking them out to go to New Lodge – & recollect nothing more of what I did with them till on Monday morning as I woke – I found I did not know. Concluded after careful



PHILOSOPHICAL TRANSACTIONS.

I. *On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and other Substances, both metallic and non-metallic, and on some Photographic Processes. By Sir* JOHN F. W. HERSCHEL, *Bart. K.H. V.P.R.S. &c.*

Received and Read February 20, 1840.

1. LEST the title of this communication should induce an expectation of its containing any regular and systematic series of researches developing definite laws, or pointing to any distinct theory of photographic action, it may be as well to commence it by stating its pretensions to be of a much lower kind, its object being simply to place on record a number of insulated facts and observations respecting the relations both of white light and of the differently refrangible rays to various chemical agents, which have offered themselves to my notice in the course of photographic experiments originating in the announcement of M. DAGUERRE's discovery. The facts themselves, in the present state of our knowledge, will, I believe, be found by no means devoid of interest, and may lead, in the hands of others more favourably situated for such researches, and, I may add, in a better climate than ours, to inquiries of the utmost interest.

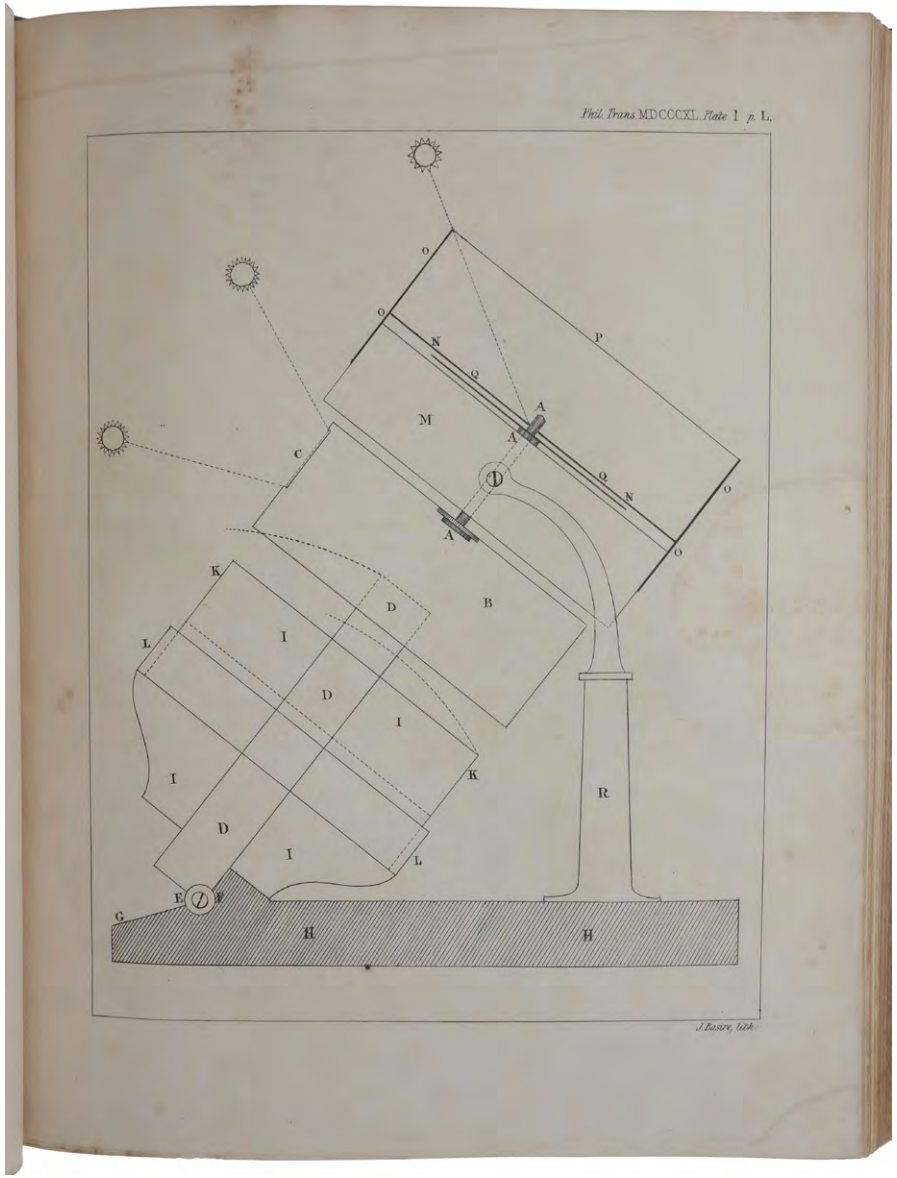
2. In a communication to this Society, which was read on the 14th of March, 1839, and of which an abstract will be found in the notices of its proceedings for that sitting, I have stated the circumstances which first directed my attention to this subject, and the progress I had then made, both in the scientific part of the inquiry and in its application to the photographic art. As that paper was (at my own request) withdrawn from the further immediate notice of the Society, and as the abstract alluded to may not fall into the hands of those who may read the present communication, a brief recapitulation of its contents will be necessary to preserve the connexion by which my inquiries have been linked together.

3. The principal points of that communication are as follows. 1st. The use of the liquid hyposulphites for fixing the photographic impression, in virtue of the property which they possess (and which was, I believe, first pointed out in my paper on those

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thought that I must have put them in my fob, and have taken them out unwittingly with a £5 note at the Railway ticket office – spent £44 on advert – & an agent – & on 8 Oct. they were restored to me 'found on platform'."

An extraordinary collection of works by Sir John Herschel (1792-1871), the outstanding astronomer and physical scientist of his day, assembled for presentation to his son William James Herschel (not to be confused with John's father, the astronomer Frederick William). The collection includes offprints of Herschel's three most important publications on photography, the first two of which have corrections and annotations in his hand. These offprints are of extreme rarity – ABPC/RBH list no other copy of any of them in the past 75 years. Herschel's intensive investigations in photography and photochemistry during the late 1830s and early 1840s led to enormous advances: he coined the terms 'positive' and 'negative,' invented new photographic processes and improved existing ones, and experimented with colour reproduction. Among the mathematical works are several on the 'calculus of operators', as well as Herschel's corrected galley proofs of a very important article on the theory of probability which was read by James Clerk Maxwell and led him to introduce probabilistic methods into the theory of gases, and thereby lay the foundations of statistical physics. There is also an offprint of a little studied paper in which Herschel describes a mechanical calculating machine, developed "In the course of a conversation with Mr. Babbage on the subject of applying machinery to the performance of numerical computations". The astronomy papers include an offprint of Herschel's great catalogue of 380 double stars (i.e., binary stars). All of the offprints are rare, with most either not listed on OCLC, or listed in only a handful of copies. "Herschel's university years at St. John's College, Cambridge, were devoted primarily to mathematics. Not only did he carry away the top academic prizes during this time, he was also elected a Fellow of the Royal Society, and co-founded the Analytical Society with Charles



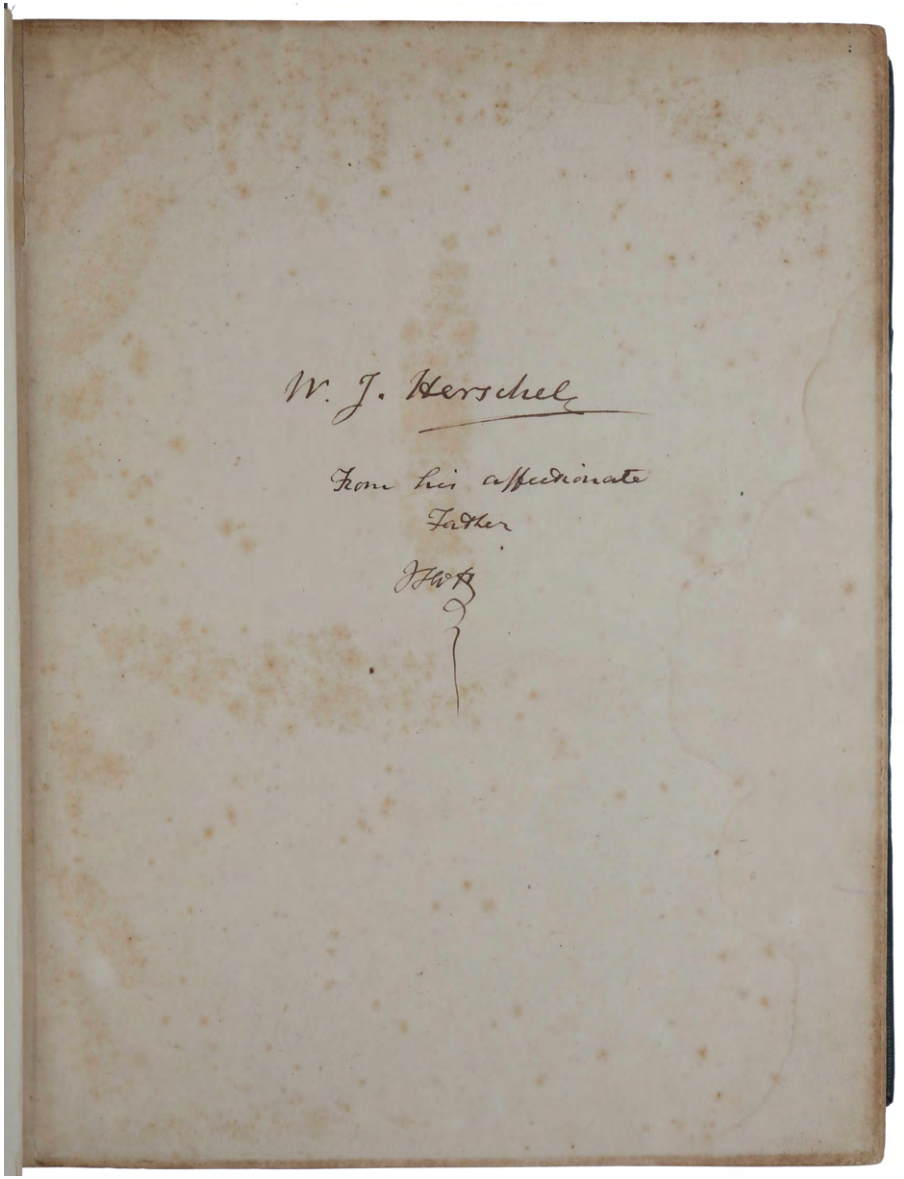
Babbage and George Peacock … Even at this early stage of his career, Herschel's zeal to "leave the world wiser than [he] found it", was already fully formed, and this clearly motivated his approach to photography when that too appeared on his horizon. His brief forays into legal studies and then into an academic career at Cambridge, ended abruptly at the close of 1816 when he settled finally on learning the trade of astronomer as his father's assistant. Herschel's life as a scientist of independent means, at a time when such a profession hardly existed, allowed him the freedom to pursue his personal interests, among them the study of light" (Hannavy, *Encyclopedia of Nineteenth-Century Photography*, p. 653).

Provenance: William James Herschel (inscription in John Herschel's hand on front free endpaper of Vol. III: 'W. J. Herschel // From his affectionate father // JFWH'); Dr. Sydney Ross, Rensselaer Polytechnic Institute (small red book label on each front paste-down). W. J. Herschel (1833-1917), the eldest son of John Herschel, is credited with being the first European to note the value of fingerprints for identification. Sydney Ross (1915-2013), leading chemist and bibliophile, was a former Professor of Colloid Science at Rensselaer Polytechnic Institute, Troy, New York, and founder, and until his death, president of the James Clerk Maxwell Foundation. In 2001 he published a 590-page annotated *Catalogue of the Herschel Library of William and John Herschel*.

In the following description of the works in these volumes, the numbers refer to the list of contents below.

Photography (43-47, 51, 59)

"Photography was announced at the very height of Herschel's career. He had just returned from four years in South Africa, having completed an examination of the skies of the Southern Hemisphere, and had reluctantly been raised to a



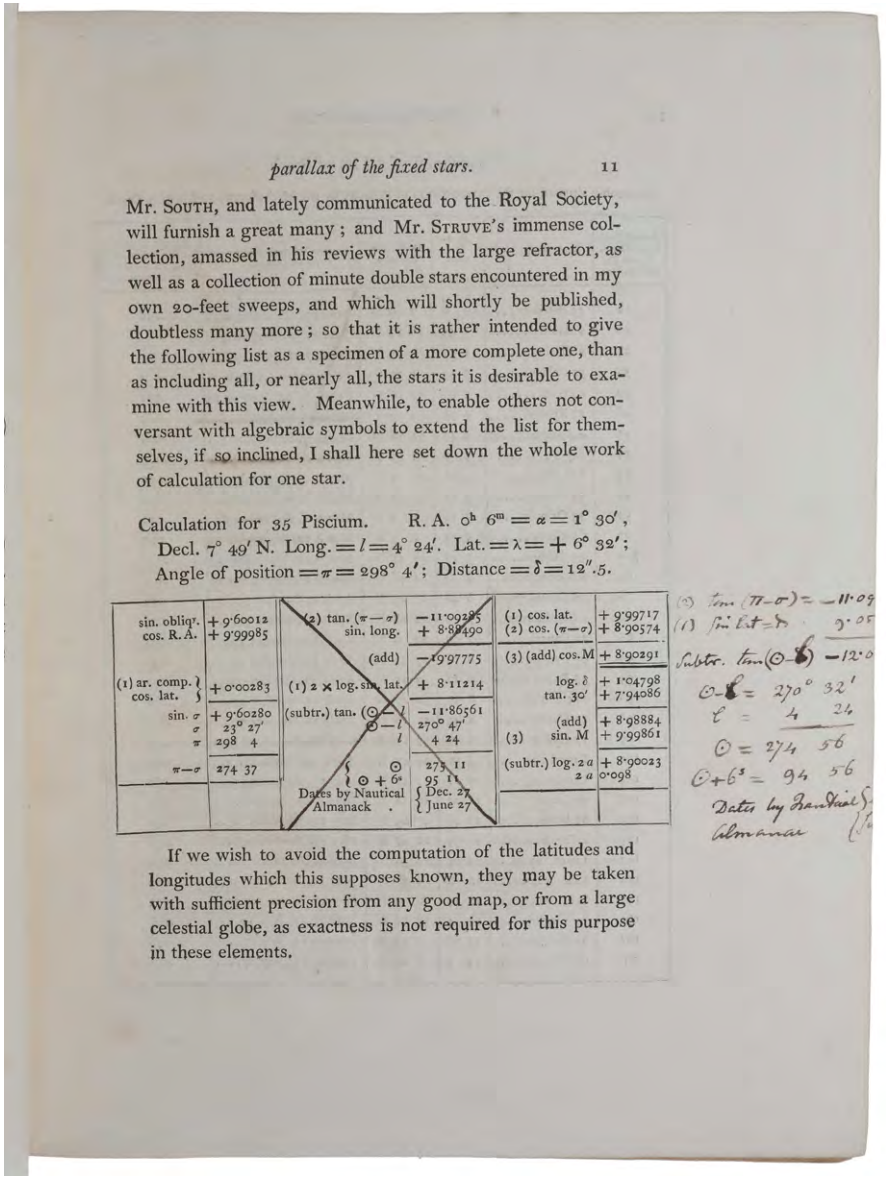
baronetcy. Herschel learned of the announcement of the Daguerreotype on 22 January [1839], and of Talbot's competing process within the space of a few days. By the 30th, needing no help from either inventor, he had made and fixed his own photographs on paper. Envisioning even the necessary steps to reverse the tones of the original, converting the negative image into a positive.

"Herschel did not coin the name 'photography' … What Herschel did was to endorse this name and encourage its adoption within the scientific community. Herschel employed 'photography' in a paper titled 'Note on the Art of Photography' presented before the Royal Society on 14 March 1839, but he withdrew the paper from publication" (Hannavy, p. 654).

"We have found proof that this action was taken out of consideration for Talbot, whose achievement Herschel did not wish to belittle by his own independent discovery … Herschel briefly referred to the matter in his next communication of 20 February 1840 (no. 43), in which after recapitulation of the contents of the previous paper, necessitated by its withdrawal, he says, 'of course it will be understood that I have no intention here of interfering with Mr. Talbot's just and long antecedent claims'. This second communication, entitled 'On the chemical action of the rays of the solar spectrum on preparations of silver and other substances, both metallic and non-metallic, and on some photographic processes abounds in important statements and observations which had a great bearing on the future of photography. Only the most significant can be enumerated here:

1. Herschel stressed the absolute necessity of perfect achromatism in photographic lenses, which he said was one of their three indispensable qualities, the others being flatness of field and sharpness of focus.

2. He introduced the terms 'negative' and 'positive' into photographic nomenclature.



parallax of the fixed stars. 11

Mr. South, and lately communicated to the Royal Society, will furnish a great many; and Mr. Struve's immense collection, amassed in his reviews with the large refractor, as well as a collection of minute double stars encountered in my own 20-feet sweeps, and which will shortly be published, doubtless many more; so that it is rather intended to give the following list as a specimen of a more complete one, than as including all, or nearly all, the stars it is desirable to examine with this view. Meanwhile, to enable others not conversant with algebraic symbols to extend the list for themselves, if so inclined, I shall here set down the whole work of calculation for one star.

Calculation for 35 Piscium. R. A. $0^h\ 6^m = \alpha = 1°\ 30'$, Decl. 7° 49′ N. Long. $= l = 4°\ 24'$. Lat. $= \lambda = +\ 6°\ 32'$; Angle of position $= \pi = 298°\ 4'$; Distance $= \delta = 12''.5$.

sin. obliq.	+ 9·60012	(2) tan. $(\pi-\sigma)$	−11·09285	(1) cos. lat.	+ 9·99717
cos. R. A.	+ 9·99985	sin. long.	+ 8·88490	(2) cos. $(\pi-\sigma)$	+ 8·90574
		(add)	−19·97775	(3) (add) cos. M	+ 8·90291
(1) ar. comp. cos. lat.	+ 0·00283	(1) 2 × log. sin. lat.	+ 8·11214	log. δ	+ 1·04798
				tan. 30′	+ 7·94086
sin. σ	+ 9·60280	(subtr.) tan. $(\odot - l)$	−11·86561		
σ	23° 27′	$\odot - l$	270° 47′	(add)	+ 8·98884
π	298 4	l	4 24	(3) sin. M	+ 9·99861
$\pi-\sigma$	274 37	$\odot$	275 11	(subtr.) log. 2 a	+ 8·90023
		$\odot + 6^s$	95 11	2 a	0·098
		Dates by Nautical Almanack	Dec. 27 / June 27		

If we wish to avoid the computation of the latitudes and longitudes which this supposes known, they may be taken with sufficient precision from any good map, or from a large celestial globe, as exactness is not required for this purpose in these elements.

3. He described a process for obtaining direct positive photographs on paper

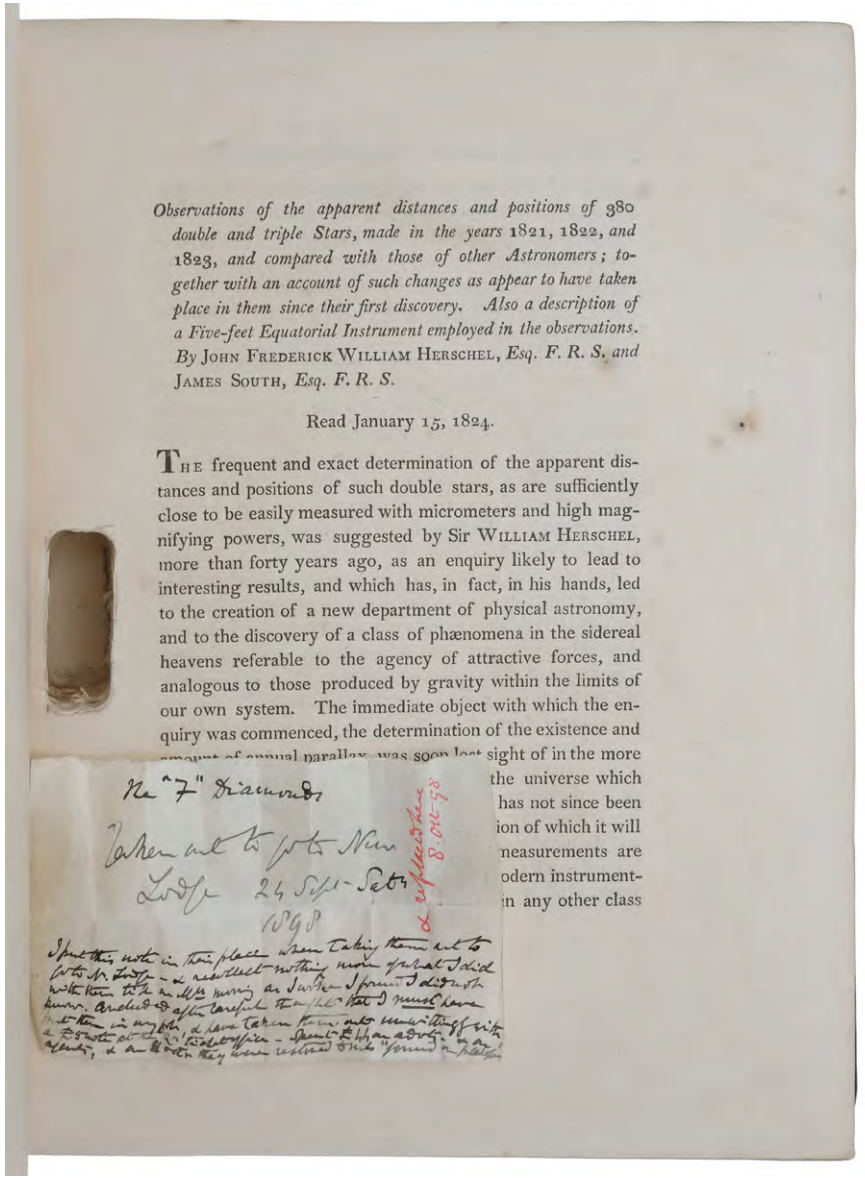
4. Having experimented with photographs on glass, he found that when laid on a black background, or smoked at the back, their character could be changed from negative to positive — a procedure introduced many years later in the ambrotype. He also made positive prints from his glass negatives.

5. Herschel found that bromide of silver was far more light-sensitive than any other silver salt.

6. He indicated the possibility of photography in natural colours at some future date, having obtained in July 1839 a good colour photograph of the spectrum, without, however, succeeding in fixing the colours" (Gernsheim, *The History of Photography* 1685-1914, p. 97).

"Although Herschel's time was increasingly monopolized by the completion of his astronomical catalogues, he continued to follow up his photochemical experiments for the next three years … Early in 1842, the electro-chemist Alfred Smee sent Herschel a quantity of the bright red compound now called potassium ferricyanide. While testing the sensitivity of this substance under the light of the spectrum, Herschel noted that it acted with much the same sensitivity as guaiacum, and when thrown into water, it became a deep Prussian blue. Smee suggested two further compounds, Ammonio Citrate and Ammonio Tartrate of Iron, and by June of 1842, Herschel had developed both the Chrysotype, named for its use of gold "to bring about the dormant picture", and the Cyanotype, his most practical and enduring process (no. 44).

"Herschel's 16 June 1842 paper presented his experiments not as independent inventions of processes. But as a series of observations on the basic principles



Observations of the apparent distances and positions of 380 double and triple Stars, made in the years 1821, 1822, and 1823, and compared with those of other Astronomers; together with an account of such changes as appear to have taken place in them since their first discovery. Also a description of a Five-feet Equatorial Instrument employed in the observations. By John Frederick William Herschel, *Esq. F. R. S. and* James South, *Esq. F. R. S.*

Read January 15, 1824.

The frequent and exact determination of the apparent distances and positions of such double stars, as are sufficiently close to be easily measured with micrometers and high magnifying powers, was suggested by Sir William Herschel, more than forty years ago, as an enquiry likely to lead to interesting results, and which has, in fact, in his hands, led to the creation of a new department of physical astronomy, and to the discovery of a class of phænomena in the sidereal heavens referable to the agency of attractive forces, and analogous to those produced by gravity within the limits of our own system. The immediate object with which the enquiry was commenced, the determination of the existence and

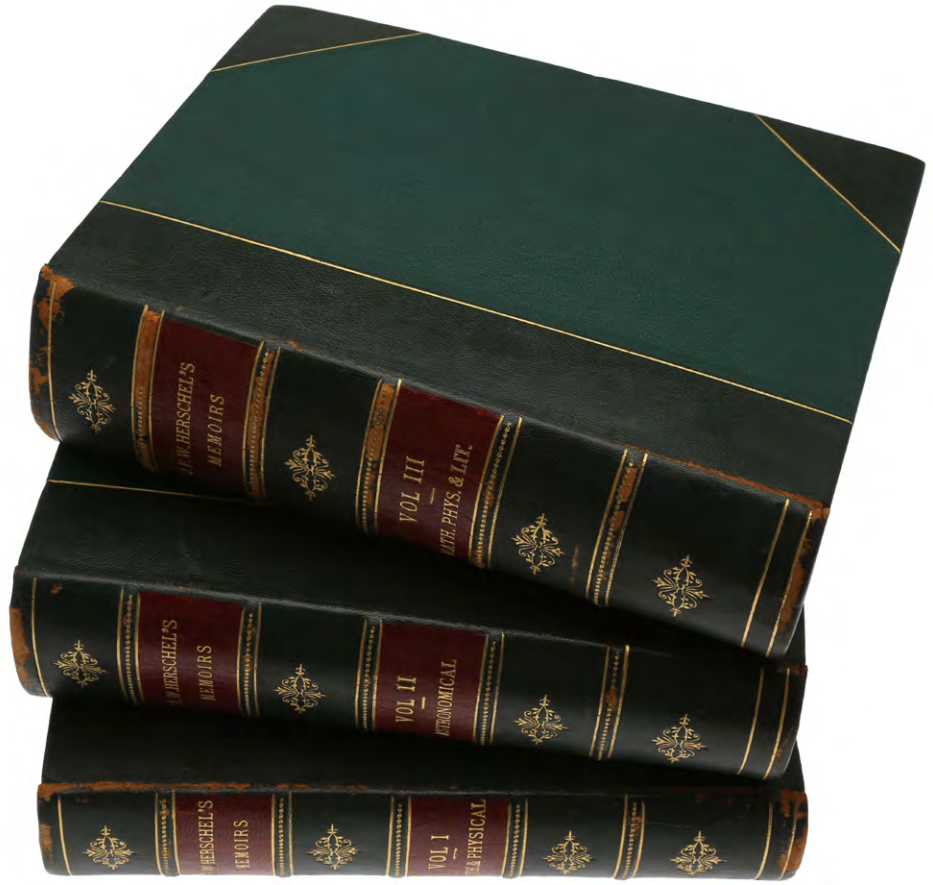
the universe which has not since been ion of which it will neasurements are odern instrument- in any other class

of photographic chemical action. Although he describes his many experiments, both organic and metallic, he refrains from naming them or presenting wholly functional working processes. It would only be in November of 1842 that he would systematically describe the working details of his processes (no. 45).

"Herschel's experiments on photographic subjects came to a halt in 1843, victims of his astronomical writing and public duties. But his interest in photography never ceased … In 1845 Herschel published his final contribution to photographic research, an observation of what he called 'epipolic dispersion' (nos. 46 & 47). George Gabriel Stokes would later rename this phenomenon 'fluorescence', the study of which led directly to radiation photography of all types" (Hannavy, p. 655).

Astronomy – double stars, nebulae and calculating machines (17, 19-25, 27-42, 48, 52-54, 57, 64, 66)

"Though William Herschel is now remembered above all for his general surveys of nebulae and his star-counts, another field which he pioneered, and in which John was to make his mark, was the study of double stars. The appearance of pairs of stars close together was a noticeable feature of William's sweeps, and led him to think that here, perhaps, was a means of determining stellar parallax and thus discovering the distances of the stars. Assuming that all stars were approximately of equal brightness … William argued that if one of the stars were dimmer than its companion, then it could be assumed to be much further away. The annual parallax of the brighter (and therefore nearer) star – that is, its apparent shift across the sky as the earth orbits the sun – could be detected by observing its varying separation from the more distant one (which would show negligible parallax) …



"Though John Herschel's first published astronomical paper was in 1822 and on a new method of calculating lunar eclipses, his first serious observational work was his measurement of double stars. This he did in cooperation with James South … it was a fruitful cooperation … for they were able to record details of no less than 380 double stars. These were catalogued systematically according to their right ascension (the celestial equivalent of terrestrial longitude), and in this represented an advance on the method adopted previously by William Herschel. Moreover, the colour and brightness of the component stars of each system was also given, as well as a comparison of the values for separation and position-angle which they had obtained with those of others. This excellent catalogue was published in the *Philosophical Transactions* in 1824 [no. 23]. This work also earned them the Lalande Prize of the Académie des Sciences in Paris for 1825, and in the next year, 1826, each received the Gold Medal of the Astronomical Society.

"… in 1826 [John] produced three novel results. The first was his design for an 'actinometer' for measuring solar energy … The second was a monograph on the nebulae in Orion and Andromeda, as well as other observations made with his father's '20-foot' telescope [no. 28]. John's aim in re-examining the Orion nebula was to see whether any changes could be detected.

"However, the most important of the three was a paper 'On the parallax of the fixed stars' [no. 20], which was published in the *Philosophical Transactions*. This contained a description of how the position-angle of a double star could possibly be used for determining annual parallax … such parallax was sought by his father using a micrometer to determine the change over time in relative separation between the stars. The angle to be determined was so small that it had remained undetected. What John Herschel now pointed out was that the orbital shift in space of the Earth would also give rise to a change in apparent position-angle … he gave annual parallaxes for some seventy stars; these ranged from 0.013 to 0.136 arc seconds which, if they did no more, at least indicated the extremely tiny angles that were involved [and hence the great distances of the stars] …

"John's preoccupation with double stars between 1825 and 1833 led him to develop a method for determining the orbits of those doubles which were in orbit around each other [no. 25]. It was an entirely graphical method based on Kepler's laws of elliptical orbits. At this time it was significant work, for it applied physical considerations of gravity and techniques of computation out in the depths of space. For it he was awarded the Royal Medal of the Royal Society in 1833. When South moved to France, John had returned to Slough and his other work there included an examination of nebulae and star-clusters, resulting in the issue in 1833 of a list of 2307 objects [nos. 32 & 34]. This increased his father's work by 525 new items, most of them very faint, and John's catalogue gave their positions correct to 15 arc minutes in both right ascension and declination" (Ronan, pp. 43-7 in *John Herschel 1792-1871: A Bicentennial Commemoration*, 1992). The computations involved in his determination of the elliptic orbits of binary stars led him to devise a mechanical calculating machine for solving the equations, involving trigonometric functions, which arose (no. 26). The paper resulted from a conversation with Babbage, who was at the time building his Difference Engine.

"Between 1834 and 1838 John Herschel and his family were in South Africa, John observing the southern skies as well as indulging in other of his scientific interests. In 1840, soon after his return, he caused some stir in astronomical circles by announcing that the very bright star Betelgeuse (α Orionis) underwent variations in brightness [no. 41]; it was in fact a long-period variable. Unfortunately he did not follow this significant observation by an onslaught on the subject of stellar variability which, in his paper, he referred to as "a highly interesting branch of Physical Astronomy" …

"Innovative as ever, in 1841 he initiated a proposal for reform of the constellations [no. 42]. His concern was their ill-defined boundaries, and what he proposed, after discussions with his friends Francis Baily and William Whewell, was that these should be defined by quadrangles themselves specified by parallels to the celestial equator lying between agreed declinations. However, continental astronomers would not agree, and as international agreement was essential, John withdrew his scheme" (Ronan, p. 47).

Mathematics – the calculus of operators, theory of probability (1-7, 49, 56, 69)

Items 1-7 are concerned with Herschel's work on the 'calculus of operators' and the 'calculus of functions', beginning with a contribution to the *Memoirs of the Analytical Society*. "A group of undergraduates, among whom were most notably George Peacock (1791-1858), Charles Babbage (1791-1871) and John Herschel (1792-1871), founded in 1812 the 'Analytical Society'. Its objective was to propagate the heresy of 'pure d-ism against the Dot-age of the University' [the former representing the Leibnizian approach to the calculus favoured on the Continent, the latter Newtonian fluxions still in use in Britain]. A project was set up to translate the second edition of Lacroix's short treatise on the calculus (1802): the aim of the Analytical Society's members was clearly that of changing the kind of education provided at Cambridge. However, the Analytical Society collapsed around 1814, having produced only a volume of *Memoirs* … Lacroix's treatise was intended for the students at Cambridge, as were the volumes of exercises on differential and integral calculus, functional equations and finite differences … After the collapse of the Analytical Society these works were published through the efforts of Babbage, Herschel and Peacock. After 1820 only Peacock remained at Cambridge … Herschel continued his father's work on astronomy; Babbage was engaged in work on his difference and analytical engines …

"Babbage, Herschel and Peacock were of great importance for the early nineteenth-century British calculus … [The] most important contribution from the Analytical Society's members was that they initiated a trend of research which characterized much of British mathematics up to Cayley and Boole. The *Memoirs of the Analytical Society* were centred on the calculus of operators and on functional equations.

"The calculus of operators dealt with the algebraical properties of the symbols of derivative and integral, and the related symbols of finite difference and summation. From this study it was possible to develop symbolic methods of integration of differential and difference equations … the application of the calculus of operators to the theory of integration originated mainly from Lagrange … Nevertheless, on the continent the Lagrangian school never played a prominent role. In Great Britain, on the contrary, the introduction of operational methods … launched a programme of research which continued up until the 1840s.

"In addition, use of the calculus of functions started in Great Britain with the Analytical Society's members, especially Babbage. The problem of recognizing the form of the arbitrary functions which occur in the integration of partial differential equations was the motivation for developing a theory of functional equations … The importance given to this theory is demonstrated by the inclusion in the *Encyclopaedia Metropolitana* of a lengthy essay on 'functional equations' by the young Augustus De Morgan (1836) …

"The researches of the British Lagrangian school on the calculus of operators and the calculus of functions were the origin of important contributions to algebra and logic, such as Peacock's 'pure algebra', and De Morgan's and Boole's algebras of logic. But the predominance of the algebraical approach to the calculus had its own drawback: it did not allow many British mathematicians influenced by the

Analytical Society to appreciate the importance of Cauchy's rigorization of the calculus, which was motivated by the desire to avoid the 'generalities of algebra'" (Guicciardini, *The Development of Newtonian Calculus in Britain 1700-1800*, pp. 135-8).

Koppelman ('The calculus of operations and the rise of abstract algebra,' *Archive for the History of Exact Sciences*, Vol. 8 (1971), p. 156) has argued that the reason that the most important contributions of British mathematicians in the first half of the nineteenth century were to abstract algebra "was a direct response of the English to a specific aspect of the work of Continental analysts which became accessible to them. The subject came to be called, by the English, the calculus of operations."

Item 69, although 'merely' a review of a work by Adolphe Quetelet, is actually an important contribution to the theory of errors which had a crucial influence on James Clerk Maxwell. These are, in fact, Herschel's corrected galley proofs, with copious manuscript annotations by Herschel. "The work of greatest influence on Maxwell's development of gas theory may well be a review in the July 1850 *Edinburgh Review* of the magnificently titled collection of essays by Adolphe Quetelet, *Letters Addressed to H.R.H. the Grand Duke of Saxe-Coburg and Gotha on the Theory of Probabilities as Applied to the Moral and Political Sciences*. The author of the review was Sir John Herschel. It ranged over many statistical questions, social and otherwise; a contemporary letter from Maxwell to his friend and future biographer, Lewis Campbell, strongly suggested that he had read it. The letter was undated and Campbell from memory put it as "June ? 1850." But there can be little doubt that it was written just after the publication of Herschel's review in July 1850. Maxwell discoursed on probability theory with remarks such as the following: "[T]he true Logic for this world is the Calculus of Probabilities…" Whether, indeed, Maxwell read the review in 1850, it was reprinted in Herschel's Essays in 1857, and we know that Maxwell read and admired these essays" (Garber, Brush & Everitt (eds.), *Maxwell on Molecules and Gases*, p. 9). "More than two decades ago Charles Gillispie pointed out the similitude of the approaches to probability to be found in this review and in Maxwell's paper ['Illustrations of the dynamical theory of gases,' 1860], and Stephen Brush subsequently recognized that the formal derivation of the error law given by Maxwell was in every important respect identical to the one introduced by Herschel in this essay" (Porter, *The Rise of Statistical Thinking 1820-1900*, p. 118).

Physics – electricity, magnetism and optics (8-16, 18, 58, 63)

"Like many active scientists in the early 19th century, Herschel was intent on discovering what light really was, and whether it moves in waves or in particles. Although no one in his generation, or indeed in the following generation, would formulate an answer to this question, Herschel believed that light travels in waves, that is, he believed in undulatory theory and not particle theory. He also believed, and would use photography to prove, that the visible part of the spectrum was a small portion of the actual spectrum. In 1819 Herschel began an exhaustive study of the nature of polarized light (nos. 9-12) …

"The late 1820s were a busy time for Herschel, who was rapidly attaining a level of fame that would surpass his father's. In 1827 he wrote his essay on Light (no. 13) for the *Encyclopaedia Metropolitana*. The essay … quickly attained the status of a classic and set out many of the principles on which he would conduct his photographic investigations" (Hannavy, p. 654).

No. 8 is an important practical paper on the construction of telescope lenses, Herschel's "His aim was a genuinely useful result, since previous attempts to derive conditions for an aplanatic achromatic doublet[i.e., one free of chromatic

and spherical aberration] had yielded formulae too complex to be handled by a practical optician and had used data irrelevant to his methods and materials. Herschel's analysis concluded with a set of tables, "set down for the convenience of those who may be inclined to make a trial of this construction," of radii of curvature and focal lengths of the lenses of a compound object glass – an achromatic doublet that would be free from spherical aberration, both for celestial objects and for terrestrial objects situated on the axis of the telescope. The values could easily be adjusted for any focal length" (Bennet, 'The first aplanatic object glass,' *Journal for the History of Astronomy* 13 (1982), p. 206).

No. 18 is the most interesting paper on electromagnetism in the collection, and also one of only two collaborative papers. "[François] Arago reported in 1824 an impressive new phenomenon which invited explanation. He arranged a disc made of copper – a non-magnetic – so that it could rotate in a horizontal plane, and above it he positioned a freely suspended magnet. When the disc was rotated, the magnet was first caused to move from its initial position, and it was subsequently dragged round by the disc. However, this force was not apparent when the disc was stationary. A year later Charles Babbage and John Herschel presented a paper to the Royal Society [no. 18] in which they varied the arrangement, for example, by rotating the copper disc between the poles of a powerful horseshoe magnet. They also determined the magnetic 'susceptibility' – their term to describe the observed effect – of different substances. Another important observation was that the magnetic effect produced by the disc was largely destroyed when it was punctured by radial slits. The observed phenomena were explained on the assumption that the interaction between, say, the rotating disc and the sympathetically moving magnet, was due to induction and, moreover, the particles comprising the magnet were affected by an inductive process. The other important point to notice about this paper is the authors' insistence that the inductive process does not occur instantaneously but that 'time enters as an essential element'" (Cantor, *Michael Faraday*, pp. 234-5). But the authors fell short of realizing that a totally new phenomenon need be postulated: the induction of eddy currents, and the discovery of electromagnetic induction had to wait another six years.

Varia (50, 55, 60, 61, 62, 65, 67, 68)

"In 1847, [Herschel] was asked by the First Lord of the Admiralty, Lord Auckland, to act as editor for a proposed "Manual of Scientific Inquiry" and to contribute an article on meteorology to it. The manual was intended to be a textbook for cadets at the Royal Naval College, Greenwich, and was to provide basic information on the scientific subjects that concerned naval officers. Articles on various aspects of astronomy, physics, and mathematics were contributed by Airy, Whewell, the geologist Adam Sedgewick, Sabine and Beaufort. *A Manual of Scientific Enquiry* was published in 1849. Herschel's article on "Meteorology" (no. 60) was reprinted in the eighth edition of the *Encyclopaedia Britannica* and later published as a separate volume. A shortened and popularized version of it, entitled "On Weather and Weather Prophets," was included in Herschel's *Familiar Lectures on Scientific Subjects*" (Buttmann, *The Shadow of the Telescope* (1974), pp. 166-7).

"His few hours of leisure Herschel devoted primarily to poetry – his own and translations. In 1842 he wrote a verse translation of Friedrich von Schiller's "The Walk," a poem he particularly liked because its evocations of nature reminded him of his own walks in the delightful countryside around Feldhausen [a farmhouse Herschel rented while he was observing in South Africa], which he often recalled with a certain degree of nostalgia" (*ibid*., pp. 171-2).

CONTENTS

Vol. I

1. A memoir on equations of differences and their applications to the determination of functions from given conditions. Separately-paginated offprint from *Memoirs of the Analytical Society*. Cambridge: J. Smith, 1813. Pp. [ii], [1], 2-31, [1] (only, of 51 pages, lacking the third part 'On functional equations').

2. On the development of exponential functions; together with several new theorems relating to finite differences. Separately-paginated offprint from *Philosophical Transactions*, Vol. 106. London: W. Bulmer, 1816. Pp. [ii], [1], 2-21.

3. Consideration of various points of analysis. Separately-paginated offprint from *Philosophical Transactions*, Vol. 104. London: W. Bulmer, 1814. Pp. [ii], [1], 2-29. Autograph page correction of number 33 to 29.

4. On circulating functions, and on the integration of a class of equations of finite differences into which they enter as coefficients. Separately-paginated offprint from *Philosophical Transactions*, Vol. 108. London: W. Bulmer, 1818. Pp. [ii], [1], 2-25.

5. Note on an application of the inverse theory of functions to the integral calculus. Offprint from William Spence's *Mathematical Essays* (edited by Herschel). London: J. Moyes, 1819. Pp. [ii], [1], 152-170.

6. On the reduction of certain classes of functional equations to equations of finite differences. Offprint from *Transactions of the Cambridge Philosophical Society*. Cambridge: J. Smith, 1820. Pp. [ii], [1], 2-11.

7. [Drop-head title:] Isoperimetrical problems. Offprint (because first page blank) from the *Edinburgh Encyclopaedia*, Vol. XII. N.p., n.d. [Edinburgh: Blackwood, 1830]. Pp. 320-328.

8. On the aberrations of compound lenses and object-glasses. Offprint from *Philosophical Transactions*. London: W. Bulmer & W. Nicol, 1821. Pp. [ii], [1], 4-48, with one engraved plate.

9. On the action of crystallized bodies on homogeneous light, and on the causes of the deviation from Newton's scale in the tints which many of them develope on exposure to a polarised ray. Offprint from *Philosophical Transactions*. London: W. Bulmer & W. Nicol, 1820. Pp. [ii], [1], 2-56, with one engraved plate.

10. On a remarkable peculiarity in the law of the extraordinary refraction of differently-coloured rays exhibited by certain varieties of Apophyllite. Offprint from *Transactions of the Cambridge Philosophical Society*. Cambridge: J. Smith, 1821. Pp. [ii], [1], 2-7, [1].

11. On certain remarkable instances of deviation from Newton's scale in the tints developed by crystals with one axis of double refraction on exposure to polarized light. Offprint from *Transactions of the Cambridge Philosophical Society*, Vol. I. N.p., n.d. [Cambridge: J. Smith, 1822]. Pp. [ii], [1], 2-21. Autograph corrections to pp. 5 & 12.

12. On the rotation impressed by plates of rock crystal on the planes of polarization of the rays of light, as connected with certain peculiarities in its crystallization. Offprint from *Transactions of the Cambridge Philosophical Society*. Cambridge: J. Smith, 1820. Pp. [ii], [1], 2-10, with one engraved plate. Autograph correction to p. 9.

13. [Drop-head title:] Light. Extract (?) from the *Encyclopaedia Metropolitana*, Vol. IV. N.p., n.d. [1827]. Pp. [1], 342-586, with 14 engraved plates. Autograph corrections to pp. 341, 410, 414, 420, 428, 434, 439, 452, 456-7, 473, 476, 518, 531, 541, 544, 551, 556-7, 577.

14. [Drop-head title:] Sound. Extract (?) from the *Encyclopaedia Metropolitana*, Vol. IV. N.p., n.d. [1827]. Pp. [1], 748-824, [2], with 6 engraved plates.

15. On the separation of iron from other metals. Offprint from *Philosophical Transactions*. London: W. Bulmer & W. Nicol, 1821. Pp. [ii], [1], 4-9.

16. The Bakerian Lecture. On certain motions produced in fluid conductors when transmitting the electric current. Offprint from *Philosophical Transactions*. London: W. Nicol, 1824. Pp. [ii], [1], 4-57.

17. On the astronomical causes which may influence geological phaenomena. Offprint from *Transactions of the Geological Society of London*, 1832. London: Richard Taylor, 1832. Pp. [ii], [1], 294-299.

18. [With Charles BABBAGE] Account of the repetition of M. Arago's experiments on the magnetism manifested by various substances during the act of rotation. Separately-paginated offprint from *Philosophical Transactions*. London: W. Nicol, 1825. Pp. [ii], [1], 2-30.

Vol. II

19. Correction of an error in a paper published in the *Philosophical Transactions*, entitled, "On the parallax of the fixed stars." London: W. Nicol, 1827. Title page only, followed by pp, 25-50 of Herschel's article 'Account of a series of observations for determining the differences of meridians, etc.' Ink stamp of W. J. Herschel on title.

20. On the parallax of the fixed stars. Separately-paginated offprint from *Philosophical Transactions*. London: W. Nicol, 1826. Pp. [ii], [1], 2-15. Autograph corrections to pp. 9, 11-13, 15.

21. On a new method of computing occultations of the fixed stars. Offprint from *Memoirs of the Astronomical Society of London*. London: Richard Taylor, 1824. Pp. [ii], [1], 326-328.

22. Subsidiary tables for facilitating the computation of annual tables of the apparent places of forty-six principal fixed stars: &c. &c. Offprint from *Memoirs of the Astronomical Society of London*. London: Richard Taylor, 1824. Pp. [ii], [1], 422-496.

23. [With James SOUTH] Observations of the apparent distances and positions of 380 double and triple stars, made in the years 1821, 1822, and 1823, and compared with those of other astronomers: together with an account of such changes as appear to have taken place in them since their first discovery. Also a description of a five-feet equatorial instrument employed in the observations. Offprint from *Philosophical Transactions*. London: W. Nicol, 1824. Pp. [ii], [1], 2-412, [11], with 4 engraved plates (one folding).

24. [Drop-head title:] Micrometrical measures of 364 double stars with a 7-feet equatorial achromatic telescope, taken at Slough, in the years 1828, 1829, and 1830. Extract (?) from *Memoirs of the Royal Astronomical Society*, 1831. Pp. 13-90, [1].

25. On the investigation of the orbits of revolving double stars: being a supplement to a paper entitled "Micrometrical measures of 364 double stars," etc. etc. Separately-paginated offprint from *Memoirs of the Royal Astronomical Society*, Vol. V, Part I. London: J. Moyes, 1832. Pp. [ii], [1], 4-54, with two folding engraved plates.

26. Description of a machine for resolving by inspection certain important forms of transcendental equations. Separately-paginated offprint from *Transactions of the Cambridge Philosophical Society*, Vol. IV. Cambridge: J. Smith, 1832. Pp. [ii], [1], 2-16 with one engraved plate.

27. Notices of the elliptic orbits of ξ Boötis and η Coronae; with a second approximation to the orbit of γ Virginis. Offprint from *Memoirs of the Royal Astronomical Society*, Vol. VI. London: J. Moyes, 1833. Pp. [ii], 3-11.

28. Account of observations made with a twenty-feet reflecting telescope: Comprehending, 1. Descriptions and approximate places of 321 new double and triple stars. 2. Observations of the second comet of 1825. 3. An account of the actual state of the great nebula in Orion, compared with those of former astronomers. 4. Observations of the nebula in the girdle of Andromeda. Offprint from *Memoirs of the Royal Astronomical Society*. London: Richard Taylor, 1826. Pp. [ii], [1], 460-497, with three engraved plates.

29. Account of observations made with a twenty-feet reflecting telescope, containing, a second catalogue of 295 new double and triple stars (reduced to the beginning of 1830); together with some observations of double stars previously known. Offprint from *Memoirs of the Royal Astronomical Society*, Vol. III. London: Richard Taylor, 1827. Pp. [ii], [1], 48-63.

30. Third series of observations with a twenty-feet reflector, containing, a catalogue of 384 new double stars (reduced to the beginning of 1830); together with some observations of double stars previously known. Offprint from *Memoirs of the Royal Astronomical Society*, Vol. III. London: Richard Taylor, 1828. Pp. [ii], [1], 178-213.

31. Fourth series of observations with a twenty-feet reflector; containing the places, description, and measures of 1236 double stars, (reduced to the beginning of 1830.) The greater part of them not previously described. Offprint from *Memoirs of the Royal Astronomical Society*, Vol. IV. London: J. Moyes, 1830. Pp. [ii], [1], 332-378.

32. [Drop-head title:] Fifth catalogue of double stars observed at Slough in the years 1830 and 1831 with the 20-feet reflector; containing the places, descriptions, and measured angles of position of 2307 of those objects, of which 1304 have not been found described in any previous collection; the whole reduced to the epoch 1830.0. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. VI. N.p., n.d. [London: J. Moyes, 1833]. Pp. [1], 2-81.

Vol. III

33. [Drop-head title:] Observations of Biele's comet. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. VI. N.p., n.d. [London: J. Moyes, 1833]. Pp. 99-109. Ink stamp of W. J. Herschel on first page of text.

34. Observations of nebulae and clusters of stars. Made at Slough, with a twenty-feet reflector, between the years 1825 and 1833. Offprint from *Philosophical Transactions*. London: Richard Taylor, 1833. Pp. [ii], [359]-505, with 8 engraved plates. Autograph corrections to pp. 371, 433, 481.

35. Notices of the elliptic orbits of ξ Boötis and η Coronae; with a second approximation to the orbit of γ Virginis. Offprint from *Memoirs of the Royal Astronomical Society*, Vol. VI. London: J. Moyes, 1833. Pp. [ii], 3-11.

36. [Drop-head title:] A second series of micrometrical measures of double stars, chiefly performed with the 7-feet equatorial, at Slough, in the years 1831, 2, and 3. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. VIII. N.p., n.d. [London: J. Moyes, 1835]. Pp. 37-59. Autograph corrections to p. 47.

37. [Drop-head title:] On the satellites of Uranus. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. VIII. N.p., n.d. [London: J. Moyes, 1835]. Pp. 24.

38. [Drop-head title:] A list of test objects, principally double stars, arranged in classes, for the trial of telescopes in various respects, as to light, distinctness, &c. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. X. N.p., n.d. [London: J. Moyes, 1838]. Pp. 25-32.

39. A sixth catalogue of double stars, observed at Slough, in the years 1831 and 1832, with the 20-feet reflector. Offprint from *Memoirs of the Royal Astronomical Society*, Vol. IX. London: J. Moyes, 1836. Pp. [ii]. [1], 4-14. Autograph correction to p. 7.

40. [Drop-head title:] Observations of the Comet of Halley, after the perihelion passage in 1836; made at Feldhausen, Cape of Good Hope. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. X. N.p., n.d. [London: J. Moyes, 1838]. Pp. 325-335. Autograph corrections to p. 326.

41. [Drop-head title:] On the variability and periodical nature of the star α Orionis. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. XI. N.p., n.d. [London: J. Moyes, 1840]. Pp. 269-278.

42. On the advantages to be attained by a revision and re-arrangement of the constellations, with especial reference to those of the southern hemisphere, and on the principles upon which such re-arrangement ought to be conducted. Offprint from *Memoirs of the Royal Astronomical Society*, Vol. XII. London: Moyes & Barclay, 1841. Pp. [ii], [1], 4-26, with one engraved plate. Autograph corrections to pp. 7, 10, 17.

43. On the chemical action of the rays of the solar spectrum on preparations of silver and other substances, both metallic and non-metallic, and on some photographic processes. Offprint from *Philosophical Transactions*, Part I, for 1840. London: R. & J. E. Taylor, 1840. Pp. [ii], [1], 2-59 with two engraved plates. Autograph corrections to pp. 2, 18, 22, 35-37, 51, 55 and final blank page.

44. On the action of the rays of the solar spectrum on vegetable colours, and on some new photographic processes. Offprint from *Philosophical Transactions*, Part II, for 1842. London: R. & J. E. Taylor, 1842. Pp. [ii], [181]-214, with one folding engraved plate. Autograph corrections to pp. 183, 196.

45. On certain improvements on photographic processes described in a former communication, and on the parathermic rays of the solar spectrum. Offprint from *Philosophical Transactions*, Part I, for 1843. London: R. & J. E. Taylor, 1843. Pp. [ii], [1], 2-6.

46. On a case of superficial colour presented by a homogeneous liquid internally colourless. Offprint from *Philosophical Transactions*, Part I, for 1845. London: R. & J. E. Taylor, 1845. Pp. [ii], [291]-293.

47. On the epipŏlic dispersion of light, being a supplement to a paper entitled, "On a case of superficial colour presented by a homogeneous liquid internally colourless." Offprint from *Philosophical Transactions*, Part I, for 1845. London: R. & J. E. Taylor, 1845. Pp. [ii], [295]-301.

48. [Drop-head title:] On the determination of the most probable orbit of a binary star. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. XVIII. N.p., n.d. [London: G. Barclay, 1850]. pp. 47-68.

49. On the algebraic expression of the number of partitions of which a given number is susceptible. Offprint from *Philosophical Transactions*, Part II, for 1850. London: R. & J. E. Taylor, 1850. Pp. [ii], [399]-422.

50. [Drop-head title:] Letter from Sir J. Herschel, explanatory of the phenomena exhibited by the freezing cavern. [With:] On some phenomena observed on glaciers, and on the internal temperature of large masses of ice or snow. [Abstracts of papers published in *Proceedings of the Geological Society*, Vol. III, 1842, pp. 697-699 & 699-702.] Extract from *The Athenaeum*, No. 753, April 2, 1842. N.p., n.d. [London: James Holmes, 1842]. Pp. 296-7, laid down on blank sheet. Part number and date added in autograph.

51. [Drop-head title:] Contributions to Actino-chemistry. On the Amphitype, a new photographic process. Extract from *British Association Report*, Part 2, 1844. N.p., n.d. [London: John Murray, 1845]. Two pages (pp. 12-13), laid down on blank sheet.

52. [Drop-head title:] The Comet. Extract from The Times, March 12, 1843. One page, laid down on blank sheet. Part number and date added in autograph and with two autograph corrections.

53. [Drop-head title:] Miss Caroline Lucretia Herschel [Obituary]. Extract from *The Athenaeum*, No. 1056, January 22, 1848. N.p., n.d. [London: James Holmes, 1848]. One page (p. 84), laid down on blank sheet. Part number and date added in autograph.

54. [Drop-head title:] Lunar rainbow. Extract from *The Athenaeum*, No. 1099, November 18, 1848. N.p., n.d. [London: James Holmes, 1848]. Two pages (pp. 1149-50), laid down on blank sheet. Date added in autograph.

55. [Drop-head title:] The great explosion at Dover. Extract from *The Athenaeum*, No. 797, February 4, 1843. N.p., n.d. [London: James Holmes, 1843]. One page (p. 111), laid down on blank sheet. Date added in autograph.

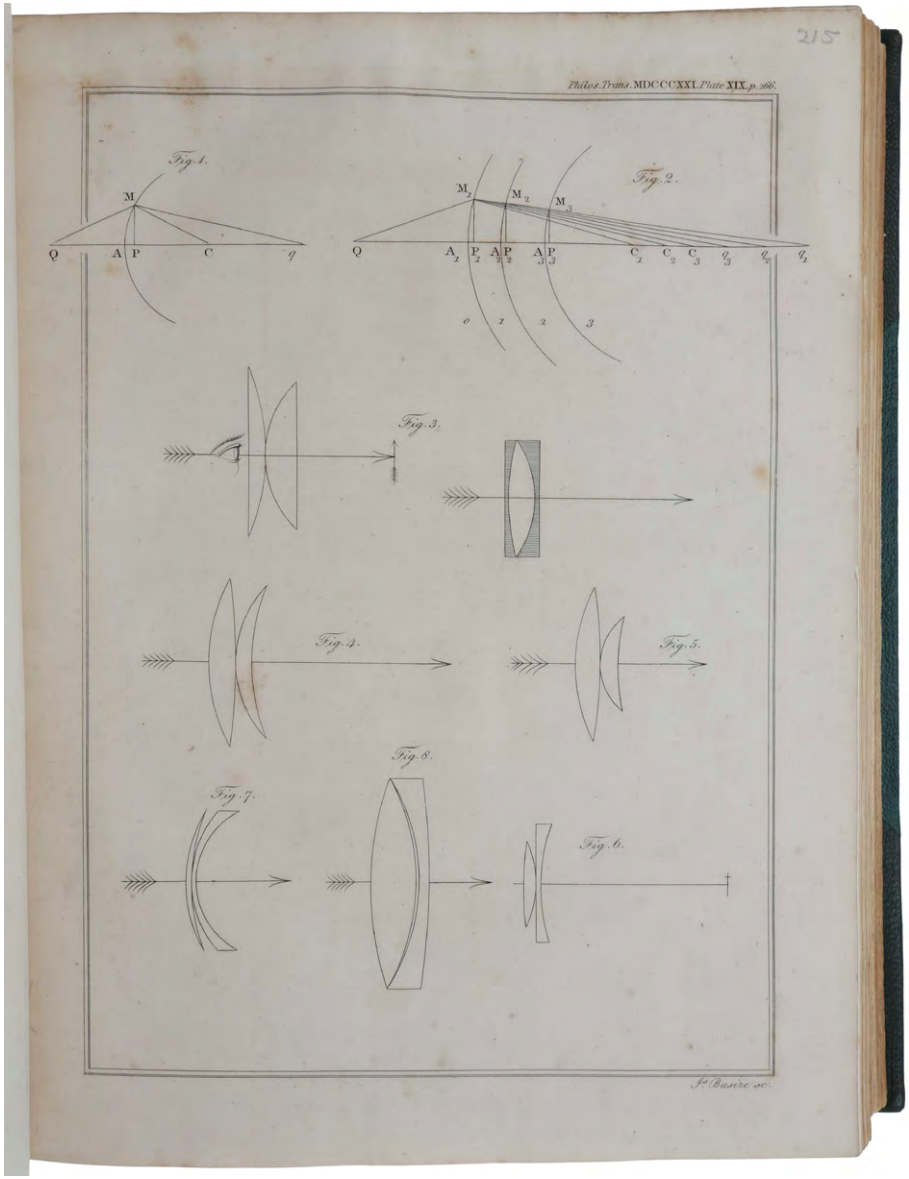
56. [Drop-head title:] A problem in perspective. Extract from *The Athenaeum*, No. 1100, November 25, 1848. N.p., n.d. [London: James Holmes, 1848]. One page (p. 1179), laid down on blank sheet. Part number and date added in autograph.

57. [Drop-head title:] The planet Neptune. Extract from *The Athenaeum*, No. 1100, November 25, 1848. N.p., n.d. [London: James Holmes, 1848]. One page (p. 1176), laid down on blank sheet. Part number and date added in autograph.

58. On the absorption of light by coloured media, viewed in connexion with the undulatory theory. Offprint from *Philosophical Magazine*, Third Series, Vol. 3, No. 18, December, 1833. 8vo, pp. [1], 402-412. Individual leaves framed in larger blank sheets.

59. On the action of the rays of the solar spectrum on the Daguerreotype plate. Offprint from P*hilosophical Magazine*, February, 1843. 8vo, pp. [1], 2-13, with one engraved plate. Individual leaves framed in larger blank sheets.

60. Meteorology. Offprint from *The Admiralty Manual of Scientific Enquiry*, 1849. N.p., n.d. [London: His Majesty's Stationer, 1849]. 8vo, pp. [3], 4-57, [1]. Individual leaves framed in larger blank sheets.



61. An address to the subscribers to the Windsor and Eton public library and reading room, delivered at the first general meeting of the subscribers, held at the Christopher Inn, Eton, on Tuesday, 29th Jan. 1833. London: Smith, Elder & Co., 1833. 8vo, pp. [3], 6-36 (possibly lacking half-title). Individual leaves framed in larger blank sheets.

62. The Walk, translated in the original metre from the German of F. Schiller [by J. F. W. Herschel]. For private circulation. Text in German and English on facing pages. N.p., n.d. [London, 1842]. Pp. [3], 4-23. Individual leaves framed in larger blank sheets. Autograph corrections to pp. 2, 6, 7, 8, 10, 11, 12, 14, 16, 18, 20, 21, 22.

63. Terrestrial magnetism. Offprint from the *Quarterly Review*, No. CXXI, June, 1840. 8vo, pp. [3], 4-44. 'From my own collection (B)' in autograph on title. Individual leaves framed in larger blank sheets.

64. Address of the President, Sir J. F. W. Herschel, Bart. On the presentation of the Gold Medal of the Royal Astronomical Society to Professor Bessel, at the Anniversary Meeting, February 12, 1841. For his Observations and researches on the parallax of 61 Cygni. 8vo, pp. [1], 2-10. Individual leaves framed in larger blank sheets.

65. Whewell on the Inductive Sciences. Offprint from the *Quarterly Review*, No. CXXXV, 1841. 8vo, pp. [2], [1]-62. Extensive autograph notes on pp. 34, 36, 45. Individual leaves framed in larger blank sheets.

66. Memoir of Francis Baily. Offprint from *Monthly Notices of the Royal Astronomical Society*, Vol. VI, November 1844. London: Moyes & Barclay, 1844. 8vo, pp. [3], 2-48. Individual leaves framed in larger blank sheets.

67. [Drop-head title:] [Report of the Annual General Meeting of the Royal Astronomical Society]. Extract (?) from *Memoirs of the Royal Astronomical Society*, Vol. IX. No. 4, February 9, 1849. 8vo, pp. [13]-14. One leaf framed in larger blank sheet.

68. [Drop-head title:] [Review of Kosmos, by Alexander von Humboldt]. Offprint from *The Edinburgh Review*, January 1848. 8vo, pp. [1], 2-60. 'Kosmos' in autograph at top of first page, with autograph correction to p. 23.

69. [Drop-head title:] [Review of Lettres a S. A. R. le Duc regnant de Saxe-Coburg et Gotha sur la Théorie des Probabilités appliquée aux Sciences Morales et Politiques by Adolphe Quetelet, and of its English translation by Olinthus Gregory.] Corrected page proofs for publication in *The Edinburgh Review*, No. CLXXXV, July 1850. 8vo, pp. [1], 2-57. With extensive autograph corrections and additions on almost every page, these all being incorporated into the final published version. Individual leaves framed in larger blank sheets.

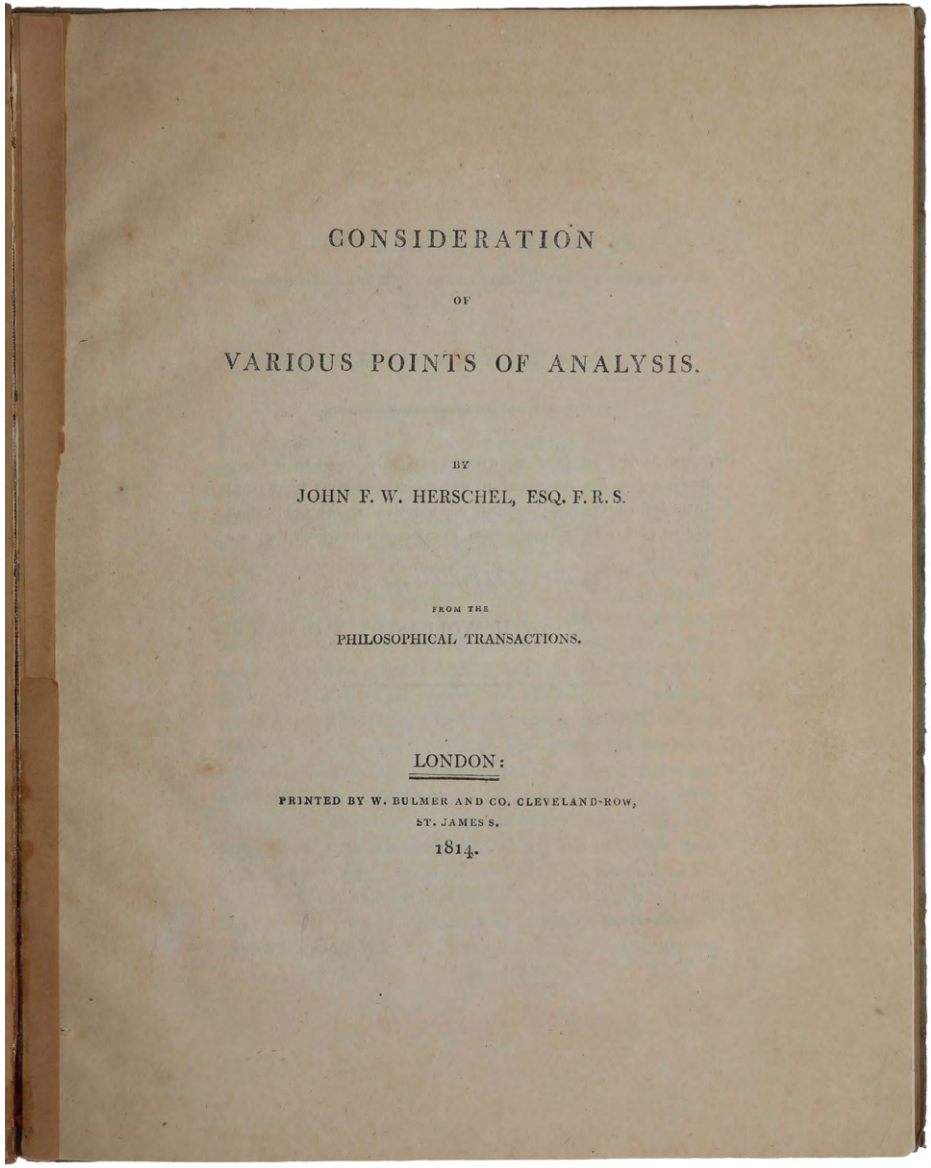
CALCULUS OF OPERATIONS

HERSCHEL, John Frederick William. *Consideration of Various Points of Analysis.* Offprint from *Philosophical Transactions.* London: printed by W. Bulmer [for the Royal Society], 1814.

$4,500

4to (272 x 213 mm), pp. [ii], [1] 2-29 (the last mis-paginated '33' and corrected in ink manuscript). Later nineteenth-century cloth-backed boards. Inner hinges starting, internally fine and clean.

Very rare separately-paginated offprint of Herschel's account of the 'calculus of operations,' which formed a major part of the programme of reform promoted by the group of Cambridge mathematicians (which included Babbage and Herschel) who called themselves the 'Analytical Society.' Koppelman (p. 156) has argued that the reason that the most important contributions of British mathematicians in the first half of the nineteenth century were to abstract algebra "was a direct response of the English to a specific aspect of the work of Continental analysts which became accessible to them. The subject came to be called, by the English, the calculus of operations." The most important such contributions were made by Duncan Gregory and George Boole. "This technique – the calculus of operations, as it was known – was also important to Boole's early mathematical research on the calculus of finite differences; and Boole's knowledge of Gregory's defence of the technique underlies his algebraic analysis of logic in [*Mathematical analysis of logic* (1847)]" (Ewald, *From Kant to Hilbert*, p. 322). OCLC lists one copy only (Northwestern).


CONSIDERATION

OF

VARIOUS POINTS OF ANALYSIS.

BY

JOHN F. W. HERSCHEL, ESQ. F. R. S.

FROM THE

PHILOSOPHICAL TRANSACTIONS.

LONDON:

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW,
ST. JAMES'S.

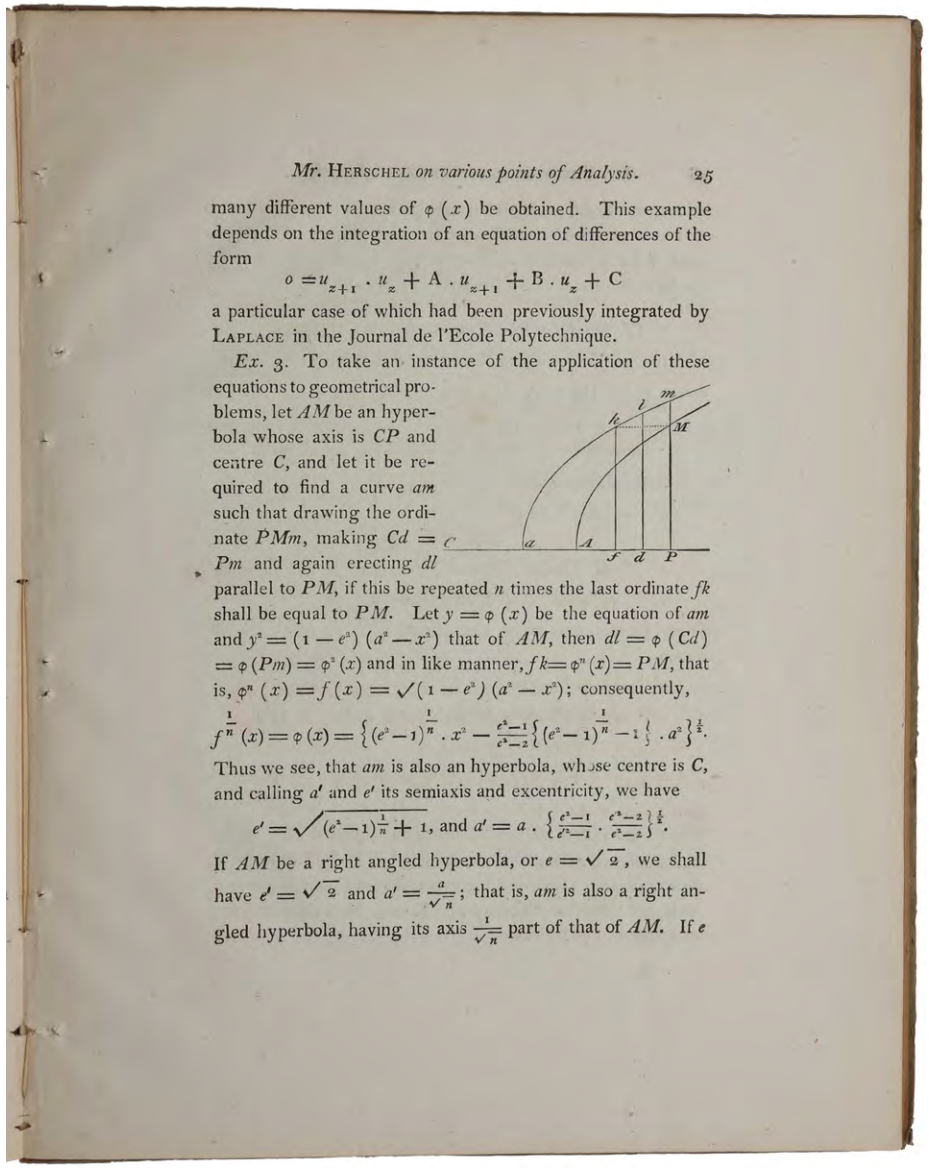
1814.

Provenance: Marcus Nathan Adler (bookplate). Adler (1803-1890) was the Orthodox Chief Rabbi of the British Empire from 1845 until his death, probably the most prominent nineteenth-century rabbi in the English-speaking world. See image of bookplate at http://www.virtualjudaica.com/Item/27334/Ex-Libris_of_Rabbi_Marcus_Nathan_Adler

"The calculus of operations involved separating symbols of operation – such as differentiation – from symbols of quantity. Using general properties of algebra, the separated symbols of operation were then simplified in order to find a solution to an analytical problem or theorem…

"[In 1812] a group of Cambridge reformers, who called themselves the Analytical Society, established their own journal for the diffusion of what they saw as a radical new approach to mathematics. This approach was an algebraically based conception of the calculus developed by Lagrange that emphasized series expansions instead of limits or infinitesimals. With this new conception, Analytical Society members saw themselves as leaving behind the fluxional, Newtonian notation that had been tenaciously adhered to throughout the eighteenth century in Britain. At the same time, the Society felt that it could connect with a "century of foreign improvement" in the calculus…

"Facing the graduation of almost all its members and with few new recruits, the Analytical Society disintegrated in 1814. After the Society's demise, Babbage, Herschel and Peacock continued to write reform-minded publications. For example, in 1816, the three published an English translation of Silvestre Lacroix's *Calcul différential et intégral* as a way to bring Lacroix's formulation of the differential calculus to Cambridge and also as a platform from which to promote their own allegiance to the Lagrangian development of the calculus. In this as well as in a series of articles by Herschel from 1814 to 1822, the calculus of operations



Mr. HERSCHEL *on various points of Analysis.* 25

many different values of $\varphi(x)$ be obtained. This example depends on the integration of an equation of differences of the form

$$o = u_{z+1} \cdot u_z + A \cdot u_{z+1} + B \cdot u_z + C$$

a particular case of which had been previously integrated by LAPLACE in the Journal de l'Ecole Polytechnique.

Ex. 3. To take an instance of the application of these equations to geometrical problems, let AM be an hyperbola whose axis is CP and centre C, and let it be required to find a curve am such that drawing the ordinate PMm, making $Cd = Pm$ and again erecting dl parallel to PM, if this be repeated n times the last ordinate fk shall be equal to PM. Let $y = \varphi(x)$ be the equation of am and $y^2 = (1 - e^2)(a^2 - x^2)$ that of AM, then $dl = \varphi(Cd) = \varphi(Pm) = \varphi^2(x)$ and in like manner, $fk = \varphi^n(x) = PM$, that is, $\varphi^n(x) = f(x) = \sqrt{(1 - e^2)(a^2 - x^2)}$; consequently,

$$f^{\frac{1}{n}}(x) = \varphi(x) = \left\{(e^2-1)^{\frac{1}{n}} \cdot x^2 - \frac{e^2-1}{e^2-2}\left\{(e^2-1)^{\frac{1}{n}} - 1\right\} \cdot a^2\right\}^{\frac{1}{2}}.$$

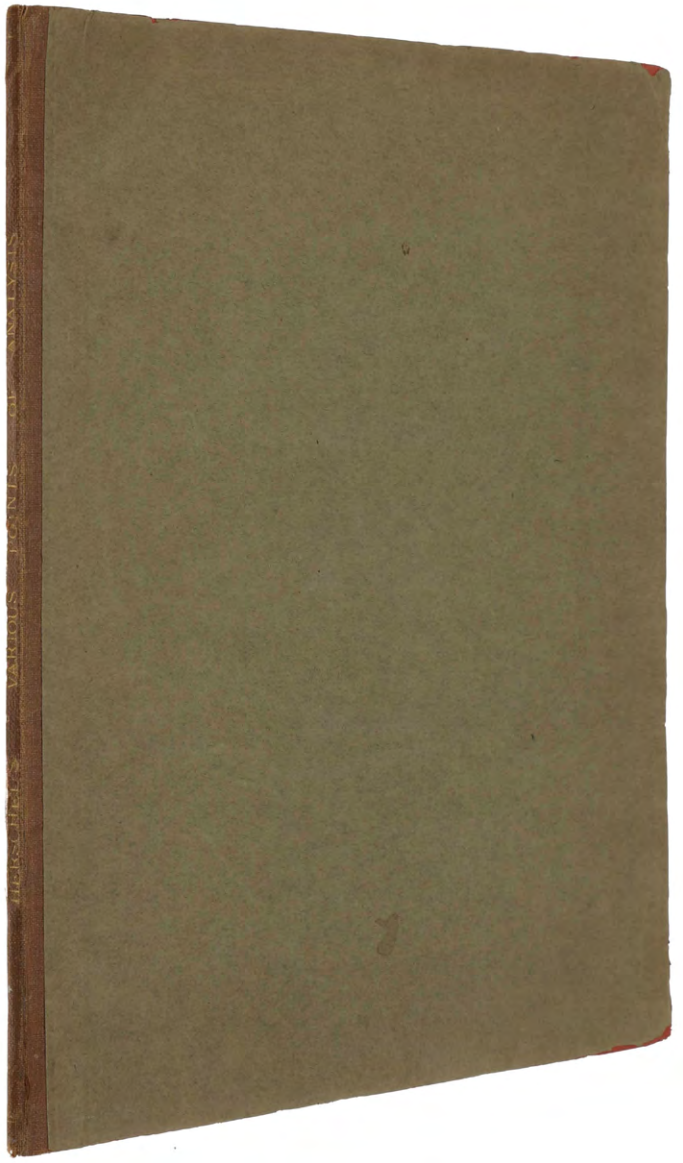
Thus we see, that am is also an hyperbola, whose centre is C, and calling a' and e' its semiaxis and excentricity, we have

$$e' = \sqrt{(e^2-1)^{\frac{1}{n}} + 1}, \text{ and } a' = a \cdot \left\{\frac{e^2-1}{e'^2-1} \cdot \frac{e^2-2}{e'^2-2}\right\}^{\frac{1}{2}}.$$

If AM be a right angled hyperbola, or $e = \sqrt{2}$, we shall have $e' = \sqrt{2}$ and $a' = \frac{a}{\sqrt{n}}$; that is, am is also a right angled hyperbola, having its axis $\frac{1}{\sqrt{n}}$ part of that of AM. If e

was ever present. For example, in his 1814 *Philosophical Transactions* article, "Consideration of Various Points of Analysis," Herschel professed his belief in the fruitfulness of the method of separating the symbols of operation from those of quantity. He explained that "[t]his method I have, perhaps, extended and carried somewhat farther than has hitherto been customary; but, I trust, without losing sight of its grand and ultimate object, the union of extreme generality with conciseness of expression [p. 12]" (Despaux, pp. 52-4).

