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A SUPBERB COLLECTION OF HITHERTO UNPUBLISHED MATHEMATICAL TRACTS

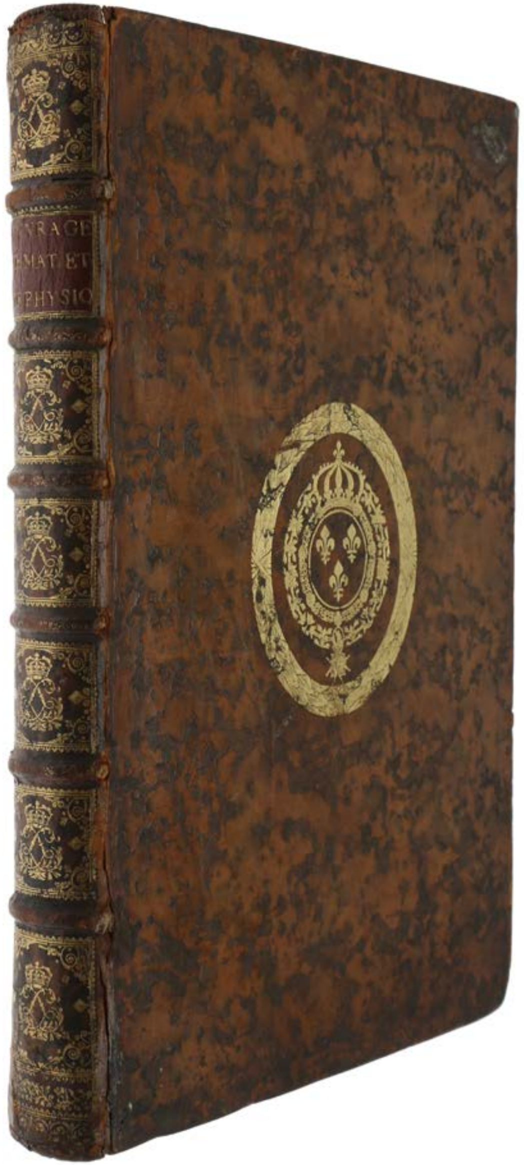
ACADÉMIE ROYALE DES SCIENCES [AUZOUT, FRENICLE, HUYGENS, MARIOTTE, PICARD, ROBERVAL, RØMER]. *Divers ouvrages de mathématique et de physique.* Paris: L'Imprimerie Royale, 1693.

$15,000

Folio (365 x 240 mm), pp. [viii, last leaf blank], 518, [2, colophon], with numerous woodcut diagrams and illustrations in text. Contemporary mottled calf with the arms of Louis XIV in the centre of each cover (Olivier 2494, fer 10), and with his monogram in each spine compartment, hinges with some wear and top capital chipped, an entirely unrestored copy in its original state.

First edition of this superb collection of thirty-one treatises by the leading scientists of seventeenth-century France, almost all of which are published here for the first time. This is one of the earliest important publications of the Académie des Sciences, and one of the most magnificent, and the present copy was probably intended for presentation: it is bound in contemporary calf with the arms of Louis XIV on each cover. Of the eight works by Christiaan Huygens (1629-95) in the present volume, all appear here for the first time except for his treatise on gravity, *De la cause de la pesanteur*, which was first published three years earlier as an appendix to the *Traitéde la lumière*. Most of these works were reprinted at The Hague in 1731 in quarto format (in three separate volumes).

Founded on 22 December 1666, one of the principal functions of the Académie



was to facilitate publication of the works of its members. Frenicle and Roberval were founding members (as was Huygens), and without the assistance of the Académie it is likely that many of their works would have remained unpublished (only two works by Frenicle and two by Roberval were published in their lifetimes). After the death of Frenicle and Roberval in 1675, their books and manuscripts were entrusted to the astronomer Jean Picard; eight treatises by Huygens were also sent to Picard for publication in this collection. After Picard's death in 1682, publication of the works was brought to fruition by Philippe de la Hire. La Hire also included in the *Divers ouvrages* five treatises by Picard himself, including an unusual 37-page work on dioptrics, one by Mariotte and two each by Auzout and Rømer. The most important work in the volume is probably Roberval's *Traité des indivisibles*, composed around the same time as Cavalieri's *Geometria indivisibilibus* (1635) but independent of it and published here for the first time. The treatises by Frenicle, a close correspondent of Fermat, treat topics in number theory and related fields. See below for a full list of contents.

Gilles Personne de Roberval (1602-75) arrived in Paris in 1628 and put himself in contact with the Mersenne circle. "Mersenne, especially, always held Roberval in the highest esteem. In 1632 Roberval became professor of philosophy at the Collège de Maître Gervais. On 24 June 1634, he was proclaimed the winner in the triennial competition for, the Ramus chair (a position that he kept for the rest of his life) at the Collège Royal in Paris, where at the end of 1655 he also succeeded to Gassendi's chair of mathematics. In 1666 Roberval was one of the charter members of the Académie des Sciences in Paris … He himself published only two works: *Traité de méchanique* (1636) and *Aristarchi Samii de mundi systemate* (1644). A rather full collection of his treatises and letters was published in the *Divers ouvrages de mathématique et de physique par messieurs de I'Académie royale des sciences* (1693), but since few of his other writings were published in the following period, Roberval was for long eclipsed by Fermat, Pascal, and, above all, by Descartes, his irreconcilable adversary.

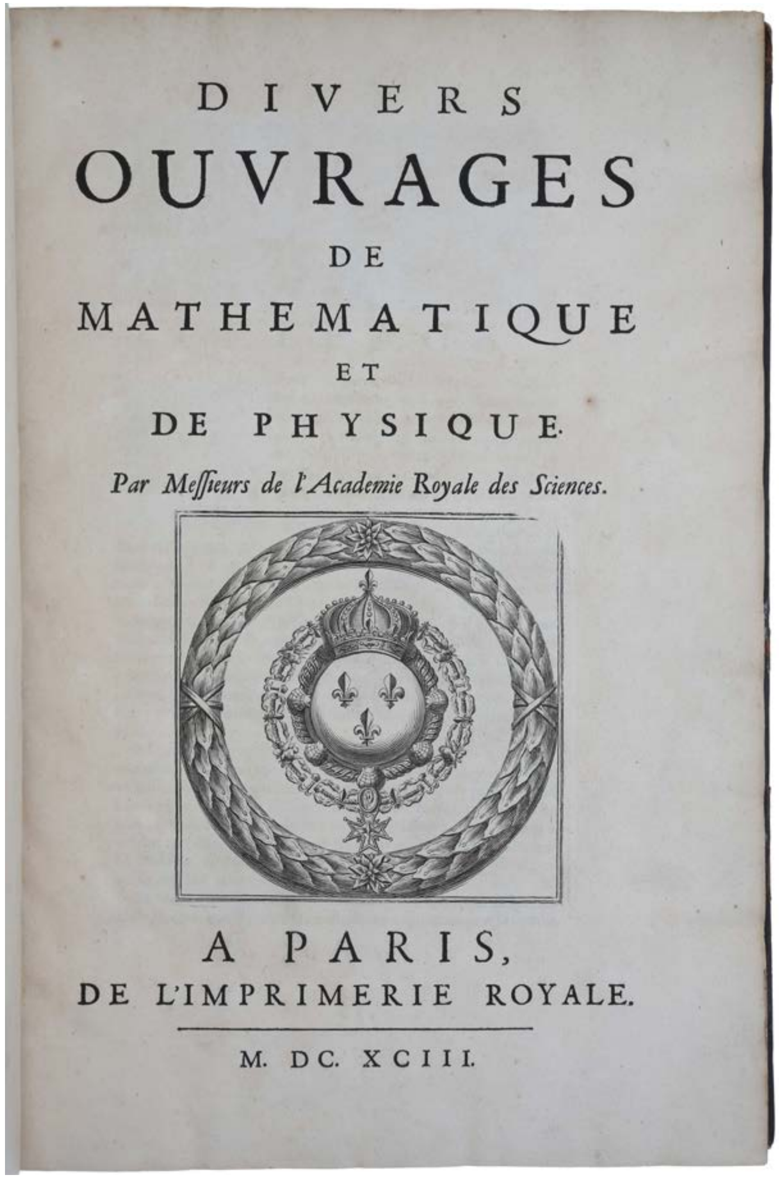
"Roberval was one of the leading proponents of the geometry of infinitesimals, which he claimed to have taken directly from Archimedes, without having known the work of Cavalieri. Moreover, in supposing that the constituent elements of a figure possess the same dimensions as the figure itself, Roberval came closer to the integral calculus than did Cavalieri, although Roberval's reasoning in this matter was not free from imprecision. The numerous results that he obtained in this area are collected in the *Divers ouvrages*, under the title of *Traité des indivisibles*. One of the first important findings was, in modern terms, the definite integration of the rational power, which he most probably completed around 1636, although by what manner we are not certain. The other important result was the integration of the sine … the most famous of his works in this domain concerns the cycloid. Roberval introduced the "compagne" ("partner") of the original cycloidal curve and appears to have succeeded, before the end of 1636, in the quadrature of the latter and in the cubature of the solid that it generates in turning around its base …

"On account of his method of the "composition of Movements" Roberval may be called the founder of kinematic geometry. This procedure had three applications—the fundamental and most famous being the construction of tangents. "By means of the specific properties of the curved line," he stated, "examine the various movements made by the point which describes it at the location where you wish to draw the tangent: from all these movements compose a single one; draw the line of direction of the composed movement, and you will have the tangent of the curved line." Roberval conceived this remarkably intuitive method during his earliest research on the cycloid (before 1636). At first, he kept the invention secret, but he finally taught it between 1639 and 1644; his disciple François du Verdus recorded his lessons in *Observations sur la composition des mouvemens, et sur le moyen de trouver les touchantes des lignes courbes* … In the second place, he also applied this procedure to comparison of the lengths of curves, a subject almost untouched since antiquity … The third application consisted in determining extrema …

"Roberval composed a treatise on algebra, *De recognitione aequationum*, and another on analytic geometry, *De geometrica planarum et cubicarum aequationum resolutione*. Before 1632, he had studied the "logistica speciosa" of Viète; but the first treatise, which probably preceded Descartes's *Géométrie*, contains only the rudiments of the theory of equations. On the other hand, in 1636 he had already resorted to algebra in search of a tangent. By revealing the details of such works, he would have assured himself a more prominent place in the history of analytic geometry, and even in that of differential calculus …

"In 1647 [Roberval] wrote to Torricelli: "We have constructed a mechanics which is new from its foundations to its roof, having rejected, save for a small number, the ancient stones with which it had been built" (p. 301) … around 1669, Roberval wrote *Projet d'un livre de mechanique traitant des mouvemens composez* … Roberval dreamed, certainly with too great temerity, of a vast physical theory based uniquely on the composition of motions" (DSB).

Bernard Frenicle de Bessy (1605-75) was an accomplished amateur mathematician who corresponded with Descartes, Huygens, Mersenne and, perhaps most importantly, Fermat. "Frenicle de Bessy is best known for his contributions to number theory. In fact, Fermat, in a letter to Roberval, writes: 'For some time M Frenicle has given me the desire to discover the mysteries of numbers, an area in which he is highly versed' … He solved many of the problems posed by Fermat but he did more than find numerical solutions for he also put forward new ideas and posed further questions" (Mactutor).



In "*Méthode pour trouver la solution des problèmes par les exclusions*, Frenicle says that in his opinion, arithmetic has as its object the finding of solutions in integers of indeterminate problems. He applied his method of exclusion to problems concerning rational right triangles, e.g., he discussed right triangles, the difference or sum of whose legs is given … The most important of these works by Frenicle is the treatise *Des quarrez ou tables magiques*. These squares, which are of Chinese origin and to which the Arabs were so partial, reached the Occident not later than the fifteenth century. Frenicle pointed out that the number of magic squares increased enormously with the order by writing down 880 magic squares of the fourth order, and gave a process for writing down magic squares of even order" (DSB).

In 1666 Jean Picard (1620-82) "was named a founding member of the Académie Royale des Sciences and, even before its opening, participated in several astronomical observations. In collaboration with Adrien Auzout he perfected the movable-wire micrometer and utilized it to measure the diameters of the sun, the moon, and the planets. During the summer of 1667 he applied the astronomical telescope to the instruments used in making angular measurements—quadrants and sectors—and was aware that this innovation greatly expanded the possibilities of astronomical observation. The making of meridian observations by the method of corresponding heights, which he suggested in 1669, was not put into practice until after his death. Yet when the Academy decided to remeasure an arc of meridian in order to obtain a more accurate figure for the earth's radius, Picard was placed in charge of the operation … it was primarily through the use of instruments fitted with telescopes, quadrants, and sectors for angular measurements that Picard attained a precision thirty to forty times greater than that achieved previously … This increased precision made possible a great advance in the determination of geographical coordinates and in cartography, and enabled Newton in 1684 to arrive at a striking confirmation of the accuracy of his principle of gravitation …

"In 1673 Picard moved into the Paris observatory and collaborated with Cassini, Romer, and, later, Philippe de La Hire on the institution's regular program of observations. He also joined many missions away from the observatory. The first of these enabled him to provide more precise data on the coordinates of various French cities (1672-1674); others, conducted from 1679 to 1681 with La Hire, had the purpose of establishing the bases of the principal triangulation of a new map of France. The results of these geodesic observations were published in 1693 by La Hire [pp. 368-370 of the present work]" (DSB). "In 1692 William Molyneux, who was familiar with [Isaac] Barrow's *Lectiones XVIII*, published his *Dioptrica nova*, which was a practical treatise on lenses and telescopes. He independently arrived at Huygens's rule for images in thin lenses, though in a slightly different form and stated less generally. In the following year Jean Picard's posthumous writings on dioptrics [pp. 375-412] also contained a similar rule for thin lenses as well as a series of equations for thick lenses. Picard had read and admired the *Lectiones XVIII* shortly after it had appeared" (Feingold, *Before Newton*: *The Life and Times of Isaac Barrow* (1990), p. 151).

Adrien Auzout (1622-91) made a significant contribution to the final development of the micrometer and to the replacement of open sights by telescopic sights ... By the summer of 1666 Auzout and Picard were making systematic observations with fully developed micrometers. In a letter sent on 28 December 1666 to Henry Oldenburg, the first secretary of the Royal Society of London, Auzout explained how his new micrometer, with two parallel wires either of silk of silver, one of which could be moved by a screw, could be used to calculate the diameters of the planets and the parallax of the moon. His treatise *Du micrometre* (pp. 413-422) appears to be the first published account of Auzout's work.

CONTENTS:

FRENICLE: *Méthode pour trouver la solution des Problèmes par les exclusions (1-44); Abregé des Combinaisons* (45-64); *Des Quarrez magiques (423-483); Table générale des Quarrez magiques de quatre de coste* (484-507)

ROBERVAL: *Observations sur la composition des mouvements & sur le moyen de trouver les touchantes des lignes courbes* (69-111); *Projet d'un livre de Méchanique, traitant des mouvements composez* (112-113); *De Recognitione aequationum* (114-135); *De Geometrica planarum & cubicarum equationum resolutio* (136-189); *Traité des Indivisbles* (190-245); *De Trochoide ejusque spatio* (246-278); *Epistola Aegedii Personerii de Roberval ad R. P. Mersennum* (278-282); *Epistola Evangelista Torricellii ad Robervallium* (283-284); *Epistola Aegedii Personerii de Roberval ad Evangelistam Torricellium* (284-302)

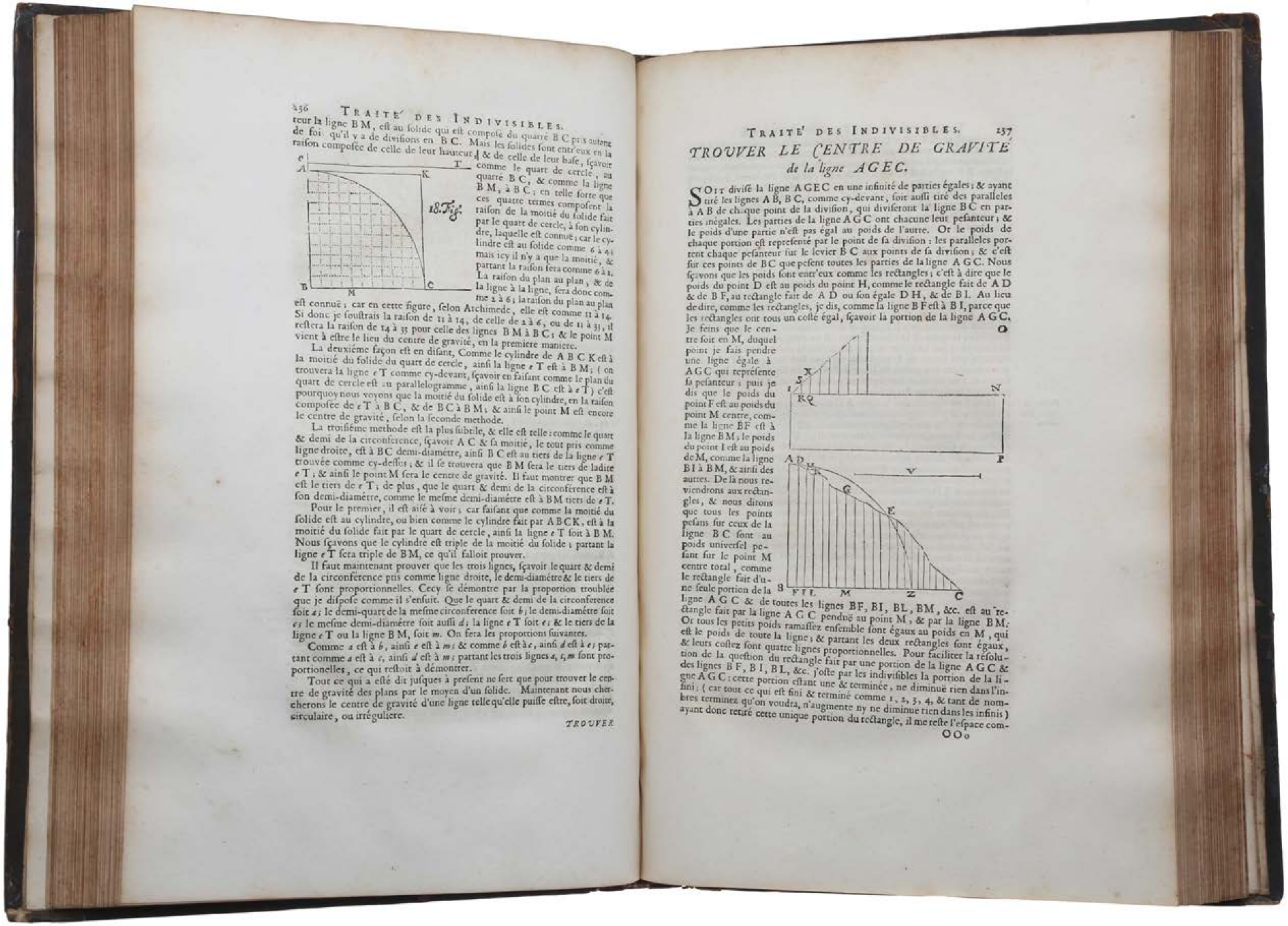
HUYGENS: *De la cause de la pesanteur* (305-312); *Démonstration de l'equilibre de la balance* (313-316); *De potentiis fila funesque trahentibus* (317-319); *Nouvelle force mouvante par le moyen de la poudre a canon & de l'air* (320-321); *Constructio loci ad Hyperbolam per Asymptotos* (322-325); *Demonstratio regula de maximis et minimis* (326-330); *Regula ad inveniendas Tangentes curvarum* (330-335); *Construction d'un problème d'Optique* (336)

PICARD: *De la pratique des grands Cadrans par le calcul* (341-365); *De mensuris* (366-368); *Mésures prises sur les originaux, & comparés avec le pied du Chastelet de Paris* (368-370); *De mensura liquidorum & aridorum* (370-374); *Fragments de Dioptrique* (375-412)

AUZOUT: *Du micromètre* (413-422)

MARIOTTE: *Règles pour les jets d'eau, & de la depense qui se fait par différens ajustages, selon les diverses élévations des reservoirs* (508-516)

RØMER: *De crassitie & viribus tuborum in aqua-ductibus secundum diversae fontium altitudines, diversaequae tuborum diametros* (516-517); *Experimenta circa altitudines & amplitudeines projectionis corporum gravium, institute cum argento vivo* (517-518).



236 TRAITÉ DES INDIVISIBLES.

teur la ligne B M, est au solide qui est composé du quarré B C pris autant de fois qu'il y a de divisions en B C. Mais les solides sont entr'eux en la raison composée de celle de leur hauteur, & de celle de leur base, sçavoir comme le quart de cercle, au quarré B C, & comme la ligne B M, à B C; en telle sorte que ces quatre termes composent la raison de la moitié du solide fait par le quart de cercle, à son cylindre, laquelle est connuë; car le cylindre est au solide comme 6 à 4; mais icy il n'y a que la moitié, & partant la raison sera comme 6 à 2. La raison du plan au plan, & de la ligne à la ligne, sera donc comme 2 à 6; la raison du plan au plan est connuë; car en cette figure, selon Archimede, elle est comme 11 à 14. Si donc je soustrais la raison de 11 à 14, de celle de 2 à 6, ou de 11 à 33, il restera la raison de 14 à 33 pour celle des lignes B M à B C; & le point M vient à estre le lieu du centre de gravité, en la premiere maniere.

La deuxiéme façon est en disant, Comme le cylindre de A B C K est à la moitié du solide du quart de cercle, ainsi la ligne e T est à B M; (on trouvera la ligne e T comme cy-devant, sçavoir en faisant comme le plan du quart de cercle est au parallelogramme, ainsi la ligne B C est à e T) c'est pourquoy nous voyons que la moitié du solide est à son cylindre, en la raison composée de e T à B C, & de B C à B M; & ainsi le point M est encore le centre de gravité, selon la seconde methode.

La troisiéme methode est la plus subtile, & elle est telle: comme le quart & demi de la circonference, sçavoir A C & sa moitié, le tout pris comme ligne droite, est à BC demi-diamétre, ainsi B C est au tiers de la ligne e T trouvée comme cy-dessus; & il se trouvera que B M sera le tiers de ladite e T; & ainsi le point M sera le centre de gravité. Il faut montrer que B M est le tiers de e T; de plus, que le quart & demi de la circonférence est à son demi-diamétre, comme le mesme demi-diamétre est à BM tiers de e T.

Pour le premier, il est aisé à voir; car faisant que comme la moitié du solide est au cylindre, ou bien comme le cylindre fait par A B C K, est à la moitié du solide fait par le quart de cercle, ainsi la ligne e T soit à B M. Nous sçavons que le cylindre est triple de la moitié du solide; partant la ligne e T sera triple de BM, ce qu'il falloit prouver.

Il faut maintenant prouver que les trois lignes, sçavoir le quart & demi de la circonférence pris comme ligne droite, le demi-diamétre & le tiers de e T sont proportionnelles. Cecy se démontre par la proportion troublée que je dispose comme il s'ensuit. Que le quart & demi de la circonference soit a; le demi-quart de la mesme circonference soit b; le demi-diamétre soit c; le mesme demi-diamétre soit aussi d; la ligne e T soit e; & le tiers de la ligne e T ou la ligne B M, soit m. On fera les proportions suivantes.

Comme a est à b, ainsi e est à m; & comme b est à c, ainsi d est à e; partant comme a est à c, ainsi d est à m; partant les trois lignes a, c, m sont proportionelles, ce qui restoit à démontrer.

Tout ce qui a esté dit jusques à present ne sert que pour trouver le centre de gravité des plans par le moyen d'un solide. Maintenant nous chercherons le centre de gravité d'une ligne telle qu'elle puisse estre, soit droite, circulaire, ou irréguliere.

TROUVER

TRAITÉ DES INDIVISIBLES. 237

TROUVER LE CENTRE DE GRAVITÉ *de la ligne* AGEC.

SOIT divisé la ligne A G E C en une infinité de parties égales; & ayant tiré les lignes A B, B C, comme cy-devant, soit aussi tiré des paralleles à A B de chaque point de la division, qui diviseront la ligne B C en parties inégales. Les parties de la ligne A G C ont chacune leur pesanteur; & le poids d'une partie n'est pas égal au poids de l'autre. Or le poids de chaque portion est representé par le point de sa division: les paralleles portent chaque pesanteur sur le levier B C aux points de sa division; & c'est sur ces points de B C que pesent toutes les parties de la ligne A G C. Nous sçavons que les poids sont entr'eux comme les rectangles; c'est à dire que le poids du point D est au poids du point H, comme le rectangle fait de A D & de B F, au rectangle fait de A D ou son égale D H, & de B I. Au lieu de dire, comme les rectangles, je dis, comme la ligne B F est à B I, parce que les rectangles ont tous un costé égal, sçavoir la portion de la ligne A G C.

Je feins que le centre soit en M, duquel point je fais pendre une ligne égale à A G C qui représente sa pesanteur; puis je dis que le poids du point F est au poids du point M centre, comme la ligne BF est à la ligne B M; le poids du point I est au poids de M, comme la ligne B I à B M, & ainsi des autres. De là nous reviendrons aux rectangles, & nous dirons que tous les points pesans sur ceux de la ligne B C sont au poids universel pesant sur le point M centre total, comme le rectangle fait d'une seule portion de la ligne A G C & de toutes les lignes BF, BI, BL, BM, &c. est au rectangle fait par la ligne A G C penduë au point M, & par la ligne B M. Or tous les petits poids ramassez ensemble sont égaux au poids en M, qui est le poids de toute la ligne; & partant les deux rectangles sont égaux, & leurs costez sont quatre lignes proportionnelles. Pour faciliter la résolution de la question du rectangle fait par une portion de la ligne A G C & des lignes B F, B I, B L, &c. j'oste par les indivisibles la portion de la ligne A G C: cette portion estant une & terminée, ne diminuë rien dans l'infini; (car tout ce qui est fini & terminé comme 1, 2, 3, 4, & tant de nombres terminez qu'on voudra, n'augmente ny ne diminuë rien dans les infinis) ayant donc retiré cette unique portion du rectangle, il me reste l'espace com-

OOo

THE ONLY PRESENTATION COPY KNOWN, IN A SPECIAL GIFT BINDING

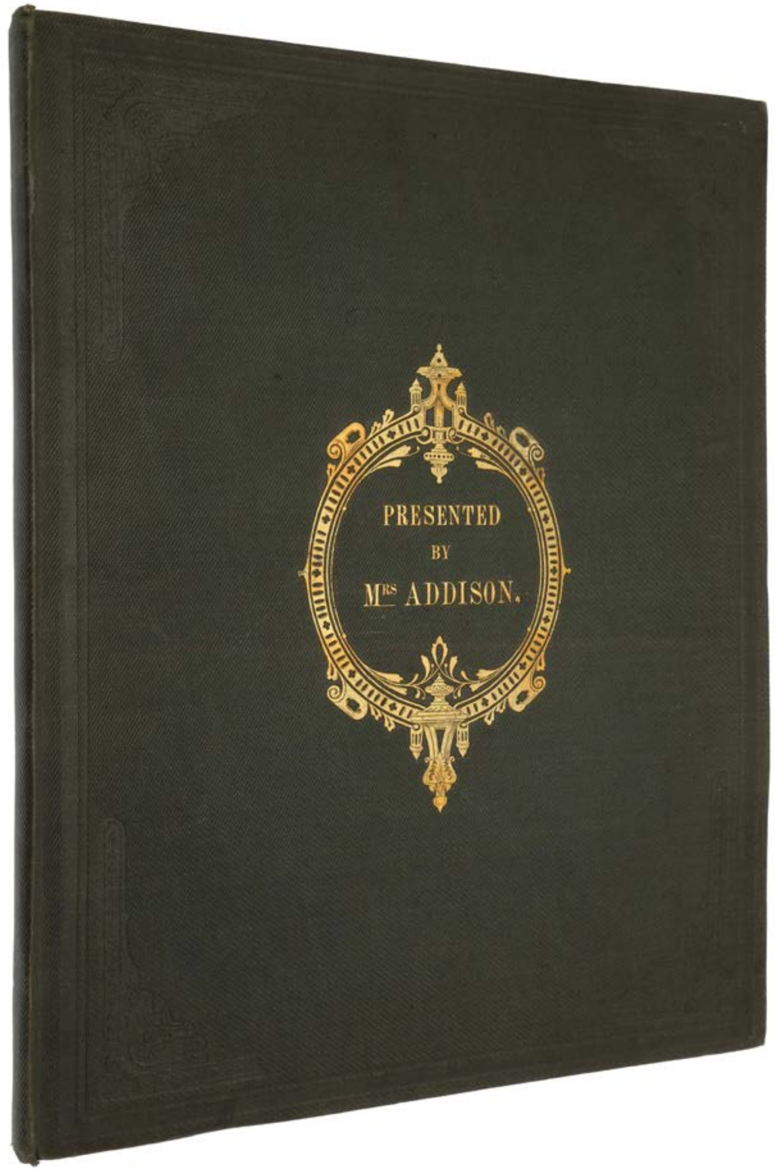
Grolier/Norman, *One Hundred Books Famous in Medicine* 60c

ADDISON, Thomas. *On the Constitutional and Local Effects of Disease of the Supra-Renal Capsules.* London: Samuel Highley, 1855.

$28,000

4to (323 x 249 mm). viii, 43, [1]pp. 11 hand-colored lithograph plates by W. Hurst and M. and N. Hanhart after drawings by W. Hurst and John Tupper. Original green cloth stamped in gilt and blind, very slight wear at extremities. Fine, clean copy, presented by Addison's widow to Addison's friend Henry Lonsdale (1816-76), with a unique binding with the gilt-stamped ornament on the front cover reading "Presented by Mrs. Addison," instead of the usual title lettering, and inscription on the front free endpaper, presumably in the hand of Mrs. Addison, reading: "To Dr. Lonsdale one of the Author's best & kind friends." A very fine copy, preserved in a custom leather box.

First edition, the only known presentation copy, presented by Addison's widow to Addison's friend Henry Lonsdale (1816-76), with a unique binding with the gilt-stamped ornament on the front cover reading "Presented by Mrs. Addison," instead of the usual title lettering, and inscription on the front free endpaper, presumably in the hand of Mrs. Addison, reading: "To Dr. Lonsdale one of the Author's best & kind friends." It is in a very special original binding, that was undoubtedly bound specially for the purpose, in which the normal lettering within the gilt cartouche on the upper cover ("On Disease of the Supra Renal



Capsules by Thomas Addison, M.D.") is replaced by the words "Presented by Mrs. Addison." The work was inscribed to Dr. Henry Lonsdale, who was physician to the Cumberland Infirmary in Carlisle; he was also the author of *The Worthies of Cumberland* (1873), which contains a 12-page memoir of Addison. This copy is the only nineteenth century medical or scientific work in a cloth presentation binding of this type we have seen.

Addison's monograph inaugurated the study of diseases of the ductless glands and the disturbances in chemical equilibrium known as pluriglandular syndromes; it also marks the beginning of modern endocrinology. Addison chanced upon adrenal disease while searching for the causes of pernicious anemia; his initial report on the subject, a short paper entitled "On anemia: Disease of the suprarenal capsules" (1849), attempted to link the two diseases. The present monograph focuses on diseases of the suprarenal capsules and contains the classic description of the endocrine disturbance now known as "Addison's disease," and also includes his superb account of pernicious anemia ("Addison's anemia"), in which he suggested that the existence of anemia together with supra-renal disease was not coincidental. Addison was the first to suggest that the adrenal glands are essential for life, and his monograph inspired a burst of experimental research that led, among other things, to Vulpian's discovery of adrenalin in 1856.

Addison "was born in April 1793, at Long Benton, Newcastle-upon-Tyne and died on June 29 1860, at 15 Wellington Villas, Brighton. The son of Sarah and Joseph Addison, a grocer and flour dealer in Long Benton, Addison was first sent to school in a roadside cottage where his teacher was John Rutter, the parish clerk, who years later also taught Robert Stephenson.

"He proceeded to the Royal Free Grammar School, Newcastle-upon-Tyne, and learned Latin so well that he made notes in that language. This explains his

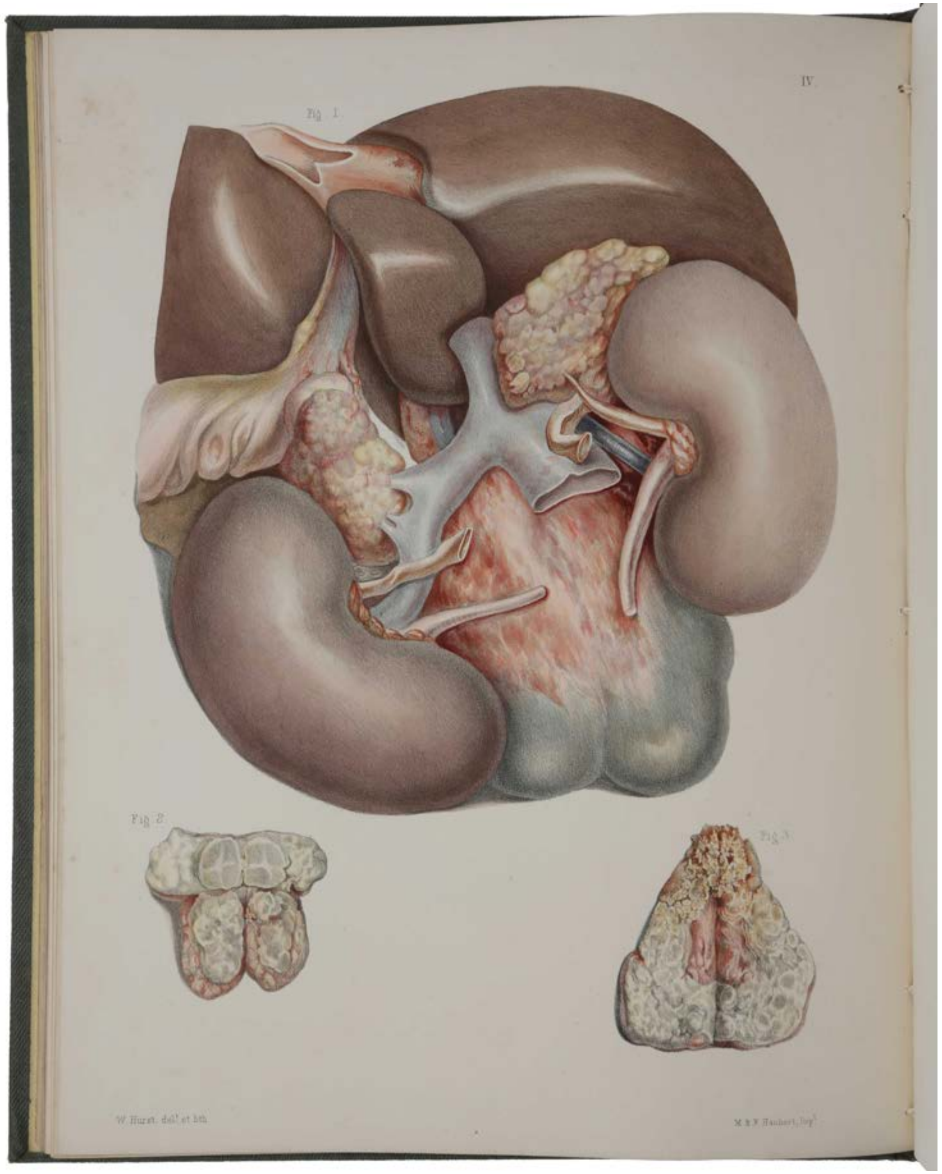
lifelong precision in language. His father endeavoured to provide an education and a social status much higher than his own. In 1812 Thomas became a medical student at the University of Edinburgh and in August 1815 gained an MD with a thesis 'Concerning Syphilis and Mercury' (now in the Wellcome Library, London). In that year he moved to Skinner Street, Snow Hill, London, to become house surgeon at the Lock Hospital, and entered as pupil to the Public Dispensary. Thomas Bateman (1778-1821), an acclaimed dermatologist, instilled in him a lasting interest in skin diseases. He progressed rapidly: the 1817 Guy's Hospital records show:

'Dec. 13, 1817, from Edinburgh, T. Addison, M.D., paid pounds 22-1s to be a perpetual Physician's pupil.'

"He obtained his LRCP in December 1819, was promoted to assistant physician and in 1827 became lecturer in *materia medica*. His lectures were so popular that his lecture-fees were assessed at £700 or £800 a year. In 1835 Addison with Richard Bright gave lectures on practical medicine, and in 1837 Addison became full physician to Guy's Hospital. Unlike the charming and cheerful Bright with wealthy parentage and broad education, Addison concealed nervousness and timidity beneath a proud and haughty exterior. In the words of Samuel Wilks:

'a quick hasty and impassioned manner of expression is not unfrequently the result of a deficient controlling power. We know … that, although wearing the outward garb of resolution, he was beyond most other men, most liable to sink under trial.'

"Probably for these reasons his professional preferment came late in life. For example, not until 1838 was he elected a Fellow of the Royal College of Physicians. His shyness and occasional severity stood in the way of a large private practice;



nevertheless, his diagnostic brilliance and his lucid and forceful teaching were appreciated at Guy's, where he showed devotion to patients and students alike. His enquiring mind and scientific curiosity were apparent, for in a biographical prefix to his published writings he was described as

'Possessing unusually vigorous perceptive powers, being shrewd and sagacious beyond the average of men, the patient before him was scanned with a penetrating glance from which few diseases could escape detection... [he] would remain at the bedside with a dogged determination to track out the disease to its very source for a period which often wearied his class and his attendant friends.'

"The story of 'Addison's disease' begins with the adrenal glands, first described by Eustachius in 1714. Addison first wrote a short article in the *London Medical Gazette* (1849): 'Anaemia—disease of the suprarenal capsules in which the disease is not distinctly separated from a new form of anaemia'. Then, in 1855, came his monograph, one of the unsurpassed medical works of the nineteenth century. Addison describes here for the first time two chronic diseases which he could not clearly separate—'On the Constitutional and Local Effects of Disease of the Suprarenal Capsule'. The entity he related was doubted by Hughes Bennett (1812-1875) in Edinburgh but confirmed by Trousseau (1801-1867) in Paris, who recognized suprarenal failure and named it Addison's disease. The monograph describes how, when investigating a peculiar form of anaemia, he found pathological changes in both suprarenal glands that appeared to be independent of the anaemia. He had with Samuel Wilks collected 11 patients.

"He described the symptoms of 11 cases:

'The discoloration pervades the whole surface of the body, but is commonly most strongly manifested on the face, neck, superior extremities, penis, scrotom, and in the flexures of the axillae and around the navel... The leading and characteristic features of the morbid state to which I would direct your attention are, anaemia, general languor and debility, remarkable feebleness of the heart's action, irritability of the stomach, and a peculiar change of the colour in the skin, occurring in connection with a diseased condition of the suprarenal capsules.'

"One patient had been treated by Bright, who had noted typical clinical features but failed to incriminate the adrenals. Indeed Addison critically commented:

'It did not appear that Dr. Bright either entertained a suspicion of the disease of the capsules before death, or was led at any period to associate the colour of the skin with the diseased condition of the organs, although his well-known sagacity induced him to suggest the probable existence of some internal malignant disease. In this as in most other cases, we have the same remarkable prostration, the usual gastric symptoms, the same absence of any very obvious and adequate cause of the patient's actual condition together with a discoloration of the skin, sufficiently striking to have arrested Dr. Bright's attention even during the life of the patient.'

"Interestingly, to his pupils his essay on suprarenal failure ranked far below his elucidation of phthisis and his impressive teaching.

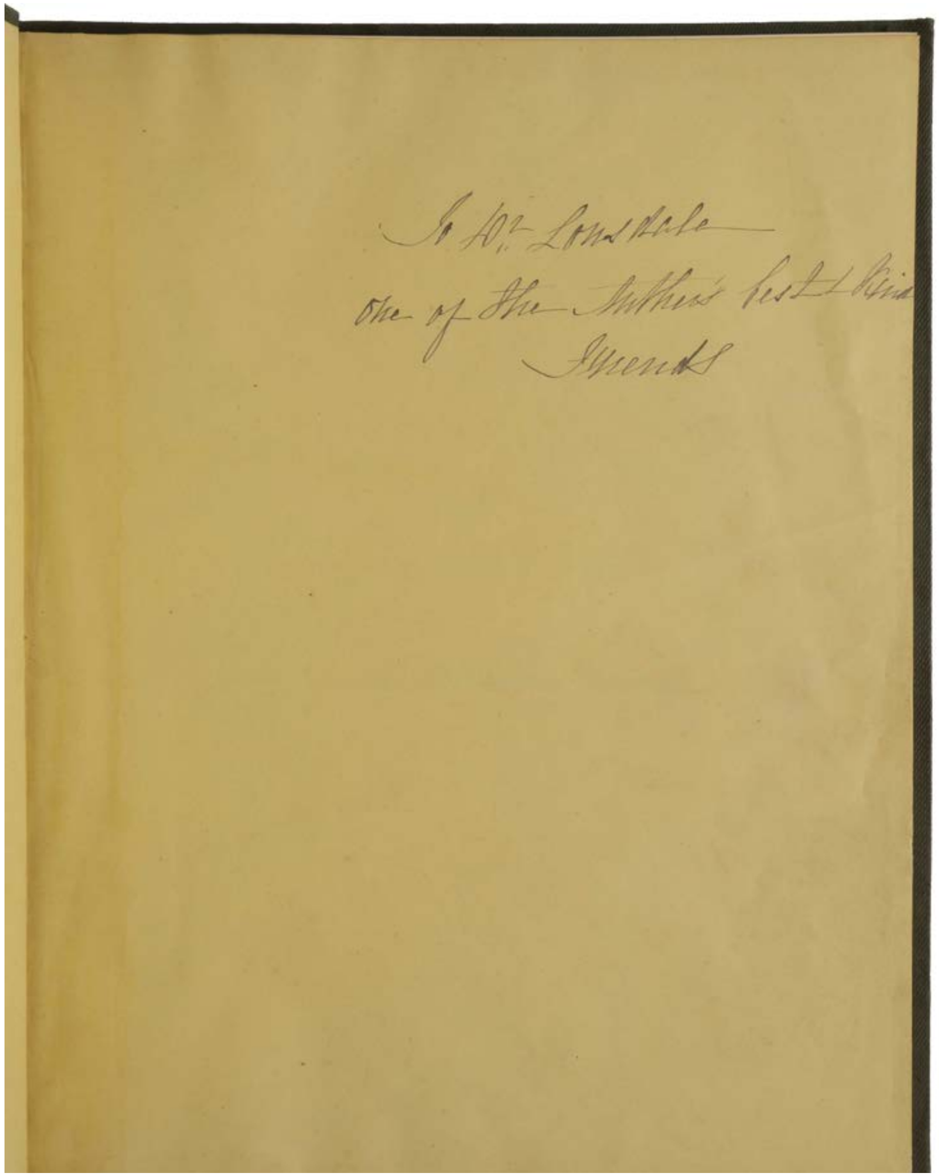
"In most cases of Addison's disease today the pathogenesis is autoimmune, as exemplified by the polyglandular autoimmune syndromes where it is evident in two-thirds of type 1 and almost all cases of type 2. Tuberculosis now accounts for about 20% of primary adrenal insufficiency in developed countries, whereas in Addison's day it was found at autopsy in 70-90% of cases.

"The description of 'Addison's anaemia' came in 1849, in a lecture to the South London Medical Society. But in 1822 James Scarth Combe, in the *Transactions* *of the Medico-Surgical Society of Edinburgh*, had described 'idiopathic anaemia' and never sought priority for this new disease—pernicious anaemia. In the *Medical Times and Gazette* of London in 1874, Biermer of Zurich wrote of a new 'idiopathic anaemia' not yet described in England. Within a week, Samuel Wilks refuted this claim in the *British Medical Journal*, stating that the disease was well known in England since Addison had lectured on it in 1843. Addison observed its insidious onset in either sex, usually in middle life. He related:

'the countenance gets pale, the whites of the eyes become pearly, the general frame flabby rather than wasted... the whole surface of the body presents a blanched, smooth and waxy appearance; the lips, gums, and tongue seem bloodless... extreme languor and faintness supervene, breathlessness and palpitations being produced by the most trifling exertion or emotion; some slight oedema is probably perceived in the ankles; the debility becomes extreme... the disease... resisted all remedial efforts and sooner or later terminated fatally... On examining the bodies I have failed to discover any organic lesion that could properly or reasonably be assigned as an adequate cause...'

"The condition became known as pernicious anaemia—usually caused by loss of the 'intrinsic factor' required for absorption of cyanocobalamin.

"Addison made other signal contributions. He wrote volume 1 of *Elements of the Practice of Medicine* (1839), but the planned two further volumes with Bright never emerged. It contained an early and comprehensive account of 'Inflammation of the caecum and appendix vermiformis'. He also gave an identifiable account of biliary cirrhosis, previously described by Pierre-François-Olive Rayer in *Traité théorique et pratique des maladies de la peau* (1826-1827), Paris, 1835. In 1824 Addison founded the Department of Dermatology at Guy's, which still possesses



a collection of wax models of skin disorders prepared under his supervision. 'On a certain affection of the skin, vitilogoidea plana tuberosa', presents a seminal account of *xanthoma planum et tuberosum*, a common sequel to hypercholesterolaemia. Addison with Sir William Gull (1816-1890) described *xanthoma diabeticorum*, and he also depicted circumscribed scleroderma (morphoea). In 1843 he correctly described the pathology of pneumonia, which until that time was thought to be an interstitial pneumonitis. He had traced the bronchial branches to their alveolar termination where he discovered 'pneumonic deposits in the air cells'" (Pearce).

Addison suffered from several bouts of severe depression during his lifetime, and eventually committed suicide in 1860. It would seem that Addison's mental instability precluded him from giving any copies of his *Disease of the Supra-Renal Capsules* to his friends, as we know of no other presentation copies of this work apart from this one from his widow.

Grolier, *One Hundred Books Famous in Medicine* 60c; *Heirs of Hippocrates* 1502; Norman 8; Garrison-Morton.com 3864 ; Goldschmid, p. 194; McCann, pp. 87-89; Medvei, pp. 225-230.; Pearce, 'Thomas Addison (1793-§860),' Journal of the Royal Society of Medicine 97 (2004), pp. 297-300.

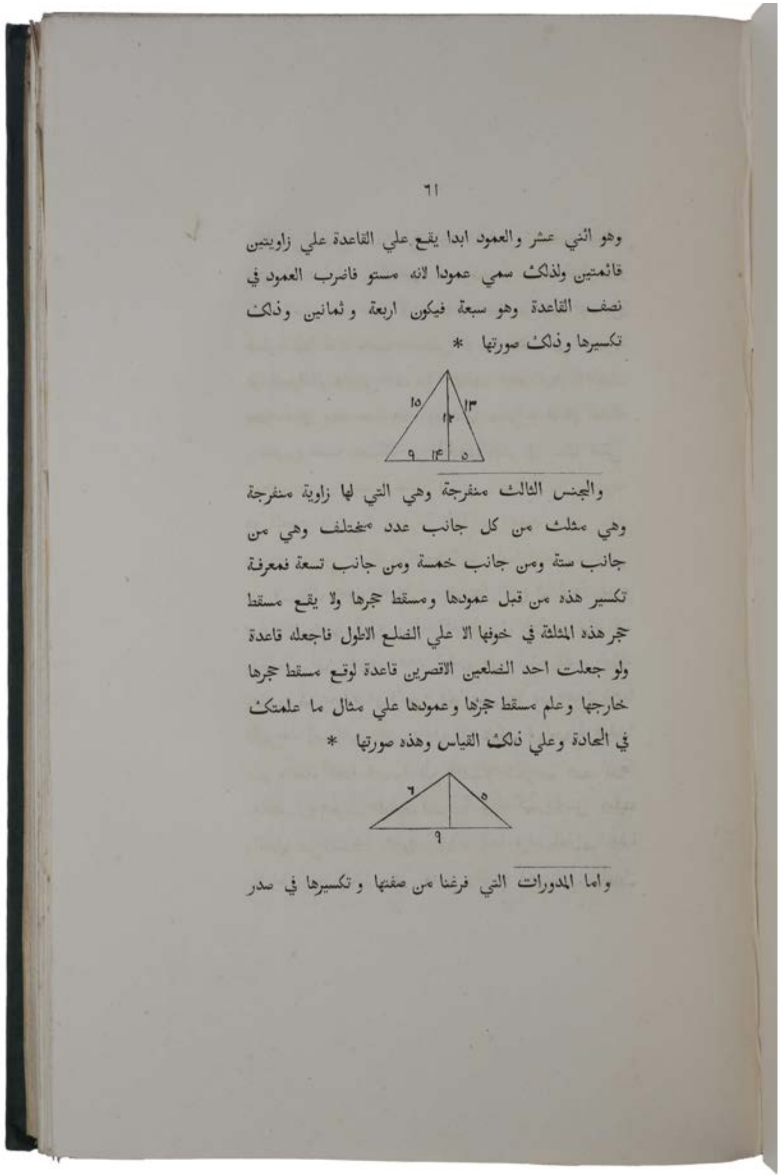
FIRST PRINTING OF AL-KHWĀRIZMĪ'S ALGEBRA IN ANY LANGUAGE

[AL-KHWĀRIZMĪ, Abū Ja'far Muhammad ibn Mūsā]. *The Algebra of Mohammed Ben Musa. Edited and translated by Frederic Rosen. [Title in Arabic] Al-kitab al-mukhtasar fi hisab al-jabr wa'l-muqabalah.* London: for the Oriental Translation Fund, 1831.

$5,500

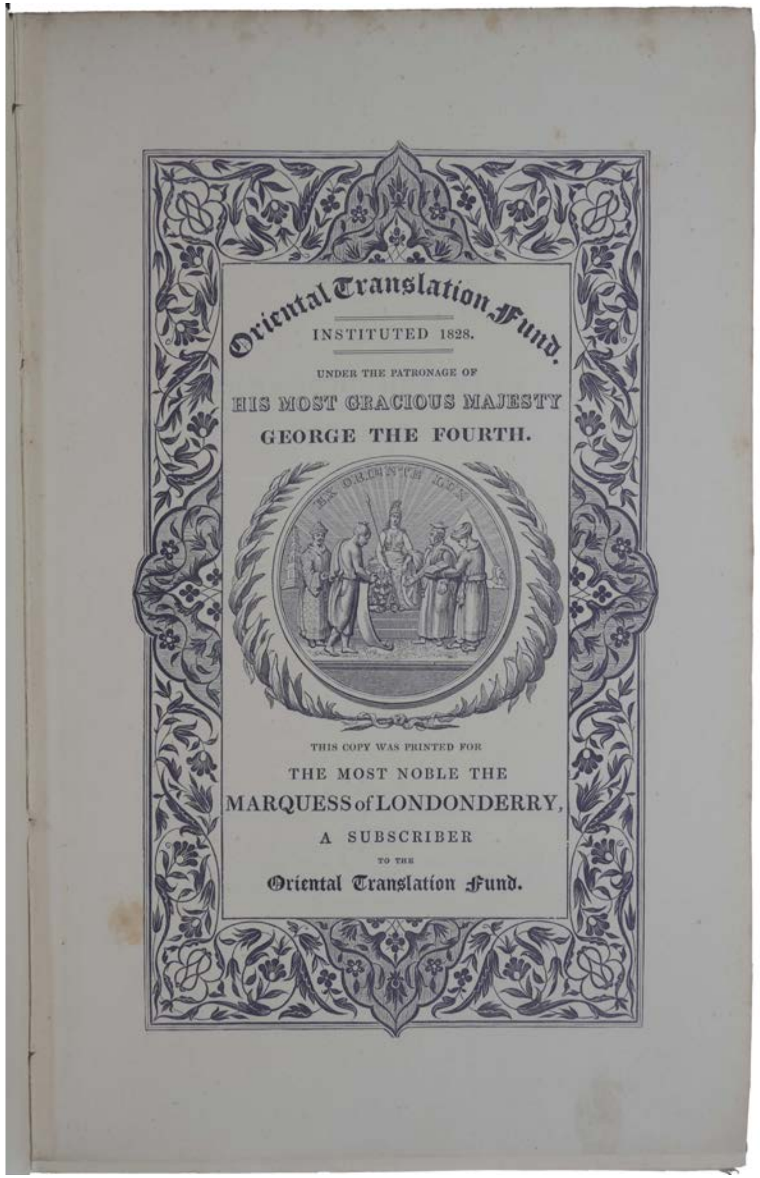
Large 8vo (260 x 170mm), pp. xvi, 208 (English); [4], 122, [2] (Arabic); 8 (Oriental Translation Fund list of patrons and officers, regulations, and list of publications); in English and Arabic, with some Sanskrit, algebraic notation and diagrams; a very good, clean, partly unopened copy, on large paper; in contemporary green cloth, paper spine label; a very few marks; subscriber's plate tipped in before title: 'This copy was printed for the most noble the Marquess of Londonderry'.

First edition, a handsome subscriber's copy on large paper, of the Arabic text of al-Khwārizmī's pioneering *Algebra*, with an English translation by the German orientalist Friedrich August Rosen. These are the first printings of al-Khwārizmī's *Algebra* in any language. Rosen (1805-37), who based this edition on a fourteenth-century Arabic manuscript at the Bodleian Library (*Hunt*. 214), was professor of oriental literature at the University of London and secretary of the Royal Asiatic Society, before his premature death. The Oriental Translation Fund was founded in 1828, under the patronage of George IV, to finance the translation and printing of oriental works in English. Individual and institutional subscribers paying ten guineas or more annually were entitled to a fine paper copy of each work published by the Fund, with their name on an ornamental title-page.



"One of the earliest Islamic algebra texts, entitled *Al-kitab al-mukhtasar fi hisab al-jabr wa'l-muqabalah* (or *The Compendious Book on the Calculation of al-Jabr and al-Muqabala*), was written around 825 by Muhammad ibn Mūsā al-Khwārizmī (ca. 780-850) and ultimately had immense influence not only in the Islamic world but also in Europe" (Katz & Parshall, p. 138). "The first and in some respects the most illustrious of the Arabian mathematicians was Muhammad ibn Mūsā Djefar al-Khwārizmī … The algebra of al-Khwārizmī holds a most important place in the history of mathematics, for we may say that the subsequent Arabian and the early medieval works on algebra were founded on it, and also that through it the Arabic or Indian system of decimal numeration was introduced into the West … It was from this book that the Italians first obtained not only the ideas of algebra but also of an arithmetic founded on the decimal system. This arithmetic was long known as *algorism*, or the art of al-Khwārizmī, which served to distinguish it from the arithmetic of Boethius; this name remained in use till the eighteenth century" (Rouse Ball, *A Short Account of the History of Mathematics*, pp. 162-4). 'Algorism' is, of course, the root of our word 'algorithm', so ubiquitous in our modern technology; and our 'algebra' is derived from 'al-jabr' in the title of this work. The *Algebra* presents the systematic solution of linear and quadratic equations, demonstrating how to solve the latter by completing the square, discusses the rule of three, and deals with practical mensuration and problems relating to legacies under Islamic law. A protégé of the Caliph al-Ma'mūn, al-Khwārizmī served as astronomer and librarian at the 'House of Wisdom' in Baghdad. ABPC/RBH list only one copy (Swann, March 8, 2018, lot 217, $1375), an ex-library copy with the usual markings and printed on ordinary paper (217 x 140mm, compared to 260 x 170mm for our large paper copy).

Provenance: Charles Vane, 3rd Marquess of Londonderry (1778-1854). Vane, the half-brother of Lord Castlereagh, served with considerable gallantry during the Peninsular War and acted as an ambassador at the Congress of Vienna, where he



earned the sobriquet 'the golden peacock' for his love of fine dress and shocked his peers with his drinking and womanising. In spite of his wealth, Vane was often in financial difficulties, so much so that he almost followed his half-brother's example of suicide.

"The *Algebra* is a work of elementary practical mathematics, whose purpose is explained by the author (Rosen trans., p. 3) as providing 'what is easiest and most useful in arithmetic, such as men constantly require in cases of inheritance, legacies, partition, lawsuits, and trade, and in all their dealings with one another, or where the measuring of lands, the digging of canals, geometrical computations, and other objects of various sorts and kinds are concerned.' Indeed, only the first part of the work treats of algebra in the modern sense. The second part deals with practical mensuration, and the third and longest with problems arising out of legacies. The first part (the algebra proper) discusses only equations of the first and second degrees. According to al-Khwārizmī, all problems of the type he proposes can be reduced to one of six standard forms … Such an elaboration of cases is necessary because he does not recognize the existence of negative numbers or zero as a coefficient … He also explains how to reduced any given problem to one of these standard forms. This is done by means of the two operations *al-jabr* and *al-muqābala. Aljabr*, which we may translate as 'restoration' or 'completion,' refers to the process of eliminating negative quantities [transposing a subtracted quantity on one side of an equation to the other side when it becomes an added quantity] … *Al-muqābala*, which we may translate as 'balancing,' refers to the process of reducing positive quantities of the same power on both sides of the equation [i.e., the reduction of a positive term by subtracting equal amounts from both sides of the equation] … These two operations, combined with the arithmetical operations of addition, subtraction, multiplication, and division (which al-Khwārizmī also explains in their application to the various powers), are sufficient to solve all types of problems propounded in the *Algebra*. Hence they are used to characterize the work, whose full title is *al-Kitāb al-mukhtasar fī hisāb al-jabr wa'l-muqābala* ("The Compendious Book on Calculation by Completion and Balancing"). The appellation *al-jabr wa'l-muqābala*, or *al-jabr* alone, was commonly applied to later works in Arabic on the same topic; and thence (via medieval Latin translations from the Arabic) is derived the English 'algebra.'

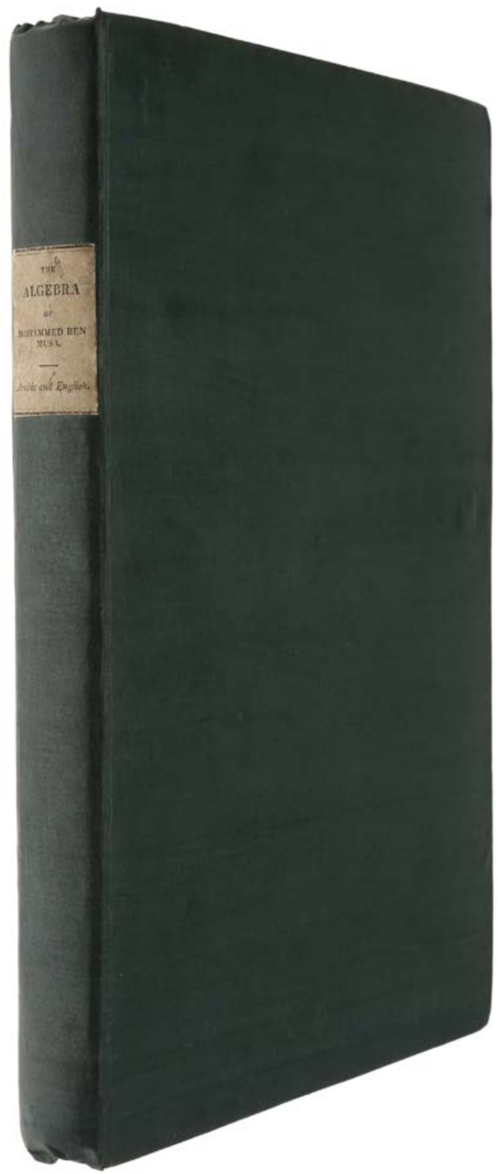
"In his *Algebra* al-Khwārizmī employs no symbols (even for numerals) but expresses everything in words. For the unknown quantity he employs the word *shay*' ('thing' or 'something'). For the second power of a quantity he employs *māl* ('wealth,' 'property'), which is also used to mean only 'quantity.' For the first power, when contrasted with the second power, he uses *jidhr* ('root'). For the unit he uses *dirham* (a unit of coinage) …

"After illustrating the rules he has expounded for solving problems by a number of worked examples, al-Khwārizmī, in a short section headed 'On Business Transactions,' expounds the 'rule of three,' or how to determine the fourth member in a proportion sum where two quantities and one price, or two price and one quantity, are given. The next part concerns practical mensuration. He gives rules for finding the area of various plane figures, including the circle, and for finding the volume of a number of solids, including cone, pyramid, and truncated pyramid. The third part, on legacies, consists entirely of solved problems. These involve only arithmetic or simple linear equations but require considerable knowledge of the complicated Islamic law of inheritance …"

"Only a few details of al-Khwārizmī's life can be gleaned from the brief notices in Islamic bibliographical works and occasional remarks by Islamic historians and geographers. The epithet 'al-Khwārizmī' would normally indicate that he came from Khwārizm (Khorezm, corresponding to the modern Khiva and the district surrounding it, south of the Aral Sea in central Asia). But the historian al-Tabarī gives him the additional epithet 'al-Qutrubbullī,' indicating that he came from Qutrubbull, a district between the Tigris and Euphrates not far from Baghdad, so perhaps his ancestors, rather than he himself, came from Khwārizm; this interpretation is confirmed by some sources which state that his 'stock' (*asl*) was from Khwārizm. Another epithet given to him by al-Tabarī, 'al-Majũsī,' would seem to indicate that he was an adherent of the old Zoroastrian religion. This would still have been possible at that time for a man of Iranian origin, but the pious preface to al-Khwārizmī's *Algebra* shows that he was an orthodox Muslim, so al-Tabarī's epithet could mean no more than that his forebears, and perhaps he in his youth, had been Zoroastrians.

"Under the Caliph al-Ma'mũn (reigned 813-833), al-Khwārizmī became a member of the 'House of Wisdom' (Dār al-Hikma), a kind of academy of scientists set up at Baghdad, probably by Caliph Harũn al-Rashīd, but owing its pre-eminence to the interest of al-Ma'mũn, a great patron of learning and scientific investigation. It was for al-Ma'ũn that al-Khwārizmī composed his astronomical treatise, and his *Algebra* also is dedicated to that ruler" (DSB).

DSB VII, 364. For a detailed analysis of this work, see Katz & Parshall, *Taming the Unknown* (2014), pp. 138-147.



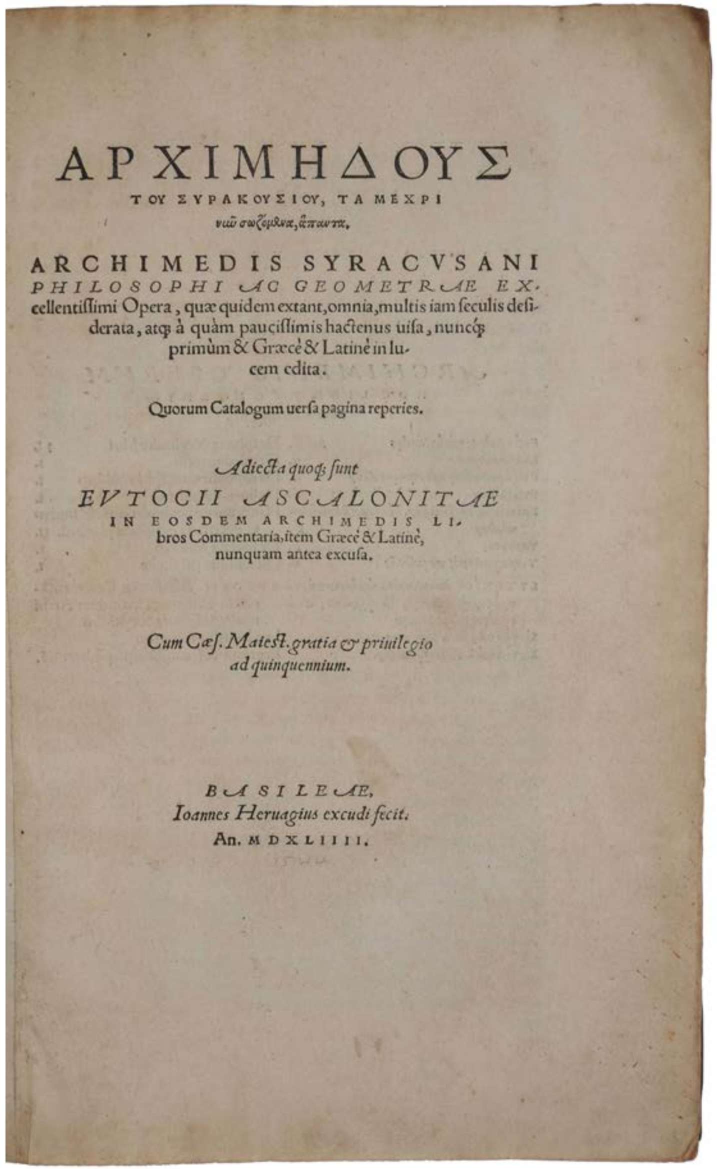
PMM 72 - 'GIVE ME A PLACE TO STAND, AND I WILL MOVE THE EARTH' - EXTENSIVELY ANNOTATED

ARCHIMEDES. *Opera, quae quidem extant, omnia ... nuncque primum & Graece Latine in lucem edita ... adiecta quoque sunt Eutocii Ascalonitae in eosdem Archimedis libros commentaria item Graece & Latine, nunquam antea excusa.* Basle: Joannes Hervagius, 1544.

$75,000

Folio (310 x 205 mm), pp. [8], 1-139, [1], [8], [1], 2-163, [1], [4], 1-65, [1], 1-68, [1, colophon]. Numerous woodcut diagrams and initials, text in Greek and Latin. Seventeenth-century (Dutch?) calf with gilt-stamped armorial on covers (rebacked retaining the original endpapers), red speckled edges.

First edition of one of the key scientific books of the Renaissance, representing a decisive step forward in the history of mathematics, containing the first printings of the majority of the surviving works of the greatest mathematician, physicist and engineer of antiquity. This is a fascinating copy with numerous contemporary annotations by a well-informed reader, both in the margins and in the text itself (in a minuscule neat hand that does not obscure the original text). In addition, there is a full-page manuscript entitled 'Cristiani Hugenii / Alia demonstratio propositionis 18 / Archimedis de spiralibus / ad paginam 111,' and including a large geometrical diagram, which provides Huygens' alternative proof to a proposition about Archimedes' spiral demonstrated on the facing page of text. This suggests that the annotator was probably a member of Huygens' circle. This


ΑΡΧΙΜΗΔΟΥΣ
ΤΟΥ ΣΥΡΑΚΟΥΣΙΟΥ, ΤΑ ΜΕΧΡΙ
νῦν σωζόμενα, ἅπαντα.

ARCHIMEDIS SYRACVSANI
PHILOSOPHI AC GEOMETRAE EXcellentissimi Opera, quæ quidem extant, omnia, multis iam seculis desiderata, atq; à quàm paucissimis hactenus uisa, nuncq; primùm & Græcè & Latinè in lucem edita.

Quorum Catalogum uersa pagina reperies.

Adiecta quoq; sunt
EVTOCII ASCALONITAE
IN EOSDEM ARCHIMEDIS LIbros Commentaria, item Græcè & Latinè, nunquam antea excusa.

Cum Cæs. Maiest. gratia & priuilegio ad quinquennium.

BASILEAE,
Ioannes Heruagius excudi fecit.
An. MDXLIIII.

book constitutes "the first printing of the original Greek text of seven Archimedean mathematical texts, accompanied by Jacopo de Cremona's Latin translation from a manuscript corrected by Regiomontanus, and the commentaries (in both Greek and Latin) of the sixth-century mathematician Eutocius of Ascalon" (Norman). "Archimedes – together with Newton and Gauss – is generally regarded as one of the greatest mathematicians the world has ever known, and if his influence had not been overshadowed at first by Aristotle, Euclid and Plato, the progress of modern mathematics might have been much faster. As it was, his influence began to take full effect only after this first printed edition which enabled Descartes, Galileo, and Newton in particular to build on what he had begun" (PMM). The seven treatises included in the present work are: *On the Sphere & Cylinder*; *On the Measurement of the Circle*; *On Conoids & Spheroids*; *On Spirals*; *On the Equilibrium of Planes* (and *Centres of Gravity*); *The Arenarius*, or *Sand-Reckoner*; and *On the Quadrature of the Parabola*. "Publication of this *editio princeps* inspired a multiplication of texts on Archimedes and his methods, which exerted a strong influence on the development of mathematics during the sixteenth and seventeenth centuries. One of the important effects of that influence can be seen in Kepler's *Astronomia nova* (1609), in which Archimedes' so-called 'exhaustion procedure' was applied to the measurement of time elapsed between any two points if Mars's orbit" (Norman). "Apart from one small tract published in 1503 and an imperfect edition by Tartaglia in 1543, [this] is the first complete edition of Archimedes' works" (PMM). This volume also includes for the first time the description of the heliocentric system of Aristarchus, who had conceived this theory centuries before Copernicus.

"The principal results in *On the Sphere and Cylinder* (in two books) are that the surface area S of any sphere of radius r is four times that of its greatest circle (in modern notation, $S = 4\pi r^2$) and that the volume V of a sphere is two-thirds that of the cylinder in which it is inscribed (leading immediately to the formula for the volume, $V = 4/3\pi r^3$). Archimedes was proud enough of the latter discovery to leave instructions for his tomb to be marked with a sphere inscribed in a cylinder. Marcus Tullius Cicero (106–43 BC) found the tomb, overgrown with vegetation, a century and a half after Archimedes' death.

"*Measurement of the Circle* is a fragment of a longer work in which π, the ratio of the circumference to the diameter of a circle, is shown to lie between the limits of 3 10/71 and 3 1/7. Archimedes' approach to determining π, which consists of inscribing and circumscribing regular polygons with a large number of sides, was followed by everyone until the development of infinite series expansions in India during the 15th century and in Europe during the 17th century. That work also contains accurate approximations (expressed as ratios of integers) to the square roots of 3 and several large numbers.

"*On Conoids and Spheroids* deals with determining the volumes of the segments of solids formed by the revolution of a conic section (circle, ellipse, parabola, or hyperbola) about its axis. In modern terms, those are problems of integration.

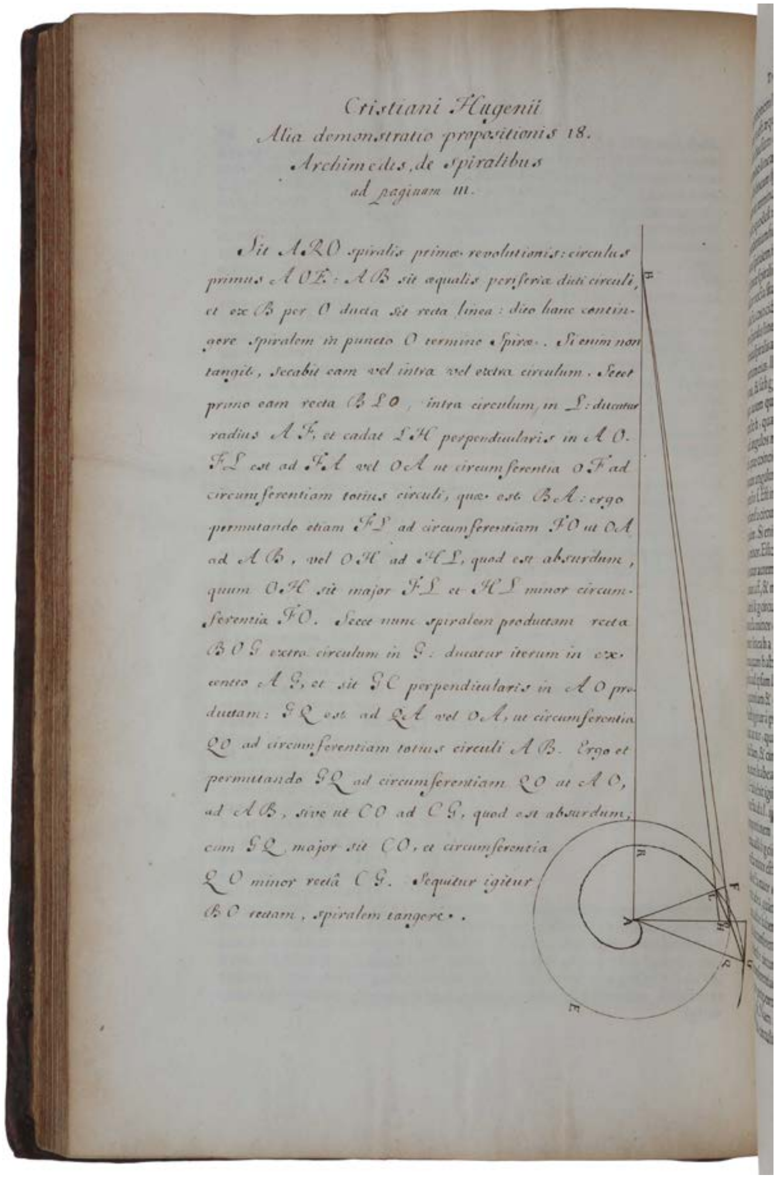
"*On Spirals* develops many properties of tangents to, and areas associated with, the spiral of Archimedes—i.e., the locus of a point moving with uniform speed along a straight line that itself is rotating with uniform speed about a fixed point. It was one of only a few curves beyond the straight line and the conic sections known in antiquity.

"*On the Equilibrium of Planes* (or *Centres of Gravity of Planes*; in two books) is mainly concerned with establishing the centres of gravity of various rectilinear plane figures and segments of the parabola and the paraboloid. The first book purports to establish the 'law of the lever' (magnitudes balance at distances from the fulcrum in inverse ratio to their weights), and it is mainly on the basis of that treatise that Archimedes has been called the founder of theoretical mechanics. Much of that book, however, is undoubtedly not authentic, consisting as it does of inept later additions or reworkings, and it seems likely that the basic principle of the law of the lever and—possibly—the concept of the centre of gravity were established on a mathematical basis by scholars earlier than Archimedes. His contribution was rather to extend those concepts to conic sections.

"*Quadrature of the Parabola* demonstrates, first by 'mechanical' means and then by conventional geometric methods, that the area of any segment of a parabola is 4/3 of the area of the triangle having the same base and height as that segment.

"*The Sand-Reckoner* is a small treatise that is a jeu d'esprit written for the layman—it is addressed to Gelon, son of Hieron [see below]—that nevertheless contains some profoundly original mathematics. Its object is to remedy the inadequacies of the Greek numerical notation system by showing how to express a huge number—the number of grains of sand that it would take to fill the whole of the universe. What Archimedes does, in effect, is to create a place-value system of notation, with a base of 100,000,000. (That was apparently a completely original idea, since he had no knowledge of the contemporary Babylonian place-value system with base 60.) The work is also of interest because it gives the most detailed surviving description of the heliocentric system of Aristarchus of Samos (c. 310–230 BC) and because it contains an account of an ingenious procedure that Archimedes used to determine the Sun's apparent diameter by observation with an instrument …

"Archimedes' mathematical proofs and presentation exhibit great boldness and originality of thought on the one hand and extreme rigour on the other, meeting the highest standards of contemporary geometry. While he arrived at the formulas for the surface area and volume of a sphere by 'mechanical' reasoning



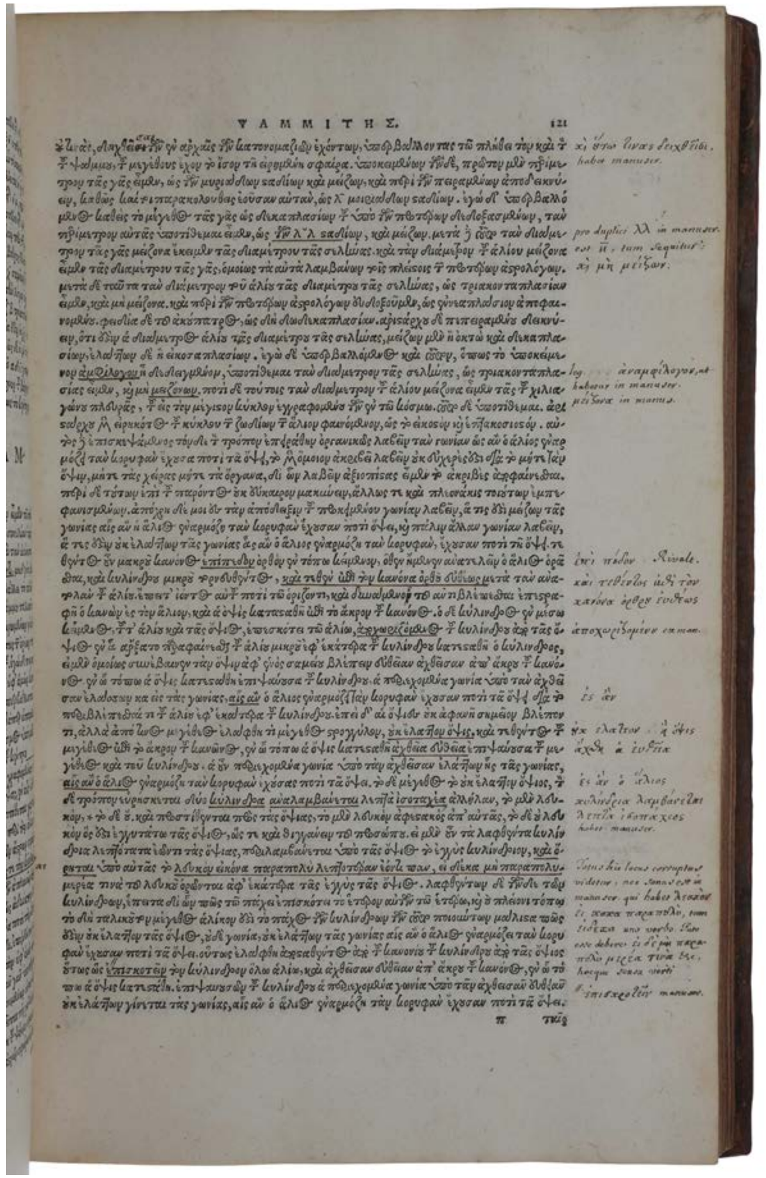
involving infinitesimals, in his actual proofs of the results in *Sphere and Cylinder* he uses only the rigorous methods of successive finite approximation that had been invented by Eudoxus of Cnidus in the 4th century BC. These methods, of which Archimedes was a master, are the standard procedure in all his works on higher geometry that deal with proving results about areas and volumes. Their mathematical rigour stands in strong contrast to the 'proofs' of the first practitioners of integral calculus in the 17th century, when infinitesimals were reintroduced into mathematics. Yet Archimedes' results are no less impressive than theirs. The same freedom from conventional ways of thinking is apparent in the arithmetical field in *Sand-Reckoner*, which shows a deep understanding of the nature of the numerical system" (Britannica).

Although Eutocius (480-540) was not an original thinker, his commentaries contain much historical information which might otherwise have been lost. It is to Eutocius that we owe the Archimedean solution of a cubic by means of intersecting conics, referred to in *On the Sphere & Cylinder* (Book II.4) but not otherwise extant except through his commentary. Eutocius also records the solution of the original problem of II.4 by Diocles (c. 240 – c. 180 BC), avoiding the use of the cubic, and the solution by Dionysodorus (c. 250 – c. 190 BC) of the auxiliary cubic. It is thought that Eutocius did not know of the four remaining works, *On Conoids & Spheroids*, *On Spirals*; *The Sand-Reckoner*, and *On the Quadrature of the Parabola*.

"In contrast to Euclid's *Elements*, the writings of Archimedes were not widely known in antiquity. Survival of their texts was due to interest in Archimedes' writings at the Byzantine capital of Constantinople from the sixth through the tenth centuries. "It is true that before that time individual works of Archimedes were obviously studied at Alexandria, since Archimedes was often quoted by three eminent mathematicians of Alexandria: Hero, Pappus, and Theon. But it is with the activity of Eutocius of Ascalon, who was born toward the end of the fifth century and studied at Alexandria, that the textual history of a collected edition of Archimedes properly begins. Eutocius composed commentaries on three of Archimedes' works: *On the Sphere and the Cylinder*, *On the Measurement of the Circle*, and *On the Equilibrium of Planes*. These were no doubt the most popular of Archimedes' works at that time … The works of Archimedes and the commentaries of Eutocius were studied and taught by Isidore of Miletus (442-537) and Anthemius of Tralles (474-534), Justinian's architects of Hagia Sophia in Constantinople. It was apparently Isidore who was responsible for the first collected edition of at least the three works commented on by Eutocius as well as the commentaries. Later Byzantine authors seem gradually to have added other works to this first collected edition until the ninth century when the educational reformer Leon of Thessalonica produced the compilation represented by Greek manuscript A (adopting the designation used by the editor, J. L. Heiberg [*Opera omnia, cum commentariis Eutocii*, 3 vols., Leipzig, 1880-1]). Manuscript A contained all of the Greek works now known excepting *On Floating Bodies*, *On the Method*, *Stomachion*, and *The Cattle Problem*. This was one of the two manuscripts available to William of Moerbeke (1215-86) when he made his Latin translations in 1269. It was the source, directly or indirectly, of all of the Renaissance copies of Archimedes. A second Byzantine manuscript, designated as B, included only the mechanical works: *On the Equilibrium of Planes*, *On the Quadrature of the Parabola* and *On Floating Bodies* (and possibly *On Spirals*). It too was available to Moerbeke, but it disappears after an early fourteenth-century reference. Finally we can mention a third Byzantine manuscript, C, a palimpsest whose Archimedean parts are in a hand of the tenth century. It was not available to the Latin West in the Middle Ages, or indeed in modern times until its identification by Heiberg in 1906 at Constantinople (where it had been brought from Jerusalem).

"In the fifteenth century, knowledge of Archimedes in Europe began to expand. A new Latin translation was made by James of Cremona (1400-56) in about 1450 by order of Pope Nicholas V. Since this translation was made exclusively from manuscript A, the translation failed to include *On Floating Bodies*, but it did include the two treatises in A omitted by Moerbeke, namely *The Sand Reckoner* and Eutocius' *Commentary on the Measurement of the Circle*. It appears that this new translation was made with an eye on Moerbeke's translation. . . . There are at least nine extant manuscripts of this translation, one of which was corrected by Regiomontanus and brought to Germany about 1468 … Greek manuscript A itself was copied a number of times. Cardinal Bessarion had one copy prepared between 1449 and 1468 (MS E). Another (MS D) was made from A when it was in the possession of the well-known humanist George [Giorgio] Valla (1447-99). The fate of A and its various copies has been traced skilfully by J. L. Heiberg in his edition of Archimedes' *Opera*. The last known use of manuscript A occurred in 1544, after which time it seems to have disappeared.

"The first printed Archimedean materials were in fact merely Latin excerpts that appeared in George Valla's *De expetendis et fugiendis rebus opus* (Venice, 1501) and were based on his reading of manuscript A. But the earliest actual printed texts of Archimedes were the Moerbeke translations of *On the Measurement of the Circle* and *On the Quadrature of the Parabola* (*Teragonismus, id est circuli quadratura* etc.) published from the Madrid manuscript by L[uca] Gaurico (Venice, 1503). In 1543 also at Venice N[iccolo] Tartaglia republished the same two translations directly from Gaurico's work, and in addition, from the same Madrid manuscript, the Moerbeke translations of *On the Equilbrium of Planes* and Book I of *On Floating Bodes* (leaving the erroneous impression that he had made these translations from a Greek manuscript, which he had not since he merely



repeated the texts of the Madrid manuscript, with virtually all their errors) … The key event, however, in the further spread of Archimedes was the aforementioned *editio princeps* of the Greek text with the accompanying Latin translation of James of Cremona at Basel in 1544" (Marshall Clagett in DSB).

For this *editio princeps* the editor Thomas Gechauff, called Venatorius (d. 1551), was able to use the above-mentioned manuscript of James of Cremona's Latin translation corrected by Regiomontanus, which included the commentaries of Eutocius. For the Greek text Gechauff used a manuscript which had been acquired in Rome by humanist Willibald Pirckheimer (1470-1530), and is preserved today in Nuremberg City Library. Gechauf, Nuremberg scholar and theologian, was born about 1490 and was a pupil of Johannes Schöner (1477-1547) and a friend of Pirckheimer. He wrote in both Latin and German, published an edition of Aristophanes 'Plutus' (1531), and his name is found in some works in conjunction with that of Andreas Osiander (1498-1552), who famously added the preface to Copernicus.

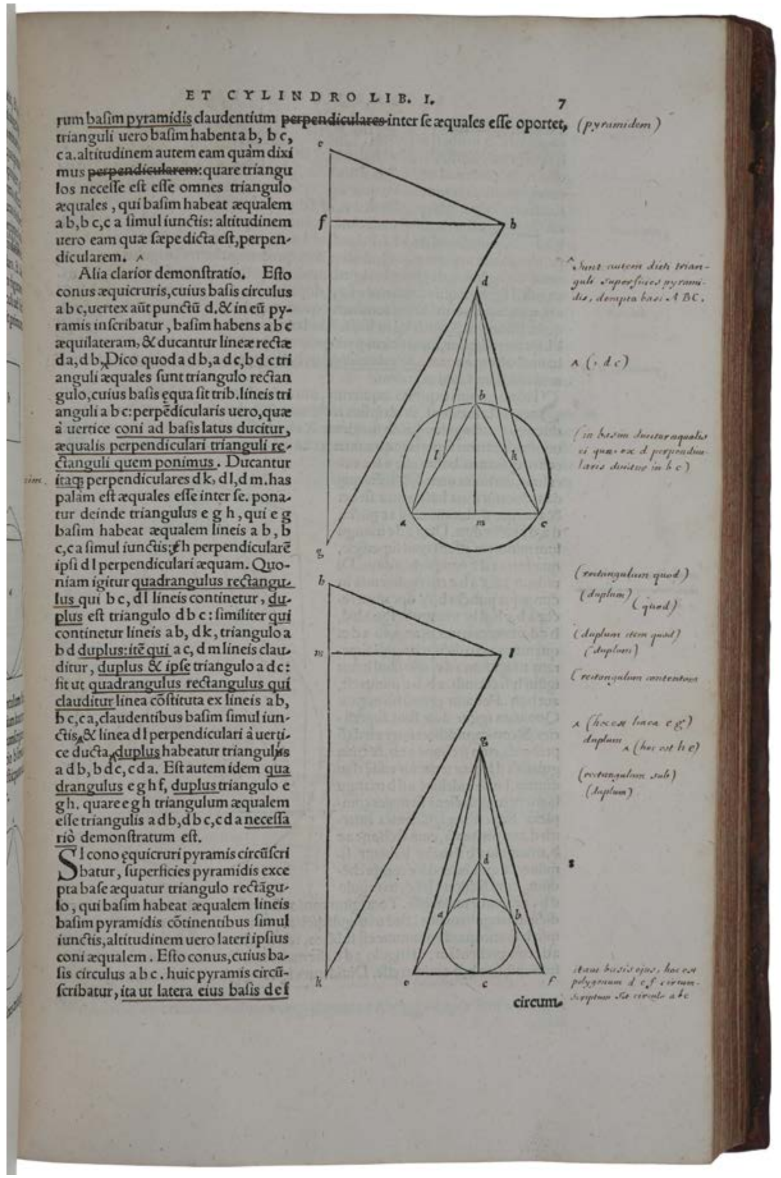
Manuscripts A and B are now lost. However, after disappearing into a European private collection in the early twentieth century, the third key record of Archimedes' texts discussed above, the tenth century Byzantine manuscript C, known as the Archimedes Palimpsest, re-appeared at a Christie's auction in New York on October 28, 1998, where it was purchased by a private collector in the United States. Since then it has been made widely available to scholars, and has been the subject of much research. It contains the only extant manuscript of Archimedes' *Method Concerning Mechanical Theorems*, which describes how he used a 'mechanical' method to arrive at some of his key discoveries, including the area of a parabolic segment and the surface area and volume of a sphere. The technique consists of dividing each of two figures into an infinite but equal number of infinitesimally thin strips, then 'weighing' each corresponding pair of these strips against each other on a notional balance to obtain the ratio of the two original figures. Archimedes emphasizes that this procedure, though useful as a heuristic method, does not constitute a rigorous proof. Nevertheless, his method is a clear precursor of Cavalieri's method of indivisibles (1635), and of the integral calculus of Newton and Leibniz.

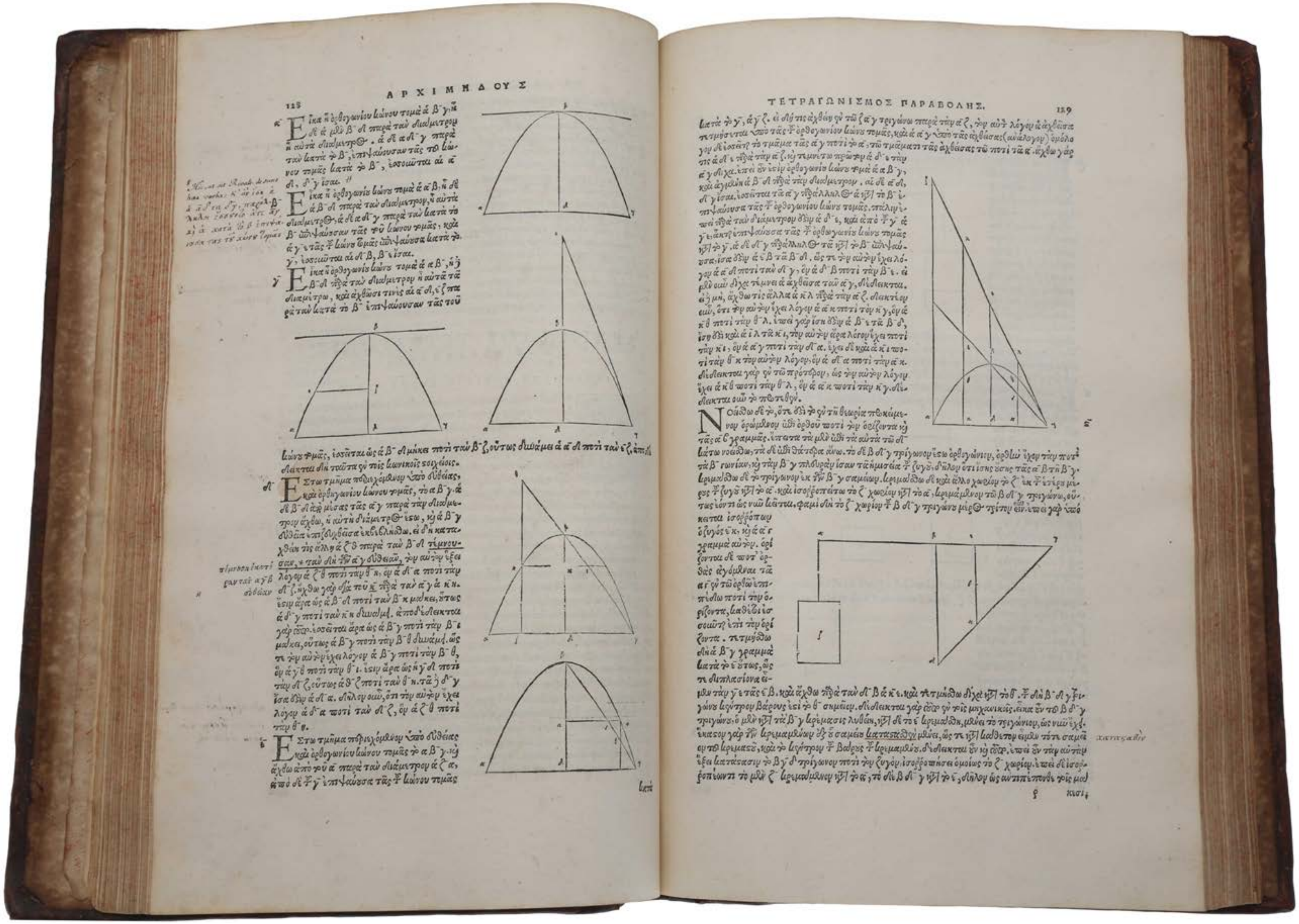
Archimedes (c. 287 – 212/211 BC) "probably spent some time in Egypt early in his career, but he resided for most of his life in Syracuse, the principal Greek city-state in Sicily, where he was on intimate terms with its king, Hieron II. Archimedes published his works in the form of correspondence with the principal mathematicians of his time, including the Alexandrian scholars Conon of Samos and Eratosthenes of Cyrene. He played an important role in the defence of Syracuse against the siege laid by the Romans in 213 BC by constructing war machines so effective that they long delayed the capture of the city. When Syracuse eventually fell to the Roman general Marcus Claudius Marcellus in the autumn of 212 or spring of 211 BC, Archimedes was killed in the sack of the city.

"Far more details survive about the life of Archimedes than about any other ancient scientist, but they are largely anecdotal, reflecting the impression that his mechanical genius made on the popular imagination. Thus, he is credited with inventing the Archimedes screw, and he is supposed to have made two 'spheres' that Marcellus took back to Rome—one a star globe and the other a device (the details of which are uncertain) for mechanically representing the motions of the Sun, the Moon, and the planets. The story that he determined the proportion of gold and silver in a wreath made for Hieron by weighing it in water is probably true, but the version that has him leaping from the bath in which he supposedly got the idea and running naked through the streets shouting 'Heurēka!' ("I have found it!") is popular embellishment. Equally apocryphal are the stories that he used a huge array of mirrors to burn the Roman ships besieging Syracuse; that he said, 'Give me a place to stand and I will move the Earth'; and that a Roman soldier killed him because he refused to leave his mathematical diagrams—although all are popular reflections of his real interest in catoptrics (the branch of optics dealing with the reflection of light from mirrors, plane or curved), mechanics, and pure mathematics" (Britannica).

Active in the early 6th century, Eutocius apparently was a pupil of the Neo-Platonist Ammonius Saccas (175-242), and perhaps a colleague of Anthemius of Tralles. If so, he was trained as a Neo-Platonist philosopher. In this tradition, it was customary to pay attention to the mathematical sciences and even to write some commentaries on them, but Eutocius is the only Neo-Platonist we know to concentrate uniquely on mathematical commentary. In addition to his commentaries on Archimedes, he also wrote an important commentary on the first four books of the *Conics* of Apollonius (c. 262 BC – c. 190 BC).

PMM 72; Adams A1531; Dibner 137; Grolier/Horblit 5; Hoffman I, 228; Macclesfield 179 & 180; Norman 61.

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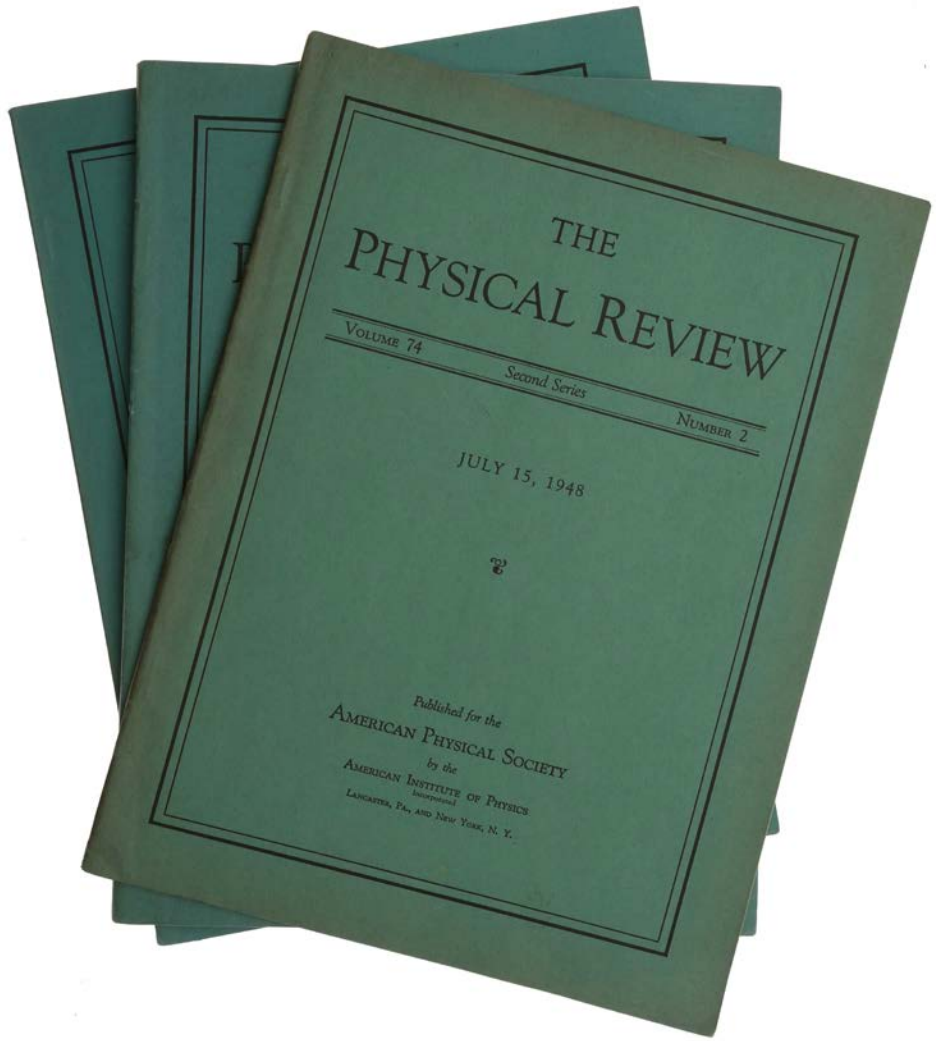
ΑΡΧΙΜΗΔΟΥΣ
ΤΕΤΡΑΓΩΝΙΣΜΟΣ ΠΑΡΑΒΟΛΗΣ.
129

INVENTION OF THE TRANSISTOR

BARDEEN, J. & BRATTAIN, W. H. *'The transistor, a semi-conductor triode,'* pp. 230-1 [AND] **BRATTAIN, W. H. & BARDEEN, J.** *'Nature of the forward current in Germanium point contacts,'* pp. 231-2 [AND] **SHOCKLEY, W. & PEARSON, W. L.** *'Modulation of conductance of thin films of semi-conductors by surface charges,'* pp. 232-3, in Physical Review Vol. 74, No. 2, July 15, 1948. *[Offered with:]* **BARDEEN, J. & BRATTAIN, W. H.** *'Physical principles involved in transistor action,'* pp. 1208-25 in Physical Review Vol. 75, No. 8, April 15, 1949. [Offered with:] **SHOCKLEY, William, SPARKS, Morgan & TEAL, Gordon K.** *'p-n junction transistors,'* pp. 151-162 in Physical Review Vol. 83, No. 1, July 1, 1951. Lancaster, PA., and New York: American Physical Society, 1948-51.

$6,500

Three journal issues, 8vo (268 x 200 mm), pp. 131-233; 1115-1338; 1-248. Original printed wrappers, very light wear to spines, a fine set.



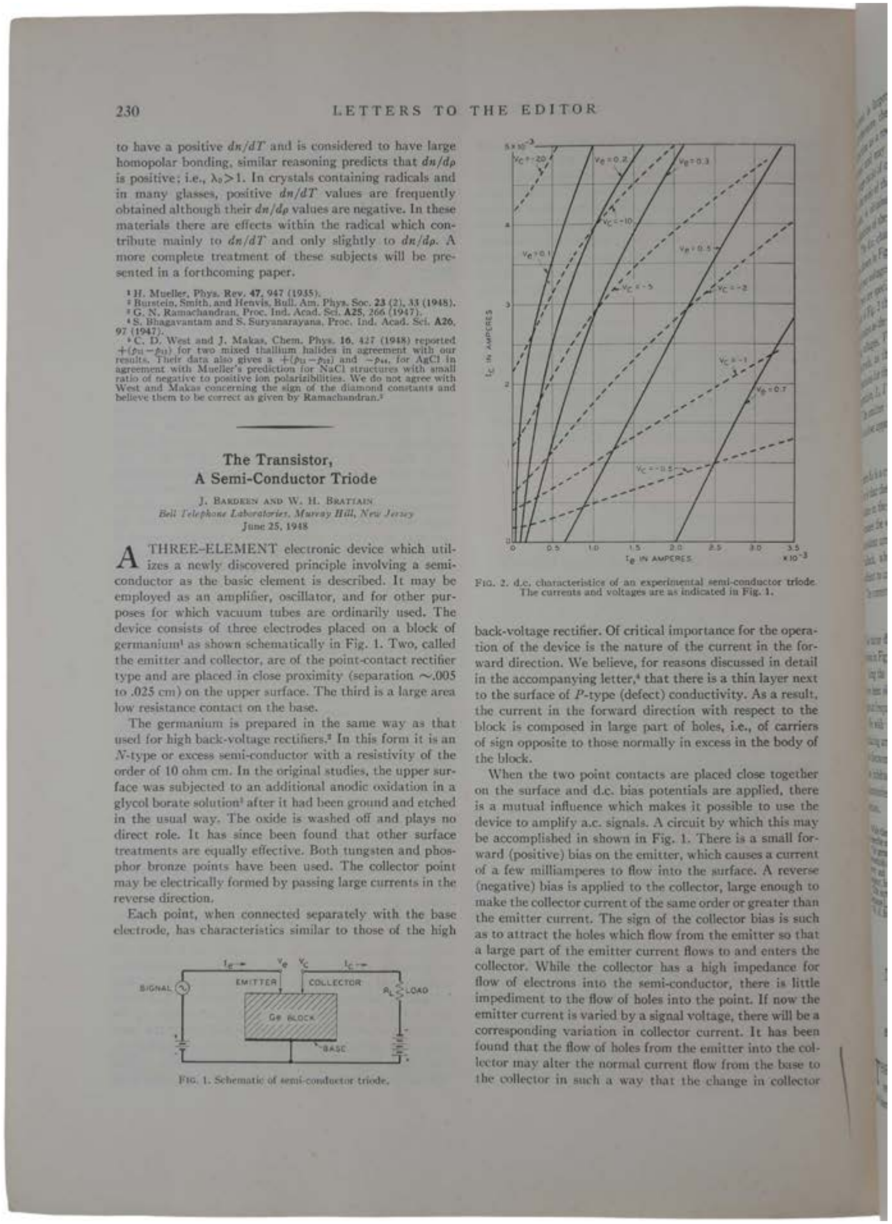
First edition, journal issues, documenting the invention of the transistor, "which has been called 'the most important invention of the 20th Century.' Developed from semiconductor material, the transistor was the first device that could both amplify an electrical signal, as well as turn it on and off, allowing current to flow or to be blocked. It was small in size, generated very low heat, and was very dependable, making possible a breakthrough in the miniaturization of complex circuitry. The transistor heralded in the 'Information Age' and paved the way for the development of almost every electronic device, from radios to computers to space shuttles. For their monumental 'researches on semiconductors and their discovery of the transistor effect,' Bardeen, Shockley and Brattain were presented with the Nobel Prize in Physics in 1956 “for their researches on semiconductors and their discovery of the transistor effect”.

“The genesis of the transistor emanates, interestingly enough, from a marketing problem. In the early part of the 20th Century, AT&T was engrossed in expanding its telephone service across the continent in an effort to beat the competition. The company turned to its research and development arm, Bell Laboratories, to develop innovations to meet this need.

“At the time, telephone technology was based on vacuum tubes, which were essentially modified light bulbs that controlled electron flow, allowing for current to be amplified. But vacuum tubes were not very reliable, and they consumed too much power and produced too much heat to be practical for AT&T’s needs. Furthermore, as scientists at Bell Labs discovered, transcontinental telephone communication required the use of ultrahigh frequency waves and the vacuum tubes were incapable of picking up rapid vibrations.

“An all-star team of scientists was assembled at Bell Labs to develop a replacement for the vacuum tubes based on solid-state semiconductor materials. Shockley, who had received his Ph.D. in physics from the Massachusetts Institute of Technology in 1936 and joined Bell Labs the same year, was selected as the team leader. He recruited several scientists for the project, including Brattain and Bardeen.

“Walter Brattain had been working for Bell Labs since 1929, the year he received his Ph.D. in physics from the University of Minnesota. His main research interest was on the surface properties of solids.John Bardeen was a theoretical physicist with an industrial engineering background. With a Ph.D. in physics from Princeton University, he was working as an assistant professor at the University of Minnesota when Shockley invited him to join the group.


230 LETTERS TO THE EDITOR

to have a positive dn/dT and is considered to have large homopolar bonding, similar reasoning predicts that $dn/d\rho$ is positive; i.e., $\lambda_0>1$. In crystals containing radicals and in many glasses, positive dn/dT values are frequently obtained although their $dn/d\rho$ values are negative. In these materials there are effects within the radical which contribute mainly to dn/dT and only slightly to $dn/d\rho$. A more complete treatment of these subjects will be presented in a forthcoming paper.

[1] H. Mueller, Phys. Rev. **47**, 947 (1935).
[2] Burstein, Smith, and Henvis, Bull. Am. Phys. Soc. **23** (2), 33 (1948).
[3] G. N. Ramachandran, Proc. Ind. Acad. Sci. **A25**, 266 (1947).
[4] S. Bhagavantam and S. Suryanarayana, Proc. Ind. Acad. Sci. **A26**, 97 (1947).
[5] C. D. West and J. Makas, Chem. Phys. **16**, 427 (1948) reported $+(p_{11}-p_{12})$ for two mixed thallium halides in agreement with our results. Their data also gives a $+(p_{11}-p_{12})$ and $-p_{44}$, for AgCl in agreement with Mueller's prediction for NaCl structures with small ratio of negative to positive ion polarizibilities. We do not agree with West and Makas concerning the sign of the diamond constants and believe them to be correct as given by Ramachandran.[3]

The Transistor, A Semi-Conductor Triode

J. Bardeen and W. H. Brattain
Bell Telephone Laboratories, Murray Hill, New Jersey
June 25, 1948

A THREE-ELEMENT electronic device which utilizes a newly discovered principle involving a semiconductor as the basic element is described. It may be employed as an amplifier, oscillator, and for other purposes for which vacuum tubes are ordinarily used. The device consists of three electrodes placed on a block of germanium[1] as shown schematically in Fig. 1. Two, called the emitter and collector, are of the point-contact rectifier type and are placed in close proximity (separation ~.005 to .025 cm) on the upper surface. The third is a large area low resistance contact on the base.

The germanium is prepared in the same way as that used for high back-voltage rectifiers.[2] In this form it is an N-type or excess semi-conductor with a resistivity of the order of 10 ohm cm. In the original studies, the upper surface was subjected to an additional anodic oxidation in a glycol borate solution[3] after it had been ground and etched in the usual way. The oxide is washed off and plays no direct role. It has since been found that other surface treatments are equally effective. Both tungsten and phosphor bronze points have been used. The collector point may be electrically formed by passing large currents in the reverse direction.

Each point, when connected separately with the base electrode, has characteristics similar to those of the high back-voltage rectifier. Of critical importance for the operation of the device is the nature of the current in the forward direction. We believe, for reasons discussed in detail in the accompanying letter,[4] that there is a thin layer next to the surface of P-type (defect) conductivity. As a result, the current in the forward direction with respect to the block is composed in large part of holes, i.e., of carriers of sign opposite to those normally in excess in the body of the block.

When the two point contacts are placed close together on the surface and d.c. bias potentials are applied, there is a mutual influence which makes it possible to use the device to amplify a.c. signals. A circuit by which this may be accomplished in shown in Fig. 1. There is a small forward (positive) bias on the emitter, which causes a current of a few milliamperes to flow into the surface. A reverse (negative) bias is applied to the collector, large enough to make the collector current of the same order or greater than the emitter current. The sign of the collector bias is such as to attract the holes which flow from the emitter so that a large part of the emitter current flows to and enters the collector. While the collector has a high impedance for flow of electrons into the semi-conductor, there is little impediment to the flow of holes into the point. If now the emitter current is varied by a signal voltage, there will be a corresponding variation in collector current. It has been found that the flow of holes from the emitter into the collector may alter the normal current flow from the base to the collector in such a way that the change in collector

Fig. 1. Schematic of semi-conductor triode.

Fig. 2. d.c. characteristics of an experimental semi-conductor triode. The currents and voltages are as indicated in Fig. 1.

"The team commenced work on a new means of current amplification. In 1945, Shockley designed what he hoped would be the first semiconductor amplifier, an apparatus that consisted of "a small cylinder coated thinly with silicon, mounted close to a small, metal plate". The device didn't work, and Shockley assigned Bardeen and Brattain to find out why.

"In 1947, during the so-called 'Miracle Month' of November 17 to December 23, Brattain and Bardeen performed experiments to determine what was preventing Shockley's device from amplifying. They noticed that condensation kept forming on the silicon. Could this be the deterrent? Brattain submerged the experiment in water "inadvertently creating the largest amplification thus far." Bardeen was emboldened by this result, and suggested they modify the experiment to include a [gold] metal point that would be pushed into the silicon surrounded by distilled water. At last there was amplification, but disappointingly, at a trivial level.

"But the scientists were galvanized by the meager result, and over the next few weeks, experimented with various materials and set ups. They replaced the silicon with germanium, which resulted in amplification 330 times larger than before. But it only functioned for low frequency currents, whereas phone lines, for example, would need to handle the many complicated frequencies of the human voice.

"Next, they replaced the liquid with a layer of germanium dioxide. When some of the oxide layer accidentally washed away, Brattain continued the experiment shoving the gold point into the germanium and *voila*! Not only could he still achieve current amplification, but he could do so at all frequencies. The gold contact had put holes in the germanium and the punctures 'canceled out the effect of the electrons at the surface, the same way the water had.' Their invention was finally increasing the current at all frequencies.

"Bardeen and Brattain had achieved two special results: the ability to get a large amplification at some frequencies, and a small amplification for all frequencies. Their goal now was to combine the two. The essential components of the device thus far were the germanium and two gold point contacts that were fractions of a millimeter apart. With this in mind, Brattain placed a gold ribbon around a plastic triangle, and cut it through one of the points. When the point of the triangle touched the germanium, electric current entered through one gold contact and increased as it rushed out the other. They had done it – it was the first point-contact transistor. On December 23, Shockley, Bardeen and Brattain presented their "little plastic triangle" to the Bell Labs VIPs and it became official: the super star team had invented the first working solid state amplifier.

"Following the triumph of the transistor, the three amplifying architects went their separate ways. Shockley left Bell Labs in 1955 to become the Director of the "Shockley Semi-Conductor Laboratory of Beckman Instruments, Inc. in Mountain View, Ca. His company was one of the first of its kind in Northern California and quickly attracted more semiconductor labs and related computer firms to the area. Soon the region had a new moniker: Silicon Valley.

"Bardeen left Bell Labs in 1951 for a professorial appointment in electrical engineering and physics at the University of Illinois. He was named a member of the Center for Advanced Study of the University in 1959. He continued his research in solid state physics and in 1972 shared a second Nobel Prize in physics for the first successful explanation of superconductivity.

"Brattain remained at Bell Labs and received various honorary degrees and awards for his work, including being named a Fellow of the APS, the American Academy of Arts and Sciences and the American Association for the Advancement of

Science” (aps.org/programs/outreach/history/historicsites/transistor.cfm).

The first announcement of the invention of the transistor, ‘The transistor, a semi-conductor triode,’ appeared in the July 15, 1948 issue of *Physical Review*. This was followed by a detailed account, ‘Physical principles involved in transistor action,’ in the same journal in April of the following year (and slightly later in the *Bell System Technical Journal*).

“After Bardeen and Brattain’s December 1947 invention of the point-contact transistor, Bell Labs physicist William Shockley began a month of intense theoretical activity. On January 23, 1948 he conceived a distinctly different transistor based on the p-n junction discovered by Russell Ohl in 1940. Partly spurred by professional jealousy, as he resented not being involved with the point-contact discovery, Shockley also recognized that its delicate mechanical configuration would be difficult to manufacture in high volume with sufficient reliability.

“Shockley also disagreed with Bardeen’s explanation of how their transistor worked. He claimed that positively charged holes could also penetrate through the bulk germanium material - not only trickle along a surface layer. Called “minority carrier injection,” this phenomenon was crucial to operation of his junction transistor, a three-layer sandwich of n-type and p-type semiconductors separated by p-n junctions. This is how all “bipolar” junction transistors work today.

“After William Shockley’s theories about p-n junctions had been validated by tests, fabricating a working junction transistor still presented formidable challenges. The main problem was lack of sufficiently pure, uniform semiconductor materials.

1210 J. BARDEEN AND W. H. BRATTAIN

Fig. 2. Microphotograph of a cutaway model of a transistor.

higher voltage so that a d.c. current of a few milliamperes flows out through the collector point and through the load circuit. It is found that the current in the collector circuit is sensitive to and may be controlled by changes of current from the emitter. In fact, when the emitter current is varied by changing the emitter voltage, keeping the collector voltage constant, the change in collector current may be larger than the change in emitter current. As the emitter is biased in the direction of easy flow, a small a.c. voltage, and thus a small power input, is sufficient to vary the emitter current. The collector is biased in the direction of high resistance and may be matched to a high resistance load. The a.c. voltage and power in the load circuit are much larger than those in the input. An over-all power gain of a factor of 100 (or 20 db) can be obtained in favorable cases.

Terminal characteristics of an experimental transistor[15] are illustrated in Fig. 3, which shows how the current-voltage characteristic of the collector is changed by the current flowing from the emitter. Transistor characteristics, and the way they change with separation between the points, with temperature, and with frequency, are discussed in Section II.

The explanation of the action of the transistor depends on the nature of the current flowing from the emitter. It is well known that in semiconductors there are two ways by which the electrons can carry electricity which differ in the signs of the effective mobile charges.[16] The negative carriers are excess electrons which are free to move and are denoted by the term conduction electrons or simply electrons. They have energies in the conduction band of the crystal. The positive carriers are missing or defect "electrons" and are denoted by the term "holes." They represent unoccupied energy states in the uppermost normally filled band of the crystal. The conductivity is called *n*- or *p*-type depending on whether the mobile charges normally in excess in the material under equilibrium conditions are electrons (negative carriers) or holes (positive carriers). The germanium used in the transistor is *n*-type with about 5×10^{14} conduction electrons per cc; or about one electron per 10^{8} atoms. Transistor action depends on the fact that the current from the emitter is composed in large part of *holes*; that is, of carriers of opposite sign to those normally in excess in the body of the semiconductor.

The collector is biased in the reverse, or negative direction. Current flowing in the germanium toward the collector point provides an electric field which is in such a direction as to attract the holes flowing from the emitter. When the emitter and collector are placed in close proximity, a large part of the hole current from the emitter will flow to the collector and into the collector circuit. The nature of the collector contact is such as to provide a high resistance barrier to the flow of electrons from the metal to the semiconductor, but there is little impediment to the flow of holes into the contact. This theory explains how the change in collector current might be as large as but not how it can be larger than the change in emitter current. The fact that the collector current may actually change more than the emitter current is believed to result from an alteration of the space charge in the barrier layer at the collector by the hole current flowing into the junction. The increase in density of space charge and in field strength make it easier for electrons to flow out from the collector, so that there is an increase in electron current. It is better to think of the hole current from the emitter as modifying the current-voltage characteristic of the collector, rather than as simply adding to the current flowing to the collector.

In Section III we discuss the nature of the conductivity of germanium, and in Section IV the theory of the current-voltage characteristic of a germanium-point contact. In the latter section we attempt to show why the emitter current is composed of carriers of opposite sign to those normally in excess in the body of germanium. Section V is concerned with some aspects of the theory of transistor action. A complete quantitative theory is not yet available.

There is evidence that the rectifying barrier in germanium is internal and occurs at the free

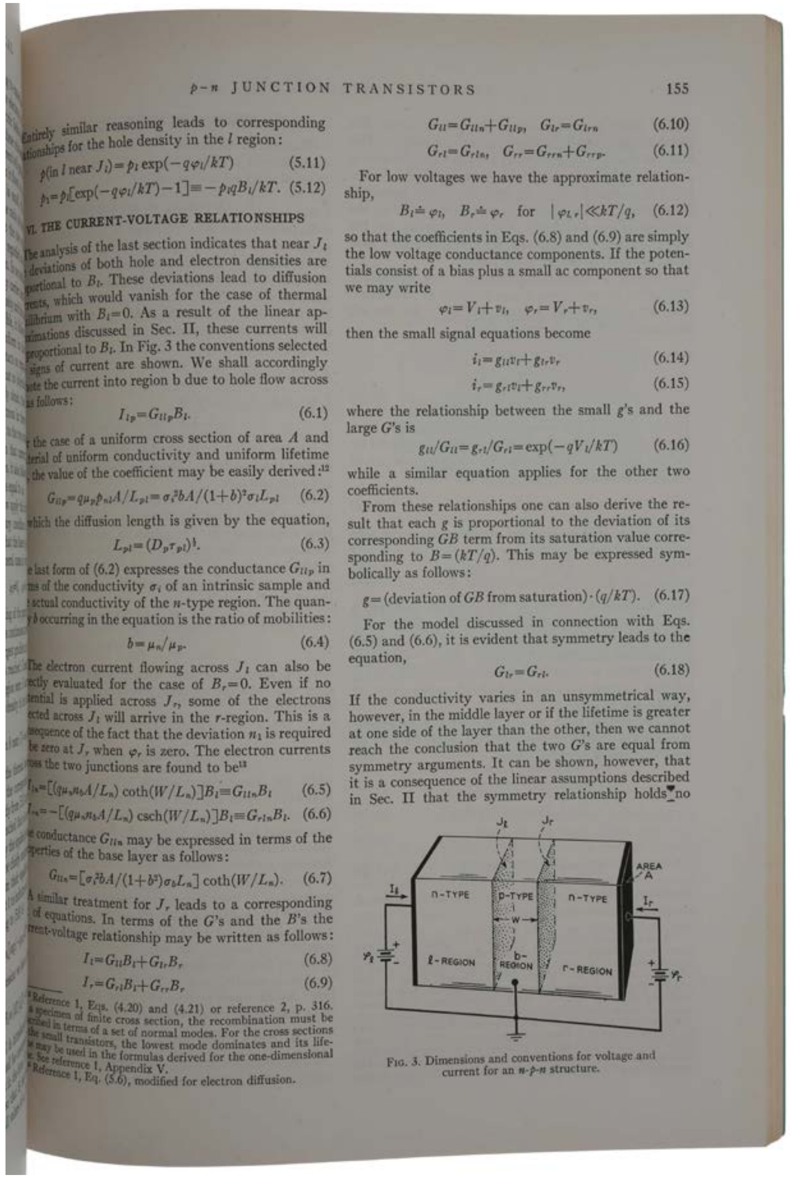
[15] The transistor whose characteristics are given in Fig. 3 is one of an experimented pilot production which is under the general direction of J. A. Morton.

[16] See, for example, A. H. Wilson, *Semi-Conductors and Metals* (Cambridge University Press, London, 1939) or F. Seitz, *The Modern Theory of Solids* (McGraw-Hill Book Company, Inc., New York, 1940), Sec. 68.

Bell Labs chemist Gordon Teal argued that large, single crystals of germanium and silicon would be required, but few – including Shockley – were listening. "With little support from management, Teal built the needed crystal-growing equipment himself, with help from mechanical engineer John Little and technician Ernest Buehler. Based on techniques developed in 1917 by the Polish chemist Jan Czochralski, he suspended a small 'seed' crystal of germanium in a crucible of molten germanium and slowly withdrew it, forming a long, narrow, single crystal. Shockley later called this achievement 'the most important scientific development in the semiconductor field in the early days.'

"Employing this technique, Bell Labs chemist Morgan Sparks fabricated p-n junctions by dropping tiny pellets of impurities into the molten germanium during the crystal-growing process. In April 1950, he and Teal began adding two successive pellets into the melt, the first with a p-type impurity and the second n-type, forming n-p-n structures with a thin inner, or base, layer. A year later, such 'grown-junction transistors'surpassed the best point-contact transistors in performance. Bell Labs announced this advance on July 4, 1951 in a press conference featuring Shockley" (computerhistory.org).

The construction of the first junction transistor was described by Shockley, Sparks & teal in their *Physical Review* paper 'p-n junction transistors.' This paper was not printed in the *Bell System Technical Journal*, but was reprinted in the proceedings of a symposium on transistors held at Bell Labs in the week beginning September 17, 1951.



p-n JUNCTION TRANSISTORS 155

...tirely similar reasoning leads to corresponding ...tionships for the hole density in the *l* region:

$$p(\text{in } l \text{ near } J_l)=p_l\exp(-q\varphi_l/kT) \quad (5.11)$$

$$p_l=p_l[\exp(-q\varphi_l/kT)-1]=-p_lqB_l/kT. \quad (5.12)$$

VI. THE CURRENT-VOLTAGE RELATIONSHIPS

The analysis of the last section indicates that near J_l ...deviations of both hole and electron densities are ...portional to B_l. These deviations lead to diffusion ...rents, which would vanish for the case of thermal ...ilibrium with $B_l=0$. As a result of the linear ap...imations discussed in Sec. II, these currents will ...proportional to B_l. In Fig. 3 the conventions selected ...signs of current are shown. We shall accordingly ...te the current into region b due to hole flow across ...as follows:

$$I_{lp}=G_{llp}B_l. \quad (6.1)$$

...the case of a uniform cross section of area A and ...terial of uniform conductivity and uniform lifetime ...the value of the coefficient may be easily derived:[12]

$$G_{llp}=q\mu_p p_{nl}A/L_{pl}=\sigma_i^2bA/(1+b)^2\sigma_lL_{pl} \quad (6.2)$$

...which the diffusion length is given by the equation,

$$L_{pl}=(D_p\tau_{pl})^{\frac{1}{2}}. \quad (6.3)$$

...e last form of (6.2) expresses the conductance G_{llp} in ...ms of the conductivity σ_i of an intrinsic sample and ...actual conductivity of the *n*-type region. The quan...*b* occurring in the equation is the ratio of mobilities:

$$b=\mu_n/\mu_p. \quad (6.4)$$

The electron current flowing across J_l can also be ...ctly evaluated for the case of $B_r=0$. Even if no ...tential is applied across J_r, some of the electrons ...cted across J_l will arrive in the *r*-region. This is a ...sequence of the fact that the deviation n_1 is required ...be zero at J_r when φ_r is zero. The electron currents ...oss the two junctions are found to be[13]

$$I_{ln}=[(q\mu_nn_bA/L_n)\coth(W/L_n)]B_l=G_{lln}B_l \quad (6.5)$$

$$I_{rn}=-[(q\mu_nn_bA/L_n)\,\mathrm{csch}(W/L_n)]B_l=G_{rln}B_l. \quad (6.6)$$

...e conductance G_{lln} may be expressed in terms of the ...perties of the base layer as follows:

$$G_{lln}=[\sigma_i^2bA/(1+b^2)\sigma_bL_n]\coth(W/L_n). \quad (6.7)$$

A similar treatment for J_r leads to a corresponding ...of equations. In terms of the G's and the B's the ...rent-voltage relationship may be written as follows:

$$I_l=G_{ll}B_l+G_{lr}B_r \quad (6.8)$$

$$I_r=G_{rl}B_l+G_{rr}B_r \quad (6.9)$$

[12] Reference 1, Eqs. (4.20) and (4.21) or reference 2, p. 316. ...a specimen of finite cross section, the recombination must be ...cribed in terms of a set of normal modes. For the cross sections ...the small transistors, the lowest mode dominates and its life... may be used in the formulas derived for the one-dimensional ... See reference 1, Appendix V.
[13] Reference 1, Eq. (5.6), modified for electron diffusion.

$$G_{ll}=G_{lln}+G_{llp},\quad G_{lr}=G_{lrn} \quad (6.10)$$

$$G_{rl}=G_{rln},\quad G_{rr}=G_{rrn}+G_{rrp}. \quad (6.11)$$

For low voltages we have the approximate relationship,

$$B_l\doteq\varphi_l,\quad B_r\doteq\varphi_r \quad \text{for} \quad |\varphi_{l,r}|\ll kT/q, \quad (6.12)$$

so that the coefficients in Eqs. (6.8) and (6.9) are simply the low voltage conductance components. If the potentials consist of a bias plus a small ac component so that we may write

$$\varphi_l=V_l+v_l,\quad \varphi_r=V_r+v_r, \quad (6.13)$$

then the small signal equations become

$$i_l=g_{ll}v_l+g_{lr}v_r \quad (6.14)$$

$$i_r=g_{rl}v_l+g_{rr}v_r \quad (6.15)$$

where the relationship between the small g's and the large G's is

$$g_{ll}/G_{ll}=g_{rl}/G_{rl}=\exp(-qV_l/kT) \quad (6.16)$$

while a similar equation applies for the other two coefficients.

From these relationships one can also derive the result that each g is proportional to the deviation of its corresponding GB term from its saturation value corresponding to $B=(kT/q)$. This may be expressed symbolically as follows:

$$g=(\text{deviation of } GB \text{ from saturation})\cdot(q/kT). \quad (6.17)$$

For the model discussed in connection with Eqs. (6.5) and (6.6), it is evident that symmetry leads to the equation,

$$G_{lr}=G_{rl}. \quad (6.18)$$

If the conductivity varies in an unsymmetrical way, however, in the middle layer or if the lifetime is greater at one side of the layer than the other, then we cannot reach the conclusion that the two G's are equal from symmetry arguments. It can be shown, however, that it is a consequence of the linear assumptions described in Sec. II that the symmetry relationship holds no

FIG. 3. Dimensions and conventions for voltage and current for an *n-p-n* structure.

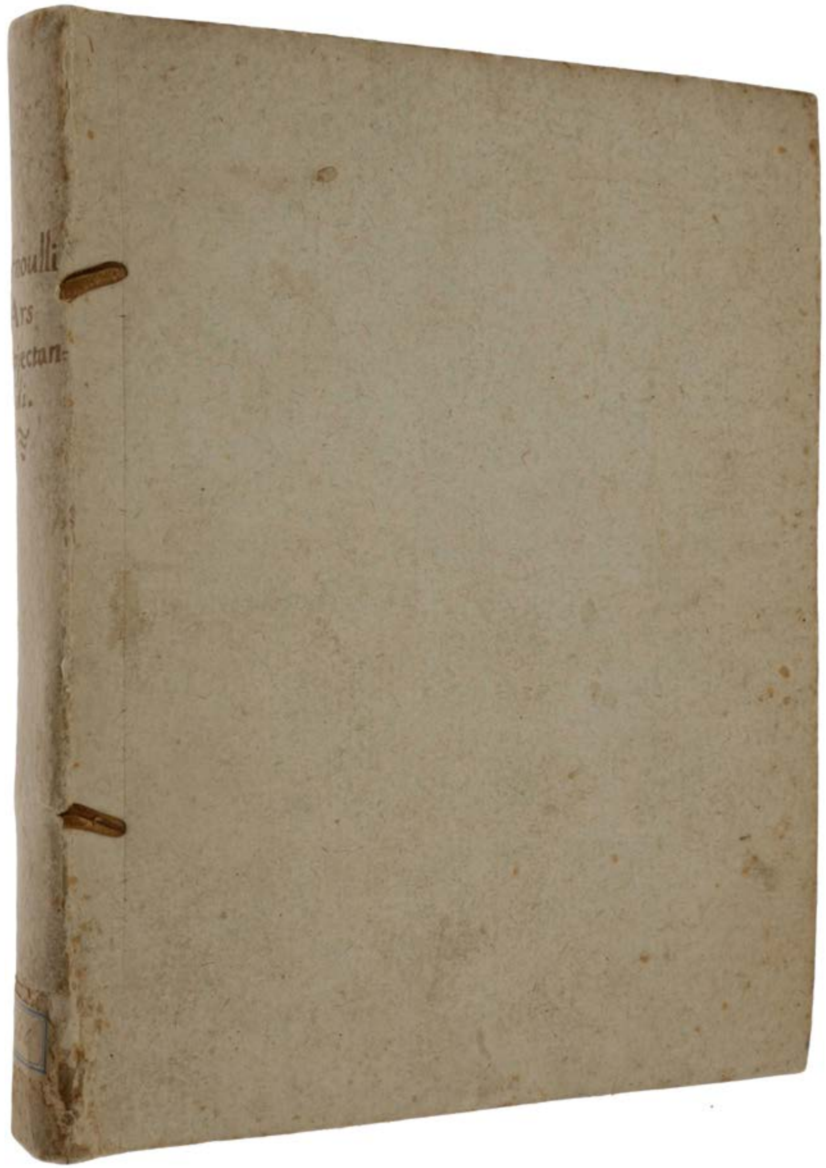
PMM 197 - AN EXCEPTIONALLY FINE COPY, UNCUT IN ORIGINAL BOARDS

BERNOULLI, Jakob. *Ars conjectandi, Opus posthumum. Accedit tractatus de seriebus infinitis, et epistola Gallicè scripta de ludo pilae reticularis.* Basel: Thurneisen Brothers, 1713.

$45,000

4to, pp. [iv], 306, [2, blank], 35, [1, errata], with woodcut title device, folding sheet with woodcut diagrams, two folding letterpress tables (a few leaves with unimportant browning). Uncut in the original boards, spine lettered in manuscript with small shelf-label at foot.

First edition, a truly exceptional copy, uncut in original boards. It is hard to imagine a finer copy. "Jakob 1 Bernoulli's posthumous treatise, edited by his nephew [Nicholas I Bernoulli], (the title literally means "the art of [dice] throwing") was the first significant book on probability theory: it set forth the fundamental principles of the calculus of probabilities and contained the first suggestion that the theory could extend beyond the boundaries of mathematics to apply to civic, moral and economic affairs. The work is divided into four parts, the first a commentary on Huygens's *De ratiociniis in ludo aleae* (1657), the second a treatise on permutations (a term Bernoulli invented) and combinations, containing the Bernoulli numbers, and the third an application of the theory of combinations to various games of chance. The fourth and most important part contains Bernoulli's philosophical thoughts on probability: probability as a measurable degree of certainty, necessity and chance, moral versus mathematical expectation, a priori and a posteriori probability, etc. It also contains his attempt to prove what is still called Bernoulli's Theorem: that if the number of trials is made large enough,



then the probability that the result will lie between certain limits will be as great as desired" (Norman). This was the first statement of the law of large numbers.

"In the first Part (pp. 2-71) Jakob Bernoulli complemented his reprint of Huygens's tract by extensive annotations which contained important modifications and generalisations. Bernoulli's additions to Huygens's tract are about four times as long as the original text. The central concept in Huygens's tract is expectation. The expectation of a player A engaged in a game of chance in a certain situation is identified by Huygens with his share of the stakes if the game is not played or not continued in a 'just' game. For the determination of expectation Huygens had given three propositions which constitute the 'theory' of his calculus of games of chance. Huygens's central proposition III maintains:

"If the number of cases I have for gaining a is p, and if the number of cases I have for gaining b is q, then assuming that all cases can happen equally easily, my expectation is worth $(pa + qb)/(p + q)$."

"Bernoulli not only gives a new proof for this proposition but also generalizes it in several ways …

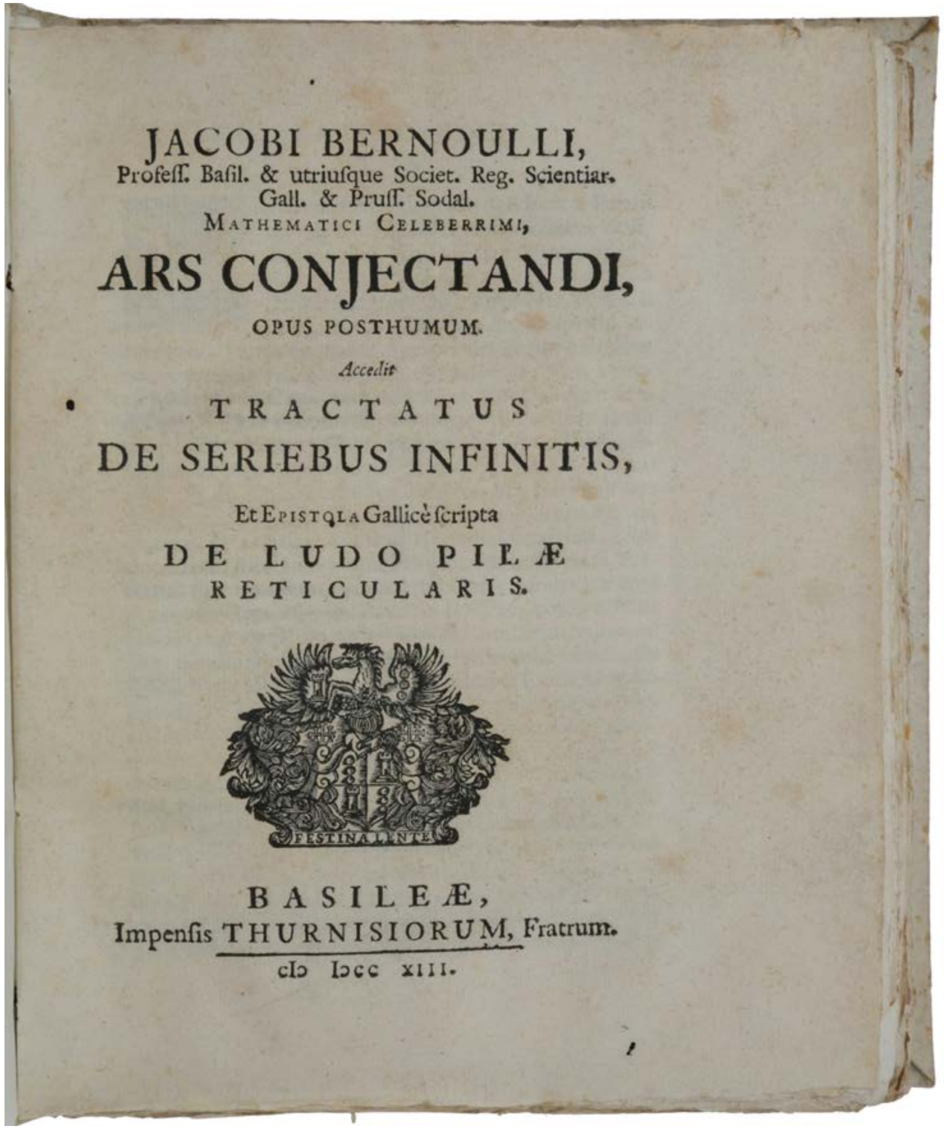
"Huygens's propositions IV to VII treat the problem of points, also called the problem of the division of stakes, for two players; propositions VIII and IX treat three and more players. Bernoulli returns to these problems in Part II of the *Ars Conjectandi*. In his annotations to Huygens's proposition IV he generalised Huygens's concept of expectation … This is the only instance in the annotations and commentaries to Huygens's tract where Bernoulli uses the word 'probabilitas', or probability as understood in everyday life. Later in Part IV of the *Ars Conjectandi* Bernoulli replaced Huygens's main concept, expectation, by the concept of probability for which he introduced the classical measure of favourable to all possible cases. The remaining propositions X to XIV of Huygens's tract deal with dicing problems of the kind: What are the odds to throw a given number of points with two or three dice? or: With how many throws of a die can one undertake it to throw a six or a double six? … The meaning of Huygens's result of proposition X, that the expectation of a player who contends to throw a six with four throws of a die is greater than that of his adversary, is explained by Bernoulli in a way which relates to the law of large numbers proved in Part IV of the *Ars Conjectandi* …

"In the second Part (pp. 72-137) Bernoulli deals with combinatorial analysis, based on contributions of van Schooten, Leibniz, Wallis, and Jean Prestet … [It] consists of nine chapters dealing with permutations, the number of combinations of all classes, the number of combinations of a particular class, figurate numbers and their properties (especially the multiplicative property), sums of powers of integers, the hypergeometric distribution, the problem of points for two players with equal chances to win a single game, combinations with repetitions and with restricted repetitions, and variations with repetitions and with restricted repetitions.

"Evidently Bernoulli did not know Blaise Pascal's *Triangle arithmétique,* published posthumously in 1665, though Leibniz had alluded to it in his last letter to him in 1705. Not only does Bernoulli not mention Pascal in the list of authors that he had consulted concerning combinatorial analysis, except for Pascal's letter to Fermat of 24 July 1654; it would also be difficult to explain why he repeated results already published by Pascal in the *Triangle arithmétique,* such as the multiplicative property for binomial coefficients for which Bernoulli claims the first proof for himself. His arrangement differs completely from that of Pascal, whose proof for the multiplicative property of the binomial coefficients has been judged to be clearer than Bernoulli's. It is fair to add that in the *Ars Conjectandi*, which Bernoulli left as an unpublished manuscript, he was much more honest concerning the achievements of his predecessors than Pascal in the *Triangle arithmétique*. It is also true that Bernoulli was concerned with combinatorial analysis in the *Ars Conjectandi* first of all because it constituted for him a most useful and indispensable universal instrument for dealing numerically with conjectures, since 'every conjecture is founded upon combinations of the effective causes' (p. 73) …

"In the third Part (pp. 138-209) Bernoulli gives 24 problems concerning the determination of the modified Huygenian concept of expectation in various games. Here he uses extensively conditional expectations without, however, distinguishing them from unconditional expectations. All the games are games of chance with dice and cards including games en vogue at the French court of the time like Cinque et neuf, Trijaques, or Basette. He solves these problems mainly by combinatorial methods, as introduced in Part II, and by recursion …

"[The fourth Part] (pp. 210-239) is the most interesting and original Part; but it is the one that Bernoulli was not able to complete. In the first three of its five chapters it deals with the new central concept of the art of conjecturing, probability, its relation to certainty, necessity and chance, and ways of estimating and measuring probability" (Schneider, pp. 92-100). "The relevant point for our analysis is his introduction in the fourth part of *Ars Conjectandi of* what has come to be regarded as the first law of large numbers. Bernoulli began the discussion leading up to his theorem by noting that, in games employing homogeneous dice with similar faces or urns with equally accessible tickets of different colors, the a priori determination of chances was straightforward. One would simply enumerate the possible cases and take the ratio of the number of 'fertile' cases to the total number of cases, whether 'fertile' or 'sterile.' But, Bernoulli asked, what about problems such as those involving disease, weather, or games of skill, where the causes are hidden and the enumeration of equally likely cases impossible?


JACOBI BERNOULLI,
Profeff. Bafil. & utriufque Societ. Reg. Scientiar. Gall. & Pruff. Sodal.
MATHEMATICI CELEBERRIMI,
ARS CONJECTANDI,
OPUS POSTHUMUM.
Accedit
TRACTATUS
DE SERIEBUS INFINITIS,
Et EPISTOLA Gallicè fcripta
DE LUDO PILÆ RETICULARIS.

BASILEÆ,
Impenfis THURNISIORUM, Fratrum.
clↄ Iↄcc XIII.

In such situations, Bernoulli wrote, "It would be a sign of insanity to attempt to learn anything in this manner." Instead, Bernoulli proposed to determine the probability of a fertile case a posteriori: "For it should be presumed that a particular thing will occur or not occur in the future as many times as it has been observed, in similar circumstances, to have occurred or not occurred in the past" (p. 224). The proportion of favorable or fertile cases could thus be determined empirically. Now this empirical approach to the determination of chances was not new with Bernoulli, nor did he consider it to be new. What was new was Bernoulli's attempt to give formal treatment to the vague notion that the greater the accumulation of evidence about the unknown proportion of cases, the closer we are to certain knowledge about that proportion.

"Bernoulli took it as commonly known that uncertainty decreased as the number of observations increased: "For even the most stupid of men, by some instinct of nature, by himself and without any instruction (which is a remarkable thing), is convinced that the more observations have been made, the less danger there is of wandering from one's goal" (p. 225). Bernoulli sought both to provide a proof of this principle and to show that there was no natural lower bound to the residual uncertainty: By multiplying the observations, 'moral certainty' about the unknown proportion could be approached arbitrarily closely" (Stigler, pp. 64-5).

The main work concludes with *Tractatus de seriebus infinitis earumque summa finite, et usu in quadraturis spatiorum & rectificationibus curvarum* (pp. 241-306), which had first appeared as a series of five extremely rare pamphlets entitled *Positiones arithmeticae de seriebus infinitis, earumque summa finita*. "The five dissertations in the *Theory of Series* (1682–1704) contain sixty consecutively numbered propositions. These dissertations show how Bernoulli (at first in close cooperation with his brother) had thoroughly familiarized himself with the appropriate formulations of questions to which he had been led by the conclusions of Leibniz in 1682 (series for [pi]/4 and log 2) and 1683 (questions dealing with compound interest). Out of this there also came the treatise in which Bernoulli took into account short-term compound interest and was thus led to the exponential series. He thought that there had been nothing printed concerning the theory of series up until that time, but he was mistaken: most conclusions of the first two dissertations (1689, 1692) were already to be found in Pietro Mengoli (*Novae quadraturae arithmeticae, seu de additione fractionum*, 1650), as were the divergence of the harmonic series (Prop. 16) and the sum of the reciprocals of infinitely many figurate numbers (Props. 17–20) … At the end of the first dissertation Bernoulli acknowledged that he could not yet sum [the inverse squares of the integers] in closed form (Euler succeeded in doing so first in 1737) … Informative theses, based on Bernoulli's earlier studies, were added to the dissertations: and theses 2 and 3 of the second dissertation are based on the still incomplete classification of curves of the third degree according to their shapes into thirty-three different types.