S.E. Despeaux, "Very Full of Symbols," in *Episodes in the History of Modern Algebra (1800-1950)*, (2007), ed. by J.J. Gray & K.H. Parshall; E. Koppelman, 'The calculus of operations and the rise of abstract algebra,' *Archive for the History of Exact Sciences*, Vol. 8 (1971), pp. 155-242.



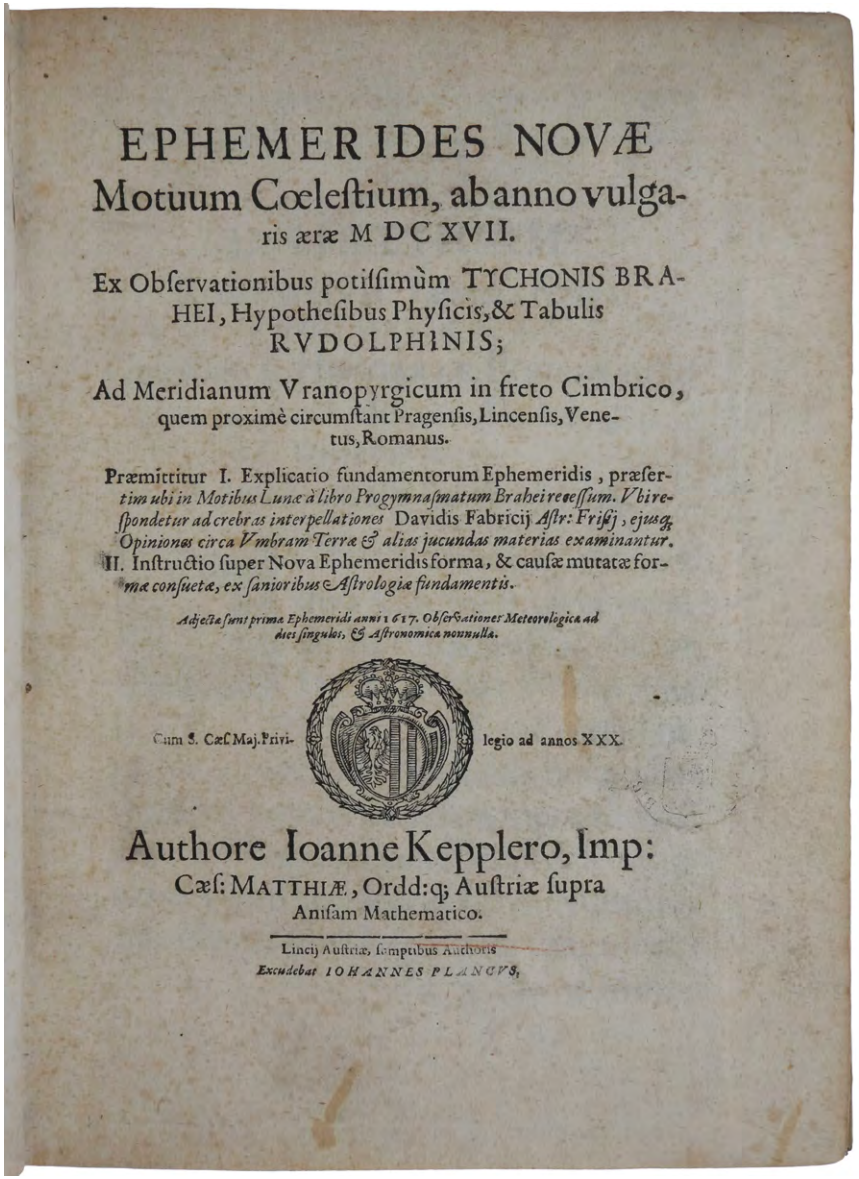
THE FIRST ASTRONOMICAL TABLES BASED ON HIS 'ASTRONOMIA NOVA'

KEPLER, Johannes. *Ephemerides novae motuum coelestium, ab anno vulgaris aerae MDCXVII [-MDCXVIII, MDCXIX, MDCXX]: Ex observationibus potissimum Tychonis Brahei, hypothesibus physicis, & tabulis Rudolphinis; ...* Linz: Johannes Plank, [1617-1619].

$35,000

4to (245 x 178 mm), pp. [xii], 1-39, [1, blank], [1], 40-45, [24, tables], [1, blank]; [7], [24, tables], [1, blank]; [7], [24, tables], [1, blank]; [7], [24, tables], [1, blank]. Printer's device on titles to 1617 and 1618 ephemerides (those of the 1619 and 1620 ephemerides having drop-titles only), engraved initials, woodcut diagrams in text, stamp of Francis Napier to title partially erased. Nineteenth-century vellum, remains of ties, red lettering-piece on spine, 'OBER RATH' stamped in gilt on front cover. Fine and crisp throughout.

First edition, very rare and with an exceptional royal provenance, of Kepler's *Ephemerides* for the years 1617-1620. These were the first tables of astronomical data calculated by Kepler on the basis of the new celestial mechanics he had published in *Astronomia nova* (1609), and also the first calculated using logarithms, preceding by a decade the *Tabulae Rudolphinae* (1627). The accuracy and reliability of Kepler's ephemerides greatly surpassed those of other ephemerides of the time; as a result, the Copernican doctrine gained in esteem, and it was more and more accepted that it was not just a new and useful mathematical hypothesis, but a reflection of reality. In his important introductions to the tables, Kepler



EPHEMERIDES NOVÆ
Motuum Cœleſtium, ab anno vulgaris æræ M DC XVII.

Ex Obſervationibus potiſſimùm TYCHONIS BRAHEI, Hypotheſibus Phyſicis, & Tabulis RVDOLPHINIS;

Ad Meridianum Vranopyrgicum in freto Cimbrico, quem proximè circumſtant Pragenſis, Lincenſis, Venetus, Romanus.

Præmittitur I. Explicatio fundamentorum Ephemeridis, præſertim ubi in Motibus Lunæ à libro Progymnaſmatum Brahei receſſum. Vbi reſpondetur ad crebras interpellationes Davidis Fabricij Aſtr: Friſij, ejusq; Opiniones circa Vmbram Terræ & alias jucundas materias examinantur. II. Inſtructio ſuper Nova Ephemeridis forma, & cauſæ mutatæ formæ conſuetæ, ex ſanioribus Aſtrologiæ fundamentis.

Adjecta ſunt prima Ephemeridis anni 1617. Obſervationes Meteorologica ad dies ſingulos, & Aſtronomica nonnulla.

Cum S. Cæſ. Maj. Privilegio ad annos XXX.

Authore Ioanne Kepplero, Imp: Cæſ: MATTHIÆ, Ordd: q; Auſtriæ ſupra Aniſam Mathematico.

Lincij Auſtriæ, ſumptibus Authoris
Excudebat IOHANNES PLANCVS,

discusses sunspots (including his claim to have been the first of the century to observe them), his lunar theory, the application of logarithms (the ephemeris of 1620 is dedicated to their inventor, Napier), the camera obscura (which for Kepler was of great importance to astronomy), the final chapter in Kepler's relations with the friend-turned-critic David Fabricius, and many other topics of the greatest interest in the history of science. "Already in 1609, at the time his *Astronomia nova* appeared, Kepler had formulated the plan of publishing ephemerides giving daily positions of the Sun, Moon, and planets for eighty years, beginning in 1582 with Tycho's first systematic observations of the planets; the calculated positions would be derived from Tycho's observations and from the new celestial physics that Kepler had developed in the course of his "war on Mars." He hoped to enlist the assistance of Magini, the Bolognese astronomer, in carrying out the calculations, but this idea came to naught. The project on its way to realization had to be greatly curtailed in scope; its execution was delayed by the claims of other enterprises and by numerous calamities" (Wilson). A second volume of ephemerides, covering the years 1621-36, was published by Kepler at Zagan in 1630, shortly before his death on 15 November. ABPC/RBH list only one copy since the Honeyman sale (Swann, Apr 19, 2001, lot 184, $25,300). The Honeyman copy sold for £4,000 ($9,216) in 1980.

Provenance: Queen Sophia of Württemberg (17 June 1818 - 3 June 1877), wife of King William III of the Netherlands, full-page hand-written dedication in Latin, dated 1863, on front free endpaper from her to; Francis Napier, 10th Lord Napier and 1st Baron Ettrick (15 September 1819 - 19 December 1898), his bookplate on front paste-down. Queen Sophia corresponded with several European scholars, protected and stimulated the arts, and maintained warm ties with Emperor Napoleon III and Queen Victoria. Francis Napier, descendant of John Napier of Merchiston (1550-1617), the inventor of logarithms, was a Scottish polyglot, diplomat and colonial administrator; he served as British Minister to



22 RESPONSIO AD INTERPEL-

Pictura Lunæ ſiniſtra in umbra, de quâ inferius agᵗ. non eſt Fabricij, ſed illa ſorte.

Pictura Apiani.

Obſervator bonus & diligens es, obſervata hæc tua in dubium vocanda ſunt minimè. At num verè concludas, verèq; pingas, id videndum. Id quidem ego nego, nihil enim ſequitur, cùm cauſæ multæ eſſe poſſint, cur viſus ſit Tibi defectus oculis nudis, non ſit viſus perſpicillo.

Poſſem dicere, cùm poſterius adhiberes perſpicillum, interim dum quæris Lunam colligendo, illam ex umbrâ exiſſe: poſſem etiam contrarium, nudis oculis Te non defectum, ſed ſaltem debile lumen vidiſſe, partis illius quæ proximè umbram ſtabat.

Quòd enim quibuſdam, adeòq; etiam mihi, qui debili viſu fruor, luna defectura ſatis diu ante verum initium ſuſpecta eſt de initio, ejus rei cauſa ſita eſt in penumbra terræ, quæ partib⁹ lunæ ad umbram appropinquantibus eripit potiſſimam partem radiorũ Solarium, relinquit nonniſi parum aliqd. Tunc igitur quæ minus illuſtrantur, minus etiam movent oculum, minusq; in illo ſeſe diffundunt. Deficit igitur non aër circa lunam, ſed vitioſus ille limbus picturæ retiformis in oculo, ſeu potius vigor ejus in movendo viſu. ¶ *Vide ſupra obſervationem Eclipſis anno 1616.*

Sed miſſis his, utamur tuis ipſius verbis, q̃ proximè præmittis, ſic ſcribens: *pars obſcurata lunæ Cœlo ſereno per hoc teleſcopium facile cerni poteſt, quippe quæ aliquid tamen luminis habet, non penitus tenebroſa eſt.* Hæc tu de ijs lunæ falcatæ partibus, quæ ſunt penitus involutæ nocti, qua illas involvit opacitas globi ſui proprij, cujus pars ſunt: q̃ multo magis ego poſſum uſurpare de partibus ijs lunæ deficientis, quæ licet ſint in nocte, quam globus terræ à longiſſimo intervallo erigit, ſunt tamen in radijs Solis refractis: aut de iis, quæ jam veluti quoddam diluculum ſentiunt.

Nam cur non videatur perſpicillo pars lunæ deficiens, q̃ negęt videri oculis nudis? Cum pars licet deficiens, ſit tamen ex viſibilibus, & ſit inſigni rubore picta? cùm ea natura ſpiritalis ſit, ut q̃ debiliter videretur, videantur fortiter? Nonnè rectè fecero, ſi Tibi Germano diligenti quidem obſervatori, ſed indulgenti tuis opinionibus, Galilæum Italum oppoſuero, qui in uſu de quo agimus, Teleſcopij cæteris plerisq; ducem ſe præbuit. Verba ejus de Eclipſi Lunæ anni 1610. 30 Dec. extant in ejus Epiſtolâ, præfixâ meæ Dioptricæ; *Meta umbræ indiſtincta, confuſa, & veluti obnubilata apparuit, Cauſa quia conſurgit umbra a Terra, longiſſimè à Luna corpore.* Hæc ille: quæ potuiſti legere, indeq; diſcere, limbum illum Lunæ à Te viſum, fuiſſe quidem in umbra, ſed quæ erat diluta luce ſecundâ. Non ſequitur igitur, *Videtur limbus per Teleſcopium, ergo non eſt in umbrâ.*

Aliud firmamentum affers ex alia obſervatione: *limbus partis obſcuratæ in Eclipſi minimâ, non poteſt nudis oculis cerni, cum tamen nudis oculis cernatur & æſtimetur Luna defectus. Ergo id quod deficit, eſt tunica illa aëria lunæ, luce ſua tegens lunam totam extra umbram exiſtentem, & dependens ulterius veluti ſupra pedes lunæ, ſic ut deliberi ab umbra & quaſi proſcindi poſſit, illaſi illius veluti pedibus.*

Nam ſi limbus ſcri corporis Lunæ maneret extra umbram, non regeretur a tunicâ lucidâ, & ſic ipſa tunica ſeſe exereret ulterius in umbram, poſſet a nobis videri etiam in umbra nudis oculis: non caret enim corporis limbus ſuo rubore ſaltem conſpicuo, etiam cum in umbra eſt, & videri poteſt in profundum umbram progreſſus, cur non videretur in umbra extremo?

Reſpon-

the Netherlands from 1859 to 1860, to Russia from 1861 to 1864, and as Viceroy of India from February to May 1872. A number of letters from Queen Sophia to Napier are preserved in the archives of the Malet family at Duke University. Kepler was born in the Duchy of Württemberg (now in southern Germany) in 1571, and his early studies were supported by the Duke.

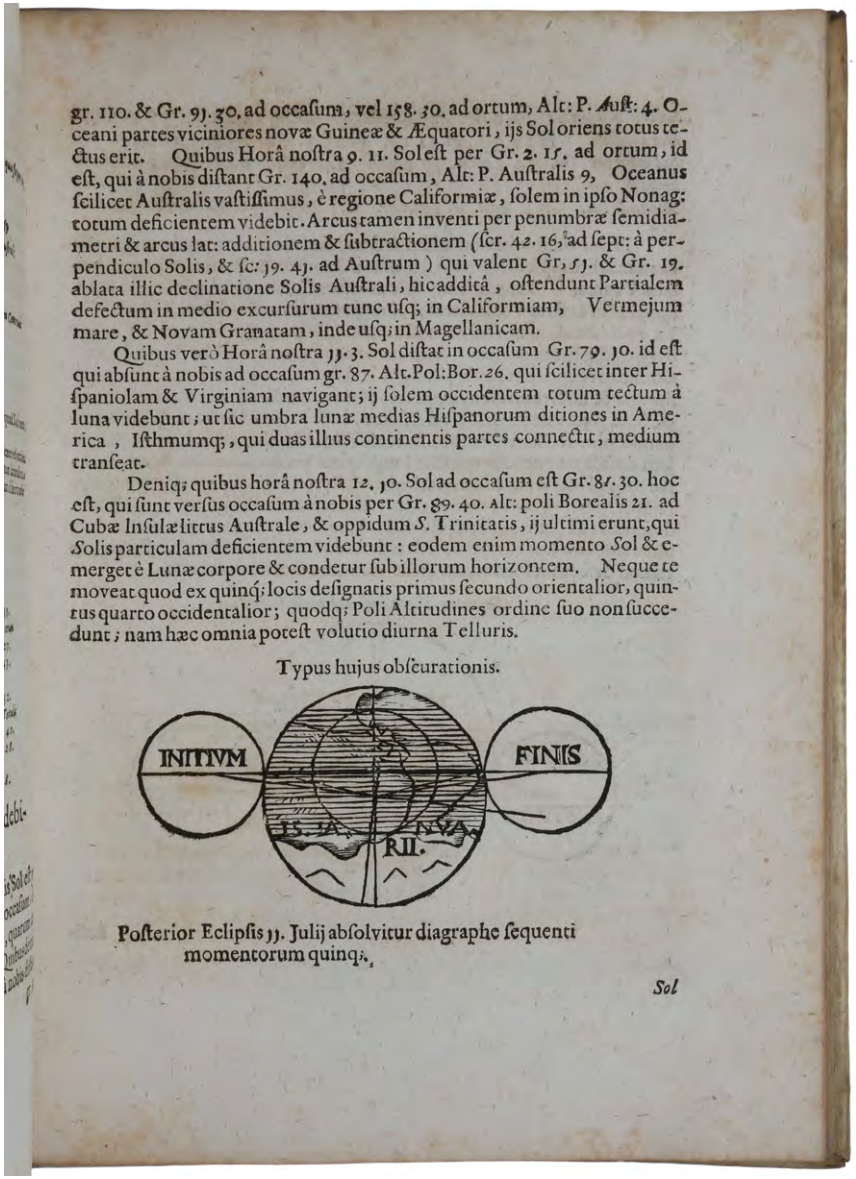
In 1605 Kepler, then Imperial Mathematician to Emperor Rudolph II in Prague, completed the manuscript of *Astronomia nova*, which contained the first two of his three laws of planetary motion. "The great task which lay ahead of him, the great commission which he had to carry out, was to produce the astronomical tables planned by Tycho Brahe … Kepler could no longer depend on the theoretical hypotheses previously made in the calculation of the planet orbits. By the discovery of the planet laws he had created an entirely new foundation for this calculation. But since he had derived these laws solely from the observations of Mars, it was now necessary to demonstrate their validity for the other planets also and to calculate the elements of the orbits of these on the new basis … two more decades were to pass before the work was published [as *Tabulae Rudolphinae*, 1627].

"Associated with these astronomical researches is another work which likewise did not pass beyond the preparatory phase in the Prague period. The astronomical tables form the basis for calculating the ephemerides, that is the year-books which give the positions of the sun, moon and planets for each day of a year. Such ephemerides were very much in demand by astronomers who used them in their scientific researches, by seafarers who needed them for place determination, and last but not least also by calendar makers and astrologers to whom they were indispensable as supports for their prophecies. Kepler now planned to issue such an ephemerides and, moreover, for no less than eighty years. The year 1582 was to be the starting point because that was the year in which Brahe



had begun his observations. From the year 1593 on, Kepler wanted to add his weather observations which dated from then, to get a sure basis for investigating the dependence of atmospheric conditions on the constellations in the sky. He wanted to complete this big work before publishing the [Rudolphine] tables. A double purpose guided him. In the first place, he wanted to be the one to reap the harvest which the treasure of Tycho's observations and his own improved planet calculations promised; after the publication of the tables someone else could easily precede him in this. Secondly, the ephemerides, which he proposed to calculate in advance for the years to come, were to form a test of the accuracy of this new mode of reckoning and consequently for the trustworthiness of his theoretical bases for these calculations which were to be published in the tables" (Caspar, *Kepler*, pp. 178-9).

In 1611, the growing political-religious tension in Prague came to a head. Emperor Rudolph was forced to abdicate, and Kepler's prospects under Rudolph's successor (his brother Matthias) were dim. In the same year, his wife Barbara died from fever, and one of his children succumbed to smallpox. Following Rudolph's death in 1612, Kepler moved to Austria to take up a position as teacher and district mathematician in Linz. "There were two plans which he now wanted to realize; both had already engrossed his attention in Prague. The one concerned the ephemeris … Kepler was occupied first in calculating the elements of the orbits of Venus and Mercury, a difficult task considering the extent of the Tychonic observational material. For whole months in 1614 - 1615 he sat over these calculations. The moon, with the numerous inequalities in its motion, gave even greater trouble. He worried over attempts to explain these inequalities physically, especially the variation, and to follow through the calculation of its physical concept accordingly. Hitherto it has hardly been noticed that here, too, he ran up against innovations in the framing of mathematical questions which deserve the interest of the mathematician and which were difficult to overcome



gr. 110. & Gr. 91. 30. ad occaſum, vel 158. 30. ad ortum, Alt: P. Auſt: 4. Oceani partes viciniores novæ Guineæ & Æquatori, ijs Sol oriens totus tectus erit. Quibus Horâ noſtra 9. 11. Sol eſt per Gr. 2. 15. ad ortum, id eſt, qui à nobis diſtant Gr. 140. ad occaſum, Alt: P. Auſtralis 9, Oceanus ſcilicet Auſtralis vaſtiſſimus, è regione Californiæ, ſolem in ipſo Nonag: totum deficientem videbit. Arcus tamen inventi per penumbræ ſemidiametri & arcus lat: additionem & ſubtractionem (ſcr. 42. 16, ad ſept: à perpendiculo Solis, & ſc: 19. 43. ad Auſtrum) qui valent Gr. 55. & Gr. 19. ablata illic declinatione Solis Auſtrali, hic additâ, oſtendunt Partialem defectum in medio excurſurum tunc uſq; in Californiam, Vermejum mare, & Novam Granatam, inde uſq; in Magellanicam.

Quibus verò Horâ noſtra 11. 3. Sol diſtat in occaſum Gr. 79. 30. id eſt qui abſunt à nobis ad occaſum gr. 87. Alt. Pol: Bor. 26. qui ſcilicet inter Hiſpaniolam & Virginiam navigant; ij ſolem occidentem totum tectum à luna videbunt; ut ſic umbra lunæ medias Hiſpanorum ditiones in America, Iſthmumq; qui duas illius continentis partes connectit, medium tranſeat.

Deniq; quibus horâ noſtra 12. 30. Sol ad occaſum eſt Gr. 81. 30. hoc eſt, qui ſunt verſus occaſum à nobis per Gr. 89. 40. Alt: poli Borealis 21. ad Cubæ Inſulæ littus Auſtrale, & oppidum S. Trinitatis, ij ultimi erunt, qui Solis particulam deficientem videbunt: eodem enim momento Sol & emerget è Lunæ corpore & condetur ſub illorum horizontem. Neque te moveat quod ex quinq; locis deſignatis primus ſecundo orientalior, quintus quarto occidentalior; quodq; Poli Altitudines ordine ſuo non ſuccedunt; nam hæc omnia poteſt volutio diurna Telluris.

Typus hujus obſcurationis.

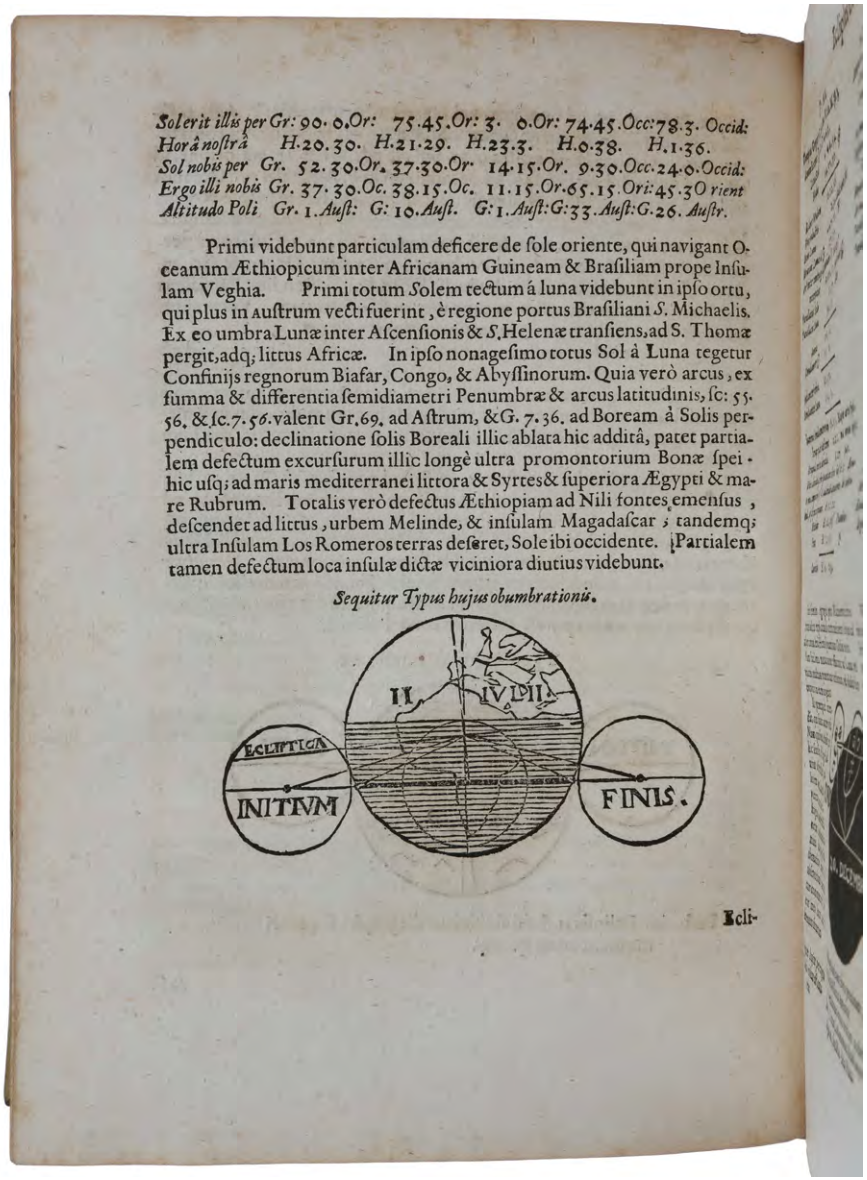
Poſterior Eclipſis 11. Julij abſolvitur diagraphe ſequenti momentorum quinq;.

Sol

with his tools. The year 1616 was primarily devoted to this painstaking work. Attacking the calculation of the ephemerides, the zealous scholar felt as though he had emerged from an abysmal sea. Waiting from March to May, 1617, at the court in Prague on the Emperor's order, he utilized his free time to complete the ephemeris for 1617; that for 1618 he calculated in the following months at home in Linz. Johannes Plank began the printing immediately" (ibid., pp. 238-9). The second of Kepler's plans was the preparation of his *Epitome astronomiae Copernicanae*.

In 1615, Ursula Reingold, a woman in a financial dispute with Kepler's brother Christoph, claimed that Kepler's mother Katharina had made her sick with an evil brew. The dispute escalated, and in 1617 Katharina was accused of witchcraft; in the autumn of that year Kepler was forced to travel to Württemberg to attend her trial (she was eventually imprisoned for fourteen months). After his return in the first weeks of the following year, he resumed work on the *Ephemerides* and *Epitome*, but this was again interrupted by calamity. "On February 9, his little daughter Katharina died. The father was oppressed by sorrow over this loss. "I set the *Tables* aside since they required peace, and turned my mind to the contemplation of the *Harmony* [i.e., *Harmonices mundi*]"" (ibid., p. 265).

"In the years 1617 - 1619, simultaneously with the *Epitome*, the first volume of the *Ephemerides* for the years 1617 - 1620 was completed. The publication of this volume had, as he says, given more printing work to him than to the printer himself. The supplementary printed ephemerides for 1617 contained his weather observations for each day of the year" (*ibid*., p. 300). Ephemerides were primarily numbers and the printer Plank did not have enough. Kepler therefore had to invest in his own set of numerical type. Only when these were available could Plank continue with the printing of the *Ephemerides*.



Solerit illis per Gr: 90. 0. *Or:* 75.45. *Or:* 3. 0. *Or:* 74.45. *Occ:* 78.3. *Occid:*
Horâ noſtrâ H.20.30. H.21.29. H.23.3. H.0.38. H.1.36.
Sol nobis per Gr. 52.30. *Or.* 37.30. *Or* 14.15. *Or.* 9.30. *Occ.* 24.0. *Occid:*
Ergo illi nobis Gr. 37.30. *Oc.* 38.15. *Oc.* 11.15. *Or.* 65.15. *Ori:* 45.30 *rient*
Altitudo Poli Gr. 1. *Auſt:* G: 10. *Auſt.* G: 1. *Auſt:* G: 33. *Auſt:* G. 26. *Auſtr.*

Primi videbunt particulam deficere de ſole oriente, qui navigant Oceanum Æthiopicum inter Africanam Guineam & Braſiliam prope Inſulam Veghia. Primi totum Solem tectum á luna videbunt in ipſo ortu, qui plus in auſtrum vecti fuerint, è regione portus Braſiliani S. Michaelis. Ex eo umbra Lunæ inter Aſcenſionis & S. Helenæ tranſiens, ad S. Thomæ pergit, adq; littus Africæ. In ipſo nonageſimo totus Sol à Luna tegetur Confinijs regnorum Biafar, Congo, & Abyſſinorum. Quia verò arcus, ex ſumma & differentia ſemidiametri Penumbræ & arcus latitudinis, ſc: 55. 56. & ſc. 7. 56. valent Gr. 69. ad Aſtrum, & G. 7. 36. ad Boream à Solis perpendiculo: declinatione ſolis Borealis illic ablata hic additâ, patet partialem defectum excurſurum illic longè ultra promontorium Bonæ ſpei hic uſq; ad maris mediterranei littora & Syrtes & ſuperiora Ægypti & mare Rubrum. Totalis verò defectus Æthiopiam ad Nili fontes emenſus, deſcendet ad littus, urbem Melinde, & inſulam Magadaſcar; tandemq; ultra Inſulam Los Romeros terras deſeret, Sole ibi occidente. Partialem tamen defectum loca inſulæ dictæ viciniora diutius videbunt.

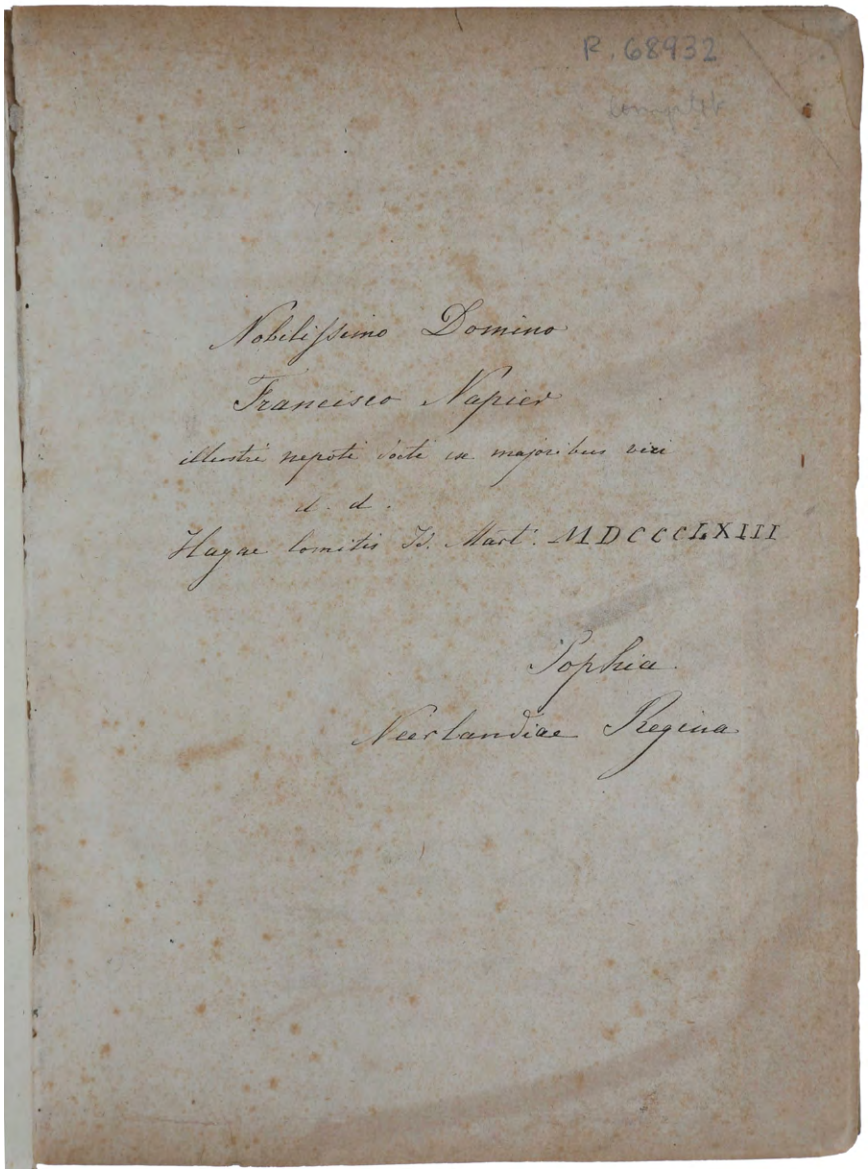
Sequitur Typus hujus obumbrationis.

Ecli-

"Prefixed to the ephemerides for 1617 is the twelve-page "Responsio ad Interpellationes D. Davidis Fabricij Astronomi Frisij, Insertas Prognosticis Suis Annorum 1615, 1616, 1617"; it deals primarily with the nature of sunspots and the history of their discovery" (Wilson). "In *Phaenomenon singulare* (1609) [Kepler] reported on a presumed transit of Mercury that he had observed on 29 May 1607. Unfortunately, Kepler had caught between clouds only two glimpses of a "little daub" that appeared on the solar image projected through a crack in the roof. After Galileo's discovery of sunspots, [Michael] Maestlin pointed out the error and Kepler ultimately printed a retraction in his *Ephemerides* for 1617, noting that unwittingly he had been the first of his century to observe a sunspot" (DSB).

"An additional topic [in the *Responsio*] is the optics of the *camera obscura*, which for Kepler had considerable astronomical importance. Fabricius had found from observations of occultations of stars by the Moon that the dark part of the Moon appeared to be of smaller radius than the illuminated part, and had concluded from this that the Moon was surrounded by an envelope of air which absorbed the sunlight. Kepler argued (correctly) that the phenomenon derived simply from the operation of the eye, which is a *camera obscura* and like all such has a finite aperture: each point of the illuminated object sends a ray of light through each point of the aperture, and as a consequence the object as "caught" on the retina appears larger than it really is" (Wilson).

"Logarithms play a major role in the construction of the ephemerides. Kepler first heard of Napier's invention in 1617, obtained a full description of it in 1618, and by 1619 was computing his own logarithms, although he had not yet seen the Napierean logarithms or received an account of the way in which they were computed. The ephemeris of 1620 is dedicated to Napier, and all the ephemerides from that year onwards were calculated by means of logarithms"

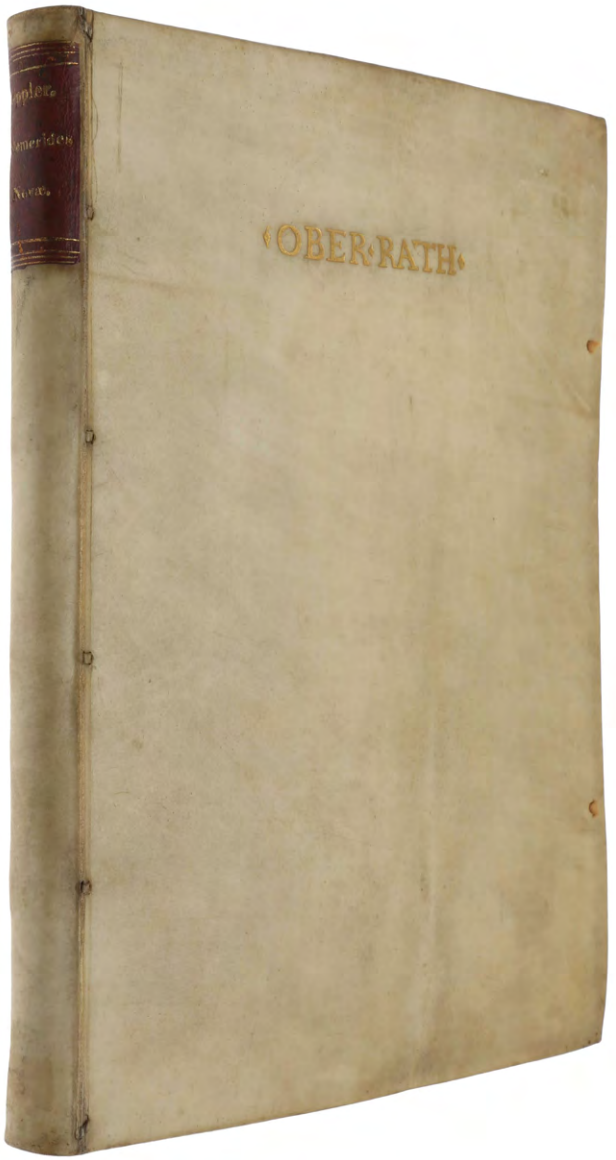


(*ibid.*). Kepler published his own logarithm tables, and an account of their construction, at Marburg in 1624/5 (*Chilias logarithmorum*).

"Of the accuracy of the tabular entries in Kepler's ephemerides Bialas gives us a thorough account (pp. 534-47), using Tuckerman's *Planetary, lunar, and solar positions* as a basis of comparison … he finds standard errors in Kepler's ephemerides of about 6' for Saturn, Jupiter, and the Sun, of about 4' for Mars, and of about 10' for Venus and Mercury. These errors, he shows, are only occasionally due to computational mistakes; they derive principally from the exaggerated eccentricity that Kepler attributes to the Earth's orbit, and from small errors in the orbital elements of the other planets. In a similar comparison with Tuckerman of the ephemerides for 1617 of Magini and [David] Origanus, both of them based on the Prutenic tables, Bialas finds the standard errors to be, for Saturn, 10'; for Jupiter, 13'; for Mars, 57'; for Venus, 37'; for Mercury, 3°26'; and for the Sun, 27' (Magini) or 5' (Origanus). The superiority of the Keplerian ephemerides is thus especially striking in the cases of Mars, Venus, and Mercury" (*ibid.*).

Extracts from Kepler's *Ephemerides* were reprinted by Frisch in 1868 (Joannis *Kepleri astronomi opera Omnia*, vol. VII, pp. 479-666), but with the tables mostly omitted. The entire work was first reprinted in vol. XI of the *Gesammelte Werke*.

Caspar, *Bibliographia Kepleriana* 52; Houzeau & Lancaster 15040; Zinner 4594. Curtis Wilson, 'Review of Johannes Kepler Gesammelte Werke, vol. XI, part I: Ephemerides novae motuum coelestium. Edited by Volker Bialas (Münich: C. H. Beck, 1983)', *Journal for the History of Astronomy*, vol. 17 (1986), pp. 63-65.



THE JESUIT ART OF LIGHT AND SHADOW

KIRCHER, Athanasius. *Ars magna lucis et umbrae in decem libros digesta: Quibus admirandae lucis et umbrae in mundo, atque adeò universa natura, vires effectus[que] uti nova, ita varia novorum reconditiorum[que] speciminum exhibitione, ad varios mortalium usus, panduntur.* Rome: Sumptibus Hermanni Scheus, ex typographia Ludovici Grignani, 1646.

$18,500

Two volumes in one, folio (299 x 202 mm), pp. [xl] (engraved frontispiece, letterpress title, four-page dedication to Archduke Ferdinand (1633-1654), son of Ferdinand III, Holy Roman Emperor (1608-1657), two-page preface by Ferdinand III, six-page preface by the author, 21 pages of table of contents, two pages of poems in praise of the author and one page of privileges), 370 [i.e., 470], including engraved frontispiece signed P. Miotte (vol. 1); [2, separate title-page], 471-568, 567-935, [15, index and errata] (vol. 2), with 40 engraved plates (35 numbered, 5 unnumbered, of which one is large and folding). Several hundred woodcut diagrams and illustrations in text, a few white on black. Contemporary vellum, manuscript lettering to spine, spine with some wormholes, occasional light spotting but in general a very clean and crisp copy.

First edition of Kircher's principal contribution to optics, treating light, shadow, colour, refraction, projection, distortion and luminescence, and providing early descriptions of the *camera obscura* and magic lantern. The work also includes some of the earliest observations with a microscope, preceding those of Hooke and van Leeuwenhoek by two decades. "The first published account of the illumination and projection of images appeared in the first edition of Athanasius Kircher's *Ars magna lucis et umbrae* (1646)" (Bud & Warner, p. 365). "The use of mirrors to



project secret messages into dark spaces was taken up in the seventeenth century by Athanasius Kircher, who, though he ridiculed the extravagant claims of Agrippa, described methods for projecting texts using both sunlight and candles, with the aid of both flat and concave mirrors, and a convex lens. Kircher described this art as "Catoptric Steganography", and if we are to believe that the magic lantern anticipated the slide-show, Kircher's Catoptric Steganography was the early modern version of the Powerpoint presentation" (Lefèvre, p. 44). "Kircher's theory of colour was at root Aristotelian and Aguilonian in type … Kircher has amplified the Aguilonian scheme by introducing the mixtures of the 'median colours' with black and white, but the basic pattern is identical, relying upon the white and black poles between which come yellow, red and blue together with the three 'composites', orange, green and purple … Kircher had come tantalisingly close to the later system of primary and secondary colours. All that was required was the elimination of white and black as 'colours' – though this was not easy given their place in the Aristotelian order of things" (Kemp, pp. 280-1). "Understanding the nature of light was one of the central questions of natural philosophy in the mid-seventeenth century, as was the phenomenon of magnetism … Kircher's virtue was to recognize the significance of these questions. He attempted to collect all the relevant information and began to analyse it. While his conclusions often seemed flawed to the most critical and knowledgeable readers at the time, they nonetheless mined his books for useful data and specific insights that they might incorporate into their own natural philosophies" (DSB). This is a complete copy of a work often found lacking plates, or without the separate title to the second volume.

The full title of the work may be translated, "The Great Art of Light and Shadow, divided into ten books, in which the admirable powers and effects of light and shade are propounded in new and varied experiments and in recondite ways, for the diverse uses of mankind." It was composed shortly after Kircher's principal work on magnetism, *Magnes, sive de Arte Magnetica* (1641), in which he argued that magnetism was the principal force organizing and controlling nature, including optical phenomena. "The title in Latin, *Ars magna lucis et umbrae*, was intended as a play on words: "We say 'Magna' on account of a kind of hidden allusion to the magnet," Kircher wrote in his introductory pages, meaning that the title could also be read as "The Magnetic Art of Light and Shadow" (Glassie, p. 115).

"In his introduction to the book Kircher wrote that the idea of producing a study on optics came to him a few years before when the Emperor [Ferdinand III] put to him some questions about light and shadow. He had written out his answers and then, with the encouragement, as well as the essential financial backing of many friends, he had decided to expand those answers into a full treatise … [Kircher] follow[ed] the introduction with some laudatory poems and epigrams, with himself as the subject. The poems were by James Alan Gibbs, Kircher's English medical friend, and the epigrams by A. F. Fayenus, Doctor of Laws and professor at the Academy of Avignon.

"In the tradition of the Schoolmen, Kircher began the main section of the book with a series of definitions. He explained the meaning of "Ars Sciagnomica" (of shadows), "Ars Chromocritica" (of colours), and so on before dealing with "Photosophia," or the science of light. The sun, he wrote, as the first of all the lights, had to be considered before any other source of light. Rather surprisingly, instead of describing, as one might have expected, the glory and the power of the sun, he began by treating of its "defects", the sunspots. He recalled the time when he first observed them, in 1625, but gives credit for priority of discovery to Galileo, Scheiner and other astronomers who had preceded his observations by many years. The sun, he wrote, is like a great ocean of fire, showing disturbances and ebullations on its surface, a mass of burning, bubbling matter, producing light

and heat, and affecting life on earth in various ways. He illustrated this section with a half-page plate showing a band of sunspots along the equatorial region of the sun's face …

"Even though the moon, Kircher continued, is merely a secondary light, acting as a reflecting surface for the rays of the sun with no light of its own, it nevertheless also has an effect on the earth in many ways. The moon modifies the light it reflects, and thus, for example, causes the tidal motion of the ocean. Kircher was here referring to his theory of the "flux and reflux" of the sea, in which the tides were said to be due to the "nitrous quality" given to the waters by the light reflected from the surface of the moon. The pure light of the sun is first contaminated by nitrous effluvia in its brief contact with the moon, and it then conveys these effluvia to earth.

"Kircher considered that the reason why the moon has such an influence on earth was the fact that the two planets are very similar. Aristotle was wrong in saying that the moon, like the other heavenly bodies, was made of the unchanging quintessence, the fifth element. On the contrary, Kircher continued, "the moon is a rough-surfaced body, very like the earth in appearance, as is proved by the many observations which have been made by members of the Academy of the Lincei in Rome" [such as Galileo] …. He described what he had seen through the telescope, mountains, areas that appear to be seas and lakes, and so he concluded "the moon, like our planet, is composed of the elements of earth and water."

"Kircher had a great deal more to say about the light of the planets, the rings of Jupiter, and other such heavenly bodies. From them, he returned to earth and to a study of terrestrial fire. He defined this as "air ignited by the violent collision of two bodies, which by the heat thus generated causes inflammable substances to burst into flame." He supported this view by referring to the igniting of tinder by



sparks produced by striking a flint against a piece of iron, and also to the way in which certain tribes in South America make fire by spinning a stick in a groove cut in another piece of wood …

"It was in this section that Kircher treated of "Animal Light" in a passage that was to receive favourable comment from later writers. He discussed the light of fireflies, which he had studied while in Malta. He had collected a considerable number of these insects, which were abundant on the island, and had studied them with particular care. He noted that the light they emitted was coloured like that of burning sulphur, and that they seemed to have voluntary control of the flashes. The light he compared to the phosphorescence of decaying wood or of the scales of certain fishes which he had observed in the Mediterranean … Kircher had also at one time during his stay in the south of France examined certain zoophytes such as jellyfish. These also emit a phosphorescent glow in the dark. He explained this phenomenon from the teleological point of view, that the ability to produce light is given by nature to these creatures so that they can seek food in the dark far under the surface of the sea, and also use it as a means of frightening off their enemies.

"Some stones, he continued, have a somewhat similar property. The best known of these is the Bolognian stone, the phosphor which had been first discovered and described by Cascariolo early in the seventeenth century. Kircher had discovered a similar mineral during his explorations and had purified it by calcination in a furnace. His product, after exposure to sunlight, glowed like coal, but gradually this light faded. It could be restored by exposing it to sunlight once more. His explanation for this was that the stone absorbed from the sunlight "a most subtile vapour which is capable of producing light." This study anticipated by forty years the well-known investigations by Robert Boyle of "aerial noctilucia" (fireflies) and "icy noctilucia" (phosphorous).

"From a consideration of fire, Kircher passed on to the study of the nature of colour. He rejected the view, which he attributed to Pythagoras, that colour is nothing more than the appearance of the surface of things. He preferred the theory that colour is the movement of the diaphanous medium which has been illuminated and thus light is actually distinct from the colour it produces.

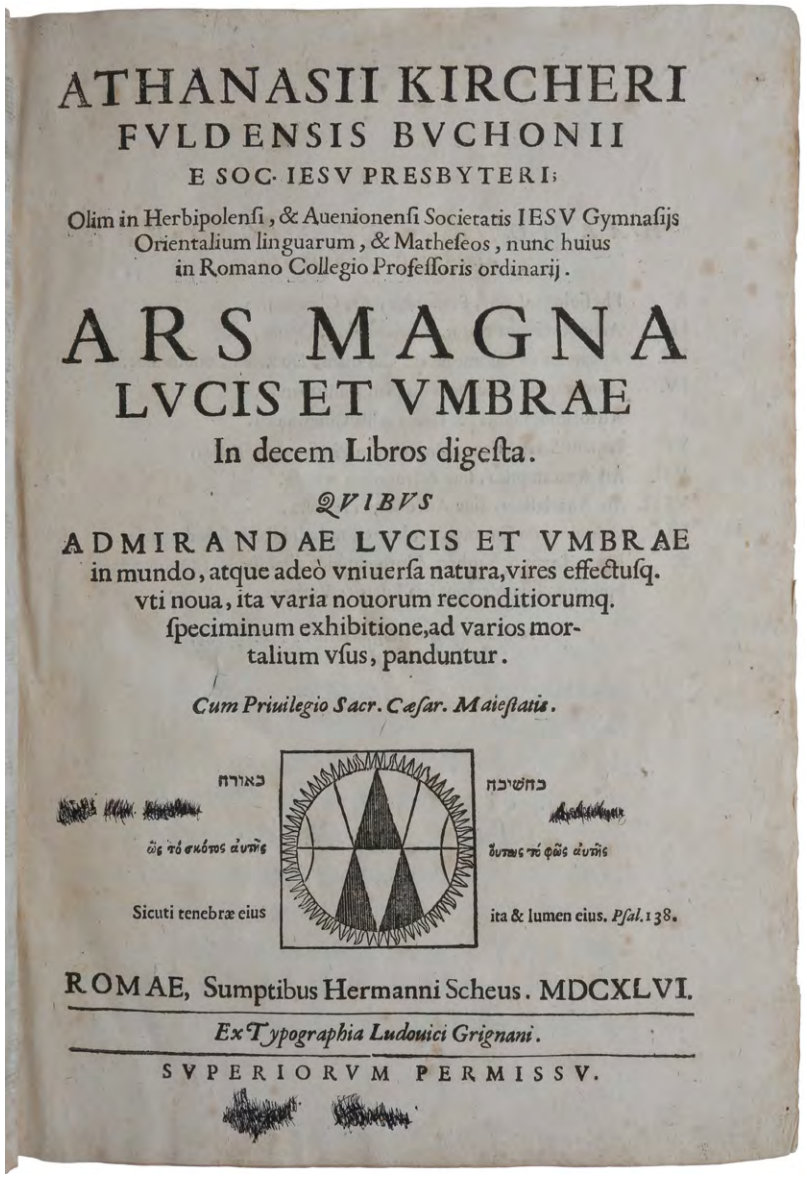
"Kircher held that the rainbow is produced by the light of the sun passing from one medium to another, from air into a raindrop, where it is refracted, reflected and then passed out again to reach the eye. In this he followed the generally accepted explanation of the primary rainbow given by Marc Antonio de Dominis and by René Descartes. But he had his own explanation of why different colours are formed by these refractions and reflections. Pure light of the sun is "debilitated" by its passage through the raindrop, and thus the various colours are really due to contamination of white light. According to Kircher, pure light produces the colour white, and its absence is blackness. In between these two extremes are three colours, yellow, red and purple, produced by the contamination or debilitation of sunlight which results from its passage from air to another medium. All the other colours are mixtures of whiteness and blackness and of the three intermediate colours …

"Kircher applied his theory of the formation of colour to explain the strange properties of the Mexican wood known as "Lignum Nephriticum". The wood had originally been used in the preparation of a decoction for the treatment of kidney disease or nephritis (hence its name) but it had also been found to have remarkable fluorescent properties. Its source was "Eysenhardtia polystadia", the "palo dulce" tree of Mexico, and had been brought to Europe by missionaries some years before. To Kircher it was known by its Mexican names of "Coatl" and "Tlapazathi", and he greatly treasured a piece which had been given to him by a missionary friend. When water was put into a bowl which had been carved from

this wood it became tinged with a bluish colour which later turned to purple. Kircher found that if some of the wood were ground into a powder and was sprinkled into water, the water was also tinged with colour. However, on standing the powder lost its power to affect the water in this way. He had presented a cup made from the wood to the Emperor, who treasured it as a great rarity. Though Kircher did not attempt to go into the real nature of this phenomenon, and contented himself by referring the reader to his theory of the formation of colour for an explanation, this section on "Lignum Nephriticum" is of importance. As E. Newton Harvey has noted in his *History of Luminescence*, it is one of the earliest published accounts of natural fluorescence.

"In the following sections Kircher treated at length of colour in minerals, plants and animals. These chapters give evidence of his enormous industry and the breadth of his interests … Among the animals he discussed at some length was the chameleon. A Franciscan missionary had brought him one of these little creatures from Palestine in 1639 and Kircher examined it very carefully. He discovered that many of the claims that had been made about it were false, or at least greatly exaggerated. It was however true, he found, that the chameleon can change colour to suit its background, green on green and white on white and so on. According to Kircher these changes of colour were "voluntary", for when the animal finally died, it no longer reacted to changes in background colour. Perhaps its ability, Kircher surmised, could be compared to the changes that take place in the fact of a man, who blushes when embarrassed, turns red when angry and pale when afraid.

"In Book Two Kircher treated of what he called "Actinobolismus" or radiation. He discussed the transmission of light in straight lines, giving general geometrical propositions on the projection of light from a point source. He quoted with approval Kepler's description of the projection of an image, such as is seen in


ATHANASII KIRCHERI
FVLDENSIS BVCHONII
E SOC. IESV PRESBYTERI;
Olim in Herbipolenſi, & Auenionenſi Societatis IESV Gymnaſijs Orientalium linguarum, & Matheſeos, nunc huius in Romano Collegio Profeſſoris ordinarij.
ARS MAGNA
LVCIS ET VMBRAE
In decem Libros digeſta.
QVIBVS
ADMIRANDAE LVCIS ET VMBRAE
in mundo, atque adeò vniuerſa natura, vires effectuſq. vti noua, ita varia nouorum reconditiorumq. ſpeciminum exhibitione, ad varios mortalium vſus, panduntur.
Cum Priuilegio Sacr. Cæſar. Maieſtatis.
באורה
כחשיכה
ὡς τὸ σκότος αὐτῆς
οὕτως τὸ φῶς αὐτῆς
Sicuti tenebræ eius
ita & lumen eius. Pſal. 138.
ROMAE, Sumptibus Hermanni Scheus. MDCXLVI.
Ex Typographia Ludouici Grignani.
SVPERIORVM PERMISSV.

the famous "camera obscura", which is also discussed by John Baptist della Porta. He described in detail his own development of the same device, with an apology that "though the experiment is well known, yet I will enlarge on it here since it explains what I wish to say." A small hole, he wrote, is made in the shutter of a darkened room, and a little lens, such as that used as a magnifying glass, is fitted into the hole. A white card is placed at a suitable distance from the lens, and "whatever is outside the window is seen depicted on the card, with its true colours, only differing from reality in its size and disposition" …

"From the subject of the projection of light, it was an easy step for Kircher to that of the propagation of sound. He compared the echo with the transmission and reflection of light. He also described some acoustic instruments he had designed. One of these instruments was to be the subject of a controversy between him and Sir Samuel Morland, Fellow of the Royal Society of London, some years later. This was his reconstruction of "the horn of Alexander the Great, which the Emperor used to call his army together in battle" …

"Kircher finally returned from sound to vision, with a chapter on the structure of the eye. He had been introduced to this subject by a skilled anatomist who had taught him how to perform dissections and examinations. These showed him that the eye was like a lens, with a screen behind it on to which the image was projected, somewhat as happened in the "camera obscura." He explained how to see this for oneself. "Take the eye of a bull or of any large animal, or even of a man if the occasion arises. Wash it well and remove the outer, thicker layers, and then set it in a screen in a darkened place … (it will be seen) that the image of objects placed in front of the eye pass through the crystalline humours right into the interior, with their true colours and shape which are there accurately and faithfully reproduced. As if they were drawn with a brush."

"In an interesting passage Kircher described aftervision, the image that remains in the eye after we have stared at an object for some time and then looked in the dark. The object is still seen, but in changing colours, because Kircher explained, the formation of an image in the eye is like the effect of sunlight on a phosphor stone. Light is absorbed by the stone, and so it glows in the dark, but gradually the glow fades as the light leaves the stone. And so it is with the image retained in the eye, it is bright at first, but gradually it fades and its colour changes, because colour, as had been explained earlier, is due to the debilitation of light.

"The eye then, is like a crystal globe in which images of external objects can be received, as is the case with the great globe which Father Christopher Scheiner had once presented to Archduke Maximilian of Austria. When vision is correct, the crystalline lens focuses the image sharply on the retina, as does the globe on its inner surface. But if the lens does not function correctly, the image is blurred. This is the case with short-sighted people, in which the lens fails to focus sharply on the retina.

"This theory of vision provided a more satisfactory account of the function of the eye than did that of Alhazen which was believed by many at that time. As Kircher himself admitted, his theory owed much to Kepler and Scheiner and even to Porta's "camera obscura." However he had tested the theory for himself, even, it would appear, experimenting with the human eye. He had noted how the crystalline lens refracts light rays to form converging cones whose vertices lie on the retina. Even his view, that some physical change which can be compared to phosphorescence takes place in the retina and so may account for aftervision, is not entirely out of keeping with our modern view in which light energy is transmuted into chemical energy in the retina …

"The following four books of his treatise contain many chapters on methods of designing and constructing sundials. Kircher always was interested in the design of such devices, and a great deal of his time must have been spent in the workshop directing the construction of his "Horographia Babylonio-Italica", his great "Pyramidal Sundial", his "Botanical Clocks", and many other strange dials and clocks which are mainly of antiquarian interest today. The construction of such instruments was a popular occupation in the sixteenth and seventeenth centuries and many books describing various designs and techniques had been written by his contemporaries. These designs of Kircher and many of his fellow dial makers were intended to delight the eye and cause wonderment, rather than solve the real problem of time-keeping which Huygens and Hooke were to take so seriously. For until a reliable, universal timepiece had been invented, the great question of how to navigate correctly at sea and to compute longitude with accuracy could not be solved. In an age when voyages of exploration were so important this was indeed a serious problem.

"Kircher did, however, give some of his attention to this problem, and he described an instrument which, he claimed, was the answer to mariners' prayers. Many have tried, he wrote, to devise such an instrument for the accurate measurement of longitude. They had based their attempts on theories of magnetic variation, on observations of the eclipse of the moons of Jupiter, on the position of sunspots and the mountains of the moon, but all with little success. But with the instrument he had invented, his "Horologium Catholicum" or universal clock, the problem could be solved. This clock had actually been designed originally for another purpose, as a sort of Jesuitical timepiece, "which could tell the time everywhere, but especially in the Colleges of the Society of Jesus, scattered throughout the world", but it could be applied to other uses.



"Kircher's idea was more or less based on a method suggested by Galileo. This was that if the moment of some astronomical event, such as the eclipse of the moons of Jupiter, could be predicted with the help of tables in some standard time related to a fixed degree of longitude, then a person in some other part of the world, observing the same event in terms of local time, could readily calculate the longitude of the place where he made his observations. Kircher wrote, "we intend to describe how a clock can be made which will tell the time in any part of the world, but especially in the various Jesuit Colleges. This cannot be done without a knowledge of longitudes, and to find these I have spent a great deal of time in study. I have used information supplied to me by Jesuit mathematicians who made observations of eclipses in Europe, Eastern India, China, Peru, Brazil, Canada and Mexico, and used these to determine longitudes. Knowing the difference between the time when an eclipse was observed, for example in Nanking in China, from that of the same eclipse observed at Goa in India, Mozambique in Africa, Pernambuco in Brazil and in several other stations, it was possible, though not without difficulty, to calculate the longitudes of each of these places, as well as of other places lying between them. Using this information, then, we were able to construct the timepiece for the whole Jesuit order."

"We may pass very briefly through some of the following sections of the treatise, merely picking out some points of particular interest. One such section is that in which Kircher claimed to have calculated the thickness of the atmosphere by measuring the refraction of sunlight in air. The figure he arrived at was about 43,000 paces but, he adds, this was on a plain, for as you rise above sea level the thickness of the atmosphere decreases. Moreover, in cold regions air condenses and forms a thinner layer, while in the tropics it rarefies and the atmosphere rises. This passage would not have displeased Blaise Pascal who, in 1648 had his brother-in-law Périer perform the famous experiment with a barometer on the Puy-de-Dôme in Auvergne in which he demonstrated that atmospheric pressure was less on the summit than at the base of the mountain. Of course, Kircher and Pascal would have disagreed on the interpretation of this finding, for the Jesuit denied that the mercury in a barometer was supported by the weight of the atmosphere.

"The title Kircher gave to the last part, Book Ten, of his treatise was "Magia Lucis et Umbrae." Here he let himself go, delighting to describe and illustrate the fascinating instruments and devices "in which the little known powers of light and shadow are put to diverse uses" … Kircher actually used them to introduce a very important section of his treatise which he entitled "Dioptrics, or concerning lenses of pantoscopes and telescopes and their various forms and applications" … He described how lenses must be placed in the tube to make, for example, a microscope. A single lens, if well made, will make a satisfactory instrument, but it is much better if two lenses are employed. "Thus can be made a microscope which will amplify a fly into an elephant and a flea into a camel." An instrument of this kind was what he used in many of his investigations. Of the result of his observations he wrote, "Who would have believed, had he not used a microscope, that vinegar and milk abound with an innumerable multitude of worms … that leaves of plants are made up of tiny filaments, those of the Castor Oil Plant contain a collection of star-like bodies … you will observe that each plant produces its own peculiar animals by the putrefaction of its humours, as from a pullulating seed bed." What a pity Kircher did not devote a whole book to the results of his microscopical investigations, which preceded those of Robert Hooke and Anthony van Leeuwenhoek by so many years.

"Kircher followed his treatment of lenses with a discussion of mirrors … Here he goes into details of "burning mirrors", such as those said to have been made by Archimedes at Syracuse and used by him to destroy a fleet" (Reilly, pp. 72-86).

"The book's ten-part structure, as Kircher explained it, connected to the ten-stringed harmony of the Greek instrument the decachord. This in turn represented the well-ordered harmony of nature, and the Decalogue, or Ten Commandments, and the Pythagorean notion of the number ten as the number of the universe and perfection, as well as the Sefirot, the ten emanations of God, by which, according to Kabbalah, the universe was created. "For just as the wise men of the Hebrews claim a world built from ten rays of divinity," Kircher wrote in his preface, "so we completed ten separate themes or books, as it were, ten books in ten parted rays, the world of light and shadow, that is, our art"" (Glassie, p. 116).

The remarkable engraved frontispiece summarizes elegantly Kircher's efforts of linking hermetic elements to Christian concepts, and his views on the metaphysics of light. Angels form an arc under the central light, which is YHWH, the Hebrew letters for God. Daylight is the source of direct light, refracted light, and light reflected by night (on the right). Divine authority, a hand writing a book that absorbs light directly from the source of all light, oversees the daylight, and it is a little higher than Reason, the hand writing a book above the night, which receives a more modest eye's light. Below daylight is Profane Authority, which receives only a lantern's light; below Reason is Sense, which points to an image produced by a telescope. Emperor Ferdinand enters the picture as one of Kircher's patrons.

Norman 1216 (recording 38 plates, incomplete?); Becker 219 (39 plates); Vagnetti EIIIb42; Merrill, *Athanasius Kircher* 7 (erroneous collation); Linda Hall Library, *Jesuit Science*, 10; Bud & Warner, *Instruments of Science*, 1998; Glassie, *A Man of Misconceptions*, 2012; Kemp, *Science of Art*, 1992; Lefèvre, *Inside the Camera Obscura – Optics and Art under the Spell of the Projected Image*, 2007; Reilly, *Studia Kircheriana*, 1974.



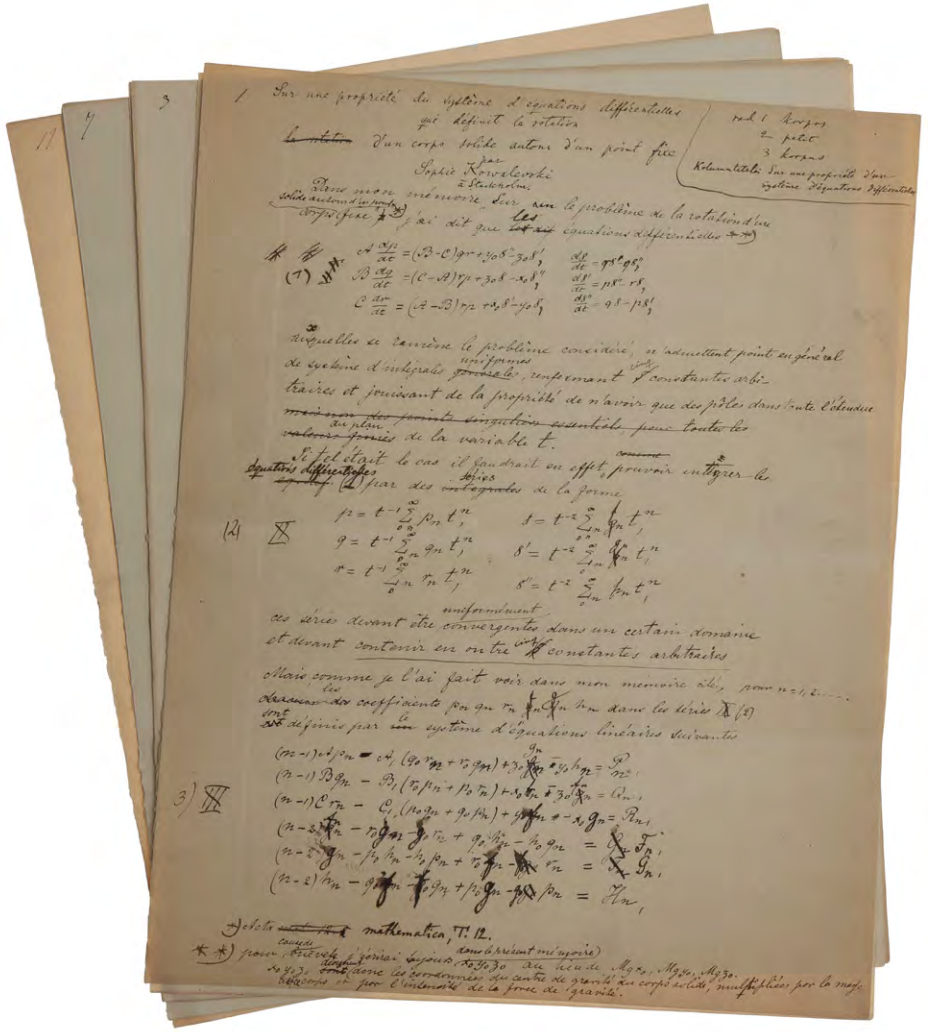
EXCEPTIONAL MANUSCRIPT BY THE GREATEST FEMALE MATHEMATICAN

KOWALEVSKY, Sonya (or KOVALEVSKAYA, Sofya Vasilyevna). *Sur une proprieté du système d'equations différentielles qui définit la rotation d'un corps solide autour d'un point fixe.* Autograph manuscript signed, 11 leaves, written on recto only, with some corrections and additions. Undated, but first published in *Acta Mathematica* 14 (1890), pp. 81-93.

$24,500

11 loose leaves, written on recto only, 290 x 232 mm, first leaf with very mild toning, some leaves with fingerprints in ink, most likely by the author. Three light horizontal creases from having been foled for postage. Very fine and clean.

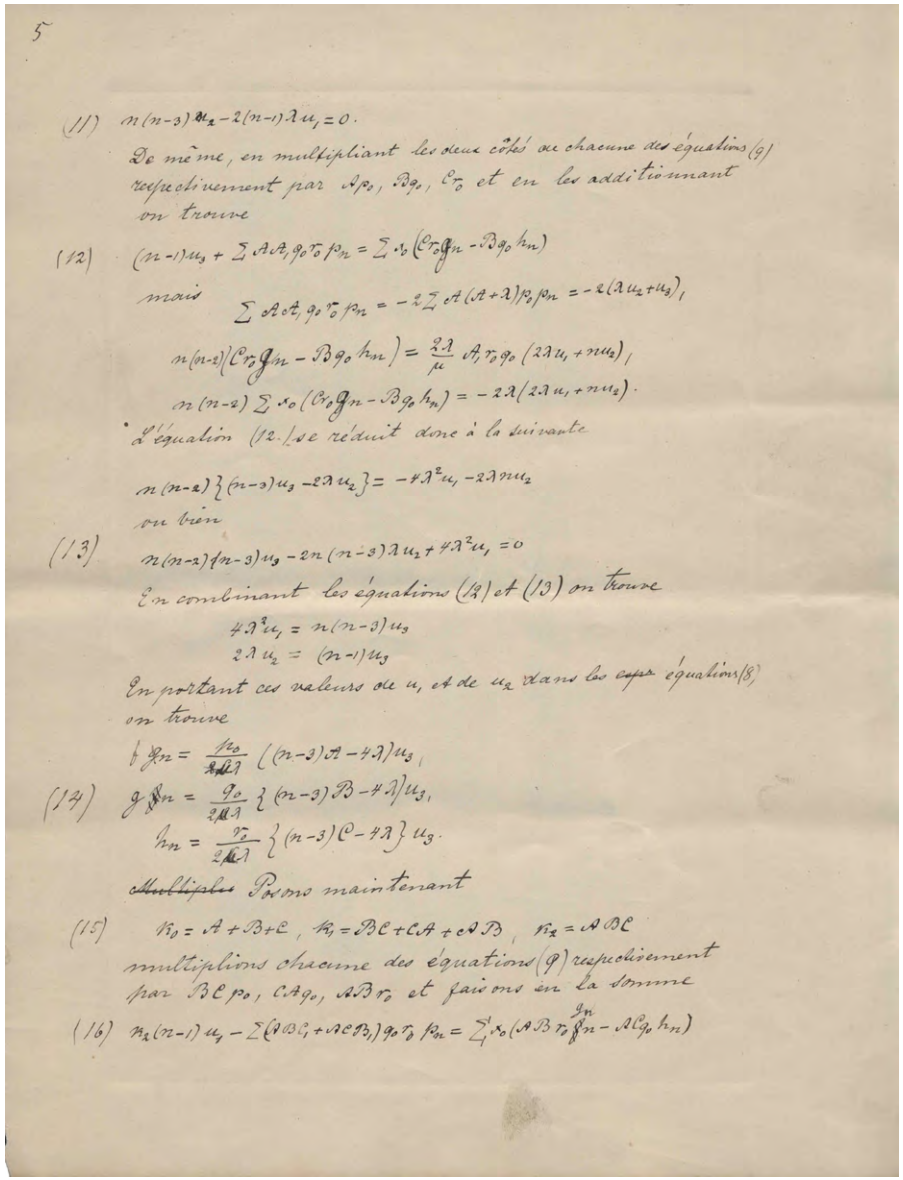
Important autograph manuscript by "the greatest woman mathematician prior to the twentieth century" (DSB) and the first woman in Europe to obtain a doctorate in mathematics. This paper complements and completes her most famous work, 'Sur le problème de la rotation d'un corps solide autour d'un point fixe' (*Acta Mathematica* 12 (1889), pp. 177-232), for which she received the 1888 Bordin Prize from the French Academy of Sciences. "Prior to Sofya Kowalevsy's work the only solutions to the motion of a rigid body about a fixed point had been developed for the two cases where the body is symmetric. In the first case, developed by Euler, there are no external forces, and the center of mass is fixed within the body. This is the case that describes the motion of the earth. In the second case, derived by Lagrange, the fixed point and the center of gravity both lie on the axis of symmetry of the body. This case describes the motion of the top. Sofya Kowalevsy developed the first of the solvable special cases for an unsymmetrical top. In this case the



center of mass is no longer on an axis in the body. She solved the problem by constructing coordinates explicitly as ultra-elliptic functions of time" (Rappaport, p. 570). Kovalevskaya continued her work in the present paper, which shows that the Euler, Lagrange and Kovalevskaya cases are, in fact, the *only* solvable cases of the motion of a rigid body. "Kovalevskaya's contribution was remarkable in many respects. First, she applied a recently developed and highly abstract mathematical theory (the theory of Abelian functions) to solve a physical problem [in the 1889 paper]. Second, she introduced one of the first proofs of the non-integrability of a physical system [in the present paper]. This result, together with Poincaré's and Bruns' work on the three-body problem, announced the failure of the program of classical mechanics to integrate exactly and for all times the equations of motion" (Goriely, p. 8).

"To appreciate Kovalevskaya's contributions to the problem of a rotating rigid body it is necessary to understand the state of the problem before and after she worked on it. The physical problem – to express the position of the figure axes of a rigid body explicitly as functions of time – arises naturally from Newtonian mechanics. The first person to make progress on the problem was Euler, who over a period of twenty years gave various mathematical formulations of the problem and solved the resulting differential equations completely for the case of a body free of torque. Lagrange studied the problem, formulating it in terms of curvilinear coordinates that were better adapted to the problem than the rectangular coordinates used by Euler. Lagrange solved the case of greatest physical interest, when the body has the symmetry of a spinning top.

"In both the Euler and Lagrange cases the solutions are expressed by writing time as an elliptic integral of a spatial variable. By the time of Lagrange elliptic integrals had been extensively studied by Legendre … Further progress on the problem awaited advances in elliptic functions and sufficient liberation from


5

(11) $n(n-3)u_2 - 2(n-1)\lambda u_1 = 0$.

De même, en multipliant les deux côtés de chacune des équations (9) respectivement par Ap_0, Bq_0, Cr_0 et en les additionnant on trouve

(12) $(n-1)u_3 + \sum Ap_0 q_0 r_0 p_n = \sum x_0 (Cr_0 g_n - Bq_0 h_n)$

mais

$$\sum Ap_0 q_0 r_0 p_n = -2\sum A(A+\lambda)p_0 p_n = -2(\lambda u_2 + u_3),$$

$$n(n-2)(Cr_0 g_n - Bq_0 h_n) = \frac{2\lambda}{\mu} Ar_0 q_0 (2\lambda u_1 + n u_2),$$

$$n(n-2)\sum x_0 (Cr_0 g_n - Bq_0 h_n) = -2\lambda(2\lambda u_1 + n u_2).$$

L'équation (12.) se réduit donc à la suivante

$$n(n-2)\{(n-3)u_3 - 2\lambda u_2\} = -4\lambda^2 u_1 - 2\lambda n u_2$$

ou bien

(13) $n(n-2)(n-3)u_3 - 2n(n-3)\lambda u_2 + 4\lambda^2 u_1 = 0$

En combinant les équations (12) et (13) on trouve

$$4\lambda^2 u_1 = n(n-3)u_3$$
$$2\lambda u_2 = (n-1)u_3$$

En portant ces valeurs de u_1 et de u_2 dans les deux équations (8) on trouve

$$f g_n = \frac{r_0}{2\mu\lambda}\{(n-3)A - 4\lambda\}u_3,$$

(14) $$g f_n = \frac{q_0}{2\mu\lambda}\{(n-3)B - 4\lambda\}u_3,$$

$$h_n = \frac{r_0}{2\mu\lambda}\{(n-3)C - 4\lambda\}u_3.$$

~~Multiplions~~ Posons maintenant

(15) $k_0 = A+B+C$, $k_1 = BC+CA+AB$, $k_2 = ABC$

multiplions chacune des équations (9) respectivement par BCp_0, CAq_0, ABr_0 et faisons en la somme

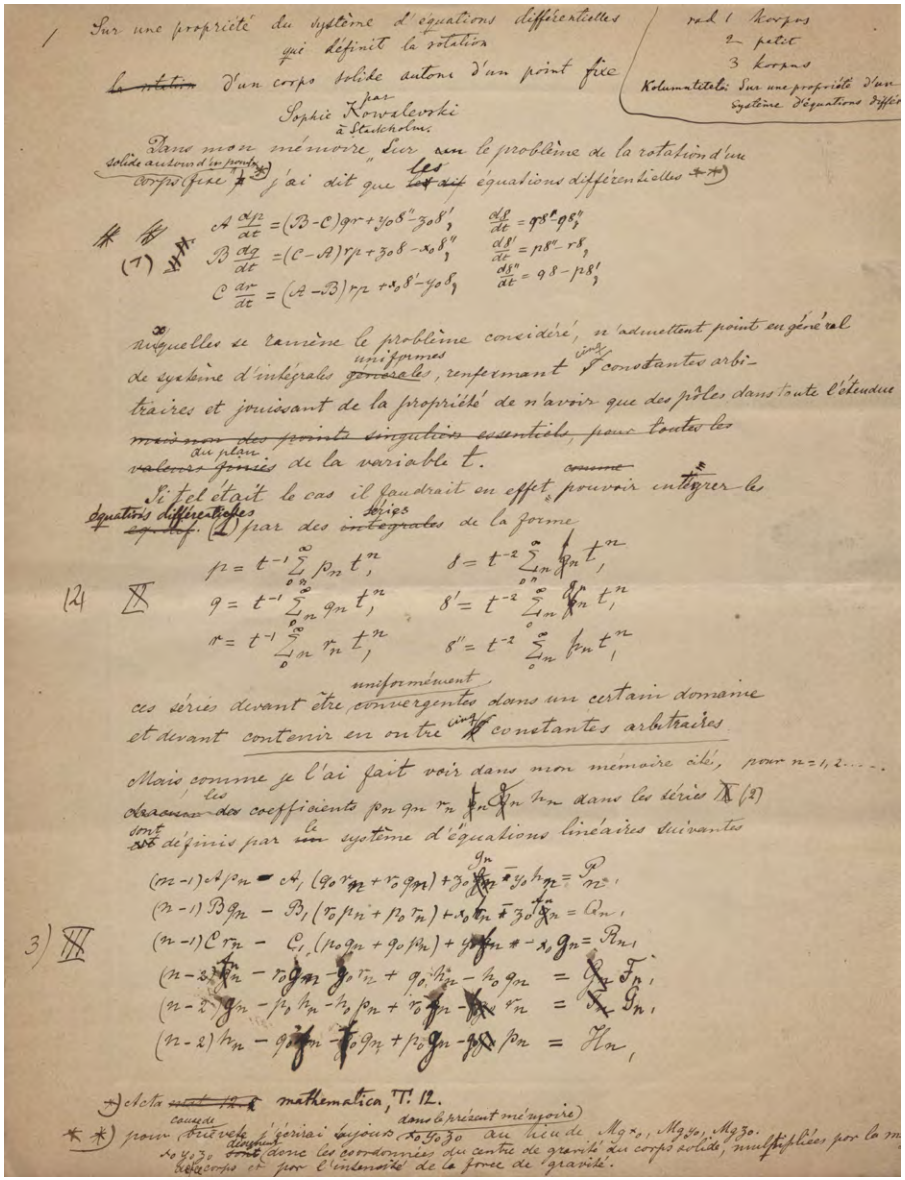
(16) $k_2(n-1)u_1 - \sum(ABC_1 + ACB_1)q_0 r_0 p_n = \sum' x_0 (ABr_0 g_n - ACp_0 h_n)$

physical intuition to regard time as a complex variable, at least for purposes of mathematical analysis …

"The necessary steps were taken by Jacobi, who had discovered the double-periodicity of the inverse functions and investigated them as functions of a complex variable. Jacobi's researches on elliptic functions led him to the discovery that such functions are best represented as quotients of theta functions. Jacobi applied these theta functions to represent the elliptic functions which arise in the Euler case and produced some very elegant formulas to represent the motion. This work, published only two years before Jacobi's death in 1851, clearly marked the beginning of a new epoch in the study of the rotation problem. For the first time since the equations of motion had been agreed upon a genuinely new tool – theta functions – was available for the study of the problem. Jacobi's death prevented him from making the study himself. To encourage others to take up the work the Prussian Academy of Sciences posed the following problem for competition in July 1852: "To integrate the differential equations of a body rotating about a fixed point under the influence of gravity alone. All quantities necessary to express the motion to be represented explicitly as functions of time using uniformly progressing [converging] series" …

"Despite the interesting nature of the problem and the opportunity to use the new techniques discovered by Jacobi (not to mention the prize of 100 ducats), no one entered the competition. It is difficult to believe that no one worked on the problem, however. The problem is simply very difficult. It was not forgotten and was a prominent unsolved problem when Kovalevskaya became a student of Weierstrass fifteen years later.