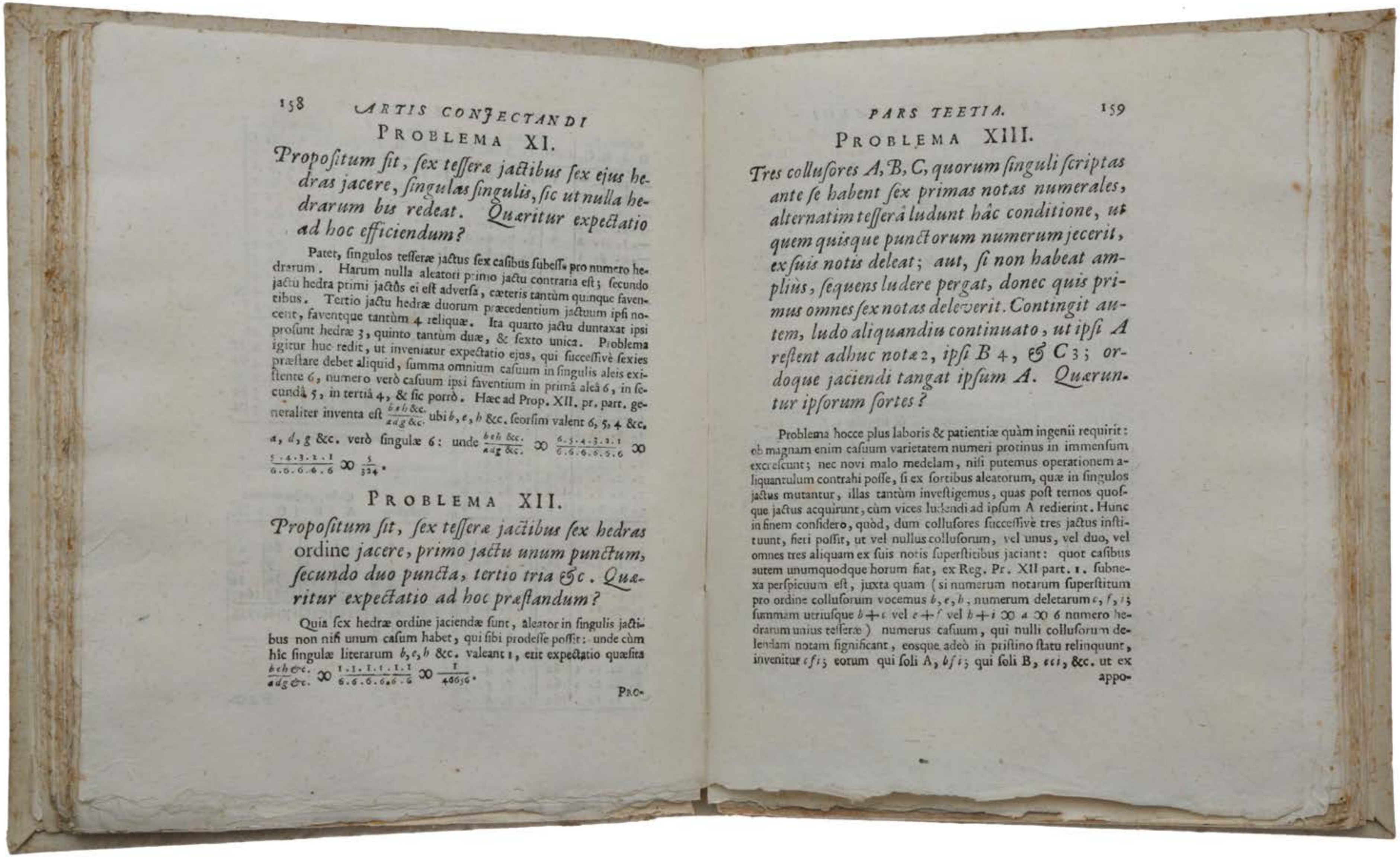
"The third dissertation was defended by Jakob Hermann, who wrote Bernoulli's obituary notice in *Acta eruditorum* (1706). In the introduction L'Hospital's *Analyse* is praised. After some introductory propositions, there appear the logarithmic series for the hyperbola quadrature (Prop. 42), the exponential series as the inverse of the logarithmic series (Prop. 43), … and the series for the arc of the circle and the sector of conic sections (Props. 45, 46). All of these are carefully and completely presented with reference to the pertinent results of Leibniz (1682; 1691). In 1698 previous work was supplemented by Bernoulli's reflections on the catenary (Prop. 49) and related problems, on the rectification of the parabola (Prop. 41), and on the rectification of the logarithmic curve (Prop. 52).

"The last dissertation (1704) was defended by Bernoulli's nephew, Nikolaus I, who helped in the publication of the *Ars conjectandi* (1713) and the reprint of the dissertation on series (1713) and became a prominent authority in the theory of series. In the dissertation Bernoulli first (Prop. 53) praises Wallis' interpolation through incomplete induction. In Proposition 54 the binomial theorem is presented, with examples of fractional exponents, as an already generally known theorem. Probably for this reason there is no reference to Newton's presentation in his letters to Leibniz of 23 June and 3 November 1676, which were made accessible to Bernoulli when they were published in Wallis' *Opera* (Vol. III, 1699)" (DSB).

The volume concludes with a separately-paginated 35-page *Lettre à un Amy, sur les Parties du Jeu de Paume* (in French). "In his *Letter to a Friend on the Game of Tennis,* Bernoulli begins with a summary of his considerations in the *Ars Conjectandi* on the difference between games of chance and games that depend on the skill of the players, on the corresponding determination of probabilities a priori and a posteriori, and on the law of large numbers, which justifies the use of the relative frequency of winning as a measure of the probability of winning. Apart from this short introduction, the letter is really an exercise in probability theory and could well have been included in Part 3 of the *Ars Conjectand.* "Bernoulli writes that he will not explain the rules of the game because they are well known. The game is more complicated than tennis but with the same scoring rules … Bernoulli analyzes many problems of tennis. There are, however, no new methods used in his analysis; he keeps strictly to the methods used by Huygens, solving most of the problems by recursion between expectations. The letter is an imposing work, demonstrating Bernoulli's pedagogical qualities, his ability to systematize, and his thoroughness" (Hald, p. 241).

"Important sections of the *Ars Conjectandi* were sketched out in Jakob Bernoulli's scientific diary, the 'Meditationes', from the mid 1680s onwards. When he died in 1705, the *Ars Conjectandi* was not finished, especially lacking good examples for the applications of his 'art of conjecturing' to what he described as civil and moral affairs. Concerning the time that it would have needed to complete it, opinions differ from a few weeks to quite a few years, depending on assumptions about his own understanding of completeness. His heirs did not want his brother Johann, the leading mathematician in Europe at this time, to complete and edit the manuscript, fearing that Johann would exploit his brother's work. Only after Pierre Rémond de Montmort (1678-1719), himself a pioneer of the theory of probability, had sent an offer via Johann to print the manuscript at his own expense in 1710, and after some admonitions that the *Ars conjectandi* soon would become obsolete if not published, Jakob's son, a painter, agreed to have the unaltered manuscript printed. It appeared in August 1713 … A short preface was contributed by Nikolaus Bernoulli (1687-1759), Jakob's nephew. He had read the manuscript when his uncle was still alive, and had made considerable use of it in his thesis of 1709 [*De usu artis conjectandi in jure*] and in his correspondence with Montmort. He was asked twice to complete and edit the manuscript. The first time he excused himself by his absence when he travelled in 1712 to Holland, England and France. After his return Nikolaus Bernoulli declared himself as too inexperienced to do the job and in his preface he asked Montmort, the anonymous author of the *Essay sur les jeux de hazard*, and Abraham de Moivre (1667-1754) to complete his uncle's work" (Schneider, p. 90).

Dibner 110; Evans 8; Horblit 12; Norman 216; Parkinson p. 140; PMM 179; Sparrow p. 21. *Hald, History of Probability and Statistics and their Applications before 1750*, 2003. Schneider, 'Jakob Bernoulli, *Ars Conjectandi* (1713)', pp. 88-104 in *Landmark Writings in Western Mathematics 1640-1940*, I. Grattan-Guinness (ed.), 2005. Stigler, *The History of Statistics*, 1986.



158 ARTIS CONJECTANDI

PROBLEMA XI.

Propositum sit, sex tesseræ jactibus sex ejus hedras jacere, singulas singulis, sic ut nulla hedrarum bis redeat. Quæritur expectatio ad hoc efficiendum?

Patet, singulos tesseræ jactus sex casibus subesse pro numero hedrarum. Harum nulla aleatori primo jactu contraria est; secundo jactu hedra primi jactûs ei est adversa, cæteris tantùm quinque faventibus. Tertio jactu hedræ duorum præcedentium jactuum ipsi nocent, faventque tantùm 4 reliquæ. Ita quarto jactu duntaxat ipsi prosunt hedræ 3, quinto tantùm duæ, & sexto unica. Problema igitur huc redit, ut inveniatur expectatio ejus, qui successivè sexies præstare debet aliquid, summa omnium casuum in singulis aleis existente 6, numero verò casuum ipsi faventium in primâ aleâ 6, in secundâ 5, in tertiâ 4, & sic porrò. Hæc ad Prop. XII. pr. part. generaliter inventa est $\frac{beh\,\&c.}{adg\,\&c.}$ ubi b, e, h &c. seorsim valent 6, 5, 4 &c. a, d, g &c. verò singulæ 6: unde $\frac{beh\,\&c.}{adg\,\&c.} \infty \frac{6.5.4.3.2.1}{6.6.6.6.6.6} \infty \frac{5.4.3.2.1}{6.6.6.6.6} \infty \frac{5}{324}$.

PROBLEMA XII.

Propositum sit, sex tesseræ jactibus sex hedras ordine *jacere, primo jactu unum punctum, secundo duo puncta, tertio tria &c. Quæritur expectatio ad hoc præstandum?*

Quia sex hedræ ordine jaciendæ sunt, aleator in singulis jactibus non nisi unum casum habet, qui sibi prodesse possit: unde cùm hic singulæ literarum b, e, h &c. valeant 1, erit expectatio quæsita $\frac{beh\,\&c.}{adg\,\&c.} \infty \frac{1.1.1.1.1.1}{6.6.6.6.6.6} \infty \frac{1}{46656}$.

PRO-

PARS TERTIA. 159

PROBLEMA XIII.

Tres collusores A, B, C, quorum singuli scriptas ante se habent sex primas notas numerales, alternatim tesserâ ludunt hâc conditione, ut quem quisque punctorum numerum jecerit, ex suis notis deleat; aut, si non habeat ampliùs, sequens ludere pergat, donec quis primus omnes sex notas deleverit. Contingit autem, ludo aliquandiu continuato, ut ipsi A restent adhuc notæ 2, ipsi B 4, & C 3; ordoque jaciendi tangat ipsum A. Quæruntur ipsorum sortes?

Problema hocce plus laboris & patientiæ quàm ingenii requirit: ob magnam enim casuum varietatem numeri protinus in immensum excrescunt; nec novi malo medelam, nisi putemus operationem aliquantulum contrahi posse, si ex sortibus aleatorum, quæ in singulos jactus mutantur, illas tantùm investigemus, quas post ternos quosque jactus acquirunt, cùm vices ludendi ad ipsum A redierint. Hunc in finem considero, quòd, dum collusores successivè tres jactus instituunt, fieri possit, ut vel nullus collusorum, vel unus, vel duo, vel omnes tres aliquam ex suis notis superstitibus jaciant: quot casibus autem unumquodque horum fiat, ex Reg. Pr. XII part. 1. subnexa perspicuum est, juxta quam (si numerum notarum superstitum pro ordine collusorum vocemus b, e, h, numerum deletarum c, f, i; summam utriusque $b+c$ vel $e+f$ vel $h+i \infty a \infty 6$ numero hedrarum unius tesseræ) numerus casuum, qui nulli collusorum delendam notam significant, eosque adeò in pristino statu relinquunt, invenitur cfi; eorum qui soli A, bfi; qui soli B, eci, &c. ut ex

appo-

ONE OF THE GREAT MACHINE BOOKS OF THE SEVENTEENTH CENTURY

BÖCKLER, Georg Andreas. *Theatrum Machinarum Novum...* Nuremberg: Christoff Gerhard for Paul Fürst, 1661.

$18,500

Folio (312 x 223 mm), pp. [xii], 68, including engraved title, and letterpress title printed in red and black, ornamental initials and head and tailpieces, with 154 engraved plates. Contemporary vellum with yapped edges.

First edition, an exceptionally fine copy in an untouched contemporary binding, of this superbly illustrated work with 154 plates of various types of powered mills and hydraulic machinery. Born in Cronheim ca. 1617, Böckler was an architect in Nuremberg. In addition to the present work he is the author of *Architectura Curiosa Nova* (Nuremberg, 1664), which dealt with the theory and application of hydrodynamics for water-jets, fountains and well heads with many designs for free-standing fountains. Böckler died in Ansbach in 1667.

"Here is another of the great 'machine' books with many beautiful engravings of gunpowder mills, saw mills, water raising devices, fire engines, roasting spits and so on. Böckler was a German architect and engineer interested in masses of gearing, complex workings, and devices that even by modern standards invite awe and admiration" (Hoover). The magnificent plates are of various types of motion drives powered by intricate systems of wheels employing water, wind, weights, horse power, human muscle, or some striking combinations of these. Plates 73 and 74 depict paper-making equipment and processes, which are "the

clearest delineation of the art to this date" (Hunter, *The Literature of Papermaking*, page 18): they show the linen rags being pulped with water-powered hammers, while the vatman stands ready with a mould, the coucher presses the post, and the drying sheets hang on ropes above, ready to be sized, calendared, gathered into reams, and packaged. Plate 5 pictures a hand mill for making ink for copperplate printing. The last plate (no. 154) is extraordinary for its depiction of a fire engine water pump made by the Nuremberg inventor Hans Hautsch in 1658. The suction-and-force mechanism of Hautsch's clever device (described on pp. 60-1 of Böckler's text) enabled twenty-four men to raise water to a height of eighty to one hundred feet in a continuous stream. Current historians of engineering view it as the basis of the modern fire engine. Among the other ingenious machines is a famous attempt at designing a perpetual motion machine (p. 59). However, Jakob (pp. 124-5) emphasizes that Böckler's work is at a higher technical level, and has far fewer 'miraculous machines' than other Baroque machine books, such as those of Agostino Ramelli and Salomon de Caus. Many of the engravings are familiar because they were plagiarized in technology publications for the next hundred years. Although the book appears with some regularity on the market, copies in such fine condition as ours are notably rare.

"It was in Germany that two of the most eminent engineering visionaries lived. The first of these was Georg Andreas Böckler who published a truly remarkable book … called *Theatrum Machinarum Novum*, written and illustrated as a record of the progress of the art of engineering.

"As one might gather, not just from the title page but from a knowledge of the general conditions pertaining in Germany after the Thirty Years' War, Böckler's acquaintance with machinery was restricted almost entirely to mills of one sort or another. In most of these, regardless of the motive power, he depicts the precursor of the geared transmission familiar to this day.

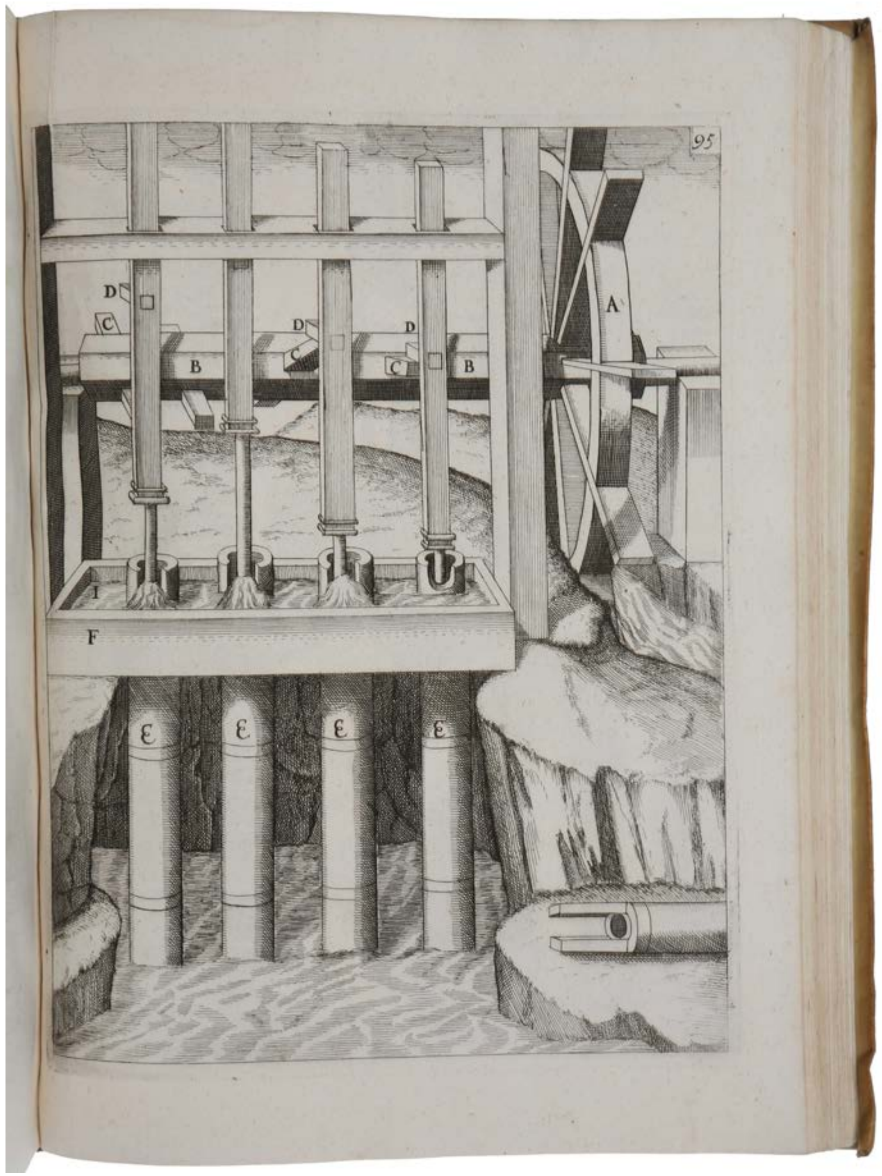
"The seventeenth-century engineers had neither the theoretical knowledge nor the technical equipment to design and shape gear-wheels which would mesh with minimum friction. In fact, friction as such, although made use of in such applications as the sack-lift in a mill, was little understood. The construction of pinions to mesh with larger gear-wheels had yet to assume the form common today. However, the problem of shifting the direction of rotation of a drive through 90° was solved by the invention of the *wallower* driven either by a contrite wheel (a wooden wheel with tooth pegs protruding around the circumference parallel to the axis) or by a cogwheel having teeth projecting radially around the circumference at right-angles to the axis … The wallower comprised two discs of wood, each drilled with a matching set of concentric holes near the perimeter. The discs, usually with square centre bores to aid fixing and to transmit rotary motion, were mounted on a shaft separate by a gap of as much as the mechanism dictated, and threaded through the holes, from one disc to the next, were wooden rods. The result looked not unlike a birdcage or lantern, hence its more common name. When it was meshed with a large wheel having suitably spaced wooden pegs around either its diameter (the contrite) or its periphery (the cog), depending on whether parallel or perpendicular motion was desired, a serviceable gear train was the result. Friction, though cut efficiency drastically. All this is depicted in Böckler's *Theatre of New Machines*" (Ord-Hume, *Perpetual Motion* (2015), pp. 47-8).

Among the many other remarkable mills illustrated in Böckler's Theatre, we mention Plate 72, which shows a 'fulling mill'. Fulling is a step in fabric production that involves pounding cloths to clean them. In the illustration, tappets, C, on the shaft, B, raise and lower mallets, D, which pound the woollen cloth. Other mills are used for grinding flour (Plates 37, 41, 46, 47, 59), for sharpening stones (Plate 34), and for sharpening tools and knives (Plate 36).

"The continuous rotary motion produced by the vertical water wheel was applied to both working wood and rolling and cutting metals during the medieval period, as well as to milling and grinding … The rotary motion of the water wheel may also have been used in the medieval period for other tasks. There is evidence … of water-powered chain-of-bucket or rag-and-chain pumps for draining mines" (Reynolds, *Stronger Than a Hundred Men*: *A History of the Vertical Water Wheel* (2002), pp. 76-8). This last application is illustrated on Plate 116.

"Probably the most widely applied water-powered innovation in the food processing industry in this period was the mechanical bolter, used in flour mills to automatically sift flour. The bolter was basically a sheet or roll of wire mesh or cloth (most often canvas or linen, but sometimes silk or another fabric). The flour produced by the mill was fed through or over the device, which was shaken by a mechanism (several were possible) taking power from the drive train leading from the water to the millstones … The bolter depicted by Böckler [on Plate 45] indicates just how simple the device could sometimes be" (*ibid*, p. 138). Other devices powered by water wheels are an irrigation pump (Plates 35, 95, 98, 109, 110), a forge (Plate 79) with bellows and hammer powered by an undershot water wheel, and a corn mill (Plates 45, 46, 47).

The key breakthrough in fire-fighting arrived in the seventeenth century with the first fire engines. Manual pumps, rediscovered in Europe after 1500, were only force pumps and had a very short range due to the lack of hoses. Hans Hautsch (1595-1670) improved the manual pump by creating the first suction and force pump and adding some flexible hoses to the pump. His fire-engine consisted of a water cistern about 8 feet long, 4 feet high, and 2 feet in width, and was drawn on a kind of sledge somewhat larger than the cistern. It was worked by 28 men, and a stream of water an inch in diameter was forced, by means of this engine, to an elevation of nearly 80 feet. This remarkable machine is illustrated on Plate 154.



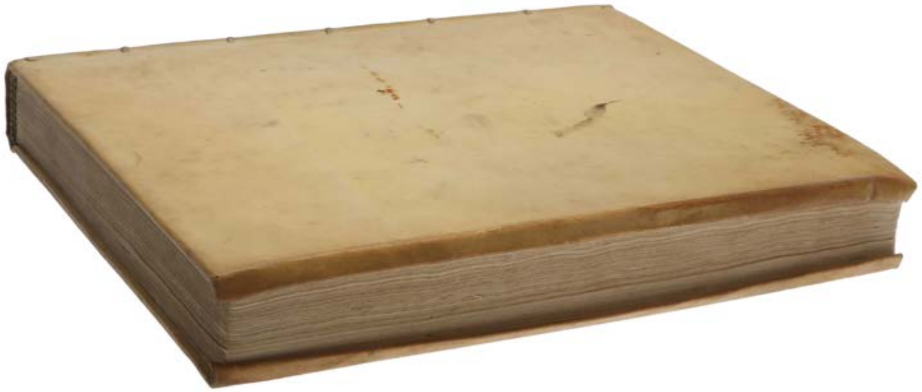
A recurring theme in the book is the perpetual motion machine. "The use of water-power seems particularly prone to implant the idea in the human mind. This is probably attributable to the assumption that water comes from nowhere in particular and costs Man nothing. This deludes the miller into assuming that his power costs him nothing by concealing the fact that his power is bought and paid for in terms of units of energy and that it can be delivered to him but once. In any event, it would seem that the proprietor of a water-mill – especially of one whose driving stream was subject to seasonal diminutions of flow – was forever trying to make his water run back uphill and work for him again. Later and wiser mill engineers accumulated their energy when it was plentiful by constructing mill-ponds with sluice gates so that when the natural water flow was diminished, reserves could be drawn upon which did not defy the laws of Nature.

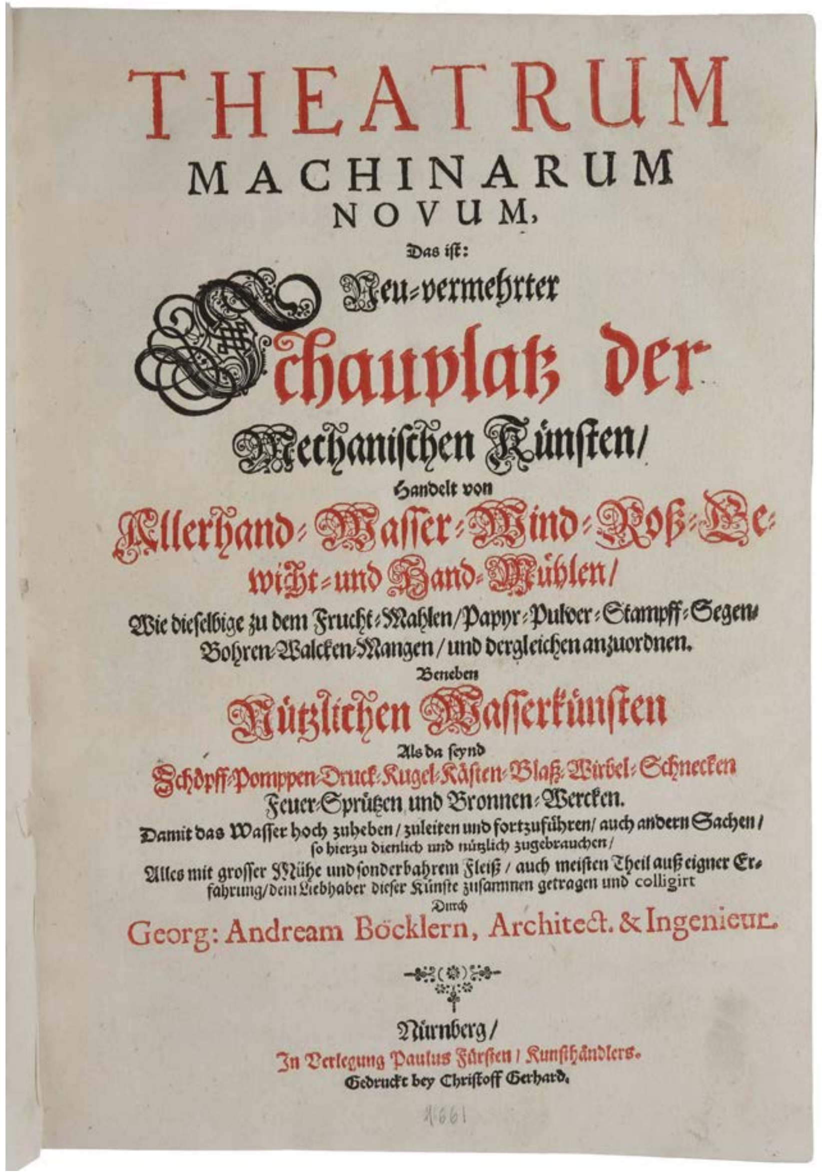
"Unfortunately for the peace of the medieval mind, it knew of at least one highly plausible scheme for making water run uphill. If the end of a pipe, coiled like the thread of a screw, is immersed in water, and the whole pipe rotated like a screw, the water will climb up the pipe and keep on climbing so long as the pipe is kept turning. This strange but perfectly workable invention is called an Archimedean screw … What we know and understand about the Archimedean screw is that the pipe must be turned by some outside agency. This illuminating piece of information was not understood by our ancestors who, with glinting eyes, asked 'What could be more simple than to connect such an Archimedean screw with the water-wheel of a mill, and make the mill run the screw, and the screw run the mill?' To Böckler, as to so many others both before and after his time, the answer was that nothing indeed was more simple. Böckler's mills, which he illustrated in plenitude, all worked on this principle … One notable feature of the Archimedean perpetual motion machine depicted here is the shape of the motive blades which bear a strong resemblance to the modern turbine" (Ord-Hume, pp. 48-50). This Archimedean screw arrangement is beautifully illustrated on Plate 54, which shows an overshot water wheel that powers a grindstone and an Archimedes screw that returns the water to the reservoir driving the wheel.

"Another means for making a mill raise the water for its own power which Böckler used in several instances consisted of a series of cups attached to an endless rope. The cups were expected to re-deliver the water direct to the wheel. But it was the self-moving wheel that appeared the most plausible. Here, with an apparent permanent preponderance of weights on one side of the wheel, once motion was started it seemed obvious that the wheel would not only rotate continuously but would generate enough power to pump water" (*ibid*., p. 50). This perpetual motion machine is illustrated on Plate 130: the water wheel, C, through the gearing network D, E, F G, H, I, drove a chain-of-buckets pump K which lifted water to reservoir A, from which the water wheel derived its power. As Knoespel points out (p. 110), a comparison of Böckler' work with earlier machine books indicates the transformation then underway in the status of human beings relative to that of machines, "for the focal point within the illustrations has shifted from the human to the mechanical … Böckler shows a more developed awareness of how machines alter the status of workers was well as owners in his seventeenth-century illustrations. In one, a curtain has been drawn aside to reveal a couple at leisure eating dinner while the mill, tended by workers, operates on the floor beneath them (plate 53). Here the machine begins to determine the roles played by humans. The various illustrations portraying humans attentively watching a machine's operation indicate even further the centrality of the apparatus in the drama being portrayed."

Böckler's *Theatre* proved extremely popular, with later German editions appearing in 1673, 1703 and 1705, and Latin translations in 1662 and 1686.

Graesse I, 459; Stanitz 46; Zachert/Zeidler I, 220; [for the 1662 Latin edition see:] Macclesfield 2195; Horblit 132; Honeyman 359. Singer, *History of Technology* III.16-17. Hoover, *Bibliotheca De re Metallica*, 142 (for the 1686 Latin edition). Jakob, *Maschine. Mentales Modell und Metapher. Studien zur Semantik und Geschichte der Techniksprache*, 1991; Knoespel, 'Gazing on Technology: Theatrum Mechanorum and the Assimilation of Renaissance Machinery,' *Literature and Technology* (Greenberg & Schachterle, eds.), 1992, pp. 99-124.

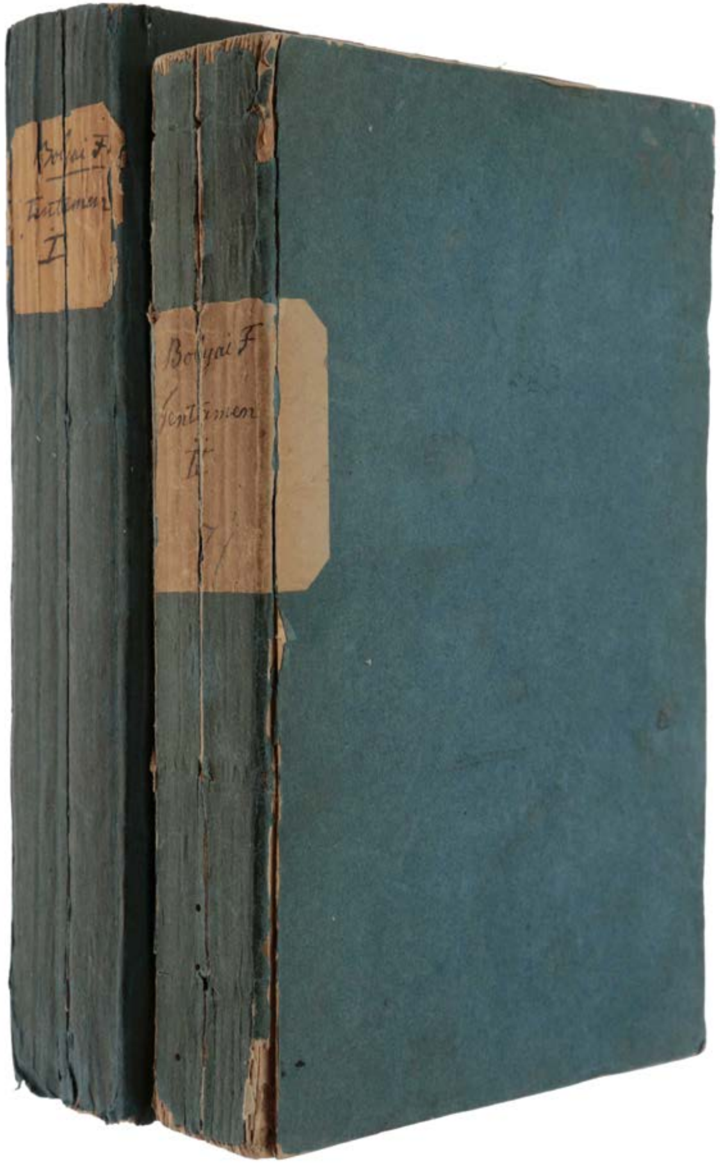




THEATRUM
MACHINARUM
NOVUM,
Das ist:
Neu-vermehrter
Schauplatz der
Mechanischen Künsten/
handelt von
Allerhand-Wasser-Wind-Roß-Ge-
wicht-und Hand-Mühlen/
Wie dieselbige zu dem Frucht-Mahlen/Pappyr-Pulver-Stampff-Segen-
Bohren-Walcken-Mangen/ und dergleichen anzuordnen.
Beneben
Nützlichen Wasserkünsten
Als da seynd
Schöpff-Pomppen-Druck-Kugel-Kästen-Blaß-Wirbel-Schnecken
Feuer-Sprützen und Bronnen-Wercken.
Damit das Wasser hoch zuheben/ zuleiten und fortzuführen/ auch andern Sachen/
so hierzu dienlich und nützlich zugebrauchen/
Alles mit grosser Mühe und sonderbahrem Fleiß / auch meisten Theil auß eigner Er-
fahrung/dem Liebhaber dieser Künste zusammen getragen und colligirt
Durch
Georg: Andream Böcklern, Architect. & Ingenieur.

Nürnberg/
In Verlegung Paulus Fürsten / Kunsthändlers.
Gedruckt bey Christoff Gerhard.
1661

'THE MOST EXTRAORDINARY TWO DOZEN PAGES IN THE HISTORY OF THOUGHT'



BOLYAI, János. *Appendix. Scientiam Spatii Absolute Veram exhibens: a veritate aut falsitate Axiomatis XI Euclidei (a priori haud unquam decidenda) independentem: adjecta ad casum falsitatis, quadratura circuli geometrica. [in:]BÓLYAI, Farkas. Tentamen Juventutem Studiosam in Elementa Matheseos Purae. Tomus primus [-secundus].* Maros Vásárhelyini: Joseph and Simon Kaili, at the press of the Reform College, 1832-1833.

$240,000

2 vols, 8vo (I: 228 x 145 mm; II: 214 x 125 mm), I: pp [iv] XCVIII; 502; [ii] 26 [2, errata] XVI [subscriber's list and Latin-Hungarian lexicon of mathematical terms], with one large folding letterpress table, and 4 folded engraved plates (plate 3 with 7 small folding slips); II: pp [vi] xvi [Index Tom II] 402, with 10 folded engraved plates (plate 7 with 10 slips, plate 8 with 4 slips, plate 9 with 3 slips and plate 10 with 5 slips and 1 volvelle), manuscript corrections to line 6 of p 380 vol II; vol II with some worming to inner blank margins and text in several gatherings, affecting some letters but still legible, also affecting first four plates in same vol, just touching some of the figures, some paper flaws as often; first volume uncut, second with some outer edges uncut, together in uniform contemporary blue boards, paper labels on spines, spines and joints cracked but sound, preserved in a morocco box. Provenance: stamp of the publisher, press of the Reformed College, Maros Vásárhely (now the Romanian city of Târgu Mures) on free flyleaf and title of first volume.

First edition of 'the most extraordinary two dozen pages in the history of thought'

(Halsted) and one of the few absolute rarities among the classics of science. This work contains the independent foundation (along with the work of Lobachevsky) of non-Euclidean geometry. 'I have located some 23 other copies worldwide, all of them exhibiting variations in issue or completeness (the present copy represents the most complete state of the text for both volumes).' (William Patrick Watson).

Lobachevsky and János Bolyai had independently created non-Euclidean systems by challenging the 'parallel postulate' of Euclid. János Bolyai's work was conceived in 1823, when he wrote to his father 'I have now resolved to publish a work on the theory of parallels ... I have created a new universe from nothing'. It was published as an appendix to his father's mathematical treatise, the *Tentamen*, 1832–3. Lobachevsky's work appeared in a Kazan academic periodical between 1829–1830, and in fuller form as *Geometrische Untersuchungen*, Berlin 1840. Whereas Lobachevsky initially had only demonstrated the possibility of a geometry in which Euclid's fifth postulate (or 11th axiom) was untrue, János developed a geometry completely independent of the fifth postulate and applicable to varieties of curved space. However, the epochal significance of the work of these two was to remain largely unappreciated until the beginning of the twentieth century when it provided the mathematical basis for the Theory of Relativity.

János began working on his new geometry early in the 1820s. His father tried to discourage him from attempting to prove or refute Euclid's fifth postulate:

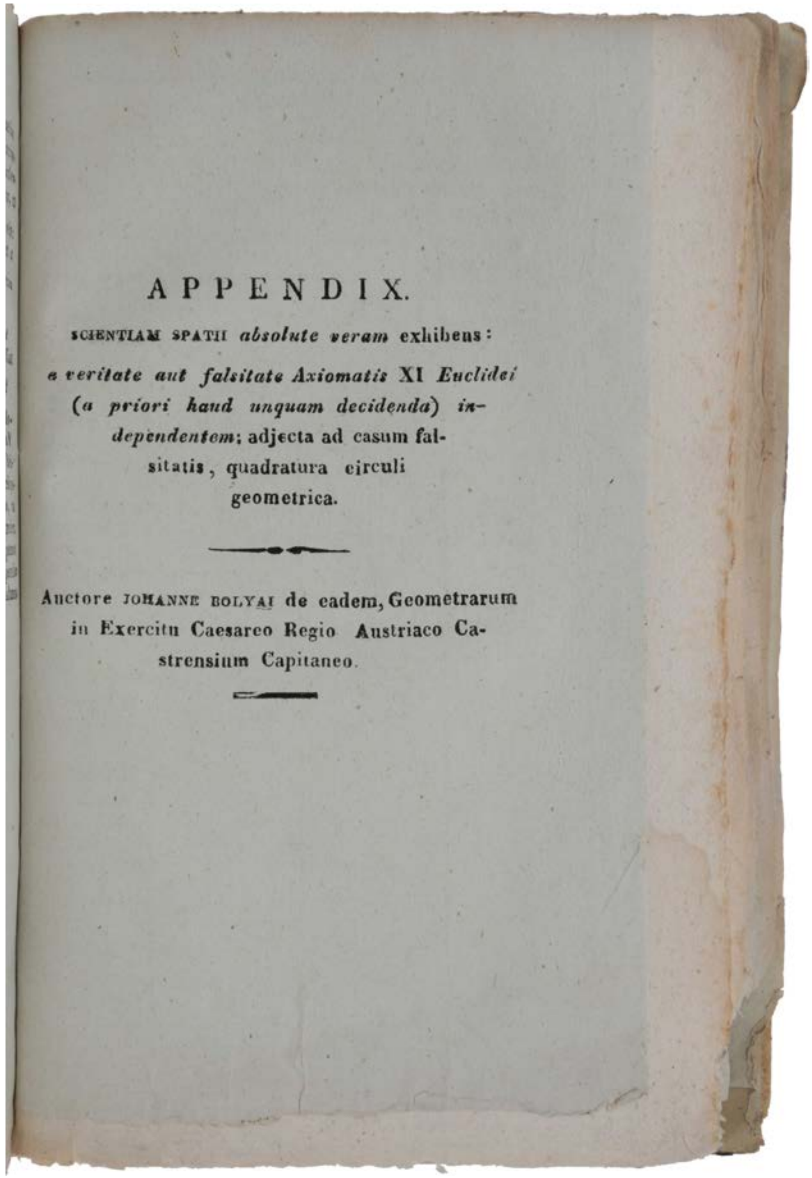
'You should not tempt the parallels in this way. I know this way until its end – I also have measured this bottomless night, I have lost in it every light, every joy of my life ... You should shy away from it as if from lewd intercourse, it can deprive you of all your leisure, your health, your peace of mind and your entire happiness. This infinite darkness might perhaps absorb a thousand giant Newtonian towers, it will never be light on earth, and the miserable human race will never have something absolutely pure, not even geometry' (quoted from DSB). Farkas Bolyai, however diffidently he felt about his son's researches, did send the manuscript of the *Appendix* to Gauss: the first letter went unanswered, and a second letter only elicited the reply that Gauss could not praise it, because he himself had reached the same conclusions some 30 years earlier although he had not published his discovery! This assertion so discouraged János that it effectively terminated his career in creative mathematics, but his father did publish his paper as an appendix to his own textbook. Published in this form, in a small edition (the two lists of subscribers give some 79 names accounting for 156 copies), by an obscure Hungarian college publisher, in a small town in Transylvania, the work was guaranteed immediate oblivion. It remained a forgotten masterpiece until, 35 years later, Riemann's paper on the hypotheses of geometry reawakened interest in this field, with profound consequences for the mathematical description of real space.

In the rediscovery of János's masterpiece, the father's work was largely neglected. Farkas Bolyai was a close friend of Gauss and regarded by the latter as the only man who fully understood Gauss's metaphysics of mathematics. 'He can be taken as a precursor of Gottlob Frege, Pasch, and Georg Cantor; but, as with many pioneers, he did not enjoy the credit that accrued to those that followed him' (DSB). He had worked on the parallel postulate and the possibilities of a non-Euclidean geometry from his earliest days as a mathematician in Göttingen, and had corresponded with Gauss on the subject, even sending him a manuscript entitled *Theoria parallelarum*, but it was his son János who was to achieve the breakthrough. The *Appendix* appears at the end of volume one, and is separately paginated [ii] 26 [2, errata], and with one plate in the volume specifically pertaining to the *Appendix*. There are further substantial references to the *Appendix* in the main body of Farkas's text, primarily in the section 'Generalis conspectus geometriae' (Vol I, pp 442–502) and an important supplement to the *Appendix* in the second volume (pp 380–383). Apart from the *Appendix*, hardly any two copies of the *Tentamen* agree in collation, and the great variation amongst them, including cancel leaves and gatherings, indicates that the publishing history of this work was confused, and remains confusing.

This copy is unusual in a further respect; the first volume is larger than any others known. The Norman copy was also uncut but measured only 219 x 137 mm, whereas its second volume was 225 x 143 mm, closer to the measurements of our first volume.

Bolyai illustrates his textbook with 14 folding plates, five of which are augmented with numerous small flaps. These plates contain as many as 10 slips, often concealed one behind the other; plate 10 also displays a single volvelle, which has gone unrecorded in most bibliographies to date; although not described in the printed or on-line catalogue entries, it is present in most copies. One point of bibliographic confusion has been clarified: the Horblit/Grolier Catalogue (based on the Smithsonian copy) lists an overslip on plate 6 that is not recorded in any other copy. Upon investigation, it appears that an integral part of the plate (the lower portion of the diagram labelled T.144) was inadvertently detached during rebinding and subsequently reattached on a stub, leading to the conclusion that this was a required flap.

Currently 24 copies of the *Tentamen* are known to exist, including the present copy, and one (Berlin) that was lost in WWII. Of these 24, one comprises Janos Bolyai's *Appendix* only. A further three comprise volume one only. In addition, some copies are seriously defective, apart from the standard issue variations. There are numerous variations in collation, etc. amongst these copies. Samuel Lemley is compiling a detailed census and concordance which will be available shortly.



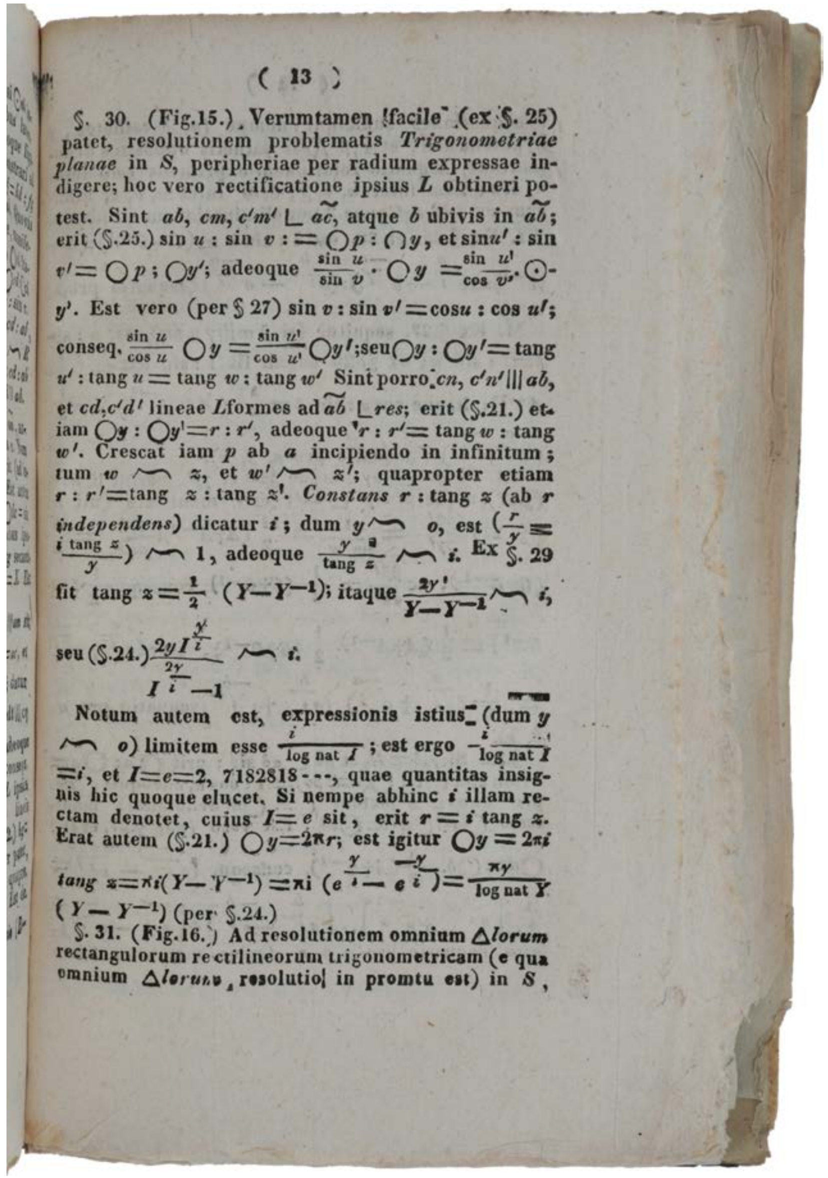
APPENDIX.

SCIENTIAM SPATII *absolute veram* exhibens:
a veritate aut falsitate Axiomatis XI *Euclidei*
(*a priori haud unquam decidenda*) *independentem*; adjecta ad casum falsitatis, quadratura circuli geometrica.

Auctore JOHANNE BOLYAI de eadem, Geometrarum in Exercitu Caesareo Regio Austriaco Castrensium Capitaneo.

Collation note: both volumes are in the most complete state (of several) possible, with all of the various addenda issued.

Dibner 116; Evans 13; Horblit 69b; Norman 259; Parkinson pp 295 and 296; Nagy, Ferenc, *Bolyai: Biographia, biblioteka, bibliografia* pp 353–4 (incorrectly calling for an additional (4) ff of preliminaries in vol I, and in volume II requiring 584 pp instead of 402 pp, perhaps a typographical error for 384, as in the first issue?)



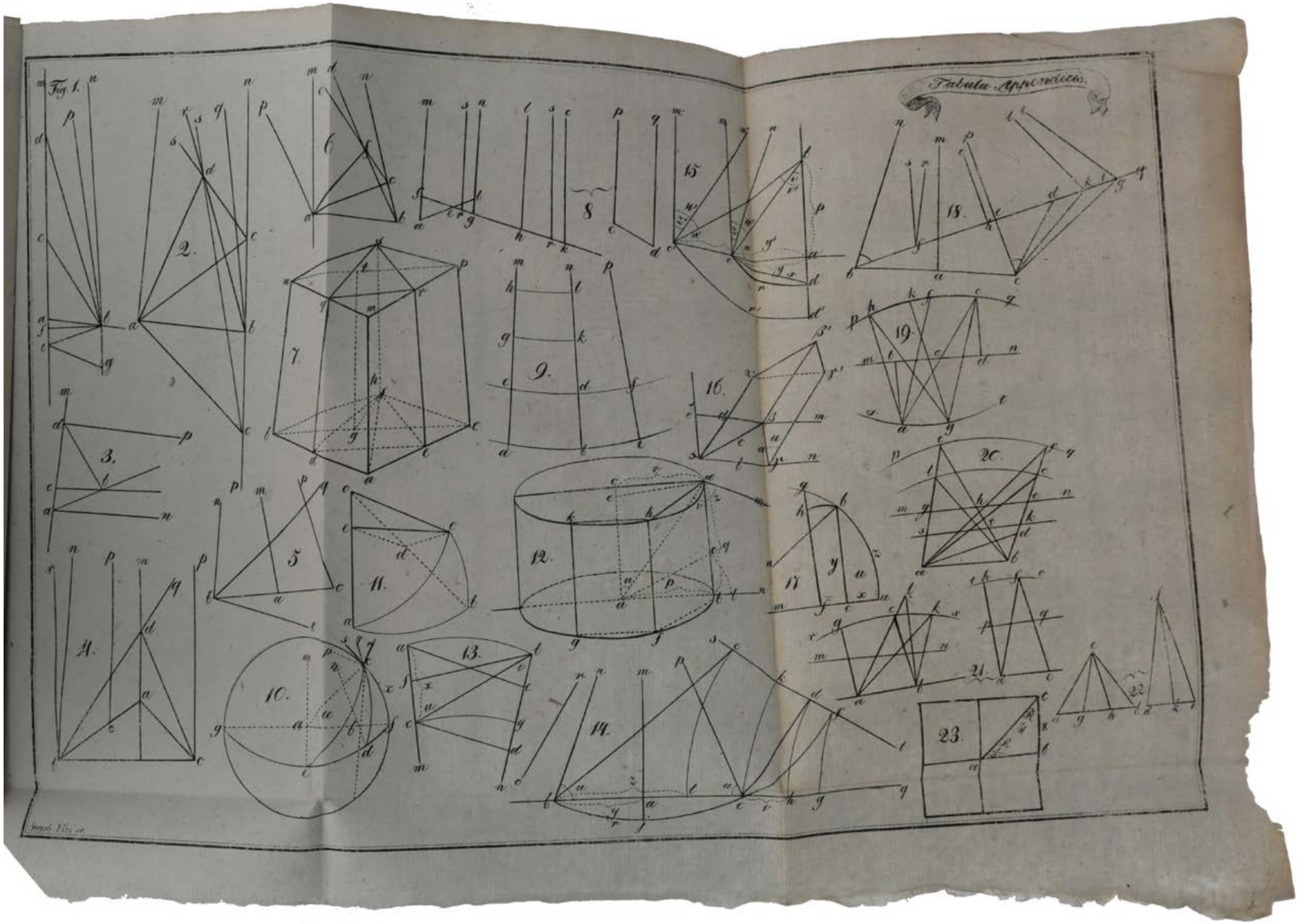
(13)

§. 30. (Fig.15.) Verumtamen facile (ex §. 25) patet, resolutionem problematis *Trigonometriae planae* in *S*, peripheriae per radium expressae indigere; hoc vero rectificatione ipsius *L* obtineri potest. Sint *ab*, *cm*, *c'm'* ⊥ *ac*, atque *b* ubivis in *ab*; erit (§.25.) sin *u* : sin *v* := ○*p* : ○*y*, et sin*u'* : sin *v'* = ○*p* : ○*y'*; adeoque $\frac{\sin u}{\sin v} \cdot ○y = \frac{\sin u'}{\cos v'} \cdot ○y'$. Est vero (per § 27) sin *v* : sin *v'* = cos *u* : cos *u'*; conseq. $\frac{\sin u}{\cos u}\, ○y = \frac{\sin u'}{\cos u'}\, ○y'$; seu ○*y* : ○*y'* = tang *u'* : tang *u* = tang *w* : tang *w'*. Sint porro *cn*, *c'n'* ||| *ab*, et *cd*, *c'd'* lineae *L*formes ad *ab* ⊥ *res*; erit (§.21.) etiam ○*y* : ○*y'* = *r* : *r'*, adeoque *r* : *r'* = tang *w* : tang *w'*. Crescat iam *p* ab *a* incipiendo in infinitum; tum *w* ⁀ *z*, et *w'* ⁀ *z'*; quapropter etiam *r* : *r'* = tang *z* : tang *z'*. *Constans r* : tang *z* (ab *r independens*) dicatur *i*; dum *y* ⁀ *o*, est $\left(\frac{r}{y} = \frac{i \tan z}{y}\right)$ ⁀ 1, adeoque $\frac{y}{\tan z}$ ⁀ *i*. Ex §. 29 fit $\tan z = \frac{1}{2}(Y - Y^{-1})$; itaque $\frac{2y}{Y - Y^{-1}}$ ⁀ *i*,

seu (§.24.) $\frac{2y I^{\frac{y}{i}}}{I^{\frac{2y}{i}} - 1}$ ⁀ *i*.

Notum autem est, expressionis istius (dum *y* ⁀ *o*) limitem esse $\frac{i}{\log \text{nat} I}$; est ergo $\frac{i}{\log \text{nat} I} = i$, et *I* = *e* = 2, 7182818 - - -, quae quantitas insignis hic quoque elucet. Si nempe abhinc *i* illam rectam denotet, cuius *I* = *e* sit, erit *r* = *i* tang *z*. Erat autem (§.21.) ○*y* = 2π*r*; est igitur ○*y* = 2π*i* tang *z* = π*i*(*Y* — *Y*⁻¹) = π*i* $\left(e^{\frac{y}{i}} - e^{\frac{-y}{i}}\right) = \frac{\pi y}{\log \text{nat} Y}(Y - Y^{-1})$ (per §.24.)

§. 31. (Fig.16.) Ad resolutionem omnium △lorum rectangulorum rectilineorum trigonometricam (e qua omnium △lorum, resolutio in promtu est) in *S*,


Tabula Appendicis.

THE FIRST BOOK ON THE LAWS OF PERCUSSION, BOUND WITH THE VERY RARE *RISPOSTA*

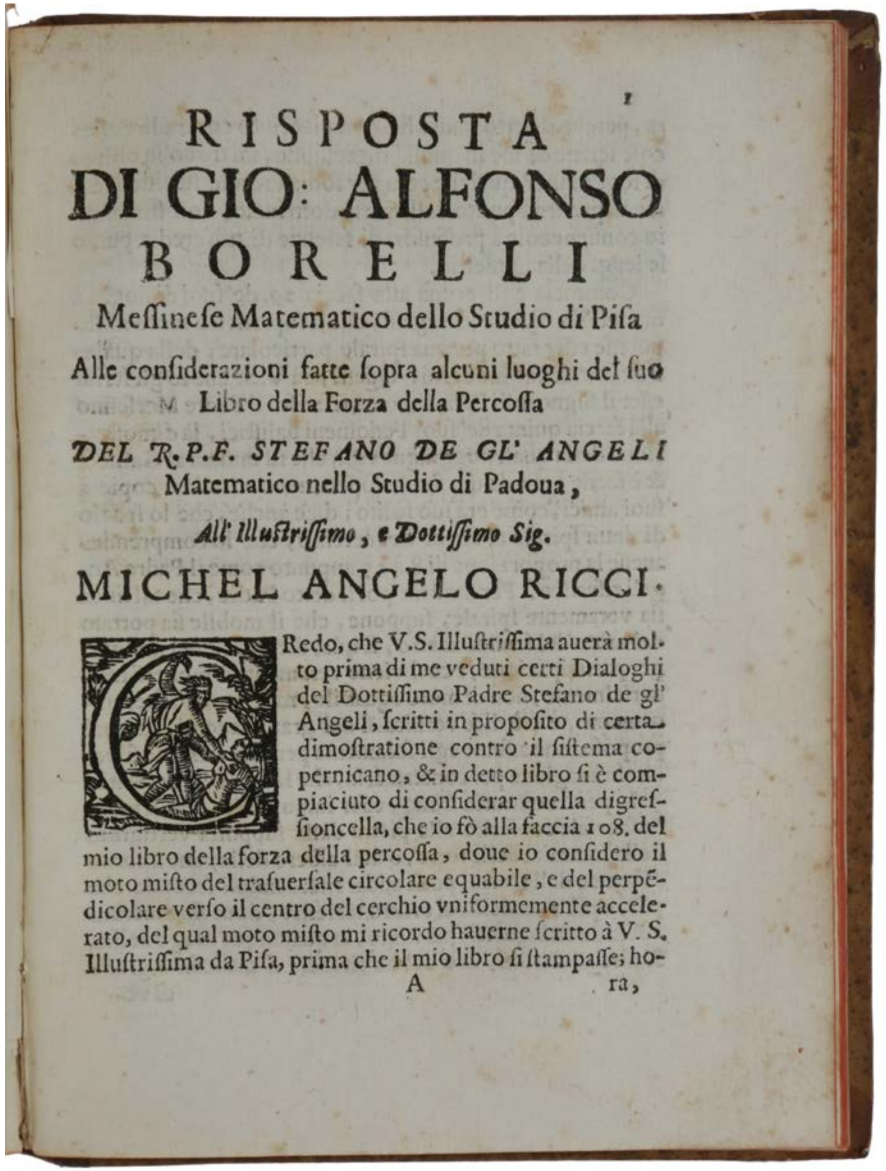
BORELLI, Giovanni Alfonso. *De vi percussionis liber.* Bologna: Giacopo Monti, 1667. [Bound with:] [Drop-title:] *Risposta ... alle considerazioni fatte sopra alcuni luoghi del suo libro della forza della percossa del R. P. F. Stefano de gl' Angeli ... all'illustrissim.* Messina [after 29 February, 1668].

$5,500

Two works in one volume, 4to (200 x 150mm). I. Pp. [xii], 300, 30, [2, errata & imprimatur], with 5 folding engraved plates, printer's device on title, one ornamental initial. II. Pp. 37 [3, blank], with numerous woodcut illustrations in text. Eighteenth- or nineteenth-century half-calf and marbled boards, spine gilt with black lettering-piece.

First edition of the first published book on the laws of percussion, and containing important hitherto unpublished material from the lectures of Galileo and Torricelli. This copy is bound with Borelli's very rare *Risposta*, intended as a supplement to *De vi percussionis* (it was issued without a separate title-page), which contains his reply to criticisms by Stefano degli Angeli of Borelli's views on the motion of bodies in free fall under gravity. *Provenance*: Bookplate of G[iovanni]. B[attista]. Tomaselli (1650-1730) on front paste-down; faded contemporary ownership inscription on title.

"In this, Borelli's first book on mechanics, he quotes Galileo's youthful work on percussion, the fourth *Dialogo*, and lectures by Torricelli. As well as the detailed discussion of impact, the book deals with the dynamics of falling bodies, vibration,



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RISPOSTA
DI GIO: ALFONSO
BORELLI
Meſſineſe Matematico dello Studio di Piſa
Alle conſiderazioni fatte ſopra alcuni luoghi del ſuo Libro della Forza della Percoſſa
DEL R.P.F. STEFANO DE GL' ANGELI
Matematico nello Studio di Padoua,
All' Illuſtriſſimo, e Dottiſſimo Sig.
MICHEL ANGELO RICCI.

Credo, che V.S. Illuſtriſſima auerà molto prima di me veduti certi Dialoghi del Dottiſſimo Padre Stefano de gl' Angeli, ſcritti in propoſito di certa dimoſtratione contro il ſiſtema copernicano, & in detto libro ſi è compiaciuto di conſiderar quella digreſſioncella, che io fò alla faccia 108. del mio libro della forza della percoſſa, doue io conſidero il moto miſto del traſuerſale circolare equabile, e del perpẽdicolare verſo il centro del cerchio vniformemente accelerato, del qual moto miſto mi ricordo hauerne ſcritto à V. S. Illuſtriſſima da Piſa, prima che il mio libro ſi ſtampaſſe; hora,

A ra,

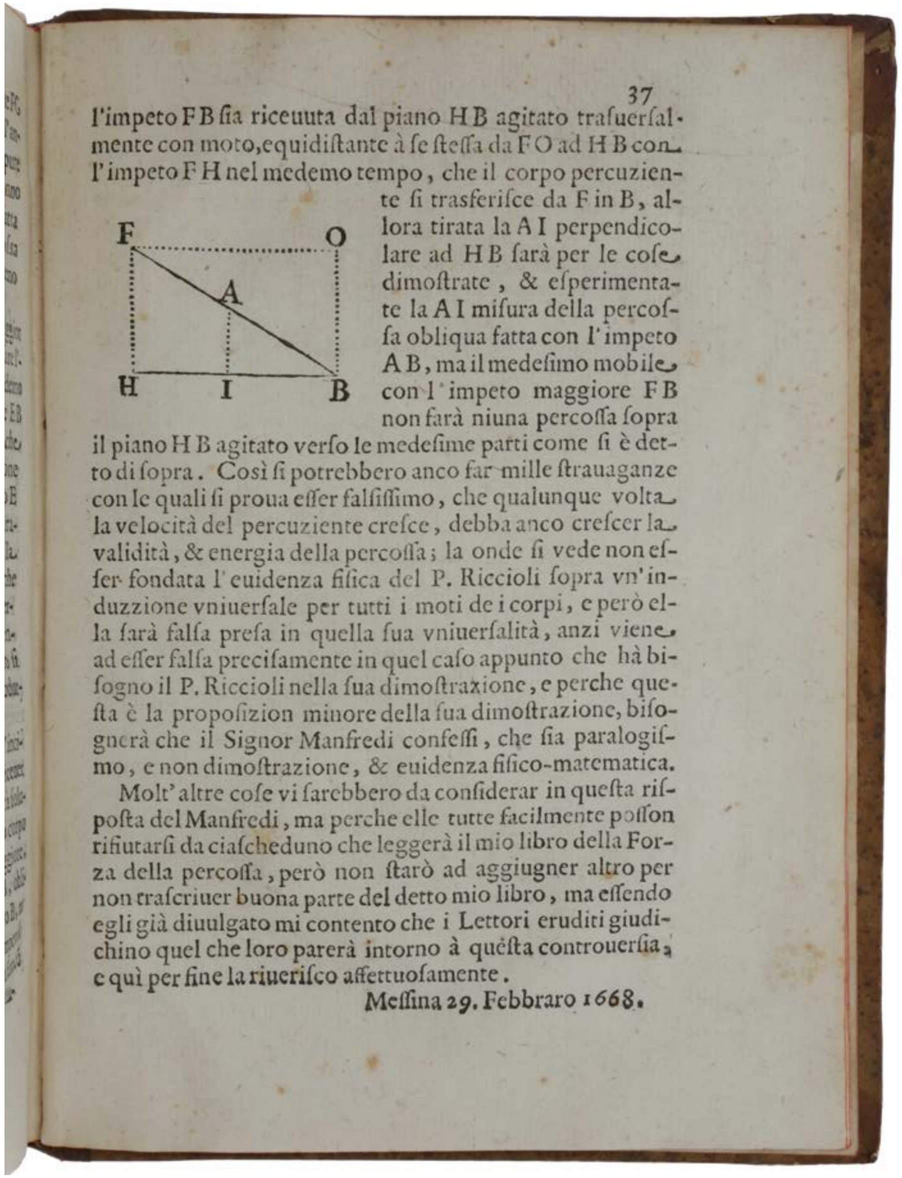
gravity, fluid mechanics, magnetism, and pendular motion … he gives the name resilience for the first time to a number of problems now classed under this name" (Roberts & Trent). This is "the earliest book on the laws of percussion, which undoubtedly influenced John Wallis who, in 1668, published his discovery of the laws governing the percussion of non-elastic bodies, and Christiaan Huygens, who deals with the percussion of elastic bodies in his treatise *De motu corporum ex percussione*, published in 1669' (Zeitlinger I, 174). Thanks to the *Risposta*, Borelli 'can be credited to be the first person to have examined in quantitative detail the deflection that falling bodies undergo due to the earth's diurnal rotation' (Theo Gerkema, *On Borelli's analysis concerning the deflection of falling bodies*, 2009). Borelli regarded *De vi percussionis*, together with his *De motionibus naturalibus* (1670), as necessary preparation for his masterpiece, *De motum animalium* (1680-81), on which he had worked since the early 1660s. Although *De vi percussionis* is found without great difficulty on the market, this is the first copy we have seen that is complete with its supplement, the *Risposta*. OCLC records five copies of the *Risposta* in US (Burndy; Hagley Library, Columbia; Cornell; New York Academy of Medicine; and Wisconsin); no copies in auction records.

"In May 1665, Cardinal Michelangelo Ricci, Roman correspondent and adviser to the Tuscan Court, wrote to Borelli's patron, Leopoldo de Medici, encouraging Borelli to apply himself to the composition of a treatise on motion. According to Ricci, motion was a particularly important topic since so many contemporaries, famed for their contributions to mathematics and philosophy, had dedicated so much time to the topic and had explained so many of nature's secrets. Borelli's initial response was that he was instead concentrating on a treatise on anatomy within which he would insert some words regarding collision of moving bodies. At some point in this discussion, seemingly prompted by an insistence from Ricci, Borelli decided to publish *On the Force of Percussion* independently from his main project. The intention of the book on colliding bodies was to establish crucial propositions concerning motion as a means of introducing issues related to human and animal movements.

"The main problem in question, as Aristotle had put it, was to explain why a heavy axe, as an example, has virtually no effect when rested on a piece of wood but has a much greater impact when it is made to fall from a significant height. Aristotelians believed that the increased force is a result simply of the velocity of the movable; the velocity supposedly artificially increases the weight of the object. For Italian natural philosophers in the seventeenth century, the first point of reference in response to this Aristotelian position is the work carried out by Galileo concerned with motion and mechanics including percussion. In his *Mechanics* (c. 1590), Galileo claimed that to study percussion, one must consider 'that which has been seen to happen in all other mechanical operations, which is that the force, the resistance, and the space through which the motion is made respectively follow that proportion and obey those laws by which a resistance equal to the force will be moved by this force through an equal space and with equal velocity to that of the mover.' That was to say that it is not only the weight of the body in motion that determines the force of percussion but the distance it travels and its velocity before impact that is required to overcome the resistance of the body being impacted upon. Galileo elaborated on his argument in *Discourse Concerning Two New Sciences* (1638), where he presented several experiments in which the force of percussion was tested and measured by relying on the proportions of opposing forces (including distances and velocities) rather than simply differences in weight.

"In *On the Force of Percussion*, Borelli agrees with the Galilean proposition that the energies of colliding bodies are not measurable through weight alone. To prove his point, he begins with a series of propositions explaining how a body must be first moved by an impeller in order to acquire a "motive virtue" or "impetus". Upon colliding with another body at rest, that impetus is transmitted to the stationary body, overcomes its resistance proportional to the mass and velocity of the first body and itself sets in motion. Borelli puts it succinctly: 'Despite the horror of some Aristotelians for the migration of the motive virtue, it seems certain that part of the virtue or impetus which was concentrated in the impelling body is distributed and expanded in the struck body.' The 'distribution and expansion' of impetus does not mean that the struck body acquires the same speed as the first, 'impelling' body, only that the motive virtue is preserved and shared between the two bodies—the reactions of these bodies to the collision is proportional to their respective masses. In sum, the impact of colliding bodies occurs in only a moment, but the result of that instant of time—the cause and effect—is dependent upon proportions of velocity and mass …

"This is a strictly mechanical explanation of moving parts of nature reminiscent of Cartesian natural philosophy, whereby motion is explained by the measurements and properties of bodies in contact with each other. In fact, Borelli is explicit in his mechanistic outlook. He contends that the motive virtue of colliding bodies 'clearly occurs in similar machines'. He makes this point with particular regard to bodies with elastic, spring-like properties … 'If indeed thrown against a wall or against a steady racket, a playing ball or a water-skin or a spring or any elastic machine is compressed or bent proportionally to its impetus and percussion. The water-skin then rebounds with a more violent, i.e. doubled, impetus. The compression and bending of the machine is increased, doubled, in so far as the percussive compression is doubled.' Aside from the allusions to the mechanical properties of nature, the reference here to force resulting from compression is also rather important for our understanding of Borelli's philosophy of motion as it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l'impeto F B ſia riceuuta dal piano H B agitato traſuerſalmente con moto, equidiſtante à ſe ſteſſa da F O ad H B con l'impeto F H nel medemo tempo, che il corpo percuziente ſi trasferiſce da F in B, allora tirata la A I perpendicolare ad H B ſarà per le coſe dimoſtrate, & eſperimentate la A I miſura della percoſſa obliqua fatta con l'impeto A B, ma il medeſimo mobile con l'impeto maggiore F B non farà niuna percoſſa ſopra il piano H B agitato verſo le medeſime parti come ſi è detto di ſopra. Così ſi potrebbero anco far mille ſtrauaganze con le quali ſi proua eſſer falſiſſimo, che qualunque volta la velocità del percuziente creſce, debba anco creſcer la validità, & energia della percoſſa; la onde ſi vede non eſſer fondata l'euidenza fiſica del P. Riccioli ſopra vn'induzzione vniuerſale per tutti i moti de i corpi, e però ella ſarà falſa preſa in quella ſua vniuerſalità, anzi viene ad eſſer falſa preciſamente in quel caſo appunto che hà biſogno il P. Riccioli nella ſua dimoſtrazione, e perche queſta è la propoſizion minore della ſua dimoſtrazione, biſognerà che il Signor Manfredi confeſſi, che ſia paralogiſmo, e non dimoſtrazione, & euidenza fiſico-matematica.

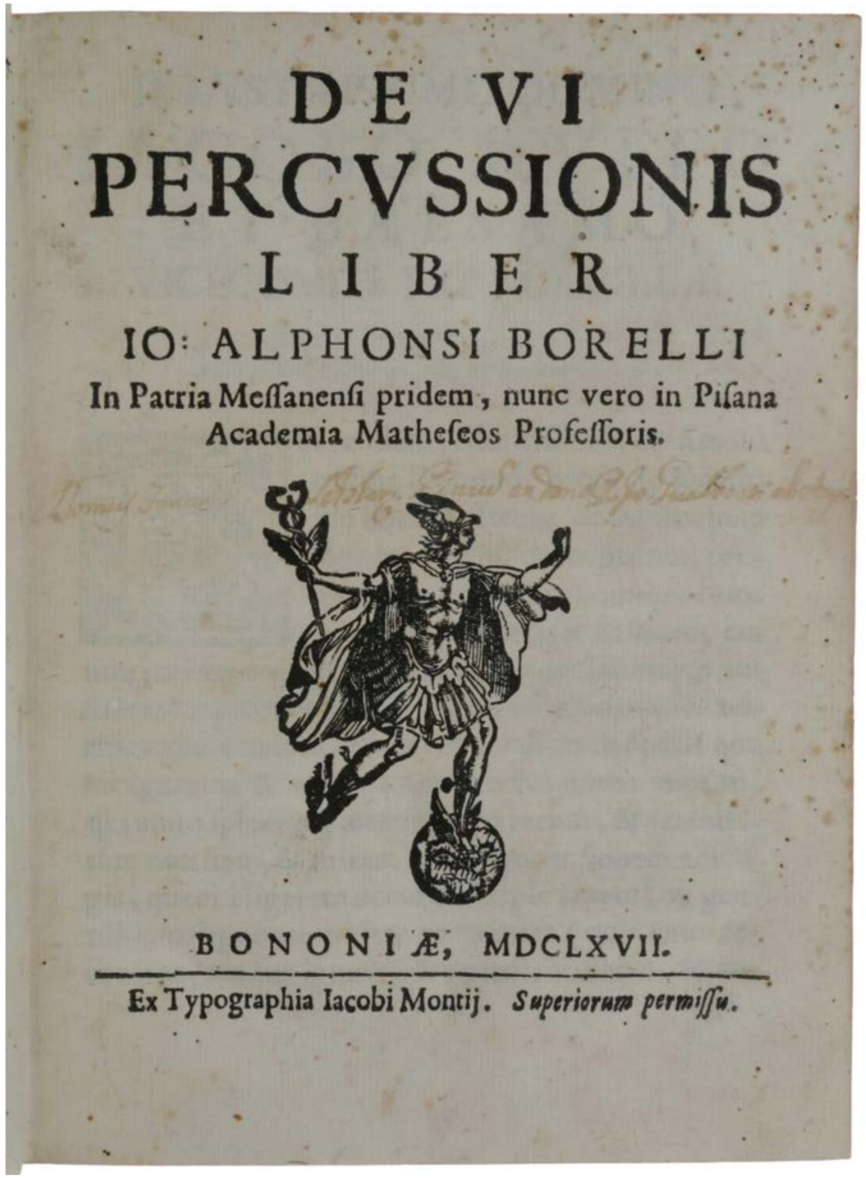
Molt'altre coſe vi ſarebbero da conſiderar in queſta riſpoſta del Manfredi, ma perche elle tutte facilmente poſſon rifiutarſi da ciaſcheduno che leggerà il mio libro della Forza della percoſſa, però non ſtarò ad aggiugner altro per non traſcriuer buona parte del detto mio libro, ma eſſendo egli già diuulgato mi contento che i Lettori eruditi giudichino quel che loro parerà intorno à queſta controuerſia, e quì per fine la riueriſco affettuoſamente.

Meſſina 29. Febbraro 1668.

affords him the possibility of considering the spring-like properties of corpuscles. This is an issue to which he returns in his subsequent book [*De motionibus naturalibus*]" (Boschiero, pp. xiv-xvi).

Borelli also makes in this work an important contribution to the discussion of the laws of falling bodies (pp. 107-110). "Borelli's analysis of the trajectory of the falling body, though it is, of course, erroneous, is the best ever made before Hooke and Newton. He is the only one who succeeds in disentangling the purely mathematical point of view from the physical. He is, too, the only man before Hooke who is not dominated and befogged by the traditional conception according to which, whether the Earth moves or stands still, a heavy body has, in any case, to move to the center of the Earth on a perfectly straight line … Borelli continues to explain that the force of an "oblique" percussion is to be measured no by the impetus along the "oblique" path, but only by that on the perpendicular, which, he adds, enables us to dispose of the argument proposed some time ago by a celebrated author (Borelli does not name Riccioli) according to whom, if the Earth moved, the motion of a body falling from the top of a tower would be uniform, and, therefore, the force of percussion would not increase, at least not perceptibly, with the increase of the altitude of the fall. The celebrated author forgets, Borelli explains, that the point or plane of impact does not stand still in world-space, but is transported together with the tower and the body falling from its top" (Koyré, pp. 358-60).

In Chapter 25, Borelli gives a discussion of magnetism thought to be based upon a short treatise by Benedetto Castelli entitled *Discorso sopra la calamita*, which was discovered in manuscript form among some papers by Galileo in the eighteenth century and eventually published in 1883. This treatise is important as the first example of a theory of magnetism which uses elementary magnets



DE VI
PERCVSSIONIS
LIBER
IO: ALPHONSI BORELLI
In Patria Messanensi pridem, nunc vero in Pisana Academia Matheseos Professoris.

BONONIÆ, MDCLXVII.
Ex Typographia Iacobi Montij. Superiorum permissu.

to account for magnetization of iron. The main modification introduced by Borelli is the introduction of a magnetic effluvia or vapour from the loadstone to replace Castelli's propagation of magnetism through the air. Borelli explains magnetization of iron by supposing that "one must postulate that in the iron there are innumerable active and spirited particles. These particles, however, are disposed in a very confused manner, all intertwined in a variety of ways so that not all their Northern poles point in the same direction but are all confusedly mixed...it is necessary to imagine that when the iron is brought near the loadstone and within its sphere, which stems from the exhalation of the vapour of the loadstone, just as by the process of stirring up, the magnetic particles which are within the interstices of the iron are stirred up and turned, and once loosened and set free … they direct their poles in proper orientation toward the pole of the loadstone." This is not exactly Castelli's theory, but Borelli borrowed Castelli's ideas and combined them with the currently prevailing magnetic theories.

The primary object of Angeli's attack was the Jesuit astronomer Riccioli who, in his *Astronomia Reformata* of 1665, 'provided a list of seventy-seven arguments against Copernicanism. The Jesuits had great difficulty accepting that different motions could be combined without one of them overpowering and annulling the effects of the other. A key feature of Riccioli's position was that there were observable differences in the behaviour of bodies depending on whether the Earth was in motion or not. Since observations showed the effects he expected on a stationary Earth, he concluded that the Earth does not move. In this extreme form, his views about composition of motion were rather isolated … This issue, however, involved not only the composition of motion but also the motions being composed, and this was an area of considerable uncertainty.

"Following a preliminary essay sent anonymously to Riccioli in 1666, Borelli put forward his arguments in *De vi percussionis* (Bologna 1667). He argued that the trajectory of a body dropped from a tower in the hypothetical situation of a rotating Earth would be an irregular curve resulting from a uniform curvilinear motion acquired from the Earth's rotation and a uniformly accelerated one … Stefano degli Angeli entered the controversy against both Riccioli and Borelli. His attacks created considerable embarrassment because degli Angeli was a closet Copernican and a student of Cavalieri's, just as Borelli had been a student of Castelli's. As Koyré rightly put it, degli Angeli 'grasped the meaning of the Galilean relativity of motion.' According to degli Angeli, however, the circular component of the velocity of the body falling from the tower was not conserved but diminished as the body fell. More precisely, the circular motion is proportional to the radius and thus diminishes exactly in the amount to make the body fall parallel to the tower, with no deviation … There was another issue raised by degli Angeli. He pointed out that according to Borelli's view the two different cases of a moving versus a stationary Earth would lead to a different trajectory for falling bodies. In the latter case the trajectory would always be parallel to the tower, whereas in the former it would deviate towards the east.

"In his reply, Borelli introduced a novel element in the form of experiments designed to show that transverse velocity is conserved, an estimation of the order of magnitude of the eastward deviation, and a correction to his previous understanding of the body's trajectory … Addressing now the deviation from the perpendicular, Borelli admitted that he was troubled by this phenomenon, and he tried to counter it with a two-pronged approach. On the one hand he estimated the actual deviation following his initial hypothesis, showing that for a fall of 240 feet the deviation was an eighteenth of an inch and therefore, in his opinion, unobservable …

"Borelli's concerns produced another important correction. Neglecting the action of gravity, he had initially assumed the motion of the body to be circular, but now he realized that its motion must be rectilinear. This correction came conveniently to hand because it enabled him to reduce further the amount of the eastward deviation … Borelli's attempts towards a quantitative estimation of the eastward deviation were significant, especially if compared with degli Angeli's work' (Domenico Bertoloni Meli, *Thinking with Objects. The Transformation of Mechanics in the Seventeenth Century* pp. 198-201). Borelli's theory of an eastward deviation of a falling body was experimentally proved by G. G. Guglielmini in 1791.

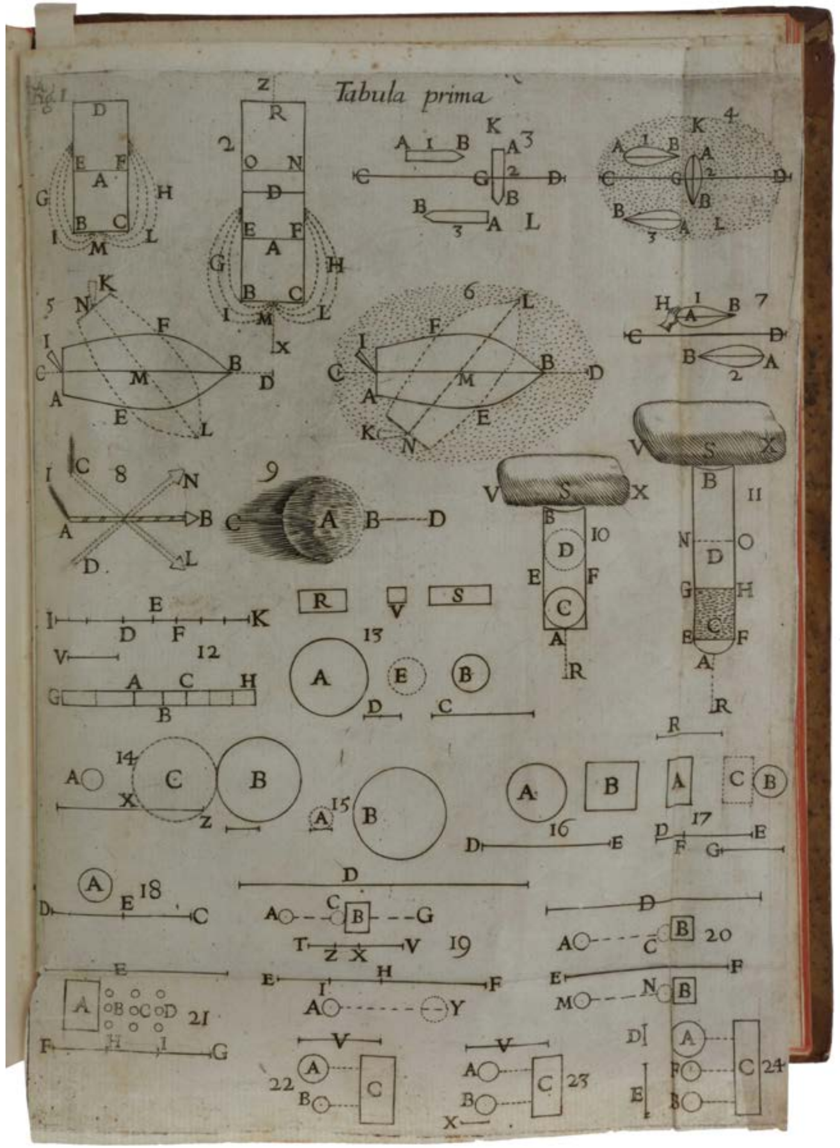
The *Risposta* was published in the form of two letters to Michelangelo Ricci; there was no title-page, only the drop-title on A1, and no printer given. The first letter is dated 'Messina 29 Novembre 1667' and the second 'Messina 29 Febbraro 1668'. Although intended as a supplement to *De vi percussionis*, it is sometimes bound separately (e.g, the copy at the ETH Zürich digitized on e-rara).

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 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.

"One year after Borelli arrived in Tuscany the *Accademia del Cimento* held its first session; the year Borelli left, the *Cimento* quietly died. Indeed, Borelli seems to have been the principal animus of the academy … the Tuscan court had been thoroughly infected by Galileo's ideas and those of his pupils. Grand Duke Ferdinand II, from the time of his accession to power in 1628 until his death in 1670, maintained a personal laboratory as did Prince Leopold. From the time of the death of the Master, Galileo, informal gatherings met at the court and presented and discussed experiments … Then, possibly under the crystallizing influence of Borelli, Leopold asked for and received permission from Ferdinand to organize formally an academy for purely experimental research. Under Leopold's aegis it met for the first time in June of 1657 … Lorenzo Magalotti, after attending the University of Pisa as a student, was appointed secretary in 1660. The *Cimento* had adopted a policy of submerging the identities of its members and presenting itself as a group. Accordingly, when Magalotti brought out the *Saggi di naturali esperienzi fatte nell'Accademia del Cimento* in 1666–1667, it appeared anonymously and refrained from identifying the individual contributions of the members … During the life of the *Cimento* dissension appeared among the membership; Borelli may have originated some of it. He seems to have chafed under the requirement of anonymity, and by all accounts he was a touchy person to get along with under any circumstances …

"[Borelli] produced two major studies which were not only exercises in pure mechanics but also, in the eyes of Borelli himself, necessary introductions to what he would consider to be his most important work, the *De motu animalium*. Respectively, these were *De vi percussionis* (1667) and *De motionibus naturalibus a gravitate pendentibus* (1670). Both cover considerably more subject matter than their titles indicate. In the first, for instance, Borelli discusses percussion in detail, some general problems of motion, gravity, magnetism, the motion of fluids, the vibrations of bodies, and pendular motion, to cite just a few items. Likewise, in the second, he 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. When we consider that all this was the product of years of experimental and theoretical investigation, we should not wonder that he objected to giving it over to be brought out anonymously by the *Cimento* just because he happened to present a good deal of it before that society. To the apparent displeasure of Leopold, Borelli published *De vi percussionis* in Bologna. And in the early summer of 1667 he set out once more 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. As far as Borelli was concerned, he had been, and afterward remained, on excellent terms with Leopold; and Leopold maintained his high regard for Borelli" (DSB).

I. Carli & Favaro 303; Cinti 147; De Caro 52; Honeyman 396; Poggendorff I, 240; Riccardi I, 159; Roberts & Trent, *Bibliotheca Mechanic*a, pp. 39-40; Sotheran 474; Wellcome II, 204. II. Carli and Favaro 311; Riccardi I 159.7. L. Boschiero, Introduction to Borelli's *On the Movement of Animals – On the Force of Percussion*, P. Maquet (tr.), 2015; A. Koyré, 'A Documentary History of the Problem of Fall from Kepler to Newton,' *Transactions of the American Philosophical Society* 45 (1955), 329-395.



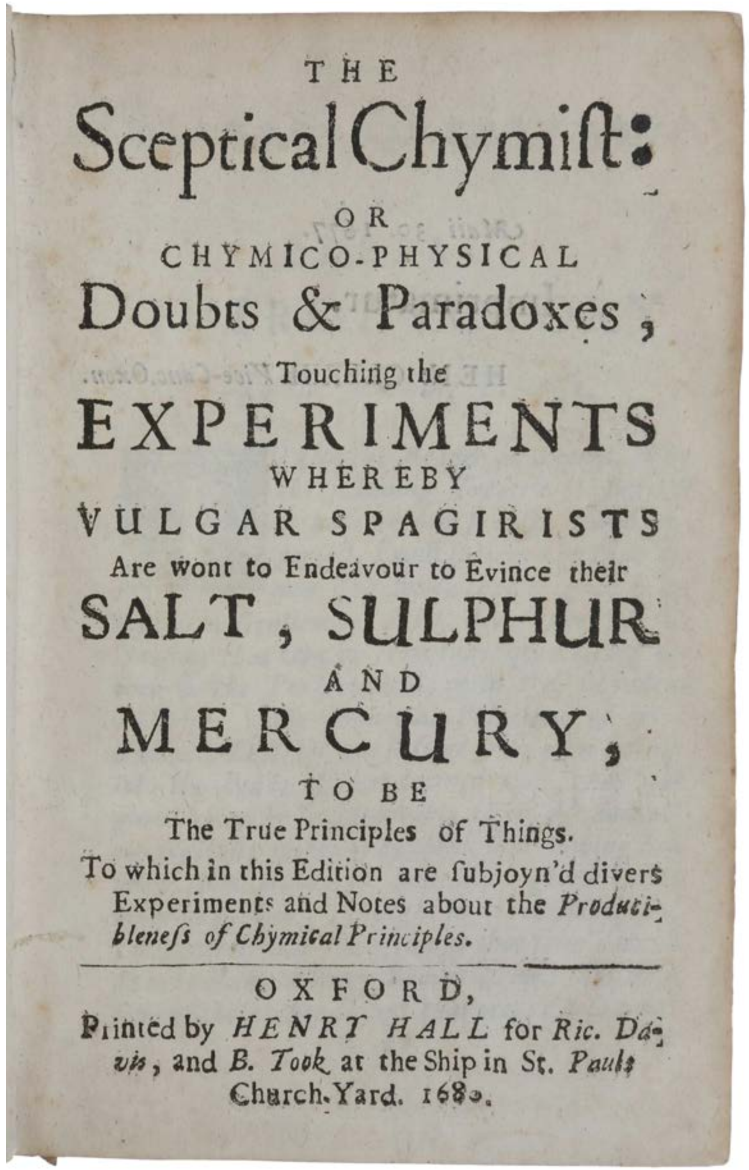
PMM 141 - THE FOUNDATION OF CHEMISTRY

BOYLE, Robert. *The Sceptical Chymist: or Chymo-Physical Doubts and Paradoxes, touching the experiments whereby vulgar spagirists are wont to endeavour to evince their salt, sulphur and mercury, to be the true principles of things. To which in this edition are subjoyn'd divers Experiments and Notes about the Producibleness of Chymical Principles.* Oxford: Henry Hall for R. Davis and B. Took, 1680.

$40,000

Two parts in one vol., 8vo (167 x 105 mm), pp. [22], 440; [28], 268. Contemporary vellum, spine lettered in manuscript (pale red marking to spine and rear board), a very fine copy. Custom blue cloth slipcase and chemise, blue morocco spine label.

Second edition in English (first, 1661), complete with the very rare advertisement leaf which is lacking from most copies, of this landmark in the history of science, "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). The physicists, Boyle called them 'hermetick philosophers', upheld the Peripatetical or Aristotelian doctrine of the four elements – fire, air, earth, and water. The chemists, 'vulgar spagyrists', were disciples of Paracelsus who believed in the *tria prima* – salt, sulphur, and mercury. Boyle showed that both of these theories were totally inadequate to explain chemistry and was the first to give a satisfactory definition of an element. This second edition of the



Sceptical Chymist contains the first printing of the second part, *Experiments and Notes about the Producibleness of Chymical Principles*. The first edition of the *Sceptical Chymist* hardly ever appears on the market and now commands a very high price – the last complete copy sold at auction realized £362,500 in 2015. Fulton located five copies of this second edition complete with the advertisement leaf; four are recorded on ABPC/RBH in the last 40 years (only one since the Norman sale, and that in a modern binding).

Provenance: Gift inscription to the chemist A. W. Tangye, manager and director of Brunner Mond, from [?] Hutchins, dated 12 December 1922, on the front free endpaper.

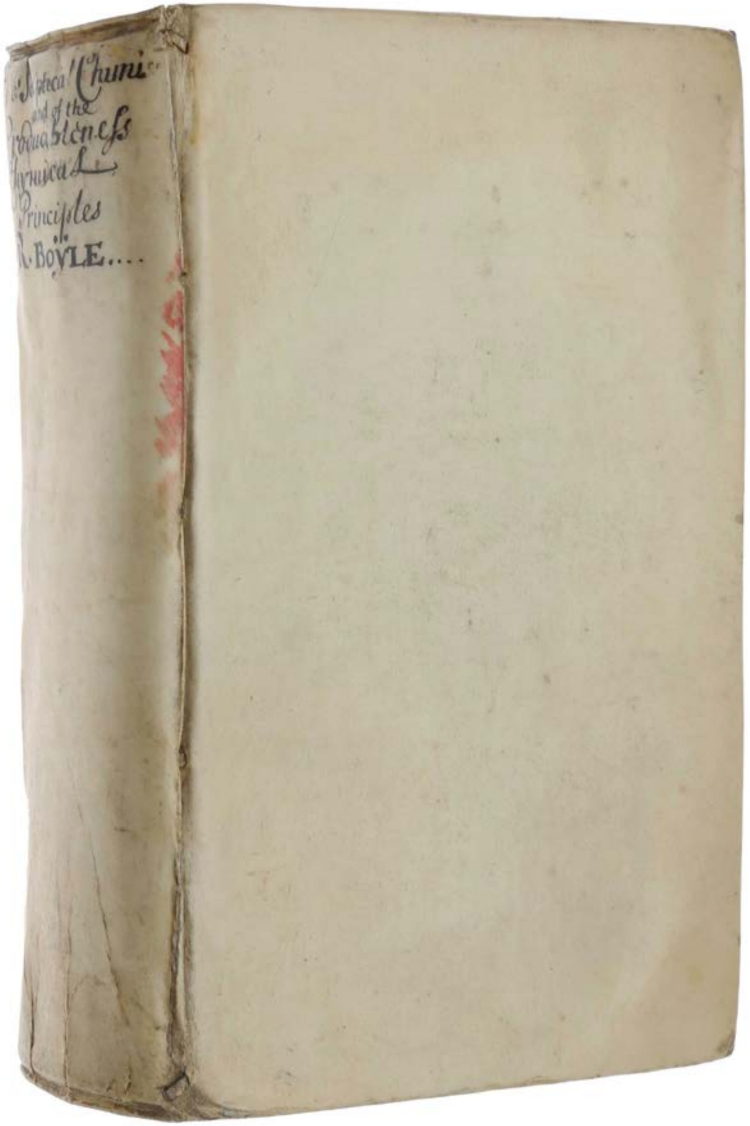
"The 'Sceptical Chymist' is one of the great books in the history of scientific thought, for it not only marks the transition from alchemy to modern chemistry but is a plea, couched in most modern terms, for the adoption of the experimental method. Boyle inveighed against the inaccurate terminology of 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 (1627-91) 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.

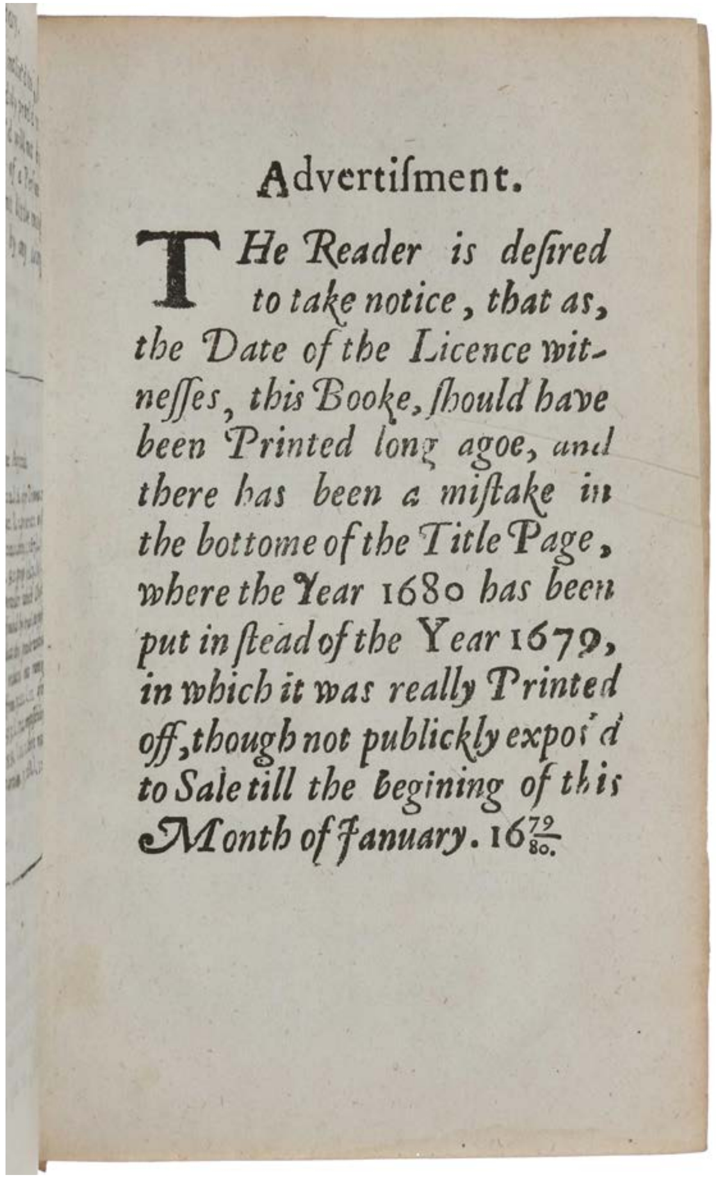
"This work has often been acclaimed as a turning point in the evolution of modern chemistry, a crushing blow to traditional alchemy, but in fact Boyle's message is a more complex one. In his text he made a clear distinction between 'the true *Adepti*' and 'those Chymists that are either Cheats, or but Laborants.' While dismissive of the latter, his view of the former was that, 'could I enjoy their Conversation, I would both willingly and thankfully be instructed' by them. In other words, Boyle had no quarrel with those who aspired to the higher mysteries of alchemy. Rather, his book was targeted at distillers, refiners and others, who were so preoccupied with hands-on processes that they lacked an interest in theory, and also at the authors of chemical textbooks who combined a similar preoccupation with practical preparations with a reliance on Paracelsian principles. Hence *The Sceptical Chymist* is primarily an attack on the Paracelsian tradition, and particularly on its theory that the world was made up of the three principles of salt, sulphur and mercury … But he also made a broader appeal for chemical investigation to be informed by a clear



explanatory structure, criticising the practical chemists whom he attached in the book on the grounds that 'there is a great Difference between being able to make Experiments, and being able to give a Philosophical Account of them'" (Hunter, pp. 119-120).

Experiments and Notes about the Producibleness of Chymical Principles, here in its first edition, has separate title and pagination (and was issued separately – Wing B3972). It "echoes the earlier work in containing a vindication of alchemical adepts able to carry out transmutation, in contrast to those who wrote 'courses of Chymistry' and the like. Boyle also reiterated his criticism of the Paracelsian concept of the three principles of salt, sulphur and mercury, supplementing *The Sceptical Chymist* by illustrating the extent to which these and the 'spirits' which chemists also commonly – and rather vaguely – invoked could be more precisely defined. In the case of salts, he argued that there were three distinct families: acid salts, volatile salts, and alkalies or lixiviate salts – all of which could be produced or destroyed by chemical processes (in this connection he also criticized the acid/ alkali theory). He also dealt with sulphur, but it was his treatment of mercury which was most complicated, reflecting the interest in this substance that underlay his alchemical concerns, and particularly the conviction that common mercury could be converted into a more potent 'philosophical mercury' … This part of *Producibleness* echoes earlier alchemical writers both in its language and in its conceptual apparatus, the work as a whole being potentially 'useful to fellow aspiring adepts.' Boyle's commitment to alchemy is not to be underestimated" (*ibid*., p. 186).

Fulton initially believed that the advertisement leaf, which states that the book was actually printed in 1679 rather than 1680, existed in only one copy, but he later found a few other examples. The leaf exists in two states, one in which the date



Advertiſment.

THe Reader is deſired to take notice, that as, the Date of the Licence witneſſes, this Booke, ſhould have been Printed long agoe, and there has been a miſtake in the bottome of the Title Page, where the Year 1680 has been put in ſtead of the Year 1679, in which it was really Printed off, though not publickly expoſ'd to Sale till the begining of this Month of January. 16 79/80.

of publication is given as January 1679/80 (as here), and another, with a different setting of type, giving 1679/78. Fulton suggests that Boyle insisted on having the date corrected "lest continental readers might suspect him of plagiarizing writers who had been guilty of plagiarizing him." The imprimatur is dated 30 May 1677. Boyle's name does not appear on the title.

Dibner, *Heralds* 39; Grolier/Horblit 14; Norman 299; PMM 141; Sparrow, *Milestones* 27 (all for the first edition). ESTC R16310; Fulton 34; Madan 3261, 3260. Partington II, pp. 495 *et seq*. Hunter, *Boyle. Between God and Science*, 2009.

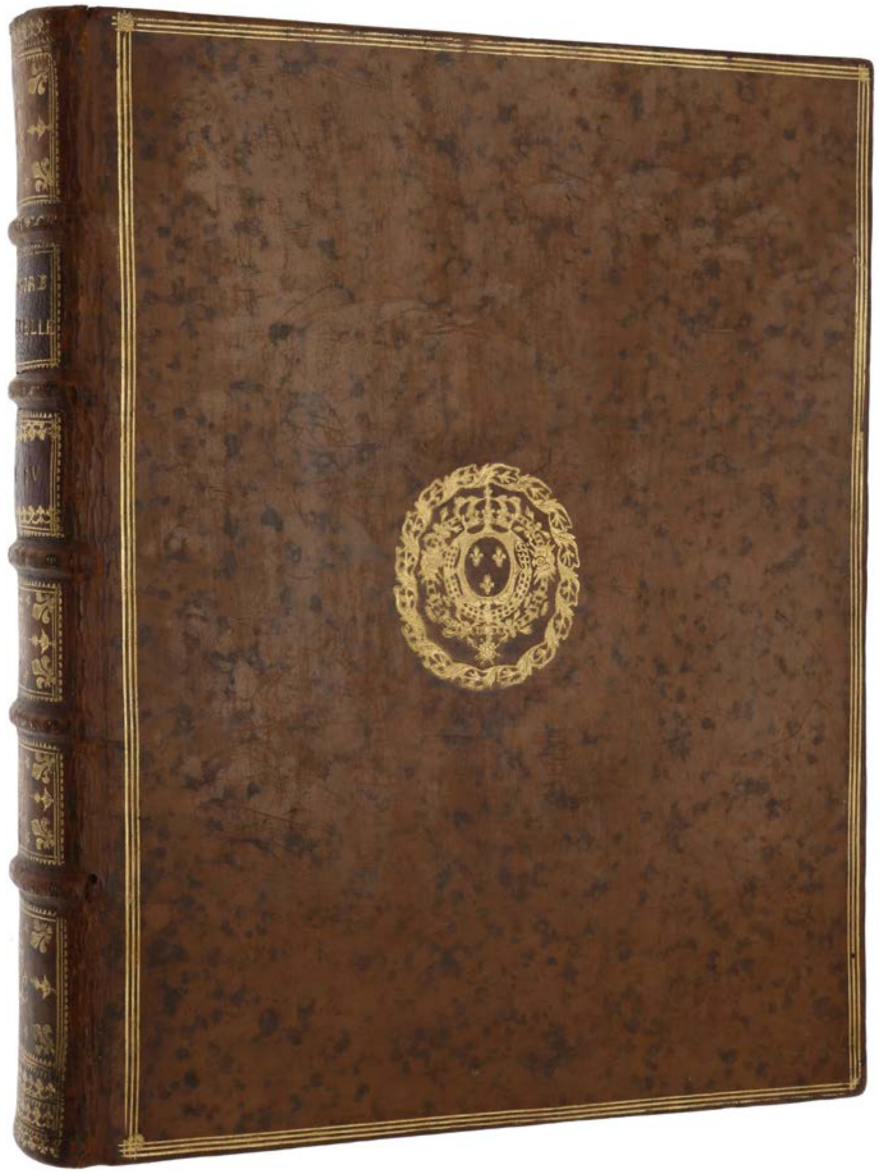
PMM 198 - AN EXCEPTIONALLY FINE SET BOUND WITH THE ARMS OF LOUIS XVI

BUFFON, Georges-Louis Leclerc, Comte de. *Histoire Naturelle, Générale et Particulière, avec la Description du Cabinet du Roi*. Paris: Imprimerie Royale, 1749–1804. Comprising: *Histoire naturelle, générale et particulière, avec la description du Cabinet du Roi*, 15 vols, 1749–67; *Histoire naturelle des oiseaux*, 9 vols, 1770–83; *Histoire naturelle des minéraux*, 5 vols, 1783–88; *Supplément à l'histoire naturelle*, 7 vols, 1774–89; *Histoire naturelle des quadrupèdes ovipares et des serpens*, 2 vols, De Thou, 1788–89; *Histoire naturelle des poissons*, 5 vols, Plassan, An VI–XI (1798–1803); *Histoire naturelle des cétacés*, 1 vol, Plassan, 1804.

$95,000

44 vols., 4to (252 x 193 mm), with engraved vignettes on the titles of the first 15 vols., numerous engraved headpieces, and 1262 engraved plates (including two allegorical plates in vol. I and engraved portrait frontispiece in first vol. of Supplément), 12 maps, and 4 folding tables, complete with the polar bear plate which is often missing (half-title of Vol. V misbound at beginning of vol. IV). Contemporary, and uniform, marbled calf, covers with gilt fillet and gilt arms of Louis XVI in the centre, his monogram in each spine panel, spines richly gilt with two red-morocco lettering-pieces.

First edition, a fine and absolutely complete copy in unrestored contemporary French calf of this monumental work, "the most celebrated treatise on animals ever produced" (Dibner), but also including treatises on cosmology, geology and palaeontology. This copy was almost certainly bound for a member of the Royal family. All volumes have the arms of Louis XVI on the covers and in the spine panels – we have found only one other copy with these arms on the covers and spines, namely that in the Bibliothèque nationale, which was bound in red



morocco, presumably for the King himself. Over 1,000 of the plates are the work of Jacques de Sève, père et fils: a full list of the artists is provided by Nissen. Most sets lack some or all of the *Supplément* volumes, and/or various plates. Vol. III of the Oiseaux sometimes contains a duplicate plate; one being a cancel as it has the wrong plate number engraved on it, otherwise both versions are identical. Plate counts differ sometimes because the first volume contains two maps which are often included in the plate count, whereas the other 10 maps are very large folding maps. These and the tables are sometimes bound in a separate volume, or, as here, in with the main work.

Buffon "was the first to present the universe as one complete whole and to find no phenomenon calling for any but a purely scientific explanation" (PMM). "Buffon's work is of exceptional importance because of its diversity, richness, originality, and influence. Buffon was among the first to create an autonomous science, free of any theological influence. He emphasized the importance of natural history and the great length of geological time. He envisioned the nature of science and understood the roles of paleontology, zoological geography, and animal psychology. He realised both the necessity of transformism and its difficulties" (DSB). This work also represents the birth of evolutionary theory. "Georges Buffon set forth his general views on species classification in the first volume of his *Histoire Naturelle*. Buffon objected to the so-called 'artificial' classifications of Andrea Cesalpino and Carolus Linnaeus, stating that in nature the chain of life has small gradations from one type to another and that the discontinuous categories are all artificially constructed by mankind. Buffon suggested that all organic species may have descended from a small number of primordial types; this is an evolution predominantly from more perfect to less perfect forms" (Parkinson). "It is a great pity that his [Buffon's] ideas were scattered and diffused throughout the vast body of his Natural History with its accounts of individual animals. Not only did this concealment make his interpretation difficult, but it lessened the impact of his evolutionary ideas … However, almost everything necessary to originate a theory of natural selection existed in Buffon. It needed only to be brought together and removed from the protective ecclesiastical coloration which the exigencies of his time demanded" (Eiseley, p. 45). In addition to its comprehensive coverage of natural history (including mankind) and minerals, the work incorporates in the first volume Buffon's highly important *Théorie de la terre*, elaborated in the fifth volume of the *Supplément* as *Des Époques de la nature* – these treatises contain Buffon's theory that the earth was created by a collision between the sun and a comet, the first attempt to reconstruct geological history in a series of stages, and his notion of 'lost species', which opened the way to the development of palaeontology. Like that other great product of the enlightenment, the *Encyclopédie*, the *Histoire Naturelle* was a collaborative enterprise, outliving its instigator and chief author. The two scientists who were foremost among the several contributors were Daubenton and Lacépède (first as Comte de, then as Citoyen): they completed the work after Buffon's death in 1788. We purchased the present copy from a collector who had acquired it at auction in 1973 (Priollaud & Lavoissière, La Rochelle, 18/19 October); the auction catalogue (included here) singles out the Buffon for mention on its front cover.

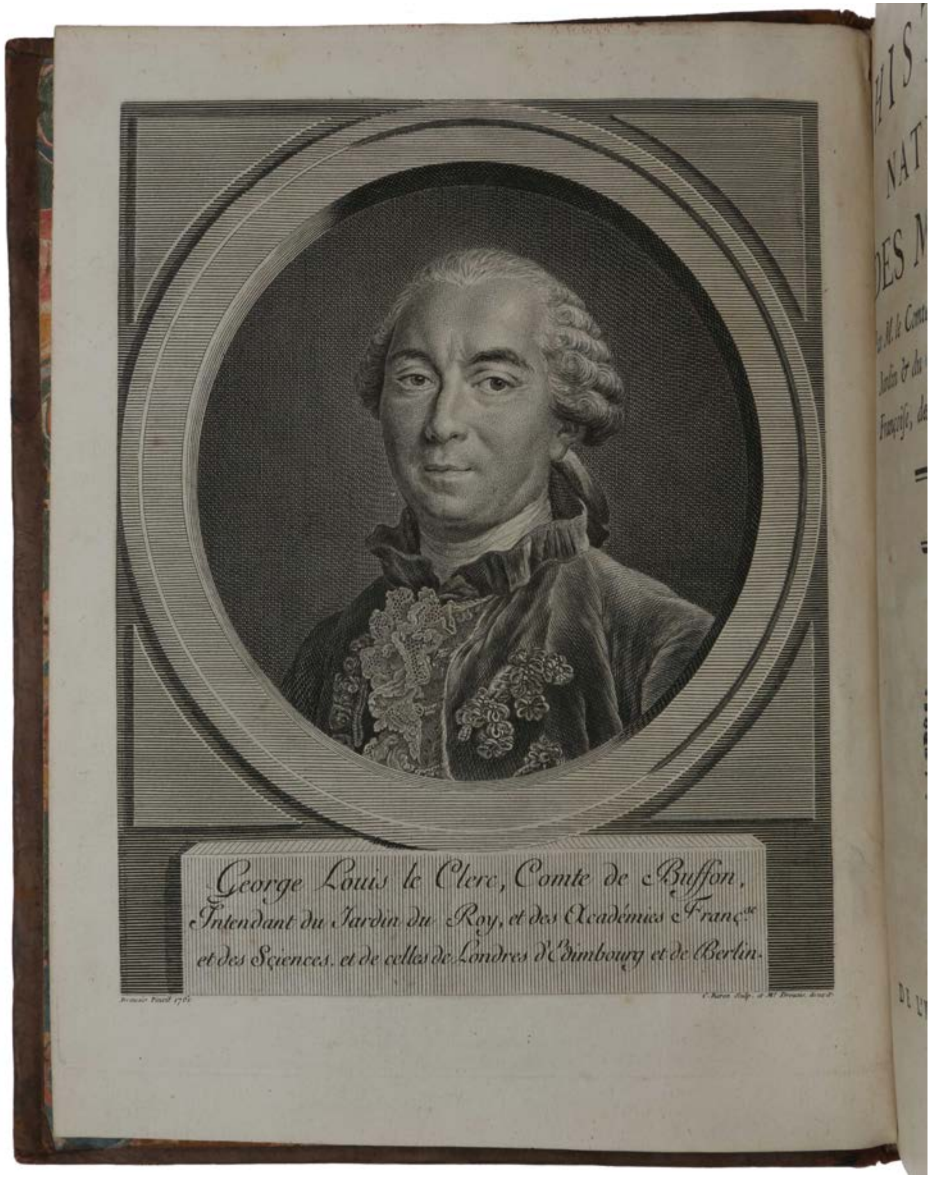
"Buffon's monumental *Histoire Naturelle, Générale et Particulière*, his one great work, was the principal commercial rival to Diderot's thirty-five volume *Encyclopédie*, the most impressive publishing venture of the age, and the two have often been compared. Like the *Encyclopédie*, the *Histoire Naturelle* was a vast repository of authenticated fact that remained in use as a general reference work long after the death of its compiler. It differed from the *Encyclopédie*, however, in that the presentation of factual data was everywhere subordinated to the steady unfolding of a singularly unified world view. Buffon, like Diderot, employed collaborators, but only a few and never on the basis of equality; their contributions, by their own efforts and his editing, were transformed into pastiches of his style;

the guiding ideas were always his. The *Encyclopédie* was a team effort, given a measure of unity by the organising genius of Diderot; the *Histoire Naturelle*, despite assistance received, was entirely Buffon's.

"In the early 1740s, following his appointment as superintendent of the Jardin du Roi in Paris, Buffon undertook the preparation of an analytical and descriptive catalogue of the Jardin's already extensive collection of botanical and zoological specimens, and the *catalogue raisonné* shortly developed into the *Histoire Naturelle* … The thirty-five volumes he himself saw through the press included three general introductory volumes, twelve volumes on mammals, nine volumes on birds, five volumes on minerals, and six volumes entitles *Suppléments*.

"In the more general essays, Buffon digressed freely, touching upon (or discussing at considerable length) a variety of moral, social, theological, and philosophical questions that have little obvious connection to natural history … On the whole, however, the *Histoire Naturelle* was devoted to matters more suitable for presentation to the Académie Royale des Sciences, of which Buffon was perpetual treasurer, such as accounts of experiments he himself had performed, extended descriptions of specimens gathered from the four corners of the earth, general, definitive descriptive accounts of mammals, birds, and minerals, and the exposition of theories concerning the probably interrelationships of the phenomena described" (Fellows & Milliken, pp. 21-2).

"Buffon's longest and most ambitious project within his central work, the *Histoire Naturelle*, was his attempt to extract a simple and straightforward account of the origin and development of the 'terraqueous globe' from data drawn from the almost brand-new earth sciences of his time. The first essay to be completed, *La Théorie de la Terre* (Vol. I), was dated 1744, and the definitive essay, the most famous of all his essays, the *Époques de la Nature* (Supp. Vol. V), was published in 1773, a third



of a century later … the field of cosmogony, the elaboration of a general theory explaining both the origin and 'mechanism' of the solar system and the earth itself, seemed to offer an opportunity to 'complete' the work of Sir Isaac Newton, to add something new, something definitely Buffonian, to the fundamental principles of the 'New Physics' created by Newton, and thus to assume, in the eyes of future generations, a stature equal to that of Newton himself" (*ibid*., p. 66).

"In the *Théorie de la terre*, Buffon, like most of his contemporaries, states neptunian views. He has no hesitations about animal or plant fossils or the stratigraphic principles set forth by Steno. The presence of sea fossils and sedimentation of rock beds indicate former submersion of present continents, of which the topography, shaped under the water by ocean currents, is diminished by erosion and the action of the waters that carry earth to the sea. No explanation of the re-emergence of formerly submerged continents is offered. Buffon resolutely refused to accept the notion of catastrophes, including the biblical flood, which many of his contemporaries upheld. He offered several hypotheses (such as subsidence of the ground or earthquakes) to account for the displacement of the sea, but he insisted that such changes 'came about naturally'. Buffon was an advocate of 'real causes': 'In order to judge what has happened, or even what will happen, one need only examine what is happening… Events which occur every day, movements which succeed each other and repeat themselves without interruption, constant and constantly reiterated operations, those are our causes and our reasons'.

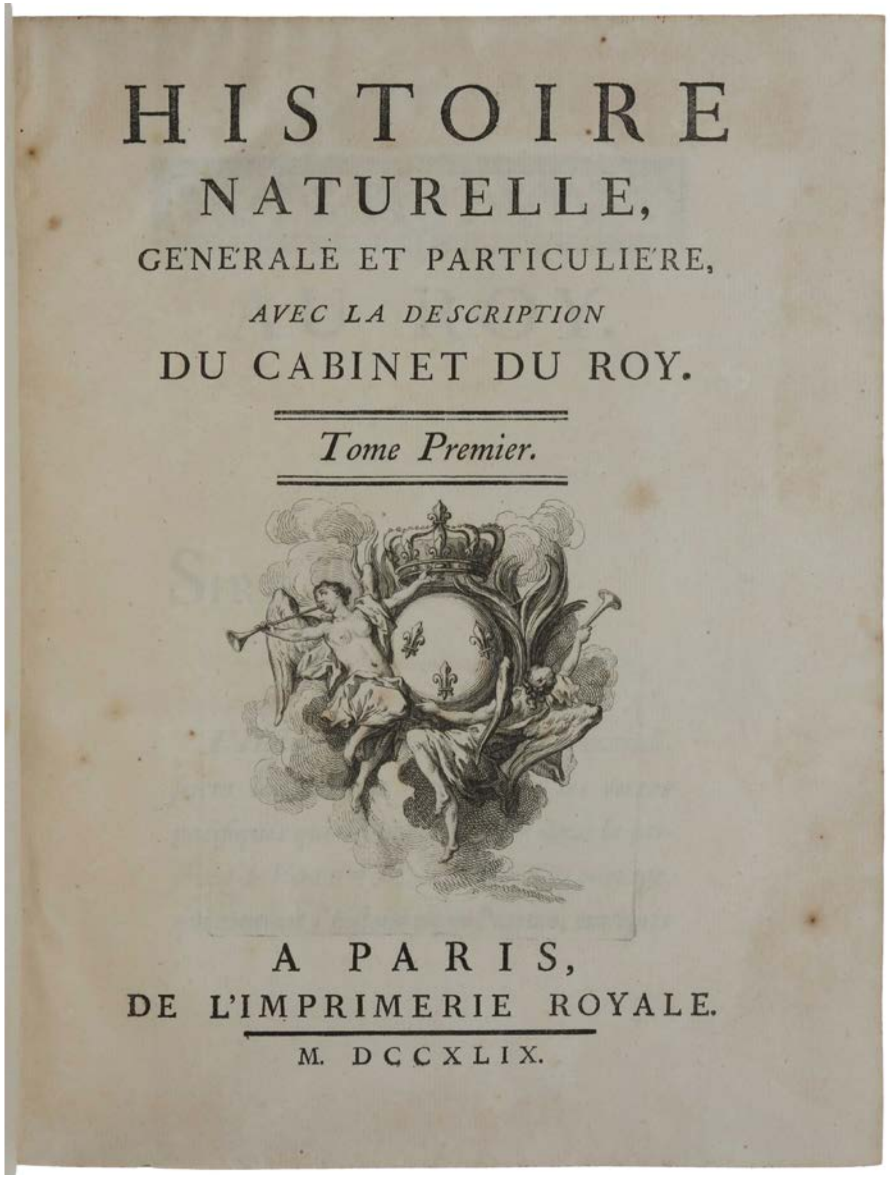
"On the other hand, in his cosmogony Buffon also rejected slow causes. According to Newton, planets and their movement had been created directly by God: this was the only possible explanation of the circumstance that the six planets then known revolved in the same direction, in concentric orbits, and almost on the same plane. Buffon's cosmogony was designed to replace the intervention of God by means of a natural phenomenon, a 'cause whose effect is in accord with the laws of mechanics'. He then hypothesized that a comet, hitting the sun tangentially, had projected into a space a mass of liquids and gases equal to 1/650 of the sun's mass. These materials were then diffused according to their densities and reassembled as spheres which necessarily revolved in the same direction and on almost the same plane. These spheres turn on their own axis by virtue of the obliquity of the impact of the comet on the sun; as they coalesced, they assumed the form of spheroids flattened on both poles. Centrifugal force, due to their rapid rotation, tore from these spheres the material that then became the satellites of the new planets.

"This cosmogony, one of the first based on Newtonian celestial mechanics, is remarkable for its coherence. It is founded on the then generally accepted idea that comets are very dense stars, at least at their nucleus. But it also raises some serious difficulties, which were brought to light by Euler: according to the laws of mechanics, the material torn from the sun should have fallen back into it after the first revolution; the densest planets should be farthest away from the sun; and the planetary orbits should always coincide at the point of initial impact. Finally, as early as 1770, it became apparent that comets had a very low density, which destroyed the impact hypothesis.

"The *Époques de la nature* presents a plutonian history of the earth—a piece was torn from the sun, the mass took form, the moon was torn from it by centrifugal force, and then the globe solidified during the first epoch. In the course of this solidification, primitive mountains, composed of 'vitreous' matter, and mineral deposits were formed (marking the second epoch). The earth cooled, and water vapors and volatile materials condensed and covered the surface of the globe to a great depth. The waters were soon populated with marine life and displaced the 'primitive vitreous material', which was pulverized and subjected to intense chemical activity. Sedimentary soil was thus formed, derived from rocks composed of primitive vitreous matter, from calcareous shells, or from organic debris, especially vegetable debris such as coal. In the meantime, the water burst through the vaults of vast subterranean caverns formed during the cooling period; as it rushed in, its level gradually dropped (third epoch). The burning of the accumulated combustible materials then produced volcanoes and earthquakes, the land that emerged was shaped in relief by the eroding force of the waters (fourth epoch). The appearance of animal life (fifth epoch) preceded the final separation of the continents from one another and gave its present configuration to the surface of the earth (sixth epoch) over which man now rules (seventh epoch).

"This work is of considerable interest because it offers a history of nature, combining geology with biology, and particularly because of Buffon's attempt to establish a universal chronology. From his experiments on cooling, he estimated the age of the earth to be 75,000 years. This figure is considerable in comparison to contemporary views which set the creation of the world at 4000–6000 BC. In studying sedimentation phenomena, however, Buffon discovered the need for much more time and estimated a period of as long as 3,000,000 years. That he abandoned that figure (which appears only in the manuscript) to return to the originally published figure of 75,000 years, was due to his fear of being misunderstood by his readers. He himself thought that 'the more we extend time, the closer we shall be to the truth' (*Époques de la nature*, p. 40).

"The *Époques de la nature* contains a great deal of mineralogical material that was restated and elaborated in the *Histoire naturelle des minéraux*. Buffon's work on mineralogy was handicapped by its date of appearance, immediately before the work of Lavoisier, Haüy, and Romé de l'Isle. Although it was soon out of date, Buffon's book does contain some interesting notions, particularly that of the 'genesis of minerals', that is, the concept that present rocks are the result of profound transformations brought about by physical and chemical agents. Buffon did not have a clear concept of metamorphic rocks, however. It is also noteworthy that



HISTOIRE
NATURELLE,
GÉNÉRALE ET PARTICULIÈRE,
AVEC LA DESCRIPTION
DU CABINET DU ROY.

Tome Premier.

A PARIS,
DE L'IMPRIMERIE ROYALE.
M. DCCXLIX.

Buffon was one of the first to consider coal, 'the pyritous and bituminous matter', and all of the mineral oils as products of the decomposition of organic matter.

"In the second volume of the *Histoire naturelle* (1749), Buffon offers a short treatise on general biology entitled *Histoire des animaux*. He takes up this subject again in the *Discours sur la nature des animaux* (Vol. IV) and in a great many later texts. Although he deals with nutrition and development in these, he is most interested in reproduction. This, of course, was a question much discussed at that time, but for Buffon reproduction represented the essential property of living matter.

"Buffon rejected the then widely accepted theory of the pre-existence and preformation of embryos. He spurned its dependence on the direct intervention of God and held it to be incapable of explaining heredity. He further refuted the connected theories of ovism and animalculism because no one had actually seen the egg of a viviparous animal and because spermatozoa were not 'animalcules', but rather aggregates of living matter that were also to be found in female sexual organs …

"He set forth the principle of epigenesis because it exists in nature and allows heredity to be understood. Buffon revived the ideas of certain physicians of the late seventeenth century who were faithful to an old tradition, and assumed that nutritive matter was first used to nourish the living being and then was utilized in the reproduction process when growth was completed. After being ingested, the nutritive matter received a particular imprint from each organ, which acted as a matrix in the reconstitution of that organ in the embryo. But Buffon departs from his predecessors on two points: (1) he sees the action of these molds as capable of modifying the nutritive substance internally, due to 'penetrating forces' (conceived of on the basis of Newtonian attraction), and (2) he considers nutritive material to be already living. Buffon also conceived of living universal matter composed of "organic molecules", which are a sort of living atom. His thinking was therefore formed by a mechanistic tradition, complicated by Newton's influence, and balanced by a tendency toward vitalist concepts.

"This tendency diminished as time passed. In 1779, in the *Époques de la nature*, Buffon dealt with the appearance of life on the earth—that is, the appearance of living matter, or organic molecules. He explained that organic molecules were born through the action of heat on "aqueous, oily, and ductile" substances suitable to the formation of living matter. The physicochemical conditions that made such formation possible were peculiar to that period of the earth's history; consequently spontaneous generation of living matter and organized living creatures can no longer occur. Buffon thus resolved the contradiction in his text of 1749, in which he maintained that while living matter was totally different from the original matter, nevertheless 'life and animation, instead of being a metaphysical point in being, is a physical property of matter' …

"Because he rejected the concept of family and denied the value of making classifications, Buffon also rejected, at the beginning of his work, the hypothesis of generalized transformism offered by Maupertuis in 1751 in the *Système de la nature*. Buffon's theory of reproduction and the role he attributes to the 'internal mold', as the guardian of the form of the species, prevented him from being a transformist. This same theory of reproduction did not prevent Buffon from believing in the appearance of varieties within a species, however. Buffon believed in the heredity of acquired characteristics; climate, food, and domestication modify the animal type. From his exhaustive research for the *Histoire naturelle des quadrupèdes*, Buffon came to the conclusion that it was necessary to reintroduce the notion of family. But he attributes to this word—or to the word *genus*, which he also uses—a special meaning: a family consists of animals which although separated by 'nature', instinct, life style, or geographical habitat are nevertheless able to produce viable young (that is, animals which belong biologically to the same species, e.g., the wolf and the dog). What the naturalist terms species and family, then, will thus become, for the biologist, variety and species. Buffon was thus able to write, in 1766, the essay *De la dégénération des animaux*—in which he showed himself to be a forerunner of Lamarck—while he continued to affirm the permanence of species in the two *Vues de la nature* (1764–1765) and *Époques de la nature* (1779).

"Buffon's final point of view concerning the history of living beings can be summarized as follows: No sooner were organic molecules formed than they spontaneously grouped themselves to form living organisms. Many of these organisms have since disappeared, either because they were unable to subsist or because they were unable to reproduce. The others, which responded successfully to the essential demands of life, retained a basically similar constitution— Buffon affirms unity in the plan of animals' composition and, in variations on that plan, the principle of the subordination of organs. Since the earth was very hot and 'nature was in its first stage of activity', the first creatures able to survive were extremely large. The earth's cooling drove them from the North Pole toward the equator and then finally caused their extinction. Buffon offered this in explanation of the giant fossils discovered in Europe and North America, which he studied at length (to the point of becoming one of the founders of paleontology). The organic molecules which were left free in the northern regions formed smaller creatures which in turn moved toward the equator, and then a third and fourth generation, which also moved south. Originating in Siberia, these animal species spread out to southern Europe and Africa, and toward southern Asia and North America. Only South America had an original fauna, different from that of other continents.

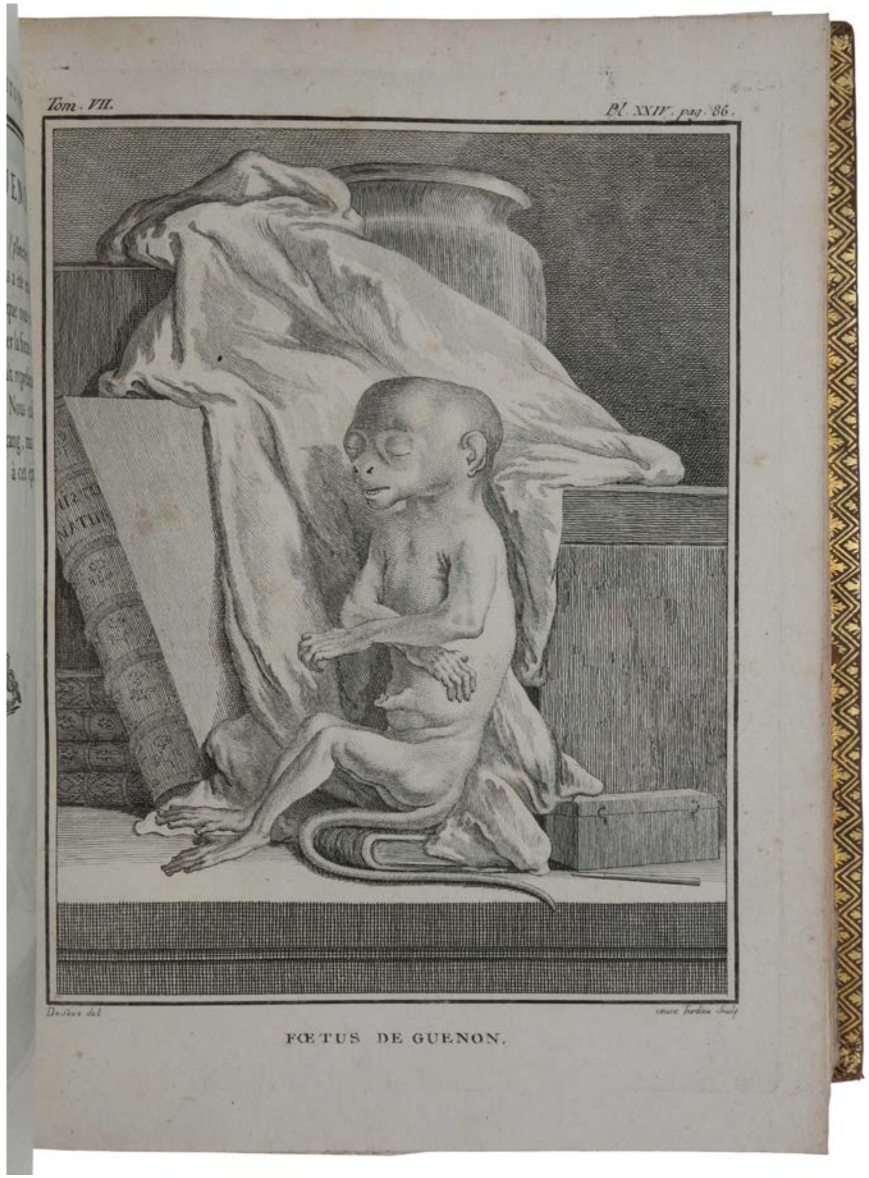
"In the process of migration, the species varied in response to environment. There are few varieties of the large mammals because they reproduce slowly. The smaller mammals because they reproduce slowly. The smaller mammals (rodents, for example) offer a large number of varieties because they are very prolific. The same is true of birds. Going back to the basic types, quadrupeds may be divided into thirteen separate species and twenty-five genera. But Buffon was not a transformist, because he believed that these thirty-eight primitive types arose spontaneously and simultaneously from an assembly of organic molecules …

"In the *Histoire naturelle de I'homme*, published in 1749 (Vols. II, III), and in many of his other works as well, Buffon studied the human species by the same methods that he applied to animal species, including the psychological, moral, and intellectual life of man. At the same time that he proclaimed the absolute superiority that the ability to reason gives man over animals, he demonstrated how the physiological organization and development of the sensory organs make reasoning possible. Throughout his work Buffon specifies that reason developed only through language, that language grew out of life in society, and that social life was necessitated by man's slow physiological growth (since man is dependent on his mother long after birth). For the same reason, the elephant is the most intelligent of animals, while social life makes beavers capable of astonishing work.

"It was, therefore, as a physiologist and as a naturalist that Buffon studied man and his reason; and it was as a biologist that he affirmed the unity of the human species. Aside from a few safe formulas, theology never comes into the picture. According to the *Époques de la nature*—and, in particular according to its manuscript—it is clear that the human species has had the same history as the animals. Buffon even explains that the first men, born on an earth that was still hot, were black, capable of withstanding tropical temperatures. Through the use of the resources of his intelligence and because of the invention of fire, clothes, and tools, man was able to adapt himself to all climates, as animals could not. Man is therefore the master of nature; and he can become so to an even greater degree if he begins to understand 'that science is his true glory, and peace his true happiness' (*Époques de la nature*, p. 220)" (DSB).

'[Buffon] brought forward an impressive array of facts suggesting evolutionary changes … It fascinated him as, a century later, it was to fascinate Darwin. He had devised a theory of 'degeneration'. The word sounds odd and a trifle morbid today, because we are in the habit of thinking of life as 'evolving', 'progressing' from one thing to another. Nevertheless, Buffon's 'degeneration' is nothing more than a rough sketch of evolution. He implied by this term simply change, a falling away from some earlier type of animal into a new mold. Curiously enough, as his work proceeded, Buffon managed, albeit in a somewhat scattered fashion, at least to mention *every significant ingredient which was to be incorporated into Darwin's great synthesis of 1859* [i.e., *Origin of Species*]" (Eiseley, p. 39).

Dibner 193 (33 vols. only); *En Français dans le texte* 152; Nissen ZBI 672; Norman 369; PMM 198; Sparrow 23; Ward and Carozzi 383 (36 vols. only). Eiseley, *Darwin's Century*, 1958. Fellows & Milliken, *Buffon*, 1972.



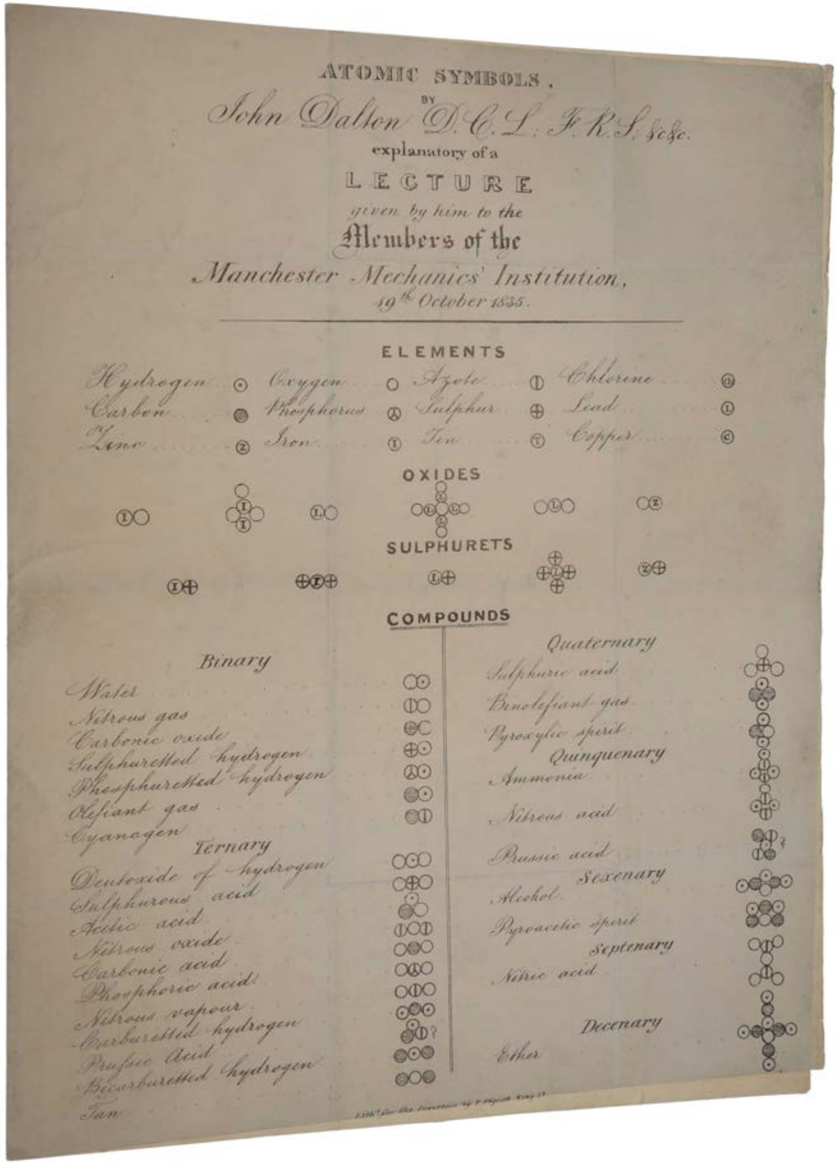
THE ATOMIC SYMBOLS EXPLAINED

DALTON, John. *Atomic Symbols by John Dalton, explanatory of a Lecture given by him to the Members of the Manchester Mechanics' Institution, October 19th, 1835.* [Manchester]: Lith[ographed] for the Directors by F. Physick, King St., [1835].

$5,000

Folio (291 x 223mm), two leaves, first leaf printed on recto only, second leaf blank, three horizontal and two vertical creases where folded.

First edition, extremely rare, of this "lithographed table of atomic symbols and the structure of various chemical compounds, which was distributed by Dalton during his last public lecture to the Manchester Mechanics' Institution in 1835. A year and a half later he suffered two paralytic strokes that left him a semi-invalid for the rest of his life. 'Following the invitation of the Directors … Dalton gave a course of five lectures on meteorology beginning in March 1835. Later in the year, Dalton gave a lecture at the Institution on Atomic Theory: To the audience was distributed a lithographed sheet of atomic symbols … the lecture-room was crowded in every part and the greatest anxiety was manifested by the audience not to lose a single word which fell from the lips of the speaker … It was his last public lecture' (Smyth). Later versions of the table, set in different type, are included in the biographies of Dalton by Henry (1854), Lonsdale (1874), and Roscoe (1895). Each of the three reprints is separately redrawn, but none includes the information about the lithographer, T. Physick, who is listed in the Manchester Directory for 1832 as a lithographic printer. The present (possibly unique) copy is described in the second edition of Smyth's bibliography of Dalton" (Neville, I, p. 321). "Dalton's chemical atomic theory was the first to give significance to the relative weights of the ultimate particles of all known compounds, and to provide a quantitative explanation of the phenomena of chemical reaction. Dalton



believed that all matter was composed of indestructible and indivisible atoms of various weights, each weight corresponding to one of the chemical elements, and that these atoms remained unchanged during chemical processes. Dalton's work with relative atomic weights prompted him to construct the first periodic table of the elements to formulate laws concerning their combination and to provide schematic representations of various possible combinations of atoms. His equation of the concepts 'atom' and 'chemical element' was of fundamental importance, as it provided the chemist with a new and enormously fruitful model of reality" (Norman 575). The number of copies of this table printed was presumably limited to the expected size of Dalton's audience for this single lecture, and its ephemeral nature must mean that only a small fraction of those printed have survived. Indeed, as noted above, the Chemical Heritage Foundation speculated in the Neville catalogue that that copy might be unique. OCLC lists only one other copy, at the University of Delaware. ABPC/RBH lists one example, tipped in to the Norman copy of Dalton's *New System of Chemical Philosophy* (this is described in the Norman sale catalogue, but not in the library catalogue).

"John Dalton is well known as the early nineteenth-century English chemist who advocated an atomic theory of chemistry. Closely connected with the atomic theory was a system of symbols in which Dalton denoted the atoms of different elements by circles containing a distinguishing pattern or letter. The important difference between Dalton's symbols and those used earlier was that the former represented a definite quantity of an element, whilst the latter signified any amount of the substance in question … This quantitative aspect of Dalton's symbols was inherited by the symbols of Berzelius and they still have this quantitative meaning today … Dalton's reason for representing atoms by circles was not arbitrary, but rather it was a deliberate attempt to picture the atoms as he imagined they really were. This applies also to the compound atoms which he usually drew symmetrically in accordance with his ideas on the repulsive influence of the atmosphere of caloric surrounding each atom" (Crosland, pp. 256-7).

"In his original paper on the atomic theory in 1803, as well as in his *New System of Chemical Philosophy* (1808), Dalton used pictorial symbols to illustrate his view of the structure of matter. He borrowed the use of pictures (instead of letters [as advocated by Berzelius]) to represent chemical elements from alchemy, with the important distinction that he meant each individual picture to represent specific quantities of atoms. Further, he placed symbols next to each other in an order which he took to be the actual spatial arrangement of the atom in a molecule … Thomas Thomson first published Dalton's symbols in the third edition of his *System of Chemistry*, and the following year Dalton himself presented a table of them in his *New System*. Despite the typographical problem which pictorial symbols presented, Dalton and Thomson continued to support their use through the 1820s … The most common justification for the continued use of pictorial symbols, despite the prevailing practice of following Berzelian notation on the continent, was its advantage in displaying the spatial configuration of compounds. This argument reflected a more central faith on the part of Dalton and his immediate followers that his atomic theory represented physical reality, and not merely a convenient device for calculating equivalent weights" (Alborn, pp. 440-1).

Born in a small village in the English Lake District, Dalton (1766-1844) moved to Manchester in 1793. After he arrived, he at first taught mathematics and natural philosophy at New College, a dissenting academy, and began observing the behavior of gases, but after six years he resigned. Thereafter he devoted his life to research, which he financed by giving private tuition. By 1800, Dalton had become the secretary of the Manchester Literary and Philosophical Society, and in 1801 he presented the first of a series of papers to the society describing the properties of 'mixed gases'. These papers laid the foundations of his atomic theory;

a paper of 1803 included the first table of atomic weights. In 1808 Dalton began the publication of his great work, *A New System of Chemical Philosophy*, which set out his atomic theory in detail; it was completed only in 1827.

Smyth's bibliography (pp. 43-45) records lectures given by Dalton in Manchester on various topics, including meteorology, mechanics, electricity, optics and astronomy, and from the mid 1820s most of these lectures were delivered to the Mechanics' Institution. However, the 1835 'Lecture on the atomic System of Chemistry' (Henry, p. 123) is his only recorded lecture on atomic theory; it was also his last lecture to the Mechanics' Institution.

The Manchester Mechanics' Institution was established on 7 April 1824. The original prospectus of the institution stated: 'The Manchester Mechanics' Institution is formed for the purpose of enabling Mechanics and Artisans, of whatever trade they may be, to become acquainted with such branches of science as are of practical application in the exercise of that trade; that they may possess a more thorough knowledge of their business, acquire a greater degree of skill in the practice of it, and be qualified to make improvements and even new inventions in the Arts which they respectively profess. It is not intended to teach the trade of the Machine-maker, the Dyer, the Carpenter, the Mason, or any other particular business, but there is no art which does not depend, more or less, on scientific principles, and to teach what these are, and to point out their practical application, will form the chief object of this Institution.'

"The establishment of societies throughout England, Wales and Scotland, and also in Ireland, having for their object the instruction of working men in the scientific principles upon which the industrial arts are based, was a phenomenon of apparently sudden appearance about the year 1824. Two immediate causes determined the year of origin. After the post-war period of economic and social chaos trade conditions were by that date improving and a two-year trade-boom had begun; and this improvement was accompanied by an abatement of social strife … Secondly, it was not until after 1820 that a group of influential public men had become aware of the success of recent experiments in the education of working men and had been personally associated with at least one of these enterprises" (Tylecote, p. 1).

Alborn, 'Negating Notation: Chemical Symbols and British Society, 1831-1835,' *Annals of Science* 46 (1989), 437-460. Henry, *Memoirs of the Life and Scientific researches of John Dalton* (1854). Neville, *The Roy G. Neville Historical Chemical Library*, 2006. Smyth, *John Dalton 1766-1844. A Bibliography of works by him and about him* (1966). Thackray, 'Fragmentary remains of John Dalton,' *Annals of Science* 22 (1966), 145-174. Tylecote, *The Mechanics Institutes of Lancashire and Yorkshire before 1851* (1957). Dibner 44; Horblit 22; Norman 575; PMM 261 (all for Dalton's *New System*).

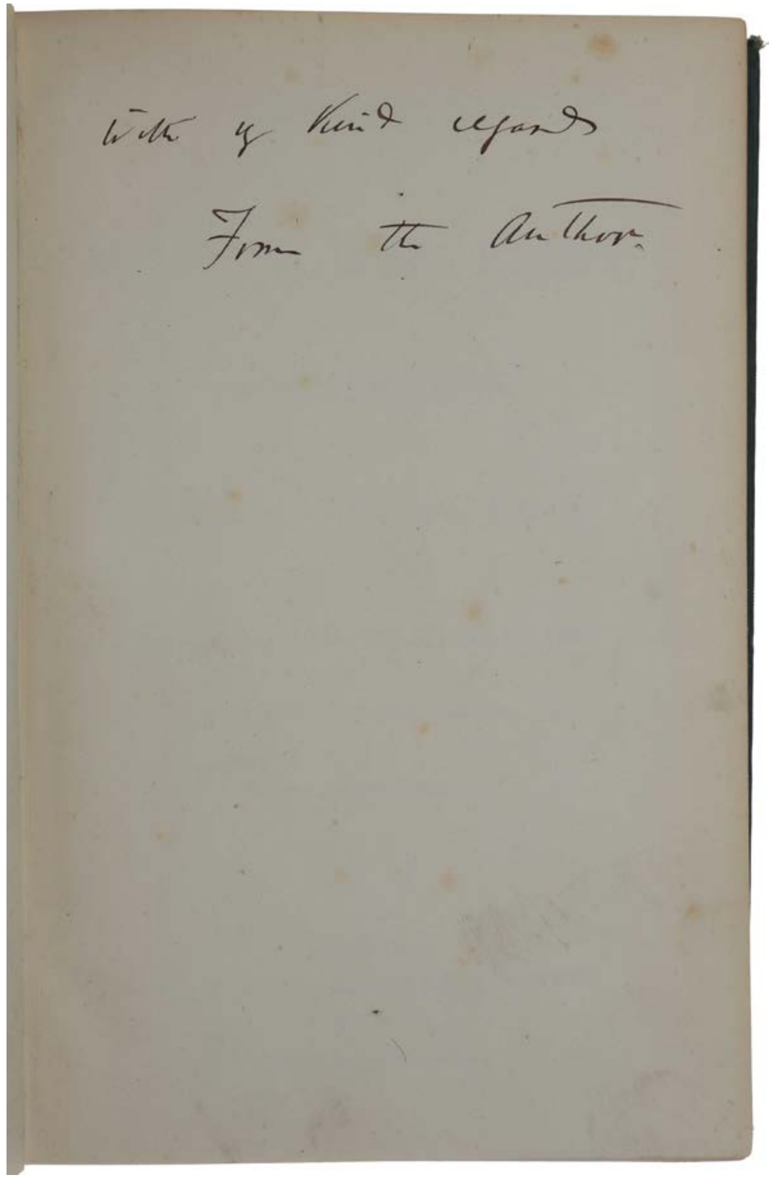
PRESENTATION COPY INSCRIBED IN DARWIN'S HAND

DARWIN, Charles. *The Variation of Animals and Plants under Domestication ... Second edition, revised, fourth thousand.* London: John Murray, 1875.

$85,000

Two volumes, crown octavo (185 x 120mm), pp. xiv, 473, [1]; x, 495, [1], 32 (advertisements for John Murray's books dated January 1876), with 43 woodblocks in text (light spotting on titles). Original green cloth, arches style, with covers stamped with blind frame, gilt spines (extremities rubbed).

Presentation copy, **inscribed in Darwin's hand**, of the second and definitive edition of the only section of Darwin's 'big book' on the origin of species which was printed in his lifetime. This copy is further remarkable in having manuscript revisions, undoubtedly dictated by Darwin, in the hand of Darwin's then amanuensis, his son Francis. These corrections were very likely for the benefit of a translator, to whom the book was presented (see below). About the first edition, published in January 1868, Francis Darwin recorded that "about half of the eight years that elapsed between its commencement and completion were spent on it. The book did not escape adverse criticism: it was said, for instance, that the public had been patiently waiting for Mr. Darwin's *pièces justicatives*, and that after eight years of expectation all they got was a mass of detail about pigeons, rabbits and silk worms. But the true critics welcomed it as an expansion with unrivalled wealth of illustration of a section of the *Origin*" (*The Autobiography of Charles Darwin and Selected Letters*, ed. F. Darwin, New York, 1958, p. 281). "Its two volumes were intended to provide overwhelming evidence for the ubiquity of variation



… He gave numerous instances of the causes of variability, including the direct effect of the conditions of life, reversion, the effects of use and disuse, saltation, prepotency, and correlated growth. The *Variation* also addressed a key criticism of the *Origin of Species*: that it lacked an adequate understanding of inheritance" (*ODNB*). Along with the ascertainable facts of artificial selection, *Variation* also contained Darwin's hypothesis of 'Pangenesis,' his hypothetical mechanism for heredity. For this second edition the text was substantially revised, with additions culled from the hundreds of letters and scores of monographs he had received over the past seven years. In this period, "Darwin eased into a more adaptationist frame of mind, suggesting that there was a role in evolutionary theory for the inheritance of acquired characteristics … Although he had never categorically excluded behaviourally or environmentally induced adaptations from his writings, he now felt that they should play a larger part" (Browne, p. 407). Most notably, Darwin modified his views on Pangenesis in ways that have been supported by recent discoveries in molecular genetics (see below). This is the final edition of the text – all subsequent editions were printed from stereotyped plates.

This copy is trimmed and in the special publisher's presentation binding. Darwin detested having to open the top edges of his books with knives and, in his later years, demanded that his publisher produce a very small number of trimmed copies for presentation purposes. Francis Darwin wrote, "This was a favourite reform of my father's. He wrote to the *Athenaeum* on the subject, Feb. 5, 1867, pointing out that a book cut, even carefully, with a paper knife collects dust on its edges far more than a machine-cut book … He tried to introduce the reform in the case of his own books but found the conservatism of booksellers too strong for him. The presentation copies of all his later books were sent out with the edges cut" (*Life and Letters*).

Among the other recipients of the 25 presentation copies of the second edition of *Variation* were Darwin's sons Francis and George, Asa Gray, Galton, Haeckel, and Huxley.

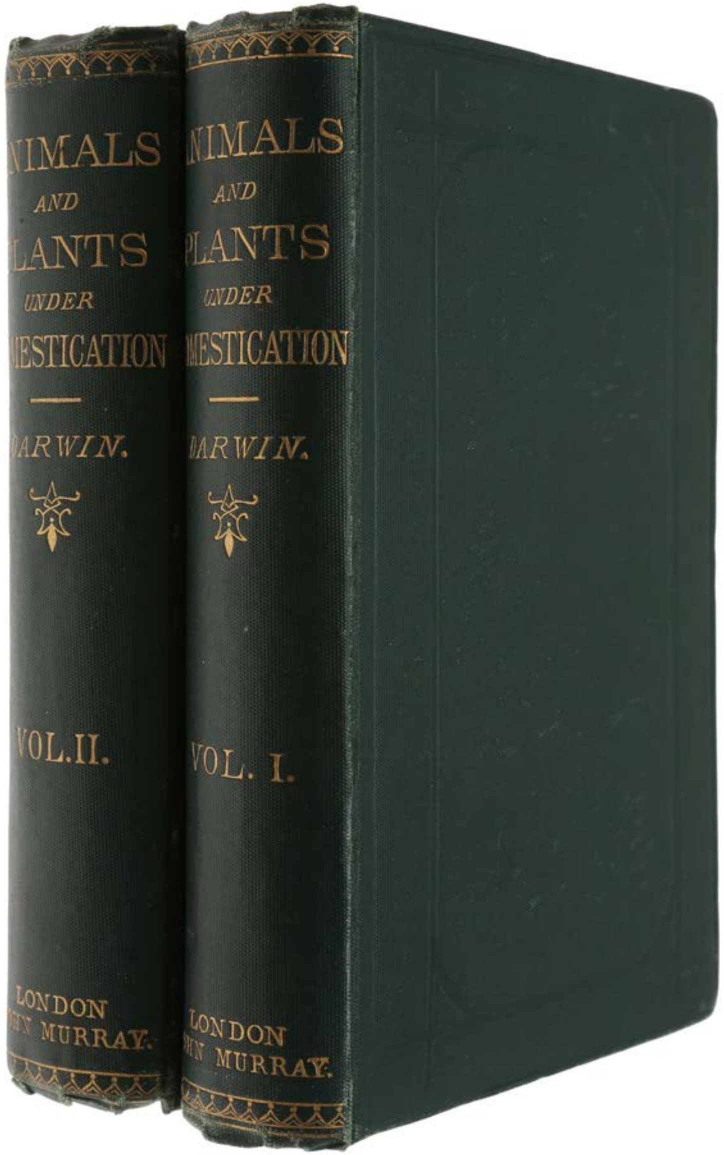
Provenance: Charles Darwin (1809-82) (presentation inscription on front free endpaper: 'With very kind regards | From the Author'). There were 25 recipients of presentation copies of this second edition (*Darwin Correspondence*, vol. 24, p. 596f.), including the German Julius Victor Carus (1823-1903), and the Italian Giovanni Canestrini (1835-1900), the translators of the 3rd German (1878; Freeman 916) and first Italian (1876; Freeman 920) editions of *Variation*, respectively. It is possible that this copy is one of these: the fact that the inscription is in Charles' hand – rather than in the hand of the publisher's clerk as often found – suggests this is an important association copy. Moreover, the corrections correspond largely with revisions in those editions. These textual corrections are found on pp. 170, 262, 264, 425, 434 and 442 of vol. I; and in the index only, on pp. 431, 439, 450, 456 and 461, of vol. II. The hand is identifiable as that of Francis Darwin, Charles' amanuensis at that period.

On the Origin of Species was only an abstract of the long manuscript Darwin had begun writing on 14 May 1856 which he originally intended to complete and publish as the formal presentation of his views on evolution. Compared with the *Origin,* this work, which was to be titled *Natural Selection*, has more abundant examples in illustration of Darwin's argument plus an extensive citation of sources. It had reached a length of over one quarter of a million words and was well over half completed when on 18 June 1858 Darwin's writing was dramatically interrupted when he received an essay from Alfred Russel Wallace in Borneo entitled *On the Tendency of Species to form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection* outlining his astonishingly parallel but independently conceived theory of natural selection. Darwin felt obliged to change his plans for initial publication; and, after the brief preliminary announcement was presented jointly with Wallace's paper at the Linnaean Society of London, he rapidly wrote out in eight months the new abstract of his views which appeared as the *Origin of Species* in 1859. But he still planned to publish a more extensive account of his views on evolution, and he did not abandon his long manuscript, nor write on the unused backs of the sheets for drafting other new publications as he so often did with other manuscripts.

"In the introduction [to *Origin of Species*, Darwin] announced that in a future publication he hoped to give 'in detail all the facts, with references, on which my conclusions have been grounded.' On 9 January 1860, two days after the publication of the second edition of *Origin*, Darwin returned to his original *Natural Selection* manuscript and began expanding the first two chapters on 'Variation under Domestication.' He had a large collection of additional notes and by the middle of June had written drafts of an introduction and two chapters on the domestication of pigeons that would eventually form part of *The Variation of Animals and Plants under Domestication*. Darwin apparently found writing the book tiresome and writes in his autobiography that he had been 'tempted to publish on other subjects which at the time interested me more' …

"Darwin continued to gather data. His own practical experiments were confined to plants but he was able to gather information from others by correspondence and even to arrange for some of his correspondents to conduct experiments on his behalf. In spite of protracted periods of illness, he made progress and in March 1865 wrote to his publisher, John Murray, saying that 'Of present book I have 7 chapters ready for press & all others very forward, except the last & concluding one' (the book as finally published consisted of 28 chapters). In the same letter he discussed illustrations for the book.

"Darwin had been mulling for many years on a theory of heredity. In May 1865



he sent a manuscript to his friend Thomas Huxley outlining his theory which he called pangenesis and asking whether he should publish it. In his accompanying letter Darwin wrote: 'It is a very rash & crude hypothesis yet it has been a considerable relief to my mind, & I can hang on it a good many groups of facts.' Huxley pointed out the similarities of pangenesis to the theories of Georges-Louis LeClerc, Comte de Buffon, and the Swiss naturalist Charles Bonnet, but eventually wrote encouraging Darwin to publish: 'Somebody rummaging among your papers half a century hence will find Pangenesis & say 'See this wonderful anticipation of our modern Theories—and that stupid ass, Huxley, prevented his publishing them".

"Just before Christmas 1866 all of the manuscript except for the final chapter was sent to the publisher. At the beginning of January, on receiving an estimate of the size of the two-volume book from the printers, he wrote to his publisher: 'I cannot tell you how sorry I am to hear of the enormous size of my Book.' He subsequently arranged for some of the more technical sections to be set in smaller type.

"Even at this late stage Darwin was uncertain as to whether to include a chapter on mankind. At the end of January he wrote to Murray: 'I feel a full conviction that my Chapter on man will excite attention & plenty of abuse & I suppose abuse is as good as praise for selling a Book,' but he then apparently decided against the idea for a week later in a letter to his close friend Joseph Hooker he explained 'I began a chapter on Man, for which I have long collected materials, but it has grown too long, & I think I shall publish separately a very small volume, 'An essay on the origin of mankind". This 'essay' would become two books: *The Descent of Man, and Selection in Relation to Sex* (1871) and *The Expression of Emotions in Man and Animals* (1872). The book had been advertised as early as 1865 with the unwieldy title *Domesticated Animals and Cultivated Plants, or the Principles of Variation, Inheritance, Reversion, Crossing, Interbreeding, and Selection under Domestication* but Darwin agreed to the shorter *The Variation of Animals and Plants Under Domestication* suggested by the compositors …

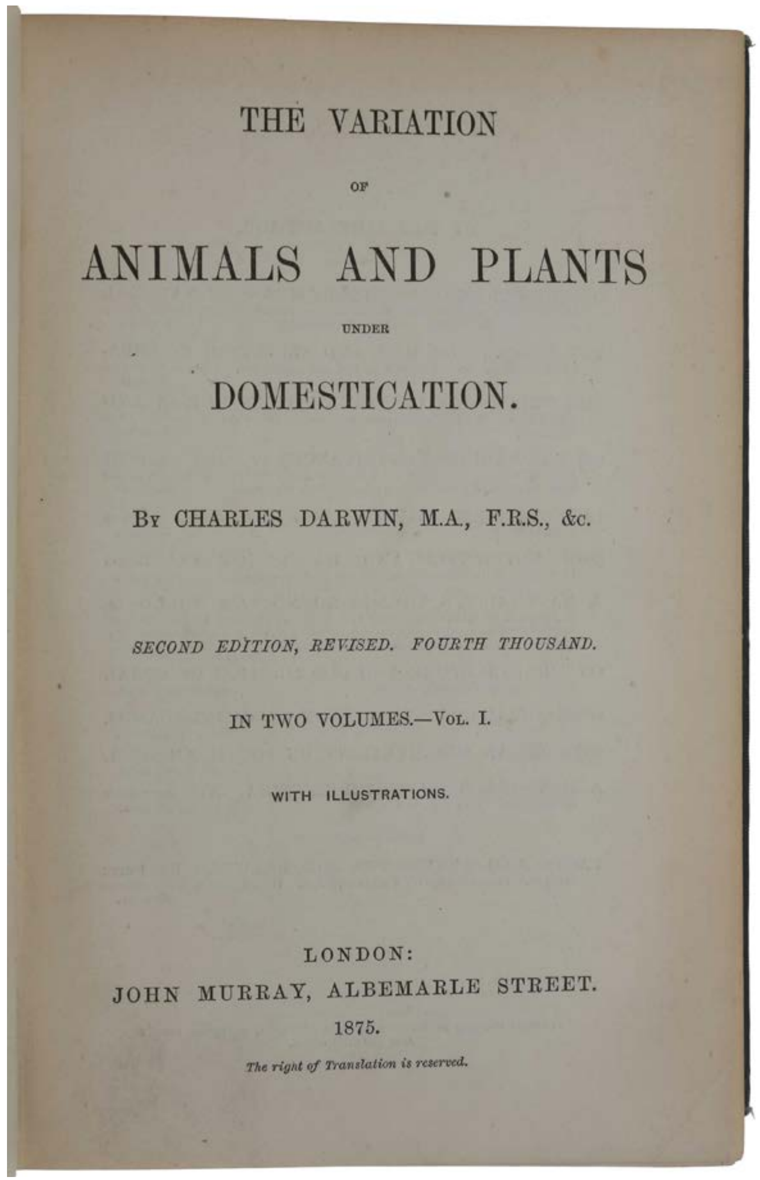
"Darwin received the first proofs on 1 March 1867. In the tedious task of making correction he was helped by his 23-year-old daughter Henrietta Emma Darwin. In the summer while she was away in Cornwall he wrote to commend her work, 'All your remarks, criticisms doubts & corrections are excellent, excellent, excellent.' While making corrections Darwin also added new material.The proofs were finished on 15 November, but there was a further delay while William Dallas prepared an index. *The Variation of Animals and Plants under Domestication* went on sale on 30 January 1868, thirteen years after Darwin had begun his experiments on breeding and stewing the bones of pigeons. He was feeling deflated, and concerned about how these large volumes would be received, writing: 'if I try to read a few pages I feel fairly nauseated ... The devil take the whole book." In his autobiography he estimated that he had spent 4 years 2 months 'hard labour' on the book.

"The first volume of *The Variation of Animals and Plants under Domestication* consists in a lengthy and highly detailed exploration of the mechanisms of variation, including the principle of use and disuse, the principle of the correlation of parts, and the role of the environment in causing variation, at work in a number of domestic species. Darwin starts with dogs and cats, discussing the similarities between wild and domesticated dogs, and musing on how the species changed to accommodate man's wishes. He attempts to trace a genealogy of contemporary varieties (or 'races') back to a few early progenitors. These arguments, as well as many others, use the vast amount of data Darwin gathered about dogs and cats to support his overarching thesis of evolution through natural selection. He then goes on to make similar points regarding horses and donkeys, sheep, goats, pigs, cattle, various types of domesticated fowl, a large number of different cultivated plants, and, most thoroughly, pigeons.

“Notably, in Chapter XXVII Darwin introduced his ‘provisional hypothesis’ of pangenesis that he had first outlined to Huxley in 1865.He proposed that each part of an organism throws off minute invisible particles which he called gemmules. These were capable of generating a similar part of an organism, thus gemmules from a foot could generate a foot. The gemmules circulated freely around the organism and could multiply by division. In sexual reproduction they were transmitted from parents to their offspring with the mixing of the gemmules producing offspring with ‘blended’ characteristics of the parents. Gemmules could also remain dormant for several generations before becoming active. He also suggested that the environment might affect the gemmules in an organism and thus allowed for the possibility of the Lamarckian inheritance of acquired characteristics.Darwin believed that his theory could explain a wide range of phenomena:

All the forms of reproduction graduate into each other and agree in their product; for it is impossible to distinguish between organisms produced from buds, from self-division, or from fertilised germs ... and as we now see that all the forms of reproduction depend on the aggregation of gemmules derived from the whole body, we can understand this general agreement. It is satisfactory to find that sexual and asexual generation ... are fundamentally the same. Parthenogenesis is no longer wonderful; in fact, the wonder is that it should not oftener occur.

“In the final pages of the book Darwin directly challenged the argument of divinely guided variation advocated by his friend and supporter the American botanist Asa Gray. He used the analogy of an architect using rocks which had broken off naturally and fallen to the foot of a cliff, asking: ‘Can it be reasonably



THE VARIATION

OF

ANIMALS AND PLANTS

UNDER

DOMESTICATION.

BY CHARLES DARWIN, M.A., F.R.S., &c.

SECOND EDITION, REVISED. FOURTH THOUSAND.

IN TWO VOLUMES.—VOL. I.

WITH ILLUSTRATIONS.

LONDON:

JOHN MURRAY, ALBEMARLE STREET.

1875.

The right of Translation is reserved.

maintained that the Creator intentionally ordered ... that certain fragments should assume certain shapes so that the builder might erect his edifice?' In the same way, breeders or natural selection picked those that happened to be useful from variations arising by 'general laws', to improve plants and animals, 'man included'. Darwin concluded with: 'However much we may wish it, we can hardly follow Professor Asa Gray in his belief that 'variation has been along certain beneficial lines,' like a 'stream along definite and useful lines of irrigation".Darwin confided to Hooker: 'It is foolish to touch such subjects, but there have been so many allusions to what I think about the part which God has played in the formation of organic beings, that I thought it shabby to evade the question.'

"Darwin was concerned whether anyone would read the massive volumes and was also anxious to receive feedback from his friends on their views on pangenesis. In October 1867 before the book was published he sent copies of the corrected proofs to Asa Gray with the comment: 'The chapter on what I call Pangenesis will be called a mad dream, and I shall be pretty well satisfied if you think it a dream worth publishing; but at the bottom of my own mind I think it contains a great truth.' He wrote to Hooker: 'I shall be intensely anxious to hear what you think about Pangenesis,' and to the German naturalist Fritz Müller: 'The greater part, as you will see, is not meant to be read; but I should very much like to hear what you think of 'Pangenesis". Few of Darwin's colleagues shared his enthusiasm for pangenesis.Wallace was initially supportive and Darwin confided to him: 'None of my friends will speak out, except to a certain extent Sir H. Holland, who found it very tough reading, but admits that some view 'closely akin to it' will have to be admitted.'

"By the end of April *Variation* had received more than 20 reviews.An anonymous review by George Henry Lewes in the *Pall Mall Gazette* praised its 'noble calmness ... undisturbed by the heats of polemical agitation' which made the far from calm Darwin laugh, and left him 'cock-a-hoop' … De Vries in 1889 praised the 'masterly survey of the phenomena to be explained' and accepted the idea that 'the individual hereditary qualities of the whole organism are represented by definite material particles.' He introduced the notion of *intracellula pangenesis* which, following August Weismann, rejected the idea that these particles were thrown off from all the cells of the body. He called the particles 'pangens', later abbreviated to 'gene.' In a similar vein, Weismann in his 1893 work *Germ-Plasm* said: 'although Darwin modestly described his theory as a provisional hypothesis, his was, nevertheless, the first comprehensive attempt to explain all the known phenomena of heredity by a common principle ... [I]n spite of the fact that a considerable number of these assumptions are untenable, a part of the theory still remains which must be accepted as fundamental and correct – in principle at any rate – not only now but for all time to come"' (Wikipedia, accessed 16 November 2016).

In this second edition of *Variation*, "Darwin imagined that gemmules were 'inconceivably minute and numerous as the stars in heaven' and that 'many thousand gemmules must be thrown off from the various parts of the body at each stage of development' (p. 399). Today, we know that small RNAs [ribonucleic acids], particularly microRNAs, can be secreted from mammalian cells and circulate in blood and other body fluids. They are also capable of moving between plant cells and through the vasculature and play important roles in gene regulation, diverse cellular and developmental processes. In recent years, thousands of different RNAs have been identified in mammalian sperm, which supports Darwin's idea that 'almost infinitely numerous and minute gemmules are contained within each bud, ovule, spermatozoon, and pollen grain' (p. 397). Most recently, Gapp and colleagues demonstrated that stress in early life alters the production of microRNAs in the sperm of mice, which results in depressive behaviors in subsequent generations. Szyf proposed that microRNAs derived from the brains of mice that had undergone stressful experiences could make their way into the reproductive organ through the circulatory system and could then target the specific gene in sperm. Obviously, this proposal is consistent with Darwin's Pangenesis …

"Throughout his career, Darwin consistently attributed the causes of hereditary variation to changes in the environment. He clearly stated, 'There can be no doubt that the evil effects of the long-continued exposure of the parent to injurious conditions are sometimes transmitted to the offspring' (p. 57). In a letter to *Nature*, he claimed that many special fears in animals, which might be acquired through habit and the utility in, for example, predator avoidance, could be strictly inherited. His claim has now been confirmed by Dias and Ressler, who examined the inheritance of parental traumatic exposure and showed that an olfactory experience could be passed onto the progeny …

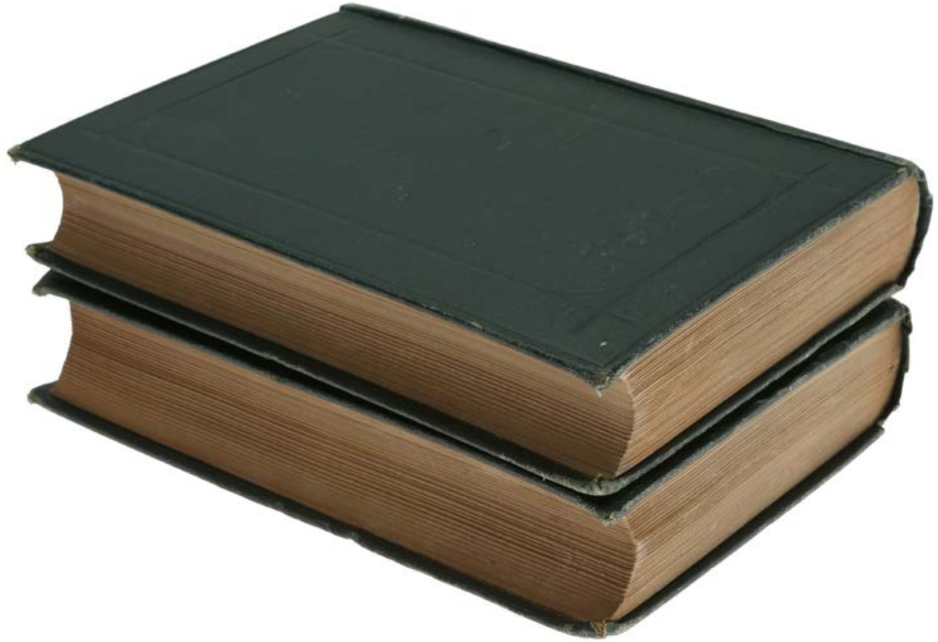
"In the history of biology, neglecting certain discoveries is not uncommon. It is well known that Mendel's experiment on plant hybridization was ignored for decades. For nearly 150 years after the formulation of Darwin's Pangenesis, it has been resolutely excluded from the pale of biological science and is now only of historical interest. However, we can affirm that Darwin's idea that pangenetic gemmules are the molecular carriers of hereditary characters and that they are diffused through the tissues or from cell to cell has been removed from the position of a provisional hypothesis to that of a well-founded theory. It is supported by the discovery of circulating nucleic acids in human blood and plant sap and the results of experimental work in inducing hereditary changes through blood transfusion in animals and through grafting in plants. The rediscovery of Darwin's Pangenesis, if and when it happens, would, like the rediscovery of Mendel's work, have a tremendous impact on genetics, evolution, cell biology, and the history of science" (Yongsheng Liu & Xiuju Li).

The corrections in this copy have been identified as being in the hand of Charles Darwin's son Francis (1848-1925), who in 1874 began acting as his amanuensis. Charles was ageing and had been unwell for many years, and he now reluctantly accepted his need for a secretary, principally to respond to the enormous volume of correspondence he received. "Then Francis Darwin offered to help with the workload, 'promising to be as civil as he could wish.' Darwin was reluctant to relinquish that task. 'When he did let me,' recalled Francis, 'he used always to say I did the civility well.' However, in 1874, Darwin capitulated and employed Francis as his secretary and assistant … That same year Francis married Amy Ruck, the daughter of a family friend from Wales, and came to live in a house in Downe village. Francis walked up the road every day to aid his father with botanical experiments and reply to correspondents. It seems not to have occurred to Francis that Darwin was giving him employment to compensate for his failure to purse the medical profession for which he was trained" (Browne, pp. 389-390).

"The first edition in English, of 1868, was in two volumes demy octavo, the only Murray Darwin to appear in this format, and it occurs in two issues. 1,500 copies of the first were published on January 30th, having been held up for the completion of the index. Murray had sold 1,250 at his autumn sale in the previous year and *Life and Letters* (Vol. III, p. 99) states that the whole issue was sold out in a week … The second, of 1,250 copies, was issued in February. In the present second edition the format was reduced to the usual crown octavo. The case is in arches style, with 32 pages of inserted advertisements dated January 1876" (darwinonline). "Darwin began work on the second edition of *Variation* on 6 July

1875, having suggested a new edition to his publisher, John Murray, in February. However, Darwin spent much of the spring of 1875 working on *Insectivorous plants*, which was published in July 1875. Publication of *Variation* 2d ed. was initially expected in November, and then December, but was held up by floods at the printers, William Clowes & Sons. It was finally published by the second half of February 1876; although it carries an 1875 imprint, it seems that the index did not reach the printer and the number of copies to print was not decided until 1876" (*Correspondence*, vol. 24, Appendix III).

Freeman 880; Norman 597 (for the first edition). Browne, *Charles Darwin. The Power of Place*, 2002. Yongsheng Liu & Xiuju Li, 'Has Darwin's Pangenesis Been Rediscovered?' *BioScience*, Vol. 64 (2014), pp. 1037–1041.



THE FIRST BOOK ON ALGEBRA IN FRENCH

DE LA ROCHE, Estienne. *Larismethique nouellement composee par maistre Estienne de La Roche dict Villefra[n]che natif de Lyo[n] sus le Rosne diuisee en deux parties dont la p[re]miere tracte des p[ro]prietes p[er]fectio[n]s et regles de la dicte scie[n]ce: come le no[m]bre entire, le no[m]bre rout, le regle de troys, la regle d'une faulse position, de deux faluses position[n]s, d'apposition et remotio[n], de la regle la chose, et de la qua[n]tite des p[ro]gressio[n]s et p[ro]portio[n]s. La seco[n]de tracte de la practique dicelle applicquee en fait des mo[n]oyes, en toutes marcha[n]dises comme drapperie, espicerie, mercerie et en toutes aultres marcha[n]dises qui se vendent a mesure au pois ou au nombre, en co[m]paignies et en tro[n]ques, es changes et merites, en fin dor et dargent et en lavaluer diceux. En arge[n]t le rey et en fin darge[n]t doze. Es deneraulx allyages et effaiz, tant de lot que de large[n]t. Et en geometrie aplicquee aux ars mecha[n]ique come aux masons charpe[n]tiers et a tous aultres besongna[n]s en art de mesure.* [Lyon]: Guillaume Huyon for Constantin Fradin, June 2, 1520.

$50,000

Folio, ff. [iv], 230, title printed in red and black within a beautiful woodcut border, woodcut printer's device on title-page, woodcut initials and diagrams (small tear to head of title-page, ink stains on n4v, lacking front free endpaper). Contemporary binding using a fifteenth-century vellum manuscript leaf (slightly soiled).

First edition, extremely rare, of the first published work on algebra in French. This is a fine copy in a beautiful contemporary binding. Born in Lyon, then the principal commercial centre of France, La Roche was a student of Nicolas Chuquet and published for the first time in the present work large sections from Chuquet's

Le Triparty en la Science des Nombres, the most original mathematical work of the fifteenth century. Chuquet's work, of which a single manuscript survives (BNF Fonds français 1346), remained unpublished until 1881. La Roche's work thus printed for the first time several important innovations in arithmetic and algebra introduced by Chuquet: the use of exponents to denote powers of a number, often credited to Descartes who introduced them in his *Géométrie* more than a century later; the use of the 'second unknown' (see below) in the solution of systems of linear equations, which was an important step towards the invention of symbolic algebra by Viète; the use of negative numbers in the solution of equations; and the introduction of our terms 'million', 'billion' and 'trillion' for powers of 10^6. La Roche also includes Chuquet's 'règle des nombres moyens,' according to which a fraction could be found between any two given fractions by taking the sum of their numerators and dividing by the sum of their denominators; this rule could be used to find the solution of any problem soluble in rational numbers, once an upper and a lower bound for the solution had been found. La Roche intended his work to serve the mercantile class, and his account of commercial arithmetic goes considerably beyond Chuquet. "The second, and greater, part of La Roche's work has, apart from some geometrical calculations at the end, a commercial character. The author states that as a basis he used 'the flower of several masters, experts in the art' of arithmetic, such as Luca Pacioli, supplemented by his own knowledge of business practice … [La Roche's work] presented an outstanding view of contemporary methods of computation and their applications in trade" (DSB). OCLC lists copies at Columbia and Harvard only in North America. ABPC/RBH list only the Macclesfield copy (rebound in the 19th century) since Honeyman (Sotheby's, April 14, 2005, lot 1204, £19,200 = $36,409). The present copy was offered by Librairie Thomas-Scheler in 1996 (Catalogue Nouvelle Série No. 15, n. 296, 120,000F).

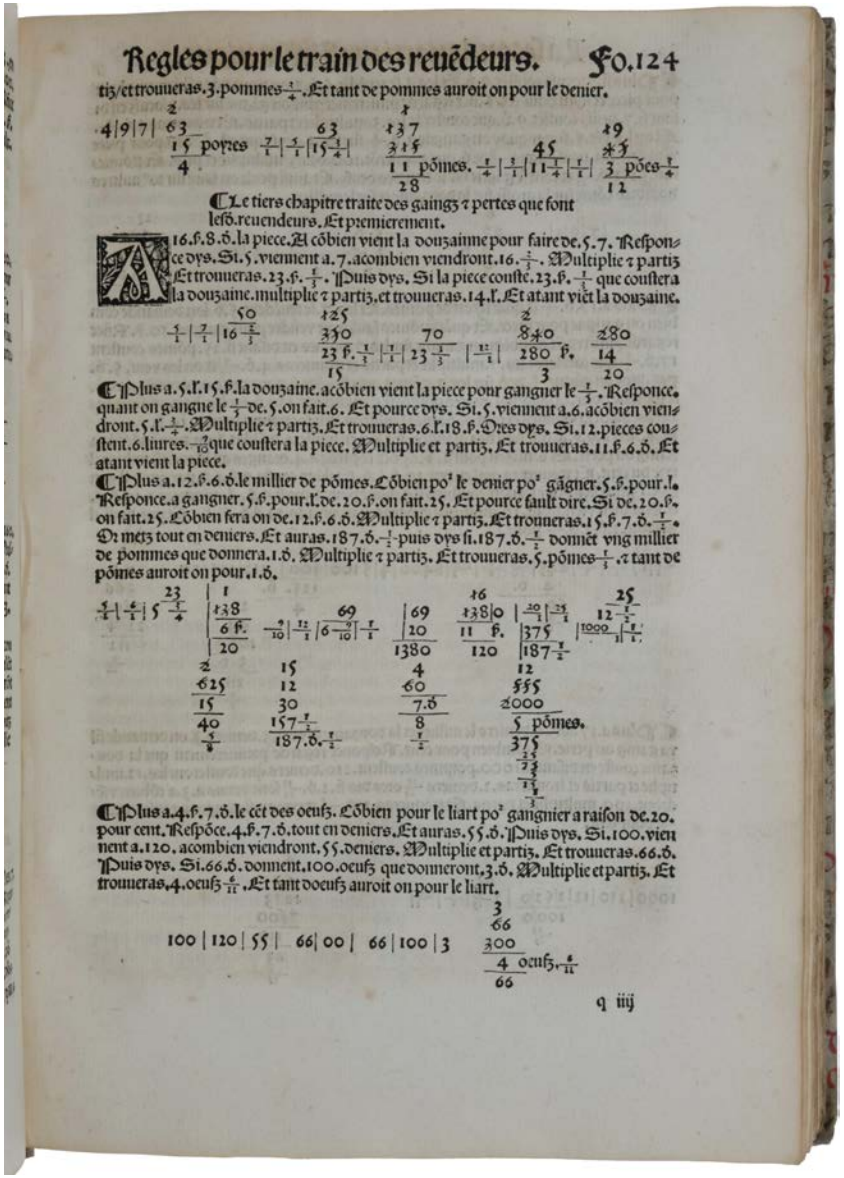
"We do not know much about de la Roche (c. 1470-1530). Tax registers from Lyon reveal that his father lived in the Rue Neuve in the 1480s and that Estienne owned more than one property in Villefranche, from which he derived his nickname. De la Roche is described as a 'master of argorisme' as he taught merchant arithmetic for 25 years at Lyon. He owned the manuscript of the *Triparty* after the death of Chuquet (1488). It is therefore considered that de la Roche was on friendly terms with Chuquet and possibly learned mathematics from him.

"The importance of the *Larismethique* has been seriously underestimated. There are several reasons for this. Probably the most important one is Aristide Marre's misrepresentation of the *Larismethique* as a grave case of plagiarism. Marre discovered that the printed work by Estienne de la Roche, contained large fragments that were literally copied from Chuquet's manuscript (Marre, *Le Triparty … par Maistre Nicolas Chuquet* (1881), introduction)" (Heeffer, pp. 1-2). But Barbara Moss argues that "the charge of plagiarism against Estienne de la Roche is largely an anachronism … Before the spread of printing, academic knowledge had been disseminated through the copying of manuscripts, and Chuquet, like many of his contemporaries, must have written down for reference a large number of examples from the work of others, with or without a note of their source … De la Roche's use of citations and sources is similar to that in a number of printed arithmetics of that period. Following the usual commendation of mathematics for its 'great utility and necessity', he continues: 'I have collected and amassed the flowers of several masters expert in this art, such as master Nicolas Chuquet, Parisian, Philippe Frescobaldi, Florentine, and Brother Luke of Borgo [Pacioli], with some small addition of what I have been able to invent and test out in my time in its practice' [first (unnumbered) page]" (Moss, pp. 117-9).

Moreover, "giving a transcription of the problem text only, Marre withholds that for many of the solutions to Chuquet's problems de la Roche uses different methods and an improved symbolism. In general, the *Larismethique* is a much better structured text than the *Triparty* and one intended for a specific audience. Chuquet was a bachelor in medicine educated in Paris within the scholarly tradition and well acquainted with Boethius and Euclid. On the other hand, de la Roche was a reckoning master operating within the abbaco tradition. It becomes clear from the structure of the book that de la Roche had his own didactic program in mind. He produced a book for teaching and learning arithmetic and geometry which met the needs of the mercantile class. He rearranges Chuquet's manuscript using Pacioli's *Summa* as a model. He even adopts Pacioli's classification in books, distinctions and chapters. He moves problems from Chuquet's *Appendice* to relevant sections within the new structure. He adds introductory explanations to each section of the book, such as for the second unknown, discussed below. With the judgment of an experienced teacher, he omits sections and problems from the *Triparty* which are of less use to merchants and craftsmen and adds others which were not treated by Chuquet such as problems on exchange and barter" (Heeffer, p. 2).

"As far as the first book of the *Triparty* is concerned, de la Roche is reasonably faithful to his teacher. He does include an extra chapter, on the connotations of the numbers 1 to 12, which he took from Pacioli, and Pacioli from St Augustine's *Civita Dei*. He also prefers some of the more conventional names, like 'rule of false position' rather than 'rule of one position', and disagrees with Chuquet's assessment that the rule of apposition and remotion for solving indeterminate equations in integers 'is a science of little recommendation'. However, he includes two of the distinctive contributions of the *Triparty*: nomenclature in terms of the powers of 10^6 up to the nonillion [$(10^6)^9$], and the rule of intermediate terms, or 'rule of mediation between the greater and the less' [règle des nombres moyens]" (Moss, p. 121).

"[Chuquet] employs the words byllion, trillion, quadrillion, quyllion, sixlion,



septillion, octillion, nonillion, 'et ainsi des aultres se plus oultre ou voulait proceder' to denote the second, third, etc. powers of a million. Evidently Chuquet had solved the difficult question of numeration. The new words used by him appear in 1520 in the printed work of La Roche. Thus, the great honour of having simplified numeration of large numbers appears to belong to the French. In England and Germany the new nomenclature was not introduced until about a century and a half later. In England the words billion, trillion, etc. were new when Locke wrote, about 1687 [*Human Understanding*, Chap. XVI]. In Germany these new terms appear for the first time in 1681 … but they did not come into general use until the eighteenth century" (Cajori, p. 144).

"De la Roche's algebra contains several topics that are not found in the *Triparty* … he has chapters on algebraic fractions, on the rule of quantity (a mixture of algebra and the rule of false position used for solving equations in more than one unknown), and on equations with more than one solution. He also gives methods for 'proving', or checking, results on algebra analogous to Chuquet's proofs in arithmetic.

"The four canons of the rule of the first terms (one for solving generalized linear equations, and three for the three acceptable forms of the generalized quadratic, with all coefficients positive) are stated in Chuquet's terms, although de la Roche gives more elementary examples, and fewer which require solution by means of compound roots or roots of high order. However, special cases of cubic equations, with no constant term, are included among the quadratics.

"The fourth canon, for solving equations of the form $x^2 + b = ax$, which may have two positive roots, gave rise to a minor controversy. In 1559, Jean Buteon [in his *Logistica*] attacked de la Roche's rule, and claimed that it is impossible for an equation to have more than one solution. De la Roche's example to the contrary should be disallowed because he gives 1 as one of the roots, and 1, according to Buteon, should not be considered as a number (!) …

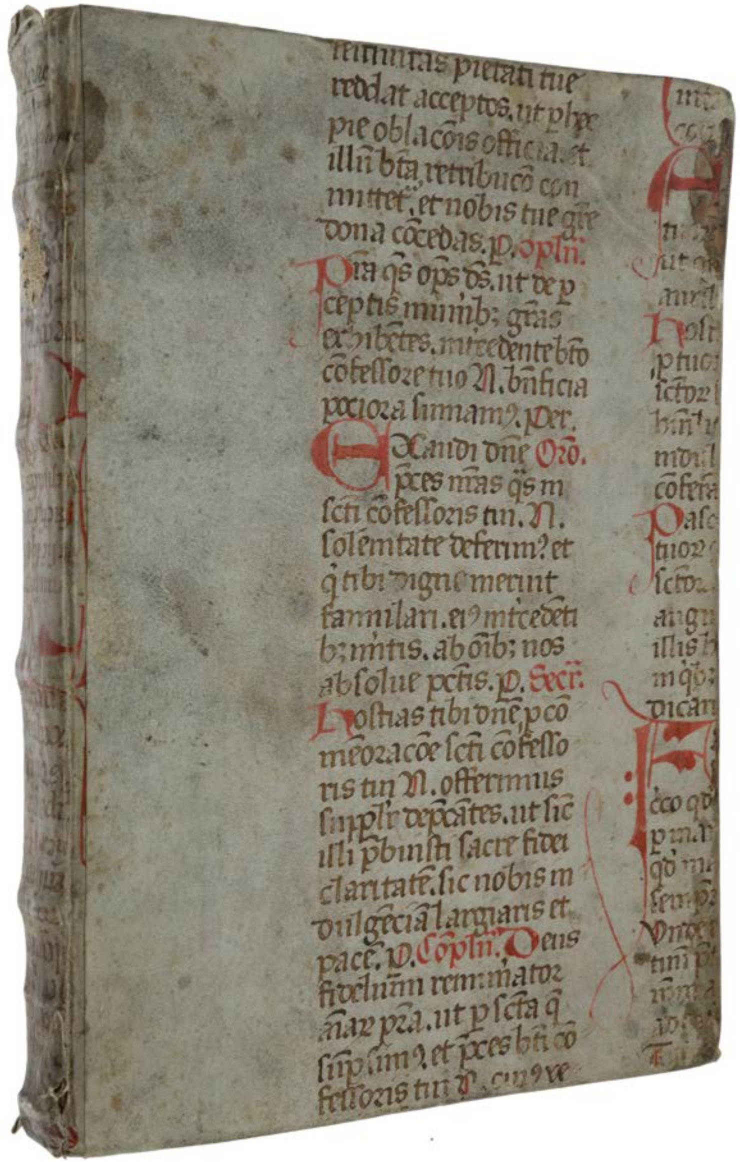
"The weaknesses [in de la Roche's algebra] are essentially those of conservatism, though his work, like Chuquet's, is not free from careless errors. As in the case of radicals, he presents the concepts of algebra both in the terminology and notation of the *Triparty* and in a more traditional, restricted, and qualitative system involving symbols not involving numbers for powers of the unknown, and he prefers to use the latter … However, the index notation *is* presented. It attracted the attention of Michel Chasles in the nineteenth century … Chasles saw no anticipation of Viete's ideas among the Italians; but he instanced the German Stifel [*Arithmetica integra*, 1544] and the Frenchmen Peletier [*L'Algèbre*, 1554] and Buteon, because they used letters for unknowns and a crude form of index notation. The development of an adequate notation for exponents had hitherto been accredited to Descartes; but Chasles claimed that such a notation is already present in de la Roche's *Larismethique nouellement composee*" (Moss, pp. 121-4).

"That de la Roche made an important contribution to the emergence of symbolic algebra during the sixteenth century can best be argued by his treatment of the second unknown, sometimes called 'Regula quantitatis' or 'Rule of Quantity' … The importance of the use of letters to represent several unknowns goes much further than the introduction of a useful system of notation. It contributed to the development of the modern concept of unknown and that of a symbolic equation. These developments formed the basis on which Viète [*In artem analyticum isagoge*, 1591] could build his theory of equations … De la Roche first mentions 'la regle de la quantite' in the beginning of distinction six together with 'la regle de la chose'. He properly introduces the second unknown in a separate chapter titled *Le neufiesme chapitre de la regle de la quantite annexee avec le dict primier canon, et de leur application*, in the sixth distinction of the first part [f. 42v] …

"In the Rule of Quantity, de la Roche sees a perfection of algebra itself. The use of several unknowns allows for an easy solution to several problems which might otherwise be more difficult or even impossible to solve … After this introduction, de la Roche gives six examples of the rule of quantity applied to the typical linear problems, though he removes the practical context. Then he presents five indeterminate problems under the heading 'questions which have multiple responses', without use of the second unknown. Finally he solves five problems using the second unknown under the heading 'other inventions on numbers'. At the end of the book there is a chapter on applications in which four more problems are given (ff. 149v-150r). In total, there are twenty problems solved by the regle de la quantite" (Heeffer, pp. 3-7).

The section on commercial arithmetic provides information about the economic life of fifteenth-century Lyon, which had become one of the most important commercial centres of the Western world after Louis XI gave royal protection to the fairs established there in 1464. De la Roche discusses the currencies in use, the weights and measures, and the financial arrangements between partners in business enterprises. The various currencies mentioned come from different parts of France, and from Germany and Italy, probably all to be found in the market at Lyon. From some examples one can infer that exchange rates fluctuated considerably. Other examples may relate to actual methods of counting used by money-changers.

"La Roche begins the commercial section independently of Chuquet by stating that numbers can be considered in three ways according to the Bible's Book of Wisdom, that God the creator ordered all in measure, number and weight. And thus, all the affairs of the world are governed and managed by these three things, which la Roche continues to examine from the monetary perspective … Dealing



with numbers, la Roche returns to the basic arithmetic operations in relation to the monetary and coinage system, then in relation to measure, and third as weight. La Roche divides the entire second part into ten chapters, each divided into a number of subsections …

"La Roche first states that all countries use the £ (Lire, Pound), the solz (sous, shilling), and deniers (penny). The Lire is always worth 20s (solz), and the solz is 12d (denier, penny). But the £ of one country has a different value from that of another country … The section 'numbers as measure' is divided into three kinds according to the three dimensions in geometry: length (ell, toyse, cane, brasse, palme, etc.), which is used for cloth measures. The second is the square (toyse, pye, etc.) used for tapestries, mural work, fields. The third way number can be considered is as weight of silver, copper, lead, saffron, ginger, pepper, etc. The weights systems are similar but the values differ. After the introduction follow a number of examples of calculation of different monetary values by addition and subtraction. The examples la Roche mainly took from Chuquet.

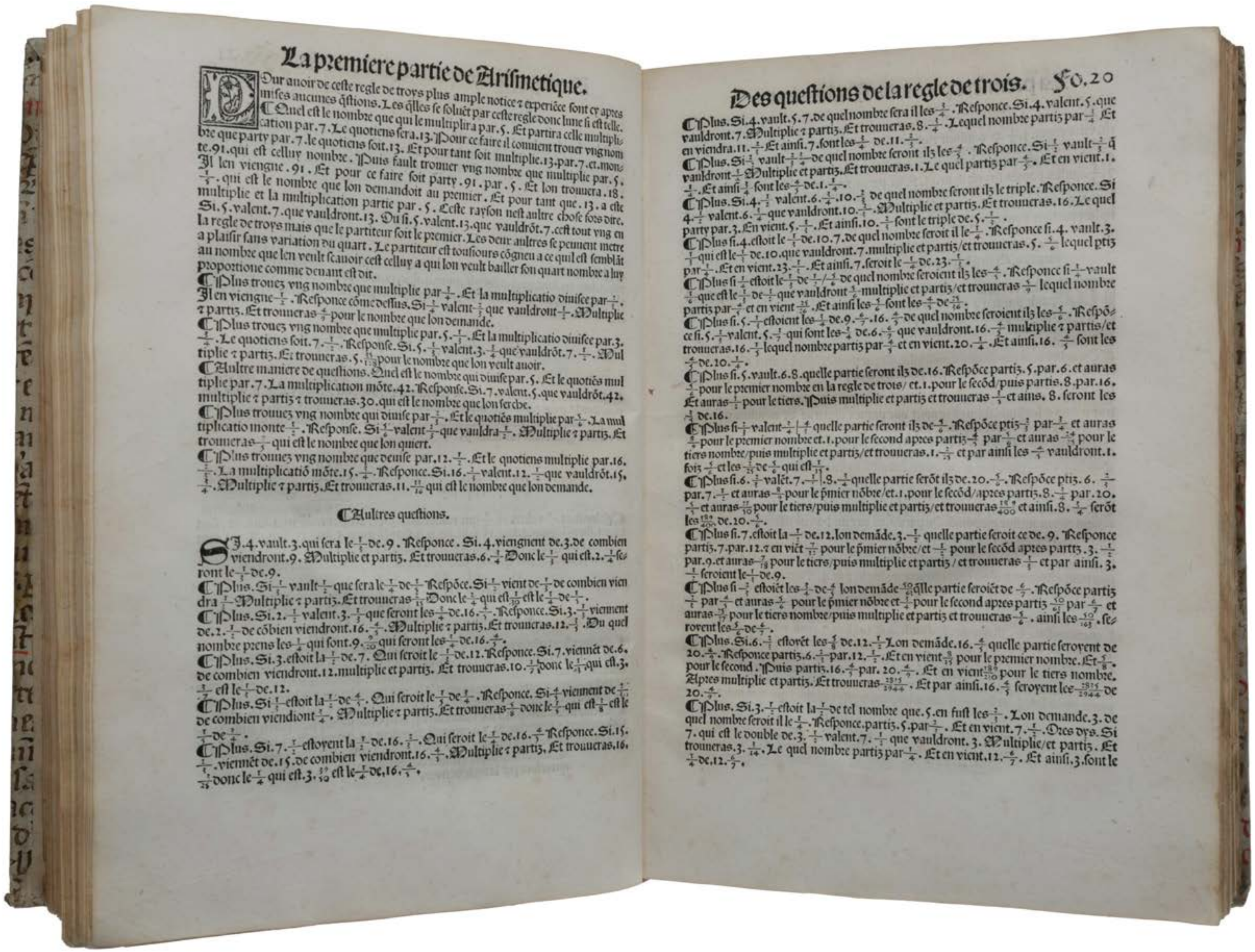
"La Roche proceeds to explain multiplication by taking a fraction of a fraction of a higher unit … The method is very simple and easy to explain: if you buy 1200 items at 1 Euro per item, the total price will be 1200 Euro; if the price is ½ Euro (50 cents), the price is half the number of items, which is 600 Euro, etc. … The second chapter is about merchandise sold by length or size, such as linen … Similar counting methods are followed in calculating the price of merchandise sold by weight in the third chapter, and in the fourth chapter, which describes selling merchandise by number (dozen, gross, hundred, thousand). Subsequent chapters deal with liquid measures (5), corporations (6), barter (7), exchange of money and banking (8), profit and discount (9), and gold and silver (10)" (Ulff-Møller).

The final section of the book, on the application of the science of numbers to geometry, includes the measurement of areas and volumes. There are problems on circles, triangles and polygons, including many examples about inscribed figures, the representation of square and cube roots, various simple constructions, and notes on the quadrature of the circle. This last presented some results about lunes, the triangulation of the circle, and series of inscribed and circumscribed circles and squares. Algebra is applied to problems of volumes leading to simple cubic equations. Thus, geometry is viewed as an area to which algebra can be applied, rather than a means of justifying algebraic rules.

"The last few pages are devoted to gauging and show the same instrument as was described by La Court (*La fabrique et usage de la jauge*, 1588)" (Tomash). This gauge or compass (so called because of its shape) was used in measuring barrels. The results achieved with this instrument were at best approximate as only the bung diameter and a diagonal distance were measured – the shape of the individual barrels was not taken into account.

A second edition of la Roche's book was published at Lyon in 1538. There is no modern edition.

Bechtel L-47; Brunet III 842; Hoock & Jeannin L5.1; USTC 30158; von Gültlingen, Huyon 12. Cajori, *History of Elementary Mathematics*, 2007. Smith, *Rara Arithmetica*, p. 128. Ulff-Møller, 'Estienne de la Roche. Larismethique nouellement compose. Lyon 1520, and second edition 1538,' in *Rechenmeister und Mathematiker der frühen Neuzeit*, Gebhardt (ed.), 2017. For Chuquet, see Flegg, Graham, Hay, C., Moss, B. *Nicolas Chuquet, Renaissance Mathematician. A study with extensive translation of Chuquet's mathematical manuscript completed in 1484*, 1985.


La premiere partie de Arismetique.

Des questions de la regle de trois. Fo. 20

Aultres questions.

A LANDMARK WORK ON THE FOUNDATIONS OF MATHEMATICS

Landmark Writtings in Western Mathematics 43; *Breakthroughs* 415.

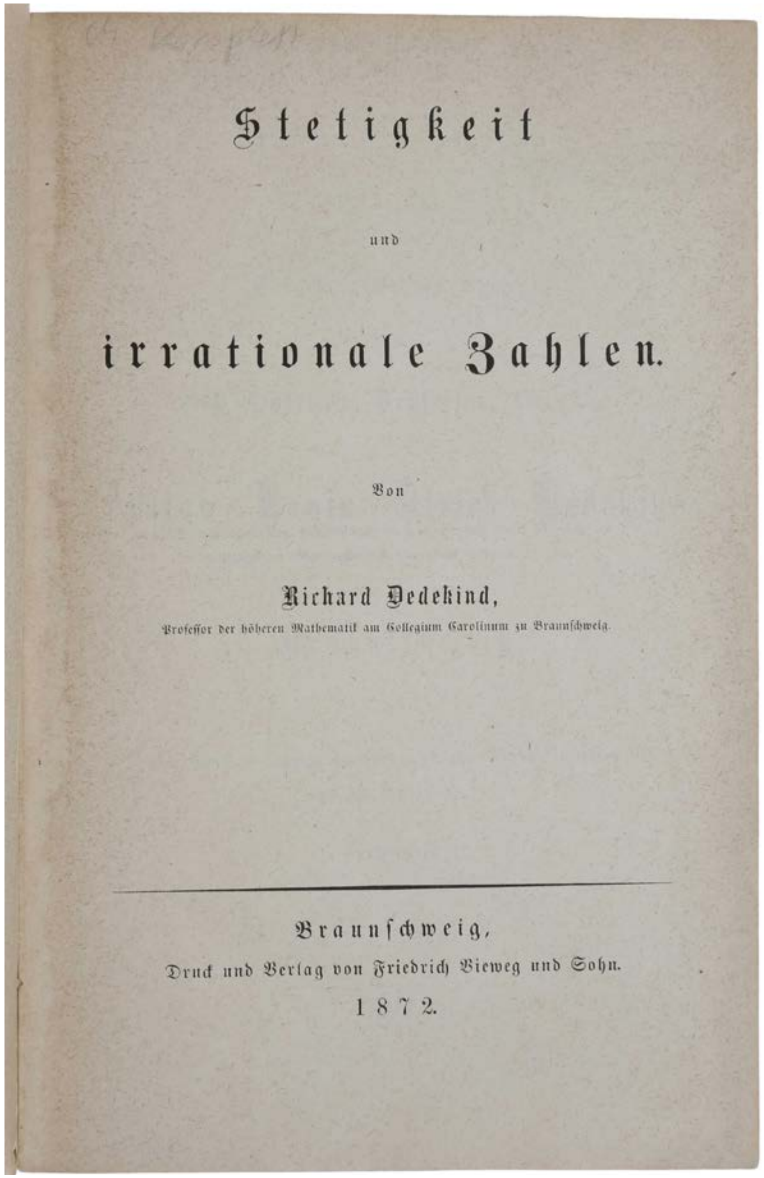
DEDEKIND, Richard. *Stetigkeit und irrationale Zahlen.* Braunschweig: Friedrich Vieweg, 1872.

$12,500

8vo (202 x 130 mm), pp. 31, [1]. Contemporary cloth-backed marbled boards (light browning throughout). Preserved in a folding clamshell case. A fine copy.

First edition, very rare in commerce, of Dedekind's great work on the foundations of mathematics. "This short work marks a significant epoch in the movement known as the arithmetization of analysis, that is, the replacement of intuitive geometric notions by concepts described in precise words" (*Landmark Writings*, p. 553). "This article, whose central idea was worked out by Dedekind while he was teaching in Zürich in 1858, presents a rigorous arithmetical foundation for the theory of real numbers … Despite Dedekind's assertion in the introductory paragraphs of *Continuity and irrational numbers* that he originally did not publish his theory because he did not regard it as being very fruitful, it laid the foundations for much of modern-day real analysis and point-set topology" (Ewald, pp. 765-6). No copies listed on ABPC/RBH.

"In 1858, Dedekind had noted the lack of a truly scientific foundation of arithmetic in the course of his Zürich lectures on the elements of differential calculus. On 24 October, Dedekind succeeded in producing a purely arithmetic definition of the essence of continuity and, in connection with it, an exact formulation of the



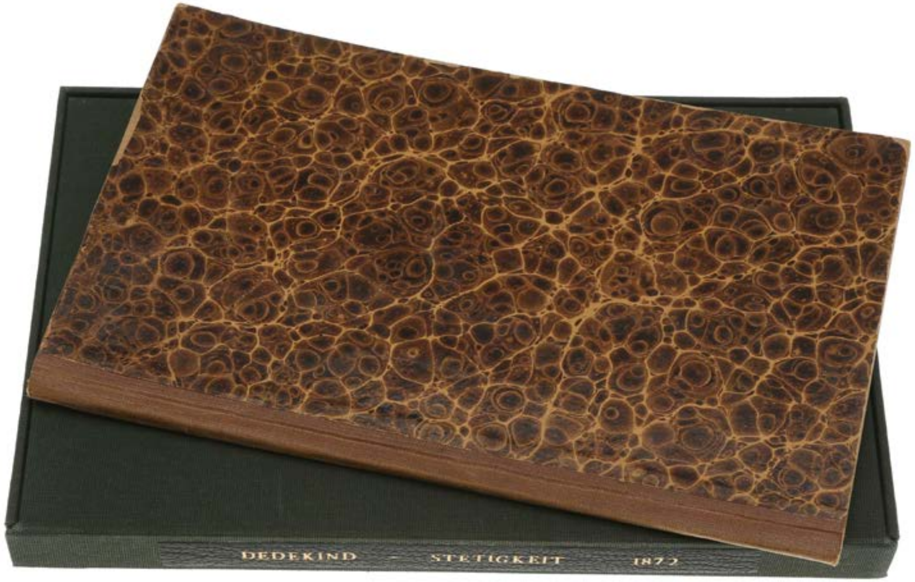
concept of the irrational number. Fourteen years later, he published the result of his considerations, *Stetigkeit und irrationale Zahlen* (Brunswick, 1872, and later editions), and explained the real numbers as "cuts" in the realm of rational numbers. He arrived at concepts of outstanding significance for the analysis of number through the theory of order. The property of the real numbers, conceived by him as an ordered continuum, with the conceptual aid of the cut that goes along with this, permitted tracing back the real numbers to the rational numbers: Any rational number a produces a resolution of the system R of all rational numbers into two classes A_1, A_2, in such a way that each number a_1 of the class A_2 is smaller than each number a_2 of the second class A_2. (Today, the term "set" is used instead of "system.") The number a is either the largest number of the class A_1 or the smallest number of the class A_2. A division of the system R into the two classes A_1, A_2, whereby each number a_1 in A_1 is smaller than each number a_2 in A_2 is called a "cut" (A_1, A_2) by Dedekind. In addition, an infinite number of cuts exist that are not produced by rational numbers. The discontinuity or incompleteness of the region R consists in this property. Dedekind wrote, "Now, in each case when there is a cut (A_1, A_2) which is not produced by any rational number, then we *create* a new, *irrational* number α, which we regard as completely defined by this cut; we will say that this numberα corresponds to this cut, or that it produces this cut" (*Stetigkeit*, § 4).

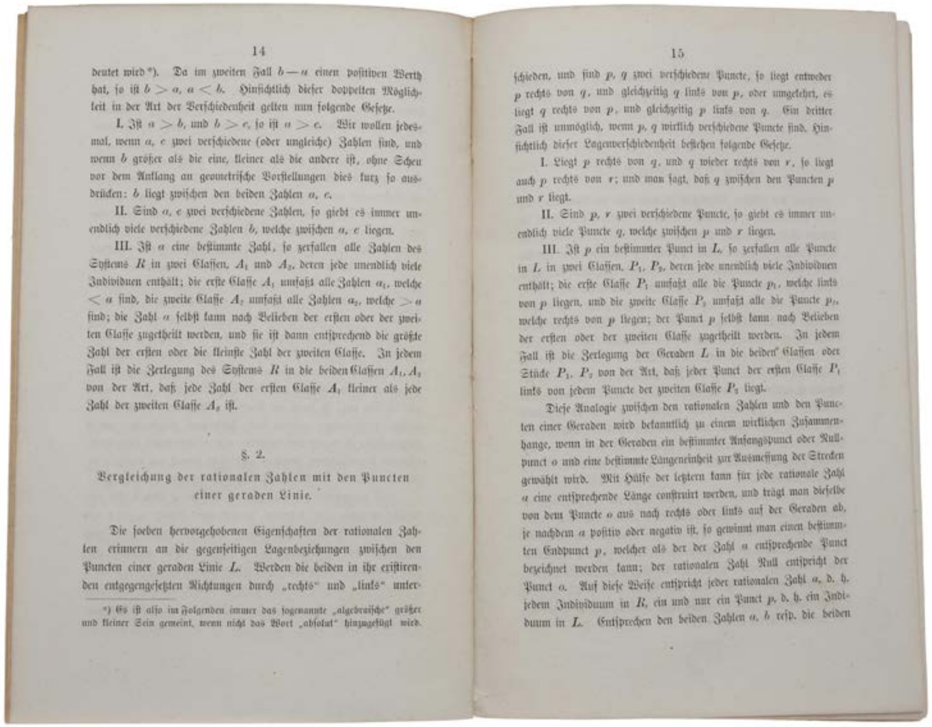
"Occasionally Dedekind has been called a "modern Eudoxus" because an impressive similarity has been pointed out between Dedekind's theory of the irrational number and the definition of proportionality in Eudoxus' theory of proportions (Euclid, *Elements*, bk. V, def. 5). Nevertheless, Oskar Becker correctly showed that the Dedekind cut theory and Eudoxus' theory of proportions do not coincide: Dedekind's postulate of existence for all cuts and the real numbers that produce them cannot be found in Eudoxus or in Euclid. With respect to this, Dedekind said that the Euclidean principles alone—without inclusion of the principle of continuity, which they do not contain—are incapable of establishing a complete theory of real numbers as the proportions of the quantities. On the other hand, however, by means of his theory of irrational numbers, the perfect model of a continuous region would be created, which for just that reason would be capable of characterizing any proportion by a certain individual number contained in it (letter to Rudolph Lipschitz, 6 October 1876).

"With his publication of 1872, Dedekind had become one of the leading representatives of a new epoch in basic research, along with Weierstrass and Georg Cantor. This was the continuation of work by Cauchy, Gauss, and Bolzano in systematically eliminating the lack of clarity in basic concepts by methods of demonstration on a higher level of rigor. Dedekind's and Weierstrass' definition of the basic arithmetic concepts, as well as Georg Cantor's theory of sets, introduced the modern development, which stands "completely under the sign of number," as David Hilbert expressed it.

"Dedekind entered the University of Göttingen in 1850; he studied mathematics and physics, attending Gauss's lectures on the method of least squares and on advanced geodesy. One of his friends was a fellow mathematics student, five years older than he, Bernhard Riemann. In 1852 Dedekind took his doctorate; the dissertation, written under the supervision of Gauss, was on the theory of Eulerian integrals. Both Riemann and Dedekind qualified as university lecturers in 1854 … In 1855, P.G. Lejeune-Dirichlet left Berlin to succeed to Gauss' professorship in Göttingen … From 1858 to 1862 he taught at the Polytechnic in Zürich; it was during this time that he developed his ideas on the foundations of real analysis. In 1862 he was appointed to a professorship at the Polytechnic in his native city of Brunswick; he remained there until his death" (Ewald, pp. 753-4).

Honeyman 840. Ewald (ed.), *From Kant to Hilbert*, 1996. *Landmark Writings in Western Mathematics 1640-1940*, I. Grattan-Guinness (ed.), Chapter 43. Parkinson, *Breakthroughs*, p. 415. Stedall, *Mathematics Emerging: A Sourcebook 1540-1900*, 2008.

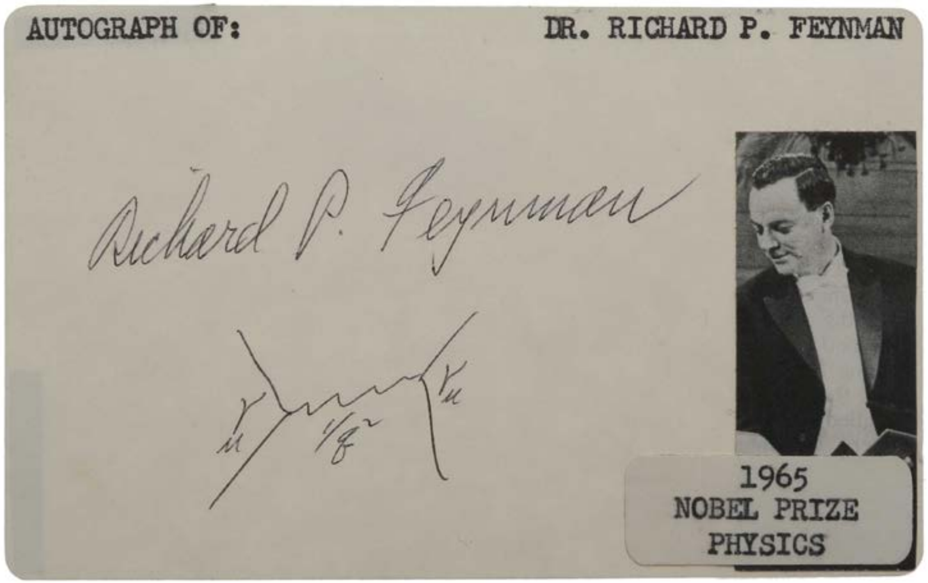




AUTOGRAPH FEYNMAN DIAGRAM

FEYNMAN, Richard Phillips. *Card bearing Feynman's signature 'Richard P. Feynman', with one of his famous 'Feynman diagrams' below in his hand, and an affixed newspaper photograph of Feynman receiving the Nobel Prize in Physics 1965.*

$15,000



118 x 75 mm. In fine condition.

A rare example of Feynman's signature, with a much rarer example of one of his eponymous diagrams in his hand. In fact, this diagram is almost identical to the first ever Feynman diagram that he drew in public, on the blackboard at the famous Pocono conference in the spring of 1948, where he first explained his diagrammatic approach to the problems of quantum electrodynamics. Widely regarded as the most brilliant, influential, and iconoclastic figure in theoretical physics in the post-World War II era, Feynman shared the Nobel Prize in Physics 1965 with Sin-Itiro Tomonaga and Julian Schwinger "for their fundamental work in quantum electrodynamics, with deep-ploughing consequences for the physics of elementary particles." The rarity of any form of manuscript material by Feynman is well-known. When his autobiographical work *Surely You're Joking Mr. Feynman!* was about to be published, Feynman told his editor "I'm not going to go on TV and I'm not going to sign any books!" Requests for Feynman's signature were referred routinely to his secretary, who returned instead a printed card stating firmly that 'Professor Feynman has found it necessary to refuse all requests for autographs'. Feynman's signature is here greatly enhanced by the presence of one of his iconic Feynman diagrams, which have since become ubiquitous in theoretical physics. Schwinger later wrote: "Like the silicon chip of more recent years, the Feynman diagram was bringing computation to the masses" (Brown & Hoddesdon, p. 329). In 1973, the great Dutch theoretical physicist and Nobel laureate Gerardus t'Hooft commented (CERN 79-9): "Few physicists object nowadays to the idea that diagrams contain more truth than the underlying formalism." We know of only two other examples of Feynman's signature accompanied by an autograph Feynman diagram, one on a copy of *Feynman's Lectures on Physics* (commons.wikimedia.org/wiki/File:Feynman'sDiagram.JPG), and another on a copy of Feynman's popular work *QED* owned by the physicist and bibliophile Jay Pasachoff (see: chapin.williams.edu/pasachoff/collecting.html).

"QED explains the force of electromagnetism – the physical force that causes like charges to repel each other and opposite charges to attract – at the quantum-mechanical level. In QED, electrons and other fundamental particles exchange virtual photons – ghostlike particles of light – which serve as carriers of this force. A virtual particle is one that has borrowed energy from the vacuum, briefly shimmering into existence literally from nothing. Virtual particles must pay back the borrowed energy quickly, popping out of existence again, on a time scale set by Werner Heisenberg's uncertainty principle.

"Two terrific problems marred physicists' efforts to make QED calculations. First, as they had known since the early 1930s, QED produced unphysical infinities, rather than finite answers, when pushed beyond its simplest approximations. When posing what seemed like straightforward questions – for instance, what is the probability that two electrons will scatter? – theorists could scrape together reasonable answers with rough-and-ready approximations. But as soon as they tried to push their calculations further, to refine their starting approximations, the equations broke down. The problem was that the force-carrying virtual photons could borrow any amount of energy whatsoever, even infinite energy, as long as they paid it back quickly enough. Infinities began cropping up throughout the theorists' equations, and their calculations kept returning infinity as an answer, rather than the finite quantity needed to answer the question at hand.

"A second problem lurked within theorists' attempts to calculate with QED: The formalism was notoriously cumbersome, an algebraic nightmare of distinct terms to track and evaluate. In principle, electrons could interact with each other by shooting any number of virtual photons back and forth. The more photons in the fray, the more complicated the corresponding equations, and yet the quantum-mechanical calculation depended on tracking each scenario and adding up all the contributions.

"All hope was not lost, at least at first. Heisenberg, Wolfgang Pauli, Paul Dirac and the other interwar architects of QED knew that they could approximate this infinitely complicated calculation because the charge of the electron (e) is so small: $e^2 \sim 1/137$, in appropriate units. The charge of the electrons governed how strong their interactions would be with the force-carrying photons: Every time a pair of electrons traded another photon back and forth, the equations describing the exchange picked up another factor of this small number, e^2. So a scenario in which the electrons traded only one photon would 'weigh in' with the factor e^2, whereas electrons trading two photons would carry the much smaller factor e^4. This event, that is, would make a contribution to the full calculation that was less than one one-hundredth the contribution of the single-photon exchange. The term corresponding to an exchange of three photons (with a factor of e^6) would be ten thousand times smaller than the one-photon-exchange term, and so on. Although the full calculations extended in principle to include an infinite number of separate contributions, in practice any given calculation could be truncated after only a few terms. This was known as a *perturbative* calculation: theorists could approximate the full answer by keeping only those few terms that made the largest contribution, since all of the additional terms were expected to contribute numerically insignificant corrections.

"Deceptively simple in the abstract, this scheme was extraordinarily difficult in practice … by the start of World War II, QED seemed an unholy mess, as calculationally intractable as it was conceptually muddled.

"In his Pocono Manor Inn talk, Feynman told his fellow theorists that his diagrams offered new promise for helping them march through the thickets of QED calculations. As one of his first examples, he considered the problem of electron-electron scattering. He drew a simple diagram on the blackboard, similar to the one later reproduced in his first article on the new diagrammatic techniques" (Kaiser, pp. 156-8).

"The first published example of what is now called a Feynman diagram appeared in Feynman's 1949 *Physical Review* article ['The theory of positrons,' Vol. 76, pp. 749-59]. It depicted the simplest contribution to an electron-electron interaction, with a single virtual photon (wavy line) emitted by one electron and then absorbed by the other. In Feynman's imagination – and in the equations – this diagram also represented interactions in which the photon is emitted by one electron and travels back in time to be absorbed by the other, which is allowed within the Heisenberg time uncertainty" (https://physics.aps.org/story/v24/st3).

The first diagram Feynman drew at the Pocono conference is virtually identical to the one he drew on the offered card. The diagram represents events in two dimensions, with space on the horizontal axis and time on the vertical axis. The straight lines at bottom left and bottom right represent the paths of two electrons. In classical physics there is an electromagnetic force that causes the electrons to repel each other. In QED this interaction takes place via the exchange of a virtual photon, represented by the wavy line. After the virtual photon has been exchanged the subsequent motion of the electrons is represented by the straight lines at the top left and top right. At the left hand vertex, where the two straight lines and the wavy line meet, the energy and momentum of the left-hand electron changes. Since energy-momentum (technically, the relativistic 4-momentum) is always conserved, the change in the 4-momentum of the electron is balanced by the 4-momentum of the virtual photon emitted at this vertex. This virtual photon then interacts with the second electron at the right-hand vertex, where its 4-momentum is added to that of the electron, causing it to scatter.

But the Feynman diagram is much more than a pictorial representation of the interaction of the two electrons. It enables one to calculate a complex quantity called the 'amplitude' for the diagram. Its absolute square, apart from simple factors, is the 'cross section' describing the probability for the process to occur. (Strictly speaking, before taking the absolute square the amplitudes of all possible diagrams having the same initial and the same final states must be added, but as explained above only the first few diagrams need to be retained in practice.) To calculate the amplitude for the diagram one needs to know the 'propagator' for the virtual photon, which is the factor $1/q^2$ Feynman has written under the wavy line representing it. Here q^2 is the squared length of the 4-momentum of the virtual photon (energy^2 – momentum^2). In classical electrodynamics this would be zero, because it is equal to the square of the rest mass of the particle, which for a photon is zero. However, q^2 need not be zero for a virtual photon (physicists say that virtual photons are 'off shell'). To calculate the amplitude one also needs to know the 'vertex factors,' representing the likelihood that an electron would emit or absorb a photon. This is $e\gamma_\mu$, where e is the electron's charge and γ_μ a vector of 'Dirac matrices' (arrays of numbers to keep track of the electron's spin). Feynman has indicated these vertex factors by writing γ_μ next to each of the two vertices of the diagram.

"In this simplest process, the two electrons traded just one photon between them; the straight electron lines intersected with the wavy photon line in two places, called 'vertices.' The associated mathematical term therefore contained two factors of the electron's charge, e – one for each vertex. When squared, this expression gave a fairly good estimate for the probability that two electrons would scatter. Yet both Feynman and his listeners knew that this was only the start of the calculation. In principle, as noted above, the two electrons could trade any number of photons back and forth.

"Feynman thus used his new diagrams to describe the various possibilities. For example, there were nine different ways that the electrons could exchange two photons, each of which would involve four vertices (and hence their associated mathematical expressions would contain e^4 instead of e^2). As in the simplest case (involving only one photon), Feynman could walk through the mathematical contribution from each of these diagrams …

"By using the diagrams to organize the calculational problem, Feynman had thus solved a long-standing puzzle that had stymied the world's best theoretical physicists for years. Looking back, we might expect the reception from his colleagues at the Pocono Manor Inn to have been appreciative, at the very least. Yet things did not go well at the meeting. For one thing, the odds were stacked against Feynman: His presentation followed a marathon day-long lecture by Harvard's Wunderkind, Julian Schwinger. Schwinger had arrived at a different method (independent of any diagrams) to remove the infinities from QED calculations, and the audience sat glued to their seats – pausing only briefly for lunch – as Schwinger unveiled his derivation.

Coming late in the day, Feynman's blackboard presentation was rushed and unfocused. No one seemed able to follow what he was doing. He suffered frequent interruptions from the likes of Niels Bohr, Paul Dirac and Edward Teller, each of whom pressed Feynman on how his new doodles fit in with the established principles of quantum physics. Others asked more generally, in exasperation, what rules governed the diagrams' use. By all accounts, Feynman left the meeting disappointed, even depressed" (Kaiser, pp. 159-160).

Feynman diagrams were eventually accepted largely due to the efforts of the British mathematician Freeman Dyson, who had regularly served as discussion partner to Feynman at Cornell and was probably the only person at that time who was really familiar with both Schwinger's and Feynman's theories. In his paper, 'The radiation theories of Tomonaga, Schwinger and Feynman,' (*Physical Review* 75 (1949), pp. 486-501), Dyson constructed a bridge between the two theories, showing that Feynman's methods could be derived from the more traditional techniques used by Schwinger.

"Soon the [Feynman] diagrams gained adherents throughout the fields of nuclear and particle physics. Not long thereafter, other theorists adopted – and subtly adapted – Feynman diagrams for solving many-body problems in solid-state theory. By the end of the 1960s, some physicists even used versions of Feynman's line drawings for calculations in gravitational physics. With the diagrams' aid, entire new calculational vistas opened for physicists. Theorists learned to calculate things that many had barely dreamed possible before World War II. It might be said that physics can progress no faster than physicists' ability to calculate. Thus, in the same way that computer-enabled computation might today be said to be enabling a genomic revolution, Feynman diagrams helped to transform the way physicists saw the world, and their place in it" (Kaiser, p. 156).

Richard Phillips Feynman was born on 11 May 1918 in the New York borough of Queens to Jewish parents originally from Russia and Poland. As a child, he was heavily influenced both by his father, Melville, who encouraged him to ask questions to challenge orthodox thinking, and his mother, Lucille, from whom he inherited the sense of humour that he maintained throughout his life. From an early age he delighted in repairing radios and demonstrated a talent for engineering. At Far Rockaway High School in Queens, he excelled in mathematics, and won the New York University Math Championship by a large margin in his final year there. He was refused entry to his first choice Columbia University because of the 'Jewish quota' and attended instead the Massachusetts Institute of Technology, where he received a bachelor's degree in 1939, and was named a Putnam Fellow. He obtained an unprecedented perfect score on the graduate school entrance exams to Princeton University (although he did rather poorly on the history and English portions), where he went to study mathematics under his advisor John Archibald Wheeler (1911-2008). He obtained his PhD in 1942, with a thesis on the 'path-integral' formulation of quantum mechanics. During his time at Princeton, he married his first wife, Arline Greenbaum; she died of tuberculosis just a few years later in 1945.

While at Princeton, Feynman was persuaded by the physicist Robert Wilson to participate in the Manhattan Project. At Los Alamos Feynman immersed himself in the work on the atomic bomb, was soon made a group leader under Hans Bethe, and was present at the Trinity bomb test in 1945. During his time at Los Alamos, Niels Bohr sought him out for discussions about physics, and he became a close friend of laboratory head Robert Oppenheimer, who unsuccessfully tried to lure him to the University of California in Berkeley after the war. Looking back, Feynman thought his decision to work on the Manhattan Project was justified at the time, but he expressed grave reservations about the continuation of the project after the defeat of Nazi Germany, and suffered bouts of depression after the destruction of Hiroshima.

After the war, Feynman declined an offer from the Institute for Advanced Study in Princeton, New Jersey, despite the presence there of such distinguished faculty members as Albert Einstein, Kurt Gödel and John von Neumann. Instead he followed Hans Bethe to Cornell, where he taught theoretical physics from 1945 to 1950. Feynman then opted for the position of Professor of Theoretical Physics at the California Institute of Technology (partly for the climate, as he admits), despite offers of professorships from other renowned universities. He remained there for the rest of his career.

During his years at Caltech, he continued the work on quantum electrodynamics (the theory of the interaction between light and matter) he had begun at Cornell, and for which he was awarded the 1965 Nobel Prize in Physics. He developed an important tool known as Feynman diagrams to help conceptualize and calculate interactions between particles, notably the interactions between electrons and their anti-matter counterparts, positrons. Feynman diagrams, which are easily visualized graphic analogues of the complicated mathematical expressions needed to describe the behaviour of systems of interacting particles, have permeated many areas of theoretical physics in the second half of the twentieth century. He also worked on the physics of the superfluidity of supercooled liquid helium and its quantum mechanical behaviour; a model of weak decay (such as the decay of a neutron into an electron, a proton and an anti-neutrino) in collaboration with fellow Caltech professor Murray Gell-Mann; and his parton model for analyzing high-energy hadron collisions. At Caltech Feynman gained a reputation for being able to explain complex elements of theoretical physics in an easily understandable way – he opposed rote learning, although he could also be strict with unprepared students. His 1964 *Feynman Lectures On Physics* remains a classic.

In December 1959, Feynman gave a visionary and ground-breaking talk entitled 'There's Plenty of Room at the Bottom' at an American Physical Society meeting at Caltech. In it, he suggested the possibility of building structures one atom or molecule at a time, an idea which seemed fantastic at the time, but which has since become widely known as nanotechnology. He was also one of the first scientists to conceive of the possibility of quantum computers and played a crucial role in developing the first massively parallel computer, finding innovative uses for it in numerical computations, building neural networks and physical simulations using cellular automata.

Just two years before his death, Feynman played an important role in the Rogers Commission investigation of the 1986 Challenger Space Shuttle disaster. During a televised hearing, Feynman famously demonstrated how the O-rings became less resilient and subject to seal failures at ice-cold temperatures by immersing a sample of the material in a glass of ice water. He developed two rare forms of cancer, Liposarcoma and Waldenström's macroglobulinemia, and died on 15 February 1988 in Los Angeles.

Brown & Hoddesdon (eds.), *The Birth of Particle Physics*, 1983. Kaiser, 'Physics and Feynman diagrams,' *American Scientist*, Vol. 93 (2005), pp. 156-165 (http://web.mit.edu/dikaiser/www/FdsAmSci.pdf).

THE FOUNDATION OF MODERN OBSERVATIONAL ASTRONOMY

FLAMSTEED, John. *Historiae coelestis libri duo: quorum prior exhibet catalogum stellarum fixarum Britannicum novum & locupletissimum, una cum earundem planetarumque omnium observationibus sextante, micrometro, &c. habitis; ....* London: John Matthews, 1712.

$185,000

Large folio (390 x 267 mm), pp. [vi], vi, 60; [1], 2-360, [2], 362, [1], 363-388, [1]; [2], 120, [2, errata], with four folding plates engraved by John Senex. Engraved and illustrated half title with author's portrait, signed by Juan Bautista Catenaro and George Vertue, following full-page dedication letter illustrated and engraved by Jacobus Gibs and Louis du Guernier. Inscription on front fly-leaf, 'in hoc catalogo britannico continentur 2348 stellae fixae & variationes ascentiones recte & declinationes in illo exhibitae annis 71 perficiuntur, nempe variationes hisce locis applicatae dant illos qui egrediente anno domini 1760 caelo correspondebunt' ('this British catalogue contains the right ascension and declination of 2348 fixed stars and their variation over a period of 71 years, and can be used to obtain these variations up to the year 1760'). A few 18th century ink notes in margins of catalogue of fixed stars at beginning, including the following in the lower margin of the first leaf of text: 'NB The variations in R[ight] A[scension] and D[istance] to P[ole] are for the time in which the stars are changing their procession by one degree that is to say in 72 years. Hence as from the year 1690 to the year 1786 there are 96 years then 96 – 72 = 24 & 72 ÷ 24 = 3 [therefore] for the year 1786 add the variation and 1/3 of it'. Contemporary calf with gilt arms of Queen Anne in centre of each cover.

The true first edition, extremely rare, of Flamsteed's catalogue of fixed stars

and sextant observations, the foundation of modern observational astronomy. Flamsteed's catalogue was far more extensive and accurate than anything that had gone before. It was the first constructed with instruments using telescopic sights and micrometer eyepieces; Flamsteed was the first to study systematic errors in his instruments; he was the first to urge the fundamental importance of using clocks and taking meridian altitudes; and he insisted on having assistants to repeat the observations and the calculations. The catalogue contains about 3000 naked eye stars (Ptolemy and Tycho listed 1000, Hevelius 2000) with an accuracy of about 10 seconds of arc. However, Flamsteed, although appointed Astronomer Royal in 1675, by the turn of the eighteenth century had still not published any of his observations. Isaac Newton and Edmond Halley pressed him to do so; Flamsteed's refusal led to one of the most famous, and bitterest, disputes in the history of astronomy, and to the present work being published against Flamsteed's will. Flamsteed's response, in 1716, was to destroy 300 of the 400 copies printed, so just a few years after publication no more than 100 copies survived. Flamsteed published his own, 'authorised', version of his star catalogue in 1725. ABPC/RBH list three copies: 1. Sotheby's, April 3, 1985, lot 287, £11,000; Bonham's, November 26, 1975, lot 171, £5,400; previously sold: Sotheby's, May 7, 1935, lot 98, £29 (Halley's annotated copy, lacking the star catalogue). 3. Sotheby's, May 7, 1935, lot 99, £10.10s (the present copy). OCLC lists 11 copies in the US.

Provenance: Edward Henry Columbine (1763-1811), hydrographer and colonial governor (signature 'E. H. Columbine' on title); Radcliffe Observatory, Oxford (Sotheby's Catalogue of the Valuable Library Removed From, The Radcliffe Observatory, Oxford, Tuesday, 7th May, 1935).

"Born a somewhat sickly child at Denby, near Derby, Flamsteed's condition seems to have worsened in 1660 by what sounds like an attack of rheumatic fever. He was taken away from school and devoted himself to the study of mathematics and astronomy. A visit to Ireland in 1665 to be touched by Vincent Greatrakes, a famous healer of the day as a seventh son of a seventh son, had no effect upon his health. Shortly afterwards, however, his work began to be noticed by a number of Fellows of the Royal Society. Amongst these was Sir Jonas Moore, who was considering building a private observatory for Flamsteed. It proved unnecessary, for in 1675 Flamsteed was appointed to be the first Astronomer Royal by Charles II. As the first holder of the post, Flamsteed was responsible for the building and organisation of the new observatory at Greenwich. He also found that on a salary of £100 a year he was expected to engage and pay his own staff, and to provide his own instruments. Although some instruments were donated by Moore and others, Flamsteed still found it necessary to spend £120 of his own money on a mural arc. Made and divided by Abraham Sharp it was ready for use in September 1689. As a result of this expenditure, all observations made after 1689 seemed to Flamsteed to be unarguably his own property, and his to do with as he willed.

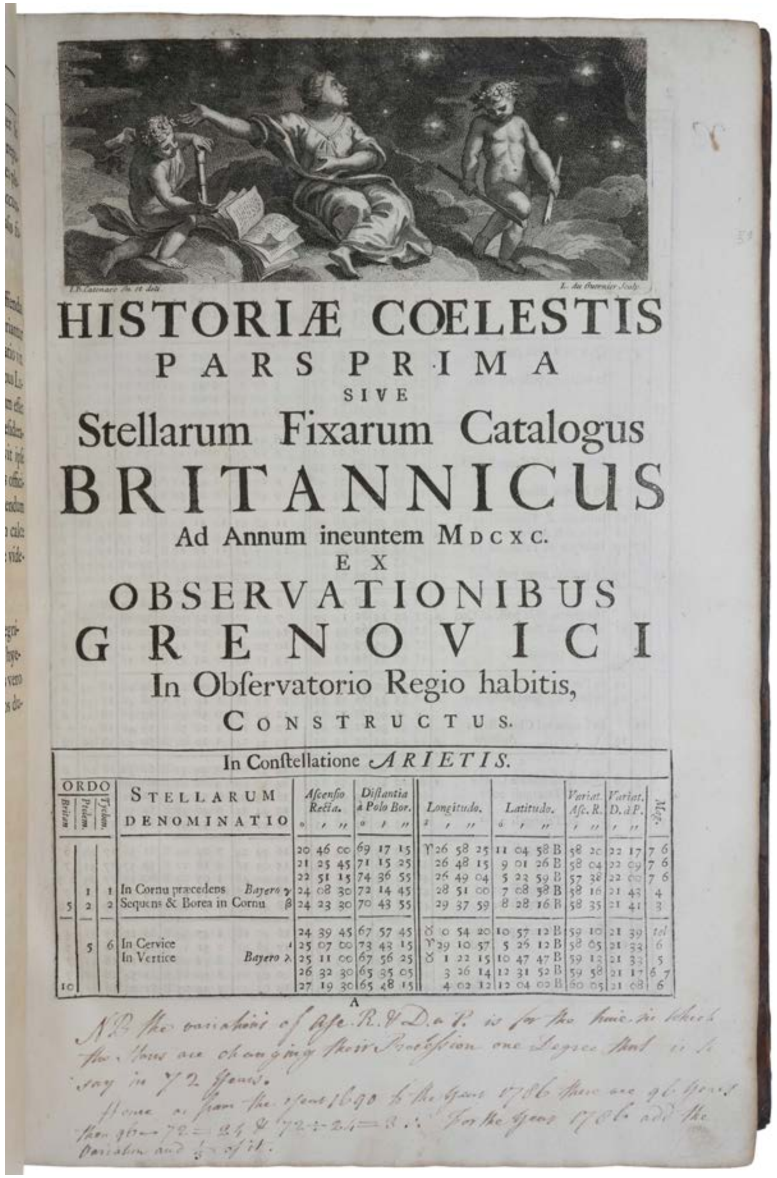
"He met Newton for the first time in Cambridge in 1674. The first substantial issue between them arose over the nature of the comet of 1680-1. Newton was convinced that two comets were present and in letters to Flamsteed argued so at length. Flamsteed, however, insisted only one comet was present, a position Newton finally accepted in September 1685. Relations remained cordial and in 1687 Flamsteed was one of the few scholars selected to receive a presentation copy of *Principia*. It contained, he noted, only 'very slight acknowledgements' to his Greenwich observations.

"On 1 September 1694 Newton paid his first visit to Greenwich. He spoke with Flamsteed about the moon. Newton was keen to examine Flamsteed's lunar data in order to correct and improve the lunar theory presented in *Principia*. Flamsteed offered to loan Newton 150 'places of the moon' on two conditions: firstly, that Newton would not show the work to anyone else; secondly, and more unreasonably, Newton would have to agree not to reveal any results derived from Flamsteed's observations to any other scholar. It was the beginning of an ill-tempered dispute which would last until Flamsteed's death. His own version of the quarrel is contained in his *History of his own Life and Labors* published in Baily (*An Account of the Revd John Flamsteed* (1966), pp. 7-105). It is a most bitter document.

"None of Newton's proposals found favour with Flamsteed. The offer in November 1694 'to gratify you to your satisfaction' brought the answer that he was not tempted with 'covetousness' and the lament that Newton could have ever thought so meanly of him. An offer in 1695 to pay Flamsteed's scribe two guineas for his transcriptions brought an equally forthright rejection. It was enough, Newton was told, to offer 'verball acknowledgements'; a 'superfluity of monys', he found, 'is always pernicious to my Servants it makes them run into company and wast their time Idly or worse'. If Newton asked for 'your Observations only', Flamsteed complained of being treated like a drudge; if, however, calculations were asked for as well, Flamsteed would respond that such work required all kinds of tedious analysis for which he had little time …

"Over the period 1694-5 Newton received another 150 observations. They were, however, none too reliable, having been made with the help of a stellar catalogue constructed with the help of a sextant alone. By this time Flamsteed was beginning to resent Newton's somewhat imperial tone. 'But I did not think myself obliged', he complained, 'to employ my pains to serve a person that was so inconsiderate as to presume he had a right to that which was only a courtesy (Baily, p. 63). Consequently, he returned to his own work, leaving Newton to work through the observations he had already received.

"The two continued to see each other and to discuss Flamsteed's lunar observations



HISTORIÆ CŒLESTIS
PARS PRIMA
SIVE
Stellarum Fixarum Catalogus
BRITANNICUS
Ad Annum ineuntem MDCXC.
EX
OBSERVATIONIBUS
GRENOVICI
In Obſervatorio Regio habitis,
CONSTRUCTUS.

In Conſtellatione *ARIETIS*.

ORDO Britan.	Ptolem.	Tychon.	STELLARUM DENOMINATIO	Aſcenſio Recta. ° ′ ″	Diſtantia à Polo Bor. ° ′ ″	Longitudo. ♈ ′ ″	Latitudo. ° ′ ″	Variat. Aſc. R. ′ ″	Variat. D. à P. ′ ″	Mag.
				20 46 00	69 17 15	♈26 58 25	11 04 58 B	58 20	22 17	7 6
				21 25 45	71 15 25	26 48 15	9 01 26 B	58 04	22 09	7 6
				22 51 15	74 36 55	26 49 04	5 22 59 B	57 38	22 00	7 6
	1	1	In Cornu præcedens Bayero γ	24 08 30	72 14 45	28 51 00	7 08 58 B	58 16	21 43	4
5	2	2	Sequens & Borea in Cornu β	24 23 30	70 43 55	29 37 59	8 28 16 B	58 35	21 41	3
				24 39 45	67 57 45	♉ 0 54 20	10 57 12 B	59 10	21 39	tel
	5	6	In Cervice ι	25 07 00	73 43 15	♈29 10 57	5 26 12 B	58 05	21 33	6
			In Vertice Bayero λ	25 11 00	67 56 25	♉ 1 22 15	10 47 47 B	59 13	21 33	5
				26 32 30	65 35 05	3 26 14	12 31 52 B	59 58	21 17	6 7
10				27 19 30	65 48 15	4 02 12	12 04 02 B	60 05	21 08	6

A

N.B. the variation of Asc. R. & D. a P. is for the time in which the Stars are changing their Procession one Degree that is to say in 72 Years.
Hence as from the year 1690 to the year 1786 there are 96 Years then 96 – 72 = 24 & 72 ÷ 24 = 3 ∴ for the year 1786 add the Variation and ⅓ of it.

until January 1699. This part of the correspondence ends with Flamsteed lecturing Newton on pride and humility. His own humility, he proudly told Newton, allowed him to 'excuse small faults in all mankind', and to 'bear great injurys without resentment'.

"The second stage of the dispute began on 11 April 1704 with a visit by Newton to Greenwich. Newton had yet to complete his lunar theory and could scarcely have looked forward to the prospect of another prolonged quarrel with Flamsteed. He seems to have decided to attempt to resolve the problem in a more direct manner. Using his position as President of the Royal Society, and his connections at Court, he sought to pressurise Flamsteed into publishing his long-awaited catalogue, thus putting all his observations into the public domain. The approach was rejected. Newton, Flamsteed noted, was too obviously someone who 'would be my friend no further than to serve his own ends … spiteful, and swayed by those that were worse than himself' (Baily, p. 66).

"Newton went over Flamsteed's head and gained the backing of Prince George, husband to Queen Anne, for the project. Scientists in the eighteenth century did not reject the offer of royal patronage. Consequently, Flamsteed in November drew up an estimate of his three-volume catalogue. The work would be 1,450 pages long and the printing of the first volume could begin immediately. Unwilling to leave the task to Flamsteed, Newton arranged instead for a Committee of Referees to examine Flamsteed's papers and to oversee publication. The members of the Committee were either, like Francis Aston and David Gregory, Newton's men or, like Sir Christopher Wren, too old and busy to concern themselves with such a task. Newton also extracted from Prince George the sum of £863 to finance the project. It soon became clear that Newton and Flamsteed had different visions of the planned work. Flamsteed had hoped to present his work within a detailed historical context by including in the third volume, along with his own stellar catalogue, all important earlier catalogues from Ptolemy to Hevelius. He also wished to add a celestial atlas consisting of sixty large star-charts. Newton's aim was much more restricted and consisted of no more than completing and publishing Flamsteed's observations.

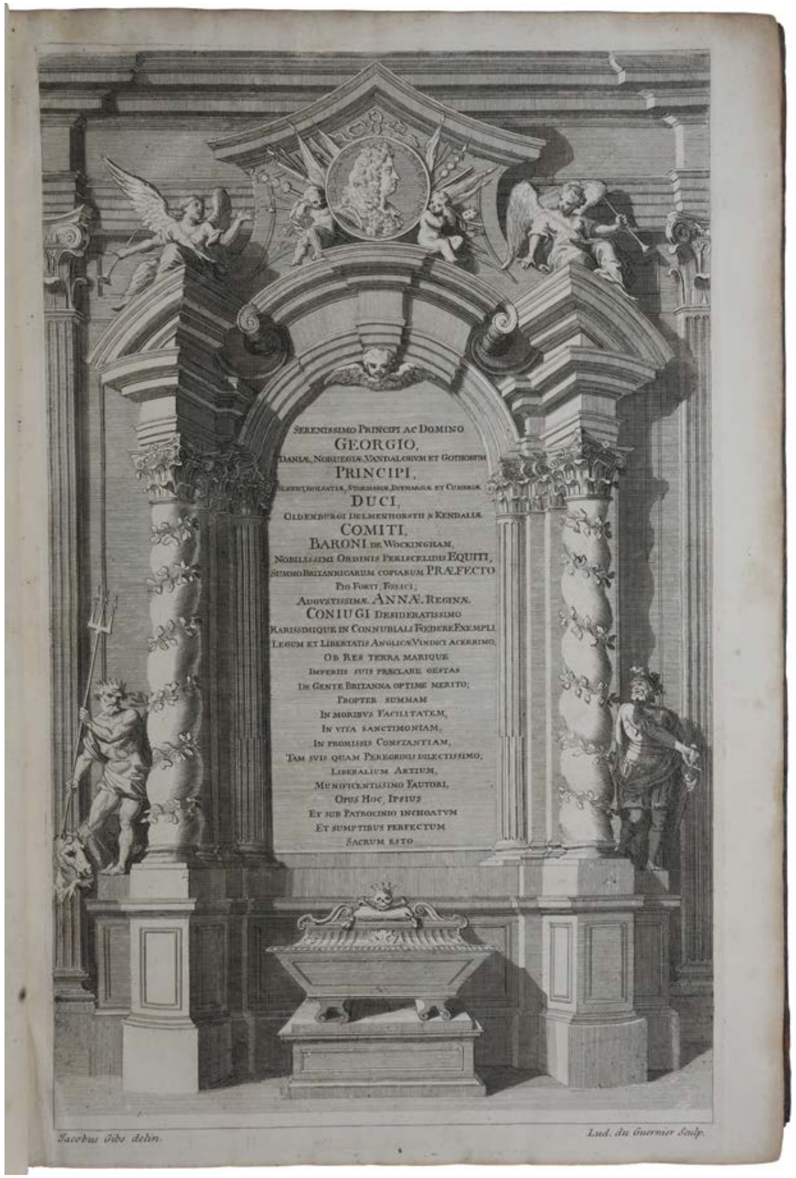
"Flamsteed could do little more than delay the project. In this he was quite successful as by 1708, when Prince George died, the first volume was still incomplete. With the death of Prince George the Referees no longer had control over Flamsteed's text. Newton's response was to have himself, as President of the Royal Society, appointed in 1710 a 'constant Visitor' to the Greenwich Observatory, with access to all observations and the right to direct the work of the Astronomer Royal. Shortly afterwards Flamsteed heard that the Queen had commanded him to hand over all outstanding material and so allow the work to be finally completed.

"It finally appeared in 1712, edited by Halley, as *Historiae coelestis* (History of the Heavens). It was not to Flamsteed's liking, seeming to him to be no more than a parody of the work he had once dreamed of publishing. Equally distressing to him was the fact that it had been produced by Halley, a man he despised as an atheist, a libertine and a plagiarist (Baily, p. xxxi)" (Gjertsen, pp. 209-212). Four hundred copies of Halley's edition were printed.

In the period 1699–1701, Flamsteed had begun to draw up pages listing stars in the zodiacal constellations, working his way through Orion and Monoceros to Lyra and Cygnus. After a hiatus of several years, Flamsteed began in August 1708 till January 1709 to work up some of the remaining constellations such as Ursa Minor and Draco. "Generally [Halley] preserved Flamsteed's order of the stars and constellations, so that his final product looked much like the lists Flamsteed had originally submitted. Halley numbered the stars in each constellation, and arranged them in groups of five instead of the triplets used by Flamsteed … However, the most conspicuous systematic alterations concerned the verbal descriptions of the places of the stars within the mythological constellation figures. Foe example, Halley always changed Flamsteed's use of *dexter* or *sinister* to less ambiguous words such as *sequens*, *praecedens*, *superior* or *Boreus*. The 1725 edition reverted to Flamsteed's original description, but, in retrospect, Halley's terminology seems generally preferable. Halley's other major task was the completion of the six northern constellations, among them Ursa Major, Ursa Minor, Cepheus, Draco and Cassiopeia …

"Ironically, one of the features of the 'corrupted' 1712 edition that Flamsteed rejected were the serial numbers for the stars in each constellation, a convenience added by Halley, and in its revised form regularly employed by astronomers today. Flamsteed omitted such numbers from the 'authorised' 1725 edition and his atlas … Thus, the familiar 'Flamsteed numbers', which eponymises the First Astronomer Royal for hundreds of amateur astronomers who might never otherwise have heard of him, were actually an invention spurned by the ever-proper Revd. John Flamsteed" (Gingerich, pp. 195-7).

With the accession of George I in 1714 Flamsteed found that at last he had friends at Court. "On his petition, Flamsteed was accordingly awarded a warrant ordering that of the 340 copies of the 1712 *Historia coelestis* still in the hands of [the printer] Awnsham Churchill, 300 should be handed over to him 'as a present from his Maj[ty]'. After a long delay, the copies were delivered. Flamsteed immediately took them to Greenwich. There he separated out the section printed when he had still been able to correct the press, setting aside 'Halley's corrupted edition of my catalogue, and [his] abridgement of my observations, no less spoiled by him.' He kept the former

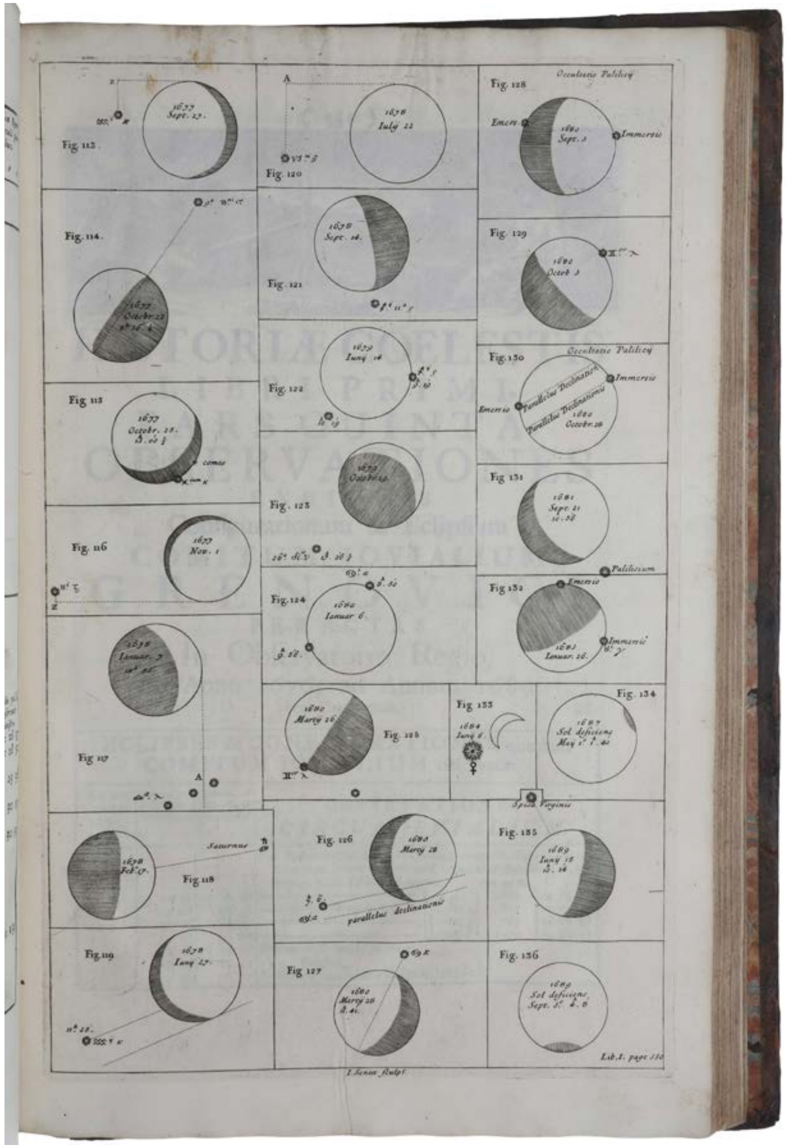


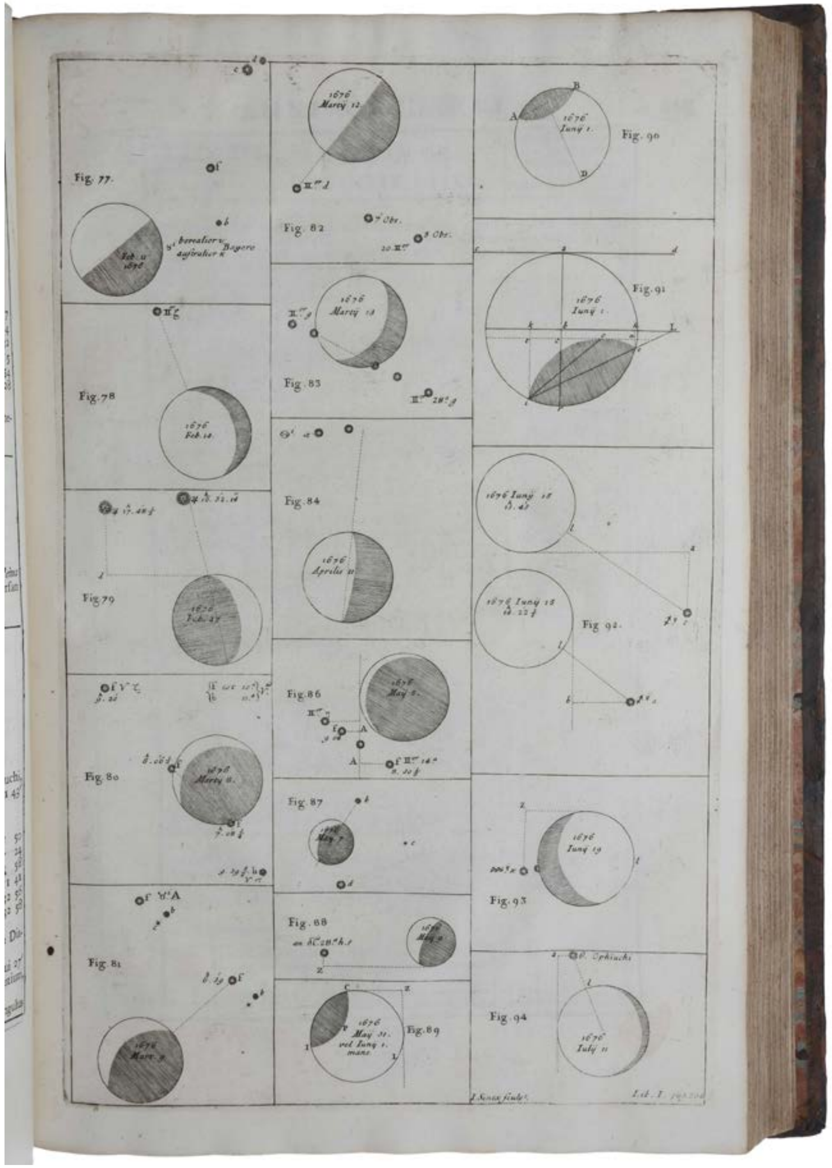
to be inserted into the edition he himself still hoped to complete. A few copies of the latter he annotated and sent to friends, as cautionary 'Evidence of y^e malice of Godlesse persons'. But in spring 1716 Flamsteed built a pyre on Greenwich Hill, and burned the sheets containing the catalogue and abridged observations. As he himself put it, they made a good 'sacrifice to TRUTH.' He would do the same to 'all the rest of my editor's pains of the like nature,' he declared, 'if the Author of Truth should hereafter put them into my power.'

"If 340 copies remained, some 60 had been distributed. Along with the remaining books, the government also demanded a full account of the dispersal of these copies. The account made clear that, as had always been intended, the first copies of the *Historia coelestis* were not published in a commercial sense. Instead they had been envisaged as royal 'presents', to be given to a selective list of recipients … More than fifty of the copies no longer in Churchill's warehouse had been dispersed. Ten had gone to courtiers, thirty to the Treasury (richly bound for use as diplomatic gifts), and ten more to the observatory and Royal Academy in Paris. Newton and Halley had got one each, Flamsteed two" (Johns, pp. 607-9).

For the rest of his life Flamsteed laboured on, and the work was completed, after his death, by his former assistants resulting in a publication containing a revised catalogue, more observations and reprints of earlier star catalogues to compare with Flamsteed's own. This was the *Historia coelestis Britannica* published in 1725, much as Flamsteed had wanted it, except for the omission of the details of his quarrels with Newton and Halley, which he had wanted to include. The atlas, which he originally intended to publish with the catalogue, was issued separately in 1729. Flamsteed only burnt Halley's preface and the catalogue from the 1712 work, re-using the sextant observations, the proofs of which he had corrected before the final rift with the Royal Society. This means that no more than the 100 copies of the 1712 work already issued can have remained, and no more than 300 copies of the 1725 work issued. Flamsteed continued to attempt to round up copies of the 1712 work and left instructions for his widow to do the same. She even wrote to the Vice-Chancellor of Oxford University in 1726 politely asking him to have the 1712 work removed from the Bodleian Library (he declined).

Gingerich, 'A unique copy of Flamsteed's Historia Coelestis,' pp. 189-197 in *Flamsteed's Stars*: *New Perspectives on the Life and Work of the First Astronomer Royal, 1646-1719* (Willmoth, ed.) (1997); Gjertsen, *The Newton Handbook* (1986); Johns, *The Nature of the Book*: *Print and Knowledge in the Making* (1998).





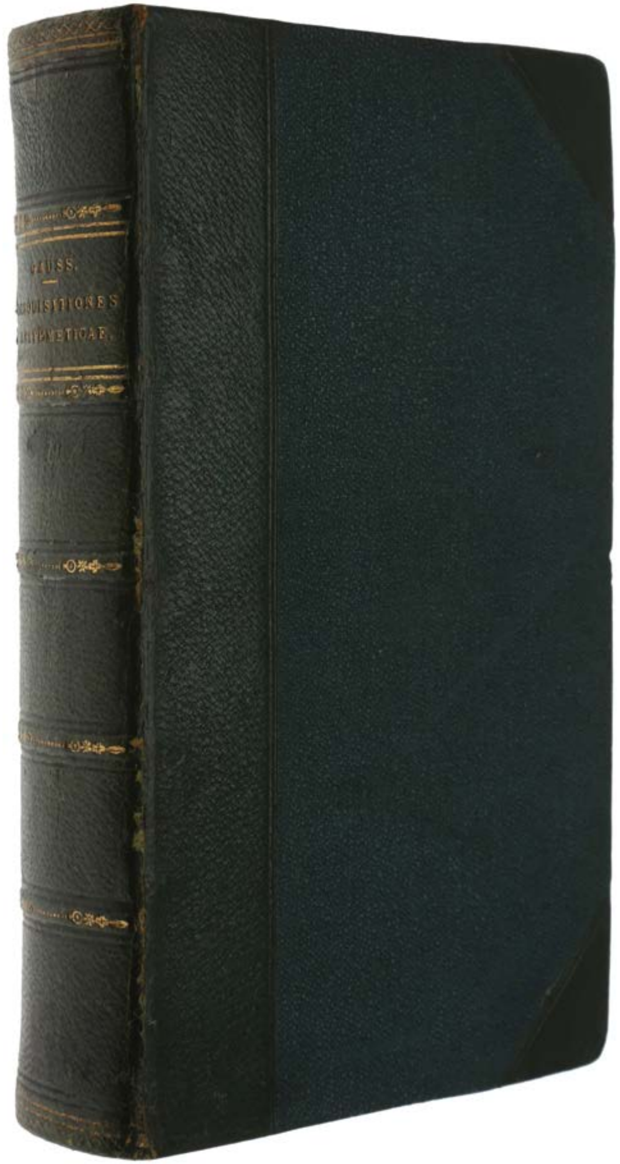
PMM 257 - 'THE PRINCE OF MATHEMATICS'

GAUSS, Carl Friedrich. *Disquisitiones arithmeticae.* Leipzig: Gerh. Fleischer, 1801.

$38,000

8vo (203 x 118 mm), contemporary green half morocco, pp [i-vi] vii-xviii [1] 2-668 [3:tables as the Horblit copy] [4:errata] with B7, G4, K3, 2F7, and 2T6 cancels (as usual), first and final leaves with some spotting as is usally seen with this work. An entirely unrestored copy.

First edition, rare, of Gauss' masterpiece, "a book that begins a new epoch in mathematics … Gauss ranks, together with Archimedes and Newton, as one of the greatest geniuses in the history of mathematics" (PMM). "Published when Gauss was just twenty-four, *Disquisitiones arithmeticae* revolutionized number theory. In this book Gauss standardized the notation; he systemized the existing theory and extended it; and he classified the problems to be studied and the known methods of attack and introduced new methods … The *Disquisitiones* not only began the modern theory of numbers but determined the direction of work in the subject up to the present time. The typesetters of this work were unable to understand Gauss' new and difficult mathematics, creating numerous elaborate mistakes which Gauss was unable to correct in proof. After the book was printed Gauss insisted that, in addition to an unusually lengthy four-page errata, the worst mistakes be corrected by cancel leaves to be inserted in copies before sale … Gauss's highly technical work was printed in a small edition, and the difficulty of understanding it was hardly alleviated by the sloppy typesetting" (Norman).



"In the late eighteenth century [number theory] consisted of a large collection of isolated results. In his *Disquisitiones* Gauss summarized previous work in a systematic way, solved some of the most difficult outstanding questions, and formulated concepts and questions that set the pattern of research for a century and still have significant today. He introduced congruence of integers with respect to a modulus ($a \equiv b$ (mod c) if c divides a - b), the first significant algebraic example of the now ubiquitous concept of equivalence relation. He proved the law of quadratic reciprocity, developed the theory of composition of quadratic forms, and completely analyzed the cyclotomic equation. The *Disquisitiones* almost instantly won Gauss recognition by mathematicians as their prince" (DSB).

"The awe that [*Disquisitiones arithmeticae*] inspired in mathematicians was displayed to the cultured public of the *Moniteur universel ou Gazette nationale* as early as March 21, 1807, when Louis Poinsot, who would succeed Joseph-Louis Lagrange at the Academy of Sciences six years later, contributed a full page article about the French translation of the *Disquisitiones arithmeticae*: 'The doctrine of numbers, in spite of [the works of previous mathematicians] has remained, so to speak, immobile, as if it were to stay for ever the touchstone of their powers and the measure of their intellectual penetration. This is why a treatise as profound and as novel as his *Arithmetical Investigations* heralds M. Gauss as one of the best mathematical minds in Europe.'

"A long string of declarations left by readers of the book, from Niels Henrik Abel to Hermann Minkowski, from Augustin-Louis Cauchy to Henry Smith, bears witness to the profit they derived from it. During the XIXth century, its fame grew to almost mythical dimensions. In 1891, Edouard Lucas referred to the *Disquisitiones Arithmeticae* as an 'imperishable monument [which] unveils the vast expanse and stunning depth of the human mind,' and in his Berlin lecture course on the concept of number, Leopold Kronecker called it 'the Book of all Books' … Gauss's book is now seen as having created number theory as a systematic discipline in its own right, with the book, as well as the new discipline, represented as a landmark of German culture …

"Gauss began to investigate arithmetical questions, at least empirically, as early as 1792, and to prepare a number-theoretical treatise in 1796 (i.e., at age 19 and, if we understand his mathematical diary correctly, soon after he had proved both the constructibility of the 17-gon by ruler and compass and the quadratic reciprocity law). An early version of the treatise was completed a year later. In November 1797, Gauss started rewriting the early version into the more mature text which he would give to the printer bit by bit. Printing started in April 1798, but proceeded very slowly for technical reasons on the part of the printer. Gauss resented this very much, as his letters show; he was looking for a permanent position from 1798. But he did use the delays to add new text, in particular to sec. 5 on quadratic forms, which had roughly doubled in length by the time the book finally appeared in the summer of 1801.

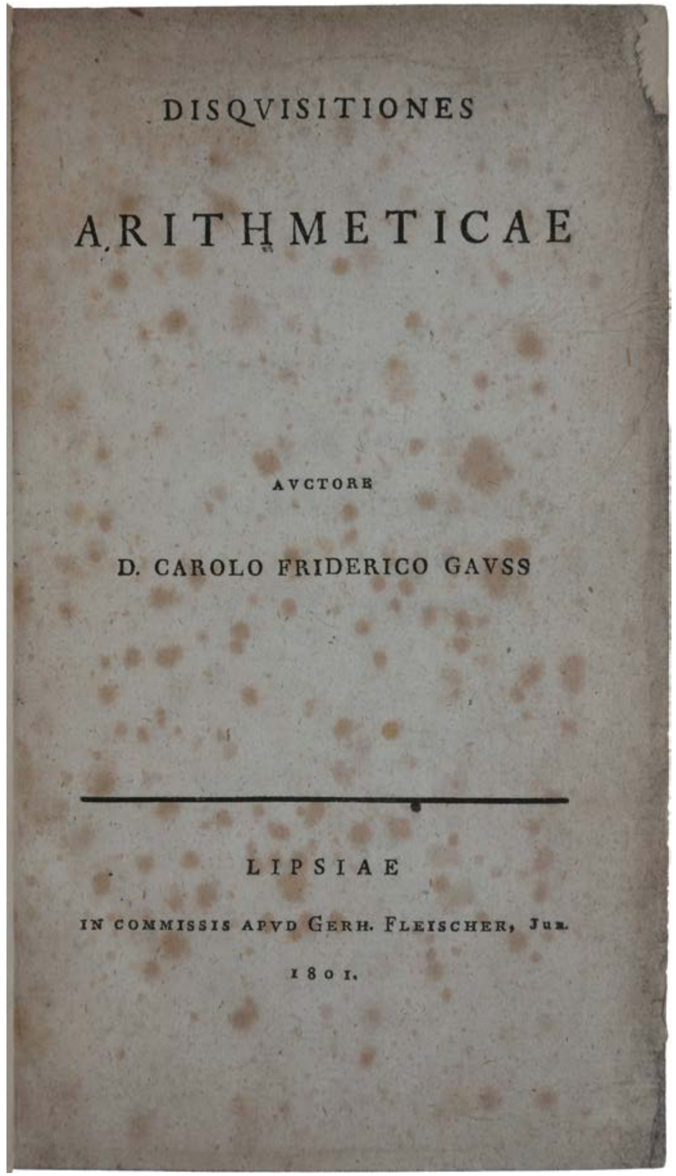
"The 665 pages and 355 articles of the main text are divided unevenly into seven sections. The first and smallest one (7 pp., 12 arts.) establishes a new notion and notation which, despite its elementary nature, modified the practice of number theory:

'If the number a measures the difference of the numbers b, c, then b and c are said to be congruent according to a; if not, incongruent; this a we call the modulus. Each of the numbers b, c are called a residue of the other in the first case, a nonresidue in the second.' The corresponding notation $b \equiv c$ (mod a) is introduced in art. 2. The remainder of sec. 1 contains basic observations on convenient sets of residues modulo a and on the compatibility of congruences with the arithmetic operations …

"Section 2 (33 pp., 32 arts.) opens with several theorems on integers including the unique prime factorization of integers (in art. 16), and then treats linear congruences in arts. 29–37, including the Euclidean algorithm and what we call the Chinese remainder theorem. At the end of sec. 2, Gauss added a few results for future reference which had not figured in the 1797 manuscript, among them: (i) properties of the number $\varphi(A)$ of prime residues modulo A (arts. 38–39); (ii) in art. 42, a proof that the product of two polynomials with leading coefficient 1 and with rational coefficients that are not all integers cannot have all its coefficients integers; and (iii) in arts. 43 and 44, a proof of Lagrange's result that a polynomial congruence modulo a prime cannot have more zeros than its degree.

"Section 3 (51 pp., 49 arts.) is entitled 'On power residues.' As Gauss put it, it treats 'geometric progressions' 1, a, a^2, a^3, ... modulo a prime number p (for a number a not divisible by p), discusses the 'period' of a modulo p and Fermat's theorem, contains two proofs for the existence of 'primitive roots' modulo p, and promotes the use of the 'indices' of 1, ..., p - 1 modulo p with respect to a fixed primitive root, in analogy with logarithm tables. After a discussion, in arts. 61–68, of nth roots mod p from the point of view of effective computations, the text returns to calculations with respect to a fixed primitive root, and gives in particular in arts. 75–78 two proofs – and sketches a third one due to Lagrange – of Wilson's theorem, $1 \cdot 2 \cdots (p - 1) \equiv -1 \pmod{p}$ …

"Section 4 (73 pp., 59 arts.), 'On congruences of degree 2,' develops a systematic theory of 'quadratic residues' (i.e., residues of perfect squares). It culminates in the 'fundamental theorem' of this theory, from which 'can be deduced almost everything that can be said about quadratic residues,' and which Gauss stated as: 'If p is a prime number of the form $4n + 1$, then $+p$, if p is of the form $4n + 3$, then $-p$, will be a [quadratic] residue, resp. nonresidue, of any prime number


DISQVISITIONES

ARITHMETICAE

AVCTORE

D. CAROLO FRIDERICO GAVSS

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1801.

which, taken positively, is a residue, resp. nonresidue of *p*.' Gauss motivated this quadratic reciprocity law experimentally, gave the general statement and formalized it in tables of possible cases … He also gave here the first proof of the law, an elementary one by induction. A crucial nontrivial ingredient (used in art. 139) is a special case of a theorem stated in art. 125, to the effect that, for every integer which is not a perfect square, there are prime numbers modulo which it is a quadratic nonresidue.

"The focus changes in sec. 5 of the *Disquisitiones arithmeticae*, which treats 'forms and indeterminate equations of the second degree,' mostly binary forms, in part also ternary. With its 357 pp. and 156 arts., this section occupies more than half of the whole *Disquisitiones Arithmeticae*. Leonhard Euler, Joseph-Louis Lagrange, and Adrien-Marie Legendre had forged tools to study the representation of integers by quadratic forms. Gauss, however, moved away from this Diophantine aspect towards a treatment of quadratic forms as objects in their own right, and, as he had done for congruences, explicitly pinpointed and named the key tools. This move is evident already in the opening of sec. 5: 'The form $axx + 2bxy + cyy$, when the indeterminates x, y are not at stake, we will write like this, (a, b, c).' Gauss then immediately singled out the quantity $bb - ac$ which he called the 'determinant' – 'on the nature of which, as we will show in the sequel, the properties of the form chiefly depend' – showing that it is a quadratic residue of any integer primitively represented by the form (art. 154).

The first part of sec. 5 (arts. 153–222, 146 pp.) is devoted to a vast enterprise of a finer classification of the forms of given determinant, to which the problem of representing numbers by forms is reduced. Gauss defined two quadratic forms (art. 158) to be equivalent if they are transformed into one another under substitutions of the indeterminates, … Two equivalent forms represent the same numbers … After generalities relating to these notions and to the representation of numbers by forms … the discussion then splits into two very different cases according to whether the determinant is negative or positive. In each case, Gauss showed that any given form is properly equivalent to a so-called 'reduced' form (art. 171 for negative, art. 183 for positive discriminants), not necessarily unique, characterized by inequalities imposed on the coefficients. The number of reduced forms – and thus also the number of equivalence classes of forms – of a given determinant is finite … Gauss settled the general problem of representing integers by quadratic forms (arts. 180–181, 205, 212), as well as the resolution in integers of quadratic equations with two unknowns and integral coefficients (art. 216) …

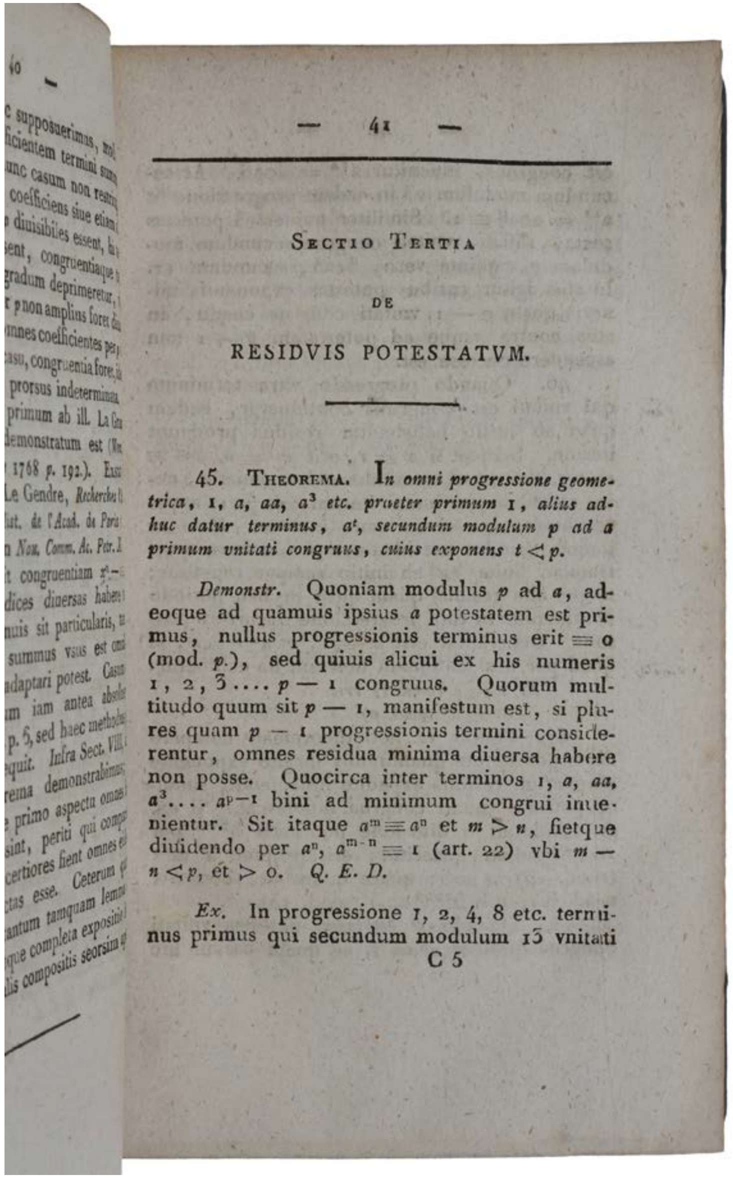
"The classification of forms also ushers the reader into the second half of sec. 5, entitled 'further investigations on forms' … In art. 226, certain classes are grouped together into an 'order' according to the divisibility properties of their coefficients. There follows (arts. 229–233) a finer grouping of the classes within a given order according to their 'genus' … This rich new structure gave Gauss a tremendous leverage: to answer new questions, for instance, on the distribution of the classes among the genera (arts. 251–253); to come back to his favourite theorem, the quadratic reciprocity law, and derive a second proof of it … (arts. 261–262); to solve a long-standing conjecture of Fermat's (art. 293) to the effect that every positive integer is the sum of three triangular numbers. For this last application, as well as for deeper insight into the number of genera, Gauss quickly generalized (art. 266 ff.) the basic theory of reduced forms, classes etc., from binary to ternary quadratic forms. This gave him in particular explicit formulae for the number of representations of binary quadratic forms, and of integers, by ternary forms, implying especially that every integer $\equiv 3 \pmod 8$ can be written as the sum of three squares, which is tantamount to Fermat's claim …

"Explicit calculations had evidently been part and parcel of number theory for Gauss ever since he acquired a copy of [Lambert, *Zusätze zu den logarithmischen* *und trigonometrischen Tabellen zur Erleichterung und Abkürzung der bey Anwendung der Mathematik vorfallenden Berechnungen*. Berlin: Haude und Spener, 1770] at age 15, and launched into counting prime numbers in given intervals in order to guess their asymptotic distribution. In these tables, Johann Heinrich Lambert made the memorable comment: 'What one has to note with respect to all factorization methods proposed so far, is that primes take longest, yet cannot be factored. This is because there is no way of knowing beforehand whether a given number has any divisors or not.' The whole *Disquisitiones arithmeticae* is illustrated by many non-trivial examples and accompanied by numerical tables. Section 6 (52 pp., 27 arts.) is explicitly dedicated to computational applications. In the earlier part of sec. 6, Gauss discussed explicit methods for partial fraction decomposition, decimal expansion, and quadratic congruences. Its latter part (arts. 329–334) takes up Lambert's problem and proposes two primality tests: one is based on the fact that a number which is a quadratic residue of a given integer M is also a quadratic residue of its divisors and relies on results of sec. 4; the second method uses the number of values of $\sqrt{-D}$ mod M, for $-D$ a quadratic residue of M, and the results on forms of determinant $-D$ established in sec. 5.

"The final Section 7 on cyclotomy (74 pp., 31 arts.) is probably the most famous part of the *Disquisitiones Arithmeticae*, then and now, because it contains the conditions of constructibility of regular polygons with ruler and compass. After a few reminders on circular functions … Gauss focused on the prime case and the irreducible equation

$$X = x^{n-1} + x^{n-2} + \ldots + x + 1 = 0, \quad n > 2 \text{ prime},$$

which his aim is to 'decompose gradually into an increasing number of factors in such a way that the coefficients of these factors can be determined by equations



— 41 —

SECTIO TERTIA

DE

RESIDVIS POTESTATVM.

45. THEOREMA. *In omni progressione geometrica, 1, a, aa, a^3 etc. praeter primum 1, alius adhuc datur terminus, a^t, secundum modulum p ad a primum vnitati congruus, cuius exponens $t < p$.*

Demonstr. Quoniam modulus p ad a, adeoque ad quamuis ipsius a potestatem est primus, nullus progressionis terminus erit $\equiv 0$ (mod. p.), sed quiuis alicui ex his numeris $1, 2, 3 \ldots\ldots p - 1$ congruus. Quorum multitudo quum sit $p - 1$, manifestum est, si plures quam $p - 1$ progressionis termini considerentur, omnes residua minima diuersa habere non posse. Quocirca inter terminos $1, a, aa, a^3 \ldots\ldots a^{p-1}$ bini ad minimum congrui inuenientur. Sit itaque $a^m \equiv a^n$ et $m > n$, fietque diuidendo per a^n, $a^{m-n} \equiv 1$ (art. 22) vbi $m - n < p$, et > 0. *Q. E. D.*

Ex. In progressione 1, 2, 4, 8 etc. terminus primus qui secundum modulum 13 vnitati

C 5

of as low a degree as possible, until one arrives at simple factors, i.e., at the roots of *X*.' Art. 353 illustrates the procedure for $n = 19$, which requires solving two equations of degree three and one quadratic equation (because $n - 1 = 3{\cdot}3{\cdot}2$); art. 354 does the same for $n = 17$ which leads to four quadratic equations ($n - 1 = 2{\cdot}2{\cdot}2{\cdot}2$) …

"Complementary results on the auxiliary equations, i.e., those satisfied by the sums over all the roots of unity in a given period, are given in art. 359, applications to the division of the circle in the final arts. 365 and 366. As a by product of his resolution of $X = 0$, Gauss also initiated a study of what are today called 'Gauss sums,' i.e., certain (weighted) sums of roots of unity, like the sum of a period, or of special values of circular functions …

"Despite the impressive theoretical display of sec. 5, one cannot fully grasp the systemic qualities of the *Disquisitiones arithmeticae* from the torso that Gauss published in 1801. At several places in the *Disquisitiones arithmeticae* and in his correspondence a forthcoming volume II is referred to. The only solid piece of evidence we have is what remains of Gauss's 1796–1797 manuscript of the treatise. This differs from the structure of the published *Disquisitiones arithmeticae* in that it contains an (incomplete) 8th chapter (*caput octavum*), devoted to higher congruences, i.e., polynomials with integer coefficients taken modulo a prime and modulo an irreducible polynomial. Thus, according to Gauss's original plan, sec. 7 would not have been so conspicuously isolated, but would have been naturally integrated into a greater, systemic unity. The division of the circle would have provided a model for the topic of the *caput octavum*, the theory of higher congruences; it would have appeared as part of a theory which, among many other insights, yields two entirely new proofs of the quadratic reciprocity law" (Goldstein & Schappacher).

PMM 257; Evans 11; Horblit 38; Dibner 114. Goldstein & Schappacher, 'A book in search of a discipline (1801-1860),' pp. 3-66 in *The Shaping of Arithmetic after C. F. Gauss's Disquisitiones Arithmeticae*, 2007.

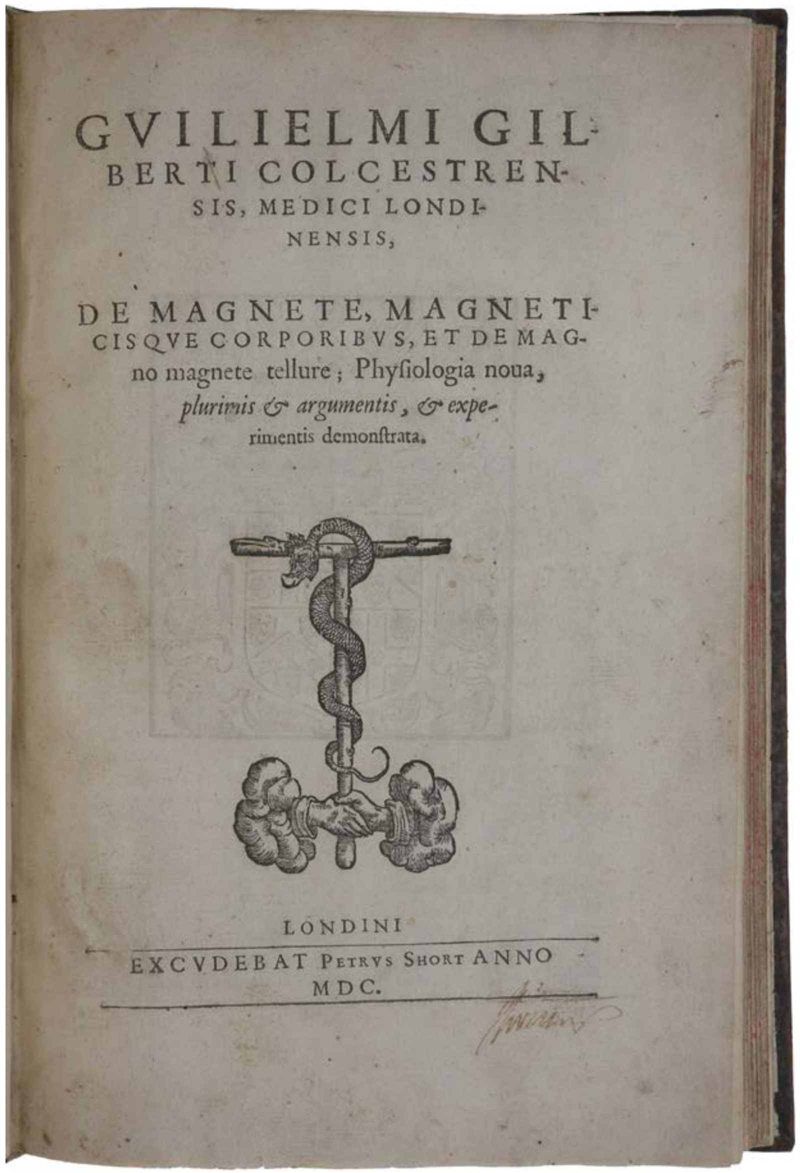
PMM 107 - THE FOUNDER OF ELECTRICAL SCIENCE

GILBERT, William. *De magnete, magneticisque corporibus, et de mango magnete tellure; Physiologia nova, plurimis & argumentis, & experimentis demonstrate.* London: Peter Short, 1600.

$78,000

Folio (282 x 182 mm), pp. [xvi], 240, woodcut printer's device (McKerrow 119) on title, large woodcut arms of Gilbert on title verso, one woodcut folding plate, 88 woodcut illustrations and diagrams in text (4 full-page), ornamental woodcut headpieces and initials. Contemporary calf, an excpetionally fine and crisp copy.

First edition of "the first major English scientific treatise based on experimental methods of research. Gilbert was chiefly concerned with magnetism; but as a digression he discusses in his second book the attractive effect of amber (electrum), and thus may be regarded as the founder of electrical science. He coined the terms 'electricity,' 'electric force' and 'electric attraction.' His 'versorium', a short needle balanced on a sharp point to enable it to move freely, is the first instrument designed for the study of electrical phenomena, serving both as an electroscope and electrometer. He contended that the earth was one great magnet; he distinguished magnetic mass from weight; and he worked on the application of terrestrial magnetism to navigation. Gilbert's book influenced Kepler, Bacon, Boyle, Newton and, in particular, Galileo, who used his theories [in the *Dialogo*] to support his own proof of the correctness of the findings of Copernicus in cosmology" (PMM). "Gilbert provided the only fully developed theory … and the first comprehensive discussion of magnetism since the thirteenth century *Letter on the Magnet* of Peter Peregrinus" (DSB). Although this book does appear



GVILIELMI GIL-
BERTI COLCESTREN-
SIS, MEDICI LONDI-
NENSIS,

DE MAGNETE, MAGNETI-
CISQVE CORPORIBVS, ET DE MAG-
no magnete tellure; Physiologia noua,
plurimis & argumentis, & expe-
rimentis demonstrata.

LONDINI
EXCVDEBAT PETRVS SHORT ANNO
MDC.

with some regularity on the market, copies such as ours in fine condition and in untouched contemporary bindings are rare.

“During the fifteenth century the widespread interest in navigation had focused much attention on the compass. Since at that time the orientation of the magnetic needle was explained by an alignment of the magnetic poles with the poles of the celestial sphere, the diverse areas of geography, astronomy, and phenomena concerning the lodestone overlapped and were often intermingled. Navigators had noted the variation from the meridian and the dip of the magnetic needle and had suggested ways of accounting for and using these as aids in navigation. The connection between magnetic studies and astronomy was less definite; but so long as the orientation of the compass was associated with the celestial poles, the two studies were interdependent …

“Gilbert divided his *De magnete* into six books. The first deals with the history of magnetism from the earliest legends about the lodestone to the facts and theories known to Gilbert’s contemporaries … In the last chapter of book I, Gilbert introduced his new basic idea which was to explain all terrestrial magnetic phenomena: his postulate that the earth is a giant lodestone and thus has magnetic properties … The remaining five books of the *De magnete* are concerned with the five magnetic movements: coition, direction, variation, declination and revolution. Before he began his discussion of coition, however, Gilbert carefully distinguished the attraction due to the amber effect from that caused by the lodestone. This section, chapter 2 of book II, established the study of the amber effect as a discipline separate from that of magnetic phenomena, introduced the vocabulary of electrics, and is the basis for Gilbert’s place in the history of electricity …

“Having distinguished the magnetic and amber effects, Gilbert presented a list of many substances other than amber which, when rubbed, exhibit the same effect. These he called electrics. All other solids were nonelectrics. To determine whether a substance was an electric, Gilbert devised a testing instrument, the versorium. This was a small, metallic needle so balanced that it easily turned about a vertical axis. The rubbed substance was brought near the versorium. If the needle turned, the substance was an electric; if the needle did not turn, the substance was a nonelectric.

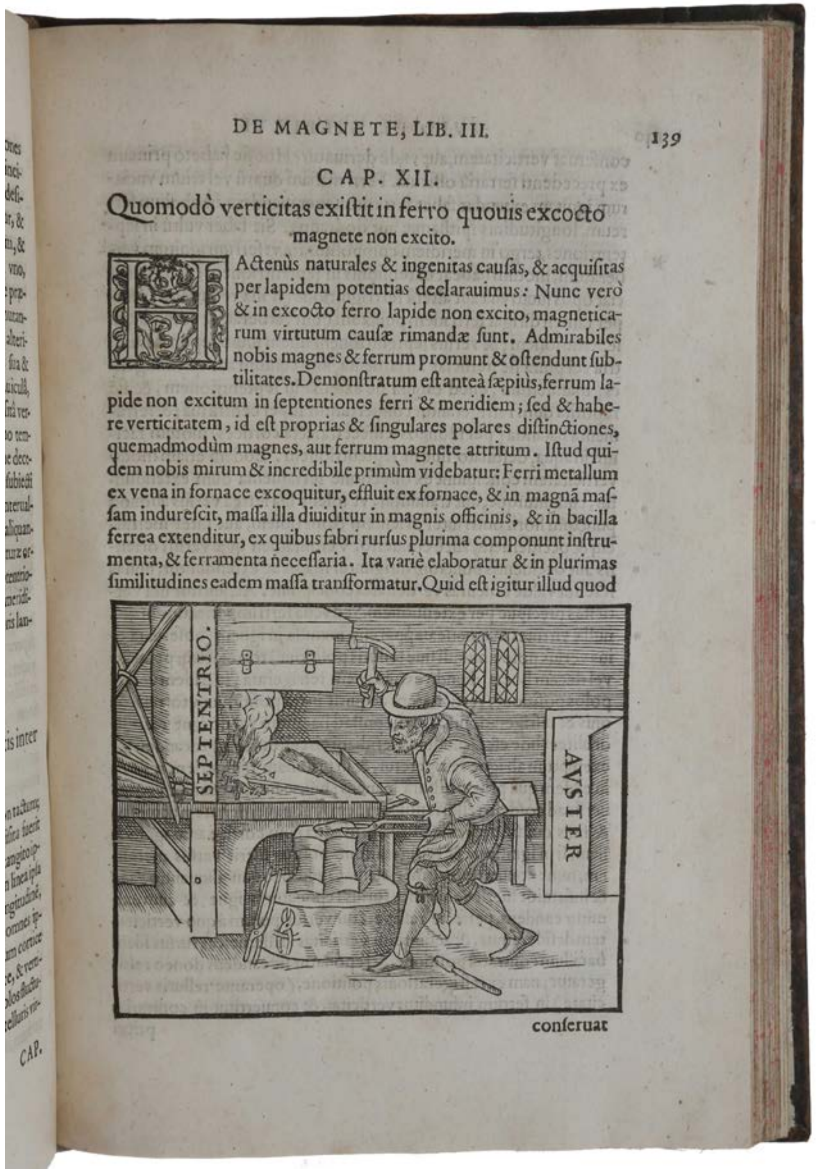
“After disposing of the amber effect, Gilbert returned to his study of the magnetic phenomena. In discussing these, Gilbert relied for his explanations on several assumptions: (1) the earth is a giant lodestone and has the magnetic property; (2) the magnetic property is due to the form of the substance; (3) every magnet is surrounded by an invisible orb of virtue which extends in all directions from it; (4) pieces of iron or other magnetic materials within this orb of virtue will be affected by and will affect the magnet within the orb of virtue; and (5) a small, spherical magnet resembles the earth and what can be demonstrated with it is applicable to the earth. This small spherical magnet he called a terrella …

“In discussing coition Gilbert was careful to distinguish magnetic coition from other attractions. For him magnetic coition was a mutual action between the attracting body and the attracted body. At the beginning of the *De magnete* he explained several terms that were necessary for understanding his work. One of these was “magnetic coition,” which he said he “used rather than attraction because magnetic movements do not result from attraction of one body alone but from the coming together of two bodies harmoniously (not the drawing of one by the other)” (P. Fleury Mottelay, *William Gilbert of Colchester ... on the Great Magnet of the Earth*, 1893, p. liv) …

"Book III of the *De magnete* contains Gilbert's explanation of the orientation taken by a lodestone that is balanced and free to turn, that is, the behavior of the magnetic compass … the orientation of the compass was simply an alignment of the magnetic needle with the north and south poles of the earth. Gilbert gave numerous demonstrations of this with the terrella as well as directions for magnetizing iron.

"By the end of the sixteenth century, navigators were well acquainted with variations from the meridian in the orientation of the compass. Thus, after discussing orientation, Gilbert turned in book IV to the variations in that orientation. Here he again used the comparison of the phenomena that can be demonstrated with the terrella and those that occur on the surface of the globe. Just as a very small magnetic needle will vary its orientation if the terrella on which it is placed is not a perfect sphere, so will the compass needle vary its orientation on the surface of the earth according to the proximity or remoteness of the masses of earth extending beyond the basic spherical core. Also, the purity of these masses (the amount of primary magnetic property retained by them) will affect the orientation of the compass just as stronger lodestones have greater attractive powers than weaker ones.

"The next magnetic movement that Gilbert discussed was declination, the variation from the horizontal. This phenomenon had been described by Robert Norman in his book on magnetism, *The New Attractive* (1581). Although Norman had also given an effective means of constructing the compass needle so that it would not dip but would remain parallel to the horizontal, he had made no attempt to account for this strange behavior. As with the other magnetic effects of the compass, Gilbert explained declination in terms of the magnetic property of the earth and the experiments with the terrella. The small needle placed on the terrella maintained a horizontal position only when placed on the equator.



DE MAGNETE, LIB. III. 139

CAP. XII.

Quomodò verticitas exiſtit in ferro quouis excocto magnete non excito.