"Like her interest in differential equations, Kovalevskaya's interest in the Euler equations was constant from her days as a student, throughout her career. She



Sur une propriété du système d'équations différentielles qui définit la rotation d'un corps solide autour d'un point fixe

par Sophie Kowalevski à Stockholm

Dans mon mémoire sur le problème de la rotation d'un corps solide autour d'un point fixe, j'ai dit que les équations différentielles

$$A\frac{dp}{dt} = (B-C)qr + y_0\beta'' - z_0\beta', \qquad \frac{d\beta}{dt} = q\beta'' - r\beta'$$
$$B\frac{dq}{dt} = (C-A)rp + z_0\beta - x_0\beta'', \qquad \frac{d\beta'}{dt} = p\beta'' - r\beta,$$
$$C\frac{dr}{dt} = (A-B)pq + x_0\beta' - y_0\beta, \qquad \frac{d\beta''}{dt} = q\beta - p\beta',$$

auxquelles se ramène le problème considéré, n'admettent point en général de système d'intégrales uniformes, renfermant n constantes arbitraires et jouissant de la propriété de n'avoir que des pôles dans toute l'étendue du plan de la variable t.

Si tel était le cas il faudrait en effet pouvoir intégrer les équations différentielles par des séries de la forme

$$p = t^{-1}\sum_0^\infty p_n t^n, \qquad \beta = t^{-2}\sum_0^\infty q_n t^n,$$
$$q = t^{-1}\sum_0^\infty q_n t^n, \qquad \beta' = t^{-2}\sum_0^\infty \beta_n t^n,$$
$$r = t^{-1}\sum_0^\infty r_n t^n, \qquad \beta'' = t^{-2}\sum_0^\infty \beta_n t^n,$$

ces séries devant être uniformément convergentes dans un certain domaine et devant contenir n constantes arbitraires.

Mais comme je l'ai fait voir dans mon mémoire cité, pour $n = 1, 2, \ldots$ les coefficients $p_n\ q_n\ r_n\ \beta_n\ \beta'_n\ \beta''_n$ dans les séries (2) sont définis par un système d'équations linéaires suivantes

$$(n-1)Ap_n - A_1(q_0r_n + r_0q_n) + y_0\beta_n - z_0\beta'_n = P_n,$$
$$(n-1)Bq_n - B_1(r_0p_n + p_0r_n) + z_0\beta_n - x_0\beta''_n = Q_n,$$
$$(n-1)Cr_n - C_1(p_0q_n + q_0p_n) + x_0\beta'_n - y_0\beta_n = R_n,$$
$$(n-2)\beta_n - r_0q_n + q_0r_n + p_0\beta'_n - \beta_0p_n = S_n,$$
$$(n-2)\beta'_n - p_0\beta''_n + r_0\beta_n - \beta_0q_n = T_n,$$
$$(n-2)\beta''_n - q_0\beta_n + p_0\beta'_n - \beta_0r_n = U_n,$$

*) Acta mathematica, T. 12.

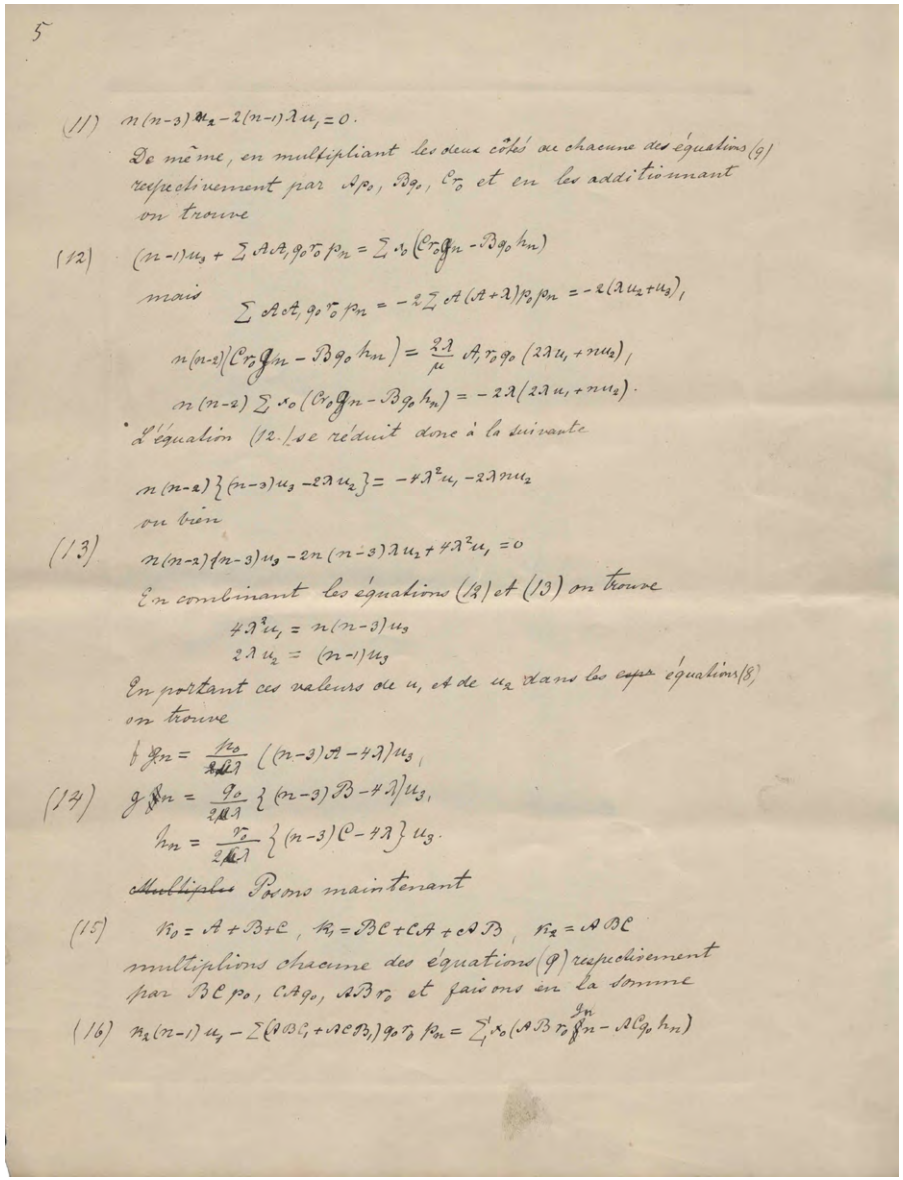
**) pour trouver j'écrirai toujours $x_0y_0z_0$ au lieu de Mgx_0, Mgy_0, Mgz_0 dans le présent mémoire) dans lequel $x_0y_0z_0$ sont les coordonnées du centre de gravité du corps solide, multipliées par la masse du corps et par l'intensité de la force de gravité.

studied it without much success while she was a student of Weierstrass, and then found her interest rekindled at a rather inopportune time, in 1881, when she was trying to solve the Lamé equations … The step she took at this time, however, was prophetically close to the solution she ultimately achieved. She was already considering the use of theta functions for certain values of the parameters … Kovalevskaya's formulation of the problem led her to a physically weird special case that she knew was the only remaining case in which the solutions might be meromorphic functions of the time for all possible initial values …

"The next mention of Kovalevskaya's work in a dated letter occurs in June 1886, when Hermite refers to "your beautiful discovery on rotation" … [but] there is an undated letter addressed to "Cher Monsieur", in which the discovery is communicated. "Cher Monsieur" is probably Mittag-Leffler … in this letter Kovalevskaya states the Euler equations and says that she has succeeded in integrating them in a new case (now known as the Kovalevskaya case) and, "I can show that these three cases [Euler, Lagrange, Kovalevskaya] are the only ones in which the general integral is a single-valued analytic function having no singularities but poles for finite values of *t*" …

"Judging from the note just quoted, we see that Kovalevskaya's work had progressed to such a point that she knew when the equations could have single-valued solutions and when they could not … but the harder part of her work still lay ahead: to find the explicit solution in the case she described (the Kovalevskaya case). The work on this part (essentially the last fifty pages of her memoirs on the subject) took more than two years of intense concentration …

"Kovalevskaya's description of her work is as follows. She finally reduced the problem to quadratures by Herculean labor, involving copious combinatorial tricks and heavy use of some transformations used by Weierstrass in his lectures


5

(11) $n(n-3)u_2 - 2(n-1)\lambda u_1 = 0.$

De même, en multipliant les deux côtés de chacune des équations (9) respectivement par $A\rho_0$, $B\rho_0$, $C\tau_0$ et en les additionnant on trouve

(12) $(n-1)u_3 + \sum A\alpha_1 p_0 \tau_0 p_n = \sum x_0 (C\tau_0 q_n - B p_0 h_n)$

mais

$$\sum A\alpha_1 p_0 \tau_0 p_n = -2\sum A(A+\lambda)p_0 p_n = -2(\lambda u_2 + u_3),$$

$$n(n-2)(C\tau_0 q_n - B p_0 h_n) = \frac{2\lambda}{\mu} A\tau_0 p_0 (2\lambda u_1 + n u_2),$$

$$n(n-2)\sum x_0 (C\tau_0 q_n - B p_0 h_n) = -2\lambda(2\lambda u_1 + n u_2).$$

L'équation (12) se réduit donc à la suivante

$$n(n-2)\{(n-3)u_3 - 2\lambda u_2\} = -4\lambda^2 u_1 - 2\lambda n u_2$$

ou bien

(13) $$n(n-2)(n-3)u_3 - 2n(n-3)\lambda u_2 + 4\lambda^2 u_1 = 0$$

En combinant les équations (12) et (13) on trouve

$$4\lambda^2 u_1 = n(n-3)u_3$$
$$2\lambda u_2 = (n-1)u_3$$

En portant ces valeurs de u_1 et de u_2 dans les deux équations (8) on trouve

$$f q_n = \frac{\mu_0}{2\lambda}\{(n-3)A - 4\lambda\}u_3,$$

(14) $$g p_n = \frac{q_0}{2\lambda}\{(n-3)B - 4\lambda\}u_3,$$

$$h_n = \frac{r_0}{2\lambda}\{(n-3)C - 4\lambda\}u_3.$$

~~Multiplies~~ Posons maintenant

(15) $$k_0 = A+B+C,\quad k_1 = BC+CA+AB,\quad k_2 = ABC$$

multiplions chacune des équations (9) respectivement par $BC\rho_0$, $CA q_0$, $AB r_0$ et faisons en la somme

(16) $$k_2(n-1)u_1 - \sum(AB\gamma_1 + AC\beta_1)q_0 r_0 p_n = \sum' x_0 (AB r_0 g_n - AC p_0 h_n)$$

on applications of elliptic functions. She was then faced with the problem of integrating some elliptic integrals of the first kind. As she remarked to Mittag-Leffler, "Now you will probably understand that these formulas led me to believe that I was really dealing with elliptic functions here. I searched and searched, but to no avail; [then] to my great joy and wonder, I discovered that every symmetric function of [the variables] is an *ultraelliptic function of time*, i.e. can be expressed by rational functions of quotients [of theta functions of two variables whose arguments are linear functions of time]."

"She was able to write the system of integrals as a complete system of integrals of the first kind in genus 2, i.e. involving the square root of a fifth-degree polynomial in the denominator of the integrand and a linear function in the numerator. At this glorious point she knew she had the result she wanted. Weierstrass's advice, when it came, contained only some minor technical points on how to simplify the formulas. All that Kovalevskaya needed was time to prepare a clean manuscript, and the Bordin prize was assured." (Cooke, pp. 37-45).

"Kovalevskaya was vividly criticized by the Russian academician Markov on a minor point related to the nonexistence of other cases where the equations could be integrated. This problem was settled by Lyapunov, who proved in 1894 that Kovalevskaya's claim was correct" (Goriely, p. 8).

Kovalevskaya (1850-91) "was the daughter of Vasily Korvin-Krukovsky, an artillery general, and Yelizaveta Shubert, both well-educated members of the Russian nobility … In *Recollections of Childhood* (and the fictionalized version, *The Sisters Rajevsky*), Sonya Kovalevsky vividly described her early life: her education by a governess of English extraction; the life at Palabino (the Krukovsky country estate); the subsequent move to St. Petersburg; the family social circle, which included Dostoevsky; and the general's dissatisfaction with the "new" ideas of his daughters. The story ends with her fourteenth year. At that time the temporary wallpaper in one of the children's rooms at Palabino consisted of the pages of a text from her father's schooldays, namely, Ostrogradsky's lithographed lecture notes on differential and integral calculus. Study of that novel wall-covering provided Sonya with her introduction to the calculus. In 1867 she took a more rigorous course under the tutelage of Aleksandr N. Strannolyubsky, mathematics professor at the naval academy in St. Petersburg, who immediately recognized her great potential as a mathematician.

"Sonya and her sister Anyuta were part of a young people's movement to promote the emancipation of women in Russia. A favorite method of escaping from bondage was to arrange a marriage of convenience which would make it possible to study at a foreign university. Thus, at age eighteen, Sonya contracted such a nominal marriage with Vladimir Kovalevsky, a young paleontologist, whose brother Aleksandr was already a renowned zoologist at the University of Odessa. In 1869 the couple went to Heidelberg, where Vladimir studied geology and Sonya took courses with Kirchhoff, Helmholtz, Koenigsberger, and du Bois-Reymond. In 1871 she left for Berlin, where she studied with Weierstrass, and Vladimir went to Jena to obtain his doctorate. As a woman, she could not be admitted to university lectures; consequently Weierstrass tutored her privately during the next four years. By 1874 she had completed three research papers on partial differential equations, Abelian integrals, and Saturn's rings. The first of these was a remarkable contribution, and all three qualified her for the doctorate in absentia from the University of Göttingen.

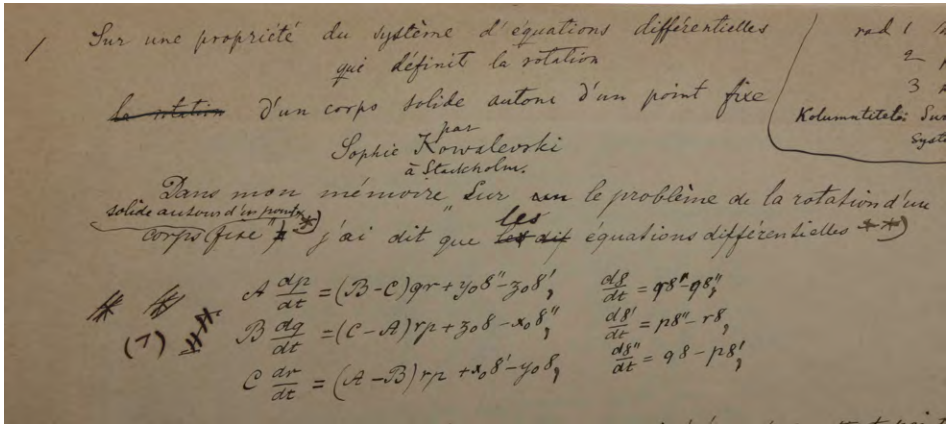
"In spite of Kovalevsky's doctorate and strong letters of recommendation from Weierstrass, she was unable to obtain an academic position anywhere in Europe. Hence she returned to Russia where she was reunited with her husband. The couple's only child, a daughter, "Foufie," was born in 1878. When Vladimir's

lectureship at Moscow University failed to materialize, he and Sonya worked at odd jobs, then engaged in business and real estate ventures. An unscrupulous company involved Vladimir in shady speculations that led to his disgrace and suicide in 1883. His widow turned to Weierstrass for assistance and, through the efforts of the Swedish analyst Gösta Mittag-Leffler, one of Weierstrass' most distinguished disciples, Sonya Kovalevsky vas appointed to a lectureship in mathematics at the University of Stockholm. In 1889 Mittag-Leffler secured a life professorship for her.

"During Kovalevsky's years at Stockholm she carried on her most important research and taught courses (in the spirit of Weierstrass) on the newest and most advanced topics in analysis. She completed research already begun on the subject of the propagation of light in a crystalline medium. Her memoir, *On the Rotation of a Solid Body About a Fixed Point* (1888), won the Prix Bordin of the French Academy of Sciences. The judges considered the paper so exceptional that they raised the prize from 3,000 to 5,000 francs. Her subsequent research on the same subject won the prize from the Swedish Academy of Sciences in 1889. At the end of that year she was elected to membership in the Russian Academy of Sciences. Less than two years later, at the height of her career, she died of influenza complicated by pneumonia …

"An unusual aspect of Sonya Kovalevsky's life was that, along with her scientific work, she attempted a simultaneous career in literature. The titles of some of her novels are indicative of their subject matter: *The University Lecturer, The Nihilist* (unfinished), The *Woman Nihilist*, and, finally, *A Story of the Riviera*. In 1887 she collaborated with her good friend and biographer, Mittag-Leffier's sister, Anne Charlotte Leffler-Edgren (later Duchess of Cajanello), in writing a drama, *The Struggle for Happiness*, which was favorably received when it was produced at the Korsh Theater in Moscow. She also wrote a critical commentary on George Eliot, whom she and her husband had visited on a holiday trip to England in 1869" (DSB).

Cooke, 'Sonya Kovalevskaya's place in nineteenth century mathematics,' pp. 17-52 in *The Legacy of Sonya Kovalevskaya* (Linda Keen, ed.), American Mathematical Society, 1987. Goriely, 'A brief history of Kovalevskaya exponents and modern developments,' *Regular and Chaotic Dynamics* 5 (1999), pp. 3-15. Rappaport, 'S. Kovalevsky: a mathematical lesson,' *American Mathematical Monthly* 88 (1981), pp. 564-574.



SECOND ONLY TO THE 'PRINCIPIA'

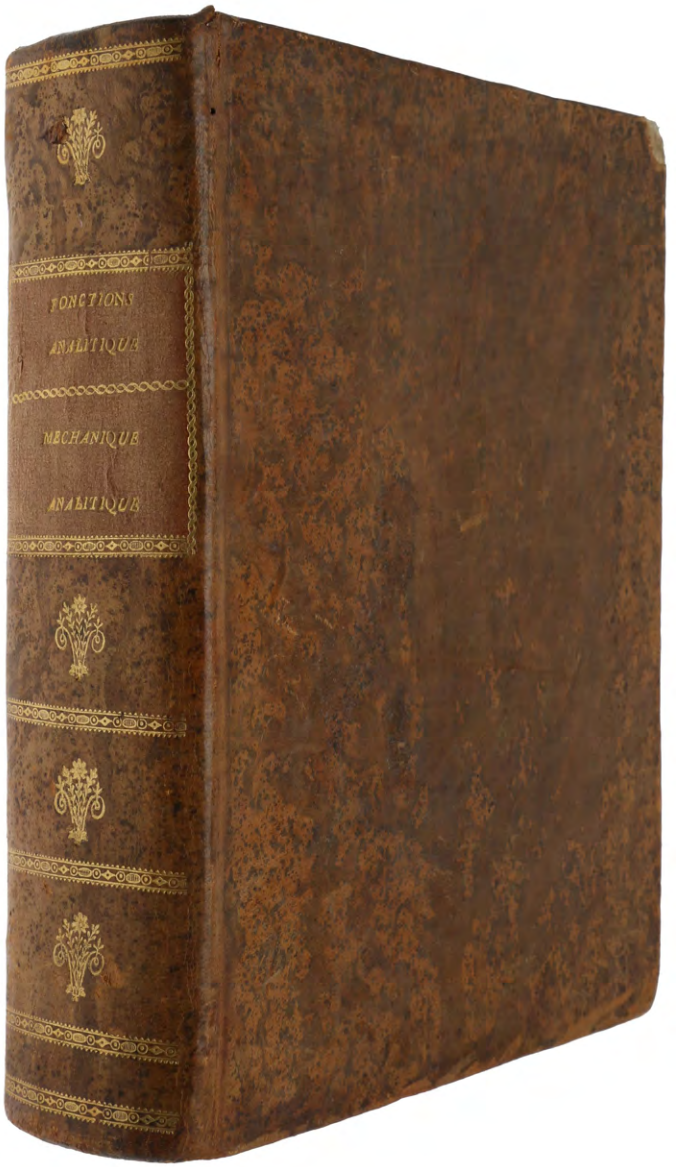
[Horblit 61; Evans 10; Dibner 112; Sparrow 120; Norman 1257]

LAGRANGE, Joseph Louis de. *Méchanique analitique. Paris: Veuve Desaint, 1788.* [Bound with:] *Théorie des fonctions analytiques.* Paris: de l'Imprimerie de la République, An V [1797].

$15,000

4to (248 x 200 mm), [Méchanique:] [i-v], vi-xii, [1], 2-512; [Théorie:] pp. [4], [i], ii-viii, [1], 2-276. Contemporary full calf, corners with slight wear, but very good and unrestored. Internally very fine and fresh.

An exceptional volume, in a fine contemporary binding, containing the first edition of Lagrange's masterpiece, the *Méchanique*, "one of the outstanding landmarks in the history of both mathematics and mechanics" (Sarton, p. 470) and "perhaps the most beautiful mathematical treatise in existence," together with the corrected second printing of the *Théorie*, containing Lagrange's formulation of calculus in terms of infinite series, which provided the basis for Augustin-Louis Cauchy's development of complex function theory in the first decades of the next century. The *Méchanique* contains the discovery of the general equations of motion, the first epochal contribution to theoretical dynamics after Newton's *Principia*" (Evans). "Lagrange's masterpiece, the *Méchanique Analitique* (Paris, 1788), laid the foundations of modern mechanics, and occupies a place in the history of the subject second only to that of Newton's *Principia*" (Wolf). "The year 1797 … saw the appearance of the famous work of Lagrange, *Théorie des fonctions analytiques* … This book developed with care and completeness the characteristic definition and method in terms of 'fonctions dérivées,' based upon Taylor's series,



which Lagrange had proposed in 1772 … Lagrange's *Théorie* des fonctions was only one, but by far the most important, of many attempts made about this time to furnish the calculus with a basis which would logically modify or supplant those given in terms of limits and infinitesimals" (Cajori).

"With the appearance of the *Mécanique Analytique* in 1788, Lagrange proposed to reduce the theory of mechanics and the art of solving problems in that field to general formulas, the mere development of which would yield all the equations necessary for the solution of every problem ... [it] united and presented from a single point of view the various principles of mechanics, demonstrated their connection and mutual dependence, and made it possible to judge their validity and scope. It is divided into two parts, statics and dynamics, each of which treats solid bodies and fluids separately. There are no diagrams. The methods presented require only analytic operations, subordinated to a regular and uniform development. Each of the four sections begins with a historical account which is a model of the kind." (DSB).

"In [*Méchanique Analitique*] he lays down the law of virtual work, and from that one fundamental principle, by the aid of the calculus of variations, deduces the whole of mechanics, both of solids and fluids. The object of the book is to show that the subject is implicitly included in a single principle, and to give general formulae from which any particular result can be obtained. The method of generalized co-ordinates by which he obtained this result is perhaps the most brilliant result of his analysis. Instead of following the motion of each individual part of a material system, as D'Alembert and Euler had done, he showed that, if we determine its configuration by a sufficient number of variables whose number is the same as that of the degrees of freedom possessed by the system, then the kinetic and potential energies of the system can be expressed in terms of those variables, and the differential equations of motion thence deduced by simple differentiation … Amongst other minor theorems here given I may mention the proposition that the kinetic energy imparted by the given impulses to a material system under given constraints is a maximum, and the principle of least action. All the analysis is so elegant that Sir William Rowan Hamilton said the work could only be described as a 'scientific poem'" (Ball, *History of Mathematics*).

The *Méchanique analitique* "was certainly regarded as the most important unification of rational mechanics at the turn of the 18th century and as its 'crowning' (Dugas). This achievement of unification and the abstract-formal nature of the work, physically reflected in immediate applications, earned the extravagant praise of Ernst Mach: 'Lagrange […] strove to dispose of all necessary considerations once and for all, including as many as possible in one formula. Every case that arises can be dealt with according to a very simple, symmetric and clearly arranged scheme […] Lagrangian mechanics is a magnificent achievement in respect of the economy of thought' (Mach, *Die Mechanik in ihrer Entwicklung*, 1933, 445)" (Pulte, p. 220).

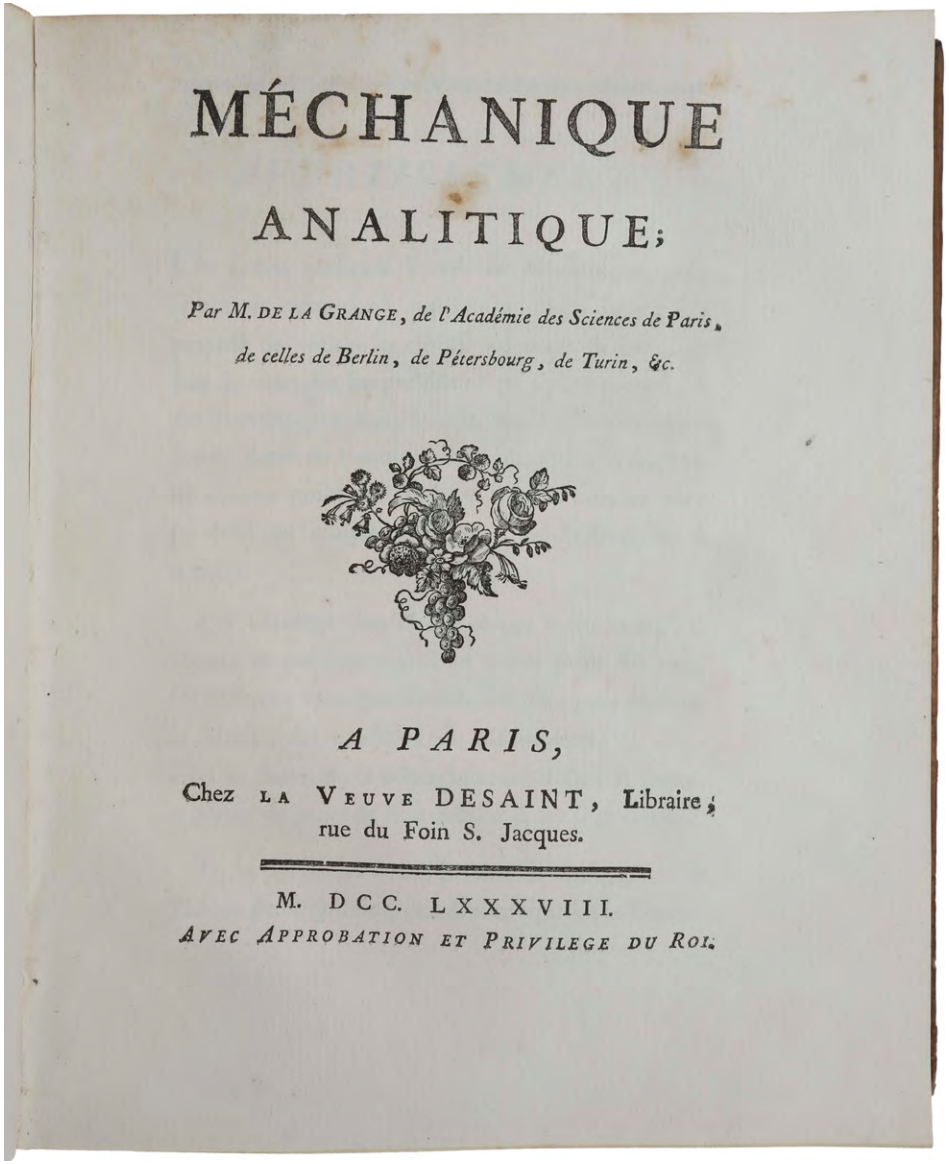
"Lagrange produced the *Méchanique analitique* during his time in Berlin. He referred as early as 1756 and 1759 to an almost complete textbook of mechanics, now lost; a later draft first saw the light of day in 1764. But it was not until the end of 1782 that Lagrange seems to have put the textbook into an essentially complete form, and the publication of the book was delayed a further six years" (Pulte, p. 209).

"By 1790 a critical attitude had developed both within mathematics and within general scientific culture. As early as 1734 Bishop George Berkeley in his work *The Analyst* had called attention to what he perceived as logical weaknesses in the reasonings of the calculus arising from the employment of infinitely small quantities. Although his critique was somewhat lacking in mathematical cogency, it at least stimulated writers in Britain and the Continent to explain more carefully

the basic rules of the calculus. In the 1780s a growing interest in the foundations of analysis was reflected in the decisions of the academies of Berlin and Saint Petersburg to devote prize competitions to the metaphysics of the calculus and the nature of the infinite. In philosophy Immanuel Kant's *Kritik der reinen Vernunft* (1787) set forth a penetrating study of mathematical knowledge and initiated a new critical movement in the philosophy of science …

"The full title of the *Théorie* explains its purpose: 'Theory of analytical functions containing the principles of the differential calculus disengaged from all consideration of infinitesimals, vanishing limits or fluxions and reduced to the algebraic analysis of finite quantities'. Lagrange's goal was to develop an algebraic basis for the calculus that made no reference to infinitely small magnitudes or intuitive geometrical and mechanical notions …

"In Lagrange's conception of analysis, one is given a universe of functions, each expressed by a formula $y = f(x)$ and consisting of a single analytical expression involving variables, constants and algebraic and transcendental operations. During the 18th century such functions were called continuous, and the *Théorie* is devoted exclusively to functions that are continuous in this sense. (Mathematicians were aware of the possibility of other sorts of functions, but alternate definitions never caught on.) Such functions were naturally suited to the usual application of calculus to geometrical curves. In studying the curve the calculus is concerned with the connection between local behaviour, or slope, and global behaviour, or area and path-length. If the curve is represented by a function $y = f(x)$ given by a single analytical expression then the relation between x and y is permanently established in the form of f. Local and global behaviour become identified in this functional relation.



MÉCHANIQUE

ANALITIQUE;

Par M. de la Grange, de l'Académie des Sciences de Paris, de celles de Berlin, de Pétersbourg, de Turin, &c.

A PARIS,

Chez la Veuve DESAINT, Libraire, rue du Foin S. Jacques.

M. DCC. LXXXVIII.

Avec Approbation et Privilege du Roi.

"It is also necessary to call attention to the place of infinite series in Lagrange's system of analysis. Each function has the property that it may be expanded as [a] power series. Nevertheless, an infinite series as such is never defined to be a function. The logical concept of an infinite series as a functional object defined *a priori* with respect to some criterion such as convergence or summability was foreign to 18th-century analysis. Series expansions were understood as a tool for obtaining the derivative, or a way of representing functions that were already given.

"For the 18th-century analyst, functions are things that are given 'out there', in the same way that the natural scientist studies plants, insects or minerals, given in nature. As a general rule, such functions are very well-behaved, except possibly at a few isolated exceptional values. It is unhelpful to view Lagrange's theory in terms of modern concepts (arithmetical continuity, differentiability, continuity of derivatives and so on), because he did not understand the subject in this way …

"Part One of the *Théorie* begins with some historical matters and examines the basic expansion of a function as a Taylor power series. There is considerable discussion of values where the expansion may fail, and a derivation of such well-known results as l'Hôpital's rule. Lagrange then turned to methods of approximation and an estimation of the remainder in the Taylor series, followed by a study of differential equations, singular solutions and series methods, as well as multi-variable calculus and partial differential equations. He outlined and supplemented topics explored in some detail in memoirs of the 1760s and 1770s.

"Part Two on geometry opens with an investigation of the geometry of curves. Here Lagrange examined in detail the properties that must hold at a point where two curves come into contact—the relationships between their tangents and osculating circles. Corresponding questions concerning surfaces are also investigated, and Lagrange referred to Gaspard Monge's memoirs on this subject in the *Académie des Sciences*. He derived some standard results on the quadrature and rectification of curves. The theory of maxima and minima in the ordinary calculus, a topic Lagrange suggested could be understood independently of geometry as part of analysis, is taken up. Also covered are basic results in the calculus of variations, including an important theorem of Adrien-Marie Legendre in the theory of sufficiency …

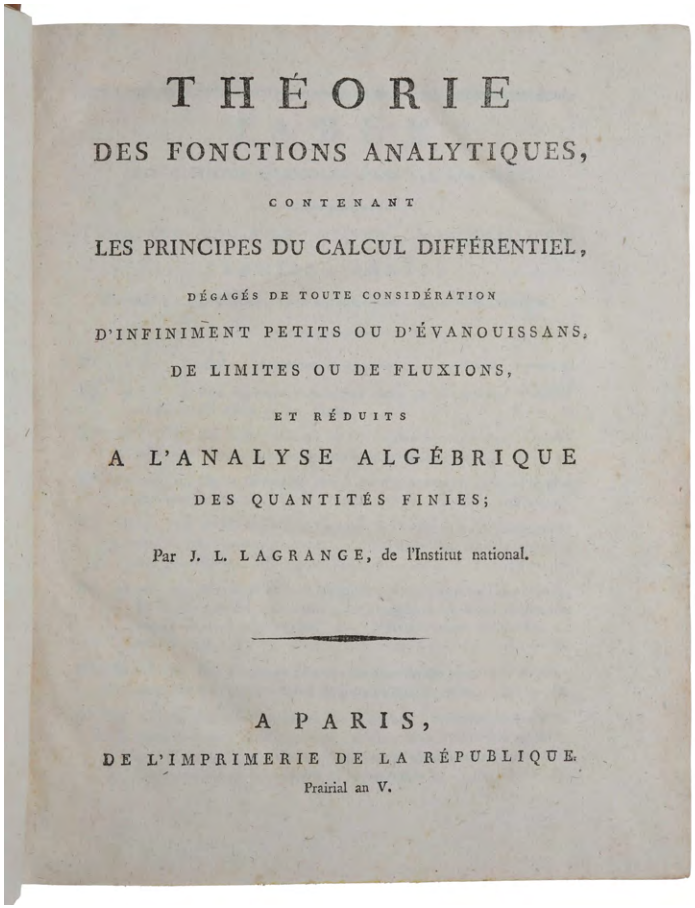
"The third part on dynamics is somewhat anticlimactic, given the publication nine years earlier of his major work *Méchanique analitique*. In this part Lagrange presented a rather kinematically-oriented investigation of particle dynamics, including a detailed discussion of the Newtonian problem of motion in a resisting medium. He also derived the standard conservation laws of momentum, angular momentum and live forces. The book closes with an examination of the equation of live forces as it applies to problems of elastic impact and machine performance.

"… some of the major original contributions of this work: the formulation of a coherent foundation for analysis; Lagrange's conception of theorem-proving in analysis; his derivation of what is today called the Lagrange remainder in the Taylor expansion of a function; his formulation of the multiplier rule in the calculus and calculus of variations; and his account of sufficiency questions in the calculus of variations" (Fraser, p. 260).

The Norman catalogue identifies two versions of the *Théorie des fonctions analytiques*, Version A with 276 pages, and Version B with 277, and states that no priority has been established. Craig Fraser (*Landmark Writings*, p. 258) states that version B is the first edition but does not mention version A. Ivor Grattan-Guinness (*Convolutions*, p. 129) states that Version A is the first edition. In our opinion Version B is certainly the true first edition, and Version A is actually the

second edition (not just the second issue). The two versions are not merely different issues: the last several pages of text (and also a few earlier pages) are completely reset. In Version A the text ends on p. 276 and the following page is blank (reader.digitale-sammlungen.de/de/fs1/object/display/bsb10053590_00294.html); in Version B the last 6 lines of the main text appear on p. 277 (these lines are identical to the last 6 lines of Version A), followed by a list of errata on the same page and the imprint 'Par les soins de P. D. Duboy-Laverne, directeur de l'Imprimerie de la République' on the following unnumbered page (see goo.gl/db7V6z). A further difference is that in Version A the verso of the half-title is blank; in Version B it carries the text 'Se trouve à Paris, Chez Bernard, libraire, quai des Augustins, no. 37'. The priority of Version B is revealed by the 8-page index that appears at the beginning of both versions; *in both versions this index refers to the 'Conclusion' on pp. 276-277*. Clearly, Version A cannot have been issued with an index that is appropriate only to a version printed later; thus, Version B must be first. Its priority is confirmed by the fact that, in Version A the errata listed in Version B have been corrected. As Norman notes, there is a third version of the *Théorie* which forms Vol. III of the ninth cahier of the *Journal de l'Ecole Polytechnique*, published in 1801. Norman states that this journal issue is Version A, but in fact it is Version B (see goo.gl/g5RTqV). The fact that the uncorrected Version B was used for the journal issue suggests that Version A was not only issued later than Version B, but after the journal issue, i.e., no earlier than 1801. The inscription of this copy indicates that copies of Version B were still available in 1804.

[*Méchanique*:] Grolier/Horblit 61; Evans 10; Dibner 112; Sparrow 120; Norman 1257; Wolf II, 69. Pulte, 'Joseph Louis Lagrange, Méchanique analitique (1788),' Chapter 16 in *Landmark Writings in Western Mathematics 1640–1940*, Grattan-Guinness (ed.). Sarton, 'Lagrange's personality,' *Proceedings of the American Philosophical Society* (1944), pp. 457-98. [*Fonctions analytiques*:] Norman 1258; Barchas 1198; Honeyman 1881; Stanitz 250. Fraser, 'Joseph Louis Lagrange, *Théorie des fonctions analytiques* (1797),' Chapter 19 in *Landmark Writings*. Grattan-Guinness, *Convolutions in French Mathematics 1800-1840*, 1990 (see especially pp. 129-133).



THÉORIE

DES FONCTIONS ANALYTIQUES,

CONTENANT

LES PRINCIPES DU CALCUL DIFFÉRENTIEL,

DÉGAGÉS DE TOUTE CONSIDÉRATION

D'INFINIMENT PETITS OU D'ÉVANOUISSANS,

DE LIMITES OU DE FLUXIONS,

ET RÉDUITS

A L'ANALYSE ALGÉBRIQUE

DES QUANTITÉS FINIES;

Par J. L. LAGRANGE, de l'Institut national.

A PARIS,

DE L'IMPRIMERIE DE LA RÉPUBLIQUE.

Prairial an V.

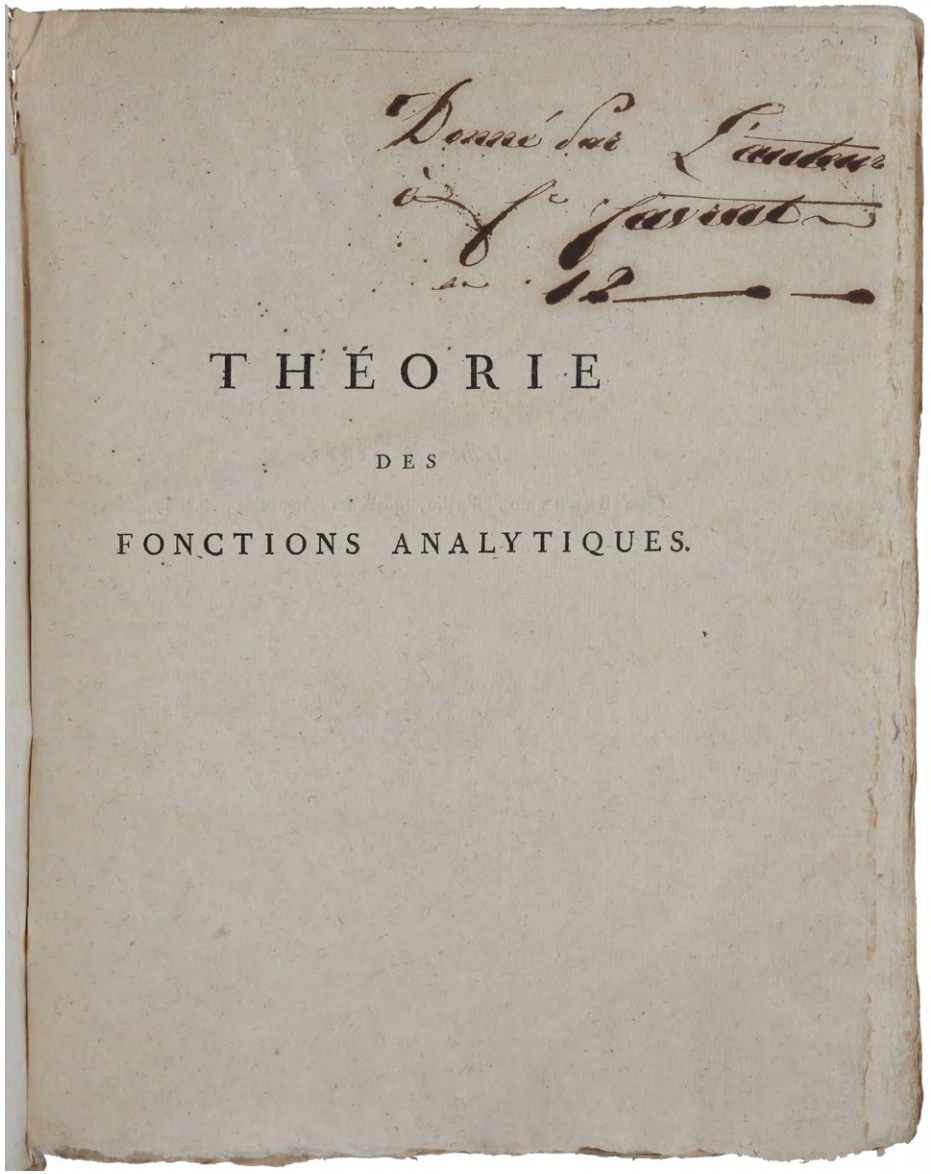
PRESENTATION COPY IN THE ORIGINAL BOARDS

LAGRANGE, Joseph Louis de. *Théorie des fonctions analytiques contenant les principes de calcul différentiel, dégagés de toute considération d'infiniment petits, d'évanouissants, de limites et de fluxions et réduits à l'analyse algébrique des quantités finies.* Paris: de l'Imprimerie de la République, An V [1797].

$7,500

4to (277 x 213), pp [4] [i] ii-viii, [1] 2-277 [3], uncut and unopned in original boards, spine with some wear, hinges with cracks (still firm), half-title incribed. A fine copy in its original state.

First edition, inscribed presentation copy from the author, of Lagrange's formulation of calculus in terms of infinite series, which provided the basis for Augustin-Louis Cauchy's development of complex function theory in the first decades of the next century. The full title of the work indicates "the author's rather utopian program … In it Lagrange intended to show that power series expansions are sufficient to provide differential calculus with a solid foundation. Today mathematicians are partially returning to this conception in treating the formal calculus of series. As early as 1812, however, J. M. H. Wronski objected to Lagrange's claims. The subsequent opposition of Cauchy was more effective. Nevertheless, … completed by convergence considerations, it dominated the study of the functions of a complex variable throughout the nineteenth century" (DSB). "The year 1797 … saw the appearance of the famous work of Lagrange, *Théorie des fonctions analytiques* … This book developed with care and completeness the characteristic definition and method in terms of 'fonctions dérivées,' based upon


Donné par l'auteur
THÉORIE
DES
FONCTIONS ANALYTIQUES.

Taylor's series, which Lagrange had proposed in 1772 … Lagrange's *Théorie des fonctions* was only one, but by far the most important, of many attempts made about this time to furnish the calculus with a basis which would logically modify or supplant those given in terms of limits and infinitesimals" (Cajori). "Lagrange's view of the calculus was influential, especially from the authoritative presentation in his *Théorie des fonctions analytiques* of 1797. Here functions were treated as algebraic formulae: in particular, the value of "any" function $f(x + h)$ was held to be expressible as a power-series in the increment h on the value x. The 'derived functions of f at x were then defined in terms of the coefficients of h in the expansion, and were claimed to be calculable by purely algebraic means without recourse to limits or infinitesimals. The integral was defined as the inverse of the first derived function" (Grattan-Guinness, p. 37).

Provenance: Inscription on half-title 'Donné par l'auteur à S. Favart [?] an 12,' probably in a secretarial hand.

"By 1790 a critical attitude had developed both within mathematics and within general scientific culture. As early as 1734 Bishop George Berkeley in his work *The Analyst* had called attention to what he perceived as logical weaknesses in the reasonings of the calculus arising from the employment of infinitely small quantities. Although his critique was somewhat lacking in mathematical cogency, it at least stimulated writers in Britain and the Continent to explain more carefully the basic rules of the calculus. In the 1780s a growing interest in the foundations of analysis was reflected in the decisions of the academies of Berlin and Saint Petersburg to devote prize competitions to the metaphysics of the calculus and the nature of the infinite. In philosophy Immanuel Kant's *Kritik der reinen Vernunft* (1787) set forth a penetrating study of mathematical knowledge and initiated a new critical movement in the philosophy of science …

"The full title of the *Théorie* explains its purpose: 'Theory of analytical functions containing the principles of the differential calculus disengaged from all consideration of infinitesimals, vanishing limits or fluxions and reduced to the algebraic analysis of finite quantities'. Lagrange's goal was to develop an algebraic basis for the calculus that made no reference to infinitely small magnitudes or intuitive geometrical and mechanical notions …

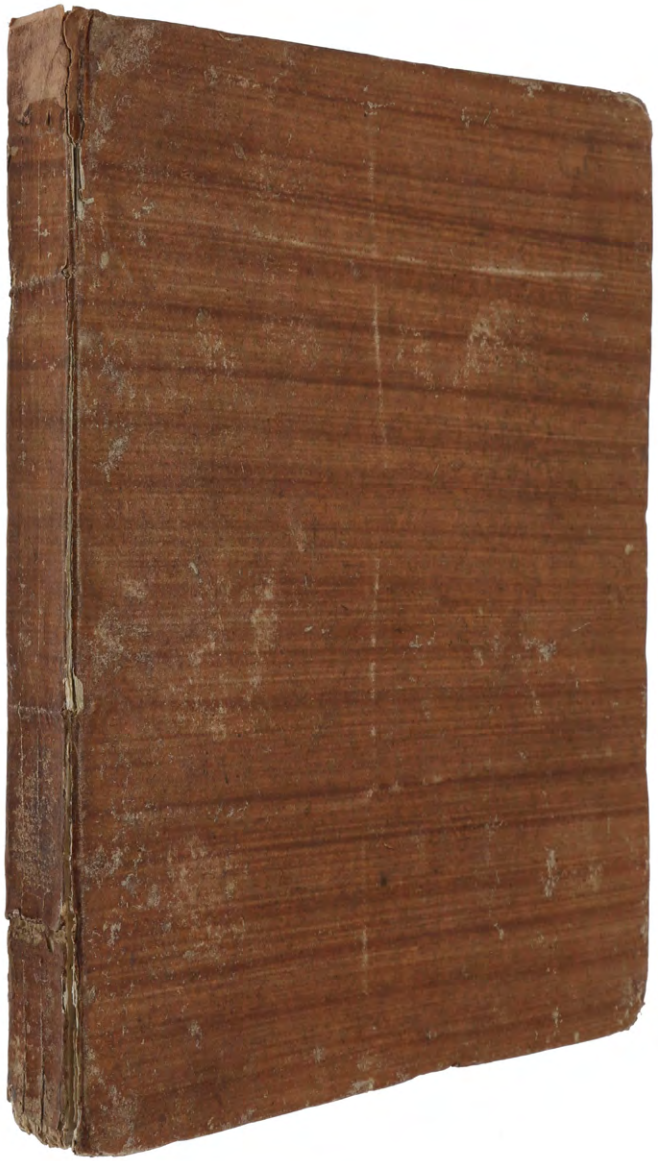
"In Lagrange's conception of analysis, one is given a universe of functions, each expressed by a formula $y = f(x)$ and consisting of a single analytical expression involving variables, constants and algebraic and transcendental operations. During the 18th century such functions were called continuous, and the *Théorie* is devoted exclusively to functions that are continuous in this sense. (Mathematicians were aware of the possibility of other sorts of functions, but alternate definitions never caught on.) Such functions were naturally suited to the usual application of calculus to geometrical curves. In studying the curve the calculus is concerned with the connection between local behaviour, or slope, and global behaviour, or area and path-length. If the curve is represented by a function $y = f(x)$ given by a single analytical expression then the relation between x and y is permanently established in the form of f. Local and global behaviour become identified in this functional relation.

"It is also necessary to call attention to the place of infinite series in Lagrange's system of analysis. Each function has the property that it may be expanded as [a] power series. Nevertheless, an infinite series as such is never defined to be a function. The logical concept of an infinite series as a functional object defined *a priori* with respect to some criterion such as convergence or summability was foreign to 18th-century analysis. Series expansions were understood as a tool for obtaining the derivative, or a way of representing functions that were already given.

"For the 18th-century analyst, functions are things that are given 'out there', in the same way that the natural scientist studies plants, insects or minerals, given in nature. As a general rule, such functions are very well-behaved, except possibly at a few isolated exceptional values. It is unhelpful to view Lagrange's theory in terms of modern concepts (arithmetical continuity, differentiability, continuity of derivatives and so on), because he did not understand the subject in this way …

"Part One of the *Théorie* begins with some historical matters and examines the basic expansion of a function as a Taylor power series. There is considerable discussion of values where the expansion may fail, and a derivation of such well-known results as l'Hôpital's rule. Lagrange then turned to methods of approximation and an estimation of the remainder in the Taylor series, followed by a study of differential equations, singular solutions and series methods, as well as multi-variable calculus and partial differential equations. He outlined and supplemented topics explored in some detail in memoirs of the 1760s and 1770s.

"Part Two on geometry opens with an investigation of the geometry of curves. Here Lagrange examined in detail the properties that must hold at a point where two curves come into contact—the relationships between their tangents and osculating circles. Corresponding questions concerning surfaces are also investigated, and Lagrange referred to Gaspard Monge's memoirs on this subject in the *Académie des Sciences*. He derived some standard results on the quadrature and rectification of curves. The theory of maxima and minima in the ordinary calculus, a topic Lagrange suggested could be understood independently of geometry as part of analysis, is taken up. Also covered are basic results in the calculus of variations, including an important theorem of Adrien-Marie Legendre in the theory of sufficiency …



"The third part on dynamics is somewhat anticlimactic, given the publication nine years earlier of his major work *Méchanique analitique*. In this part Lagrange presented a rather kinematically-oriented investigation of particle dynamics, including a detailed discussion of the Newtonian problem of motion in a resisting medium. He also derived the standard conservation laws of momentum, angular momentum and live forces. The book closes with an examination of the equation of live forces as it applies to problems of elastic impact and machine performance.

"… some of the major original contributions of this work: the formulation of a coherent foundation for analysis; Lagrange's conception of theorem-proving in analysis; his derivation of what is today called the Lagrange remainder in the Taylor expansion of a function; his formulation of the multiplier rule in the calculus and calculus of variations; and his account of sufficiency questions in the calculus of variations" (Fraser, pp. 259-261).

The Norman catalogue identifies two versions of this book, Version A with 276 pages, and Version B with 277, and states that no priority has been established. Craig Fraser (*Landmark Writings*, p. 258) states that version B is the first edition but does not mention version A. Ivor Grattan-Guinness (*Convolutions*, p. 129) states that Version A is the first edition. In our opinion Version B is certainly the true first edition, and Version A is actually the second edition (not just the second issue). The two versions are not merely different issues: the last several pages of text (and also a few earlier pages) are completely reset. In Version A the text ends on p. 276 and the following page is blank (reader.digitale-sammlungen.de/de/fs1/object/display/bsb10053590_00294.html); in Version B the last 6 lines of the main text appear on p. 277 (these lines are identical to the last 6 lines of Version A), followed by a list of errata on the same page and the imprint 'Par les soins de P. D. Duboy-Laverne, directeur de l'Imprimerie de la République' on the following unnumbered page (see goo.gl/db7V6z). A further difference is that in Version A the verso of the half-title is blank; in Version B it carries the text 'Se trouve à Paris, Chez Bernard, libraire, quai des Augustins, no. 37'. The priority of Version B is revealed by the 8-page index that appears at the beginning of both versions; in both versions this index refers to the 'Conclusion' on pp. 276-277. Clearly, Version A cannot have been issued with an index that is appropriate only to a version printed later; thus, Version B must be first. Its priority is confirmed by the fact that, in Version A the errata listed in Version B have been corrected. As Norman notes, there is a third version of the *Théorie* which forms Vol. III of the ninth cahier of the *Journal de l'Ecole Polytechnique*, published in 1801. Norman states that this journal issue is Version A, but in fact it is Version B (see gallica.bnf.fr/ark:/12148/bpt6k86263h/f1.image). The fact that the uncorrected Version B was used for the journal issue suggests that Version A was not only issued later than Version B, but after the journal issue, i.e., no earlier than 1801. The inscription of this copy indicates that copies of Version B were still available in 1804.

Norman 1258; Barchas 1198; Honeyman 1881; Stanitz 250. Fraser, 'Joseph Louis Lagrange, *Théorie des fonctions analytiques* (1797),' Chapter 19 in *Landmark Writings in Western Mathematics 1640-1940*, 2005. Grattan-Guinness, *Convolutions in French Mathematics* 1800-1840, 1990 (see especially pp. 129-133).

THE NEWTON OF FRANCE

LAPLACE, Pierre Simon, Marquis de. *Oeuvres* [*Traité de Mécanique Céleste; Exposition du Système du Monde; Théorie Analytique des Probabilités* (including, as the introduction, *Essai Philosophique sur les Probabilités*)]. Paris: Imprimerie Royale, 1843-1847.

$4,500

Seven vols., 4to. Vol. I [Traité de Mécanique Céleste]: pp. [vi], xv, 420; Vol. II [ibid.]: pp. [iv], xvi, 440; Vol. III [ibid.]: pp. [viii], xix, 381; Vol. IV [ibid.]: pp. [iv], xxxix, 552, with one folding engraved plate; Vol. V [ibid.]: pp. [vi], v, 540, [2, errata to vols. I-III]; Vol. VI [Exposition du Système du Monde]: pp. [iv], vii, 479; Vol. VII [Théorie Analytique des Probabilités]: pp. [vi], cxcv, 691. Contemporary red morocco, covers decorated in blind and with 'Concours général des départements' and arms of Napoleon III stamped in gilt, spines lettered in gilt, a.e.g., moiré endpapers (light browning and foxing). A very fine copy.

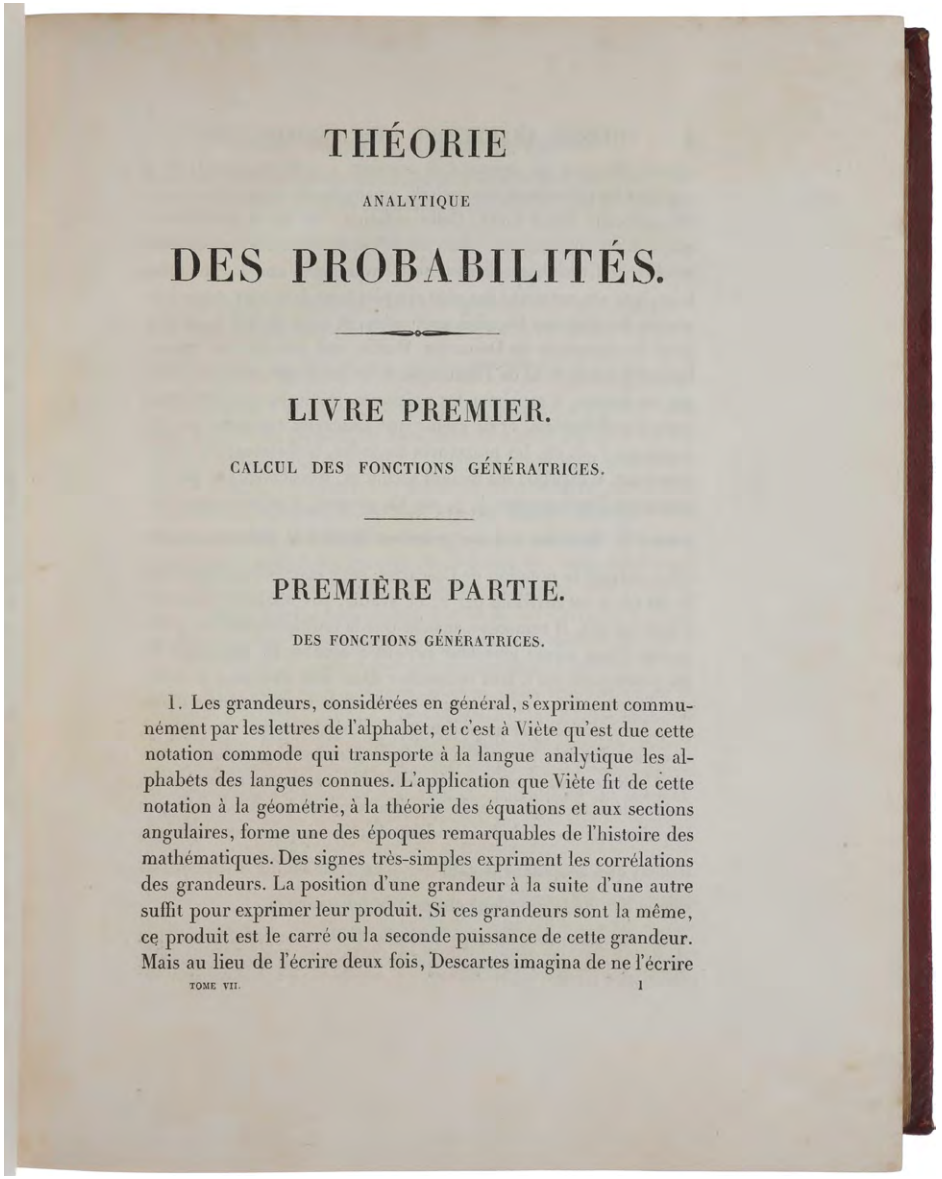


First edition, a beautiful copy bound in contemporary red morocco with the arms of Napoleon III, of the collected works of Laplace, comprising his principal contributions to celestial mechanics and probability theory. The distinguished American historian of science Charles Gillispie considers Laplace to be "among the most influential scientists in all history" (*Pierre-Simon-Laplace, 1749-1827: A life in exact science* (1997), p. vii). "Laplace gave the world three great works: the Mécanique Céleste, the Exposition du Système du Monde, and the Théorie Analytique des Probabilités. Besides these he presented numerous important memoires before the French Academy and the Academy of Sciences. Laplace's works were first published in seven volumes by the French government in 1843" (Richeson, p. 73). This national edition of the works of the 'Newton of France'

was the result of laws passed by the two legislative chambers in 1842 and 1843, which authorised a special budget of 40,000 francs for publication. Napoleon Bonaparte's nephew, Napoleon III (1808-73), was President of the French Second Republic (1848-52) and Emperor (1852-70) during the Second French Empire. He succeeded Louis XVIII, under whose rule Laplace had been elevated to the title of Marquis.

The *Traité de Mécanique Céleste*, which was first published in 1799-1825, codified and developed the theories and achievements of Newton, Euler, d'Alembert, and Lagrange. In the tradition of Newton's *Principia*, Laplace (1749-1827) "applied his analytical mathematical theories to celestial bodies and concluded that the apparent changes in the motion of planets and their satellites are changes of long periods, and that the solar system is in all probability very stable" (Dibner 14). "Laplace maintained that while all planets revolve round the sun their eccentricities and the inclinations of their orbits to each other will always remain small. He also showed that all these irregularities in movements and positions in the heavens were self-correcting, so that the whole solar system appeared to be mechanically stable. He showed that the universe was really a great self-regulating machine and the whole solar system could continue on its existing plan for an immense period of time. This was a long step forward from the Newtonian uncertainties in this respect. Laplace also offered a brilliant explanation of the secular inequalities of the mean motion of the moon about the earth - a problem which Euler and Lagrange had failed to solve. He also investigated the theory of the tides and calculated from them the mass of the moon" (PMM).

The *Exposition du Système du Monde* is "one of the most successful popularizations of science ever composed" (DSB). It is "an elegant, non-mathematical classic on astronomy. It is in this work that Laplace introduced one of his most notable contributions (although he himself did not take it very seriously at first) – the



THÉORIE

ANALYTIQUE

DES PROBABILITÉS.

LIVRE PREMIER.

CALCUL DES FONCTIONS GÉNÉRATRICES.

PREMIÈRE PARTIE.

DES FONCTIONS GÉNÉRATRICES.

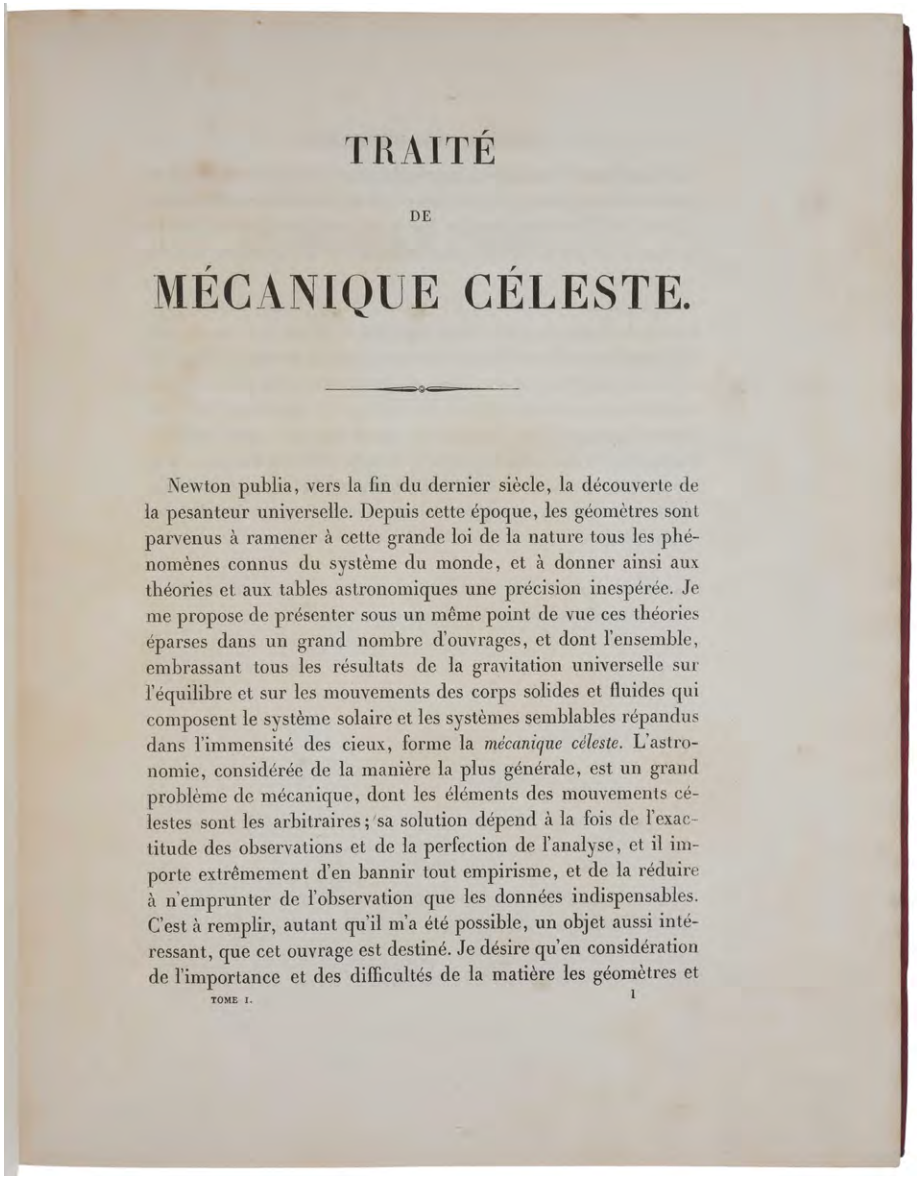
1. Les grandeurs, considérées en général, s'expriment communément par les lettres de l'alphabet, et c'est à Viète qu'est due cette notation commode qui transporte à la langue analytique les alphabets des langues connues. L'application que Viète fit de cette notation à la géométrie, à la théorie des équations et aux sections angulaires, forme une des époques remarquables de l'histoire des mathématiques. Des signes très-simples expriment les corrélations des grandeurs. La position d'une grandeur à la suite d'une autre suffit pour exprimer leur produit. Si ces grandeurs sont la même, ce produit est le carré ou la seconde puissance de cette grandeur. Mais au lieu de l'écrire deux fois, Descartes imagina de ne l'écrire

TOME VII. 1

so-called nebular hypothesis, which provided a conjectural account of the origin of the solar system" (PMM). According to this hypothesis, the Sun, planets, and their moons began as a whirling cloud of gas. Laplace argued that as a hot nebula cooled, it would require a proportionately faster rate of rotation in order to conserve its angular momentum, and the increased centrifugal force resulting from this increased speed would throw out material which would eventually condense into planets. In essence, this theory of the origin of the solar system is still accepted.

The *Théorie Analytique des Probabilités*, first published in 1812, is "the most influential book on probability and statistics ever written" (Anders Hald), which John Herschel called "the *ne plus ultra* of mathematical skill and power" (*Essays from the Edinburgh and Quarterly Reviews* (1857), p. 382). "In the Théorie Laplace gave a new level of mathematical foundation and development both to probability theory and to mathematical statistics. … [It] emerged from a long series of slow processes and once established, loomed over the landscape for a century or more." (Stephen Stigler, *Landmark Writings in Western Mathematics 1640-1940*, pp. 329-30). "It was the first full–scale study completely devoted to a new specialty, … [and came] to have the same sort of relation to the later development of probability that, for example, Newton's Principia Mathematica had to the later science of mechanics" (DSB). The *Théorie analytique des Probabilités* contains, besides an introduction, two books, *Du calcul des Fonctions génératrices* and *Théorie générale des Probabilités*, together with four supplements which appeared over the period 1816-1825.

The *Essai Philosophique sur les Probabilités* is Laplace's accessible summary of his work on probability theory. "Inevitably, Laplace's technical writings have come to have the same sort of relation to the later development of the discipline of probability that, for example, Newton's *Principia mathematica* had for the later



TRAITÉ

DE

MÉCANIQUE CÉLESTE.

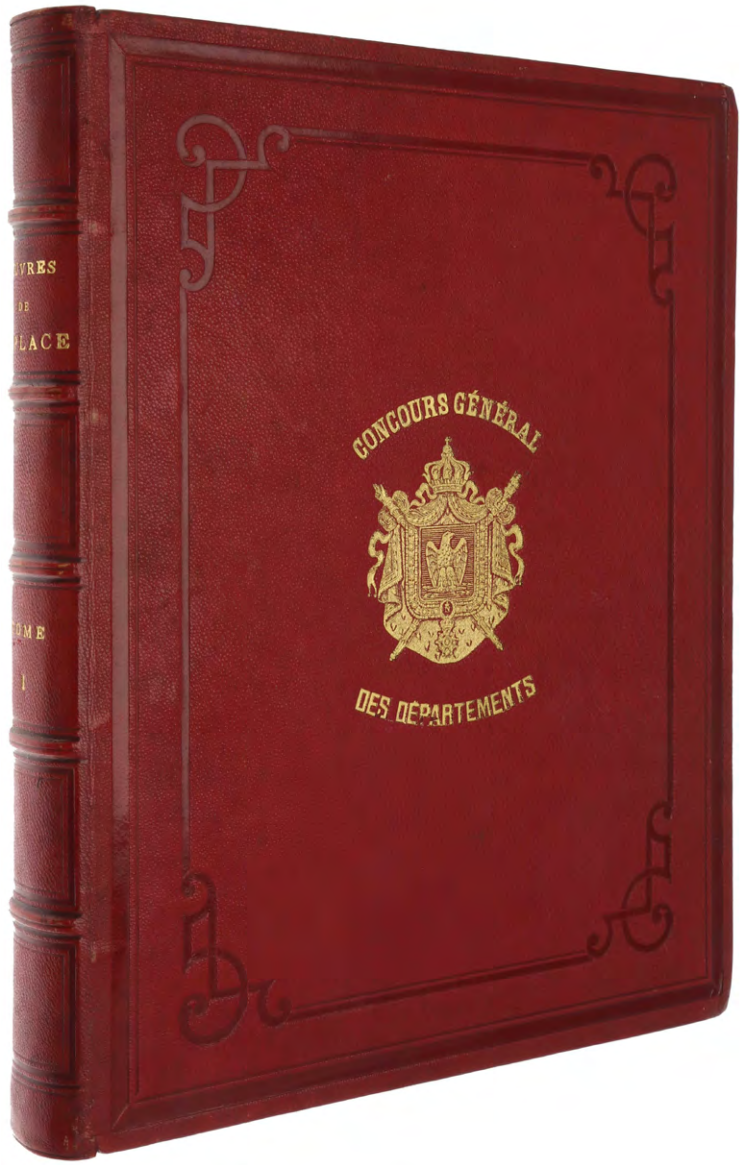
Newton publia, vers la fin du dernier siècle, la découverte de la pesanteur universelle. Depuis cette époque, les géomètres sont parvenus à ramener à cette grande loi de la nature tous les phénomènes connus du système du monde, et à donner ainsi aux théories et aux tables astronomiques une précision inespérée. Je me propose de présenter sous un même point de vue ces théories éparses dans un grand nombre d'ouvrages, et dont l'ensemble, embrassant tous les résultats de la gravitation universelle sur l'équilibre et sur les mouvements des corps solides et fluides qui composent le système solaire et les systèmes semblables répandus dans l'immensité des cieux, forme la *mécanique céleste*. L'astronomie, considérée de la manière la plus générale, est un grand problème de mécanique, dont les éléments des mouvements célestes sont les arbitraires; sa solution dépend à la fois de l'exactitude des observations et de la perfection de l'analyse, et il importe extrêmement d'en bannir tout empirisme, et de la réduire à n'emprunter de l'observation que les données indispensables. C'est à remplir, autant qu'il m'a été possible, un objet aussi intéressant, que cet ouvrage est destiné. Je désire qu'en considération de l'importance et des difficultés de la matière les géomètres et

TOME I. 1

science of mathematics. Even if there were no other reason, that would suffice to explain why most readers who wish to repair to the fountainhead of what is often called the subjective interpretation of probability, in contrast to the frequency view, have recourse to the *Essai Philosophique*" (DSB). The Essai first appeared as the introduction to the second edition of the *Théorie Analytique* (1814), and separately in the same year, and is here again included as the introduction to the *Théorie Analytique*.

"Pierre Simon Laplace was born in Normandy, France, in 1749 and died in 1827. Very little is known of his youth, since in after life he refused to speak of his childhood days. At eighteen through the aid of D'Alembert he secured a position as professor of mathematics at the École Militaire in Paris. He was elected a member of the French Academy and the Academy of Sciences; later he was made President of the Bureau of Longitude. It was during this time that he carried on the greater part of his scientific researches that gained for him the title of "Newton of France"" (Richeson, p. 73).

Richeson, 'Laplace's contributions to pure mathematics,' *National Mathematics Magazine* 17 (1948), pp. 73-8.



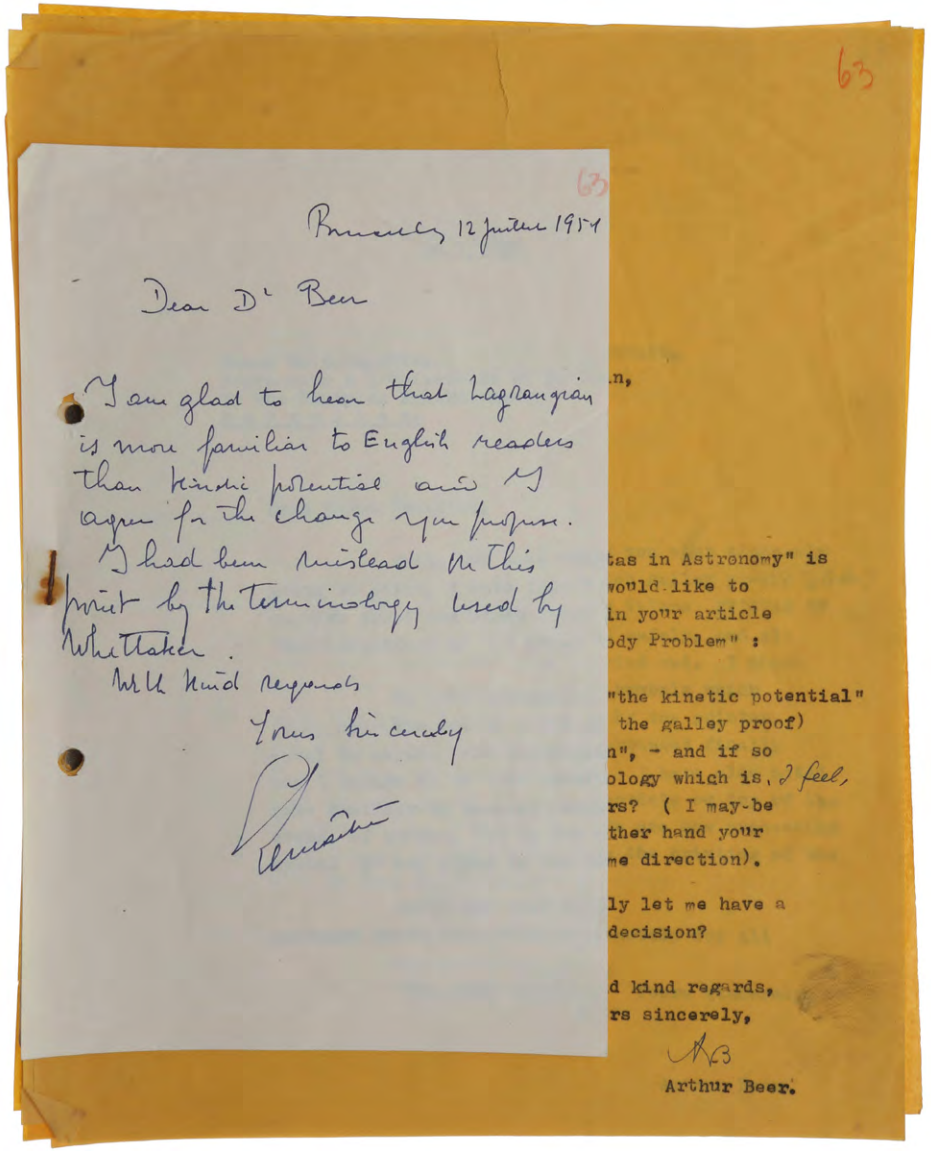
BY THE FOUNDER OF THE CONCEPT OF THE EXPANDING UNIVERSE

LEMAÎTRE, Georges Henri Joseph Édouard. *Three autograph letters signed and one autograph postcard signed from Lemaître to the Anglo-German astronomer Arthur Beer concerning Lemaitre's contribution 'Regularization of the three-body problem' to the volume of essays Vistas in Astronomy (1955), edited by Beer. Together with six typed letters from Beer soliciting and responding to Lemaître's correspondence.*

$2,800

Three signed autograph letters (with punch holes in inner margin), two written on one side only (one with Lemaître's signature cut out), and one written on both sides; together with a signed autograph postcard written on one side (with punch holes on lower margin, not affecting Lemaître's writing). The first two pages of the first volume of Vistas in Astronomy are filled with reproductions of the signatures of the participants. Beer obtained most of these by cutting out the signatures from letters he had received from the authors of the articles in the volume.

An exceptionally rare collection of autograph materials by the founder of the concept of the expanding universe, and the first person to state 'Hubble's law', two years before Edwin Hubble found observational evidence for it. "It could as well have been named Lemaître's law" (Helge Kragh, *Cosmology and Controversy*, p. 29). Lemaître "was the first to recognize that a number of things that are now taken to be important and obvious must be true about a general-relativistic universe (including the fact that it must expand)" (Kragh in *Biographical Encyclopedia of Astronomers* I, p. 689). We have been unable to find any record of any other autograph material by this great cosmologist ever having appeared on the market.



Lemaître (1894-1966) studied mathematics and physics, and later theology, at the University of Louvain. He was ordained a priest in 1923, and in the same year he was awarded a traveling postdoctoral fellowship. "He went to Cambridge and studied under Eddington, under whose influence he specialized in general relativity. In 1924 and 1925 he continued his post-graduate studies in the United States, at Harvard and MIT. During his stay in the United States, he attended the American Astronomical Society conference in Washington, at which Hubble's discovery of the Cepheid variables in the Andromeda nebula was announced. He became increasingly occupied with cosmology and, while at MIT in 1925, suggested a modification of de Sitter's cosmological theory. Lemaître's model was non-static and included a red shift caused by the Doppler effect. After his return to Louvain, he developed his theory further, and in 1927 he published a new cosmological theory that combined the advantages of the earlier theories of Einstein and de Sitter ['Un univers homogène de masse constant et de rayon croissant rendant compte de la vitesse radiale des nébuleuses extra-galactiques,' *Annales de la Société Scientifique de Bruxelles* 47 (1927)]. Lemaître showed that the field equations of general relativity allowed an expanding universe and derived a velocity-distance relation" (Kragh in DSB). "'The receding velocities of the galaxies cause their received light to be redshifted,' he wrote … For the proportionality factor – later the Hubble constant – Lemaître suggested a value of about 625 km/s/MPc, which is not very different from the observational result obtained by Hubble two years later … The famous Hubble law is clearly in Lemaître's paper" (Kragh, *Cosmology and Controversy*, pp. 29-31).