HActenùs naturales & ingenitas cauſas, & acquiſitas per lapidem potentias declarauimus: Nunc verò & in excocto ferro lapide non excito, magneticarum virtutum cauſæ rimandæ ſunt. Admirabiles nobis magnes & ferrum promunt & oſtendunt ſubtilitates. Demonſtratum eſt anteà ſæpiùs, ferrum lapide non excitum in ſeptentiones ferri & meridiem; ſed & habere verticitatem, id eſt proprias & ſingulares polares diſtinctiones, quemadmodùm magnes, aut ferrum magnete attritum. Iſtud quidem nobis mirum & incredibile primùm videbatur: Ferri metallum ex vena in fornace excoquitur, effluit ex fornace, & in magnã maſſam indureſcit, maſſa illa diuiditur in magnis officinis, & in bacilla ferrea extenditur, ex quibus fabri rurſus plurima componunt inſtrumenta, & ferramenta neceſſaria. Ita variè elaboratur & in plurimas ſimilitudines eadem maſſa tranſformatur. Quid eſt igitur illud quod

conſeruat

When moved north or south of this position, the end of the needle closer to one pole of the terrella dipped toward that pole. The amount of dip increased as the needle was moved nearer the pole, until it assumed a perpendicular position when placed on the pole. A compass on the earth, according to Gilbert, behaved in a similar manner.

"In discussing the variations from the meridian and the horizontal, Gilbert suggested practical applications of his theory. Navigators of the period were concerned with determining the longitude and latitude of their positions on the open seas. Since the deviation from the meridian was constant at a given point, Gilbert thought that if the seamen would record these variations at many points, an accurate table of variation for various positions could be compiled and the problem would be solved. He included detailed instructions for the construction of the instruments necessary for this task …

"The final book of the *De magnete*, book VI, deals with rotation and in this section Gilbert expounded his cosmological theories. Without discussing whether the universe is heliocentric or geocentric, Gilbert accepted and explained the diurnal rotation of the earth. From the time of Peter Peregrinus' *Letter on the magnet*, written in the thirteenth century, rotation had been considered one of the magnetic movements. The assumption was that a truly spherical, perfectly balanced lodestone, perfectly aligned with the celestial poles, would rotate on its axis once in twenty-four hours. Since the earth was such a lodestone, it would turn upon its axis in that manner and thus the diurnal motion of the earth was explained. The theory was taken from Peter's *Letter*; the application to the earth was Gilbert's addition … Much of the criticism directed by Bacon and others against Gilbert's writing was based upon the sixth book of the *De magnete*, where Gilbert extended to the cosmos his magnetic theory and the results obtained from his experiments.

"Throughout the *De magnete*, Gilbert discussed and usually dismissed previous theories concerning magnetic phenomena and offered observational data and experiments which would support his own theories. Most of the experiments are so well described that the reader can duplicate them if he wishes, and the examples of natural occurrences which support his theory are well identified. Where new instruments are introduced (for example, the versorium, to be used in identifying electrics), directions for their construction and use are included. The combination, a new theory supported by confirming evidence and demonstrations, is a pre-Baconian example of the new experimental philosophy which became popular in the seventeenth century" (DSB).

Dibner 54; Grolier/Horblit 41; Heilbron, Electricity in the 17th and 18th Centuries, pp. 169-179; Norman 905; PMM 107; STC 11883; Wellcome 2830.

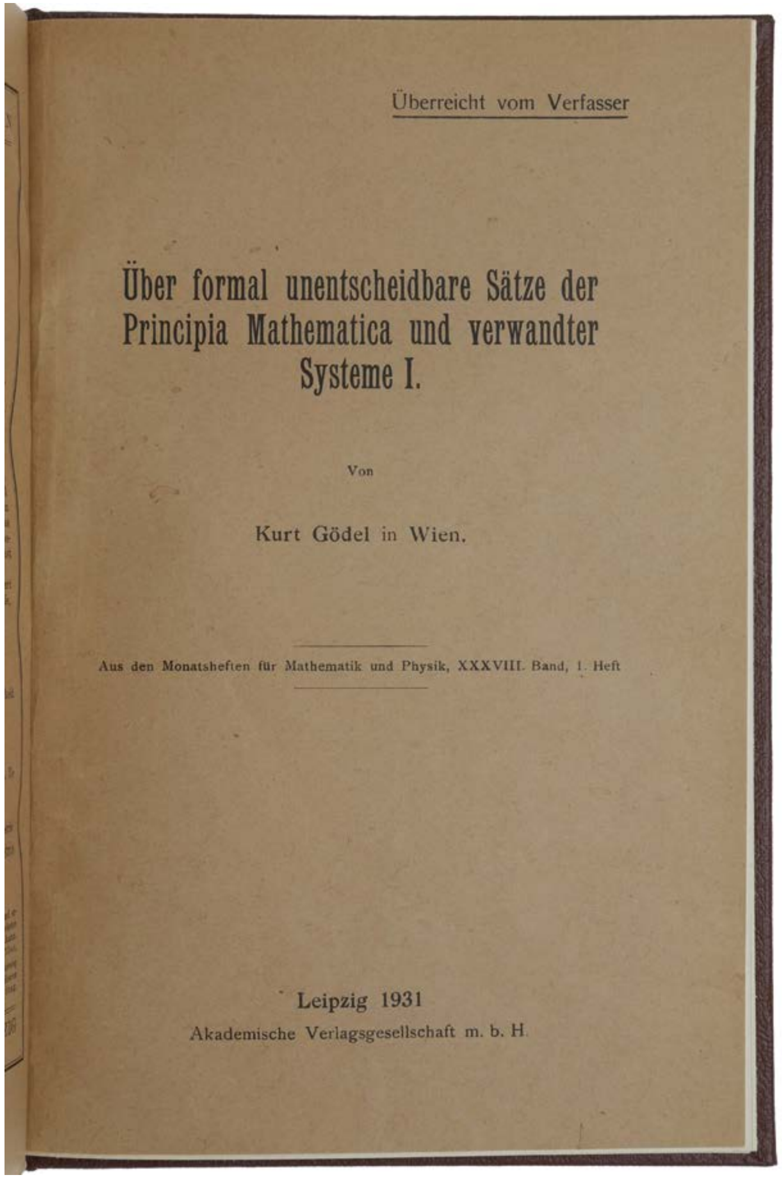
THE INCOMPLETENESS THEOREM

GÖDEL, Kurt. *Über formal unentscheidbare Sätze der Principia Mathematica undver wandter Systeme I. Offprint from: Monatshefte für Mathematik und Physik 38, 1931. [Bound with:] Über die Vollständigkeit der Axiome des logischen Funktionenkalküls. Offprint from: Monatshefte für Mathematik und Physik 37, 1930. Leipzig: Akademische Verlagsgesellschaft, 1931, 1930. [Bound with:] VON WRIGHT, Georg Henrik. Typed letter signed 'Georg Henrik von Wright' in Swedish on Academy of Finland letterhead.* Leipzig: Akademische Verlagsgesellschaft, 1931; 1930.

$140,000

8vo (227 x 153 mm). [1931:] pp. 173-198. [1930:] pp. 349-360. Original tan printed wrappers, front wrappers each with printed presentation statement in German 'Überreicht vom Verfasser.' A few light pencil notations in the 1931 offprint, presumably in the hand of Eino Kaila. Light diagonal crease to upper corner of the 1930 offprint, some faint crinkling and some small spots to front wrapper of second offprint, but otherwise fine. Bound together in brown cloth.

First edition, extremely rare author's presentation offprint, of Gödel's famous incompleteness theorem, "one of the major contributions to modern scientific thought" (Nagel & Newman). "Every system of arithmetic contains arithmetical propositions, by which is meant propositions concerned solely with relations between whole numbers, which can neither be proved nor be disproved within the system. This epoch-making discovery by Kurt Gödel, a young Austrian mathematician, was announced by him to the Vienna Academy of Sciences in 1930 and was published, with a detailed proof, in a paper in the *Monatshefte für*



Mathematik und Physik, Volume 38, pp. 173-198" (R. B. Braithwaite in Gödel/Meltzer, p. 1). "This theorem is an important limiting result regarding the power of formal axiomatics, but has also been of immense importance in other areas, such as the theory of computability" (Zach, p. 917). Gödel "obtained what may be the most important mathematical result of the 20th century: his famous incompleteness theorem, which states that within any axiomatic mathematical system there are propositions that cannot be proved or disproved on the basis of the axioms within that system; thus, such a system cannot be simultaneously complete and consistent. This proof established Gödel as one of the greatest logicians since Aristotle, and its repercussions continue to be felt and debated today" (Britannica). The offprint of Gödel's incompleteness theorem is here accompanied by an author's presentation offprint of his earlier completeness theorem for first-order logic. "In his doctoral thesis, 'Über die Vollständigkeit des Logikkalküls' ('On the Completeness of the Calculus of Logic'), published in a slightly shortened form in 1930, Gödel proved one of the most important logical results of the century—indeed, of all time—namely, the completeness theorem, which established that classical first-order logic, or predicate calculus, is complete in the sense that all of the first-order logical truths can be proved in standard first-order proof systems. This, however, was nothing compared with what Gödel published in 1931—namely, the incompleteness theorem: 'Über formal unentscheidbare Sätze der *Principia Mathematica* und verwandter Systeme I' ('On Formally Undecidable Propositions of *Principia Mathematica* and Related Systems')" (Britannica). Gödel intended to write a second part to the 1931 paper, but this was never published. OCLC lists two copies of the 1931 offprint (both in Canada), and none of the 1930 offprint. ABPC/RBH list two copies of each offprint, the most recent being those sold at Christie's, London, 19 November 2014, which realised £104,500 ($167,000) and £35,000 ($55,930), respectively.

Provenance: The history of the present volume is explained in the accompanying letter from von Wright to von Plato, which reads, in translation:

11 Oct. 2000

Dear Jan,

The two essays were in the estate of Eino Kaila. In all probability, he had them directly from the Author. I hope that you appreciate having them. I had them bound together and hand them now, on the day of your inaugural lecture as Swedish professor of philosophy, to you with my wishes for the best of luck.

Your devoted,

Georg Henrik von Wright.

The Finnish philosopher Eino Kaila (1890-1958) worked in the early 1930s in Vienna and became associated to the Vienna Circle. He introduced its ideas to Finnish philosophical debate in *Der Logistische Neupositivismus* (1930, *The new logical-positivism*) and *Inhimillinen tieto* (1939, *Human knowledge*), an overview of the epistemological theory of logical empiricism. Kaila knew personally several members of the Circle and took part in its sessions, as did Gödel.

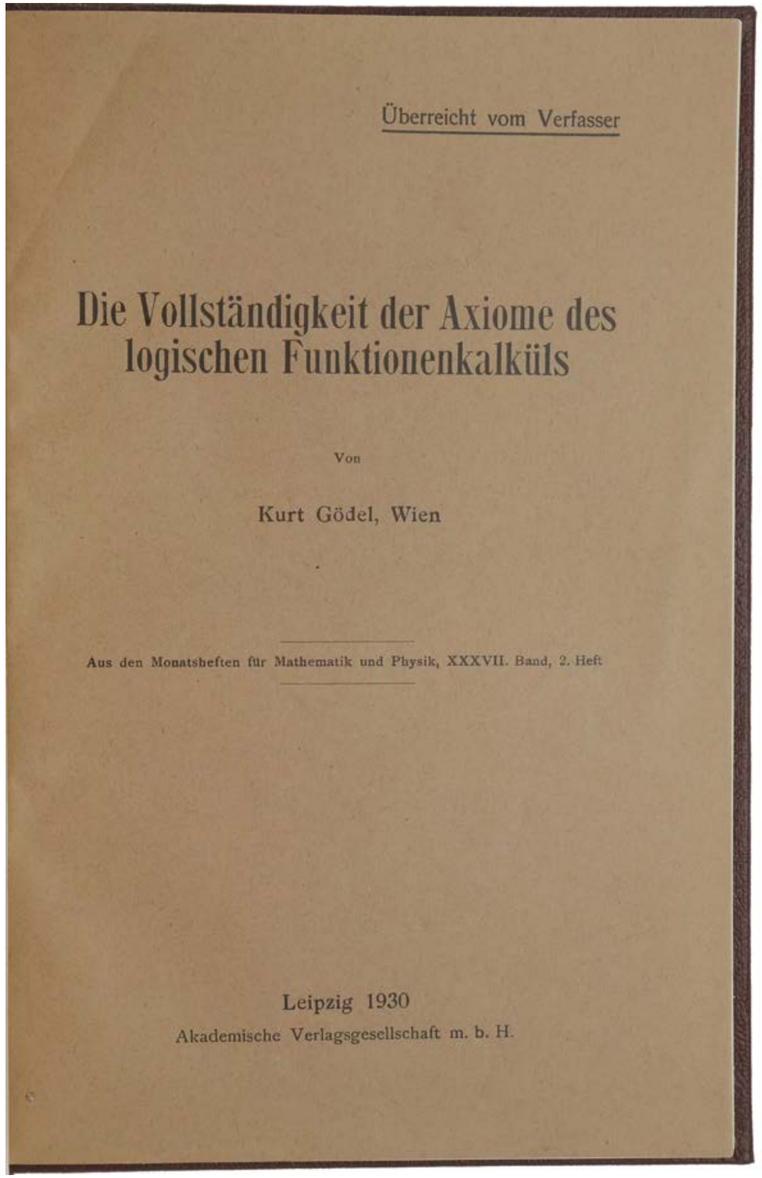
After Kaila's death, the offprints were acquired by Georg Henrik von Wright (1916-2003), the famous Finnish philosopher (partly of Scottish ancestry) who had studied under Kaila at the University of Helsinki. Von Wright was also a relative of Kaila: his mother was the cousin of Kaila's wife Anna. Von Wright, who made major contributions to logic and the philosophy of science, and latterly in ethics and the humanities, was deeply influenced by Ludwig Wittgenstein, succeeded him as professor at Cambridge University from 1948-52, and was later executor of Wittgenstein's estate. Von Wright was the first holder of the Swedish-language Chair of Philosophy at the University of Helsinki (he was a member of the Swedish-speaking minority in Finland), a post he held from 1946 until his retirement in 1961, when he was appointed to the 12-member Academy of Finland. He is one of the very few philosophers to whom a volume is dedicated in the *Library of Living Philosophers* series. Its current editor, Randall E. Auxier, has written: 'There is no Nobel Prize in philosophy, but being selected for inclusion in the *Library of Living Philosophers* is, along with the Gifford Lectures, perhaps the highest honor a philosopher can receive.'

In the year 2000, von Wright had the two offprints bound together as the present volume, with the spine lettered 'Gödel: ZWEI AUFSATZE' (Gödel: Two Essays), and presented it to his successor as Swedish-language Chair of Philosophy at the University of Helsinki, Jan von Plato (b. 1951). Von Plato works on proof theory, and is the author of *Structural Proof Theory* (2001) and *Proof Analysis*: *A Contribution to Hilbert's Last Problem* (2011).

Following his graduation from the Gymnasium in Brno, Moravia, in 1924, Gödel (1906-78) went to Vienna to begin his studies at the University. Vienna was to be his home for the next fifteen years, and in 1929 he was also to become an Austrian citizen. Gödel's principal teacher was the German mathematician Hans Hahn (1879-1934), who was interested in modern analysis and set-theoretic topology, as well as logic, the foundations of mathematics, and the philosophy of science. It was Hahn who introduced Gödel to the group of philosophers around Moritz Schlick (1882-1936); this group was later baptized as the 'Vienna Circle' and became identified with the philosophical doctrine called logical positivism. Gödel attended meetings of the Circle quite regularly in the period 1926-1928, but in the following years gradually moved away from it as his own developing philosophical views were opposed to those of the Circle. Nevertheless, the lectures on mathematical logic of one member of the Circle, Rudolph Carnap (1891-1970), were one of the main influences on Gödel in his choice of direction for creative work. The other was the *Grundzüge der theoretischen Logik* (1928) by David Hilbert (1862-1943) and Wilhelm Ackermann (1896-1962), which posed as an open problem the question whether a certain system of axioms for the first-order predicate calculus is complete. In other words, does it suffice for the derivation of every statement that is logically valid (in the sense of being correct under every possible interpretation of its basic terms and predicates)? Gödel arrived at a positive solution to the completeness problem and with that notable achievement commenced his research career. The work, which was to become his doctoral dissertation at the University of Vienna, was finished in the summer of 1929. The degree itself was granted in February 1930, and a revised version of the dissertation was published as 'Über die *Vollständigkeit* der Axiome des logischen Funktionenkalküls.' Although recognition of the fundamental significance of this work would be a gradual matter, at the time the results were already sufficiently distinctive to establish Gödel as a rising star.

"Gödel's solution of the completeness problem posed in Hilbert and Ackermann constituted his first major result … The question was whether validity in the first-order predicate calculus (or the restricted functional calculus, as it was then called) is equivalent to provability in a specific system of axioms and rules of inference. Gödel's affirmative solution actually established more, implying one version of the 'downward' Löwenheim-Skolem theorem … Gödel also extended this result to denumerable sets of formulas which, if consistent with the system, have a denumerable model [this is now called the 'compactness theorem']. The paper largely follows the dissertation, with two significant exceptions, one being a deletion and one an addition. First of all, the very interesting informal section with which [the dissertation] began was omitted in [the published paper]. In that section Gödel had situated his work relative to the ideas of Hilbert and Brouwer, arguing against both in certain respects. Even more noteworthy is that he had already raised the possibility of incompleteness of mathematical axiom systems in the deleted introduction. Secondly, Gödel added … the completeness theorem [which] proved to be fundamental for the subject of model theory some years later" (Feferman *et al*, p. 17).

The ten years 1929-1939 were a period of intense work for Gödel which resulted in his major achievements in mathematical logic. In 1930 he began to pursue Hilbert's programme for establishing the consistency of formal axiom systems for mathematics by finitary means. "According to Hilbert, there is a central 'finitary' core of mathematics that is unquestionably reliable. Its subject matter is strings of characters on a finite alphabet or, equivalently, natural numbers. There are, of course, infinitely many strings and natural numbers, but Hilbert did not regard them as a 'complete and closed' totality. The domains are merely 'potentially infinite', in the sense that there is no upper bound on the size of strings that can be considered — given any string, one can always produce a larger one. As indicated, unrestricted quantifiers are banned from finitary mathematics; every quantifier must be restricted to a finite domain. To be sure, mathematics goes well beyond the finitary and, unlike the intuitionists and constructivists, Hilbert is not out to restrict available methodology. The idea is that the non-finitary parts of mathematics be regarded as meaningless, akin to the ideal 'points at infinity' sometimes introduced into geometry. The purpose of non-finitary systems is to streamline inferences leading to finitary conclusions. With a view like this, we need some assurance that employing the non-finitary methods will not lead to results that are refuted on finitary grounds; that is, we need a guarantee that the non-finitary system is consistent with finitary mathematics. To achieve this, the Hilbert programme called for the discourse of each branch of mathematics to



be cast in a rigorously specified deductive system. These deductive systems are to be studied syntactically, with the aim of establishing their consistency. For this metamathematics, only finitary methods are to be employed. Thus, if the programme were successful, finitary mathematics would establish that deductive systems are consistent, and can be used to derive finitary results with full assurance that the latter are correct" (Shapiro, pp. 647-8).

"Gödel started by working on the consistency problem for analysis, which he sought to reduce to that for arithmetic, but his plan led him to an obstacle related to the well-known paradoxes of truth and definability in ordinary language. While Gödel saw that these paradoxes did not apply to the precisely specified languages of the formal systems he was considering, he realized that analogous non-paradoxical arguments could be carried out by substituting the notion of provability for that of truth. Pursuing this realization, he was led to the following unexpected conclusions. Any formal system S in which a certain amount of theoretical arithmetic can be developed and which satisfies some minimal consistency conditions is *incomplete*: one can construct an elementary arithmetical statement *A* such that neither *A* nor its negation is provable in S. In fact, the statement so constructed is true, since it expresses its own unprovability in S via a representation of the syntax of S in arithmetic (the technical device used for this construction is now called 'Gödel numbering'). Furthermore, one can construct a statement *C* which expresses the consistency of S in arithmetic, and *C* is not provable in S if S is consistent. It follows that, if the body of finitary combinatorial reasoning that Hilbert required for execution of his consistency program could all be formally developed in a single consistent system S, then the program could not be carried out for S or any stronger (consistent) system. The incompleteness results were published as ['Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I']; the stunning conclusions and the novel features of his argument quickly drew wide attention and brought Gödel recognition as a leading thinker in the field … Gödel's incompleteness work became his Habilitationsschrift (a kind of higher dissertation) at the University of Vienna in 1932. In his report on it, Hahn lauded Gödel's work as epochal, constituting an achievement of the first order" (Feferman *et al*, pp. 6-7). On 23 October 1930, Hahn presented an abstract of Gödel's paper to the Vienna Academy of Sciences; the full paper was received for publication by the *Monatshefte*, which Hahn edited, on 17 November 1930 and published early in 1931.

Gödel succinctly summarizes his paper in the first paragraph (translation from Gödel/Meltzer): "The development of mathematics in the direction of greater exactness has – as is well-known – led to large tracts of it being formalized, so that proofs can be carried out by following a few mechanical rules. The most comprehensive formal systems yet set up are, on the one hand, the system of *Principia Mathematica* and, on the other hand, the axiom system for set theory of Zermelo-Fraenkel (later extended by J. v. Neumann). These two systems are so extensive that all methods of proof used in mathematics today have been formalized in them, i.e., reduced to a few axioms and rules of inference. It may therefore be surmised that these axioms and ruled of inference are also sufficient to decide *all* mathematical questions which can in any way at all be expressed formally in the systems concerned. It is shown below that this is not the case, and that in both the systems mentioned there are in fact relatively simple problems in the theory of ordinary whole numbers which cannot be decided from the axioms. This situation is not due in some way to the special nature of the systems set up, but holds for a very extensive class of formal systems, including, in particular, all those arising from the addition of a finite number of axioms to the two systems mentioned, provided that thereby no false propositions … become provable."

"One of the first to recognise the potential significance of Gödel's incompleteness results and to encourage their full development was John von Neumann. Only three years older than Gödel, the Hungarian-born von Neumann was already well known in mathematical circles for his brilliant and extremely diverse work in set theory, proof theory, analysis and mathematical physics" (Feferman *et al*, p. 6). Von Neumann said: "Kurt Gödel's achievement in modern logic is singular and monumental. Indeed it is more than a monument, it is a landmark which will remain visible far in space and time. The subject of logic has certainly completely changed its nature and possibilities with Gödel's achievement" (Halmos, p. 383).

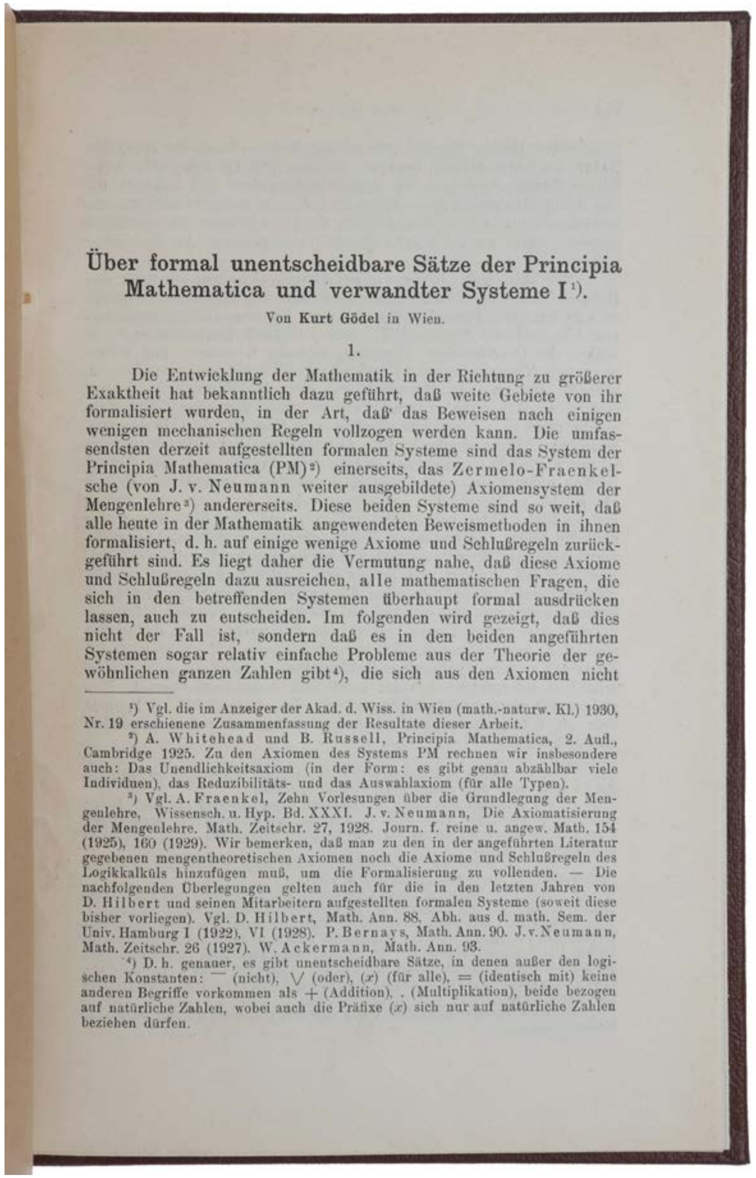
"The immediate effect of Gödel's theorem was that the assumptions of Hilbert's program were challenged. Hilbert assumed quite explicitly that arithmetic was complete in the sense that it would settle all questions that could be formulated in its language—it was an open problem he was confident could be given a positive solution … up to 1930 it was widely assumed that arithmetic, analysis, and indeed set theory could be completely axiomatized, and that once the right axiomatizations were found, every sentence of the theory under consideration could be either proved or disproved in the object-language theory itself. Gödel's theorem showed that this was not so …

"Gödel's results had a profound influence on the further development of the foundations of mathematics. One was that it pointed the way to a reconceptualization of the view of axiomatic foundations. Whereas a prevalent assumption prior to Gödel—and not only in the Hilbert school—was that incompleteness was at best an aberrant phenomenon, the incompleteness theorem showed that it was, in fact, the norm. It now seemed that many of the open questions of foundations, such as the continuum problem, might be further examples of incompleteness. Indeed, he succeeded not long after in showing that the axiom of choice and the continuum hypothesis are not refutable in Zermelo–Fraenkel set theory: [Paul] Cohen later showed that they were also not provable. The incompleteness theorem also played an important role in the negative solution to the decision problem for first-order logic by [Alonzo] Church. The incompleteness phenomenon not only applies to provability, but … also to the notion of computability and its limits.

"Perhaps more than any other recent result of mathematics, Gödel's theorems have ignited the imagination of non-mathematicians. They inspired Douglas Hofstadter's best- seller *Gödel, Escher, Bach* (1979), which compares phenomena of self-reference in mathematics, visual art, and music. They also figure prominently in the work of popular writers such as Rudy Rucker. Although they have sometimes been misused, as when self-described postmodern writers claim that the incompleteness theorems show that there are truths that can never be known, the theorems have also had an important influence on serious philosophy. John Lucas, in his paper 'Minds, machines, and Gödel' (1961) and more recently Roger Penrose in *Shadows of the mind* (1994) have given arguments against mechanism (the view that the mind is, or can be faithfully modeled by a digital computer) based on Gödel's results. It has also been of great importance in the philosophy of mathematics: for instance, Gödel himself saw them as an argument for Platonism" (Zach, pp. 923-5).

After the publication of the incompleteness theorem, Gödel became an internationally known intellectual figure. He travelled to the United States several times and lectured extensively at Princeton University in New Jersey, where he met Albert Einstein. This was the beginning of a close friendship that would last until Einstein's death in 1955. When war broke out in 1939, he fled Europe taking his wife to Princeton where, with Einstein's help, he took up a position at the newly formed Institute for Advanced Studies. He spent the remainder of his life working and teaching there, retiring in 1976.

Feferman *et al* (eds.), *Kurt Gödel*: *Collected Works*: Volume I, 1986. Gödel, *On formally undecidable propositions of Principia Mathematica and related Systems*, Meltzer (tr.), 1962. Halmos, 'The Legend of John von Neumann,' *American Mathematical Monthly* 80 (1973), pp. 382-394. Nagel & Newman, *Gödel 's Proof*, 1958. Shapiro, 'Metamathematics and computability,' in *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences*, I. Grattan-Guinness (ed.), 1994. Zach, 'Kurt Gödel, Paper on the Incompleteness Theorems (1931),' pp. 917-925 in *Landmark Writings in Western Mathematics 1640-1940*, I. Grattan-Guinness (ed.), 2005.



Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I[1]).

Von **Kurt Gödel** in Wien.

1.

Die Entwicklung der Mathematik in der Richtung zu größerer Exaktheit hat bekanntlich dazu geführt, daß weite Gebiete von ihr formalisiert wurden, in der Art, daß das Beweisen nach einigen wenigen mechanischen Regeln vollzogen werden kann. Die umfassendsten derzeit aufgestellten formalen Systeme sind das System der Principia Mathematica (PM)[2]) einerseits, das Zermelo-Fraenkelsche (von J. v. Neumann weiter ausgebildete) Axiomensystem der Mengenlehre[3]) andererseits. Diese beiden Systeme sind so weit, daß alle heute in der Mathematik angewendeten Beweismethoden in ihnen formalisiert, d. h. auf einige wenige Axiome und Schlußregeln zurückgeführt sind. Es liegt daher die Vermutung nahe, daß diese Axiome und Schlußregeln dazu ausreichen, alle mathematischen Fragen, die sich in den betreffenden Systemen überhaupt formal ausdrücken lassen, auch zu entscheiden. Im folgenden wird gezeigt, daß dies nicht der Fall ist, sondern daß es in den beiden angeführten Systemen sogar relativ einfache Probleme aus der Theorie der gewöhnlichen ganzen Zahlen gibt[4]), die sich aus den Axiomen nicht

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[1]) Vgl. die im Anzeiger der Akad. d. Wiss. in Wien (math.-naturw. Kl.) 1930, Nr. 19 erschienene Zusammenfassung der Resultate dieser Arbeit.

[2]) A. Whitehead und B. Russell, Principia Mathematica, 2. Aufl., Cambridge 1925. Zu den Axiomen des Systems PM rechnen wir insbesondere auch: Das Unendlichkeitsaxiom (in der Form: es gibt genau abzählbar viele Individuen), das Reduzibilitäts- und das Auswahlaxiom (für alle Typen).

[3]) Vgl. A. Fraenkel, Zehn Vorlesungen über die Grundlegung der Mengenlehre, Wissensch. u. Hyp. Bd. XXXI. J. v. Neumann, Die Axiomatisierung der Mengenlehre. Math. Zeitschr. 27, 1928. Journ. f. reine u. angew. Math. 154 (1925), 160 (1929). Wir bemerken, daß man zu den in der angeführten Literatur gegebenen mengentheoretischen Axiomen noch die Axiome und Schlußregeln des Logikkalküls hinzufügen muß, um die Formalisierung zu vollenden. — Die nachfolgenden Überlegungen gelten auch für die in den letzten Jahren von D. Hilbert und seinen Mitarbeitern aufgestellten formalen Systeme (soweit diese bisher vorliegen). Vgl. D. Hilbert, Math. Ann. 88. Abh. aus d. math. Sem. der Univ. Hamburg I (1922), VI (1928). P. Bernays, Math. Ann. 90. J. v. Neumann, Math. Zeitschr. 26 (1927). W. Ackermann, Math. Ann. 93.

[4]) D. h. genauer, es gibt unentscheidbare Sätze, in denen außer den logischen Konstanten: $\overline{}$ (nicht), $\vee$ (oder), (x) (für alle), $=$ (identisch mit) keine anderen Begriffe vorkommen als $+$ (Addition), . (Multiplikation), beide bezogen auf natürliche Zahlen, wobei auch die Präfixe (x) sich nur auf natürliche Zahlen beziehen dürfen.

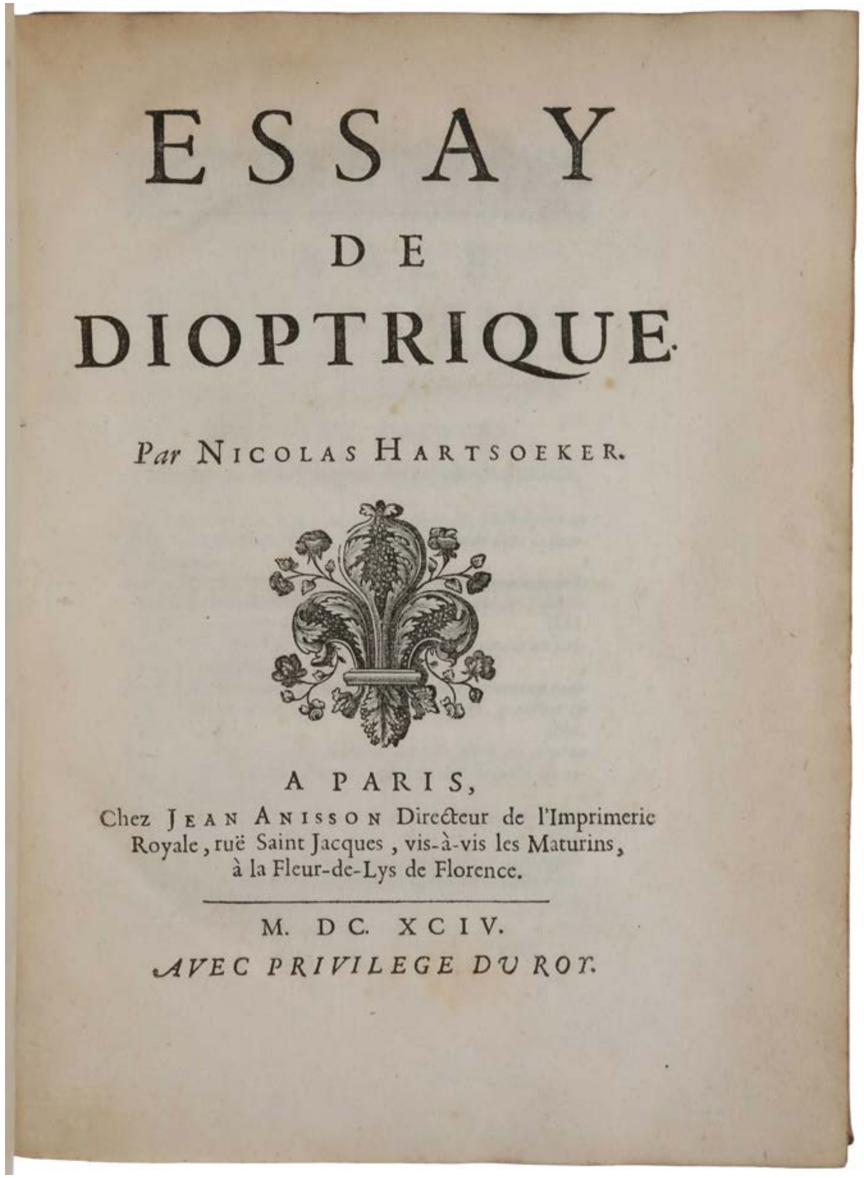
ANATOMY OF THE EYE, PHYSICS OF VISION, THEORY OF COLOR, AND OPTICAL LENSES

HARTSOEKER, Nicolas. *Essay de dioptrique.* Paris: J. Anisson, 1694.

$9,500

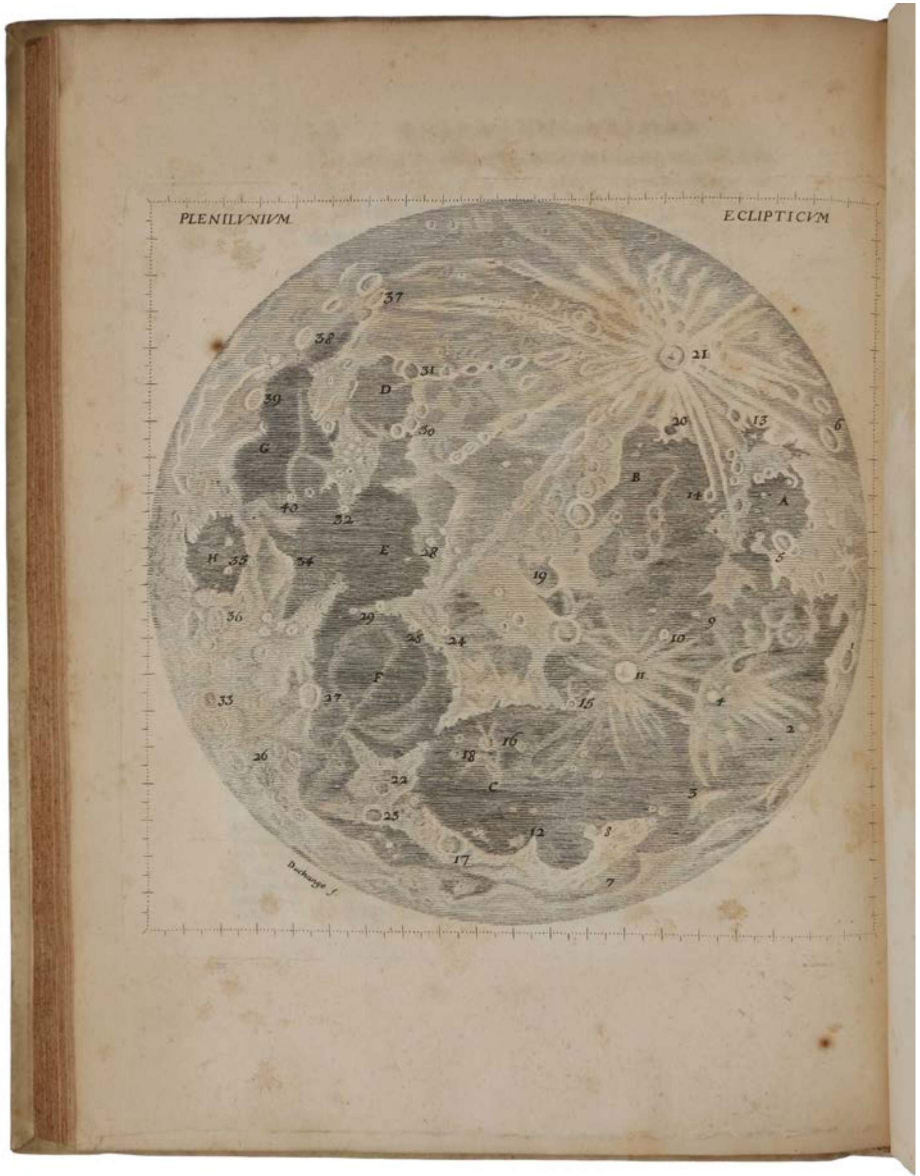
4to (254 x 192 mm), pp. [xxiv], 178, [2], 179-233, [1], with one folding engraved plate of the Moon and numerous woodcut diagrams in the text. Contemporary vellum, a very fine copy.

First edition, rare, of Hartsoeker's first and most important work, in which he reviewed the principles of optics as far as they were known by the end of the 17th century. In addition to the physics of light and the physiology of vision, the book also treats in great detail techniques for the production of lenses for microscopes and telescopes. "Hartsoeker was always interested in optical instruments. He claimed to have developed a method of making small glass globules for microscopes, though his priority in this is doubted. He definitely made lenses of different focal lengths, some of which survive; one lens is said to have had a focal length of 600 feet. He made a number of instruments, not just optical instruments, for the Paris observatory. He constructed a burning glass of great size" (Galileo Project). The *Essay* also documents many observations Hartsoeker made with these instruments. Like Leeuwenhoek, Malpighi and others, Hartsoeker was a preformationist at a time when explanations of animal reproduction were a confused blend of Aristotelian theory, religious orthodoxy, and pure speculation.



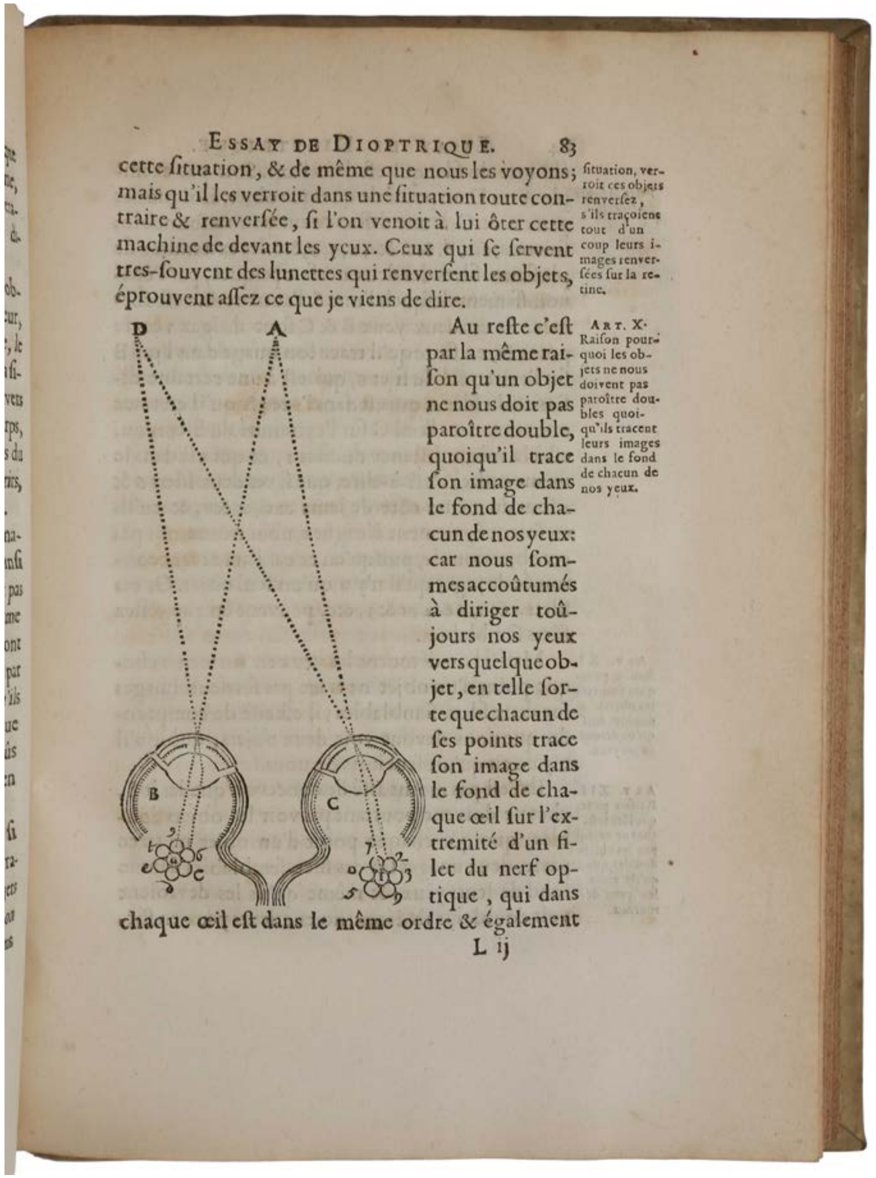
Towards the end of the book, on p. 230, he presented a picture of a little preformed human figure in the head of a spermatozoon. This picture has since become famous and more than a little notorious as an example of observations biased by theoretical prejudice. Hartsoeker's reputation has been forever linked to this picture and if his name appears today at all, he is usually held up as an example of a scientist who saw what he wanted to see. That was not, in fact, the case. In the text accompanying this famous picture he says: 'si l'on pouvoit voir le petit animal au travers de la peau qui le cache, nous le verrions peut-etre comme cette figure le represente, sinon que la tete seroit peut-etre plus grande a proportion du reste du corps, qu'on ne l'a deffinee ici,' i.e., if one's instruments were good enough, here is a suggestion of what one *might* see. ABPC/RBH record only two copies in the past 35 years (Burgersdijk & Niermans, 2003, €3240 (modern binding); Christie's 1999, $3680).

Born in Gouda, "Hartsoeker (1656-1725) was the son of Christiaan Hartsoeker, an evangelical minister, and Anna van der Mey. Although his father wished him to study theology, Hartsoeker preferred science; he secretly learned mathematics and lens grinding. Most sources suggest that he may have studied anatomy and philosophy at the University of Leiden in 1674; a letter from Constantijn Huygens to his brother Christiaan, however, refers to him as having had no higher education, so it is possible that he was largely self-educated in his chosen fields. It is known that by 1672 he had visited Leeuwenhoek and that in 1678 he accompanied Christiaan Huygens to Paris, where he met some of the French scientists and worked for a time at the Paris observatory. In his correspondence with Christiaan Huygens from about this period, Hartsoeker claimed to have invented the technique of making small globules of glass for use as lenses for microscopes, but it is more probable that priority in this belongs to Johann Hudde" (DSB).



"What distinguished Hartsoeker from other opticians was how candid he was about his lens craft and other technical inventions. Unlike Van Leeuwenhoek and the Campani brothers who warily shrouded their lens making methods, Hartsoeker did not keep trade secrets. In fact, his treatise on dioptrics explained in great detail what kind of glass to use, how best to grind lenses, and how to configure them most effectively. Even before his *Essay de dioptrique* came out in 1694, he shared with Huygens his idea of the simple microscope. He then collaborated with Huygens and Rømer on the design that was eventually published in the *Journal des Sçavans* in 1678. Only when Huygens appropriated Hartsoeker's design of the simple microscope and passed it off as his own in the *Journal des Sçavans*, did Hartsoeker react with indignation. He wanted authorial credit for his invention. But when he began manufacturing telescope lenses, he continued to share his techniques and lenses with Huygens and others at the Academy … To some extent, it appears he deliberately capitalized on transparency when he first asked Huygens to be his benefactor. After the debacle with Huygens in 1678, he conducted himself more prudently. He learnt to protect his ideas and inventions in print" (Abou-Nemeh, pp. 9-10).

"In this treatise [i.e., the *Essay*], Hartsoeker not only explained how to work lenses but also made some natural philosophical claims on generation. For instance, he argued for pre-formation based on his observations with a microscope. The theory stipulated that parts of an organism, for example the fly, were already present in miniature forms in the maggot and merely grew as the organism further developed. The way in which he did this shows nicely how his philosophical hypotheses complemented his observations. For many years he had been observing the sperm of quadrupeds and human beings, which he thought resembled tadpoles. Ever since he had made these observations, he realized that birds, flies and butterflies, were born out of these "worms which enclose them inside and hide them from our view." No doubt the idea of pre-formation influenced Hartsoeker's observations.



ESSAY DE DIOPTRIQUE. 83

cette ſituation, & de même que nous les voyons; mais qu'il les verroit dans une ſituation toute contraire & renverſée, ſi l'on venoit à lui ôter cette machine de devant les yeux. Ceux qui ſe ſervent tres-ſouvent des lunettes qui renverſent les objets, éprouvent aſſez ce que je viens de dire.

ſituation, verroit ces objets renverſez, s'ils traçoient tout d'un coup leurs images renverſées ſur la retine.

Au reſte c'eſt par la même raiſon qu'un objet ne nous doit pas paroître double, quoiqu'il trace ſon image dans le fond de chacun de nos yeux: car nous ſommes accoûtumés à diriger toûjours nos yeux vers quelque objet, en telle ſorte que chacun de ſes points trace ſon image dans le fond de chaque œil ſur l'extremité d'un filet du nerf optique, qui dans chaque œil eſt dans le même ordre & également

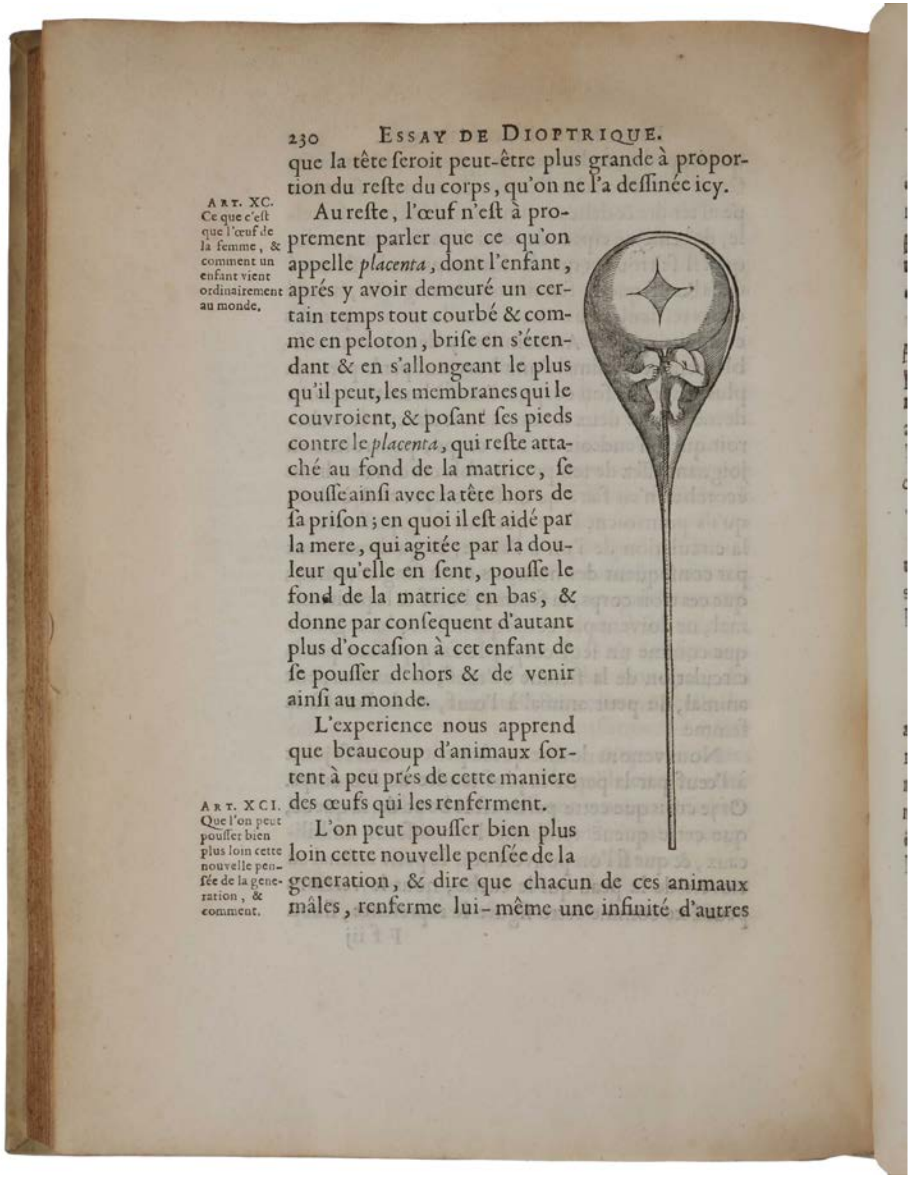
ART. X. Raiſon pourquoi les objets ne nous doivent pas paroître doubles quoiqu'ils tracent leurs images dans le fond de chacun de nos yeux.

L ij

For instance, be believed “that each worm that one sees in the semen of birds encloses actually a male or female [organism] of the same species” as the parent. He presupposed “the same thing of the … [little animals] that there are found in the semen of men and quadrupeds.” Namely, “each animalcule contains and for the moment conceals … either a male or female animal from the same species.”

“In his *Essay de dioptrique*, Hartsoeker speculated that for man, the spermatic animalcule would look a “little animal”, popularly coined as a homunculus. That is, he only *supposed* it would resemble a tiny replica of a human being, with a head that is larger than his body, crouching in a foetal position inside the delicate membrane of the spermatozoon. Yet he never explicitly stated that he *had seen* a homunculus precisely like the one he described: it was a presumption rather than an observation …

“By 1692, Huygens was curious about Hartsoeker’s philosophical ideas, all the while anticipating controversy. Huygens and Marquis de L’Hôpital remained in the know about Hartsoeker’s optical and philosophical ideas throughout the 1690s … During this time, Hartsoeker persistently tried to convince Huygens of his method for grinding telescopic lenses with long focal lengths. Hartsoeker’s lens making method and accompanying system of the world eventually resulted in the *Essay de dioptrique*, in which he advertised for the first time all the aforementioned observations and suppositions. Christiaan Huygens, his brother Constantijn, L’Hôpital and other savants eagerly awaited the publication of the treatise. They expected it to contain details of Hartsoeker’s microscopes and, more importantly, his lens making method. L’Hôpital received the treatise with mixed feelings: on the one hand he was thrilled about its possible novelties; but on the other hand, he reacted indignantly to the ideas it contained. Although the book dealt with optics and lens manufacture, it also offered Hartsoeker’s microscopical

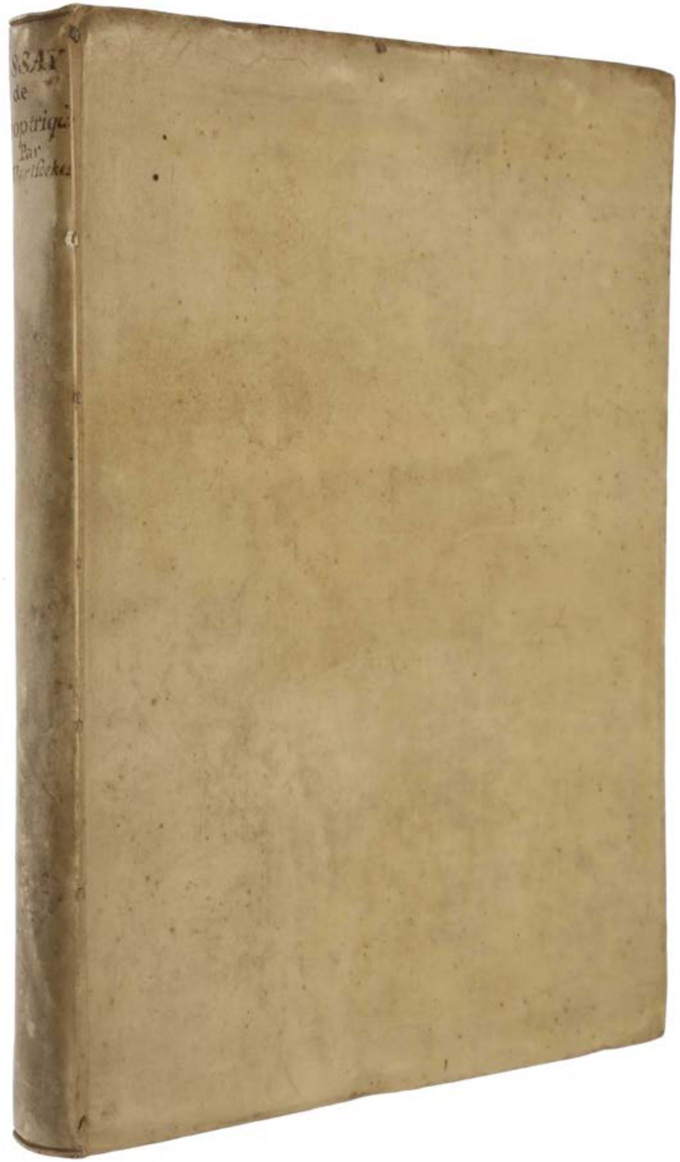


observations and ideas on generation. L'Hôpital and Huygens had looked forward to Hartsoeker's lens-making secrets with bated breath, but they did not expect him to publish his ideas on natural philosophy in the book as well … In the eyes of Huygens and L'Hôpital, [Hartsoeker] was a skilled, educated craftsman who made a living by grinding lenses for the Academy and not yet a full-fledged philosopher who could make knowledge claims about nature. What set the two men apart from Hartsoeker and his ilk even further was the importance they granted mathematics" (*ibid.*, pp. 16-19).

"Of Hartsoeker's lenses, two known to be by his hand are preserved, one signed "Nicolaas Hartsoeker, pro Academia Ludg. Batav: Parisiorum 1688" in the museum of natural history in Leiden, and the other in the museum of the University of Utrecht. It is known, however, that he made three telescopes for the Utrecht observatory at the time of Pieter van Musschenbroek's arrival in 1723.

"Hartsoeker was elected a foreign member of the Académie des Sciences in 1699 and was later also a member of the Berlin Royal Society. His work may be said to have been more honored in France than in his native Holland" (DSB).

Bierens de Haan, 1925; British Optical Association Library I, 91; Hirsch III, 77; Poggendorff I, 1026; Wellcome II, 217. Abou-Nemeh, 'The Natural Philosopher and the Microscope: Nicolas Hartsoeker Unravels Nature's "Admirable Œconomy"', *History of Science* 51 (2015), pp. 1-32.

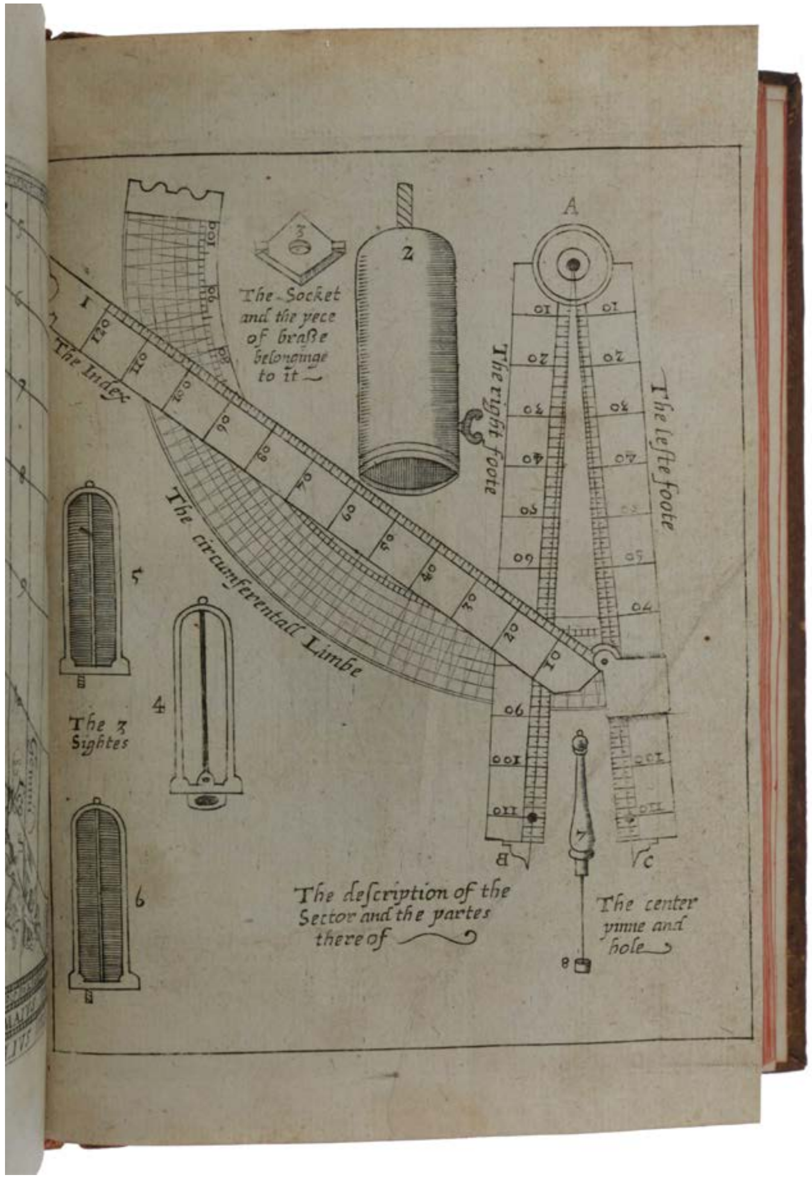


THE FIRST PUBLISHED WORK ON THE GEOMETRICAL COMPASS

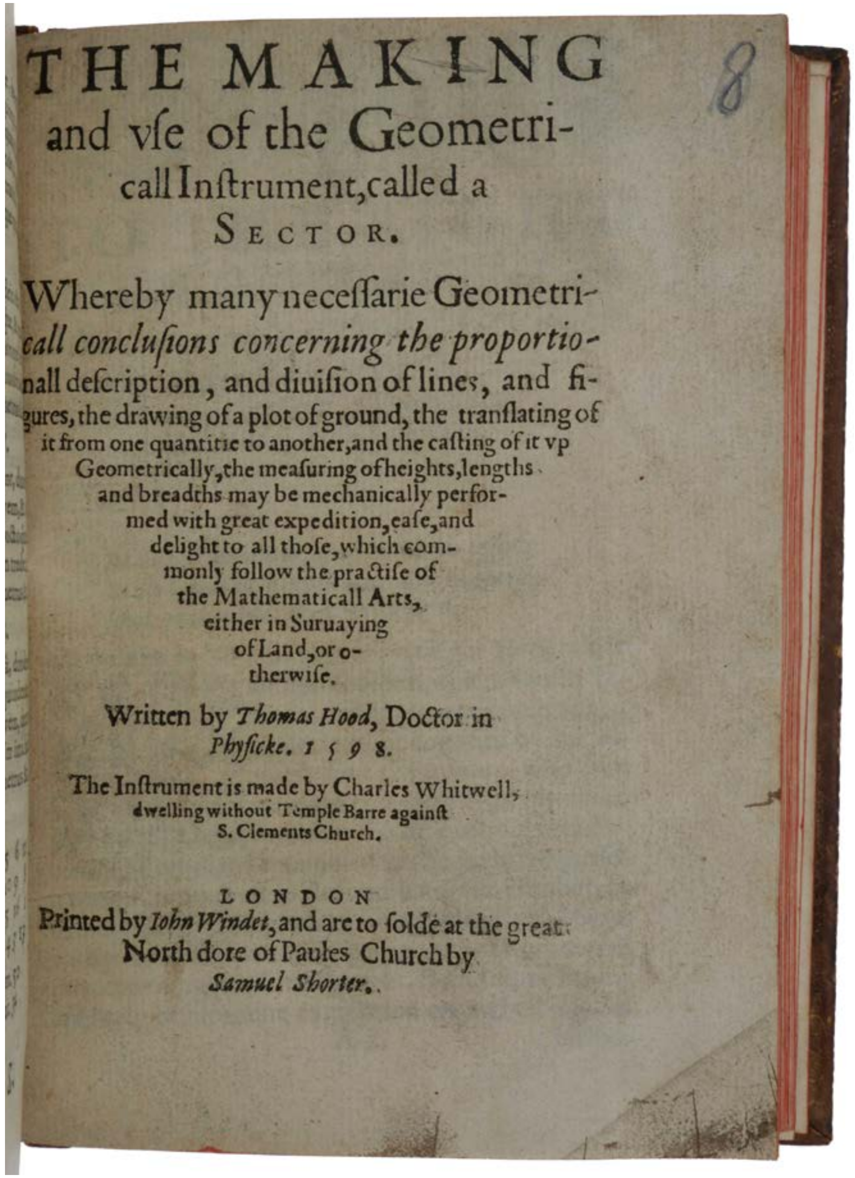
HOOD, Thomas; MOHR, Georg; BEDWELL, William; STURM, Johannes. *A precious sammelband containing eight extremely rare works, all in first edition, notably:* HOOD, *The making and use of the geometricall instrument, called a sector* ... London: J. Windet and sold by S. Shorter, [1598] & MOHR, *Euclides Danicus* ... Amsterdam: Jacob van Velsen for the author, 1672.

$65,000

Eight works in one volume, small 4to (185 x 130mm). Mohr, Euclides: pp. [iv], 36, with three folding plates, signed by the author in authentication. Mohr, Compendium: pp. [4], 5-24 with one folding plate. Mohr (?), Gegenübung: pp. [4], 5-24 with two folding plates. Bierens de Haan 15, p. 263 (giving Mohr as the author of Gegenübung and incorrectly giving the date of the Compendium as 1672). Bedwell, Tottenham: ff. [22] (lacking final blank), partly in verse, second part with separate dated title page on C1r with printer's fleur de lys device with motto 'In Domino confido'; register is continuous. STC 19925; Upcott II, p. 587. Bedwell, Mesolabium: ff. [13], with two engraved plates. Harris 34; STC 1796; Taylor p. 346, 147. Bedwell, De numeris: pp. [vi], 82, with folding table titled 'Trigonum atchitectonicum' at end (lacking initial blank leaf). STC 21825. Sturm: pp. [xxiv], 72. Woodcut printer's device on the title-page, with motto 'Rebus in humanis fortuna volubilis errat' in cartouche, six-line decorated woodcut initials. Poggendorff II, 1018; Sotheran 4631. Hood: ff. [5], 50, [1, errata], with two full-page plates. Black Letter, woodcut diagrams in text. STC 13695. Eighteenth-century mottled calf, spine gilt in compartments, red morocco lettering-piece, red edges (occasional cropping, upper joint weak). Preserved in a cloth folding box with black morocco lettering-piece.



One of the most remarkable sammelbands from the Macclesfield library, containing the extremely rare first edition of the first published work on the 'sector', also called the 'geometrical compass' by Galileo who developed it independently in the late 1590s as an instrument for military engineering (although he did not publish an account of it until 1606). "Hood's sector was the first mechanical calculating device of general practical use to be published since the abacus of remote antiquity" (Stillman Drake, p. 17). "Although credit for the sector is often given to Galileo, it is clear that the instrument was well known and used in England before Galileo published his work on it" (Tomash & Williams, p. 1416). "The sector was one of the most familiar of mathematical instruments between the 17th and 19th centuries. It was however devised just before 1600 and was first published in 1598 by the English mathematical practitioner Thomas Hood. An independent version developed by Galileo Galilei in the 1590s was published early in the 17th century [1606], and many other designs subsequently followed" (mhc.ox.ac.uk). OCLC lists no copies in North America, but we have located one (Folger), though it lacks the plates (present in this copy); ABPC/RBH list three copies (including Horblit and Kenney), all lacking the plates. Also included in this volume are three geometrical works by the Danish mathematician Georg Mohr that are so rare that they were thought to be lost until a copy of one of them, *Euclides Danicus*, was discovered in 1928. This work proves the 'Mohr-Mascheroni' theorem, according to which all geometrical constructions that can be carried out with ruler and compasses can, in fact, be carried out using compasses alone – it was proved independently by Lorenzo Mascheroni (1750-1800) 125 years after Mohr in his *Geometria del Compasso*. OCLC lists only one copy of *Euclides Danicus* in North America (Harry Ransom Center, University of Texas); only one other copy has appeared at auction. The present volume includes two further works attributed to Mohr, as rare as *Euclides Danicus*, also on geometrical constructions (no copies on OCLC). They are accompanied by three rare works by William Bedwell (1561-1632), two on architectural measuring instruments, the 'carpenter's rule' and the



THE MAKING
and vſe of the Geometri-
call Inſtrument, called a
SECTOR.

8

Whereby many neceſſarie Geometri-
call concluſions concerning the proportio-
nall deſcription, and diuiſion of lines, and fi-
gures, the drawing of a plot of ground, the tranſlating of
it from one quantitie to another, and the caſting of it vp
Geometrically, the meaſuring of heights, lengths
and breadths may be mechanically perfor-
med with great expedition, eaſe, and
delight to all thoſe, which com-
monly follow the practiſe of
the Mathematicall Arts,
either in Suruaying
of Land, or o-
therwiſe.

Written by *Thomas Hood*, Doctor in
Phyſicke. 1598.

The Inſtrument is made by Charles Whitwell,
dwelling without Temple Barre againſt
S. Clements Church.

LONDON
Printed by *Iohn Windet*, and are to ſolde at the great
North dore of Paules Church by
Samuel Shorter.

'trigon,' the third being the earliest published work on Tottenham, where Bedwell resided (now part of London but then a village to the north of the City). The final work in this volume, by Johannes Sturm (1507-89), is a contribution to the controversy which raged in the late sixteenth and early seventeenth century between Clavius, van Roomen, Viète, and Scaliger over the squaring of the circle.

HOOD, Thomas. The making and use of the geometricall instrument, called a sector. Whereby many necessarie geometricall conclusions concerning the proportionall description, and division of lines, and figures, the drawing of a plot of ground, the translating of it from one quantitie to another, and the casting of it up geometrically, the measuring of heights, lengths and breadths may be mechanically performed with great expedition, ease, and delight to all those, which commonly follow the practise of the mathematicall arts. London: Printed by Iohn Windet, and are to solde at the great North dore of Paules Church by Samuel Shorter, [1598].

First edition of the first published work on the sector. "The sector, also known as the proportional, geometric, or military compass, was an analog calculating instrument used widely from the late sixteenth century until modern times … Requirements for extensive arithmetic calculation grew rapidly during the Renaissance and in the early years of the scientific and industrial revolutions. It soon became apparent to practitioners that calculation by hand, particularly the multiplication and division of large numbers, was both laborious and error-prone. It was small wonder that talented mathematicians and scientists sought to develop methods and mechanisms that would lessen the burden of computation while increasing accuracy" (Tomash & Williams, p. 1456).

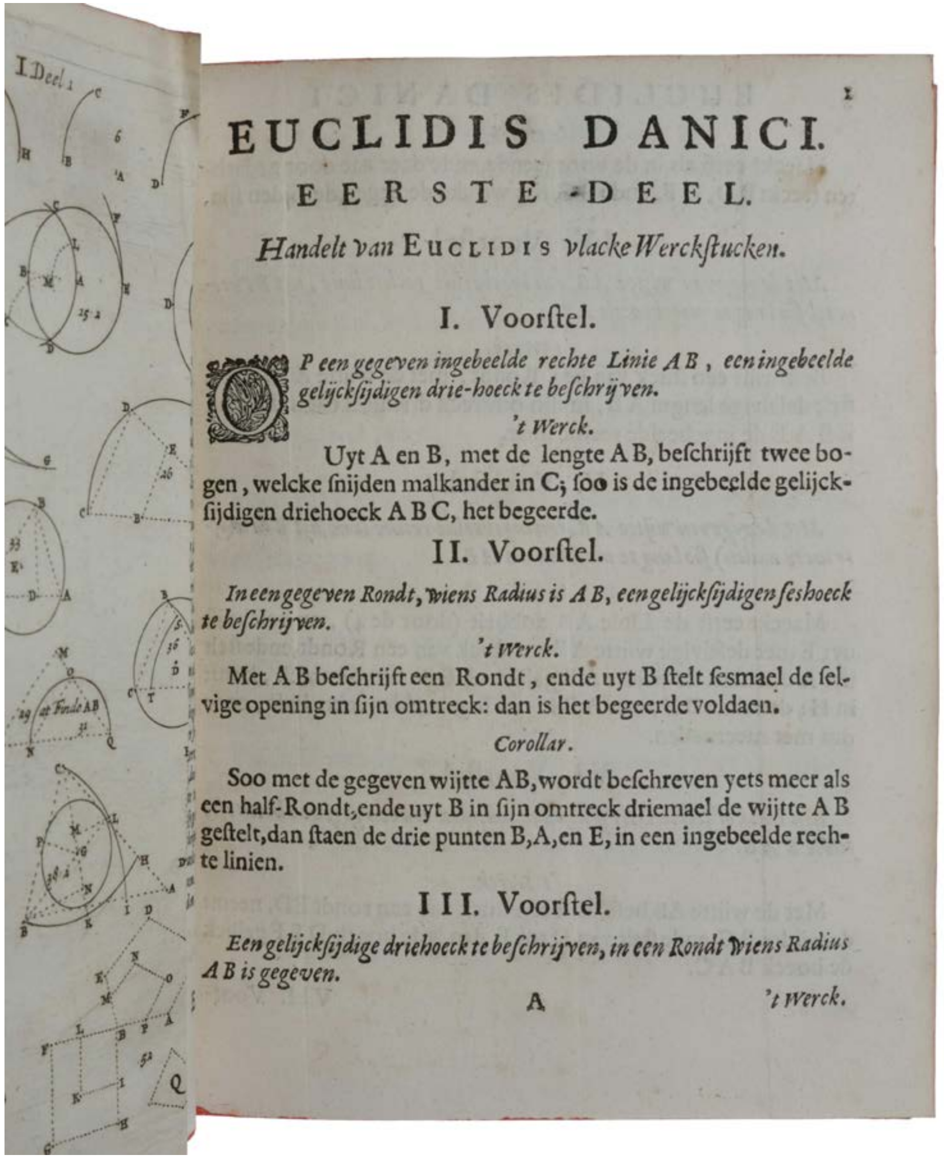
Hood's sector consisted of "a pair of flat rules hinged stiffly at one end and bearing identical scales engraved on the two arms, different on the two faces … It had


EUCLIDES DANICUS,
Beſtaende in twee Deelen:
Het eerſte Deel : Handeldt van de Meetkonſtige Werckſtucken, begrepen in de ſes eerſte Boecken *Euclidis*:
Het tweede Deel : Geeft aenleyding om verſcheyde Werckſtucken te maecken, als van Snijding, Raecking, Deeling, Perſpective en Sonnewijſers:
Alleenig met een *Paſſer* te wercken (ſonder Rye ofte Liniael te gebruycken) door Snijding van Ronden.
Voorgeſtelt
Door
GEORG MOHR.
t'AMSTERDAM,
Gedruckt by *Iacob van Velſen*, voor de *Autheur*, welcke geen Exemplaren voor de zijne erkent, als die hy ſelver met zijn handt onderteeckent heeft; 1672.

three scales and was fitted with removable sights and a graduated quadrant, plumb line, and accessory graduated arm … Hood's principal scale was one of equal linear divisions from pivot to end of either arm. On the other face he provided a scale which gave the side of various regular polygons inscribed in a circle of diameter equal to the separation of the ends, and another which gave the side of a square having an area which was an integral multiple of the area of a unit square. The enormous value of Hood's sector for speedy mechanical approximation to a wide variety of commonest practical mathematical problems is obvious, and he explained these at great length in his book" (Drake, pp. 17-18).

"We know little of Thomas Hood (1556-1620) other than that he was the first mathematical lecturer for the City of London and gave public lectures there on topics such as the sector and other instruments. We do no know where or how Hood might have first learned of the instrument, but we presume that he learned of it through contacts with the military. In 1598, he published *The making and use of the geometricall instrument, called a sector*. With this book title he seems to have coined the English word sector (at least as it applies to a mathematical instrument). The book is well organized and contains useful diagrams, examples and exercises. It is obviously not a work created in haste, and this fact leads one to the conclusion that Hood must have been familiar with the sector for some time prior to 1598. Further, the book notifies the reader that Hood's sectors were available for sale around 1594-1611 by the instrument maker Charles Whitwell, who engraved the illustrations for Hood's book. Indeed, a sector signed by Whitwell bears the date 1597. Another sector from the same year, made by Robert Becket, has survived. Both instruments closely resemble the illustration in Hood's publication" (Tomash & Williams, p. 1459).

"The work begins by describing the sector and its scales together with a short



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EUCLIDIS DANICI.

EERSTE DEEL.

Handelt van EUCLIDIS *vlacke Werckſtucken.*

I. Voorſtel.

Op een gegeven ingebeelde rechte Linie AB, een ingebeelde gelijckſijdigen drie-hoeck te beſchrijven.

't Werck.

Uyt A en B, met de lengte AB, beſchrijft twee bogen, welcke ſnijden malkander in C; ſoo is de ingebeelde gelijckſijdigen driehoeck ABC, het begeerde.

II. Voorſtel.

In een gegeven Rondt, wiens Radius is AB, een gelijckſijdigen ſeshoeck te beſchrijven.

't Werck.

Met AB beſchrijft een Rondt, ende uyt B ſtelt ſesmael de ſelvige opening in ſijn omtreck: dan is het begeerde voldaen.

Corollar.

Soo met de gegeven wijtte AB, wordt beſchreven yets meer als een half-Rondt, ende uyt B in ſijn omtreck driemael de wijtte AB geſtelt, dan ſtaen de drie punten B, A, en E, in een ingebeelde rechte linien.

III. Voorſtel.

Een gelijckſijdige driehoeck te beſchrijven, in een Rondt wiens Radius AB is gegeven.

A *'t Werck.*

description of their use. These are followed by several chapters devoted to explaining individual operations. These range from performing simple multiplication (usually couched in terms of finding lines in certain proportions) to expanding and contracting figures, if given the radius of a circle to find the length of a chord of an angle, inscribing various figures inside squares and circles and similar basic functions performed with a sector" (*ibid*., p. 1416).

"In the same year (1598) there appeared at Venice a book on mathematical instruments written by G. P. Gallucci in which was illustrated a different kind of sector, having in common with Hood's only the scales for construction of regular polygons … though Gallucci did not name the inventor, other evidence points to Guidobaldo del Monte, Galileo's friend and patron. It had but two scales, one on either face, the second being designed to permit the division of a line into equal segments, just as the first was used to divide a circle into equal arcs. Guidobaldo's sector was in no significant sense a calculating instrument; it simply gave direct mechanical solutions to two very common problems in drafting, designing, and instrument construction" (Drake, p. 18).

MOHR, Georg. Euclides Danicus, bestaende in twee deelen. Het eerste deel: handelt van de meetkonstige werckstucken, begrepen in de ses eerste boecken Euclidis: het tweede deel: geest aenleyding om verscheyde werckstucken te maecken als van Snijding, Raecking, Deeling, Perspective en Sonnewijsers … Amsterdam: Jacob van Velsen for the author, 1672.

Ibid. Compendium Euclidis curiosi: dat is, meetkonstigh passer-werck, hoe me meet een gegeven opening van een passer en een liniael, de werck-stucken van Euclides, ontbinden kan te samen gestelt door een Lief-hebber der selver konst. Amsterdam: J. Jansson van Waesberge, 1673.

S., J. D. [MOHR, Georg?]. Gegen-übung auf ein mathematisch Tractätlein, Compendium Euclidis Curiosi genant, worin nebst kurtzem Anweis um verscheidene Euclidische Aufgaben mit einer gegebenen Oeffnung des Zirkels noch auf andere Ahrt zu machen; zu mehrerem Nutzen wird vorgestellet eine kurze, iedoch grundrichtige Manier um den cörperlichen Inhalt einer Festung mit geringer Mühe aus zu rechnen Amsterdam: J. Jansson van Waesberge, 1673.

First editions of these three extremely rare works on Euclidean geometrical constructions. For the Greeks the straight line and the circle were the most perfect geometrical figures, and it was therefore of great importance to determine which geometrical figures could be constructed using straightedge and compass. Such questions remained of interest in the modern period. Indeed, one of the first discoveries made by Carl Friedrich Gauss was that a regular polygon with 17 sides is constructible using straightedge and compass. Later, in his great work *Disquisitiones Arithmeticae* (1801), Gauss determined exactly which regular polygons can be so constructed. A related question is whether the constructions that can be made by straightedge and compass can be made by straightedge alone, or compass alone. In *Euclides Danicus*, Mohr was the first to establish that all Euclidean constructions can be made using compass alone, while in *Compendium Euclidis Curiosi* he shows for the first time that such constructions can be made using straightedge and a compass with a single opening – this "was posed in the contests of the great Renaissance mathematicians" (DSB); it also contains mathematical problems related to fortification. Although the latter work was published anonymously, it has been established by Arthur E. Hallerburg that Mohr is its author. Hallerburg was not able to trace a copy of *Gegenübung*, and although this work has been ascribed to Mohr by Bierens de Haan and others, Andersen & Meyer have argued that its author, identified as 'J.D.S.' on the title page, is not Mohr: "The largest part of [*Gegenübung*] consists of calculations connected to fortification and is not related to Mohr's theorems on fortification in *Euclides Curiosus*. Only the first five pages deal with constructions performed with a ruler and a fixed compass. In these pages J.D.S. offers alternative solutions to the problems Mohr had numbered IV, V. VI. VIII. XII and XIV." However, Andersen & Meyer were unable to determine the identity of 'J.D.S.'

Georg Mohr (1640-97) was born in Copenhagen, but spent much of his life in Holland (to which he travelled in 1662) before visiting England and France and then returning to Denmark. While in England he met Henry Oldenburg and John Collins. In a letter to Leibnitz (30 September 1675), Oldenburg writes of Mohr, of whom he says that he is 'algebrae et mechanices probe peritus', that he has recently left England for Paris and has left with John Collins "a certain work written in the Flemish tongue, a copy of which I was glad to communicate to you, because, according to Collins, the said Mohr asserted, this work … completes Cardan's rules … and supplies roots of equations of the kind which are represented by surds" (Oldenburg, *Correspondence* XI, letter 2754). This work has not been identified, but if Mohr left Collins with one of his works it is surely possible that he also left him with copies of the geometrical works offered here. As the Macclesfield sale catalogue notes (Part 2, p. 12), "It is probably not unreasonable to suppose that anything published before 1683 [in the Macclesfield Library] belonged to him [Collins]." Does the presence of (Collins' copy (?) of) *Gegenübung* in this sammelband together with Mohr's other two works suggest that *Gegenübung* is also Mohr's?

Euclides Danicus was published simultaneously, by the same publisher, in Danish and Dutch. "The obscurity that befell Mohr and his book can be attributed, in some degree, to the presentation of the material. In the body of the book, Mohr does not state the issue until the very last paragraph, although the lines are referred to as 'imagined' (*gedachte*). In the dedication to Christian V, he does say that he believes he has done something new, and on the title page the issue is explicitly stated. Still, it would be easy for an inattentive reader to misjudge the value of the book" (DSB). It is likely that the languages of composition and a small print run also played a part. J. Hjelmslev published a German translation in 1928, the year of its rediscovery. *Compendium Euclidis Curiosi* seems to have been published only in Dutch, although it was translated into English in 1677.

"Word of the discovery of Mohr's book travelled quickly, and by 1929 there was an enthusiastic report on its contents by the eminent Berkeley historian, Florian Cajori, who also reported on Mohr's contacts with Leibniz, whom Mohr met in 1676 … Any bibliophile has to be inspired by the discovery of the Mohr volume in a stack of 'used books', containing a geometrical theorem that would not become public for another 125 years. Needless to say, copies of Mohr's book are exceedingly scarce. In 2005 a copy of the original (accompanied by the 1928 facsimile) appeared in the catalogue of a book auction house in San Francisco [PBA Galleries]. A Bay Area collector acquired it for the ridiculously low price of roughly $13,000" (Alexanderson).

It appears that all copies of *Euclides Danicus* are signed on the title page by the author with a flourish in authentication. Alexanderson suggests that "the flourish under Mohr's signature follows a long Spanish tradition of certifying an author's signature on documents, possibly a holdover from the Spanish Habsburg's influence in the Low Countries that lasted into the early 18th century".

OCLC lists one copy of *Euclides Danicus* in North America, and no copy of the other two works.

PILKINGTON, Gilbert [BEDWELL, William]. The turnament of Tottenham. Or, the wooing, winning, and wedding, of Ribbe, the reeu's daughter there. Written long since in verse, by Mr. Gilbert Pilkington, at that time as some haue thought parson of the parish. Taken out of an ancient manuscript, and published for the delight of others, by Wilhlm (sic.) Bedwell, now pastour there. [Including as second part:] A briefe description of the towne of Tottenham High Cross in Middlesex: together with an historical narration of such memorable things, as are there to be seene and obserued. Collected, digested, and written by Wilhelm Bedwell. London: J. Norton, 1631.

BEDWELL, William (1561-1632). Mesolabium architectonicum: that is, a most rare, and singular instrument, for the easie, speedy, and most certaine measuring of plaines and solids by the foote: necessary to be knowne of all men whatsoeuer, who would not in this case be notable defrauded: invented long since by Mr Thomas Bedwell Esquire: and now published, and the vse thereof declared, by Wilhelm [sic] Bedwell, his nephew, Vicar of Tottenham. London: J. N[orton]. for William Garet, 1631.

BEDWELL, William [SCHÖNER, Lazarus]. De numeris geometricis. Of the nature and proprieties of geometricall numbers. First written by Lazarus Schonerus, and now Englished, enlarged and illustrated with diuers and sundry tables and obseruations concerning the measuring of plaines and solids: all teaching the fabricke, demonstration and vse of a singular instrument, or rular, long since inuented and perfitted by Thomas Bedwell Esquire. London: R[ichard]. Field, 1614.

First editions of three very rare works by William Bedwell (1563-1632), English mathematician and Arabist. Educated at St. John's College, Cambridge, Bedwell served as Vicar of All Hallows, Tottenham (known at the time as Tottenham High Cross) from 1607 until his death, and was the author of the first local history of the area, *A briefe description of the towne of Tottenham High Cross in Middlesex*. The poem, a burlesque upon the old feudal custom of marrying an heiress to a knight who vanquished all his opponents, was lent by George Wither. Thomas Pilkington was in fact the transcriber, not the author.

The other two works describe instruments devised by Bedwell's uncle, Thomas Bedwell (d. 1595). "Bedwell's 'Carpenter's rule' was intended for … carpenter's, surveyors, shipbuilders, and indeed merchants who had to measure timber but who had no academic knowledge of mensuration. In the text of the *Mesolabium architectonicum*, his reconstruction of the 'little treatise' which he had seen amongst his uncle Thomas' papers and which he had tried to have published as early as 1602, Bedwell claimed that his object was to assist even 'the meanest of understanding' to measure a piece of timber accurately and to avoid being defrauded … The instrument of which Bedwell provided a diagram in his *Mesolabium architectonicum* was a 'flat ruler or oblong parallelogram', about two foot long and two and a half inches broad. On one side it contained 'a scale of equall divisions' of inches divided into halves, quarters, eighths and so on, and of an inch divided into 7, 11, 13, 17, 19, 23, 'and other such equall parts.' On the other side was 'a scale of unequall divisions, serving for the measuring of Board and Tymber consisting of 'two sortes of straight lines, the one Bevelling or Slanting, drawne askue from side to side. The other Parallell that is equidistant one from another running along the Ruler, from the one end toward the other. And therefore cutting those former, and dividing them into unequall portions, whereby not onely their sayd Quadrate or square measure is performed: But also all other whatsoever…' In the rest of the treatise Bedwell gave instructions as to how planes and solids of various shapes could be measured with his instrument

…

"The 'Carpenter's Rule' was also presented in a different shape to the rectangular diagram with parallel and slanting lines. For Bedwell gave the same name to the *Trigonum architectonicum*, first published at his own expense in 1612 and reissued in *De numeris geometricis* in 1614 and at the end of *Mesolabium architectonicum* in 1631 … The principle of the triangular table is the same as that of the rectangular rule, but in the trigon the answer is given in numerals at the intersection of the columns corresponding to the known numbers which run, from 1 to 24, along the base and the side of the triangle …

"Bedwell's presentation of his instrument did acquire an element of novelty when he appended the trigon to *De Numeris geometricis* and entirely transformed the mathematical treatise *De Numeris figuratis* by Lazarus Schöner (c. 1543-1607) into an introduction to the instrument devised by his uncle … Bedwell seems to have believed that that Schöner's treatise, of which he retained little more than the propositions, was ideally suited for teaching 'the nature and proprieties' of 'Geometricall Numbers' which were essential for understanding Thomas Bedwell's instrument. The emphasis in Bedwell's preface is on application and simplification, and what Bedwell did was to take a selection of Schöner's propositions on 'geometrical numbers' (Schöner himself chose to call them 'figurate', the term by which they are still known) and apply them to the measurement of boards, glass, cloth, wainscoting, and paving in feet and inches" (Hamilton, pp. 56-59). Schöner's work originally appeared as an appendix to Pierre de la Ramée's *Arithmetices libri duo* (1586).

OCLC lists four copies of *Tottenham*, two of *Mesolabium* and two of *De Numeris* in North America. ABPC/RBH list only one copy of *Tottenham* since 1980 (Christie's, 6 July, 2000, lot 149, £4000); two copies of *Mesolabium* in the last 80 years (at least one of which was incomplete); and only the Kenney copy of *De numeris* (Sotheby's, 1968).

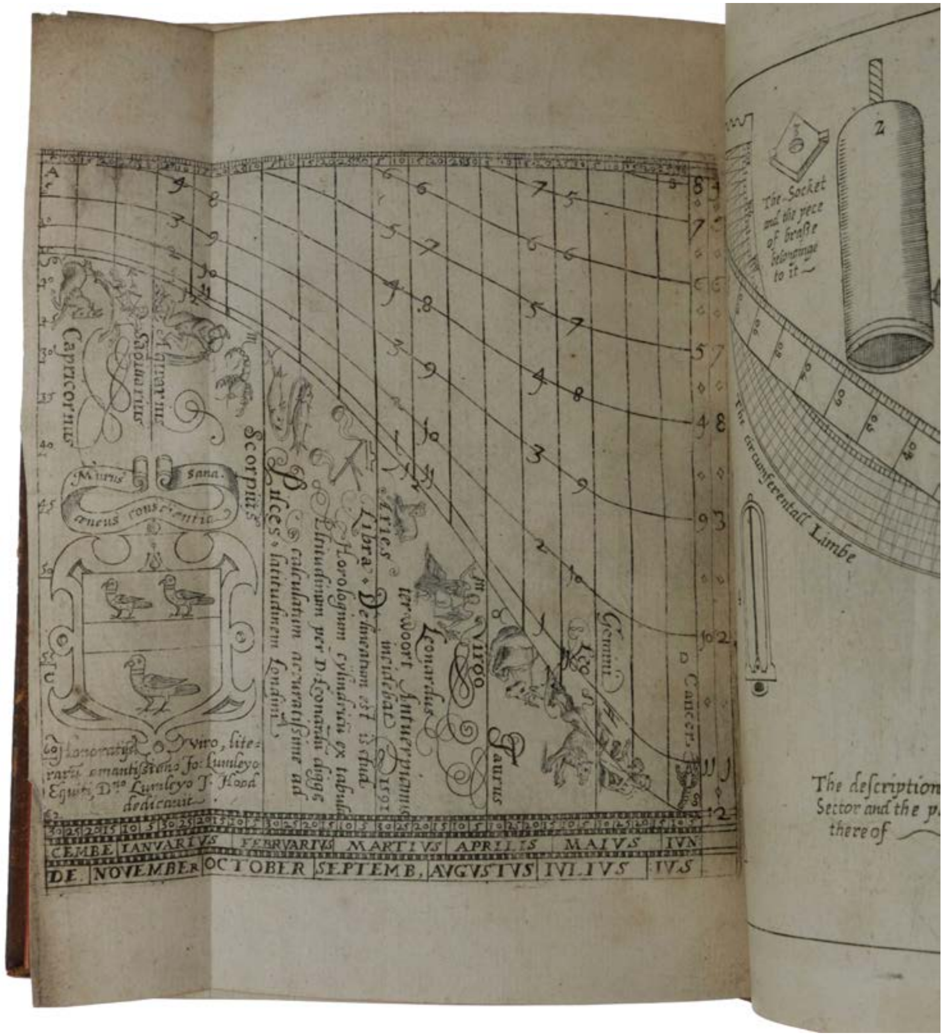
STURM, Johannes (1507-1589). De accurata circuli dimensione et quadratura cum sylvula epigrammatum, aenigmatum, aliorumque Versuum de numeris, ad animum, partim instruendum, partim recreandum, inventis. Leuven: François Simon, 1633.

Sturm (1559-1650) studied at Leuven University, graduating Master of Arts before the age of 20, and on 12 June 1579 he was appointed to a benefice of the chaplaincy of St Margaret in Mechelen. In 1585 he was appointed to teach dialectics and metaphysics in Lily College, Leuven, while pursuing further studies in the Faculty of Medicine. In 1591 he graduated licentiate in medicine and was appointed to the university's academic council. He audited the lectures of Adriaan van Roomen. In 1593 Sturm was appointed to the chair in mathematics vacated by Roomen, graduating doctor of medicine the same year. In 1603 he was appointed regent of Lily College, resigning in 1606 in order to marry Catherine van Thienen. After her death in 1619, Sturm took holy orders. He died in Leuven, and was buried in the church of St Kwinten.

"This is a strange and curious work in which Sturm presents elementary mathematical and geometrical problems and their solution – often in Latin verse. The work contains a section on squaring the circle in which Sturm gives values of π (one to over seventy places of decimals – so that it had to be printed vertically on the page because there would not have been enough room to accommodate it across the page)" (Tomash & Williams, p. 1249).

OCLC lists only five copies worldwide (all in Germany). ABPC/RBH list only one copy.

G. L. Alexanderson, 'About the cover: two theorems on geometric constructions,' *Bulletin of the American Mathematical Society* 51 (2014), 463-7. K. Andersen and H. Meyer, 'Georg Mohr's three books and the Gegenübung auf Compendium Euclidis Curiosi', *Centaurus* 28 (1985), 139-144. Stillman Drake, *Essays on Galileo and the History and Philosophy of Science*, vol. 3, 2000. A. Hamilton, *William Bedwell the Arabist*: *1563-1632*, 1985. E. Tomash & M. R. Williams, *The Erwin Tomash Library on the history of computing*, 2009.



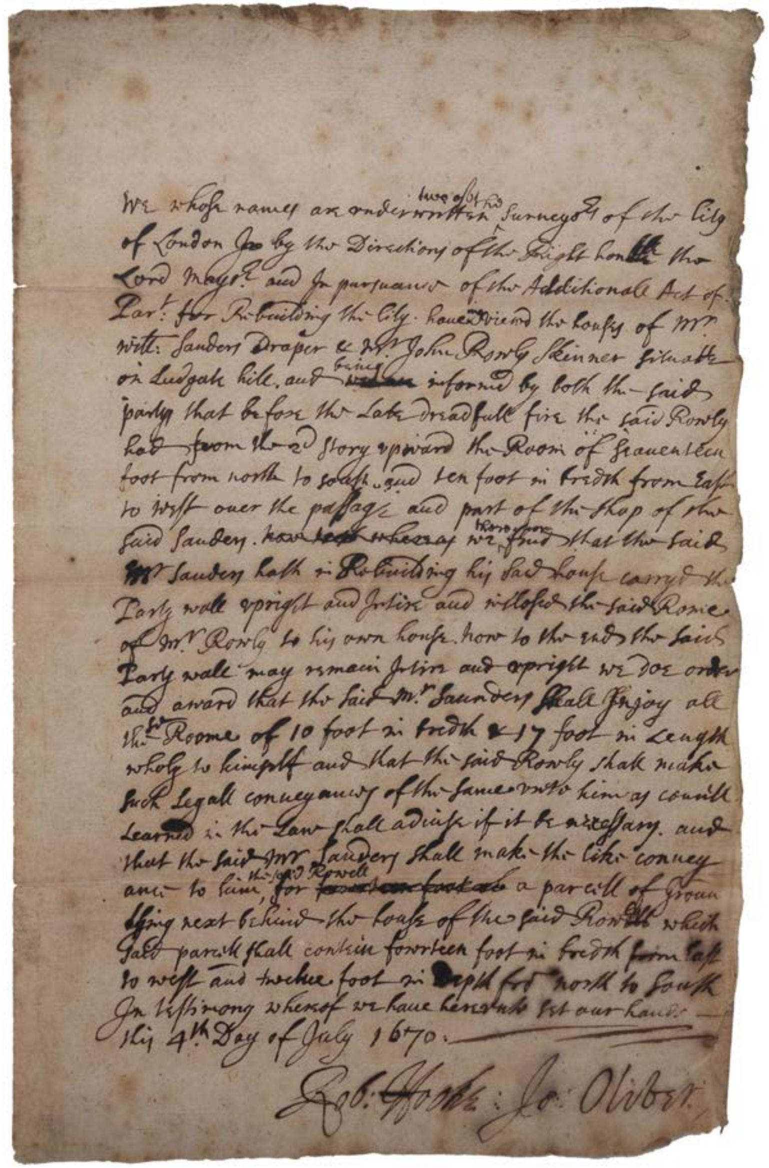
VERY RARE DOCUMENT DETAILING THE GREAT FIRE OF LONDON BY HOOKE

HOOKE, Robert. Autograph report in Hooke's hand, and signed by him, as surveyor of the City of London following the Great Fire, concerning a disagreement arising from the rebuilding of a structure on Ludgate Hill in the burnt district. Countersigned by Hooke's fellow City Surveyor John Oliver. Dated 4 July 1670. *[MATTED WITH:]* **HOLLAR, Wenceslaus.** *A Map or Groundplot of the Citty of London and the Suburbes thereof, that is to say all which is within the iurisdiction of the Lord Mayor or properlie calld't London: by which is exactly demonstrated the present condition thereof since the last sad accident of fire. The blanke space signifeing the burnt part & where the houses are exprest, those places yet standig [sic].* London: Sold by John Overton at the White House in little Brittaine, next door to S. Bartholomew gate, 1666.

$95,000

Autograph document: one page, single foolscap sheet of laid paper (290 x 190 mm), 28 lines in Hooke's hand with several contemporaneous corrections and additions. Map: Sheet size 302 x 368 mm.

A very rare document related to the Great Fire of London written and signed by the great polymath Robert Hooke (1635-1703), with an equally rare separately-issued map showing the destruction caused by the fire. Starting at a bakery on Pudding Lane sometime after midnight on September 2, 1666, The Great Fire of London consumed over 13,000 houses, as well as numerous churches (including St. Paul's cathedral) and other buildings. Charles II sought to rebuild as soon as possible to limit unrest and possible rebellion and called for plans from Robert



Hooke, John Evelyn, Christopher Wren, and others. Hooke was appointed Surveyor of the City of London and, with Wren, was the chief architect for its rebuilding. As Surveyor Hooke was the arbiter of disputes erupting out of the staking-out process whereby party walls had been altered or streets widened. The present document is a report on such a dispute, between William Sanders (or Saunders) Draper and John Rowly Skinner over the rebuilding of their shop and residence on Ludgate Hill within the burnt district. Autograph documents by Hooke are extremely rare, with only two examples on the market in the last quarter century: Hooke's manuscript notebook recording proceedings of the Royal Society (sold by private treaty to the Royal Society by Bonham's in 2006 for a reported £1,000,000) and a signed document being a King's Warrant for a patent for Hooke's watches with springs (sold by Bloomsbury Auctions for £23,100 in 1991). The present autograph document is accompanied by an important map of London following the fire, published in December 1666, and described by John Evelyn as 'the most accurate hitherto extant' (see *Letterbooks*, epistle CCLXXXI). "Hollar was to be employed in the preparation of surveys for rebuilding the city and was in close touch with the cartographic elite of his day, the quality of his work is apparent" (Glanville). The present map is an example of the first state, with Overton's 'White horse in little Brittaine' address. We find no examples of this map appearing on the market, and only three institutional holdings (British Library, Harvard and the Bibliothèque Nationale).

"In the early morning of Sunday, September 2, 1666, embers in the oven of Thomas Farriner's bakery set fire to the wharves along the Thames. Despite the dry summer beforehand, the city administration reacted without much concern; Lord Mayor Thomas Bludworth, London's chief official, infamously quipped that 'a woman might piss it out.' As if in a Greek tragedy, hubris in the face of a mightier power became the city's downfall. Whipped up by the wind and enabled by a lack of adequate firebreaks, the fire spread rapidly, engulfing the city for three more days. Forced onto a boat on the Thames, diarist Samuel Pepys watched the flames from nearly the same view as the creators of the city's maps and prints. Instead of an idyllic medieval town, Pepys saw 'one entire arch of fire from this to the other side of the bridge, and in a bow up the hill.' Only when the winds died down on Wednesday the 5th did the blaze subside, revealing the extent of the devastation. Evelyn's diary entry from the 10th reads in full: 'I went again to the ruines, for it was now no longer a Citty.' Indeed, while only eight people perished in the flames, London was left fundamentally changed. Over four-fifths of the walled city lay in ashes, with at least 13,000 houses and hundreds of shops, halls, and churches destroyed. Hundreds of thousands of people wandered without shelter, displaced from their now charred homes. Beyond the human cost, London's former cityscape, upon which the city had long been mapped and conceived, lay ruined. The conflagration 'obliterated at a stroke virtually every trace of a medieval city that had been six centuries in the making,' observed historian Neil Hanson. Whether tragedy or opportunity, the Great Fire burnt down one London and left open the possibility of creating another. Evelyn did not exaggerate in concluding, '*London* was, but is no more.'

"Still staggering from the scale of the losses, King Charles II and the city government acted swiftly but without a coherent plan. Five days after the fire, the Court of Common Council forbid property owners from immediate reconstruction. Charles himself then issued a proclamation on the matter three days later. On the surface, he promised an idealistic vision of 'a much more beautiful city' that would become 'the most convenient and noble for the advancement of trade of any city in Europe.' He prohibited hasty and unplanned rebuilding, authorizing the removal of any unapproved construction. Nonetheless, Charles denied that 'any particular person's right and interest [would] be sacrificed to the public benefit or convenience.' As such, his grand ideas, like widening the main streets and building a city wharf, lacked any specific locational detail. Instead, he pledged a comprehensive survey of the destroyed properties before any plan was finalized and promised ‘a plot or model … for the whole building through those ruined places.’ Regardless of the specifics, Charles recognized the necessity of cartography and surveys in order to realize his vision. Mapping would no longer be a years-long pursuit for travel guides and artists. Charles needed a map—a new kind of map—and he needed it fast.

“The king’s plan required two elements: a detailed survey of land ownership and a map of which areas had been burnt down. For the latter, Charles turned to the man most experienced at depicting London: Wenceslaus Hollar. Within days, Hollar’s request to map the fire’s results received an enthusiastic response from a government desperate to use cartography to reshape the city. On September 10, Hollar and associate Francis Sandford were tasked ‘to take an exact plan and survey of the city, as it now stands after the calamity of the late fire.’ They set to work immediately, surveying the damage and creating a map at an unprecedented speed …

“Hollar’s map shows a London hollowed to its very core—but ripe for transformation. The drawing strikingly depicts the old city as an empty swath. ‘The blanke space,’ as Hollar captioned it, lies raggedly demarcated from the unaffected outer districts beyond. Hollar included few buildings within the fire zone, all drawn as simple rectangles viewed from above, suggesting their ashen foundations. Streets and the blocks they surround receive little contrast, as if to say that they could be shifted around without any obstacle. Of course, Hollar may have been forced by approaching deadlines to leave out details and use blank space. But Hollar borrowed from his unfinished pre-fire map for much of the non-affected area—meaning he was not as rushed as it might seem …

“Hollar’s maps influenced the thinking of the key players in the rebuilding of London. His work impressed King Charles, who named Hollar His Majesty’s

Scenographer, a position affording some financial and anti-piracy protection. Hollar's maps, though, were no mere trifle of the king. As historian Ralph Hyde relates, all the major committees and organs of rebuilding utilized Hollar's plots" (Wasserman).

"At a meeting with the Privy Council in early October 1666, representatives of the City were told that the King had already appointed Hugh May, Roger Pratt and Christopher Wren as his Commissioners for Rebuilding, to work with three men to be nominated by the City. May and Pratt were experienced architects and administrators of large building works, but Wren was by far the youngest and least experienced of the three. The City responded by nominating two experienced master craftsmen – the carpenter Edward Jerman, and the City Surveyor, the bricklayer Peter Mills …

"The King showed foresight in appointing Christopher Wren – a clever and ambitious young man – as his third Commissioner … The City had to respond with a nominee who had intellectual abilities and ambitions similar to Wren's and who could work harmoniously with him. They knew that Hooke and Wren – distant cousins, and friends for many years – were successfully working together in experimental science. Hooke's *Micrographia* (1665) had begun as a cooperative venture with Wren … the City might have been accused of taking an undue risk in nominating Hooke as their third Surveyor of New Buildings. But it was a wise choice …

"Regulations had to be devised which would lead to significant improvements in the appearance and convenience of the city. Hooke's first surveying work took place in the exceptionally cold winter of 1666-67, when he represented the City in drafting the building regulations for the parliamentary rebuilding acts … The Rebuilding Acts went as far as was feasible to ensure that the new city would be a healthier and more pleasant place in which to live. The Acts classified new buildings according to their locations, and specified the form and maximum height of each class. All walls were to be made of brick or stone, and were to be built vertically from the ground up. The old timber-framed buildings with upper stories that jutted out above crooked, narrow lanes leading only into small, enclosed yards were all forbidden …

"The Rebuilding Acts set up Fire Courts specifically to deal with disputes about tenancies, leases, rents and disagreements about who should pay the costs of private rebuilding. Although under the Acts the City had the authority and obligation to carry out public works, … they delegated to the Surveyors the responsibility and obligation to do what was necessary. More often than not, Hooke was involved, and from the outset he took on the leading role. The City had nominated Mills, Hooke and Jerman as Surveyors, but Jerman preferred to work for private clients, … and when Mills died soon after rebuilding had begun, the City appointed the glazier John Oliver in his place. Hooke was the only City Surveyor who worked throughout the rebuilding programme. He did as much routine surveying in private rebuilding as Mills and Oliver together, and took on nearly all the surveying for public rebuilding …

"When private rebuilding began, complaints inevitably arose between neighbours Allegations were made of infringements of rights to light, or drainage, or access. Party walls were a common source of complaint. The cost of rebuilding a party wall had to be paid initially by the person rebuilding first, but finally had to be shared equally. Sometimes the second neighbour refused to pay because no holes had been left in the brickwork for his joists. In many cases the new vertical party walls resulted in all or part of an upper room which formerly extended over a neighbour's lower room being lost to the advantage of the neighbour.

The intermixtures of interest had to be investigated and settled by payment of appropriate compensation by one neighbour to the other … All of these complaints had to be investigated by the Surveyors, who reported in writing to the City the evidence they had found and what settlement they had arranged, subject to the City's approval. The complexity of the allegations and counter-allegations, and the general intransigence of the parties involved, made views (reports) far more demanding on the Surveyors' time and patience than certifying lost ground and new foundations, but in fewer than 1% of about a thousand views did the matter go beyond the jurisdiction of the City, acting on the Surveyors' recommendations. Hooke produced at least 550 views on infringements" (Cooper, pp. 166-175).
The autograph document offered here is one such 'view'. It reads:

We whose names are underwritten, two of the Surveyors of the City of London, by the Directions of the Right hon[ble.] the Lord Mayor and for pursuance of the Additional Act of Par[liamen][t.] for Rebuilding the City. Having viewed the houses of Mr. Will. Sanders Draper & Mr. John Rowly Skinner situated on Ludgate Hill, and being informed by both the said partys that before the Late dreadful fire the said Rowly had from the 2[d] story upward the Room of seaventeen foot from north to south and ten foot in bredth from East to West over the passage and part of the shop of the said Sanders. We therefore find the said Mr. Sanders hath in Rebuilding his said house carryd the Party wall upright and Intire and inclosed the said Rome of Mr. Rowly to his own house. Now to the ends the said Party wall may remain Intire and upright we doe order and award that the said Mr. Saunders shall Injoy all these Rooms of 10 foot in bredth and 17 foot in Length wholy to himself and that the said Rowly shall make such Legall conveyancing of the same unto him as councill Learned in the Law shall advise if it be necessary, and that the said Mr. Sanders shall make the like conveyance to him, the said Rowell [sic], a parcill of Groun[d] lying next behind the house of the said Rowly which said parcill shall continue fourteen foot in bredth from East to West and twelve foot in depth from North to South. In testimony whereof we have herewith set our hands, this 4[th] Day of July 1670.
Rob: *Hooke*; *Jo*: *Oliver.*

"In his work as City Surveyor, Hooke came face-to-face with literally thousands of individual Londoners when he certified their lost ground, staked out their foundations, and took views of their complaints and allegations. The citizens, eager to resume normal domestic and business life, demanded a speedy and efficient service from the City and from its Surveyors in particular. Hooke's services to private citizens were in demand throughout the seven years from mid-1667, during which period he spent most of his mornings (except Sundays) on his duties as Surveyor … Much of his time during those mornings was spent either in the City's streets taking measurements, looking for evidence of earlier foundations in the rubble, taking note of oral and written evidence in a dispute, or in coffee houses and inns, writing his reports" (*ibid*., pp. 175-6).

Hollar's first map of post-fire London was produced in November 1666 (Pennington 1003). About a month later he published the second, more extensive, map offered here (Pennington 1004). Both maps provided a bird's-eye-view of London, showing the burnt area. Our larger map covers the area from Lincoln's Inn Fields in the west to the Tower in the east, and from Southwark and the River Thames north to 'Clerkenwell Greene' and 'Fynsbury Fields' (the smaller map did not go so far east or west, omitting Lincoln's Inn Fields and Bankside). In the lower right corner of the map is a compartment containing refs. 1-100, and headed 'Annotations of the Churches, and other remarkable places in the Map.' Inset is a small compartment of refs. A-Z, a-o, indicating the locations of various churches and landmarks, respectively. Along the bottom of the larger compartment is a scale marked 'This length is one English mile from one end to the other.' In the bottom left corner of the main map is a small-scale map of the City of London, Westminster and Southwark (this was not included in the first version of the map): *A GENERALL MAP of the whole Citty of London with Westminster & all the Suburbs, by which may bee computed the proportion of that which is burnt, with other parts standing. W. Hollar fecit 1666*. In the upper left corner of this small-scale map are two columns of refs. A-K, and beneath the title two columns of refs. a-s.

One of the greatest etchers and engravers, Wenceslaus Hollar (1607-77) was born in Prague, but lived a peripatetic life, mostly spent in London, but with periods in Stuttgart, Strasbourg, Frankfurt, Cologne and Antwerp. In London he was employed as 'Serviteur domesticque' to Prince James, perhaps as a drawing master to Prince Charles (later King Charles I) and Prince James, and in 1660 appointed as King's Iconographer, or Designer of Prospects to the King. From 1652 Hollar became increasingly preoccupied with the creation of a 5 feet by 10 feet, 24 sheet, bird's-eye style wall map depicting every important building in London, which he seems to have intended to survey himself. Although only one trial sheet of the proposed map, showing the streets around Covent Garden, now survives, he seems to have made good progress, and this map undoubtedly served as the basis for his quickly produced post-fire maps of London, including the map offered here. He partnered with John Leake and other surveyors to engrave two updated versions of the present map, in 1667 and 1669.

For the map: Howgego, *Printed Maps of London circa 1553-1850* (1964), 19.1; Glanville, *London in Maps* (1972), plate 11; Pennington, *A descriptive catalogue of the etched work of Wenceslaus Hollar* (*1607-1677*) (2002), 1004. Cooper, 'The civic virtue of Robert Hooke,' pp. 161-186 in *Robert Hooke and the English Renaissance*, Kent & Chapman (eds.), 2005. Wasserman, *In the heat of the moment*: *cartography, rebuilding, and reconceptualization after the Great Fire of London* (historicalreview.yale.edu/sites/default/files/files/Wasserman.pdf).

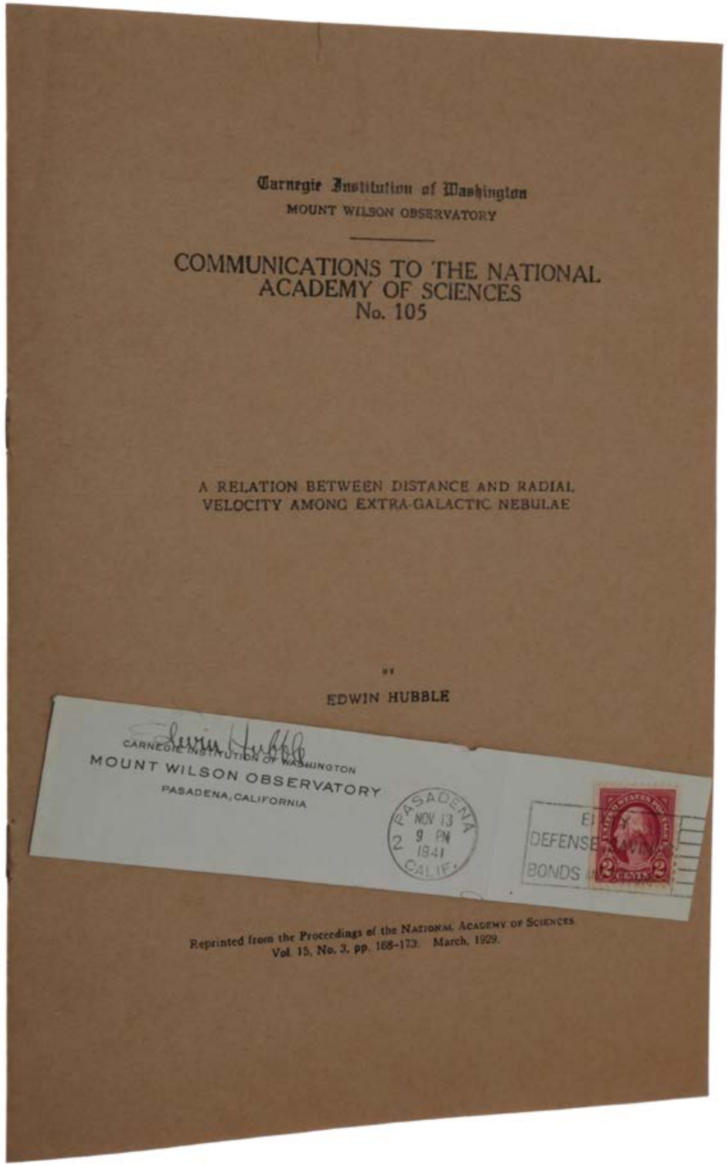
HUBBLE'S LAW - THE EXPANSION OF THE UNIVERSE

HUBBLE, Edwin. *'A Relation between Distance and Radial Velocity among Extra-Galactic Nebulae.'* Offprint from Proceedings of the National Academy of Sciences, Vol. 15, No. 3. N. p. [Washington, D.C.]: Carnegie Institution, 1929. [Offered with:] Signature of Edwin Hubble on section of an envelope sent on November 13, 1941 from Mount Wilson, where the observations leading to Hubble's discovery of the expansion of the universe were carried out, retaining printed address of the Mount Wilson Observatory and stamp. Washington: National Academy of Sciences, 1929.

$55,000

8vo (258 x 175 mm), original printed wrappers, [1] 2-6 [7-8:blank]. First text leaf with some offsetting of the verso, otherwise a very fine and unmarked copy.

First edition, very rare offprint, of Hubble's landmark paper, which "made as great a change in man's conception of the universe as the Copernican revolution 400 years before" (DSB). This paper "is generally regarded as marking the discovery of the expansion of the universe" (*Biographical Encyclopedia of Astronomers*). It established what would later become known as Hubble's Law: that galaxies recede from us in all directions and more distant ones recede more rapidly in proportion to their distance. "… the repercussions were immense. The galaxies were not randomly dashing through the cosmos, but instead their speeds were mathematically related to their distances, and when scientists see such a relationship they search for a deeper significance. In this case, the significance was nothing less than the realization that at some point in history all the galaxies in the universe had been compacted into the same small region. This was the first



observational evidence to hint at what we now call the Big Bang" (Simon Singh, *Big Bang*). Hubble's "result has come to be regarded as the outstanding discovery in twentieth-century astronomy" (DSB). Autograph material by Hubble of any kind, even his signature, is hardly ever seen on the market.

In the early 1920s most astronomers believed that the universe was static and unchanging on the large scale. Einstein himself had introduced his 'cosmological constant' in 1917 to allow solutions of the equations of general relativity corresponding to a static universe. Two such solutions were found: Einstein's matter-filled universe and Willem de Sitter's empty universe. The latter model attracted much interest because it predicted redshifts for very distant objects, something which had been observed as early as 1912 by Vesto Slipher. However, De Sitter's model was conceived by astronomers to be no less static than Einstein's. In 1922 Alexander Friedmann developed a model of an evolutionary universe, which could be expanding, and this was re-discovered by Georges Lemaître in 1927. But Lemaître went further: he established theoretically the proportional relationship between the rate of expansion and distance. Important as these theoretical developments were, it was only observational data that could establish which of the models, if any, corresponded to the actual universe.

Edwin Powell Hubble (1889-1953) "was born in 1889 in Missouri. As a young man, he was tall and athletic, known especially for his talent at boxing, basketball, and track. He earned an undergraduate degree in math and astronomy at the University of Chicago, and then studied law at Oxford on a Rhodes scholarship, following his father's wishes. Hubble returned to the US and joined the Kentucky bar, but quickly decided law wasn't for him. He taught high school Spanish for a year before heading back to the University of Chicago to earn his PhD in astronomy in 1917. After serving in the Army in World War I, he went to southern California to work at the Mt. Wilson observatory, home of the 100-inch Hooker telescope, the largest in the world at the time.

"In the early 1920s many astronomers believed that objects then known as nebulae were nearby gas clouds in our own galaxy, and that the Milky Way was the entire universe, while others thought the nebulae were actually more distant 'island universes' separate from our own galaxy.

"At Mt. Wilson, Hubble began measuring the distances to nebulae to try to resolve the issue, using a method based on an earlier discovery by Henrietta Leavitt. She had found that a type of star known as a Cepheid variable had a predictable relationship between its luminosity and its pulsation rate. Measuring the period of the star's fluctuations in brightness would give its absolute brightness, and comparing that with the star's apparent brightness would yield a measure of the star's distance.

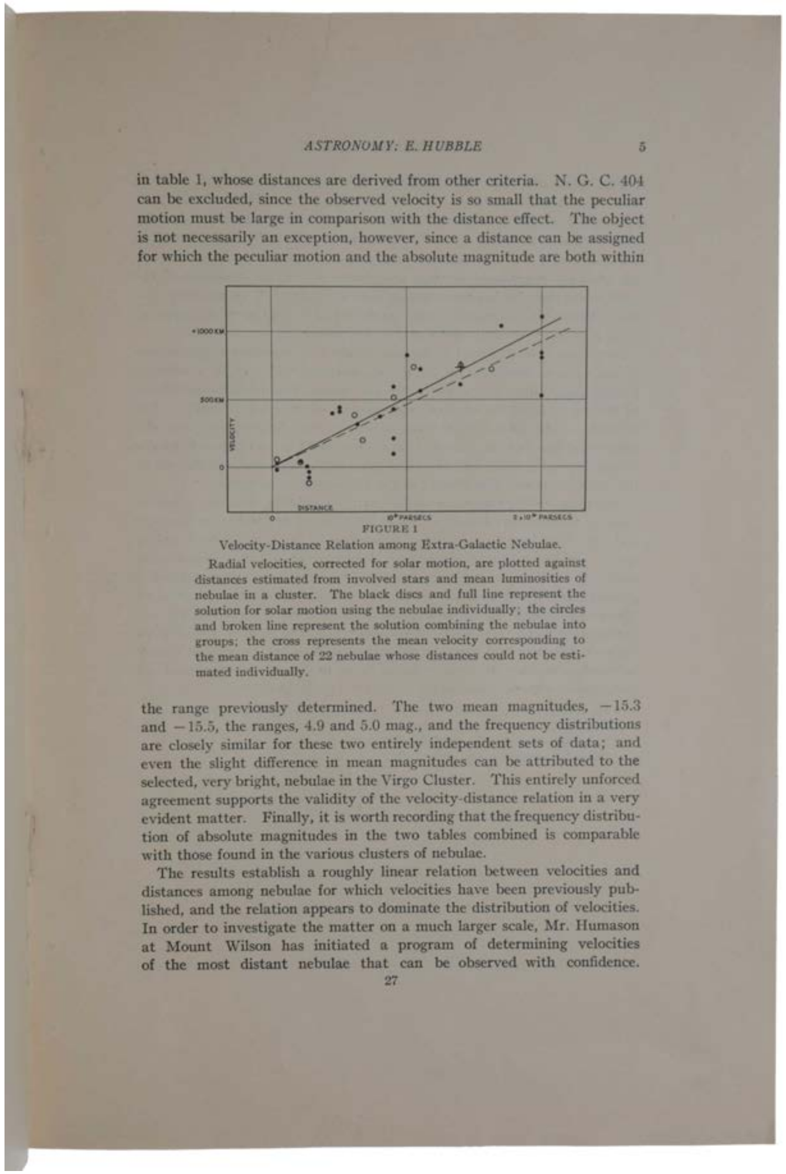
"Hubble found he was able to resolve Cepheid variables in the Andromeda nebula, showing that the nebula was in fact a separate galaxy rather than a gas cloud within the Milky Way. He also showed that the galaxy was much farther away than previously thought, greatly expanding our view of the universe. Hubble announced the finding on January 1, 1925 at a meeting of the American Astronomical Society in Washington DC.

"Following the ground-breaking announcement, Hubble continued measuring the distances to far away astronomical objects, measurements that in a few years would lead to a discovery with even more radical implications for cosmology.

"It was already known that nebulae appeared redder than they should be. Astronomers, notably Vesto Slipher, had found that the light from most nebulae was red-shifted, indicating that most of the nebulae were receding at high speeds. But it wasn't understood why other galaxies would all appear to be moving away from us.

"Hubble continued his meticulous astronomical measurements. He collaborated with Milton Humason, who had begun working as a janitor at the Mt. Wilson observatory, then rose to become a night assistant and then an assistant astronomer. Humason observed spectra, while Hubble concentrated on finding distances to various objects" ('This Month in Physics History: Edwin Hubble Expands our View of the Universe,' *APS News*, Vol. 17, No. 1, January 2008, aps.org/publications/apsnews/200801/physicshistory.cfm).

"By 1929 Hubble had obtained distances for eighteen isolated galaxies and for four members of the Virgo cluster. In that year he used this somewhat restricted body of data to make the most remarkable of all his discoveries and the one that made his name famous far beyond the ranks of professional astronomers. This was what is now known as Hubble's law of proportionality of distance and radial velocity of galaxies. Since 1912, when V. M. Slipher at the Lowell Observatory had measured the radial velocity of a galaxy (M 31) for the time by observing the Doppler displacement of its spectral lines, velocities had been obtained of some forty-six galaxies, forty-one by Slipher himself. Attempts to correlate these velocities with other properties of the galaxies concerned, in particular their apparent diameters, had been made by Carl Wirtz, Lundmark, and others; but no definite, generally acceptable result had been obtained. In 1917 W. de Sitter had constructed, on the basis of Einstein's cosmological equations, an ideal world-model (of vanishingly small average density) which predicted red shifts, indicative of recessional motion, in distant light sources; but no such systematic effect seemed to emerge from the empirical data. Hubble's new approach to the problem, based on his determinations of distance, clarified an obscure situation. For distances out to about 6,000,000 light-years he obtained a good approximation to a straight line in the graphical plot of velocity against distance. Owing to the



ASTRONOMY: E. HUBBLE 5

in table 1, whose distances are derived from other criteria. N. G. C. 404 can be excluded, since the observed velocity is so small that the peculiar motion must be large in comparison with the distance effect. The object is not necessarily an exception, however, since a distance can be assigned for which the peculiar motion and the absolute magnitude are both within

FIGURE 1

Velocity-Distance Relation among Extra-Galactic Nebulae.

Radial velocities, corrected for solar motion, are plotted against distances estimated from involved stars and mean luminosities of nebulae in a cluster. The black discs and full line represent the solution for solar motion using the nebulae individually; the circles and broken line represent the solution combining the nebulae into groups; the cross represents the mean velocity corresponding to the mean distance of 22 nebulae whose distances could not be estimated individually.

the range previously determined. The two mean magnitudes, −15.3 and −15.5, the ranges, 4.9 and 5.0 mag., and the frequency distributions are closely similar for these two entirely independent sets of data; and even the slight difference in mean magnitudes can be attributed to the selected, very bright, nebulae in the Virgo Cluster. This entirely unforced agreement supports the validity of the velocity-distance relation in a very evident matter. Finally, it is worth recording that the frequency distribution of absolute magnitudes in the two tables combined is comparable with those found in the various clusters of nebulae.

The results establish a roughly linear relation between velocities and distances among nebulae for which velocities have been previously published, and the relation appears to dominate the distribution of velocities. In order to investigate the matter on a much larger scale, Mr. Humason at Mount Wilson has initiated a program of determining velocities of the most distant nebulae that can be observed with confidence.

27

tendency of individual proper motions to mask the systematic effect in the case of the nearer galaxies, Hubble's straight-line graph depended essentially on the data obtained from galaxies in the Virgo cluster. These indicated that over the observed range of distance, velocities increased at the rate of roughly 100 miles a second for every million light-years of distance.

"Einstein paid a special visit to Hubble at Mount Wilson in 1931 to thank him for his work, and said that introducing the cosmological constant in order to ensure a static universe had been 'the greatest blunder of my life.'

"Hubble's discovery stimulated much theoretical work in relativistic cosmology and aroused great interest in fundamental papers on expanding world models by A. Friedmann and G. Lemaître that had been written several years before but had attracted little attention. The interpretation of the straight line in Hubble's graph of velocity against distance and of its slope were eagerly discussed. The constant ratio of velocity to distance is now usually denoted by the letter *H* and is called Hubble's constant. It has the dimensions of an inverse time – its reciprocal, according to Hubble's original determination, being approximately two (since revised to about ten) billion years. If the galaxies recede uniformly from each other, as was suggested by E. A. Milne in 1932, this could be interpreted as the age of the universe; but, whatever the true law of recessional motion may be, Hubble's constant is generally regarded as a fundamental parameter in theoretical cosmology.

"Hubble's work was characterized not only by his acuity as an observer but also by boldness of imagination and the ability to select the essential elements in an investigation. In his careful assessment of evidence he was no doubt influenced by his early legal training. He was universally respected by astronomers, and on his death N. U. Mayall expressed their feelings when he wrote: 'It is tempting to think that Hubble may have been to the observable region of the universe what the Herschels were to the Milky Way and what Galileo was to the solar system'" (DSB VI: 530-531).

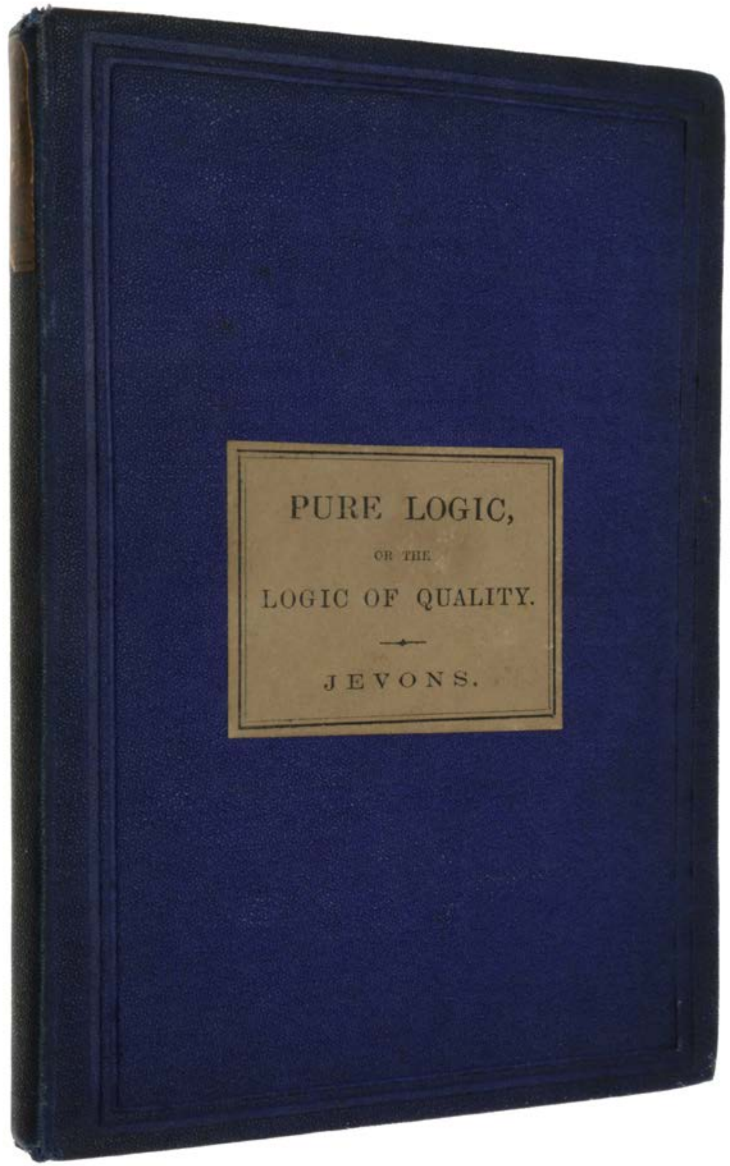
BOOLE'S LOGIC IMPROVED

JEVONS, William Stanley. *Pure Logic or the Logic of Quality apart from Quantity: with remarks on Boole's System and on the Relation of Logic and Mathematics.* London: Edward Stanford, 1864.

$2,850

Small 8vo, pp. [vi], 87, [1]. Original pebble-grained blue cloth, printed paper label to spine and front cover, boards panelled in blind, brown coated endpapers (very minor rubbing to extremities). A fine copy.

First edition of Jevons's first work on logic, and a fine copy in original condition. "Jevons' logical system was regarded by him as being to a large extent founded on the work of Boole. 'The forms of my system,' he says (*Pure Logic*, p. 3), may in fact be reached by divesting his (i.e., Boole's) system of a mathematical dress, which, to say the least, is not essential to it'" (Mays & Henry, p. 485). "Jevons actually improved on Boole in some important details, as, for instance, in showing that the Boolean operations of subtraction and division were superfluous" (DSB 7:105). Jevons had already consulted Boole before sending him a copy of the present work; in a letter of 1863 he recorded, 'I have written on the subject to Professor Boole, on whose logical system mine is an improvement. In his answer he does not explain away an objection I had raised against his system. He seems to think that my paper viz. *Pure Logic* probably does not contain more than he himself knows, this being a common failing of philosophers and others; but still he tells me very civilly that if I think still that there is anything new in my paper I ought to publish, which of course I shall do one way or another before long.' Jevons's principal advance was to reduce the operations of the Boolean calculus to a mechanical procedure. He here stood at the start of a road that led to the modern application



of logic in computer-programming; he himself designed a 'logical abacus' and 'logical piano', which "solved problems with superhuman speed and accuracy, and some of its features can be traced in modern computer designs" (*ibid*.).

Provenance: Charles J. Poynting (bookplate on front paste-down and signature on initial blank dated 30 April [18]75). Erwin Tomash (book label on front paste-down). Tomash & Williams suggest that Poynting may be the son of the physicist John Henry Poynting, known for the 'Poynting vector' which describes the direction and magnitude of electromagnetic energy flow: "Both had a close association with Owens College, Manchester, where Jevons was at the time of this publication" (Tomash Library Catalogue, p. 681).

"There is a need for a revaluation of the logical work of Jevons, especially as he was a pioneer in the mechanisation of logic. His achievements in this direction have been overlooked and remain relatively unknown. Jevons seriously believed that he was the discoverer of a new kind of logic, and records in his Journal an illumination resembling that of Descartes when he discovered coordinate geometry. He tells us: 'As I awoke in the morning, the sun was shining brightly into my room. There was a consciousness on my mind that I was the discoverer of the true logic of the future. For a few minutes I felt a delight such as one can seldom hope to feel.' If Jevons were alive today it is unlikely that he would be surprised by modern digital computers and the arithmetical marvels which they perform" (Mays & Henry, p. 484).

"Jevons' Logic Piano anticipates contemporary computing in an oblique fashion. Computers as we know run on 1s and 0s. Indeed the logical aspect of a computer sits at the bottom of its structure and is embodied in the circuitry (presuming it is electronic of course). Jevons system of logic uses the same basic logic operations, and indeed, in 1940 a young American engineer called Claude Shannon showed in his Masters thesis, 'A symbolic analysis of relay switching circuits', that Boolean algebra could be used to describe switching circuits. The architecture of the contemporary computer has subsequently proceeded from this point. However, the system that we presently understand as Boolean Algebra is quite different to that originally developed by Boole in the 1840s and 1850s. According to Nathan Houser and Ivor Grattan–Guinness, Boole's system has been much modified in order to become the Boolean Algebra we now know, and that this is a process of modification begun by Jevons. Although he was not interested in what we call truth-value calculus—1s and 0s—Jevons' logic machine was actually performing a function provided today by a truth table. Indeed, Wolf Mays defines Jevons as the first user of matrix analysis. In essence Jevons' primary legacy in the history of computing is his mechanization of Boolean logic, a key aspect of contemporary computing. It is also reasonable to assert that Jevons was of the key figures in the reformulation of Boolean logic into Boolean algebra, such that it could be employed by Claude Shannon to describe the switching of circuits" (Barrett & Connell).

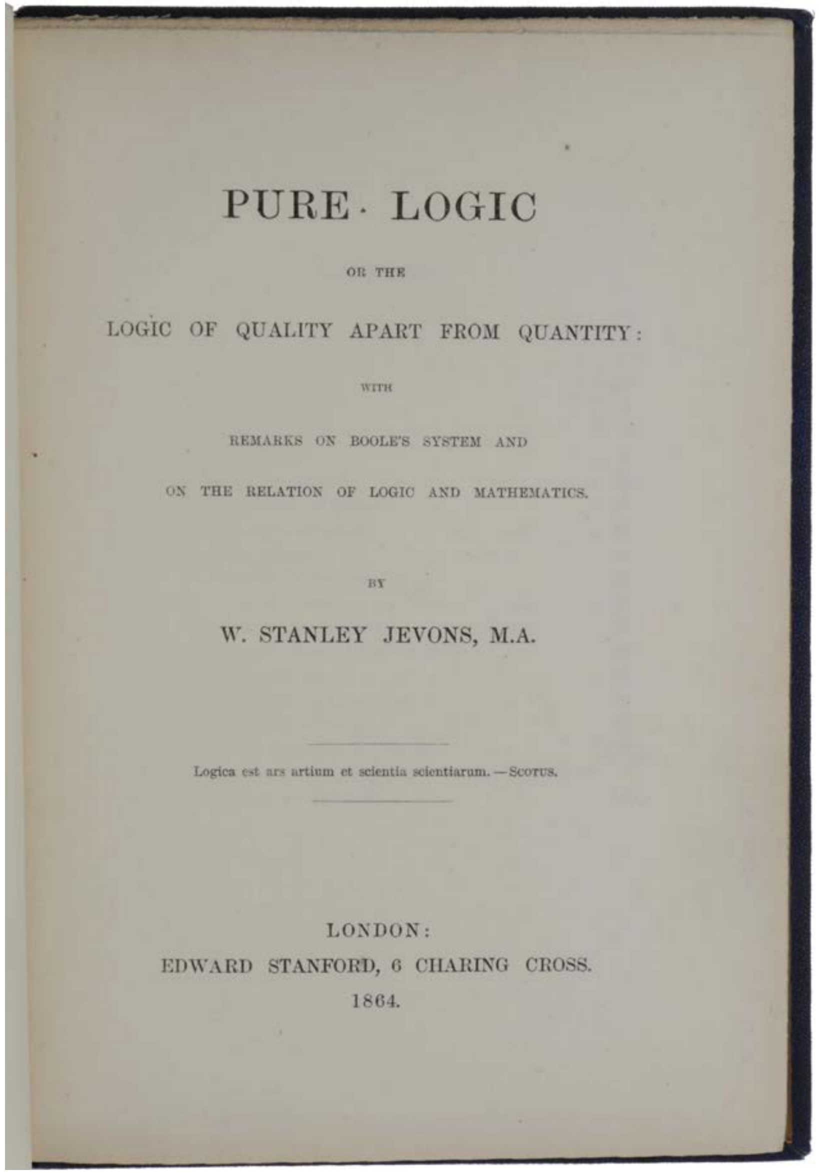
"Jevons's fame as the inventor of a logic machine has tended to obscure the important role he played in the history of both deductive and inductive logic … At a time when most British logicians ignored or damned with faint praise the remarkable achievements of George Boole, Jevons was quick to see the importance of Boole's work as well as many of its defects. He regarded Boole's algebraic logic as the greatest advance in the history of the subject since Aristotle. He deplored the fact that Boole's two revolutionary books, published as early as 1847 [*The Mathematical Analysis of Logic*] and 1854 [*The Laws of Thought*], had virtually no effect on the speculations of leading logicians of the time.

"On the other hand, Jevons believed (and modern logicians agree with him) that Boole had been led astray by efforts to make his logical notation resemble algebraic notation. 'I am quite convinced,' Jevons stated in a letter, 'that Boole's forms . . . have no real analogy to the similar mathematical expressions.' He also saw clearly the weakness in Boole's preference for the exclusive rather than the inclusive interpretation of 'or.'

"It was to overcome what he regarded as unnecessary obscurity and awkwardness in Boole's notation that Jevons devised a method of his own that he called the 'method of indirect inference.' 'I have been able to arrive at exactly the same results as Dr. Boole,' he wrote, 'without the use of any mathematics; and though the very simple process which I am about to describe can hardly be said to be strictly Dr. Boole's logic, it is yet very similar to it and can prove everything that Dr. Boole proved.' Jevons's system is also very similar to Venn's diagrammatic method as well as a primitive form of the familiar matrix or truth-table technique …

"[Jevons'] method … correspond[s] closely to a truth-table analysis. The logical alphabet is simply another way of symbolizing all the possible combinations of truth-values. Each premise forces us to eliminate certain lines of this 'truth table.' What remain are of course the lines that are consistent with the premises. If the premises contain a contradiction, then all the lines will be eliminated just as all the compartments will become shaded if contradictory truth-value premises are diagramed on Venn circles. Jevons likes to call his system a 'combinatorial logic,' and although he did not apply it to propositional functions, he clearly grasped the principles of matrix analysis that had eluded Boole …

"To increase the efficiency of his combinatorial method, Jevons devised a number of laborsaving devices, culminating in the construction of his logic machine … As early as 1863 Jevons was using a 'logical slate.' This was a slate on which a logical alphabet was permanently engraved so that problems could be solved by chalking out the inconsistent lines. Still another device, suggested to Jevons by



PURE LOGIC

OR THE

LOGIC OF QUALITY APART FROM QUANTITY:

WITH

REMARKS ON BOOLE'S SYSTEM AND

ON THE RELATION OF LOGIC AND MATHEMATICS.

BY

W. STANLEY JEVONS, M.A.

Logica est ars artium et scientia scientiarum.—Scotus.

LONDON:

EDWARD STANFORD, 6 CHARING CROSS.

1864.

a correspondent, is to pencil the alphabet along the extreme edge of a sheet of paper, then cut the sheet between each pair of adjacent combinations. When a combination is to be eliminated, it is simply folded back out of sight" (Gardner, pp. 92-100).

Jevons was the ninth child of Thomas Jevons, a Liverpool iron merchant, and Mary Arm, daughter of William Roscoe, a noted banker, historian and art collector of the same city. The family were Unitarians and Stanley's background was thus that of a cultured and well-to-do Nonconformist family; but his childhood was shadowed by the death of his mother in 1845, the illness of his eldest brother, which began in 1847, and the failure of the family business in 1848. Jevons's schooling, begun at the Mechanics Institute High School in Liverpool, was continued at University College School, London, and in 1851 he entered University College London itself to study chemistry and mathematics. At this stage Jevons apparently intended to enter a business career without completing his degree but when a post as assayer to the newly established Mint in Sydney, Australia, was offered to him in 1853 he decided to take it, encouraged by his father, whose finances had never been restored after the family bankruptcy in 1848. Jevons spent the years from 1854 to 1859 in Australia, applying his knowledge of chemistry at the Sydney Mint and studying, mainly botany and meteorology, in his spare time. From 1857 onwards his interest turned towards social and economic questions; he began to see his life-work as lying in 'the study of Man' and decided that this involved returning to England to improve his academic qualifications. Arriving home in September 1859 he re-enrolled at University College London, completing his BA in 1860 and then the MA course in 1862. Jevons's attempts to make a career as a journalist in London met with little success and he followed up a suggestion made by his cousin, Henry Enfield Roscoe, who was already Professor of Chemistry at Owens College, Manchester, that he should apply for a vacancy there as a general tutor. When Jevons took up this very junior post in 1863 he was quite unknown even in academic circles, but he had already produced two works which were to prove of seminal importance in economics - his 'Brief Account of a General Mathematical Theory of Political Economy', first read before the British Association in 1862, and his outstanding applied research into changes in the value of gold. In the following year, 1864, his first published contribution to the study of logic (*Pure Logic*) also appeared, and during the next ten years he produced a series of works which established his standing as one of the leading thinkers of his time in both political economy and logic.

Tomash J-20 (this copy). Barrett & Connell, 'Jevons and the Logic 'Piano',' *The Rutherford Journal*, vol. 1 (2005); Gardner, *Logic Machines and Diagrams*, 1958. Mays & Henry, 'Jevons and Logic,' *Mind*, vol. 62 (1953), pp. 484-505.

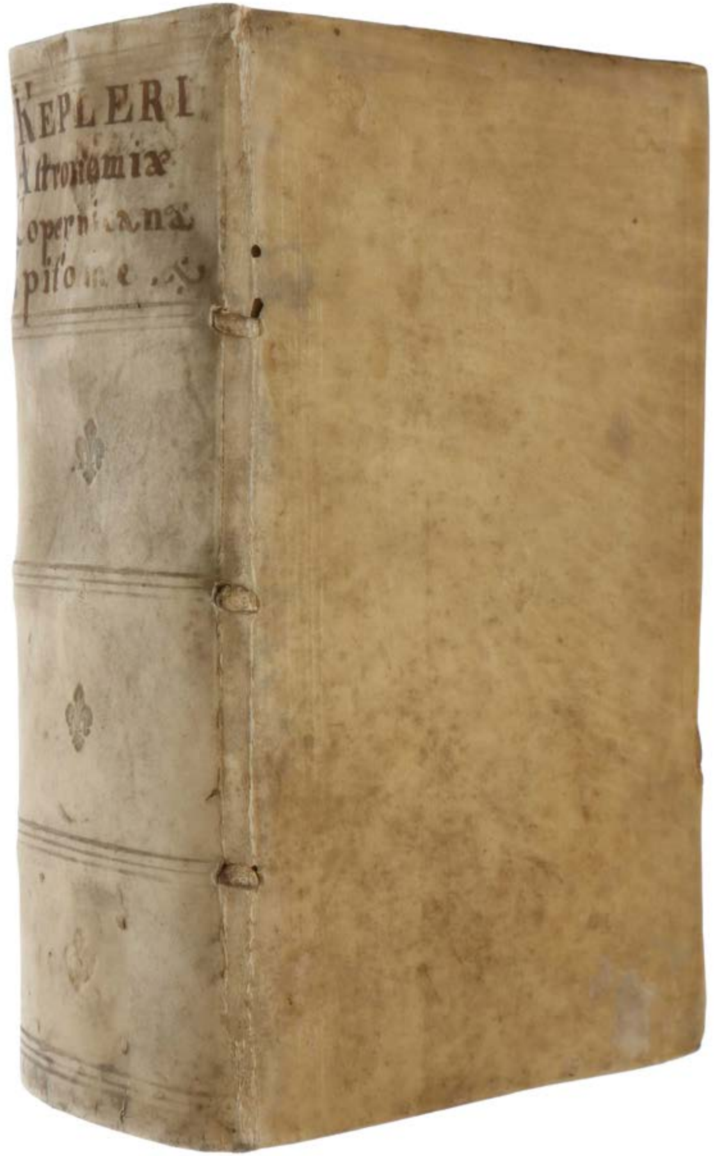
KEPLER'S THIRD LAW OF PLANETARY MOTION

KEPLER, Johannes. *Epitome astronomiae Copernicanae: usitatâ formâ quæstionum & responsionum conscripta, inque VII. libros digesta, quorum tres hi priores sunt de doctrina sphæricâ. Habes, amice lector, hac prima parte, præter physicam accuratam explicationem motus terræ diurni, ortusq[ue] ex eo circulorum sphæræ, totam doctrinam sphæricam nova & concinniori methodo, auctiorem, additis exemplis omnis generis computationum astronomicarum & geographicarum, quæ integrarum præceptionum vim sunt complexa.* Linz: Johann Planck, 1618 [Books I-III] & 1622 [Book IV]; Frankfurt: Georg Tampach, 1621 [Books V-VII].