Born in Reichenberg, Bohemia, Arthur Beer (1900-1980) received his doctorate in 1927 with a dissertation entitled 'Zur Charakterisierung der spektroskopischen Doppelsterne' ('On the characterization of spectroscopic binaries'). He then worked at Breslau Observatory, and later at the German Maritime Observatory at Hamburg, before emigrating to Cambridge, England, in 1934 due to the persecution of Jewish scientists in Nazi Germany. He carried out astrophysical research under F. J. M. Stratton (1881-1960) at the Cambridge Solar Physics Observatory from 1934 to 1937, and later served as Senior Assistant Observer at Cambridge Observatories until 1968. "In the early 1950s Arthur committed himself to a voluminous and thorough survey of current astronomy. Ultimately, 215 scientists, mathematicians and historians contributed to what was to become the multi-volume *Vistas of Astronomy*, covering both historical and current topics, for which he was a writer and editor-in-chief (1955-1956) for the first two volumes" (Wikipedia). These volumes were dedicated to Stratton.

The correspondence opens with a two-page letter dated 22 February 1953 from Beer thanking Lemaître for agreeing to contribute an article to the Vistas volume "on some cosmological problem near to your heart – i.e. the expanding universe" and expresses satisfaction that this will "touch on Eddington's work, which otherwise does not appear to be represented in the volume."

Brussels 11 May 1953
9 rue Henry de Bachelier (?)
I must apologise to be a little late with the preparation of my paper for the forthcoming Stratton volume.
I have taken as a subject Regularisation in the Three Body Problem.
The paper is nearly ready and will be send [sic] within the week.
With kind regards
Sincerely yours
G Lemaître.

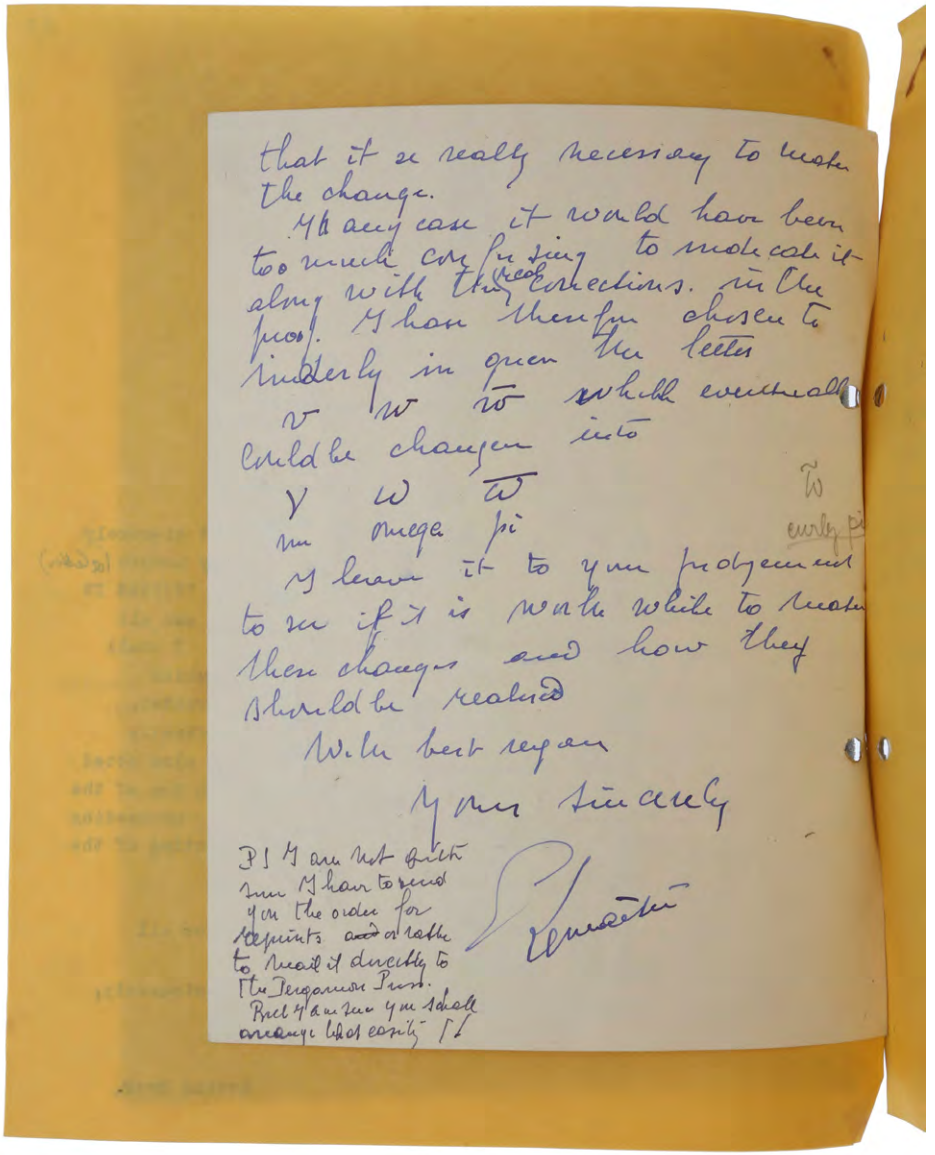
Beer replies on 17 May, acknowledging receipt of Lemaître's letter of 11 May.

On 5 June Beer writes to Lemaître, acknowledging receipt of his manuscript and requests that he send one or two pages of general introduction and concluding survey, pointing out that the articles in the Vistas volume are intended to be understood by a general audience of astronomers.

June 26 1953
Dear Dr Beer
I send you a few words of introduction and some of conclusion to my paper.
I think the small amount of repetition which result[s] is tolerable and it will be difficult to change it without reworking the whole paper.
I apologise for the delay.
With best regards
Yours sincerely
[G Lemaître – signature excised]

On 29 June Beer writes to acknowledge receipt of the introduction and concluding survey.

Brussels February 19 1954
Dear Dr Beer
Excuse me to have delayed (?) so much to send you back the proofs of my paper.
I have introduced a few heading[s] as I was requested to do, they are written in red ink.
For words in the works (?) I see only Three body problem.
There is a point I should like to mention here. All through the paper 1) the letter v nu of my manuscript [h]as been interpreted as a v 2) my ω omega has been read w double u 3) my ϖ has been read [w bar]. The confusion really arises from this omega and I am not quite sure [page 2] that [it] is really necessary to make the change.
In any case it would have been too much confusing to include (?) it along with the


that it is really necessary to make the change.
In any case it would have been too much confusing to make all along with the corrections. in the proof. I have therefore chosen to indicate in green the letter
v w ϖ which eventually could be changed into
ν ω ϖ
nu omega pi
I leave it to your judgement to see if it is worth while to make these changes and how they should be realised
With best regards
Yours sincerely
P.S. I am not quite sure I have to send you the order for reprints and I take to mail it directly to the Telegraaf Print.
But I am sure you shall arrange that easily

real corrections in the proof. I have therefore chosen to underline in pen (?) the letters v w [w bar] which eventually could be changed into v nu ω omega ϖ pi
I leave it to your judgment to see if it worth while [sic] to make these changes and how they should be realized.
With best regards
Yours sincerely
G Lemaitre
P.S. I am not quite sure [if] I have to send you the order for reprints or rather to mail it directly to the Pergamon Press. But I am sure you shall arrange this easily.

The symbol ϖ is a variant of lower case π (with legs bent inward to meet), often used in technical contexts, and often rendered as a lower case ω with a bar on top.

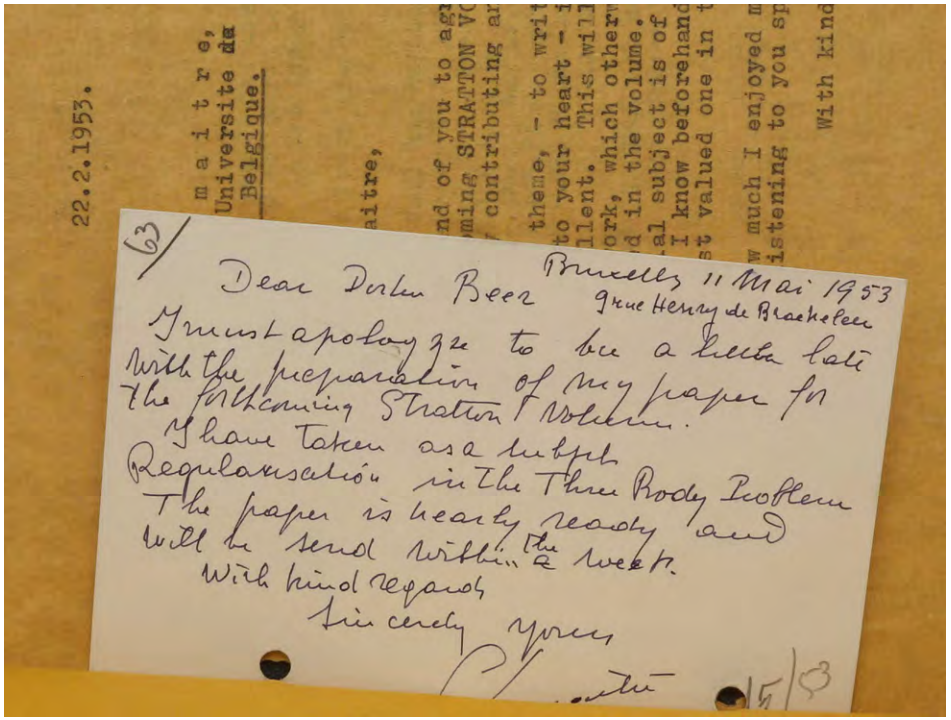
On 7 March Beer writes to acknowledge receipt of the corrected proofs, reassures Lemaître that the symbols will be corrected by the printer, and notes his request for 50 reprints in addition to the 25 normally provided.

On 10 July Beer writes to ask Lemaître if the term 'kinetic potential' in his article is the same as that the term 'Lagrangian' as understood by English-speaking readers.

Brussels 12 July 1954
Dear Dr Beer
I am glad to hear that Lagrangian is more familiar to English readers than kinetic potential and I agree for the change you propose.
I had been mislead in this point by the terminology used by Whittaker.
With kind regards
Yours sincerely
G Lemaitre

Lemaître is probably referring to the classic *Treatise on the Analytical Dynamics of Particles and Rigid Bodies* by Edmund Taylor Whittaker.

Lemaitre's article, 'Regularization of the three-body problem,' appeared on pp. 207-215 of volume I of *Vistas in Astronomy*, London & New York: Pergamon Press, 1955.



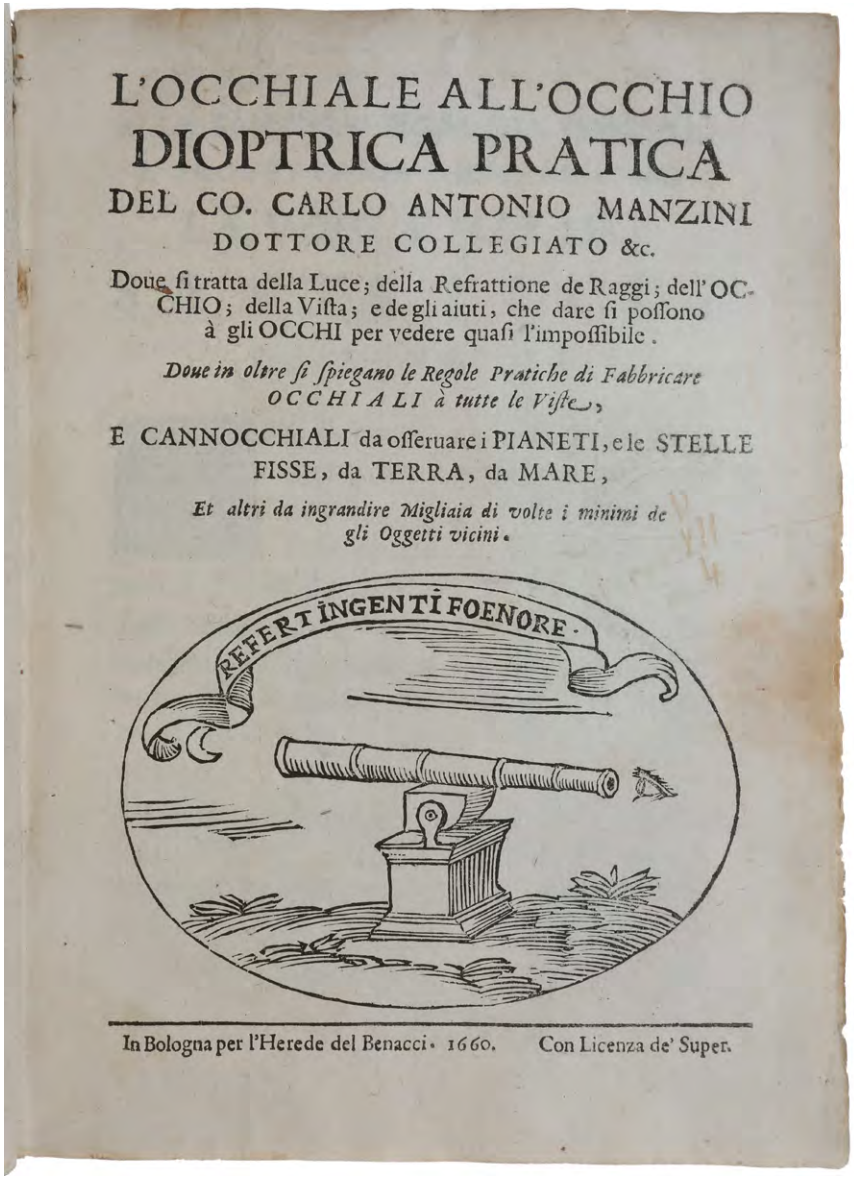
LENSES, SPECTACLES, TELESCOPES AND MICROSCOPES

MANZINI, Count Carlo Antonio. *L'Occhiale all'Occhio. Dioptrica Pratica […] Dove si tratta della Luce, della Refrattione de raggi, dell'Occhio, della Vista e degli aiuti, che dare si possono à gli occhi per vedere quasi l'impossibile. Dove in oltre si spiegano le regole pratiche di fabbricare Occhiali à tutte le viste, e Cannocchiali da osservare i pianeti, e le stelle fisse, da terra, da mare, et altri da ingrandire migliaia di volte i minimi de gli oggetti vicini.* Bologna: per l'herede del Benacci, 1660.

$12,500

4to (203 x 148 mm), pp. [xii], 268, [4], with full-page engraved portrait of Eustachio Divini by the Austrian artist Johann Paul Schor (1615-74), known in Rome as 'Giovanni Paolo Tedesco,' and the Genoese engraver Joseph Testana. Woodcut illustration of telescope on title and seventeen woodcut illustrations in text. Manuscript corrections by Manzini to text. Contemporary limp vellum, some worming and a small hole to spine, first two leaves with a dampstain to outer margin, otherwise a very fine and crisp copy.

First and only edition, printed on thick paper, of this important work on the making of lenses, spectacles, telescopes and microscopes, "the most comprehensive book on the subject" (Ilardi, p. 229). "Seventeenth-century account of dioptrics, dealing with light, refraction, the eye, vision, the invention of spectacles, the making of spectacles and telescopes. The book, which is essentially practical, aims at showing the optical worker how lenses are ground and how they may be used both to remedy visual defects and also for telescopes. The dedication ... is to Saint Lucia, the patron saint of the blind and those with diseased eyes" (*British Optical*


L'OCCHIALE ALL'OCCHIO
DIOPTRICA PRATICA
DEL CO. CARLO ANTONIO MANZINI
DOTTORE COLLEGIATO &c.
Doue si tratta della Luce; della Refrattione de Raggi; dell'OCCHIO; della Vista; e de gli aiuti, che dare si possono à gli OCCHI per vedere quasi l'impossibile.
Doue in oltre si spiegano le Regole Pratiche di Fabbricare OCCHIALI à tutte le Viste,
E CANNOCCHIALI da osseruare i PIANETI, e le STELLE FISSE, da TERRA, da MARE,
Et altri da ingrandire Migliaia di volte i minimi de gli Oggetti vicini.
REFERT INGENTI FOENORE
In Bologna per l'Herede del Benacci. 1660. Con Licenza de' Super.

Association Catalogue). "An important work in the history of optics, valuable as one of the earliest detailed accounts of methods of grinding and polishing lenses. A large number of fine woodcuts illustrate the machinery and processes described by the author" (*Becker Catalogue*). The first chapter contains an account of the invention of the telescope, and of Galileo's role in it, and also of the invention of spectacles. The following three chapters deal respectively with optics, the anatomy of the eye, and the mechanics of vision. The remaining ca. 200 pages are devoted to the manufacture of lenses, with extensive discussion of the different kinds of lenses required for various purposes, and the different methods of their design and manufacture. There are descriptions of the Murano glassworks, opticians' tables for the precise measurement of lenses, and illustrations of a lens grinder, glass-cutting shears, lathes and several optical instruments. The full-page portrait of Eustachio Divini, aged 49 years, bears an inscription stating that he is judged by scientists of optics to be the first to have fabricated large telescopes (*occhialoni*). As several of Divini's works are addressed to Manzini it is reasonable to suppose that this work documents in part Divini's discoveries. This is an unpressed copy on thick paper, bound in contemporary vellum, and with manuscript corrections in Manzini's hand – see, for example, the copy at the Universidad Complutense de Madrid, which has the same corrections in the same hand as our copy (catalog. hathitrust.org/Record/009353165).

Born of the Bolognese nobility, "Manzini established his own astronomical observatory on the grounds of his estates around Bologna and made his own telescopes, grinding the lenses for them himself … Manzini's [book] was both a theoretical and practical compendium of what was known on optics and on the art of spectacle making from the fusion of glass and crystal to the fitting of glasses for various refractive errors and the insertion of precision lenses in telescopes and microscopes …



"In his preface, Manzini expressed his consternation in seeing the art of spectacle making being passed orally from one generation to the other without written instructions and often in strict secrecy so that much valuable information was lost forever. His book was designed to serve as a guide both in theoretical optics as developed by medieval authorities such as Alhacen (965-1040), Witelo (ca. 1230-80), [Roger] Bacon (1214/20-92) and by writers closer to his age such as Johannes Hevelius (1611-87), [Francesco] Maurolico (1494-1575), [Giambattista] della Porta (1535-1615), Christoph Scheiner (1573-1650), [Johannes] Kepler (1571-1630), Marin Mersenne (1588-1648) and [René] Descartes (1596-1650) among others, and in the actual shop practices followed by leading makers of scientific instruments, some of which he had helped to develop. He revealed that he had learned the first rudiments of hand-polishing lenses from a former mirror maker in Venice, Domenico Rambottino, a man without any education (*huomo idiota affato*) but very skilled in polishing lenses for telescopes, which he supplied throughout Italy and the New World (New Indies) (pp. 238-9). He received additional theoretical and practical instruction from some celebrated instrument makers of the age such as Francesco Fontana (1580-ca. 1656) in Naples, who brought the art to such a degree of perfection that he could rightly boast to be the most "sharp-eyes man from the creation of the universe to his time" (Preface). He reserved the highest praise for Eustachio Divini (1610-85) in Rome who rose above all others in the practice of this art, which can now be called "divine" (an allusion to his last name) because of his accomplishments (Preface). Even great Princes in Italy and elsewhere, he claimed, have not disdained to use their hands in this art through which men can now scan the skies and the stars and contemplate God's creation. And, he observed, "there are few in the world that would not need the benefits of this art before dying" (Preface). There could hardly be a more enthusiastic and eloquent celebration of the usefulness, dignity, nobility, and even "divine" function of the relatively new profession of optical scientist and practitioner.



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De varij manubrij detti communemente Bacchetti, ſopra de quali ſi attaccano i Vetri, ò Criſtalli per lauorare: & delli Bitumi, ò Cole per attaccarli, & in che maniera ſi facciano i filetti attorno alle Lenti, & Occhiali.

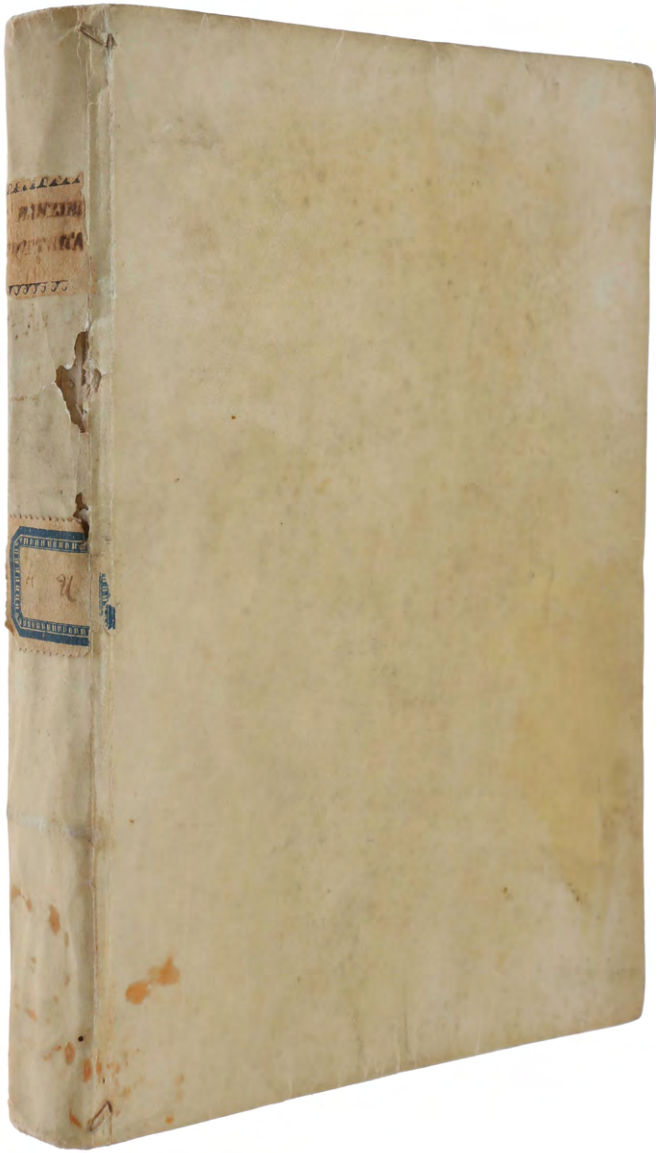
Cap. XXIII.

PEr gli Occhiali ordinarij da tutte le Viſte ſi adoprano certi Cilindretti di legno forte fatti al Tor-

"The preface also emphasized the practical aspects of the art. Although Manzini distilled optical theory in his chapters on light and refraction for the benefit of those more skilled in mathematics, he advised other readers that these sections could safely be skipped, because they were not necessary to become "a perfect master" (*maestro*). They were advised instead to imitate Divini's career, whose portrait graces the frontispiece of his book. Divini, according to Manzini, had relied more on experience, ingenuity, and good judgment than on books to achieve his astounding results in making the best lenses and telescopes in Europe. He was, indeed, credited by his colleagues to be the "first to have perfected the making of telescopes" [Preface].

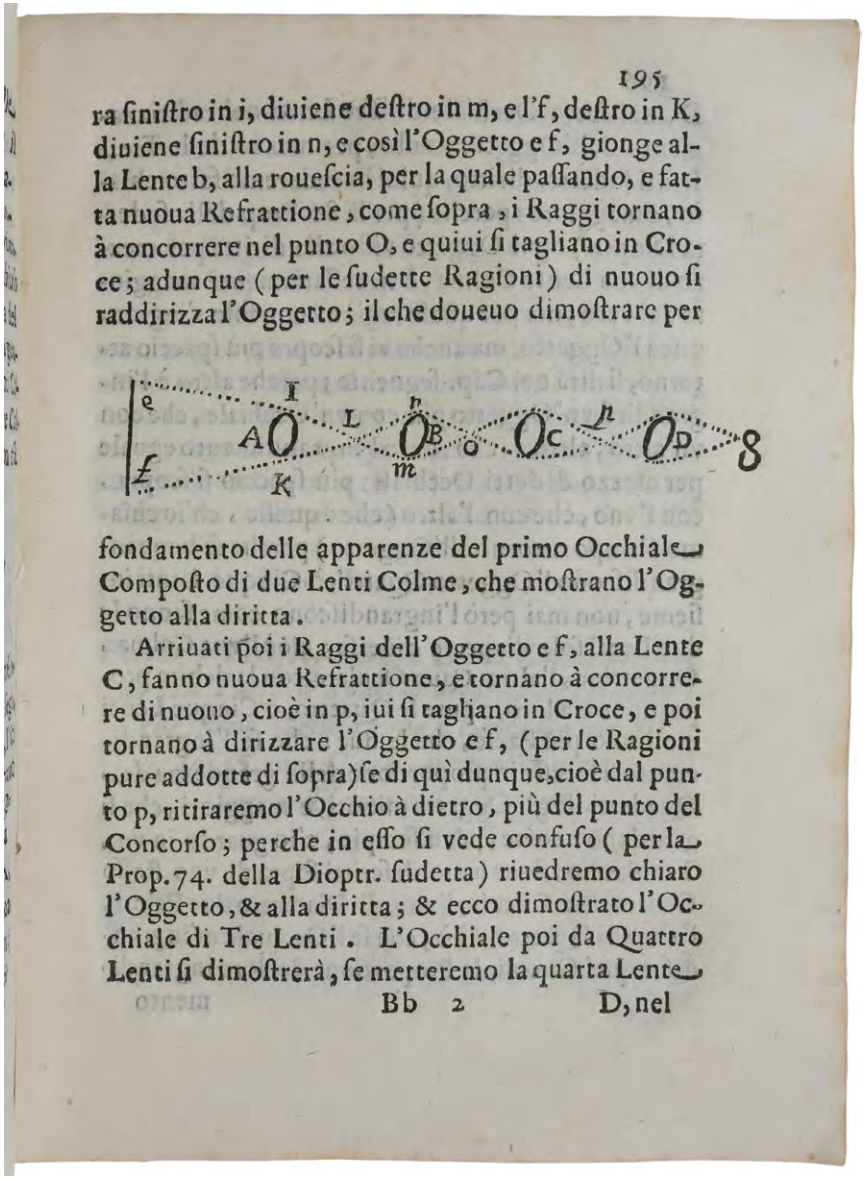
"Manzini's detailed and extensive description of lens grinding and polishing surpassed by far earlier treatments, including those published by Della Porta in his *Magia naturalis* (1589) and by Giovanni Sirtori in his *Telescopium* (1618). His exposition is based on these and other writings and above all on personal observation and practice as he consorted and worked with top-level masters of the art. It would be impossible to distill in a few sentences the complex steps of this process, which occupies the longest section of the book (pp. 199-263) … For our purposes it is sufficient to identify the five stages in the production of lenses: making templates for the tools for grinding and polishing, producing the sets of tools for different focal lengths, the selection of the glass blanks, the grinding process and the polishing process.

"The templates were made of metal (copper, steel, pewter, lead, and preferably brass) and were constructed in pairs, one convex, the other concave, of the same radius of curvature and each set was turned to a different focal length and labeled in feet as a measure of the focal length of the lens to be ground. The pairs in each set were ground against each other to ensure the accuracy of the surfaces of the lenses … The templates were used to form the grinding tools themselves, also



made in pairs. Metals were used for these tools, usually iron but preferably brass. The glass blanks were cut into disc shapes from a larger plate of glass, and several of them were fixed with cement to a convex tool or mallet, which was cemented in turn to a post for grinding. All the lenses were then ground at the same time against the tools by hand, using progressively finer sizes of washed emery to bring the lens surface to a semi-polished state. The final polishing was done on a concave cast iron shell covered with a heavy woolen cloth without its nap, which was pressed into place by working the corresponding convex brass tool on it. The pores of the cloth were then filled with enough *tripoli* (putty powder, calcined tin) to make its surface level.

"Manzini advised that grinding and polishing of lenses was accomplished more precisely by hand than by machine. He wrote that he had never seen a lathe or machine capable of doing what the hand could despite the claims of inventors and mathematicians. He even added with a tinge of irony that he had seen one of these mathematicians/inventors, a non-Italian (*Oltremontano*) residing in a leading Italian city, working "many lenses" by hand "with extraordinary diligence and patience" [pp. 159-60]. But he was not a Luddite for his text is illustrated with various grinding/polishing machines, including one of his own improved lathes, which was superior to the lathe used by Ippolito Francini (1593-1653) (nicknamed *Tordo*), Galileo's skilled lens-maker. He stressed, however, the necessity that theory must be informed by practice for the production of excellent lenses following his own example by observing artisans and working with them. This experience had taught him that the final testing of the lenses be done by a person with keen eyesight, operating in a darkened chamber to observe objects at various distances through the lenses with the light being provided by candles, torches, or oil lamps (pp. 252-55) …



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ra ſiniſtro in i, diuiene deſtro in m, e l'f, deſtro in K, diuiene ſiniſtro in n, e così l'Oggetto e f, gionge alla Lente b, alla roueſcia, per la quale paſſando, e fatta nuoua Refrattione, come ſopra, i Raggi tornano à concorrere nel punto O, e quiui ſi tagliano in Croce; adunque (per le ſudette Ragioni) di nuouo ſi raddirizza l'Oggetto; il che doueuo dimoſtrare per

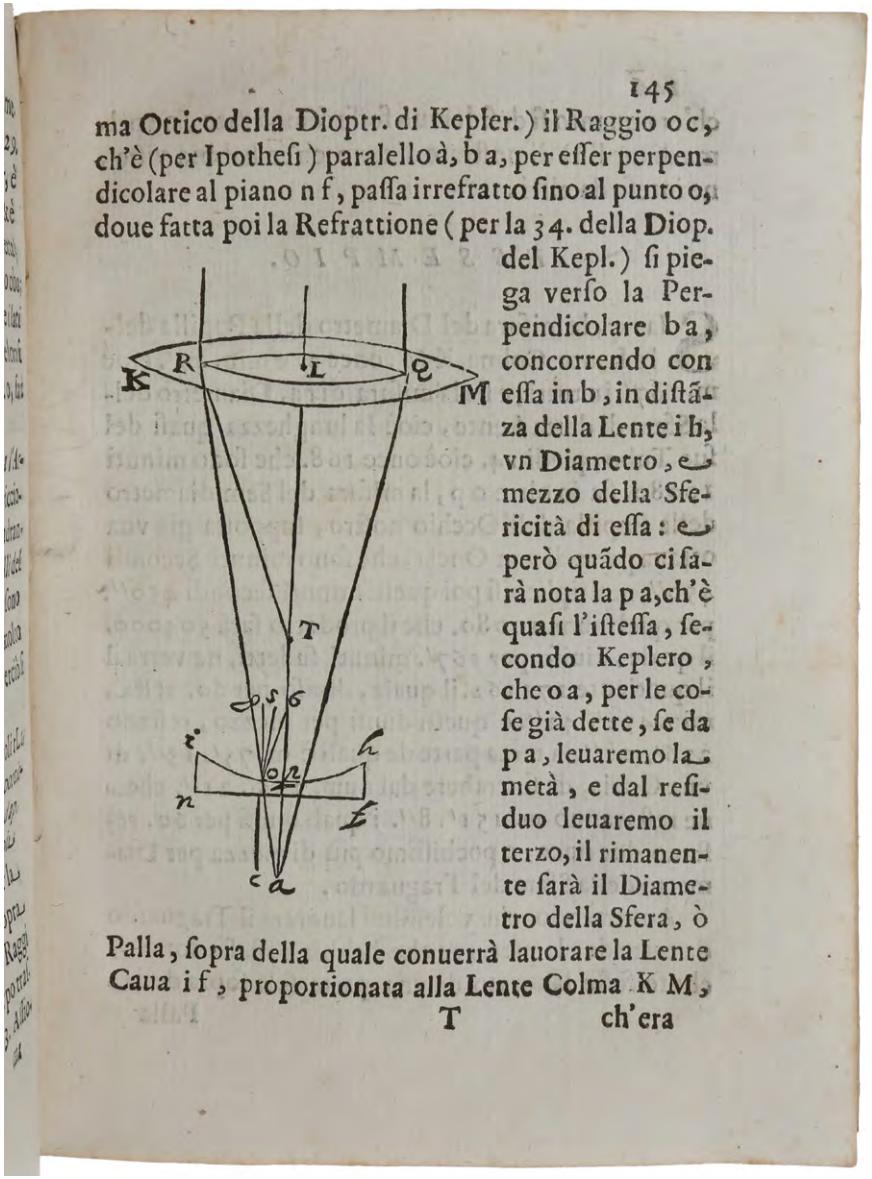
fondamento delle apparenze del primo Occhiale Compoſto di due Lenti Colme, che moſtrano l'Oggetto alla diritta.

Arriuati poi i Raggi dell'Oggetto e f, alla Lente C, fanno nuoua Refrattione, e tornano à concorrere di nuouo, cioè in p, iui ſi tagliano in Croce, e poi tornano à dirizzare l'Oggetto e f, (per le Ragioni pure addotte di ſopra) ſe di quì dunque, cioè dal punto p, ritiraremo l'Occhio à dietro, più del punto del Concorſo; perche in eſſo ſi vede confuſo (per la Prop. 74. della Dioptr. ſudetta) riuedremo chiaro l'Oggetto, & alla diritta; & ecco dimoſtrato l'Occhiale di Tre Lenti. L'Occhiale poi da Quattro Lenti ſi dimoſtrerà, ſe metteremo la quarta Lente

"From the marriage of theory and practice, Manzini evolved tables of specific ages requiring appropriate radii of curvature for spectacle lenses … He distinguished six power degrees of lenses for various ages and for cataracts: 1. 40-50, known as "common vision" (*vista commune*); 2. 50-60; 3. 60-70; 4. 70-80; 5. Glasses for a half cataract; 6. Glasses for the entire cataract. As had been established two centuries earlier, he recognized two degrees of myopia – "weak and short vision" (*viste deboli e viste corti*). He cautioned, however, that these measures were averages for there were several "middle" degrees both for presbyopes and myopes and age alone was a very imprecise determining measure. It was necessary to try different models in spectacle shops [pp. 95-105]" (Ilardi, pp. 229-32).

"The preface mentions a long list of authorities … although few of them play any role in Manzini's directions for telescope making. Among the few that do, Kepler is by far the one most frequently cited. As we shall see, Manzini uses Kepler's theory in many ways and discovers in the *Dioptrice* (1611) results that seem to suggest to him new telescope designs.

"For one thing, Manzini appeals to Kepler's authority to back well-known basic results of telescope making, as if they gained a new status by Kepler's demonstrations in the *Dioptrice*. This is the case for the claim that in Dutch telescopes lenses must stand as far apart as the focal distance of the convex objective lens or nearly; that the aperture must change according to the brightness of the observed object; or that objective lenses of great convexity require eye-pieces of great concavity. Contrariwise to what [Cherubin] d'Orleans does [in *La dioptrique oculaire*, 1671], Manzini never follows or repeats Kepler's geometrical arguments closely. However, he brings Kepler in when he wants to explain some relevant optical effect. For instance, he provides a qualitative, very general explanation of vision through telescopes with two or more convex lenses grounded on proposition 80 of the *Dioptrice* (the upright image of a visible thing seen through a convex



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ma Ottico della Dioptr. di Kepler.) il Raggio o c, ch'è (per Ipotheſi) paralello à, b a, per eſſer perpendicolare al piano n f, paſſa irrefratto ſino al punto o, doue fatta poi la Refrattione (per la 34. della Diop. del Kepl.) ſi piega verſo la Perpendicolare b a, concorrendo con eſſa in b, in diſtãza della Lente i h, vn Diametro, ق mezzo della Sfericità di eſſa: ق però quãdo ci ſarà nota la p a, ch'è quaſi l'iſteſſa, ſecondo Keplero, che o a, per le coſe già dette, ſe da p a, leuaremo la metà, e dal reſiduo leuaremo il terzo, il rimanente ſarà il Diametro della Sfera, ò Palla, ſopra della quale conuerrà lauorare la Lente Caua i f, proportionata alla Lente Colma K M, ch'era

T

lens is enlarged necessarily); Manzini uses it to explain why the combinations he sets forth will produce magnified vision. In another instance, he explains why astronomical telescopes have a wider field of view than Dutch telescopes do with references to *Dioptrice*'s propositions 81 (in which Kepler links the distance of the eye to the focus of a convex lens to the field of view), 86 (which contains Kepler's account of the combination of two convex lenses) and 109 (where it is proved that in Dutch telescopes the concave ocular is always located very near the focus of the objective). In other instances, Manzini uses results from the *Dioptrice* to ground new rules of his invention. For instance, Manzini produces a table for Dutch telescopes that gives the recommended aperture size in relation to focal length. This is complemented by a new table that determines the best concavity of the ocular in relation to apertures. Manzini's calculations of concavities for ocular lenses involve a geometrical argument grounded on Kepler's theoretical understanding of the role of ocular lenses and on his law of refraction. In other instances, Manzini uses results from the *Dioptrice* to analyse instruments that Kepler never mentions. This is the case for the microscope (with one or more lenses), whose workings Manzini grounds on Kepler's proposition 37 in the *Dioptrice*.

"Kepler's interest in 'cryptical' instruments [discussed in the last propositions of *Dioptrice*] lead him to explore the optical effects of new combinations of lenses. Manzini does not talk of 'cryptical' instruments, but suggests a vast array of new telescope designs in which three, four and even five lenses are combined, and whose optical properties Manzini knows in a qualitative general way thanks to Kepler's results about 'cryptical' instruments. He analyses first in some detail a variation upon the basic design of the Dutch telescope in which the tube is fitted with an additional convex lens whose convexity is about ¾ that of the main objective lens and which is located about half the length of the tube. Rules for fixing the ocular are to be obtained by trial and error. This design, says Manzini,



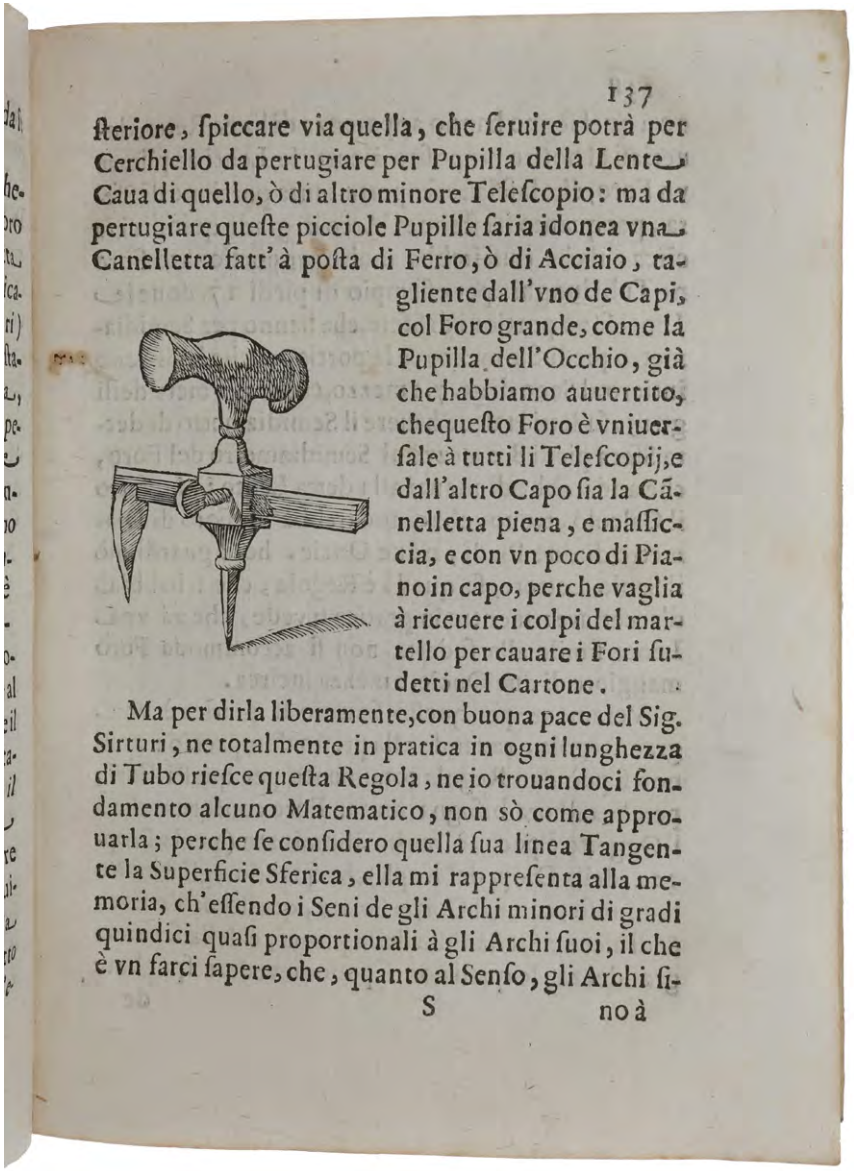
comes from experience. Manzini reveals, however, that he knows in advance its main properties because they are foretold by two propositions Kepler devoted to 'cryptical' instruments. The instrument will be shorter, as demonstrated in the *Dioptrice*, proposition 135 (it shows how to make a working Dutch telescope whose objective is of small convexity but shorter than the focal distance of the objective lens), and it will magnify less, by proposition 125 (which proves that the contiguous application of two equally convex lenses halves their focal distance). On the other hand, experience shows that images are clearer and more distinct and the field of vision is wider. Manzini fills ten pages of his book (from page 177 to 186) with recipes which seem to originate in a combination of practical knowledge and general theoretical rules provided by Kepler's *Dioptrice*" (Malet, pp. 292-4).

Little is known of Manzini's life. Born at Bologna on 5 October 1600, he graduated in philosophy at the city's University on 22 December 1625. A student of Giovanni Antonio Magini (1555-1617), he was a member of a group of Bolognese scientists who supported Galileo. In the first volume of his *Almagestum novum* (Bologna, 1651), Giovanni Battista Riccioli (1598-1671) praised Manzini's philosophical and scientific knowledge. He was in contact with many of the leading scientists of the time, including Mario Bettini (1582-1657) (with whom he performed some experiments), Bonaventura Cavalieri (1598-1647) (who was supported by Manzini in developing mathematics at the University of Bologna), and Divini. Two letters from Divini to to Manzini were published, *Lettera di E. D. all' Sig. Conte C. A. Manzini. Si ragguaglia di un nuovo lavoro e componimento di lenti che servono à occhialoni ó semplici ó composti* (1663) and *Lettera di E. D. intorno alle macchie nuovamente scoperte nel mese di Luglio 1665 nel Pianeta di Giove con suoi cannocchiali* (1666). In addition to the present work, Manzini published *Tabulae primi mobilis* (1626), which presented tables for the construction of astrological charts, *Stella Gonzaga sive geographica ad terrarum orbis ambitum,*



et meridianorum differentias (1654), containing his proposal for an improved method of measuring the earth, and *Le comete discorso* (1665), an account of his observations of the spectacular comets of 1664 and 1665. He died at Bologna in 1677.

Albert et al 1475; BL/STC *17th-century Italian Books* II, p. 530; BOA p. 137; Becker 153; NLM/Krivatsy 7389; Riccardi II 96; Wellcome IV, p. 48. Vincent Ilardi, *Renaissance vision from spectacles to telescopes*, 2007 (pp. 229-234); Antoni Malet, 'Kepler's legacy: telescopes and geometrical optics, 1611-1669', pp. 281-300, in *The Origins of the Telescope, van Helden et al* (eds.), 2010.



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ſteriore, ſpiccare via quella, che ſeruire potrà per Cerchiello da pertugiare per Pupilla della Lente Caua di quello, ò di altro minore Teleſcopio: ma da pertugiare queſte picciole Pupille ſaria idonea vna Canelletta fatt'à poſta di Ferro, ò di Acciaio, tagliente dall'vno de Capi, col Foro grande, come la Pupilla dell'Occhio, già che habbiamo auuertito, chequeſto Foro è vniuerſale à tutti li Teleſcopij, e dall'altro Capo ſia la Cãnelletta piena, e maſſiccia, e con vn poco di Piano in capo, perche vaglia à riceuere i colpi del martello per cauare i Fori ſudetti nel Cartone.

Ma per dirla liberamente, con buona pace del Sig. Sirturi, ne totalmente in pratica in ogni lunghezza di Tubo rieſce queſta Regola, ne io trouandoci fondamento alcuno Matematico, non sò come approuarla; perche ſe conſidero quella ſua linea Tangente la Superficie Sferica, ella mi rappreſenta alla memoria, ch'eſſendo i Seni de gli Archi minori di gradi quindici quaſi proportionali à gli Archi ſuoi, il che è vn farci ſapere, che, quanto al Senſo, gli Archi ſi-

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THE COPY OF CHARLES WHEATSTONE

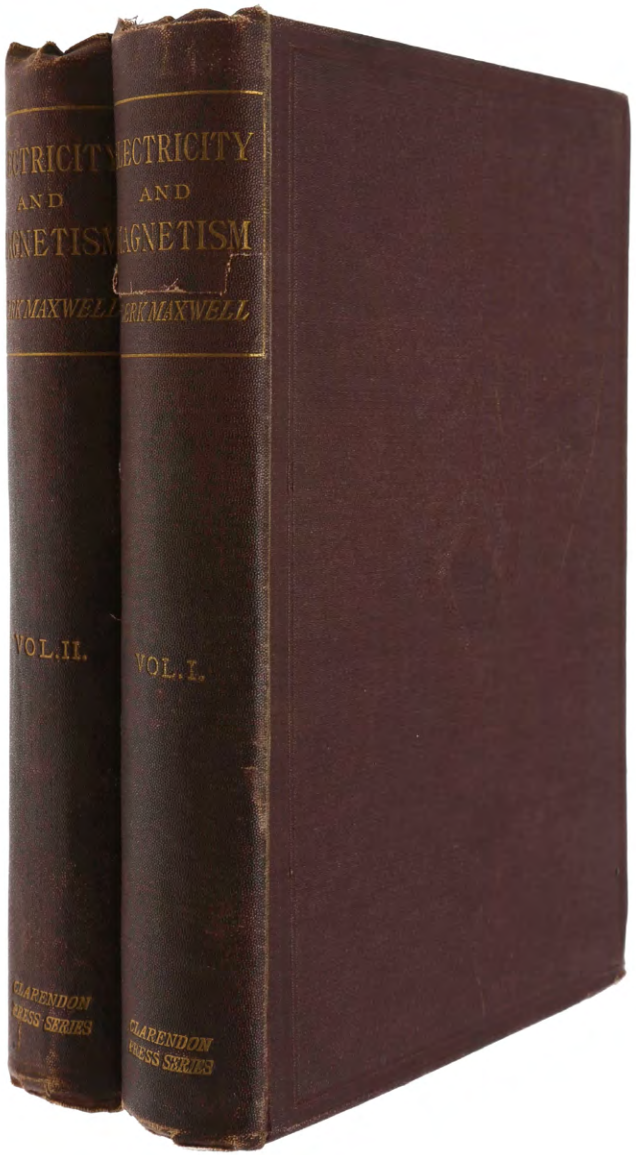
[PMM 355; Grolier *One Hundred Books Famous in Science* 72]

MAXWELL, James Clerk. *A Treatise on Electricity and Magnetism.* Oxford: Clarendon Press, 1873.

$20,000

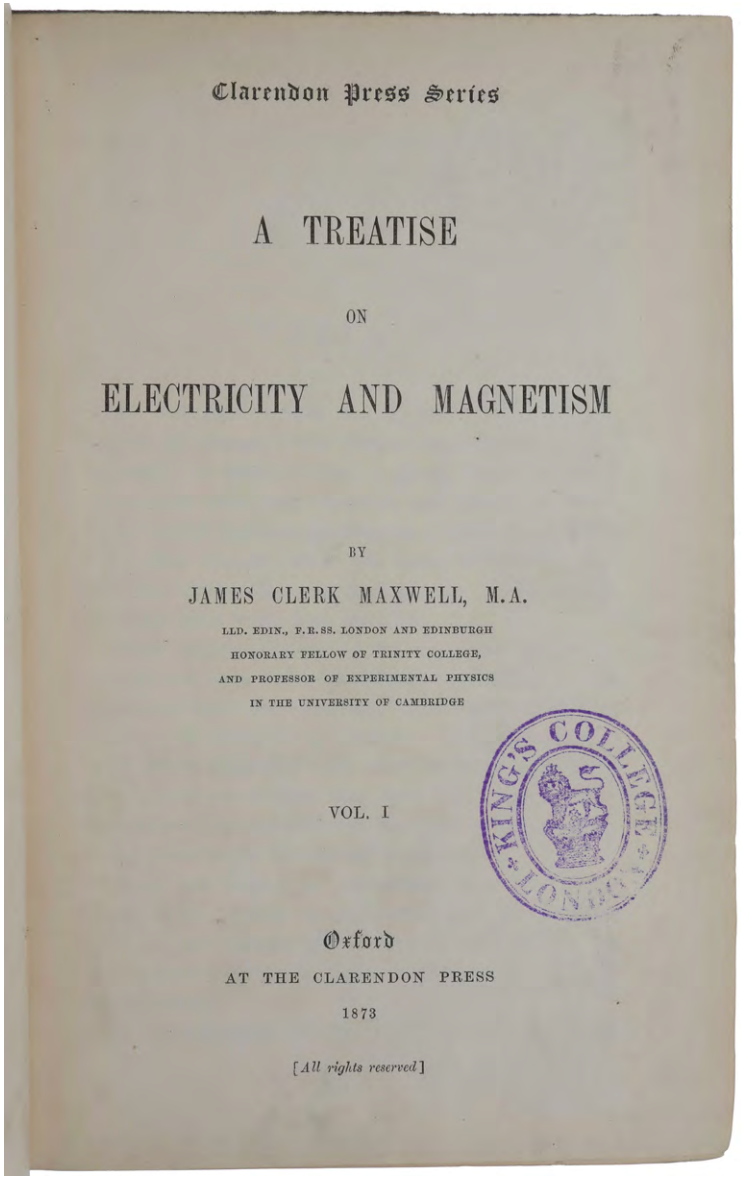
Two volumes, 8vo (223 x 140 mm), pp. xxix, [3], 425, [5] and 13 plates (errata slip before leaf B1); xxiii [1], 444, [2] and 7 plates, 15 [1] (advertisements). Original publisher's blind-stamped plum cloth, small paper tear with loss to lower outer corner of p. xxiii/xxiv of volume 1, lacks errata slips, inner hinges broken, rubbed, some corner wear, volume 1 torn at head of spine without loss, volume 2 frayed on joints. This first issue can be distinguished from the second from the eight leaves of publisher's advertisements in the rear of volume 2; the entry for Maxwell's Treatise itself states "just published."

First edition, first issue, and a wonderful association copy, of Maxwell's presentation of his theory of electromagnetism, advancing ideas that would become essential for modern physics, including the landmark "hypothesis that light and electricity are the same in their ultimate nature" (Grolier/Horblit). "This treatise did for electromagnetism what Newton's *Principia* had done from classical mechanics. It not only provided the mathematical tools for the investigation and representation of the whole electromagnetic theory, but it altered the very framework of both theoretical and experimental physics. It was this work that finally displaced action-at-a-distance physics and substituted the physics of the field" (*Historical Encyclopedia of Natural and Mathematical Sciences*, p. 2539). "From a long view of the history of mankind — seen from, say, ten thousand years from now —



there can be little doubt that the most significant event of the 19th century will be judged as Maxwell's discovery of the laws of electrodynamics" (R. P. Feynman, in *The Feynman Lectures on Physics* II (1964), p. 1-6). "[Maxwell] may well be judged the greatest theoretical physicist of the 19th century ... Einstein's work on relativity was founded directly upon Maxwell's electromagnetic theory; it was this that led him to equate Faraday with Galileo and Maxwell with Newton" (PMM). "Einstein summed up Maxwell's achievement in 1931 on the occasion of the centenary of Maxwell's birth: 'We may say that, before Maxwell, Physical Reality, in so far as it was to represent the process of nature, was thought of as consisting in material particles, whose variations consist only in movements governed by [ordinary] differential equations. Since Maxwell's time, Physical Reality has been thought of as represented by continuous fields, governed by partial differential equations, and not capable of any mechanical interpretation. This change in the conception of Reality is the most profound and the most fruitful that physics has experienced since the time of Newton'" (Longair).

Provenance: The Wheatstone Collection, King's College, London (ink stamps 'King's College London' on recto, 'Kings College London withdrawn' on verso, of title of both volumes, bookplates of the Wheatstone Collection on front paste-downs, shelf-mark in pencil on half-titles). Maxwell was Professor of Natural Philosophy at King's from 1860 to 1865. This was probably the most productive period of his career, during which he displayed the world's first light-fast colour photograph, further developed his ideas on the viscosity of gases, and proposed a system for defining physical quantities, now known as dimensional analysis. But this time is especially noteworthy for the advances Maxwell made in the fields of electricity and magnetism. He examined the nature of both electric and magnetic fields in his two-part paper 'On physical lines of force,' which was published in 1861. In it he provided a conceptual model for electromagnetic induction, consisting of tiny spinning cells of magnetic flux. In two subsequent parts,


Clarendon Press Series

A TREATISE

ON

ELECTRICITY AND MAGNETISM

BY

JAMES CLERK MAXWELL, M.A.

LLD. EDIN., F.R.SS. LONDON AND EDINBURGH
HONORARY FELLOW OF TRINITY COLLEGE,
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KING'S COLLEGE LONDON

VOL. I

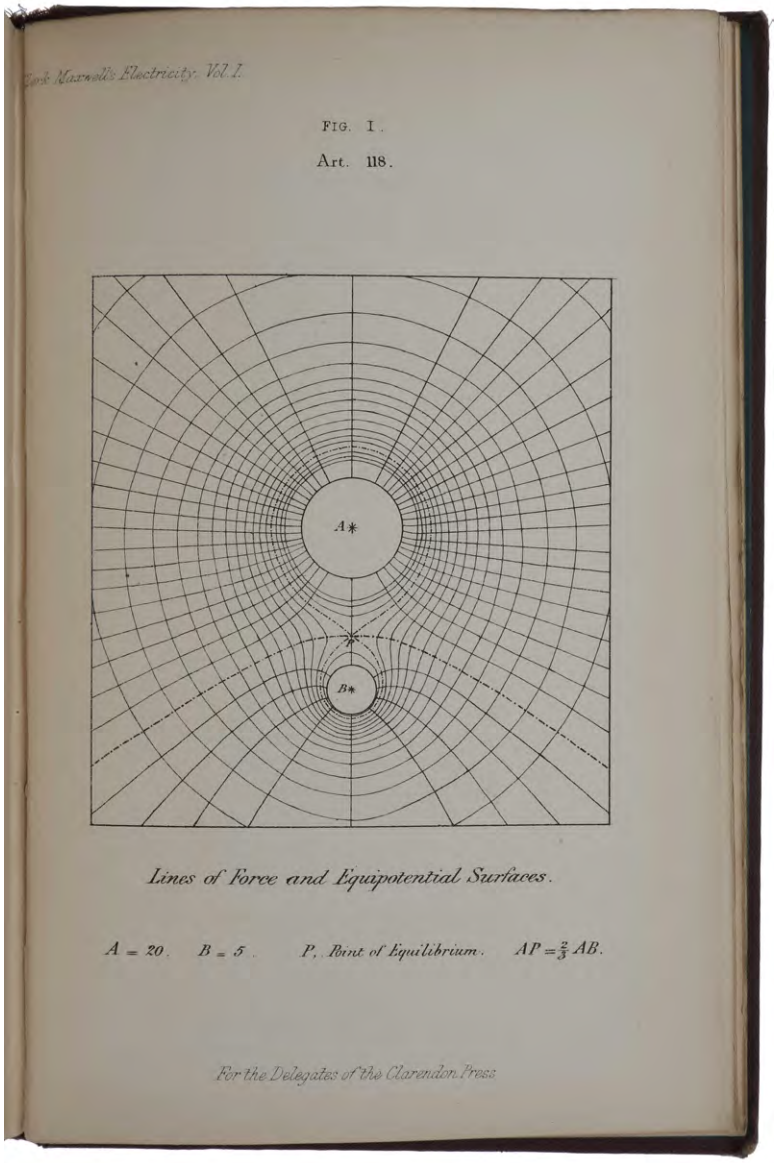
Oxford
AT THE CLARENDON PRESS
1873

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published in 1862, he introduced the concept of displacement current and gave an explanation for the Faraday effect, the rotation of the plane of the polarization of light in a magnetic field. These researches culminated in the publication of his greatest work, 'A dynamical theory of the electromagnetic field,' in 1865. "According to a famous passage in its preface, the *Treatise* (1873) represented the outcome of this programme" (*Landmark Writings*, p. 569). Charles Wheatstone (1802-75) was Maxwell's colleague at King's in the 1860s, having been appointed Professor of Natural Philosophy there in 1834. He was responsible for a number of inventions but is perhaps best known today for the 'Wheatstone bridge,' a device that accurately measured electrical resistance and became widely used in laboratories. He was knighted in 1868.

"Maxwell's great paper of 1865 established his dynamical theory of the electromagnetic field. The origins of the paper lay in his earlier papers of 1856, in which he began the mathematical elaboration of Faraday's researches into electromagnetism, and of 1861–1862, in which the displacement current was introduced. These earlier works were based upon mechanical analogies. In the paper of 1865, the focus shifts to the role of the fields themselves as a description of electromagnetic phenomena. The somewhat artificial mechanical models by which he had arrived at his field equations a few years earlier were stripped away. Maxwell's introduction of the concept of fields to explain physical phenomena provided the essential link between the mechanical world of Newtonian physics and the theory of fields, as elaborated by Einstein and others, which lies at the heart of twentieth and twenty-first century physics" (Longair).

The 1865 paper "provided a new theoretical framework for the subject, based on experiment and a few general dynamical principles, from which the propagation of electromagnetic waves through space followed without any special assumptions … In the *Treatise* Maxwell extended the dynamical formalism by a more

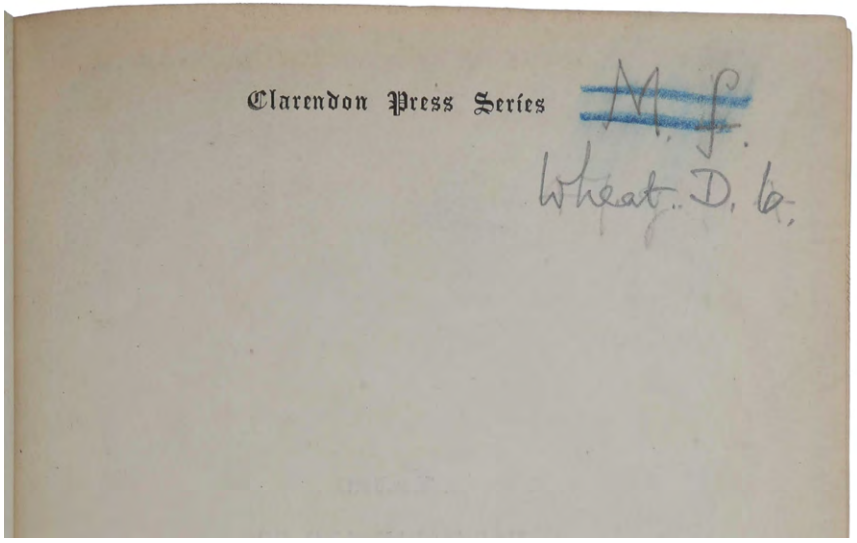


thoroughgoing application of Lagrange's equations than he had attempted in 1865. His doing so coincided with a general movement among British and European mathematicians about then toward wider use of the methods of analytical dynamics in physical problems … Using arguments extraordinarily modern in flavor about the symmetry and vector structure of the terms, he expressed the Lagrangian for an electromagnetic system in its most general form. [George] Green and others had developed similar arguments in studying the dynamics of the luminiferous ether, but the use Maxwell made of Lagrangian techniques was new to the point of being almost a new approach to physical theory—though many years were to pass before other physicists fully exploited the ground he had broken …

"In 1865, and again in the *Treatise*, Maxwell's next step after completing the dynamical analogy was to develop a group of eight equations describing the electromagnetic field … The principle they embody is that electromagnetic processes are transmitted by the separate and independent action of each charge (or magnetized body) on the surrounding space rather than by direct action at a distance. Formulas for the forces between moving charged bodies may indeed be derived from Maxwell's equations, but the action is not along the line joining them and can be reconciled with dynamical principles only by taking into account the exchange of momentum with the field" (DSB).

"Maxwell once remarked that the aim of his *Treatise* was not to expound the final view of his electromagnetic theory, which he had developed in a series of five major papers between 1855 and 1868; rather it was to educate himself by presenting a view of the stage he had reached in his thinking. Accordingly, the work is loosely organized on historical and experimental, rather than systematically deductive, lines. It extended Maxwell's ideas beyond the scope of his earlier work in many directions, producing a highly fecund (if somewhat confusing) demonstration of the special importance of electricity to physics as a whole. He began the investigation of moving frames of reference, which in Einstein's hands were to revolutionize physics; gave proofs of the existence of electromagnetic waves that paved the way for Hertz's discovery of radio waves; worked out connections between electrical and optical qualities of bodies that would lead to modern solid-state physics; and applied Tait's quaternion formulae to the field equations, out of which Heaviside and Gibbs would develop vector analysis" (Norman).

DSB IX, p.198; Grolier/Horblit 72; Norman 1666 (second issue); PMM 355; Poggendorff III, p. 889; Wheeler Gift Catalogue 1872. Achard, 'James Clerk Maxwell, A Treatise on Electricity and Magnetism, First Edition (1873),' Chapter 44 in *Landmark Writings in Western Mathematics* 1640-1940, Grattan-Guinness (ed.), 2005. Malcolm Longair, '. . . a paper . . . I hold to be great guns': a commentary on Maxwell (1865) 'A dynamical theory of the electromagnetic field'. *Philosophical Transactions* A 373, No. 2039, 13 April 2015.



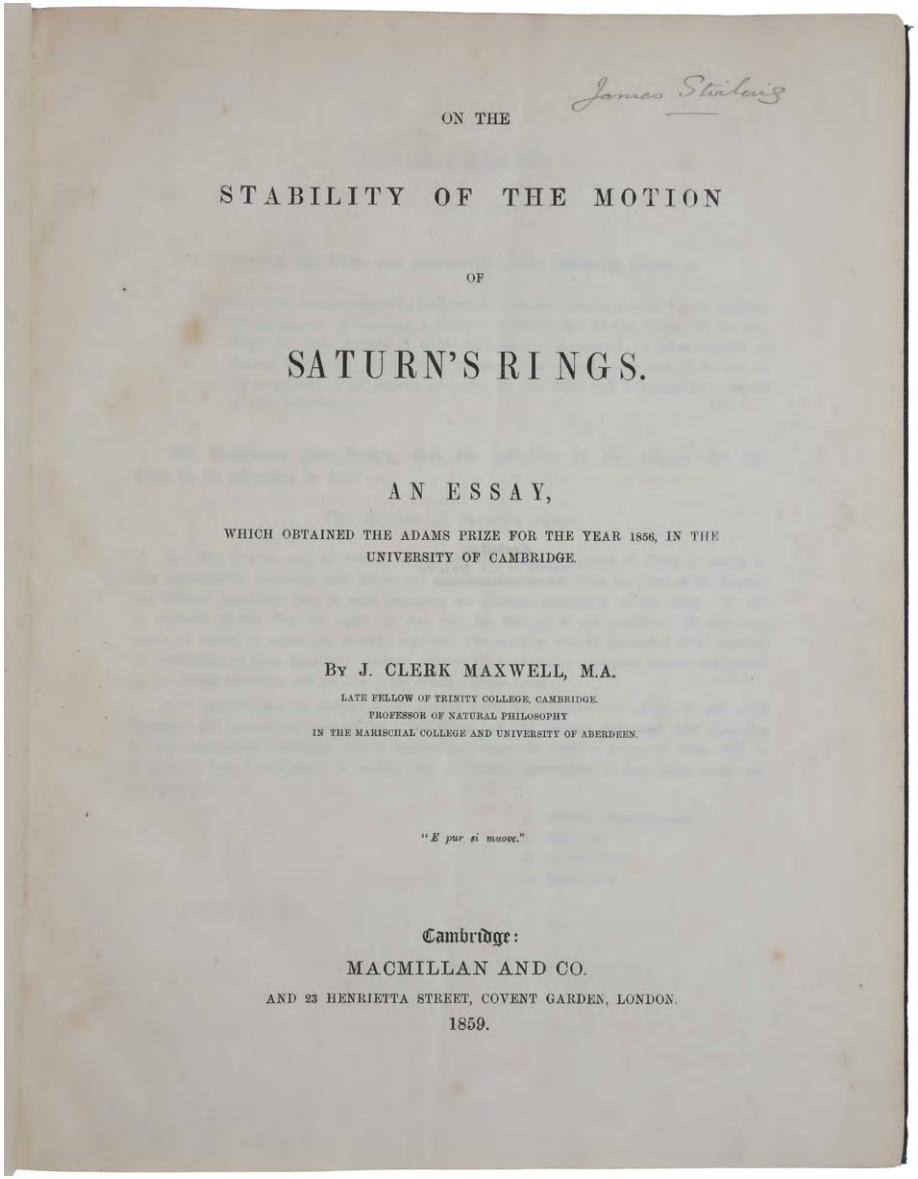
MAXWELL ON SATURN'S RINGS

MAXWELL, James Clerk. *On the stability of the motion of Saturn's rings. An essay which obtained the Adams prize for the year 1856, in the University of Cambridge.* [Bound with journal extracts of five other papers by Maxwell, see below]. Cambridge: Macmillan & Co, 1859.

$8,500

[Saturn's rings:] 4to (270 x 215 mm), pp. vii, 71 with one plate. It is likely that this work was issued as a disbound pamphlet (the copy listed by Sotheran is described as 'sewn'). 19th century blue cloth with gilt spine lettering. Very fine and clean througout.

First edition, very rare in commerce. "The work that made Maxwell's reputation in his day, amongst his contemporaries in Britain at least, was his Adams Prize essay 'On the Stability of the Motion of Saturn's Rings' … The prize topic was announced by the University of Cambridge in 1855 for submission in 1857, following correspondence between James Challis, Director of the Cambridge Observatory, and William Thomson [Lord Kelvin]. It was a subject that was particularly topical on both sides of the Atlantic. Maxwell's submission, one third of a kilogram in weight of closely argued mathematical physics of the highest calibre, was received in December 1856 and he was awarded the prize in 1857, barely three years after he was an undergraduate. Maxwell did not publish his submitted manuscript, but spent more time on developing it while at Aberdeen than on any other research topic. He had some lengthy correspondence with William Thomson in particular, notably on issues connected with the stability of the rings. The final version was published as a free-standing work in 1859 [the offered work]. His study was a theoretical tour-de-force, effectively Maxwell's trial



ON THE

STABILITY OF THE MOTION

OF

SATURN'S RINGS.

AN ESSAY,

WHICH OBTAINED THE ADAMS PRIZE FOR THE YEAR 1856, IN THE UNIVERSITY OF CAMBRIDGE.

By J. CLERK MAXWELL, M.A.

LATE FELLOW OF TRINITY COLLEGE, CAMBRIDGE.
PROFESSOR OF NATURAL PHILOSOPHY
IN THE MARISCHAL COLLEGE AND UNIVERSITY OF ABERDEEN.

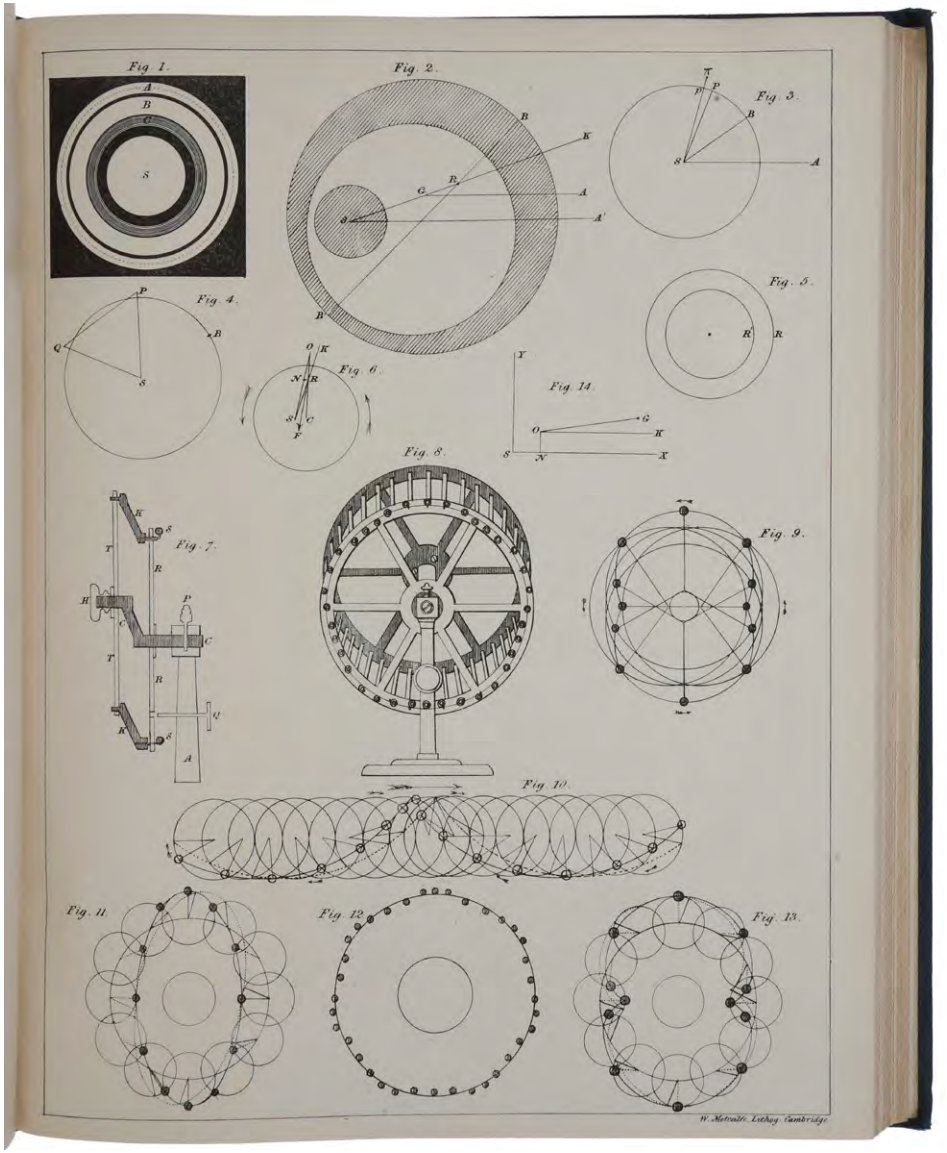
"*E pur si muove.*"

Cambridge:
MACMILLAN AND CO.
AND 23 HENRIETTA STREET, COVENT GARDEN, LONDON.
1859.

piece submitted to the guild of the elect professoriate in Britain as an entrance test. Challis, Stokes, Airy, Thomson and others absorbed the argument. The essay is largely an exploration of the stability of a number of physical models for the constitution of the rings. Maxwell makes significant deductions on points of detail, and overall his analysis proved beyond doubt to his contemporaries that 'The final result, therefore, of the mechanical theory is, that the only system of rings which can exist is one composed of an indefinite number of unconnected particles, revolving around the planet with different velocities according to their respective distances'. This before a single clear photograph of Saturn's rings had been taken … Maxwell's work has spawned the modern theory of planetary discs and astronomical accretion discs that are found around dwarf stars orbiting close to giant stars and in matter orbiting black holes" (Flood *et al*, p. 31). Although reasonably well represented in institutional collections, this is a very rare work in commerce: ABPC/RBH list only one copy, and there was no copy in any of the major collections of scientific books that have been catalogued in recent decades (Barchas, Honeyman, Norman, etc.).

Provenance: James Hutchinson Stirling (1820-1909) (signature on title of *Saturn's rings*, enclosed signed autograph letter from him dated 24 April, 1894, and manuscript table of contents). Having studied medicine, history and classics at the University of Glasgow, Stirling established a medical practice, and later became a Fellow of the Royal College of Surgeons of Edinburgh. After receiving a large inheritance from his father's estate in 1851, Stirling left medicine and thereafter devoted himself to philosophy, notably the writings of Hegel. In the enclosed letter to 'Davey', Stirling writes: "I have much pleasure in sending you a donation towards the friends of King's College Hospital."

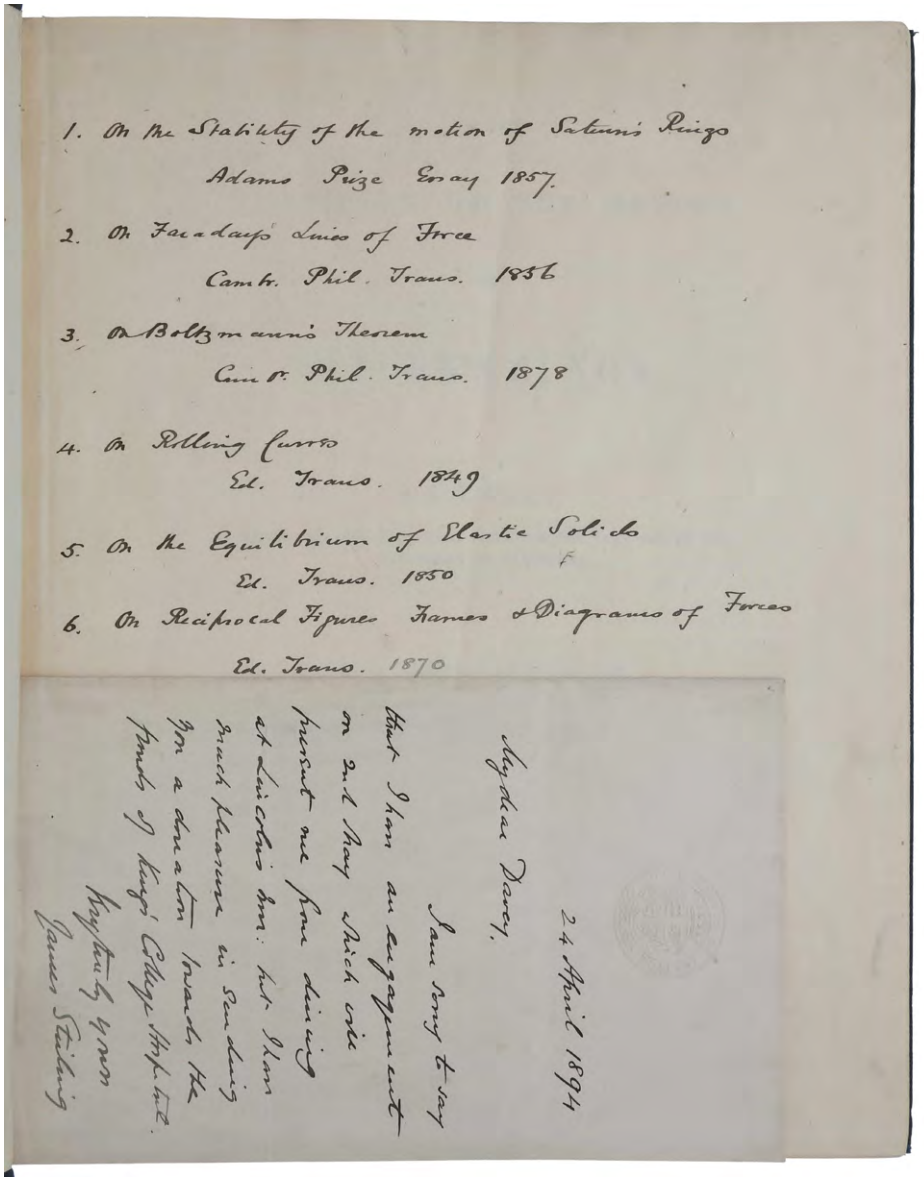
First observed through a telescope by Galileo in 1610, Saturn's brilliant rings were initially thought to be additional "stars," or perhaps solid protrusions on either



side of the planet like the handles of a cup. It wasn't until 1659 that Christiaan Huygens determined that the handles were actually an encircling ring not attached to Saturn but separated from it the same distance all around. Sixteen years later Gian Domenico Cassini observed the largest gap in Saturn's rings (which was named after him) and correctly determined that they are divided into sections. Galileo, Huygens, and Cassini all assumed the rings to be solid, and this remained the situation until the problem was taken up by Maxwell.

Maxwell (1831-79) entered the University of Cambridge as an undergraduate in 1850, graduating with a degree in mathematics in 1854, and winning the Smith Prize the following year. In October 1855 he was made a Fellow of Trinity College, but in November of the following year he left Cambridge after accepting the Chair of Mathematics at Marischal College, Aberdeen. "In his baggage on that trip north, Maxwell carried three pieces of research. The most complete was his first theoretical and experimental work on colour; the second, the nearly completed paper 'On Faraday's lines of force'; and the third, which he had just begun, 'Essay on Saturn's rings'. They were in disparate realms of physical theory, yet they all displayed the mathematical principles and practices he had developed at Cambridge in the previous three gruelling years. The experimental foundations of his work were from his apprenticeship in his own laboratory set up in a cottage on his family estate of Glenlair in Galloway and encouraged by James David Forbes (1809–68) while he was a student at Edinburgh University.