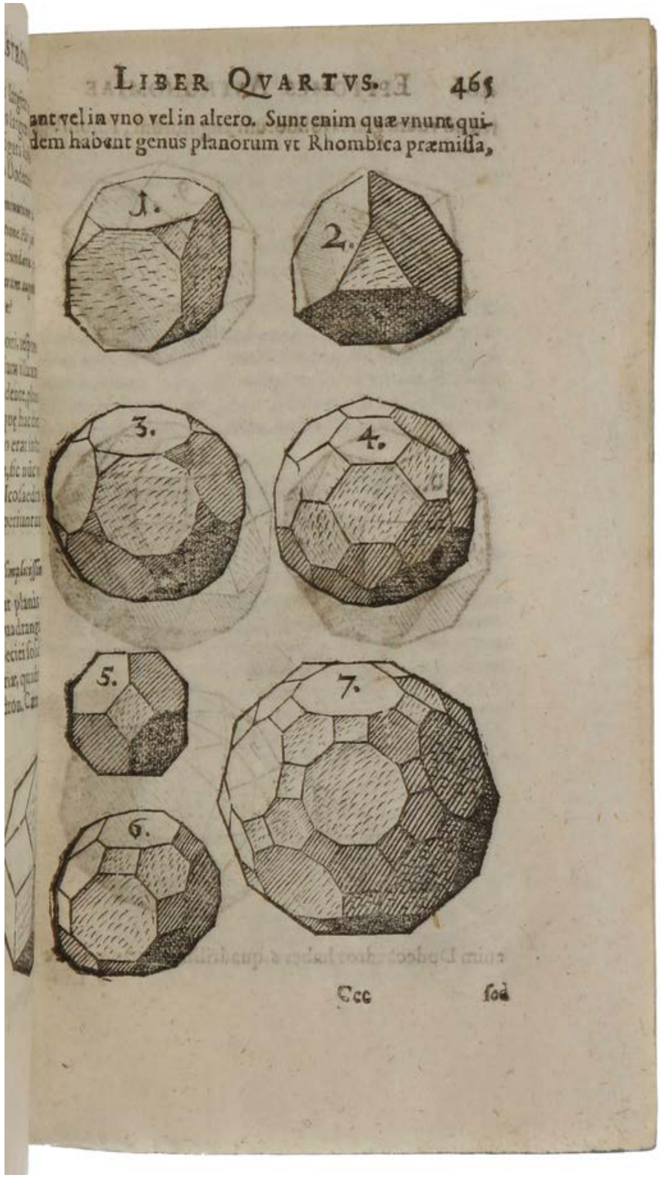
$125,000

8vo (161 x 94 mm), pp. [xxviii], 417 [recte 409], [3, blank]; [ii], 419-622, [2, errata and blank]; [xii], 641-932, [16, index], with numerous woodcut diagrams in text and one folding printed table (a few gatherings with some slight browning). Contemporary German vellum, blue edges. A large, fresh, unrestored, and attractive copy, with some lower edges uncut.

First edition, an immaculate copy, the finest we have seen, of the third of Kepler's great trilogy of astronomical treatises, following *Astronomia nova* (1609) and *Harmonice mundi* (1619), in which he introduced his three laws of planetary motion. The *Epitome* "ranks next to Ptolemy's *Almagest* and Copernicus' *De revolutionibus* … [It] is the first systematic complete presentation of astronomy to introduce the ideas of modern celestial mechanics founded by Kepler … The title gives no inkling that Kepler had erected an entirely new structure on



the foundation of the Copernican theory, that he had rescued the Copernican conception, at the time disputed and little believed, and helped it to break through by introducing his planet laws and by treating the phenomena of the motions physically" (Caspar, p. 297). "This work [the *Epitome*] would prove to be the most important theoretical resource for the Copernicans in the 17th century. Galileo and Descartes were probably influenced by it" (Britannica). Kepler "hypothesizes that force is needed to sustain motion and that hence some force must be acting on the planets. This force, he speculates, originates from the sun, can act over a vacuum, and may be magnetic. In contrast to many scientists of the time, Kepler believes much of space to be a vacuum" (Parkinson). "One important detail is Kepler's extension of his first two planetary laws to all the other planets [they originally applied only to Mars] as well as to the moon and the four satellites of Jupiter" (*Johannes Kepler Quadricentennial Celebration*, University of Texas at Austin (1971), p. 77). The *Epitome* was in seven books. "The first three books covered spherical astronomy, the fourth through sixth planetary and lunar theory, and the seventh precession and related material … The spherical astronomy of the early books was unconventional chiefly in its heliocentric, or Copernican, interpretation of the diurnal rotation of the heavens, and in its account of the likely physical causes of this motion. The later books, however, described Kepler's own theories: elliptical orbits, the area law, orbital planes passing through the center of the sun, and the various archetypal relations and physical forces underlying the structure and dynamics of the universe … This novel claim permeated the *Epitome* from beginning to end: astronomy was physics, and astronomical phenomena were best understood through mathematical study of their physical causes" (Stephenson, *Kepler's Physical Astronomy* (1987), p. 139). "The theory of the moon is easily the most original part of the *Epitome* … a subject which had occupied Kepler since the 1590s but about which he had published little prior to the *Epitome*" (*ibid.*, p. 140). Books I-III, IV and V-VII were originally issued separately and have their own title pages and imprints. In common with almost



all copies, ours has the second issue of Book IV, dated 1622 rather than 1620. OCLC lists only four copies of the first issue of Book IV (none in US); only one has appeared at auction (the Richard Green copy, Christie's, 17 June 2008, lot 208, $92,500), and we know of only one other having appeared in commerce, which we handled several years ago, having acquired it from a private collector (who had himself acquired it from another collector some thirty years previously). The only other comparably fine copy on the market in recent years was that offered by W. P. Watson in Cat. 17 (2011), no. 55, for £75,000 (then about $120,000) – like our copy, Watson's had the second issue of Book IV.

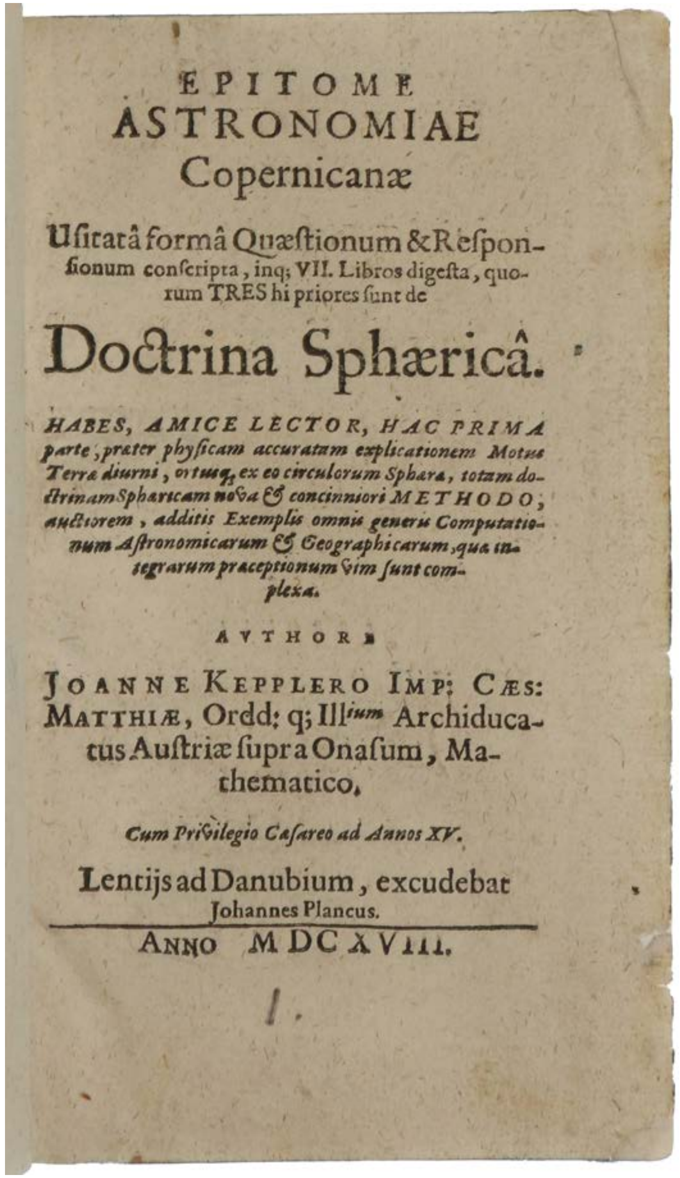
"The composition of the *Epitome* was closely intertwined with the personal vicissitudes of its author's life. Although [Kepler] had been pressed for a more popular book on Copernican astronomy when his very technical *Astronomia nova* appeared, not until the spring of 1615 were the first three books ready for the printer. This part finally appeared in 1617 [but with imprint 1618], having been delayed a year because, even though he had previously signed a contract with an Augsburg publisher, Kepler wanted the work done by his new Linz printer. By that time his seventy-year-old mother had been charged with witchcraft, and the astronomer felt obliged to go to Württemberg to aid in her legal defence. Afterward, the writing of the *Harmonice mundi* interrupted progress on the *Epitome*, so that the second instalment, book IV, did not appear until 1620. The printing was barely completed when Kepler again journeyed to Württemberg, this time for the actual witchcraft trial. During pauses in the proceedings, he consulted with Maestlin at Tübingen about the lunar theory and arranged the printing of the last three books in Frankfurt. The publisher completed his work in the autumn of 1621, just as Kepler's mother won acquittal after enduring the threat of torture.

"The first three books of this compendium deal mainly with spherical astronomy.

Occasionally Kepler went beyond the conventional subject matter, considering, for example, the spatial distribution of stars and atmospheric refraction. Of special interest are the arguments for the motions of the earth; in describing the relativity of motion, he went considerably further than Copernicus and correctly formulated the principles later given more detailed treatment in Galileo's *Dialogo* (1632). Because of these arguments, and as a result of the anti-Copernican furore stirred up by Galileo's polemical writings, the *Epitome* was placed on the *Index Librorum Prohibitorum* in 1619 …

"Book IV opened with one of his favorite analogies, one that had already appeared in the *Mysterium cosmographicum* and that stressed the theological basis of his Copernicanism: The three regions of the universe were archetypal symbols of the Trinity – the center, a symbol of the Father; the outermost sphere, of the Son; and the intervening space, of the Holy Spirit. Immediately thereafter Kepler plunged into a consideration of final causes, seeking reasons for the apparent size of the sun, the length of the day, and the relative sizes and the densities of the planets. From first principles he attempted to deduce the distance of the sun by assuming that the earth's volume is to the sun's as the radius of the earth is to its distance from the sun. Nevertheless, his assumption was tempered by a perceptive examination of the observations. In their turn the nested polyhedrons, the harmonies, the magnetic forces, the elliptical orbits, and the law of areas also found their place within Kepler's astonishing organization.

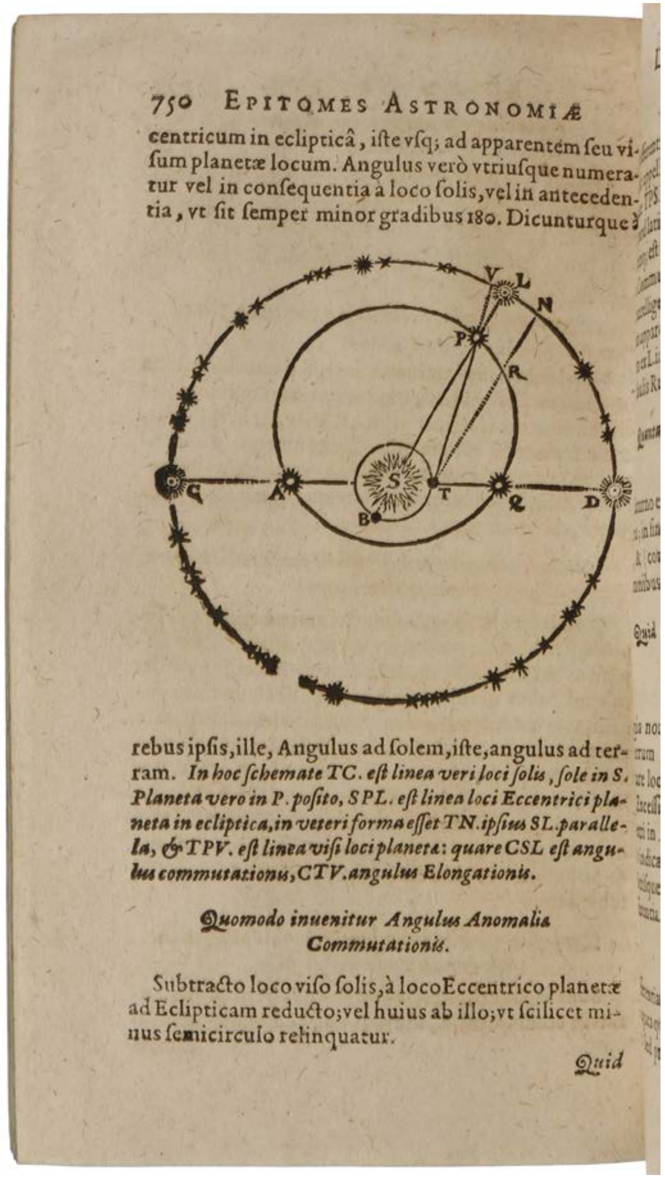
"The harmonic law, which Kepler had discovered in 1619 and announced virtually without comment in the *Harmonice mundi*, received an extensive theoretical justification in the *Epitome*, book IV, part 2, section 2. His explanation of the $P \propto a^{3/2}$ law [where P is the period of rotation of the planet in its orbit and a is its mean distance from the sun], was based on the relation P ∝ (L x M)/(S x V),


EPITOME
ASTRONOMIAE
Copernicanæ
Usitatâ formâ Quæstionum & Responsionum conscripta, inq; VII. Libros digesta, quorum TRES hi priores sunt de
Doctrina Sphæricâ.
HABES, AMICE LECTOR, HAC PRIMA parte, præter physicam accuratam explicationem Motus Terræ diurni, ortusq; ex eo circulorum Sphæræ, totam doctrinam Sphæricam nova & concinniori METHODO, auctiorem, additis Exemplis omnis generis Computationum Astronomicarum & Geographicarum, quæ integrarum præceptionum vim sunt complexa.
AVTHORE
JOANNE KEPPLERO IMP: CÆS: MATTHIÆ, Ordd: q; Ill^ium Archiducatus Austriæ supra Onasum, Mathematico.
Cum Privilegio Cæsareo ad Annos XV.
Lentijs ad Danubium, excudebat Johannes Plancus.
ANNO MDCXVIII.

where the longer the path length L, the longer the period; the greater the strength S of the magnetic emanation, the shorter the period (this magnetic 'species,' emitted from the sun, provided the push to the planet); the more matter M in the planet, the more inertia and the longer the period; the greater the volume V of the planet, the more magnetic emanation could be absorbed and the shorter the period. According to Kepler's distance rule, the driving force S was inversely proportional to the distance a, and hence L/S was proportional to a^2; thus the density M/V had to be proportional to $1/a^{1/2}$ in order to achieve the 3/2 power law. Consequently, he assumed that the density (as well as both M and V) of each planet depended monotonically on its distance from the sun, a requirement quite appropriate to his ideas of harmony. To a limited extent he could defend his choice of V from telescopic observations of planetary diameters, but generally he was obliged to fall back on vague archetypal principles.

"The lunar theory, which closed book IV of the *Epitome*, had long been a preoccupation of its author. In Tycho's original division of labor, Kepler had been assigned the orbit of Mars and [Christian] Longomontanus (1562-1647) that of the moon; but not long after Tycho's death Kepler applied his own ideas of physical causes to the lunar motion. To Longomontanus' angry remonstrance Kepler replied that it was not the same with astronomers as with smiths, where one made swords and another wagons. He believed that the moon would undergo magnetic propulsion from the sun as well as from the earth, but the complicated interrelations gave much difficulty. In 1616 Maestlin wrote to him:

'Concerning the motion of the moon, you write that you have traced all the inequalities to physical causes; I do not quite understand this. I think rather that one should leave physical causes out of account, and should explain astronomical matters only according to astronomical method with the aid of astronomical, not



750 EPITOMES ASTRONOMIÆ

centricum in ecliptica, iſte vſq; ad apparentem ſeu viſum planetæ locum. Angulus verò vtriuſque numeratur vel in conſequentia à loco ſolis, vel in antecedentia, vt ſit ſemper minor gradibus 180. Dicunturque à rebus ipſis, ille, Angulus ad ſolem, iſte, angulus ad terram. *In hoc ſchemate TC. eſt linea veri loci ſolis, ſole in S. Planeta vero in P. poſito, SPL. eſt linea loci Eccentrici planetæ in ecliptica, in veteri forma eſſet TN. ipſius SL. parallela, & TPV. eſt linea viſi loci planetæ: quare CSL eſt angulus commutationis, CTV. angulus Elongationis.*

Quomodo inuenitur Angulus Anomaliæ Commutationis.

Subtracto loco viſo ſolis, à loco Eccentrico planetæ ad Eclipticam reducto; vel huius ab illo; vt ſcilicet minus ſemicirculo relinquatur.

Quid

physical, causes and hypotheses. That is, the calculation demands astronomical bases in the field of geometry and arithmetic …'

In other words, the circles, epicycles, and equants that Kepler had ultimately abandoned in his *Astronomia nova*.

"Kepler persisted in seeking the physical causes for the moon's motion and by 1620 had achieved the basis for his lunar tables. The fundamental form of his lunar orbit was elliptical, but the positions were further modified by the evection and by Tycho's so-called variation. Kepler's lunar theory, as given in book IV of the *Epitome*, failed to offer much foundation for further advances; nevertheless, his very early insight into the physical relation of the sun to this problem had enabled him to discover the annual equation in the lunar motion, which he handled by modifying the equation of time.

"Books V-VII of the *Epitome* dealt with practical geometrical problems arising from the elliptical orbits, the law of areas, and his lunar theory; and together with book IV they served as the theoretical explanation to the *Tabulae Rudolphinae*. Book V introduced what is now called Kepler's equation,

$E = M - e \sin E$,

where e is the orbital eccentricity, M is the mean angular motion about the sun, and E is an auxiliary angle related to M through the law of areas; Kepler named M and E the mean and the eccentric anomalies, respectively. Given E, Kepler's equation is readily solved for M; the more useful inverse problem has no closed solution in terms of elementary trigonometric functions, and he could only recommend an approximating procedure …

"Book VI of the *Epitome* treated problems of the apparent motions of the sun, the individual planets, and the moon. The short book VII discussed precession and the length of the year. To account for the changing obliquity, Kepler placed the pole of the ecliptic on a small circle, which in turn introduced a minor variation in the rate of precession (one last remnant of trepidation); because he was not satisfied with the ancient observations, he tabulated alternative rates in the *Tabulae Rudolphinae*. Such problems, he proposed, could be left to posterity "if it has pleased God to allot to the human race enough time on this earth for learning these left-over things"" (Owen Gingerich in DSB).

Johannes Kepler (1571-1630) came from a very modest family in the small German town of Weil der Stadt and was one of the beneficiaries of the ducal scholarship; it made possible his attendance at the Lutheran *Stift*, or seminary, at the University of Tübingen where he began his studies in 1589. At Tübingen, the professor of mathematics was Michael Maestlin (1550–1631), one of the most talented astronomers in Germany, and a Copernican (though a cautious one). Maestlin lent Kepler his own heavily annotated copy of *De revolutionibus,* and so while still a student, Kepler made it his mission to demonstrate rigorously Copernicus' theory.

In 1594 Kepler moved to Graz in Austria to take up a position as teacher at the Lutheran school there, and as provincial mathematician. Just over a year after arriving in Graz, Kepler discovered what he thought was the key to the universe: 'The earth's orbit is the measure of all things; circumscribe around it a dodecahedron, and the circle containing this will be Mars; circumscribe around Mars a tetrahedron, and the circle containing this will be Jupiter; circumscribe around Jupiter a cube, and the circle containing this will be Saturn. Now inscribe within the earth an icosahedron, and the circle contained in it will be Venus;

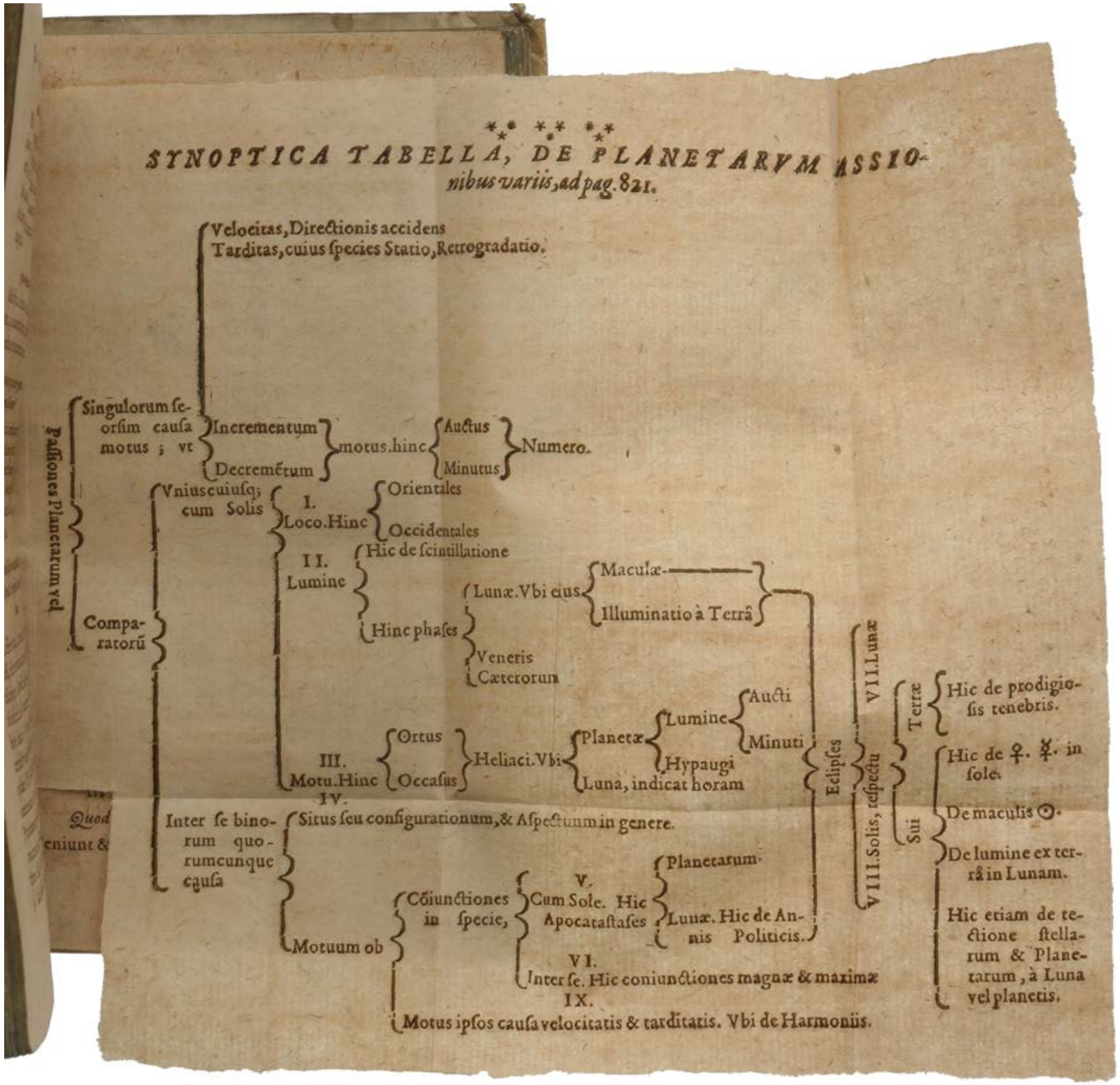
inscribe within Venus an octahedron, and the circle contained in it will be Mercury. You now have the reason for the number of planets.' This remarkable idea was published in *Mysterium cosmographicum* (1596), "the first unabashedly Copernican treatise since *De revolutionibus*" (DSB).

In place of the tradition that individual incorporeal souls push the planets and instead of Copernicus's passive, resting Sun, Kepler hypothesised that a single force from the Sun accounts for the increasingly long periods of motion as the planetary distances increase. A few years later he acquired William Gilbert's *De Magnete* (1600), and he generalized Gilbert's theory that the Earth is a magnet to the view that the universe is a system of magnetic bodies in which the rotating Sun sweeps the planets around by a magnetic force. This force, varying inversely with distance, was the major physical principle that guided Kepler's struggle to construct a better orbital theory for Mars.

The great Danish astronomer Tycho Brahe (1546–1601) had set himself the task of amassing a completely new set of planetary observations. In 1600 Tycho invited Kepler to join his court at Castle Benátky near Prague. When Tycho died suddenly in 1601, Kepler quickly succeeded him as imperial mathematician to Holy Roman Emperor Rudolf II. The relatively great intellectual freedom possible at Rudolf's court was now augmented by Kepler's unexpected inheritance of a critical resource: Tycho's observations. Without data of such precision to support his solar hypothesis, Kepler would have been unable to discover his 'first law', that Mars moves in an elliptical orbit. He published this discovery, together with his second or 'area law', that the time necessary for Mars to traverse any arc of its orbit is proportional to the area of the sector contained by the arc and the two radii from the sun, in *Astronomia nova*.

In 1611 Emperor Rudolf abdicated, and Kepler was forced to move to Linz where he was appointed district mathematician. The Linz authorities had anticipated that Kepler would use most of his time to work on and complete the astronomical tables begun by Tycho, but the work was tedious, and Kepler continued his search for the world harmonies that had inspired him since his youth. In 1619 his *Harmonice mundi*, which contained his third law, brought together more than two decades of investigations into the archetypal principles of the world: geometrical, musical, metaphysical, astrological, astronomical, and those principles pertaining to the soul. Eventually Newton would simply take over Kepler's laws while ignoring all reference to their original theological and philosophical framework.

Barchas 1147; Carli and Favaro 76 and 92; Caspar 55, 69, 66; Cinti 60, 72, 67; Houzeau & Lancaster 11831; Lalande p. 205; Parkinson 70; Zinner 4662, 4820, 4870. See PMM 112.


SYNOPTICA TABELLA, DE PLANETARVM PASSIO-
nibus variis, ad pag. 821.
Velocitas, Directionis accidens
Tarditas, cuius species Statio, Retrogradatio.
Passiones Planetarum vel
Singulorum seorsim causa motus ; vt
Incrementum
Decremẽtum
motus. hinc
Auctus
Minutus
Numero.
Comparatorũ
Vniuscuiusq; cum Solis
I. Loco. Hinc
Orientales
Occidentales
II. Lumine
Hic de scintillatione
Hinc phases
Lunæ. Vbi eius
Maculæ
Illuminatio à Terrâ
Veneris
Cæterorum
III. Motu. Hinc
Ortus
Occasus
Heliaci. Vbi
Planetæ
Lumine
Aucti
Minuti
Hypaugi
Luna, indicat horam
IV.
Eclipses
VII. Lunæ
VIII. Solis, respectu
Terræ
Hic de prodigiosis tenebris.
Sui
Hic de ♀. ☿. in sole.
De maculis ☉.
De lumine ex terrâ in Lunam.
Hic etiam de tectione stellarum & Planetarum, à Luna vel planetis.
Inter se binorum quorumcunque causa
Situs seu configurationum, & Aspectuum in genere.
Motuum ob
Cõiunctiones in specie,
V. Cum Sole. Hic Apocatastases
Planetarum.
Lunæ. Hic de Annis Politicis.
VI. Inter se. Hic coniunctiones magnæ & maximæ
IX. Motus ipsos causa velocitatis & tarditatis. Vbi de Harmoniis.

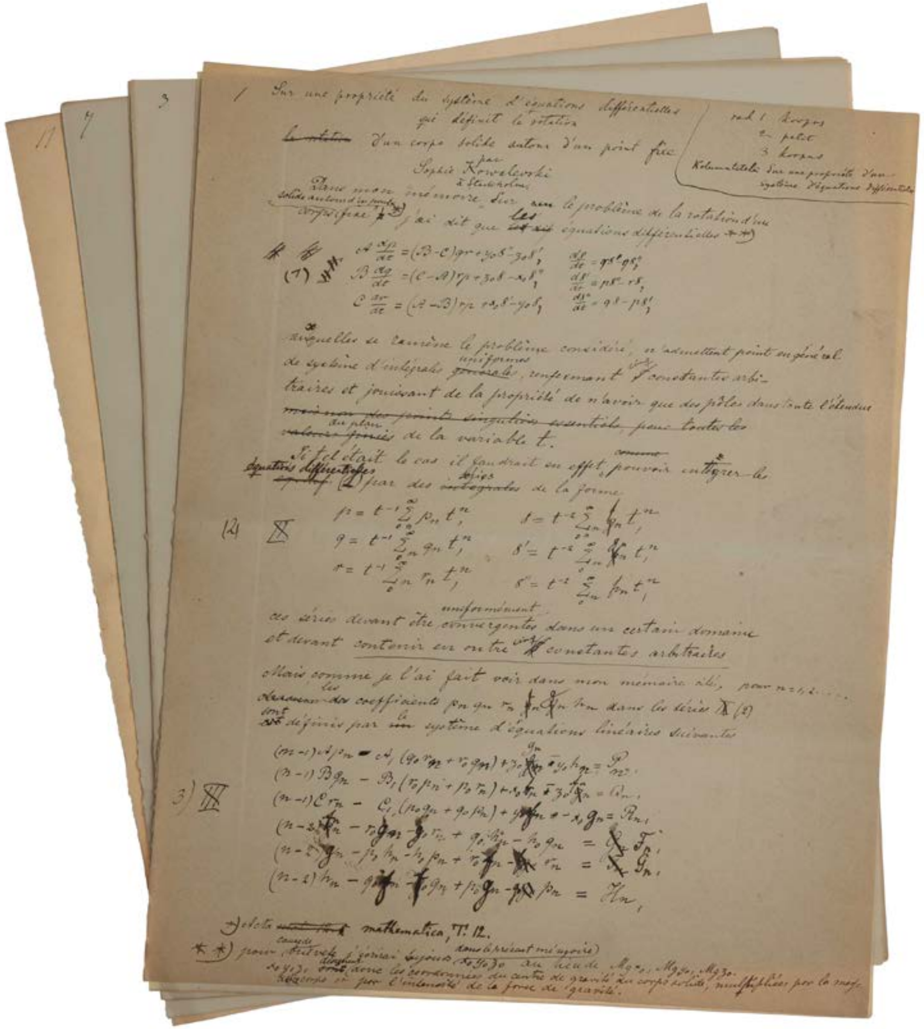
THE GREATEST KNOWN WOMAN SCIENTIST BEFORE THE 20TH CENTURY

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.* [1890].

$29,500

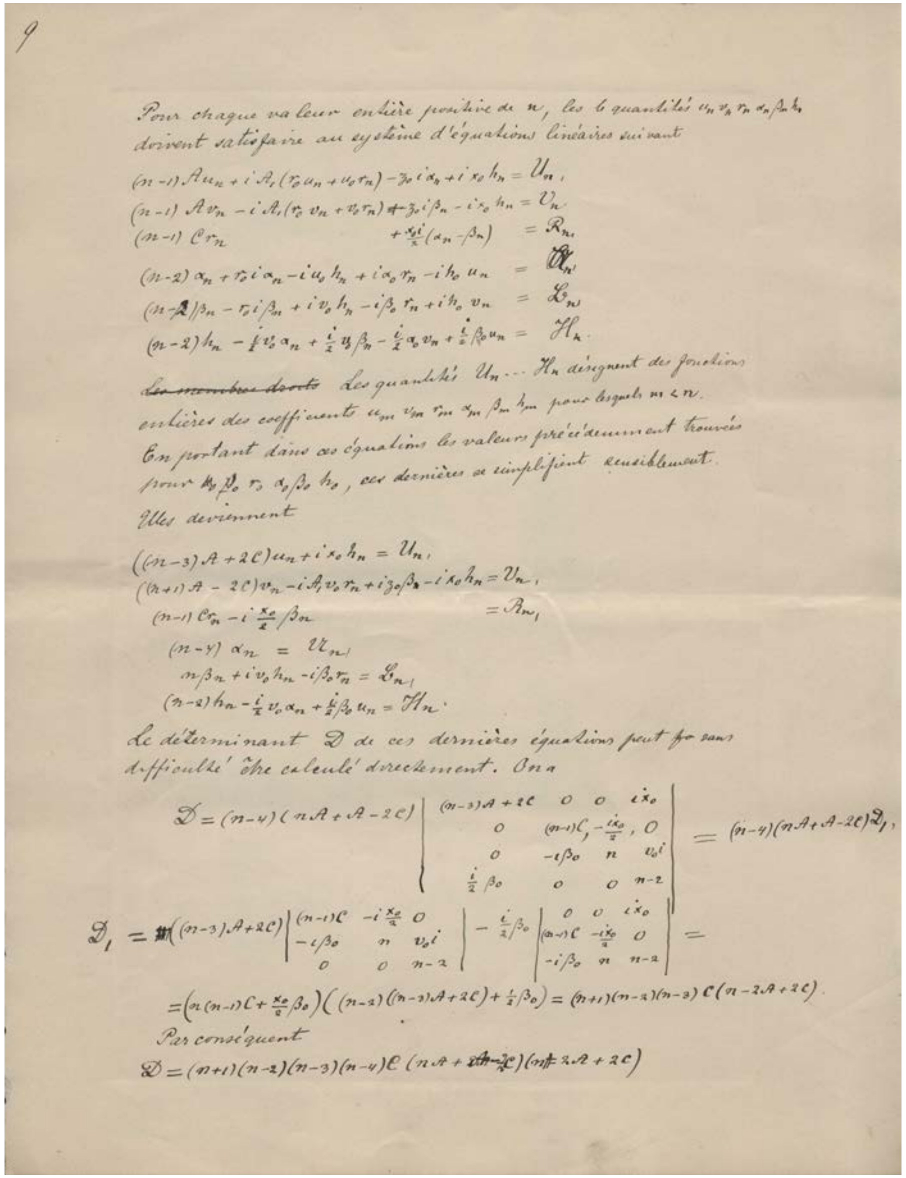
11 loose leaves, written on recto only, 290 x 232 mm, first leaf with extremely mild toning, some leaves with fingerprints in ink probably by the author. Three light horizontal creases from having been foled for postage. Very fine and clean.

Important autograph manuscript by "the greatest woman mathematician prior to the twentieth century" (DSB). Kowalewsky was a pioneer for women in mathematics around the world – the first woman to obtain a doctorate (in the modern sense) in mathematics, the first woman appointed to a full professorship in Northern Europe and one of the first women to work for a scientific journal as an editor. According to historian of science Ann Hibner Koblitz, Kowalewsky was "the greatest known woman scientist before the twentieth century". 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 symnmetric. 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 of 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.


9

Pour chaque valeur entière positive de n, les 6 quantités $u_n v_n r_n \alpha_n \beta_n h_n$ doivent satisfaire au système d'équations linéaires suivant

$$(n-1)Au_n + iA_1(r_0u_n + u_0r_n) - \gamma_0 i\alpha_n + ix_0h_n = U_n,$$
$$(n-1)Av_n - iA_1(r_0v_n + v_0r_n) + \gamma_0 i\beta_n - ix_0h_n = V_n$$
$$(n-1)Cr_n \qquad + \frac{x_0 i}{2}(\alpha_n - \beta_n) = R_n,$$
$$(n-2)\alpha_n + r_0 i\alpha_n - iu_0h_n + i\alpha_0 r_n - ih_0u_n = \mathfrak{A}_n$$
$$(n-2)\beta_n - r_0 i\beta_n + iv_0h_n - i\beta_0 r_n + ih_0v_n = \mathfrak{L}_n$$
$$(n-2)h_n - \frac{i}{2}v_0\alpha_n + \frac{i}{2}u_0\beta_n - \frac{i}{2}\alpha_0v_n + \frac{i}{2}\beta_0u_n = H_n.$$

~~Les membres droits~~ Les quantités $U_n \cdots H_n$ désignent des fonctions entières des coefficients $u_m v_m r_m \alpha_m \beta_m h_m$ pour lesquels $m < n$.

En portant dans ces équations les valeurs précédemment trouvées pour $u_0 \beta_0 r_0 \alpha_0 \beta_0 h_0$, ces dernières se simplifient sensiblement.

Elles deviennent

$$((n-3)A + 2C)u_n + ix_0h_n = U_n,$$
$$((n+1)A - 2C)v_n - iA_1v_0r_n + i\gamma_0\beta_n - ix_0h_n = V_n,$$
$$(n-1)Cr_n - i\frac{x_0}{2}\beta_n \qquad = R_n,$$
$$(n-4)\alpha_n = \mathfrak{A}_n,$$
$$n\beta_n + iv_0h_n - i\beta_0r_n = \mathfrak{L}_n,$$
$$(n-2)h_n - \frac{i}{2}v_0\alpha_n + \frac{i}{2}\beta_0u_n = H_n.$$

Le déterminant $\mathfrak{D}$ de ces dernières équations peut fo sans difficulté être calculé directement. On a

$$\mathfrak{D} = (n-4)(nA + A - 2C)\begin{vmatrix} (n-3)A+2C & 0 & 0 & ix_0 \\ 0 & (n-1)C, & -\frac{ix_0}{2}, & 0 \\ 0 & -i\beta_0 & n & v_0 i \\ \frac{i}{2}\beta_0 & 0 & 0 & n-2 \end{vmatrix} = (n-4)(nA + A - 2C)\mathfrak{D}_1,$$

$$\mathfrak{D}_1 = ((n-3)A+2C)\begin{vmatrix} (n-1)C & -i\frac{x_0}{2} & 0 \\ -i\beta_0 & n & v_0 i \\ 0 & 0 & n-2 \end{vmatrix} - \frac{i}{2}\beta_0\begin{vmatrix} 0 & 0 & ix_0 \\ (n-1)C & -\frac{ix_0}{2} & 0 \\ -i\beta_0 & n & n-2 \end{vmatrix} =$$

$$= \left(n(n-1)C + \frac{x_0}{2}\beta_0\right)\left((n-2)((n-3)A+2C) + \frac{i}{2}\beta_0\right) = (n+1)(n-2)(n-3)C(n-2A+2C).$$

Par conséquent

$$\mathfrak{D} = (n+1)(n-2)(n-3)(n-4)C(nA + A - 2C)(n-2A+2C)$$

"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 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 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*" …

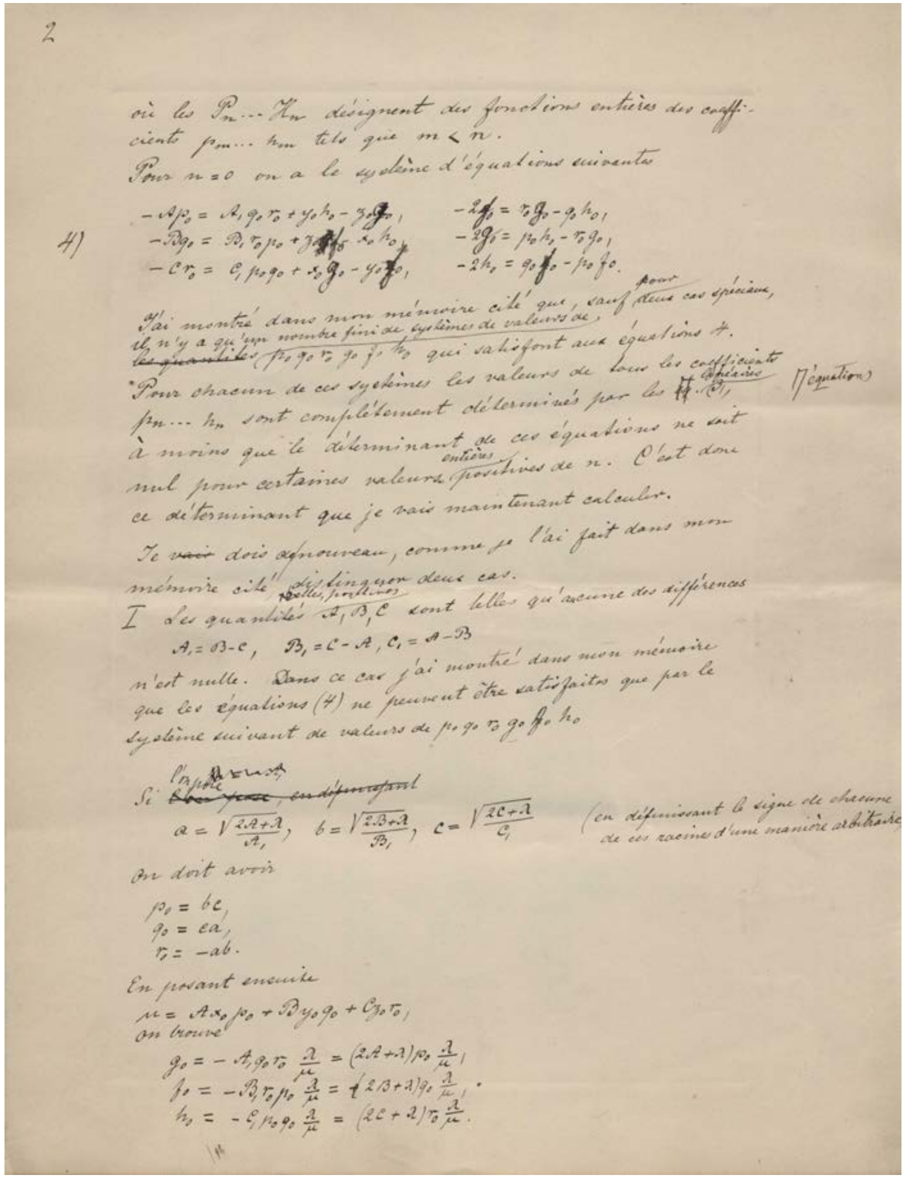
"Judging from the note just quoted, we see that Kovalevskaya's work had progressed to such a point that she knew when the equations could have single-valued solutions and when they could not … but the harder part of her work still lay ahead: to find the explicit solution in the case she described (the 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 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*



2

où les $P_n \dots H_n$ désignent des fonctions entières des coefficients $p_m \dots h_m$ tels que $m < n$.

Pour $n = 0$ on a le système d'équations suivantes

4) $-\lambda p_0 = A_1 q_0 r_0 + y_0 h_0 - z_0 f_0$, $-\lambda g_0 = r_0 f_0 - q_0 h_0$,
$-\lambda q_0 = B_1 r_0 p_0 + z_0 g_0 - x_0 h_0$, $-\lambda f_0 = p_0 h_0 - r_0 g_0$,
$-\lambda r_0 = C_1 p_0 q_0 + x_0 f_0 - y_0 g_0$, $-\lambda h_0 = q_0 g_0 - p_0 f_0$.

J'ai montré dans mon mémoire cité que, sauf pour deux cas spéciaux, il n'y a qu'un nombre fini de systèmes de valeurs de ~~les quantités~~ $p_0 q_0 r_0 g_0 f_0 h_0$ qui satisfont aux équations 4.

Pour chacun de ces systèmes les valeurs de tous les coefficients $p_n \dots h_n$ sont complètement déterminés par les équations (l'équation) à moins que le déterminant de ces équations ne soit nul pour certaines valeurs entières positives de n. C'est donc ce déterminant que je vais maintenant calculer.

Je ~~vais~~ dois de nouveau, comme je l'ai fait dans mon mémoire cité, distinguer deux cas.

I Les quantités A, B, C sont telles qu'aucune des différences
$A_1 = B - C$, $B_1 = C - A$, $C_1 = A - B$
n'est nulle. Dans ce cas j'ai montré dans mon mémoire que les équations (4) ne peuvent être satisfaites que par le système suivant de valeurs de $p_0 q_0 r_0 g_0 f_0 h_0$

Si l'on pose
$a = \sqrt{\frac{2A+\lambda}{A_1}}$, $b = \sqrt{\frac{2B+\lambda}{B_1}}$, $c = \sqrt{\frac{2C+\lambda}{C_1}}$ (en définissant le signe de chacune de ces racines d'une manière arbitraire)

On doit avoir
$p_0 = bc$,
$q_0 = ca$,
$r_0 = -ab$.

En posant ensuite
$\mu = A x_0 p_0 + B y_0 q_0 + C z_0 r_0$,
on trouve
$g_0 = -A_1 q_0 r_0 \frac{\lambda}{\mu} = (2A+\lambda) p_0 \frac{\lambda}{\mu}$,
$f_0 = -B_1 r_0 p_0 \frac{\lambda}{\mu} = (2B+\lambda) q_0 \frac{\lambda}{\mu}$,
$h_0 = -C_1 p_0 q_0 \frac{\lambda}{\mu} = (2C+\lambda) r_0 \frac{\lambda}{\mu}$.

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.

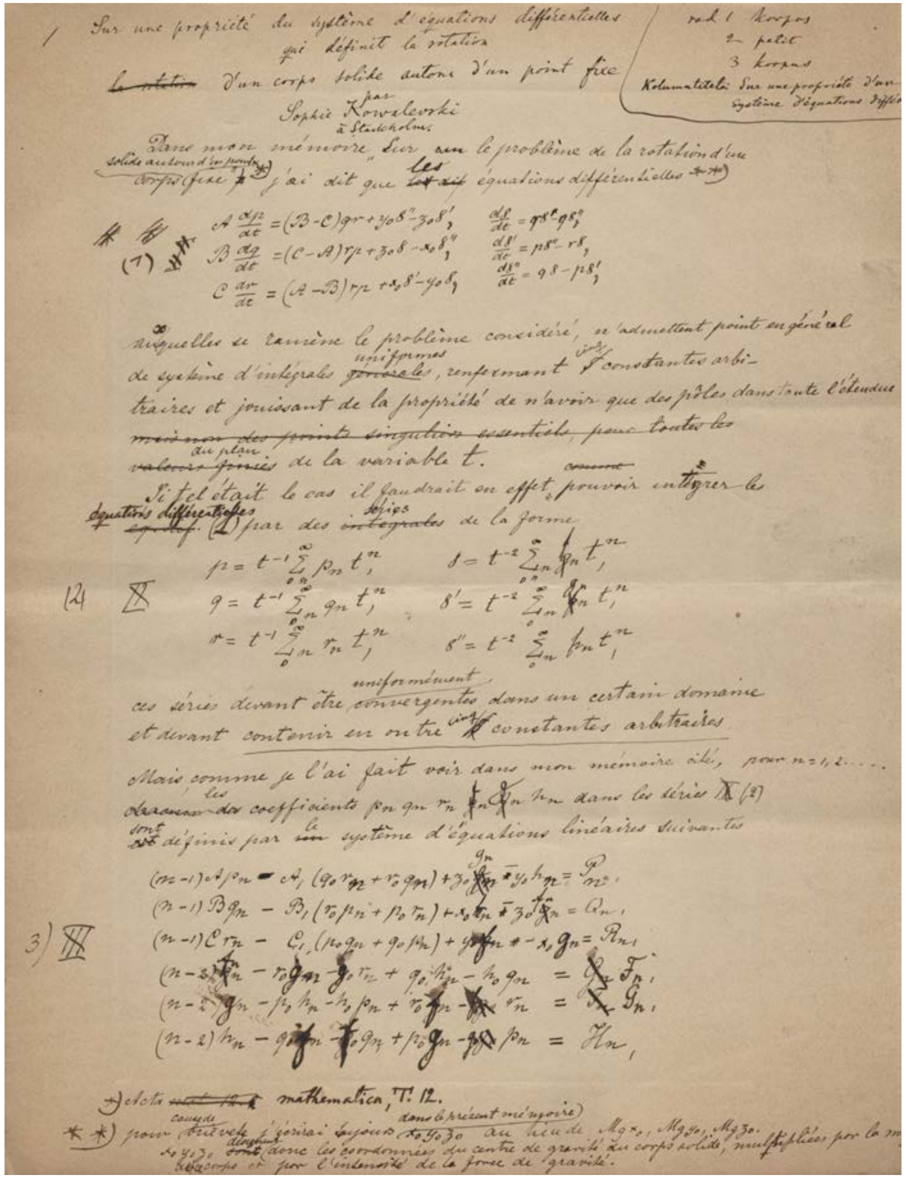
"Sonya and her sister Anyuta were part of a young people's movement to promote the emancipation of women in Russia. A favorite method of escaping from bondage was to arrange a marriage of convenience which would make it possible to study at a foreign university. Thus, at age eighteen, Sonya contracted 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 arc 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.



1 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 solide autour d'un point fixe (*) j'ai dit que les équations différentielles (**)

auxquelles se ramène le problème considéré, n'admettent point en général de système d'intégrales uniformes, renfermant cinq 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 (1) par des séries de la forme

$$p = t^{-1}\sum_0^\infty p_n t^n, \quad q = t^{-1}\sum_0^\infty q_n t^n, \quad r = t^{-1}\sum_0^\infty r_n t^n$$

ces séries devant être uniformément convergentes dans un certain domaine et devant contenir en outre cinq constantes arbitraires.

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

(*) Acta mathematica, T. 12.

(**) pour simplifier l'écriture j'écrirai toujours dans le présent mémoire x_0, y_0, z_0 au lieu de Mgx_0, Mgy_0, Mgz_0, désignant donc les coordonnées du centre de gravité du corps solide, multipliées par la masse du corps et par l'intensité de la force de gravité.

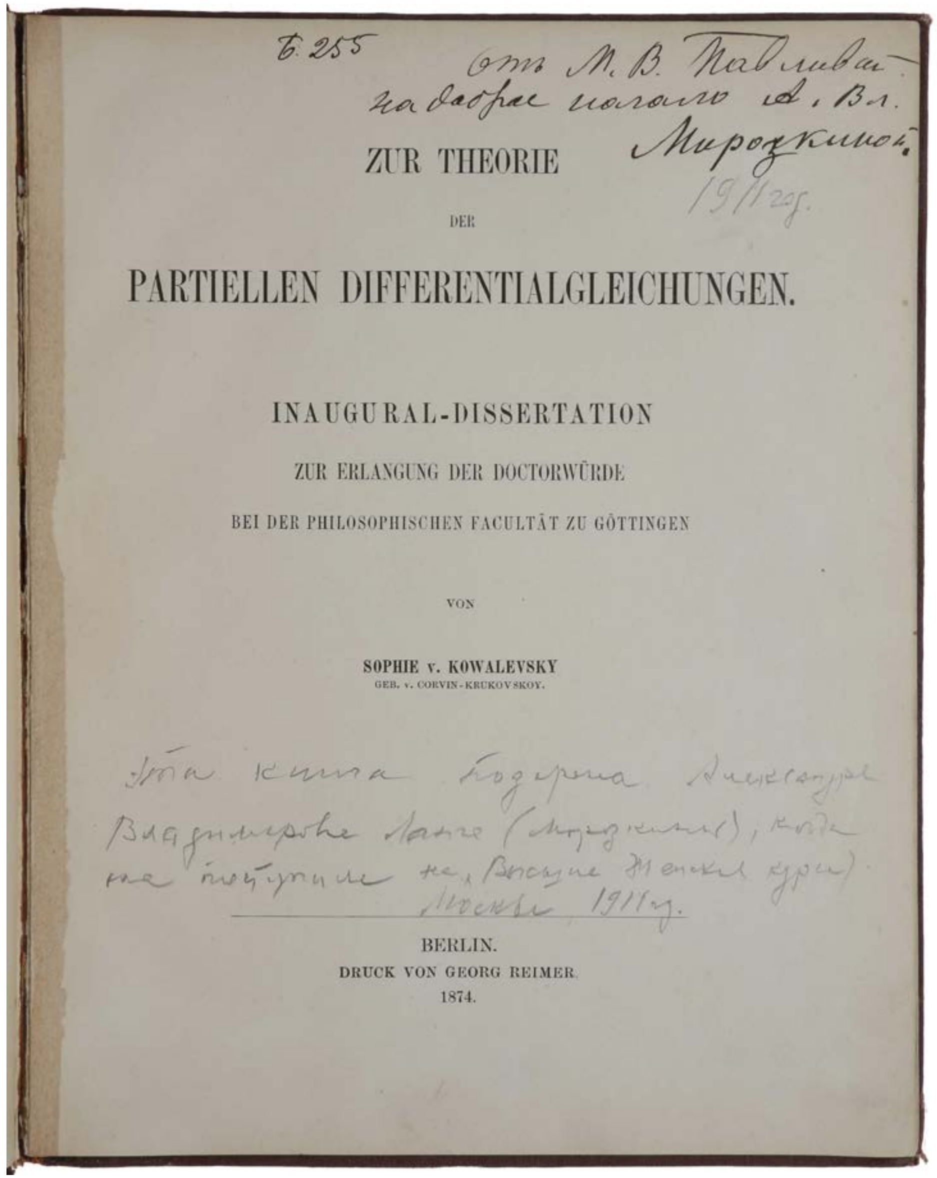
THE FIRST DOCTORIAL DISSERTATION IN MATHEMATICS BY A WOMAN

KOWALEVSKY, Sonya (or KOVALEVSKAYA, Sofya Vasilyevna). *Zur Theorie der partiellen Differentialgleichungen.* Berlin: Georg Reimer, 1874.

$9,500

4to (254 x 198 mm), pp [4], 32, contemporary half calf over blind-tooled cloth boards, front inner hinge week. A fine and fresh copy. Very rare.

First edition, rare, of the doctoral thesis of "the greatest woman mathematician prior to the twentieth century" (DSB). Kowalewsky was a pioneer for women in mathematics around the world – the first woman to obtain a doctorate (in the modern sense) in mathematics, the first woman appointed to a full professorship in Northern Europe and one of the first women to work for a scientific journal as an editor. According to historian of science Ann Hibner Koblitz, Kowalewsky was "the greatest known woman scientist before the twentieth century". The thesis contains what is now called the 'Cauchy-Kowalevsky' theorem on the existence of solutions of partial differential equations. It generalises a result obtained by the French mathematician Augustin-Louis Cauchy in 1842, although "it goes without saying that Kovalevskaya's work was completely independent of the work of Cauchy, even though there is some duplication. The important concept of normal form, which brings order to the whole topic, is due to Kovalevskaya, though probably inspired by [Carl Gustav Jacob] Jacobi" (Cooke, p. 35). Henri Poincaré said that "Kovalevsky significantly simplified the proof and gave the theorem its definitive form," and the German mathematician Karl Weierstrass, under whose supervision the thesis was written, wrote: "So you see, dear Sonya,


ZUR THEORIE
DER
PARTIELLEN DIFFERENTIALGLEICHUNGEN.
INAUGURAL-DISSERTATION
ZUR ERLANGUNG DER DOCTORWÜRDE
BEI DER PHILOSOPHISCHEN FACULTÄT ZU GÖTTINGEN
VON
SOPHIE v. KOWALEVSKY
GEB. v. CORVIN-KRUKOVSKOY.
BERLIN.
DRUCK VON GEORG REIMER.
1874.

that your observation (which seemed so simple to you) on the distinctive property of partial differential equations … was for me the starting point for interesting and very elucidating researches." In addition to being a brilliant mathematician, Kovalevskaya was also a political activist and a public advocate of feminism.

Provenance: Signed and inscribed on title page by Mariia Vasil'evna Pavlova (1854-1938) to Alexandra Mirozkina in 1911. Pavlova, a palaeontologist well known for her research on fossil hoofed-mammals, studied at the Sorbonne from 1880 to 1884, and from 1885 worked in Moscow University, initially as an unpaid volunteer in the geological museum. Shortly after the Russian Revolution she became a professor of palaeontology at Moscow University, later acting as the first female head of its department of palaeontology. She was an honorary member of the USSR Academy of Sciences, and academician of the Ukrainian Academy of Sciences. In 1926 the French Geological Society jointly awarded its prestigious Albert Gaudry Medal to her and her husband Aleksei Pavlov.

The simplest form of the Cauchy-Kowalevsky theorem states that any equation of the form $\partial z/\partial x = f(x, y, z, \partial z/\partial y)$, where the function f is analytic (has a convergent power series expansion) near a point (x_0, y_0, z_0, w_0) has an analytic solution $z(x, y)$ which is analytic near (x_0, y_0) and which is equal to a given analytic function $g(y)$ when $x = x_0$. Eighteenth-century mathematicians had developed a successful approach to solving such partial differential equations, which was called the 'method of undetermined coefficients.' This involved writing down a power series for the unknown solution f in which the coefficients of the powers of the variables were treated as unknown coefficients. Substitution in the equation to be solved then led to an infinite set of algebraic equations for the unknown coefficients. By solving these algebraic equations a solution f of the partial differential equation was found.

Eighteenth-century mathematicians used the method of undetermined coefficients in a formal way, without worrying about the convergence of the power series they obtained as a solution. The first person to worry in print about such convergence was Cauchy (1789-1857). In 'Mémoire sur l'emploi du calcul des limites dans l'intégration des équations aux dérivées partielles' (Comptes rendus, 1842), Cauchy used a technique which he called the 'calcul des limites', now called the 'method of majorants' in English, to establish that the power series obtained by the method of undetermined coefficients always converge. This was the first version of the 'Cauchy-Kowalevsky' theorem to be proved.

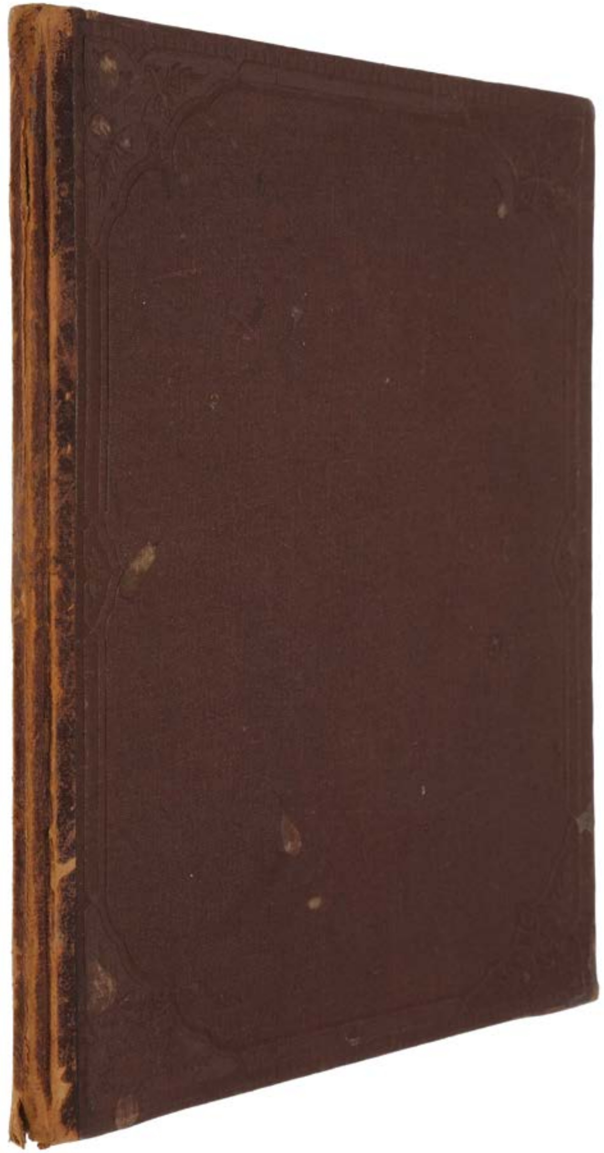
Weierstrass (1815-97) was working on problems similar to those which Cauchy studied at about the same time, apparently in ignorance of Cauchy's work. Weierstrass's work was not published until 1894, and was communicated only to his students at Berlin after 1857, one of whom was Kovalevskaya. Weierstrass pursued these questions because he wanted to use differential equations in order to define analytic functions, and so he naturally had to consider the question of the existence of solutions. "Apparently [Weierstrass] thought that a general theorem could be proved to the effect that a power series obtained formally from a partial differential equation in which only analytic functions occur would necessarily converge. He says in a letter of 25 September 1874 to [Paul] du Bois-Reymond that he had made such a conjecture. It is this conjecture which Kovalevskaya was evidently supposed to prove, if possible" (Cooke, p. 30). She succeeded completely. "In the general theorem, the simple case illustrated [above] is generalized to functions of more than two independent variables, to derivatives of order higher than the first, and to systems of equations" (DSB).

"Kovalevskaya worked the material into its final form in July 1874 and submitted it to the University of Gottingen as one of the dissertations for the doctoral degree. The following month she submitted it to the *Journal für die reine und angewandte*

Mathematik, where it appeared in 1875 [Bd. 80, pp. 1-32]. While the article was in press, Kovalevskaya returned to Russia. The final episode in the story is told in a letter from Weierstrass to Kovalevskaya dated 21 April 1875. Weierstrass says that he received the early 1875 issues of the *Comptes rendus* rather late, as a consequence of a delay in renewing his subscription. He was amazed to find in them two articles by [Jean Gaston] Darboux 'On the existence of an integral in differential equations containing an arbitrary number of functions and independent variables.' He continues, "So you see, my dear, that this question is one which is awaiting an answer, and I am very glad that my student was able to anticipate her rivals" … Weierstrass sent a copy of Kovalevskaya's dissertation to Darboux's mentor [Charles] Hermite in Paris and advised Kovalevskaya to ask [Carl] Borchardt, the editor of the *Journal für die reine und angewandte Mathematik*, to state in the journal that Kovalevskaya's article had been received in August 1874. His fears of a priority dispute were exaggerated, however. Hermite and Darboux became Kovalevskaya's closest friends and admirers in Paris and were later instrumental in promoting her participation in the Bordin Competition for 1888, in which she won international fame and 5,000 francs …

"Taken in the context of the time, Kovalevskaya's paper can be considered significant for at least three reasons. First, it gave systematic conditions which the method of undetermined coefficients must work. Second, it charted the terrain, so to speak, for the application of analytic function theory in differential equations, showing under what conditions a differential equation was likely to have an analytic solution. Third, it showed that a differential equation could be used as the definition of an analytic function, when taken together with certain initial conditions" (Cooke, pp. 34-6).

Kovalevskaya (1850-91) "was the daughter of Vasily Korvin-Krukovsky, an artillery general, and Yelizaveta Shubert, both well-educated members of the



in *absentia* from the University of Göttingen.

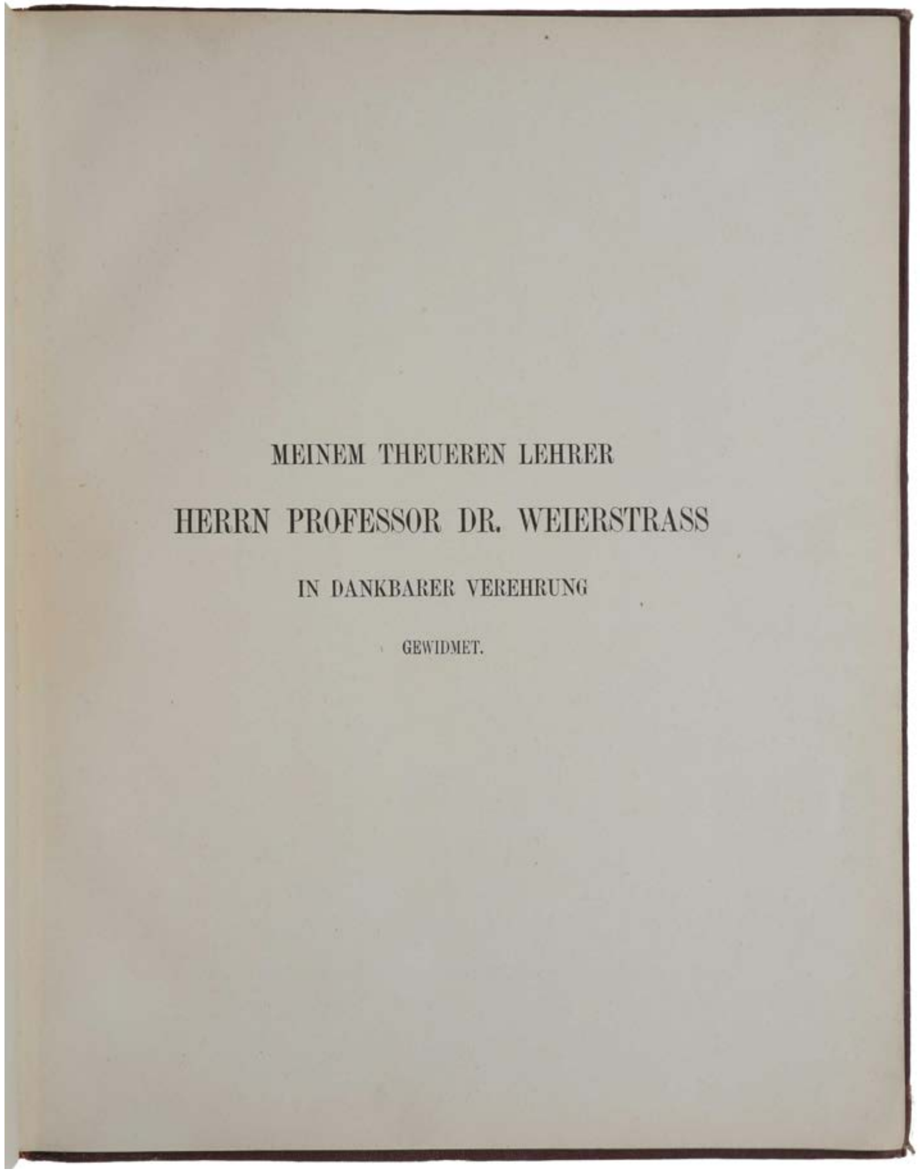
"In spite of Kovalevsky's doctorate and strong letters of recommendation from Weierstrass, she was unable to obtain an academic position anywhere in Europe. Hence she returned to Russia where she was reunited with her husband. The couple's only child, a daughter, "Foufie," was born in 1878. When Vladimir's 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 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 work, she attempted a simultaneous career in literature. The titles of some of her novels arc 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, *The Mathematics of Sonya Kovalevskaya*, Springer, 1984.



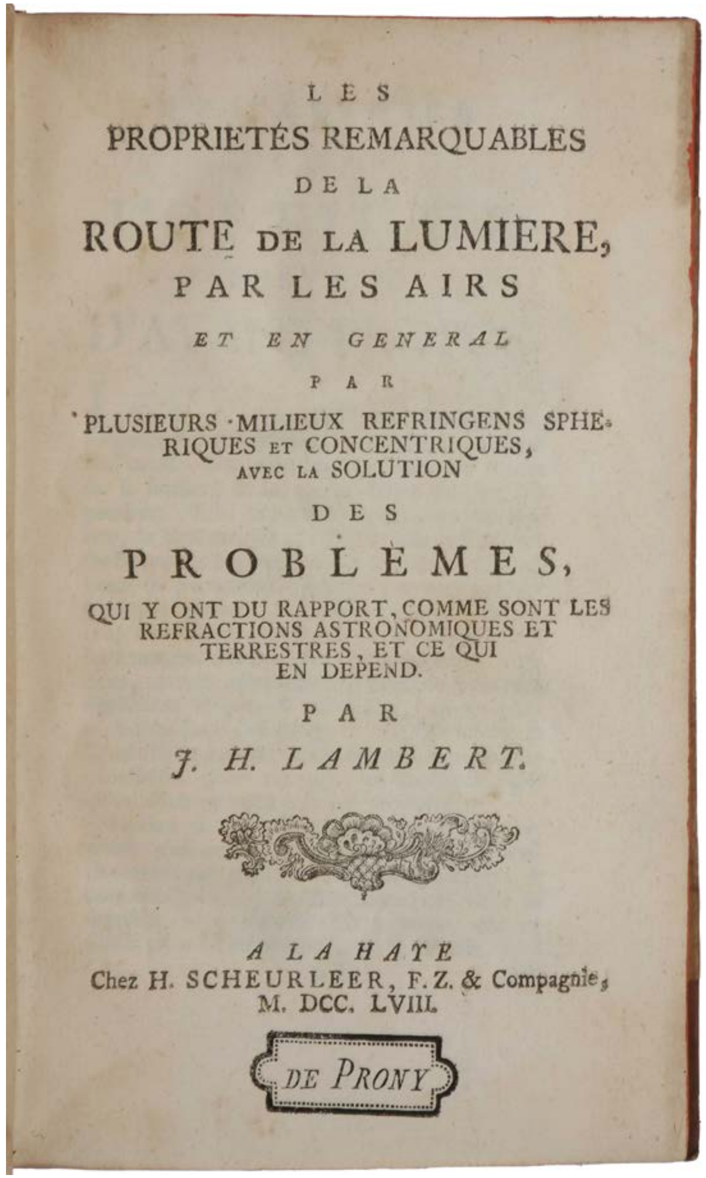
LAMBERT'S FIRST WORK ON LIGHT PUBLISHED TWO YEARS BEFORE THE PHOTOMETRIA

LAMBERT, Johann Heinrich. *Les propriétés remarquables de la route de la lumière par les airs et en général par plusieurs milieux réfringens sphériques et concentriques, avec la solution des problèmes qui y ont rapport...* The Hague: H. Scheurleer, 1758.

$15,000

8vo (196 x 117 mm), pp. 116 with two folding engraved plates. Contemporary calf, spine gilt with red lettering-piece, spine with some wear.

First edition, extremely rare, of Lambert's first published book, dealing with the path of light rays in air and other media. Lambert began working on aspects of refraction through the atmosphere in 1752, and from 1754 this work was carried out in parallel with his work on photometry. Lambert began the manuscript for the *Propriétés remarquables* in January 1758, and by March 1758 he had finished it and submitted it for publication in The Hague. The book is divided into three parts: 'Les propriétés générales de la route, qui la lumière prend, en passant par des milieux réfringens, sphériques et concentriques'; 'Des réfractions astronomiques, de la manière de les déterminer par approximation aussi exactement que l'on voudra, et de leur rapport à divers autres problèmes'; 'Des réfractions circulaires, de leur usage pour la détermination des réfractions terrestres: et de divers autres problèmes dépendant des réfractions tant astronomiques que terrestres'. It is remarkable that during the entire period 1756-58 Lambert was traveling



LES
PROPRIETÉS REMARQUABLES
DE LA
ROUTE DE LA LUMIÈRE,
PAR LES AIRS
ET EN GENERAL
PAR
PLUSIEURS MILIEUX REFRINGENS SPHE-
RIQUES ET CONCENTRIQUES,
AVEC LA SOLUTION
DES
PROBLÈMES,
QUI Y ONT DU RAPPORT, COMME SONT LES
REFRACTIONS ASTRONOMIQUES ET
TERRESTRES, ET CE QUI
EN DEPEND.
PAR
J. H. LAMBERT.

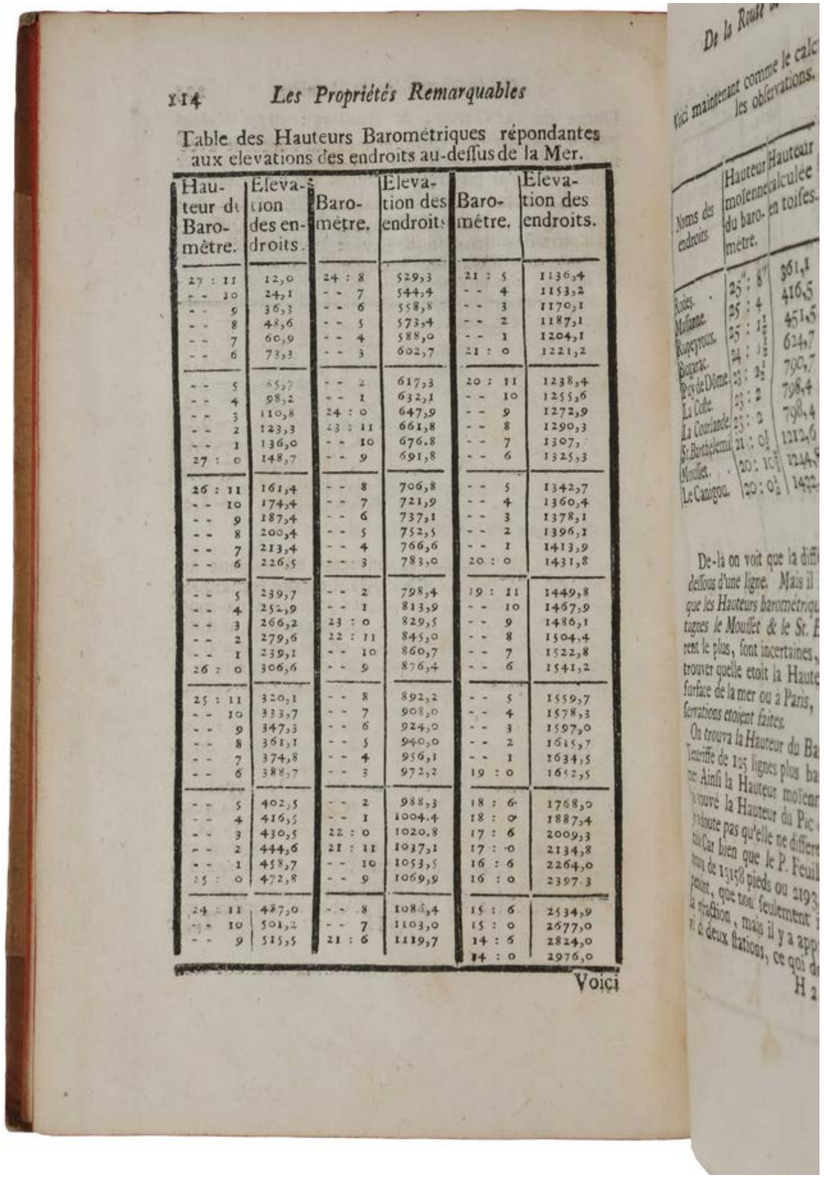
A LA HAYE
Chez H. SCHEURLEER, F. Z. & Compagnie,
M. DCC. LVIII.

DE PRONY

through northern Europe. He had with him a small library but also bought and read several additional optics books on his journey. In the *Propriétés remarquables* Lambert mentions that he is working on another work in optics and promises to publish it soon – this would eventually be his *Photometria*. In his preface to that work, Lambert notes that he has made good on his promise. The *Propriétés remarquables* was an immediate success, and was translated into German by Tempelhoff and published twice in Berlin in 1772 and 1773. This work, and his *Photometria*, undoubtedly influenced Euler, Gauss, Hamilton, Jacobi, Arago, and perhaps Fresnel and Cauchy. ABPC/RBH list no copies since the Andrade sale in 1965. COPAC lists only the Royal Society copy.

Provenance: title page with ex-libris of the great mathematician and engineer Gaspard de Prony (1755 – 1839).

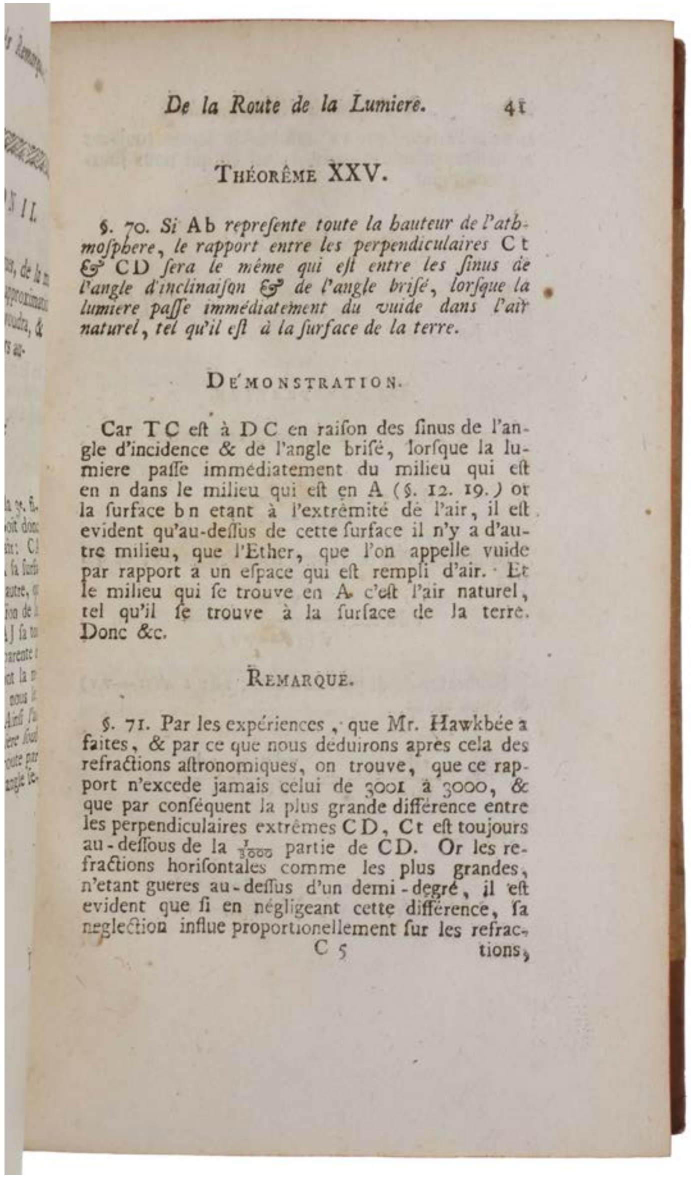
The history of the study of astronomical refraction to which the present work is devoted goes back to Tycho Brahe and Johannes Kepler. The first physical theories developed in this field are due to Thomas Simpson, who calculated the refraction in 1743 as a function of air density, and Edmund Halley, who calculated it as a function of pressure. In the foreword Lambert mentions Bouguer's *Essai d'Optique* (1729), Smith's *A Complete System of Optics* (1738), and Euler's 'Réflexion sur les différents degrés du soleil et dès autres corps célestes' (*Mémoires de l'Académie des Sciences de Berlin* 1752, pp. 280-302). However, Lambert's approach to the problem of atmospheric refraction differs entirely from Euler's. Euler attacked the physical problem in its entirety and also studied the hypotheses on the relation between temperature, pressure and density. Lambert, on the other hand, refrains from making such hypotheses and his merit is precisely to show how far we can go into this problem by using considerations independent of any physical hypothesis, because, at the time when



he wrote, scientists were far from a good understanding of thermodynamics.

Lambert undertook this work for practical applications – this was also the motivation for much of the work of many other contemporary scientists such as Euler: Lambert's theory of the refraction of light through the atmosphere served to correct astronomical observations and geodetic measurements. The organization of Lambert's book is determined by this requirement. Another characteristic feature of the work is the presentation of the theory in deductive form *more geometrico* in the Euclidean style: hypothesis, theorem, proof, corollary. Lambert limits himself to using only two 'experiments' as a starting point for his geometric reasoning. He refuses, moreover, to take into account physical relations between refraction and density. Lambert managed to avoid having to make use of such relations by assimilating the light ray to an arc of a circle whose radius he determines from observations. Although Lambert did not advance physical optics in this book, he did so two years later in the *Photometria*.

The behaviour of light rays at the boundary between two media of different refractive indices had been understood since Snel and Descartes, and is described by 'Snel's law'. In the atmosphere the refractive index of the air varies due to the varying temperature and pressure but this variation is continuous. Lambert's aim in the present work is to determine, using geometrical arguments, the path of light rays in a medium with continously varying refractive index. He treats the case of a system of concentric spherical layers. The principal result is Theorem 7: 'for any pair of points along the ray, the ratio between the normals which go from the centre of the spheres to the tangent of the ray is constant whatever the angle of incidence'. This result is known nowadays under the name of 'Bouguer's law' – we do not know if Lambert took it from Bougeur's 1729 work or discovered it independently. Lambert deduces from it Theorem 16, in which he establishes that


De la Route de la Lumiere. 41

THÉORÊME XXV.

§. 70. *Si* Ab *represente toute la hauteur de l'athmosphere, le rapport entre les perpendiculaires* C t *&* CD *sera le même qui est entre les sinus de l'angle d'inclinaison & de l'angle brisé, lorsque la lumiere passe immédiatement du vuide dans l'air naturel, tel qu'il est à la surface de la terre.*

DÉMONSTRATION.

Car T C est à D C en raison des sinus de l'angle d'incidence & de l'angle brisé, lorsque la lumiere passe immediatement du milieu qui est en n dans le milieu qui est en A (§. 12. 19.) or la surface b n etant à l'extrêmité de l'air, il est evident qu'au-dessus de cette surface il n'y a d'autre milieu, que l'Ether, que l'on appelle vuide par rapport à un espace qui est rempli d'air. Et le milieu qui se trouve en A c'est l'air naturel, tel qu'il se trouve à la surface de la terre. Donc &c.

REMARQUE.

§. 71. Par les expériences, que Mr. Hawkbée a faites, & par ce que nous deduirons aprés cela des refractions astronomiques, on trouve, que ce rapport n'excede jamais celui de 3001 à 3000, & que par conséquent la plus grande différence entre les perpendiculaires extrêmes CD, Ct est toujours au-dessous de la $\frac{1}{3000}$ partie de CD. Or les refractions horisontales comme les plus grandes, n'etant gueres au-dessus d'un demi-degré, il est evident que si en négligeant cette différence, sa neglection influe proportionellement sur les refrac-

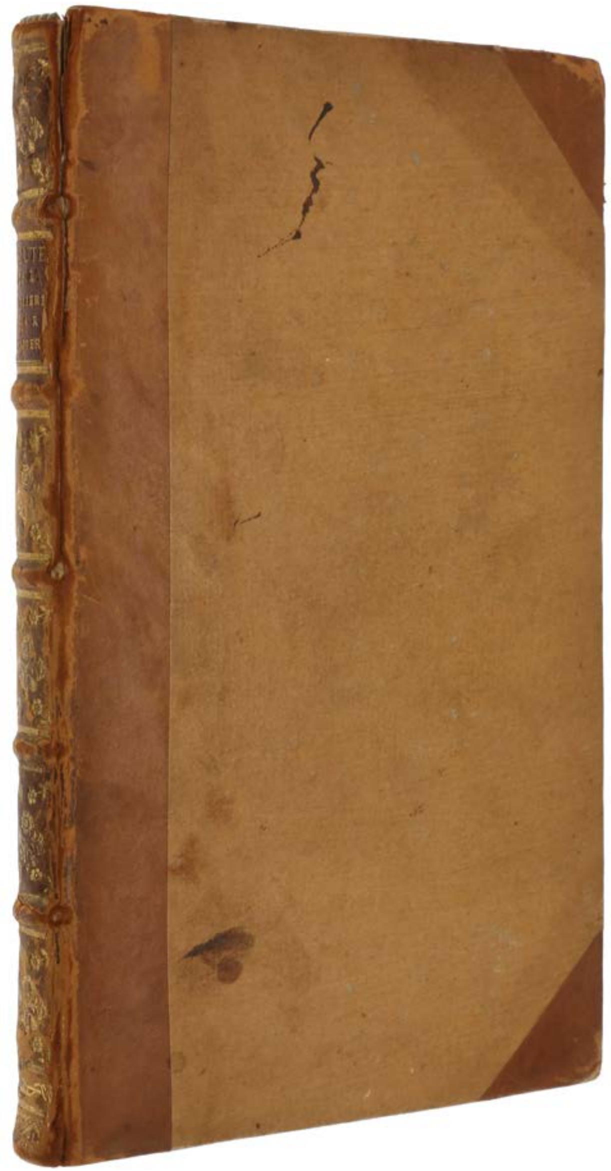
C 5 tions,

the locus of the centres of curvature of the trajectory of the ray lie on a straight line orthogonal to the line which passes through the centre of the spheres and the point where the ray enters the nonuniform medium (where the angle of incidence is defined). This theorem is, in the context of the optics of Hamilton-Jacobi, analogous to the conservation of angular momentum. It is used repeatedly in the sequel. In particular, Lambert shows that it can be applied to the atmosphere if one disregards the variation of the separation between the two media at the price of a minor error that decreases with the angle of incidence.

"But it is the applications that constitute the object of the second and third parts of the work. They are presented in the form of problems and to solve them Lambert once again resorts to the geometry of the circle. The starting point of his reasoning is the assimilation of the curve of the light rays to a circular arc, which is a hypothesis permitted in the particular case of atmospheric refraction. The discussion is especially detailed, and provides us with a vivid illustration of the scientific activities of the eighteenth century.

"The book testifies to the spirit of its time, using geometric methods to the very end, with the aim of reducing or eliminating the hypotheses. It would be false to say that the *Propriétés remarquables* lacks originality, but all things condidered, it is the pursuit of *classic* ideas through to their very last consequnces that characterizes the book" (Speiser & Williams, p. 247).

Speiser & Williams (eds.), *Discovering the Principles of Mechanics 1600-1800*, 2008.



AN EXCEPTIONAL COPY IN ORIGINAL WRAPPERS COMPLETE WITH ALL SUPPLEMENTS

LAPLACE, Pierre-Simon, Marquis de. *Traité de mécanique céleste.* Paris: Crapelet for Duprat, An VII [1799] [Vols. I-II]; Crapelet for Duprat, An XI–1802 [Vol. III]; Courcier, An XIII–1805 [Vol. IV]; Bachelier, 1825 [i.e., 1823-1827] [Vol. V].

$35,000

Five vols. & four supplements bound in six vols. (Supplement to Vol. V bound separately), large 4to (275 x 211mm), pp. [i-v], vi-xxxii (errata on xxxi-xxxii), [1], 2-368 [Vol. I]; [iv], [1], 2-382 [Vol. II]; [i-vii], viii-xxiv, [1], 2-303, [1, errata], [1], 2-24 [Vol. III and first Supplement]; [i-v], vi-xl, [1], 2-347, [1, errata], [2, half-title], [1], 2-65, [1, blank], [1], 2-78, [2, index and errata for both supplements], with one folding engraved plate [Vol. IV and second and third Supplements]; [i-v], vi-viii, [1], 2-419, [1, errata] [Vol. V]; [ii, half-title], [1], 2-35 [fourth Supplement]. Original publisher's magenta paste-paper wrappers with original printed spine labels, uncut and almost entirely unopened (spines sunned, wear to extremities, about half of spine of Vol. V missing but label intact, fraying with minor loss to lower edge of front cover of Vol. III and damp-staining to lower margin of first few leaves, spine label of Vol. IV with minor loss to printed border but not text). Despite these minor faults an attractive set in original condition, and extremely rare thus.

First edition, complete with all the supplements, of this monumental work. This is the only complete copy of the first edition we have seen, or seen described, that is uncut in the original publisher's wrappers (with the original printed paper

spine labels) – with the possible exception of the Grolier/Horblit copy (described as 'uncut' but binding unspecified). The *Mécanique céleste* is the foundation of modern theoretical astronomy, termed 'the eighteenth-century Almagest' and 'a sequel to Newton's *Principia*' by Grolier/Horblit. Here, Laplace, rightly called the 'Newton of France' (*En Français dans le Texte*), codified and developed the theories and achievements of Newton, Euler, d'Alembert, and Lagrange, producing "a *locus classicus* for celestial mechanics on a scale unmatched since Newton, and also a valuable source for a cluster of important mathematical theories and methods" (Grattan-Guinness, p. 256). "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). Volumes I and II of Laplace's *Traité* constitute a general theoretical basis for mathematical astronomy. Volume I comprises Book 1, 'On the general laws of equilibrium and motion' and Book 2, 'On the law of universal gravitation, and the motions of the centres of gravity of the heavenly bodies'; Volume II, in three books, treats the shapes of celestial bodies and their motion around their centres of gravity, and the tides. Volume III, in two books, deals with the theory of the Moon, and Volume IV treats the motion of the moons of Jupiter, Saturn and Uranus, and the theory of comets. The final volume elaborates on some topics covered earlier, but also treats several topics in physics only tangentially connected to astronomy, such as heat diffusion and the velocity of sound (capillarity had already been discussed in the two supplements to Volume IV). An exceptional copy.

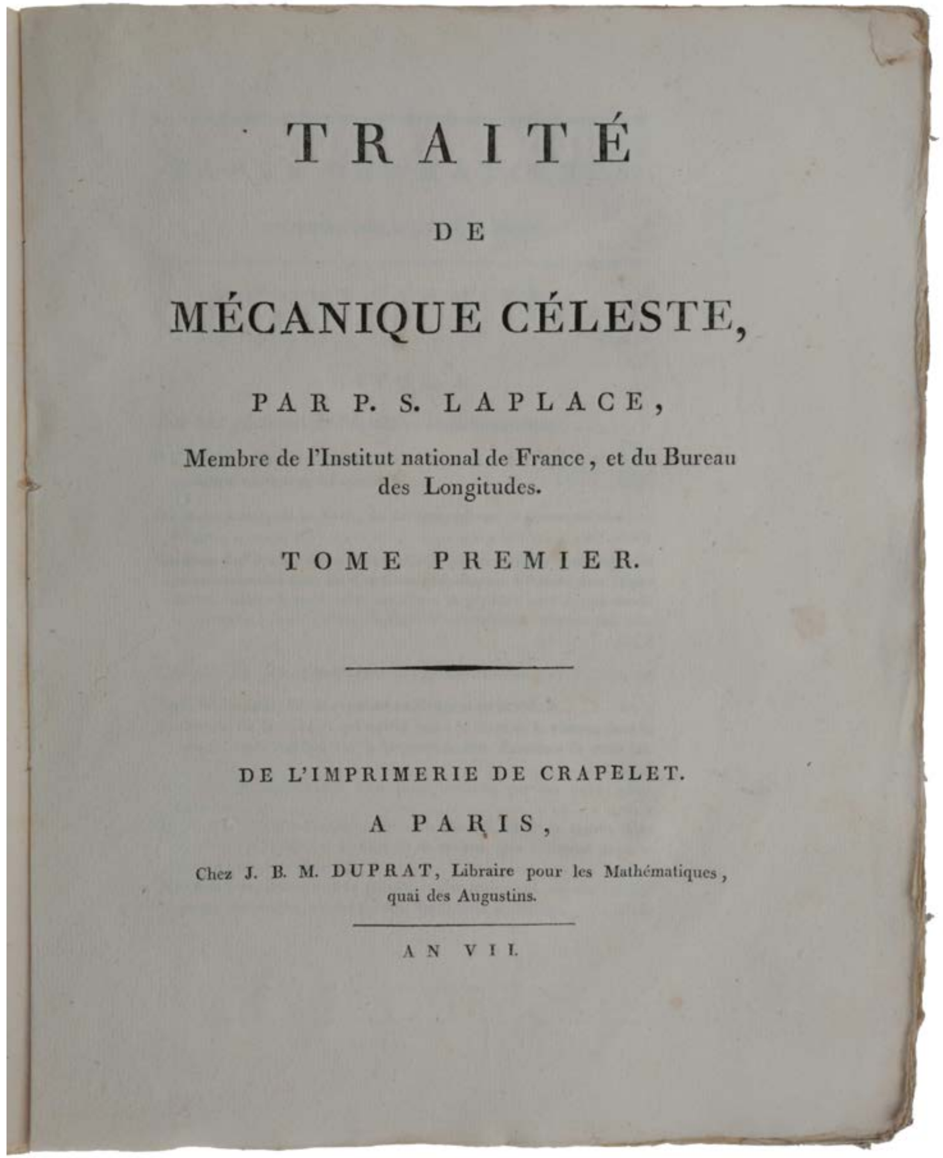
Provenance: From the library of Marchese Giulio Stanga Carlo Trecco (1794-1852), who had a passion for mathematics and physics and owned the most important private collection of scientific books and instruments of the Lombardo-Venetian Kingdom of his time. His books have not previously been on the market.

Laplace (1749–1827) exhibited his mathematical powers early; having moved to Paris from his home in Normandy in his teens, by the age of 24 he had already been elected to the *Académie des Sciences*. Mathematical astronomy soon became his dominant concern, notably "the fine details of the motions of the heavenly bodies as analysed using especially Newton's laws and allowing for perturbations; and in planetary mechanics the analysis of their shapes, especially that of the Earth following the demonstration of its oblateness in the 1740s, and consequent topics such as the motion of the sea and of tides, and the analysis of projectiles. The main tool was the calculus, including series, functions, and difference and differential equations, which themselves were importantly advanced … Laplace gained status when the *Bureau des Longitudes* was formed in 1795 as the national organisation to assist practical astronomy and navigation; for he was unofficially its leader until his death … In his late forties, Laplace seems to have felt ready to emulate Lagrange in writing an authoritative account, in his case of mathematical astronomy together with some new methods in the calculus" (Grattan-Guinness, pp. 243-4).

The first two volumes of the *Mécanique céleste* appeared in 1799. "The importance of the publication was expressed by publisher Duprat in a most singular way: all the sheets of paper used carried 'MECANIQUE CELESTE' as their watermark on the bottom" (*ibid*., p. 246). "In October 1799, three weeks before the coup d'état of 9 November that brought Napoleon to power as first consul, Laplace presented him with copies of the first two volumes of *Mécanique céleste*. The acknowledgement is famous. Bonaparte promised to read them 'in the six months I have free' and invited Laplace and his wife to dine the next day, 'if you have nothing better to do'" (DSB). Later, Napoleon is supposed to have remarked: "You have written this huge book on the system of the world without once mentioning the author of the universe," to which Laplace replied: "Sire, I had no need of that hypothesis" (quoted in Augustus De Morgan's *Budget of Paradoxes*).

There were no diagrams, except in the first Supplement to vol. IV. "By contrast, mathematics was everywhere, often fearfully: Nathaniel Bowditch [who translated and explicated the first four volumes] memorably remarked that 'Whenever I meet in La Place with the words 'Thus it plainly appears', I am sure that hours, and perhaps days, of hard study will alone enable me to discover *how* it plainly appears'. Unlike the earlier work [*Exposition du Système du Monde*, 1796], very few references were provided; Isaac Todhunter wittily surmised that Laplace 'supposed the erudition of his contemporaries would be sufficient to prevent them from ascribing to himself more than was justly due'" (*ibid.*). Many data were given, often calculated by Aléxis Bouvard, Laplace's assistant at the *Bureau*.

"Book 1 is a mathematical exposition of the laws of statics and dynamics in a development adapted to the formulation of astronomical problems … The sequence was canonical; first the statics and dynamics of mass points, second of systems of bodies, and third of fluids; the point of view is d'Alembert's. Dynamical laws are derived from equilibrium conditions. Apart from the motivation, only two features appear to be distinctively Laplacian. In chapter 5, which is concerned with the general principles of mechanics, Laplace incorporated his concept of an



TRAITÉ

DE

MÉCANIQUE CÉLESTE,

PAR P. S. LAPLACE,

Membre de l'Institut national de France, et du Bureau des Longitudes.

TOME PREMIER.

DE L'IMPRIMERIE DE CRAPELET.

A PARIS,

Chez J. B. M. DUPRAT, Libraire pour les Mathématiques, quai des Augustins.

AN VII.

invariant plane into the discussion of the principle of conservation of areas … the reference plane is perpendicular to the total angular momentum vector of the system … in *Mécanique céleste,* he moved the origin of coordinates from the sun to the center of the earth, no doubt because in practice astronomers refer their observations of the motion of celestial bodies to the plane of the earth's orbit. The second feature that one would not expect to find in a textbook of rational mechanics is the discussion in chapter 6 of the laws of motion of a system of bodies given any mathematically possible hypothesis concerning the relation of force to velocity" (DSB).

"Book 2 was mainly devoted to the motions of the planets about the Sun; Laplace started with two-body problems, yielding elliptical orbits for planets though more complicated for comets (2#16–39). Then as the 'second approximation' he considered the inter-planetary perturbations and analysed their 'secular inequalities' (that is, the perturbations which did not depend upon the mutual configuration of the relevant heavenly bodies). Although the configuration required only trigonometry in the invariable plane for expression, Newton's inverse square law caused some horrible expressions in the astronomical variables; so in the late 1740s Euler had made the wonderful simplification of converting them into infinite trigonometric series in multiples of the relevant angles. (They resemble Fourier series but have a quite different theory.) This procedure became normal especially for French astronomy, with Lagrange and then Laplace, who gave the basic details in 2#46–52. One ground for Laplace's support of them seem to have been his belief that periodic forces produce periodic effects (explicitly in 13#1), and therefore needed periodic functions in the mathematics. To solve the system of associated differential equations he often deployed a method of 'successive approximations' (2#40–45).

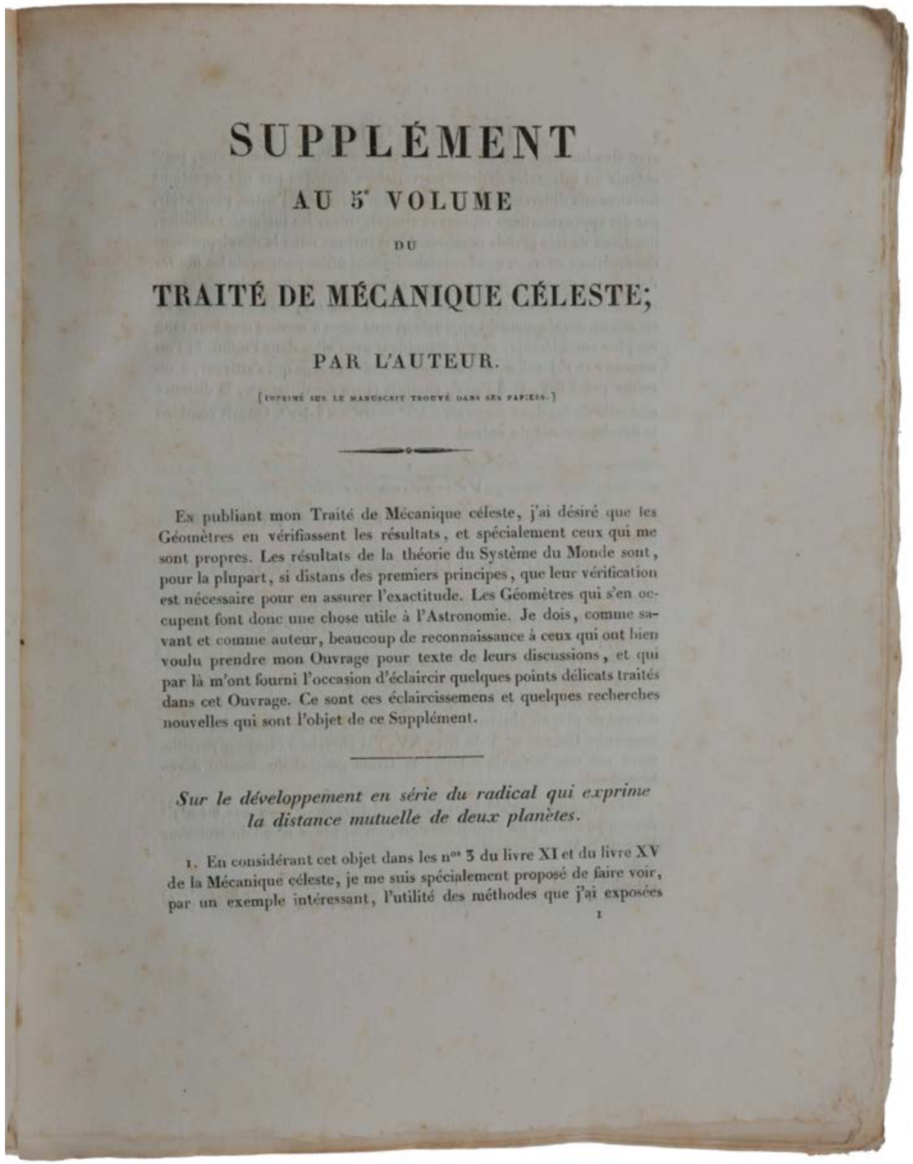
"A further major question was to prove that the planetary system was stable, which then meant that the eccentricities and inclinations of the orbit of each planet were strictly bounded, to that it would neither go out of finite bounds within the ecliptic nor fly out of that plane. By a brilliant transformation of variables Lagrange had tackled this problem in 1778 as the motion of many point-masses; modifying the analysis somewhat to fit the planetary system, Laplace summarised the findings in 2#55–62 … Another important perturbation was the apparent resonance in the mean motions of Jupiter and Saturn, and of three of Jupiter's satellites. Pioneering the analysis of perturbation terms in powers of eccentricity and/or inclination, Laplace's studies of the mid-1780s had been among his early successes, and he summarised his and Lagrange's findings in 1#65–72.

"In Book 3 Laplace turned to questions concerning the shape of the planets, especially the Earth. He took the potential of body [in the gravitational field due to an attracting point], showed that it satisfied Laplace's equation, and when set in spherical polar coordinates he solved it by spherical harmonics. The language here is modern, and Laplace's presentation (in other terms) is familiar (3#1–17) apart from the names (not even Laplace would have referred to 'Laplace's equation'!); but in fact it was an *early* account of the theory, which he had done much to develop since the 1770s … Laplace used the power-series expansion (assumed to be convergent) and the generating function, orthogonality, and the expansion of 'any' function in an infinite series of the functions …

"For mechanics Laplace naturally focused upon the nearly spherical spheroid, and handled its ellipticity by means similar to his method of successive approximations (3#33). His first application was to determine the 'figure' of a homogeneous fluid of constant thickness covering it and rotating in equilibrium (3#22–37) … To compare his findings with available data for the Earth, Laplace gave statistics a rare airing in the *Mécanique céleste,* refining earlier studies by R.J. Boscovich (3#39–40) … Book 4 was devoted to the closely related topic of sea-flow, where in earlier work Laplace had pioneered a dynamic analysis, with trigonometric series well to the fore. It rested on distinguishing three different periodicities: one monthly and partly annual, and due to the orbit of the Earth; one diurnal, and caused by its rotation; and one semi-diurnal, largely blamed upon the Moon (4#5–9). Comparison with data again led to discrepancy, especially for the port of Brest, which had been well studied for the length and heights of its tides (4#23); but he discussed in detail the difference between tides in syzygy and in quadrature with Sun and Moon (4#22–42).

"In Book 5 of this volume Laplace briefly analysed lunar librations and the motions of its nodes (5#15–19). The main attack on the Moon would come later; the motivation here was to consider effects of the rotation of a body about its centre of gravity, the subject of this Book. He followed with the potential of the 'ring' of Saturn. Relying upon observations that claimed it actually to be two concentric rings, he concluded that each one was a ellipse with small thickness rotating at its own angular velocity, and that its material was not distributed uniformly so that its centre of gravity did not coincide with that of the planet (3#44–46). The latter finding led him later in the volume to analyse the motion of the rings about their centres (5#20–22). Finally here he analysed the (assumedly solid) Earth, where he handled the precession and nutation of the polar axis by means of Euler's equations for the rotation of such a body about a point; he deduced that the effects of the sea as a stratum, and of winds, could be ignored (5#12–14).

"The last two volumes [sic] appeared in 1802 and 1805, constituting the 'Second Part' of the work. Laplace dealt with the 'Particular theories' of the motions after the generalities and principles just expounded; but the final Book 10 dealt with



SUPPLÉMENT

AU 5e VOLUME

DU

TRAITÉ DE MÉCANIQUE CÉLESTE;

PAR L'AUTEUR.

[IMPRIMÉ SUR LE MANUSCRIT TROUVÉ DANS SES PAPIERS.]

En publiant mon Traité de Mécanique céleste, j'ai désiré que les Géomètres en vérifiassent les résultats, et spécialement ceux qui me sont propres. Les résultats de la théorie du Système du Monde sont, pour la plupart, si distans des premiers principes, que leur vérification est nécessaire pour en assurer l'exactitude. Les Géomètres qui s'en occupent font donc une chose utile à l'Astronomie. Je dois, comme savant et comme auteur, beaucoup de reconnaissance à ceux qui ont bien voulu prendre mon Ouvrage pour texte de leurs discussions, et qui par là m'ont fourni l'occasion d'éclaircir quelques points délicats traités dans cet Ouvrage. Ce sont ces éclaircissemens et quelques recherches nouvelles qui sont l'objet de ce Supplément.

Sur le développement en série du radical qui exprime la distance mutuelle de deux planètes.

1. En considérant cet objet dans les nos 3 du livre XI et du livre XV de la Mécanique céleste, je me suis spécialement proposé de faire voir, par un exemple intéressant, l'utilité des méthodes que j'ai exposées

1

various other topics … The largest single theoretical effort was given over to lunar theory, to which Book 7 was devoted. Various methods had been introduced during the 18th century to analyse the many perceived perturbations of this nearby object; Laplace principally favoured one due to d'Alembert in which time was set as a function of the Moon's true longitude in the ecliptic, not vice versa. The equations took the form of integral–differential, then unusual (7#1); after finding solutions Laplace desimplified them by introducing knowingly neglected factors such as the effect of the action of the Sun and of the Earth's eccentricity upon the Moon's secular acceleration (7#10, 16). In his analysis of lunar parallax he allowed for the oblateness of the Earth (7#20–21). He claimed good correspondence with certain observational data, such as the lunar perigee (7#16).

"Another subject of especial difficulty was the theory of comets. In Book 2 Laplace had conducted a preliminary analysis in which all conic sections were permitted as paths (2#23); now in Book 9 he again approximated by taking the path to be nearly elliptic and using generating functions to effect quadratures … Some of the material here could have been presented earlier, and may have constituted afterthoughts or late news. For example, Laplace analysed the path of a projectile falling to Earth from a great height; a striking feature of this use of Newton's second law is his allowance for the rotation of the Earth, where he included components of the force named now after his successor G.G. Coriolis (10#15–16). One motivation for this excursion into the stratosphere may have been recent French experience of meteorites; Laplace had wondered if they were rocks detached from the Moon, and around 1800 Biot and S.D. Poisson (1781–1840) had examined the consequences. Another speculation concerned one of Newton's greatest mysteries: *how* does that gravitational force pass between bodies? Laplace presumed that 'the successive transmission of gravity' was carried by an elastic aether, and thereby analysable by the usual equations; by making assumptions about the (minute) loss of mass by the Sun caused by the attractions, he found the velocity to be 'about seven millions of times greater than that of light' (10#22).

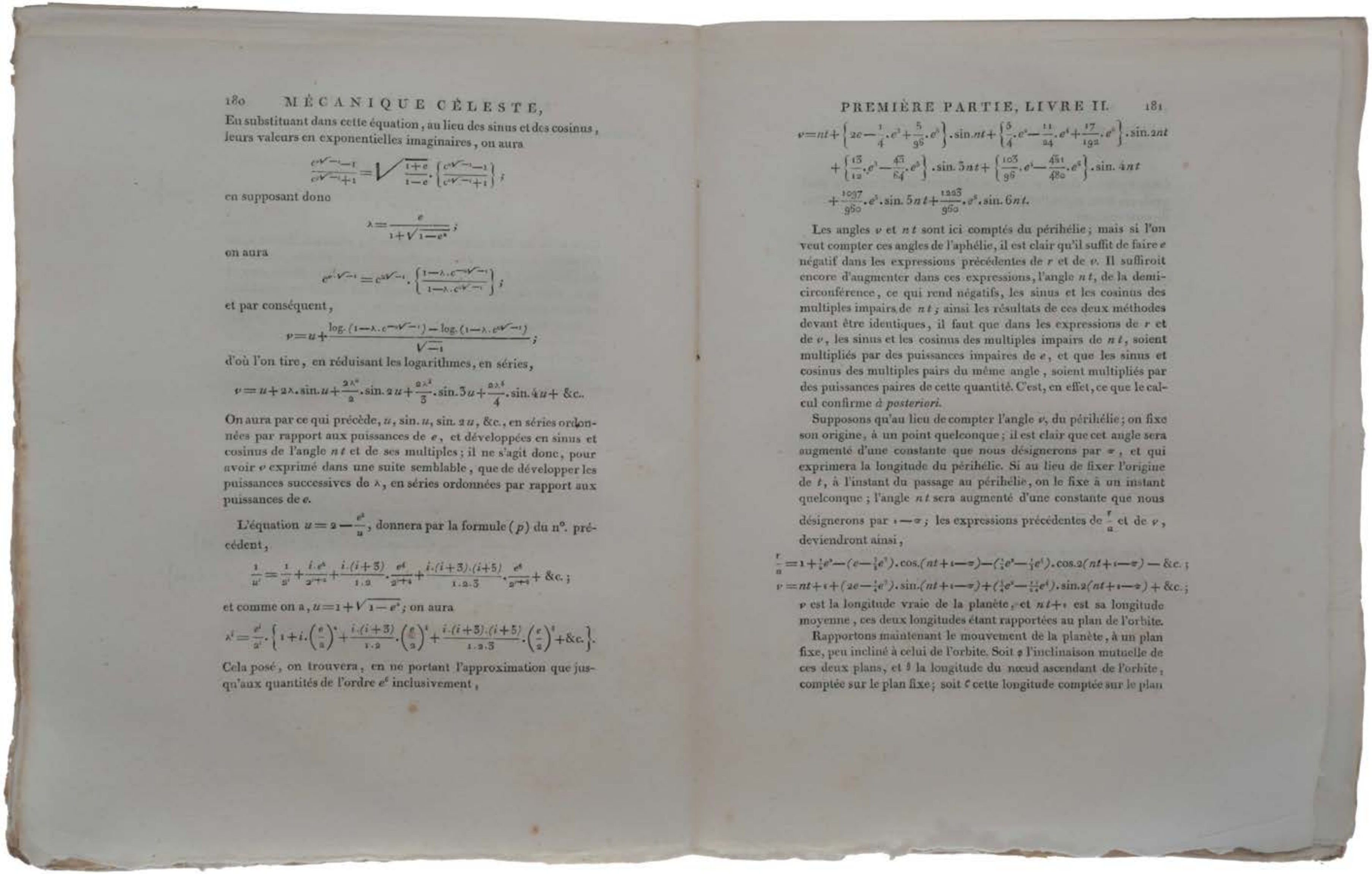
"The major feature of this Book was its attention to physics. This analysis of gravitation involved light from the Sun, and the first and largest part of the Book was an analysis of atmospheric refraction, an exercise in small effects partly motivated by the ever-improving accuracy of astronomical instruments. Adopting a form of Newton's optics, that light was composed of tiny fast-moving bullets, Laplace construed refraction to be caused by interaction with the molecular constituents of the atmosphere by central forces … However, much greater ambition attended Laplace's analysis of the use of the barometer. In the Book he only related atmospheric pressure to density (10#14); but soon afterwards he subjected the analysis of the meniscus to an intense molecularist analysis. It appeared in various papers and especially two supplements to this volume, published in 1806 and 1807 and at 145 pages longer than several of the Books … In the first supplement Laplace explained the meniscus in the capillary tubes of barometers and thermometers in terms of action between the molecules of the fluid contained therein and those in the surrounding glass; in particular, the closer the fluid to the glass, the stronger the attracting force … He also analysed the shape of fluid trapped between two glass tubes or between planes (supp.1#6–8) … In the second supplement he re-derived the basic differential equation, considered the capacity of the surface and fluid to bear weight, and especially studied the shape of a blob of mercury in equilibrium on a horizontal plane …

"Laplace's interest in celestial mechanics revived in the 1820s, especially when he published the fifth and final volume of the *Mécanique céleste* in six *fascicules* between 1823 and 1825, with a posthumous supplement in 1827 … he elaborated upon a wide variety of the concerns of the earlier volumes … The interest in heat diffusion showed in a discussion of the cooling and the age of the Earth (11#9–10); and in the velocity of sound, which for Laplace depended upon the specific heats of air (12#7). His increased involvement with probability and mathematical statistics is evident in several forays: for example, the tides (13#5), and the probability of the existence of a 'lunar atmospheric tide' (13#ch.6; and supp#5, his last piece of work). He also fulfilled an aim manifest already in the *Exposition* of writing at some length on the history of astronomy" (Grattan-Guinness, pp. 249-254).

As Norman remarks, "The bibliographical makeup of *Mécanique céleste* is among the most complex of science classics." Our copy collates as the Norman copy, except that in the Norman copy Vol. V has an 'Avertissement' leaf at the beginning and part-titles to each of the Books XI-XV, dated March 1823, April 1823, February 1824, July 1824, and December 1824, respectively; these are not present in our copy. The 'Avertissement' explains that, as Vol. V is appearing many years after the first four, the individual books will be issued as soon as they are ready, each with their own part-titles (though in fact the final Book XVI was not issued with a part-title). These part-titles (and the 'Avertissement') would, of course, have been superfluous in copies of Vol. V issued by the publisher from 1825. One would therefore expect that they would be absent from complete copies of Vol. V in the original publisher's binding, and present only in copies that had been assembled from the individual books and then rebound. This is consistent with their absence from our copy and their presence in the Norman copy – although the first four volumes of that copy were in the publisher's binding, Vol. V was rebound. The Grolier/Horblit copy, described as 'uncut', also lacked these part-titles. A further variant is that a few copies of the second supplement to Vol. IV have a separate title with imprint, not present in our copy or in the Grolier/Horblit or Norman copies. The imprint has date 1807, two years after Vol. IV was published, so again one would expect that copies of Vol. IV sold by the publisher after both supplements were printed would not require the separate title. The supplement to Vol. V, which in our copy is bound separately in publisher's wrappers, has a part-title dated 1827 (but without imprint), as in all copies. It must in fact have been issued in the latter part of 1827, as in our copy it is bound with a printed catalogue of Bachelier's publications dated July 1827 (we have not found this catalogue in any other copy of the book). Laplace had died on 5 March.

There is a second issue of Vols. I-II, dated '1799' with the additional imprint reading 'Berlin: chez F. T. de la Garde, Libraire.' Vols. I-IV were reprinted by Bachelier in 1829-39, and all five volumes were reprinted in the *Oeuvres*, 1843-6, and in the *Oeuvres complètes*, 1878-82. Nathaniel Bowditch published an English translation of Vols. I-IV, with extensive commentary, in 1829-39 (and separate English translations of Book 1, and of Vol. I, were published earlier), but Vol. V has never been translated.

PMM 252; Dibner, *Heralds of Science*, 14; D.S.B., XV, pp. 273-403; *En Français dans le Texte* 201 ("Le *Traité de mécanique celeste* de Laplace est sans aucun doute, avec le *Mécanique analytique* de Lagrange et *Les Méthodes nouvelles de la Mécanique céleste* de Poincaré, l'un des ouvrages les plus importants parus depuis les *Principia* de Newton"); Grolier/Horblit 63; Roberts & Trent, *Bibliotheca Mechanica*, pp. 197-98; Sparrow, *Milestones of Science*, 125. Grattan-Guinness, 'P. S. Laplace, *Exposition du Système du Monde*, first edition (1796); *Traité de mécanique celeste* (1799-1823/1827),' Chap. 18 in *Landmark Writings in Western Mathematics 1640-1940*, Grattan-Guinness (ed.), 2005.



En substituant dans cette équation, au lieu des sinus et des cosinus, leurs valeurs en exponentielles imaginaires, on aura

$$\frac{c^{v\sqrt{-1}}-1}{c^{v\sqrt{-1}}+1}=\sqrt{\frac{1+e}{1-e}}\cdot\left\{\frac{c^{u\sqrt{-1}}-1}{c^{u\sqrt{-1}}+1}\right\};$$

en supposant donc

$$\lambda=\frac{e}{1+\sqrt{1-e^2}};$$

on aura

$$c^{v\sqrt{-1}}=c^{u\sqrt{-1}}\cdot\left\{\frac{1-\lambda.c^{-u\sqrt{-1}}}{1-\lambda.c^{u\sqrt{-1}}}\right\};$$

et par conséquent,

$$v=u+\frac{\log.(1-\lambda.c^{-u\sqrt{-1}})-\log.(1-\lambda.c^{u\sqrt{-1}})}{\sqrt{-1}};$$

d'où l'on tire, en réduisant les logarithmes, en séries,

$$v=u+2\lambda.\sin.u+\frac{2\lambda^2}{2}.\sin.2u+\frac{2\lambda^3}{3}.\sin.3u+\frac{2\lambda^4}{4}.\sin.4u+\&c.$$

On aura par ce qui précède, u, $\sin.u$, $\sin.2u$, &c., en séries ordonnées par rapport aux puissances de e, et développées en sinus et cosinus de l'angle nt et de ses multiples; il ne s'agit donc, pour avoir v exprimé dans une suite semblable, que de développer les puissances successives de λ, en séries ordonnées par rapport aux puissances de e.

L'équation $u=2-\frac{e^2}{u}$, donnera par la formule (p) du n°. précédent,

$$\frac{1}{u^i}=\frac{1}{2^i}+\frac{i.e^2}{2^{i+2}}+\frac{i.(i+3)}{1.2}.\frac{e^4}{2^{i+4}}+\frac{i.(i+3).(i+5)}{1.2.3}.\frac{e^6}{2^{i+6}}+\&c.;$$

et comme on a, $u=1+\sqrt{1-e^2}$; on aura

$$\lambda^i=\frac{e^i}{2^i}.\left\{1+i.\left(\frac{e}{2}\right)^2+\frac{i.(i+3)}{1.2}.\left(\frac{e}{2}\right)^4+\frac{i.(i+3).(i+5)}{1.2.3}.\left(\frac{e}{2}\right)^6+\&c.\right\}.$$

Cela posé, on trouvera, en ne portant l'approximation que jusqu'aux quantités de l'ordre e^6 inclusivement,

$$v=nt+\left\{2e-\frac{1}{4}.e^3+\frac{5}{96}.e^5\right\}.\sin.nt+\left\{\frac{5}{4}.e^2-\frac{11}{24}.e^4+\frac{17}{192}.e^6\right\}.\sin.2nt$$
$$+\left\{\frac{13}{12}.e^3-\frac{43}{64}.e^5\right\}.\sin.3nt+\left\{\frac{103}{96}.e^4-\frac{451}{480}.e^6\right\}.\sin.4nt$$
$$+\frac{1097}{960}.e^5.\sin.5nt+\frac{1223}{960}.e^6.\sin.6nt.$$

Les angles v et nt sont ici comptés du périhélie; mais si l'on veut compter ces angles de l'aphélie, il est clair qu'il suffit de faire e négatif dans les expressions précédentes de r et de v. Il suffiroit encore d'augmenter dans ces expressions, l'angle nt, de la demi-circonférence, ce qui rend négatifs, les sinus et les cosinus des multiples impairs de nt; ainsi les résultats de ces deux méthodes devant être identiques, il faut que dans les expressions de r et de v, les sinus et les cosinus des multiples impairs de nt, soient multipliés par des puissances impaires de e, et que les sinus et cosinus des multiples pairs du même angle, soient multipliés par des puissances paires de cette quantité. C'est, en effet, ce que le calcul confirme *à posteriori*.

Supposons qu'au lieu de compter l'angle v, du périhélie; on fixe son origine, à un point quelconque; il est clair que cet angle sera augmenté d'une constante que nous désignerons par ϖ, et qui exprimera la longitude du périhélie. Si au lieu de fixer l'origine de t, à l'instant du passage au périhélie, on le fixe à un instant quelconque; l'angle nt sera augmenté d'une constante que nous désignerons par $\varepsilon-\varpi$; les expressions précédentes de $\frac{r}{a}$ et de v, deviendront ainsi,

$$\frac{r}{a}=1+\tfrac{1}{2}e^2-(e-\tfrac{3}{8}e^3).\cos.(nt+\varepsilon-\varpi)-(\tfrac{1}{2}e^2-\tfrac{1}{3}e^4).\cos.2(nt+\varepsilon-\varpi)-\&c.;$$
$$v=nt+\varepsilon+(2e-\tfrac{1}{4}e^3).\sin.(nt+\varepsilon-\varpi)+(\tfrac{5}{4}e^2-\tfrac{11}{24}e^4).\sin.2(nt+\varepsilon-\varpi)+\&c.;$$

v est la longitude vraie de la planète, et $nt+\varepsilon$ est sa longitude moyenne, ces deux longitudes étant rapportées au plan de l'orbite.

Rapportons maintenant le mouvement de la planète, à un plan fixe, peu incliné à celui de l'orbite. Soit φ l'inclinaison mutuelle de ces deux plans, et θ la longitude du nœud ascendant de l'orbite, comptée sur le plan fixe; soit c cette longitude comptée sur le plan

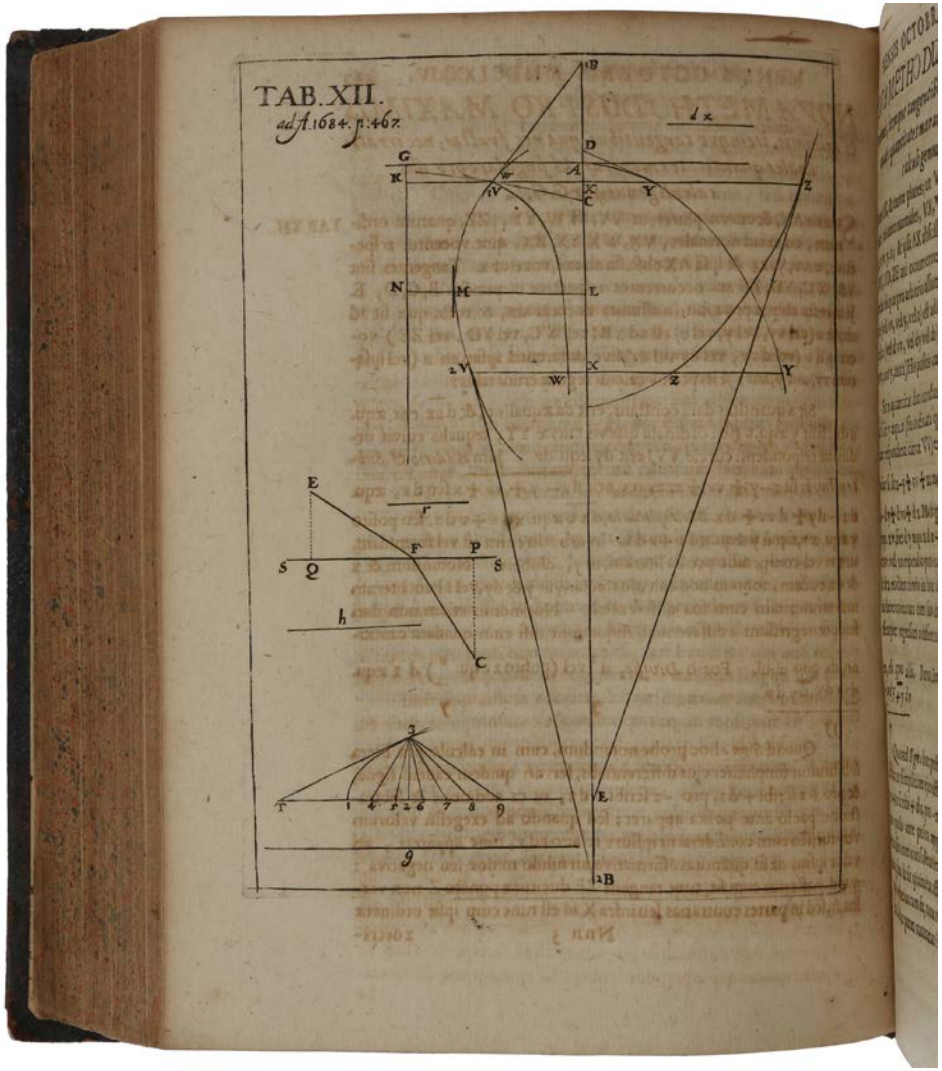
PMM 160 - DISCOVERY OF CALCULUS

LEIBNIZ, Gottfried Wilhelm. *'Nova methodus pro maximis et minimis, itemque tangentibus, quae nec fractas nec irrationales quantitates moratur, et singulare pro illis calculi genus.' In: Acta Eruditorum, Vol. III (1684), pp. 467-73 and Tab. XII.* Leipzig: Christopher Günther for J. Gross & J. F. Gleditsch, 1684.

$25,000

In: Acta Eruditorum, Vol. III (1684), bound with Vol. IV (1685) of the same journal. 4to: 195 x 154 mm. Vol. III: pp. [10], 591, [16] with 14 plates (Nova methodus: pp. 467-73 and Tab. XII); Vol. IV: pp. [6], 595, [16] with 15 plates. A fine and unrestored copy bound in contemporary sheep, spine gilt, red and green sprinkled edges (a little rubbed), some browning though less than usual for this journal, a few contemporary annotations and a little underlining (not in the Leibniz papers). Bookplate of Prince Liechtenstein on front paste-down. The two volumes of Acta Eruditorum contain five other papers by Leibniz.

First edition of Leibniz's invention of the differential calculus. "His epoch-making papers give rules of calculation without proof for rates of variation of functions and for drawing tangents to curves … With the calculus a new era began in mathematics, and the development of mathematical physics since the seventeenth century would not have been possible without the aid of this powerful technique" (PMM). "Leibniz's first paper on the differential calculus, published nine years after he had independently discovered it. Although Newton had probably discovered the calculus earlier than Leibniz, Leibniz was the first to publish his method, which employed a notation superior to that used by Newton. The priority dispute between Newton and Leibniz over the calculus is one of the



most famous controversies in the history of science; it led to a breach between English and Continental mathematics that was not healed until the nineteenth century" (Norman).

"The invention of the Leibnizian infinitesimal calculus dates from the years between 1672 and 1676, when Gottfried Wilhelm Leibniz (1646–1716) resided in Paris on a diplomatic mission. In February 1667 he received the doctor's degree by the Faculty of Jurisprudence of the University of Altdorf and from 1668 was in the service of the Court of the chancellor Johann Philipp von Schönborn in Mainz. At that time his mathematical knowledge was very deficient, despite the fact that he had published in 1666 the essay *De arte combinatoria*. It was Christiaan Huygens (1629–1695), the great Dutch mathematician working at the Paris Academy of Sciences, who introduced him to the higher mathematics. He recognised Leibniz's versatile genius when conversing with him on the properties of numbers propounded to him to determine the sum of the infinite series of reciprocal triangular numbers. Leibniz found that the terms can be written as differences and hence the sum to be 2, which agreed with Huygens's finding. This success motivated Leibniz to find the sums of a number of arithmetical series of the same kind, and increased his enthusiasm for mathematics. Under Huygens's influence he studied Blaise Pascal's *Lettres de A. Dettonville*, René Descartes's *Geometria*, Grégoire de Saint-Vincent's *Opus geometricum* and works by James Gregory, René Sluse, Galileo Galilei and John Wallis.

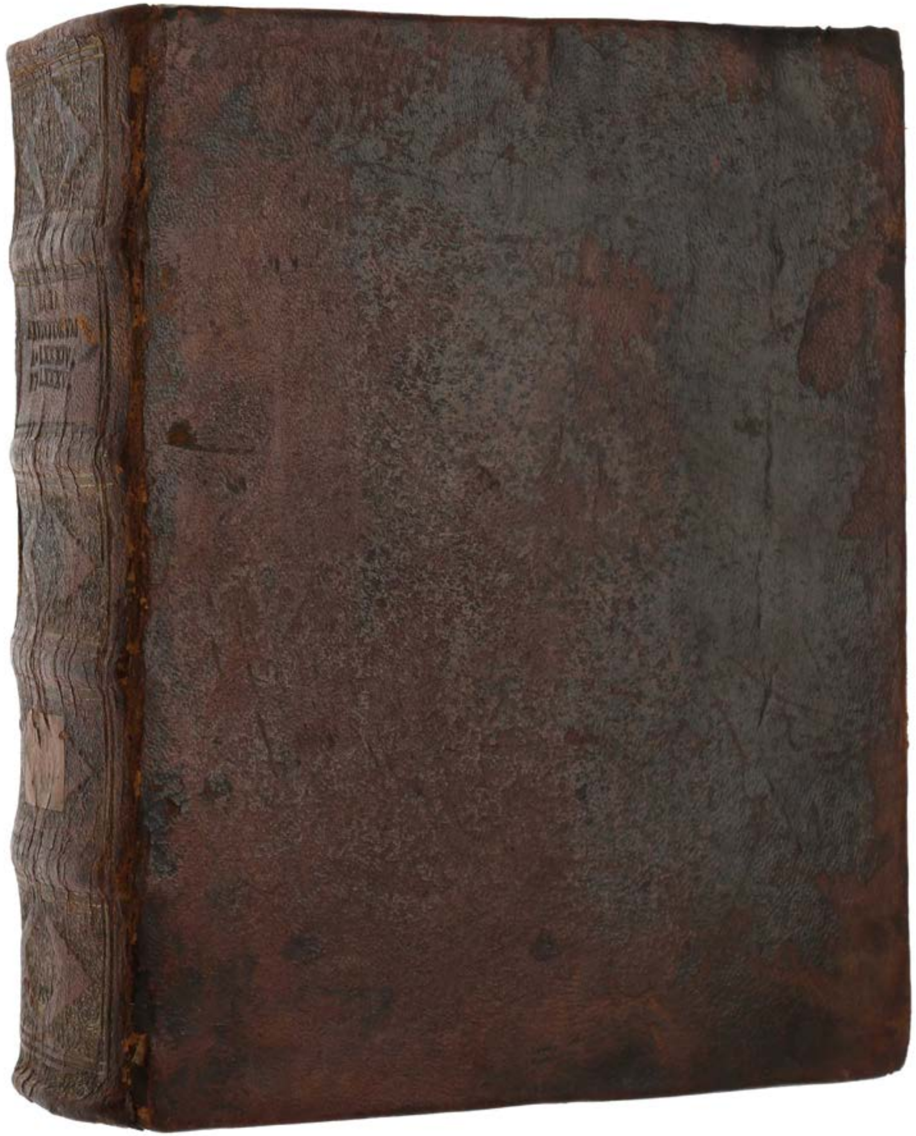
"In Leibniz's recollections of the origin of his differential calculus he relates that reflecting on the arithmetical triangle of Pascal he formed his own harmonic triangle in which each number sequence is the sum-series of the series following it and the difference-series of the series that precedes it. These results make him aware that the forming of difference-series and of sum-series are mutually inverse operations. This idea was then transposed into geometry and applied to the study of curves by considering the sequences of ordinates, abscissas, or of other variables, and supposing the differences between the terms of these sequences infinitely small. The sum of the ordinates yields the area of the curve, for which, signifying Bonaventura Cavalieri's 'omnes lineae', he used the sign '∫', the first letter of the word 'summa'. The difference of two successive ordinates, symbolized by '*d*', served to find the slope of the tangent. Going back over his creation of the calculus Leibniz wrote to Wallis in 1697: 'The consideration of differences and sums in number sequences had given me my first insight, when I realized that differences correspond to tangents and sums to quadratures'.

"The Paris mathematical manuscripts of Leibniz … show Leibniz working out these ideas to develop an infinitesimal calculus of differences and sums of ordinates by which tangents and areas could be determined and in which the two operations are mutually inverse. The reading of Blaise Pascal's *Traité des sinus du quart de circle* gave birth to the decisive idea of the characteristic triangle, similar to the triangles formed by ordinate, tangent and sub-tangent or ordinate, normal and sub-normal. Its importance and versatility in tangent and quadrature problems is underlined by Leibniz in many occasions, as well as the special transformation of quadrature which he called the transmutation theorem by which he deduced simply many old results in the field of geometrical quadratures. The solution of the 'inverse-tangent problems', which Descartes himself said he could not master, provided an ever stronger stimulus to Leibniz to look for a new general method with optimal signs and symbols to make calculations simple and automatic.

"The first public presentation of differential calculus appeared in October 1684 in the new journal *Acta Eruditorum*, established in Leipzig, in only six and an half pages, written in a disorganised manner with numerous typographical errors. In the title, 'A new method for maxima and minima as well as tangents, which is impeded neither by fractional nor irrational quantities, and a remarkable type of calculus for them', Leibniz underlined the reasons for which his method differed from—and excelled—those of his predecessors. In his correspondence with his contemporaries and in the later manuscript 'Historia et origo calculi differentialis', Leibniz predated the creation of calculus to the Paris period, declaring that other tasks had prevented publication for over nine years following his return to Hannover.

"Leibniz's friends Otto Mencke and Johann Christoph Pfautz, who had founded the scientific journal *Acta Eruditorum* in 1682 in Leipzig, encouraged him to write the paper; but it was to be deemed very obscure and difficult to comprehend by his contemporaries. There is actually another more urgent reason which forced the author to write in such a hurried, poorly organised fashion. His friend Ehrenfried Walter von Tschirnhaus (1651–1708), country-fellow and companion of studies in Paris in 1675, was publishing articles on current themes and problems using infinitesimal methods which were very close to those that Leibniz had confided to him during their Parisian stay; Leibniz risked having his own invention stolen from him. The structure of the text, which was much more concise and complex than the primitive Parisian manuscript essays, was complicated by the need to conceal the use of infinitesimals. Leibniz was well aware of the possible objections he would receive from mathematicians linked to classic tradition who would have stated that the infinitely small quantities were not rigorously defined, that there was not yet a theory capable of proving their existence and their operations, and hence they were not quite acceptable in mathematics.

"Leibniz's paper opened with the introduction of curves referenced to axis *x*, variables (abscissas and ordinates) and tangents. The context was therefore geometric, as in the Cartesian tradition, with the explicit representation of the abscissa axis only. The concept of function did not yet appear, nor were dependent variables distinguished from independent ones. The characteristics of the



introduced objects were specified only in the course of the presentation: the curve was considered as a polygon with an infinity of infinitesimal sides (that is, as an infinitangular polygon), and the tangent to a point of the curve was the extension of an infinitesimal segment of that infinitangular polygon that represented the curve. Differentials were defined immediately after, in an ambiguous way. Differential *dx* was introduced as a finite quantity: a segment arbitrarily fixed *a priori*. This definition however would never be used in applications of Leibniz's method, which was to operate with infinitely small dx in order to be valid. The ordinate differential was introduced apparently with a double definition: '*dv* indicates the segment which is to *dx* as *v* is to *XB,* that is, *dv* is the difference of the *v*'.

"In the first part Leibniz establishes the equality of the two ratios ($dv : dx = v : XB$), the equality deduced by the similitude between the finite triangle formed by the tangent, the ordinate and the subtangent, and the infinitesimal right-angle triangle whose sides are the differentials thereof and is called 'characteristic triangle'. But the proportion contains a misprint in the expression for the subtangent that would be corrected only in the general index of the first decade of the journal [*Acta Eruditorum*, 1693], 'Corrigenda in Schediasmatibus *Leibnitianis*, quae Actis Eruditorum Lipsiensibus sunt inserta'). The second part ('*dv* is the difference of the *v*') mentioned the difference between the two ordinates which must lie infinitely close:

$$dv = v(x + dx) - v(x).$$

In actual fact, the proportion was needed to determine the tangent line and the definition of *dv* was consequently the second, as explicitly appeared in three of Leibniz's Parisian manuscripts. Considering the corresponding sequences of infinitely close abscissas and ordinates, Leibniz called differentials into the game as infinitely small differences of two successive ordinates (*dv*) and as infinitely small differences of two successive abscissae (*dx*), and established a comparison with finite quantities reciprocally connected by the curve equation.

"These first concepts were followed, without any proof, by differentiation rules of a constant *a*, of *ax*, of $y = v$, and of sums, differences, products and quotients. For the latter, Leibniz introduced double signs, whereby complicating the interpretation of the operation … Conscious of the criticism that the use of the infinitely small quantities would have had on the contemporaries, Leibniz chose to hide it in his first paper; many years later, replying to the objections of Bernard Nieuwentijt, he showed in a manuscript how to prove the rules of the calculus without infinitesimals, based on a law of continuity. In his 'Nova methodus' of October 1684 he would then go onto studying the behaviour of the curve in an interval, specifically increasing or decreasing ordinates, maxima and minima, concavity and convexity referred to the axis, the inflexion point and deducing the properties of differentials …

"After introducing the concept of convexity and concavity referred to the axis and linked to increase and decrease of ordinates and of the prime differentials, Leibniz dealt with the second differentials, simply called 'differences of differences' for which constant *dx* was implicitly presupposed. The inflexion point was thus defined as the point where concavity and convexity were exchanged or as a maximum or minimum of the prime differential. These considerations, burdened by the previous incorrect double implications, would lead him to state as necessary and sufficient conditions which were in fact only necessary. They will be elucidated in l'Hôpital's textbook of 1696.

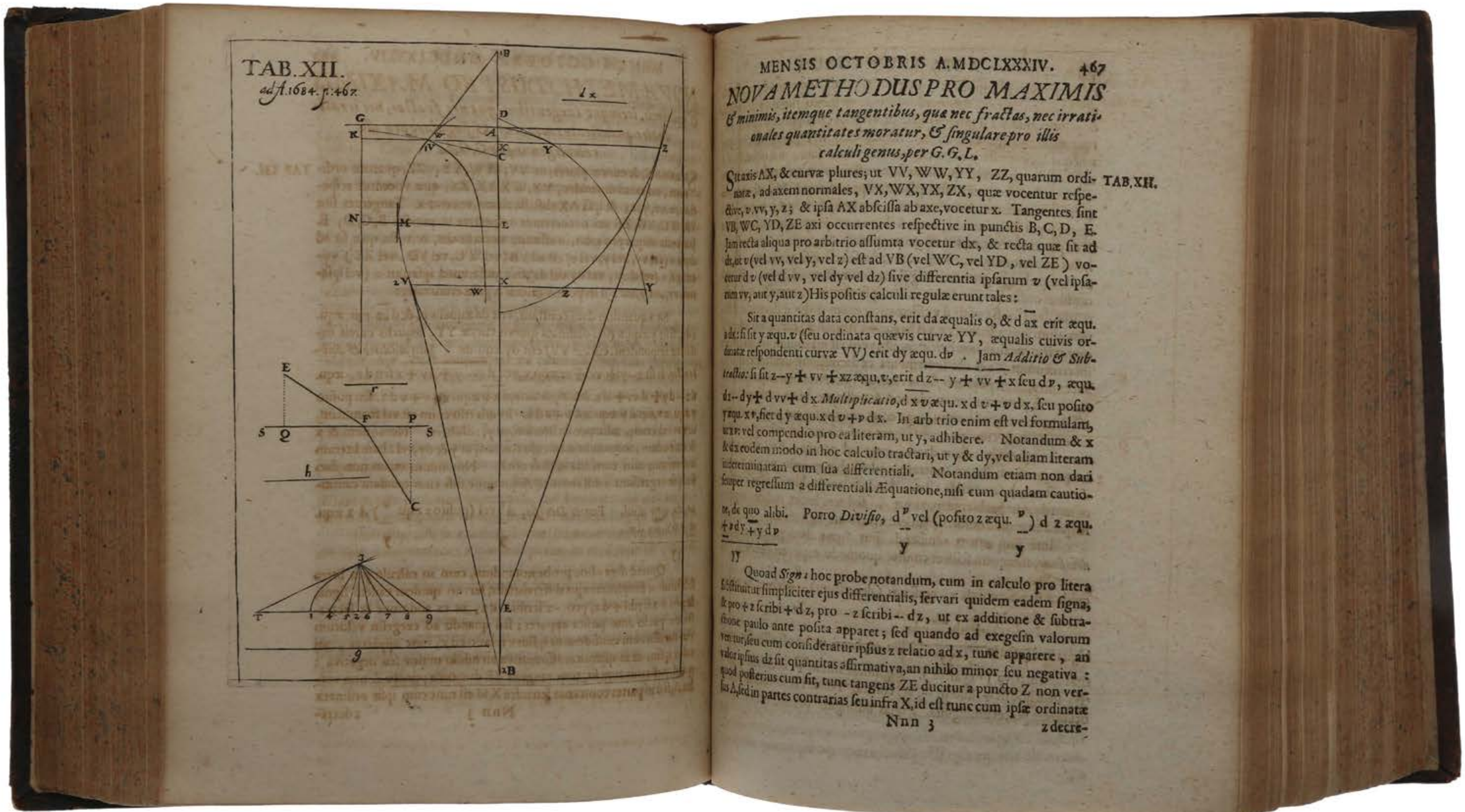
"Leibniz then set out the differentiation rules for powers, roots and composite functions. In the latter case, he chose to connect a generic curve to the cycloid because he wanted to demonstrate that his calculus was easily also applied to transcendent curves, possibility that Descartes wanted to exclude from geometry. It was a winning move to attract the attention on one of the most celebrated curves of the time, and his mentor Huygens expressed to him his admiration when in 1690 Leibniz sent him in detail the calculation of the tangent to the cycloid.

"Finally, Leibniz demonstrated how to apply his differential method on four current problems which led him to proudly announce the phrase quoted at the beginning of this paper. The first example, on the determination of a tangent to a curve, was very complex, containing many fractions and radicals. Earlier methods of past and contemporary mathematicians, such as Descartes, P. de Fermat, Jan Hudde and Sluse, would have required very long calculations. The second example was a minimum problem occurring in refraction of light studied by Descartes and by Fermat. Fermat's method for maxima and minima led to an equation containing four roots, and hence to long and tedious calculations. The third example was a problem that Descartes had put to Fermat, deeming it 'of insuperable difficulty' because the equation of the curve whose tangent was to be determined contained four roots. Leibniz complicated the curve whose tangent was sought even more because his equation contained six. He solved a similar problem in a letter sent to Huygens on 8 September 1679. The last argument was the 'inverse-tangent problem', which corresponded to the solution of a differential equation, that is, find a curve such that for each point the subtangent is always equal to a given constant. In this case, the problem was put by Florimond de Beaune to Descartes, who did not manage to solve it, while Leibniz reached the goal in only a few steps. By these four examples he demonstrated the power of his differential method …

"From the first, when Leibniz was living in Paris, he had understood that the algorithm that he had invented was not merely important but revolutionary for mathematics as a whole. Although his first paper on differential calculus proved to be unpalatable for most of his readers, he had the good fortune to find champions like the Bernoulli brothers, and a populariser like de l'Hôpital, who helped to promote and advance his methods at the highest level. There was certainly no better publicity for the Leibnizian calculus than the results published in the *Acta Eruditorum*, and in the Memoirs of the Paris and Berlin Academies. They not only offered a final solution to open problems such as those of the catenary, the brachistochrone, the velary (the curve of the sail when moved by the wind), the paracentric isochrone, the elastica, and various isoperimetrical problems; they also provided tools for dealing with more general tasks, such as the solution of differential equations, the construction of transcendental curves, the integration of rational and irrational expressions, and the rectification of curves. Both the mathematicians and the scholars of applied disciplines such as optics, mechanics, architecture, acoustics, astronomy, hydraulics and medicine, were to find the Leibnizian methods useful, nimble and elegant as an aid in forming and solving their problems" (Roero, pp. 47-55).