"At Aberdeen Maxwell focused his research energies on Saturn's rings, the subject of the 1855 Adams Prize essay. The Adams Prize competition, on a subject in pure mathematics, astronomy or natural philosophy, was announced once every 2 years and was open to all who had been admitted to a degree at Cambridge. The two major examiners for this competition were William Thomson (1821–1907) and James Challis (1803–82). Without considering the planet's satellites,


1. On the Stability of the motion of Saturn's Rings
Adams Prize Essay 1857.
2. On Faradays Lines of Force
Camb. Phil. Trans. 1856
3. On Boltzmann's Theorem
Camb. Phil. Trans. 1878
4. On Rolling Curves
Ed. Trans. 1849
5. On the Equilibrium of Elastic Solids
Ed. Trans. 1850
6. On Reciprocal Figures Frames & Diagrams of Forces
Ed. Trans. 1870
24 April 1894
My dear Darcy,
I am sorry to say that I am engagement on 2nd May which will prevent me from dining at Luncheon &c: but I am much pleased to see during a vacation towards the funds of King's College Hospital.
Yours truly
James Stirling

the candidates had to examine the stability of Saturn's rings, assuming them to be concentric with the planet. They could be solid, liquid or 'in part aeriform'. In the 1850s, Saturn and other planets were scrutinized by telescopes as they had been for some 200 years. The complex, yet predictable, path of a planet was still its most characteristic feature. Hence the emphasis on stability in the setting of the Adams Prize question. In addition, the contestants had to take into account the third, inner 'dusky' ring, recently discovered by the American astronomer George Bond (1825–1865) at Harvard. They had two years to complete their investigations.

"Drawn from the latest available astronomical observations, the image of Saturn was of a bright white, marble smooth body, around whose equator were two flat rings. If you had a telescope as fine as that of George Bond, you could discern the third ring inside the other two, and the separations within the inner ones and the division of the outmost ring into two sets of thin rings. The Adams Prize question was a timely subject on a current theoretical puzzle in astronomy.

"It was also just the subject for a competition to honour the work of the still active astronomer John Couch Adams (1819–92). Honouring a living scientist was still quite extraordinary; but then so was Adams. In 1845, using his formidable mathematical skills he had postulated the existence of the planet Neptune, although he was happy to acknowledge Urbain Jean Joseph Leverrier's priority in actually sighting it. There are other instances of his lack of ambition and absolute control of the intricate mathematics necessary for theoretical planetary astronomy. The 1855 prize questions echoed the kinds of problems he tackled in his career and recognized 'his humour, modesty and grace', as his colleagues at St John's College put it in 1848 when establishing the prize in his name …

"As a young, recent Cambridge graduate, who was placed second in the tripos, then first in the Smith Prize competition, Maxwell still had to consolidate his reputation. His work on Saturn's rings completed at Aberdeen did just that, something neither his work on Faraday's lines of force nor that on colour accomplished in his lifetime. He began his research on Saturn's rings with the obvious theoretical work on the subject, Pierre Simon, Marquis de Laplace's, four-volume [sic] magnum opus, *Traité de Mécanique Céleste* (1799–1825). The pattern of Laplace's work on physical or astronomical problems was to develop the mathematics abstractly, sometimes with many arbitrary functions and constants, and then simplify the expressions in the face of known data. His four-volume treatise was the most complete, and important, mathematical investigation of the heavens. It was the reference Maxwell had to consult and contradict only with very good reasons.

"In his investigation of Saturn's rings, Laplace considered the stability of a single solid ring. He found that it would collapse into the planet if it was at rest. To meet the observed stability of the system meant that solid rings revolved about Saturn. Laplace also gave upper limits on their density. By 1855, in the light of recent observations, the structure and stability of Saturn's rings needed revisiting. Any investigation would require the exercise of intricate mathematics tied to sure-footed physical arguments.

"In his Adams Prize essay, Maxwell first disposed of Laplace's argument for the stability of the rotating solid ring. It was incomplete. He found several types of solid rings unstable. For stability, this ring had to be weighted at one point by a mass approximately 41/2 times that of the ring. As Maxwell remarked, such a mass would surely be obvious to astronomers and none had been reported. In addition, a slight change in the load or in its position would shatter the ring. The problem of a liquid ring was that perturbations would tend to break the liquid into drops; these would accumulate and destroy the ring's stability.

"He was left with rings made up of single 'satellites' of equal size revolving about Saturn and found that considerations of stability severely limited the masses of the satellites. Maxwell reduced the rings to one ring of satellites. Any satellite could be displaced from its mean positions radially, normally and tangentially to the ring. Instead of tackling the impossible task of dealing with their displacements individually, Maxwell also used Fourier analysis to show the propagation of waves in such a ring, which, under certain conditions, would not lead to collisions between the satellites and thus were stable. He used Fourier analysis to express the displacements of a single satellite as functions of time. He found that the tangential displacement was enhanced by the attraction of the satellite towards which it was moving. If at the same time the satellite was moving radially outwards, it would fall behind other members in the ring. The result was that the satellite would fall behind the others, its motions would be retarded and it would move inward again. The radial and tangential motions were coupled. Small disturbances normal or tangential to the plane of the ring would not disrupt it but lead to waves being propagated around it. Under the condition of stability these coupled motions led to four different kinds of waves each with its own velocity traversing the circumference of the ring. He then considered the motions of the satellites under unstable conditions.

"However, Saturn has rings and he turned to the case of two such rings rotating about Saturn at their appropriate speeds. Maxwell found that in most cases their motions were stable, except for certain ratios of their radii. Then, the resonant waves would grow indefinitely until they would break up and their satellites would fly off in all directions colliding with each other in the process. Under stable conditions the two satellite rings produce eight different kinds of waves of different frequencies propagated around such rings.

"Maxwell noted difficulties in taking the analysis further. For certain ratios of the radii of the rings, a wave of one type in one of the rings would come into resonance with a wave of the other type in the other rings. These would grow indefinitely and the rings 'will be thrown into confusion' and the satellites 'would fly off in all directions and collide with members of other rings'. He understood that he could not describe the motions of a ring, or rings, made up of randomly moving particles. He also understood that all that he could do was examine the stability of a ring or systems of rings of particles under various disturbing forces.

"He concluded by noting the inability of dynamics to address this last problem: 'When we come to deal with collisions among bodies of unknown number, size, and shape, we can no longer trace the mathematical laws of their motion with distinctness'. Mechanics cannot deal with collisions among many bodies flying around randomly. All he could do was gather the possible scenarios for stability and instability.

"Maxwell did not consider a ring made up of independent particles, something that he is sometimes assumed to have done. He had shown the circumstances under which waves would produce collisions between the particles. However, we must remember that Maxwell's object was none other than to demonstrate the conditions for stability. Within the limits of dynamics, Maxwell concluded that the only possible structure for Saturn's rings was concentric rings of satellites, each revolving with an appropriate speed. They would act on one another and produce perturbations in their motions. He also demonstrated the existence of conditions under which the motions of one or both rings became unstable. To further illustrate this system Maxwell had a model, constructed by the Aberdeen instrument makers Smith and Ramage, to demonstrate the motions of a ring of 36 satellites, which he used to illustrate his results on satellites' motions" (Garber).

In 2004 the NASA Cassini probe to Saturn showed that Maxwell's conclusion about the structure of the rings was correct. He is commemorated by having a feature of the rings named after him – the 'Maxwell Gap' within the C ring.

Bound with Maxwell's Adams Prize essay are extracts of the following five papers by Maxwell:

1. On Faraday's Lines of Force. (Read Dec. 10, 1855, and Feb. 11, 1856). *Transactions of the Cambridge Philosophical Society*, Vol. 10, 1856, pp. 27-83. Maxwell's first paper on electromagnetism. "Maxwell's first paper, "On Faraday's Line of Force" (1855–1856), was divided into two parts … Part 1 was an exposition of the analogy between lines of force and streamlines in an incompressible fluid … Part 2 covered electromagnetism proper. In it Maxwell developed a new formal theory of electromagnetic processes … The 1856 paper has been eclipsed by Maxwell's later work, but its originality and importance are greater than is usually thought. Besides interpreting Faraday's work and giving the electrotonic function, it contained the germ of a number of ideas which Maxwell was to revive or modify in 1868 and later: (1) an integral representation of the field equations, (2) the treatment of electrical action as analogous to the motion of an incompressible fluid, (3) the classification of vector functions into forces and fluxes, and (4) an interesting formal symmetry in the equations connecting **A**, **B**, **E**, and **H**, different from the symmetry commonly recognized in the completed field equations. The paper ended with solutions to a series of problems, including an application of the electrotonic function to calculate the action of a magnetic field on a spinning conducting sphere" (DSB).

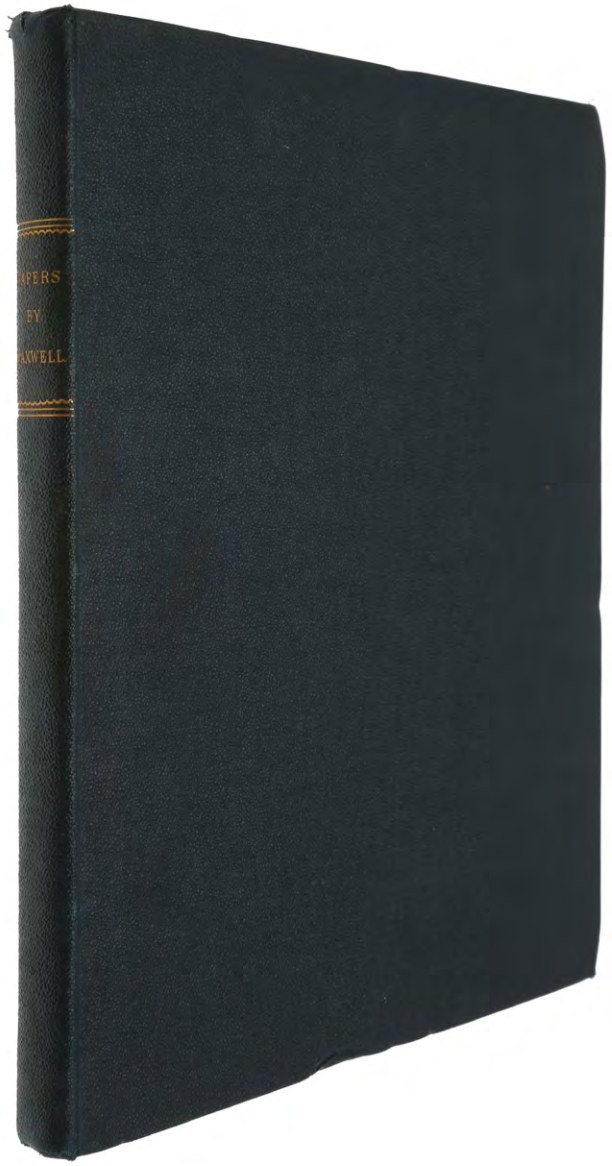
2. On Boltzmann's theorem on the average distribution of energy in a system of material points. (Read May 6, 1878). *Transactions of the Cambridge Philosophical Society*, Vol. 13, 1878, 547-570. "During his last two years Maxwell returned to molecular physics in earnest and produced two full-length papers, strikingly different in scope, each among the most powerful he ever wrote. The first, "On Boltzmann's Theorem on the Average Distribution of Energy in a System of Material Points," followed a line of thought started by Boltzmann, who in 1868 had offered a new conjectural derivation of the distribution law based on combinatorial theory … Maxwell now gave his own investigation of the statistical problem … and adopted the device of representing the state of motion of a large number n of particles by the location of a single point in a "phase-space" of $2n$ dimensions, the coordinates of which are the positions and momenta of the particles … Maxwell then postulated, as Boltzmann had done, that the system would in the course of time pass through every phase of motion consistent with the energy equation … The validity of this hypothesis, sometimes called the ergodic hypothesis, was afterwards much discussed, often with considerable misrepresentation of Maxwell's opinions …Together with Boltzmann's articles this paper of Maxwell's marks the emergence of statistical mechanics as an independent science. One feature of the paper "On Boltzmann's Theorem," eminently characteristic of Maxwell, is that the analysis, for all its abstraction, ends with a concrete suggestion for an experiment, based on considering the rotational degrees of freedom. Maxwell proved that the densities of the constituent components in a rotating mixture of gases would be the same as if each gas were present by itself. Hence gaseous mixtures could be separated by means of a centrifuge … Many years later it became a standard technique for separating gases commercially" (DSB).

3. On the Theory of Rolling Curves. Communicated by Professor Kelland. (Read, 19th Feb. 1849). *Transactions of the Royal Society of Edinburgh*, Vol. 16, 1849, pp. 519-540. This paper and the next were completed while

Maxwell was still a student at the Edinburgh Academy. Both papers were read before the Society by somebody else because "it was not thought proper for a boy in a round jacket to mount the rostrum there."

4. On the Equilibrium of Elastic Solids. (Read 18th February, 1850). *Transactions of the Royal Society of Edinburgh*, Vol. 20, 1853, pp. 87-120.
4. On reciprocal figures, frames, and diagrams of forces. *Transactions of the Royal Society of Edinburgh*, Vol. 26, 1853, pp. 1-40, with three plates. "During his regular lectures at King's College, London, Maxwell was accustomed to present some of Rankine's work on the calculation of stresses in frameworks. In 1864 Rankine offered an important new theorem, which Maxwell then developed into a geometrical discussion entitled "On Reciprocal Figures and Diagrams of Forces." The principle was an extension of the well-known triangle of forces in statics. Corresponding to any rectilinear figure, another figure may be drawn with lines parallel to the first, but arranged so that lines converging to a point in one figure form closed polygons in the other. The lengths of lines in the polygon supply the ratios of forces needed to maintain the original point in equilibrium. Maxwell gave a method for developing complex figures systematically, and derived a series of general theorems on properties of reciprocal figures in two and three dimensions … Reciprocal theorems and diagrams are useful in many fields of science besides elasticity" (DSB).

For Maxwell on Saturn's rings, see: Houzeau & Lancaster, II, p. 1438; Sotheran 11673. Brush, Everitt & Garber, *Maxwell on Saturn's rings*. Cambridge: MIT Press, 1983; Flood, McCartney & Whittaker (eds.); Garber, 'Subjects great and small: Maxwell on Saturn's rings and kinetic theory,' *Philosophical Transactions*, vol. A366 (2008), pp. 1697-1705. For Maxwell's other contributions, see *James Clerk Maxwell. Perspectives on his Life and Work*, Oxford: University Press, 2014.



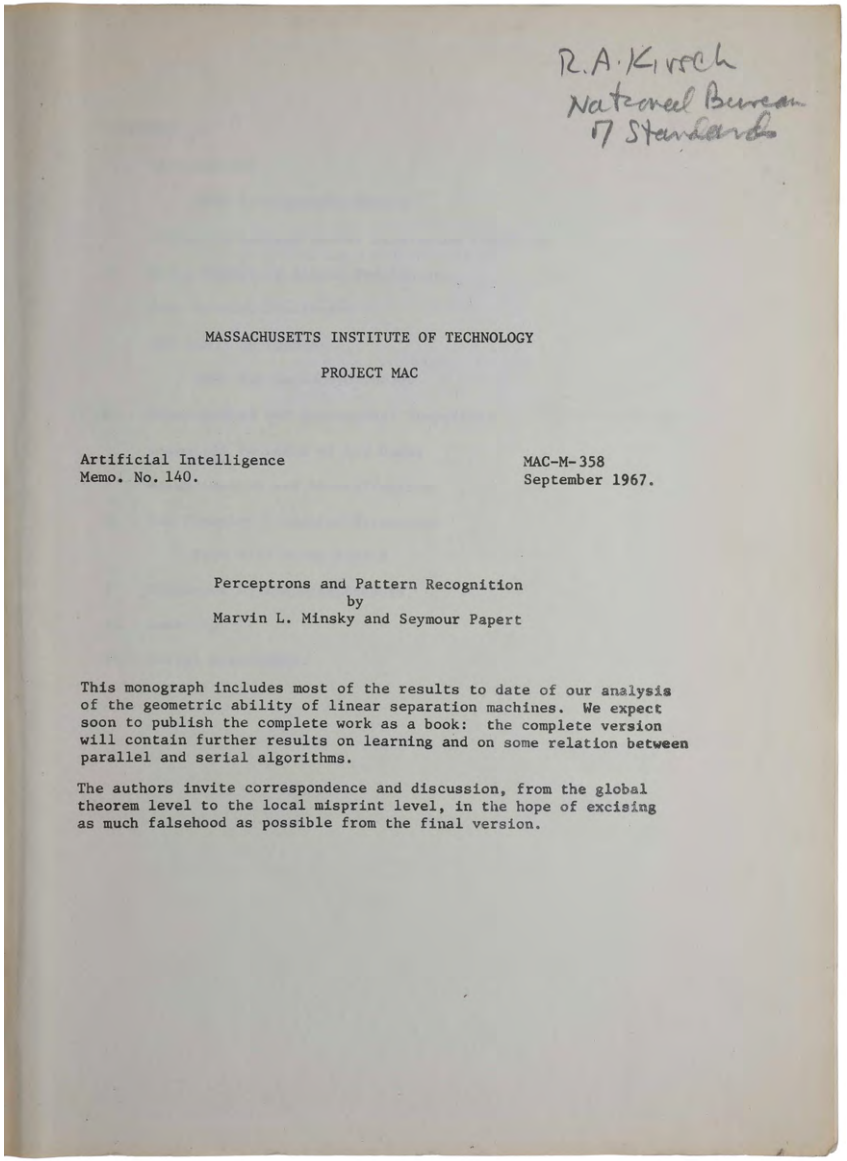
ARTIFICIAL INTELLIGENCE

MINSKY, Marvin & PAPERT, Seymour. *Perceptrons and Pattern Recognition. Artificial Intelligence Memo no. 140. MAC-M-358. Project MAC.* Cambridge, MA, September 1967.

$17,500

4to (278 x 214 mm), pp. [viii], 26; 15; 11; 8; 10; 3; 26; 24;19; 9; 14; 12; 8 (each of the 11 lectures is separately-paginated). Stapled as issued into clear plastic covers (front cover loose at two of the staples), punch holes for ring binder.

First edition, extremely rare pre-publication issue, of this important early work in Artificial Intelligence (AI), containing the first systematic study of parallelism in computation. It was first published in book form in 1969 as *Perceptrons. An Introduction to Computational Geometry* (second edition 1987). It "has remained a classical work on threshold automata networks for nearly two decades. It marked a historical turn in artificial intelligence, and it is required reading for anyone who wants to understand the connectionist counterrevolution that is going on today. Artificial-intelligence research, which for a time concentrated on the programming of Von Neumann computers, is swinging back to the idea that intelligence might emerge from the activity of networks of neuron-like entities. Minsky and Papert's book was the first example of a mathematical analysis carried far enough to show the exact limitations of a class of computing machines that could seriously be considered as models of the brain. Now the new developments in mathematical tools, the recent interest of physicists in the theory of disordered matter, the new insights into and psychological models of how the brain works, and the evolution of fast computers that can simulate networks of automata have given *Perceptrons* new importance" (from the introduction to the second



R.A. Kirsch
National Bureau
of Standards

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

PROJECT MAC

Artificial Intelligence
Memo. No. 140.

MAC-M-358
September 1967.

Perceptrons and Pattern Recognition
by
Marvin L. Minsky and Seymour Papert

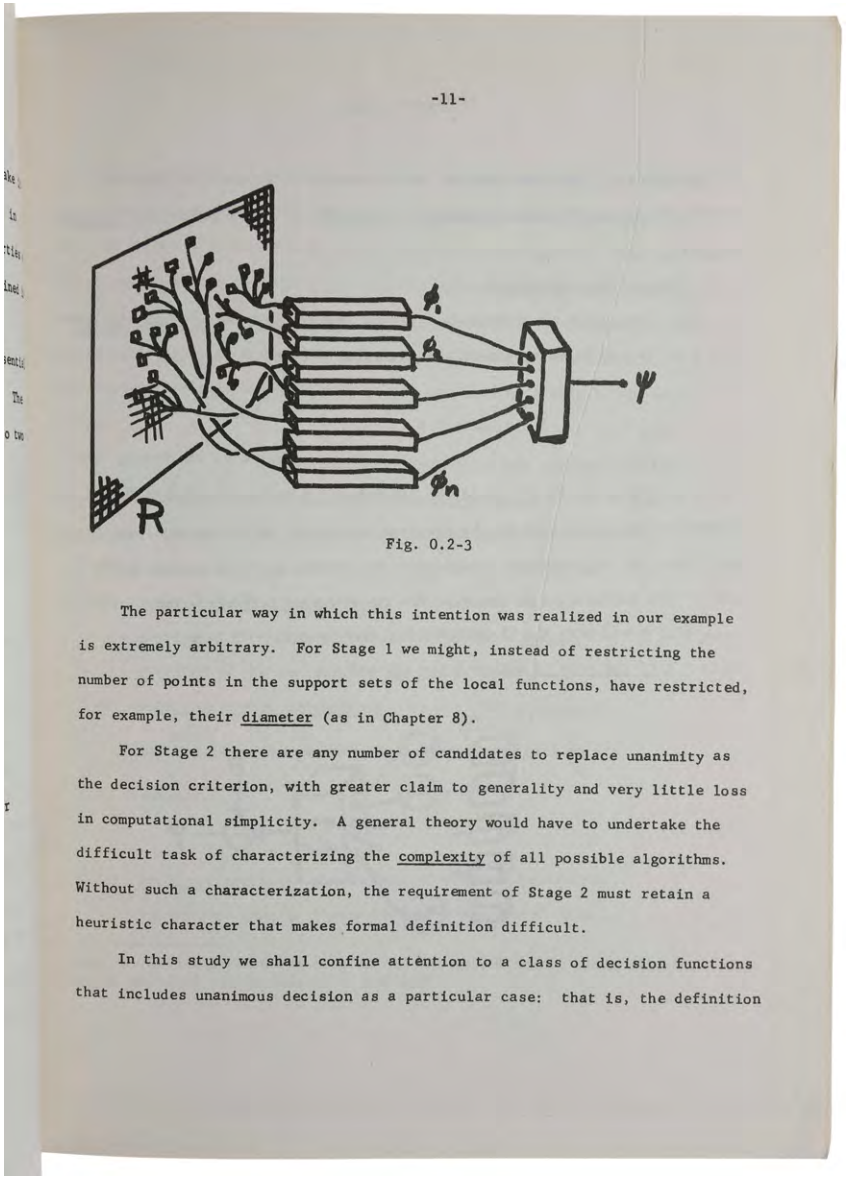
This monograph includes most of the results to date of our analysis of the geometric ability of linear separation machines. We expect soon to publish the complete work as a book: the complete version will contain further results on learning and on some relation between parallel and serial algorithms.

The authors invite correspondence and discussion, from the global theorem level to the local misprint level, in the hope of excising as much falsehood as possible from the final version.

book edition). This pre-publication issue is extremely rare and seems to be little known. Most discussions of Minsky & Papert's work refer only to the 1969 book edition. OCLC lists only two copies (Stanford and National Research Council Canada). There appears to be no copy of this report at MIT, where the research was carried out and where this work was published. We are not aware of any other comparably early report by Minsky on AI having appeared on the market. No copies in auction records.

Provenance: Russell A. Kirsch (b. 1929) (inscription "R. A. Kirsch National Bureau of Standards" on title). Like Minsky, Kirsch attended the Bronx High School of Science, graduating in 1946, and he continued his studies at Harvard in 1952, and later at MIT. In 1951 Kirsch joined the National Bureau of Standards as part of the team which ran SEAC, the Standards Eastern Automatic Computer, which was the first stored-programme computer to become operational in the United States (it enetered service in 1950). In 1957 Kirsch developed a digital image scanner. One of the first images scanned, a picture of Kirsch's three-month-old son, was named by *Life Magazine* in 2003 as one of the "100 Photographs That Changed the World".

Some of the earliest work in AI used networks or circuits of connected units to simulate intelligent behaviour. Examples of this kind of work, called 'connectionism', include Walter Pitts and Warren McCullough's first description of a neural network for logic and Minsky's work on the SNARC system. In the late 1950s, most of these approaches were abandoned when researchers began to explore symbolic reasoning as the essence of intelligence. However, one type of connectionist work continued: the study of perceptrons, invented by Frank Rosenblatt (1928-71) in 1958, who kept the field alive with his salesmanship and the sheer force of his personality. The perceptron was a simple neural network, namely, an algorithm which allows for learning the process of deciding whether an input



-11-

Fig. 0.2-3

The particular way in which this intention was realized in our example is extremely arbitrary. For Stage 1 we might, instead of restricting the number of points in the support sets of the local functions, have restricted, for example, their diameter (as in Chapter 8).

For Stage 2 there are any number of candidates to replace unanimity as the decision criterion, with greater claim to generality and very little loss in computational simplicity. A general theory would have to undertake the difficult task of characterizing the complexity of all possible algorithms. Without such a characterization, the requirement of Stage 2 must retain a heuristic character that makes formal definition difficult.

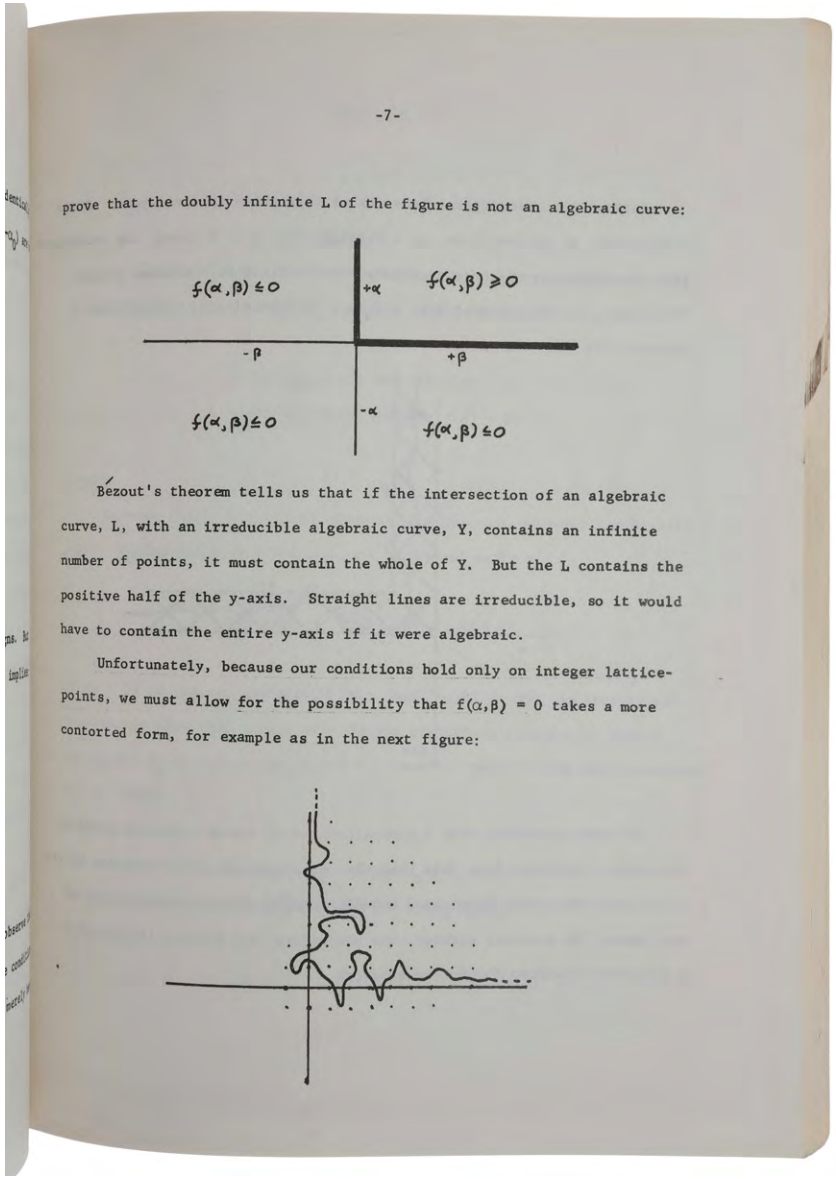
In this study we shall confine attention to a class of decision functions that includes unanimous decision as a particular case: that is, the definition

belongs to some given class or not ('binary classifier'). Rosenblatt optimistically predicted that the perceptron "may eventually be able to learn, make decisions, and translate languages". He had been a schoolmate of Minsky at the Bronx High School of Science. Minsky had toyed with neural networks, in fact his PhD dissertation concerned them, but he had dismissed their worth at that time. Thus the claims made by Rosenblatt purporting to demonstrate the learning powers of the perceptron were viewed right from the start with skepticism by Minsky.

An active research program into perceptrons was carried out throughout the 1960s but came to a sudden halt with the publication of Minsky and Papert's book *Perceptrons*. It suggested that there were severe limitations to what perceptrons could do and that Frank Rosenblatt's predictions had been grossly exaggerated. While the book highlights some of perceptrons' strengths, it also shows some previously unknown limitations. The most important of these is related to the computation of some predicates, such as the XOR function (which outputs 'true' when exactly one of two inputs is true), and also the important connectedness predicate (to tell whether an input pattern is connected, i.e., forms 'one piece', or not).

The effect of the book was devastating: virtually no research at all was done in connectionism for 10 years. Major funding for connectionist projects was difficult to find in the 1970s and early 1980s. The 'winter' of connectionist research came to an end in the middle 1980s, when the work of John Hopfield, David Rumelhart and others revived large-scale interest in neural networks.

"Minsky (1927-2016) was a uniquely brilliant, creative, and charismatic person, and his intellect and imagination shone through in his work. His ideas helped shape the computer revolution that has transformed modern life over the past few decades, and they can still be felt in modern efforts to build intelligent machines—one of the most exciting and important endeavors of our age.



-7-

prove that the doubly infinite L of the figure is not an algebraic curve:

Bezout's theorem tells us that if the intersection of an algebraic curve, L, with an irreducible algebraic curve, Y, contains an infinite number of points, it must contain the whole of Y. But the L contains the positive half of the y-axis. Straight lines are irreducible, so it would have to contain the entire y-axis if it were algebraic.

Unfortunately, because our conditions hold only on integer lattice-points, we must allow for the possibility that f(α,β) = 0 takes a more contorted form, for example as in the next figure:

"Minsky grew up in New York City, and he attended Harvard, where his curiosity led him to study an eclectic range of subjects, including mathematics, biology, and music. He then completed a PhD in the prestigious mathematics program at Princeton, where he mingled with scientists including the physicist Albert Einstein and the mathematician and computer pioneer John von Neumann.

"Inspired by mathematical work on logic and computation, Minsky believed that the human mind was fundamentally no different than a computer, and he chose to focus on engineering intelligent machines, first at Lincoln Lab, and then later as a professor at MIT, where he cofounded the Artificial Intelligence Lab in 1959 with another pioneer of the field, John McCarthy.

"Minsky's early achievements include building robotic arms and grippers, computer vision systems, and the first electronic learning system, a device, which he called Snarc, that simulated the functioning of a simple neural network fed visual stimuli. Remarkably, while at Harvard in 1956, he also invented the confocal scanning microscope, an instrument that is still widely used today in medical and scientific research.

"Minsky was also central to a split in AI that is still highly relevant. In 1969, together with Seymour Papert, an expert on learning, Minsky wrote a book called *Perceptrons*, which pointed to key problems with nascent neural networks. The book has been blamed for directing research away from this area of research for many years.

"Today, the shift away from neural networks may seem like a mistake, since advanced neural networks, known as deep learning systems, have proven incredibly useful for all sorts of tasks.

"In fact, the picture is a little more complicated. *Perceptrons* highlighted important problems that needed to be overcome in order to make neural networks more useful and powerful; Minsky often argued that a purely "connectionist" neural network-focused approach would never be sufficient to imbue machines with genuine intelligence. Indeed, many modern-day AI researchers, including those who have pioneered work in deep learning, are increasingly embracing this same vision" (Knight, 'What Marvin Minsky still means for AI,' *MIT Technology Review*, January 26, 2016).

Seymour Papert (1928-2016) was one of the early pioneers of AI, and was also a seminal thinker regarding computers and pedagogy for children. Born in South Africa, he went to MIT in the early 1960s where, with Minsky, he founded the AI Lab.

THE BIRTH OF QUANTUM PHYSICS

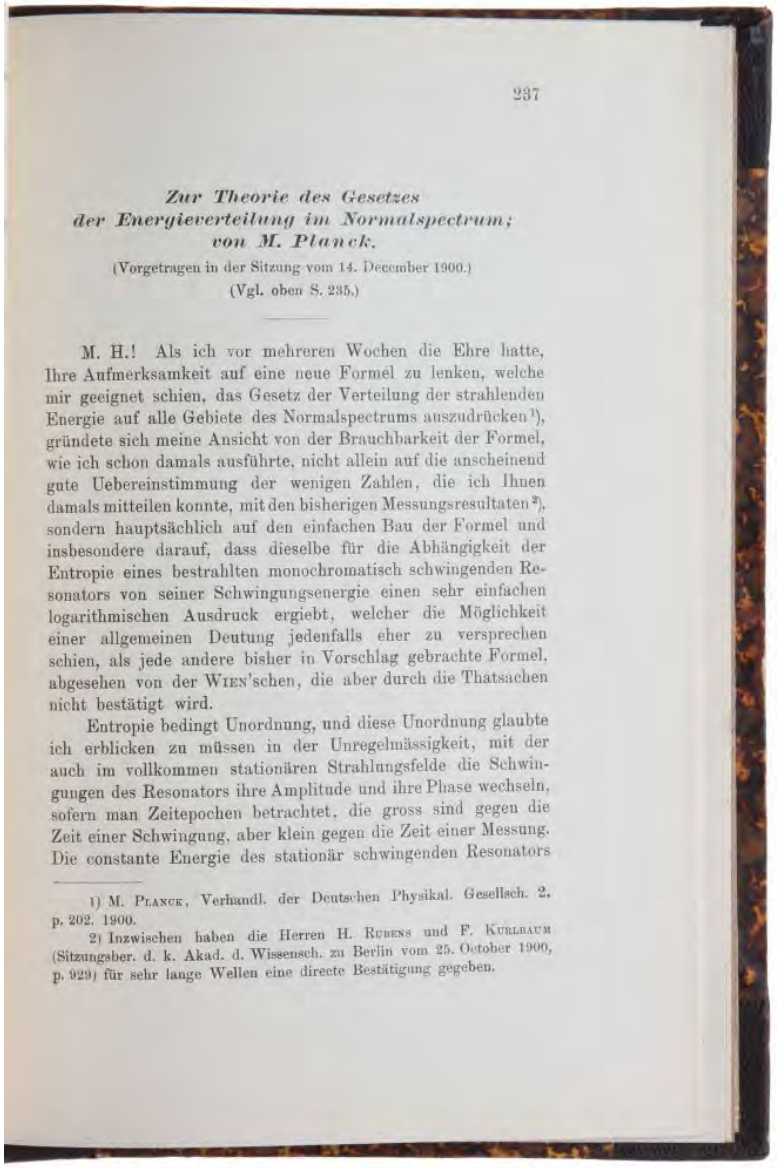
[PMM 391a; Dibner 166; Evans 47; Horblit 26a; Sparrow 162]

PLANCK, Max. *Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum, pp. 237-245 in Verhandlungen der Deutschen Physikalischen Gesselschaft im Jahre 1900, Zweiter Jahrgang.* Leipzig: Johann Ambrosius Barth, 1900.

$25,000

4to (223 x 150 mm), [complete volume:] pp vi 260. Contemporary half-calf over marbled boards, a fine copy with no repairs and no library markings at all. Fresh and clean throughout.

First edition of the first appearance of Planck's revolutionary quantum theory, arguably the most important development in twentieth-century physics. "In this important paper [Planck] stated that energy flowed not in continuous, indefinitely divisible currents, but in pulses or bursts of action [or *quanta*]" (Dibner). This hypothesis "contradicted the mechanics of Newton and the electromagnetics of Faraday and Maxwell. Moreover it challenged the notion of the continuity of nature" (PMM). In this paper Planck uses his eponymous constant *h*, the quantum of action, to explain the observed distribution of energy as a function of frequency in the radiation emitted by a black body. Although Planck's new theory was initially resisted by many in the scientific community, it gained acceptance after Einstein used the constant *h* to explain the photoelectric effect in 1905, and after Niels Bohr applied it in 1913 to explain the spectrum of the hydrogen atom. The importance of quantum theory is impossible to overstate: it not only revolutionized theoretical physics, but also technology and hence everyday life: modern inventions based on quantum theory include the laser, the transistor, the electron microscope, and magnetic resonance imaging – the entire electronics industry is based on it.



237

Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum;
von M. Planck.

(Vorgetragen in der Sitzung vom 14. December 1900.)
(Vgl. oben S. 235.)

M. H.! Als ich vor mehreren Wochen die Ehre hatte, Ihre Aufmerksamkeit auf eine neue Formel zu lenken, welche mir geeignet schien, das Gesetz der Verteilung der strahlenden Energie auf alle Gebiete des Normalspectrums auszudrücken[1]), gründete sich meine Ansicht von der Brauchbarkeit der Formel, wie ich schon damals ausführte, nicht allein auf die anscheinend gute Uebereinstimmung der wenigen Zahlen, die ich Ihnen damals mitteilen konnte, mit den bisherigen Messungsresultaten[2]), sondern hauptsächlich auf den einfachen Bau der Formel und insbesondere darauf, dass dieselbe für die Abhängigkeit der Entropie eines bestrahlten monochromatisch schwingenden Resonators von seiner Schwingungsenergie einen sehr einfachen logarithmischen Ausdruck ergiebt, welcher die Möglichkeit einer allgemeinen Deutung jedenfalls eher zu versprechen schien, als jede andere bisher in Vorschlag gebrachte Formel, abgesehen von der Wien'schen, die aber durch die Thatsachen nicht bestätigt wird.

Entropie bedingt Unordnung, und diese Unordnung glaubte ich erblicken zu müssen in der Unregelmässigkeit, mit der auch im vollkommen stationären Strahlungsfelde die Schwingungen des Resonators ihre Amplitude und ihre Phase wechseln, sofern man Zeitepochen betrachtet, die gross sind gegen die Zeit einer Schwingung, aber klein gegen die Zeit einer Messung. Die constante Energie des stationär schwingenden Resonators

1) M. Planck, Verhandl. der Deutschen Physikal. Gesellsch. 2, p. 202. 1900.

2) Inzwischen haben die Herren H. Rubens und F. Kurlbaum (Sitzungsber. d. k. Akad. d. Wissensch. zu Berlin vom 25. October 1900, p. 929) für sehr lange Wellen eine directe Bestätigung gegeben.

"In 1859–60 [Gustav] Kirchhoff had defined a blackbody as an object that reemits all of the radiant energy incident upon it; i.e., it is a perfect emitter and absorber of radiation. There was, therefore, something absolute about blackbody radiation, and by the 1890s various experimental and theoretical attempts had been made to determine its spectral energy distribution—the curve displaying how much radiant energy is emitted at different frequencies for a given temperature of the blackbody. Planck was particularly attracted to the formula found in 1896 by his colleague Wilhelm Wien at the Physikalisch-Technische Reichsanstalt (PTR) in Berlin-Charlottenburg, and he subsequently made a series of attempts to derive "Wien's law" on the basis of the second law of thermodynamics. By October 1900, however, other colleagues at the PTR, the experimentalists Otto Richard Lummer, Ernst Pringsheim, Heinrich Rubens, and Ferdinand Kurlbaum, had found definite indications that Wien's law, while valid at high frequencies, broke down completely at low frequencies.

"Planck learned of these results just before a meeting of the German Physical Society on October 19 [1900]. He knew how the entropy of the radiation had to depend mathematically upon its energy in the high-frequency region if Wien's law held there. He also saw what this dependence had to be in the low-frequency region in order to reproduce the experimental results there. Planck guessed, therefore, that he should try to combine these two expressions in the simplest way possible, and to transform the result into a formula relating the energy of the radiation to its frequency.

"The result, which is known as Planck's radiation law, was hailed as indisputably correct. To Planck, however, it was simply a guess, a "lucky intuition." If it was to be taken seriously, it had to be derived somehow from first principles. That was the task to which Planck immediately directed his energies, and by December 14, 1900, he had succeeded—but at great cost. To achieve his goal, Planck found that he had to relinquish one of his own most cherished beliefs, that the second law of thermodynamics was an absolute law of nature. Instead he had to embrace Ludwig Boltzmann's interpretation, that the second law was a statistical law. In addition, Planck had to assume that the oscillators comprising the blackbody and re-emitting the radiant energy incident upon them could not absorb this energy continuously but only in discrete amounts, in quanta of energy; only by statistically distributing these quanta, each containing an amount of energy $h\nu$ proportional to its frequency ν, over all of the oscillators present in the blackbody, could Planck derive the formula he had hit upon two months earlier. He adduced additional evidence for the importance of his formula by using it to evaluate the constant h (his value was 6.55×10^{-27} erg-second, close to the modern value of 6.626×10^{-27} erg-second), as well as the so-called Boltzmann constant (the fundamental constant in kinetic theory and statistical mechanics), Avogadro's number, and the charge of the electron. As time went on physicists recognized ever more clearly that—because Planck's constant was not zero but had a small but finite value—the microphysical world, the world of atomic dimensions, could not in principle be described by ordinary classical mechanics. A profound revolution in physical theory was in the making.

"Planck's concept of energy quanta, in other words, conflicted fundamentally with all past physical theory. He was driven to introduce it strictly by the force of his logic; he was, as one historian put it, a reluctant revolutionary. Indeed, it was years before the far-reaching consequences of Planck's achievement were generally recognized, and in this Einstein played a central role. In 1905, independently of Planck's work, Einstein argued that under certain circumstances radiant energy itself seemed to consist of quanta (light quanta, later called photons), and in 1907 he showed the generality of the quantum hypothesis by using it to interpret the temperature dependence of the specific heats of solids. In 1909 Einstein introduced the wave–particle duality into physics. In October 1911 Planck and

Einstein were among the group of prominent physicists who attended the first Solvay conference in Brussels. The discussions there stimulated Henri Poincaré to provide a mathematical proof that Planck's radiation law necessarily required the introduction of quanta—a proof that converted James Jeans and others into supporters of the quantum theory. In 1913 Niels Bohr also contributed greatly to its establishment through his quantum theory of the hydrogen atom. Ironically, Planck himself was one of the last to struggle for a return to classical theory, a stance he later regarded not with regret but as a means by which he had thoroughly convinced himself of the necessity of the quantum theory. Opposition to Einstein's radical light quantum hypothesis of 1905 persisted until after the discovery of the Compton effect in 1922.

"Planck was 42 years old in 1900 when he made the famous discovery that in 1918 won him the Nobel Prize for Physics and that brought him many other honours. It is not surprising that he subsequently made no discoveries of comparable importance. Nevertheless, he continued to contribute at a high level to various branches of optics, thermodynamics and statistical mechanics, physical chemistry, and other fields. He was also the first prominent physicist to champion Einstein's special theory of relativity (1905)" (Britannica).

"Planck had the physical interpretation of his radiation law in hand before the middle of November 1900, but he presented his results first at the meeting of the German Physical Society in Berlin on 14 December 1900 in a contribution entitled 'Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum' ('On the Theory of the Law of Energy Distribution in Normal Spectrum'). In this contribution he outlined in short 'the most essential point of the whole theory as clearly as possible' (p. 238) … In his *Atombau und Spektrallinien* (1919), Arnold Sommerfeld called 14 December 1900 the 'birthday of quantum theory' ('*Geburtstag der Quantentheorie*', p. 213). He referred in particular to the fact


Verhandlungen

der

Deutschen Physikalischen Gesellschaft

im Jahre 1900.

Zweiter Jahrgang.

Im Auftrage der Gesellschaft herausgegeben

von

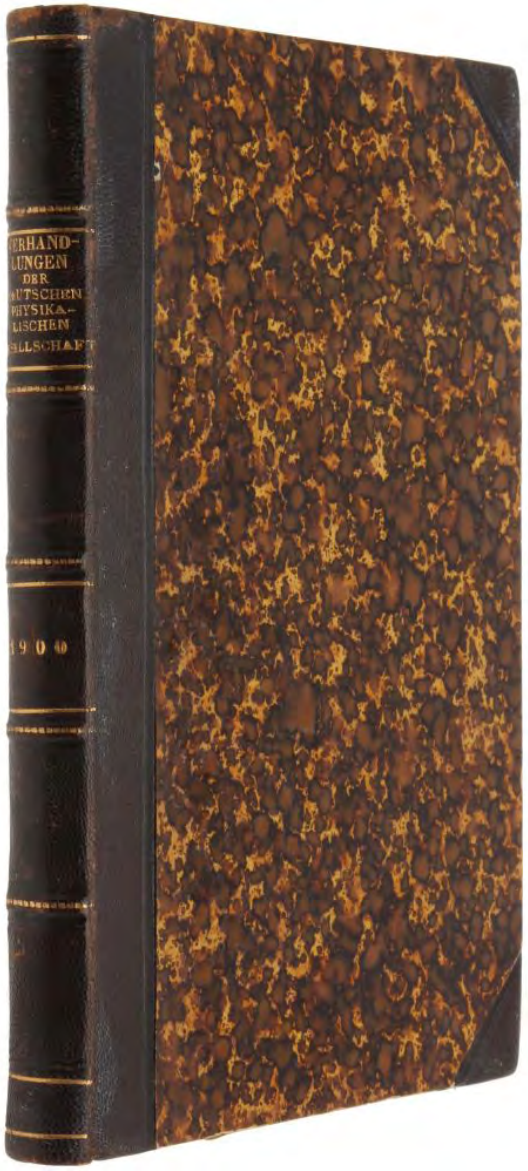
Arthur König.

Leipzig, 1900.

Verlag von Johann Ambrosius Barth.

which Planck had considered to be the 'most essential point' of his derivation of the blackbody radiation law: namely, the assumption that the energy is distributed among the cavity resonators only in integral multiples of finite energy elements … Planck himself continued with his attempts to deepen the theoretical foundations and to elucidate the physical interpretation of his theory of blackbody radiation. In a paper, which he submitted to *Annalen der Physik* in January 1901 ['Über das Gesetz der Energieverteilung im Normalspectrum,' 4 Folge, 4 Band, pp. 553-563], Planck again presented the theoretical derivation of his radiation formula, introducing certain changes in his earlier treatment" (Mehra & Rechenberg, pp. 50-53).

Dibner, *Heralds of Science* 166; Evans, *First Editions of Epochal Achievements in the History of Science* 47; Grolier/Horblit, *One Hundred Books Famous in Science* 26a; Norman 1713; PMM 391a; Sparrow, *Milestones of Science* 162. Hermann, *The Genesis of Quantum Theory* (1899-1913), 1971. Mehra & Rechenberg, *The Historical Development of Quantum Theory*, Vol. 1, 1982. The most detailed account of the development of quantum theory remains Kuhn's classic *Black-Body Theory and the Quantum Discontinuity*, 1894-1912.



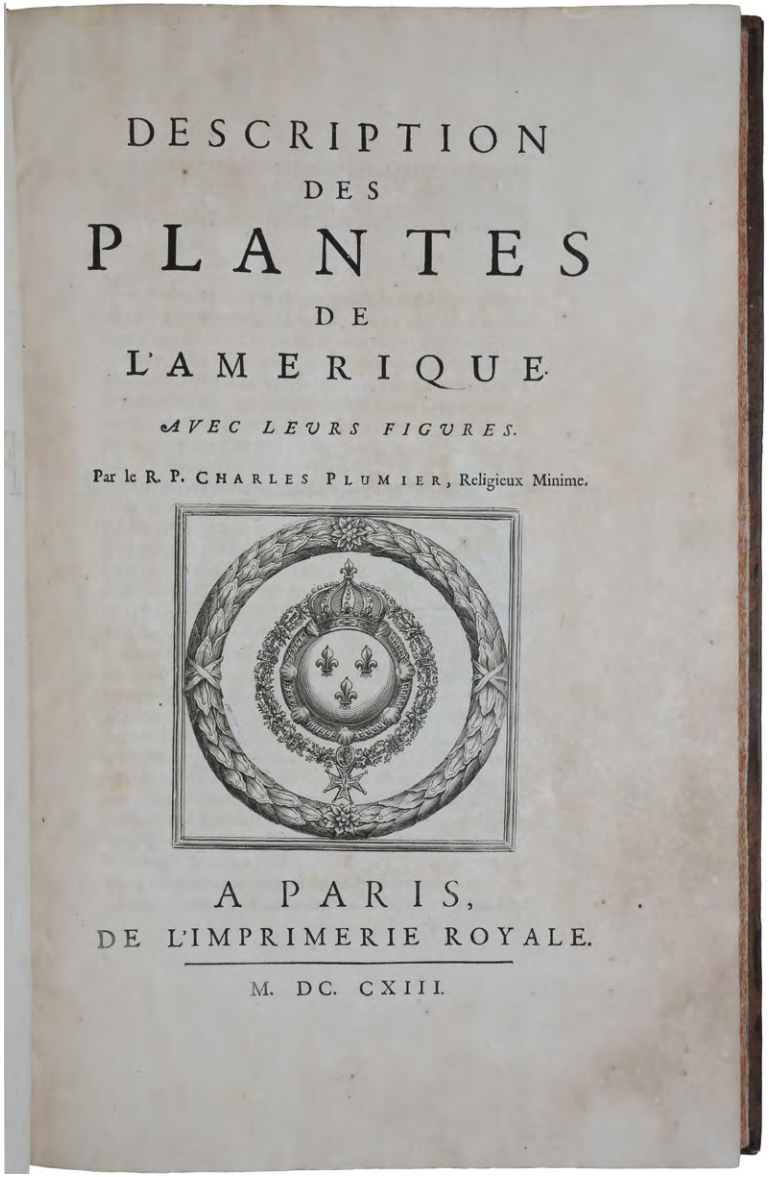
THE FRENCH ACADEMY OF SCIENCES

PLUMIER, Charles. *Descriptions des Plantes de l'Amérique.* Paris: L'Imprimerie Royale, 1693.

$25,000

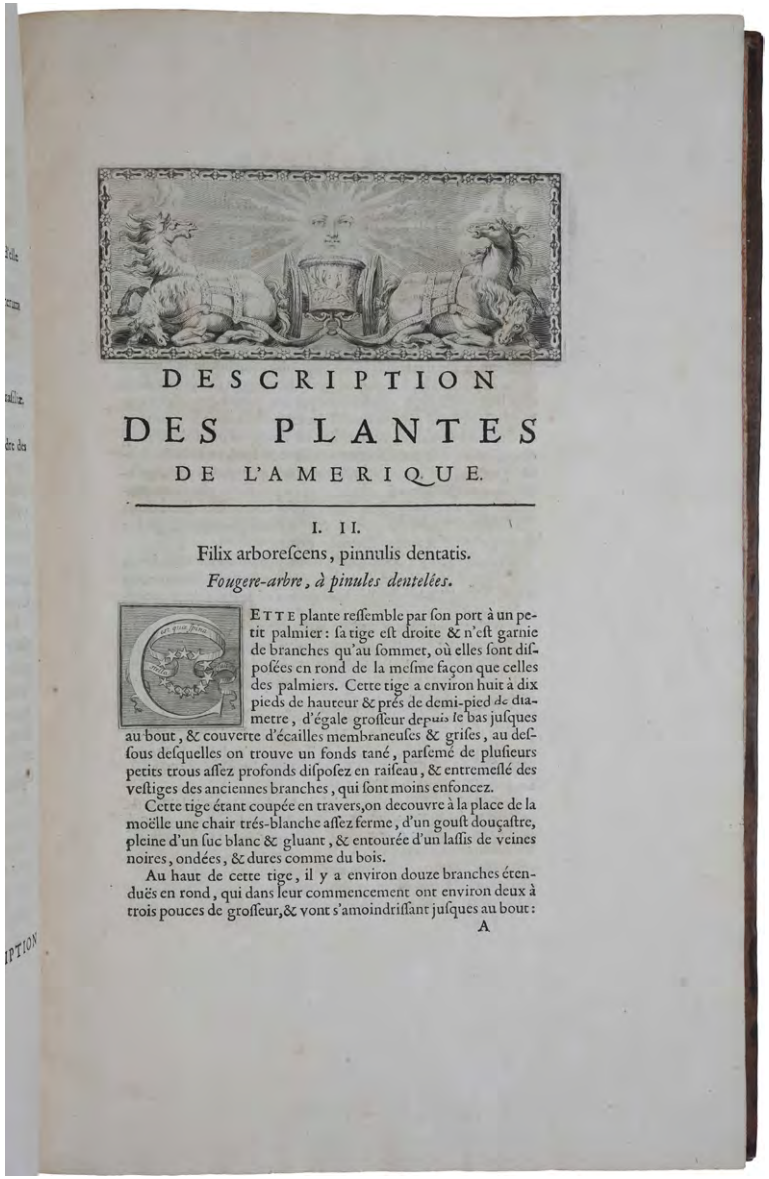
Folio (422 x 268 mm), pp. [viii], 94, [10, the last blank], with 108 leaves of engraved plates after drawings by Plumier, large engraved title vignette, engraved headpiece and initial. Contemporay calf, 20th century ex-libris to front pastedown, 17th or 18th century engraved ex-libris pasted to verso of title. A very large and fine copy.

First edition, first issue with the incorrect date MDCCXIII (rather than MDCXCIII) on the title page (but the correct date on the colophon). This is a fine copy in an untouched contemporary binding. "Plumier made three trips to the French Antilles between 1690 and 1697, was appointed royal botanist in 1693, and published his first work, *Description des Plantes de l'Amérique*, in the same year. Plumier was the first 'modern' botanist to describe the flora of the Caribbean in a time when natural history underwent significant qualitative changes as a result of the European expansion and transatlantic contacts. Plumier's ambition was to replace the confusing multitude of names given to New World plants with a universal taxonomically based nomenclature. His modernity and scientific ethos manifest themselves in his neutral way of organizing the plants according to a taxonomic system and his use of a Latin nomenclature, often naming plants after well-known botanists" (Hollsten, p. 37). Many of Plumier's generic names were adopted by Linnaeus and are still in use today, among them *Fuchsia, Caesalpinia, and Magnolia*. "Le Père Plumier, a monk in the order of St. Francesco di Paula, was an important botanical traveller. Tournefort and he became friends and they herborized together throughout the Midi. After that, Plumier's travels included


DESCRIPTION
DES
PLANTES
DE
L'AMERIQUE
AVEC LEURS FIGURES.
Par le R. P. CHARLES PLUMIER, Religieux Minime.
A PARIS,
DE L'IMPRIMERIE ROYALE.
M. DC. CXIII.

the Antilles and several long voyages to other islands in the West Indies and to America, where he discovered, drew, and described hundreds of new plants, many of which are shown in his own books" (Hunt). "Had he not died prematurely, before he was able to publish the bulk of his discoveries, Plumier would have had few equals among the higher ranks of the early naturalists" (Mottram, p. 81). The present volume is one of a number of special publications of the French Academy of Sciences, three of which (though not this one) are discussed by Dibner. "Printed at the royal press in small editions, they were intended as gifts for the King and Academy. In size, binding and beauty of the plates, they are among the most sumptuous books in science" (Dibner 84). Only three copies listed on ABPC/RBH in the last quarter-century (one second issue, the other two not specified).

"Plants had great economic value in the early modern era and new exotic plants from the tropics were sought-after resources, bought and sold for their value as food or medicine. As the European colonial empires expanded their territories and searched for ways of making use of their colonies, botanists were increasingly employed in the service of the government. In France, the Académie des Sciences had correspondents stationed in the Americas at least from the 1690s onwards, and Guy-Crescent Fagon, the king's physician and director of the Jardin du Roi after 1693, had a great interest in Caribbean and South American plants. The collection and display of imported exotic plants took on a strategic significance in 17th-century France. Exhibiting imported plants from the colonies displayed the nation's capacity and intelligence in the use of its territorial resources and the ability to improve life at home. The plant demonstrations at the Jardin du Roi were exhibitions of advanced natural knowledge, the capacity to administrate distant colonies, and the ability to use the plants to improve the health of the king and his subjects. As a result, medicinal plants such as aloe, sarsaparilla, and guaiac, as well as timber used in shipbuilding and spices from the Antilles, were of both practical and symbolic significance. Natural history was hence closely associated with colonization.



DESCRIPTION

DES PLANTES

DE L'AMERIQUE.

I. II.

Filix arboreſcens, pinnulis dentatis.

Fougere-arbre, à pinules dentelées.

ETTE plante reſſemble par ſon port à un petit palmier: ſa tige eſt droite & n'eſt garnie de branches qu'au ſommet, où elles ſont diſpoſées en rond de la meſme façon que celles des palmiers. Cette tige a environ huit à dix pieds de hauteur & prés de demi-pied de diametre, d'égale groſſeur depuis le bas juſques au bout, & couverte d'écailles membraneuſes & griſes, au deſſous deſquelles on trouve un fonds tané, parſemé de pluſieurs petits trous aſſez profonds diſpoſez en raiſeau, & entremeſlé des veſtiges des anciennes branches, qui ſont moins enfoncez.

Cette tige étant coupée en travers, on decouvre à la place de la moëlle une chair trés-blanche aſſez ferme, d'un gouſt douçaſtre, pleine d'un ſuc blanc & gluant, & entourée d'un laſſis de veines noires, ondées, & dures comme du bois.

Au haut de cette tige, il y a environ douze branches étenduës en rond, qui dans leur commencement ont environ deux à trois pouces de groſſeur, & vont s'amoindriſſant juſques au bout:

A

"Botanists usually travelled by ships that were involved in long-distance trade. With the help of assistants, botanists collected plants and brought them back to the 'knowledge centres' of Europe. Before the French crown started sending botanists to the colonies, its representatives received information about the flora and fauna of the colonies from missionaries; for instance, the Jesuit Thomas Gouyé (1650–1725) served as an intermediary between his order and the Académie des Sciences. The first botanists were sent to the French Antilles in the 1690s. Being a botanist meant being prepared to travel and endure hardships in order to collect, analyse, and describe plants. Plumier's teacher Joseph Pitton de Tournefort, professor at the Jardin du Roi from 1683 and member of the Academy from 1692, went on several voyages to observe plants, travelling to Greece and the Orient …

"On his botanical excursions to the Alps and to Provence, Tournefort brought along his student Charles Plumier. Plumier was suited for the life of a botanist in many ways. Born in Marseille in 1646, he had been accepted into the order of the Minims at the age of 16. He studied mathematics, physics, and painting, learned to make optical instruments, and became a proficient draughtsman and turner. He studied botany under the Cistercian botanist, Paolo Boccone, in Rome, and on his return to France became a student of Tournefort" (Hollsten, pp. 39-41).

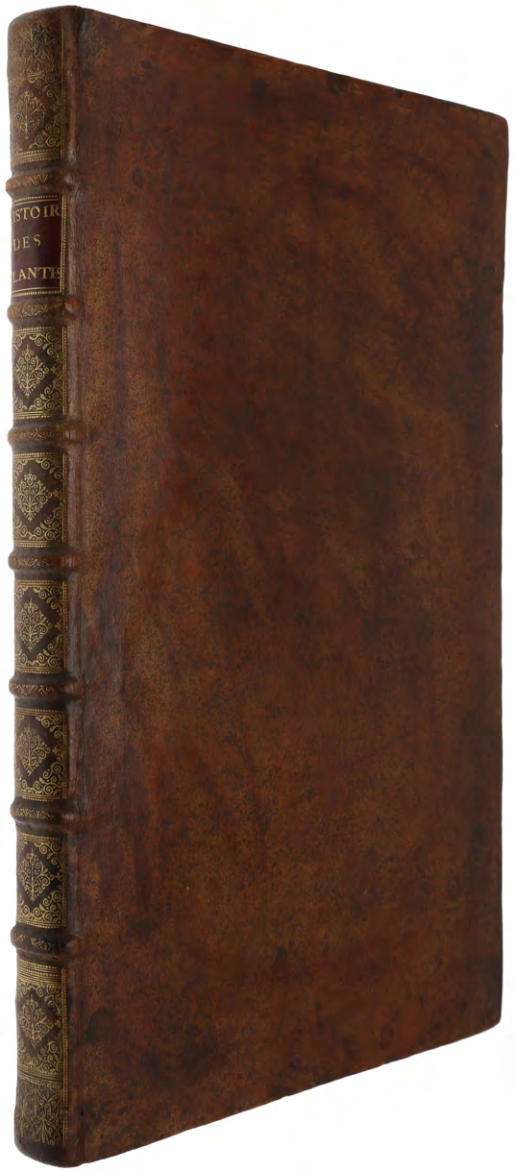
In 1689, the French crown gave Joseph Donat Surian the task of mounting an expedition to the French Antilles (Martinique, Guadeloupe and Haiti) in 1689 and 1690, and of finding a botanist to accompany him. Surian choose Charles Plumier, both an excellent botanist and engraver. On the journey Surian focused on the medicinal properties of the plants while Plumier documented and classified the physical aspects of the plants. During this voyage they became shipwrecked, and Plumier lost his herbarium containing all his seeds, pressed flowers, and dissected portions of animals. Fortunately, however, Plumier had sent his drawings, together with Surian's herbarium, to France on another ship.



At the end of the eighteen-months' journey, the two men quarrelled and Plumier published his results alone on his return to France, as the present work.

"In the preface of his book Plumier lists some works he had consulted on the nature of South America and the Caribbean, whose authors include Gaspard Bauhin, Leonhard Fuchs, Gonzalo Oviedo, Jean Baptiste du Tertre, José (Christophorus) Acosta, Piso, and Marcgrave. Likewise, [Hans] Sloane announced in his natural history of Jamaica [*A Voyage to the Islands Madeira, Barbados, Nieves, S. Christophers and Jamaica*, London, 1707] that he had not included pictures of plants Charles Plumier had already illustrated: '[Plumier] has saved me a great deal of trouble, finding his Figures so Good, that I did not judge it necessary the same plants should be engraved again, but only referr'd to, in my History."

"Plumier's *Descriptions des Plantes de l'Amérique* is a well-organized study divided into two parts, the first consisting of descriptions and the second of illustrations. Carefully drawn pictures, often of full size, are included. The descriptions are short and objective, eventual medicinal properties are mentioned briefly, if at all (one of the reasons for this may have been that Plumier made his first trip together with his colleague J. D. Surian, whose task it had been to examine and report on the medicinal plants). In the preface, Plumier praised earlier writers treating the flora of the Antilles, but acknowledged the difficulty in finding the plant classes, as the local names of the plants were being used. He brought order to the matter by regrouping the plants in three classes, divided into genera, and by giving them Latin names: the first class included *Fougeres, Hemionites, Polypodes, Langues-de-cerf*, and *Capillaires*, the second *Arum & Dracontium* and *Saururus*, and the third *Perploques* (climbing plants). *Saururus* was a new genus for which Plumier invented the name based on the Greek word for lizard tail because Plumier thought that the plants in this genus were reminiscent of lizards' tails. The plants were grouped according to the structure of the flower



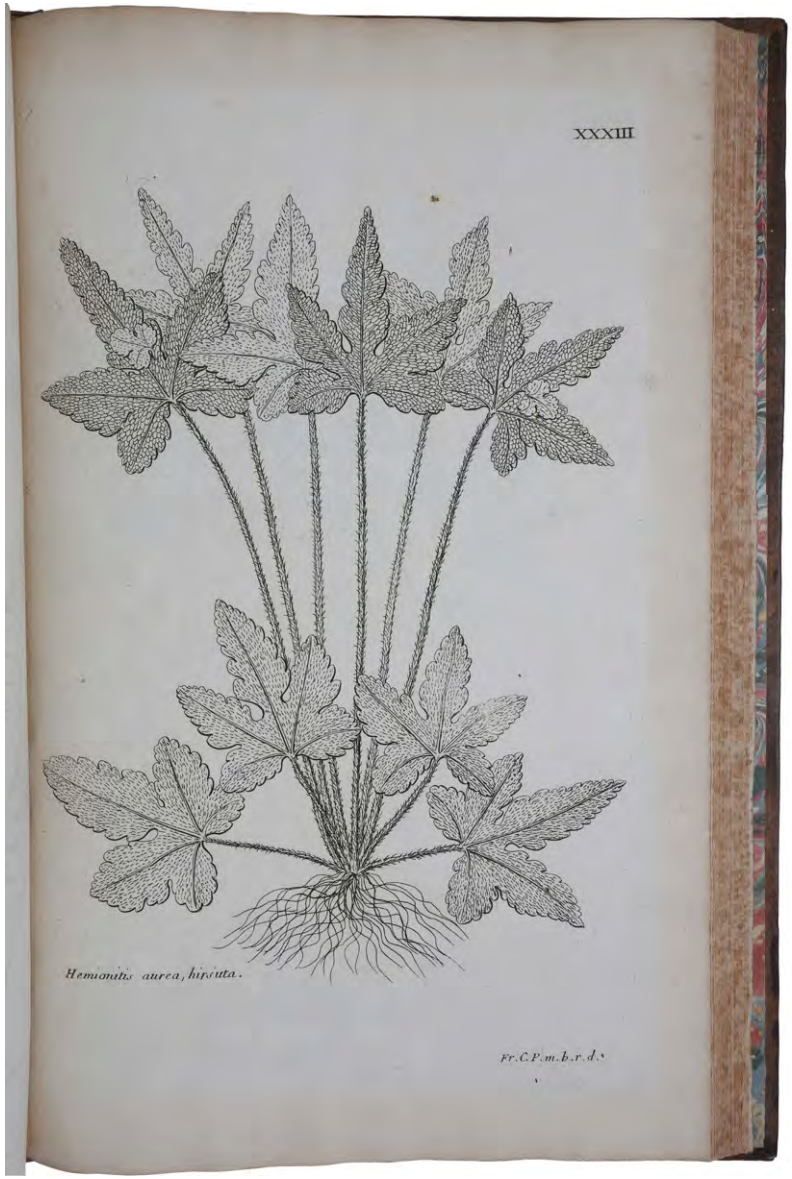
and the fruit, as recommended by Tournefort. The plants were mostly named according to a polynomial nomenclature in which the names were composed of several Latin words describing the plant. Linné first used the polynomial system but abandoned it in favour of the binomial one in which a generic name was followed by a specific epithet, using only the two first parts of the polynomial. Thus *Plumier's Fuchsia triphylla flore coccinea* (which means 'three-leaved fuchsia with red flowers') became simply *Fuchsia triphylla*. Carl von Linné accepted, almost without change, Plumier's descriptions and arrangement of several genera and species … (*ibid.*, pp. 43-44).

"Different nationalities bioprospecting in the Caribbean used different names for indigenous species. For example, the English gave the pineapple its name because of its resemblance to a pine-cone while the French word *ananas* is derived from the Guarani word for exquisite fruit. Plumier was caught in the confusion of the naming practices of New World plants. For example, he called one plant used to cure snake bites *Arum hederaceum, amplis foliis & perforates*, although according to him the plant was called *Clematis malabarensis, solus vitis, colore dracunculi* by Gaspard Bauhin and *lignum colubrinum primum* by Acosta, whereas Father du Tertre for his part had called this same plant simply *bois des couleuvres*. The correct taxonomic placement would come later in 1829 when the genus name was changed and the plant was named *Philodendron hederaceum*. Another plant used for serpent bites Plumier named *Clematis baccifera, glabra & villosa, rotundo & umbilico folio*. The French population of Martinique, however, called it, according to Plumier, *Liane a serpent* and in South America the plant had several names: *Caapeba des Brasiliens*, (in Marcgrave's work) *l'erva di nostra Senora, Herbe de Nostre-Dame*, and *Cipo de Cobras des Portugais*. Plumier by no means had the last word on this plant: it was called *Cissampelos caapeba* by Linné, and later became known as *Pareira brava* and *Cissampelos pareira*.



"With so many parallel names in use, it is no wonder that Plumier felt there should be a common universal system of nomenclature. He writes that he had profited greatly from the work of earlier authors, but that their accounts may appear confusing to readers as they often only give the name of the plant in the 'vulgar language' of the country. Therefore, those who have never seen these plants in nature will have much trouble finding out what genera they represent; these people, Plumier states, will be grateful to him. As Plumier set out to bring order to the situation, he converted indigenous names to Latin. For example, he renamed 'vainillia', which the Spanish physician Hernandez had found in Mexico in 1571, *Vanilla planifolia*. He rarely adopted names used by the indigenous people. He described an Antillean bean he named *Phaseolus siliquis latis, hispidis, & rugolis, fructu nigro* (in French *Phaseole à gousses larges, values, & froncées*). After a long and detailed description of the physical characteristics and different stages of the plant, he mentions that the indigenous people, the Caribs, used the beans for food and the plant's leaves in the making of their hammocks. In addition, Plumier explains that this was the plant named *Mucuna des Brasiliens* by Marcgrave and *phaseolus Nigritarum* (*phaseole des Négres*) by Clusius. In the midst of all these names one would expect Plumier to refer to a local Carib name, but he does not do so. Many naturalists received information from the indigenous people (although these are seldom credited in the final works) but Plumier gives very little information about any possible contacts with indigenous people …

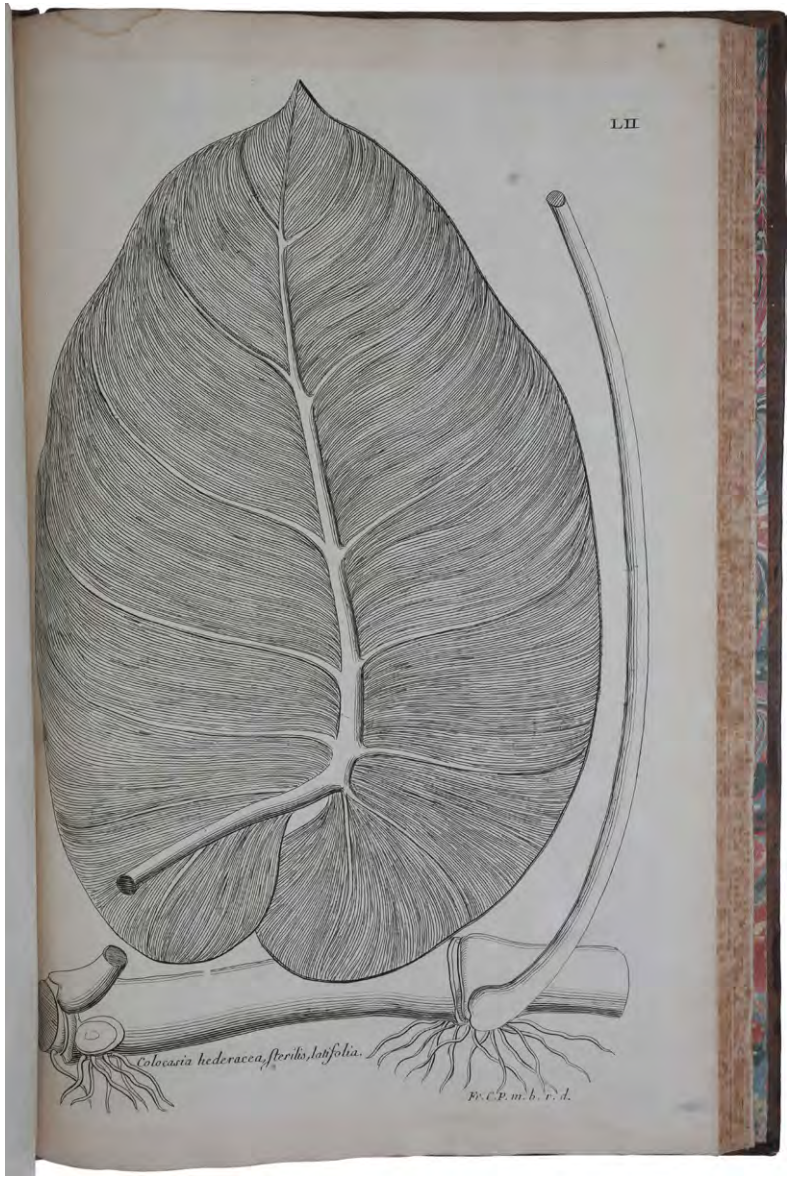
"Plumier provides few personal anecdotes but, on rare occasions, he abandons his cool and objective descriptions to recount something personally experienced. After a long description of *Colocasia montante* (which he calls *Colocasia* because the leaves are almost of the same consistency as *Colocasia d'Egypte*, described by Gaspar Bauhin) he tells how, after tasting the plants, his mouth became so inflamed that he was not able to speak for two hours and could taste nothing for two weeks. This experience explained to him the vernacular name of the plant,



liane brulante. Because of his habit of tasting plants, Plumier has been called an experimentalist …

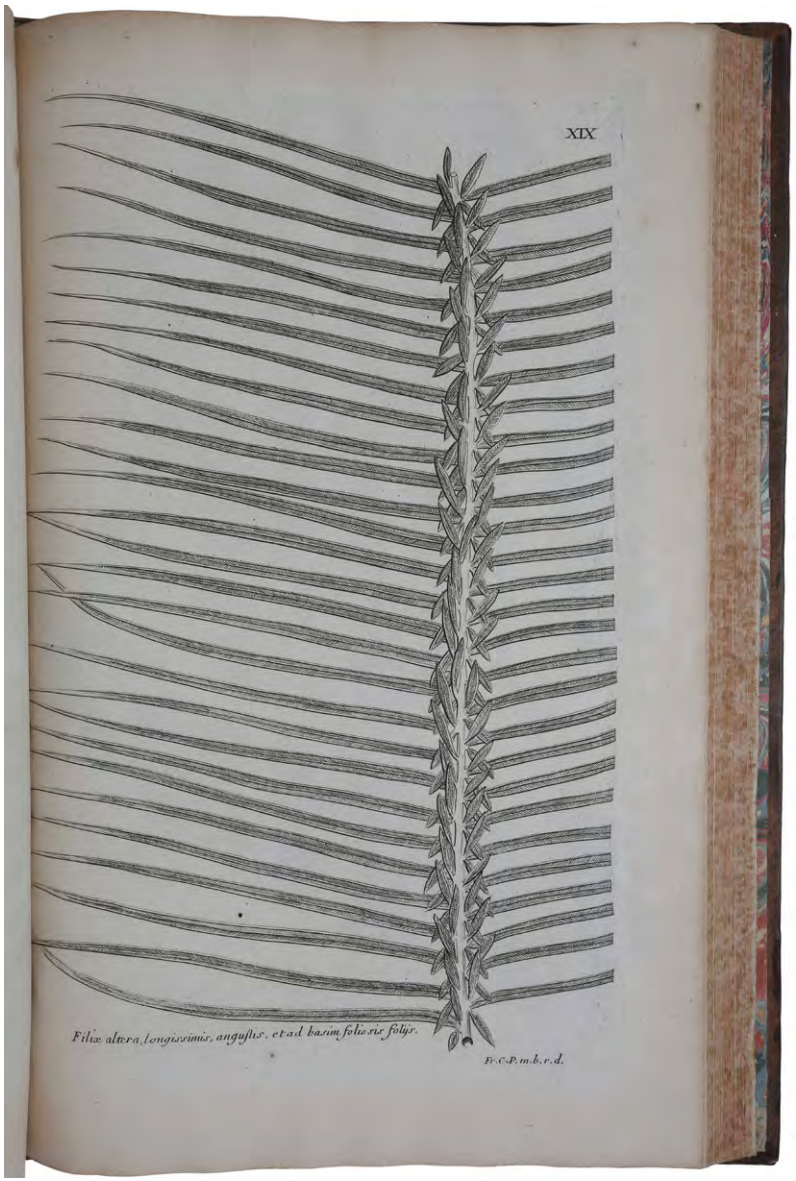
"The Latin names given by Plumier often give detailed descriptions of the plants. In some cases, however, he named plants after distinguished European men. The naming of plants after people was an ancient Greek practice he revived. He named a plant he found in South America *Pittonia* after Joseph Pitton de Tournefort (although Linné later changed its name to *Tournefortia*). The *Begonia* received its name after one of Plumier's benefactors, Michel Begon, the former governor of St. Domingue, who decided that a botanical survey of the Antilles was necessary. In addition, he named plants *Sloanea* after Hans Sloane, *Suriana* after his dead colleague Surian, *Lobelia* after the curator of the botanical garden at Oxford, and *Magnolia* after Pierre Magnol (1637–1715), director of the botanical garden at Marseille. *Bromelia* was named after the Swedish physician Olaf Bromelius (1639–1705) while *Ximenia* was named in honour of the Spanish monk Ximénes. The name *Bauhinia* was chosen as a tribute to Gaspard Bauhin. This paying of homage to his teachers, colleagues, and benefactors places Plumier in a tradition and a network of botanists who had provided him with inspiration and furthered his career" (*ibid.*, pp. 45-47).

Based on the success of this first expedition, Plumier was appointed *botaniste du roi* by Louis XIV in 1693, and given support for a second voyage to the French Antilles, on which he spent six months gathering over 924 new species of plants leading to the discovery of 100 new genera. From his observations in Martinique, Plumier proved that the cochineal, used to create an intense scarlet dye used in the European textile trade, should be classified as an insect. On his third voyage, in 1695, Plumier visited Guadeloupe and the foothills of Santo Domingo, as well as Martinique. It was here that he discovered the Fuchsia as well as many ferns. These findings were published in his *Nova plantarum Americanarum genera*



(1703-1704). Plumier died of pleurisy at Cadiz, on 20 November 1704, just as he was about to embark on a fourth voyage. This was to have been to Peru, to search for *Cinchona officinalis*, the cinchona tree that yielded the miraculous powder quinine, then popularly known as 'Jesuits' powder.' At his death Plumier left a work in French and Latin ready to be printed entitled *Traité des fougères de l'Amérique* (1705), which contained 172 plates, as well as thirty-one manuscript volumes containing notes and descriptions, and about 6,000 drawings, 4,000 of which were of plants, while the remainder reproduced American animals of nearly all classes, especially birds and fishes. The great Dutch botanist Herman Boerhaave (1667-1738) had 508 of these drawings copied at Paris; these were published later in a *hommage* by Jean Burmann (1707-80), under the title: *Plantarum americanarum, quas olim Carolus Plumerius botanicorum princeps detexit, fasc. I-X* (Amsterdam, 1755–1760), containing 262 plates. In 1701, Plumier published his *L'Art du Tourner*, the first work to explain in detail the technology of turning (as with the modern lathe). It was translated into Russian by Peter the Great.