Aiton. Leibniz: A Biography, 1985. Glaser, A History of Binary and Other Non-Decimal Numeration, 1971. Van der Blij, 'Combinatorial Aspects of the Hexagrams in the Chinese Book of Changes,' Scripta Mathematica 28 (1967), pp. 37-49.

Horblit 66a; Norman 1326; PMM 160; Dibner 109; Honeyman 1972; Ravier 88. Roero, 'Gottfried Wilhelm Leibniz. First three papers on the calculus (1684,1686, 1693).' Chapter 4 in Grattan-Guinness (ed.), *Landmark Writings in Western Mathematics 1640-1940*, 2005.



TAB. XII.
ad A. 1684. p. 467.

MENSIS OCTOBRIS A. MDCLXXXIV. 467

NOVA METHODUS PRO MAXIMIS & minimis, itemque tangentibus, quæ nec fractas, nec irrationales quantitates moratur, & singulare pro illis calculi genus, per G. G. L.

TAB. XII.

Sit axis AX, & curvæ plures, ut VV, WW, YY, ZZ, quarum ordinatæ, ad axem normales, VX, WX, YX, ZX, quæ vocentur respective, v, vv, y, z; & ipsa AX abscissa ab axe, vocetur x. Tangentes sint VB, WC, YD, ZE axi occurrentes respective in punctis B, C, D, E. Jam recta aliqua pro arbitrio assumta vocetur dx, & recta quæ sit ad dx, ut v (vel vv, vel y, vel z) est ad VB (vel WC, vel YD, vel ZE) vocetur dv (vel d vv, vel dy vel dz) sive differentia ipsarum v (vel ipsarum vv, aut y, aut z) His positis calculi regulæ erunt tales:

Sit a quantitas data constans, erit da æqualis o, & d $\overline{ax}$ erit æqu. adx: si sit y æqu. v (seu ordinata quævis curvæ YY, æqualis cuivis ordinatæ respondenti curvæ VV) erit dy æqu. dv. Jam *Additio & Subtractio*: si sit z − y + vv + x æqu. v, erit d $\overline{z - y + vv + x}$ seu dv, æqu. dz − dy + dvv + dx. *Multiplicatio*, d $\overline{xv}$ æqu. x dv + v dx, seu posito y æqu. xv, fiet dy æqu. x dv + v dx. In arbitrio enim est vel formulam, ut xv, vel compendio pro ea literam, ut y, adhibere. Notandum & x & dx eodem modo in hoc calculo tractari, ut y & dy, vel aliam literam indeterminatam cum sua differentiali. Notandum etiam non dari semper regressum a differentiali Æquatione, nisi cum quadam cautione, de quo alibi. Porro *Divisio*, d $\frac{v}{y}$ vel (posito z æqu. $\frac{v}{y}$) d z æqu. $\frac{\pm v\,dy \mp y\,dv}{yy}$

Quoad *Signa* hoc probe notandum, cum in calculo pro litera substituitur simpliciter ejus differentialis, servari quidem eadem signa, & pro + z scribi + dz, pro − z scribi − dz, ut ex additione & subtractione paulo ante posita apparet; sed quando ad exegesin valorum venitur, seu cum consideratur ipsius z relatio ad x, tunc apparere, an valor ipsius dz sit quantitas affirmativa, an nihilo minor seu negativa: quod posterius cum fit, tunc tangens ZE ducitur a puncto Z non versus A, sed in partes contrarias seu infra X, id est tunc cum ipsæ ordinatæ

Nnn 3 z decre-

THE DEDICATION COPY - MAURICAEU'S ESTABLISHMENT OF OBSTETRICS AS A SCIENCE

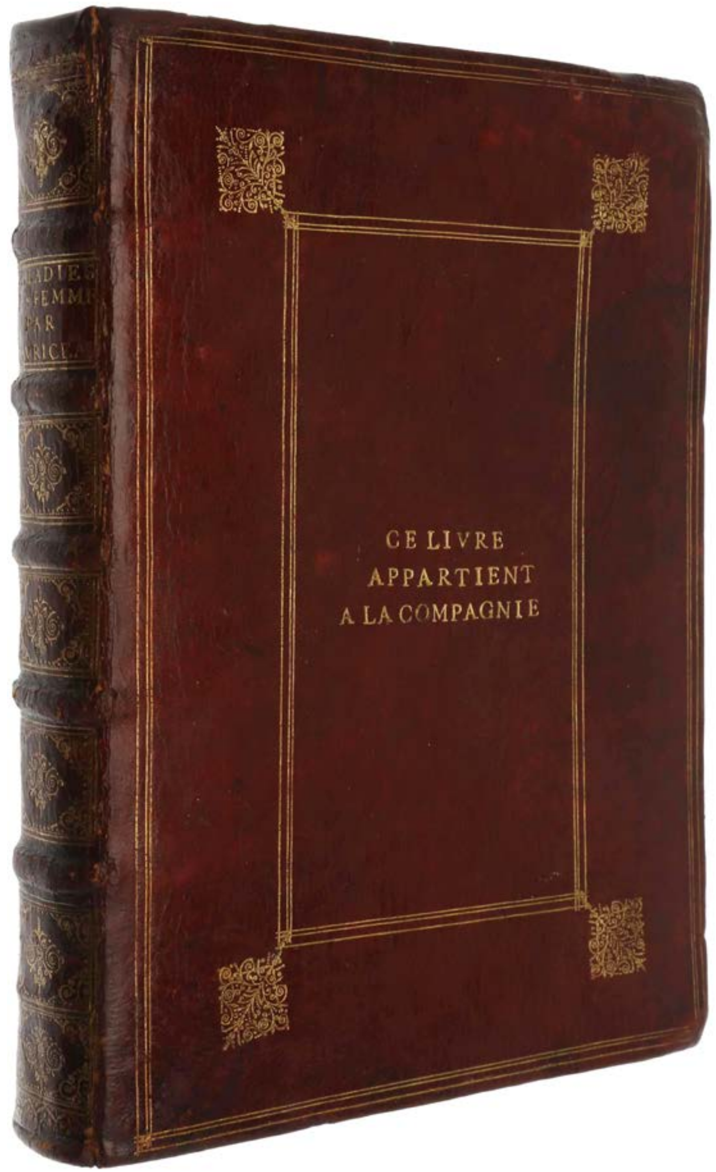
Grolier, *One Hundred Books Famous in Medicine* 33; *En français dans le texte* 107

MAURICEAU, François. *Des maladies des femmes grosses et accouchées. Avec la bonne et veritable méthode de les bien aider en leurs accouchemens naturels, & les moyens de remedier à tous ceux qui sont contre-nature, & aux indspositions des enfans nouveau-nés.* Paris: Chez Jean Henault, Jean d'Houry, Robert de Ninville, Jean Baptiste Coignard, 1668.

$45,000

4to (245 x 185 mm). [24, including engraved frontispiece by Guillaume Vallet after Antoine Paillet and letter-press title] 536 pp., including 11 full or nearly full-page, 15 half-page and 3 quarter page engravings in text. In a contemporary presentation binding of red morocco gilt, spine in 7 compartments richly tooled, covers triple-gilt-ruled with fleurons at corners, tooled in the center of the upper and lower covers: "Ce Livre Appartient à la Compagnie / Des Maistres Chirurgiens Iurez de Paris." Extremities and corners expertly repaired, preserved in a cloth box. Ruled in red throughout. Mauriceau's autograph cipher at the end of the printed dedication followed by three inscriptions signed by Mauriceau's cipher at the end of the printed dedication, dated 1675, 1681 and 1694. Correction in manuscript on p. 196. Some minor toning in extreme outer margin, but generally a broad-margined magnificent copy, in a splendid binding with an important historic association.

The dedication copy of the first edition of this groundbreaking medical work which



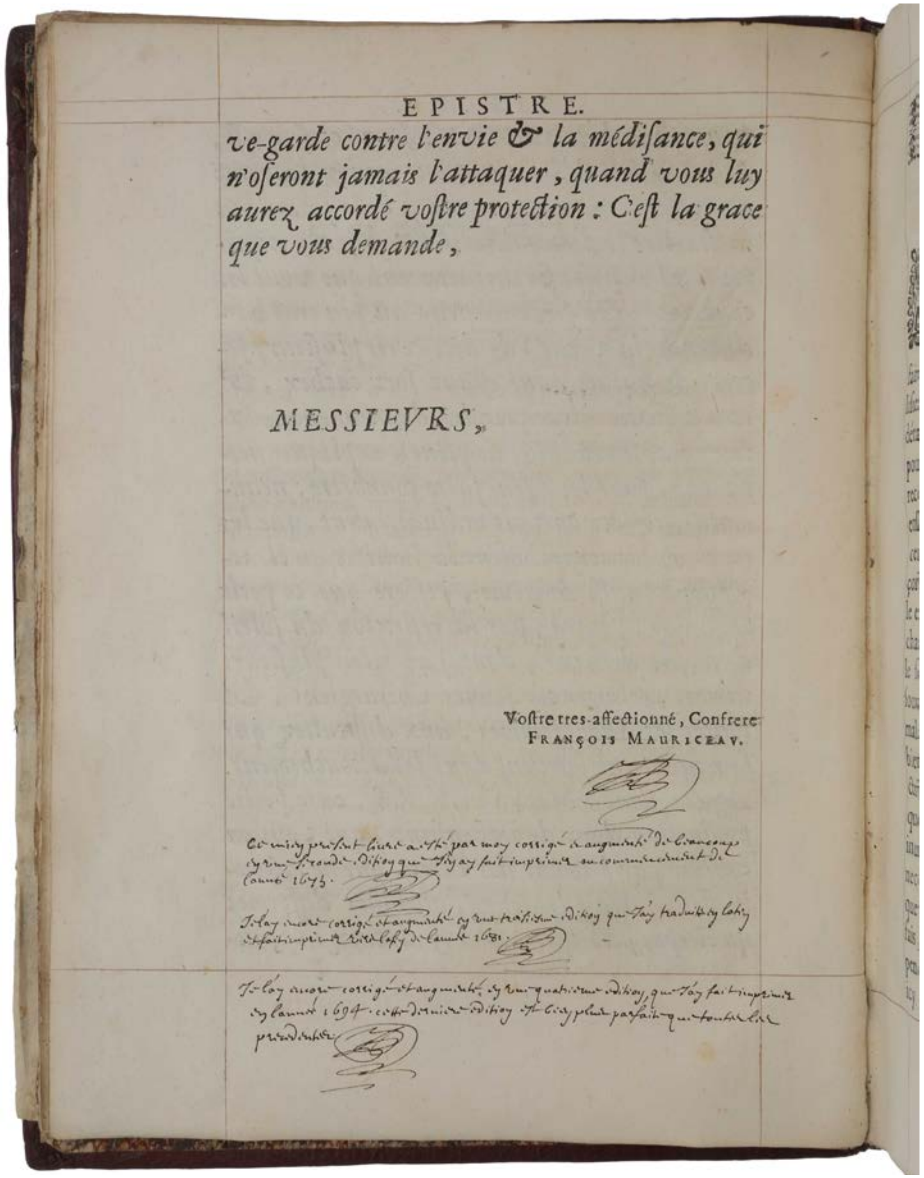
"established obstetrics as a science" (G&M). This is a superb copy in a presentation binding of contemporary red morocco stamped with the name of the dedicatees—Les Maistres Chirurgiens Jurez de Paris—and with three signed inscriptions by the author at the end of the printed dedication, announcing the publication of his work's later 17th century revised editions. "This book was without question the most practical, explicit and accurate of the then known treatises on midwifery" (Cutter & Viets, *A Short History of Midwifery*, p. 51). Mauriceau was "the first to write on tubal pregnancy, epidemic puerperal fever, and the complications that arise in labor from misplacement of the umbilical cord" (Le Fanu, *Notable Medical Books from the Lilly Library*, p. 85). Mauriceau popularised the idea of delivery in bed rather than on a birth stool, and while recommending the reading of other learned authors, cautioned that "the most part of them, having never practised the art they undertake to teach, resemble…those geographers who give us the description of many countries which they never saw". "While much in Mauriceau's treatise echoed the teachings of his predecessors, the work also included several important new features, such as Mauriceau's detailed analysis of the mechanism of labor, his introduction of the practice of delivering women in bed rather than in the obstetric chair, the earliest account of the prevention of congenital syphilis by antisyphilitic treatment during pregnancy, and the rebuttal of Paré's erroneous account of pubic separation during birth" (Norman). For more than seventy years and through numerous translations and editions, *Des maladies des femmes grosses* contributed to the spread of good obstetric practice throughout Europe.

Provenance: The present copy's presentation binding testifies directly to Mauriceau's practical training in obstetrics and importance in the Parisian medical community: its covers declare its owner to be a member of Les Maîtres Chirurgiens Jurés (also known as the Confraternity of Saint-Come), the venerated guild of Paris surgeons established in the 13th century. Mauriceau's printed dedication,

similarly addressed to “Mes chers Confrères,” has manuscript addenda in this copy: Three inscriptions written by the author and signed with his cipher, the first noting the publication of the corrected and augmented second edition of Des maladies des femmes in 1675, the second the publication of the third French and the first Latin editions in 1681, and the third noting the publication of the revised fourth edition “bien plus parfaite que toutes les précédentes.” It seems probable that after Mauriceau originally presented this copy to the library of the Confraternity he continued to revisit the copy on their shelves and documented, in this dedication copy of the first edition, the fact that he had continued to make improvements to his text in later editions.

It is worth noting Mauriceau’s relationship with the Confraternity to whose library the present volume was presented. The prestigious society had originally served to distinguish its members, usually academics, from “barber-surgeons” who had no university training. Yet in 1655 the two guilds had merged—in large part because the practical skills of itinerant surgeons often surpassed those of their academic competitors! In this context, Mauriceau’s hands-on apprenticeship at the Hôtel-Dieu is significant, as is the publication of his work in French instead of Latin—a fact noted by the Bibliothèque Nationale’s inclusion of the present volume in its exhibition catalogue *En Français dans le texte* (1990). Of interest also is Mauriceau’s advertisement of his medical practice at the foot of the engraved frontispiece, which includes his portrait. He states, admittedly in small print, that his office is on rue St. Severin at the corner of rue Zacharie, etc., etc.

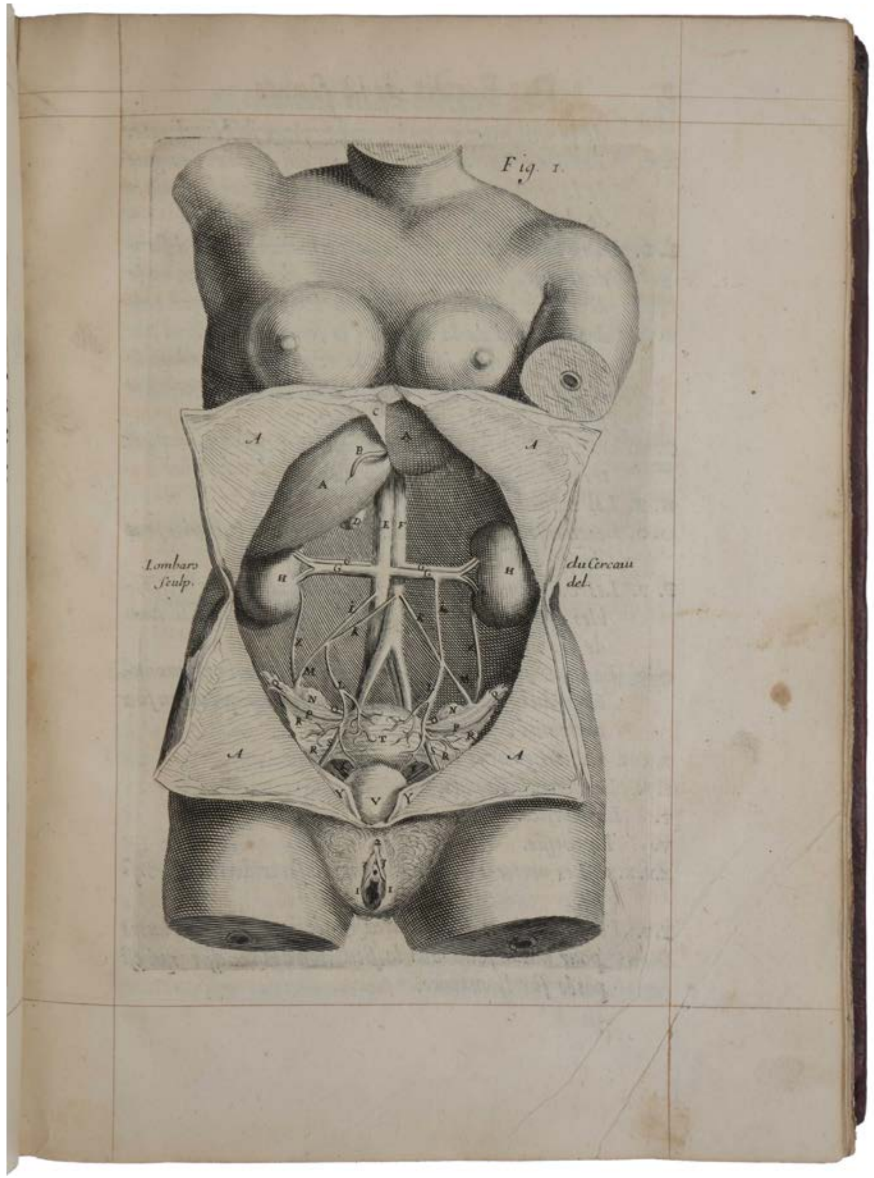
“François Mauriceau had an extensive practice in midwifery in Paris, both private and in the Hotel Dieu, which was at that time the leading establishment for lying in women in Europe. In 1668, when only 31, he published his great work *Traité des Maladies des Femmes Grosses et Accouchies*, ‘which, according to Andre Levret drew from the cradle’ the art of midwifery. Two years later Mauriceau received a



visit from Hugh Chamberlen, a member of the British family that possessed the secret of the obstetric forceps, who then translated his text, making it available to the English-speaking world. The influence of this work on many aspects of midwifery was immense, and Mauriceau is still remembered eponymously for his description of delivery of the after coming head in breech presentation. Mauriceau's book also contains a section entitled 'Of children newborn and their ordinary Distempers, together with necessary directions to chuse a Nurse'. Among the 18 chapters are ones on 'Of cutting the Tongue when Tongue-ty'd' and 'How to cure the Venereal Lues in infants.' Perhaps, though, in retrospect, his greatest impact was in the influence his advice had on the position that women should adopt during delivery. From earliest times women throughout the world had usually assumed an upright posture during parturition. In Europe, the birthing chair was particularly popular. As Atwood has written, 'The first major obstetrical change in the position of the parturient occurred when François Mauriceau substituted the bed for the birth stool. The time honoured 'position' assumed in an obstetric chair was replaced with the recumbent position to facilitate examinations and obstetric operations for the obstetrician.'

"Let us study what Mauricaeu actually wrote on this subject"

'The bed must be so made, that the woman being ready to be delivered, should lie on her back upon it, having her body in a convenient figure, that is, her head and breast a little raised, so that she be neither lying nor sitting; for in this manner she breathes best, and will have more strength to help her pains, than if she were otherwise, or sunk down in her bed. Being in this posture, she must spread her thighs abroad, folding her legs a little towards her buttocks … and have her feet stayed against some firm thing; besides this, let her hold some persons with her hands, that she may better stay herself during her pains … bearing them down



when they take her, which she may do by holding her breath, and forcing herself, as much as she can, just as when she goeth to stool …'

"The semirecumbent delivery position described by Mauriceau became known as the 'French' position and its use steadily spread throughout Europe and North America in the centuries that followed. Gradually in many countries it evolved into the fully recumbent or lithotomy position. More recently, with the diffusion of Western obstetrics, the dorsal position has also been introduced into many developing countries. Though some authors have credited Mauriceau with this change in delivery position, others regard the dorsal recumbent posture to be the most mischievous intervention in modern obstetrics, causing parturition to be more drawn out, more painful for the mother, and less safe for the fetus. To be fair to Mauriceau it should be recorded that he also recommended ambulation during labour, writing:

'… she may walk about her chamber … The patient may likewise by intervals rest herself on her bed, to regain her strength; but not too long, especially little, or short thick women, for they have always worse labours if they be much on their beds in their travail, and yet much worse of their first children, than when they are prevailed with to walk about the chamber, supporting them under their arms, if necessary; for by this means, the weight of the child, the woman being on her legs, causeth the inward orifice of the womb to dilate sooner than in bed; and her pains to be stronger and frequenter, that her labour be nothing near so long'" (Dunn).

Dunn suggests that Mauriceau took his recommendation of the dorsal recumbent position from Aristotle, who recommended it around 350 BC, although Hippocrates, Soranus of Ephesus and other classical writers all recommended an upright posture for parturition.

"Mauriceau, who was an ordinary surgeon and not a doctor of medicine, was a skilful practitioner and an acute observer, publishing his observations in an admirably clear form. Mauriceau was the first to study the conformation of the female pelvis, showing that in a woman with a large pelvis birth could take place without separation of the bones. He studied the movements of the fetus in different positions, the circulation in the pregnant uterus, and the formation of milk. He advised the bimanual extraction of the head, and was the first to describe the complication of strangulation of the newborn by the umbilical cord. He strongly condemned cephalic version, and introduced a number of technical improvements. His treatment of haemorrhage was excellent, and he gave careful rules for the treatment of *placenta previa*. He condemned Caesarean section, which he regarded as fatal. Contrary to the opinion of his predecessors, he recognized the puerperal flow as a secretion analogous to the suppuration of a wound" (Castiglione, *A History of Medicine* (1941), pp. 555-6).

Garrison-Morton.com 6147. *En français dans le texte* 107. Norman 1461, Grolier, *One Hundred Books Famous in Medicine*, no. 33; Heirs of Hippocrates 604 (2nd. Edition); NLM/Krivatsy 7588; Wellcome IV, p. 85. Dunn, 'François Mauricea (1637-1709) and maternal posture for parturition,' Archives of Disease in Childhood 66 (1991), pp. 78-79.

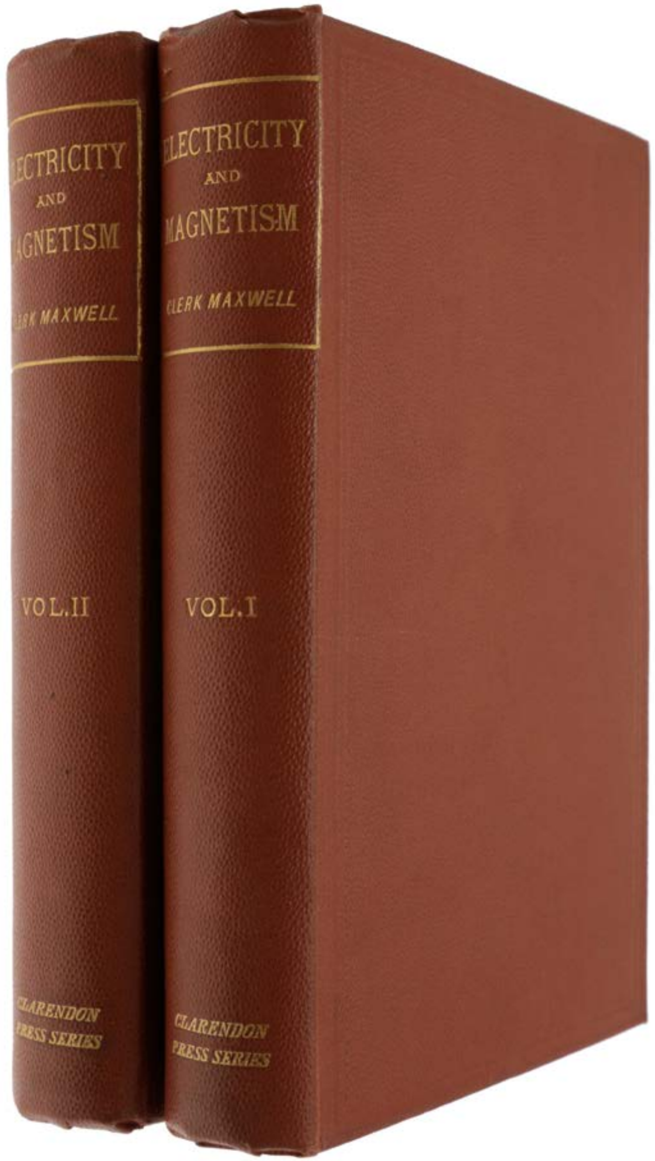
THE FOUNDATION WORK OF ELECTROMAGNETISM - THE FINEST COPY WE HAVE SEEN

MAXWELL, James Clerk. *A treatise on electricity and magnetism.* Oxford: Clarendon Press, 1873.

$16,000

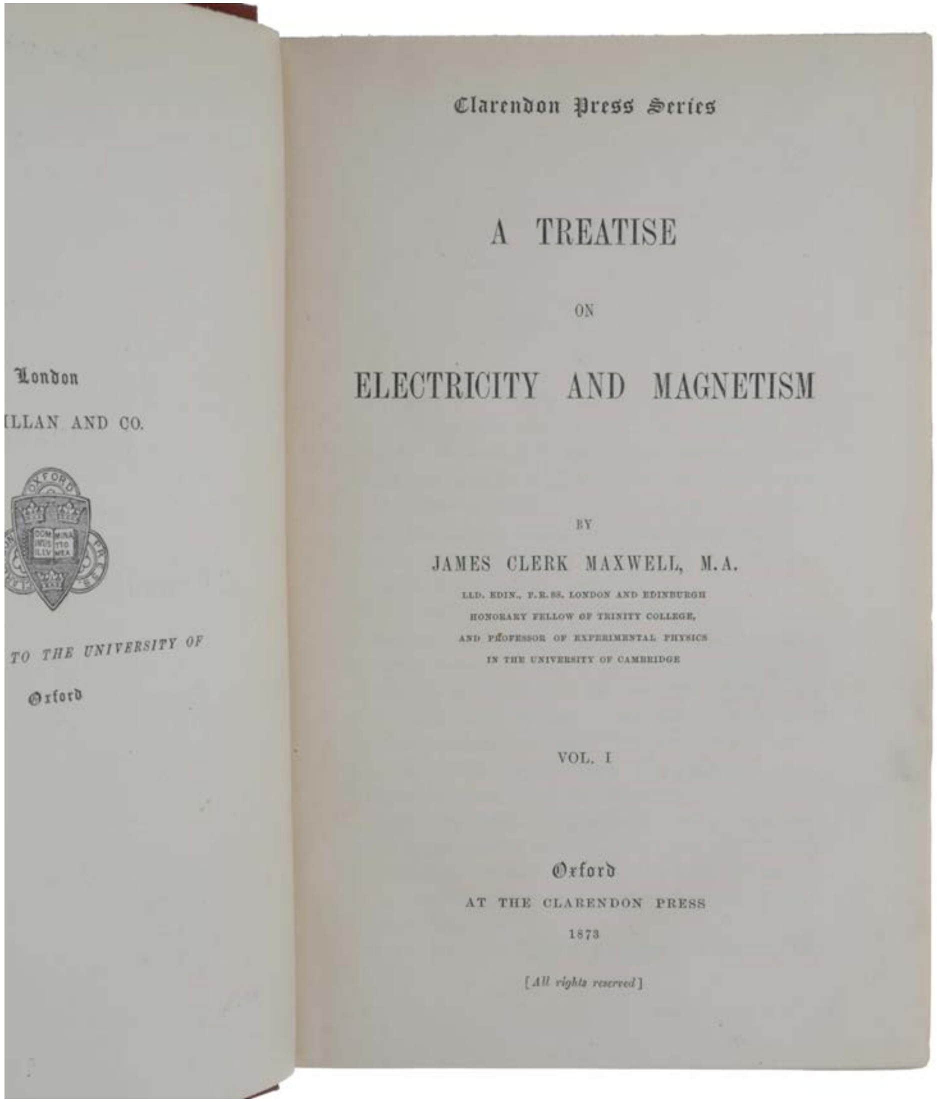
Two volumes, 8vo (223 x 140 mm), pp. [i-v] vi-xxix [1:blank], [2:errata leaf] [2:part-title] [1] 2-425, [3], lithographed plate insereted after p. 148, and 13 plates bound in at the end; [i-v] vi-xxiii [1:blank], [2:errata leaf], [1] 2-444, [2] and 7 plates. Second volume enirely unopened. Original publisher's cloth. An immaculate and completely mint set. Highly scarce in such prestine condition.

First edition, second issue (see below), 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).

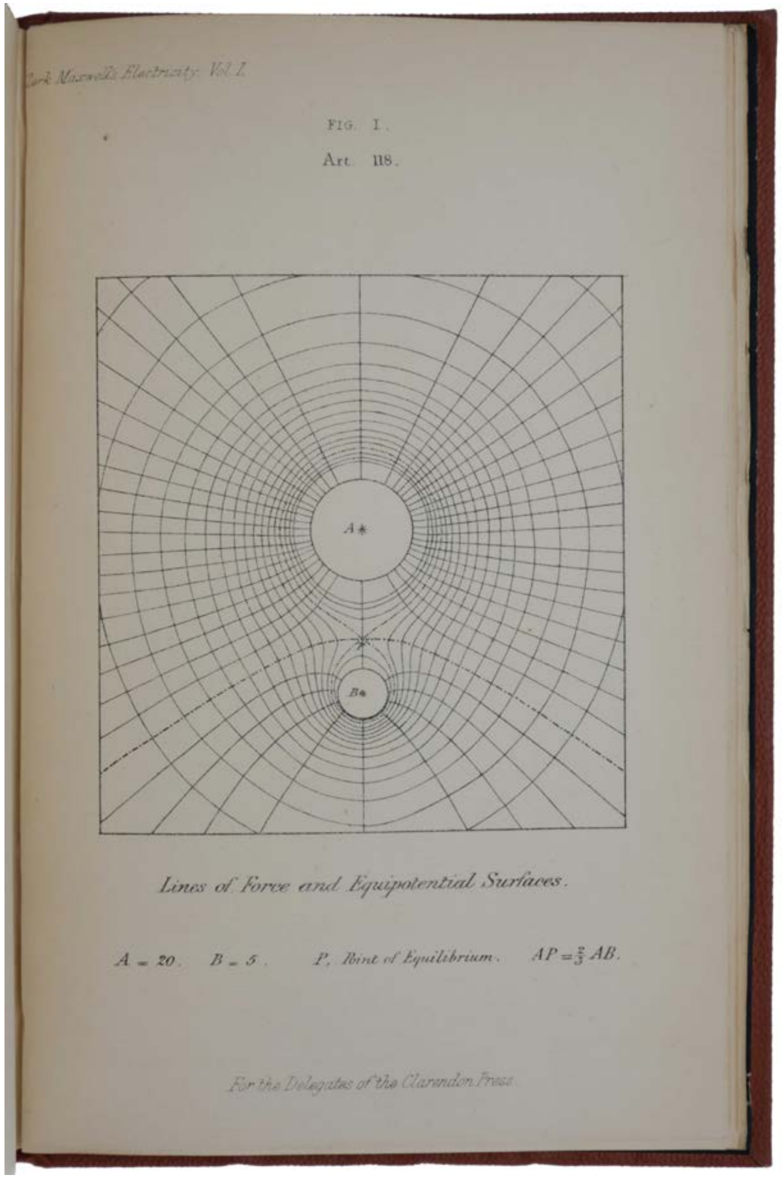
Issues: The *Treatise* is found in various forms and publisher's bindings. We know of two different cloth bindings, both certainly official publisher's bindings: one with the arms of Clarendon Press blind stamped to the boards; another slightly brighter cloth without the arms (as the offered copy). The copies in the first type of binding (with arms to the boards) seem always to have advertisements for the Clarendon Press bound in the end of volume 2. These adds can be found in two different forms: one with 'just published' at the entry for Maxwell's *Treatise* itself, and one not mentioning 'just published'. A copy with the just-published-adds seems always to have the short errata slips in volume 1 and 2. Copies with the adds not mentioning 'just published' seems always to have the extended errata leaves, with further corrections than the errata slips. A set in the blind stamped bindings with just-published-adds and short errata slips are usually said to be first issue and a set with longer errata leaves and adds not mentioning 'just published' are usually called third issue. Sets in the brighter publisher's bindings (i.e., without the arms of the Clarendon Press blind stamped to the boards), which in our experience are rarer than the darker blind stamped form, seem always to be



without the advertisements for the Clarendon Press in the end of volume 2. These sets can however be found with the shorter errata slips or with the extended errata leaves (as the offered copy). We have so far been unable to determine the exact chronological order in which these various forms were issued. Hence there seems so far to be two first issues (with short errata slips), two second issues (with extended errata leaves), and one third issue (with adds not mentioning 'just-published)'.

"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

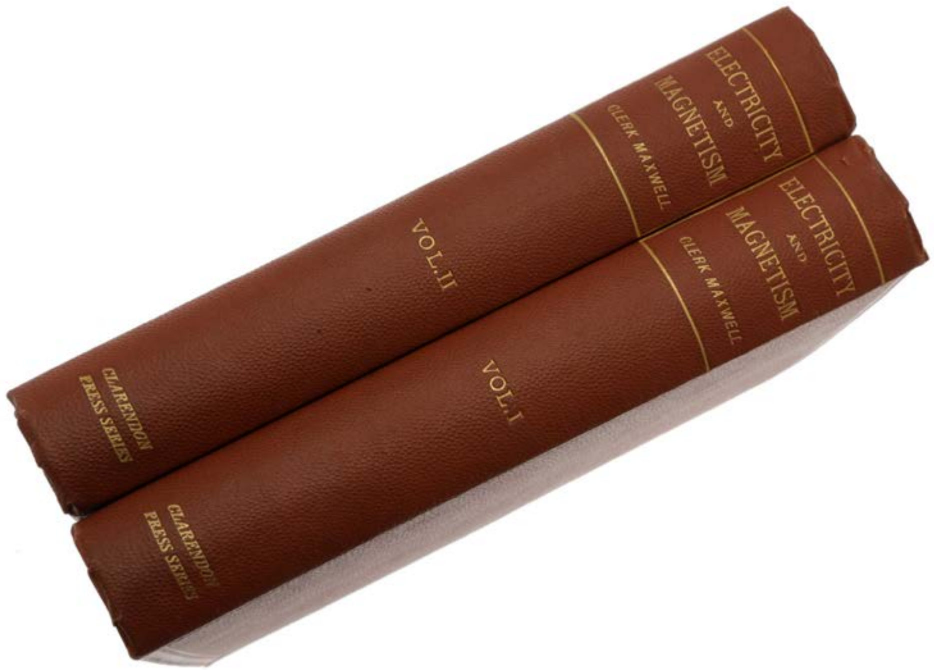


an electromagnetic system in its most general form. [George] Green and others had developed similar arguments in studying the dynamics of the luminiferous ether, but the use Maxwell made of Lagrangian techniques was new to the point of being almost a new approach to physical theory—though many years 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).

Horblit 72 (not noting extra plate or errata in second volume); Norman 1466 (second issue); see PMM 355; Wheeler Gift Catalogue 1872.



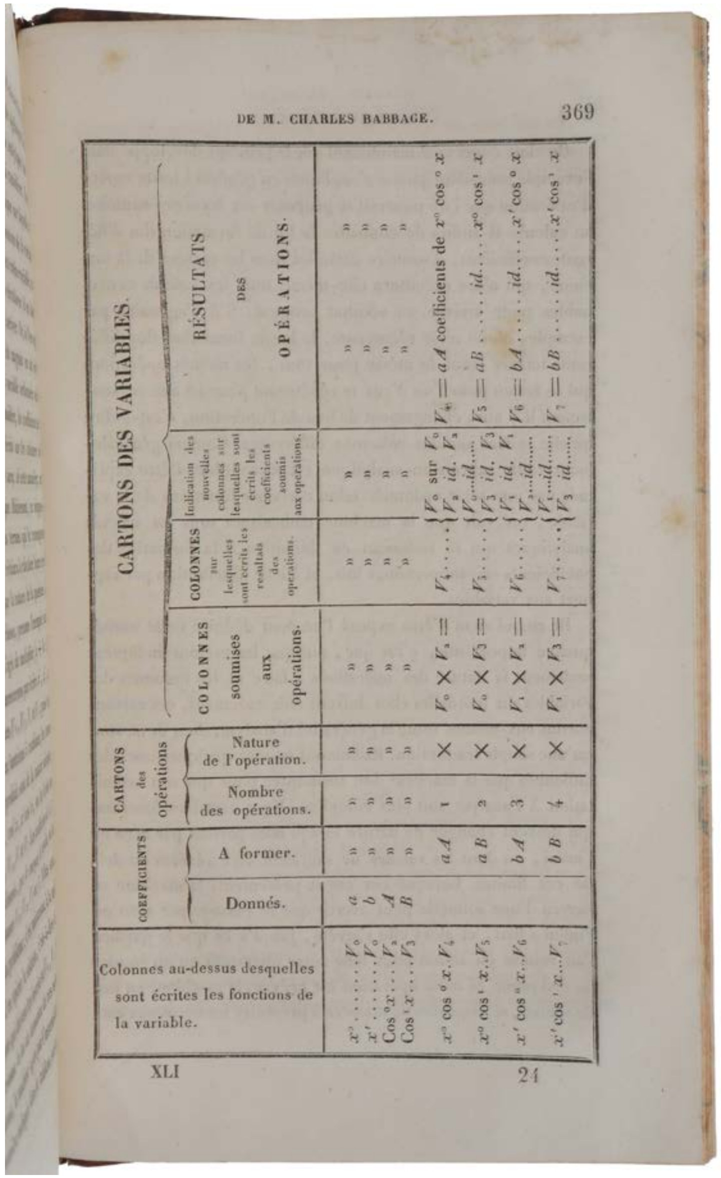
BABBAGE'S ANALYTICAL ENGINE - THE FIRST EXAMPLES OF COMPUTER PROGRAMS EVER PUBLISHED

MENABREA, Luigi Federico. *Notions sur la machine analytique de M. Charles Babbage.* Paris: Anselin, 1842.

$25,000

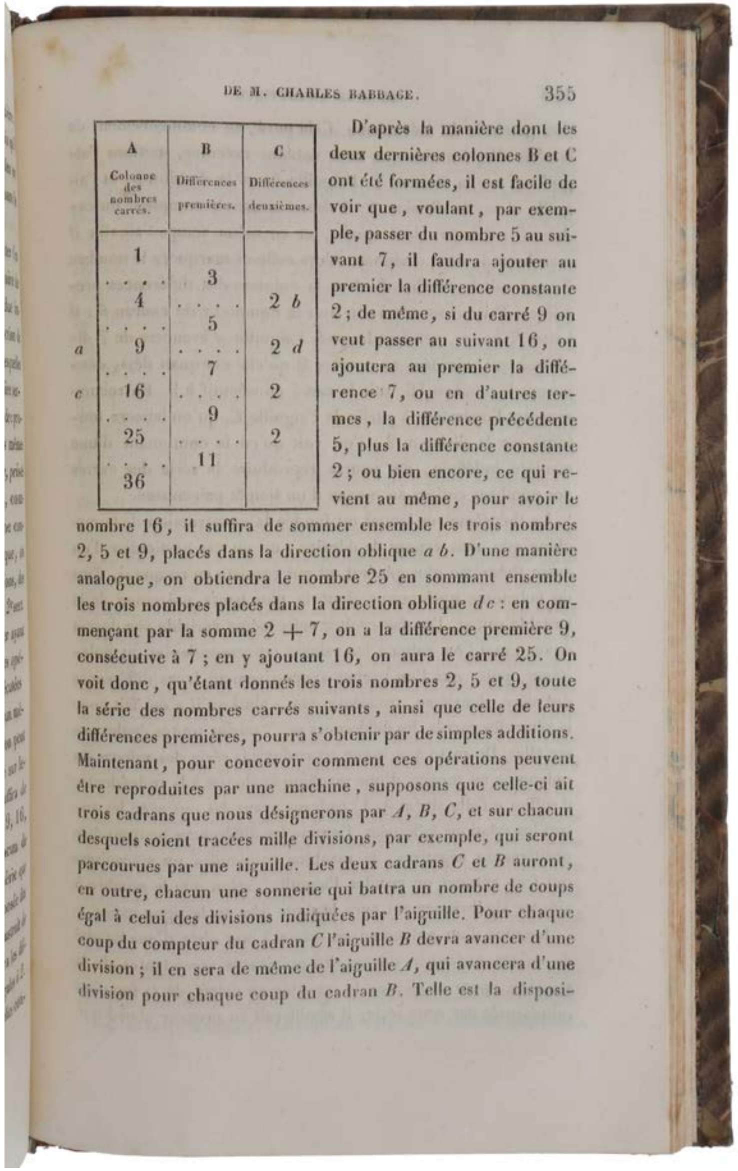
Pp. 352-376 in: Bibliothèque Universelle de Genève. Nouvelle série, Tome 41. 8vo (213 x 128 mm), pp. [5], 6-420, with folding table and plate, several tables printed within text. Contemporary quarter-calf and marbled boards, spine richly gilt with two lettering-pieces (binding a little rubbed, two old institutional stamps on title, faint dampstain in lower margin at beginning, light browning and foxing).

First edition, journal issue, of the only public presentation that Babbage ever made concerning the design and operation of the Analytical Engine. "This was the first published account of Charles Babbage's Analytical Engine and the first account of its logical design, including the first examples of computer programs ever published. As is well known, Babbage's conception and design of his Analytical Engine—the first general purpose programmable digital computer—were so far ahead of the imagination of his mathematical and scientific colleagues that few expressed much curiosity regarding it. Babbage first conceived the Analytical Engine in 1834. This general-purpose mechanical machine—never completely constructed—embodied in its design most of the features of the general-purpose programmable digital computer. In its conception and design Babbage incorporated ideas and names from the textile industry, including



data and program input, output, and storage on punched cards similar to those used in Jacquard looms, a central processing unit called the 'mill,' and memory called the 'store.' In 1840 Babbage traveled to Turin to make a presentation on the Analytical Engine. Babbage's talk, complete with charts, drawings, models, and mechanical notations, emphasized the Engine's signal feature: its ability to guide its own operations—what we call conditional branching. In attendance at Babbage's lecture was the young Italian mathematician Luigi Federico Menabrea (1809-1896), who prepared from his notes an account of the principles of the Analytical Engine. Reflecting a lack of urgency regarding radical innovation unimaginable to us today, Menabrea did not get around to publishing his paper until two years after Babbage made his presentation, and when he did so he published it in French in a Swiss journal [offered here]. Shortly after Menabrea's paper appeared Babbage was refused government funding for construction of the machine" (historyofinformation.com). "In keeping with the more general nature and immaterial status of the Analytical Engine, Menabrea's account dealt little with mechanical details. Instead he described the functional organization and mathematical operation of this more flexible and powerful invention. To illustrate its capabilities, he presented several charts or tables of the steps through which the machine would be directed to go in performing calculations and finding numerical solutions to algebraic equations. These steps were the instructions the engine's operator would punch in coded form on cards to be fed into the machine; hence, the charts constituted the first computer programs [emphasis ours]. Menabrea's charts were taken from those Babbage brought to Torino to illustrate his talks there" (Stein, *Ada*: *A Life and a Legacy*, p. 92). ABPC/RBH list only the OOC copy (Christie's, 23 February 2005, lot 32, $10,800).

In 1828, during his grand tour of Europe, Babbage had suggested a meeting of Italian scientists to the Grand Duke of Tuscany. On his return to England Babbage corresponded with the Duke, sending specimens of British manufactures and



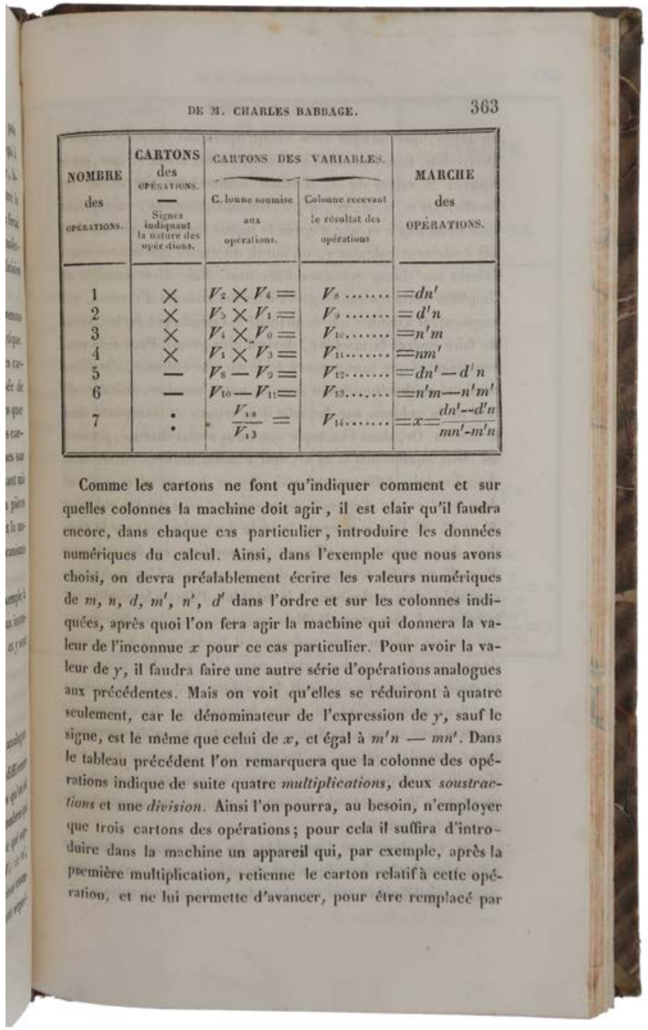
DE M. CHARLES BABBAGE. 355

	A Colonne des nombres carrés.	B Différences premières.	C Différences deuxièmes.
	1		
	. . . .	3	
	4	. . . .	2 *b*
	. . . .	5	
a	9	. . . .	2 *d*
	. . . .	7	
c	16	. . . .	2
	. . . .	9	
	25	. . . .	2
	. . . .	11	
	36		

D'après la manière dont les deux dernières colonnes B et C ont été formées, il est facile de voir que, voulant, par exemple, passer du nombre 5 au suivant 7, il faudra ajouter au premier la différence constante 2; de même, si du carré 9 on veut passer au suivant 16, on ajoutera au premier la différence 7, ou en d'autres termes, la différence précédente 5, plus la différence constante 2; ou bien encore, ce qui revient au même, pour avoir le nombre 16, il suffira de sommer ensemble les trois nombres 2, 5 et 9, placés dans la direction oblique *a b*. D'une manière analogue, on obtiendra le nombre 25 en sommant ensemble les trois nombres placés dans la direction oblique *d c* : en commençant par la somme 2 + 7, on a la différence première 9, consécutive à 7; en y ajoutant 16, on aura le carré 25. On voit donc, qu'étant donnés les trois nombres 2, 5 et 9, toute la série des nombres carrés suivants, ainsi que celle de leurs différences premières, pourra s'obtenir par de simples additions. Maintenant, pour concevoir comment ces opérations peuvent être reproduites par une machine, supposons que celle-ci ait trois cadrans que nous désignerons par *A*, *B*, *C*, et sur chacun desquels soient tracées mille divisions, par exemple, qui seront parcourues par une aiguille. Les deux cadrans *C* et *B* auront, en outre, chacun une sonnerie qui battra un nombre de coups égal à celui des divisions indiquées par l'aiguille. Pour chaque coup du compteur du cadran *C* l'aiguille *B* devra avancer d'une division; il en sera de même de l'aiguille *A*, qui avancera d'une division pour chaque coup du cadran *B*. Telle est la disposireceiving on one occasion from the Duke a thermometer from the time of Galileo. In 1839 Babbage was invited to attend a meeting of Italian scientists at Pisa, but he was not ready and declined. "In 1840 a similar meeting was arranged in Turin. By then Babbage did feel ready, and accepted the invitation from [Giovanni] Plana (1781-1864) to present the Analytical Engine before the assembled philosophers of Italy … In the middle of August 1840, Babbage left England …

"Babbage had persuaded his friend Professor MacCullagh of Dublin to abandon a climbing trip in the Tyrol to join him at the Turin meeting. There in Babbage's apartments for several mornings met Plana, Menabrea, Mosotti, MacCullagh, Plantamour, and other mathematicians and engineers of Italy. Babbage had taken with him drawings, models and sheets of his mechanical notations to help explain the principles and mode of operation of the Analytical Engine. The discussions in Turin were the only public presentation before a group of competent scientists during Babbage's lifetime of those extraordinary forebears of the modern digital computer. It is an eternal disgrace that no comparable opportunity was ever offered to Babbage in his own country …

"The problems of understanding the principles of the Analytical Engines were by no means straightforward even for the assembly of formidable scientific talents which gathered in Babbage's apartments in Turin. The difficulty lay not as much in detail but rather in the basic concepts. Those men would certainly have been familiar with the use of punched cards in the Jacquard loom, and it may reasonably be assumed that the models would have been sufficient to explain the mechanical operation in so far as Babbage deemed necessary. Mosotti, for example, admitted the power of the mechanism to handle the relations of arithmetic, and even of algebraic relations, but he had great difficulty in comprehending how a machine could handle general conditional operations: that is to say what the machine does if its course of action must be determined by results arising from its own



DE M. CHARLES BABBAGE. 363

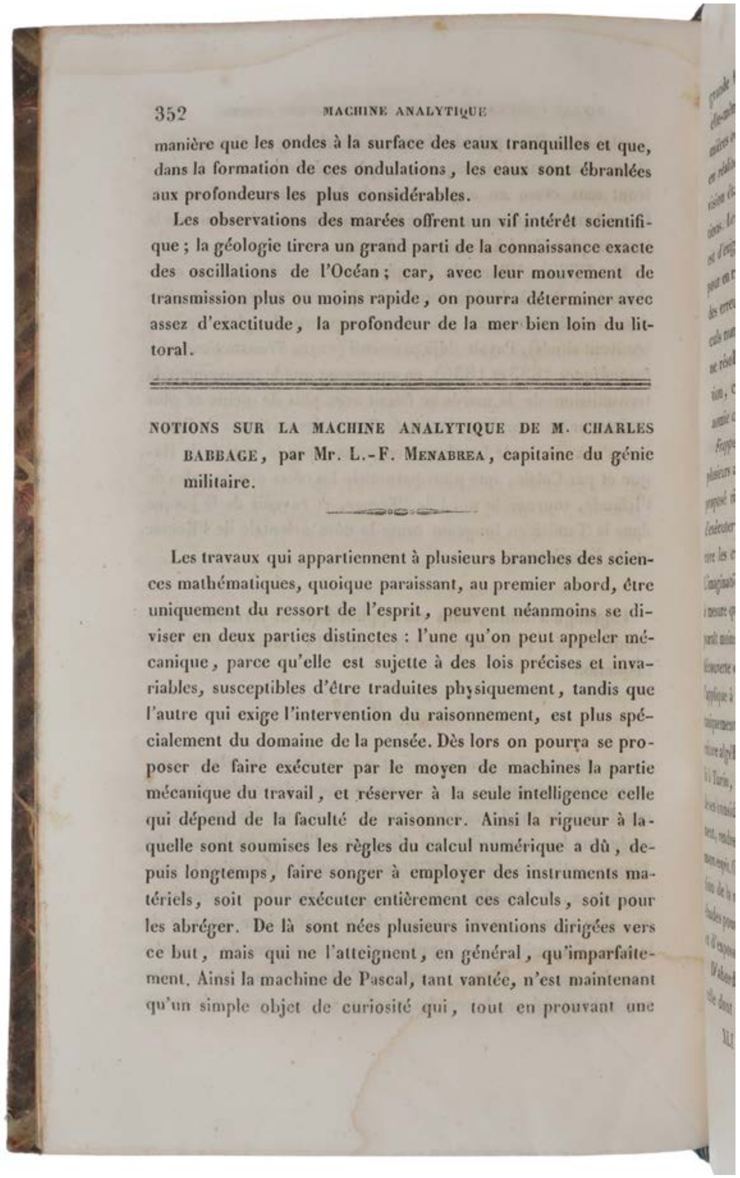
NOMBRE des OPÉRATIONS.	CARTONS des OPÉRATIONS. — Signes indiquant la nature des opérations.	CARTONS DES VARIABLES. Colonne soumise aux opérations.	Colonne recevant le résultat des opérations	MARCHE des OPÉRATIONS.
1	$\times$	$V_2 \times V_4 =$	V_8 …….	$=dn'$
2	$\times$	$V_5 \times V_1 =$	V_9 …….	$=d'n$
3	$\times$	$V_4 \times V_0 =$	V_{10}…….	$=n'm$
4	$\times$	$V_1 \times V_3 =$	V_{11}…….	$=nm'$
5	$-$	$V_8 - V_9 =$	V_{12}…….	$=dn'-d'n$
6	$-$	$V_{10} - V_{11} =$	V_{13}…….	$=n'm-n'm'$
7	$:$	$\frac{V_{12}}{V_{13}} =$	V_{14}…….	$=x=\frac{dn'-d'n}{mn'-m'n}$

Comme les cartons ne font qu'indiquer comment et sur quelles colonnes la machine doit agir, il est clair qu'il faudra encore, dans chaque cas particulier, introduire les données numériques du calcul. Ainsi, dans l'exemple que nous avons choisi, on devra préalablement écrire les valeurs numériques de m, n, d, m', n', d' dans l'ordre et sur les colonnes indiquées, après quoi l'on fera agir la machine qui donnera la valeur de l'inconnue x pour ce cas particulier. Pour avoir la valeur de y, il faudra faire une autre série d'opérations analogues aux précédentes. Mais on voit qu'elles se réduiront à quatre seulement, car le dénominateur de l'expression de y, sauf le signe, est le même que celui de x, et égal à $m'n - mn'$. Dans le tableau précédent l'on remarquera que la colonne des opérations indique de suite quatre *multiplications*, deux *soustractions* et une *division*. Ainsi l'on pourra, au besoin, n'employer que trois cartons des opérations; pour cela il suffira d'introduire dans la machine un appareil qui, par exemple, après la première multiplication, retienne le carton relatif à cette opération, et ne lui permette d'avancer, pour être remplacé par previous calculations. By a series of particular examples, Babbage gradually led his audience to understand and accept the general principles of his engine. In particular, he explained how the machine could, as a result of its own calculations, advance or back the operation cards, which controlled the sequence of operations of the Engine, by any required number of steps. This was perhaps the crucial point: only one example of conditional operations within the Engine, it was a big step in the direction of the stored program, so familiar today to the tens of millions of people who use electronic computers.

"In explaining the Engines Babbage was forced to put his thoughts into ordinary language; and, as discussion proceeded his own ideas crystallized and developed. At first Plana had intended to make notes of the discussions so that he could prepare a description of the principles of the Engines. But Plana was old, his letters of the time are in a shaky hand, and the task fell upon a young mathematician called Menabrea, later to be Prime Minister of the newly united Italy. It is interesting to reflect that no one remotely approaching Menabrea in scientific competence has ever been Prime Minister of Britain …

"Babbage's primary object in attending the Turin meeting had been to secure understanding and recognition for the Analytical Engine. He hoped that Plana would make a brief formal report on the Engine to the Academy of Turin and that Menabrea would soon complete his article. Babbage sent him further explanations to complement the notes he had made during Babbage's exposition and the discussions in Turin. Babbage had certainly little hope of government comprehension or support in England but he was determined not to miss the slightest opportunity of securing recognition for his Engines.

"He set down his own thoughts in a letter written at about this time to Angelo Sismoda, whom he had often seen during the Turin meeting: 'The discovery of



352 MACHINE ANALYTIQUE

manière que les ondes à la surface des eaux tranquilles et que, dans la formation de ces ondulations, les eaux sont ébranlées aux profondeurs les plus considérables.

Les observations des marées offrent un vif intérêt scientifique ; la géologie tirera un grand parti de la connaissance exacte des oscillations de l'Océan ; car, avec leur mouvement de transmission plus ou moins rapide, on pourra déterminer avec assez d'exactitude, la profondeur de la mer bien loin du littoral.

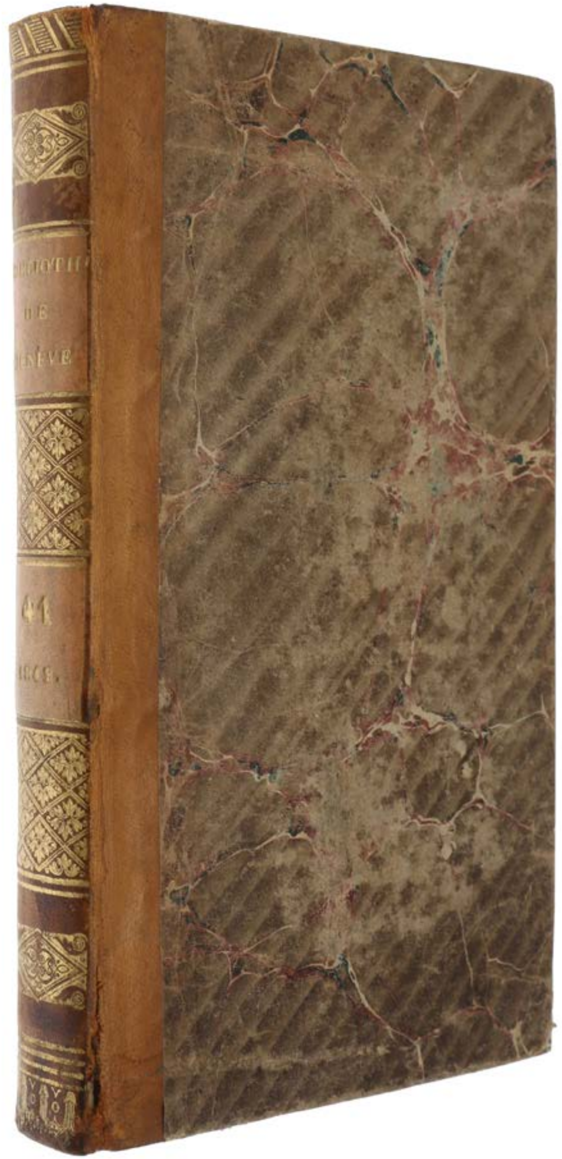
NOTIONS SUR LA MACHINE ANALYTIQUE DE M. CHARLES BABBAGE, par Mr. L.-F. Menabrea, capitaine du génie militaire.

Les travaux qui appartiennent à plusieurs branches des sciences mathématiques, quoique paraissant, au premier abord, être uniquement du ressort de l'esprit, peuvent néanmoins se diviser en deux parties distinctes : l'une qu'on peut appeler mécanique, parce qu'elle est sujette à des lois précises et invariables, susceptibles d'être traduites physiquement, tandis que l'autre qui exige l'intervention du raisonnement, est plus spécialement du domaine de la pensée. Dès lors on pourra se proposer de faire exécuter par le moyen de machines la partie mécanique du travail, et réserver à la seule intelligence celle qui dépend de la faculté de raisonner. Ainsi la rigueur à laquelle sont soumises les règles du calcul numérique a dû, depuis longtemps, faire songer à employer des instruments matériels, soit pour exécuter entièrement ces calculs, soit pour les abréger. De là sont nées plusieurs inventions dirigées vers ce but, mais qui ne l'atteignent, en général, qu'imparfaitement. Ainsi la machine de Pascal, tant vantée, n'est maintenant qu'un simple objet de curiosité qui, tout en prouvant une the Analytical Engine is so much in advance of my own country, and I fear even of the age, that it is very important for its success that the fact should not rest upon my unsupported testimony. I therefore selected the meeting at Turin as the time of its publication, partly from the celebrity of its academy and partly from my high estimation of Plana, and I hoped that a report on the principles on which it is formed would have been already made to the Royal Academy.' But Plana was old and ill: no report was forthcoming …

"Babbage returned from the sunny hills and valleys of Tuscany where he had basked in Ducal warmth and the approbation of philosophers to a chilly climate in England. He sent further explanations to Menabrea who in turn entirely rewrote the article. On 27 January 1842 Menabrea wrote to Babbage from Turin: 'Je donnerai la dernière main a l'écrit qui vous concerne et j'espère dans quelques jours l'envoyer a Genève au bureau de la *Bibliothèque Universelle*.' In number 82 of October 1842 the article finally appeared" (Hyman, *Charles Babbage* (1982), pp. 181-190).

"Menabrea's 23-page paper was translated into English the following year by Lord Byron's daughter, Augusta Ada King, Countess of Lovelace, daughter of Lord Byron, who, in collaboration with Babbage, added a series of lengthy notes enlarging on the intended design and operation of Babbage's machine. Menabrea's paper and Ada Lovelace's translation represent the only detailed publications on the Analytical Engine before Babbage's account in his autobiography (1864). Menabrea himself wrote only two other very brief articles about the Analytical Engine in 1855, primarily concerning his gratification that Countess Lovelace had translated his paper" (historyofinformation.com).

Hook & Norman, *Origins of Cyberspace* (2002), No. 60.

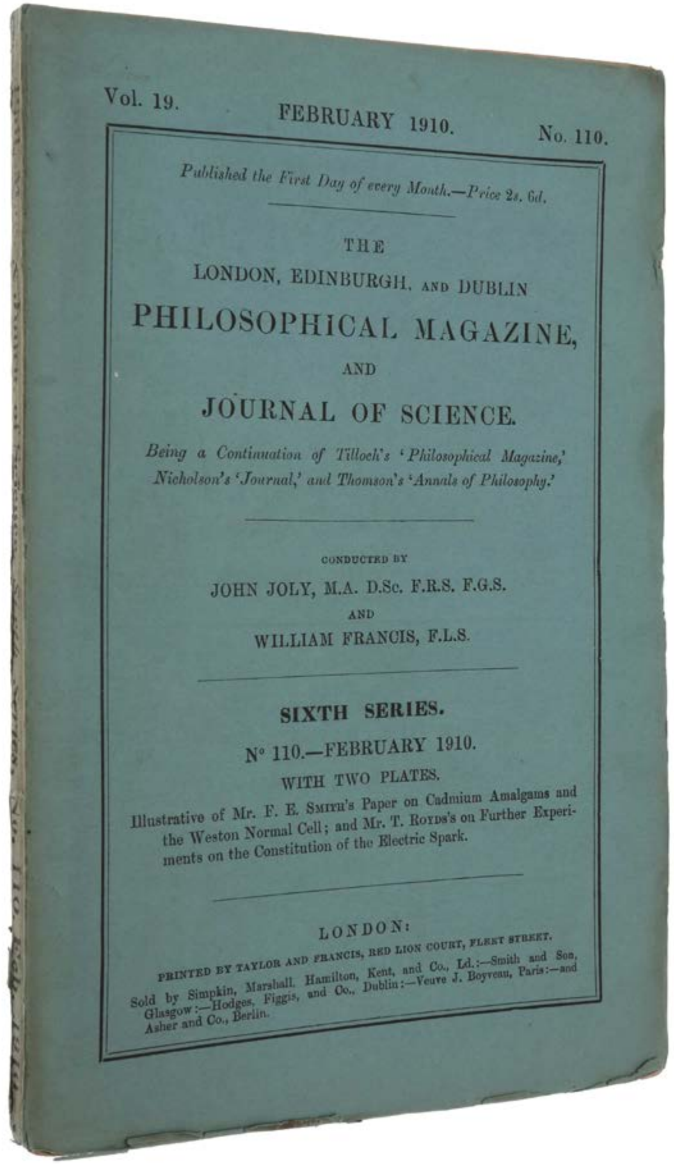


THE FIRST AMERICAN-BORN NOBEL LAUREATE

MILLIKAN, Robert Andrews. *A New Modification of the Cloud Method of Determining the Elementary Electrical Charge and the Most Probable Value of that Charge.* London: Taylor & Francis, 1910.

$4,500

Contained in: The Philosophical Magazine for February 1910, vol 19, no. 110, pp. 209-228. The entire issue offered here, uncut and unopened in the original blue printed wrappers (spine strip with some very good restoration, hardly noticible). 8vo (225 x 147 mm). Rare in such fine condition.

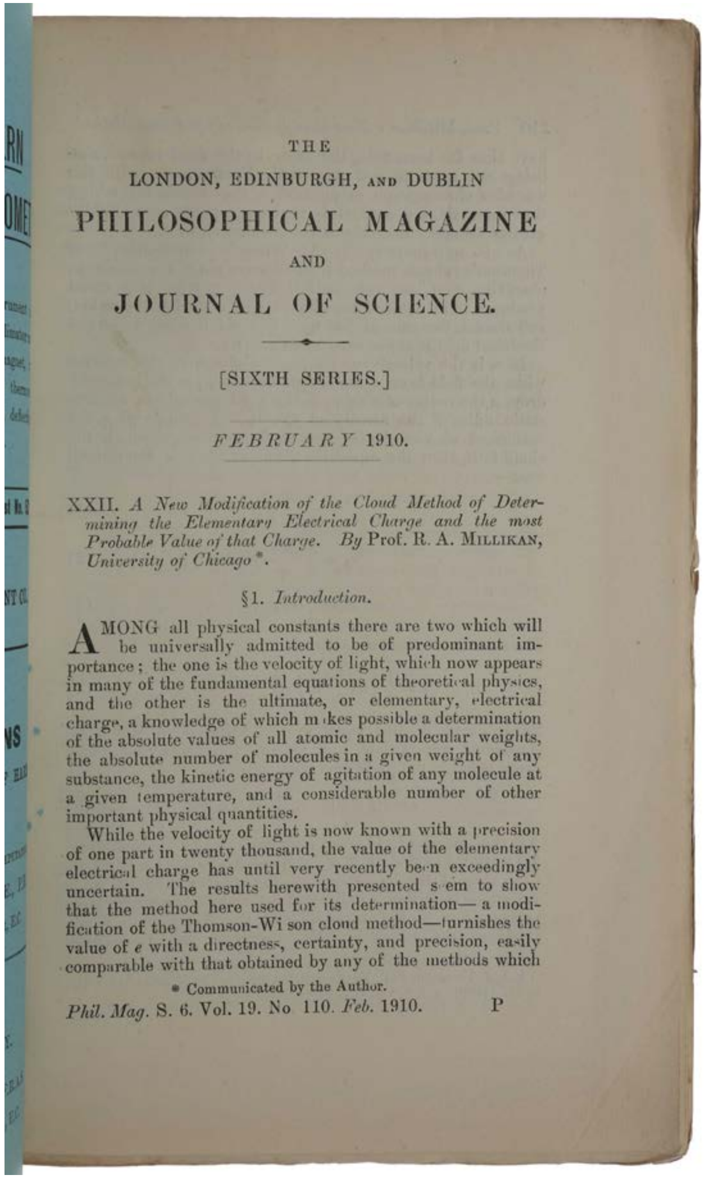


A fine copy of Millikan's famous experiment, later known as the 'oil-drop experiment', in which he first provided the definitive proof that all electrical charges are exact multiples of a definite, fundamental value—the charge of the electron. Millikan's experiment is nowadays known as the 'oil-drop experiment' due to a later improvement by Millikan and his student Harvey Fletcher in 1910 – using oil in the cloud chamber – but it was in this paper (although water and alcohol were the liquids used) that Millikan first made precise measurements of the charge on single isolated droplets instead of as earlier just statistical averages on the surface of clouds of droplets.

Although important, the fundamental breakthrough in Millikan's work was not his measurement of the actual value of the electron's charge (in fact he was as close to the correct value in this paper dated October 1909 as he was in the later oil experiment of 1910), but the fact that Millikan was able to produce, isolate, and observe single droplets with electrical charges, and show that repeated measurements of the charges always revealed exact integral multiples of one fundamental unity value. Previous experiments by Thomson and Wilson had in fact revealed the same value of the electron's charge as Millikan's experiment did but their determinations were based on statistical averages on the surface of large clouds of numerous water droplets and repeated measurements on the clouds gave fractional values of the electron's charge. This fact implied to some antiatomistic Continental physicists that it was not the constant of a unique particle but a statistical average of diverse electrical energies. However, in this 1909 experiment Millikan showed that his single droplets could not hold a fractional charge of the electron's but always had a charge that was an exact integral multiple of the electron's (e.g., *2e, 3e, 4e, …*). In 1910 Millikan and Fletcher improved and simplified the whole experiment by using oil, mercury, and glycerin as liquids instead of water; they could now observe the droplets for several hours instead of just under one minute and also neglect having to compensate for the evaporation of the water and alcohol droplets. And thus the experiment became known as the 'oil-drop experiment', but the crucial breakthrough had already taken place in this 1909 experiment.

In this paper Millikan emphasized that the very nature of his data refuted conclusively the minority of scientists who still held that electrons (and perhaps atoms too) were not necessarily fundamental, discrete particles. And he provided a value for the electronic charge which, when inserted in Niels Bohr's theoretical formula for the hydrogen spectrum, accurately gave the Rydberg constant—the first and most convincing proof of Bohr's quantum theory of the atom.

'Among all physical constants there are two which will be universally admitted to be of predominant importance; the one is the velocity of light, which now appears



THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[SIXTH SERIES.]

FEBRUARY 1910.

XXII. *A New Modification of the Cloud Method of Determining the Elementary Electrical Charge and the most Probable Value of that Charge. By* Prof. R. A. Millikan, *University of Chicago* *.

§1. *Introduction.*

AMONG all physical constants there are two which will be universally admitted to be of predominant importance; the one is the velocity of light, which now appears in many of the fundamental equations of theoretical physics, and the other is the ultimate, or elementary, electrical charge, a knowledge of which makes possible a determination of the absolute values of all atomic and molecular weights, the absolute number of molecules in a given weight of any substance, the kinetic energy of agitation of any molecule at a given temperature, and a considerable number of other important physical quantities.

While the velocity of light is now known with a precision of one part in twenty thousand, the value of the elementary electrical charge has until very recently been exceedingly uncertain. The results herewith presented seem to show that the method here used for its determination— a modification of the Thomson-Wilson cloud method—furnishes the value of *e* with a directness, certainty, and precision, easily comparable with that obtained by any of the methods which

* Communicated by the Author.

Phil. Mag. S. 6. Vol. 19. No 110. *Feb.* 1910. P *in many of the fundamental equations of theoretical physics, and the other is the ultimate, or elementary, electrical charge, a knowledge of which makes possible a determination of the absolute values of all atomic and molecular weights, the absolute number of molecules in a given weight of any substance, the kinetic energy of agitation of any molecule at a given temperature, and a considerable number of other important physical quantities.*' (First paragraph of the offered paper).

In 1923 Millikan became the first American-born Nobel laureate for this work together with his 1916 determination of Planck's constant on the basis of Einstein's theory of the photoelectric effect.

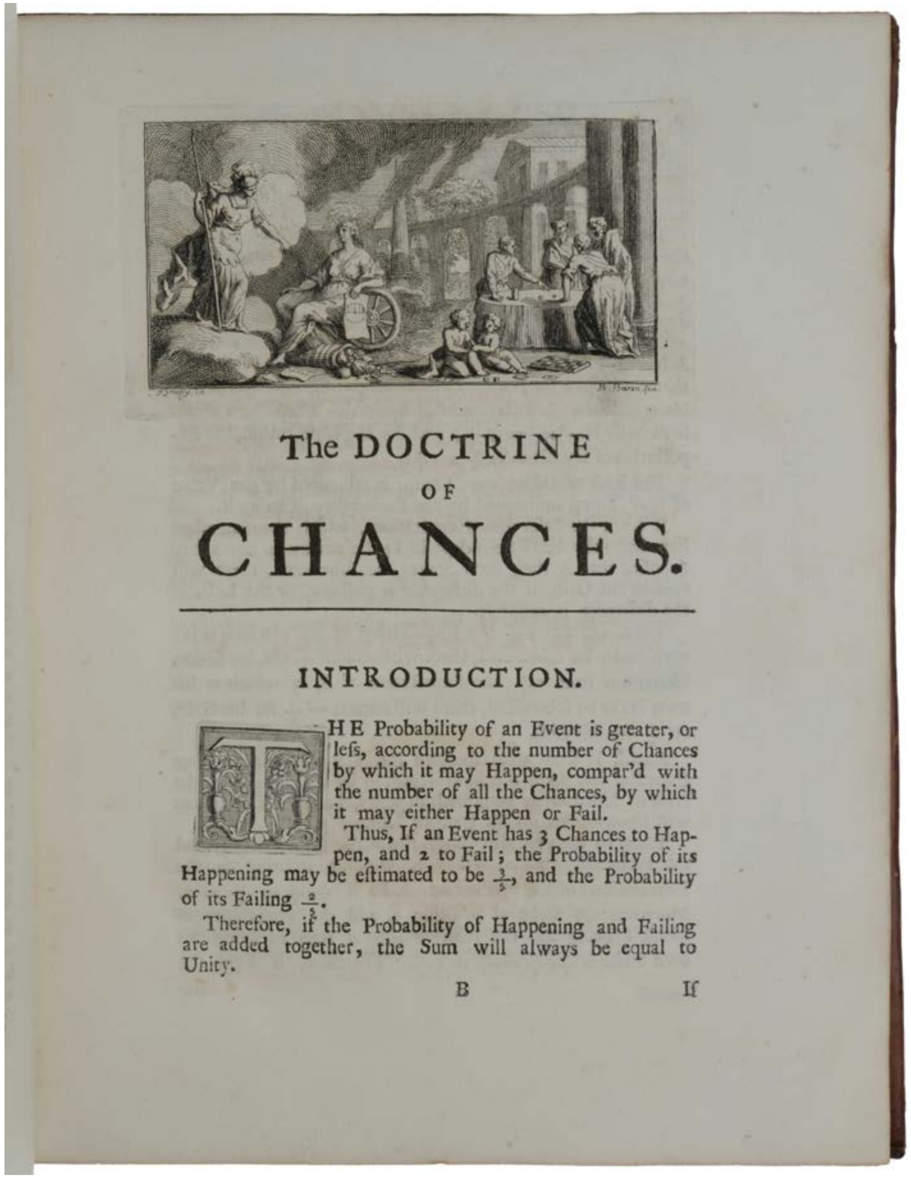
THE FOUNDING WORK OF THE FIELD OF PROBABILITY AND STATISTICS

MOIVRE, Abraham de. *The Doctrine of Chances: or, A Method of Calculating the Probability of Events in Play.* London: W[illiam]. Pearson for the Author, 1718.

$18,500

Large 4to, pp. [iv], xiv, 175, [1, blank]. Engraved vignette on title and engraved head- & tailpieces. Contemporary panelled calf, spine richly gilt in compartments with red-lettering piece. Preserved in a black morocco-backed folding box, gilt lettering on spine.

First edition, and an unusually fine copy without any restoration, of this classic on the theory of probability, the first original work on the subject in English. "De Moivre's book on chances is considered the foundation for the field of probability and statistics" (Tomash). "De Moivre's masterpiece is *The Doctrine of Chances*" (DSB). "His work on the theory of probability surpasses anything done by any other mathematician except P. S. Laplace. His principal contributions are his investigations respecting the Duration of Play, his Theory of Recurring Series, and his extension of the value of Daniel Bernoulli's theorem by the aid of Stirling's theorem" (Cajori, *A History of Mathematics*, p. 230). "He was among the intimate friends of Newton, to whom this book is dedicated. It is the second book devoted entirely to the theory of probability and a classic on the subject" (Babson 181). De Moivre's interest in probability was raised by Pierre-Rémond de Montmort's *Essay d'analyse sur les jeux de hazard* (1708), the first separately-published work on probability. "The [*Doctrine*] is in part the result of a competition between De



The DOCTRINE OF CHANCES.

INTRODUCTION.

THE Probability of an Event is greater, or lefs, according to the number of Chances by which it may Happen, compar'd with the number of all the Chances, by which it may either Happen or Fail.

Thus, If an Event has 3 Chances to Happen, and 2 to Fail; the Probability of its Happening may be eftimated to be $\frac{3}{5}$, and the Probability of its Failing $\frac{2}{5}$.

Therefore, if the Probability of Happening and Failing are added together, the Sum will always be equal to Unity.

B If

Moivre on the one hand and Montmort together with Nikolaus Bernoulli on the other. De Moivre claimed that his representation of the solutions of the then current problems tended to be more general than those of Montmort, which Montmort resented very much. This situation led to some arguments between the two men, which finally were resolved by Montmort's premature death in 1719 … De Moivre had developed algebraic and analytical tools for the theory of probability like a 'new algebra' for the solution of the problem of coincidences which somewhat foreshadowed Boolean algebra, and also the method of generating functions or the theory of recurrent series for the solution of difference equations. Differently from Montmort, De Moivre offered in [*Doctrine*] an introduction that contains the main concepts like probability, conditional probability, expectation, dependent and independent events, the multiplication rule, and the binomial distribution" (Schneider, p. 106).

Provenance: Nathaniel Cholmley (1721-91), British Member of Parliament from 1756 to 1774 (bookplate on front paste-down). Erwin Tomash (book label on front paste-down).

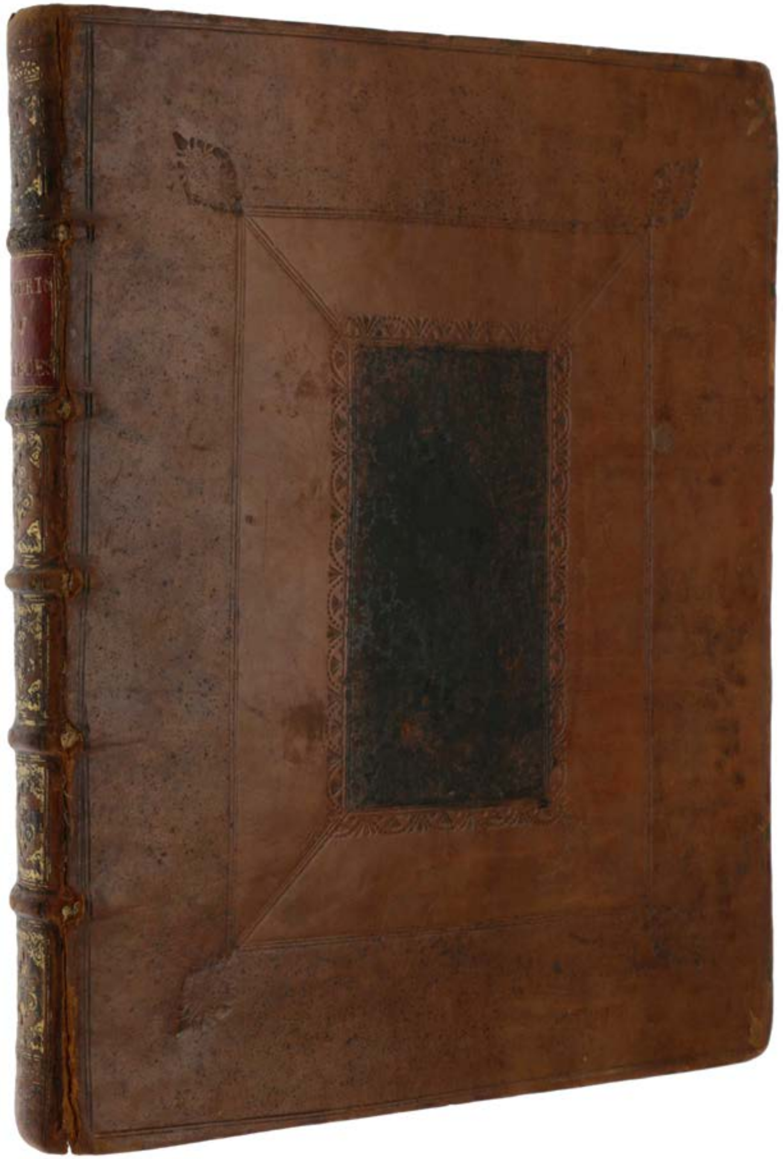
The modern theory of probability is generally agreed to have begun with the correspondence between Pierre de Fermat and Blaise Pascal in 1654 on the solution of the 'Problem of points'. Pascal included his solution as the third section of the second part of his 36-page *Traité du triangle arithmétique* (1665), which was essentially a treatise on pure mathematics. "Huygens heard about Pascal's and Fermat's ideas [on games of chance] but had to work out the details for himself. His treatise *De ratiociniis in ludo aleae* … essentially followed Pascal's method of expectation. … At the end of his treatise, Huygens listed five problems about fair odds in games of chance, some of which had already been solved by Pascal and Fermat. These problems, together with similar questions inspired by other card and dice games popular at the time, set an agenda for research that continued for nearly a century. The most important landmarks of this work are [Jakob] Bernoulli's *Ars conjectandi* (1713), Montmort's *Essay d'analyse sur les jeux de hazard* (editions in 1708 and 1711 [i.e., 1713]) and De Moivre's *Doctrine of Chances* (editions in 1718, 1738, and 1756). These authors investigated many of the problems still studied under the heading of discrete probability, including gamblers ruin, duration of play, handicaps, coincidences and runs. In order to solve these problems, they improved Pascal and Fermat's combinatorial reasoning, summed infinite series, developed the method of inclusion and exclusion, and developed methods for solving the linear difference equations that arise in using Pascal's method of expectations." (Glenn Schafer in *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences* (1994), Grattan-Guiness (ed.), p. 1296).

"De Moivre's earliest book on probability, the first edition of the *Doctrine of Chances*, was an expansion of a long (fifty-two pages) memoir he had published in Latin in the *Philosophical Transactions of the Royal Society* in 1711 under the title 'De mensura sortis' (literally, 'On the measurement of lots'). De Moivre tells us that in 1711 he had read only Huygens' 1657 tract *De Ratiociniis in Ludo Aleae* and an anonymous English 1692 tract based on Huygens' work (now known to have been written by John Arbuthnot). By 1718 he had encountered both Montmort's *Essay d'analyse sur les jeux de hazard* (2nd ed., 1713) and Bernoulli's *Ars Conjectandi* (1713), although the latter had no pronounced effect on De Moivre at that early date" (Stigler, p. 71).

The *Doctrine* consists of an introduction with definitions and elementary theorems, followed by a series of numbered problems. "De Moivre begins with the classical measure of probability, 'a fraction whereof the numerator be the number of chances whereby an event may happen, and the denominator the number of all the chances whereby it may either happen or fail'. He gives the summation rule for probabilities of disjunct events explicitly only for the case of the happening and the not happening of an event. Expectation is still on the level of Huygens defined as the product of an expected sum of money and the probability of obtaining it, the expectation of several sums is determined by the sum of the expectations of the singular sums. He defines independent and dependent events and gives the multiplication rule for both. But whereas today the criterion for independence of two events is the validity of the multiplication rule in the [*Doctrine*], the multiplication rule follows from the independence of the events, which seems to be a self-evident concept for De Moivre …

"With these tools 'those who are acquainted with Arithmetical Operations' (as De Moivre remarked in the preface) could tackle many problems, in part already well known but which he gradually generalized. Because the majority of the solved problems depends on rules 'being entirely owing to Algebra' and to combinatorics, De Moivre tried to convince those readers who had not studied algebra yet to 'take the small Pains of being acquainted with the bare Notation of Algebra, which might be done in the hundredth part of the Time that is spent in learning to write Short-hand'. Remarks of this kind are typical of the private teacher of mathematics De Moivre, who was accustomed to ask his clients before he began with his instructions about their mathematical knowledge" (Schneider, pp. 107-9).

Following the introduction are 53 numbered problems: I-XIV are various problems solvable with the rules contained in the introduction including problems dealing with the games of Bassette (XIII) and Pharaon (XIV); XV-XXXII are problems solvable by combinatorial methods, including some dealing with lotteries (XXI



and XXII), and of Pharaon (XXIII); XXXIII-XLVI are concerned with the problem of the duration of play, or the ruin problem; and XLVII-LIII are further problems solvable by combinatorial methods, including Hazard (XLVII, LIII), Whisk (XLVIII), Raffling (XLIX) and Piquet (LI, LII).

"Some problems, as already stated by Jakob Bernoulli (1654–1705) in his *Ars conjectandi*, can be solved more easily by the use of infinite series. As an illustration de Moivre offers the problem to determine the amounts each of two players A and B has to stake under the condition that the player who throws the first time an Ace with an ordinary die wins the stake and that A has the first throw. He considers it as reasonable that A should pay 1/6 of the total stake in order to have the first throw, B should pay 1/6 of the rest which is 1/6.5/6 for having the second throw, A should pay 1/6 of the remainder for having the third throw, etc. The part that A has to stake altogether is the sum of a geometrical series with 1/6 as the first term and the quotient 25/36, which is 6/11 of the total stake. Accordingly B's share is 5/11 of the total stake. De Moivre claims that in most cases where the solution affords the application of infinite series the series are geometrical [in which each term is a fixed multiple of the preceding term]. The other kind of infinite series which relate to the problem of the duration of play are recurrent series the terms of which can be connected with the terms of geometrical series. Other problems depend on the summation of the terms of arithmetical series of higher orders and a 'new sort of algebra'" (Schneider, pp. 109-110).

Recurrent series – those in which each term of the series is related to a fixed number of preceding terms by a fixed (linear) relation – are needed in the solution of the problem of the duration of play. "It resulted from a generalization of the last problem that Huygens had posed to his readers at the end of his treatise *De ratiociniis in ludo aleae* (1656). The first to deal with the problem in the new form seems to be Montmort, and after him Nikolaus Bernoulli. De Moivre concerned himself with it at about the same time. His formulation of the problem in the [*Doctrine*] of 1718 is nearly the same as he used in the third edition (p. 191):

'Two gamesters A and B whose proportion of skill is as a to b, each having a certain number of pieces, play together on condition that as often as A wins a game, B shall give him one piece; and that as often as B wins a game, A shall give him one piece; and that they cease not to play till such time as either one or the other has got all the pieces of his adversary: now let us suppose two spectators R and S concerning themselves about the ending of the play, the first of them laying that the play will be ended in a certain number of games which he assigns, the other laying to the contrary. To find the probability that S has of winning his wager'" (Schneider, p. 112).

De Moivre gave a complete solution of the problem of duration of play in *Doctrine,* but he did not indicate how he had obtained the results, and this became a challenge to the next generation of probabilists, notably Laplace (see Hald, pp. 361 et seq).

One of the most important devices introduced by De Moivre is that of a 'generating function', later developed extensively by Laplace. De Moivre introduces generating functions in his solution of Problem III. "It asks after the number of chances to throw a given number $p + 1$ of points with n dice, each of them of the same number f of faces. Here the word 'dice' or 'die' is used in the more general sense of, for example, a roulette wheel with f sectors" (Schneider, p. 110). De Moivre introduced a series whose coefficients are the chances sought, and was able to determine the sum of the series, from which the chances were easily found. Indeed, he formed the series

$$f(r) = 1 + r + r^2 + \ldots + r^{f-1} = (1 - r^f)/(1 - r)$$

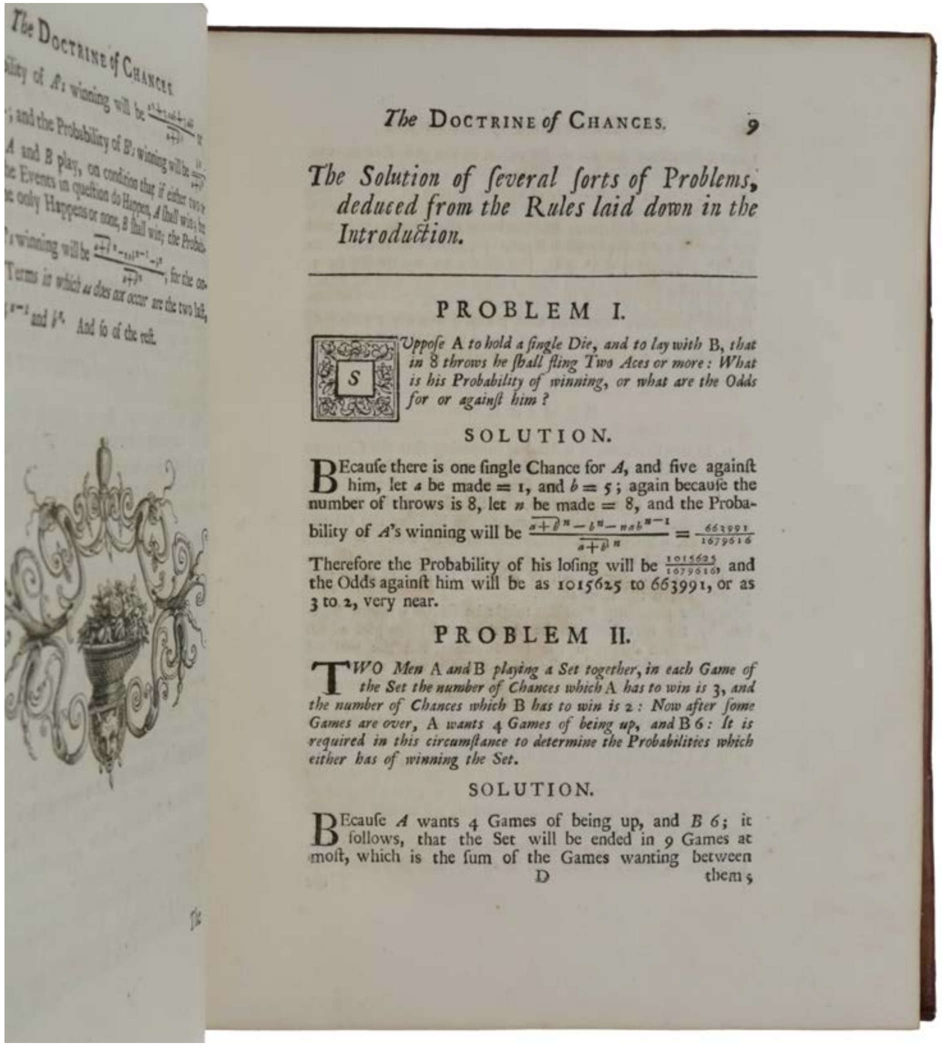
and noted that the number of chances required is equal to the coefficient of the term with exponent $p + 1 - n$ in the expansion of

$f(r)^n = (1 - r^f)^n.(1 - r)^{-n}$,

an expansion which is easily obtained using the binomial theorem.

"An early reaction to the book which surely accounts for the high estimation it was held at least in England is its exploitation by the Englishman Thomas Simpson, who in his *Treatise on the nature and laws of chance* (1740) just repeated the results achieved in the [*Doctrine*]. The fact that De Moivre had specialized in the theory of probability, for which he had prepared appropriate tools and to which he had contributed the solutions of the most interesting problems posed to him by his competitors and by his clients for some decades, made [*Doctrine*], especially the last edition of 1756, the most complete representation of the new field in the second half of the 18th century.

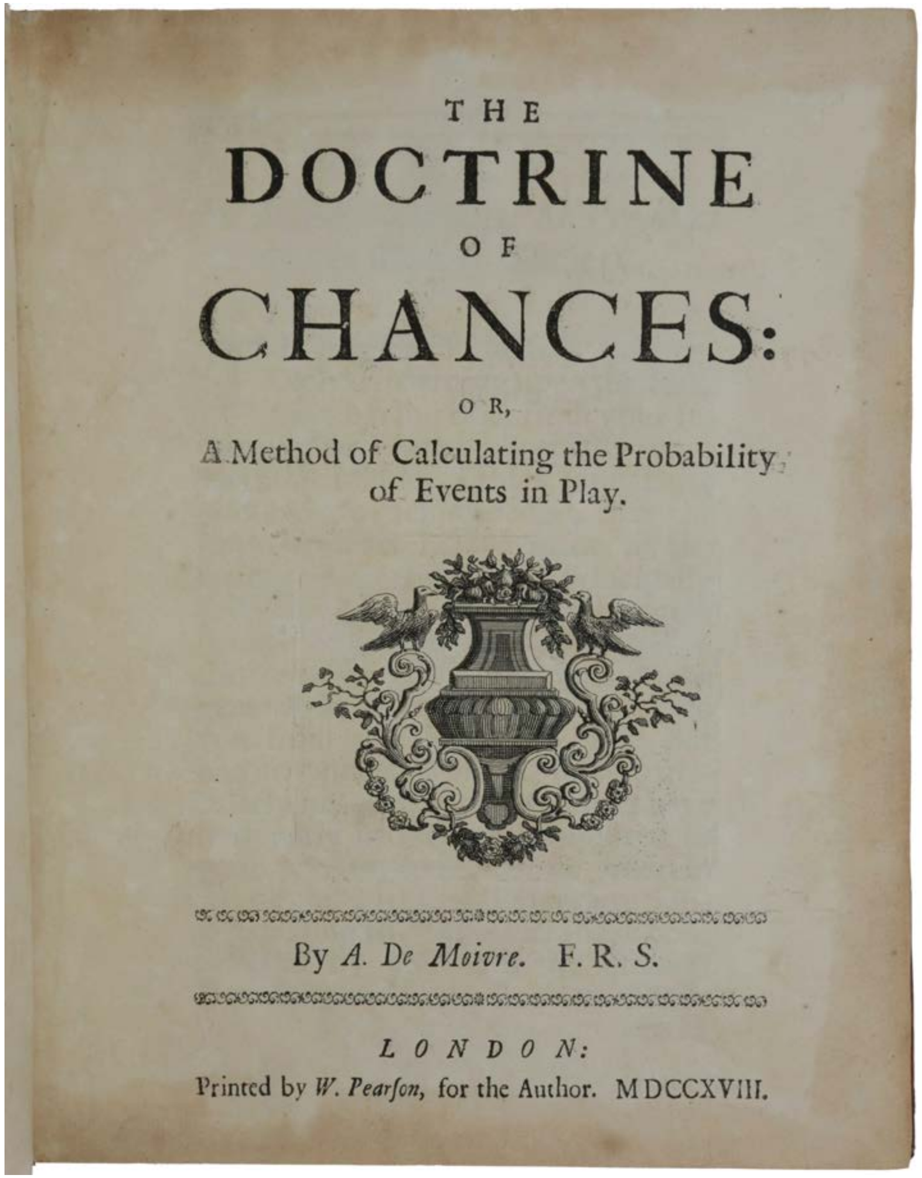
"This was felt by the leading mathematicians of the next generation. In particular, J.L. Lagrange and Laplace had planned a French translation of the book which however was never realized. Their interest goes back to De Moivre's solution of the problem of the duration of play by means of what he called 'recurrent series' and what amounts to the solution of a homogeneous linear difference equation with constant coefficients. In fact, the most effective analytical tool developed by Laplace for the calculus of probabilities, the theory of generating functions, is a consequence of his concern with recurrent series. Indeed, the most important results of the book reappear in Laplace's probability theory in a new mathematical form and in a new philosophical context. This, more than anything else, confirms de Moivre's status as a pioneer in the field and as a predecessor of Laplace" (Schneider, p. 119).



At the top of page 1 of the text is an engraving which De Moivre himself had designed. It shows Minerva, on the left of the picture, pointing to a piece of paper with a circle on it; this alluded to his solution to the problem of the duration of play, the details of which he had withheld in the book. The piece of paper is held by Fortuna, the goddess of fortune. She is identified by the wheel of fortune behind her and the cornucopia at her feet. With Minerva standing at a dominant position over Fortuna, the interpretation is that De Moivre's mathematical results dominate fickle fortune or fate. The paper under the cornucopia has some illegible writing on it. It may represent some previous work that has borne fruit, perhaps referring to Huygens' original results in *De ratiociniis*. On the right of the picture four men stand around a table with dice and a dice box on it. The clean-shaven man is De Moivre; he is instructing the other men on the theory of probability. A similar engraving is found at the beginning of Montmort's *Essay*, but there it is the God Mercury standing at the table watching a man and a woman play a game of dice. Thus De Moivre is taking a swipe at Montmort, expressing through the engraving that he does not have the effrontery to speak directly to the gods and instruct them. The middle part of the engraving has two additional swipes at Montmort. Two naked boys are sitting with a pair of dice at their feet. A short distance away are some discarded cards and further yet is a chessboard of size 4 x 6 squares rather than the standard 8 x 8 shown in Montmort's engraving. One of the boys is reading a book, perhaps *Doctrine of Chances*, to the other explaining De Moivre's newly discovered results in probability. The discarded chessboard, being incomplete, is an indication that the work in Montmort's *Essay* is also incomplete.

Abraham Moivre stemmed from a Protestant family. His father was a surgeon from Vitry-le-François in the Champagne. From the age of five to eleven he was educated by the Catholic Péres de la doctrine Chrètienne. Then he moved to the Protestant Academy at Sedan were he mainly studied Greek. After the latter was forced to close in 1681 for its profession of faith, Moivre continued his studies at Saumur between 1682 and 1684 before joining his parents who had meanwhile moved to Paris. At that time he had studied some books on elementary mathematics and the first six books of Euclid's elements. He had even tried his hand at Huygens' 1657 tract without mastering it completely. In Paris he was taught mathematics by Jacques Ozanam who had made a reputation from a series of books on practical mathematics and mathematical recreations. Ozanam made his living as a private teacher of mathematics. He had extended the usual teachings of the European reckoning masters and mathematical practitioners by what was considered fashionable mathematics in Paris. Ozanam enjoyed a moderate financial success due to the many students he attracted. It seems plausible that young Moivre took him as a model he wanted to follow when he had to support himself. After the revocation of the Edict of Nantes in 1685 the Protestant faith was no longer tolerated in France, and hundreds of thousands of Huguenots who had refused to convert to Catholicism emigrated to Protestant countries. Amongst them was Moivre who arrived in England in 1687. There he began his occupation as a tutor in mathematics. He also added a 'De' to his name, probably because he wanted to take advantage of the prestige of a (pretended) noble birth in France in dealing with his clients, many of whom were noblemen. An anecdote from this time which goes back to (De) Moivre himself tells that he cut out the pages of Newton's *Principia* of 1687 and read them while waiting for his students or walking from one to the other – the main function of this anecdote was to demonstrate that De Moivre was amongst the first true and loyal Newtonians and that as such he deserved help and protection in order to gain a better position than that of a humble tutor of mathematics. In 1692 De Moivre met with Edmond Halley and shortly afterwards with Newton. Halley ensured the publication of De Moivre's first paper on Newton's doctrine of fluxions in the *Philosophical Transactions* for 1695 and saw to his election to the Royal Society in 1697. Newton's influence concerning university positions in mathematics and natural philosophy persuaded De Moivre to engage in the solution of problems posed by the new infinitesimal calculus. In 1697 and 1698 he published the polynomial theorem, a generalization of Newton's binomial theorem, together with application in the theory of series. In 1704 De Moivre began a correspondence with Johann Bernoulli, but Bernoulli's letters showed De Moivre that he lacked the time and perhaps the mathematical power to compete with a mathematician of this calibre in the new field of analysis. De Moivre ceased his correspondence with Bernoulli after he was made a member of the Royal Society commission to adjudicate in the priority dispute between Newton and Leibniz over the invention of calculus – continuing the correspondence may have made him appear disloyal to the Newtonian cause. When the Lucasian chair in mathematics at Cambridge was given in 1711 on Newton's recommendation to Nicholas Saunderson, De Moivre realized that this only option was to continue his occupation as a tutor and consultant in mathematical affairs in the world of the coffee houses where he met his clients; additional income he could draw from the publication of books and from translations. He therefore turned to the calculus of games of chance and probability theory which was of great interest for many of his students and where he had few competitors in England.

Babson/Newton 181; ESTC T33065; Goldsmiths'-Kress 05509.2-1; Norman 1529; Tomash M114 (this copy). Bellhouse, *Abraham de Moivre* (see pp. 114-8 for a discussion of the engraved vignette). Hald, *History of Probability and Statistics and their Applications before 1750*, Chapter 22. Schneider, 'Abraham de Moivre, *The Doctrine of Chances* (1718, 1738, 1756),' Chapter 7 in *Landmark Writings in Western Mathematics 1640-1940*, Grattan-Guinness (ed.). Stigler, *The History of Statistics*, pp. 70-85.



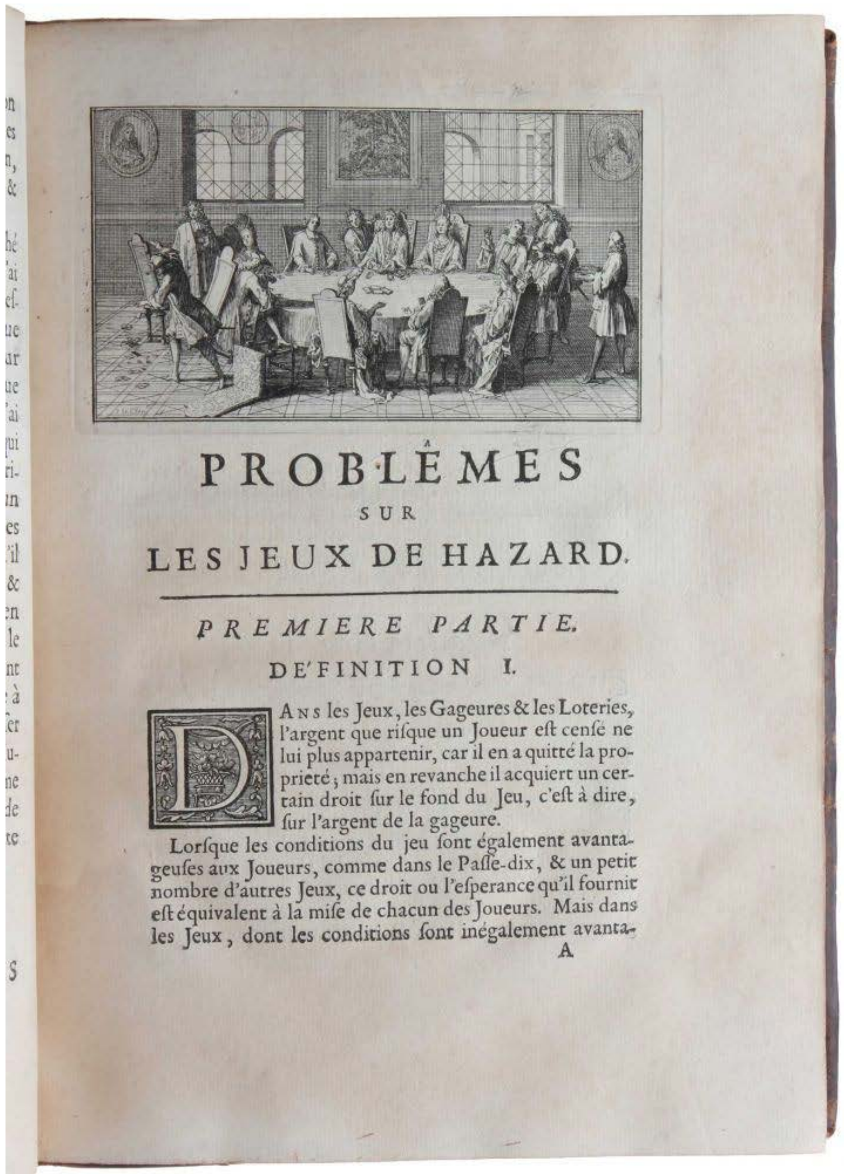
THE EARLIEST BOOK DEDICATED ENTIRELY TO PROBABILTY

[MONTMORT, Pierre Rémond de]. *Essay d'Analyse sur les Jeux de Hazard.* Paris: Jacque Quillau, 1708.

$14,000

4to (249 x 180 mm), pp. xxiv, 189, [3]. Contemporary calf, spine richly gilt, some light wear to joints and spine. Engraved vignette on title, author's name added in fine manuscript (the work was published anonymously), several headpieces showing gambling scenes, and two engraved figures in text of the backgammon board (light browning to some gatherings, old library stamp partially removed from title).

First edition, first issue, very rare, of the first separately published textbook of probability. This issue has significant textual differences from what is usually referred to as the first edition. "In 1708 [Montmort] published his work on Chances, where with the courage of Columbus he revealed a new world to mathematicians" (Todhunter, p. 78). "The *Essay* (1708) is the first published comprehensive text on probability theory, and it represents a considerable advance compared with the treatises of Huygens (1657) and Pascal (1665). Montmort continues in a masterly way the work of Pascal on combinatorics and its application to the solution of problems on games of chance. He also makes effective use of the methods of recursion and analysis to solve much more difficult problems than those discussed by Huygens. Finally, he uses the method of infinite series, as indicated by Bernoulli (1690)" (Hald, p. 290). "Montmort's book on probability, *Essay d'analyse sur les jeux de hazard*, which came out in 1708, made his reputation among scientists"



(DSB). Based on the problems set forth by Huygens in his *De Ratiociniis in Ludo Aleae* (1657) (published as an appendix to Frans van Schooten's *Exercitationum mathematicarum*), the *Essay* spawned Abraham de Moivre's two important works *De Mensura Sortis* (1711) and *Doctrine of Chances* (1718). ABPC/RBH record the sale of just three other copies of the first edition (Christie's 1981, Hartung 1987 and the Tomash copy). As Sotheby's correctly noted in the Tomash library sale catalogue (18 September 2018, lot 434), "This book was first issued in 1708 without illustrations and an uncorrected text," and indeed the three large folding tables found in the regular issue are not present in this first issue, which also has a shorter list of errata than the regular issue. The existence of two textually different issues of this work, both published in 1708, has not, as far as we are aware, been noted in the academic literature.

The modern theory of probability is generally agreed to have begun with the correspondence between Pierre de Fermat and Blaise Pascal in 1654 on the solution of the 'Problem of points'; this was published in Fermat's *Varia Opera* (1679). Pascal included his solution as the third section of the second part of his 36-page *Traité du triangle arithmétique* (1665), which was essentially a treatise on pure mathematics. "Huygens heard about Pascal's and Fermat's ideas [on games of chance] but had to work out the details for himself. His treatise *De ratiociniis in ludo aleae* … essentially followed Pascal's method of expectation. … At the end of his treatise, Huygens listed five problems about fair odds in games of chance, some of which had already been solved by Pascal and Fermat. These problems, together with similar questions inspired by other card and dice games popular at the time, set an agenda for research that continued for nearly a century. The most important landmarks of this work are Bernoulli's *Ars conjectandi* (1713), Montmort's *Essay d'analyse sur les jeux de hazard* (editions in 1708 and 1711 [i.e., 1713]) and De Moivre's *Doctrine of Chances* (editions in 1718, 1738, and 1756). These authors investigated many of the problems still studied under the heading of discrete probability, including gamblers ruin, duration of play, handicaps, coincidences and runs. In order to solve these problems, they improved Pascal and Fermat's combinatorial reasoning, summed infinite series, developed the method of inclusion and exclusion, and developed methods for solving the linear difference equations that arise in using Pascal's method of expectations." (Glenn Schafer in *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences* (1994), Grattan-Guiness (ed.), p. 1296).

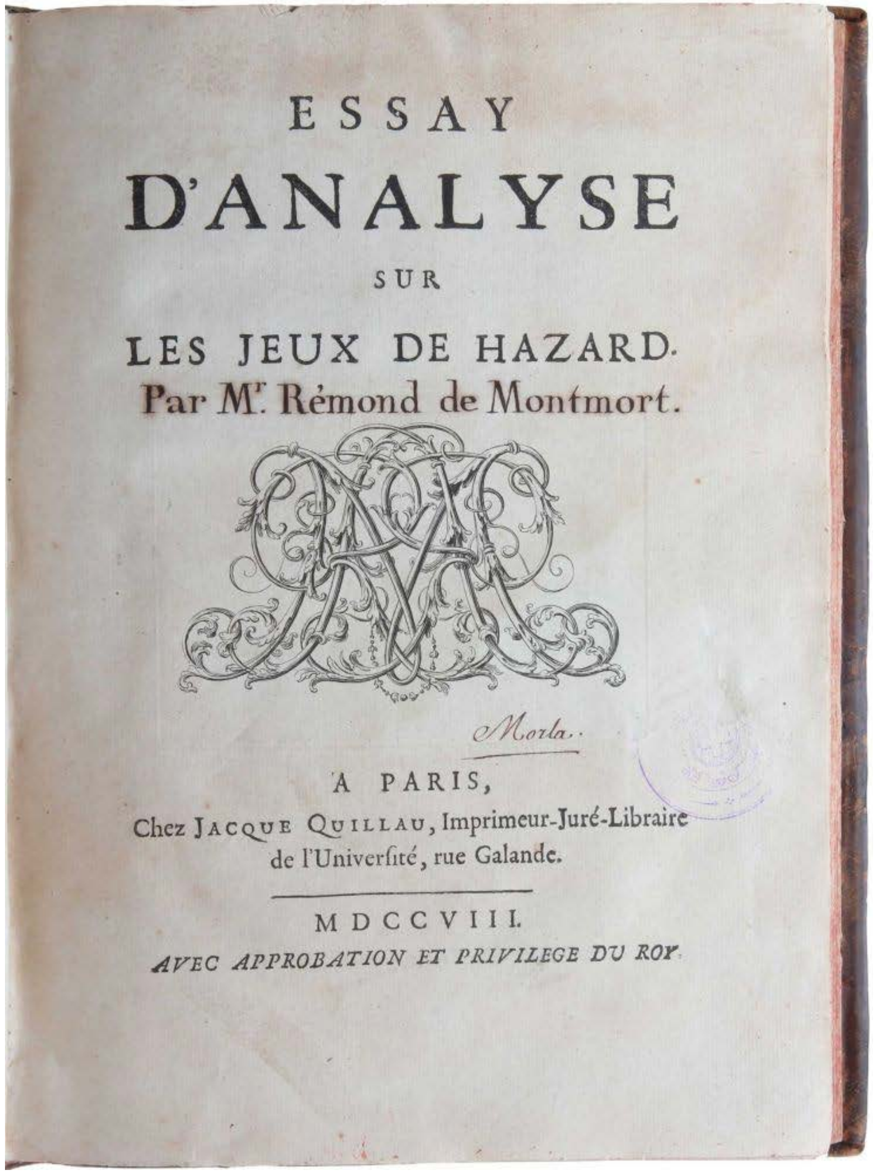
"It is not clear why Montmort undertook a systematic exposition of the theory of games of chance. Gaming was a common pastime among the lesser nobility whom he frequented, but it had not been treated mathematically since Christiaan Huygens' monograph of 1657. Although there had been isolated publications about individual games, and occasional attempts to come to grips with annuities, Jakob I Bernoulli's major work on probability, the *Ars conjectandi*, had not yet been published. Bernoulli's work was nearly complete at his death in 1705; two obituary notices give brief accounts of it. Montmort set out to follow what he took to be Bernoulli's plan …
[Montmort] continued along the lines laid down by Huygens and made analyses of fashionable games of chance in order to solve problems in combinations and the summation of series" (DSB).

"In this first edition of the *Essai d'Analyse* Montmort begins by finding the chances involved in various games of cards. He discusses such simple games as Pharaoh, Bassette, Lansquenet and Treize, and then, not so fully or successfully, Ombre and Picquet. The work is easy to read in that he prefaces each section with the rules of the game discussed, so that what he is trying to do can be explicitly understood. Possibly he found it necessary to do this because different versions of the games were in vogue, but this does not always occur to other writers. Having set down the rules, he solves simple cases in a method somewhat reminiscent of Huygens, and then takes a plunge into a general solution which appears to be correct but is not always demonstrably so. The *Problèmes divers sur le jeu du treize* are interesting indeed in that he gives the matching distribution and its exponential limit. Treize has survived today as the children's game of Snap.

'*The players draw first of all as to who shall be the Bank. Let us suppose that this is Pierre, and the number of players whatever one likes. Pierre having a complete pack of* 52 *shuffled cards, turns them up one after the other. Naming and pronouncing one when he turns the first card, two when he turns the second, three when he turns the third, and so on until the thirteenth which is the King. Now if in all this proceeding there is no card of rank agreeing with the number called, he pays each one of the Players taking part and yields the Bank to the player on his right. But if it has happened in the turning of the thirteen cards that there has been an agreement, for example turning up an ace at the time he has called one, or a two at the time he has called two, or three when he has called three, he takes all the stakes and begins again as before calling one, then two, etc.*'

"He begins by assuming Pierre has two cards and one opponent, Paul. Then Pierre has three cards, four, and finally any number [say, *p*] … He gives *p* successively values 1, 2, ... , 13 and calculates Pierre's chance at each stage. It is, however, the remarks on this which are interesting. After his calculations he says:

'*The preceding solution furnishes a singular use of the figurate numbers (of which I shall speak later), for I find in examining the formula, that Pierre's chance is expressible by an infinite series of terms which have alternate + and – signs … we have for Pierre's chance the very simple*



ESSAY
D'ANALYSE
SUR
LES JEUX DE HAZARD.
Par Mr. Rémond de Montmort.

Morla.

A PARIS,
Chez JACQUE QUILLAU, Imprimeur-Juré-Libraire de l'Université, rue Galande.

MDCCVIII.
AVEC APPROBATION ET PRIVILEGE DU ROY.

1/1 - 1/1.2 + 1/1.2.3 - 1/1.2.3.4 + 1/1.2.3.4.5 - 1/1.2.3.4.5.6 + *etc.*'

"This is possibly the first exponential limit in the calculus of probability … [the sum of the series is 1/*e*, where *e* is the base of natural logarithms].

"In the second half of the first part on Piquet, Ombre, etc. he interpolates a section on problems in combinations. This is all quite sound mathematics, although he takes a very long time to establish the Arithmetic Triangle. The principle of conditional probability, often attributed to de Moivre but probably dating back to the controversy between Huygens and Hudde, is used with facility and understanding …

"In the second part of his treatise Montmort discusses the game of Quinquenove and the game of Hazard, remarking about the latter that the game is known only in England … Montmort gives the chances of [the two players in Hazard] and then describes another game, which he says has no name and so he dubs it the game of Hope, and gives some calculations on this also. Backgammon however rather defeats him …

"From an historical point of view, however, there is interest in his game of Nuts … divination among primitive tribes is (and was) carried out by casting pebbles, grain, or nuts, etc. It is also still a puzzle that the same ritual of divination was used in games to while away the idle hour. That this duality of purpose was probably universal, not just European, appears likely from Montmort's discussion on *Problème sur le Jeu des Sauvages, appellee Jeu des Noyaux*. He writes:

'*Baron Hontan mentions this game in the second book on his travels in Canada, p.* 113 [*Nouveaux voyages dans l'Amérique septentrionale, 1703*]. *This is how he explains it. It is played with eight nuts black on one side and white on the other. The nuts are thrown in the air. If the number of black is odd, he who has thrown the nuts wins the other gambler's stake. If they are all black or all white he wins double stakes, and outside these two cases he loses his stake.*'

"Having solved the problem for four-sided nuts he concludes his book with Huygens' five problems, and some reflections on the games of Her, Ferme and Tas" (David, pp. 144-150).

"The greatest value of Montmort's book lay perhaps not in its solutions but in its systematic setting out of problems about games, which are shown to have important mathematical properties worthy of further work. The book aroused Nikolaus I Bernoulli's interest in particular and the 1713 edition includes the mathematical correspondence of the two men. This correspondence in turn provided an incentive for Nikolaus to publish the *Ars conjectandi* of his uncle Jakob I Bernoulli …

"The work of De Moivre is, to say the least, a continuation of the inquiries of Montmort. Montmort put the case more strongly—he accused De Moivre of stealing his ideas without acknowledgment. De Moivre's *De mensura sortis* appeared in 1711 and Montmort attacked it scathingly in the 1713 edition of his own *Essay*. Montmort's friends tried to soothe him, and largely succeeded. He tried to correspond with De Moivre, but the latter seldom replied. In 1717 Montmort told Brook Taylor that two years earlier he had sent ten theorems to De Moivre; he implied that De Moivre could be expected to publish them" (DSB).

"The value of Montmort's work resides partly in his scholarship. He was well-versed in the work of chance of his predecessors (Pascal, Fermat, Huygens), met Newton on one of a number of visits to England, corresponded with Leibniz, but remained on good terms with both sides during the strife between their followers. The summation of finite series is an element of Montmort's mathematical interests which enters into his probability work and distinguishes it from the earlier purely combinatorial problems arising out of enumeration of equiprobable sample points. Although the *Essay* to a large extent deals with the analysis of popular gambling games, it focuses on their mathematical properties and is thus written for mathematicians rather than gamblers … Montmort also worked with Nicolaus [I Bernoulli] on the problem of duration of play in the gambler's ruin problem, possibly prior to de Moivre, and at the time the most difficult problem solved in this subject area" (Heyde & Seneta, *Statisticians of the Centuries*, p. 53).

"Pierre Rémond de Montmort (1678-1719) was born into a wealthy family of the French nobility. As a young man he traveled in England, the Netherlands, and Germany. Shortly after his return to Paris in 1699 his father dies and left him a large fortune. He studied Cartesian philosophy under Malebranche and studied the calculus on his own. … Montmort corresponded with Leibniz whom he greatly admired. He was also on good terms with Newton whom he visited in London. In 1709 he printed 100 copies of Newton's *De Quadratura* at his own expense … through John Bernoulli, he also offered to print *Ars Conjectandi*. He was on friendly terms with Nicholas Bernoulli and Brook Taylor" (Hald, pp. 286-7). The Royal Society elected Montmort a Fellow in 1715 and the *Academic Royale des Sciences* made him an associate member (as he was not a resident of Paris) the following year.

The textual differences between the issue offered here, which we shall call Ia, and what is usually referred to as the first edition, which we shall call Ib, are very substantial. They begin already on page 3 of the text. In Ia the last two paragraphs preceding Proposition 1 read:

'Car il faut remarquer que quoiqu'il soit tres incertain si Paul gagnera, & qu'il n'y ait pas même de contradiction qu'il gagne mille fois de suite, il est neanmoins tres certain que pour acheter le droit de Pierre il faudroit lui donner quarante sols, & que si Paul s'obligeoit de jouer trois coups aux conditions précedents, Pierre pourroit aussi-bien compter sur deux écus de profit, comme sur deux écus qu'il auroit tirés de sa poche, pour les risquer à pair ou à non contre deux autres écus.

'Quoique ces termes, *avantage* & *disavantage*, semblent être clairs, parcequ'ils sont communs & familiers, j'ai crû qu'il étoit à propos pour ôter toute équivoque, d'expliquer de quelle maniere je les entends.'

In Ib, these paragraphs read:

'Car il faut remarquer que quoiqu'il soit très incertain si Paul gagnera ou ne gagnera pas, & qu'il n'y ait point de contradiction qu'il gagne mille fois de suite, il est neanmoins très certain que pour acheter le droit de Pierre il faudroit lui donner quarante sols, & que si Paul s'obligeoit de jouer trois coups aux conditions précedents, Pierre pourroit aussi-bien compter sur deux écus de profit, comme sur deux écus que Paul lui auroit donné en pur don, à condition qu'il voulût jouer trois écus contre lui à croix ou pile.

'Quoique ces termes *avantage* & *disavantage* semblent être clairs, parcequ'ils sont communs & familiers, j'ai crû qu'il étoit à propos pour ôter toute équivoque, d'expliquer de quelle maniere je les entends; il m'a paru que presque tout le monde y attachoit de fausses idées.'

In Ia, the statement of 'Proposition 1. Lemme' reads:

‘Si le nombre des hazards qui peuvent me donner a est exprime par n, & le nombre des hazards qui peuvent me donner b exprime par m, mon sort sera $na + mb / n + m$,’

while in Ib we have the significantly different (and italicized) statement:

‘*Le nombre des hazards qui peuvent faire gagner Pierre, & lui donner* A, *étant* m; *& le nombre des hazards qui peuvent le faire perdre ou lui donner zéro, étant* n, *je dis que s’il n’y a que ces deux sortes de hazards, & qu’on etende par* A *l’argent du jeu, on aura le sort de Pierre* = mA + nx0 / m + n.’

There are further substantial differences on page 4. We have not attempted to catalogue all the differences between Ia and Ib, but we note that Ib has a 15-line list of errata, the last referring to p. 166, but Ia has only an 11-line list going as far as p. 113. The nature of the changes, the absence of the plates, and the shorter errata list clearly indicate that Ia is the first issue.

Goldsmiths’-Kress no. 04527.0-2, suppl.; Sotheran 3059 (‘rare’). For detailed accounts of the work see David, *Games, Gods and Gambling* (1962), Chap. 14; Hald, *A History of Probability and Statistics and their Applications before 1750*, Chap. 18; Todhunter, *History of the Theory of Probability* (1867), Chap. 7.

PREFACE.

IL y a long-temps que les Geometres ſe vantent de pouvoir par leurs methodes découvrir dans les Sciences naturelles, toutes les verités qui ſont à la portée de l’eſprit humain; & il eſt certain que par le merveilleux alliage qu’ils ont fait depuis cinquante ans de la Geometrie avec la Phyſique, ils ont forcé les hommes à reconnoître que ce qu’ils diſent à l’avantage de la Geometrie n’eſt pas ſans fondement. Quelle gloire ſeroit-ce pour cette Science ſi elle pouvoit encore ſervir à regler les jugemens & la conduite des hommes dans la pratique des choſes de la vie!

L’aîné de Meſſieurs Bernoulli ſi connus l’un & l’autre dans le monde ſçavant, n’a pas cru qu’il fût impoſſible de porter la Geometrie juſqu’à ce

* ij

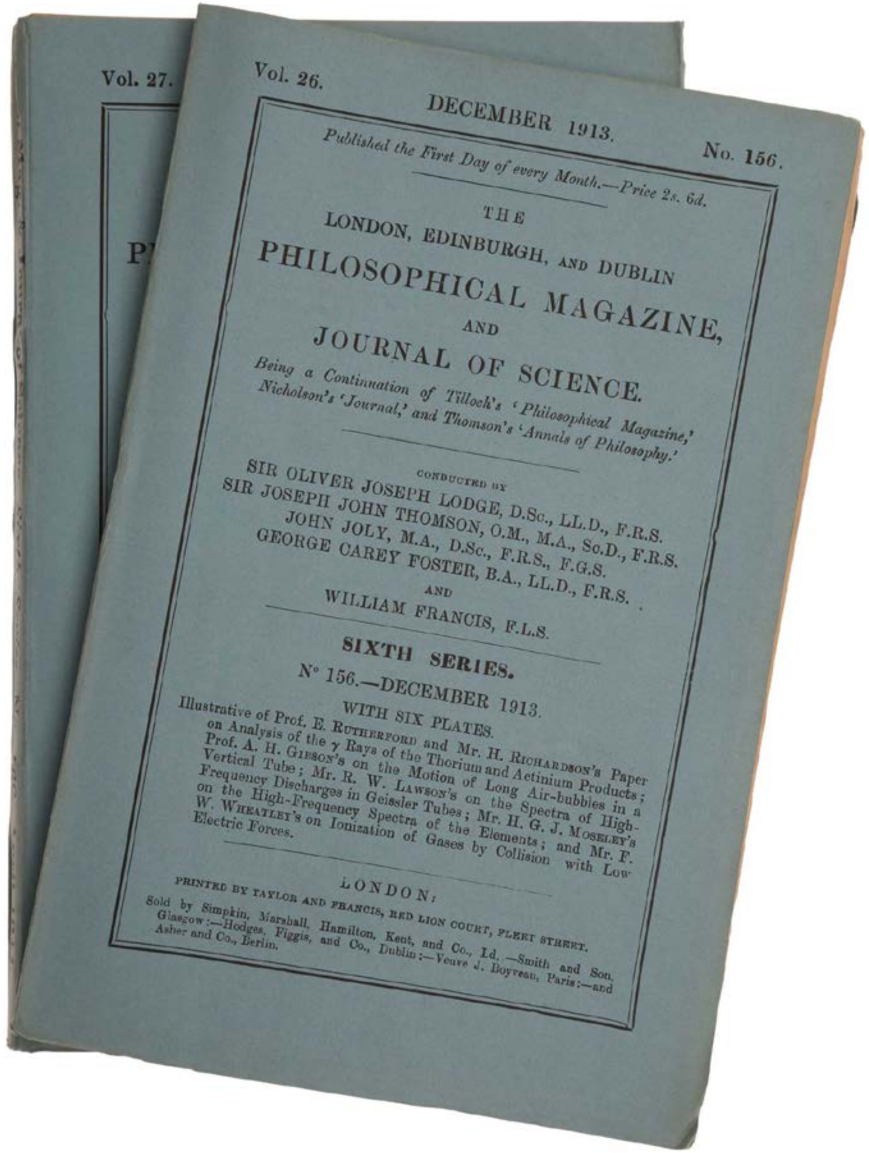
PMM 407 - THE ATOMIC TABLE

MOSELEY, Henry Gwyn Jeffreys. *The High-Frequency Spectra of the Elements, I-II.* London: Taylor and Francis, 1913-14.

$9,500

Two issues, untouched in the original printed wrappers, of the Philosophical Magazine, pp. 1024-34, Sixth Series, Vol. 26, no. 156, December 1913 [- pp. 703-13 in Vol. 27, no. 160, April 1914]. Very rare in such fine condition.

First edition, an exceptionally fine set of both parts of this landmark work, journal issue, in the original printed wrappers. "In 1913 and 1914, respectively, Moseley (1887-1915) published two papers which, once and for all, established a firm connection of the Periodic Table, which was based on empirical chemistry, to the physical structure of atoms" (Brandt, p. 97). "Moseley, working under Rutherford at Manchester, used the method of X-ray spectroscopy devised by the Braggs to calculate variations in the wavelength of the rays emitted by each element. These he was able to arrange in a series according to the nuclear charge of each element ... These figures Moseley called atomic numbers. He pointed out that they also represented a corresponding increase in extra-nuclear electrons and that it is the number and arrangement of these electrons rather than the atomic weight that determines the properties of an element. It was now possible to base the periodic table on a firm foundation, and to state with confidence that the number of elements up to uranium is limited to 92" (PMM). On the basis of his results, Moseley also predicted the existence of four new elements, later discovered and named hafnium, rhenium, technetium and promethium.



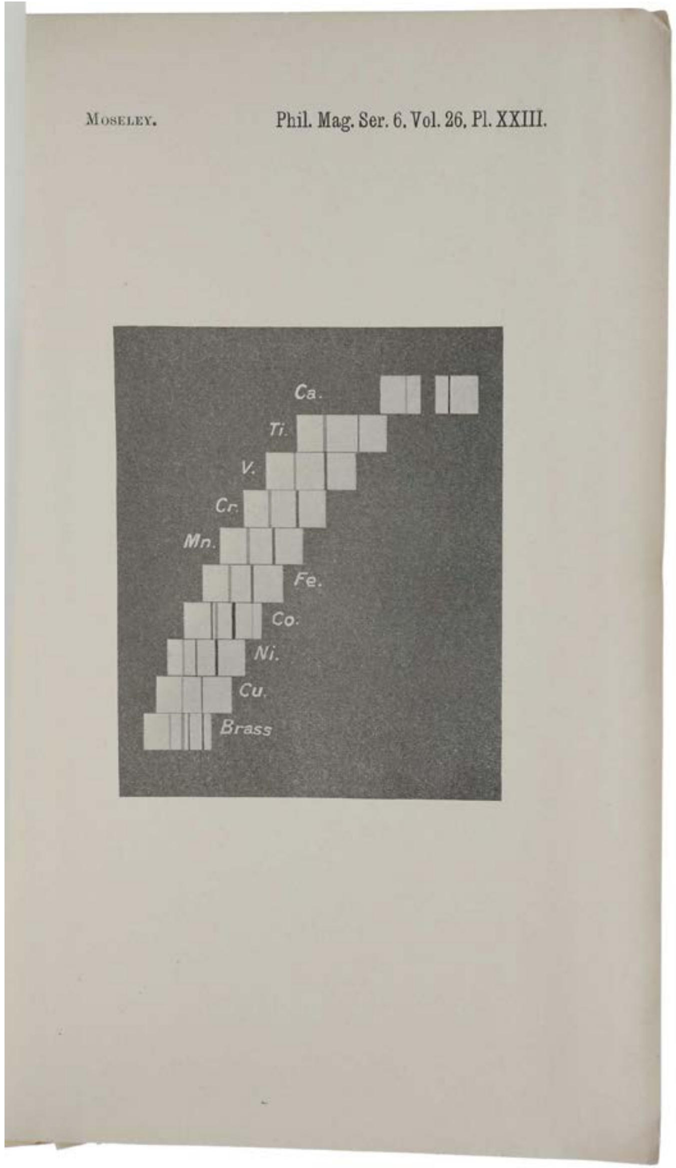
"Before 1913 the order of the elements in the periodic system was universally taken to be given by the atomic weight. Although this caused some anomalies, such as that related to the 'reversed' atomic weights of tellurium (Te = 127.6) and iodine (I = 126.9), the convention or dogma of atomic weight being the defining property of a chemical element was rarely questioned … According to Charles Galton Darwin, who at the time was a lecturer at Manchester University, the 1913 scattering experiments of Geiger and Marsden convinced Rutherford and his group that the nuclear charge was the defining quantity of a chemical element. The idea certainly was in the air, but it took until November 1913 before it was explicitly formulated, and then from the unlikely source of a Dutch amateur physicist. Trained as a lawyer, Antonius van den Broek had since 1907 published articles on radioactivity and the periodic system … In a short communication to *Nature* dated November 10 he disconnected the ordinal number from the atomic weight and instead identified it with the nuclear charge *N* (or *Z*, as it subsequently became symbolized). This hypothesis, he said, 'holds good for Mendeleev's table but the nuclear charge is not equal to half the atomic weight'. Van den Broek's suggestion was quickly adopted by Soddy, Bohr, and Rutherford and his group … In an address of 1934 celebrating the centenary of Mendeleev's birth, Rutherford credited Bohr for first having recognized the significance of an ordinal number for the chemical elements: 'The idea that the nuclear charge of an element might be given by its ordinal or atomic number was first suggested and used by Bohr in developing his theory of spectra. By a strange oversight, Bohr himself gave the credit of this suggestion to van den Broek, who later discussed the applicability of this conception to the elements in general' …

"Besides the successes from the spectra of hydrogen and helium, the strongest experimental support for Bohr's theory came from X-ray spectroscopy, a branch of science that did not yet exist when Bohr completed his trilogy … The existence of monochromatic X-rays characteristic of the element emitting the rays had been known since 1906, when the phenomenon was discovered by Charles Glover Barkla, a physicist at the University of Liverpool. Although Barkla could not determine the wavelengths of the characteristic rays he could study and classify them by means of their penetrating power. He soon found that there were two kinds of rays, which he named *K* and *L* radiation and where the first had a greater penetrating power than the latter. What was missing, among other things, was a method of determining the wavelength of the radiation, but such a method was provided after William Henry Bragg and his son William Lawrence Bragg in 1912 invented the X-ray spectrometer based on the reflection of X-rays on crystals.

"In Manchester, Henry Gwyn Moseley, who was Bohr's junior by two years, set out to employ the method of the Braggs to measure and understand the wavelengths of the characteristic radiation. He had earlier collaborated with Darwin on X-ray diffraction, but from the summer of 1913 he pursued the new research programme alone. Bohr knew Moseley, but it was only in July 1913 that he had a long discussion with him and told him about his new atomic theory. The two physicists evidently had shared interests, such as the periodic system and its relation to the atomic number. Moseley's research programme was to a large extent motivated by the possibility of confirming by means of X-ray spectroscopy van den Broek's hypothesis – or the van den Broek-Bohr hypothesis – of the atomic number. 'My work was undertaken for the express purpose of testing Broek's hypothesis, which Bohr has incorporated as a fundamental part of his theory of atomic structure', he wrote. Moseley constructed a new kind of X-ray tube where the targets could be easily interchanged and moved in position opposite to the cathode, to give out their characteristic rays. To determine the wavelengths he developed a photographic method. Having surmounted the inevitable experimental difficulties, in October 1913 he was ready to collect data, starting with the K lines from calcium to zinc" (Kragh, pp. 32-3 & 104).

"In a very short time, Moseley produced the first of his two famous papers in which he showed the spectra of K radiation of ten different substances … Moseley arranged the spectra, one below the other in a step-like fashion, in such a way that a given wavelength was in the same position for all spectra. It then became clear by simple inspection of this 'step ladder' that the spectrum of *K* radiation of each element contains two strong lines (which Moseley called K_α (for the longer wavelength) and K_β (for the shorter) and that this pair of lines moves to shorter and shorter wavelengths in a monotonic fashion if one moves step by step from calcium to zinc.

"Only a few months before Moseley's work, Bohr had published his model of the atom with *Z* electrons, each of electric charge $-e$ circling an atomic nucleus of charge *Ze*. Bohr had taken the nuclear charge number *Z* to be identical with the position number of the corresponding element in the Periodic Table. His theory could explain the visible spectra of the hydrogen atom ($Z = 1$) and the positive ion of helium ($Z = 2$) with only one electron. But he could not make calculations for atoms with more electrons. Moseley realized that, in contrast to visible spectra, the characteristic X-ray spectra, in particular the spectrum of *Z* radiation, was simple also for atoms of high *Z*. Since Bohr had conjectured that the electrons in an atom are arranged in separate rings and since in his model transitions to the innermost ring correspond to the highest energies, i.e., the shortest wavelengths, Moseley wrote: 'The very close similarity between the X-ray spectra of the different elements shows that these radiations originate inside the atom, and have no direct connexion with the complicated light-spectra and chemical properties governed by the structure of its surface.' Moseley also gave a formula describing the frequency of the *K* radiation for all elements which he had studied



and predicting it for all others and, on the basis of very sparse data, even gave a similar formula for the *L* radiation. The formulae later were called *Moseley's law*.

"Moseley's work made it clear once and for all that indeed the position number in the Periodic Table is equal to the number *Z* of positive elementary charges in the nucleus of an atom. It also showed that *Z* is more important for the spectroscopic and chemical properties of an atom than the atomic mass number *A*. This is evident in the case of the elements cobalt (*Z* = 27, *A* = 58.9) and nickel (*Z* = 28, *A* = 58.7), where even the order in *A* differs from that in *Z*.

"At this stage of his work Moseley decided to leave Manchester and to move back to Oxford, although Rutherford had offered him a fellowship for the academic year 1913/14, and although he got no paid position in Oxford. His motives are not entirely clear but it seems he thought that it would be easier eventually to obtain a professorship in Oxford if he was on the spot. With a grant of 1000 Belgian Francs from the Solvay Foundation he set up new equipment in Townsend's laboratory, where he was allowed to work as a guest … With Moseley's technique and Moseley's law it was easy to determine the number Z for virtually any known element. For elements with higher values of *Z* the *L* radiation had to be used, since the voltage available for X-ray tubes was not high enough to produce the *K* radiation with its shorter wavelength. Already in April 1914 Moseley published his results [the second offered paper]; one comprehensive diagram contains the frequencies of *K* or *L* lines for most elements between aluminium (*Z* = 13) and gold (*Z* = 79). In the conclusions he wrote: 'Known elements correspond with all numbers between 13 and 79 except three. There are here three possible elements still undiscovered.' These were the elements with *Z* =43, 61, and 75. In fact, also the element with *Z* = 72, taken to be a rare earth, was missing. Moseley had assumed its existence, because it was reported in the chemical literature, but could not get a sample of it to use in his measurements. All four elements were found between 1922 and 1945, two in terrestrial material (hafnium, *Z* = 72, and rhenium, *Z* = 75). The other two had to be produced by nuclear reactions (technetium, *Z* = 43, and promethium, *Z* = 61) since these radioactive elements do not seem to exist in the earth's crust.

"Moseley's family background and education were exceptional. His father, Henry Nottidge Moseley, and both his grandfathers, Henry Moseley and John Gwynn Jeffreys, were Fellows of the Royal Society. His father, who had been professor of zoology at Oxford, died when Moseley was only four years old. From then on his mother saw to it that he got the best education available. In 1901 he won a King's scholarship for the prestigious Public School of Eton and in 1906, again with a scholarship, he entered Trinity College, Oxford. He studied physics under Townsend and, after graduating in 1910, joined Rutherford's outstandingly successful group in Manchester. He did some work on radioactivity but, immediately after learning of Laue's theory of X-ray diffraction and the experiment by Friedrich and Knipping in the summer of 1912, he became focused on X rays.

"Moseley was invited to report on his work at the meeting of the British Association for the Advancement of Science held in Australia in August 1914. It was the month in which the First World War began. Immediately after his talk Moseley travelled back to England by the next steamer to volunteer for the army. He even 'pulled private strings' and became a lieutenant in the Royal Engineers. On 10 August 1915, he perished in the Battle of Sari Bair" (Brandt, pp. 97-101).

Norman 1599; *Printing and the Mind of Man* 407. Brandt, *The Harvest of a Century*, 2009. Kragh, *Niels Bohr and the Quantum Atom*, 2012.

NAPIER'S FIRST DESCRIPTION OF HIS METHOD OF CONSTRUCTING LOGARITHMS

NAPIER, John. *Mirifici logarithmorum canonis constructio; et eorum ad naturales ipsorum numeros habitudines; una cum appendice, de aliâ eâque præstantiore logarithmorum specie contenda. Quibus accessere propositiones ad triangula sphærica faciliore calculo resolvenda: Unà cum annotationibus aliquot doctissimi D. Henrici Briggii, in eas & memoratam appendicem.* Edinburgh: Andrew Hart, 1619. [Bound with:] **GREGORY, James**. *Vera circuli et hyperbolae quadratura, in propria sua proportionis specie, inventa, & demonstrata.* Padua: Giacomo Cadorino, [1667].

$18,500

Three works bound in one vol., small 4to. Constructio: pp. [2, woodcut title], 67. Quadratura: pp. 63, with one folding engraved plate. Printer's historiated device on title, woodcut head- and tail-pieces, framed initials. Date of publication from p. 59. Devises: pp. [2, engraved frontispiece], [viii], 87, with 10 unnumbered leaves with images of coins inserted between pp. 42 & 43 (light damp-stain to first few leaves of Constructio, some cropping of headlines and catchwords, slightly affecting text in Quadratura, engraved title of Constructio folded in at fore-edge and slightly cropped at head). Manuscript table of contents on front free-endpaper. Mid-eighteenth-century half-calf and marbled boards, spine with red-lettering piece and floral gilt ornament in each panel, red edges. Preserved in a cloth folding box with black morocco spine label.

First edition, extremely rare, of this complement to Napier's epoch-making *Mirifici logorithmorum canonis descriptio* (1614) – while the *Descriptio* gave the first ever

MIRIFICI
LOGARITHMORUM CANONIS DESCRIPTIO,
Ejusque usus, in utraque Trigonometria; ut etiam in omni Logistica Mathematica, amplissimi, facillimi, & expeditissimi explicatio.
ACCESSERVNT OPERA POSTHVMA;
Primò, Mirifici ipsius canonis constructio, & Logarithmorum ad naturales ipsorum numeros habitudines.
Secundò, Appendix de alia, eâque præstantiore Logarithmorum specie construenda.
Tertiò, Propositiones quædam eminentissimæ, ad Triangula sphærica mirâ facilitate resolvenda.
Autore ac Inventore IOANNE NEPERO, Barone Merchistonii, &c. Scoto.
EDINBVRGI, Excudebat Andreas Hart. Anno 1619.

table of logarithms, it was in the *Constructio* that Napier explained the method of their construction. It is here bound with the first edition of James Gregory's first mathematical work, highly important in the pre-history of calculus, and if anything even rarer than the Napier. "Probably no work has ever influenced science as a whole, and mathematics in particular, so profoundly as this modest little book [the *Descriptio*]. It opened the way for the abolition, once and for all, of the infinitely laborious, nay, nightmarish, processes of long division and multiplication, of finding the power and the root of numbers" (Waters, *The Art of Navigation in England in Elizabethan and Early Stuart Times* (1958), p. 402). "The 'Mirifici logorithmorum canonis constructio' is the most important of all of Napier's works, presenting as it does in a most clear and simple way the original conception of logarithms. It is, however, so rare as to be very little known, many writers on the subject never having seen a copy" (Macdonald, p. xvii). "Historically, it is important to note that in the *Constructio* the decimal notation is used with ease and power practically for the first time" (Henderson, p. 253). The second work in this volume is by the brilliant Scottish mathematician James Gregory. "Of British mathematicians of the seventeenth century, Gregory was excelled only by Newton" (Gjertsen, p. 245). The *Quadratura* "contained an astonishing number of novel and fundamental concepts, precisely formulated: concepts such as convergence, functionality, algebraic and transcendental functions, classes of transcendency, the process of iteration, the inherent likeness between circular and hyperbolic functions and the existence of functions invariant over an infinite sequence of values of their arguments. Incidentally, he calculated π to thirteen places and was the first to give the number 2.3025850929940456, or $\log_e 10$, for the zone of the hyperbola. This battery of ideas was directed with the sole aim of proving the transcendence of π and e, an investigation that was finally completed by Lindemann at the close of the nineteenth century" (Turnbull, p. 5). "Although [his] proof was defective and in consequence rapidly incurred a storm of criticism it is to Gregory's credit that he was the first to formulate a proposition of this class" (Baron, p. 231). "Indeed, by his speculations Gregory opens a new realm of mathematics … It is surprising that he quotes three important problems solved today: the squaring of the circle, the impossibility of solving the general algebraic equation, and the impossibility of reducing the pure equation of the *n*th degree [$x^n - 1 = 0$] to quadratic equations" (Turnbull, p. 495). No other copies of either the *Constructio* or the *Quadratura* on ABPC/RBH in the last half-century.

Provenance: Earls of Macclesfield (South Library bookplate on front paste-down and blind-stamp on title of *Constructio*), (Sotheby's, 4 November 2004, lot 885, £7,800). Erwin Tomash (book label on front paste-down).

The basic idea of what logarithms were to achieve is straightforward: to replace the wearisome task of *multiplying* two numbers by the simpler task of *adding* together two other numbers. To each number there was to be associated another, which Napier called at first an 'artificial number' and later a 'logarithm' (a term which he coined from Greek words meaning something like 'ratio-number'), with the property that from the sum of two such logarithms the result of multiplying the two original numbers could be recovered. An idea of this kind was known to the Greeks: take an arithmetic progression (in which there is a constant *difference* between successive terms) and a geometric progression (in which there is a constant *ratio* between successive terms); writing one progression next to the other, one sees that adding any two terms of the arithmetic progression corresponds to multiplying the corresponding terms of the geometric progression. In the *Constructio*, Napier uses this idea but expresses it in kinematical terms. Whiteside suggests that Napier may have derived the idea of using motion in his construction from the writings of William Heytesbury and Nicole Oresme.

Napier (1550-1617) imagines two points, P and L, each moving along its own straight line. P starts at a point P_0 and moves towards a fixed point Z in such a way that its speed is proportional to the distance PZ still to go, while L starts at the same time at L_0 and travels at constant speed – if the constant speed of L is 1 we can think of L_0L as the time taken by P to travel from P_0. Napier defines the time L_0L to be the *logarithm* of the distance PZ, which we will denote by NapLog(PZ). If the distance PZ = x, and the factor of proportionality r, so that the speed at P is x/r, it is easy for us to show (using calculus) that

$$\mathrm{NapLog}(x) = r \ln(r/x),$$

where 'ln' is our natural logarithm with base e. This means that, apart from the factor r, Napier's logarithms are the same as our logarithms, but with base $1/e$. Note that $\mathrm{NapLog}(r) = 0$, so that r is the distance P_0Z. For us the logarithm of 1 is zero, but Napier chooses $r = 10^7$ (see below).