Dibner, *Heralds of Science* 84; European Americana 693/137; Hunt 389; JCB p. 275; Nissen BBI 1544; Pritzel 7213; Sabin 63455. Laura Hollsten, 'An Antillean plant of beauty, a French botanist, and a German name: naming plants in the Early Modern Atlantic world,' *Estonian Journal of Ecology*, vol. 61 (2012), pp. 37-50. Roy Mottram, 'Charles Plumier, the King's Botanist – his life and work,' Bradleya, vol. 20 (2002), pp. 79-120.



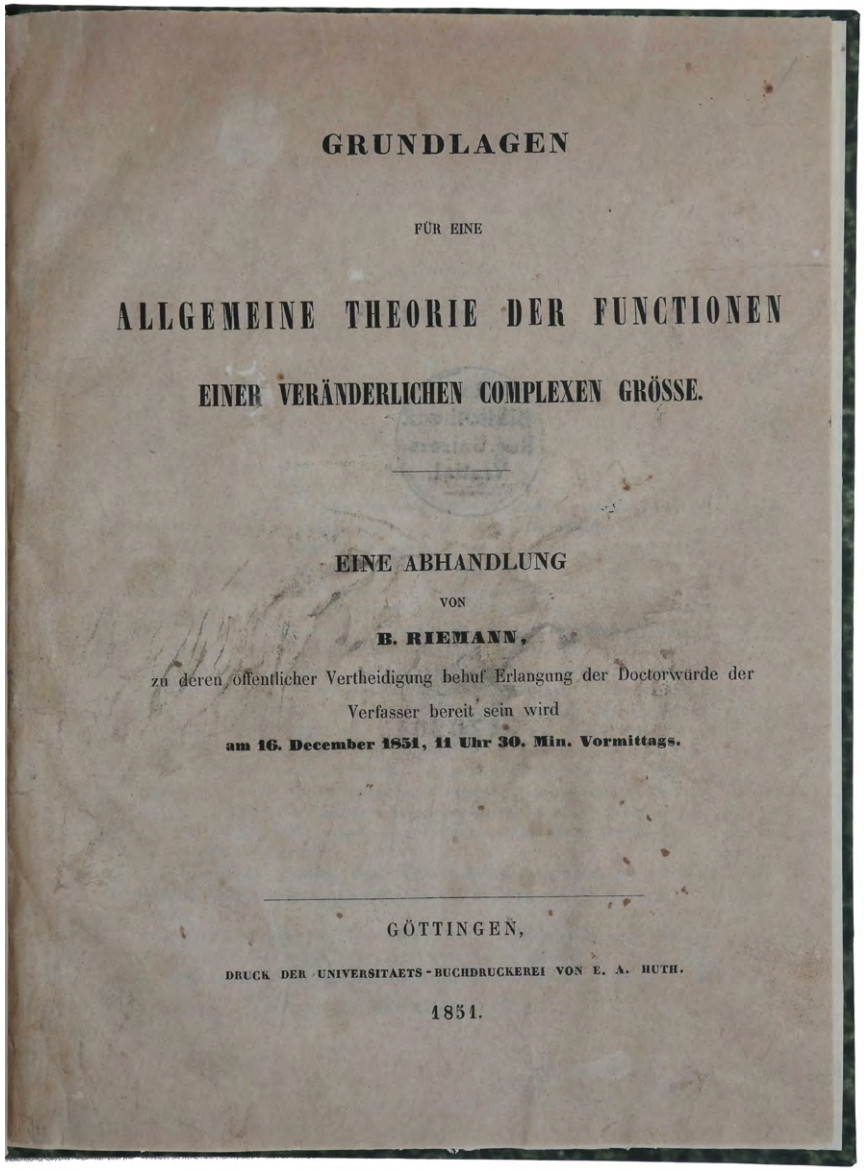
RIEMANN'S MASTERPIECE

RIEMANN, George Friedrich Bernhard. *Grundlagen für eine allgemeine Theorie der Functionen einer veränderlichen complexen Grösse. Eine Abhandlung, zu deren öffentlicher Vertheidigung behuf Erlangung der Doctorwürde der Verfasser bereit sein wird am 16. December 1851.* Göttingen: E.A. Huth, 1851.

$15,000

4to (238 x 181 mm), pp. [2], [1] 2-32, minor toning throughout; some browning to the title and last page. Stamp and small paper label to verso of title; upper inner corner of first leaves torn off and proffessionally restored (nowhere near text); small restoration to a marginal tear to least leaf. Recent green marbled paper boards.

First edition of Riemann's *Dissertation*, "one of the most important achievements of 19th century mathematics" (Laugwitz), "which marked a new era in the development of the theory of analytic functions" (Kolmogorov & Yushkevich, p. 199), introducing geometric and topological methods, notably the idea of a 'Riemann surface.' "Riemann's doctoral thesis is, in short, a masterpiece" (Derbyshire, p. 121). This paper, and its sequel *Theorie der Abel'schen Functionen* (1857), "have become famous for several reasons. They introduced what are now called Riemann surfaces, in the form of domains spread out over the complex plane. They presented enough tools to classify all compact orientable surfaces, and so gave a great impetus to topology. They provided a topological meaning for an otherwise unexplained constant which entered into Abel's work on Abelian integrals, and more generally gave a geometric framework for all of complex analysis. They are thus the first mature, obscure, papers in the study of the topology of manifolds and are equally decisive for the development of



GRUNDLAGEN

FÜR EINE

ALLGEMEINE THEORIE DER FUNCTIONEN

EINER VERÄNDERLICHEN COMPLEXEN GRÖSSE.

EINE ABHANDLUNG

VON

B. RIEMANN,

zu deren öffentlicher Vertheidigung behuf Erlangung der Doctorwürde der Verfasser bereit sein wird

am 16. December 1851, 11 Uhr 30. Min. Vormittags.

GÖTTINGEN,

DRUCK DER UNIVERSITAETS-BUCHDRUCKEREI VON E. A. HUTH.

1851.

algebraic geometry and the geometric treatment of complex analysis" (Gray, p. 21). Riemann's *Dissertation* is of great rarity, for "although [a Dissertation] was a printed booklet, it was not usually published or publicised in the normal way; the candidate had to pay for the print-run, and sales and marketing were executed on an infinitesimal scale. So the first printing of Riemann's thesis consisted only of the obligatory copies he had to hand in at Göttingen University, and a few copies for his personal use" (*Landmark Writings*, p. 454). Only three copies of Riemann's *Dissertation* have appeared at auction in the last 30 years.

Riemann begins his Dissertation by offering a new foundation for the theory of analytic functions, based not on analytic expressions but on the assumption that the complex function $w = u + iv$ of the complex variable $z = x + iy$ is 'differentiable'. Riemann noted that this condition is equivalent to requiring that u and v satisfy the 'Cauchy-Riemann equations' (as they are now called), and that, when the derivative is non-zero, it is also equivalent to requiring that the function determines a conformal mapping from the z-plane to the w-plane. In order to deal with multi-valued functions such as algebraic functions and their integrals, Riemann introduced the surfaces now named after him: the Riemann surface associated with a function is composed of as many sheets as there are branches of the function, connected in a particular way so that continuity is preserved and a single-valued function on the surface is obtained. Such a surface can be represented on a plane by a series of 'cross-cuts', which divide the surface into simply-connected regions.

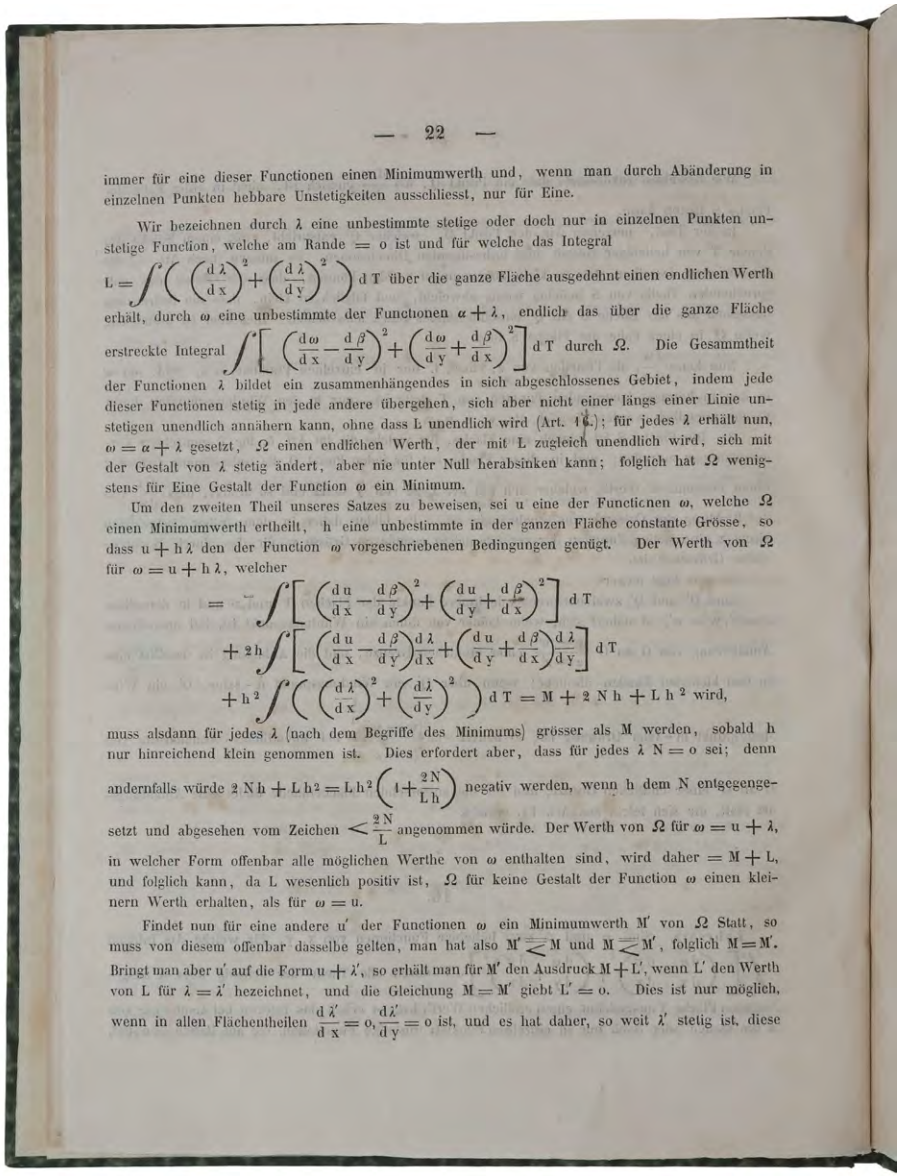
The rest of the *Dissertation* is devoted to the study of functions on Riemann surfaces. From the Cauchy-Riemann equations it follows that if $w = u + iv$ is an analytic function, then u and v are harmonic functions, i.e. solutions of Laplace's equation. To construct harmonic functions such as u and v, Riemann began with the case of a simply-connected region and made use of what he called 'Dirichlet's principle (he had learned it from Dirichlet's lectures in Berlin): this asserts that the harmonic functions are exactly those which minimize the value of a certain integral. He then extended this to the non-simply connected case using cross-cuts and other variants. This approach was later to prove controversial, as Weierstrass gave examples of situations in which the minimizing function does not exist, but it was rehabilitated by Hilbert early in the next century.

The crowning glory of the *Dissertation*, and the most difficult part of the theory of conformal mappings, is his celebrated mapping theorem. "As an application of his approach he gave a 'worked-out example', showing that two simply-connected plane surfaces can always be made to correspond in such a way that each point of one corresponds continuously with its image in the other, and so that corresponding parts are 'similar in the small', or conformal ... what is nowadays called the 'Riemann mapping theorem'." (*Landmark Writings*, p. 454).

According to Richard Dedekind (*Bernhard Riemann's Lebenslauf*, p. 7), Riemann probably conceived the main ideas of the Dissertation in autumn 1847. It was submitted on 14 November, 1851 and the Dean of the Faculty asked Gauss for his opinion. Always sparing with his praise, Gauss nevertheless wrote: "The paper submitted by Mr Riemann bears conclusive evidence of the profound and penetrating studies of the author in the area to which the topic dealt with belongs" (quoted from R. Remmert, "From Riemann surfaces to complex spaces", *Bull. Soc. Math. France* (1998), p. 207). Following the thesis examination on 16 December, 1851, Riemann was awarded his Doctor Philosophiae and Gauss recommended that he be formally appointed to a post at Göttingen.

DSB XI 449-451; U. Bottazzini & J. Gray, *Hidden Harmony – Geometric Fantasies – The Rise of Complex Function Theory*, 2013; J. Derbyshire, Prime Obsession, 2003; J. Ferreiros, *Labyrinth of Thought: A History of Set Theory and its Role in*

Modern Mathematics, 2010; I. Grattan-Guiness, *Landmark Writings in Western Mathematics*, Chapter 34; J. Gray, *Linear Differential Equations and Group Theory from Riemann to Poincaré*, 1996; A. N. Kolmogorov & A. P. Yushkevich (eds.), *Mathematics in the 19th century, Vol. II*, 1996; D. Laugwitz, *Bernhard Riemann, 1826-1866*, 1998; E. Scholz, *Geschichte des Mannigfaltigkeitsbegriffs von Riemann bis Poincaré*, 1980; J. Stillwell, *Mathematics and its History*, 3rd edition, 2010.



— 22 —

immer für eine dieser Functionen einen Minimumwerth und, wenn man durch Abänderung in einzelnen Punkten hebbare Unstetigkeiten ausschliesst, nur für Eine.

Wir bezeichnen durch λ eine unbestimmte stetige oder doch nur in einzelnen Punkten unstetige Function, welche am Rande $= o$ ist und für welche das Integral

$$L = \int \left(\left(\frac{d\lambda}{dx}\right)^2 + \left(\frac{d\lambda}{dy}\right)^2 \right) dT$$

über die ganze Fläche ausgedehnt einen endlichen Werth erhält, durch ω eine unbestimmte der Functionen $\alpha + \lambda$, endlich das über die ganze Fläche erstreckte Integral $\int \left[\left(\frac{d\omega}{dx} - \frac{d\beta}{dy}\right)^2 + \left(\frac{d\omega}{dy} + \frac{d\beta}{dx}\right)^2 \right] dT$ durch Ω. Die Gesammtheit der Functionen λ bildet ein zusammenhängendes in sich abgeschlossenes Gebiet, indem jede dieser Functionen stetig in jede andere übergehen, sich aber nicht einer längs einer Linie unstetigen unendlich annähern kann, ohne dass L unendlich wird (Art. 14.); für jedes λ erhält nun, $\omega = \alpha + \lambda$ gesetzt, Ω einen endlichen Werth, der mit L zugleich unendlich wird, sich mit der Gestalt von λ stetig ändert, aber nie unter Null herabsinken kann; folglich hat Ω wenigstens für Eine Gestalt der Function ω ein Minimum.

Um den zweiten Theil unseres Satzes zu beweisen, sei u eine der Functionen ω, welche Ω einen Minimumwerth ertheilt, h eine unbestimmte in der ganzen Fläche constante Grösse, so dass $u + h\lambda$ den der Function ω vorgeschriebenen Bedingungen genügt. Der Werth von Ω für $\omega = u + h\lambda$, welcher

$$= \int \left[\left(\frac{du}{dx} - \frac{d\beta}{dy}\right)^2 + \left(\frac{du}{dy} + \frac{d\beta}{dx}\right)^2 \right] dT$$
$$+ 2h \int \left[\left(\frac{du}{dx} - \frac{d\beta}{dy}\right)\frac{d\lambda}{dx} + \left(\frac{du}{dy} + \frac{d\beta}{dx}\right)\frac{d\lambda}{dy} \right] dT$$
$$+ h^2 \int \left(\left(\frac{d\lambda}{dx}\right)^2 + \left(\frac{d\lambda}{dy}\right)^2 \right) dT = M + 2Nh + Lh^2 \text{ wird,}$$

muss alsdann für jedes λ (nach dem Begriffe des Minimums) grösser als M werden, sobald h nur hinreichend klein genommen ist. Dies erfordert aber, dass für jedes λ $N = o$ sei; denn andernfalls würde $2Nh + Lh^2 = Lh^2\left(1 + \frac{2N}{Lh}\right)$ negativ werden, wenn h dem N entgegengesetzt und abgesehen vom Zeichen $< \frac{2N}{L}$ angenommen würde. Der Werth von Ω für $\omega = u + \lambda$, in welcher Form offenbar alle möglichen Werthe von ω enthalten sind, wird daher $= M + L$, und folglich kann, da L wesenlich positiv ist, Ω für keine Gestalt der Function ω einen kleinern Werth erhalten, als für $\omega = u$.

Findet nun für eine andere u' der Functionen ω ein Minimumwerth M' von Ω Statt, so muss von diesem offenbar dasselbe gelten, man hat also $M' \leqq M$ und $M \leqq M'$, folglich $M = M'$. Bringt man aber u' auf die Form $u + \lambda'$, so erhält man für M' den Ausdruck $M + L'$, wenn L' den Werth von L für $\lambda = \lambda'$ bezeichnet, und die Gleichung $M = M'$ giebt $L' = o$. Dies ist nur möglich, wenn in allen Flächentheilen $\frac{d\lambda'}{dx} = o, \frac{d\lambda'}{dy} = o$ ist, und es hat daher, so weit λ' stetig ist, diese

A MASTERWORK OF MATHEMATICS

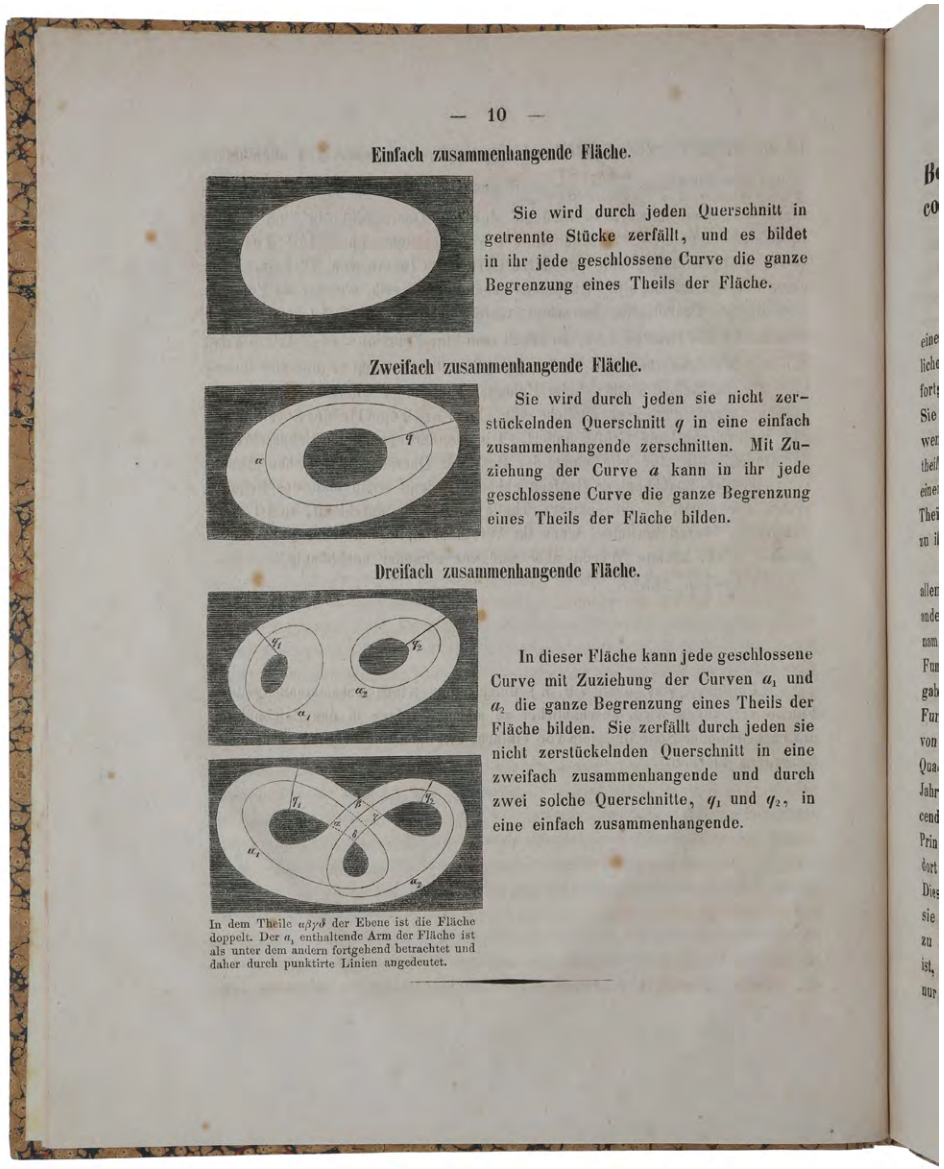
[Landmarks in Western Mathematics 34]

RIEMANN, George Friedrich Bernhard. *Theorie der Abel'schen Functionen.* Offprint from Journal fur die reine und angewandte Mathematik, Bd. 54. Berlin: G. Reimer, 1857.

$9,500

4to (250 x 194 mm), pp. [4], [1] 2-55German library and release ink stamps to recto and verso of title; some minor spotting throughout, otherwise fresh and clean. 20th century marbled boards with gilt pine label.

First edition of Riemann's paper on Abelian functions, "one of the most notable masterworks of mathematics" (DSB), which develops much further the methods of his dissertation (*Grundlagen für eine allgemeine Theorie der Functionen einer veränderlichen complexen Grösse*, 1851), using them to give the general solution of the Jacobian inversion problem (solved in a special case by Karl Weierstrass in 1854) and to establish an early version of the famous Riemann-Roch theorem. Stillwell (p. 310) considers Riemann's 1857 paper "perhaps his greatest work, which did for algebraic geometry what his *Habilitationsvortrag* [*Ueber die Hypothesen, welche die Geometrie zu Grunde liegen*, 1854] did for differential geometry." Scholz has shown that the concept of a manifold, one of the most important innovations introduced in the 1854 *Habilitationsvortrag*, grew out of an attempt to find a satisfactory conceptualization of the Riemann surfaces that he had begun to employ in 1851 and developed fully in 1857. "The papers [*Grundlagen für eine allgemeine Theorie der Functionen & Theorie der Abel'schen Functionen*] have become famous for several reasons. They introduced what



— 10 —

Einfach zusammenhangende Fläche.

Sie wird durch jeden Querschnitt in getrennte Stücke zerfällt, und es bildet in ihr jede geschlossene Curve die ganze Begrenzung eines Theils der Fläche.

Zweifach zusammenhangende Fläche.

Sie wird durch jeden sie nicht zerstückelnden Querschnitt q in eine einfach zusammenhangende zerschnitten. Mit Zuziehung der Curve a kann in ihr jede geschlossene Curve die ganze Begrenzung eines Theils der Fläche bilden.

Dreifach zusammenhangende Fläche.

In dieser Fläche kann jede geschlossene Curve mit Zuziehung der Curven a_1 und a_2 die ganze Begrenzung eines Theils der Fläche bilden. Sie zerfällt durch jeden sie nicht zerstückelnden Querschnitt in eine zweifach zusammenhangende und durch zwei solche Querschnitte, q_1 und q_2, in eine einfach zusammenhangende.

In dem Theile $\alpha\beta\gamma\delta$ der Ebene ist die Fläche doppelt. Der a_1 enthaltende Arm der Fläche ist als unter dem andern fortgehend betrachtet und daher durch punktirte Linien angedeutet.

are now called Riemann surfaces, in the form of domains spread out over the complex plane. They presented enough tools to classify all compact orientable surfaces, and so gave a great impetus to topology. They provided a topological meaning for an otherwise unexplained constant which entered into Abel's work on Abelian integrals, and more generally gave a geometric framework for all of complex analysis. They are thus the first mature, obscure, papers in the study of the topology of manifolds and are equally decisive for the development of algebraic geometry and the geometric treatment of complex analysis" (Gray, p. 21). No copy of this offprint has appeared at auction since 1974. No copies listed on COPAC.

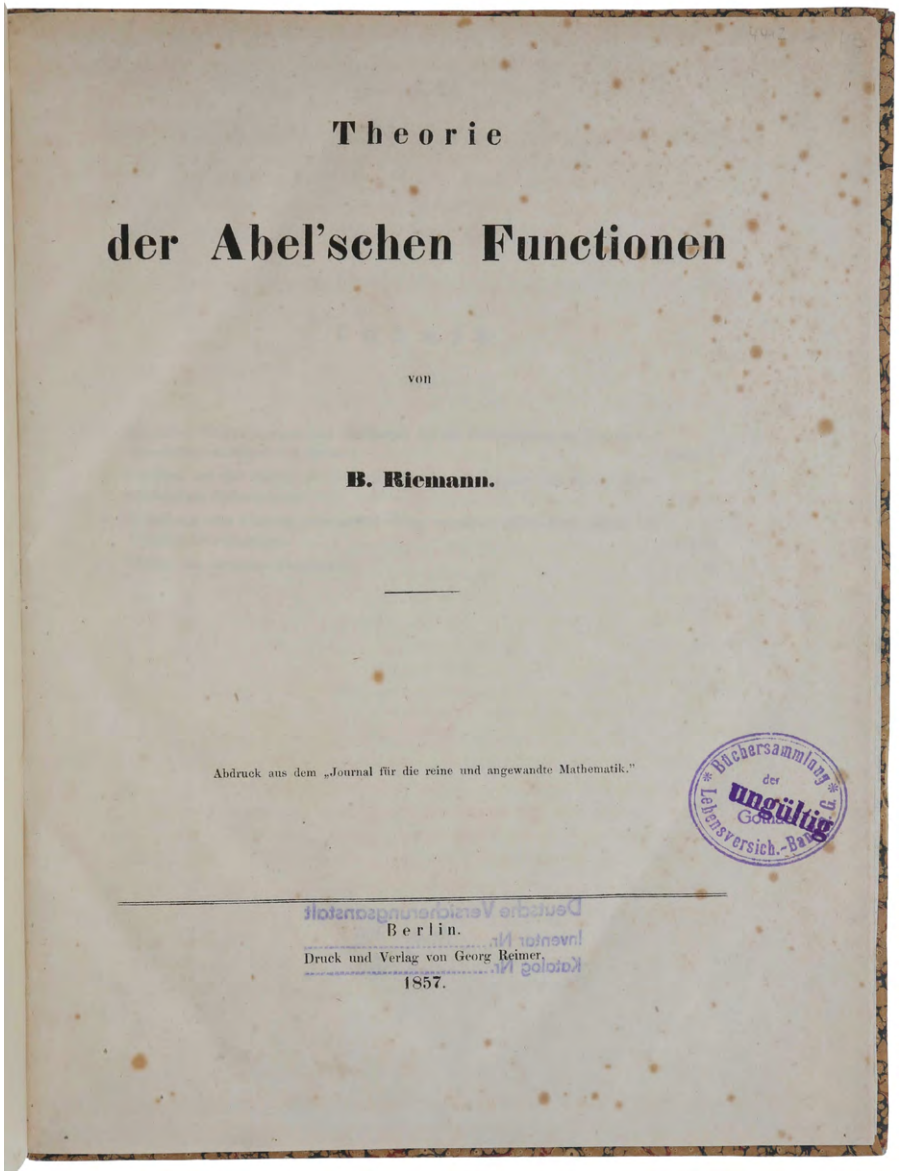
"The year 1857 was what we should call, in the language of current celebrity biography, Riemann's "breakout year." His 1851 doctoral dissertation is nowadays regarded as a classic of nineteenth-century mathematics, but it drew little attention at the time in spite of having been enthused over by Gauss. His other written papers of the early 1850s were not widely known and were published in an accessible form only after his death. To the degree that he had become known at all, it was mainly through the content of his lectures; and much of that content was too far ahead of its time to be appreciated. In 1857, however, Riemann published a paper on analysis that was at once recognized to be a major contribution. Its title was "Theory of Abelian functions." In it he tackled topical problems by ingenious and innovative methods. Within a year or two his name was known to mathematicians all over Europe. In 1859 he was promoted to full professor at Göttingen ..." (Derbyshire, p. 31).

"Riemann's function theory, known through an 1857 paper on Abelian functions, was the basis for the renown he enjoyed during his lifetime. That work was an outstanding feat, for it attempted to offer a general solution of the Jacobian inversion problem for integrals of arbitrary algebraic functions. The topic emerged from the fascinating competition that Abel and Jacobi sustained in the late 1820s on the subject of elliptic functions (the inverses of elliptic integrals). As for the importance that was attached to it, suffice it to say that Weierstrass became a rising star with his solution of the inversion problem for hyperelliptic integrals in 1854 and 1856. Riemann was tackling a much more general problem and his work, in spite of gaps in the proofs, aroused enormous excitement" (Ferreiros, p. 53). "The depth of originality in Riemann's remarkable paper can be measured by the fact that on reading it no less an authority than Weierstrass withdrew a paper of his own on the same subject, preferring to wait until he had assimilated what Riemann had to say... While Weierstrass had successfully treated integrals on a curve with equation $y^2 = f(x)$, the so-called hyperelliptic case, Riemann dealt with integrals on any algebraic curve whatever... To accomplish this feat, Riemann showed how his ideas of 1851 could be extended to provide a remarkable theory of complex functions on almost any surface" (Bottazzini & Gray, p. 286).

In his 1857 paper "Riemann found it almost indispensable, in order to study Abelian and related functions, to resort to topological considerations. He developed new methods that enabled him to define the "order of connectivity" of a surface - the Euler characteristic - and, later, what Clebsch would call the "genus" of the surface... In the 1851 dissertation he studied connected surfaces with a boundary, and analysed their topological properties by means of dissection into simply-connected components.... In 1857 he analysed closed surfaces, since he was now considering the complex plane completed by a 'point at infinity'... The most astounding example of the intimate relations between topological notions and properties of functions was the Riemann-Roch theorem, which determines the number of linearly independent meromorphic functions on a Riemann surface, having a given number of poles, as a function of the genus of the surface" (Ferreiros, p. 56).

In the first half of his 1857 paper, Riemann made use of what he called 'Dirichlet's principle (he had learned it from Dirichlet's lectures in Berlin): this asserts that the harmonic functions are exactly those which minimize the value of a certain integral. He used a version of Dirichlet's principle to define algebraic functions in terms of their branching behaviour and their poles. The most important result here is 'Riemann's inequality,' which states that the number of linearly independent meromorphic functions (i.e. quotients of analytic functions) with poles of order not greater than n_1, n_2, ..., n_m at m distinct points of a surface of genus g is not greater than $n_1 + n_2 + ... + n_m - g + 1$. In 1864, Riemann's student Gustav Roch was able to strengthen this result, obtaining a formula for the exact number of meromorphic functions – this is the famous Riemann-Roch theorem. In this part of the paper Riemann also discovered that there is a whole class of transformations that do not change the genus of the surface, and hence its topology, but which lead to different functions - these are called 'birational transformations.' Riemann showed that there exists a $(3g - 3)$-dimensional family of birationally inequivalent surfaces of genus g. These 'moduli spaces' of Riemann surfaces continue to be an object of intensive study today.

In the second half of the 1857 paper, Riemann generalized Jacobi's theory of theta functions to functions of several variables and showed that quotients of products of the new theta functions represented algebraic functions. Riemann reduced the solution of the Jacobian inversion problem to the problem of determining the zeros of the theta functions. The answer depends on the conditions under which a theta function is not identically zero on the Riemann surface. Riemann was able to obtain necessary and sufficient conditions for the vanishing of theta functions that gave a complete solution to the problem of inverting Abelian integrals. As a by-product of this study, Riemann derived a number of important identities involving theta functions. These 'Riemann theta-relations' play a vital role in


Theorie

der Abel'schen Functionen

von

B. Riemann.

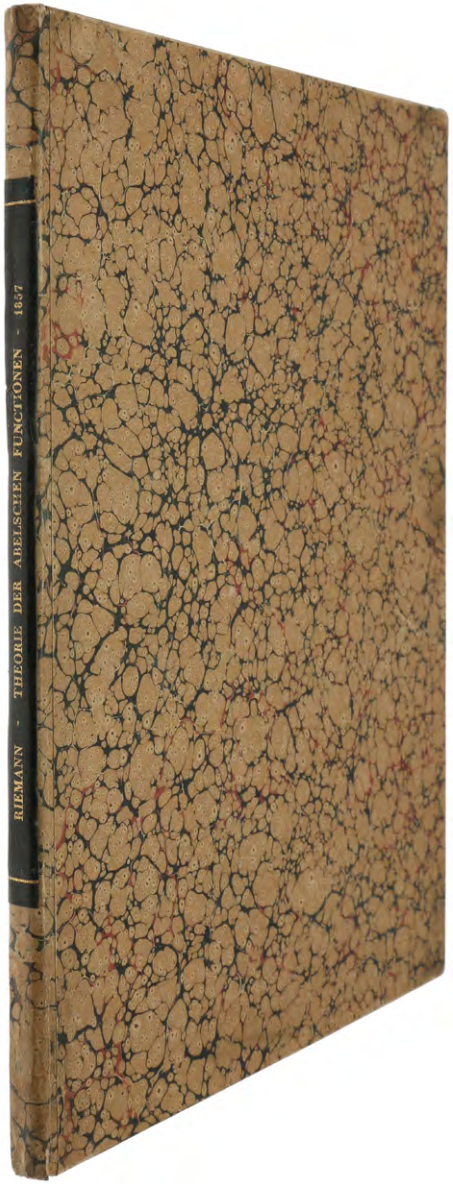
Abdruck aus dem „Journal für die reine und angewandte Mathematik."

Berlin.
Druck und Verlag von Georg Reimer.
1857.

Bücher-sammlung der Lebensversich.-Bank Gotha
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modern research in the theory of Riemann surfaces, for example, in the solution by Shiota in 1986 of the Schottky problem.

DSB XI 449-451; U. Bottazzini & J. Gray, *Hidden Harmony – Geometric Fantasies – The Rise of Complex Function Theory*, 2013; J. Derbyshire, *Prime Obsession*, 2003; J. Ferreiros, *Labyrinth of Thought: A History of Set Theory and its Role in Modern Mathematics*, 2010; I. Grattan-Guiness, *Landmark Writings in Western Mathematics, Chapter 34*; J. Gray, Linear *Differential Equations and Group Theory from Riemann to Poincaré*, 1996; A. N. Kolmogorov & A. P. Yushkevich (eds.), *Mathematics in the 19th century*, Vol. II, 1996; D. Laugwitz, *Bernhard Riemann, 1826-1866*, 1998; E. Scholz, *Geschichte des Mannigfaltigkeitsbegriffs von Riemann bis Poincaré*, 1980; J. Stillwell, *Mathematics and its History*, 3rd edition, 2010.



THE HERTZSPRUNG-RUSSELL DIAGRAM

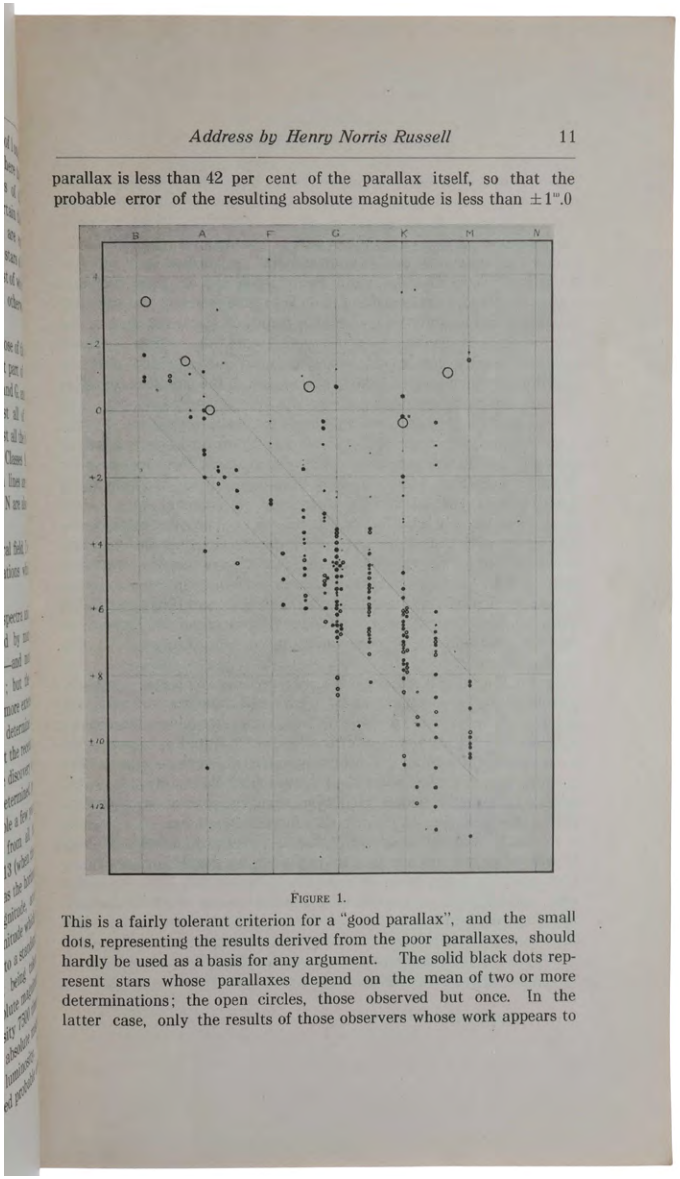
RUSSELL, Henry Norris. *Relations Between the Spectra and Other Characteristics of the Stars.* Offprint from Popular Astronomy, Vol. 22, Nos. 5 – 6, 1914. [Northfield, Minnesota: Goodsell Observatory of Carleton College, 1914.]

$1,800

8vo (245 x 158 mm), pp. [1], 2-40. Stapled as issued in original printed wrappers. Spine rubbed, staples rusted.

First edition, extremely rare separately-paginated offprint, of this classic paper describing the Hertzsprung-Russell diagram. “During his first years on the Princeton faculty, Russell’s analysis of his trigonometric parallax data led him to discover, contemporaneously with, but independently of, Danish astronomer Ejnar Hertzsprung, the correlation between a star’s intrinsic brightness and its spectral type. Hertzsprung was the first to work out a diagram showing the relationship between temperature and luminosity for a group of stars (1911/1912), but few astronomers came across its publication in a photographic journal. When Russell plotted his diagram in late 1913, leading to the publication of the Hertzsprung-Russell diagram the following year [offered here], it had an enormous effect on the scientific community. The diagram remains one of the most important in astrophysics” (*Biographical Encyclopedia of Astronomers*). The paper was published in two parts, pp. 275-294 in Vol. 22, No. 5 and pp. 331-350 in No. 6. Both parts are contained in this separately-paginated offprint. OCLC locates a single copy, at the University of Chicago. No copies in auction records.

Provenance: Richard Prager (1883-1945), German-American astronomer (name stamp on front wrapper). “Prager was born in Hannover, Germany. He became



Address by Henry Norris Russell 11

parallax is less than 42 per cent of the parallax itself, so that the probable error of the resulting absolute magnitude is less than ±1ᵐ.0

B A F G K M N

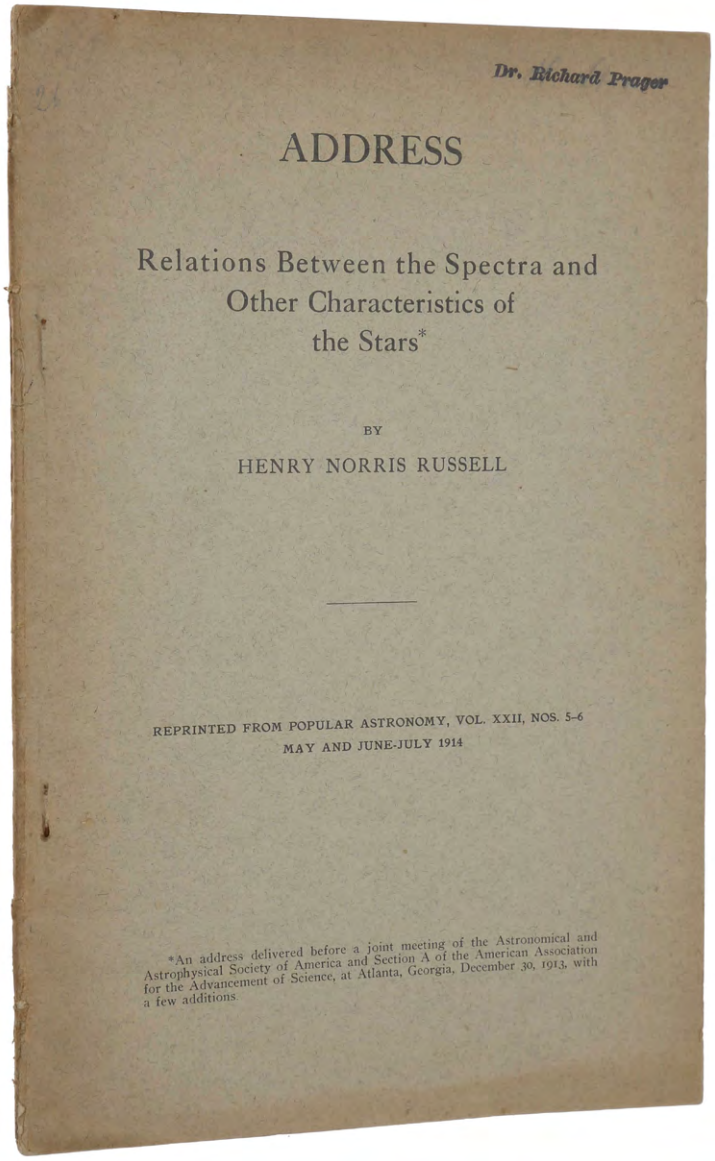
FIGURE 1.

This is a fairly tolerant criterion for a “good parallax”, and the small dots, representing the results derived from the poor parallaxes, should hardly be used as a basis for any argument. The solid black dots represent stars whose parallaxes depend on the mean of two or more determinations; the open circles, those observed but once. In the latter case, only the results of those observers whose work appears to

an assistant in the German Academy of Sciences in 1908. The following year he became head of the Observatorio Nacional in Santiago, Chile, where he remained until 1913. He then returned to Berlin, becoming an observer at the Berlin-Babelsberg Observatory. In 1916 he became a professor. He was an early pioneer of stellar photoelectric photometry. He is noted for his work in the field of variable stars, and he made numerous contributions to *Astronomische Nachrichten* on this topic. In 1938 he was imprisoned by the German Nazis. His friends in England obtained his release in 1939, and he moved to the United States where he accepted a position at the Harvard Observatory. However, his health had suffered from his imprisonment and from his separation from his family, and he died only six years later. The crater Prager on the Moon is named in his honor" (Wikipedia, accessed 6 August 2017).

"Studying the spectra of stars classified by spectral type by Antonia Maury at the Harvard College Observatory, Hertzsprung between 1906 and 1909 commented on relations between brightness and spectral type. Little note was taken of this work, however, until Russell forcefully presented the material in a lecture at a joint meeting of the American Association for the Advancement of Science and the Astronomical and Astrophysical Society in 1913 and then in a paper published in 1914. Russell had much more data than Hertzsprung had had five years earlier, and the case for a relation between absolute magnitude and spectral class was correspondingly more convincing.

"Russell began his classic paper by noting that "investigations into the nature of the stars must necessarily be very largely based upon the average characteristics of groups of stars selected in various ways – as by brightness, proper motion, and the like. The publication within the last few years of a great wealth of accumulated observational material makes the compilation of such data an easy process; but some methods of grouping appear to bring out much more definite and interesting


Dr. Richard Prager

ADDRESS

Relations Between the Spectra and Other Characteristics of the Stars*

BY

HENRY NORRIS RUSSELL

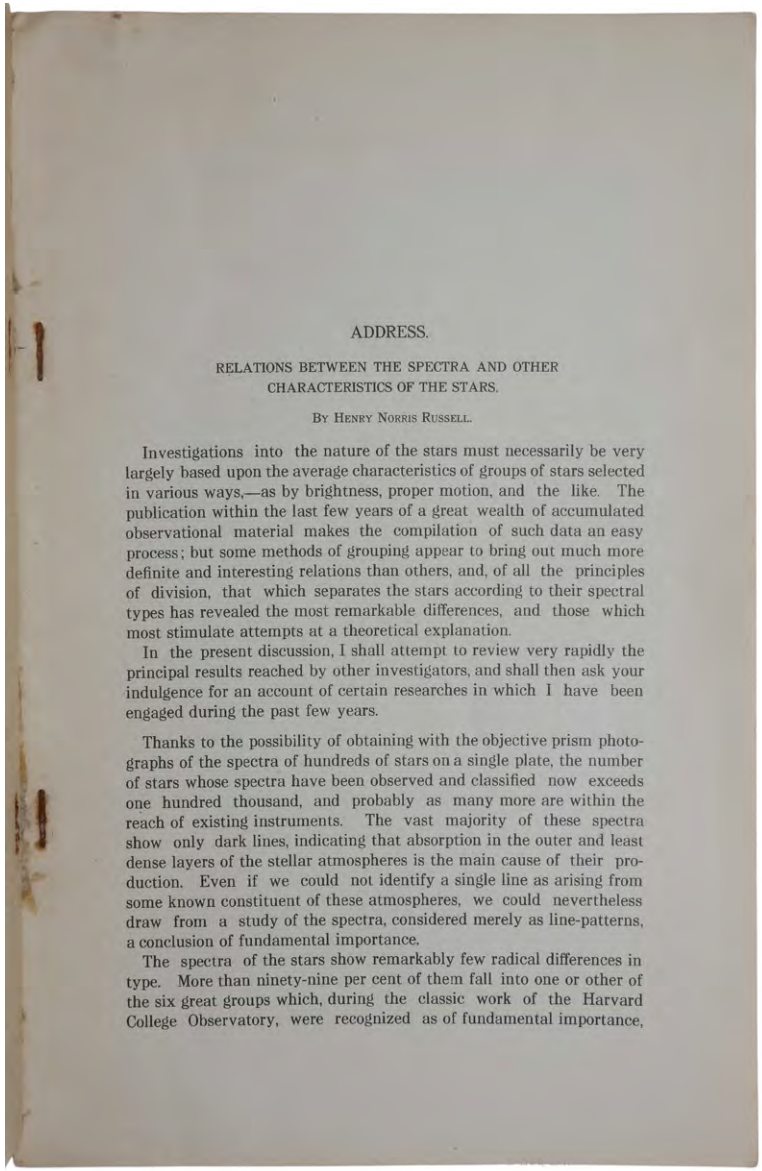
REPRINTED FROM POPULAR ASTRONOMY, VOL. XXII, NOS. 5-6
MAY AND JUNE-JULY 1914

*An address delivered before a joint meeting of the Astronomical and Astrophysical Society of America and Section A of the American Association for the Advancement of Science, at Atlanta, Georgia, December 30, 1913, with a few additions

relations than others, and, of all the principles of division, that which separates the stars according to their spectral types has revealed the most remarkable differences and those which most stimulate attempts at a theoretical explanation."

"He went on to state that that the possibility of obtaining with an objective prism photographs of spectra of hundreds of stars on a single plate had resulted in over 100,000 classified spectra of stars and some interesting conclusions. "The spectra of the stars show remarkably few radical differences in type. More than ninety-nine per cent of them fall into one or other of the six great groups which, during the classic work of the Harvard College Observatory, were recognized as of fundamental importance, and received as designations, by the process of 'survival of the fittest', the rather arbitrary series of letters B, A, F, G, K, and M. That there should be so few types is noteworthy; but much more remarkable is the fact that they form a continuous series."

"Russell's primary concern was to identify the predominant cause of the spectral differences, which he attributed to differences in temperature. Also in his classic paper he rapidly reviewed some of the relations brought to light between other characteristics of the stars and their spectral types, and then discussed in more detail the relations between spectra and brightness. Hertzsprung and others had already discussed this issue, and many of the facts presented by Russell were not new. But his material was much more extensive than any hitherto assembled. There were direct trigonometric parallaxes for a few stars, and average proper motions and thus average parallaxes or distances for many others. Russell assembled data from many astronomical studies. Using approximately 300 stars whose distances had been directly measured by 1913, and another 150 stars in four clusters with well-known distances, Russell found that graphs of absolute magnitude and spectral type produced a rapidly descending diagonal line from B to M and another, almost horizontal line, starting at B and running to M. Had he



ADDRESS.

RELATIONS BETWEEN THE SPECTRA AND OTHER CHARACTERISTICS OF THE STARS.

By Henry Norris Russell.

Investigations into the nature of the stars must necessarily be very largely based upon the average characteristics of groups of stars selected in various ways,—as by brightness, proper motion, and the like. The publication within the last few years of a great wealth of accumulated observational material makes the compilation of such data an easy process; but some methods of grouping appear to bring out much more definite and interesting relations than others, and, of all the principles of division, that which separates the stars according to their spectral types has revealed the most remarkable differences, and those which most stimulate attempts at a theoretical explanation.

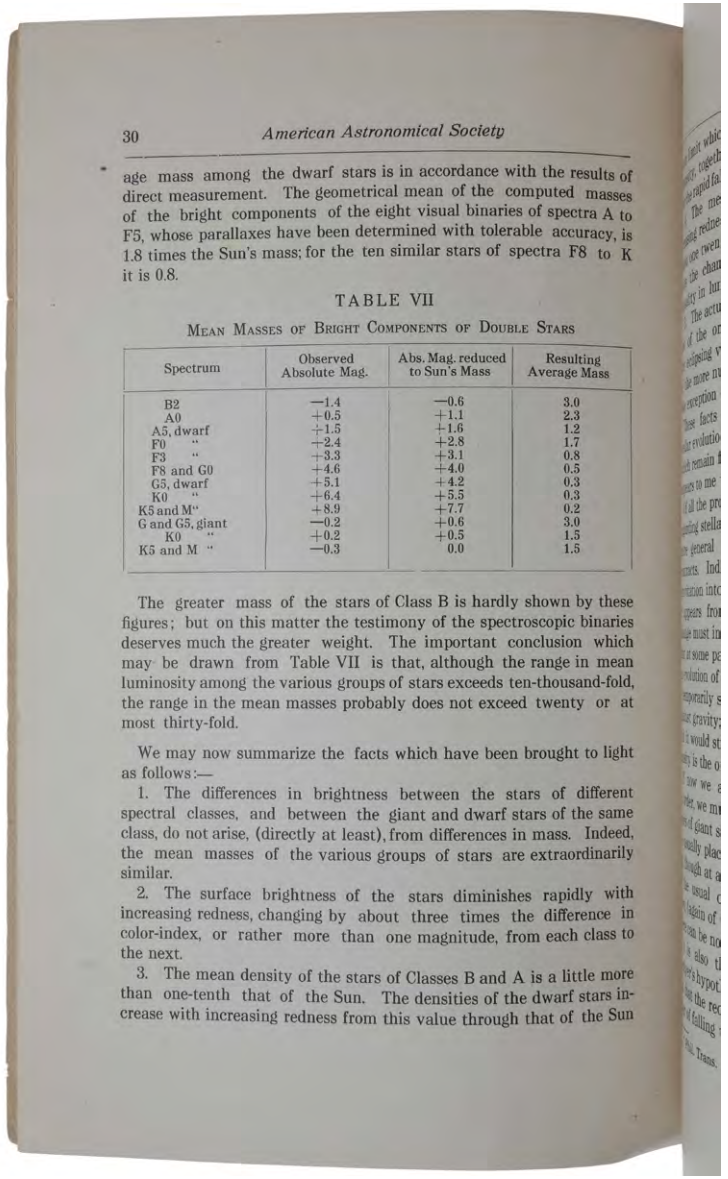
In the present discussion, I shall attempt to review very rapidly the principal results reached by other investigators, and shall then ask your indulgence for an account of certain researches in which I have been engaged during the past few years.

Thanks to the possibility of obtaining with the objective prism photographs of the spectra of hundreds of stars on a single plate, the number of stars whose spectra have been observed and classified now exceeds one hundred thousand, and probably as many more are within the reach of existing instruments. The vast majority of these spectra show only dark lines, indicating that absorption in the outer and least dense layers of the stellar atmospheres is the main cause of their production. Even if we could not identify a single line as arising from some known constituent of these atmospheres, we could nevertheless draw from a study of the spectra, considered merely as line-patterns, a conclusion of fundamental importance.

The spectra of the stars show remarkably few radical differences in type. More than ninety-nine per cent of them fall into one or other of the six great groups which, during the classic work of the Harvard College Observatory, were recognized as of fundamental importance,

had thousands of stars rather than hundreds, and had he been able to plot their absolute magnitudes without the uncertainty arising from observational errors and from using averages of distances, the data points very likely would lie even closer together. There were two great classes of stars: (1) the giant stars, about 100 times as bright as the sun and varying little with spectral type, and (2) the dwarf stars, of smaller brightness and falling off rapidly with increasing redness.

"With giant and dwarf stars differentiated, Russell went on to ask in his classic paper what in the nature of their constitutions gave rise to the differences in brightness and why did the differences show a systematic increase with spectral type. This investigation took him into the issue of a possible correlation between mass and luminosity and into the more general problem of stellar evolution. The paper was remarkable both in its range and its depth. Much that Russell said would bear revision, but he had set an agenda for others to follow, in stellar evolution and in outlining for further study and refinement what would in future be referred to as the Hertzsprung-Russell diagram" (Hetherington (ed.), *Routledge Encyclopedia of Cosmology*, pp. 278-280).



30 *American Astronomical Society*

age mass among the dwarf stars is in accordance with the results of direct measurement. The geometrical mean of the computed masses of the bright components of the eight visual binaries of spectra A to F5, whose parallaxes have been determined with tolerable accuracy, is 1.8 times the Sun's mass; for the ten similar stars of spectra F8 to K it is 0.8.

TABLE VII

MEAN MASSES OF BRIGHT COMPONENTS OF DOUBLE STARS

Spectrum	Observed Absolute Mag.	Abs. Mag. reduced to Sun's Mass	Resulting Average Mass
B2	−1.4	−0.6	3.0
A0	+0.5	+1.1	2.3
A5, dwarf	+1.5	+1.6	1.2
F0 "	+2.4	+2.8	1.7
F3 "	+3.3	+3.1	0.8
F8 and G0	+4.6	+4.0	0.5
G5, dwarf	+5.1	+4.2	0.3
K0 "	+6.4	+5.5	0.3
K5 and M"	+8.9	+7.7	0.2
G and G5, giant	−0.2	+0.6	3.0
K0 "	+0.2	+0.5	1.5
K5 and M "	−0.3	0.0	1.5

The greater mass of the stars of Class B is hardly shown by these figures; but on this matter the testimony of the spectroscopic binaries deserves much the greater weight. The important conclusion which may be drawn from Table VII is that, although the range in mean luminosity among the various groups of stars exceeds ten-thousand-fold, the range in the mean masses probably does not exceed twenty or at most thirty-fold.

We may now summarize the facts which have been brought to light as follows:—

1. The differences in brightness between the stars of different spectral classes, and between the giant and dwarf stars of the same class, do not arise, (directly at least), from differences in mass. Indeed, the mean masses of the various groups of stars are extraordinarily similar.

2. The surface brightness of the stars diminishes rapidly with increasing redness, changing by about three times the difference in color-index, or rather more than one magnitude, from each class to the next.

3. The mean density of the stars of Classes B and A is a little more than one-tenth that of the Sun. The densities of the dwarf stars increase with increasing redness from this value through that of the Sun

VERY RARE ALS FROM THE FOUNDER OF WAVE MECHANICS

SCHRÖDINGER, Erwin. *Autograph letter, signed by Schrödinger, responding to the Anglo-German astronomer Arthur Beer's request for assistance in helping the Czech physicist Kurt Sitte (1910-93) to escape from Nazi Germany and find a position in London (the body of the letter is presumably in a secretary's hand). Sitte succeeded in moving to the UK after the War, later finding a position in Israel, where in 1960 he was arrested and subsequently convicted as a Soviet spy. After his release in 1963 he moved to West Germany.*

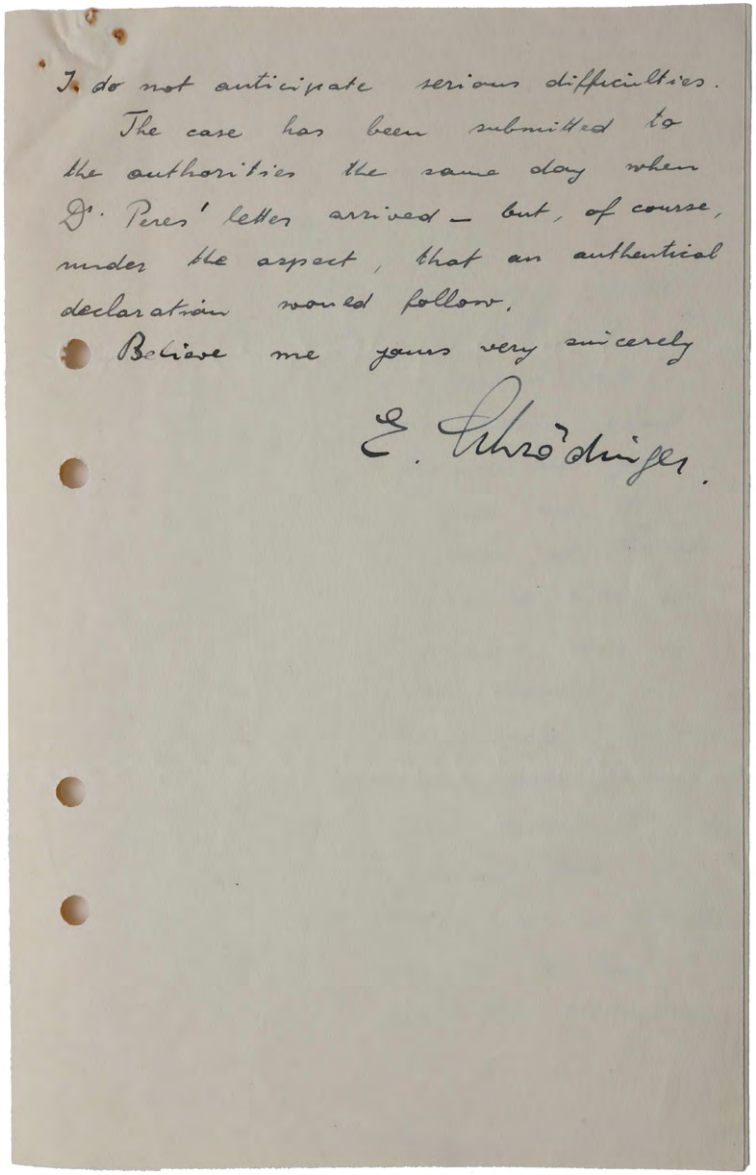
$750

One leaf (210 x 267 mm), blind stamp ' 45. Victoria Road, Clontarf, Dublin' to upper right corner, vertically folded into four pages, writing to first and third page, punch holes for ring binder, small rust stains and holes to upper left corner from staple.

21 December 1939

Dear Doctor Beer,

I thank you very much for your letter of 18th inst. I hasten to reply that I had Dr Peres' letter with details about Dr Sitte – but it did not yet contain the declaration of the Society for the Protection of Science and Learning. I should like to point out (as I already did to Dr Peres) that this would, in my view, be indispensable. On the other hand, with a declaration stating that (as you said) Dr Sitte would receive a grant from the Society and a place in a London laboratory as soon as he has left Germany


I do not anticipate serious difficulties.
The case has been submitted to the authorities the same day when Dr. Peres' letter arrived – but, of course, under the aspect, that an authentical declaration would follow.
Believe me yours very sincerely
E. Schrödinger.

– with this declaration I do not anticipate serious difficulties. The case has been submitted to the authorities the same day when Dr Peres' letter arrived – but, of course, with the aspect [expectation?] that an authorized declaration would follow.

Believe me yours very sincerely

E Schrödinger

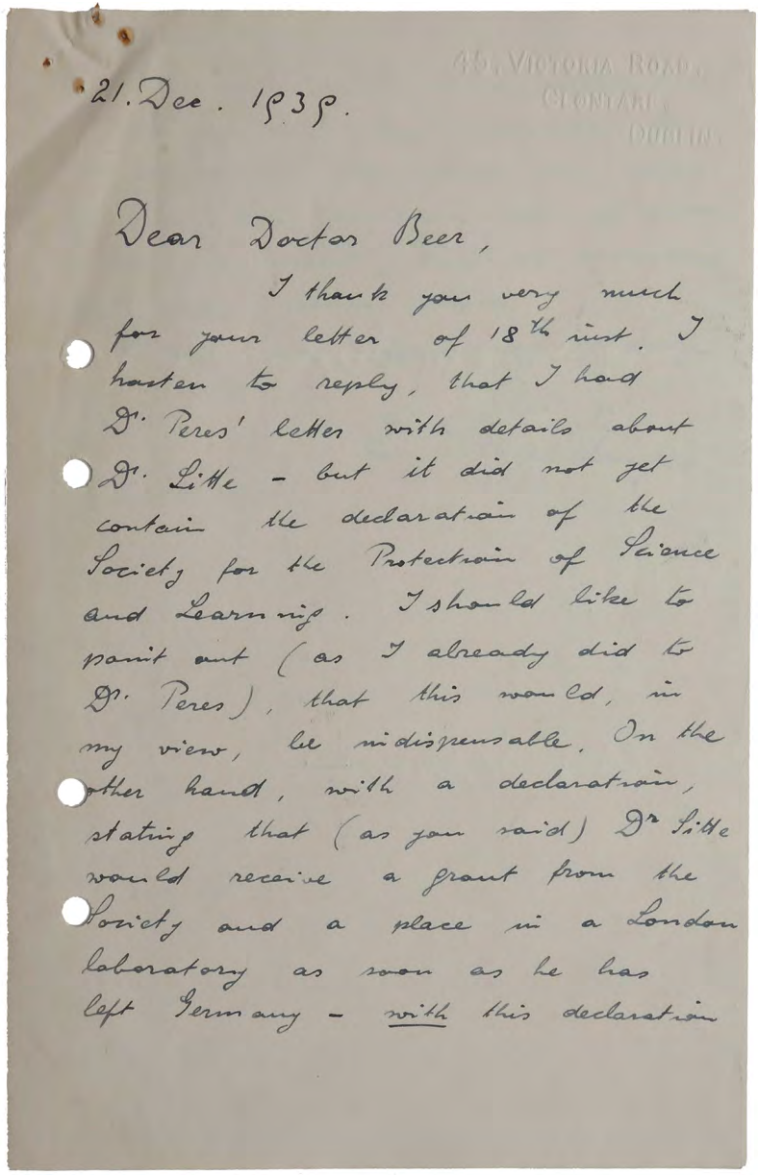
Sitte was born in Reichenberg in Bohemia, which became part of Czechoslovakia in 1919, and then part of an enlarged Germany in 1938. He obtained his doctorate from the University of Prague in 1932 and secured a teaching position there three years later. "Sitte was not a member of any party, but tried to promote good relations between Germans and Czechs, and had close contact with Beneš [the then President of Czechoslovakia]. According to a letter written by Alfred Peres of the Czech Refugee Trust Fund in London to Erwin Schrödinger in December 1939, Sitte's wife Hedwig was Jewish, but he had refused to divorce her. He had been arrested by the Gestapo on 15 March 1939, the day of Hitler's occupation of Czechoslovakia, released after two months, but then taken to Buchenwald concentration camp. Schrödinger passed on the letter to the Department of the Taoiseach, which in turn sent it to the Department of Justice, which then raised enquiries with the Department of External Affairs. Sitte, who numbered Max Planck among his referees, was seen as very promising, and correspondence between Schrödinger and the different departments shows that there was general support for him to be allowed temporary entry to Ireland, as a stepping stone to the UK. The SPSL [Society for the Protection of Science and Learning] eventually promised to support him and to approach the Home Office once Sitte was in Ireland, but the British Home Office refused him the necessary re-entry permit" (Holfter & Dickel, *An Irish Sanctuary: German-speaking Refugees in Ireland 1933–1945*, p. 322). As the end of the war approached, on 11 April 1945 Kurt Sitte was one of those freed from the Buchenwald by the United States Army. His wife had also survived Buchenwald.

Between 1946 and 1948 Sitte and his wife lived in the UK where he was employed as a research fellow at Edinburgh and Manchester. In 1948 he moved to the USA as Professor of Physics at Syracuse University, where he undertook research in nuclear physics and cosmic radiation. He came under suspicion in the age of McCarthyism because of contacts he had maintained with Czech communists he had met in Buchenwald, and in 1953, at the instigation of the FBI, his residence permit was not renewed. After a year in Brazil he accepted a post at the Israel Institute of Technology in Haifa, where he set up the Nuclear Physics department and became its head. He came under suspicion through his visits to Czechoslovakia and two stays in the Soviet Union. He was arrested on 15 June 1960 on the allegation that he had betrayed state secrets to an (unnamed) foreign power. He was tried in secret on 5 November, and on 7 February 1961 he was sentenced to five years in jail. However, because of 'good behaviour' he was released early, on 26 March 1963. In the same year he moved to West Germany, taking up a professorship at the Albert-Ludwig University of Freiburg.

Famous for his 'wave function' formulation of quantum mechanics, Schrödinger (1887-1961) left Germany in 1934, taking up a Fellowship at Magdalen College, Oxford. Shortly afterwards he was awarded the Nobel Prize in Physics (jointly with Paul Dirac). In 1938 he received an invitation from Ireland's Taoiseach, Eamon de Valera, to reside in Ireland and agree to help establish an Institute for Advanced Studies in Dublin. He moved to Dublin in 1940, became Professor of Theoretical Physics at the Institute, and remained there for 17 years.

Like Sitte, Arthur Beer (1900-1980) was also born in Reichenberg, Bohemia. He received his doctorate in 1927 with a dissertation entitled 'Zur Charakterisierung

der spektroskopischen Doppelsterne' ('On the characterization of spectroscopic binaries'). He then worked at Breslau Observatory, and later at the German Maritime Observatory at Hamburg, before emigrating to Cambridge, England, in 1934 due to the persecution of Jewish scientists in Nazi Germany. He carried out astrophysical research under F. J. M. Stratton (1881-1960) at the Cambridge Solar Physics Observatory from 1934 to 1937, and later served as Senior Assistant Observer at Cambridge Observatories until 1968.



21. Dec. 1935.

Dear Doctor Beer,

I thank you very much for your letter of 18th inst. I hasten to reply, that I had Dr. Peres' letter with details about Dr. Little – but it did not yet contain the declaration of the Society for the Protection of Science and Learning. I should like to point out (as I already did to Dr. Peres), that this would, in my view, be indispensable. On the other hand, with a declaration, stating that (as you said) Dr Little would receive a grant from the Society and a place in a London laboratory as soon as he has left Germany – with this declaration

FOUNDATION OF MODERN GEODESY

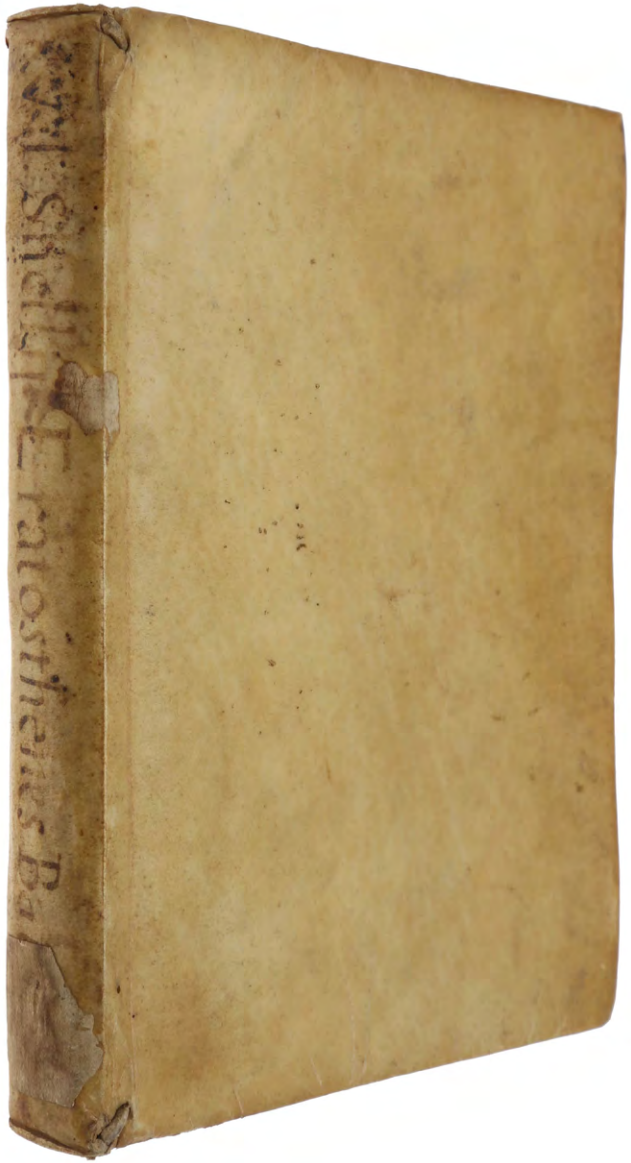
[Norman 1963; Honeyman 2864; Wheeler Gift 55]

SNEL [SNELLIUS or SNEL VAN ROYEN], Willebrord. *Eratosthenes Batavus, de terræ ambitus vera quantitate a Willebrordo Snellio, Δια των εξ αποστηματων μετρουσων διοπτρων, suscitatus.* Leiden: George Abrahamsz van Maarssen for Jodocus Colster, 1617.

$11,000

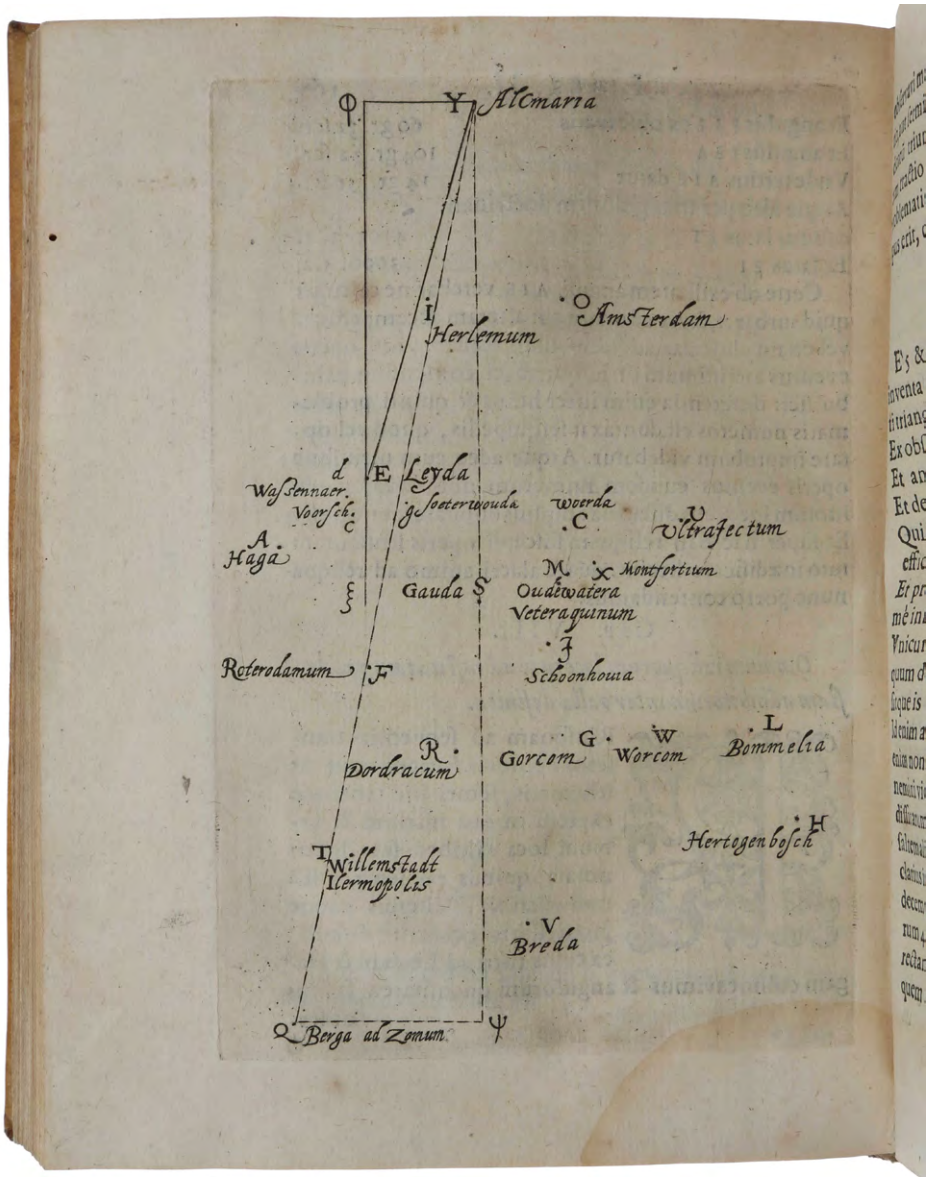
4to (193 x 148 mm), pp. [xii], 263, [1], full-page text engraving, printer's device on title-page with wreath enclosing quotation 'O quam contempta res est homo, nisi supra humana se erexerit,' decorative engraved initials, text in Latin with quotations and examples in Greek, errata on final page. Contemporary vellum, manuscript lettering along spine. Light damp stain to lower inner margin, a fine and unrepaired copy.

First edition of Snel's most important work. This was "the foundation of modern geodesy" (Galileo Project) and of great significance in the history of navigation. "With the development of their oceanic commerce, the Dutch became very interested in the most accurate possible determination of the length of a degree of longitude. A professor of mathematics at the University of Leiden, Snel undertook the task using the method of triangulation first proposed by Gemma Frisius in 1533 but developed by Snel to such an extent that he may rightfully be called one of its founders. He measured the distance between Alkmaar and Bergen-op-Zoom, which lay approximately on the same meridian, and also the distance between the parallels of Alkmaar and Leiden, and from the mean of these two measurements calculated the length of a degree to be 352,347 feet, a more accurate reckoning than any previous attempt. Snel also solved the so-called recession problem for



three points, a problem often named after him. The title of his book pays tribute to the Greek mathematician Eratosthenes, who was famous for his measurement of the circumference of the earth" (Norman). "His figure [for the length of a degree] showed the great error in the popular figure of 300,000 feet, which up to then was used by navigators. One navigator, at all events, was quick to see the advantage of using the lately calculated and more accurate figure, and used it upon his voyage to discover the North West Passage, in 1633. This was Captain Thomas James, after whom the bay to the south east of Hudson's Bay was named. In the account of his 'strange and dangerous' voyage, as he described it, he related how, before leaving Bristol, he caused many small glasses to be made, whose part of time he knew with accuracy, and marked off the log like in accordance with Snellius's measure of feet to one degree" (Hewson, *A History of the Practice of Navigation* (1963), p. 158).

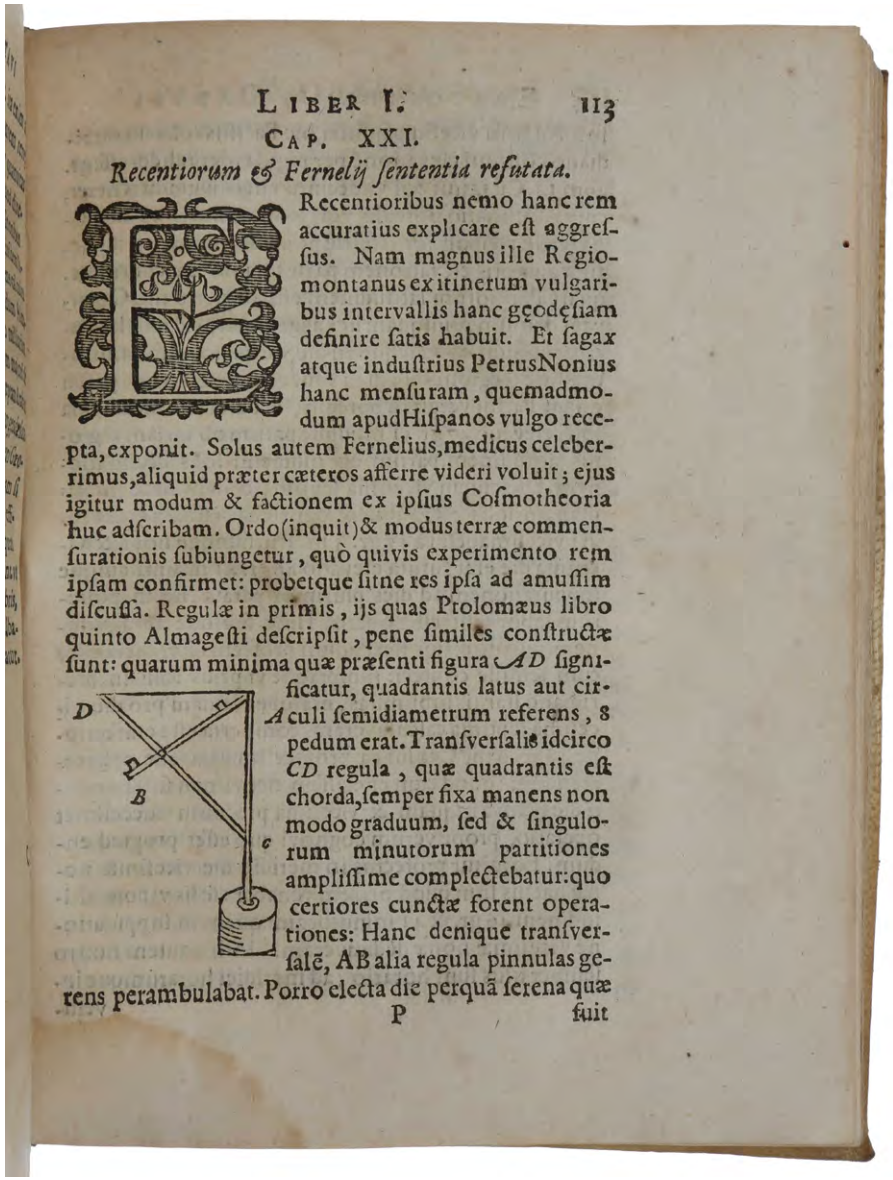
Snel's work, 'The Dutch Eratosthenes, on the true size of the circumference of the earth, recalled from the grave by means of optical instruments according to measured distances,' aimed to address both local and global issues. In his dedicatory letter to the States General, he pointed out that the question of the size of the earth was a very old one which had occupied many scientists. Moreover, the problem of determining the longitude of a ship at sea was urgent, and he proudly proclaimed his contribution to the solution of this problem: 'I have tackled a problem the solution of which has always been desired by everyone, which has been tried very often, and which has also been made famous by the achievement of great men. I present here an accurate assessment of the size of the globe.' This was relevant to navigation because one method of determining the position of a ship at sea was to estimate the distance sailed by the ship from its velocity, to convert this to an angular distance using the known length of a degree, and hence estimate the new position of the ship when the original position was known. The work also had a more local purpose: Snel surveyed a large part of Holland and



the surrounding provinces which enabled the States General "beyond doubt, to register their home-country more accurately" than the Greeks, Romans or any other rulers. A final application, not mentioned in the dedicatory letter as it was less relevant for the dedicatees, was probably also important for Snel: the size of the earth was an important parameter in some astronomical calculations, for example, of solar distance.

"Snellius applied a *triangulation*, that is, a method to survey land by dividing it into triangles. He improved earlier efforts by Gemma Frisius and Tycho Brahe. Gemma Frisius explained the principles of triangulation for the first time in an appendix to his *Cosmographicus liber Petri Apiani*, published in 1533. The surveyor was to collect the data of the directions of different places from one place by means of a magnetic compass and a large circle, then travel to the next place and repeat the procedures. The distances between these places could be determined by walking and counting the steps. Later in the book, Gemma Frisius proposed to take the angles of the network instead of the directions, draw them on a map and calculate the required distances by using proportions. Thus no trigonometrical functions were used. Snellius would improve the precision of the method by calculating the sides of the triangles in the network by means of trigonometrical functions instead of measuring them on a map. There are no indications that Frisius actually carried out a substantial triangulation.

"The direct inspiration for Snellius's endeavours was probably Tycho Brahe, who performed a triangulation in Denmark. Snellius knew Tycho personally. The latter used a combination of astronomical observations and angle measurements to interrelate the positions of a number of Danish localities. However, he did not actually calculate these positions. If he had, he would have noticed that his results were not very accurate …



LIBER I. 113

CAP. XXI.

Recentiorum & Fernelij ſententia refutata.

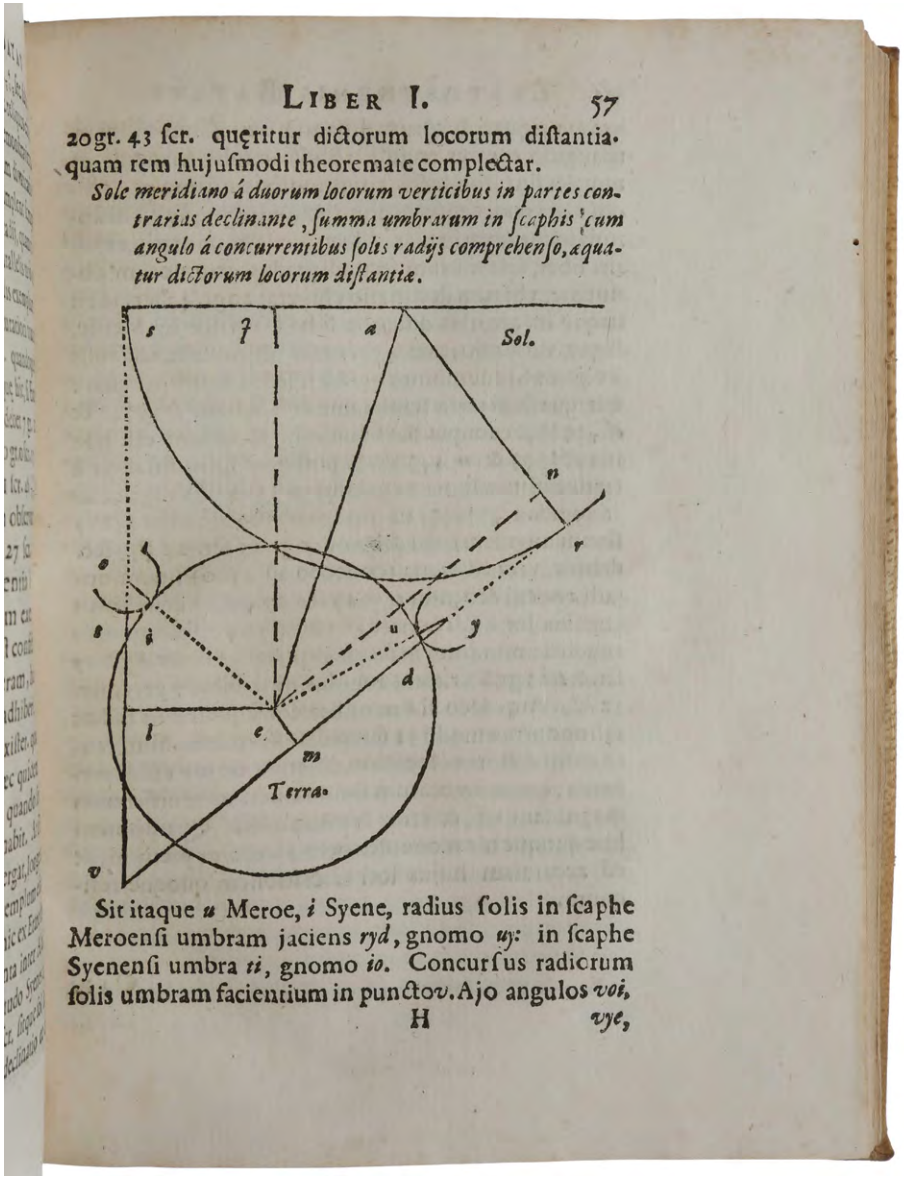
Recentioribus nemo hanc rem accuratius explicare eſt aggreſſus. Nam magnus ille Regiomontanus ex itinerum vulgaribus intervallis hanc gęodęſiam definire ſatis habuit. Et ſagax atque induſtrius PetrusNonius hanc menſuram, quemadmodum apudHiſpanos vulgo recepta, exponit. Solus autem Fernelius, medicus celeberrimus, aliquid præter cæteros afferre videri voluit; ejus igitur modum & factionem ex ipſius Coſmotheoria huc adſcribam. Ordo (inquit) & modus terræ commenſurationis ſubiungetur, quò quivis experimento rem ipſam confirmet: probetque ſitne res ipſa ad amuſſim diſcuſſa. Regulæ in primis, ijs quas Ptolomæus libro quinto Almageſti deſcripſit, pene ſimiles conſtructæ ſunt: quarum minima quæ præſenti figura ∠*AD* ſignificatur, quadrantis latus aut circuli ſemidiametrum referens, s pedum erat. Tranſverſalis idcirco *CD* regula, quæ quadrantis eſt chorda, ſemper fixa manens non modo graduum, ſed & ſingulorum minutorum partitiones ampliſſime complectebatur: quo certiores cunctæ forent operationes: Hanc denique tranſverſalē, AB alia regula pinnulas gerens perambulabat. Porro electa die perquā ſerena quæ

P fuit

"The *Eratosthenes Batavus* consists of two books. Its first book is devoted to a historical survey, Snellius shows himself a true humanist scholar here, knowing an extended range of sources and able to use them. He addressed a number of relevant issues, such as the shape of the earth, its location in the universe, and earlier endeavours to measure the earth. Snellius's most famous predecessor was Eratosthenes of Cyrene (276-194 BC), who had computed the circumference of the earth on the basis of the distance and the difference in latitude between two localities in Egypt almost on the same meridian. Snellius devoted many pages to a precise explanation of this work, including many figures and calculations … Snellius also mentioned Hipparchus and Ptolemy and discussed the work of some Arab scholars and of Jean Fernel" (pp. 118-120).

The first step in Snel's programme was to establish a unit of measurement (there were no standard units of measurement at the time). Snel proposed to use the Rhenish foot, and he included a picture of half a Rhenish foot in the book. This turned out to be problematic for, as Snel explained to his readers, the size of the paper changed in the process of printing, and indeed after printing the foot turned out to have the wrong measure, which necessitated a correction on the last page of the book.

Snel now used a surveyor's chain to measure three base lines, two in the fields between Leiden and Zoeterwoude (one on the straight line between the towers of the two places and one perpendicular to it) and one between Wassenaar and Voorschoten (all three villages in the neighbourhood of Leiden). By measuring angles between the lines connecting the end points of the base lines and points in the two places nearby, he could calculate the distance between these points in Leiden and Zoeterwoude, and between those in Wassenaar and Voorschoten. The remainder of his observations were conducted from towers, which enabled him to cover larger distances. Snel thus established a triangulation network connecting



LIBER I. 57

20gr. 43 ſcr. quęritur dictorum locorum diſtantia. quam rem hujuſmodi theoremate complectar.

Sole meridiano á duorum locorum verticibus in partes contrarias declinante, ſumma umbrarum in ſcaphis cum angulo á concurrentibus ſolis radijs comprehenſo, æquatur dictorum locorum diſtantiæ.

Sit itaque *u* Meroe, *i* Syene, radius ſolis in ſcaphe Meroenſi umbram jaciens *ryd*, gnomo *uy*: in ſcaphe Syenenſi umbra *ti*, gnomo *io*. Concurſus radiorum ſolis umbram facientium in punctov. Ajo angulos *voi*,

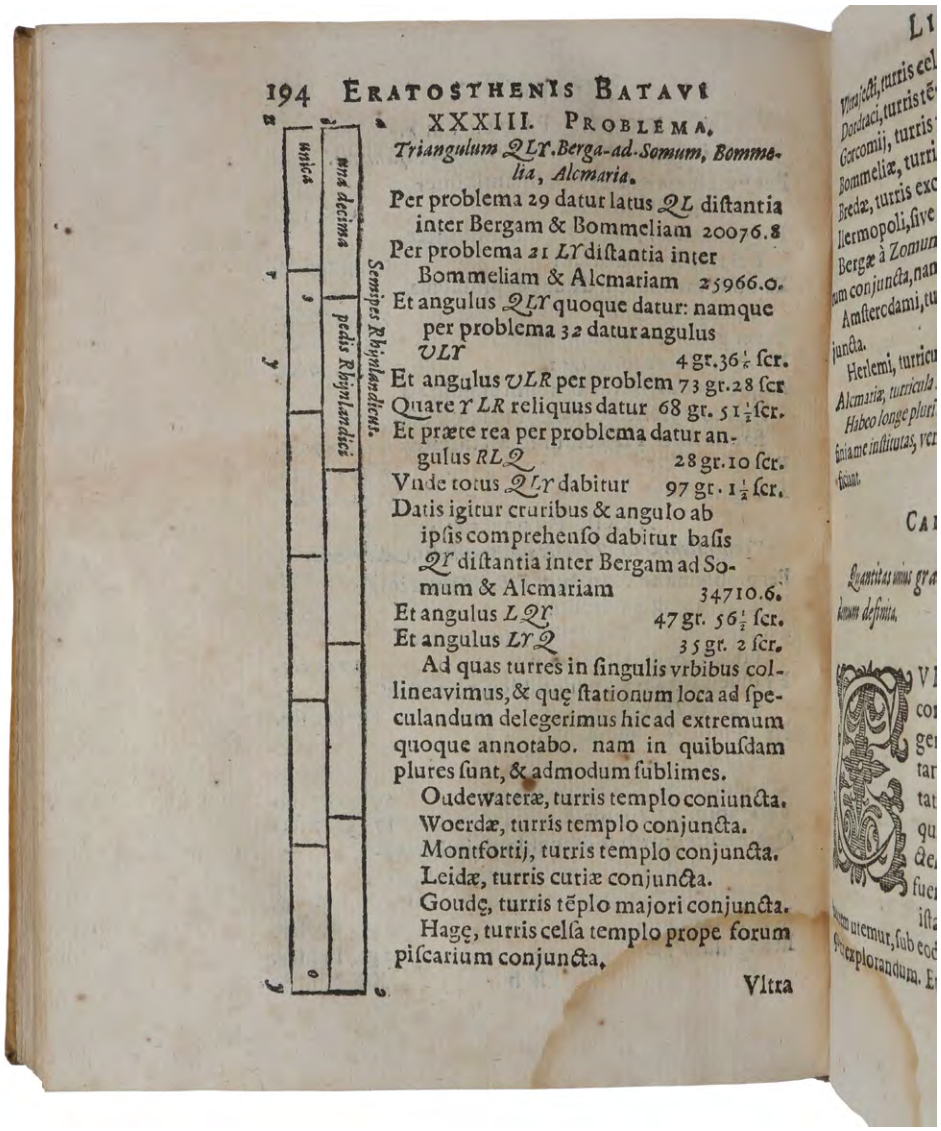
H *vye*,

Alkmaar and Bergen op Zoom. This was a schematic representation of a number of Dutch towns, interconnected by straight lines representing their distances.

The actual determination of the distances in the triangulation network was a huge task. Snel needed instruments that were both accurate and could survive transport. He used a semi-circle (diameter 32 feet, about 1.33 m) for measuring angular distances between towers and a quadrant of iron with a radius of over 52 feet for determining the polar altitudes. He used a smaller quadrant (25 feet) when determining the position of his base line between Leiden and Zoeterwoude. He made observations in Leiden, Alkmaar, Haarlem, Amsterdam, Utrecht, Gouda, Oudewater, The Hague, Zaltbommel, Breda, Willemstad, Dordrecht and Bergen op Zoom, taking all his measuring instruments with him. Fortunately, he did not have to make all these observations by himself. In 1615, he was accompanied by Erasmus and Casparus Sterrenberg, two young barons, who carried out part of the observations and calculations. Snel dedicated the second book of the *Eratosthenes Batavus* to them.

On the basis of the measured angles and the length of one side of the network (Leiden – The Hague), the directions and the lengths of all other sides could be calculated by simple trigonometry. He used his previous work to calculate the distance between lkmaar and Bergen op Zoom. He then had to find the geographical latitudes of Alkmaar, Bergen op Zoom and Leiden, which he did by measuring the height of the Pole Star. He also determined the azimuth (the direction in relation to the meridian) from his own house to the Leiden town hall and to The Hague and used it to calculate the azimuth Leiden – The Hague. This was necessary to orient his triangle network and thus to determine the difference in longitude between Alkmaar and Bergen op Zoom.

In connection to this, Snel solved a geometrical problem, called the Resection



194 ERATOSTHENIS BATAVI

XXXIII. PROBLEMA.

Triangulum QLϒ. Berga-ad-Somum, Bommelia, Alcmaria.

Per problema 29 datur latus *QL* distantia inter Bergam & Bommeliam 20076.8

Per problema 21 *Lϒ* distantia inter Bommeliam & Alcmariam 25966.0.

Et angulus *QLϒ* quoque datur: namque per problema 32 datur angulus *ϑLϒ* 4 gr. 36½ scr.

Et angulus *ϑLR* per problem 73 gr. 28 scr

Quare *ϒLR* reliquus datur 68 gr. 51½ scr.

Et præterea per problema datur angulus *RLQ* 28 gr. 10 scr.

Vnde totus *QLϒ* dabitur 97 gr. 1½ scr.

Datis igitur cruribus & angulo ab ipsis comprehenso dabitur basis *Qϒ* distantia inter Bergam ad Somum & Alcmariam 34710.6.

Et angulus *LQϒ* 47 gr. 56½ scr.

Et angulus *LϒQ* 35 gr. 2 scr.

Ad quas turres in singulis vrbibus collineavimus, & quę stationum loca ad speculandum delegerimus hic ad extremum quoque annotabo. nam in quibusdam plures sunt, & admodum sublimes.

Oudewateræ, turris templo coniuncta.

Woerdæ, turris templo conjuncta.

Montfortij, turris templo conjuncta.

Leidæ, turris curiæ conjuncta.

Goudę, turris tẽplo majori conjuncta.

Hagę, turris celsa templo prope forum piscarium conjuncta.

Vltra

Semipes Rhinlandicus. pedis Rhinlandici.

Problem, which gave him some lasting fame. Snel had to determine the distance of his house to three points in Leiden, the mutual positions of which were known. He considered his solution of the problem to be of no little importance, devoting a separate chapter to it and proudly announced his useful invention for surveying: 'I have invented an elegant theorem for that problem, which can have a widespread application in our country from now on, because the distances between so many illustrious places have been registered with such precision'.

Snellius now knew the distance between Alkmaar and Bergen op Zoom and their relative positions (latitude and difference in longitude). One more datum was needed to reach the desired result: the value of π. Snel used the approximations of Viète, Romanus and Van Ceulen, which were accurate to many more digits than his measuring accuracy necessitated. Through a long series of calculations he finally arrived at the end of his quest: the length of one degree on the meridian of Alkmaar was 28,500 rods (107.33 km) (one rod equals 12 feet), and therefore the length of a meridian was 10,260,000 rods, about 38,639 km. This is about 3.65% less than the modern value.

Willebrord Snell (1580-1626) was the son of Rudolph Snell (1546-1613), the professor of mathematics at Leiden, whom he succeeded in 1613. In 1609 Willebrord published his first major work, a study of the conic sections of Apollonius of Perga. As a self-conscious citizen of the Dutch Republic, proud of his descent from the Batavans (the inhabitants of the Rhine delta in the Roman period), Snel titled the work *Apollonius Batavus*. He went on to publish a Latin translation of Simon Stevin's *Wisconstighe Ghedachtenissen*, a commentary on Petrus Ramus's *Arithmetica*, a treatise on the comet of 1618, and a book on navigation, *Tiphys Batavus* (1624) in which, among other things, he explained the mathematical theory of the rhumb line (loxodrome), the shortest distance between two points on earth as represented by the Mercator projection as a straight line, a theory lacking in Mercator's explanation of his world map (Tiphys was the steersman of the Argonauts). Snel is best known to posterity for the discovery of 'Snell's law', according to which the ratio of the sines of the angles of incidence and refraction is a constant. The manuscript in which he formulated this law was never published and is now lost; the law was first published by Descartes in his *Discours* (1637) (for which some accused Descartes of plagiarism). It now appears that Thomas Harriot preceded Snel in this discovery.

The Latin form *Snellius* is used by Dutch scholars, and was used by the English until the nineteenth century, when the rendition *Snell* appeared, and this is still used in the physics literature when referring to "Snell's law'. Presumably the extra l arose by dropping the Latin *ius* ending.

Wheeler Gift 55; Honeyman 2864; Norman 1963; Waters, p. 424; DSB XII: 500. De Wreede, *Willebrord Snellius (1580-1626): a Humanist Reshaping the Mathematical Sciences*, Doctoral Thesis, University of Utrecht, 2007 (https://dspace.library.uu.nl/bitstream/handle/1874/22992/full.pdf?sequence=6).



BINARY STAR SYSTEMS

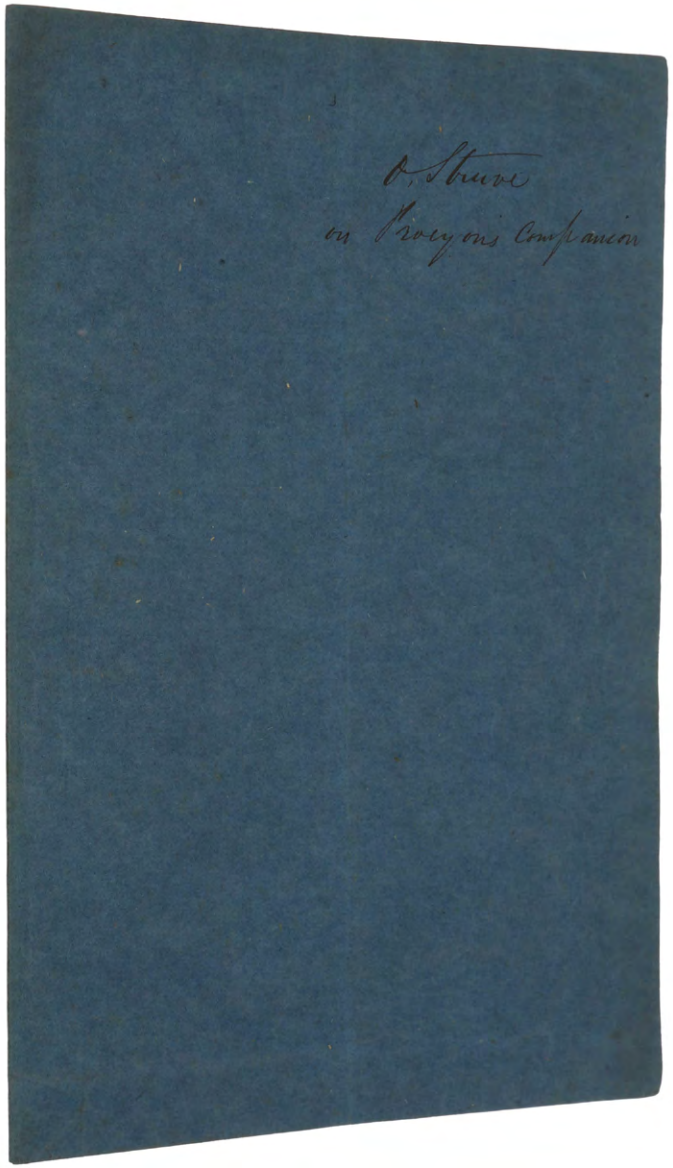
STRUVE, Otto Wilhelm von. *Beobachtung des Procyon als Doppelstern.* Offprint from *Mélanges mathématiques et astronomiques tirés du Bulletin de l'Académie impériale des sciences de Saint-Pétersbourg* (dated 27 March/8 April 1873). [St. Petersburg, 1874]. Accompanied by two related ALS from Struve to the Scottish astronomer R. S. Newall.

$1,500

8vo (219 x 143 mm), pp. 21-28. Original plain blue wrappers, slight vertical crease from having been folded for postage, some minor spotting.

First edition, extremely rare offprint, of Struve's 'discovery' of the faint companion to the star Procyon, which together form a binary system. This offprint is accompanied by two signed autograph letters from Struve to the Scottish amateur astronomer Robert Stirling Newall (1812-89), who at the time owned the largest refracting telescope in the world, asking Newall to confirm Struve's observations. Neither Newall nor other contemporary astronomers were able to do so, however, and it was later shown that Struve's observations were illusory, probably caused by a defect in his telescope lenses. Ironically, however, Struve was correct that Procyon is a binary system: the companion star was observed by the Lick Observatory in 1896. OCLC lists only one copy of this offprint (Hamburg).

Procyon is the brightest star in the constellation Canis Minor. Its name, meaning 'before the dog', derives from the fact that Procyon comes into the sky shortly before Sirius, the 'dog star'. In 1844 Friedrich Wilhelm Bessel (1784-1846) noticed that both Sirius and Procyon exhibited irregularities in their proper motions which Bessel "ascribed to the presence of unseen companions of each of these stars



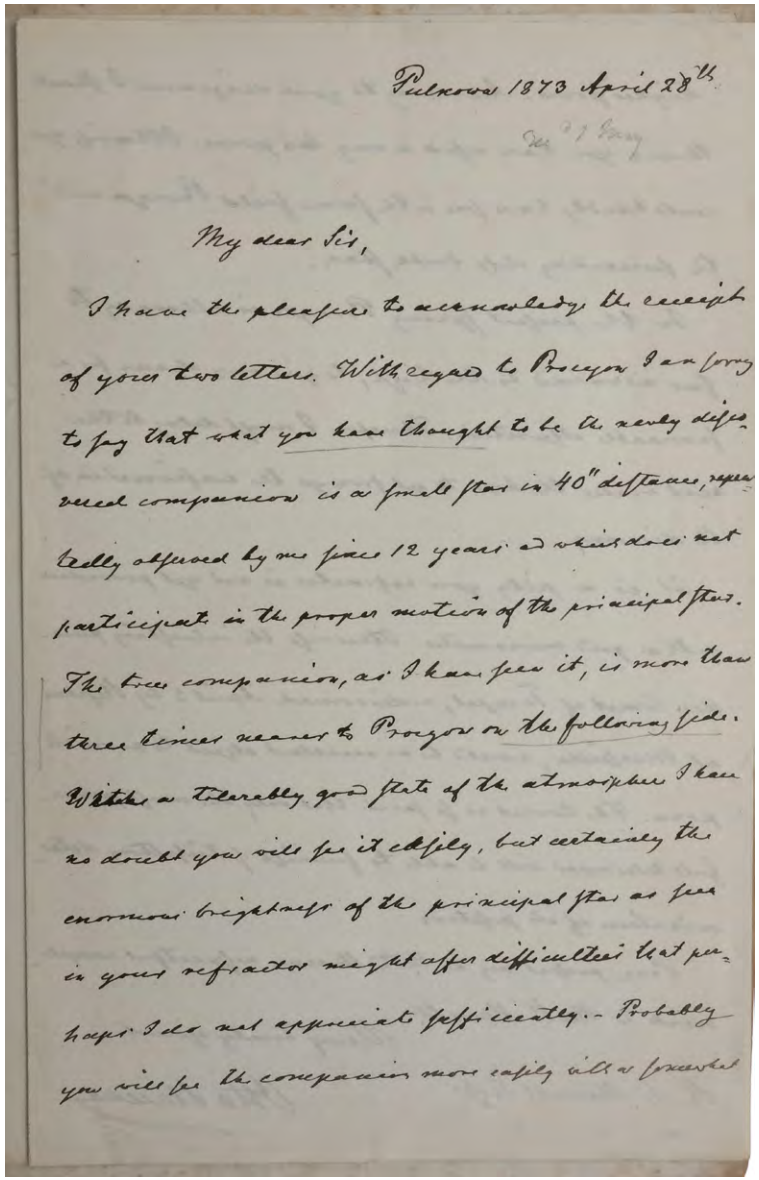
and he was dramatically vindicated in 1862 (after his death) when the American telescope maker Alvan Clark detected the companion of Sirius while testing a new telescope. The nature of this companion was not understood until the twentieth century, but it turned out to be the prototype of a whole new class of stars – the white dwarfs. Confirmation of Bessel's prediction about one star strengthened belief that he had also been right about the other, and many people began to look for a companion to Procyon. We now know that this star, too, is a white dwarf: it is a curious coincidence of constellation mythology that both dog stars should have similar companions. White dwarfs are very small and very faint. It is particularly difficult to see them with certainty when they are close to a bright star. The succeed in detecting the companion of Procyon, the would-be discoverer had to have access to a large telescope and should, by preference, be experienced in double-star observations. Naturally, Otto attempted this discovery and, in 1873, believed he had made it. He published his discovery in that year, complete with measurements of the companion's relative position, and he published further measurements the following year" (Batten, *Resolute and Undertaking Characters*, p. 181).

The two enclosed letters from Struve show his concern to have his 'discovery' confirmed by another observer.

Pulkovo 19 March 1873

Dear Sir

This evening with an extraordinary transparent sky and excellent images, I thought I could see a small companion of Procyon at a distance of somewhat more than 10". I would be most happy if this discovery could be independently confirmed by your magnificent telescope and for this reason I abstain from communicating to you in what direction that object was. Pray look out for Procyon and let me know if and



Pulkowa 1873 April 28th

My dear Sir,

I have the pleasure to acknowledge the receipt of your two letters. With regard to Procyon I am sorry to say that what you have thought to be the newly discovered companion is a small star in 40" distance, regularly observed by me since 12 years and which does not participate in the proper motion of the principal stars. The true companion, as I have seen it, is more than three times nearer to Procyon on the following side. With a tolerably good state of the atmosphere I have no doubt you will see it easily, but certainly the enormous brightness of the principal star as seen in your refractor might offer difficulties that perhaps I do not appreciate sufficiently. Probably you will see the companion more easily with a somewhat

where you see that small object. It was so distinctly seen and measured this evening that I hardly can believe it was an optical illusion.

Since we left England we have nothing heard from you and yours. I hope you have received the letter I wrote you from Cambridge. We frequently talk of you and the friendly reception we have met with in your house, but even more frequently I have to report on the excellent performances of your grand telescope. Prof. Littrow, I am told, wants to get a similar instrument from Cooke. Will he succeed with the second as well as with yours?

Please convey my and Mrs. Struve's most cordial compliments to Mrs. Newall and believe me

Very truly your

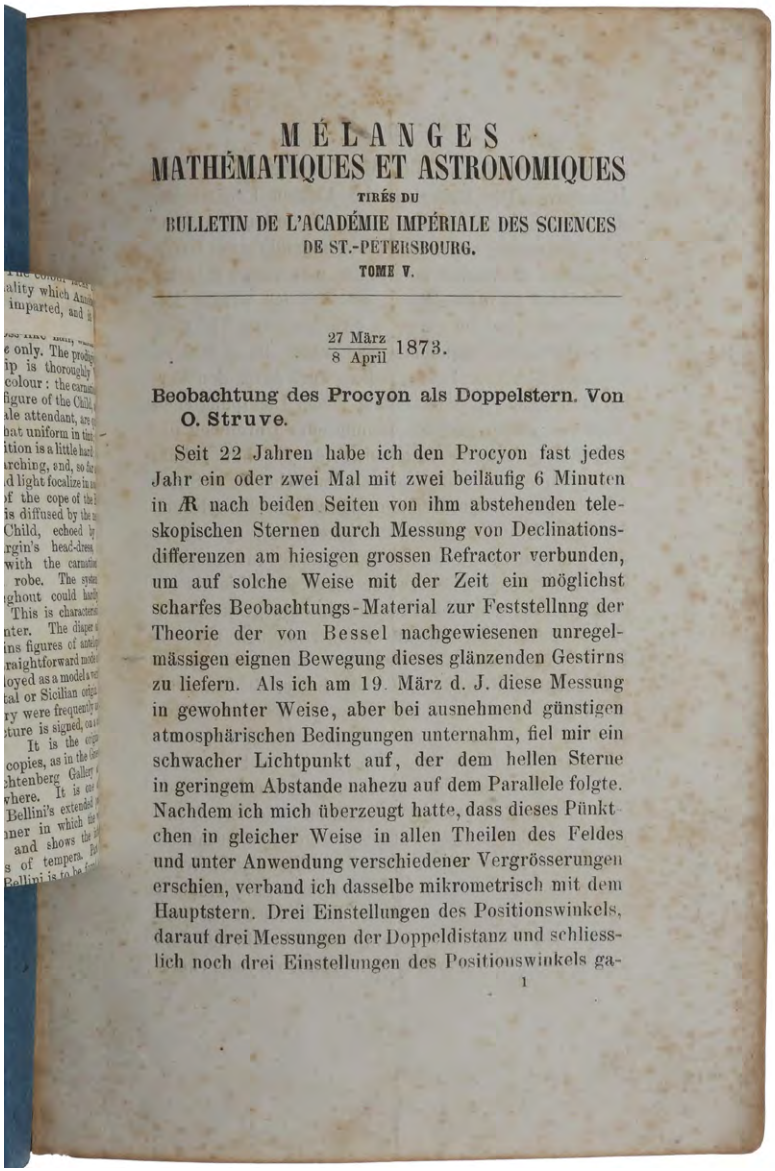
Otto Struve

Have you succeeded to provide the refractor with a good micrometer, so that we may hope to get this year a series of good distances of Neptune's satelllites?

Pulkovo 28 April 1873

My dear Sir,

I have the pleasure to acknowledge the receipt of your two letters. With regard to Procyon I am sorry to say that what you have thought to be the newly discovered companion is a small star in 40" distance, repeatedly observed by me since 12 years



MÉLANGES
MATHÉMATIQUES ET ASTRONOMIQUES
TIRÉS DU
BULLETIN DE L'ACADÉMIE IMPÉRIALE DES SCIENCES
DE ST.-PÉTERSBOURG.
TOME V.

27 März / 8 April 1873.

Beobachtung des Procyon als Doppelstern. Von O. Struve.

Seit 22 Jahren habe ich den Procyon fast jedes Jahr ein oder zwei Mal mit zwei beiläufig 6 Minuten in Æ nach beiden Seiten von ihm abstehenden teleskopischen Sternen durch Messung von Declinationsdifferenzen am hiesigen grossen Refractor verbunden, um auf solche Weise mit der Zeit ein möglichst scharfes Beobachtungs-Material zur Feststellnng der Theorie der von Bessel nachgewiesenen unregelmässigen eignen Bewegung dieses glänzenden Gestirns zu liefern. Als ich am 19. März d. J. diese Messung in gewohnter Weise, aber bei ausnehmend günstigen atmosphärischen Bedingungen unternahm, fiel mir ein schwacher Lichtpunkt auf, der dem hellen Sterne in geringem Abstande nahezu auf dem Parallele folgte. Nachdem ich mich überzeugt hatte, dass dieses Pünktchen in gleicher Weise in allen Theilen des Feldes und unter Anwendung verschiedener Vergrösserungen erschien, verband ich dasselbe mikrometrisch mit dem Hauptstern. Drei Einstellungen des Positionswinkels, darauf drei Messungen der Doppeldistanz und schliesslich noch drei Einstellungen des Positionswinkels ga-

1

and which does not participate in the proper motion of the principal star. The true companion, as I have seen it, is more than three times nearer to Procyon on the following side. With a tolerably good state of the atmosphere I have no doubt you will see it easily, but certainly the enormous brightness of the principal star as seen in your refractor might offer difficulties that perhaps I do not appreciate sufficiently. Probably you will see the companion more easily with a somewhat higher power. According to your diagrams I should think you have used a very low power. Otherwise you could hardly have seen in the same field Procyon and the succeeding close double star.

In the present spring Procyon is already too far advanced to the west to give any chance for a favourable observation. Therefore I might delay to the next winter the hope to get from you the confirmation of the discovery.

It is a pity your refractor is not yet provided with a good micrometer. Otherwise the interesting periodic Comet of Tempel rediscovered April 3 by Stephens at Marseille, would be an excellent object to prove its power. The comet is so faint that only the most powerful telescopes will be able to furnish satisfactory determinations of its position.

Pray present my and Mrs. Struve's respectful compliments to Mrs Newall.

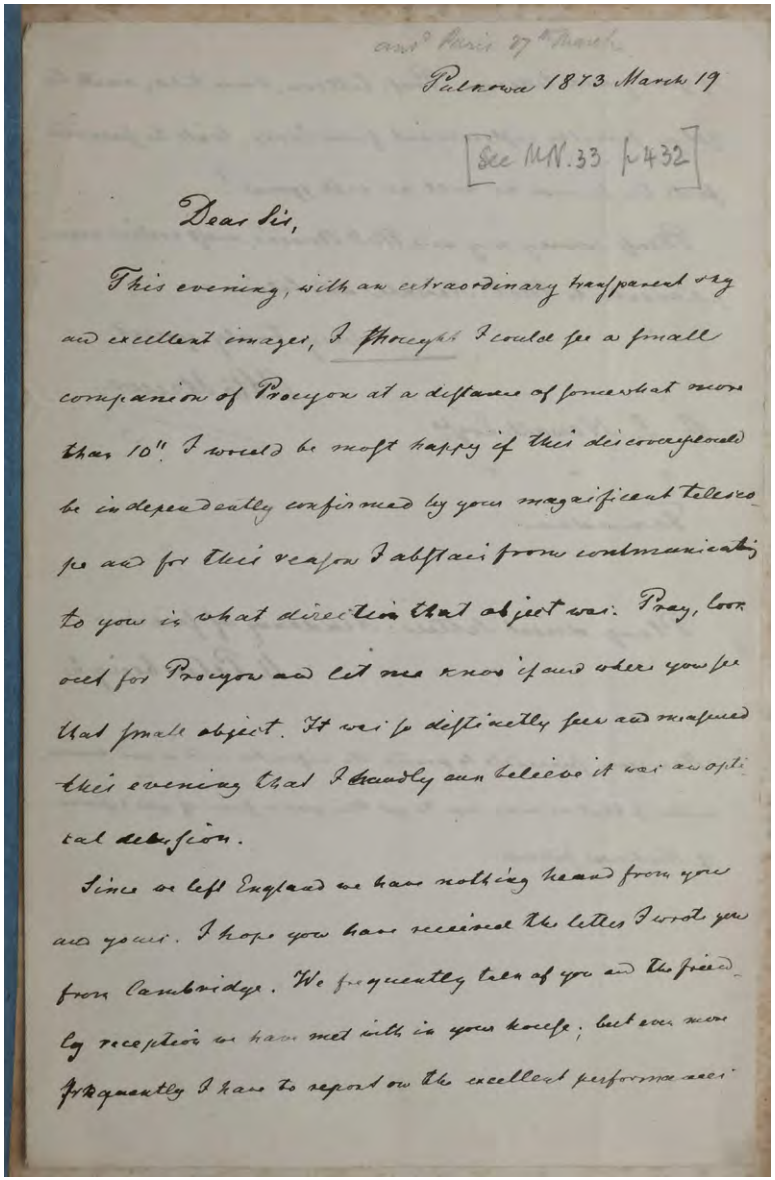
Very truly your

Otto Struve

Neither Newall nor other astronomers Struve contacted (among them the great Canadian-American astronomer Simon Newcomb) were able to verify his observations, and eventually "Otto withdrew his claim to have discovered the companion of Procyon … Despite Otto's conviction that the fault did not lie in the telescope, the objective of the 15-inch formed a ghost image of every bright star, and Otto and his assistants had taken this image for the companion of Procyon they had expected to see. Even the apparent changes in position could be explained by somewhat different positions of the telescope in the different years that the "companion" had been observed … Procyon's real companion was not discovered until 1896, when it was found by Schaeberle [with] the 36-inch refractor of the Lick Observatory. We now know that Otto's observations did not fit at all the well-determined modern orbit of the system" (*ibid.*, p. 184).

Winner of a Gold Medal of the Royal Astronomical Society and a member of the Russian Academy of Sciences, Struve (1819-1905) was a Russian astronomer who pioneered the study of double stars and contributed greatly to our modern understanding of astrophysics. Son of the Russian astronomer Friedrich Georg Wilhelm von Struve (1793-1864), he completed school education at the age of 15 and university education at the age of 20. During his time at the Imperial University of Dorpat, Struve helped his father catalogue the northern skies. Struve took on management of the Pulkovo Observatory from his father in 1858, became its Director in 1862, and remained so until his retirement in 1889. He is credited with discovering an estimated 500 double star systems along with detailed published measurements of their orbits. He completed the most accurate measurement of the earth's curve, known as the Struve Geodetic Arc, categorized the rings of Saturn and discovered the second moon of Uranus. His sons, Ludwig and Hermann, and his grandson Otto, all became successful astronomers.

Newall (1812-1889) was a Scottish engineer and astronomer. He devised a machine for making wire ropes, and made substantial improvements in the construction of telegraph cables. The first successful cable, laid between Dover and Calais on 25 Sept. 1851, was made in his factory. Newall was a keen astronomer, and he commissioned Thomas Cooke to build a telescope for his private observatory at Ferndene, his Gateshead residence. For many years, this 25-inch refractor was the largest in the world. Newall was elected a fellow of the Royal Astronomical Society in 1864, and of the Royal Society in 1875.



and Paris 27th March

Palermo 1873 March 19

[See MN. 33 p. 432]

Dear Sir,

This evening, with an extraordinary transparency and excellent images, I thought I could see a small companion of Procyon at a distance of somewhat more than 10". I would be most happy if this discovery could be independently confirmed by your magnificent telescope and for this reason I abstain from communicating to you in what direction that object was. Pray, look out for Procyon and let me know if and when you see that small object. It was to distinctly seen and measured this evening that I scarcely can believe it was an optical delusion.

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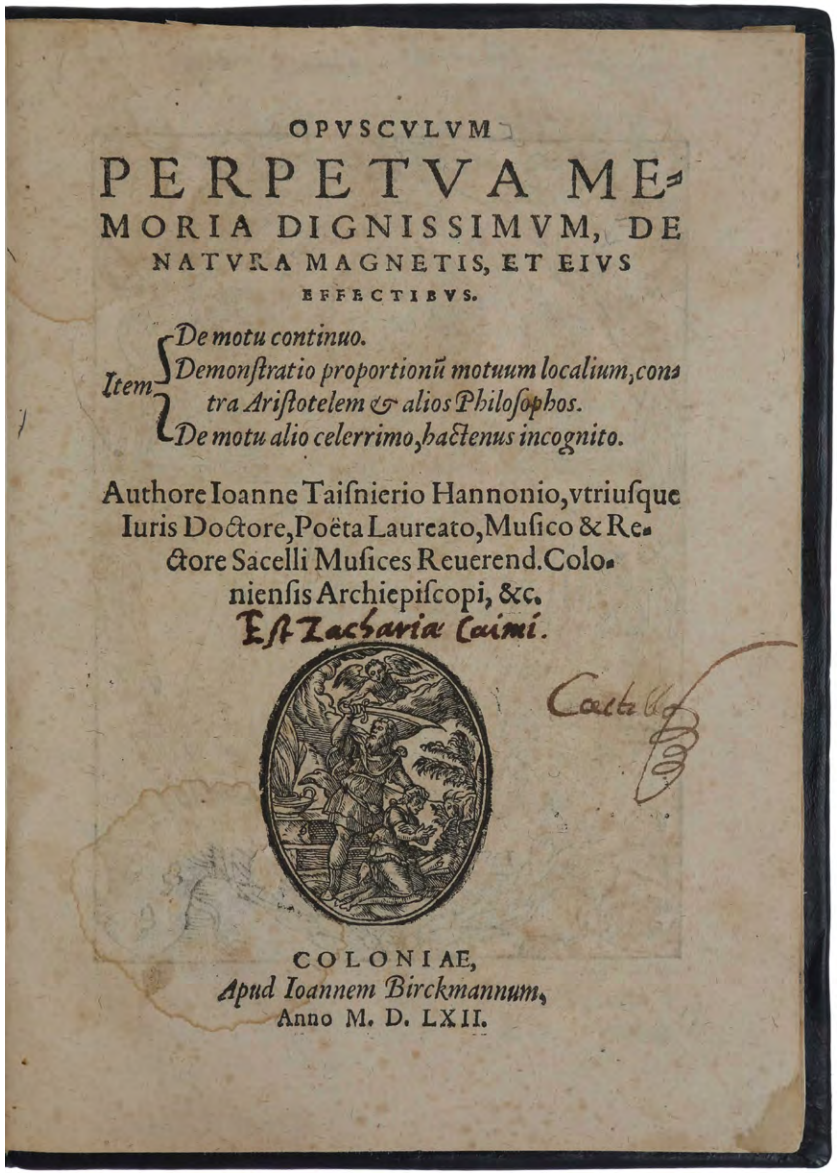
THE SECOND PRINTED WORK ON MAGNETISM

TAISNIER, Jean [PEREGRINUS, Peter; BENEDETTI, Giovanni Battista]. *Opusculum perpetua memoria dignissimum, De natura magnetis, et eius effectibus: item De motu continu; Demonstratio proportionu[m] motuum localium, contra Aristotelem & alios philosophos ; De motu alio celerrimo, hactenus incognito*
Cologne: Johann Birkmann, 1562.

$15,000

Small 4to (187 x 138 mm), pp. [iv], 84, [1]. Device on title, woodcut portrait of the author on title verso and on recto of final leaf, illustrations, diagrams and initials in woodcut (title with two faint water-stains, browning and marginal light water-staining throughout, ink from title inscription showing through to portrait on verso, short tear in outer margin of K2). Modern blue panelled calf gilt, gilt library stamps on front cover.

First edition, extremely rare, of the second printed work on magnetism, preceded only by the *Epistola de magnete of Peregrinus* (1558), which is virtually unobtainable. Taisnier's work is, in fact, a plagiarism of *Peregrinus* (fl. 13th century), which it reprints verbatim, and of the equally rare *Demonstratio proportionum motuum localium* (1554) of Benedetti (1530-90), which anticipates Galileo's theory of falling bodies. "The *Epistola* ranks as one of the most impressive scientific treatises of the Middle Ages. Not only did Peregrinus bring together virtually all the relevant, contemporary knowledge on magnetism, he added to it and, of the greatest importance, organized the whole into a science of magnetism. He formulated rules for the determination of magnetic polarity, which then enabled


OPVSCVLVM
PERPETVA ME-
MORIA DIGNISSIMVM, DE
NATVRA MAGNETIS, ET EIVS
EFFECTIBVS.

Item { De motu continuo. / Demonstratio proportionũ motuum localium, contra Aristotelem & alios Philosophos. / De motu alio celerrimo, hactenus incognito.

Authore Ioanne Taisnierio Hannonio, vtriusque Iuris Doctore, Poëta Laureato, Musico & Rectore Sacelli Musices Reuerend. Coloniensis Archiepiscopi, &c.

Est Zachariæ Caimi.

COLONIAE,
Apud Ioannem Birckmannum,
Anno M. D. LXII.

him to enunciate rules for attraction and repulsion, all of which would today form the basis of an introductory lesson on magnetism. As the two magnetic compasses and perpetual motion devices for clock and wheel testify, Peregrinus was also seriously concerned with the practical application of magnetic force. The subsequent influence of his treatise was considerable. The existence of at least thirty-one manuscript versions of it bears witness to its popularity during the Middle Ages. Of greater significance, however, was its eventual impact on Gilbert, who, in his famous *De Magnete* (1600), built upon the solid empirical rules on magnetic polarity and induction formulated by Peregrinus more than three centuries earlier" (DSB). Benedetti's *Demonstratio* sets forth his "'buoyancy theory of fall' [which] is in many respects identical with that which Galileo set forth in his first treatise *De motu*, composed at Pisa about 1590 but not published during his lifetime" (DSB). Taisnier's work enjoyed greater circulation than either of the works on which it is based, and thus assisted greatly in the diffusion of their ideas. Johannes Kepler used Taisnier's book when developing his theory of a magnetic force emanating from the Sun which drives the planets in their orbits (see DSB VIII, 295), and it was through Taisnier's book that Benedetti's ideas were transmitted to the Collegio Romano (*Encyclopedia of the Scientific Revolution*, p. 128). This is the only complete copy of Taisnier's work listed on ABPC/RBH (the remaining five all lack the leaf at the end with the author's portrait); the Andrade copy (in modern vellum, lacking the portrait leaf) sold for $1400 in 1965. Only the Honeyman copy of Peregrinus has sold at auction in the last 80 years (£11,000 in 1980), and only the Macclesfield copy of Benedetti (part of a sammelband).

Provenance: 'Est Zacharia Caimi' (early title inscription); John Crerar Library, Chicago (bookplate and binding). We have not been able to identify the early owner, but he seems to have owned a significant library: the same inscription can be found in a copy of Della Porta's *De Humana Physiognomonia* (1586) currently being offered in the trade, and in a copy of Swineshead's *Calculationes* (1520) held



IOANNES TAISNIER HANNO. 5

lapide insistat: ibi enim alter Polorum in opposita parte simili modo inuenietur. A punctum verum. B falsum erit.

Modus distinguendi alterum Polum ab altero.

Vtrum punctorum iam dicto-rum respondeat Polo Arctico, siue Septentrionali, ita distinguitur: Accipiatur vas plenũ aqua, satis amplum, in quo Magnes super as-sere leui ponatur, vel in pyxide nõ admodum profunda, ita tamen vt duo pun cta in lapi-de inuen-ta, æquali-ter eleuata iaceant in dicta pyxi de, sic vir-tute lapi-dis eousq; mouebitur pyxis, quo Polus Me-ridionalis, meridiem, alter verò oppos-

B

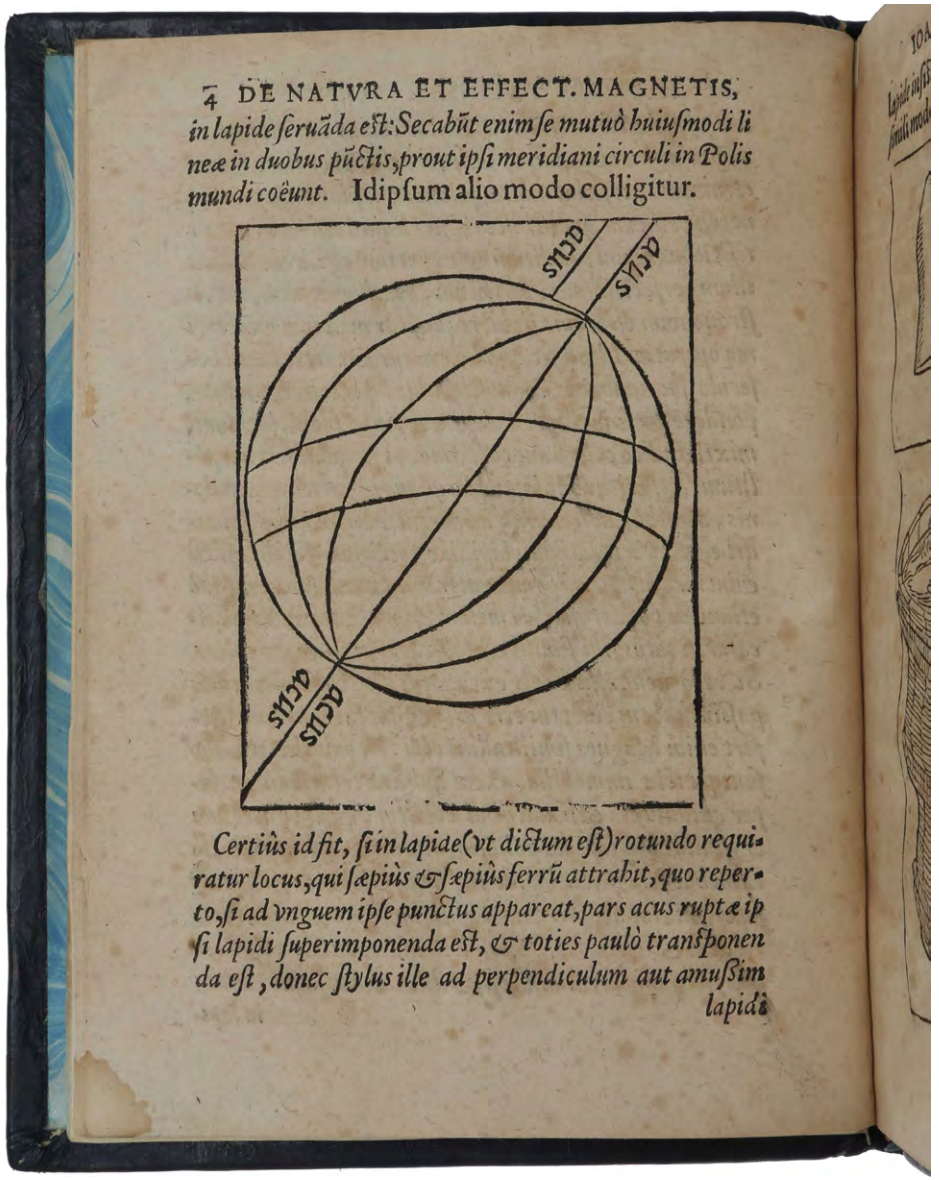
by the Thomas Fisher Library at Toronto.

"Of the early years of Peregrinus nothing is known save that he studied probably at the University of Paris, and that he graduated with the highest scholastic honors. He owes his surname to the village of Maricourt, in Picardy, and the appellation Peregrinus, or Pilgrim, to his having visited the Holy Land as a member of one of the crusading expeditions of the time. In 1269 we find him in the engineering corps of the French army then besieging Lucera, in Southern Italy, which had revolted from the authority of its French master, Charles of Anjou. To Peregrinus was assigned the work of fortifying the camp and laying mines as well as of constructing engines for projecting stones and fireballs into the beleaguered city.

"It was in the midst of such warlike preoccupations that the idea seems to have occurred to him of devising a piece of mechanism to keep the astronomical sphere of Archimedes in uniform rotation for a definite time. In the course of his work over the new motor, Peregrinus was gradually led to consider the more fascinating problem of perpetual motion itself with the result that he showed, at least diagrammatically, and to his own evident satisfaction, how a wheel might be driven round forever by the power of magnetic attraction. Elated over his imaginary success, Peregrinus hastened to inform a friend of his at home; and that his friend might the more readily comprehend the mechanism of the motor and the functions of its parts, he proceeds to set forth in a methodical manner all the properties of the lodestone, most of which he himself had discovered … The letter was addressed from the trenches at Lucera, Southern Italy, in August, 1269, to Sigerus de Foucaucourt, his "amicorum intimus," the dearest of friends …

"An analysis of the *Epistola* shows that

(1) Peregrinus was the first to assign a definite position to the poles of a



lodestone, and to give directions for determining which is north and which south;

(2) He proved that unlike poles attract each other, and that similar ones repel;

(3) He established by experiment that every fragment of a lodestone, however small, is a complete magnet, thus anticipating one of our fundamental laboratory illustrations of the molecular theory;

(4) He recognized that a pole of a magnet may neutralize a weaker one of the same name, and even reverse its polarity;

(5) He was the first to pivot a magnetized needle and surround it with a graduated circle;

(6) He determined the position of an object by its magnetic bearing as done today in compass surveying; and

(7) He introduced into his perpetual motion machine the idea of a magnetic motor, a clever idea, indeed, for a thirteenth century engineer.

This rapid summary will serve to show that the letter of Peregrinus is one of great interest in physics as well as in navigation and geodesy. For nearly three centuries, it lay unnoticed among the libraries of Europe, but it did not escape Gilbert, who makes frequent mention of it in his *De Magnete* (1600); nor the illustrious Jesuit writers, Cabasus [Cabeo], who refers to it in his *Philosophia Magnetica* (1629), and Kircher, who quotes from it in his *De Arte Magnetica* (1641); it was well known to Jean Taisnier, the Belgian plagiarist, who transferred a great part of it verbatim to the pages of his *De Natura Magnetis* (1562), without a word of acknowledgment.



IOANNES TAISNIER HANNO. 9.

ſæpè tentando, facilè experiri poteſt: Quæcũq; enim pars ſubſidet, ea grauior erit. Quo facto, extruas ei circulum Meridianum cum Horizõte, cui duos ſtylos, ſupra quibus quàm leniſſimè poterit moueri, infige, ipſoſque Polos lapidis ad Polos mundi quàm potes exactiſſimè, dirige. Quòd ſi tunc ſucceſſerit, gaudeas te inueniſſe maximum ferè

rerum naturalium miraculum: Videbis enim per eum modum aſcendens, locum Solis, horam & ſimilia quouis mo-

B 3 *mento.*

By this piece of fraud, Taisnier acquired considerable celebrity, a fact that goes to show the meritorious character of the work which he unscrupulously copied" (Introduction to the English translation).

"If Peregrinus' attempt to apply magnetic force to perpetual motion was misconceived, his use of it in the improvement of the compass was surely not. He described two compasses, one wet and one dry. The first (pt. 2, chap. 1), a floating compass, represents a considerable improvement over those that had been in use … With this instrument, perhaps the first mariner's compass with divisions, not only could the direction of a ship be determined, but also the azimuth of the sun, moon, and stars … The second compass (pt. 2, chap. 2), dry and pivoted, was deemed by Peregrinus an improvement over the floating compass … Peregrinus appears to have been the first to describe such a compass" (DSB).

"Benedetti's first important contribution to the birth of modern physics was set forth in the letter of dedication to his *Resolutio* [*omnium Euclidis problematum aliorum. ad hoc necessario inuentorum*, 1553]. The letter was addressed to Gabriel de Guzman, a Spanish Dominican priest with whom he had conversed at Venice in 1552. It appears that Guzman had shown interest in Benedetti's theory of the free fall of bodies, and had asked him to publish a demonstration in which the speeds of fall would be treated mathematically … His demonstration was based on the principle of Archimedes, which probably came to his attention through Tartaglia's publication at Venice in 1551 of a vernacular translation of the first book of the Archimedean treatise on the behavior of bodies in water. Benedetti's "buoyancy theory of fall" is in many respects identical with that which Galileo set forth in his first treatise *De motu*, composed at Pisa about 1590 but not published during his lifetime.



"Although no mention of Benedetti's theory has been found in books or correspondence of the period, lively discussions appear to have taken place concerning it, some persons denying the conclusion and others asserting that it did not contradict Aristotle. In answer to those contentions, Benedetti promptly published a second book, the *Demonstratio* (1554), restating the argument and citing the particular texts of Aristotle that it contradicted. In the new preface, also addressed to Guzman, Benedetti mentioned opponents as far away as Rome who had declared that since Aristotle could not err, his own theory must be false …

"Two editions of Benedetti's *Demonstratio*, which was by no means a mere republication of the *Resolutio*, appeared in rapid succession. The first edition maintained, as did the *Resolutio*, that unequal bodies of the same material would fall at equal speed through a given medium. The second edition stated that resistance of the medium is proportional to the surface rather than the volume of the falling body, implying that precise equality of speed for homogeneous bodies of the same material and different weight would be found only in a vacuum. This correction of the original statement was repeated in Benedetti's later treatment of the question in *Speculationum* (1585).

"Benedetti's original publication of his thesis in 1553 was designed to prevent its theft; perhaps he had in mind the fate of Tartaglia's solution of the cubic equation a few years earlier. But even repeated publication failed to protect it, and indeed became the occasion of its theft. Jean Taisnier, who pirated the work of Petrus Peregrinus de Maricourt in his *Opusculum … de natura magnetis* (1562), included with it—as his own—Benedetti's *Demonstratio*. Taisnier's impudent plagiarism enjoyed wider circulation than Benedetti's original, and was translated into English by Richard Eden about 1578. Simon Stevin cited the proposition as Taisnier's when he published his own experimental verification of it in 1586. But since Taisnier had stolen the *Demonstratio* in its earlier form, he was criticized by


IOANNES TAISNIER HANNO. 14

Magnes

stilo arge

calculo

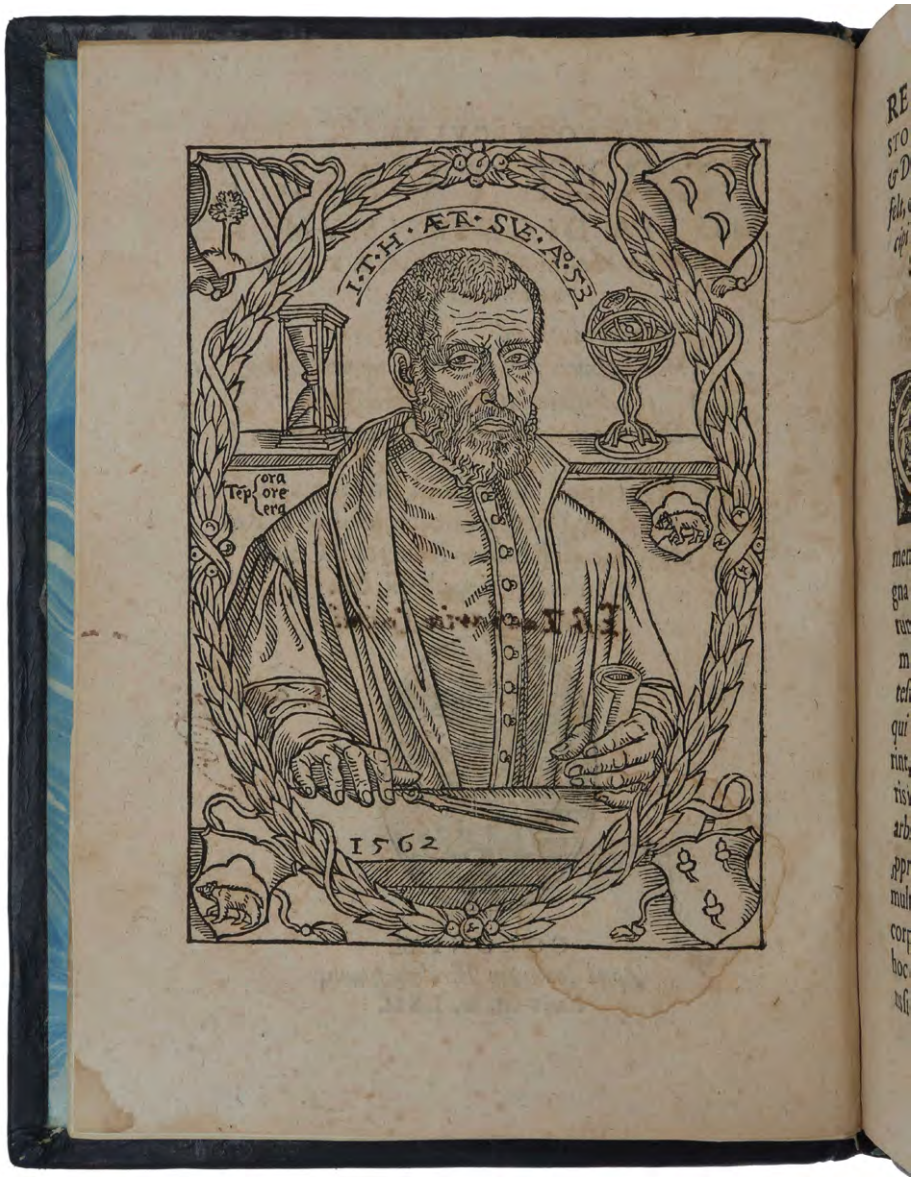
rotæ in imam partem ſit perpetuus, & caſus calculi in op poſitam, inter quoſlibet duos denticulos receptus, etiam perpetuus erit, qui quidē ſuo põdere appetet Centrum terræ, imumque locum. Quare cùm permittet denticulos circa Magnetem quieſcere, fiet tandem, vt inſtituto ſatisfactum fuerit. Sint autem loca intermedia denticulorum, tali arte excauata, vt aptè cadentem calculum recipiant, vt præſens oſtendit figura. Et hæc hactenus pauca compendiosè narrata de motu continuo.

C 2 AD

Stevin for the very fault which Benedetti had long since corrected in the second *Demonstratio* of 1554. Taisnier's appropriation of his book ultimately became known to Benedetti, who complained of it in the preface to his *De gnomonum* (1574). The relatively small circulation of Benedetti's works is evidenced by the fact that it was not until 1741 that general attention was first called to the theft, by Pierre Bayle" (DSB).

Jean Taisnier (1508-62) was a mathematician, philosopher, musician, astrologer and author of several books, among which are, as well as the present work, *Astrologiae* (1559), *De usu annuli sphaerici* (1550) and *Opus mathematicum* (1562). He travelled throughout Europe and a great part of Africa, Asia and America, collecting, he says, the views of the most expert men wherever he went. He taught at the universities of Rome, Ferrara, Venice, Padua, Florence and Palermo. Between 1530 and 1550 he was a member of the court of Emperor Charles V. He retired to Germany after the Emperor's death, where he became choirmaster to the archbishop of Cologne, to whom the present work is dedicated.

Wheeler Gift 53. The letter of Petrus Peregrinus on the magnet, A.D. 1269 (New York, 1904).



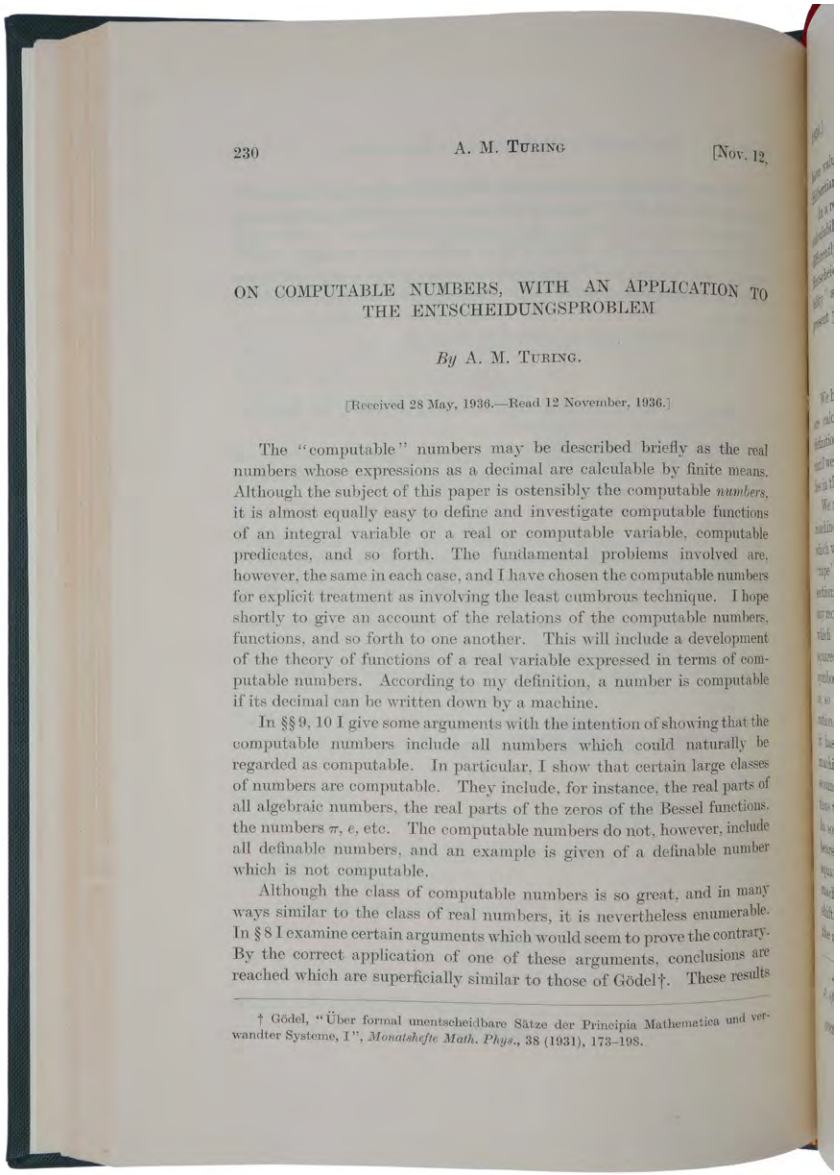
THE UNIVERSAL COMPUTER

TURING, Alan Mathison. *On computable numbers, with an application to the Entscheidungsproblem*, pp. 230-265 in *Proceedings of the London Mathematical Society*, series 2, vol. 42; [with:] *On computable numbers, with an application to the Entscheidungsproblem. A correction*, pp. 544-546 in *ibid.*, vol. 43. London: C.F. Hodgson and Son, [1936-] 1937.

$19,500

8vo (249 x 168 mm). The complete volumes 42 and 43 offered, pp. [iv] 559 [1, blank], with folding table; [iv] 546, recent cloth with gilt spine lettering, completely clean and fresh throughout.

First edition, journal issue. "'On Computable Numbers' is regarded as the founding publication of the modern science of computing. It contributed vital ideas to the development, in the 1940s, of the electronic stored-programme digital computer. 'On Computable Numbers' is the birthplace of the fundamental principle of the modern computer, the idea of controlling the machine's operations by means of a programme of coded instructions stored in the computer's memory. In addition Turing charted areas of mathematics lying beyond the scope of the Turing machine. He proved that not all precisely stated mathematical problems can be solved by computing machines. One such is the *Entscheidungsproblem* or 'decision problem' … In this one article, Turing ushered in both the modern computer and the mathematical study of the uncomputable" (Copeland, *The Essential Turing*, p. 6). The outstanding problem of mathematical logic at the time, the *Entscheidungsproblem*, posed by David Hilbert in 1928, asks whether there is an algorithm that can determine whether any given mathematical statement is true



230 A. M. TURING [Nov. 12,

ON COMPUTABLE NUMBERS, WITH AN APPLICATION TO THE ENTSCHEIDUNGSPROBLEM

By A. M. TURING.

[Received 28 May, 1936.—Read 12 November, 1936.]

The "computable" numbers may be described briefly as the real numbers whose expressions as a decimal are calculable by finite means. Although the subject of this paper is ostensibly the computable *numbers*, it is almost equally easy to define and investigate computable functions of an integral variable or a real or computable variable, computable predicates, and so forth. The fundamental problems involved are, however, the same in each case, and I have chosen the computable numbers for explicit treatment as involving the least cumbrous technique. I hope shortly to give an account of the relations of the computable numbers, functions, and so forth to one another. This will include a development of the theory of functions of a real variable expressed in terms of computable numbers. According to my definition, a number is computable if its decimal can be written down by a machine.

In §§ 9, 10 I give some arguments with the intention of showing that the computable numbers include all numbers which could naturally be regarded as computable. In particular, I show that certain large classes of numbers are computable. They include, for instance, the real parts of all algebraic numbers, the real parts of the zeros of the Bessel functions, the numbers π, e, etc. The computable numbers do not, however, include all definable numbers, and an example is given of a definable number which is not computable.

Although the class of computable numbers is so great, and in many ways similar to the class of real numbers, it is nevertheless enumerable. In § 8 I examine certain arguments which would seem to prove the contrary. By the correct application of one of these arguments, conclusions are reached which are superficially similar to those of Gödel†. These results

† Gödel, "Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme, I", *Monatshefte Math. Phys.*, 38 (1931), 173–198.

or not. "In the long view of intellectual history, I believe universal computation will stand as the single most important idea to emerge in the twentieth century. And this paper is where it first appeared with clarity" (Stephen Wolfram, in *Alan Turing. His Work and Impact*, p. 44). "Inspired by the human computer (i.e., the human engaged in computation), Turing described a notional machine that could read and write symbols along a segmented tape. The machine itself would be capable of assuming various internal states that, together with the input of a single symbol along the tape, could lead to a few primitive atomic actions. Based on the state and the current symbol, each configuration specifies a change (or not) of symbol, a move right or left, and a next state. Working under some straightforward assumptions about the finite and discrete nature of the machine, Turing was able to demonstrate the wide range of numbers (equivalently, the wide class of functions) that could be computed and, moreover, able to specify a single machine, the universal machine, that would be capable of simulating the computations of any such machine. Turing's characterization has come to be seen as a more compelling account of what it means to be effective, mechanical, or algorithmic than any of the various extensionally equivalent formulations offered by his contemporaries" (*New Dictionary of Scientific Biography*, vol. 7, p. 83). The 'Correction' was published in order to remove some formal errors made in the first paper pointed out by the Swiss mathematician Paul Bernays.

"In the spring of 1935 – at the time of von Neumann's visit to Cambridge – Turing was attending Max Newman's lectures on the foundations of mathematics when the *Entscheidungsproblem* first attracted his attention. Hilbert's challenge aroused Turing's instinct that mathematical questions resistant to strictly mechanical procedures could be proved to exist.

"Turing's argument was straightforward – as long as you threw out all assumptions and started fresh. "One of the facets of extreme originality is not to regard as obvious the things that lesser minds call obvious," says I. J. (Jack) Good, who served as an assistant to Turing (then referred to as "Prof") during World War II. Originality can be more important than intelligence, and according to Good, Turing constituted proof. "Henri Poincare did quite badly at an intelligence test, and Prof also was only about halfway up the undergraduate scale when he took such a test." Had Turing more closely followed the work of Alonzo Church or Emil Post, who anticipated his results, his interest might have taken a less original form. "The way in which he uses concrete objects such as exercise books and printer's ink to illustrate and control the argument is typical of his insight and originality," says colleague Robin Gandy. "Let us praise the uncluttered mind."
"A function is computable, over the domain of the natural numbers (0, 1, 2, 3, …), if there exists a finite sequence of instructions (or algorithm) that prescribes exactly how to list the value of the function at $f(0)$ and, for any natural number n, at $f(n + 1)$. Turing approached the question of computable functions in the opposite direction, from the point of view of the numbers produced as a result. "According to my definition," he explained, "a number is computable if its decimal can be written down by a machine."

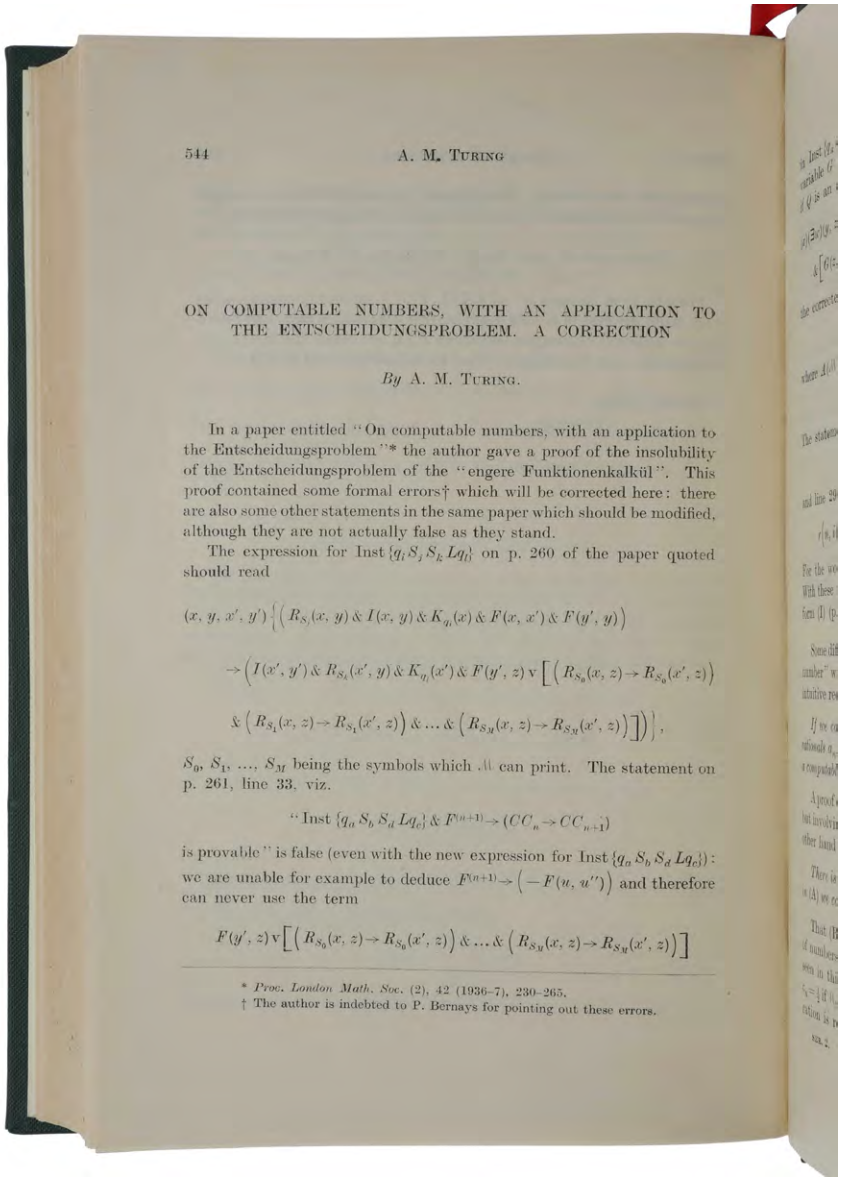
"Turing began with the informal idea of a computer – which in 1935 meant not a calculating machine but a human being, equipped with pencil, paper, and time. He then substituted unambiguous components until nothing but a formal definition of "computable" remained. Turing's machine (which he termed an LCM, or Logical Computing Machine) thus consisted of a black box (as simple as a typewriter or as complicated as a human being) able to read and write a finite alphabet of symbols to and from a finite but unbounded length of paper tape – and capable of changing its own "m-configuration," or "state of mind."

"We may compare a man in the process of computing a real number to a machine which is only capable of a finite number of conditions … which will be

called 'm-configurations,'" Turing wrote. "The machine is supplied with a 'tape' (the analogue of paper) running through it, and divided into sections (called 'squares') each capable of bearing a 'symbol.' At any moment there is just one square … which is 'in the machine' … However, by altering its m-configuration the machine can effectively remember some of the symbols which it has 'seen.' … In some of the configurations in which the scanned square is blank (i.e., bears no symbol) the machine writes down a new symbol on the scanned square; in other configurations it erases the scanned symbol. The machine may also change the square which is being scanned, but only by shifting it one place to right or left. In addition to any of these operations the m-configuration may be changed."

"Turing introduced two fundamental assumptions: discreteness of time and discreteness of state of mind. To a Turing machine, time exists not as a continuum, but as a sequence of changes of state. Turing assumed a finite number of possible states at any given time. "If we admitted an infinity of states of mind, some of them will be 'arbitrarily close' and will be confused," he explained. "The restriction is not one which seriously affects computation, since the use of more complicated states of mind can be avoided by writing more symbols on the tape."

"The Turing machine thus embodies the relationship between an array of symbols in space and a sequence of events in time. All traces of intelligence were removed. The machine can do nothing more intelligent at any given moment than make a mark, erase a mark, and move the tape one square to the right or to the left. The tape is not infinite, but if more tape is needed, the supply can be counted on never to run out. Each step in the relationship between tape and Turing machine is determined by an instruction table listing all possible internal states, all possible external symbols, and, for every possible combination, what to do (write or erase a symbol, move right or left, change the internal state) in the event that combination comes up. The Turing machine follows instructions and never makes mistakes.