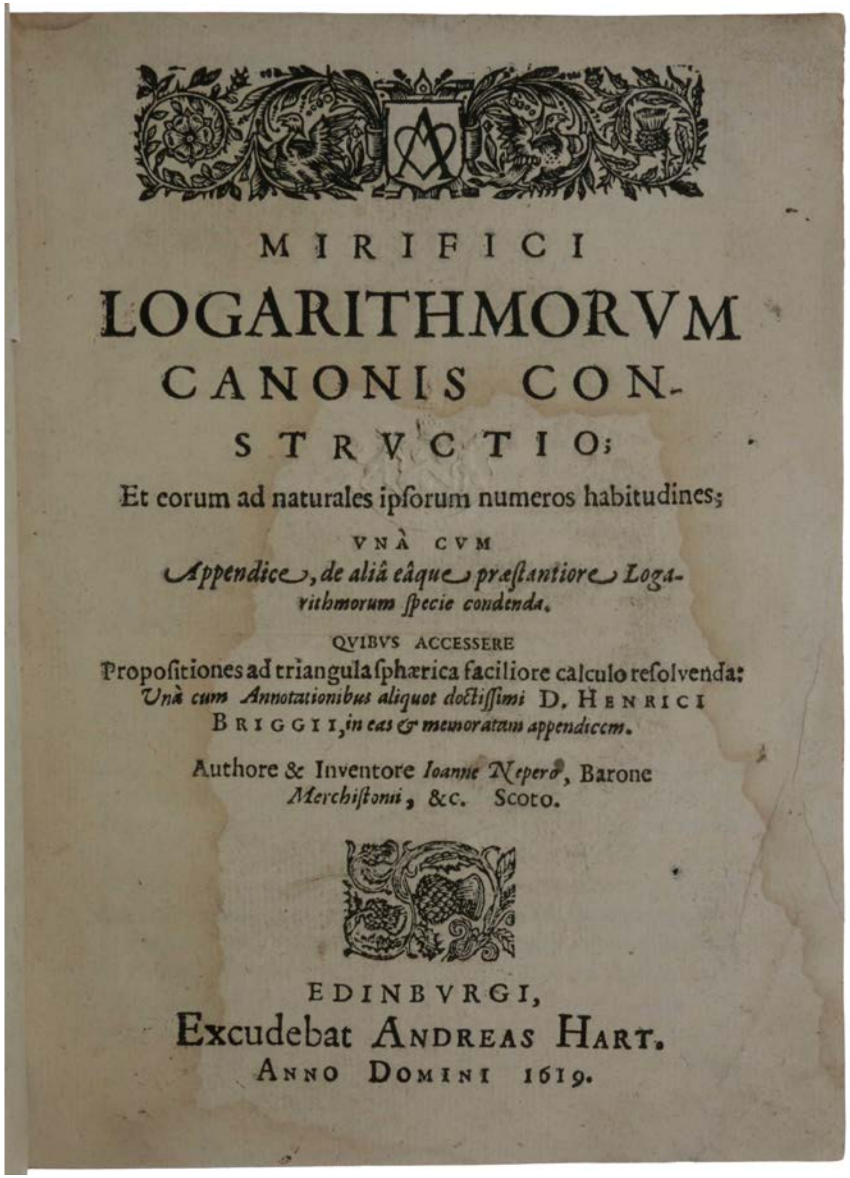
How did Napier *calculate* his logarithms? If Q is another point and QZ = y, where y is less than x, the moving point is slowing down as it travels from P to Q so the time taken to travel the distance PQ = $x - y$ is greater than the time it would have taken if the point had travelled at the speed x/r it had at P and less than the time it would have taken if it had travelled at the speed y/r it had at Q. The actual time taken is $\mathrm{NapLog}(y) - \mathrm{NapLog}(x)$, so

$$(r/x)(x - y) < \mathrm{NapLog}(y) - \mathrm{NapLog}(x) < (r/y)(x - y).$$

If the difference between x and y is very small, the two end values are almost equal so we can take the middle term to be their average:

$\mathrm{NapLog}(y) - \mathrm{NapLog}(x)$ is nearly equal to $\tfrac{1}{2}(r/x + r/y)(x - y)$.

Thus, if $\mathrm{NapLog}(x)$ is known, and y is very close to x, then $\mathrm{NapLog}(y)$ can be



MIRIFICI
LOGARITHMORVM
CANONIS CON-
STRVCTIO;
Et eorum ad naturales ipsorum numeros habitudines;
VNÀ CVM
Appendice, de aliâ eâque præstantiore Loga-
rithmorum specie condenda.
QVIBVS ACCESSERE
Propositiones ad triangula sphærica faciliore calculo resolvenda:
Unà cum Annotationibus aliquot doctissimi D. HENRICI
BRIGGII, in eas & memoratam appendicem.
Authore & Inventore Ioanne Nepero, Barone
Merchistonii, &c. Scoto.

EDINBVRGI,
Excudebat ANDREAS HART.
ANNO DOMINI 1619.

found.

But what if x and y are far apart? Napier's procedure is complex, so we shall (over) simplify. Napier constructs a geometric progression ra^n, where a is a fixed number very close to 1 (and less than 1), and n is a positive integer. Napier calculated the terms of this progression by hand. From his definition of logarithms, it follows that

$\mathrm{NapLog}(ra^n) = n\,\mathrm{NapLog}(ra)$.

Since ra is very close to r and $\mathrm{NapLog}(r)$ is known (= 0), $\mathrm{NapLog}(ra)$ can be calculated from the previous formula, and hence so can $\mathrm{NapLog}(ra^n)$ for all n. Suppose now that x is a number that is very close to ra^n for some value of n. Then, by the preceding formula again, $\mathrm{NapLog}(x)$ can be taken to be

$\mathrm{NapLog}(ra^n) + ½(1/a^n + r/x)(ra^n - x)$.

If the ratio a is small enough, every number x less than r will be close to one of the terms ra^n so this enables Napier to complete his table of logarithms. However, in order to achieve greater accuracy, Napier uses a more complex procedure involving three nested geometric progressions (these are set out in his three 'Proportional Tables'), but the essential idea is the same.

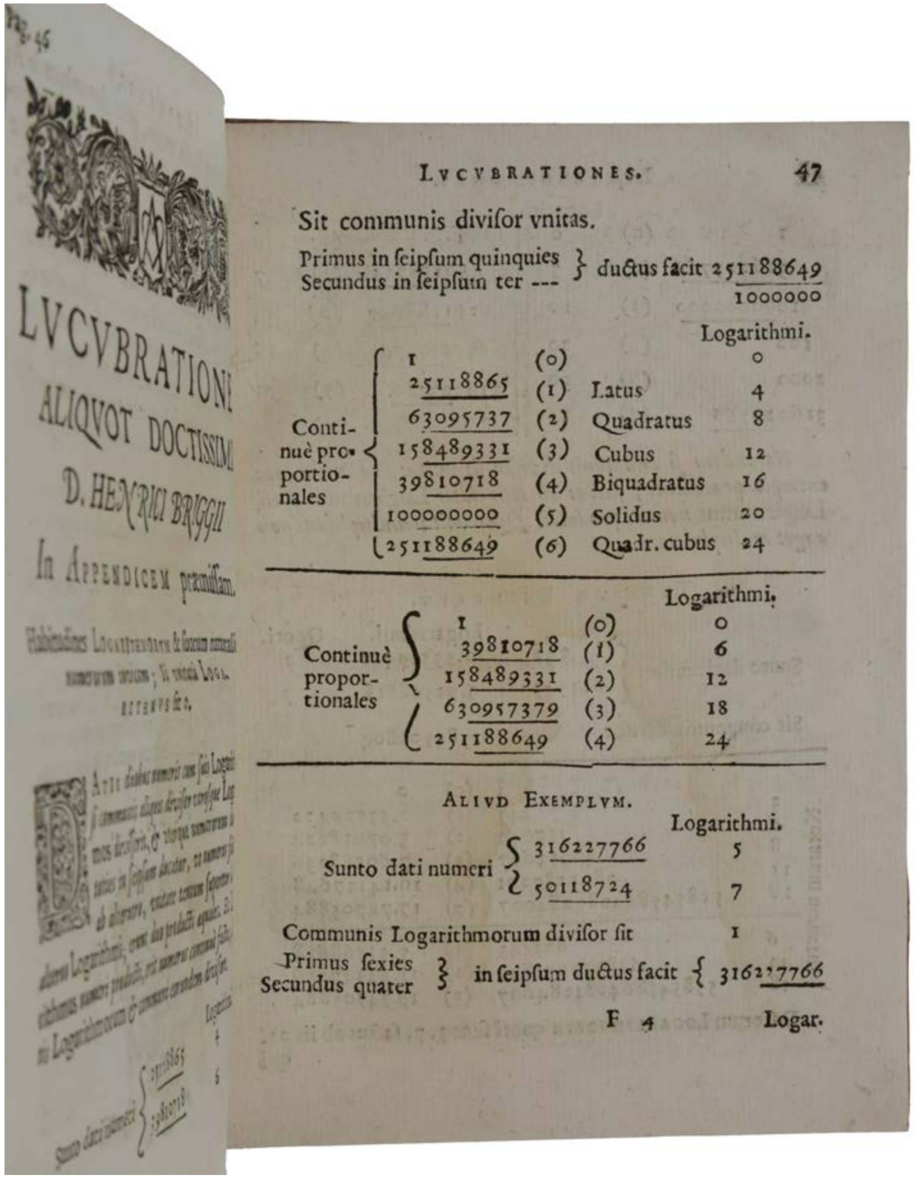
In the *Descriptio*, Napier does not actually tabulate $\mathrm{NapLog}(x)$ for various numbers x, but rather $\mathrm{NapLog}(r \sin(\alpha))$ for various angles α. This was because his tables were intended to be used to solve problem in spherical trigonometry, the type of problem most often encountered by astronomers. Trigonometric tables had been produced throughout the sixteenth century, and to avoid fractions it was usual to tabulate $10^7\sin(\alpha)$ rather than $\sin(\alpha)$ itself (the choice of the factor 10^7 goes back to Regiomontanus). This is why Napier took the proportionality factor r to be 10^7. But Napier was not satisfied with 7 significant figures and actually used four more, beyond the 'decimal point', in his 'Proportionalia Tertiae Tabulae' – this is one of the earliest occurrences of our decimal point symbol in print, and it helped to stabilise this notation in its now-familiar form.

The enthusiasm with which Napier's logarithms were received makes it clear both that this was perceived as a novel invention and that it fulfilled a pressing need. Foremost among those who welcomed the invention was Henry Briggs (1561-1630), Professor of Geometry at Gresham College, who wrote to the biblical scholar James Ussher in 1615: 'Naper, lord of Markinston, hath set my Head and Hands a Work with his new and admirable Logarithms. I hope to see him this Summer if it please God, for I never saw Book which pleased me better or made me more wonder.' Briggs did indeed visit Napier in 1615, and again the next year. Briggs convinced Napier of the advantages of having a version of his logarithms for which the logarithm of 1 is zero, and the logarithm of 10 is 1, i.e., our standard base 10 logarithms. Napier summarized his discussions with Briggs in the first Appendix in the *Constructio*, 'On the construction of another and better kind of logarithms, namely one in which the logarithm of unity is 0'; this is followed by some remarks of Briggs. The second Appendix is devoted to some new formulas in spherical trigonometry, now known as 'Napier's analogies', again followed by Briggs' comments.

The *Constructio* was completed, at least in part, before the *Descriptio*, but Napier wished to delay its publication until 'he had ascertained the opinion and criticism' (Macdonald) of the *Descriptio*. He died in 1617 and the task of publishing the *Constructio* had to be completed by his son Robert, with assistance from Briggs. This first edition seems to have been issued together with a reprint dated 1619 of the *Descriptio*. The *Constructio* had a letterpress title page as well as a woodcut title-page intended to be used as a general title for the two works together and has the wording: *Mirifici logarithmorum canonis description … accesserunt opera posthuma … Mirifici ipsius canonis constructio*. As with most copies (e.g., that in Cambridge University Library, Syn.6.61.23), this one does not contain the reissue of the *Descriptio* – "Many copies lack the first part" (ESTC). The *Descriptio* and *Constructio* were reissued together at Lyon in 1620. While the *Descriptio* was translated into English in 1616 and into other languages soon after it appeared, and was reprinted many times, the *Constructio* had to wait until 1889 before an English version was produced.

"James Gregory (1638-75) was born into a scholarly family with established connections with mathematical and philosophical work. He received a sound mathematical education and, at the age of 26, having already written and published his first work, the *Optica promota*, made contact with the Royal Society on his way to Italy, where he studied for four years (1664-8)" (Baron, pp. 228-9). The *Quadratura* "was published at Padua in 1667, where Gregory was studying mathematics for some time. His teachers at the university introduced him to the ideas of Cavalieri and Torricelli [but] the source from which he is getting his inspiration is quite unknown to us. On the other hand, we find here a singular mixture of far-reaching ideas, exact methods, incomplete deductions, and even false conclusions.

"According to the title of the work, the essential part deals with the quadrature of the circle and the hyperbola. Archimedes had included the circle between inscribed and circumscribed polygons, calculated the perimeter of the $2n$-sided polygon from that of the n-sided polygon, and by this had found the approximate value for the circumference of the circle. Gregory transforms this method into an algebraic



one, to a sort of calculus. But instead of calculating the perimeters Gregory calculates the areas, and this enables him to apply the method simultaneously to the sectors of the circle, ellipse and hyperbola" (Turnbull, pp. 468-9). "Through the skilful manipulation of inscribed and circumscribed polygons he was able to generate a double sequence (a_n, b_n) for the [area of a] sector of an ellipse, circle or hyperbola … After laying down the beginnings of a theory of convergence for such double sequences … Gregory attempted to establish the impossibility of rationally squaring the circle, ellipse or hyperbola by showing that no finite linear combination of the terms (a_n, b_n) of the above sequence could result in a rational function of (a_0, b_0)" (Baron, pp. 229-230).

"Gregory finds in one process the area of sectors for the circle and for the hyperbola. Therefore we have here, for the first time, the analytical connexion between circular and hyperbolic functions or between trigonometric and exponential functions. And this discovery was made without using the imaginary numbers … Gregory was not only the first to discover the analytical identity of the two, seemingly quite different, functions but also was quite aware of the importance of the phenomenon" (Turnbull, pp. 471-2).

"When James Gregory, while living in Italy, published in 1667 his work on the quadrature of the circle and the hyperbola, he sent one of the copies of the very limited edition to Christiaan Huygens, in Paris, who was an authority in the subject; in a polite and even flattering letter he declared himself anxious to hear the opinion on his discoveries, of so competent a critic. Huygens never answered the letter directly; in July 1668, however, he published a review of the work in the *Journal des Sçavans*, in which he acknowledged its importance and the subtlety of its demonstrations, but at the same time raised several objections against the most remarkable proposition, and claimed his priority as to some of the author's result" (Turnbull, p. 479). Gregory published a mild response in the *Philosophical Transactions*, to which Huygens replied in the *Journal des Sçavans*, rejecting Gregory's explanations and stating that the main proposition of the *Quadratura* should be considered to be unproved. Gregory's response was to publish a vigorous attack on Huygens in his *Exercitationes Geometricae* (1668). This time Huygens did not reply, and the Royal Society declined to intervene, despite Gregory's urging.

A second edition of the *Quadratura* was published at Padua in 1668, issued jointly with and at the same time as Gregory's companion work *Geometria pars universalis*.

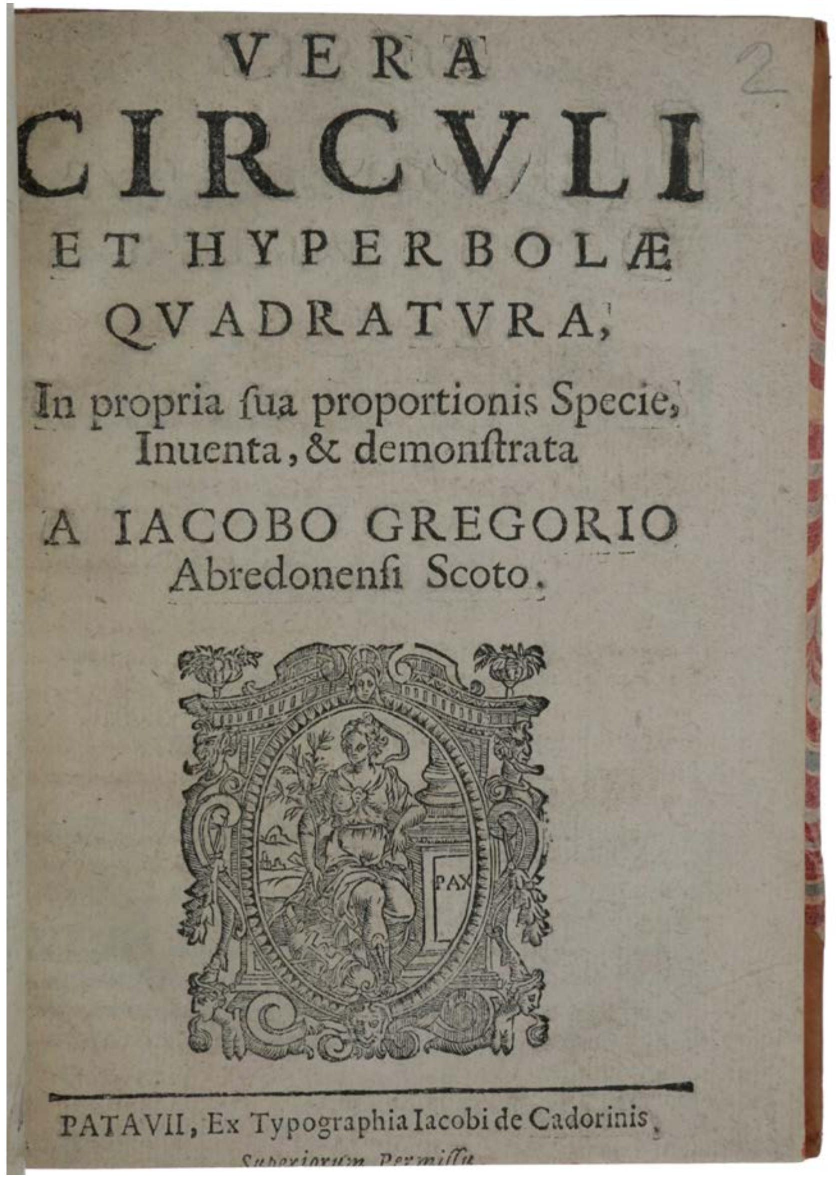
Bound after these two important mathematical works is the third English edition of Henri Estienne's emblem book:

The art of making devises: treating of hieroglyphicks, symboles, emblemes, aenigma's, sentences, parables, reverses of medalls, armes, blazons, cimiers, cyphres and rebus. First written in French by Henry Estienne, Lord of Fossez, interpreter to the French King for the Latine and Greek Tongues: *translated into English, and embelished with divers brasse figures, by T*[*homas*]. *B*[*lount*]. *of the Inner Temple, Gent. whereunto is added, a catalogue of coronet-devises, both on the kings and the Parliaments side, in the late warres*. London: Printed for John Holden, 1650.

This is a translation of *L'Art de faire les devises, où il est traicte des Hieroglyphes, symboles, emblemes, aenigmes, sentences, paraboles, revers de medailles* …, first published at Paris in 1645. English translations followed in 1646 and 1648.

Macclesfield 885; ESTC S123220 (Napier); Wing E3552 (Estienne). Tomash &

Williams N4, Add13, Add18 (this copy). Baron, *The Origins of the Infinitesimal Calculus*, 1969. Gjertsen, *The Newton Handbook*, 1986. Henderson, 'The Methods of Construction of the Earliest Tables of Logarithms,' *The Mathematical Gazette* 15 (1930), 250-256. Macdonald, *The construction of the wonderful canon of logarithms*, 1889. Turnbull, *James Gregory Tercentenary Memorial Volume*, 1939. For further details on the construction of Napier's logarithms, see Hobson, *John Napier and the Invention of Logarithms, 1614*, 1914; and Whiteside, 'And John Napier created logarithms...,' *BSHM Bulletin* 29 (2014), 154-166.



2

VERA
CIRCVLI
ET HYPERBOLÆ
QVADRATVRA,
In propria ſua proportionis Specie,
Inuenta, & demonſtrata
A IACOBO GREGORIO
Abredonenſi Scoto.

PAX

PATAVII, Ex Typographia Iacobi de Cadorinis,
Superiorum Permiſſu.

NEWTON'S IDENTITIES

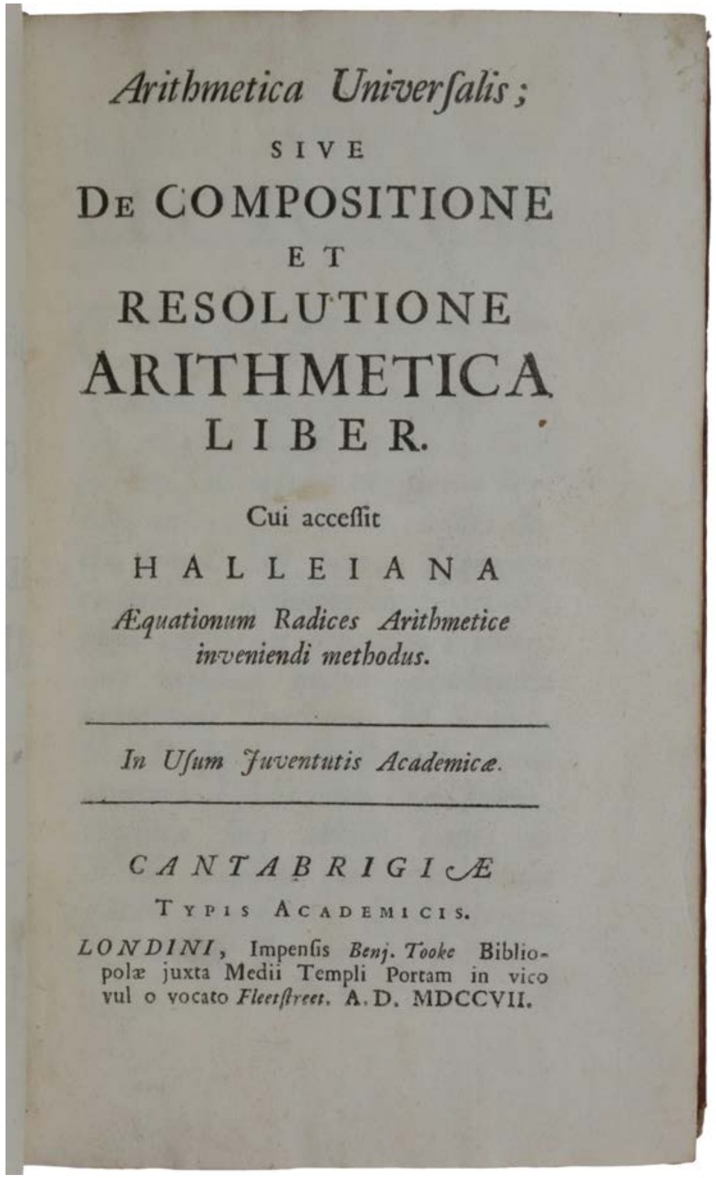
NEWTON, Sir Isaac. *Arithmetica Universalis; sive de Compositione et Resolutione Arithmetica Liber. Ciu accessit Helleiana Aequationum Radices Arithmetice Inveniendi Methodus* ... Cambridge; London: Typis Academicus; Benjamin Tooke, 1707.

$25,000

8vo, pp. [viii], 343. Woodcut diagrams throughout. Former owner's signature (F. Percy White, Feb. 1920?) on half-title partially erased. Contemporary mottled calf, covers with floral border and corner fleurons in blind.

First edition of Newton's treatise on algebra, or 'universal arithmetic,' his "most often read and republished mathematical work" (Whiteside). "Included are 'Newton's identities' providing expressions for the sums of the i^{th} powers of the roots of any polynomial equation, for any integer i [pp. 251-2], plus a rule providing an upper bound for the positive roots of a polynomial, and a generalization, to imaginary roots, of René Descartes' Rule of Signs [pp. 242-5]" (Parkinson, p. 138). About this last rule for determining the number of imaginary roots of a polynomial (which Newton offered without proof), Gjertsen (p. 35) notes: "Some idea of its originality … can be gathered from the fact that it was not until 1865 that the rule was derived in a rigorous manner by James Sylvester."

Provenance: Jesuit College at Ghent (ink inscription 'Bibliotheca Collegii Gandavensis Soc[ietatis] Jesu.' and shelfmark on title); extensive marginal annotations by a well-informed contemporary reader. This reader was possibly the English Jesuit Christopher Maier (1697-1767). Born in Durham, England, Maier entered the Society of Jesus in 1715. He taught at Liège, where he became



interested in astronomy. In 1750, Maire was commissioned by Pope Benedict XIV to measure two degrees of the meridian from Rome to Rimini with fellow Jesuit Roger Boscovich, with a view to mapping the Papal States; in turn, they proved that the earth is an oblate spheroid, as Newton had proposed in *Principia*, publishing their results in *Litteraria Expeditione* (1755). Maier spent his final years at the English Jesuit College in Ghent.

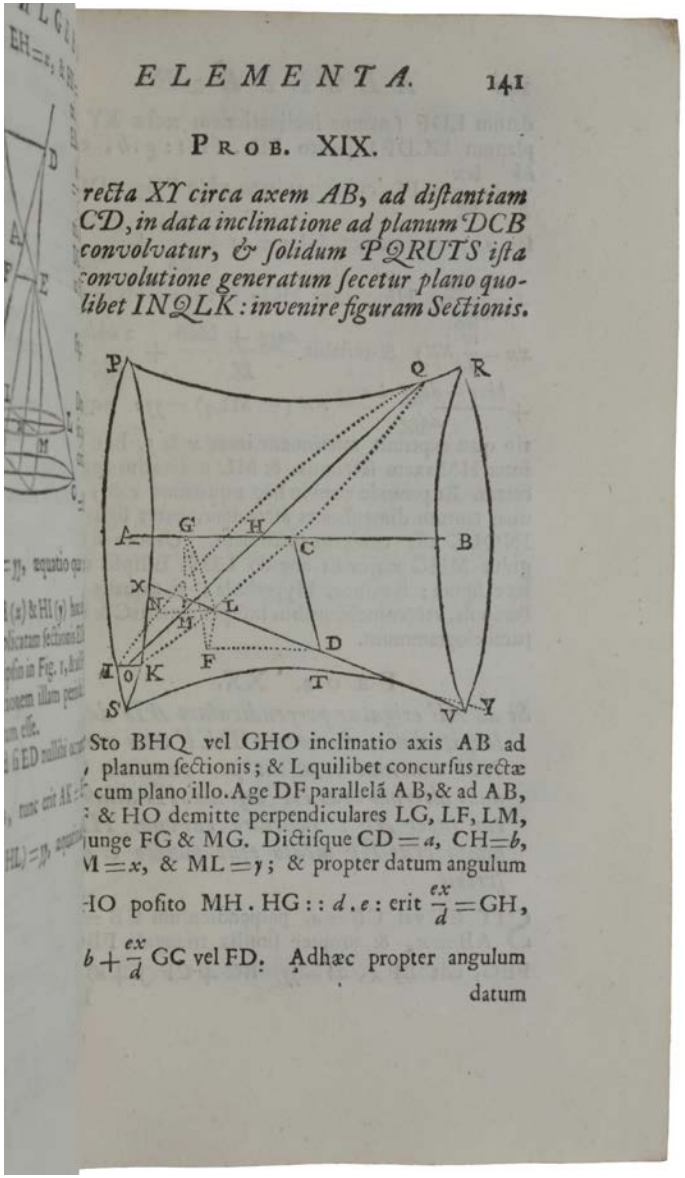
"In fulfillment of his obligations as Lucasian Professor, Newton first lectured on algebra in 1672 and seems to have continued until 1683. Although the manuscript of the lectures in [Cambridge University Library] carries marginal dates from October 1673 to 1683, it should not be assumed that the lectures were ever delivered. There are no contemporary accounts of them and, apart from Cotes who made a transcript of them in 1702, they seem to have been totally ignored. Whiteside (*Papers* V, p. 5) believes that they were composed 'over a period of but a few months' during the winter of 1683-4" (Gjertsen, pp. 33-4). The course of lectures stemmed from a project on which Newton had embarked in the autumn of 1669, thanks to the enthusiasm of John Collins: the revision of Mercator's Latin translation of Gerard Kinckhuysen's Dutch textbook on algebra, *Algebra ofte stel-konst* (1661). Newton composed a manuscript, 'Observations on Kinckhuysen', in 1670 (see Whiteside, *Papers* II) and used it in the preparation of his lectures. He took the opportunity not only to extend Cartesian algebraic methods, but also to restore the geometrical analysis of the ancients, giving his lectures on algebra a strongly geometric flavor.

"When Newton resigned his Lucasian professorship to his deputy William Whiston in December 1701, it was natural that the latter should wish to familiarize himself with the deposited lectures of his predecessor" (Whiteside, *Papers* V, p. 8). Whiston later claimed (in his *Memoirs*, London: 1749) that Newton gave him his reluctant permission to publish the lectures. Whiston arranged with the London stationer to underwrite the expense of printing the deposited manuscript and then subsequently, between September 1705 and the following June, corrected both specimen and proof sheets as they emerged from the University Press. The completed *editio princeps* finally appeared in May 1707, priced at 4s. 6d., without Newton's name on the title page, although references inside the work made no attempt to hide the author's identity. It included an appended tract by Halley on 'A new, accurate and easy method for finding the roots of any equations generally, without prior reduction' (pp. 327-343). Publication of the work had been delayed by Newton, who complained that the titles and headings were not his and that it contained numerous mistakes. Yet when he prepared a second edition in 1722 the changes he introduced were "primarily reorderings of his own manuscript, not corrections of Whiston's additions" (Westfall, p. 649). In reality, Newton's misgivings probably derived more from his reluctance to place before the public a relatively immature and poorly organized work, and one that did not take into account the developments in the subject that had taken place in the quarter century since the manuscript was composed.

For a book that was to become Newton's most often republished mathematical work, the *Arithmetica* initially made little impact in Britain, and was not even graced by a review in the *Philosophical Transactions*. On the Continent the reception accorded the lectures was more positive. "Leibniz, unhesitatingly divining their author beneath the cloak of anonymity, gave them a long review in the *Acta Eruditorum* of Leipzig in 1708. Written thirty years before, he noted, and now deservingly printed by William Whiston, he assured the reader that 'you will find in this little book certain particularities that you will seek in vain in great tomes on analysis.' His close associate, Johann Bernoulli, despite some adverse remarks paid Newton the compliment in 1728 of basing his own course on the elements of algebra upon Newton's text. Perhaps partly in consequence of Newton's recent death, in Britain too the book began about this time to arouse greater interest than when it was first issued in 1707" (Hall, p. 174).

Despite the impressive contributions of the work to the theory of equations, mentioned earlier, it is difficult to pigeonhole the work as being either algebraic or geometric. From one point of view, the *Arithmetica* can be seen as a fulfillment of the programme outlined by Descartes in the *Géométrie* because it teaches how geometrical problems (and also arithmetical and mechanical ones) can be translated into the language of algebra. Paradoxically, however, Newton criticized Descartes, maintaining that, at least in some cases, Apollonian geometry is to be preferred to Cartesian algebra in the analysis of indeterminate problems. Modern analysts, he complained, had confused algebra and geometry: "The Ancients so assiduously distinguished them one from the other that they never introduced arithmetical terms into geometry… recent people by confusing both, have lost the simplicity in which all elegance in geometry consists" (Whiteside, *Papers* V, p. 429). The last section of the work 'The linear construction of equations' (pp. 279-326), is particularly anti-Cartesian (the term 'linear' in this context does not refer to straight lines but derives from Pappus). Newton here deals with the problem of constructing cubics (third-degree equations) that Descartes solved via the intersection of a circle and a parabola. Newton proposed instead to use a curve of degree higher than the conics as a means of construction, namely the conchoid (a fourth-degree curve). Newton regarded the conchoid as preferable because it has a mechanical construction and leads to a more elegant solution of the problem.

William Whiston {1667-1752) was "a member of the first generation of Cambridge students to emulate Newton's method and principles. He went up to Cambridge in 1686, claimed to have attended one or two incomprehensible lectures by Newton on his *Principia*, and was elected a Fellow of Clare Hall in 1691. After taking orders he left Cambridge for a while, returning in 1700 when chosen by Newton to be his deputy as Lucasian Professor. About a year later, upon Newton's

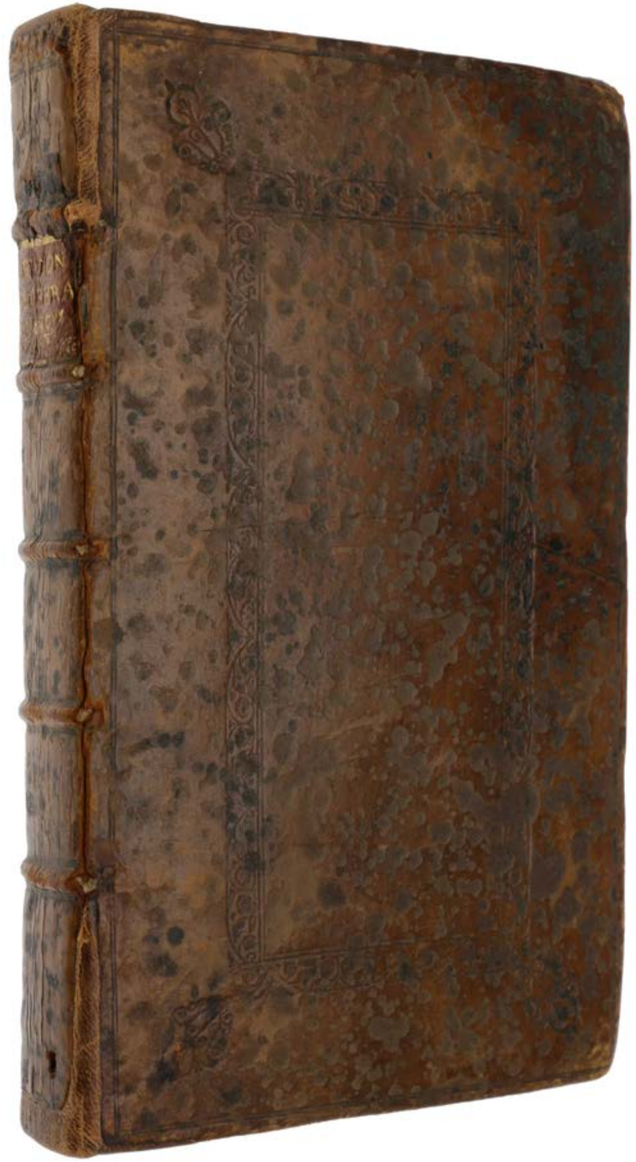

ELEMENTA. 141

PROB. XIX.

recta XY circa axem AB, ad distantiam CD, in data inclinatione ad planum DCB convolvatur, & solidum PQRUTS ista convolutione generatum secetur plano quolibet INQLK: invenire figuram Sectionis.

Sto BHQ vel GHO inclinatio axis AB ad planum sectionis; & L quilibet concursus rectæ cum plano illo. Age DF parallelá AB, & ad AB, & HO demitte perpendiculares LG, LF, LM, unge FG & MG. Dictisque CD $= a$, CH $= b$, M $= x$, & ML $= y$; & propter datum angulum IO posito MH . HG :: d . e : erit $\frac{ex}{d} =$ GH, $b + \frac{ex}{d}$ GC vel FD. Adhæc propter angulum datum resignation and commendation, Whiston succeeded him. Aberrant theology was to be his downfall. While Newton and their common friend Dr Samuel Clarke kept private their doubts about Trinitarianism, the Creed and the Thirty-nine Articles, Whiston sought publicly to amend the errors of the Anglican faith; for this he was summoned before the heads of houses in the university and dismissed from his post in 1710" (Hall, p. 175).

Babson 199; Wallis 277; D. Gjertsen, *Newton Handboo*k, 1986; A. R. Hall, *Isaac Newton*, 1992; R. S. Westfall, *Never at Rest*, 1983.



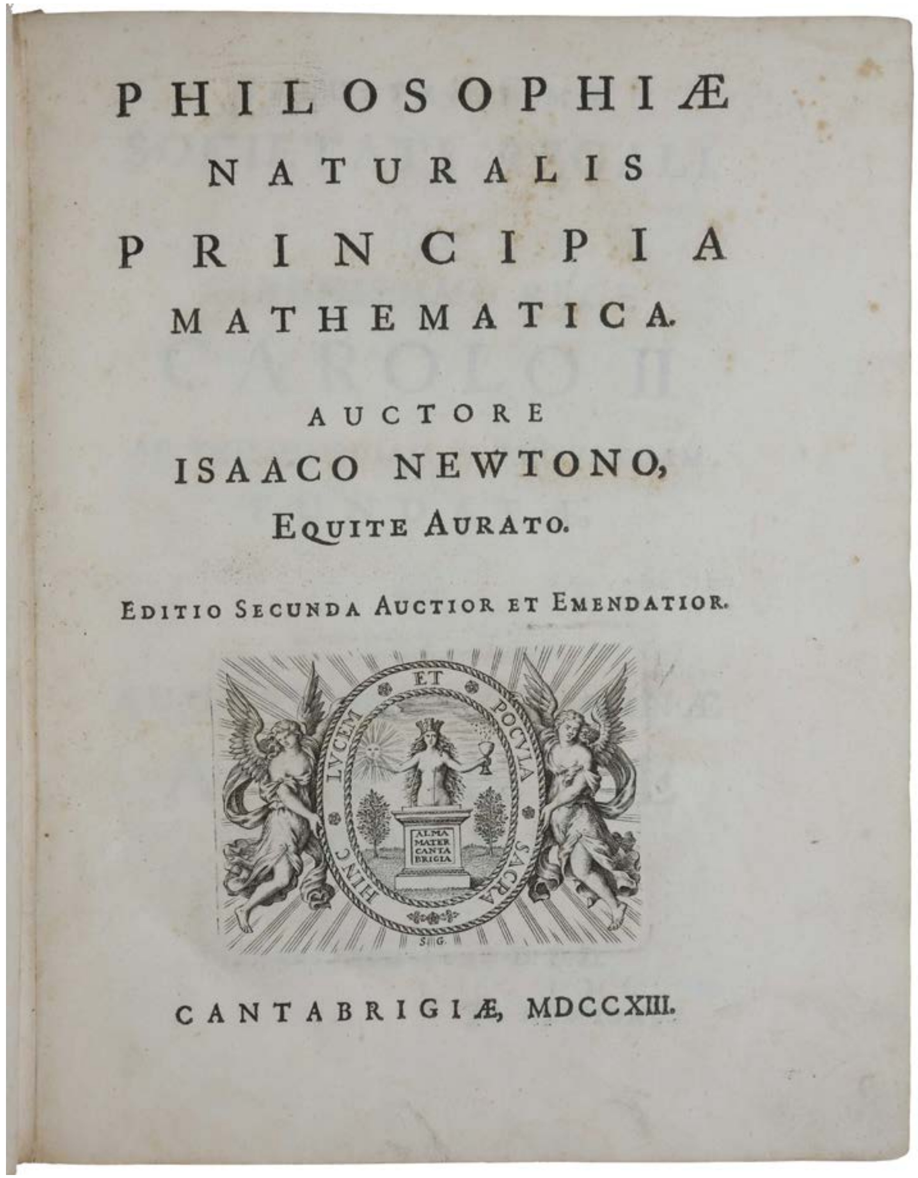
FIRST PRINTING OF THE GENERAL SCHOLIUM

NEWTON, Sir Isaac. *Philosophiae naturalis principia mathematica. Editio secunda auctior et emendatior.* Cambdridge: [Cornelius Crownfield at the University Press], 1713.

$45,000

4to (239 x 190 mm), pp. [xxviii], 484, [8], with engraved device on title, folding engraved plate of the cometary orbit at page 465, woodcut diagrams throughout. Contemporary half vellum , a 7cm split to the lower rear hinge, corners with slight wear. A very light water stain to upper right margin of final 20 leaves, otherwise fine a fresh.

The important second edition of "the greatest work in the history of science" (PMM). This is a fine copy in an unrestored contemporary binding. The *Principia* elucidates the universal physical laws of gravitation and motion which lie behind phenomena described by Newton's predecessors Copernicus, Galileo and Kepler. Newton establishes the mathematical basis for the motion of bodies in unresisting media (the law of inertia); the motion of fluids and the effect of friction on bodies moving through fluids; and, most importantly, sets forth the law of universal gravitation and its unifying role in the cosmos. "For the first time a single mathematical law could explain the motion of objects on earth as well as the phenomena of the heavens … It was this grand conception that produced a general revolution in human thought, equalled perhaps only by that following Darwin's *Origin of Species*" (PMM). Published twenty-six years after the first, this second edition of Newton's *Principia* was printed at the Cambridge University



Press, which Richard Bentley had recently revived. Edited by Roger Cotes (1682-1716), it contains his important preface in which he attacks the Cartesian philosophy "and refutes an assertion that Newton's theory of attraction is a *causa occulta*" (Babson). There is also a second preface by Newton, and substantial additions, the chapters on the lunar theory and the theory of comets being much enlarged. But the most important addition is the *Scholium generale*, which appears here in print for the first time. "The General Scholium, added to the *Principia* in 1713, is probably Newton's most famous writing … In this text, Newton not only challenges the natural philosophy of Descartes, counters criticism levelled against him by Leibniz and appeals for universal gravitation and an inductive method, but he embeds a subversive attack on the doctrine of the Trinity, which he believed was a fourth-century corruption of Christianity" (The Newton Project).

"In 1709 Cotes became heavily involved in the preparation of the second edition of Newton's great work on universal gravitation, the *Philosophiae naturalis principia mathematica*. The first edition of 1687 had few copies printed [about 250]. In 1694 Newton did further work on his lunar and planetary theories, but illness and a dispute with Flamsteed postponed any further publication. Newton subsequently became master of the mint and had virtually retired from scientific work when Bentley persuaded him to prepare a second edition, suggesting Cotes as supervisor of the work.

"Newton at first had a rather casual approach to the revision, but Cotes took the work very seriously. Gradually, Newton was coaxed into a similar enthusiasm; and the two collaborated closely on the revision, which took three and a half years to complete. The edition was limited to only 750 copies, and a pirated version printed in Amsterdam [in 1714] met the total demand" (DSB, under Cotes)

"The most significant feature remains the number of changes introduced into the edition. Rouse Ball (*An Essay on Newton's 'Principia,'* 1893) noted that, of the 494 pages of *Principia* (1687), '397 are more or less modified in the second edition.' Changes include 'the propositions on the resistance of fluids, Book II, section VII props 34 - 40; the lunar theory in Book III; the propositions on the precession of the equinoxes, Book III. prop. 39; and the propositions on the theory of comets, Book III, props. 41, 42'. In addition there was a completely new *Scholium generale.* Also included for the first time were a table of contents (*Index capitum totius opera*) which did no more than list the section headings of the first two books, and a rather sketchy index (*Index rerum alphabeticus*). Cotes also provided an important preface in which he undertook to explain and defend Newton's account of gravity" (Gjertsen, *Newton Handbook*, pp. 475-6).

"When the question of a Preface arose early in 1713, Cotes was initially in some doubt what to include. He first thought of an attack on Leibniz's dynamical treatise *Tentamen* (1689), but much preferred an alternative proposal that either Newton or Bentley should prepare a Preface that Cotes would then loyally 'own … and defend'. Bentley, however, told Cotes that he should undertake the task himself, while Newton, after some initial hesitation, warned Cotes to 'spare ye name of M. Leibniz'. He also declined to read it before its publication. He informed Newton that he would 'add something … concerning the manner of Philosophising' and indicate in particular how the Newtonian approach differed from the Cartesian.

"Cotes has been accused, with some justification, of misrepresenting Newton's notion of gravity. Unaware of Newton's *Letter to Boyle* (1679) and his *Letters to Bentley* (1694), he spoke witheringly of those who 'would have the heavens filled with a fluid matter', while of gravity he insisted that it was just as much a primary property of bodies as 'extension, mobility, and impenetrability'. Yet, to Bentley, Newton had insisted: 'You sometimes speak of gravity as essential and inherent to matter. Pray, do not ascribe that notion to me' (*Correspondence*, III, p. 240).

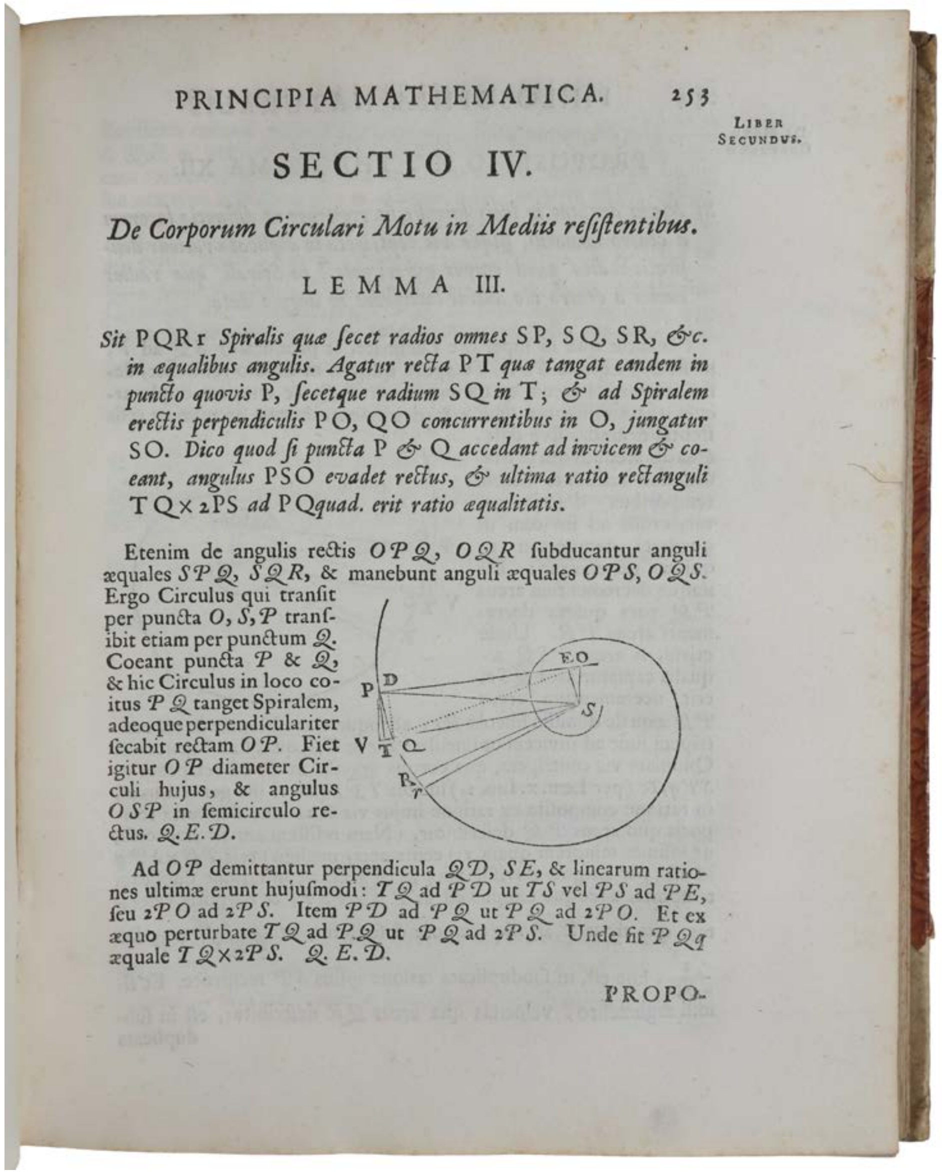
Earlier, to Boyle, he had spoken of an 'etherial substance' diffused through all space. Oddly enough Newton accepted the misrepresentation of his views without complaint, public or private.

"On two other topics Cotes was more accurate. The first was a strong attack launched against Cartesian physics in general and the vortex theory of planetary motion in particular. The second was a commitment to providentialism, with the claim that 'this world, so diversified with that variety of forms and motions we find in it, could arise from nothing but the perfectly free will of God directing and presiding over all'.

"The General *Scholium* … was sent to the editor, Roger Cotes, on 2 March 1713 with the comment: 'I intended to have said much more about the attraction of the small particles of bodies, but upon second thoughts I have chosen rather to add but one short Paragraph about that part of Philosophy' (Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes*, 1850, p. 147) …

"One reason for the new *Scholium* was to answer criticisms raised by Leibniz and Berkeley against the general cosmology of *Principia.* Newton had been accused, for example, of presenting God as no more than an incompetent watchmaker incapable of making a clock which did not need his regular attention each time it broke down. Newton chose to reply by presenting in some of his finest prose his own conception of God. It is not a creed many Christians today will find attractive.

"Newton rejected the idea that the true nature of God consisted in his possession of the familiar attributes of perfection; it lay rather in his 'dominion'. For, he declared, 'a being, however perfect, without dominion, cannot said to be Lord God'. We may well admire him for his perfections but 'we reverence and adore him on account of his dominion'. Further, this dominion was exercised 'in a


PRINCIPIA MATHEMATICA. 253

LIBER SECUNDUS.

SECTIO IV.

De Corporum Circulari Motu in Mediis resistentibus.

LEMMA III.

Sit PQRr *Spiralis quæ secet radios omnes* SP, SQ, SR, *&c. in æqualibus angulis. Agatur recta* PT *quæ tangat eandem in puncto quovis* P, *secetque radium* SQ *in* T; *& ad Spiralem erectis perpendiculis* PO, QO *concurrentibus in* O, *jungatur* SO. *Dico quod si puncta* P *&* Q *accedant ad invicem & coeant, angulus* PSO *evadet rectus, & ultima ratio rectanguli* TQ × 2PS *ad* PQ*quad. erit ratio æqualitatis.*

Etenim de angulis rectis *OPQ, OQR* subducantur anguli æquales *SPQ, SQR,* & manebunt anguli æquales *OPS, OQS.* Ergo Circulus qui transit per puncta *O, S, P* transibit etiam per punctum *Q.* Coeant puncta *P* & *Q,* & hic Circulus in loco coitus *PQ* tanget Spiralem, adeoque perpendiculariter secabit rectam *OP.* Fiet igitur *OP* diameter Circuli hujus, & angulus *OSP* in semicirculo rectus. *Q.E.D.*

Ad *OP* demittantur perpendicula *QD, SE,* & linearum rationes ultimæ erunt hujusmodi: *TQ* ad *PD* ut *TS* vel *PS* ad *PE,* seu *2PO* ad *2PS.* Item *PD* ad *PQ* ut *PQ* ad *2PO.* Et ex æquo perturbate *TQ* ad *PQ* ut *PQ* ad *2PS.* Unde fit *PQq* æquale *TQ×2PS. Q.E.D.*

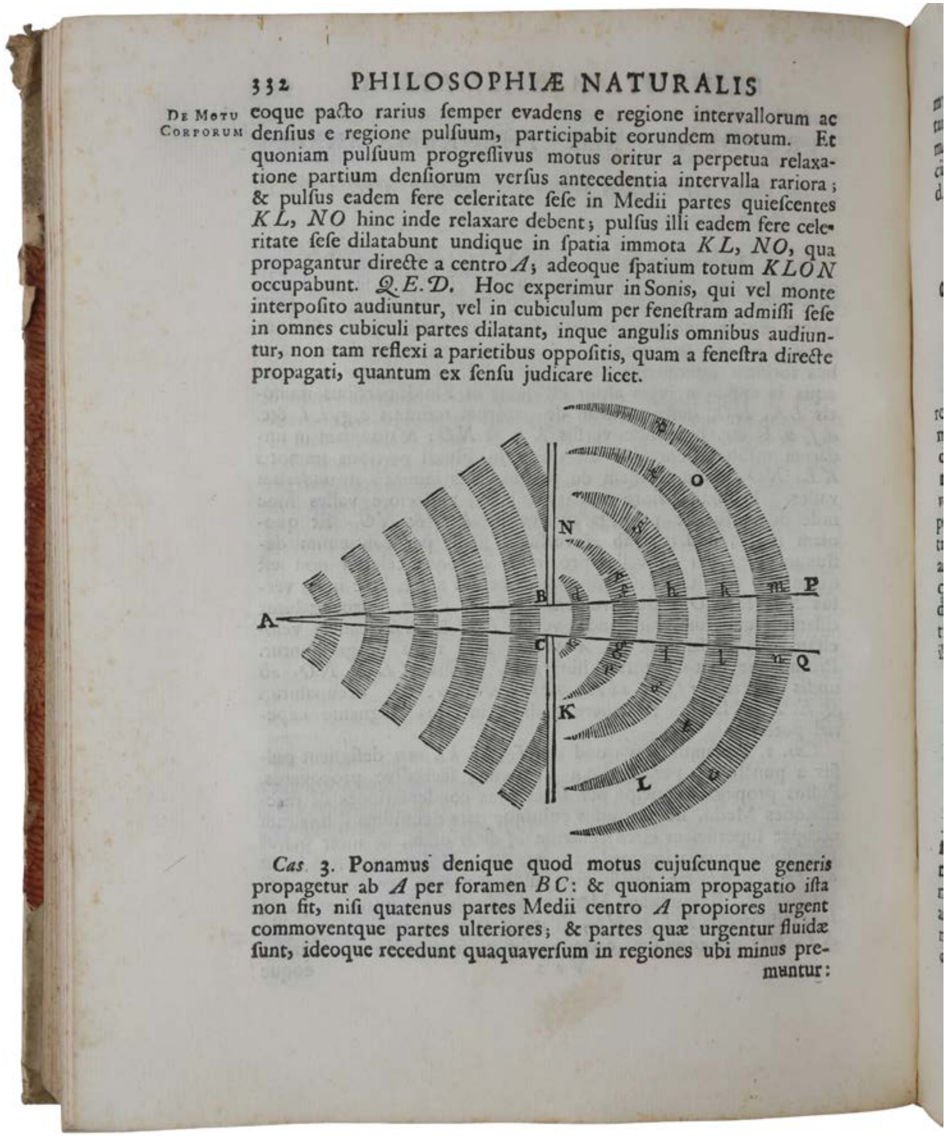
PROPOmanner not at all human … in a manner utterly unknown to us'. We know God only through his works, 'by his most wise and excellent contrivances of things'. There seems little room in Newton's austere theology for anything like a personal God. Indeed, he went out of his way to dismiss such an option. God, he insisted, 'is utterly void of all body and bodily figure, and can therefore neither be seen, nor heard, nor touched'. From God, Newton turned to gravity. In often-quoted words, he declared his failure to have discovered any cause for gravity. As, he insisted, 'I frame no hypotheses', any attempt to speculate about possible causes had no place in experimental philosophy; 'it is enough', he concluded the point, 'that gravity does really exist, and act according to the laws which we have explained'. The *Scholium* concluded with an intriguing paragraph, presumably the item referred to in the letter to Cotes above. He spoke of 'a most subtle spirit which pervades and lies hid in all gross bodies'. It is through this spirit, Newton proposed, that bodies cohere, 'light is emitted, reflected, refracted, inflected, and heats bodies', sensations are excited, electric bodies repel and attract, and the will operates. A formidable list, and one demanding some explanation. Newton merely concludes, however: 'But these are things that cannot be explained in few words'" (Gjertsen, pp. 463-4).

"The difficult style of the General Scholium reflects two dynamics in particular: first, some of the ideas present in this document were considered controversial and even heretical; second, Newton believed that his readers could be divided into two camps, the vulgar (who are not be able to understand higher truths) and the cognoscenti (who are). Newton was primarily interested in reaching those in the latter category. In order to deal with the first dynamic and to achieve the goal of the second, Newton deliberately constructed this document so that the uncontroversial and more broadly acceptable ideas appeared on the outer or "open" layers, while the specialised meanings for the *adepti* were concealed in the inner or "closed" layers, which are increasingly difficult to penetrate without specialised or privileged knowledge. For example, virtually all readers recognised and accepted Newton's natural theological argument in the fourth paragraph, but only a select few recognised the attack on the doctrine of the Trinity in the fourth through sixth paragraphs (which was precisely Newton's aim). The General Scholium is constructed much like a Russian doll and, accordingly, restricts access to its ultimate meaning. In using this strategy, Newton more closely resembles the ancient Pythagoreans, who hid higher theological and philosophical truths in similitudes and riddles, than a modern scientist (which Newton was not). When interpreting the General Scholium, it is important to take into account several backdrops: Newton's attack on Descartes' method and physics, Leibniz's contention that Newton's conception of an intervening God was weak, and the controversy surrounding the publication of Newton's follower Samuel Clarke's critique of the doctrine of the Trinity in 1712. In the General Scholium, Newton takes the dangerous step of supporting several arguments outlined in Clarke's book. Denial of the Trinity was illegal in Britain until 1813, a full century after the General Scholium first appeared. Thus, the most revolutionary and important book in the history of science, championed by the orthodox British establishment throughout the eighteenth century and beyond, ends on a subversive note" (The Newton Project).

"Printed in an edition of 750 copies, it was sold in quires for 15s and bound for a guinea. Bentley's accounts have survived and show that the total cost of the printing came to £117 4s 1½d. He sold 375 copies to various booksellers and individuals at an average cost of 13s each. The printer C. Crownfield took a further 200 copies at 11s each. This yielded Bentley a profit of £200 while still holding a substantial stock for future sale. Some of these were in fact presentation copies. Twelve were given to Cotes and a further six to Newton. There is also a distribution list in Newton's papers of another seventy or so recipients. It covers most of the great libraries, scientific institutions and Courts of Europe. Individuals listed include

Cassini, de la Hire, Varignon, Bernoulli, Leibniz and Machin. But even this list is incomplete as it contains no reference to the copies known to have been presented by Newton to Queen Anne personally on 27 July 1713, nor a copy he sent to Yale University" (Gjertsen, pp. 475-6).

Babson 12; ESTC T93210; PMM 161 (for the first edition); Wallis 8.



332 PHILOSOPHIÆ NATURALIS

De Motu Corporum

eoque pacto rarius semper evadens e regione intervallorum ac densius e regione pulsuum, participabit eorundem motum. Et quoniam pulsuum progressivus motus oritur a perpetua relaxatione partium densiorum versus antecedentia intervalla rariora; & pulsus eadem fere celeritate sese in Medii partes quiescentes *KL*, *NO* hinc inde relaxare debent; pulsus illi eadem fere celeritate sese dilatabunt undique in spatia immota *KL*, *NO*, qua propagantur directe a centro *A*; adeoque spatium totum *KLON* occupabunt. *Q.E.D.* Hoc experimur in Sonis, qui vel monte interposito audiuntur, vel in cubiculum per fenestram admissi sese in omnes cubiculi partes dilatant, inque angulis omnibus audiuntur, non tam reflexi a parietibus oppositis, quam a fenestra directe propagati, quantum ex sensu judicare licet.

Cas. 3. Ponamus denique quod motus cujuscunque generis propagetur ab *A* per foramen *BC*: & quoniam propagatio ista non fit, nisi quatenus partes Medii centro *A* propiores urgent commoventque partes ulteriores; & partes quæ urgentur fluidæ sunt, ideoque recedunt quaquaversum in regiones ubi minus premuntur:

IN EXCEPTIONALLY RARE ORIGINAL PUBLISHER'S BINDING

NIETZSCHE, Friedrich. *Die Geburt der Tragödie aus dem Geiste der Musik.* Leipzig: E.W. Fritzsch, 1872.

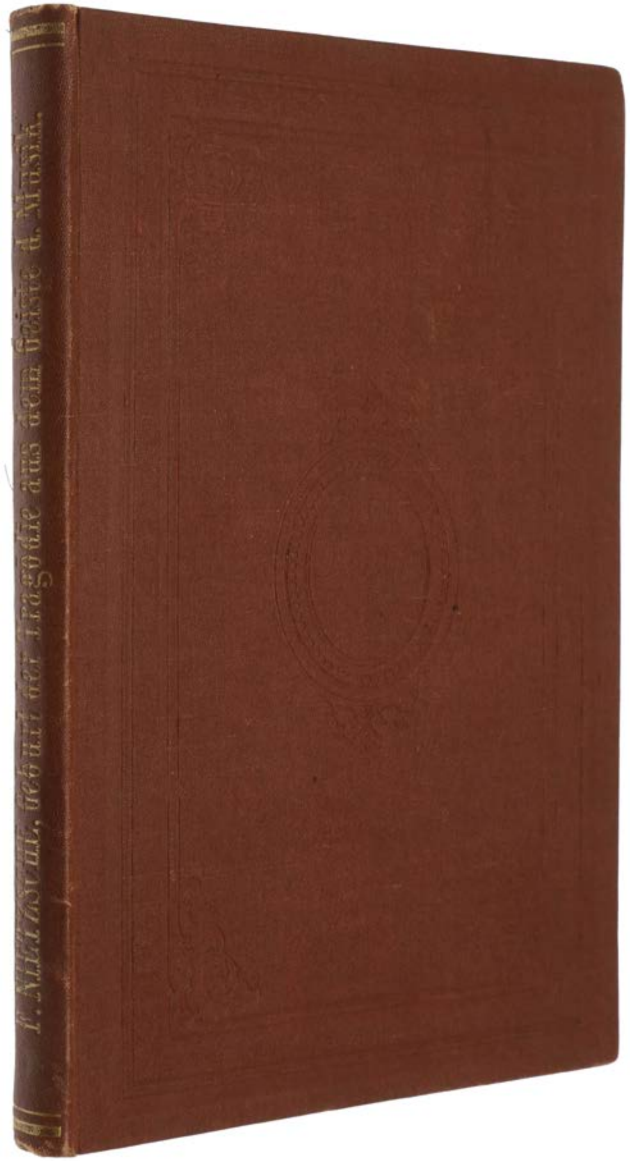
$14,500

8vo (216 x 138 mm) pp. [i-iii] iv [1] 2-143 [144]. Original publisher's dark-rust binding with an ornate blind-stamped design on the front and rear covers and the spine lettered and filleted in gilt. There is some light browning to the edges of the page margins and light foxing throughout. Rear hinge with a 10 cm split. Entirely unrestored copy in its original state. An extremely well preserved copy of this unusual and all-but-unobtainable original publisher's cloth binding.

First edition in exceptionally rare original publisher's cloth binding. This copy previously handled by Bill Schaberg: "When I wrote The Nietzsche Canon: A Publication History and Bibliography (The University of Chicago Press, 1995), I had never even heard of these cloth copies of Nietzsche's first book, put out by his publisher, Fritzsch. So, it was quite a shock when someone offered this copy to me. It turns out that Fritzsch's contemporary advertisements for the book mention a cloth binding, so this is not just a figment of some bookseller's imagination."

This, Nietzsche's first book, is a compelling argument for the necessity for art in life. It is fueled by his enthusiasms for Greek tragedy, for the philosophy of Schopenhauer and for the music of Wagner, to whom this work was dedicated.

Nietzsche argues that the tragedy of Ancient Greece was the highest form of art

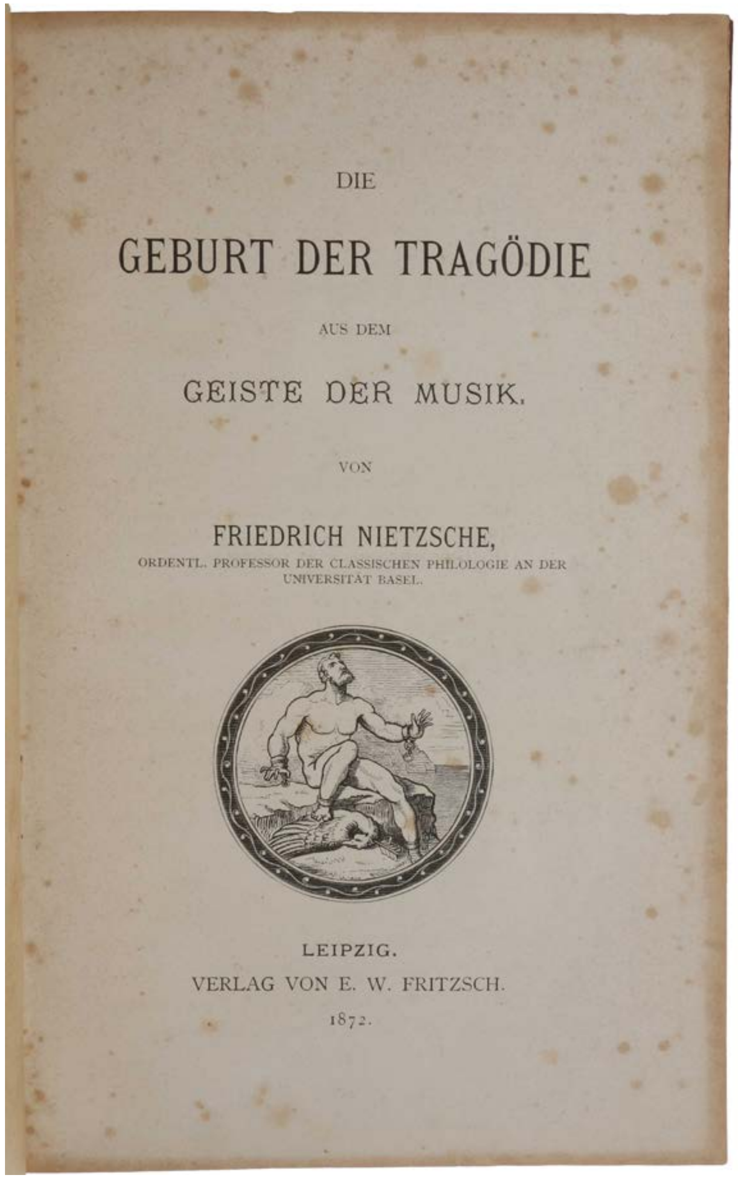


due to its mixture of both Apollonian and Dionysian elements into one seamless whole, allowing the spectator to experience the full spectrum of the human condition. The Dionysian element was to be found in the music of the chorus, while the Apollonian element was found in the dialogue which gave a concrete symbolism that balanced the Dionysiac revelry. Basically, the Apollonian spirit was able to give form to the abstract Dionysian.

In contrast to the typical Enlightenment view of ancient Greek culture as noble, simple, elegant and grandiose, Nietzsche believed the Greeks were grappling with pessimism. The universe in which we live is the product of great interacting forces; but we neither observe nor know these as such. What we put together as our conceptions of the world, Nietzsche thought, never actually addresses the underlying realities. It is human destiny to be controlled by the darkest universal realities and, at the same time, to live life in a human-dreamt world of illusions.

The issue, then, or so Nietzsche thought, is how to experience and understand the Dionysian side of life without destroying the obvious values of the Apollonian side. It is not healthy for an individual, or for a whole society, to become entirely absorbed in the rule of one or the other. The soundest (healthiest) foothold is in both. Nietzsche's theory of Athenian tragic drama suggests exactly how, before Euripides and Socrates, the Dionysian and Apollonian elements of life were artistically woven together. The Greek spectator became healthy through direct experience of the Dionysian within the protective spirit-of-tragedy on the Apollonian stage.

The Birth of Tragedy was the best selling book that Nietzsche ever published; still, it did not sell quickly. The Wagners had feared that there might not be an audience for the work and their apprehensions proved to be well-founded. A prediction that Nietzsche had once made to Rohde proved true: "The philologists won't read



it on account of the music, the musicians won't read it on account of the philology and the philosophers won't read it on account of the music and the philology." False hopes for brisk sales plagued the first half-year. In mid-April, Nietzsche was writing home that "a new edition of my book will be needed soon,"34 but the necessity of printing a second edition did not materialize quickly. By 20 July, Fritzsch complained that there had been "no results" even though he had "sent out a fair number of copies." (Schaberg, The Nietzsche Canon, p. 27).

PMM 289 - MEASURING ELECTRICITY

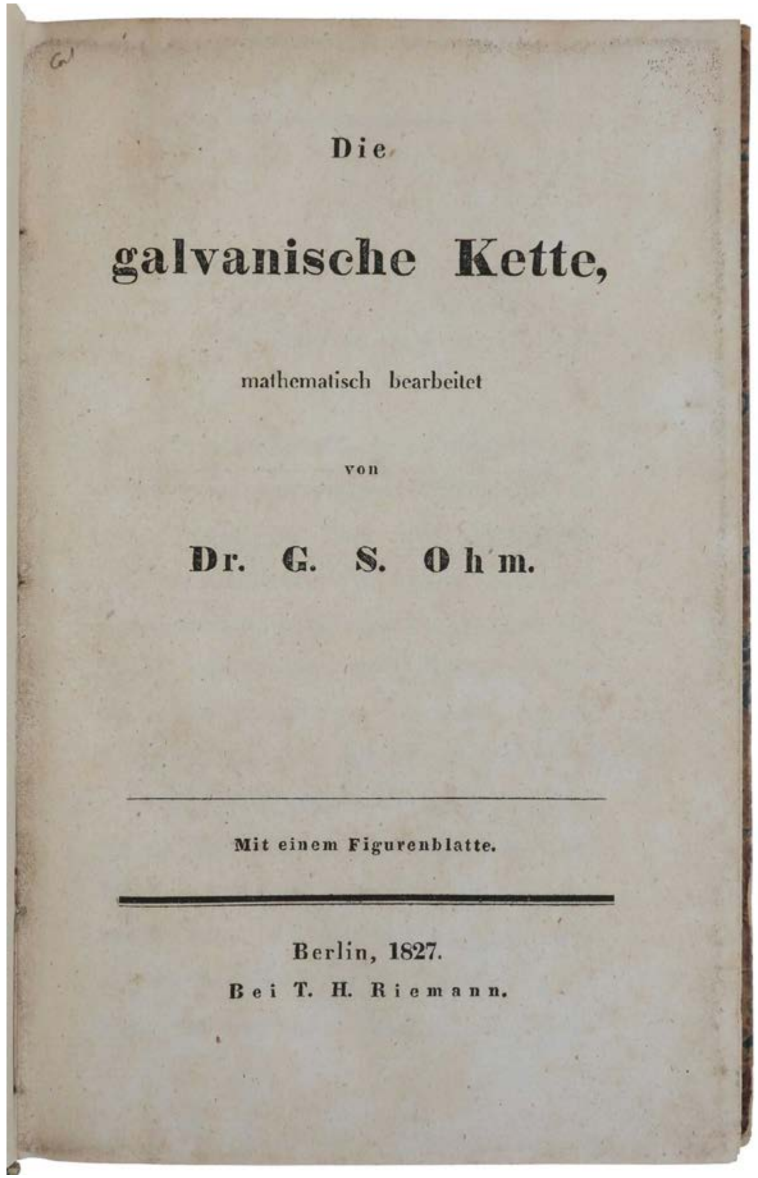
OHM, Georg Simon. *Die galvanische Kette, mathematisch bearbeitet.* Berlin: J.G.F. Kniestädt for T.H. Riemann, 1827.

$29,500

8vo (197 x 127 mm), pp. iv, 245 (errata on p. 245), [1, blank], [2, publisher's advertisements]. Contemporary marbled boards, sprinkled edges.

First edition, very rare complete copy, of "Ohm's great work" (DSB), containing the fully-developed presentation of his theory of electricity, including Ohm's Law. The present copy not only retains the errata leaf R1, often lacking, but also the one-leaf publisher's list R2, which is almost always missing (the Dibner, Horblit/Evans, Norman, Waller and Wellcome copies, and the copy described by Grolier Science, all lack it). "Ohm's great contribution – 'The Galvanic Chain Mathematically Calculated' – was to measure the rate of current flow and the effects of resistance on the current. 'Ohm's law' – that the resistance of a given conductor is a constant independent of the voltage applied or the current flowing (that is, $C = E/R$, where C = current, E = electromotive force and R = resistance) – was arrived at theoretically by analogy with Fourier's heat measurements (1800-14)" (PMM). Although copies of this book appear with some regularity on the market, we have found only three absolutely complete copies, as here, at auction since 1938. The Elihu Thomson copy, sold Christie's New York, 1999 ($11500), was subsequently offered by Jonathan Hill, who wrote (Cat. 131, No. 71), "I have had a good number of copies of this book and this is the first to have the leaf of ads".

Provenance: August Stähelin (1812-1886), Swiss politician and president of the



Swiss Council of States, 1857/1858 (signature dated 5/25/1843 to front flyleaf); Physikalische Anstalt des Bernoullianums, Basel (two old library stamps to front flyleaf and shelf-mark label to spine).

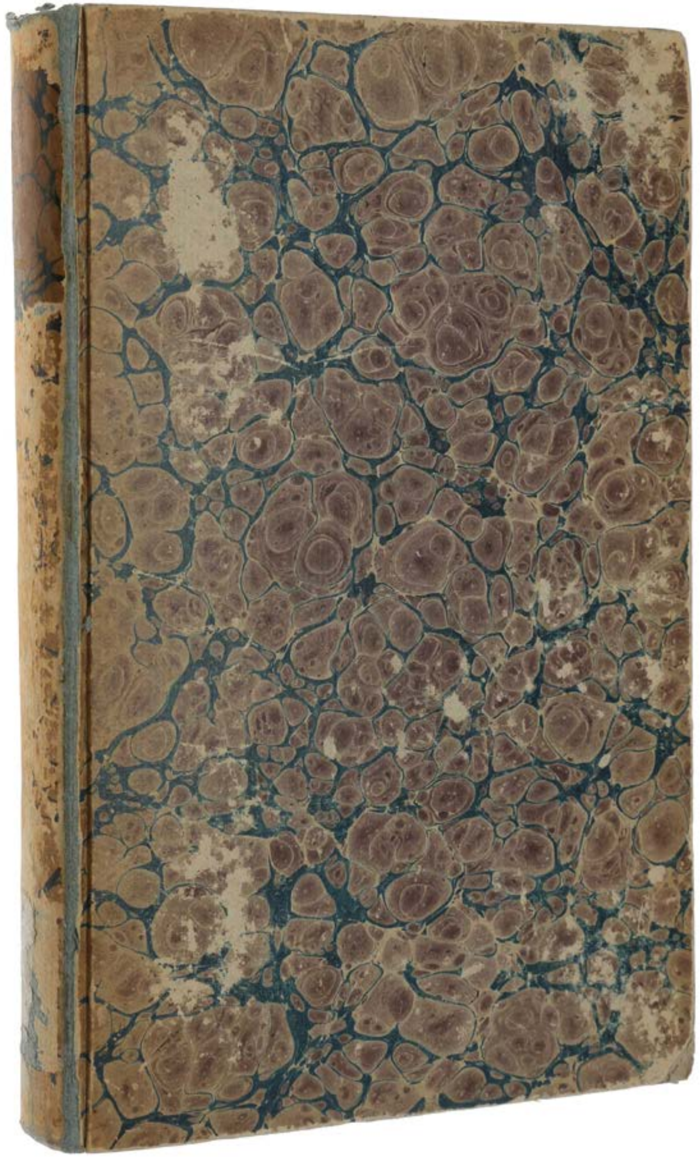
"The expression "investigated mathematically" in the title of Ohm's book described his objective: to deduce the properties of the galvanic circuit from a set of "fundamental laws." The first of these laws states that electricity passes only between adjacent particles of the conductor and that the quantity passed is proportional to the difference in electroscopic force at the two particles. Here Ohm drew on an analogy to Fourier's heat theory, in which the quantity of caloric passed between two particles is proportional to the difference between their temperatures. Ohm's second law, supported by Coulomb's experiments, states that the loss of electricity in unit time from the conductor to the air is proportional to the electroscopic force of the electricity, to the amount of surface exposed, and to a coefficient that depends on the air; acknowledging that this second law has little bearing on the phenomena of galvanic currents, Ohm included it to make the theory complete and parallel to Fourier's theory of heat. The third and last law states that two bodies in contact maintain the same difference of electroscopic force at their common surface, which is the basic tenet of the contact theory of the battery. From these three laws, Ohm derived differential equations for electric currents analogous to Fourier's and Poisson's for heat, which indicated to him an "intimate connection" between the two phenomena.

"The mathematical expression of Ohm's physical analogy between the conduction of electricity and the conduction of heat is an equation identical in form to Fourier's. The only difference is in the physical significance of the symbols entering the equation: in Fourier's the independent variable is the temperature; in Ohm's it is the electroscopic force, which is the force with which an electroscope, a body of constant electrical condition, is attracted to or repelled from a body it is brought into contact with. Following an approach Fourier had made familiar, Ohm mathematically divided the conductor into infinitely thin discs and calculated the quantity of electricity transferred per unit time across the parallel surfaces and outward through the edges of the discs. The result was the fundamental second-order, partial differential equation of Ohm's theory … Having formulated the physical problem as a differential equation, Ohm then solved it to obtain relations between directly measurable quantities. Manipulating the solution written as an infinite series of sine and cosine functions with damping coefficients, Ohm arrived at … his law relating electric current, resistance, and tension.

"The "torch of mathematics", Ohm wrote, shines through physics, illuminating its dark places. With his *Galvanic Circuit*, he could claim that mathematics had "incontrovertibly" possessed a "new field of physics, from which it had hitherto remained almost totally excluded." By means of mathematical deductions from a few experimental "principles," galvanic phenomena had been brought together in "closed connection" and presented as a "unity of thought." The deductions showed that the seemingly disparate phenomena of electric tension and current are really connected in nature, partially realizing Ohm's goal of fashioning the theory of electricity as a "whole" …

"When the *Galvanic Circuit* appeared, few physicists in Germany knew mathematical physics sufficiently to understand it. Journal editors were afraid their readers could not understand papers containing the simplest mathematics, as Ohm complained. For reviewing, Ohm sent a copy of his book to Schweigger at Halle, who did not see the point of a mathematical treatment. To have it evaluated, the Prussian minister of culture sent a copy to Kämtz, Schweigger's colleague at Halle, who could not follow the mathematical derivation, as is clear from his cautious review of it. In Berlin, which desperately needed a "mathematical physicist," Ohm's work received its most famous and, to Ohm, irritating review from Pohl, who was neither a mathematical nor a typical Berlin physicist … [Pohl] complained that Ohm had not paid attention to the "essence" of the circuit and had merely expressed some properties of electricity in formulas. This was no achievement but only a replication of Fourier's and Poisson's work in another part of physics … In general, the response to Ohm's book reflected a paucity of physicists with good mathematical knowledge in Germany in the late 1820s. But one German review of Ohm's book showed complete comprehension. Ohm sent his book to Kastner in Erlangen to be reviewed in his journal. Kastner asked the mathematician Wilhelm Pfaff to write the review, but Pfaff did not know the literature … The review that appeared under Pfaff's name was apparently written by Ohm himself, after his brother had interceded. The review was, of course, favourable, but a favourable review does not necessarily make a successful book. Sales of the *Galvanic Circuit* were unimpressive, and Ohm paid friends to order the book from out of town to make a better impression on the publisher. The book was in print for eight years, then not again for sixty years, though in the meantime it had come out in several translations. Ohm sent free copies to everyone who might help him, as he did not want to return to his teaching in Cologne" (Jungnickel & McCormmach, pp. 53-7).

Georg Simon Ohm (1789-1854) was educated, together with his brother Martin, the mathematician, principally by his father, who gave his sons a solid education in mathematics, physics, chemistry, and the philosophies of Kant and Fichte; their considerable mathematical ability was recognized in 1804 by the Erlangen professor Karl Christian von Langsdorf, who enthusiastically likened them to the Bernoullis. Ohm received his Ph.D. from the University of Erlangen in 1811, but after teaching there for three semesters as a *Privatdozent*, he was only able to find employment as a schoolteacher, first at Bamberg and then from 1817 at the recently reformed Jesuit Gymnasium at Cologne. "The ideals of *wissenschaftliche Bildung* had infused the school with enthusiasm for learning and teaching; and



this atmosphere which appears later to have waned, coupled with the requirement that he teach physics and the existence of a well-equipped laboratory, stimulated Ohm to concern himself for the first time avidly with physics. He studied the French classics – at first Lagrange, Legendre, Laplace, Biot, and Poisson, later Fourier and Fresnel – and, especially after Oersted's discovery of electromagnetism in 1820, did experimental work in electricity and magnetism. It was not until early in 1825, however, that he undertook research with an eye toward eventual publication" (DSB).

"Feeling increasingly burdened by his teaching at a secondary school in Cologne, Ohm took his father's advice and asked the Prussian minister of culture for a year off. To the minister he explained that for a long time he had divided his attention between mathematics and physics, though for practical reasons he had emphasized physics. By taking up physics he did not have to give up mathematics, he said, since the two were closely connected. His appeal to the minister contained an element of calculation: he regretted that the French had recently dominated physics, and he had been studying the mathematical works by Laplace, Fourier, Poisson, Fresnel, and other French masters to see what they had left for him to do. He had been doing purely experimental work on the whole, but he had in hand a mathematical theory of galvanic current; all he needed was time off to complete it and, he added, to work out a theory of light as well. On the recommendation of Ermann, the minister approved Ohm's request. With half salary, Ohm went off to Berlin in 1826 to live in his brother's house, where he had a small apartment with space for doing experiments. With these improved working conditions, he developed the mathematical theory of the galvanic current, perhaps with his brother's help with the calculations. The result was the *Galvanic Circuit* …

"After the *Galvanic Circuit*, Ohm carried out important researches on tones and on crystal optics, and he undertook a comprehensive theory of physics. In the year the Galvanic Circuit was published, he began to speak of a greater work to come, one that would treat the whole of molecular physics. Apparently he wanted to derive all physical phenomena from analytical mechanics and molecular hypotheses. Ohm published the first volume containing the mathematical preliminaries. In the second volume he intended to treat dynamics and in the third and fourth its application to physical phenomena. But Ohm's late call to Munich University interfered with his plan, and the volumes never appeared. The existence of the plan, however, pointed to the confidence of the author of the Galvanic Circuit in the power of mathematical physics to complete the understanding of nature that Newton had begun" (Jungnickel & McCormmach, pp. 53-8).

Widespread understanding and acknowledgement of the importance of the *Galvanic Circuit* did not come until the late 1830s and early 1840s, when Ohm's work began to receive official recognition, with corresponding memberships of the Berlin and Turin academies in 1839 and 1841 respectively, the award of the Royal Society of London's Copley Medal in 1841 and finally (just before his death), the chair of physics at the University of Munich in 1852. In 1881, when the importance of Ohm's work was fully understood, the standard unit of electrical resistance was named the ohm in his honour at the Paris Conference on international standards.

Dibner, Heralds 63; Horblit 81; Norman 1607; PMM 289; Sparrow, *Milestones of Science*, 154.

Waller 11419; Wellcome IV, p. 260; Wheeler Gift Cat. 835. Jungnickel & McCormmach, *Intellectual Mastery of Nature. Theoretical Physics from Ohm to Einstein, Volume 1: The Torch of Mathematics, 1800 to 1870*, 1990.

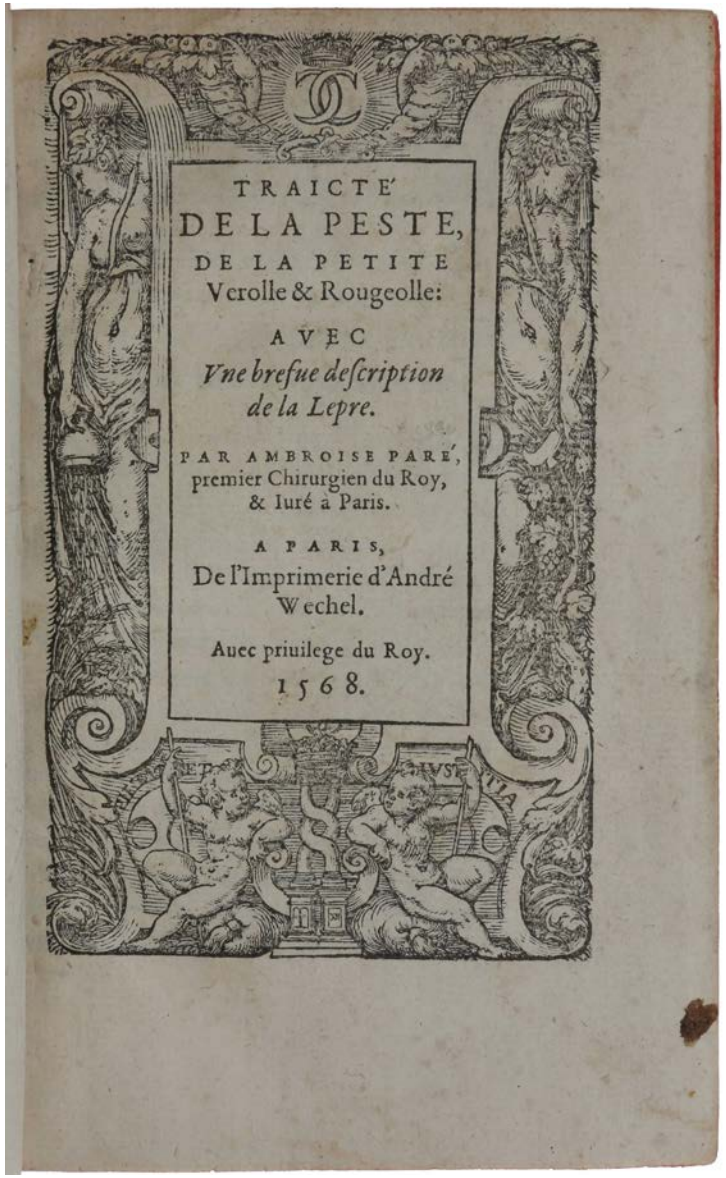
THE PLAGUE, SMALLPOX, AND MEASLES

PARÉ, Ambroise. *Traicté de la peste, de la petite verolle & rougeolle: avec une brefve description de la lepre.* Paris: André Wechel, 1568.

$65,000

8vo, pp. [xvi], 235 (recte 275), [4] (last leaf blank) (light browning and dampstaining, minor marginal worming). With woodcut title-border and woodcut printer's device at end. Seventeenth-century calf, spine with floral gilt decoration and lettering-piece (minor worming to upper part of spine, lightly rubbed). A very good and large copy, entirely unrestored.

First edition of Paré's extremely rare treatise on the plague, smallpox and measles, based upon his own direct observations of these diseases, "one of his best works" (Thornton, p. 63). "Having passed the winter of 1564-65 on tour in Provence with Catherine de Medici and the young King Charles IX, where the ravages of a plague epidemic, added to poverty and general misery, were painfully apparent, Paré was requested by the queen mother to make whatever knowledge he possessed of the disease available to the world. He therefore puts into a book his ideas as to its cause, transmission, and treatment, and says he writes only of what he has seen by long experience during his three years at the Hôtel-Dieu, his travels, his practice in Paris, and his own slight attack while he was serving his internship. This is one of Paré's most systematic treatises; for its careful symptomatology and thorough description of treatment, it deserves to rank among the best of his writings" (Doe). "His practical measures in regard to hygiene and quarantine are excellent in most respects, although he followed the generally prevalent idea that



bonfires of aromatic woods, such as juniper and pine, should be made throughout the streets to purify the air. He humanely urges that, 'The magistrates must have all sick folks attended by physicians, surgeons, and apothecaries, good men, of experience: and must treat those that are attacked and isolate them, sending them to places set apart for their treatment, or must shut them up in their own houses (but this I do not approve, and would rather they should forbid those that are healthy to hold any converse with them) and must send men to dress and feed them, at the expense of the patients, if they have the means, but if they are poor, then at the expense of the parish. Also they must forbid the citizens to put up for sale the furniture of those who have died of the plague'" (Packard, pp. 80-81). "Paré's original books, all very rare today, were handy volumes, small enough for the field surgeon's knapsack" (Hagelin, p. 35). COPAC lists Wellcome only. ABPC/RBH list only one other copy, in a rebacked 19th century binding and with the final four leaves re-margined (Sotheby's, 15 June 2005, lot 49, €18,000). The present copy, in a 17th century binding, is entirely unrestored.

"Because such a high proportion of those who suffered the symptoms of plague died from it, and in a very short space of time, it was not a disease to which people could ever become inured. Every outbreak appeared like a divine judgement. The medical men acknowledged this divine origin of plague. Ambroise Paré, surgeon to four French kings and the most celebrated surgical innovator of his day, devoted a chapter of his 1568 book on plague to 'the Divine causes of an extraordinarie Plague', claiming that:

'It is confirmed, constant, and received opinion in all Ages amongst Christians, that the Plague and other Diseases which violently assail the life of Man, are often sent by the just anger of God punishing our offences. The Prophet Amos hath long since taught it, saying *Shall there be affliction, shall there be evil in a Citie, and the Lord hath not done it?* On which we truly we ought always to meditate … For thus we shall learn to see God, our selves, the Heaven and Earth, the true knowledge of the causes of the Plague, and by a certain Divine Philosophy to teach, God to be the beginning and cause of the second causes, which well without the first cause cannot go about, nor attempt, much less perform any thing. For from hence they borrow their force, order, and constancy of order; so that they serve as instruments for God, who rules and governs us, and the whole World, to perform all his works, by that constant course of order, which he hath appointed unchangeable from the beginning. Wherefore all the cause of a Plague is not to be attributed to these near and inferior causes or beginnings, as the Epicures, and Lucianists commonly do.'

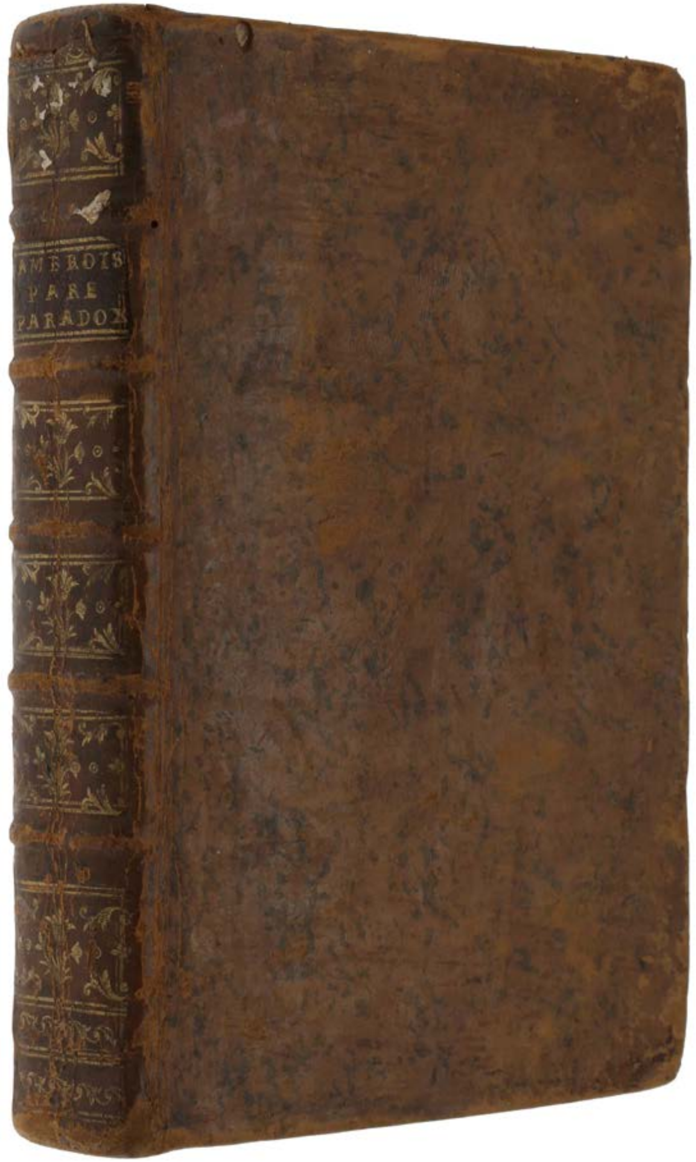
[This and subsequent quotations from Paré are from the English translation of the present work, *A Treatise of the Plague*, London: Thomas Johnson, 1630.]

"Thus only atheists and scoffers would claim that plague has only natural (secondary) causes. However the first cause – God – customarily acts through secondary causes, so Paré as a medical man could then immediately turn to the natural causes of plague to discuss its causes, course, and cure.

"Paré's account of plague, which was written at the request of the French queen-mother, Catherine de Medici, after a widespread outbreak of the disease in France in 1565, is one of the classic descriptions of the disease, and indicates how painful and fearsome it was. In Paré's view, the 'first original' of plague was a corruption of the air, entering the body and reaching the heart, 'the Mansion, or as it were the Fortress or Castle of Life', where it acted like a poison, attacking the vital spirit. If the vital spirit is weak, it 'flies back into the Fortress of the Heart, by the like contagion infecting the Heart, and so [it infects] the whole Body, being spread into it by the passages of the Arteries'. The pestiferous poison brought about a burning fever, whose effects drove sufferers to desperate measures. They had ulcerated jaws, unquenchable thirst, dryness and blackness of the tongue, 'and it causeth such a Phrensy by inflaming the Brain, that the Patients running naked out of their beds, seek to throw themselves out of Windows into the Pits and Rivers that are at hand'.

"Because he saw plague as a poison, a poison which acted on the heart and then on the blood, Paré's first concern in treatment was to provide an antidote, which by its specific property would defend the heart from the poison by opposing the specific power of the poison. It had to be quick-acting, since the poison itself was very swift. Paré's antidote of choice was a mixture of treacle and mithridatium, an ancient drug compounded of up to 60 different ingredients and thought to be a sovereign protection against poison. Taken inwardly or applied outwardly over the region of the heart and to the carbuncles, this antidote draws the poisons out 'as Amber does Chaff', and then digests the poison and robs it of its deadly force. If the plague came with eruptions or little red spots all over the body (these are the famous 'tokens' of the plague), caused by the poison increasing the heat of the blood, Paré advocated that a 'drawing' medicine should be applied, such as pig's grease mixed with mercury and herbs, to draw the poison through the skin. Alternatively, he suggests, 'if any noble or gentleman refuse to be anointed with this unguent, let them be enclosed in the body of a Mule or Horse that is newly killed, and when that is cold let them be laid in another; until the pustules and eruptions do break forth, being drawn by the natural heat' of the animal's corpse.

"Even worse than the fever or the red spots in plague were the distinctive and painful 'buboes' (or carbuncles), hard black tumours which appeared in the neck, armpits and groin. Following classical Greek medical teaching, Paré saw these buboes as 'emunctories', natural outlets for the infected matter draining from the three main organs of the body, the brain, heart and liver respectively. The pain



of the buboes was so intense that sufferers wanted to have them lanced by the surgeon, the pain increasing as the bubo hardened and ripened. Paré's remedy was to apply ointment, then a cupping-glass heated very hot; kept on for a quarter of an hour this would draw the poison from the bubo. Alternatively, 'when you see, feel and know, according to reason, that the Bubo is come to perfect suppuration, it must be opened with an incision knife, or an actual or potential cautery.' A 'potential cautery' is a corrosive of some kind which produces the same burning effect on the skin as a real cautery, such as a red-hot iron. But sufferers would also take desperate measures themselves in their agony:

'There are many that for fear of death have with their own hands pulled away the Bubo with a pair of Smith's pincers; others have digged the flesh round about it, and so gotten it fully out. And to conclude, others have become so mad, that they have thrust a hot iron into it with their own hand, that the venom might have a passage forth'.

"If a bubo was so painful that the sufferer wanted to tear it out, yet worse was what Paré called 'a pestilent carbuncle':

'A Pestilent Carbuncle is a small tumour, or rather a malign pustule, hot and raging, consisting of blood vitiated by the corruption of the proper substance … In the beginning it is scarce so big as a seed or a grain of Millet or a Pease … but shortly after it increaseth like unto a Bubo unto a round and sharp head, with great heat, pricking pain, as it if were with needles, burning and intolerable, especially a little before night, and while the meat is in concocting, more than when it is perfectly concocted. In the midst thereof appeareth a bladder puffed up and filled with sanious (bloody) matter. If you cut this bladder you shall find the flesh under it parched, burned and black, as if there had been a burning coal laid there, whereby it seemeth that it took the name of Carbuncle; but the flesh that is about the place is like a Rainbow, of divers colours, as red, dark green, purple, livid, and black; but yet always with a shining blackness, like unto stone pitch, or like unto the true precious stone which they call a Carbuncle, whereof some also say it took the name. Some call it a Nail, because it inferreth like pain as a nail driven into the flesh … a Bubo and Carbuncle are tumours of a near affinity, so that the one doth scarce come without the other'.

"In his attempt to provide the best advice for the treatment of plague, Paré had consulted widely amongst his fellow practitioners during the plague of 1565, asking all those that he came across as he travelled with Charles IX's court to Bayon, what their experience had taught them about the value of bleeding and purging in treatment for plague. They all agreed that those affected with the plague who were bled or purged all grew progressively weaker and died. So from this communal experience of medical men, Paré urged that bleeding and purging be discontinued in the plague" (Cunningham & Grell, pp. 280-284).

"Paré was born at Laval near Mayenne. His education was meagre and he never learned Latin or Greek. A rustic barber surgeon's apprentice when he came up from the provinces to Paris and afterwards a dresser at the Hôtel Dieu, the public hospital in Paris, he in 1537 became an army surgeon. France was at this time engaged in many wars: against Italy, Germany and England, and eventually at home, in the civil war so disastrous to the Huguenots. Paré joined the Forces and for the next thirty years, with a foothold in Paris in the intervals of fighting, he engaged in any campaign where he soon made himself the greatest surgeon of his time by his courage, ability, and common sense. Like Vesalius and Paracelsus he did not hesitate to thrust aside ignorance or superstition if it stood in his way. Although snubbed by the physicians and the Medical Faculty at the University and ridiculed as an upstart because he wrote in his native tongue instead of in Latin, his reputation gradually grew and he became surgeon successively to Henry II, Francis II, Charles IX and Henry Ill. It is said that Charles IX protected Paré during the Massacre of St. Bartholomew by hiding him in his bedchamber.

"Paré is responsible for the abolition of the method of applying hot iron or boiling oil in the treatment of gunshot wounds, the new feature of Renaissance surgery. During a battle in which the supply of oil gave out, Paré was forced to treat many with a mixture of egg-yolk, oil of roses, and turpentine. He was surprised to find the next morning that those treated with his mixture was in much better condition than the others, and he at once championed the new method. Control of hemorrhage by ligation of arteries had been frequently recommended but it was Paré who first practiced it systematically and brought it into general use. He invented many new surgical instruments, devised new methods in dentistry for extracting teeth, filling cavities, and making artificial dentures. He describes an artificial hand from iron, and also artificial noses and eyes of gold and silver" (Hagelin, pp. 34-35).

Paré's work is here bound after the first edition in French (first, in Italian, 1584), of a rare treatise in dialogue form by Silvestro Facio on the epidemic of plague in Milan: *Paradoxes de la peste, ou il est monstré clairement comme on peut viure & demeurer dans les villes invectées, sans crainte de la contagion. Traduicts en François par B. Barralis* (Paris: F. Bourriquant, 1620). 8vo, pp. [viii], 252, [2]. Krivatsy 3870.

One of the most interesting texts on the question of contagion was written by Silvestro Facio after the Milan epidemic of 1576 … *Paradoxes of the Plague, in which is clearly shown how one can live and stay in infected cities without fear of contagion* adopts the device already used by Boccaccio's *Decameron*: during seven days of conversations, Facio and his interlocutors debate not just whether the plague is contagious, but whether belief in contagion may not itself have deadly consequences. 'All the plagues of which we have learned through historians have been caused by the price of food and beverages, earthquakes, a large quantity of unburied dead bodies or cadavers, ponds and swamps, or else by infected air resulting from Celestial figures, and southerly winds.' Measures of isolation that governments may take are thus useless, causing unnecessary disruptions, ruining commerce, and impeding vital communications. Above all, Facio argues, one should resist the view that the plague is contagious. 'To believe that one contracts the plague by touching the hand of the cloak of a plague victim is more dangerous for the alteration of the mind than any disease'" (Huet, p. 31).

Cunningham & Grell, *The four horsemen of the Apocalypse*: *religion, war, famine and death in reformation Europe*, 2008; Doe, *A bibliography of the works of Ambroise Paré*, 14; Durling 3526; Hagelin, *Rare and important medical books in the Karolinska Institute*, 1989; Huet, *The culture of disaster*, 2012; Packard, *The life and times of Ambroise Paré* (*1510-1590*), 1926; Tchemerzine V, 36; Thornton, *Medical books, libraries and collectors*, 1949; Waller 7162. Not in Adams, BM STC, Osler, Honeyman or Norman.

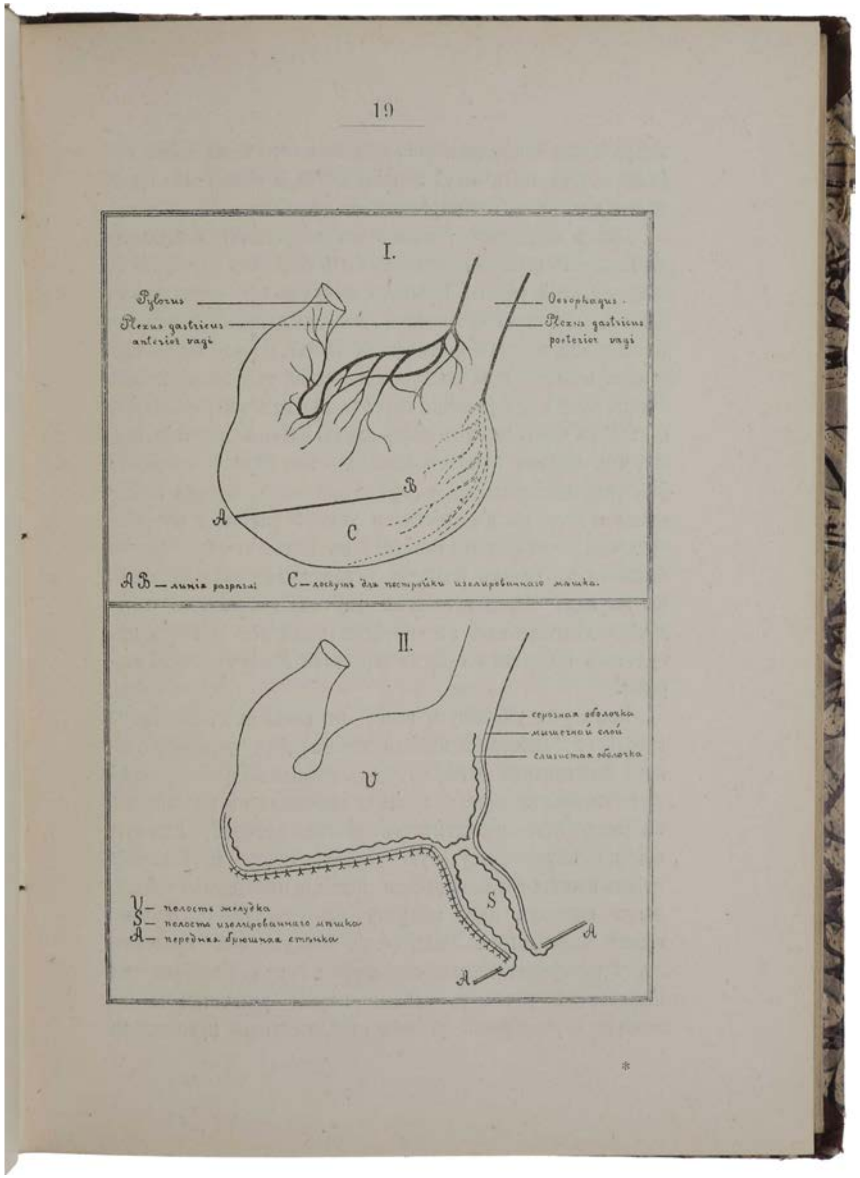
PMM 385 - CONDITIONED REFLEXES

PAVLOV, Ivan Petrovitch. *Lektsii o rabotie glavnikh pishtshevaritelnikh zhelyos.* St. Petersburg: I. N. Kushnereff & Ko., 1897.

$20,000

8vo (184 x 130 mm), pp. [vi], ii, 223, [1], contemporary Russian brown half calf with gilt spine lettering in cyrillic, initials B.C. of previous owner gilt at bottom of spine. Signature of, dated 1902, to front fly leaf, old Russian booksellers A very fine copy, completely unrestored copy in it's original state.

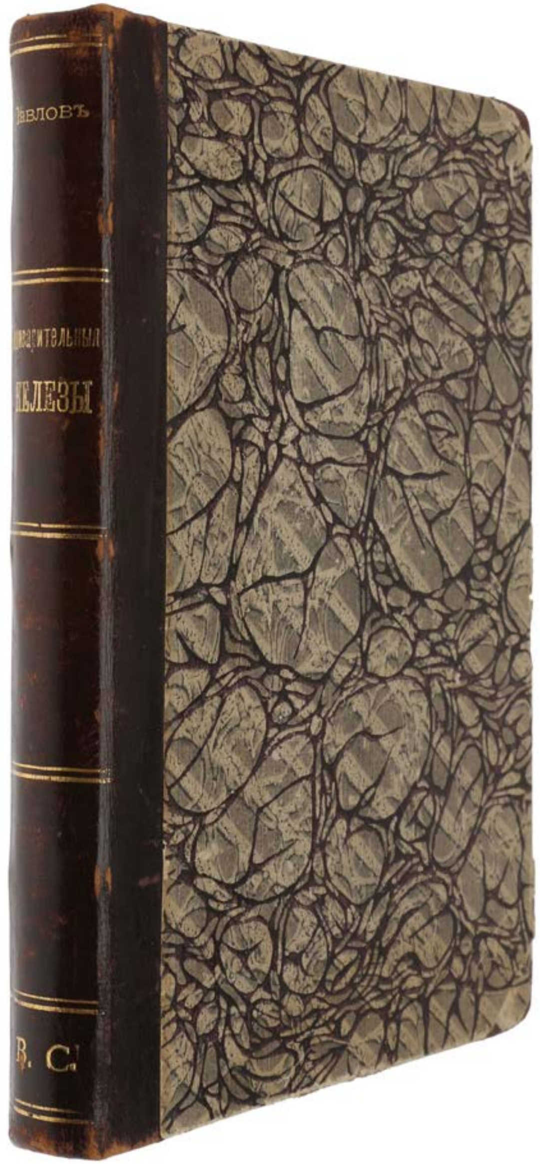
First edition of this seminal work on biology and neurology, containing the first expression of what Pavlov would later term the 'conditioned reflex'. "Mouth-watering is a familiar experience and may be induced without the sight or smell of food. The sounds of a table being laid for lunch in another room may induce salivation in man, and the rattle of a dish in which its food is usually served will cause similar reaction in a dog. By detailed analysis of such facts as these Pavlov (1849-1936) made great contributions to our knowledge of the physiology of digestion in a series of lectures delivered in St Petersburg and published in the following year [i.e., the offered work]. In the course of these lectures he described the artificial stomach for dogs used by him to produce for the first time gastric juices uncontaminated by food. Further experiments led him to the conclusion that salivation and the flow of gastric juice ensuing upon the sight or smell of food was due to a reflex process. This simple form of reaction he called first a 'psychic', later an 'unconditioned', reflex. Reflex action was familiar to physiologists, but it had never been invoked to explain such a complicated process. Pavlov now set himself to discover the far more complicated process involved in the evocation of gastric responses to stimuli other than food, for example the rattle of a familiar



platter. This was in the nature of an acquired stimulus and as reflex action was induced by a particular condition or set of conditions he called it a 'conditioned' reflex. From a series of experiments increasingly detailed, and a tabulation of results increasingly exact, he found that virtually any natural phenomenon may be developed into a conditioned stimulus to produce the selected response — 'The Activity of the Digestive Glands'. All that was necessary was to submit the animal to the selected stimulus at feeding time and the stimulus would eventually cause salivation in the absence of food. The elaboration of these experiments and their extension to children demonstrated how great a proportion of human behaviour is explicable as a series of conditioned reflexes. Indeed some psychologists seem nowadays to believe that behaviour is all. Pavlov's results are, indeed, clearly complementary to those of Freud and many regard them as of more fundamental significance. Like Freud's, this was the work of one man and a completely new departure" (PMM). The Nobel Prize in Physiology or Medicine 1904 was awarded to Ivan Petrovich Pavlov "in recognition of his work on the physiology of digestion, through which knowledge on vital aspects of the subject has been transformed and enlarged."

"Ivan Petrovich Pavlov was born on September 14, 1849 at Ryazan, where his father, Peter Dmitrievich Pavlov, was a village priest. He was educated first at the church school in Ryazan and then at the theological seminary there. Inspired by the progressive ideas which D. I. Pisarev, the most eminent of the Russian literary critics of the 1860's and I. M. Sechenov, the father of Russian physiology, were spreading, Pavlov abandoned his religious career and decided to devote his life to science. In 1870 he enrolled in the physics and mathematics faculty to take the course in natural science.

"Pavlov became passionately absorbed with physiology, which in fact was to remain of such fundamental importance to him throughout his life. It was



during this first course that he produced, in collaboration with another student, Afanasyev, his first learned treatise, a work on the physiology of the pancreatic nerves. This work was widely acclaimed and he was awarded a gold medal for it.

"In 1875 Pavlov completed his course with an outstanding record and received the degree of Candidate of Natural Sciences. However, impelled by his overwhelming interest in physiology, he decided to continue his studies and proceeded to the Academy of Medical Surgery to take the third course there. He completed this in 1879 and was again awarded a gold medal. After a competitive examination, Pavlov won a fellowship at the Academy, and this together with his position as Director of the Physiological Laboratory at the clinic of the famous Russian clinician, S. P. Botkin, enabled him to continue his research work. In 1883 he presented his doctor's thesis on the subject of «The centrifugal nerves of the heart». In this work he developed his idea of nervism, using as example the intensifying nerve of the heart which he had discovered, and furthermore laid down the basic principles on the trophic function of the nervous system. In this as well as in other works, resulting mainly from his research in the laboratory at the Botkin clinic, Pavlov showed that there existed a basic pattern in the reflex regulation of the activity of the circulatory organs.

"In 1890 Pavlov was invited to organize and direct the Department of Physiology at the Institute of Experimental Medicine. Under his direction, which continued over a period of 45 years to the end of his life, this Institute became one of the most important centres of physiological research. In 1890 Pavlov was appointed Professor of Pharmacology at the Military Medical Academy and five years later he was appointed to the then vacant Chair of Physiology, which he held till 1925.

"It was at the Institute of Experimental Medicine in the years 1891-1900 that Pavlov did the bulk of his research on the physiology of digestion. It was here that

he developed the surgical method of the «chronic» experiment with extensive use of fistulas, which enabled the functions of various organs to be observed continuously under relatively normal conditions. This discovery opened a new era in the development of physiology, for until then the principal method used had been that of «acute» vivisection, and the function of an organism had only been arrived at by a process of analysis. This meant that research into the functioning of any organ necessitated disruption of the normal interrelation between the organ and its environment. Such a method was inadequate as a means of determining how the functions of an organ were regulated or of discovering the laws governing the organism as a whole under normal conditions – problems which had hampered the development of all medical science. With his method of research, Pavlov opened the way for new advances in theoretical and practical medicine. With extreme clarity he showed that the nervous system played the dominant part in regulating the digestive process, and this discovery is in fact the basis of modern physiology of digestion. Pavlov made known the results of his research in this field, which is of great importance in practical medicine, in lectures which he delivered in 1895 and published under the title *Lektsii o rabote glavnykh pishchevaritelnyteh zhelez* (Lectures on the function of the principal digestive glands) (1897).

"Pavlov's research into the physiology of digestion led him logically to create a science of conditioned reflexes. In his study of the reflex regulation of the activity of the digestive glands, Pavlov paid special attention to the phenomenon of «psychic secretion», which is caused by food stimuli at a distance from the animal. By employing the method – developed by his colleague D. D. Glinskii in 1895 – of establishing fistulas in the ducts of the salivary glands, Pavlov was able to carry out experiments on the nature of these glands. A series of these experiments caused Pavlov to reject the subjective interpretation of «psychic» salivary secretion and, on the basis of Sechenov's hypothesis that psychic activity was of a reflex nature, to conclude that even here a reflex – though not a permanent but a temporary or conditioned one – was involved. This discovery of the function of conditioned reflexes made it possible to study all psychic activity objectively, instead of resorting to subjective methods as had hitherto been necessary; it was now possible to investigate by experimental means the most complex interrelations between an organism and its external environment …

"Subsequently, in a systematic programme of research, Pavlov transformed Sechenov's theoretical attempt to discover the reflex mechanisms of psychic activity into an experimentally proven theory of conditioned reflexes" (nobelprize.org).

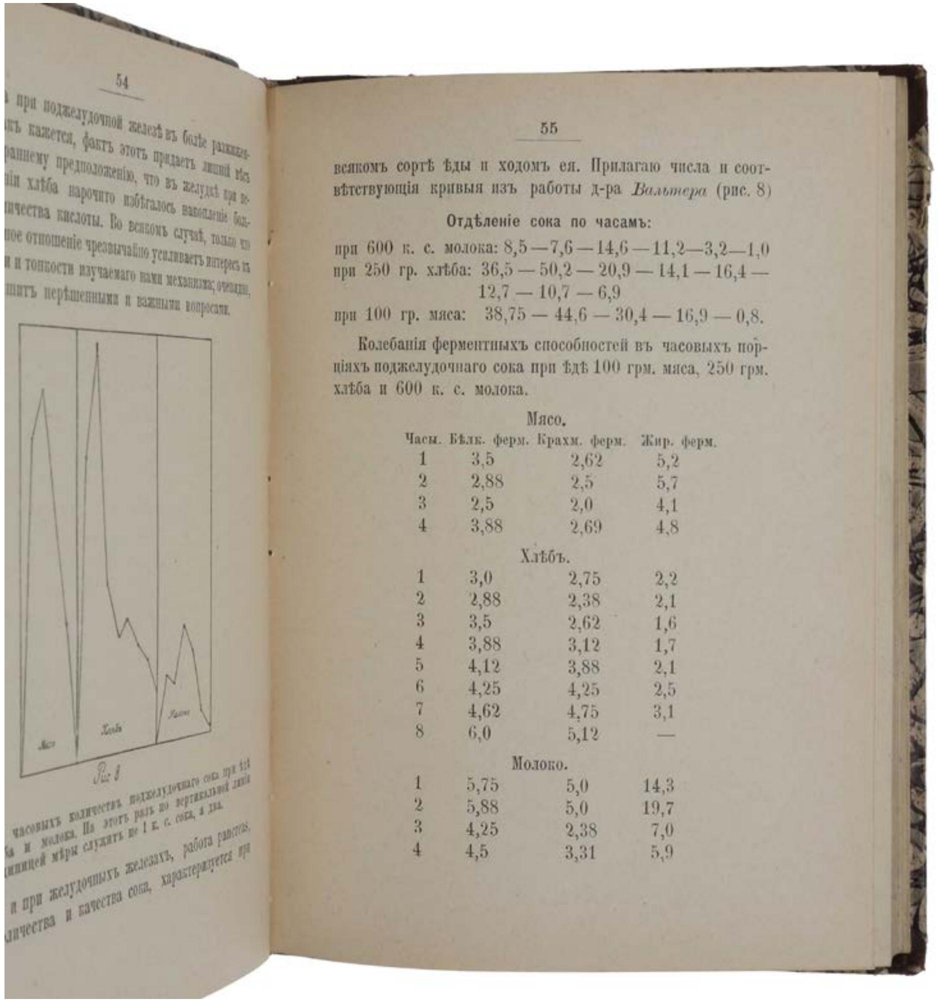
"By observing irregularities of secretions in normal unanesthetized animals, Pavlov was led to formulate the laws of the conditioned reflex, a subject that occupied his attention from about 1898 until 1930. He used the salivary secretion as a quantitative measure of the psychical, or subjective, activity of the animal, in order to emphasize the advantage of objective, physiological measures of mental phenomena and higher nervous activity. He sought analogies between the conditional (commonly though incorrectly translated as "conditioned") reflex and the spinal reflex.

"According to the physiologistSir Charles Sherrington, the spinal reflex is composed of integrated actions of the nervous system involving such complex components as the excitation and inhibition of many nerves, induction (i.e., the increase or decrease of inhibition brought on by previous excitation), and the irradiation of nerve impulses to many nerve centres. To these components, Pavlov added cortical and subcortical influences, the mosaic action of the brain, the effect of sleep on the spread of inhibition, and the origin of neurotic disturbances principally through a collision, or conflict, between cortical excitation and inhibition.

"Beginning about 1930, Pavlov tried to apply his laws to the explanation of human psychoses. He assumed that the excessive inhibition characteristic of a psychotic person was a protective mechanism—shutting out the external world—in that it excluded injurious stimuli that had previously caused extreme excitation. In Russia this idea became the basis for treating psychiatric patients in quiet and non-stimulating external surroundings. During this period Pavlov announced the important principle of the language function in the human as based on long chains of conditioned reflexes involving words. The function of language involves not only words, he held, but an elaboration of generalizations not possible in animals lower than the human" (Britannica).

Pavlov's discovery of the conditioned reflex has gained growing significance in politics and sociology. He concluded that even such concepts as freedom, curiosity and religion were conditioned reflexes of the brain. '"Essentially, only one thing in life is of real interest to us — our psychical experience,"' he said in his Nobel address. '"Its mechanism, however, was and still is shrouded in profound obscurity. All human resources — art, religion, literature, philosophy, and the historical sciences — all have joined in the attempt to throw light upon this darkness. But humanity has at its disposal yet another powerful resource — natural science with its strict objective methods."

PMM 385; Garrison-Morton 1022; Grolier/Horblit 83; Dibner 135; Grolier/Medicine 85; Lilly Library *Notable Medical Books* 241.



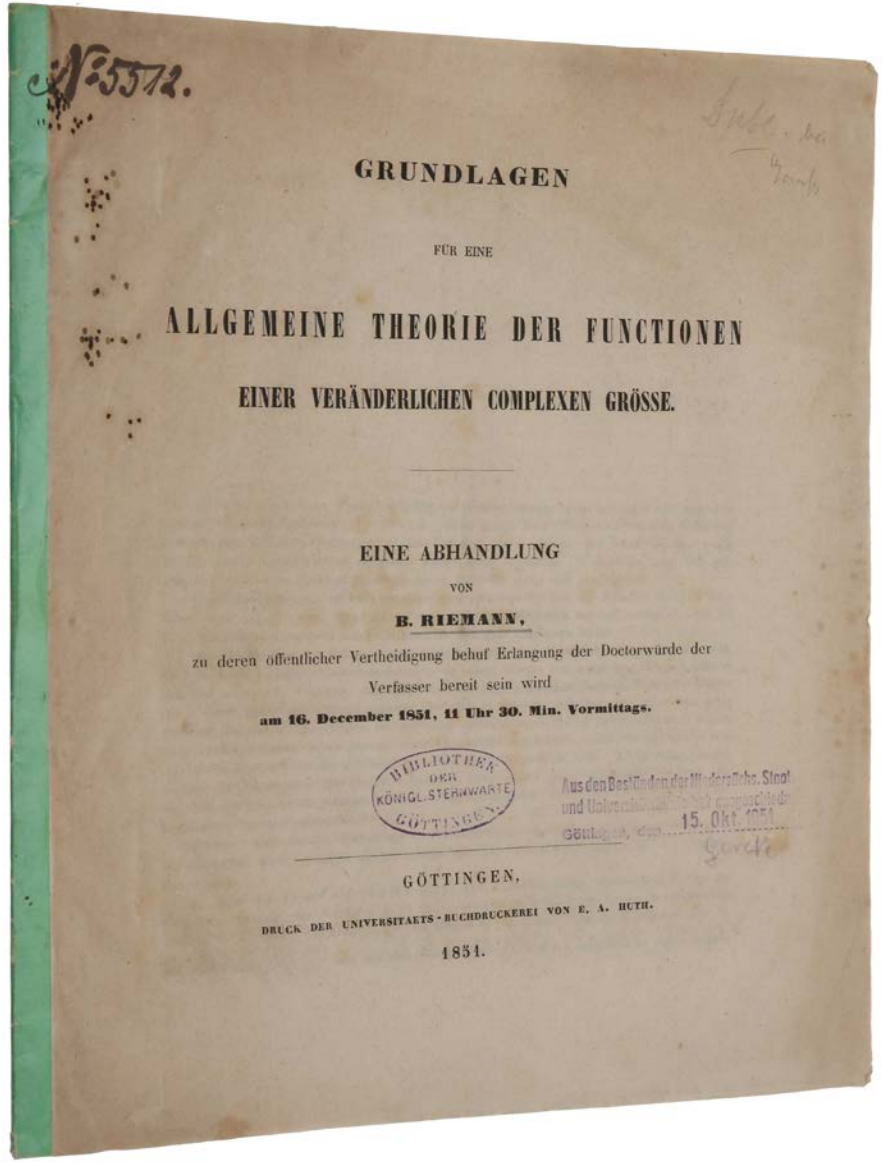
ONE OF THE MOST IMPORTANT ACHIEVEMENTS OF 19TH CENTURY MATHEMATICS

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.

$28,500

4to (255 x 211 mm), pp [ii] 32. Stamps on title of the Göttingen Royal Observatory (of which Gauss was director from 1807 to 1855), and of Göttingen State and University Library (deaccessioned by librarian). The leaves contemporarily bound with green paper strip spine. Pencil-underlining to author's name, and another pencil annotation to upper right corner of front wrapper. Old library numbering in ink to upper left corner.

Very rare 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). It is also of great rarity, for "although [it] 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" *Landmarks in Western Mathematics*, no. 34). The present copy is evidently one of those handed to the University.

Riemann begins his thesis 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. At this point he refers to Gauss's work on conformal mapping, published in 1825 in Schumacher's *Astronomische Abhandlungen*, which he had studied in Berlin (this is the only reference in the thesis to the work of others).

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. "Riemann's thesis studied the theory of complex variables and, in particular, what we now call Riemann surfaces. It therefore introduced topological methods into complex function theory... Riemann's thesis is a strikingly original piece of work which examined geometric properties of analytic functions, conformal mappings and the connectivity of surfaces" (Mactutor).

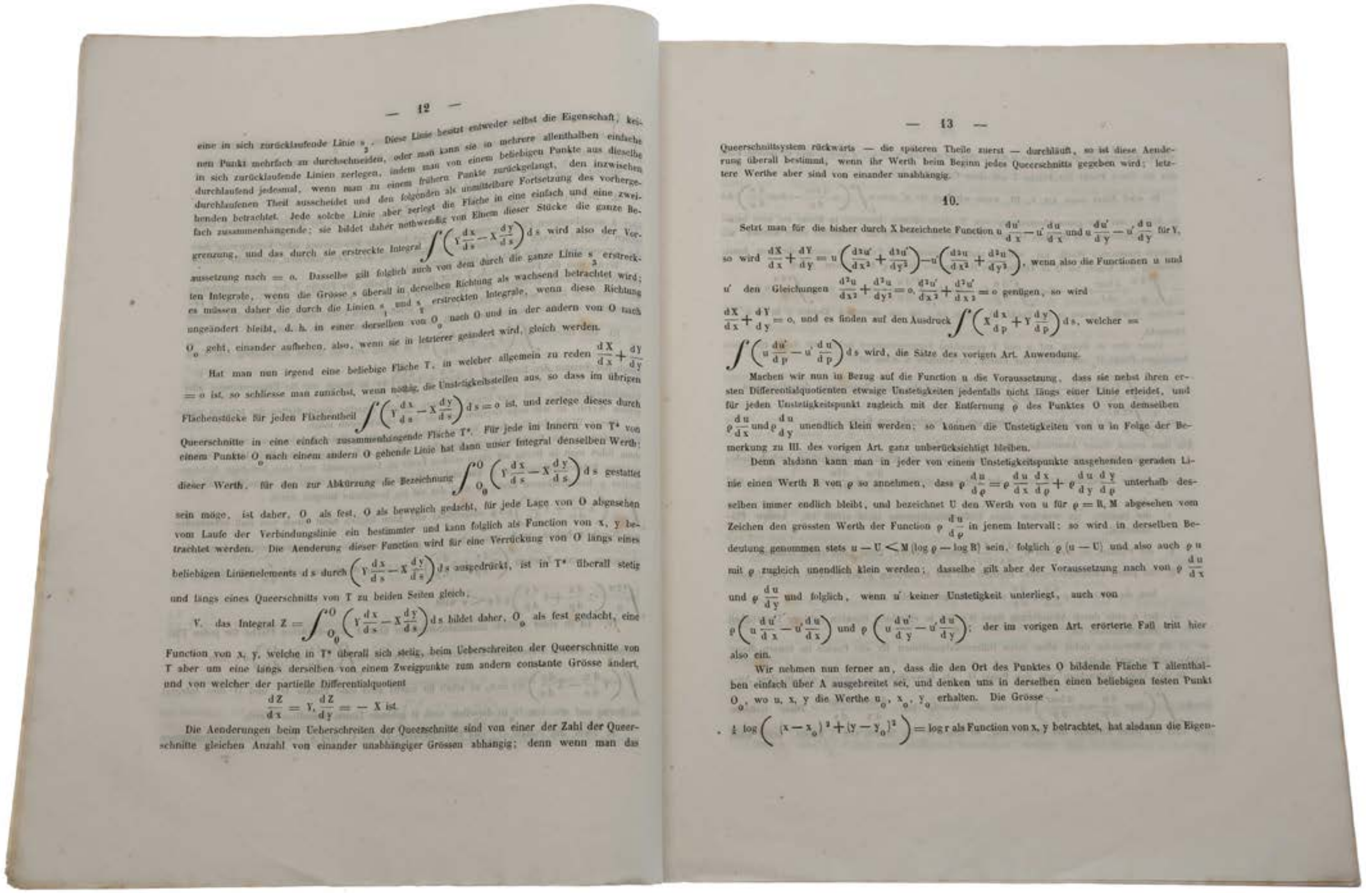
The rest of the thesis is devoted to the study of functions on Riemann surfaces. From the Cauchy-Riemann equations it follows that if $w = u + iv$ is an analytic function, then u and v are harmonic functions, i.e. solutions of Laplace's equation. This establishes a link between the theory of analytic functions and potential theory, a subject with which Riemann was familiar, having attended Gauss and Weber's Göttingen seminar on mathematical physics (Gauss himself had made a decisive contribution to potential theory in 1849). Riemann's approach in the remainder of the thesis was deeply influenced by potential theory.

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 thesis, 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'." (*Landmarks*, p. 454).

According to Richard Dedekind (*Bernhard Riemann's Lebenslauf*, p. 7), Riemann probably conceived the main ideas of the thesis 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 position at Göttingen.

I. Grattan-Guiness, *Landmarks in Western Mathematics*, Chapter 34; Poggendorff II, 641; DSB XI 449-450; J. Derbyshire, *Prime Obsession*, 2003; A. N. Kolmogorov & A. P. Yushkevich (eds.), *Mathematics in the 19th century*, Vol. II, 1996; D. Laugwitz, *Bernhard Riemann, 1826-1866*, 1998.



ONE OF THE MOST LAVISHLY ILLUSTRATED ASTRONOMICAL WORKS

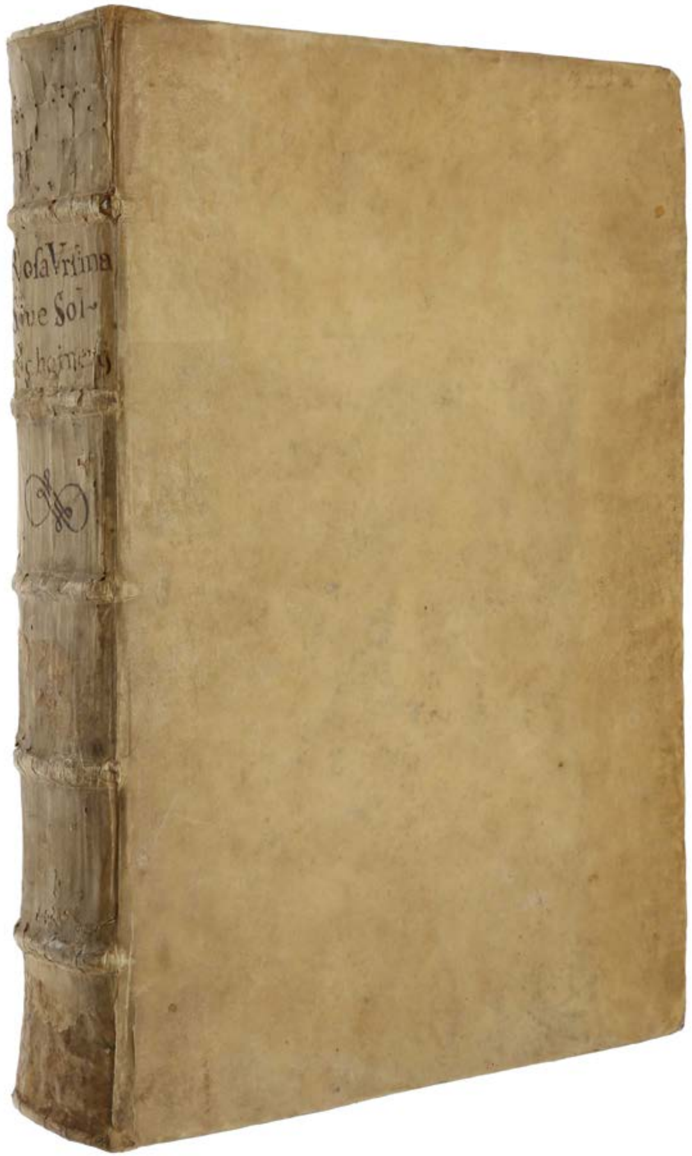
SCHEINER, Christoph. *Rosa ursina sive Sol ex admirando facularum & macularum suarum phoenomeno varius: necnon circa centrum suum et axem fixum ab occasu in ortum annua, circaq[ue] alium axem mobilem ab ortu in occasum conuersione quasi menstrua, super polos proprios, libris quatuor mobilis ostensus* ... Bracciano: Andreas Phaeus, 1626-30.

$95,000

Folio (352 x 248 mm), pp. [xl, including frontispiece], 1-66, [2, blank], [67]-125, [126]; ff. 126-149, [12, including blank R6]; pp. [2, unpaginated opening leaf of Liber tertius], 149-784, [2, blank], [36, index and errata], with engraved frontispiece, engraved plate on title, engraved portrait of Orsini, and 172 engraved plates folded in. Contemporary vellum with manuscript title on spine. Moderate browning and spotting to some leaves - much less than is usually seen in this work.

First edition of the most lavishly illustrated astronomical work published in the first half of the seventeenth century, with many full-page illustrations of Scheiner's observations of the sun and of the optical instruments he had designed for the purpose. "For his masterpiece, Scheiner produced the first monograph on a heavenly body, the Sun. Even today it is still an impressive volume, with scores of engravings of sunspots and the various instruments needed for solar observations" (*Jesuit Science in the Age of Galileo*). "Scheiner's drawings in the *Rosa Ursina* are of almost modern quality, and there was little improvement in solar imaging until 1905" (Britannica). In this work "Scheiner agreed with Galileo that sunspots are on the Sun's surface or in its atmosphere, that they are often generated and perish there, and that the Sun is therefore not perfect. Scheiner further advocated a fluid heavens (against the Aristotelian solid spheres), and he pioneered new ways of representing the motions of spots across the Sun's face" (Galileo Project). Scheiner was one of the first to observe sunspots by telescope, in March 1611, and in 1612 he published his findings anonymously. This led to a famous controversy with Galileo, who claimed to have observed sunspots earlier, involving the exchange of several letters. Galileo then turned to other matters, notably the preparation of the *Dialogo*, but Scheiner continued his observations of sunspots, culminating in the publication of the present work more than a decade later. Scheiner devised a number of new instruments in order to make his observations. Kepler had conceived the 'astronomical' telescope, consisting of two converging lenses, but he never constructed one. Scheiner was the first to do so, and he added a third convex lens which transformed the inverted image into an erect one and greatly increased the field of view and brightness of the image. Scheiner also invented the first equatorially mounted telescope. All of these instruments are described and illustrated in *Rosa Ursina*, in which "Scheiner confirmed his method and criticized Galileo for failing to mention the inclination of the axis of rotation of the sunspots to the plane of the ecliptic" (*DSB*). But when the *Dialogo* was published in 1632, Scheiner was dismayed to find that Galileo dismissed Scheiner's work and claimed there that he [Galileo] had known of the curved motion of sunspots and its explanation in terms of the inclination of the Sun's axis since 1614 (although the evidence casts serious doubt on Galileo's claims). "It has been said that his [i.e., Scheiner's] enmity toward Galileo was instrumental in starting the process against the Florentine in 1633" (Galileo Project). Although this book appears on the market from time to time, fine, complete copies in untouched contemporary bindings are rare in commerce.

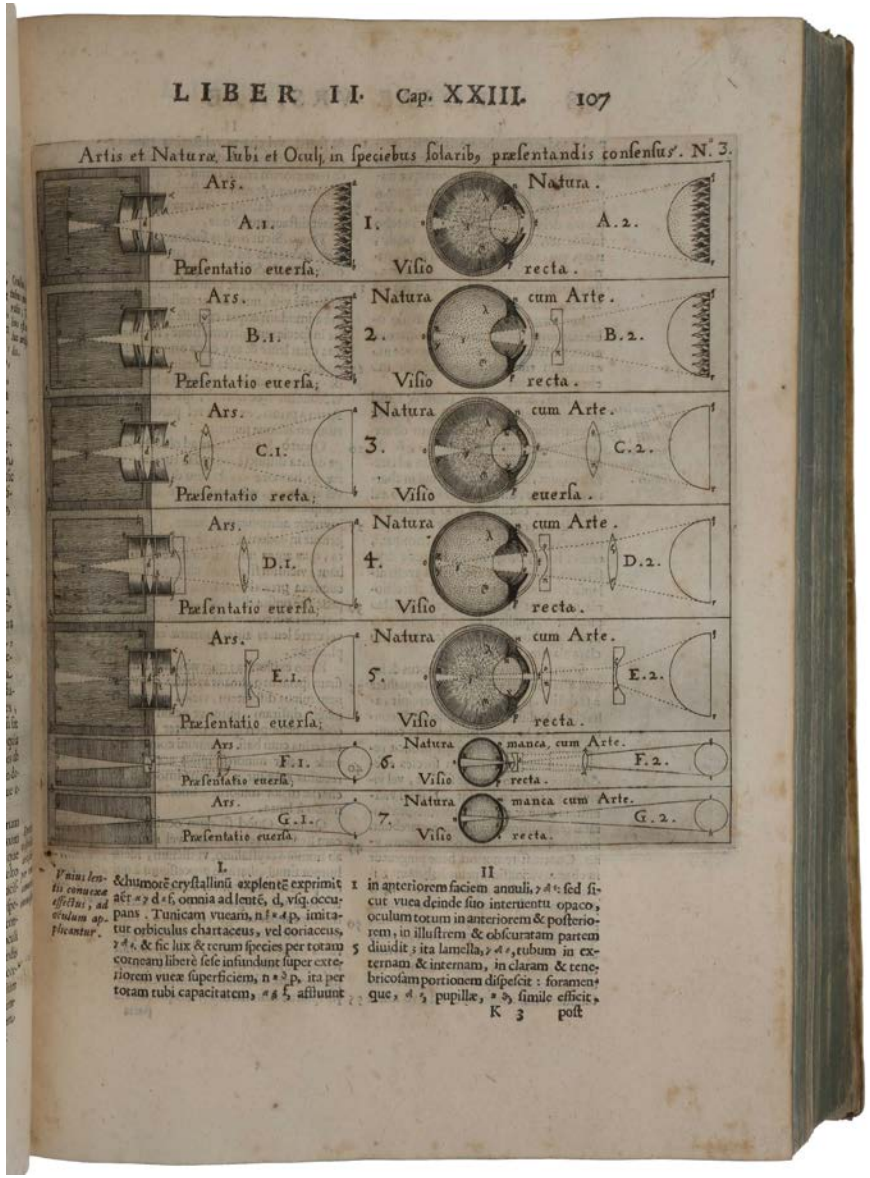
Scheiner (1573-1650) was appointed professor of Hebrew and mathematics at the Jesuit College at Ingolstadt in 1610. The following year Scheiner, together



with his student Johann Baptist Cysat (1587-1657), constructed a telescope with which to observe the satellites of Jupiter, partly to investigate the claims made by Galileo in *Sidereus nuncius* (1610). At sunrise one day in March, they decided to observe the sun and noticed dark spots on its surface, although initially they were unsure whether this might be due to flaws in the lenses or to clouds. Scheiner was preoccupied with observations of Jupiter, and also of Venus, but Cysat persuaded him to return to the solar observations using coloured glass to enable them to observe in full daylight, a technique that was used by sailors when taking the altitude of the Sun. This was on 21 October, as Scheiner tells us in *Rosa Ursina* (Ad Lectorum, p. [2]). Others soon became aware of his observations, including the well-connected Augsburg humanist Marc Welser (1558-1614). Scheiner wrote three letters to Welser, dated 12 November and 19 and 26 December, which Welser published at his private press under the title *Tres epistolae de maculis solaribus* (1612). They appeared pseudonymously, as Scheiner's Jesuit superiors urged caution, and were signed *Apelles latens post tabulum*, 'Apelles hiding behind the painting' (this refers to a story told by Pliny, well known in the Renaissance, about the famed Greek painter Apelles hiding behind one of his pictures to hear the comments of spectators). Welser sent copies abroad, notably to Galileo (1564-1642). Galileo identified Scheiner as a Jesuit and took him to task in three letters addressed to Welser, to which Scheiner replied in a further series of letters published as *De maculis solaribus … accuratior disquisitio* (1612). In this work Scheiner discussed the individual motions of the spots, their period of revolution, and the appearance of brighter patches or *faculae* on the surface of the sun. Galileo's letters were published in Rome in 1613 as *Istoria e dimostrazioni intorno alle macchie solari*. His criticism of Scheiner's priority claims was misconceived, for the sunspots were observed independently not only by Galileo in Florence and Scheiner in Ingolstadt, but also by Thomas Harriot in Oxford (who was the first to observe them by telescope), Johann Fabricius in Wittenberg (who was the first to publish a work on sunspots), and Domenico Passignani in Rome.

Having declared victory with the publication of *Istoria e dimostrazioni*, Galileo turned to other matters, notably the controversy on the comets (in which Scheiner may have played a role behind the scenes) and the preparation of the *Dialogo*. Scheiner was admonished personally by Claudio Aquaviva (1543-1615), the Superior General of the Jesuits, to follow the doctrines of traditional philosophy, and in his publications he now concentrated on the strictly mathematical and non-controversial subject of optics. Nevertheless, "it was in this period that Scheiner laid the foundations of his greatest work, *Rosa Ursina*. He had constructed a 'helioscope' for observing the Sun: the image of the Sun through the telescope was projected onto a sheet of paper placed about one metre from the eyepiece. This was a technique developed by Benedetto Castelli (1578-1643) and used by Galileo, but in his continuing study of sunspots and in demonstrating them to others, Scheiner made successive improvements. Following the sun with one's telescope in order to keep the sun's image centred on the paper was very difficult. The first problem was that the form of telescope he used projected an inverted image. In following the motion of the Sun, therefore, one has to turn the telescope in the direction contrary to the motion of the solar image … Scheiner had studied Kepler's *Dioptrice* (1611), and he knew that there was more than one combination of lenses to achieve the telescopic effect. Replacing the concave ocular with a convex one would produce an inverted direct image but an erect *projected* image, making manipulation of the telescope much easier … Since this combination of lenses presents an inverted image if one looks through it, one would expect that for terrestrial purposes it would be useless, and neutral, at best, for astronomical purposes. But when Scheiner looked through the combination, he found something unexpected:

'If you fit two like [convex] lenses in a tube … and apply your eye to it in the proper way, you will see any terrestrial object whatever in an inverted position but with an incredible magnitude, clarity and width. But also you will compel any



stars you wish you submit to your sight; for since they are all round, the inversion of the position of the total view is not confusing to the visual configuration' [fol. 130r].

"The astronomical telescope, as an instrument with a convex ocular is called, has a much larger field of view and brighter image than the Galilean form of the instrument. The replacement of the Galilean telescope by the astronomical telescope can, in fact, be dated from the publication of *Rosa Ursina* in 1630 …

"Besides using a telescope with a convex ocular for projection, Scheiner also provided the entire apparatus with a convenient mounting [p. 77]. The main axis of the mounting is made parallel to the axis of rotation of the Earth, so that an object in the sky can be followed merely by turning the telescope around this axis. This means that on the pre-drawn circle on which the image of the Sun is projected, the Sun's path (the ecliptic) is always represented by a horizontal line. Scheiner systematically taught his students and associates to draw the perpendicular to this horizontal line, in order not to make errors in the complicated motions of the spots [pp. 158-9].