544 A. M. TURING

ON COMPUTABLE NUMBERS, WITH AN APPLICATION TO THE ENTSCHEIDUNGSPROBLEM. A CORRECTION

By A. M. TURING.

In a paper entitled "On computable numbers, with an application to the Entscheidungsproblem"* the author gave a proof of the insolubility of the Entscheidungsproblem of the "engere Funktionenkalkül". This proof contained some formal errors† which will be corrected here: there are also some other statements in the same paper which should be modified, although they are not actually false as they stand.

The expression for Inst $\{q_i S_j S_k L q_l\}$ on p. 260 of the paper quoted should read

$$(x, y, x', y') \{ \left(R_{S_j}(x, y) \& I(x, y) \& K_{q_i}(x) \& F(x, x') \& F(y', y) \right) \rightarrow \left(I(x', y') \& R_{S_k}(x', y) \& K_{q_l}(x') \& F(y', z) \vee \left[\left(R_{S_0}(x, z) \rightarrow R_{S_0}(x', z) \right) \& \left(R_{S_1}(x, z) \rightarrow R_{S_1}(x', z) \right) \& \ldots \& \left(R_{S_M}(x, z) \rightarrow R_{S_M}(x', z) \right) \right] \right) \},$$

$S_0, S_1, \ldots, S_M$ being the symbols which $\mathcal{M}$ can print. The statement on p. 261, line 33, viz.

"Inst $\{q_a S_b S_d L q_c\} \& F^{(n+1)} \rightarrow (CC_n \rightarrow CC_{n+1})$

is provable" is false (even with the new expression for Inst $\{q_a S_b S_d L q_c\}$): we are unable for example to deduce $F^{(n+1)} \rightarrow \left(-F(u, u'') \right)$ and therefore can never use the term

$$F(y', z) \vee \left[\left(R_{S_0}(x, z) \rightarrow R_{S_0}(x', z) \right) \& \ldots \& \left(R_{S_M}(x, z) \rightarrow R_{S_M}(x', z) \right) \right]$$

* *Proc. London Math. Soc.* (2), 42 (1936–7), 230–265.

† The author is indebted to P. Bernays for pointing out these errors.

Complicated behavior does not require complicated states of mind. By taking copious notes, the Turing machine can function with as few as two internal states. Behavioral complexity is equivalent whether embodied in complex states of mind (m-configurations) or complex symbols (or strings of simple symbols) encoded on the tape.

"It took Turing only eleven pages of 'On Computable Numbers' to arrive at what became known as Turing's Universal Machine. "It is possible to invent a single machine which can be used to compute any computable sequence," he announced. The Universal Machine, when provided with a suitably encoded description of some other machine, executes this description to produce equivalent results. All Turing machines, and therefore all computable functions, can be encoded by strings of finite length. Since the number of possible machines is countable but the number of possible functions is not, noncomputable functions (and what Turing referred to as "uncomputable numbers") must exist.

"Turing was able to construct, by a method similar to Gödel's, functions that could be given a finite description but could not be computed by finite means. One of these was the halting function: given the number of a Turing machine and the number of an input tape, it returns either the value 0 or the value 1 depending on whether the computation will ever come to a halt. Turing called the configurations that halt 'circular' and the configurations that keep going indefinitely 'circle free,' and demonstrated that the unsolvability of the halting problem implies the unsolvability of a broad class of similar problems, including the *Entscheidungsproblem*. Contrary to Hilbert's expectations, no mechanical procedure can be counted on to determine the provability of any given mathematical statement in a finite number of steps. This put a halt to the Hilbert program, while Hitler's purge of German universities put a halt to Göttingen's position as the mathematical center of the world, leaving a vacuum for Turing's Cambridge, and von Neumann's Princeton, to fill.

"After a full year of work, Turing gave Newman a draft of his paper in April of 1936. "Max's first sight of Alan's masterpiece must have been a breathtaking experience, and from this day forth Alan became one of Max's principle protégés," says William Newman, Max's son. Max Newman lobbied for the publication of 'On Computable Numbers, with an Application to the Entscheidungsproblem,' in the *Proceedings of the London Mathematical Society*, and arranged for Turing to go to Princeton to work with Alonzo Church. "This makes it all the more important that he should come into contact as soon as possible with the leading workers on this line, so that he should not develop into a confirmed solitary," Newman wrote to Church.

"Turing arrived in Princeton carrying his sextant, and stretching his resources to survive on his King's College fellowship (of £300) for the year. The page proofs of 'On Computable Numbers' arrived by mail from London on October 3. "It should not be long now before the paper comes out," he wrote to his mother on October 6. The publication of 'On Computable Numbers' (on November 30, 1936) went largely unnoticed. "I was disappointed by its reception here,' Turing wrote to his mother in February 1937, adding that "I don't much care about the idea of spending a long summer in this country." Only two requests for reprints came in. Engineers avoided Turing's paper because it appeared entirely theoretical, and theoreticians avoided it because of the references to paper tape and machines …

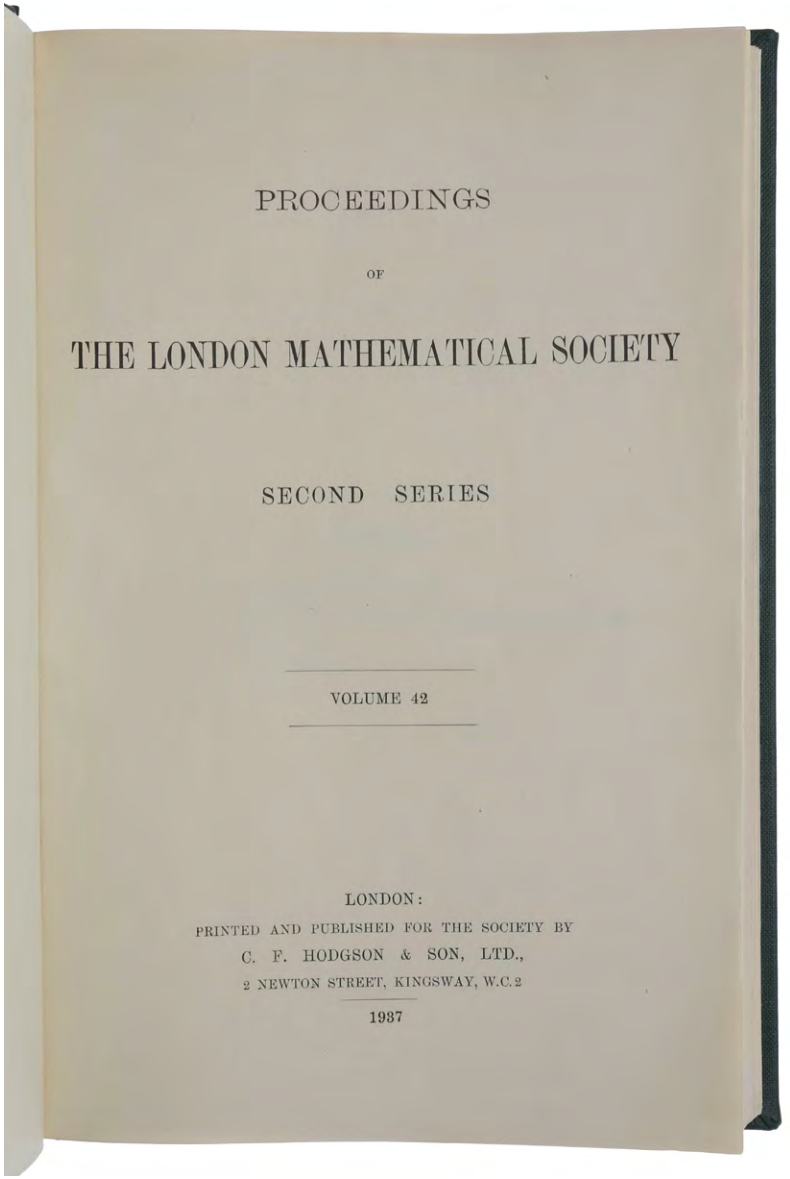
"In March of I937, Alonzo Church reviewed 'On Computable Numbers' in the *Journal of Symbolic Logic*, and coined the term *Turing machine*. "Computability by a Turing machine," wrote Church, "has the advantage of making the identification with effectiveness in the ordinary (not explicitly defined) sense evident immediately." Church's thesis – equating computability with effective calculability – would be the Church-Turing thesis from then on.

"Even Gödel, who dismissed most attempts to strengthen his own results, recognized the Church-Turing thesis as a major advance. "With this concept one has for the first time succeeded in giving an absolute definition … not depending on the formalism chosen," he admitted in 1946. Before Church and Turing, the definition of mechanical procedure was limited by the language in which the concept was defined. "For the concept of computability however … the situation is different," Gödel observed. "By a kind of miracle it is not necessary to distinguish orders, and the diagonal procedure does not lead outside the defined notion."

"It is difficult today to realize how bold an innovation it was to introduce talk about paper tapes and patterns punched in them, into discussions of the foundations of mathematics," Max Newman recalled in 1955. For Turing, the next challenge was to introduce mathematical logic into the foundations of machines. "Turing's strong interest in all kinds of practical experiment made him even then interested in the possibility of actually constructing a machine on these lines."

"The title 'On Computable Numbers' (rather than 'On Computable Functions') signaled a fundamental shift. Before Turing, things were done to numbers. After Turing, numbers began doing things. By showing that a machine could be encoded as a number, and a number decoded as a machine, 'On Computable Numbers' led to numbers (now called 'software') that were 'computable' in a way that was entirely new" (Dyson, Turing's Cathedral, pp. 246-250).

Origins of Cyberspace 394; Tomash and Williams T61, T62.


PROCEEDINGS

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1937

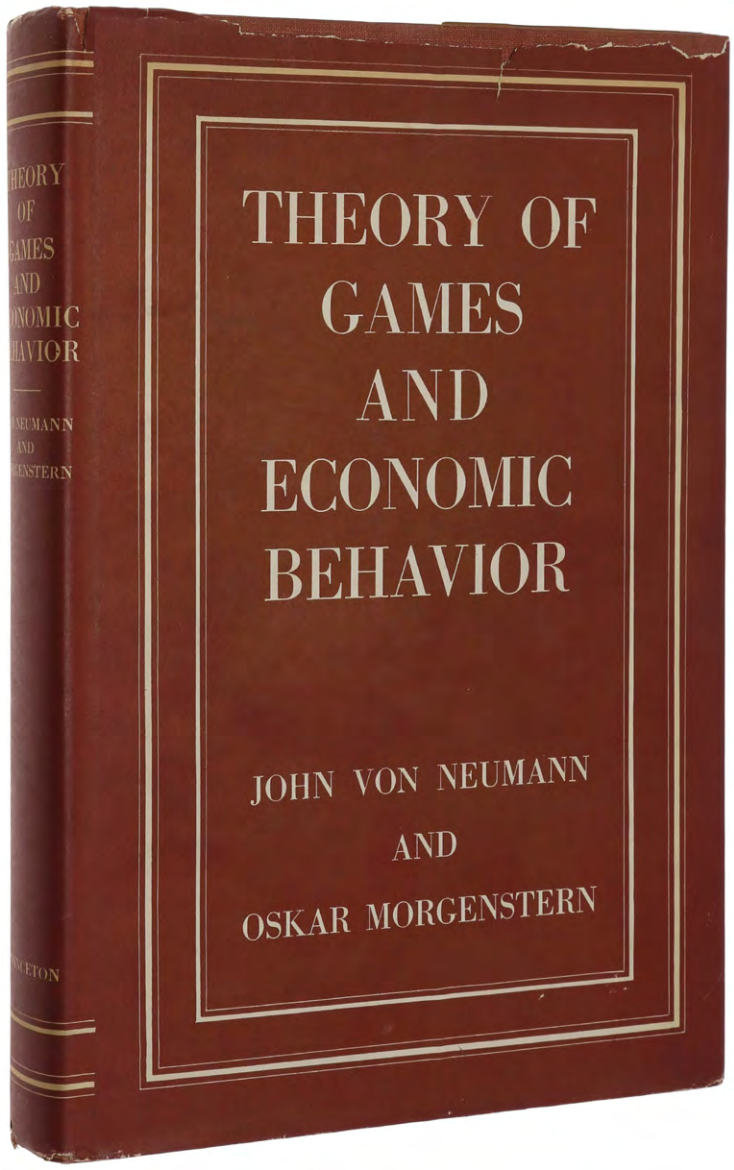
CREATION OF GAME THEORY

VON NEUMANN, John & Oskar MORGENSTERN. *Theory of Games and Economic Behavior.* Princeton: Princeton University Press, 1944.

$7,500

8vo (234 x 155 mm), pp. XVIII, 625, [1]. Errata sheet loosely inserted, as issued. Original cloth with dust jacket, binding tight and clean, jacket with some chips and tears to the extremities, not sunned or faded. Internally completely clean and fresh throughout.

First edition, and a fine copy with the very rare dust jacket, of "the classic work upon which modern-day game theory is based. What began more than sixty years ago as a modest proposal that a mathematician and an economist write a short paper together blossomed, in 1944, when Princeton University Press published *Theory of Games and Economic Behavior.* In it, John von Neumann and Oskar Morgenstern conceived a groundbreaking mathematical theory of economic and social organization, based on a theory of games of strategy. Not only would this revolutionize economics, but the entirely new field of scientific inquiry it yielded - game theory - has since been widely used to analyze a host of real-world phenomena from arms races to optimal policy choices of presidential candidates, from vaccination policy to major league baseball salary negotiations. And it is today established throughout both the social sciences and a wide range of other sciences" (from the introduction to the 60th anniversary commemorative edition). Only two copies of this book with the dust jacket are listed on ABPC/RBH (PBA, 22 February, 2007, $3163 ("sizable piece missing from spine"); and the Norman copy, Christie's, 29 October 1998, $1495).



"Quantitative mathematical models for games as poker or bridge at one time appeared impossible, since games like these involve free choices by the players at each move, and each move reacts to moves of other players. However, in the 1920s John von Neumann single-handedly invented game theory, introducing the general mathematical concept of 'strategy' in a paper on games of chance ['Zur Theorie der Gesellschaftsspiele,' *Mathematische Annalen* 100, 1928]. This contained the proof of his 'minimax' theorem that says 'a strategy exists that guarantees, for each player, a maximum payoff assuming that the adversary acts so as to minimize that payoff.' The 'minimax' principle, a key component of the game-playing computer programs developed in the 1950s and 1960s by Samuel, Newell, Simon, and others, was more fully articulated and explored in 'The Theory of Games and Economic Behavior', co-authored by von Neumann and the Austrian economist Oskar Morgenstern. Game theory, which draws upon mathematical logic, set theory and functional analysis, attempts to describe in mathematical terms the decision-making strategies used in games and other competitive situations ...

Von Neumann revolutionized mathematical economics. Had he not suffered an early death from cancer in 1957, he most probably would have received the Noble Prize in economics. Several mathematical economists influenced by von Neumann's ideas [as Nash, Harsanyi and Selten] later received the Nobel Prize in Economics" (Hook & Norman, *Origins of Cyberspace,* p. 473).

"Of the many areas of mathematics shaped by his genius, none shows more clearly the influence of John von Neumann than the Theory of Games. This modern approach to problems of competition and cooperation was given a broad foundation in his superlative paper of 1928. In scope and youthful vigor this work can be compared only to his papers of the same period on the axioms of set theory and the mathematical foundations of quantum mechanics. A decade later, when the Austrian economist Oskar Morgenstern came to Princeton, von Neumann's interest in the theory was reawakened. The result of their active and intensive collaboration during the early years of World War II was the treatise *Theory of games and economic behavior*, in which the basic structure of the 1928 paper is elaborated and extended. Together, the paper and treatise contain a remarkably complete outline of the subject as we know it today, and every writer in the field draws in some measure upon concepts which were there united into a coherent theory.

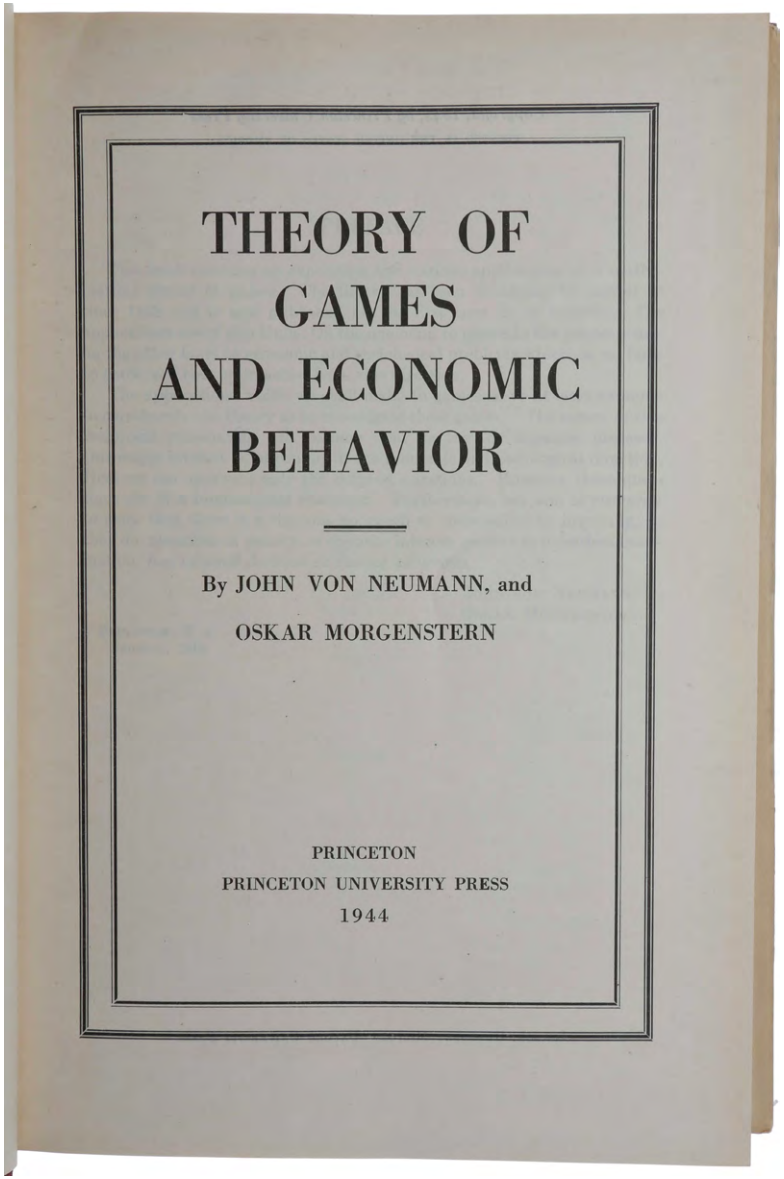
"The crucial innovation of von Neumann, which was to be both the keystone of his Theory of Games and the central theme of his later research in the area, was the assertion and proof of the Minimax Theorem. Ideas of pure and randomized strategies had been introduced earlier, especially by Êmile Borel. However, these efforts were restricted either to individual examples or, at best, to zero-sum two-person games with skew-symmetric payoff matrices. To paraphrase his own opinion, von Neumann did not view the mere desire to mathematize strategic concepts and the straight formal definition of a pure strategy as the main agenda of an 'initiator' in the field, but felt that there was nothing worth publishing until the Minimax Theorem was proved …

"For any finite zero-sum two-person game in a normalized form, [the Minimax Theorem] asserts the existence of a unique numerical value, representing a gain for one player and a loss for the other, such that each can achieve at least this favorable an expectation from his own point of view by using a randomized (or mixed) strategy of his own choosing. Such strategies for the two players are termed optimal strategies and the unique numerical value, the minimax value of the game. This is the starting point of the von Neumann-Morgenstern solution for cooperative games, where all possible partitions of the players into two coalitions are considered and the reasonable aspirations of the opposing coalitions in each

partition measured by the minimax value of the strictly competitive two-party struggle between them. In the area of extensive games, the solution of games with perfect information by means of pure strategies assumes importance only by contrast to the necessity of randomizing in the general case. The Minimax Theorem reappears in a new guise, when von Neumann turned to analyze a linear model of production. Finally, in the hands of von Neumann, it was the source of a broad spectrum of technical results, ranging from his extensions of the Brouwer fixed-point theorem, developed for its proof, to new and unexpected methods for combinatorial problems …

"The impact of von Neumann's Theory of Games extends far beyond the boundaries of this subject. By his example and through his accomplishments, he opened a broad new channel of two-way communication between mathematics and the social sciences. These sciences were fortunate indeed that one of the most creative mathematicians of the twentieth century concerned himself with some of their fundamental problems and constructed strikingly imaginative and stimulating models with which to attack their problems quantitatively. At the same time, mathematics received a vital infusion of fresh ideas and methods that will continue to be highly productive for many years to come. Von Neumann's interest in "problems of organized complexity," so important in the social sciences, went hand in hand with his pioneering development of large-scale high-speed computers. There is a great challenge for other mathematicians to follow his lead in grappling with complex systems in many areas of the sciences where mathematics has not yet penetrated deeply" (Kuhn & Tucker, 'John von Neumann's work in the theory of games and mathematical economics,' *Bulletin of the American Mathematical Society* 64 (1958), pp. 100-122).

OOC 953 [lacking jacket]; Norman 2167. For a detailed account of the genesis of this work, see Robert Leonard, *Von Neumann, Morgenstern, and the Creation of Game Theory: From Chess to Social Science, 1900-1960*, 2012.


THEORY OF
GAMES
AND ECONOMIC
BEHAVIOR

By JOHN VON NEUMANN, and
OSKAR MORGENSTERN

PRINCETON
PRINCETON UNIVERSITY PRESS
1944

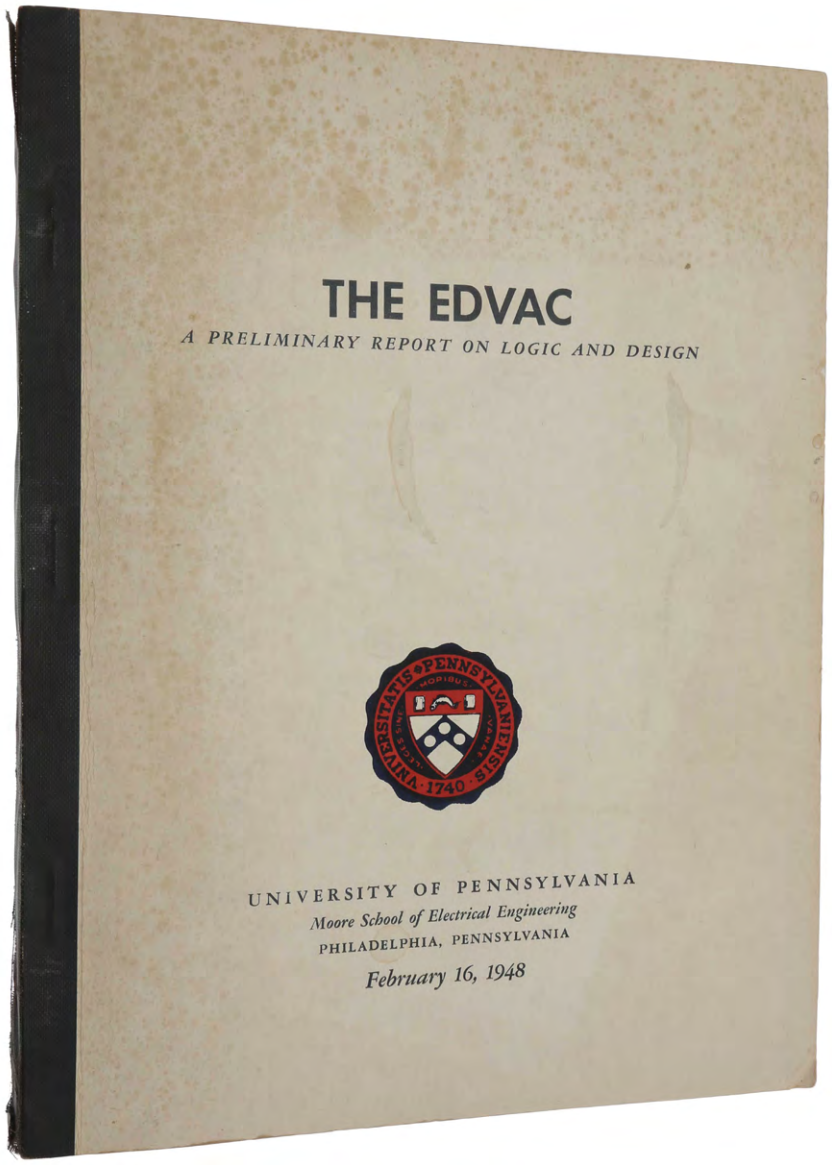
WORLD'S FIRST STORED-PROGRAM ELECTRONIC COMPUTER

[VON NEUMANN, John] PATTERSON, G. W., R. L. SNYDER, L. P. TABOR & I. TRAVIS. *The EDVAC: A Preliminary Report on Logic and Design. Report No. 48-2.* Philadelphia: University of Pennsylvania, Moore School of Electrical Engineering, February 16, 1948.

$7,500

4to (277 x 216 mm), ff. [1], iii, 100 (i.e. 102), with two full-page diagrams inserted in text and three large folding plans in pocket at rear. Original cloth-backed wrappers (cloth starting to peel away at ends of spine).

First edition of this extremely rare report on the EDVAC, the world's first stored-program electronic computer, which includes sections on the design logic as well as many pages of calculations and operating mathematical formulas. This report, which includes the Organization of the EDVAC, the Input-Output System, Performance Details, Speed of Operation, Controls, Example of Operation, Aids to Maintenance and Indicated Improvements, appears to be the only contemporary report that includes full size blue-line print drawings of components and the architecture of the EDVAC. "Its major influence was that it was the first stored-program electronic digital computer to be described at this level of detail and, as such, set the paradigm for many of the first-generation machines. The design, and in particular the concept of using mercury delay lines for the memory, influenced several of the early machines, the Cambridge EDSAC (for which Maurice Wilkes deliberately chose a similar name to show the connection) and the SEAC being the most famous" (Williams, p. 26). It is unknown how many copies of this report



were printed, but similar EDVAC reports from this time period had printings of 15 to 50 sets, most for distribution to other scientists or institutions. The copy at the University of Pennsylvania corresponds to this copy in collation. OCLC locates 5 copies (Brown, Florida, Minnesota, Pennsylvania, Wayne State). Only one copy has appeared at auction (Pacific Book Auction Galleries, 20 November 2008, lot 45, $3300).

Building on ideas formulated by innovators in computers, mathematics and physics such as V.J. Atanasoff and Alan Turing, the Moore School of Electrical Engineering of the University of Pennsylvania, led by J. Presper Eckert and John V. Mauchly, became one of the developers of what would become the modern computer. Eckert and Mauchly were tasked by the US Army's Ballistics Research Lab to create the ENIAC (Electronic Numerator, Integrator, Analyzer, and Computer) for use in solving ballistics, trajectories and ranges for new weaponry. The ENIAC machine weighed about 30 tons, covered about 1000 square feet of floor space, and used almost 18000 vacuum tubes. Even as the ENIAC was nearing operation, Eckert and Mauchly recognized its limitations. As early as January, 1944 Eckert drafted a memorandum discussing the stored-program concept of computing, which would eventually be realized in a machine called the EDVAC (Electronic Discrete Variable Automatic Computer). The EDVAC, a General-Purpose Internal Stored Program Computers as it would come to be known, is considered one of the origin points of today's modern PC computer technology. As the end of World War II approached, Eckert and Mauchly began to see value in the computer beyond military use and began to consider the need to patent their ideas. At this time, the team at the Moore School was joined by John von Neumann, from the Institute for Advanced Study at Princeton. It is von Neumann who most researchers (and patent lawyers) credit with solving the design of the EDVAC as outlined in his First Draft of a Report on the EDVAC, 1945. The EDVAC continued development with 3 or 4 official reports issued

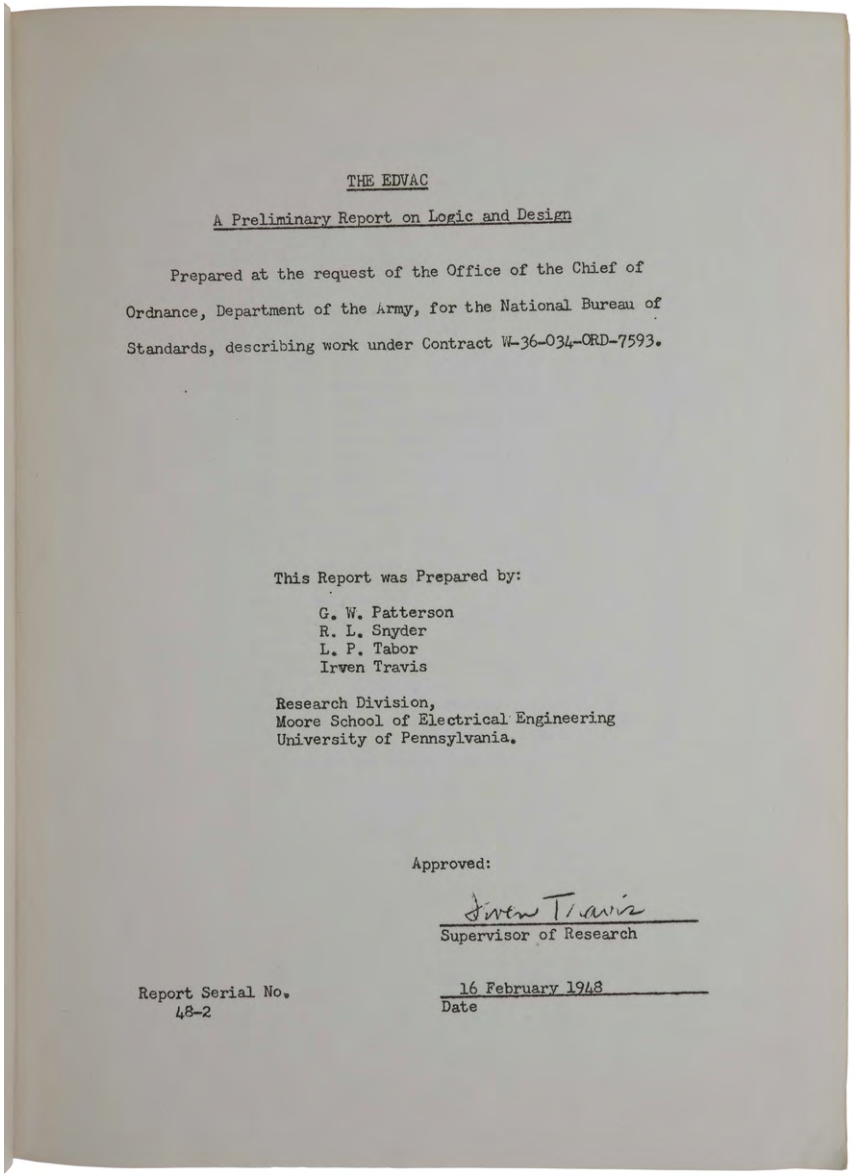


between 1945 and 1949.

"At the time von Neumann began collaborating with the ENIAC group, planning for ENIAC's delay-line successor was already under way. The Electronic Discrete Variable Automatic Computer, or EDVAC, "would be quite flexible in its control facilities, would have about 50 times as large a memory, i.e., be able to store about 1000 ten decimal digit numbers, and contain only about 1/10 as many tubes," Goldstine and von Neumann reported. The machine would be programmed by loading coded sequences into high-speed memory rather than by setting cables and switches by hand …

"In the closing months of World War II, von Neumann circulated between Princeton, Los Alamos, Washington, Philadelphia, and Aberdeen, conveying a stream of new ideas. "None of us was important enough to have persuaded people to accept this kind of thing," says Goldstine. "In the first place, von Neumann had a real built-in need at Los Alamos … They had an enormous IBM punched card installation out there doing implosion calculations. I just don't believe any of us could have gone and persuaded somebody like Fermi of the importance of numerical calculation the way von Neumann could.

"In early 1945, during the final push to finish and test the atomic bomb, von Neumann's notes on the EDVAC project were typed up under Goldstine's supervision and distilled into a 105-page report. The "First Draft of a Report on the EDVAC," reproduced by mimeograph and released into limited distribution by the Moore School on June 30, 1945, outlined the design of a high-speed stored-program electronic digital computer, including the requisite formulation and interpretation of coded instructions — "which must be given to the device in absolutely exhaustive detail."



THE EDVAC

A Preliminary Report on Logic and Design

Prepared at the request of the Office of the Chief of Ordnance, Department of the Army, for the National Bureau of Standards, describing work under Contract W-36-034-ORD-7593.

This Report was Prepared by:

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Supervisor of Research

Report Serial No.
48-2

16 February 1948
Date

"The functional elements of the computer were separated into a hierarchical memory, a control organ, a central arithmetic unit, and input/output channels, making distinctions still known as the "von Neumann architecture" today. A fast internal memory, coupled to a larger secondary memory, and linked in turn to an unlimited supply of punched cards or paper tape, gave the unbounded storage that Turing had prescribed. The impediment of a single channel between memory and processor is memorialized as the "von Neumann bottle-neck," although its namesake attempted, unsuccessfully, to nip this in the bud. "The whole system will be well balanced, so, that if it is properly and intelligently used, there will be no bottlenecks," he explained to Max Newman, "even not at the outputs and inputs of the human intellects with which it has to be matched."

"When a subject captured von Neumann's attention, he reconstituted it on his own terms from the bottom up. Digital computing required no such process of reduction; it was all axioms from the start. In 1945 the ENIAC and EDVAC were still classified military projects. Von Neumann could speak freely in logical abstractions, but not in specific electronic circuits. So he did. He was also, as Julian Bigelow put it, "clever enough to know that his forte was not in experimental work or in making things function in the real world."

"During the war, both open publication and individual credit had been suspended — for both computers and bombs. After the war, it was decided that bombs would be kept secret and computers would be made public, with a scramble for credit as a result. The EDVAC report engendered widespread controversy, despite the small number of copies that were distributed before the mimeograph stencils gave out. Von Neumann was listed as the sole author, without any acknowledgment of the contributions made by other members of the EDVAC group. Eckert and Mauchly, who had been pledged to silence about the ENIAC and EDVAC, felt slighted by a publication that was based on their own unpublished work. "Johnny was rephrasing our logic, but it was still the SAME logic," says Mauchly. Adding injury to insult, the EDVAC report would be deemed to constitute a legal publication invalidating any patents not filed within a year …

"With the war over, individual interests eclipsed the interests of the United States. The Moore School was too academic for Eckert and Mauchly, and not academic enough for von Neumann. Eckert and Mauchly left to form the Electronic Control Company and build commercial computers first BINAC and then UNIVAC, a brand synonymous with computing for a time. Von Neumann decided to go build his own computer, as a scientific instrument, somewhere else. Spare time on the ENIAC and even the EDVAC would not be enough. "It was, therefore, the most natural thing that von Neumann felt that he would like to have at his own disposal such a machine," says Willis Ware. "If he really wanted a computer, the thing to do was to build it," adds Arthur Burks.

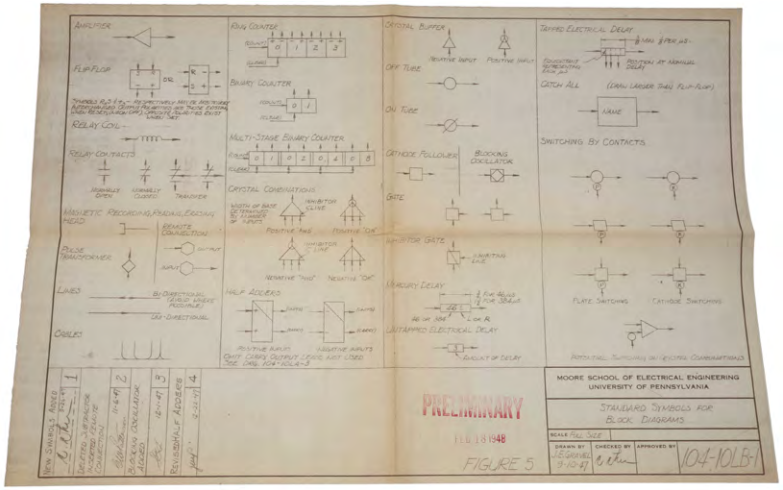
"Von Neumann's initial thinking was to transplant the entire core of the ENIAC group … Eckert declined von Neumann's invitation to lead the IAS engineering team, going into business with Mauchly for himself, while von Neumann entered into a series of lucrative personal consulting contracts with IBM" (Dyson, *Turing's Cathedral*, pp. 77-81).

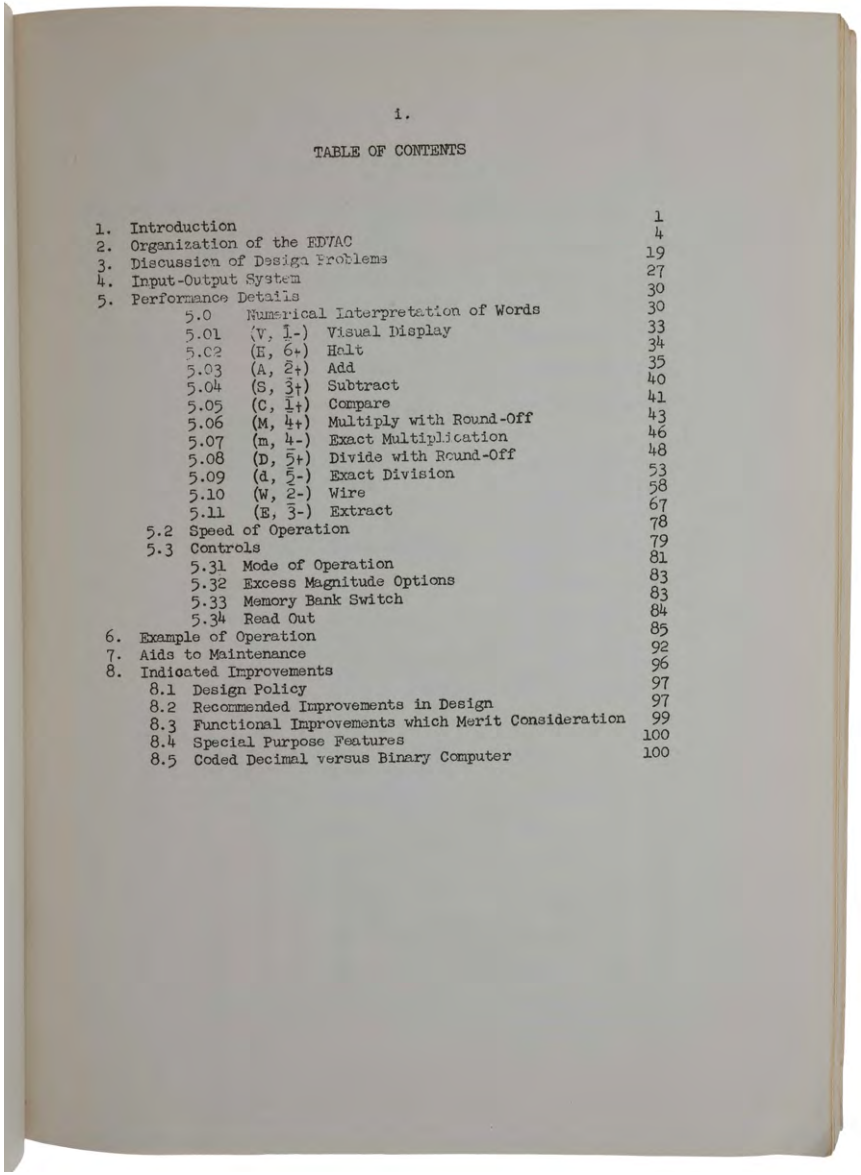
"When Eckert, Mauchly, and others left the Moore School early in 1946, the job of heading up the EDVAC project fell to T.K. (Kite) Sharpless who, after graduating with an MS in electrical engineering from the Moore School in 1943, stayed on to become a teacher and member of the ENIAC and EDVAC projects. Sharpless himself left in 1947 … The next manager to be appointed was Louis Tabor, whose tenure lasted for only a few months. Finally, the task of project manager and chief engineer was given to Richard L. Snyder, who saw the project through to the point where a machine was actually shipped from the Moore School to the Army's

Ballistic Research Laboratories at the Aberdeen Proving Ground in Maryland. Snyder left the Moore School at this time and followed the machine to the Ballistic Research Laboratories" (Williams, p. 26).

The detailed design was finalized in May, 1947, and construction then began. Since the Moore School's ability to fabricate delicate mechanical components was inadequate for the task, the National Bureau of Standards agreed to assume responsibility for the design of the magnetic wire input and output system; they in turn contracted it out to the Reeves Instrument Corporation. In late 1949 it was moved to the Army Ordnance Department, Aberdeen Proving Ground, for final assembly and testing. However, it did not run its first application programme until October 28, 1951.

Not in OOC. Williams, 'The Origins, Uses, and Fate of the EDVAC,' *IEEE Annals of the History of Computing* 15 (1993), pp. 22-38 (citeseerx.ist.psu.edu/viewdoc/download?doi=10.1.1.705.4726&rep=rep1&type=pdf)





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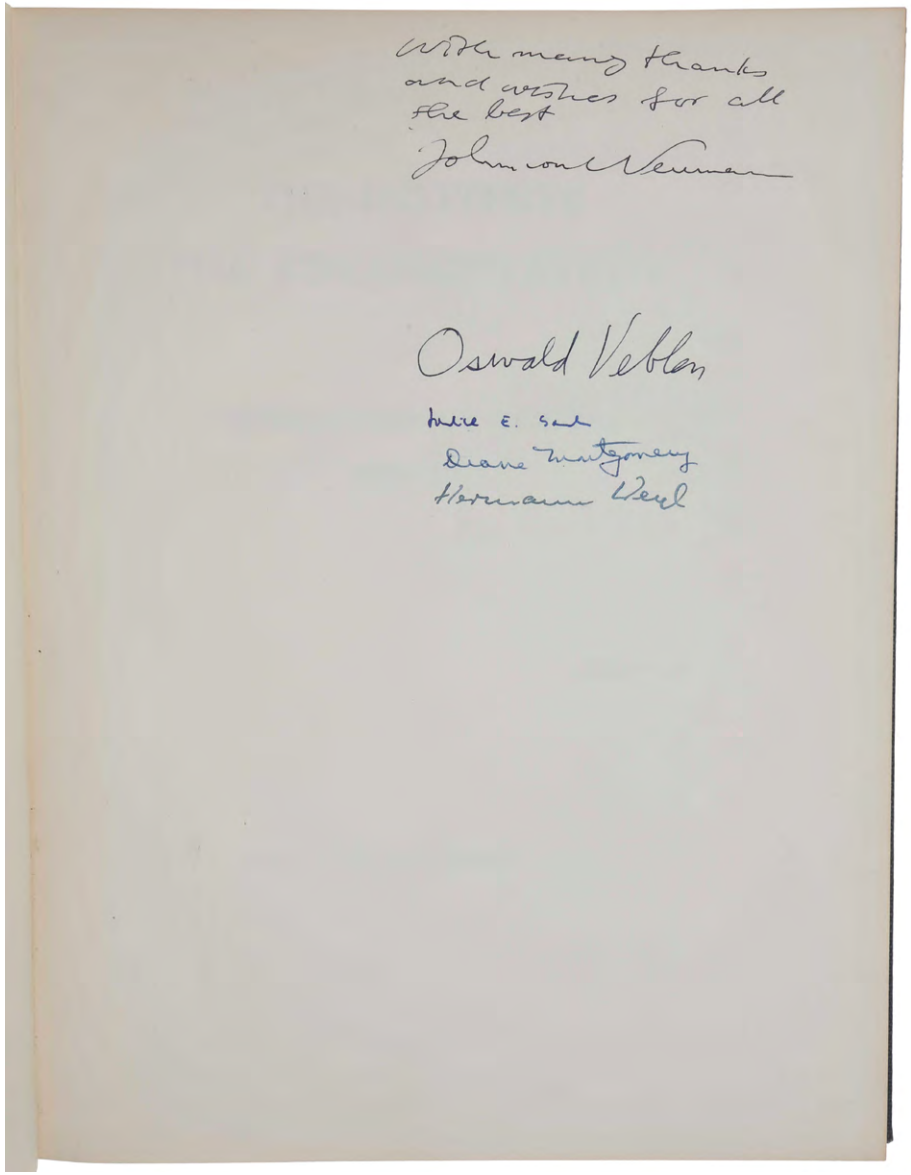
INSCRIBED BY VON NEUMANN

[VON NEUMANN, John]. *The Institute for Advanced Study: Publications of Members 1930-1954.* Princeton, NJ: Princeton University Press, 1955.

$10,000

4to (278 x 218 mm), pp. 270. Original publisher's cloth, spine lettered in gilt. Fine.

First edition of this bibliography of members of the Institute for Advanced Study in Princeton, published on the 25th anniversary of the Institute's foundation. This is a remarkable copy, inscribed and signed by the great mathematician and computer pioneer John von Neumann, as well as by Oswald Veblen, the first person (together with Albert Einstein) appointed to the Faculty of Mathematics at the Institute, and by the great Göttingen mathematician Hermann Weyl, who was offered but declined the faculty position at the Institute subsequently given to Von Neumann, which he held from 1933 until his death in 1957 (Weyl joined the Institute faculty six months later). Robbert Dijkgraaf, the current director of the Institute, has stated that Von Neumann was "perhaps an even greater genius than Einstein" (quoted in Abraham Flexner's *The Usefulness of Useless Knowledge*, Princeton University Press, 2017). "In a memoir written for the American Philosophical Society, Professor Eugene Wigner, von Neumann's close friend since their high school days in Budapest [and a Nobel Laureate], declared: "His accomplishments were manifold, his was a great mind – perhaps one of the greatest of the first half of this century"" (Leitch, *A Princeton Companion*, 1978). "Accepting an invitation from Oswald Veblen to lecture on quantum theory at Princeton University, John von Neumann was one of a group of Hungarian and Jewish intellectuals to escape to the United States from the turmoil of Europe. During the war, von Neumann's intellect tackled hydrodynamics, ballistics, meteorology, game theory, and statistics, applying mathematical rigor to practical



problems in these fields. He worked on the Manhattan Project and by the latter years of World War II was a consultant to several government committees, moving between groups of scientists in government, university, and industry research laboratories. His broad perspective allowed him to envision applications for computers beyond that of speedy calculating devices, and he initiated the Electronic Computer Project at the Institute" (ias.edu/scholars/von-neumann). Books inscribed by Von Neumann are of the utmost rarity on the market. This is the first one we have handled, and Jeremy Norman has informed us that he has seen only one other in thirty years or more.

Provenance: John von Neumann (inscription 'With many thanks and wishes for all the best' in his hand and signature 'John von Neumann' on front free endpaper); mathematicians Oswald Veblen, Hermann Weyl, Deane Montgomery and Institute librarian Judith E. Sachs (signatures on front free endpaper).

"On May 20, 1930, a certificate of incorporation for the "Institute for Advanced Study—Louis Bamberger and Mrs. Felix Fuld Foundation" was filed with the State of New Jersey. Brother-and-sister philanthropists Louis and Caroline Bamberger provided the founding $5 million gift to establish an institution dedicated to the vision of education reformer Abraham Flexner, the Institute's founding Director: "The Institute should be small and plastic (that is flexible); it should be a haven where scholars and scientists could regard the world and its phenomena as their laboratory, without being carried off in the maelstrom of the immediate; it should be simple, comfortable, quiet without being monastic or remote; it should be afraid of no issue; yet it should be under no pressure from any side which might tend to force its scholars to be prejudiced either for or against any particular solution of the problems under study; and it should provide the facilities, the tranquility, and the time requisite to fundamental inquiry into the unknown. Its scholars should enjoy complete intellectual liberty and be absolutely free from administrative responsibilities or concerns."

"In the autumn of 1932, Flexner announced the creation of the Institute's first school, the School of Mathematics, and its first Faculty appointments, Oswald Veblen and Albert Einstein. On October 11, 1932, when the *New York Times* announced the appointment of Einstein to the embryonic Institute, it reported that the founders' intention was to establish a "scholar's paradise" …

"Founded against the backdrop of the rise of Fascism, the Institute became a lifeline in the migration of European scholars to the United States. After Adolf Hitler became Chancellor in January 1933, Hermann Weyl, the mathematician who had succeeded David Hilbert to the world's most prestigious chair in mathematics at the University of Göttingen, crossed the Atlantic to join the Institute. That same year, Flexner recruited John von Neumann, a young but already renowned mathematician who had fled Europe and was then a visiting professor at Princeton University" (ias.edu/about/mission-history).

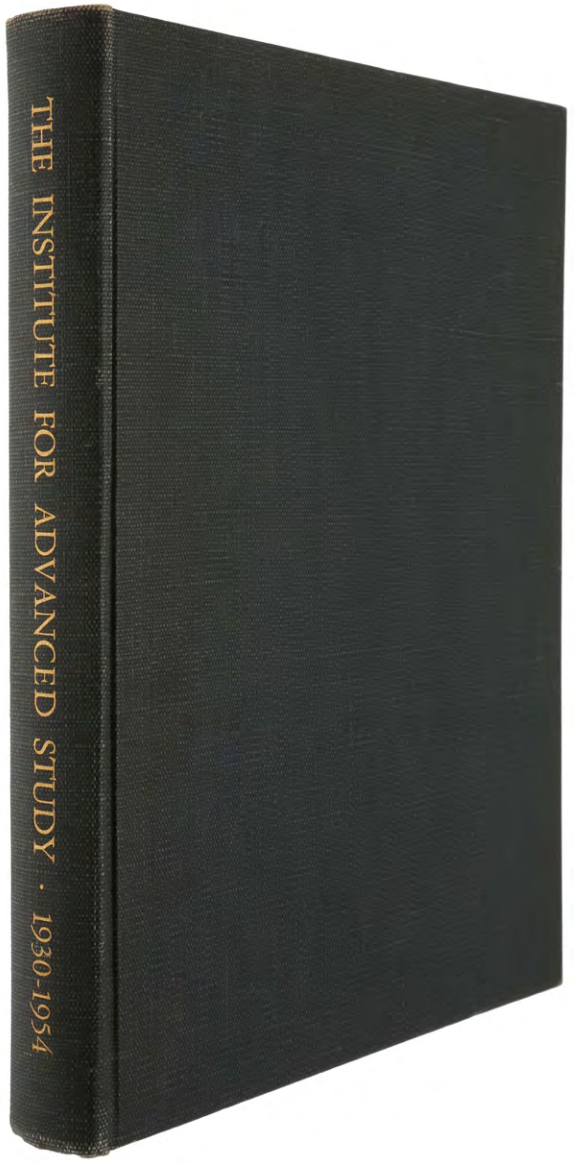
Born on December 28, 1903, the son of a well-to-do banker in Budapest, Von Neumann's exceptional mathematical gifts were recognised in high school. After receiving his PhD from the University of Budapest, he continued his research at Göttingen, Hamburg and Berlin. "In 1929 von Neumann accepted an invitation to come to Princeton as a visiting professor for one term. Given a continuing half-time appointment the following year, he spent one term each year in Princeton and one in Germany until 1933 when, at the age of 30, he accepted appointment as the youngest and one of the first professors in the newly founded Institute for Advanced Study. In 1937 he became a United States citizen.

"Von Neumann's brilliant work in mathematics also carried him into theoretical economics and technology as well as theoretical physics – areas where he was able to make vital contributions not only to science but also to the welfare of his adopted country. His work in quantum mechanics gave him a profound knowledge

concerning the application of nuclear energy to military and peacetime uses, enabling him to occupy an important place in the scientific councils of the nation. During the Second World War, he played a major role among the Los Alamos group of scientists who developed the atomic bomb. After the war he served on the advisory committee of the Atomic Energy Commission and on the commission itself from 1954 until his death.

"In collaboration with the University's Class of 1913 Professor of Political Economy, Oskar Morgenstern, he developed further the interest in game theory he had first evidenced in a treatise published in 1928. Their joint endeavors resulted in *Theory of Games and Economic Behavior* (published by Princeton University Press in 1944), which aimed to demonstrate that "the typical problems of economic behavior become strictly identical with the mathematical notions of suitable games of strategy." The theory was also considered of value for the study of government and sociology and for its application to problems of military strategy by the United States.

"Probably the best known and most dramatic of von Neumann's accomplishments was his development of one of the speediest, most accurate, and most useful computers, which made the essential calculations that enabled the United States to build and test its first full model of the hydrogen bomb" (Leitch, *A Princeton Companion*, 1978). "Through a chance encounter, von Neumann engaged with a team of engineers in the University of Pennsylvania's Moore School of Electrical Engineering. J. Presper Eckert and John W. Mauchly were constructing an Electronic Numerical Integrator and Computer (ENIAC) under the leadership of J. G. Brainerd. Herman H. Goldstine, a mathematician and Reserve Officer of the Ordnance Department who was the army liaison to the Moore School, recognized von Neumann–already famous for his contributions to mathematics, physics, and economics–on a railway platform in 1944. Goldstine approached, introduced himself and, after a brief conversation, invited von Neumann to Philadelphia.



"Even as the ENIAC was being constructed, its successor was being designed … In the spring of 1945, von Neumann drafted a document that described the logical structure of a desired high-speed automatic digital computing system powerful enough to solve complex mathematical problems, such as non linear partial differential equations (of two or three independent variables), in abstract terms drawn from biology. The abstraction freed the concepts of the nascent computer from the constraints posed by the technology of the era and, ultimately, stimulated the growth of that technology. Von Neumann's logical schema served as the basis for subsequent stored-program computers. In identifying the organs required as those relating to arithmetic, memory, control, and input and output devices, subsequently known as von Neumann Architecture, he laid down the basic schema of the modern computer" (ias.edu/electronic-computer-project)."

"As a student at the University of Gottingen, Weyl (1885-1955) came under the influence of David Hilbert. In 1913 he became professor of mathematics at the Technische Hochschule, Zürich, where he was a colleague of Albert Einstein. The outstanding characteristic of Weyl's work was his ability to unite previously unrelated subjects. In *Die Idee der Riemannschen Fläche* (1913; *The Concept of a Riemann Surface*), he created a new branch of mathematics by uniting function theory and geometry and thereby opening up the modern synoptic view of analysis, geometry, and topology. The outgrowth of a course of lectures on relativity, Weyl's *Raum, Zeit, Materie* (1918; "Space, Time, Matter") reveals his keen interest in philosophy and embodies the bulk of his findings on relativity. He produced the first unified field theory for which Maxwell's equations of electromagnetic fields and the gravitational field appear as geometric properties of space-time … Around this time (and influenced by the work of French mathematician Elié Cartan), Weyl attempted a unified field theory to unite electromagnetism and gravitation, in which he introduced the concept of gauge invariance, which describes how some quantities do not change despite a transformation in the underlying field and which became important in later particle physics. From 1923 to 1938 Weyl evolved a general theory of continuous groups, using matrix representations. He found that most of the regularities of quantum phenomena on the atomic level can be most simply understood by using group theory. With the findings published in *Gruppentheorie und Quantenmechanik* (1928; "Group Theory and Quantum Mechanics"), Weyl helped mould modern quantum theory. Weyl was appointed professor of mathematics at the University of Göttingen in 1930. The Nazi dismissal of many of his colleagues prompted him to leave Germany in 1933 and accept a position at the Institute for Advanced Study, Princeton; he became a U.S. citizen in 1939. After his retirement in 1951, Weyl remained professor emeritus of the institute and divided his time between Princeton and Zürich" (Britannica).

"A pioneer in the mathematical field of topology, Oswald Veblen was one of the first Faculty of the Institute for Advanced Study, serving from 1933 until his death in 1960. Prior to joining the Institute, Veblen was a faculty member of Princeton University's Department of Mathematics for twenty-seven years, where he was committed to improving research conditions in mathematics in the United States. He built Princeton University's mathematics department into one of eminence in the United States and subsequently had enormous influence on the development of the Institute's School of Mathematics" (ias.edu/scholars/veblen).

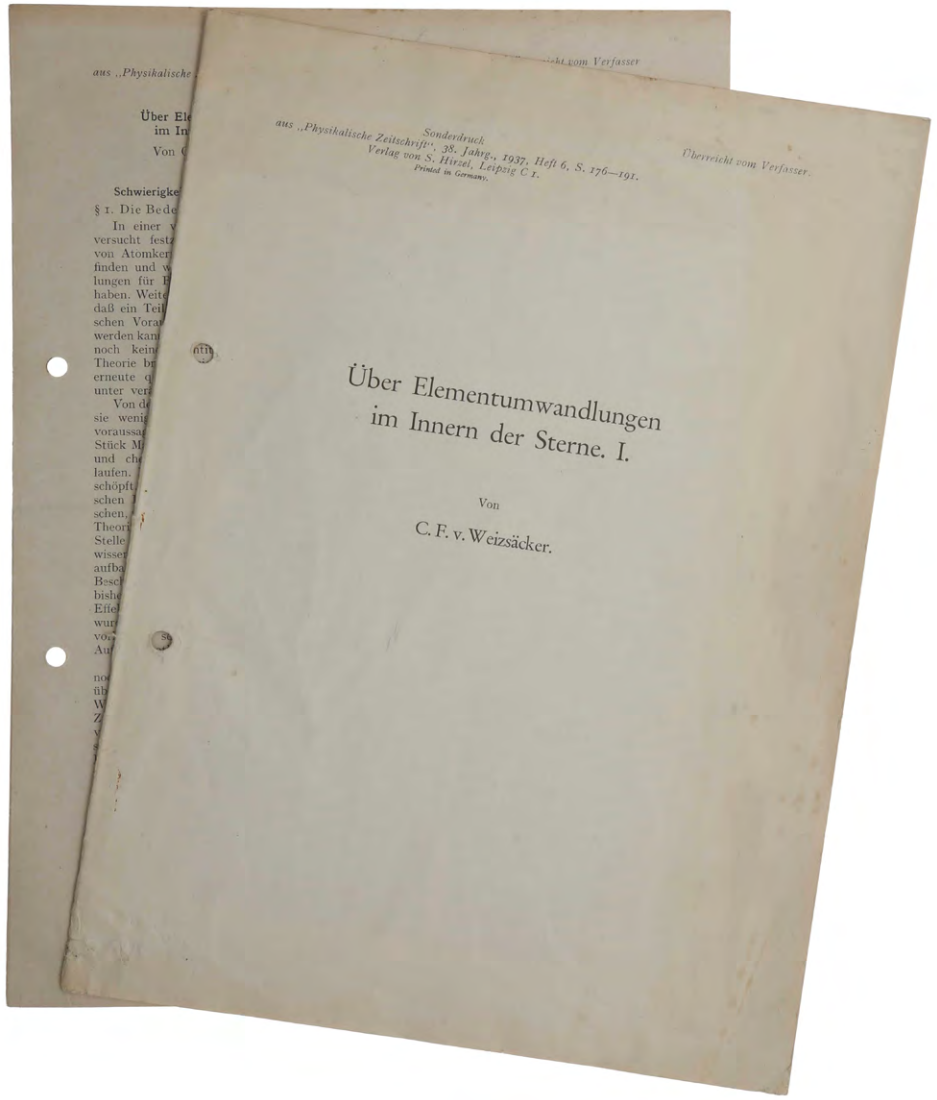
Deane Montgomery (1909-92) obtained his Ph.D. in 1933 with a thesis on point-set topology. After periods at Harvard and Princeton, he was appointed assistant professor at Smith College, rising to become full professor in 1941. During the War he worked with John von Neumann on numerical analysis. He became a permanent member at the Institute for Advanced Study in 1948, and professor in 1951, and remained there until 1988. In 1952, in collaboration with Andrew Gleason and Leo Zippin, Montgomery gave the solution to Hilbert's Fifth Problem.

CARBON-NITROGEN-OXYGEN CYCLE

WEITZSACKER, Carl Friedrich von. *Uber Elementumwandlungen im Inneren der Sterne I [- II].* Two offprints from *Physikalische Zeitschrift* 28, no. 6, 15 March 1937 [- 29, no. 17/18, 15 September 1938].

$1,800

4to (274 x 197 mm), pp. 176-191 [I], 633-646 [II]. Original printed wrappers, holes punched for ring binder (not effecting text).


Sonderdruck
aus „Physikalische Zeitschrift", 38. Jahrg., 1937, Heft 6, S. 176—191.
Verlag von S. Hirzel, Leipzig C 1.
Printed in Germany.
Überreicht vom Verfasser.
Über Elementumwandlungen
im Innern der Sterne. I.
Von
C. F. v. Weizsäcker.

First edition, the very rare offprints, of these two papers in which von Weizsäcker first proposed a detailed mechanism, called the carbon-nitrogen-oxygen (CNO) cycle, to explain how the stars shine: hydrogen atoms are fused in the centre of stars to form helium atoms using carbon as a catalyst, resulting in the release of large quantities of energy. This is still believed to be the predominant energy-producing mechanism in stars more massive than the Sun. The CNO cycle was developed independently by Hans Bethe six months later (and is now known as the Bethe-Weizsäcker cycle); Bethe was awarded the Nobel Prize in Physics 1967 “for his contributions to the theory of nuclear reactions, especially his discoveries concerning the energy production in stars” (von Weizsäcker was twice nominated for the Nobel Prize but was not awarded it). Part II of von Weizsäcker’s paper is also important in the history of cosmology, suggesting a ‘big-bang’ theory of the origin of the universe, as well as a possible explanation of the relative abundances of the elements. “Weizsacker’s paper of 1938 deserves to be recognized not only as a pioneering contribution to our understanding of why the stars shine, but also as an interesting attempt to provide the origin of the universe with a physical

explanation. Weizsacker's picture was that of a big-bang universe" (Kragh, p. 99). Not in OCLC; no copies in auction records.

"Carl Friedrich von Weizsäcker, since fall 1936 an assistant at the new *Kaiser Wilhelm-Institut fur Physik* in Berlin-Dahlem, signed on 22 January 1937, a review article entitled 'Uber Elementumwandlungen im Inneren der Sterne' (On the Transmutation of Elements in Stars). He started from what he called the '*Aufbauhypothese*' (hypothesis of 'constitution' or 'building up'), which he attributed to Arthur Stanley Eddington (1926), and stated "that, apart from negligibly small effects, the nuclear transmutations studied theoretically provide the sole origin of the composite inner regions of the stars which initially are composed of pure hydrogen and their unique source of energy" (p. 176). The empirical data obtained on the chemical constitution of stars, especially the frequency of the occurrence of heavier elements, encouraged von Weizsäcker to extend the '*Aufbauhypothese*': "The temperature in the interior of stars takes on values allowing the transmutation of the lightest nuclei on the basis of hydrogen. The energy obtained from these transmutations provides the source of the emission of radiation due to the [surface] temperature of the star. The transformations do not directly lead to the origin of heavier nuclei; however, in their course free neutrons are created as byproducts, which build-up a part of the available [lighter] nuclei into the known heavier elements" (p. 178). He added: "If this concept is correct, the star acts as an engine which, with the help of the liberated nuclear energies, sustains the necessary external conditions steadily," and: "It should be the only possible engine of this kind" (*loc. cit.*). With this fundamental hypothesis in mind, von Weizsäcker pursued a systematic survey of the various energy-producing reactions of the lightest nuclei and their relevance for the internal constitution of the stars, and then the formation of the heavy nuclei by reactions with neutrons.

"In dealing with the nuclear reactions in stars, von Weizsäcker confined himself to consider only thermally induced reactions, initially with charged light particles - namely, proton, deuteron, and triton … For the continuous production of energy the existence of 'reaction chains' was necessary, which not only produced enough energy and higher elements (such as helium from hydrogen) but also supplied the material for the initial reaction. Von Weizsäcker discussed a number of possibilities for such reactions and the resulting relative frequencies of higher nuclei in stars ...

"In spite of these initial successes, the generalized '*Aufbauhypothese*' revealed serious difficulties, as von Weizsäcker summarized in Part One of his second paper 'Uber Elementumwandlungen im Inneren der Sterne II,' submitted one-and-a-half years later. Notably, von Weizsäcker said that the coupling of the energy-producing processes with those of the building-up of higher elements seemed to be contradicted by empirical data, and the availability of larger numbers of neutrons (required for the synthesis of heavier nuclei) also could not really be justified theoretically. Hence, especially the origin of all heavy nuclei should not be included in the building-up hypothesis.

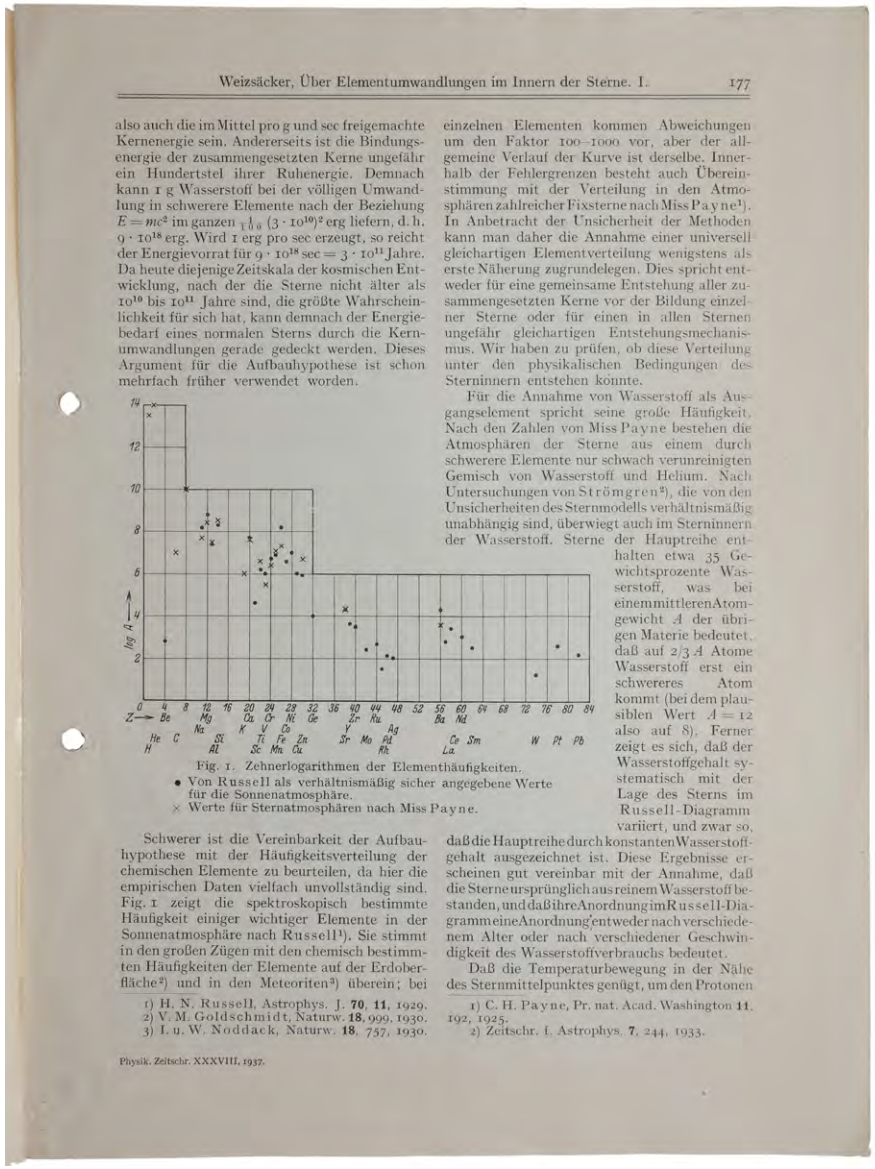
"Having stated this restriction of the fundamental hypothesis, von Weizsäcker returned in Part Two of his 1938 survey to consider the possible mechanisms of nuclear energy production in stars. Now, he also abandoned his previously preferred model cycle … Besides a process having the net result H + H = D + e^+ and those processes leading to the formation of helium via lithium, as well as several other processes, he focused on a process connected with the carbon nucleus ^{12}C, the so-called 'carbon cycle'; i.e.,

$^{12}C + H = {}^{13}N$; $^{13}N = {}^{13}C + e^+$; $^{13}C + H = {}^{14}N$;
$^{14}N + H = {}^{15}O$; $^{15}O = {}^{15}N + e^+$; $^{15}N + H = {}^{12}C + {}^{4}He$.

Von Weizsäcker finally noted that the energy source of stars would therefore first consist of a decomposition of elements below carbon, followed by the [carbon] cycle, and added: "If via side reactions the abundance of carbon finally decreases as well, an analogous cycle starting from oxygen would be available" (II, p. 639). With the carbon cycle, the known features of stellar evolution did indeed follow, while the origin of the heaviest elements certainly had to be decoupled; it required extreme conditions which von Weizsäcker thought might exist in supermassive stars" (Mehra & Rechenberg, pp. 991-3).

"In June 1938 [George] Gamow attended a conference in Warsaw on 'New Theories in Physics.' On his way back to the United States he visited Berlin and met with Weizsäcker, with whom he discussed problems of nuclear astrophysics. Weizsäcker learned from Gamow that Bethe was investigating the same nuclear processes as he was, the proton-proton and CN cycles. However, the works of the two German physicists were not only independent, they differed considerably … whereas Bethe did not deal with the origin of the elements and refrained from "speculations about this previous state of stellar matter," his colleague in Berlin had no such reservations. This makes Weizsäcker's paper … the more interesting one from a cosmological perspective.

"In 1937 Weizsäcker had endorsed the "wider synthesis hypothesis," that is, the attempt to build up heavier elements from a process starting with proton-proton reactions, but in his paper of 1938 he renounced this view. He now argued, as Bethe did independently, that it was impossible to synthesize heavier elements in stars and, therefore, that "It is quite possible that the formation of the elements took place before the origin of the stars, in a state of the universe significantly different from today's." However, contrary to Bethe, Weizsäcker found it tempting "to draw from the frequency of distribution of the elements conclusions about an earlier state of the universe in which this distribution might have originated."



Weizsäcker, Über Elementumwandlungen im Innern der Sterne. I. 177

also auch die im Mittel pro g und sec freigemachte Kernenergie sein. Andererseits ist die Bindungsenergie der zusammengesetzten Kerne ungefähr ein Hundertstel ihrer Ruhenergie. Demnach kann 1 g Wasserstoff bei der völligen Umwandlung in schwerere Elemente nach der Beziehung $E = mc^2$ im ganzen $\frac{1}{100}(3 \cdot 10^{10})^2$ erg liefern, d. h. $9 \cdot 10^{18}$ erg. Wird 1 erg pro sec erzeugt, so reicht der Energievorrat für $9 \cdot 10^{18}$ sec $= 3 \cdot 10^{11}$ Jahre. Da heute diejenige Zeitskala der kosmischen Entwicklung, nach der die Sterne nicht älter als 10^{10} bis 10^{11} Jahre sind, die größte Wahrscheinlichkeit für sich hat, kann demnach der Energiebedarf eines normalen Sterns durch die Kernumwandlungen gerade gedeckt werden. Dieses Argument für die Aufbauhypothese ist schon mehrfach früher verwendet worden.

einzelnen Elementen kommen Abweichungen um den Faktor 100–1000 vor, aber der allgemeine Verlauf der Kurve ist derselbe. Innerhalb der Fehlergrenzen besteht auch Übereinstimmung mit der Verteilung in den Atmosphären zahlreicher Fixsterne nach Miss Payne[1]. In Anbetracht der Unsicherheit der Methoden kann man daher die Annahme einer universell gleichartigen Elementverteilung wenigstens als erste Näherung zugrundelegen. Dies spricht entweder für eine gemeinsame Entstehung aller zusammengesetzten Kerne vor der Bildung einzelner Sterne oder für einen in allen Sternen ungefähr gleichartigen Entstehungsmechanismus. Wir haben zu prüfen, ob diese Verteilung unter den physikalischen Bedingungen des Sterninnern entstehen könnte.

Für die Annahme von Wasserstoff als Ausgangselement spricht seine große Häufigkeit. Nach den Zahlen von Miss Payne bestehen die Atmosphären der Sterne aus einem durch schwerere Elemente nur schwach verunreinigten Gemisch von Wasserstoff und Helium. Nach Untersuchungen von Strömgren[2], die von den Unsicherheiten des Sternmodells verhältnismäßig unabhängig sind, überwiegt auch im Sterninnern der Wasserstoff. Sterne der Hauptreihe enthalten etwa 35 Gewichtsprozente Wasserstoff, was bei einem mittleren Atomgewicht A der übrigen Materie bedeutet, daß auf $2/3\,A$ Atome Wasserstoff erst ein schwereres Atom kommt (bei dem plausiblen Wert $A = 12$ also auf 8). Ferner zeigt es sich, daß der Wasserstoffgehalt systematisch mit der Lage des Sterns im Russell-Diagramm variiert, und zwar so, daß die Hauptreihe durch konstanten Wasserstoffgehalt ausgezeichnet ist. Diese Ergebnisse erscheinen gut vereinbar mit der Annahme, daß die Sterne ursprünglich aus reinem Wasserstoff bestanden, und daß ihre Anordnung im Russell-Diagramm eine Anordnung entweder nach verschiedenem Alter oder nach verschiedener Geschwindigkeit des Wasserstoffverbrauchs bedeutet.

Daß die Temperaturbewegung in der Nähe des Sternmittelpunktes genügt, um den Protonen

Fig. 1. Zehnerlogarithmen der Elementhäufigkeiten.
• Von Russell als verhältnismäßig sicher angegebene Werte für die Sonnenatmosphäre.
× Werte für Sternatmosphären nach Miss Payne.

Schwerer ist die Vereinbarkeit der Aufbauhypothese mit der Häufigkeitsverteilung der chemischen Elemente zu beurteilen, da hier die empirischen Daten vielfach unvollständig sind. Fig. 1 zeigt die spektroskopisch bestimmte Häufigkeit einiger wichtiger Elemente in der Sonnenatmosphäre nach Russell[1]). Sie stimmt in den großen Zügen mit den chemisch bestimmten Häufigkeiten der Elemente auf der Erdoberfläche[2]) und in den Meteoriten[3]) überein; bei

1) H. N. Russell, Astrophys. J. **70**, **11**, 1929.
2) V. M. Goldschmidt, Naturw. **18**, 999, 1930.
3) I. u. W. Noddack, Naturw. **18**, 757, 1930.

1) C. H. Payne, Pr. nat. Acad. Washington **11**, 192, 1925.
2) Zeitschr. f. Astrophys. **7**, 244, 1933.

Physik. Zeitschr. XXXVIII, 1937.

He realized this to be a problematic program, not only because of the technical difficulties but also because it related to a state of the universe of which no direct empirical knowledge was available and to which the present laws of nature could not be uncritically extrapolated. He took the sensible position "to presuppose, first of all hypothetically, the permanence in time of our natural laws and … be prepared for the appearance of error when this theory is compared with historical documents still available today." The program outlined by Weizsäcker aimed at inferring the state of the unknown, early universe from present conditions. He relied on crude calculations based on the assumption that the original distribution of elements was in thermodynamic equilibrium and tried to estimate from the present distribution the original temperature and density. Relying in part on previous methods of this kind, first used by two German physical chemists, Ladislaus Farkas and Paul Harteck, Weizsäcker was led to the tentative conclusion of the early universe having "a temperature of an order of magnitude of that which would come about through the complete transformation of the nuclear binding energy into heat [about 2 x 10^{11} K]. The accompanying density is likewise already in the neighbourhood of the density of the nucleus."

"Weizsäcker argued qualitatively that the gross distribution of the elements was obtained under these extreme conditions, and that subsequently the temperature and density would decrease, with new nuclear reactions taking place that would account for the fine structure. At the end of his paper, he addressed more directly, and more speculatively, cosmological questions. He imagined "a great primeval aggregation of matter perhaps consisting of pure hydrogen" collapsing under the influence of gravity to form the extreme conditions under which element formation would take place. Admitting that the size of this primeval aggregation was purely a matter of speculation, he invited his readers to imagine "the entire universe as known to us combined in it." He saw this speculation as justified by the observation that the energy released in the original formation of the elements would impart to the nuclei a velocity of the order of magnitude of one-tenth of the velocity of light: "At approximately this speed the fragments of the [primordial] star should fly apart. If we ask where today speeds of this order of magnitude may be observed, we find them only in the recessional motion of the spiral nebulae. Therefore, we ought at least to reckon with the possibility that this motion has its cause in a primeval catastrophe of the sort considered above"" (Kragh, pp. 97-8).

Schaaf speculates on why it was Bethe who was awarded the Nobel Prize for the theory of energy production in stars, even though von Weizsäcker's theory was published first. "Bethe had worked on the energy production in stars at about the same time as Weizsäcker but independently from him. The Nobel Committee valued the structural depth of Bethe's work more than Weizsäcker's temporal priority because Bethe had described the nuclear reactions quantitatively and had shown a much deeper understanding of the nuclear processes in the centre of stars whereas Weizsäcker had worked more qualitatively. There are no reasons to believe that political resentments towards Weizsäcker played any significant role in awarding of the Nobel Prize in Physics in 1967 only to Bethe." Von Weizsäcker was the longest-living member of the team that performed nuclear research in Germany during the Second World War, under Werner Heisenberg's leadership. There is ongoing debate as to whether or not he, and the other members of the team, actively and willingly pursued the development of an atomic bomb for Germany during this time.

Kragh, *Cosmology and Controversy*, 1996; Mehra & Rechenberg, *The Historical Development of Quantum Theory*, Vol. 6, 2001; Schaaf, 'Weizsäcker, Bethe and the Nobel Prize,' *Acta Historica Leopoldina* 63 (2014), 145-56.