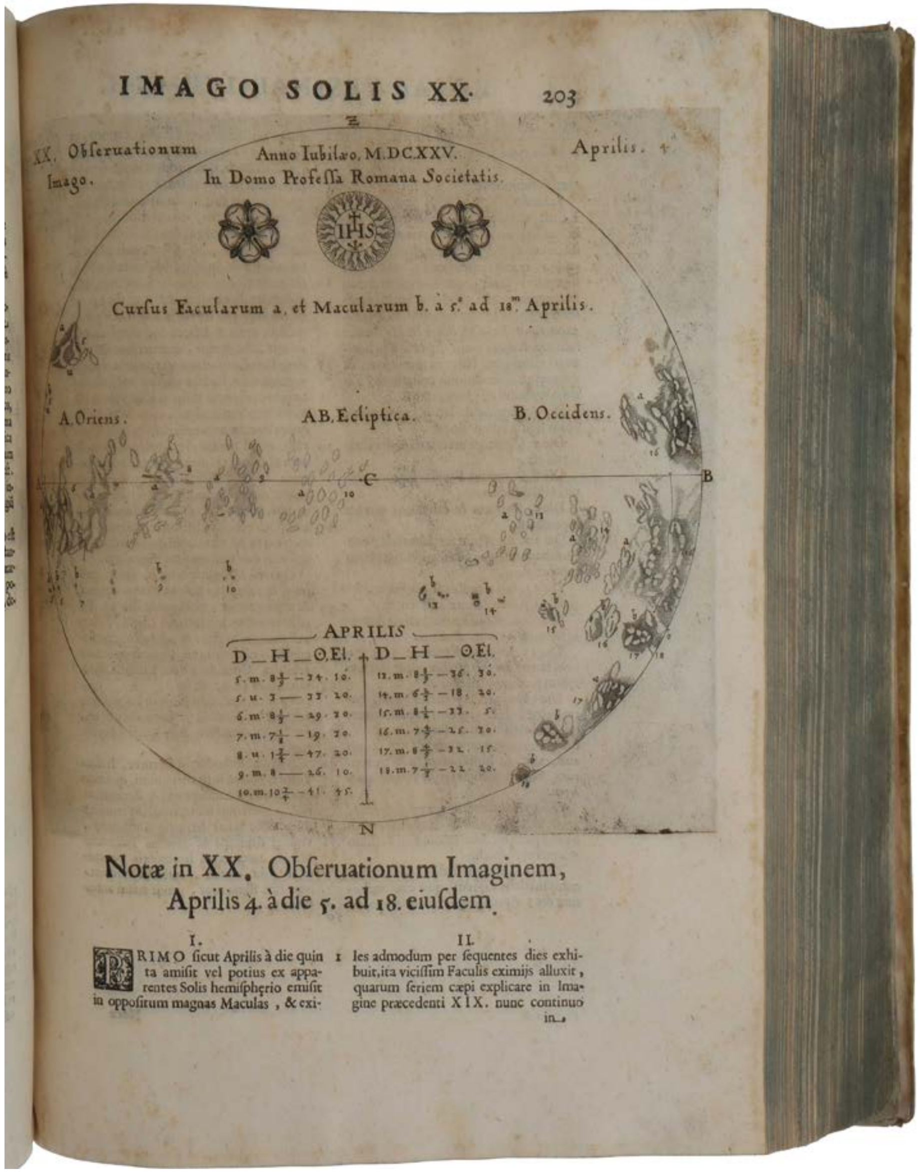
"But other duties increasingly occupied Scheiner, and it was not until he had settled in Rome in 1624 that he could return to sunspots. Obtaining the observations that he and others had made in the German region was complicated by the campaign of the Thirty Years' War, and many of those which he did manage to procure from various observers were useless because no perpendicular had been marked. His student Georg Schönberger (1596-1645) did, however, send observations with the perpendicular line, and Scheiner was able to use them to demonstrate the curved motions of the spots …

"In Rome Scheiner made a large number of excellent observations of sunspots,

using this time an equatorial mounting for his projection apparatus [p. 349], designed by his colleague Christoph Grienberger (1561-1636) and, in 1626, began the publication process. The central argument of the *Rosa Ursina* was the demonstration that Galileo had erred on the path of the spots and had concluded that the Sun's axis of rotation was perpendicular to the ecliptic in his letters on sunspots on 1612-1613. Scheiner determined that this axis is, in fact, inclined to that perpendicular by 7° 15′. Scheiner was especially eager to keep that information from Galileo before unveiling it in *Rosa Ursina* … The printing of *Rosa Ursina* began in 1626 and was finished in 1630. It was a magisterial work that was to remain the definitive study of sunspots for over a century …

"Galileo and his associates were certainly aware of Scheiner's presence in Rome in these years, and they commented occasionally on both his forthcoming work on the sunspots and on his relationship with the powerful Archduke Leopold and with Cardinal Francesco Barberini, nephew to Pope Urban VIII … In early 1626 Francesco Stelluti related that Scheiner was printing his sunspot observations, and that he had asked if it was true that Galileo was engaged in publishing a treatise called 'On the tides.' Scheiner appeared tolerably well informed, for this was in fact the subject of the eventual Fourth Day of the *Dialogue*, the original title of the work, and a question that Galileo had been investigating for its evidence of a Copernican world system about a year earlier. The Jesuit astronomer evidently added that he was eager to see such a work, and that he concurred with Galileo's opinion about the world system.

"It is certain that the exchanges in 1625-1626 between Scheiner and Galileo's friends in Rome were guarded and less than candid, as if both sides correctly sensed that the much anticipated works of the two rivals would involve open conflict. Over the next few years Galileo's friends urged him repeatedly to finish his *Dialogue*, and in early 1629 Castelli, writing from Rome, told him 'soon we

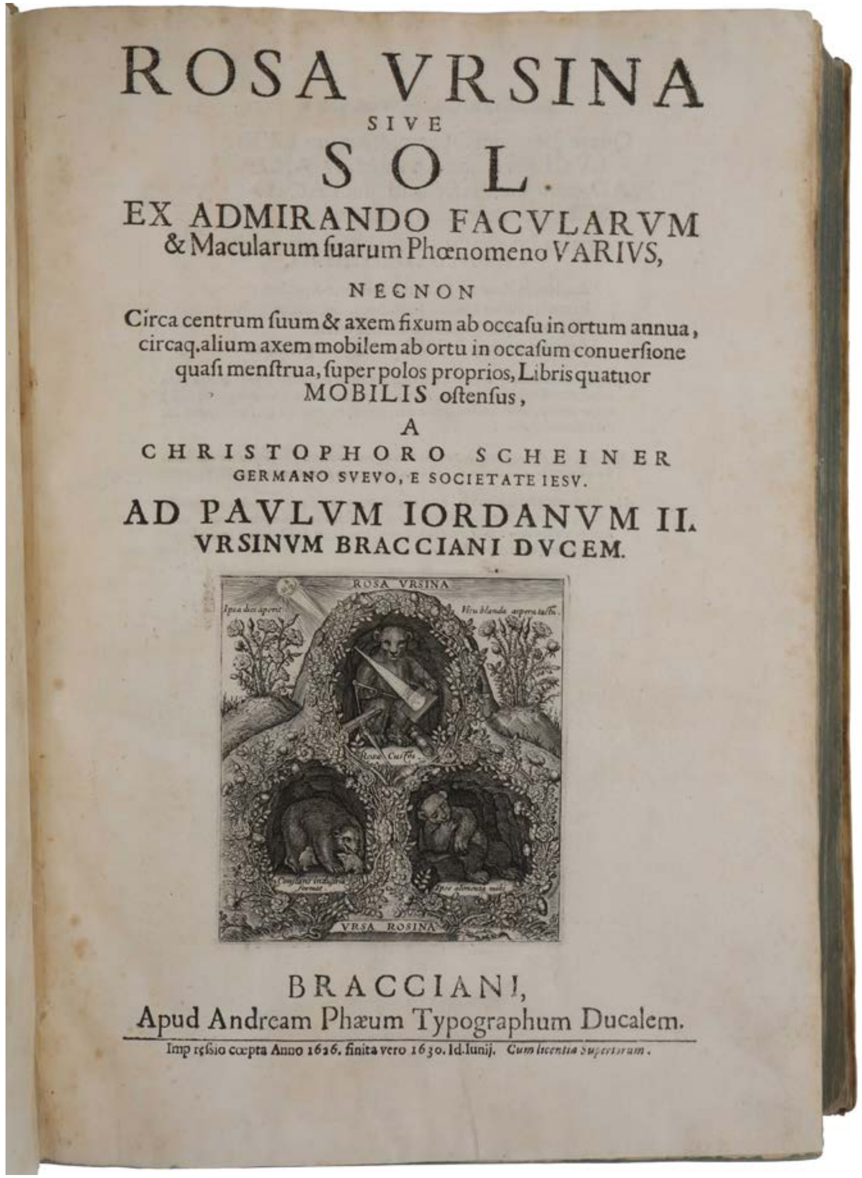


will have a big new book on sunspots from the masked Apelles. We shall see.' A month later Castelli promised to send a copy to Florence as soon as it became available, and as if to inspire Galileo to devote himself particularly to the issue of solar phenomena. He also reported on the timely return of a vast sunspot that had passed from view fifteen days earlier. Galileo, for his part, insisted upon his low expectations of his rival's work, telling another friend that spring that he was certain that wherever the *Rosa Ursina* diverged from what had earlier been established in the *History and Demonstrations*, Scheiner would simply be offering 'nonsense and lies'.

"The enormous work emerged a year later, in the spring of 1630; Juan de Alvarado S.J. of the Collegio Romano noted on 28 May 1630 that the *Rosa Ursina* had been licensed by Father Niccolo Riccardi, master of the Holy Palace. Galileo was by then in Rome seeking permission for his recently completed *Dialogue* … The imprimatur was granted in mid-September 1630.

"Though Galileo heard in mid-April 1631 that Scheiner referred to his letters on sunspots with great frequency and hostility in the *Rosa Ursina*, he claimed not to have seen the treatise until the fall or winter of that year, when he expressed his displeasure to Paolo Giordano [II] Orsini (1591-1656), duke of Bracciano, who now regretted, both for fiscal and for personal reasons, having agreed to finance the expensive publication …

"Scheiner had very accurately determined the solar axis of rotation. Accounting for this phenomenon by means of a geocentric construction was a straightforward astronomical exercise of the kind technical astronomers had done at least since Ptolemy. To the standard solar description of the Sun's diurnal and annual motions, Scheiner added a construction to make the Sun rotate in about a month

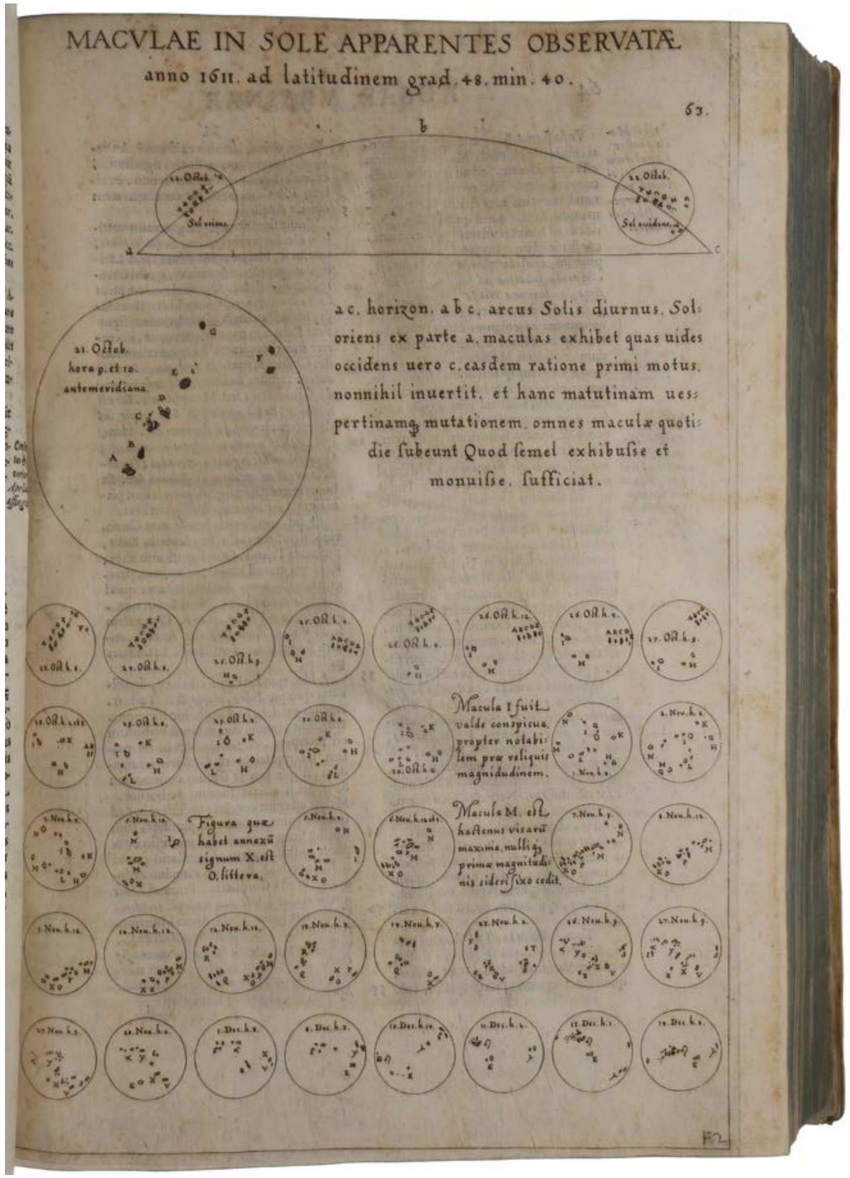


ROSA VRSINA
SIVE
SOL.
EX ADMIRANDO FACVLARVM
& Macularum suarum Phœnomeno VARIVS,
NECNON
Circa centrum suum & axem fixum ab occasu in ortum annua,
circaq. alium axem mobilem ab ortu in occasum conuersione
quasi menstrua, super polos proprios, Libris quatuor
MOBILIS ostensus,
A
CHRISTOPHORO SCHEINER
GERMANO SVEVO, E SOCIETATE IESV.
AD PAVLVM IORDANVM II.
VRSINVM BRACCIANI DVCEM.

BRACCIANI,
Apud Andream Phæum Typographum Ducalem.
Impressio cœpta Anno 1626. finita vero 1630. Id. Iunij. Cum licentia Superiorum.

on an axis that pointed to a place in the fixed stars 7° 15′ removed from the pole of the ecliptic. To keep this axis always pointing to the same spot (i.e., to keep it parallel to itself), Scheiner added a conical construction [shown on p. 565]. All of Scheiner's aims had been achieved: he had shown Galileo to be wrong about the motion of sunspots, had demonstrated the correct movement, and had supplied a mathematical model to account for that motion" (*On Sunspots*, pp. 311-323).

Scheiner's pleasure at the publication of his greatest work was to be short lived. If it seemed to some that Galileo was the target of the *Rosa Ursina*, Scheiner felt that he was likewise the victim of the *Dialogo* when it emerged in the spring of 1632. In the First Day, a discussion of sunspots formed part of Galileo's arguments against the perfection of the heavens. Salviati asked, 'But you, Simplicio, what have you thought of to reply to the objections based on these annoying spots which have come to mess up the heavens and even more so the Peripatetic philosophy?' Simplicio answered with Scheiner's original argument, that sunspots were dark bodies orbiting the Sun, and continued sarcastically, 'this seems to me to be the most convenient escape found so far to account for such a phenomenon and at the same time retain the indestructibility and ingenerability of the heavens; and, if this were not sufficient, there will be no lack of loftier intellects who will find better explanations.' But worse was to come. In the Third Day, Galileo recounted his discovery of sunspots, his initial supposition that the Sun turned on an axis of rotation perpendicular to the ecliptic, and his eventual, but still timely, conclusion to the contrary. Salviati recalled, in the only passages in the *Dialogo* that purport to contain direct quotations from his friend Galileo, that having observed a large and solitary sunspot, they noted that its passage was not exactly in a straight line, and that Galileo had then put forward the explanation that the axis around which the Sun revolves is not perpendicular to the plane of the ecliptic, but somewhat inclined to it.



There are several reasons to doubt this account. Salviati died in 1614, so the observations he describes would have to have been made before that time, and there is no evidence in Galileo's papers to support his claim. Moreover, upon receiving his copy of the *Dialogo*, Castelli wrote to Galileo: 'When I got to that false attestation of the sunspots, I was beside myself with happiness in seeing how much light these dark marks shed on the matter.' But if Galileo had made this discovery in 1613 or earlier, when Castelli was working very closely with him and had developed their method of projecting sunspots, he would surely have known about it and would not have expressed himself in this way on receiving Galileo's work. Although it is not certain, it certainly seems probable that Galileo's knowledge of the annual paths of sunspots derives from the *Rosa Ursina*, and that his focus on this issue on Day Three of the *Dialogo* reflects changes made to his text after the imprimatur had been granted (see *On Sunspots*, pp. 325-7).

Rosa Ursina contains four books. In Book I, Scheiner discusses the question of priority in regard to the discovery sunspots. Book II not only describes telescopes, different kinds of projection and the helioscope, but also compares the optics of the telescope to the physiological optics of the eye. In Book III, Scheiner presents a comprehensive collection of the data from his observation of the sunspots. Book IV consists of two parts: the first part deals once again with solar phenomena like sunspots and faculae, the Sun's rotation period of 27 days and the inclination of its axis of rotation; in the second part, Scheiner mentions numerous passages and quotations from the Bible, the writings of the Church Fathers and philosophers to prove that his geocentric view is in accordance with the teachings of the Catholic Church.

The last few pages of the main text comprise the first printing of Prince Federico Cesi's important letter of 1618 to Cardinal Robert Bellarmine, 'De caeli unitate, tenuitate, fusaque & pervia stellarum motibus natura' (pp. [775]-782) with Bellarmine's reply (pp. 783-784). In his letter Cesi (1585-1630), head of the Roman *Accademia dei Lincei* and ally of Galileo, defended the concepts of a 'fluid' and 'elemental' cosmos; he may even have written this work as part of a plan to resuscitate the Copernican cause after the Condemnation of 1616. What is equally significant is that Cardinal Bellarmine (1542-1621), who was by no means sympathetic to Copernicanism, accepted Cesi's theses with equanimity and responded that these positions were most certainly true.

The quality of the illustrations in the *Rosa Ursina* is exceptional. The engraved plate on the title is a play on the caption 'Rosa Ursina / Ursa Rosina', featuring a rose-festooned bower-cave with three bears, one with a telescope which is projecting an image of the sun onto a board. "The frontispiece of this volume is an elaborate allegory on epistemology and the sources of truth. At the top, two beams of light stream out from the Godhead, and they are labelled Sacred Authority and Reason. Both derive their certainty from God. Below, two more beams emanate from the Sun, and they illuminate Profane Authority and Sense. Note that Sense is represented by a view through the telescope of the spots on the sun. Note also that the telescopic sunspots are fuzzy and imprecise. If we return to Reason at top right, we see that Reason too is represented by a view of the Sun, but this time the spots are sharp and clear. It is Reason, Scheiner seems to be saying, that allows us to make 'sense' of our senses; Sense alone is never enough to establish anything with certainty. This frontispiece beautifully captures the divide that separated Galilean science and Jesuit science" (Ashworth, lindahall.org/christoph-scheiner/). The main anti-Copernican element of Scheiner's frontispiece is its rendering of the Rose of the Orsini, *Rosa Ursina*, which formed part of the Orsini family's coat of arms. Scheiner had dedicated his work to the Orsini family. The spotted Sun, depicted by the rose of the Orsini, can be seen in the very centre of the frontispiece, moving on the zodiac, thereby refuting the idea of heliocentricity. The portrait of Orsini (looking rather ursine) is surrounded by a garland of roses interspersed with maculate suns. Another plate, which serves as a frontispiece to Book III, shows Jesuit astronomers at work with telescopes, before which is a depiction of a darkened room in which an image of the Sun is being projected from a telescope, with one astronomer taking measurements and another transferring them onto paper, certainly a representation of how the sunspot illustrations in the book itself were made. It is signed by the engraver Daniel Widman.

"Scheiner attended the Jesuit Latin school at Augsburg and the Jesuit College at Landsberg before he joined the Society of Jesus in 1595. In 1600 he was sent to Ingolstadt, where he studied philosophy and, especially, mathematics under Johann Lanz. From 1603 to 1605 he spent his "magisterium", or period of training as a teacher, at Dillingen, where he taught humanities in the Gymnasium and mathematics in the neighbouring academy. During this period he invented the pantograph, an instrument for copying plans on any scale; and his results were published several years later in the *Pantographice, seu ars delineandi* (1631). He returned to Ingolstadt to study theology, and after completing his second novitiate or 'third year' at Edersberg, he was appointed professor of Hebrew and mathematics at Ingolstadt in 1610 … From 1633 to 1639 Scheiner lived in Vienna and then in Neisse, where he was active in pastoral work until his death in 1650" (DSB).

The collations given for this work vary because of the peculiar mixture of pagination and foliation. But this copy is complete. After page 125 [-126] the book is foliated 126-149 (the latter being P6), followed by 12 leaves (gatherings Q-R^6, all but R6 (which is blank) foliated 149); pagination recommences with aa2 (aa1, beginning *Liber tertius*, is unpaginated) which is paginated 149. Furthermore, pages 511-522 are mispaginated 459-470.

Backer-Sommervogel VII, 738 8; Carli & Favaro 116; Cinti 79; Grässe VI, 298; Parkinson p. 74; *Jesuit Science in the Age of Galileo* 6; Rowland 19. Galilei & Scheiner (Reeves & Van Helden, tr.), *On Sunspots*, 2010. On the Cesi-Bellarmine correspondence, see: Galluzzi, *The Lynx and the Telescope* (2017), Ch. 7.

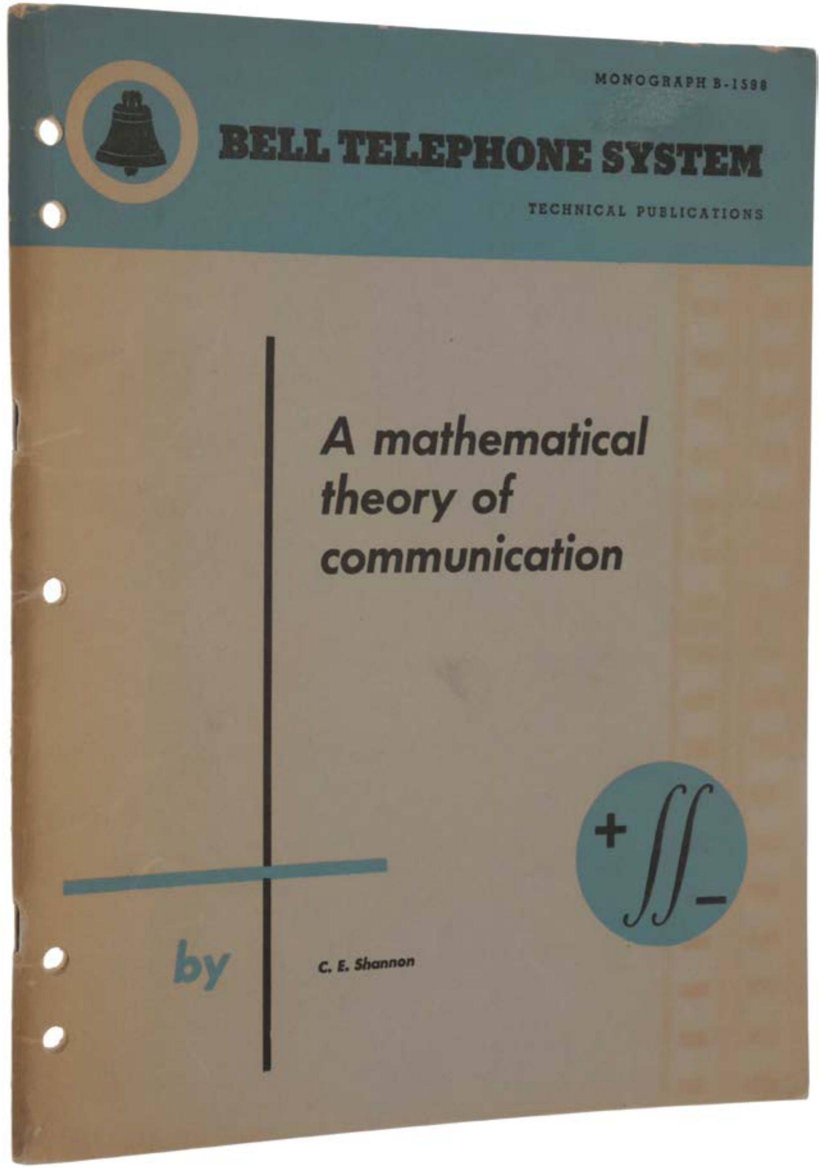
THE FOUNDING PAPER OF INFORMATION THEORY

SHANNON, Claude Elwood. *A Mathematical Theory of Communication.* Offprint from Bell System Technical Journal, Vol. 27 (July and October). New York: American Telephone and Telegraph Company, 1948.

$9,500

4to, pp. 80. Original printed wrappers, hole punched for ring binder (as always), obituary of Shannon pasted onto blank recto of rear wrapper (extracted from Nature, Vol. 410, 12 April 2001, p. 768).

First edition, the rare offprint, of "the most famous work in the history of communication theory" (*Origins of Cyberspace*). "Probably no single work in this century has more profoundly altered man's understanding of communication than C. E. Shannon's article, 'A mathematical theory of communication', first published in 1948" (Slepian). "Th[is] paper gave rise to 'information theory', which includes metaphorical applications in very different disciplines, ranging from biology to linguistics via thermodynamics or quantum physics on the one hand, and a technical discipline of mathematical essence, based on crucial concepts like that of channel capacity, on the other" (DSB). "A half century ago, Claude Shannon published his epic paper 'A Mathematical Theory of Communication.' This paper [has] had an immense impact on technological progress, and so on life as we now know it … One measure of the greatness of the [paper] is that Shannon's major precept that all communication is essentially digital is now commonplace among the modern digitalia, even to the point where many wonder why Shannon needed to state such an obvious axiom" (Blahut & Hajek). "In 1948 Shannon published

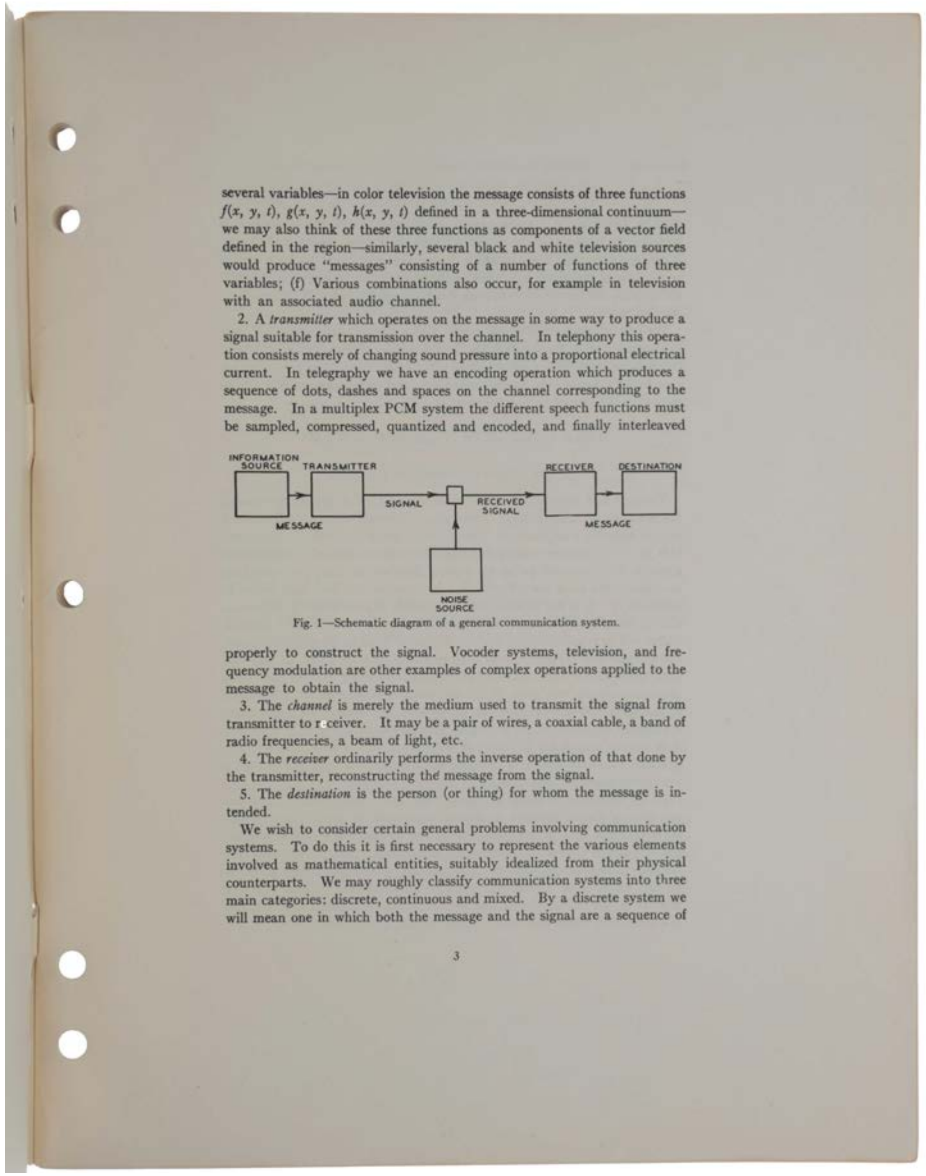


his most important paper, entitled 'A mathematical theory of communication'. This seminal work transformed the understanding of the process of electronic communication by providing it with a mathematics, a general set of theorems rather misleadingly called information theory. The information content of a message, as he defined it, has nothing to do with its inherent meaning, but simply with the number of binary digits that it takes to transmit it. Thus, information, hitherto thought of as a relatively vague and abstract idea, was analogous to physical energy and could be treated like a measurable physical quantity. His definition was both self-consistent and unique in relation to intuitive axioms. To quantify the deficit in the information content in a message he characterized it by a number, the entropy, adopting a term from thermodynamics. Building on this theoretical foundation, Shannon was able to show that any given communications channel has a maximum capacity for transmitting information. The maximum, which can be approached but never attained, has become known as the Shannon limit. So wide were its repercussions that the theory was described as one of humanity's proudest and rarest creations, a general scientific theory that could profoundly and rapidly alter humanity's view of the world. Few other works of the twentieth century have had a greater impact; he altered most profoundly all aspects of communication theory and practice" (*Biographical Memoirs of Fellows of the Royal Society,* Vol. 5, 2009). Remarkably, Shannon was initially not planning to publish the paper, and did so only at the urging of colleagues at Bell Laboratories.

"Relying on his experience in Bell Laboratories, where he had become acquainted with the work of other telecommunication engineers such as Harry Nyquist and Ralph Hartley, Shannon published in two issues of the *Bell System Technical Journal* his paper 'A Mathematical Theory of Communication.' The general approach was pragmatic; he wanted to study 'the savings due to statistical structure of the original message' (p. 379), and for that purpose, he had to neglect the semantic aspects of information, as Hartley did for 'intelligence' twenty years before. For Shannon, the communication process was stochastic in nature, and the great impact of his work, which accounts for the applications in other fields, was due to the schematic diagram of a general communication system that he proposed. An 'information source' outputs a 'message,' which is encoded by a 'transmitter' into the transmitted 'signal.' The received signal is the sum of the transmitted signal and unavoidable 'noise.' It is recovered as a decoded message, which is delivered to the 'destination.' The received signal, which is the sum between the signal and the 'noise,' is decoded in the 'receiver' that gives the message to destination. His theory showed that choosing a good combination of transmitter and receiver makes it possible to send the message with arbitrarily high accuracy and reliability, provided the information rate does not exceed a fundamental limit, named the 'channel capacity.' The proof of this result was, however, nonconstructive, leaving open the problem of designing codes and decoding means that were able to approach this limit.

"The paper was presented as an ensemble of twenty-three theorems that were mostly rigorously proven (but not always, hence the work of A. I. Khinchin and later A.N. Kolmogorov, who based a new probability theory on the information concept). Shannon's paper was divided into four parts, differentiating between discrete or continuous sources of information and the presence or absence of noise. In the simplest case (discrete source without noise), Shannon presented the [entropy] formula he had already defined in his mathematical theory of cryptography, which in fact can be reduced to a logarithmic mean. He defined the bit, the contraction of 'binary digit' (as suggested by John W. Tukey, his colleague at Bell Labs) as the unit for information. Concepts such as 'redundancy,' 'equivocation,' or channel 'capacity,' which existed as common notions, were defined as scientific concepts. Shannon stated a fundamental source-coding theorem, showing that the mean length of a message has a lower limit proportional to the entropy of the source. When noise is introduced, the channel-coding theorem stated that when the entropy of the source is less than the capacity of the channel, a code exists that allows one to transmit a message 'so that the output of the source can be transmitted over the channel with an arbitrarily small frequency of errors.' This programmatic part of Shannon's work explains the success and impact it had in telecommunications engineering. The turbo codes (error correction codes) achieved a low error probability at information rates close to the channel capacity, with reasonable complexity of implementation, thus providing for the first time experimental evidence of the channel capacity theorem" (DSB).

"The landmark event that established the discipline of information theory and brought it to immediate worldwide attention was the publication of Claude E. Shannon's classic paper 'A Mathematical Theory of Communication' in the Bell System Technical Journal in July and October 1948. Prior to this paper, limited information-theoretic ideas had been developed at Bell Labs, all implicitly assuming events of equal probability. Harry Nyquist's 1924 paper, 'Certain Factors Affecting Telegraph Speed,' contains a theoretical section quantifying 'intelligence' and the 'line speed' at which it can be transmitted by a communication system, giving the relation $W = K \log m$ (recalling Boltzmann's constant), where W is the speed of transmission of intelligence, m is the number of different voltage levels to choose from at each time step, and K is a constant. Ralph Hartley's 1928 paper, 'Transmission of Information,' uses the word information as a measurable quantity, reflecting the receiver's ability to distinguish one sequence of symbols from any other, thus quantifying information as $H = \log S^n = n \log S$, where S was the number of possible symbols, and n the number of symbols in a transmission. The unit of information was therefore the decimal digit … Alan Turing in 1940 used similar ideas as part of the statistical analysis of the breaking of the German second world war Enigma ciphers. Much of the mathematics behind information theory with events of different probabilities were developed for the



several variables—in color television the message consists of three functions $f(x, y, t)$, $g(x, y, t)$, $h(x, y, t)$ defined in a three-dimensional continuum—we may also think of these three functions as components of a vector field defined in the region—similarly, several black and white television sources would produce "messages" consisting of a number of functions of three variables; (f) Various combinations also occur, for example in television with an associated audio channel.

2. A *transmitter* which operates on the message in some way to produce a signal suitable for transmission over the channel. In telephony this operation consists merely of changing sound pressure into a proportional electrical current. In telegraphy we have an encoding operation which produces a sequence of dots, dashes and spaces on the channel corresponding to the message. In a multiplex PCM system the different speech functions must be sampled, compressed, quantized and encoded, and finally interleaved

Fig. 1—Schematic diagram of a general communication system.

properly to construct the signal. Vocoder systems, television, and frequency modulation are other examples of complex operations applied to the message to obtain the signal.

3. The *channel* is merely the medium used to transmit the signal from transmitter to receiver. It may be a pair of wires, a coaxial cable, a band of radio frequencies, a beam of light, etc.

4. The *receiver* ordinarily performs the inverse operation of that done by the transmitter, reconstructing the message from the signal.

5. The *destination* is the person (or thing) for whom the message is intended.

We wish to consider certain general problems involving communication systems. To do this it is first necessary to represent the various elements involved as mathematical entities, suitably idealized from their physical counterparts. We may roughly classify communication systems into three main categories: discrete, continuous and mixed. By a discrete system we will mean one in which both the message and the signal are a sequence of

3

field of thermodynamics by Ludwig Boltzmann and J. Willard Gibbs" (Wikipedia, accessed 20 October 2018).

"Shannon had the presight to overlay the subject of communication with a distinct partitioning into *sources*, *source encoders*, *channel encoders*, *channels*, and *associated channel and source decoders*. Although his formalization seems quite obvious in our time, it was not so obvious back then. Shannon further saw that channels and sources could and should be described using the notions of entropy and conditional entropy. He argued persuasively for the use of these notions, both through their characterization by intuitive axioms and by presentation of precise coding theorems. Moreover, he indicated how very explicit, operationally significant concepts such as the information content of a source of the information capacity of a channel can be identified using entropy and maximization of functions involving entropy.

"Shannon's revolutionary work brought forth this new subject of information theory fully formed but waiting for the maturity that fifty years of aging would bring. It is hard to imagine how the subject could have been created in an evolutionary way, though after the conception its evolution proceeded in the hands of hundreds of authors to produce the subject in its current state of maturity …

"The impact of Shannon's theory of information on the development of telecommunication has been immense. This is evident to those working at the edge of advancing developments, though perhaps not quite so visible to those involved in routine design. The notion that a channel has a specific information capacity, which can be measured in bits per second, has had a profound influence. On the one hand, this notion offers the promise, at least in theory, of communication systems with frequency of errors as small as desired for a given channel for any data rate less than the channel capacity. Moreover, Shannon's associated existence proof provided tantalizing insight into how ideal communication systems might someday fulfil the promise. On the other hand, this notion also clearly establishes a limit on the communication rate that can be achieved over a channel, offering communication engineers the ultimate benchmark with which to calibrate progress toward construction of the ultimate communication system for a given channel.

"The fact that a specific capacity can be reached, and that no data transmission system can exceed this capacity, has been the holy grail of modern design for the last fifty years. Without the guidance of Shannon's capacity formula, modern designers would have stumbled more often and proceeded more slowly. Communication systems ranging from deep-space satellite links to storage devices such as magnetic tapes and ubiquitous compact discs, and from high-speed internets to broadcast high-definition television, came sooner and in better form because of his work. Aside from this wealth of consequences, the wisdom of Claude Shannon's insights may in the end be his greatest legacy" (Blahut & Hajek).

"The 1948 paper rapidly became very famous; it was published one year later as a book, with a postscript by Warren Weaver regarding the semantic aspects of information" (DSB). The book was titled *The Mathematical Theory of Communication*, a small but significant title change reflecting the generality of this work.

OOC 880. Blahut & Hajek, Foreword to the book edition, University of Illinois Press, 1998. Slepian (ed.), *Key papers in the development of information theory, Institute of Electrical and Electronics Engineers*, 1974.

FIRST EDITION OF SPINOZA'S ETHICS

[SPINOZA, Benedictus de] B. d. S. *Opera Posthuma. Quorum series post Praefationem exhibetur.* [Amsterdam: Jan Rieuwertsz], 1677 .

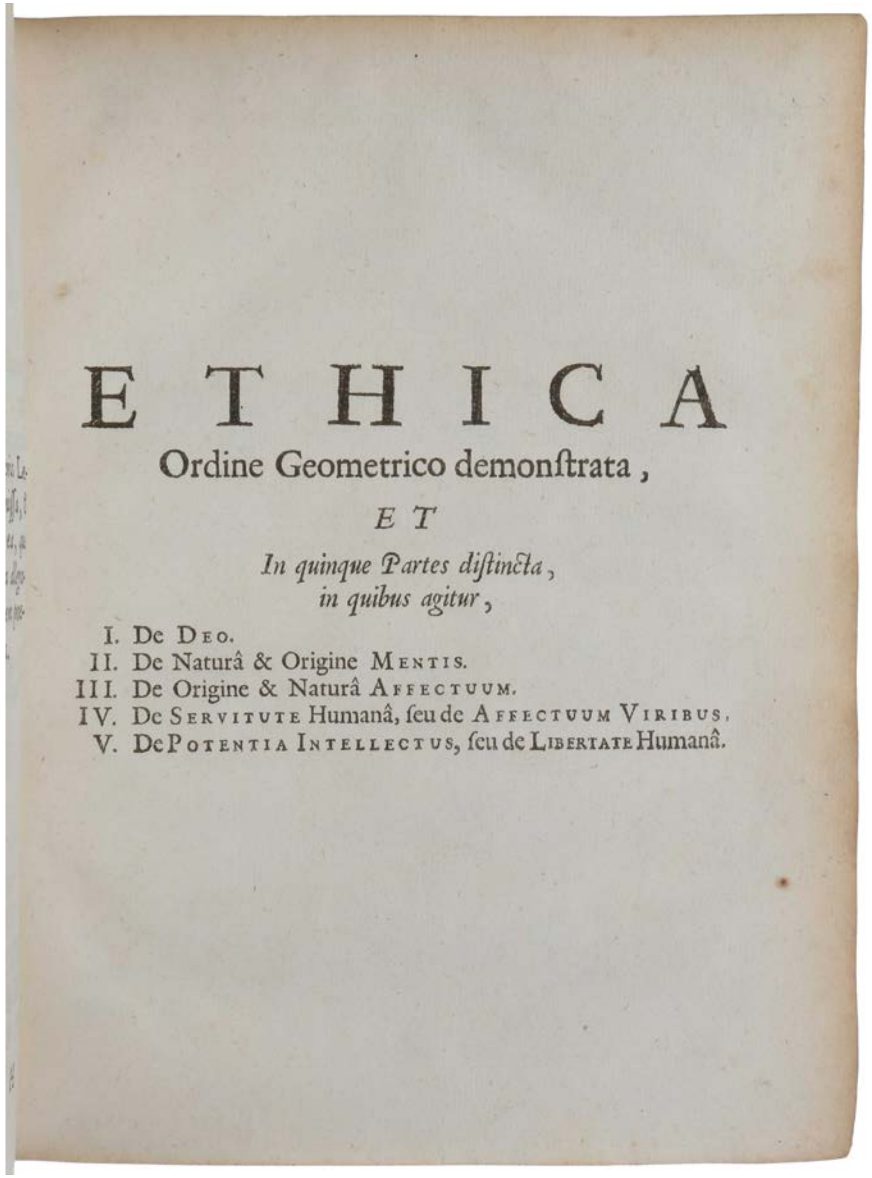
$17,500

4to, pp. [40], 614, [34], 112, [8], with woodcut vignette on title. Contemporary vellum, handwritten title to spine. A very fine and fresh copy with no restoration at all. Rare in such good condition.

First edition, and a very fine copy, of Spinoza's *Opera Posthuma* which "have served, then and since, with the *Tractatus Theologico-Politicus*, to immortalize his name" (PMM 153). The first work in the volume is "Spinoza's one indisputable masterpiece, the *Ethics*" (Bennett, *A Study of Spinoza's Ethics*, p. 7).

The first and "principal work in the *Opera Posthuma* is Spinoza's *Ethics*, in which Spinoza bridged the Cartesian duality of body and spirit by maintaining that the universe, including God, constitutes a unified infinite and all-inclusive 'Substance,' of which corporeality and spirituality were merely attributes – a unity expressed in the controversial phrase 'Deus sive Natura' (God or Nature). *Ethics* is thus considered the first systematic exposition of pantheism, the philosophy in which God is identified with the entire universe" (Norman 1988).

"Baruch (or Benedictus) Spinoza is one of the most important philosophers - and certainly the most radical - of the early modern period. His thought combines a commitment to Cartesian metaphysical and epistemological principles with elements from ancient Stoicism and medieval Jewish rationalism into a nonetheless highly original system. His extremely naturalistic views on God, the world, the human being and knowledge serve to ground a moral philosophy



E T H I C A
Ordine Geometrico demonstrata,
E T
In quinque Partes distincta,
in quibus agitur,
I. De Deo.
II. De Naturâ & Origine Mentis.
III. De Origine & Naturâ Affectuum.
IV. De Servitute Humanâ, seu de Affectuum Viribus.
V. De Potentia Intellectus, seu de Libertate Humanâ.

centered on the control of the passions leading to virtue and happiness. They also lay the foundations for a strongly democratic political thought and a deep critique of the pretensions of Scripture and sectarian religion. Of all the philosophers of the seventeenth-century, perhaps none have more relevance today than Spinoza" (*Stanford Encyclopedia of Philosophy*).

"Born in Amsterdam to a distinguished family of Sephardic exiles from Spain, Spinoza (1632-77) early absorbed all the theological and philosophical knowledge that the rabbis of his community were able to impart. Latin he learnt from an eccentric physician of materialistic tendencies, which brought him into contact with Giordano Bruno and Descartes. From this followed his break with Jewish orthodoxy, and the excommunication imposed upon him on 27 July 1656. From then on Spinoza, adopting the Latin form Benedict of his birth name Baruch, led a wandering life. Like all his Jewish contemporaries, he had learnt a handicraft: the grinding of lenses. In this, as in the theory of optics, he showed great ability. His lenses were in considerable demand, and his skill brought him into contact with Huygens and Leibniz: a tract on the rainbow, long thought to be lost, was published as recently as 1862. Thus Spinoza was able to support himself as the guest of a friend, a member of the Collegiants, an Armenian religious community, in the country outside Amsterdam out of reach of his late co-religionists, and to devote himself to concentrated thought and study. There he found himself the centre of a small philosophical club, which, originally meeting to study Cartesian philosophy, eventually parted company with Descartes; it was for them, in all probability, that Spinoza wrote his 'Ethics'" (PMM).

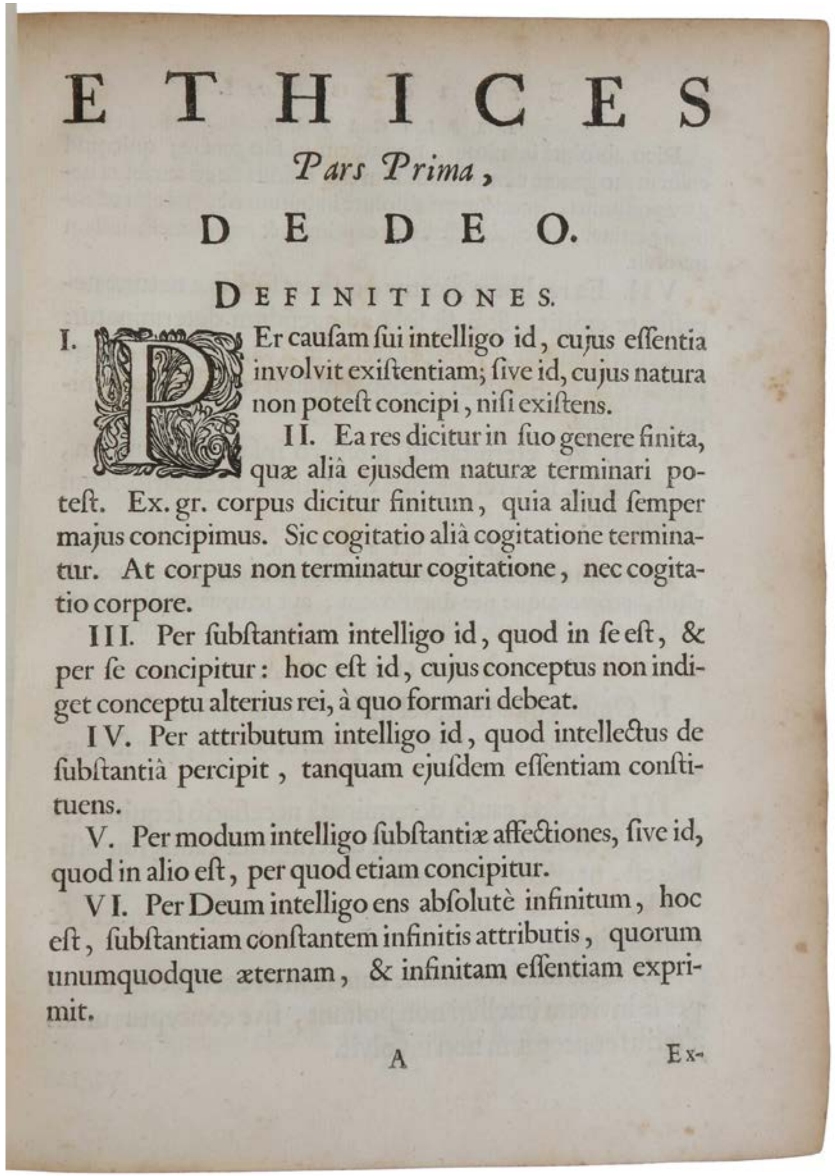
"[M]ost likely in the spring of 1662, Spinoza took up his pen to begin what would be his philosophical masterpiece, the 'Ethics' (*Ethica*) … [I]n essence, a treatise on "God, man and His Well-Being," the "Ethics" was an attempt to provide a fuller, clearer, and more systematic layout in "the geometric style" for

his grand metaphysical and moral project. He worked on it steadily for a number of years, through his move to Voorburg in 1663 and on into the summer of 1665. He envisioned at this point a three-part work, and seems to have had a fairly substantial draft in hand by June 1665. He felt confident enough of what he had written so far to allow a select few to read it, and there were Latin and even Dutch (translated by Pieter Balling) copies of the manuscript circulating among his friends. We do not know how close to a final product Spinoza considered this draft of the 'Ethics' when he put it aside, probably in the fall of 1665 ... At the time he probably saw it as mostly complete but in need of polishing. It would be a good number of years, though, before Spinoza returned to his metaphysical-moral treatise to put the finishing touches on it, which included significant additions and revisions, no doubt in the light of further reading and reflection" (Nadler, *Spinoza's Ethics. An Introduction*, p. 15).

"In 1675 he contemplated publishing his 'Ethics,' but baseless rumours, later idly repeated by Hume, of his atheism, decided him against it. On 20 February 1677 he died of consumption and his funeral was attended by a devoted and distinguished gathering" (PMM).

Immediately after his death, his friends arranged the publication of his *Ethics*, together with his other unpublished writings, in *Opera Posthuma*. It was edited by one of Spinoza's closest friends, Jarig Jelles. The *Ethics* is followed in the volume by four other works:

Tractatus de intellectus emendatione, a preliminary work to the *Ethics*, "written probably before Spinoza was thirty years old, is important not only historically, as showing how gradually and consecutively what he had to tell the world was revealed to him, but also for its own intrinsic worth" (Hale-White (translator), *Tractatus de intellectus emendation* (1895), p. 2). "The *Tractatus* is an attempt to



ETHICES

Pars Prima,

DE DEO.

DEFINITIONES.

I. PEr cauſam ſui intelligo id, cujus eſſentia involvit exiſtentiam; ſive id, cujus natura non poteſt concipi, niſi exiſtens.

II. Ea res dicitur in ſuo genere finita, quæ aliâ ejusdem naturæ terminari poteſt. Ex. gr. corpus dicitur finitum, quia aliud ſemper majus concipimus. Sic cogitatio aliâ cogitatione terminatur. At corpus non terminatur cogitatione, nec cogitatio corpore.

III. Per ſubſtantiam intelligo id, quod in ſe eſt, & per ſe concipitur: hoc eſt id, cujus conceptus non indiget conceptu alterius rei, à quo formari debeat.

IV. Per attributum intelligo id, quod intellectus de ſubſtantiâ percipit, tanquam ejuſdem eſſentiam conſtituens.

V. Per modum intelligo ſubſtantiæ affectiones, ſive id, quod in alio eſt, per quod etiam concipitur.

VI. Per Deum intelligo ens abſolutè infinitum, hoc eſt, ſubſtantiam conſtantem infinitis attributis, quorum unumquodque æternam, & infinitam eſſentiam exprimit.

A Ex-formulate a philosophical method that would allow the mind to form the clear and distinct ideas that are necessary for its perfection. It contains, in addition, reflection upon the various kinds of knowledge, an extended treatment of definition, and a lengthy analysis of the nature and causes of doubt" (Wikipedia).

The unfinished *Tractatus politicus* "is a fitting sequel to the *Ethics*. Whereas the *Ethics* reveals the path to individual freedom, the *Tractatus politicus* reveals the extent to which individual freedom depends on civil institutions. We should not be surprised to find Spinoza to be civic-minded. From his earliest writings, he claims that he is concerned not just to perfect his own nature but also "to form a society of the kind that is desirable, so that as many as possible may attain [a flourishing life] as easily and surely as possible." The *Tractatus politicus* may be seen as Spinoza's attempt to articulate some of the conditions for the possibility of such a society" (*Stanford Encyclopedia of Philosophy*).

A collection of 74 *Epistolae*, letters from and to Spinoza. "The letters are an invaluable source of information about Spinoza's life, his network of friends and acquaintances, and his works. The reason for writing the *Tractatus Theologico-Politicus* is explained in Ep. 30, and in many letters Spinoza responds to criticisms or enquiries about his views on religion … The correspondence also reflects how Spinoza's contemporaries worried about the ethical and religious implications of his philosophy, and documents the variety of subjects that were discussed under the heading of philosophy: planets (Ep. 26), hydrostatics (Ep. 41), nitre (Ep. 6, 13), probability calculus (Ep. 38). Spinoza's expertise in lens-grinding is apparent in discussions of lenses, telescopes, optics and dioptrics (Ep. 26, 32, 36, 39, 40, 46)" (*The Bloomsbury Companion to Spinoza*, p. 359).

"The fifth and final work in Spinoza's *Opera Posthuma* (with its own title page, pagination, and errata) is a Grammar of the Hebrew Language, *Compendium Grammaticus Lingua Hebraeae*. Spinoza was one of the first to subject the Bible to critical analysis but demanded that such analysis be rooted in a thorough understanding of the Hebrew language. Then, and only then, Spinoza states, may one turn to 'the life, the conduct and the pursuits of the author of each book ... [and] the fate of each book: how it was first received, into whose hands it fell, how many different versions there were of it, by whose advice it was received into the Canon, and how all the books now universally accepted as sacred, were united into a single whole'" (jewishvirtuallibrary.org).

See PMM 153; Brunet V, 492; Caillet 10309; Kingma & Offenberg 24; Norman 1988; Van der Linde 22 (apparently lacking the separate half-titles for *Ethica* and *Compendium Grammatices Linguæ Hebrææ*, which are present in our copy); Wolf Collection 378.

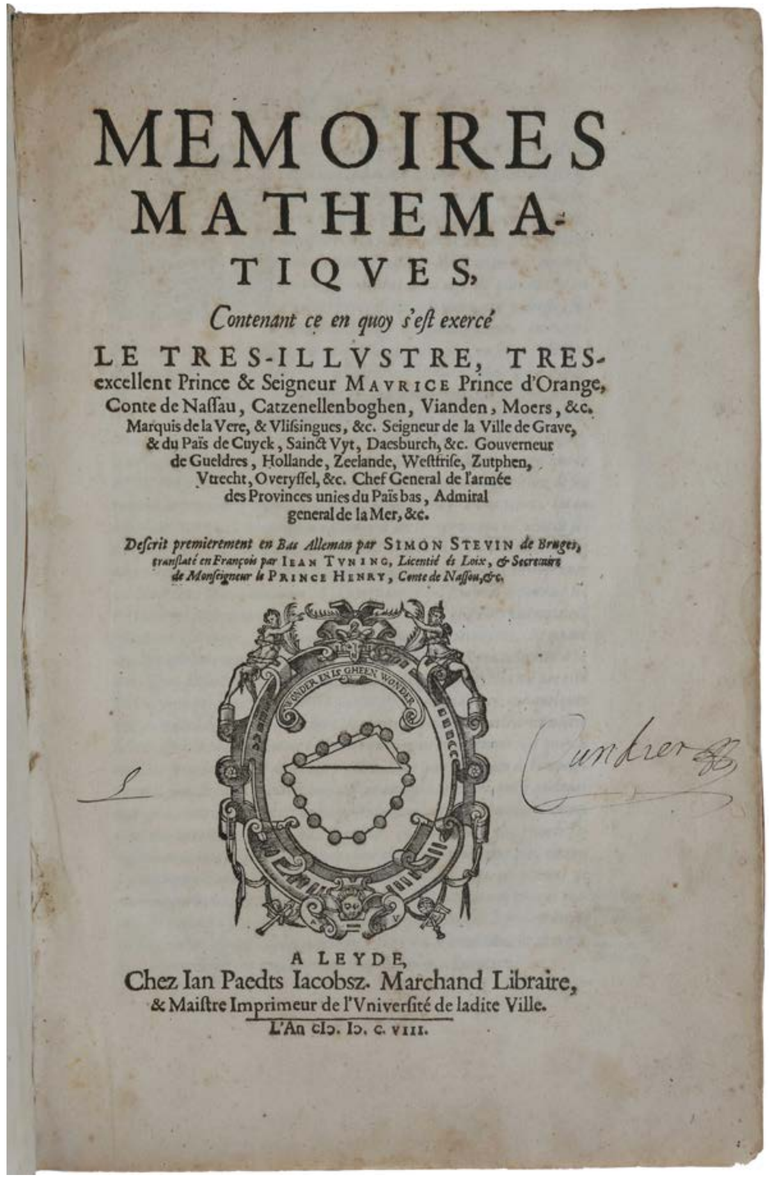
ONE OF THE MOST ORIGINAL SCIENTISTS OF THE 16TH CENTURY

STEVIN, Simon. *Mémoires Mathématiques, Contenant ce en quoy s'est exercé le très-illustre, très-excellent Prince et Seigneur Maurice Prince d'Orange, Conte de Nassau … translate en François par Jean Tuning.* Leyden: Jan Paedts Jacobsz, 1608-05-05-08.

$55,000

Four parts in one volume (numbered I, II, III & V), folio (310 x 197mm), pp. [12, last leaf blank], 1-234, 231-360; 132; 91; 10, [2, blank], 21, [3], 6, 58, [2], 8, 108 (including 'Annotation de l'autheur' on pp. 107-108), [2, blank]. Woodcut device of Stevin on title-page, woodcut device of the printer on other titles, woodcut initials and tailpieces, woodcut diagrams (those on B6r and C2r in part III with pasted-on folding flaps). Contemporary vellum over boards with yapped edges, manuscript title along spine. A fine, unrestored copy but for some intermittent browning which commonly affects this book.

Very rare first edition in French of this collection of works, which was published almost simultaneously in Dutch, French and Latin. They deal, among other topics, with geometry, trigonometry, perspective, and double-entry book-keeping – Stevin was one of the first authors to compose a treatise on governmental accounting. The *Appendice Algébraique*, which Sarton called 'one of Stevin's most important publications,' is the first published general method of solving algebraic equations; it uses what is now called the 'intermediate value theorem,' a remarkable anticipation, as it was not rigorously formulated by mathematicians until the nineteenth century. All the works appearing in this volume were first


MEMOIRES
MATHEMA-
TIQVES,
Contenant ce en quoy s'est exercé
LE TRES-ILLVSTRE, TRES-
excellent Prince & Seigneur MAVRICE Prince d'Orange,
Conte de Nassau, Catzenellenboghen, Vianden, Moers, &c.
Marquis de la Vere, & Vlissingues, &c. Seigneur de la Ville de Grave,
& du Païs de Cuyck, Saint Vyt, Daesburch, &c. Gouverneur
de Gueldres, Hollande, Zeelande, Westfrise, Zutphen,
Vtrecht, Overyssel, &c. Chef General de l'armée
des Provinces unies du Païs bas, Admiral
general de la Mer, &c.
Descrit premierement en Bas Alleman par SIMON STEVIN de Bruges,
translaté en François par IEAN TVNING, Licentié ès Loix, & Secretaire
de Monseigneur le PRINCE HENRY, Conte de Nassau, &c.
A LEYDE,
Chez Ian Paedts Iacobsz. Marchand Libraire,
& Maistre Imprimeur de l'Vniversité de ladite Ville.
L'An clɔ. Iɔ. c. VIII.

published in this collection (with one exception, where the version here is the earliest extant – see below). Stevin (1548-1620) was perhaps the most original scientist of the second half of the 16th century (the major works of Galileo did not appear until the 17th century). "He was involved in geometry, algebra, arithmetic (pioneering a system of decimals), dynamics and statics, almost all branches of engineering and the theory of music" (Kemp, p. 113). "Stevin unconditionally supported [the Copernican system], several years before Galileo and at a time when few other scientists could bring themselves to do likewise" (DSB XIII: 48). In 1593 Prince Maurice of Nassau (1567-1625) appointed Stevin quartermaster-general of the Dutch armies, a post he held until his death. From 1600 Stevin organized the mathematical teaching at the engineering school attached to Leiden University. "The Prince used to carry manuscripts of [Stevin's lectures] with him in his campaigns. Fearing that he might lose them, he finally decided to have them published, not only in the original Dutch text [*Wisconstighe Gedachtenissen*] … but also in a Latin translation by Willebrord Snel [*Hypomnemata mathematica*] … and in a French translation by Jean Tuning [offered here]" (Sarton, p. 245). The Dutch and Latin editions were published in five parts, of which the fourth consisted principally of reprints of his works on statics that had appeared separately in 1586. This fourth part was not translated into French because, we are told at the beginning of the fifth part, of the printer's impatience – he was tired of keeping the sheets already printed and suggested that additional materials could be published later when the author had prepared them. The printer's impatience also accounts for the fact that several works that are announced on the title pages of the individual volumes did not in fact appear in the Dutch, French or Latin editions. The only other complete copy of this French edition listed by ABPC/RBH is the De Vitry copy, in a nineteenth-century binding (Sotheby's, April 11, 2002, lot 779, £15,200 = $21,935). OCLC lists Columbia, Harvard and UCLA only in US.

Provenance: L. Cundier, early inscription on title-pages, i.e., Louis Cundier (c. 1615- 1681), French geometer, surveyor and engraver. He was professor of mathematics at Aix, and was responsible for a *Carte géographique de Provence*, published about 1640. Contemporary marginal annotation on R6v of final part.

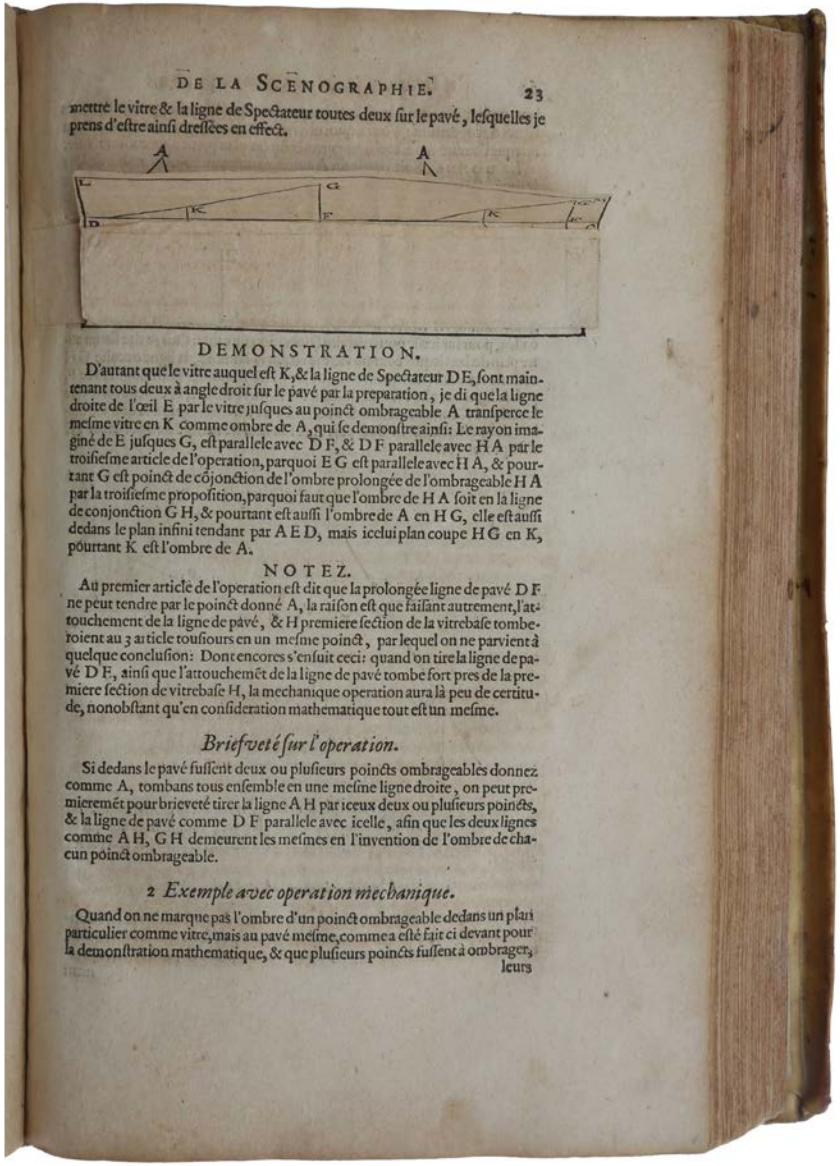
The first part of the work, entitled *Cosmographie* (1608), is a treatise on the trigonometrical techniques used in the observation of the heavens, together with extensive tables of sines, tangents and secants. "The first to use the term trigonometry seems to have been Pitiscus, whose book *Trigonometria* made its first appearance in 1595, but in 1608, when Stevin's book appeared, the term had not yet been generally accepted. The book consists of four parts, the first dealing with the construction of goniometrical tables, the second with plane triangles, and the remaining two parts with spherical trigonometry … It is mainly of interest to those who wish to see what trigonometry was like in the sixteenth century, long before Euler, in 1748, introduced the present notation. It also has some distinction as the first complete text on trigonometry written in Dutch; and one of the first – if not the first – written in any vernacular" (*Works*, IIb, p. 751).

Part II, *De la Practique de Géométrie* (1605) [in Dutch, *De Meetdaet*], "is primarily a textbook for the instruction of those who, like Prince Maurice, wanted to learn some of the more practical aspects of geometry. The course was not one for beginners, knowledge of Euclid's *Elements* being a prerequisite, while the reader was also supposed to know something about the measurement of angles and Stevin's own calculus of decimal fractions … Parts of the contents were taken from the *Problemata Geometrica,* the book which Stevin published in 1583, but to which he, curiously enough, never refers. Other parts show the influence of Archimedes and of contemporary writers such as Del Monte and Van Ceulen. Although in accordance with the title strong emphasis is laid on the practical applications of geometry, many theoretical problems are discussed. For Stevin theory and application always went hand in hand.

"The *Meetdaet* appeared in 1605, but it was drafted more than twenty years before. Already in the *Problemata Geometrica* Stevin refers to a text on geometry, 'which we hope shortly to publish' and in which the subject was to be treated by a method parallel to that used in arithmetic. At that time Stevin's *L'Arithmétique* was either finished or well advanced. We get the impression that in this period, 1583-85, Stevin decided to publish his full text on arithmetic, but of his text on geometry only those parts which he considered novel. The general outline of the two texts was laid out at the same time, and in close parallel. When at last the *Meetdaet* appeared, it had undergone many changes, resulting partly or wholly from lengthy discussions with the Prince of Orange. The underlying idea, however, remained the same.

"In the introduction to the *Meetdaet* Stevin explains what he means by this parallelism of arithmetic and geometry. In arithmetic we begin by introducing the numerical symbols, and follow this up by naming them and interpreting their value. Then come the four species, the theory of proportions, the theory of proportional division, and finally the reduction of fractions to a common denominator. Similarly, in geometry, we begin by showing the student how to draw figures, then we name them and explain how to measure them. Then follow the four species, the theory of proportions, of proportional intersections, and the reduction of figures into others of given form and equal length, area or volume. Since these topics are taken in six groups, and each group with lines, plane figures, and solids, the *Meetdaet* consists of six books, each consisting of three parts.

"The opinion of Stevin that geometry and arithmetic have to run parallel is not so artificial as it appears at first sight. Stevin expresses an opinion common to the mathematicians of his age, who insisted on enlarging the field of numbers with irrationals to something like an arithmetic continuum, who applied these numbers without discrimination to the measurement of figures, and for whom numbers were not so much the object of abstract speculation as the



DE LA SCENOGRAPHIE. 23

mettre le vitre & la ligne de Spectateur toutes deux sur le pavé, lesquelles je prens d'estre ainsi dressées en effect.

DEMONSTRATION.

D'autant que le vitre auquel est K, & la ligne de Spectateur D E, sont maintenant tous deux à angle droit sur le pavé par la preparation, je di que la ligne droite de l'œil E par le vitre jusques au poinct ombrageable A transperce le mesme vitre en K comme ombre de A, qui se demonstre ainsi: Le rayon imaginé de E jusques G, est parallele avec D F, & D F parallele avec H A par le troisiesme article de l'operation, parquoi E G est parallele avec H A, & pourtant G est poinct de cõjonction de l'ombre prolongée de l'ombrageable H A par la troisiesme proposition, parquoi faut que l'ombre de H A soit en la ligne de conjonction G H, & pourtant est aussi l'ombre de A en H G, elle est aussi dedans le plan infini tendant par A E D, mais icelui plan coupe H G en K, pourtant K est l'ombre de A.

NOTEZ.

Au premier article de l'operation est dit que la prolongée ligne de pavé D F ne peut tendre par le poinct donné A, la raison est que faisant autrement, l'attouchement de la ligne de pavé, & H premiere section de la vitrebase tomberoient au 3 article tousiours en un mesme poinct, par lequel on ne parvient à quelque conclusion: Dont encores s'ensuit ceci: quand on tire la ligne de pavé D F, ainsi que l'attouchemẽt de la ligne de pavé tombe fort pres de la premiere section de vitrebase H, la mechanique operation aura là peu de certitude, nonobstant qu'en consideration mathematique tout est un mesme.

Briefveté sur l'operation.

Si dedans le pavé fussent deux ou plusieurs poincts ombrageables donnez comme A, tombans tous ensemble en une mesme ligne droite, on peut premieremẽt pour brieveté tirer la ligne A H par iceux deux ou plusieurs poincts, & la ligne de pavé comme D F parallele avec icelle, afin que les deux lignes comme A H, G H demeurent les mesmes en l'invention de l'ombre de chacun poinct ombrageable.

2 *Exemple avec operation mechanique.*

Quand on ne marque pas l'ombre d'un poinct ombrageable dedans un plan particulier comme vitre, mais au pavé mesme, comme a esté fait ci devant pour la demonstration mathematique, & que plusieurs poincts fussent à ombrager, leurs tools for surveying, navigation, and astronomy. The subject matter of geometry is continuous quantity, wrote such men as Tartaglia and Clavius. It seemed natural that there should exist relations and analogies between the professed geometrical and the intuitively felt arithmetical continuum. Stevin only gave an early sixteenth-century version of a point of view which was to lead, within the next generations, to analytic geometry. Consciousness of the analogy between arithmetical-algebraic and geometrical considerations continued to work as a leaven throughout the further development of mathematics. Later we find it in Leibniz' proposal for an algebra of directed quantities. In another form it appeared again more recently when Hilbert probed the consistency of geometrical axioms by means of a corresponding algebraic counterpart.

"Book I of the *Meetdaet,* in accordance with the author's program, teaches methods for drawing lines and certain plane figures, and for constructing certain solids. With his keen sense of the interdependence of theory and practice Stevin gives not only rules for the drawing board, but also for the surveyor and instrument-maker. We thus meet here with a description of the surveyor's cross or diopter, already described by Heron and used for setting out perpendiculars by lines of sight. With a graduated circle instead of a cross it becomes a so-called circumferentor or theodolite. The plane figures discussed are the circle, the conic sections, and the Archimedean spiral. No fewer than four methods are given for constructing points of an ellipse when the principal axes are given in position and magnitude … The fourth ellipse construction is equivalent to the one we often use at present, and by which we find points of the ellipse by considering it the oblique parallel or orthographic projection of a circle with one of the axes as diameter. This construction may in this form be original with Stevin, though it is closely related to another one, also presented by Stevin, in which he shows how the conic sections can be constructed as plane intersections of a right circular cone. His method amounts to what we now call orthographic projection … Book I also contains Stevin's description of the five regular and of eight Archimedean solids …

"In Book II we find observations on the lengths of line segments and curves, the areas of two-dimensional figures, and the volumes of solids. Some surveyor's instruments appear, among them the ancient 'traprondt' or graduated circle for measuring horizontal angles, and the equally ancient triquetrum, consisting of two arms of equal length, hinged to a third; they are graduated and have sighting devices. The triquetrum, also called PtoIemy's rods or parallactic instrument, is used by Stevin to determine a triangle similar to a triangle in the fields, though in his days it had also received attention as a favourite measuring instrument of Copernicus and Tycho Brahe. As an application of the triquetrum Stevin shows us how to measure the distance from a given point to a point beyond reach. A number of other exercises in surveying follow, and also such problems as the computation of the altitudes of a triangle with given sides. In the section on the measuring of circumferences and areas we find a discussion of the value of π with due references to Archimedes, Romanus, and Van Ceulen …

"Book III contains the application of four species to geometry, with reference to the parallel treatment in *L'Aritbmétique.* Multiplication and division of segments, areas, and volumes is only performed by means of numerical factors; there is no reference to the multiplication of segments so as to form areas. Of interest is the addition and subtraction of solids, but the only case discussed is that of similar figures …

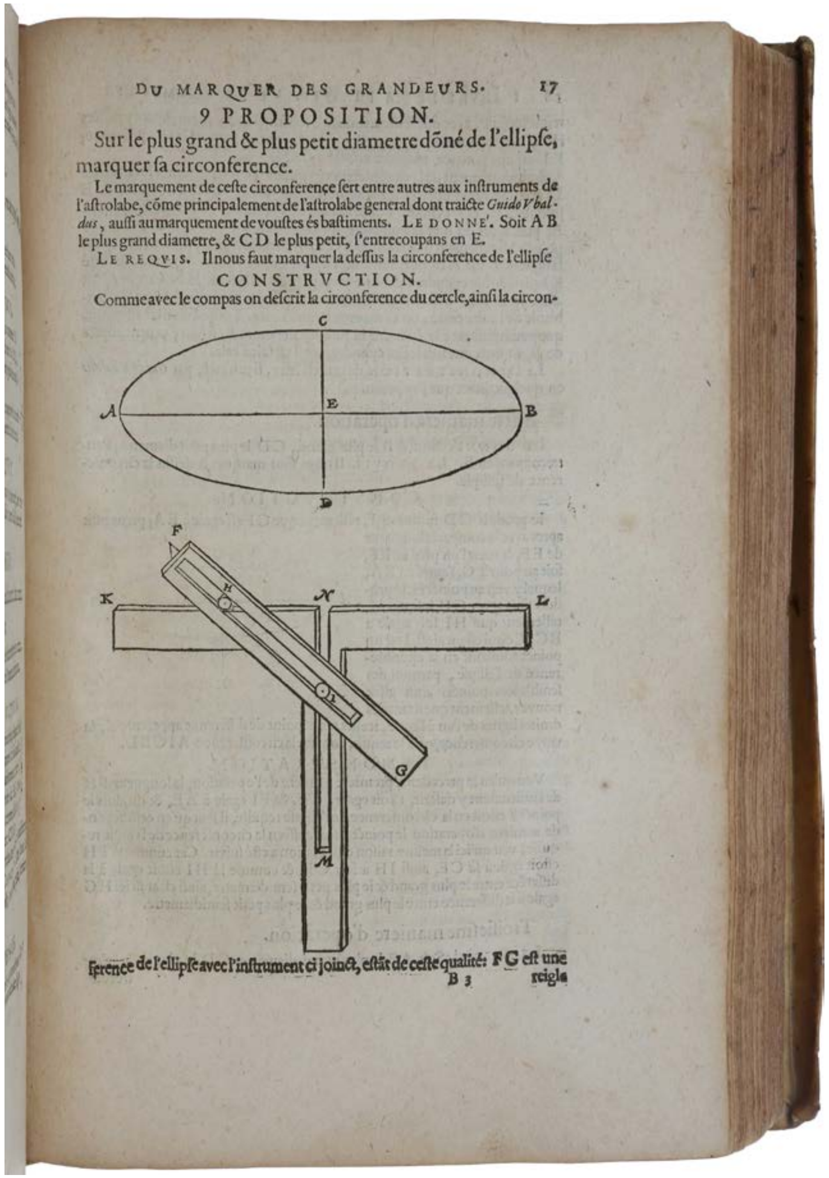
"In Book IV we find a theory of proportions. It is shown how areas and volumes proportional to given line segments can be found. The most interesting part is that in which the two mean proportionals between two line segments are discussed. As in the *Problemata Geometrica,* reference is made to Hero's construction according to

Eutocius. The Eratosthenes construction is mentioned, but not further discussed.

"Book V contains the division of plane polygons into parts of given ratio by a line satisfying certain conditions, another of the topics of the *Problemata Geometrica.* Here Stevin goes a little beyond the text of 1583 … he not only modified some of the proofs of the theorems already discussed in the *Problemata,* but added the cases where the line of division has to pass through a point outside or inside the polygon …

"Finally, Book VI deals with some transformations of figures into others of given form and given length, area or volume, such as the (approximate) construction of a straight line equal to the circumference of a given circle, of a triangle equal in area to a given circle, of a sphere equal in volume to a given cone, of a cylinder equal in volume to a given sphere, and of a segment of a sphere, similar to one of two given segments and equal in area to the other" (*ibid.*, pp. 764-8).

Part III, *Des Perspectives* (1605) [in Dutch, *Deursichtighe*], is a mathematical treatment of perspective. "Stevin's book gives an important discussion of the case in which the plane of the drawing is not perpendicular to the plane of the ground and, for special cases, solves the inverse problem of perspective" (DSB XIII: 48). "[Stevin's] approach to perspective belongs in the Commandino – Benedetti – Guidobaldo tradition, and his main demonstrations are uncompromisingly geometrical in nature. He also took up the essentially non-pictorial problem of the rotation of the picture plane into the ground plane, formulating one of the basic theorems of homology. However, he does show some of Marolois's sensitivity to the needs of practitioners. His treatise was occasioned by the desire of Prince Maurice to understand the principles of pictorial representation – 'wishing to design exactly the perspective of any given figure with knowledge of causes and mathematical proof'. Stevin accordingly provides 'abridgements' of his geometrical techniques for artists – albeit rather abstract abridgements – and



illustrates a Dürer-like perspective machine" (Kemp, pp. 113-114). Stevin "was obliged to perform a considerable amount of original work, since most of the books at his disposal had written by and for painters and architects, and were rich in directives and deficient in mathematical demonstrations. The only textbook comparable to that of Stevin in mathematical clarity and antedating it was the *Perspectiva* of his contemporary and colleague Guido Ubaldo Del Monte (1545-1607), which was published in 1600, only five years before the *Deursichtighe*.

"Stevin's work contains two books. The title of the first book, *Verschaeuwing,* is Stevin's translation of the Latin word *scenographia.* The term *Deursichtighe* is his translation of the word *perspectiva.* Since the second book of the *Deursichtighe* contains the principles of *Spiegelschaeuwen* (theory of reflection in mirrors, translation of *catoptrica),* perspective in Stevin's terminology comprises both scenography and catoptrics. It also includes the principles of refraction, called *Wanschaeflwing,* but this subject is wanting in the book" (*Works*, IIb, p. 785)

"There is much in Stevin's book which reminds us of Del Monte's, notably the extensive use of rotations and the introduction of the inverse problem of perspective, and the double solution of certain problems, called here the 'mathematical' and the 'mechanical' way. The two men had much in common; both were experts on fortifications, both were mathematicians deeply interested in problems of mechanics, both combined a love of theoretical study with engineering practice. It is understandable that their approach to perspective was similar, and it is not unlikely that Stevin thoroughly enjoyed Del Monte's work. Despite this influence (which has to be inferred rather than proved by quotations) Stevin's work is an achievement of remarkable originality. He probably had a good deal of the contents of his work ready before he studied Del Monte's *Perspective* (if ever he did), and maintained his particular way of exposition and selection throughout the book …

"The *Verschaeuwing* itself opens with certain postulates, showing how seriously the author tried to base his work on a correct mathematical foundation. One of these postulates is that a point and its perspective image lie in a straight line with the eye. Stevin's explanation of the necessity of this postulate is that the physical eye is not a mathematical point; by pressing the eye we can obtain a difference of as much as 33° in the image of a given point.

"Among the first constructions are the classical ones of finding the perspective images of a point and a line. Here we meet the demonstration of Del Monte's theorem that all sets of parallel lines have images in lines passing through one point. This point, 'saempunt', is Del Monte's 'punctum concursus'. Then comes Stevin's new approach: he takes the picture plane (the 'glass') no longer perpendicular to the ground plane (the 'floor'), but at an arbitrary angle. This leads him to two new theorems (Props. 7 and 8), by means of which the construction for this case is reduced to the case of the vertical picture plane … Stevin now undertakes the construction of the perspective images of several figures, including that of a 'tower', a quadrangular pyramid on top of a cube with a face of the cube as its base; the cube is standing on the ground plane. He also constructs the ellipse as the image of a circle. Some methods of checking the correctness of constructions follow.

"These propositions can be considered as forming the first part of the *Verschaeuwing*. The second part (from Prop. 12 onwards) deals with the inverse problem of perspective, a subject already touched by Del Monte. Given a polygon as image, and another polygon in the ground plane turned into the picture plane: to find, if possible, the eye; the angle between picture plane and ground plane is given and is not necessarily 90°. Stevin solves the problem in certain special cases; the solution of the solution of the general problem had to wait until the nineteenth century.

"The text ends with an 'Appendix', which contains certain observations on terminology, a correction of certain constructions by Serlio, and a description of a model described by Dürer, which caught the fancy of Prince Maurice to such an extent that he had it constructed. It was an instrument for drawing the perspective of a figure on a glass plate; it had helped Stevin himself to gain a better understanding of the theory.

"Book II of the *Deursichtighe,* the Catoptrics, is short and does not contain much that is of interest … Stevin must have added the sixteen pages as a tribute to an ancient tradition, but he did not develop the subject with his usual thoroughness. That part of the *Catoptrics* which deals with refraction and which was announced in the Summary, *Van de Wanschaeuwing,* was not even published" (*ibid*., pp. 790-1)

Part V, *Meslanges* (1608), contains a very important mathematical work, *Appendice algébrique contenant règle générale de toutes Equations*, as well as Stevin's treatise on double-entry bookkeeping. Sections on music, architecture, fortification and other topics, announced on the title page, were never published (in the Dutch, French or Latin editions).

The *Appendice* had been published separately in 1594, but the unique copy, kept at the University of Louvain, was destroyed during World War I and its appearance here is now the earliest extant. "This is one of Stevin's most important publications: it includes a general rule to solve numerical equations of every degree. Expressed in modern language: if $f(a) > 0$ and $f(b) < 0$, there is between a and b at least one root of the equation $f(x) = 0$" (Sarton, p. 253). This is the first clear statement of what is now known as the 'intermediate value theorem', which was rigorously formulated and proved only two centuries later by Bolzano and Cauchy. Stevin tells us that his friend Ludolph van Ceulen had also found a general rule for the same purpose, and it was probably also known to Adrianus Romanus, but priority definitely belongs to Stevin as he was the only one to publish it.

"In his *Appendice Algébraique* Stevin states that after the publication of *L'Arithmétique* he has found a general rule to solve all equations either perfectly or with any degree of approximation. His example is $x^3 = 300x + 33915024$. To find a first approximation for x, try $x = 1$, then $x = 10, 100, 1000, \ldots$ The result is that for $x = 1$, $x = 10$, $x = 100$, the value of x^3 is less than that of $300x + 33915024$, but for $x = 1000$ it is larger. Hence the first result is $100 < x < 1000$. To find a second approximation for x he now substitutes $x = 100, 200, 300, 400$ and finds $300 < x < 400$. Now he tries $x = 310, 320, 330$ and finds $320 < x < 330$, then $x = 321, 322, 323, 324$. It appears that for $x = 324$ both sides of the equation are equal, so $x = 324$ is the root.

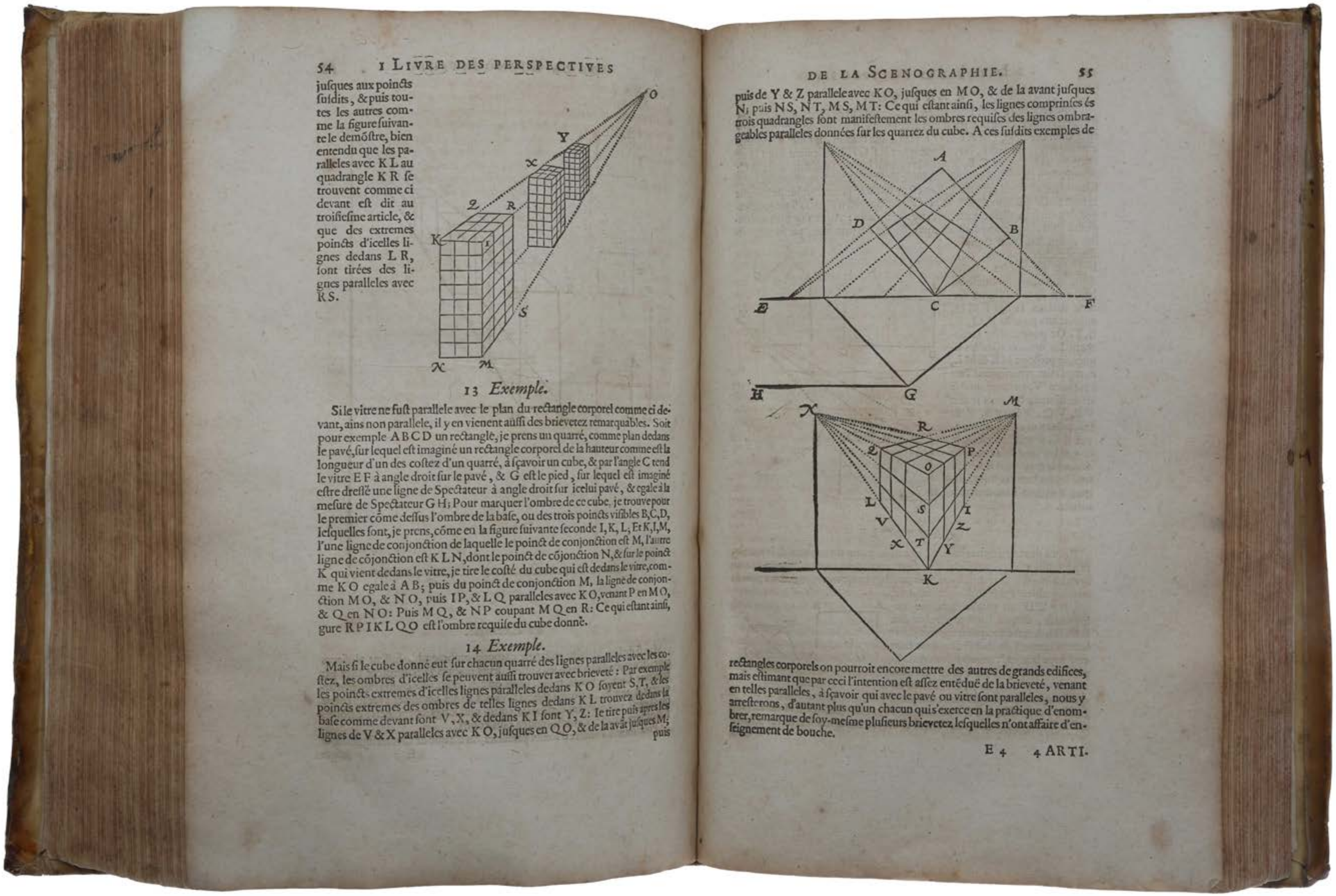
"The method can also be applied if the root is not an integral number. If $x^3 = 300x + 33900000$ we find $323 < x < 324$. Then write $x = 3230/10$ and proceed as above, first with 1/10, then 1/100, etc. This can go on indefinitely. If, for instance, the root were $x = 5/6$, the method gives first 8/10, then 83/100, then 833/1000, then 8333/10000, and so we can approach the root as closely as we like. The same holds if x were a radical, incommensurable with common numbers" (*Works*, IIb, p. 740).

The treatise on double entry bookkeeping, *Livre de compte de prince à la manière d'Italie, en domaine et finance extraordinaire* …, "was composed by Stevin at the request of Prince Maurice, and aptly dedicated to Sully, the great French economist and minister to Henry IV. It is divided into two parts: The merchant's account book, and the prince's account book, and the latter part is divided into three others: Livre de compte en domaine, Livre de compte en dépenses, Livre de compte en finances extraordinaires …

"The origin of his treatise is clearly explained in the dedication to Sully and in two preliminary dialogues. He recalls his experience as a bookkeeper and cashier in an Antwerp firm and his work in the financial administration of his native city. While doing this work he was struck by the fact that the domanial and financial accounts were kept so badly that princes were always at the mercy of their intendents and receivers, who could deceive them with impunity. It was very soon clear to him that the only way to put a stop to these abuses was to introduce into the public or princely administration the very methods used by merchants. But he had no chance to set forth his views to a competent person until the day came when Maurice of Nassau asked for his advice in that very matter. Stevin explained his ideas of reform to him, and composed the first part of his work; Maurice then asked him to compose the second part (i.e., the prince's account book). The Prince understood at once the advantage of Stevin's method and introduced it in his own domains" (Sarton, pp. 263-6). This treatise was issued separately in 1608 in French, and perhaps also in Dutch.

"The French translator, Jean Tuning, was secretary to Prince Frederik Hendrik of Nassau (1584-1647), Maurice's young brother; he was born in Leiden and matriculated at the University of Leiden in 1593" (Sarton, p. 256).

Bibliotheca Belgica S.142 (incomplete); Bierens de Haan 4571 (describing only three of the four books); Crone et al (eds.), *The Principal Works of Simon Stevin*, five vols. (in six), 1955-66; DSB XIII 47-51; Kemp, *The Science of Art*, 1990, Sarton, 'Simon Stevin of Bruges (1548-1620)', *Isis* 21 (1934), pp. 241-303.



54 I LIVRE DES PERSPECTIVES

juſques aux poincts ſuſdits, & puis toutes les autres comme la figure ſuivante le demóſtre, bien entendu que les paralleles avec K L au quadrangle K R ſe trouvent comme ci devant eſt dit au troiſieſme article, & que des extremes poincts d'icelles lignes dedans L R, ſont tirées des lignes paralleles avec R S.

13 *Exemple.*

Si le vitre ne fuſt parallele avec le plan du rectangle corporel comme ci devant, ains non parallele, il y en vienent auſſi des brievetez remarquables. Soit pour exemple A B C D un rectanglé, je prens un quarré, comme plan dedans le pavé, ſur lequel eſt imaginé un rectangle corporel de la hauteur comme eſt la longueur d'un des coſtez d'un quarré, à ſçavoir un cube, & par l'angle C tend le vitre E F à angle droit ſur le pavé, & G eſt le pied, ſur lequel eſt imaginé eſtre dreſſé une ligne de Spectateur à angle droit ſur icelui pavé, & egale à la meſure de Spectateur G H; Pour marquer l'ombre de ce cube, je trouve pour le premier cóme deſſus l'ombre de la baſe, ou des trois poincts viſibles B,C,D, leſquelles ſont, je prens, cóme en la figure ſuivante ſeconde I, K, L; Et K,I,M, l'une ligne de conjonction de laquelle le poinct de conjonction eſt M, l'autre ligne de cójonction eſt K L N, dont le poinct de cójonction N, & ſur le poinct K qui vient dedans le vitre, je tire le coſté du cube qui eſt dedans le vitre, comme K O egale à A B; puis du poinct de conjonction M, la ligne de conjonction M O, & N O, puis I P, & L Q paralleles avec K O, venant P en M O, & Q en N O: Puis M Q, & N P coupant M Q en R: Ce qui eſtant ainſi, figure R P I K L Q O eſt l'ombre requiſe du cube donné.

14 *Exemple.*

Mais ſi le cube donné eut ſur chacun quarré des lignes paralleles avec les coſtez, les ombres d'icelles ſe peuvent auſſi trouver avec brieveté: Par exemple les poincts extremes d'icelles lignes paralleles dedans K O ſoyent S, T, & les poincts extremes des ombres de telles lignes dedans K L trouvez dedans la baſe comme devant ſont V, X, & dedans K I ſont Y, Z: Ie tire puis apres les lignes de V & X paralleles avec K O, juſques en Q O, & de la avát juſques M; puis

DE LA SCENOGRAPHIE. 55

puis de Y & Z parallele avec K O, juſques en M O, & de la avant juſques N; puis N S, N T, M S, M T: Ce qui eſtant ainſi, les lignes comprinſes és trois quadrangles ſont manifeſtement les ombres requiſes des lignes ombrageables paralleles données ſur les quarrez du cube. A ces ſuſdits exemples de rectangles corporels on pourroit encore mettre des autres de grands edifices, mais eſtimant que par ceci l'intention eſt aſſez entéduë de la brieveté, venant en telles paralleles, à ſçavoir qui avec le pavé ou vitre ſont paralleles, nous y arreſterons, d'autant plus qu'un chacun qui s'exerce en la practique d'enombrer, remarque de ſoy-meſme pluſieurs brievetez leſquelles n'ont affaire d'enſeignement de bouche.

E 4 4 ARTI.

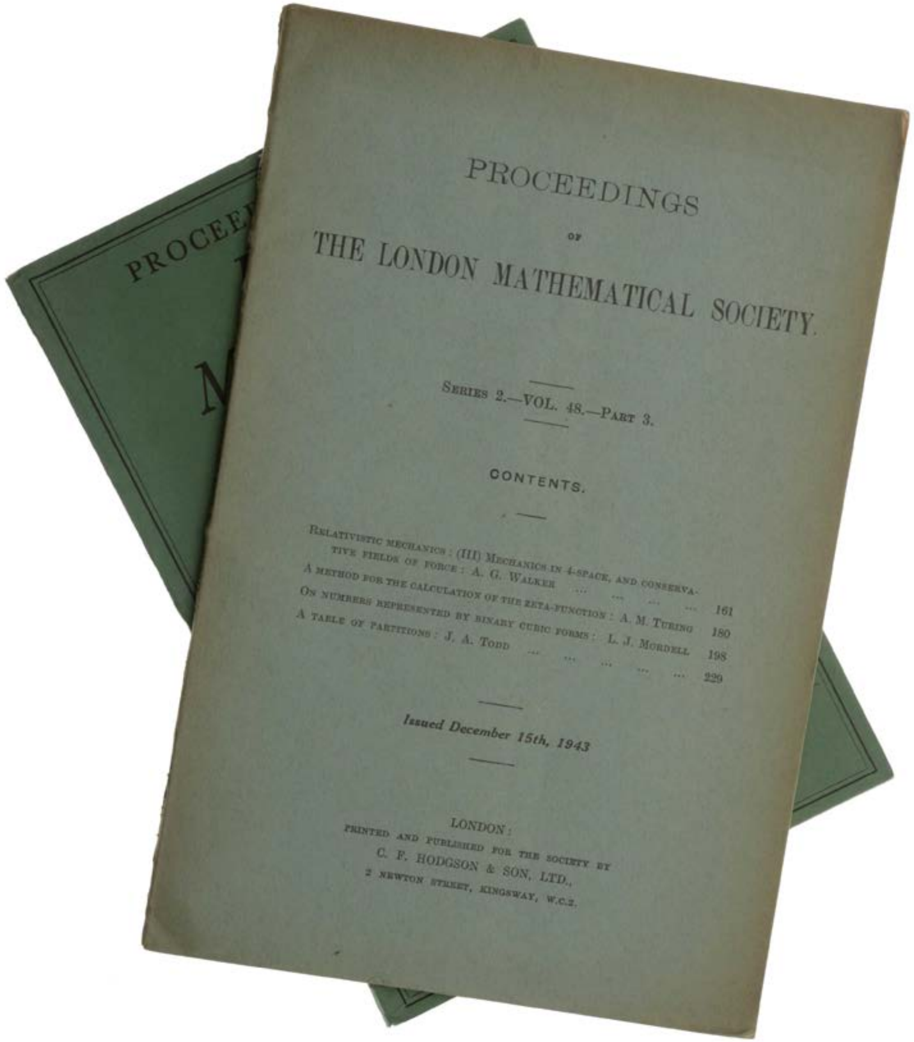
THE RIEMANN HYPOTHESIS

TURING, Alan. *'A Method for the Calculation of the Zeta-Function', pp. 180-197 in Proceedings of the London Mathematical Society, Series 2, Vol. 48, No. 3, December 15, 1943. London: C. F. Hodgson and Son, 1943. [Offered with:] 'Some calculations of the Riemann zeta-function,' pp. 99-117 in ibid., Series 3, Vol. 3, No. 9, March 1953.* London: C. F. Hodgson and Son, 1943; 1953.

$3,850

Two vols., large 8vo, pp. 161-240 & 1-128. Original printed wrappers (first volume with tiny chip from upper right corner of front wrapper, edges of wrappers lightly browned, second volume with small closed tear at top of spine). Very good copies.

First edition, journal issues in the original printed wrappers, of Turing's ground-breaking work outlining a method to decide the most famous open problem in mathematics, the so-called Riemann hypothesis. This is a conjecture about the location of the zeros of the 'Riemann zeta function' – it asserts that, apart from some 'trivial' zeros, they all lie on a certain 'critical line.' If true, this would have enormous implications for the study of prime numbers. Turing had worked on the zeta function since 1939 and in 'A Method for the Calculation of the Zeta-Function' he outlined a method of calculating the zeros using a mechanical computer. "The Turing archive contains a sketch of a proposal, in 1939, to build an analog computer that would calculate approximate values for the Riemann zeta-function on the critical line. His ingenious method was published in 1943 [as the present work]" (Downey, p. 11). Although he received a grant to build his zeta-function machine, the outbreak of World War II, and Turing's role in it as cryptanalyst, postponed the work, and the machine was never constructed.



After the War, Turing returned to the Riemann hypothesis and developed a new procedure, now known as 'Turing's method', for checking the Riemann Hypothesis (described in Section 4 of the 1953 paper). He then used the Manchester Mark I digital computer to implement this method. "Of Turing's two published papers [both offered here] on the Riemann zeta function, the second is the more significant. In that paper, Turing reports on the first calculation of zeros of [the zeta function] ever done with the aid of an electronic digital computer. It was in developing the theoretical underpinnings for this work that Turing's method first came into existence" (Hedjhal & Odlyzko, p. 265). 'Some calculations of the Riemann zeta-function' was Turing's last published mathematical paper. "This work was one of the first announcing a new chapter in which experimental mathematics performed with computers would play an important role" (Mezzadri and Snaith, *Recent Perspectives in Random Matrix and Number Theory*). Rare on the market in unrestored original printed wrappers – we know of only one copy of the first paper at auction, in the Weinreb Computer Collection (Bloomsbury Book Auctions, 28 October 1999), and no other copy of the second.

The Riemann zeta function is defined as the sum of an infinite series

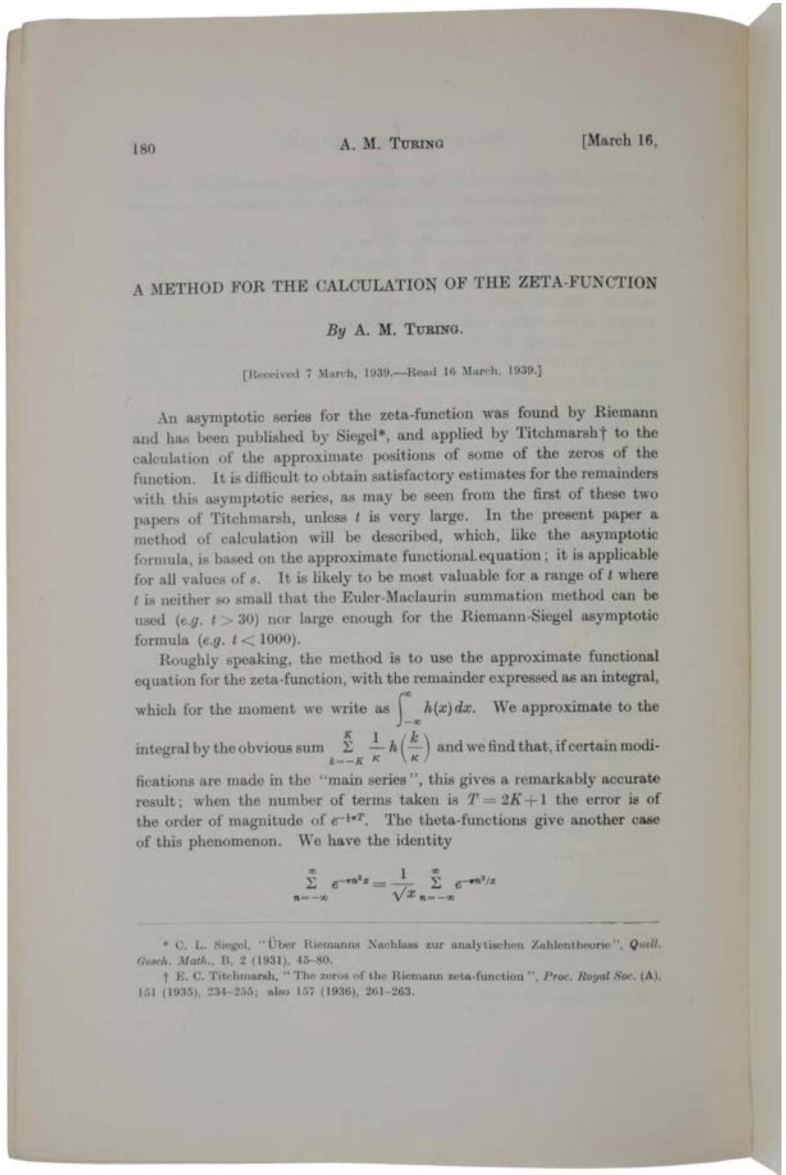
$$\zeta(s) = 1/1^s + 1/2^s + 1/3^s + 1/4^s + \ldots..$$

This actually makes sense when s is any *complex* number (except $s = 1$, when the sum is infinite). It is known that $\zeta(s) = 0$ when $s = -2, -4, -6, \ldots$ – these are called the 'trivial zeros'. The Riemann hypothesis (RH) is the assertion that all the non-trivial zeros are complex numbers of the form $s = ½ + t\sqrt{-1}$, where t is a real number – these complex numbers form a line in the complex plane, called the 'critical line'. The RH, first put forward by Bernhard Riemann in 1859, is known to be true for the first 10^{13} non-trivial zeros, but remains unproven. "The RH is widely regarded as the most famous unsolved problem in mathematics. It was one of the 23 famous problems selected by [David] Hilbert in 1900 as among the most important in mathematics, and it is one of the seven Millennium Problems selected by the Clay Mathematics Institute in 2000 as the most important for the 21st century" (Hedjhal & Odlyzko, p. 266).

"The first computations of zeros of the zeta function were performed by Riemann, and likely played an important role in his posing of the RH as a result likely to be true. His computations were carried out by hand, using an advanced method that is known today as the Riemann-Siegel formula. Both the method and Riemann's computations that utilized it remained unknown to the world-at-large until the early 1930s, when they were found in Riemann's unpublished papers by C. L. Siegel … In the mid-1930s, after Siegel's publication of the Riemann-Siegel formula, [the Oxford mathematician] E. C. Titchmarsh obtained a grant for a larger computation. With the assistance of L. J. Comrie, tabulating machines, some 'computers' (as the mostly female operators of such machinery were called in those days), and the recently published algorithm, Titchmarsh established that the 1041 nontrivial zero with $0 < t < 1468$ all satisfied the RH" (*ibid*., p. 268).

"Turing encountered the Riemann zeta function as a student, and developed a life-long fascination with it. Though his research in this area was not a major thrust of his career, he did make a number of pioneering contributions" (*ibid*., p. 266). "Apparently he had decided that the Riemann hypothesis was probably false, if only because such great efforts had failed to prove it. Its falsity would mean that the zeta-function *did* take the value zero at some point which was off the special line, in which case this point could be located by brute force, just by calculating enough values of the zeta-function … There were two aspects to the problem. Riemann's zeta-function was defined as the sum of an infinite number of terms, and although this sum could be re-expressed in many different ways, any attempt to evaluate it would in some way involve making an approximation. It was for the mathematician to find a good approximation, and to prove that it was good: that the error involved was sufficiently small. Such work did not involve computation with numbers, but required highly technical work with the calculus of complex numbers. Titchmarsh had employed a certain approximation which – rather romantically – had been exhumed from Riemann's own papers at Göttingen where it had lain for seventy years. But for extending the calculation to thousands of new zeroes a fresh approximation was required; and this Alan set out to find and to justify.

"The second problem, quite different, was the 'dull and elementary' one of actually doing the computation, with numbers substituted into the approximate formula, and worked out for thousands of different entries. It so happened that the formula was rather like those which occurred in plotting the positions of the planets, because it was of the form of a sum of circular functions with different frequencies. It was for this reason that Titchmarsh had contrived to have the dull repetitive work of addition, multiplication, and of looking up of entries in cosine tables done by the same punched-card methods that were used in planetary astronomy. But it occurred to Alan that the problem was very similar to another kind of computation which was also done on a large practical scale – that of tide prediction. Tides could also be regarded as the sum of a number of waves of different periods: daily, monthly, yearly oscillations of rise and fall. At Liverpool there was a machine which performed the summation automatically, by generating circular motions of the right frequencies and adding them up. It was a simple analogue machine; that is, it created a physical analogue of the mathematical function that had to be calculated. This was a quite different idea from that of the Turing machine, which would work away on a finite, discrete,



180 A. M. TURING [March 16,

A METHOD FOR THE CALCULATION OF THE ZETA-FUNCTION

By A. M. TURING.

[Received 7 March, 1939.—Read 16 March, 1939.]

An asymptotic series for the zeta-function was found by Riemann and has been published by Siegel*, and applied by Titchmarsh† to the calculation of the approximate positions of some of the zeros of the function. It is difficult to obtain satisfactory estimates for the remainders with this asymptotic series, as may be seen from the first of these two papers of Titchmarsh, unless t is very large. In the present paper a method of calculation will be described, which, like the asymptotic formula, is based on the approximate functional equation; it is applicable for all values of s. It is likely to be most valuable for a range of t where t is neither so small that the Euler-Maclaurin summation method can be used (*e.g.* $t > 30$) nor large enough for the Riemann-Siegel asymptotic formula (*e.g.* $t < 1000$).

Roughly speaking, the method is to use the approximate functional equation for the zeta-function, with the remainder expressed as an integral, which for the moment we write as $\int_{-\infty}^{\infty} h(x)\,dx$. We approximate to the integral by the obvious sum $\sum_{k=-K}^{K} \frac{1}{\kappa} h\left(\frac{k}{\kappa}\right)$ and we find that, if certain modifications are made in the "main series", this gives a remarkably accurate result; when the number of terms taken is $T = 2K+1$ the error is of the order of magnitude of $e^{-\frac{1}{4}\kappa T}$. The theta-functions give another case of this phenomenon. We have the identity

$$\sum_{n=-\infty}^{\infty} e^{-\pi n^2 x} = \frac{1}{\sqrt{x}} \sum_{n=-\infty}^{\infty} e^{-\pi n^2/x}$$

* C. L. Siegel, "Über Riemanns Nachlass zur analytischen Zahlentheorie", *Quell. Gesch. Math.*, B, 2 (1931), 45–80.

† E. C. Titchmarsh, "The zeros of the Riemann zeta-function", *Proc. Royal Soc.* (A), 151 (1935), 234–255; also 157 (1936), 261–263.

set of symbols. This tide-predicting machine, like a slide rule, depended not on symbols, but on the measurement of lengths. Such a machine, Alan had realised, could be used on the zeta-function calculation, to save the dreary work of adding, multiplying, and looking up of cosines.

"Alan must have described this idea to Titchmarsh, for a letter from him dated 1 December 1937 approved of this programme of extending the calculation, and mentioned: 'I have seen the tide-predicting machine at Liverpool, but it did not occur to me to use it in this way' (Hodges, pp. 140-2).

"On 24 March [1939] he applied to the Royal Society for a grant to cover the cost of constructing it, and on their questionnaire wrote, 'Apparatus would be of little permanent value. It could be added to for the purpose of carrying out similar calculations for a wider range of *t* and might be used for some other investigations connected with the zeta-function. I cannot think of any applications that would not be connected with the zeta-function.' Hardy and Titchmarsh were quoted as referees for the application, which won the requested £40. The idea was that although the machine could not perform the required calculation exactly, it could locate the places where the zeta-function took a value near zero, which could then be tackled by a more exact hand computation. Alan reckoned it would reduce the amount of work by a factor of fifty. Perhaps as important, it would be a good deal more fun" (Hodges, pp. 155-6).

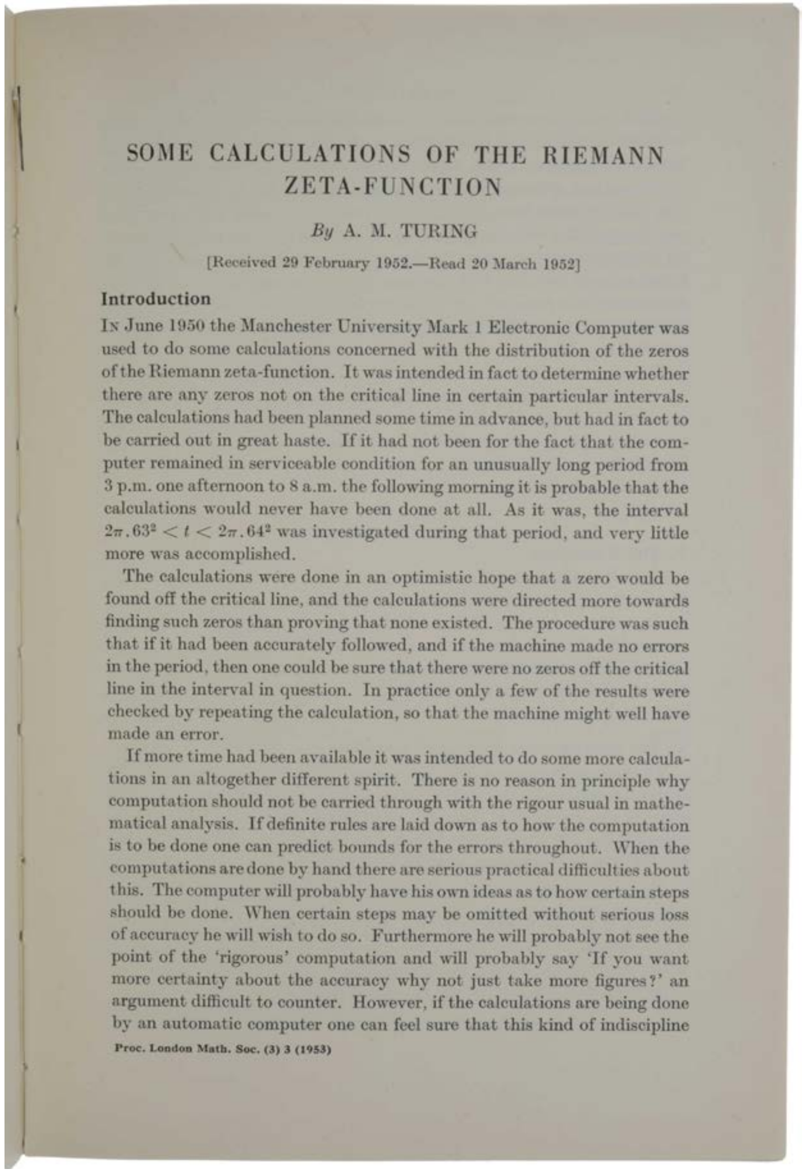
Turing started to work on the construction of his zeta-function machine, but the work was interrupted by the outbreak of World War II, and the machine was never built. "We do not know how well Turing's zeta function machine would have worked, had it been built. At least one special zeta function computer was constructed to a different design later by van der Pol (1947). By that time, though, electronic digital computers were becoming available, and Turing was the first one to utilise them to investigate the zeta function" (Hedjhal & Odlyzko, p. 268).

Soon after his involvement in the war effort ended, Turing set about plans for a general-purpose digital computer. He submitted a detailed design for the Automatic Computing Engine (ACE) to the National Physical Laboratory in early 1946. Turing's design drew on both his theoretical work 'On Computable Numbers' from a decade earlier, and the practical knowledge gained during the war from working at Bletchley Park, where the Colossus machines were developed and used. But there were several delays in realizing Turing's plans. The existence and capabilities of the Colossus machines were classified top-secret for decades after the war, so Turing was forbidden from disclosing what he already knew to be achievable. Even a scaled-down plan for a Pilot ACE met with so many bureaucratic delays that Turing resigned his post at the NPL and moved in late 1948 to Manchester at the invitation of his former lecturer at Cambridge, Max Newman. The Manchester Mark I was operational a few months after Turing's arrival, and Newman and Turing looked for mathematical problems the new computer could help to solve.

"In 1950, he used the Manchester Mark I Electronic Computer to extend the Titchmarsh verification of the RH to the first 1104 zeros of the zeta function, the ones with $0 < t < 1540$. This was a very small extension, but it represented a triumph of perseverance over a promising new technology that was still suffering from teething problems. In Turing's words: '[I]f it had not been for the fact that the computer remained in serviceable condition for an unusually long period from 3pm one afternoon to 8am the following morning it is probable that the calculations would never have been done at all.' These days, when even our simple consumer devices have gigabytes of memory, it is instructive to recall that the machine available to Turing had a grand total of 25,600 bits of memory and that Turing worked directly output 'punched on a teleprint tape' in base 32. That Turing stayed up all through the night conveys some idea of how interesting he found this experiment" (*ibid.*).

The first 10 trillion zeros of the zeta function have been found to obey the RH, as has the 10^{32}nd zero and hundreds of its neighbours; all such calculations continue to rely on Turing's method as an essential ingredient. In recent decades computers have come to play an increasingly important role in other mathematical proofs: one has only to note the computer-assisted proofs of the 'Four colour theorem' and 'Kepler's conjecture'. Such proofs have proved controversial, but Turing's view was expressed clearly in the 1953 paper: 'If definite rules are laid down as to how the computation is to be done one can predict bounds for the errors throughout. When the computations are done by hand there are serious practical difficulties about this. The [human] computer will probably have his own ideas as to how certain steps should be done … However, if the calculations are being done by an automatic computer one can feel sure that this kind of indiscipline does not occur.'

Downey (ed.), *Turing's Legacy*: *Developments from Turing's Ideas in Logic* (2014); Hedjhal & Odlyzko, 'Alan Turing and the Riemann zeta function,' pp. 265-279 in Cooper & van Leeuwen (eds.), *Alan Turing*: *His Work and Impact* (2013); Hodges, *Alan Turing*: *the Enigma* (1983).



SOME CALCULATIONS OF THE RIEMANN ZETA-FUNCTION

By A. M. TURING

[Received 29 February 1952.—Read 20 March 1952]

Introduction

In June 1950 the Manchester University Mark 1 Electronic Computer was used to do some calculations concerned with the distribution of the zeros of the Riemann zeta-function. It was intended in fact to determine whether there are any zeros not on the critical line in certain particular intervals. The calculations had been planned some time in advance, but had in fact to be carried out in great haste. If it had not been for the fact that the computer remained in serviceable condition for an unusually long period from 3 p.m. one afternoon to 8 a.m. the following morning it is probable that the calculations would never have been done at all. As it was, the interval $2\pi . 63^2 < t < 2\pi . 64^2$ was investigated during that period, and very little more was accomplished.

The calculations were done in an optimistic hope that a zero would be found off the critical line, and the calculations were directed more towards finding such zeros than proving that none existed. The procedure was such that if it had been accurately followed, and if the machine made no errors in the period, then one could be sure that there were no zeros off the critical line in the interval in question. In practice only a few of the results were checked by repeating the calculation, so that the machine might well have made an error.

If more time had been available it was intended to do some more calculations in an altogether different spirit. There is no reason in principle why computation should not be carried through with the rigour usual in mathematical analysis. If definite rules are laid down as to how the computation is to be done one can predict bounds for the errors throughout. When the computations are done by hand there are serious practical difficulties about this. The computer will probably have his own ideas as to how certain steps should be done. When certain steps may be omitted without serious loss of accuracy he will wish to do so. Furthermore he will probably not see the point of the 'rigorous' computation and will probably say 'If you want more certainty about the accuracy why not just take more figures?' an argument difficult to counter. However, if the calculations are being done by an automatic computer one can feel sure that this kind of indiscipline

Proc. London Math. Soc. (3) 3 (1953)

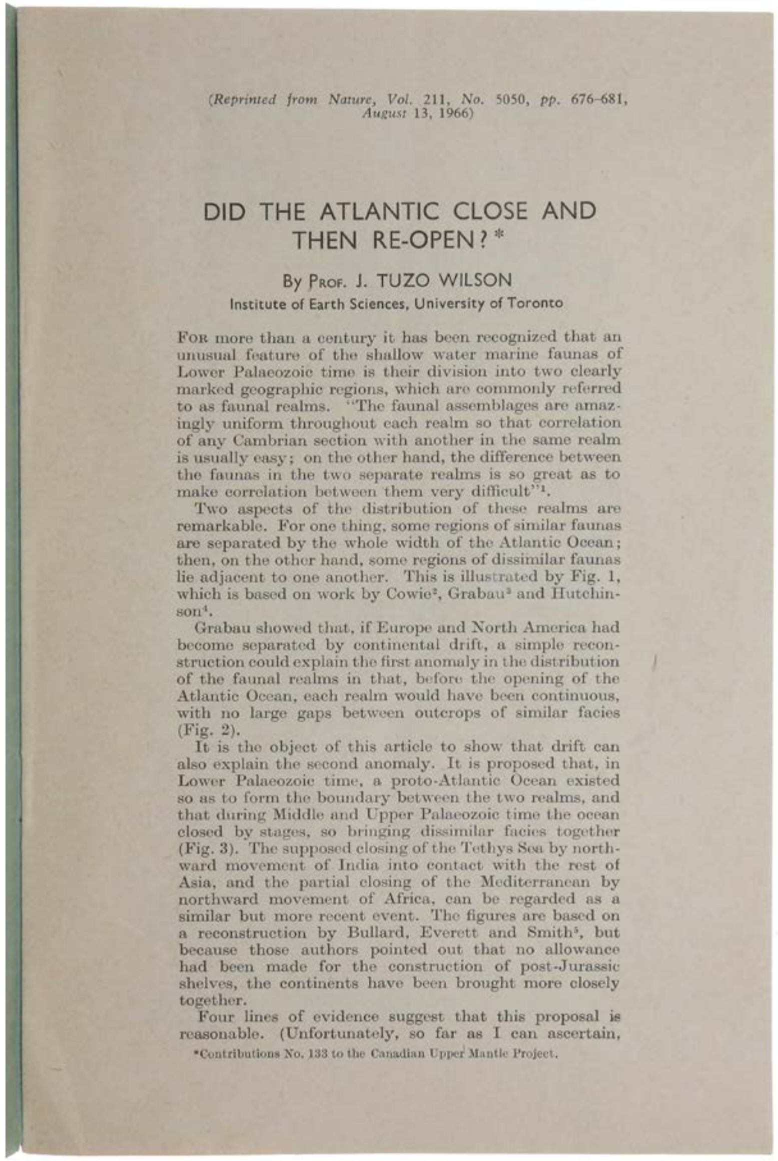
CONTINENTAL DRIFT

TUZO WILSON, John. *Did The Atlantic Close And Then Re-Open? Offprint from: Nature, Vol. 211, No. 5050, August 13, 1966.* London: Macmillan, 1966.

$2,850

8vo (213 x 140 mm), pp. [1] 2-15 [16]. Original light blue printed wrappers.

First edition, very rare offprint, of this landmark paper elucidating the history and mechanism of continental drift by "one of the most imaginative Earth scientists of his generation" (DSB). "In 1966, J. Tuzo Wilson published 'Did the Atlantic Close and then Re-Open?' in the journal *Nature*. The Canadian author introduced to the mainstream the idea that continents and oceans are in continuous motion over our planet's surface. Known as plate tectonics, the theory describes the large-scale motion of the outer layer of the Earth. It explains tectonic activity (things like earthquakes and the building of mountain ranges) at the edges of continental landmasses (for instance, the San Andreas Fault in California and the Andes in South America)" (Heron). Alfred Wegener (1880-1930) had already suggested in the early 1900s that continents move around the surface of the earth, specifically that there had been a super-continent (Pangaea) where now there is a great ocean (the Atlantic). In the present paper, Wilson explained the geological evidence that North America and Europe were once separated across an ocean *before* the Atlantic Ocean. This ocean closed in stages as the continents that used to be separated by the ocean converged by subduction and eventually collided in a mountain-building event. The combined continent was then sliced apart and the continents drawn away from each other once more as the modern Atlantic Ocean opened. The paper combined the nascent ideas of divergent and convergent plate



(Reprinted from *Nature*, Vol. 211, No. 5050, pp. 676–681, August 13, 1966)

DID THE ATLANTIC CLOSE AND THEN RE-OPEN?*

By PROF. J. TUZO WILSON

Institute of Earth Sciences, University of Toronto

FOR more than a century it has been recognized that an unusual feature of the shallow water marine faunas of Lower Palaeozoic time is their division into two clearly marked geographic regions, which are commonly referred to as faunal realms. "The faunal assemblages are amazingly uniform throughout each realm so that correlation of any Cambrian section with another in the same realm is usually easy; on the other hand, the difference between the faunas in the two separate realms is so great as to make correlation between them very difficult"[1].

Two aspects of the distribution of these realms are remarkable. For one thing, some regions of similar faunas are separated by the whole width of the Atlantic Ocean; then, on the other hand, some regions of dissimilar faunas lie adjacent to one another. This is illustrated by Fig. 1, which is based on work by Cowie[2], Grabau[3] and Hutchinson[4].

Grabau showed that, if Europe and North America had become separated by continental drift, a simple reconstruction could explain the first anomaly in the distribution of the faunal realms in that, before the opening of the Atlantic Ocean, each realm would have been continuous, with no large gaps between outcrops of similar facies (Fig. 2).

It is the object of this article to show that drift can also explain the second anomaly. It is proposed that, in Lower Palaeozoic time, a proto-Atlantic Ocean existed so as to form the boundary between the two realms, and that during Middle and Upper Palaeozoic time the ocean closed by stages, so bringing dissimilar facies together (Fig. 3). The supposed closing of the Tethys Sea by northward movement of India into contact with the rest of Asia, and the partial closing of the Mediterranean by northward movement of Africa, can be regarded as a similar but more recent event. The figures are based on a reconstruction by Bullard, Everett and Smith[5], but because those authors pointed out that no allowance had been made for the construction of post-Jurassic shelves, the continents have been brought more closely together.

Four lines of evidence suggest that this proposal is reasonable. (Unfortunately, so far as I can ascertain,

*Contributions No. 133 to the Canadian Upper Mantle Project.

boundaries into a conceptual model that matched observations of geological features around the world. The tectonic cycle he described now goes by 'the Wilson Cycle' or the 'Supercontinent Cycle' and still governs how we think of the evolution of tectonic plates through time. "Wilson's great idea was a crucial step forward. It reopened the whole question of 'what happened before Pangea?' By suggesting that his 'proto-Atlantic' had opened within an earlier supercontinent (just as the Atlantic did within Pangea) he also linked his process to a grander cycle leading from one supercontinent Earth to another" (Nield). As was often the case for offprints from *Nature* (e.g., the famous Watson/Crick DNA offprint), this offprint is printed in a smaller format than the journal issue, with the text reset. No copies in auction records or on OCLC.

In the early twentieth century the prevailing wisdom regarding how mountain belts were formed and why the sea is deep was that the Earth started out as a molten ball and gradually cooled. When it cooled, heavier metals such as iron sank down and formed the core, while lighter metals such as aluminium stayed up in the crust. The cooling also caused contraction and the pressure produced by contraction caused some parts of the crust to buckle upwards, forming mountains, while other parts of the crust buckled downwards, creating ocean basins.

"Originally a devotee of the contracting-Earth hypothesis, [Tuzo Wilson] became a convert to [continental] drift as he was entering his fifties (by which time he had been Professor of Geophysics at Toronto for a decade). Swiftly recanting his former views, Tuzo saw the way the Earth's mountain belts were often superimposed upon one another, and set about explaining it in terms of plate tectonics. In a classic paper published in *Nature* in 1966 and titled 'Did the Atlantic close and then reopen?' he addressed the coincidence of the modern Atlantic with two mountain ranges called the Caledonides in Europe and the Appalachians in the USA. It was the very first time the new plate tectonics had been extended back to the pre-Pangean Earth. "These two mountain ranges are really one and the same – except that they are now separated by the Atlantic Ocean, which cut the range in two at a low angle when it opened between them. At one time the two belts had been joined, end-to-end, Caledonides in the north, Appalachians in the south; and the collision that had created them was one event among many that built the supercontinent Pangea. Indeed, the matching of the now separated halves of this once-mighty chain provided Wegener with one of his key 'proofs' – part of his geological matching of opposing Atlantic shores …

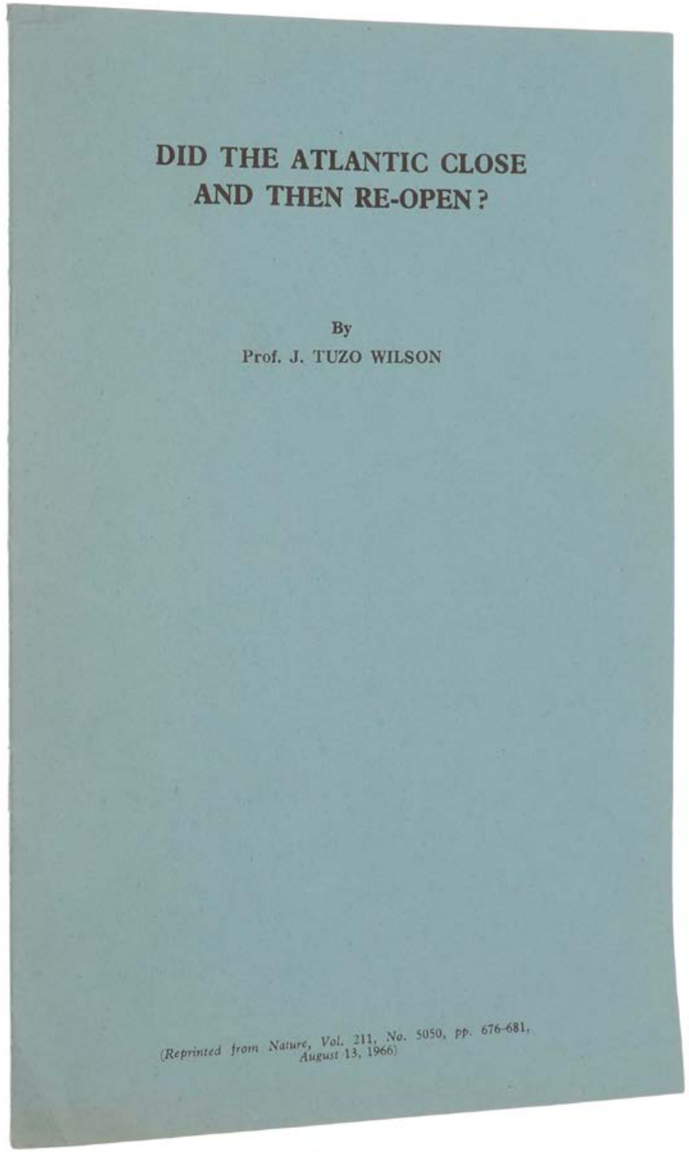
"Wegener did not speculate about how his Pangea had come together. But as the new plate tectonics emerged from studies of the ocean floor and began to revitalize drift theory, the time was ripe to see the break-up of Pangea as part of a bigger process. Professor Kevin Burke of the University of Houston, Texas, recalls that on 12 April 1968 in Philadelphia, at a meeting titled 'Gonwanaland Revisited' at the Philadelphia Academy of Sciences, Wilson told his audience how a map of the world showed you oceans opening in some places and closing in others. Burke recalls: 'He therefore suggested that, because the ocean basins make up the largest areas on the Earth's surface, it would be appropriate to interpret Earth history in terms of the life cycles of the opening and closing of the ocean basins … In effect he said: for times before the present oceans existed, we cannot do plate tectonics. Instead we must consider the life cycles of the ocean basins.' This key insight had by then already provided Wilson with the answer to an abiding puzzle in the rocks from either side of the modern Atlantic.

"Nothing pleased Tuzo more than a grand, overarching framework that made sense of those awkward facts that get thrown aside because they don't fit – ideas that philosopher William James dubbed the 'unclassified residuum.' Geologists had been aware since 1889 that within the rocks forming the Caledonian and Appalachian mountains – that is, rocks dating from the early Cambrian to about the middle Ordovician (from 542 to 470 million years ago) – were fossils that fell into two clearly different groups or 'assemblages.' This was especially true for fossils of those animals that in life never travelled far, but lived fixed to, or grubbing around in, the seabed. By analogy with modern zoology, the two assemblages represented two different faunal realms, just like those first described on the modern Earth by Philip Lutley Sclater (1829-1913) and Alfred Russel Wallace (1823-1913).

"These two ancient realms were found to broadly parallel the shores of the modern Atlantic Ocean and were described by Charles Doolittle Walcott (1850-1927) … He named these assemblages the 'Pacific' and 'Atlantic' provinces, rocks in North America containing the Pacific assemblage, and rocks of the same age in Europe the Atlantic.

"Had this split been perfect it would have raised no eyebrows among continental fixists because the division would have been easily explained by the present arrangement of continents and oceans. Unfortunately there were some distinctly awkward exceptions to the rule. In some places in Europe, such as the north of Scotland, geologists found rocks with typical 'American' fossils in them, while in some places in North America rocks turned up containing typical European species …

"This conundrum could be explained, Wilson reasoned, if the present Atlantic Ocean was not the first to have separated its opposing shores: if there had been an older Atlantic, which had closed and then reopened to form the modern one. According to his idea, the old Caledonian-Appalachian mountain chain had formed as the vice shut for the first time, eliminating a now long-vanished ocean that Wilson called the 'proto-Atlantic.' But when this suture had reopened,



more or less (but not perfectly) along the same line, some of the rocks squeezed between the forelands had stuck to the opposite jaw of the vice, stranding some American fossils of the European side and vice versa. The fossil distributions were saying that there had been continental drift *before* Pangea. Moreover, if this particular example could be extended into a general rule, mountain building itself was inherently cyclic. This process, involving the repeated opening and closing of oceans along ancient lines of suture, has since come to be known as the Wilson Cycle, a term first used in print in 1974 by Kevin Burke and the British geologist John Dewey …

"It soon turned out that Wilson's 'proto-Atlantic' had in fact been sitting right at the bottom of the world. Before 'our' Atlantic had opened, the two jaws of the vice (now represented by North America and Eurasia) had not only opened and closed (and thus helped build Pangea) but had since migrated north together as far as the Tropic of Cancer before deciding to reopen hundreds of millions of years later, in the great Pangean split-up.

"Wilson's name for this ancient vanished ocean, the 'proto-Atlantic', soon came to seem inappropriate, particularly since the same name was coming to be used for the early stages of the formation of the *modern* Atlantic. Wilson's ocean had been squeezed out of existence by about 400 million years ago: 200 million years before the present Atlantic had even begun to form within Pangea; so it was no true 'proto-Atlantic' in any real sense. Therefore, in 1972, Wilson's ocean was renamed Iapetus, which maintains a shadow of the Atlantic link, since in Greek myth Iapetus, son of Earth (Ge) and Heaven (Uranos), was brother to Tethys and Okeanos, and father of the Titan, Atlas" (Nield).

"Tuzo was one of those charismatic, larger-than-life people whose entry into a room caused heads to turn and conversations to stop. Your eyes went to him; you felt your spirits lifting. His school in Ottawa had made him head boy, and he kept the position for the rest of his life. With his resonant voice he compelled your attention and persuaded you – often against your will – that he was not only right about this but pretty much right about everything (which, by and large, he was). A positive man, not given to regrets, he would have been brilliant, you felt, at whatever career he had followed, especially, perhaps, politics; and as though to show off his wide-ranging facility, he was also a published expert on antique Chinese porcelain. But global tectonics was his passion, and the plate-tectonic revolution was made for him. It was also very largely made *by* him" (*ibid*.).

"The son of a Scottish engineer who had immigrated to Canada, Wilson (1908-93) in 1930 became the first person at any Canadian university to graduate in geophysical studies (B.A., Trinity College, University of Toronto). He then studied at St. John's College, Cambridge (B.A., 1932), Princeton University (Ph.D., 1936), and Cambridge University (M.A., 1940; Sc.D., 1958). He worked with the Geological Survey of Canada (1936–39) and served with the Royal Canadian Engineers during World War II, rising to the rank of colonel. After the war, in 1946, Wilson became professor of geophysics at the University of Toronto, where he remained until 1974, when he became director general of the Ontario Science Centre. From 1983 to 1986 he was chancellor of York University. He was president of both the Royal Society of Canada (1972–73) and the American Geophysical Union (1980–82)" (Britannica).

Heron, 'Plate tectonics: new findings fill out the 50-year-old theory that explains Earth's landmasses,' *The Conversation*, July 5, 2016. Nield, *Supercontinent*: 10 *Billion Years In The Life Of Our Planet*, 2012.

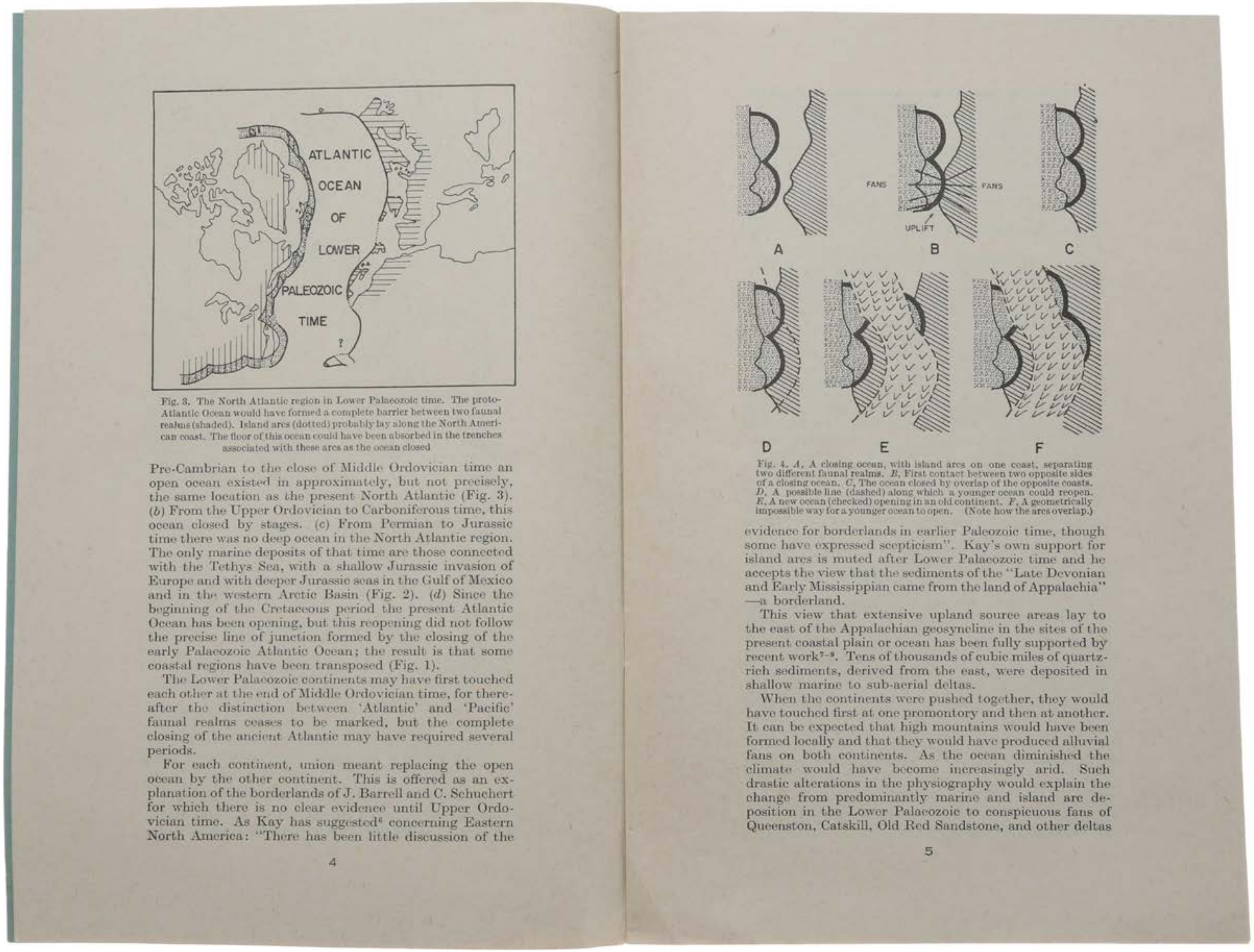


Fig. 3. The North Atlantic region in Lower Palaeozoic time. The proto-Atlantic Ocean would have formed a complete barrier between two faunal realms (shaded). Island arcs (dotted) probably lay along the North American coast. The floor of this ocean could have been absorbed in the trenches associated with these arcs as the ocean closed

Pre-Cambrian to the close of Middle Ordovician time an open ocean existed in approximately, but not precisely, the same location as the present North Atlantic (Fig. 3). (*b*) From the Upper Ordovician to Carboniferous time, this ocean closed by stages. (*c*) From Permian to Jurassic time there was no deep ocean in the North Atlantic region. The only marine deposits of that time are those connected with the Tethys Sea, with a shallow Jurassic invasion of Europe and with deeper Jurassic seas in the Gulf of Mexico and in the western Arctic Basin (Fig. 2). (*d*) Since the beginning of the Cretaceous period the present Atlantic Ocean has been opening, but this reopening did not follow the precise line of junction formed by the closing of the early Palaeozoic Atlantic Ocean; the result is that some coastal regions have been transposed (Fig. 1).

The Lower Palaeozoic continents may have first touched each other at the end of Middle Ordovician time, for thereafter the distinction between 'Atlantic' and 'Pacific' faunal realms ceases to be marked, but the complete closing of the ancient Atlantic may have required several periods.

For each continent, union meant replacing the open ocean by the other continent. This is offered as an explanation of the borderlands of J. Barrell and C. Schuchert for which there is no clear evidence until Upper Ordovician time. As Kay has suggested[6] concerning Eastern North America: "There has been little discussion of the

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Fig. 4. *A*, A closing ocean, with island arcs on one coast, separating two different faunal realms. *B*, First contact between two opposite sides of a closing ocean. *C*, The ocean closed by overlap of the opposite coasts. *D*, A possible line (dashed) along which a younger ocean could reopen. *E*, A new ocean (checked) opening in an old continent. *F*, A geometrically impossible way for a younger ocean to open. (Note how the arcs overlap.)

evidence for borderlands in earlier Paleozoic time, though some have expressed scepticism". Kay's own support for island arcs is muted after Lower Palaeozoic time and he accepts the view that the sediments of the "Late Devonian and Early Mississippian came from the land of Appalachia"—a borderland.

This view that extensive upland source areas lay to the east of the Appalachian geosyncline in the sites of the present coastal plain or ocean has been fully supported by recent work[7–9]. Tens of thousands of cubic miles of quartz-rich sediments, derived from the east, were deposited in shallow marine to sub-aerial deltas.

When the continents were pushed together, they would have touched first at one promontory and then at another. It can be expected that high mountains would have been formed locally and that they would have produced alluvial fans on both continents. As the ocean diminished the climate would have become increasingly arid. Such drastic alterations in the physiography would explain the change from predominantly marine and island arc deposition in the Lower Palaeozoic to conspicuous fans of Queenston, Catskill, Old Red Sandstone, and other deltas

5

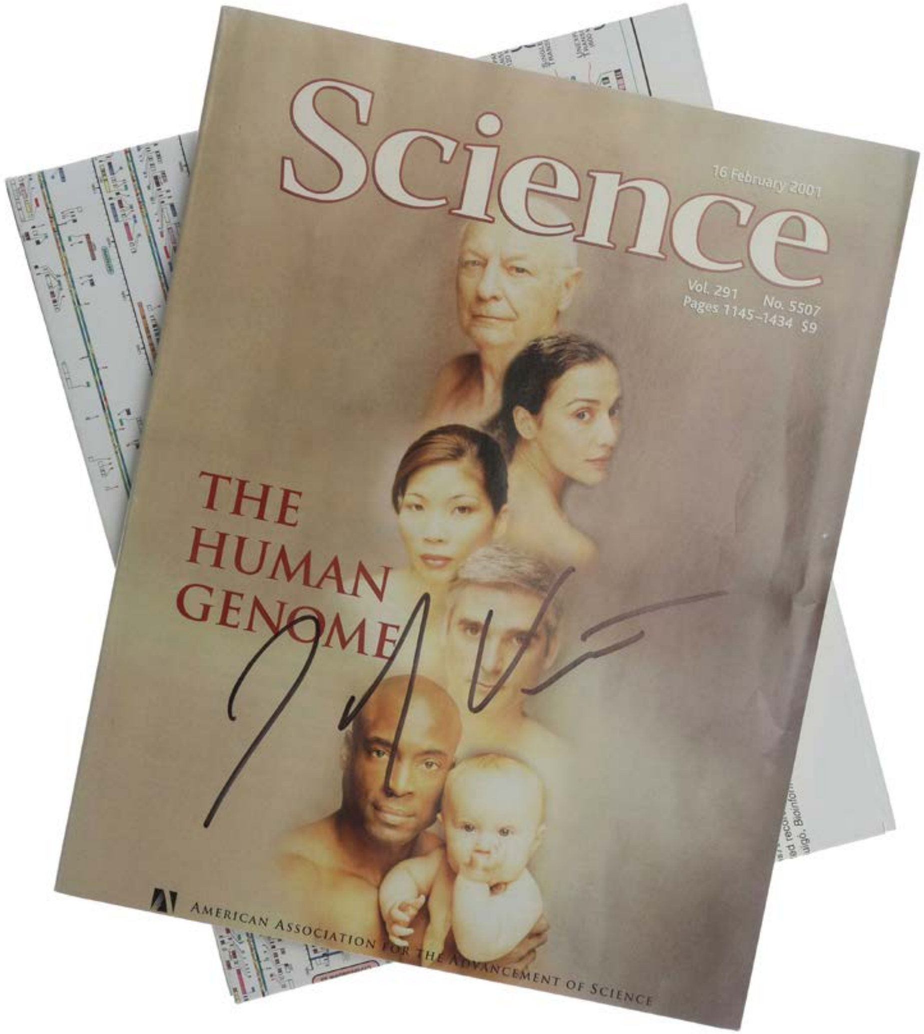
SEQUENCING THE HUMAN GENOME

VENTER, J. Craig, et al. *The sequence of the human genome.* 2001.

$2,500

Pp. 1304-51 in: Science, vol. 291, no. 5507, February 16, 2001. 4to, pp. 1155-1369. Original printed wrappers, signed by Venter on front wrapper, with the very large folding chart 'Annotation of the Celera Human Genome Assembly'.

First edition, journal issue in the original printed wrappers, **signed by Craig Venter**, of the first published announcement of Celera Genomics' sequencing of the human genome. The problem of finding the order of the building blocks of the nucleic acids that make up the entire genetic material of a human was first proposed in 1985, but it was not until 1990 that the Human Genome Project (HGP) was officially initiated in the United States under the direction of the National Institutes of Health (NIH) and the U.S. Department of Energy with a 15-year, $3 billion plan for sequencing the entire human genome composed of 2.9 billion base pairs. Other countries such as Japan, Germany, the United Kingdom, France, and China also contributed to the global sequencing effort. Venter was a scientist at the NIH during the early 1990s when the project was initiated. In 1998 his company Celera announced its intention to build a unique genome sequencing facility, to determine the sequence of the human genome over a 3-year period. The Celera approach to genome sequencing was very different from the map-based public efforts. They proposed to use 'shotgun sequencing' (sequencing of DNA that has been randomly fragmented into pieces) of the genome, subsequently putting it together. This approach was widely criticized but was shown to be successful after Celera sequenced the genome of the fruit fly *Drosophila melanogaster* in 2000 using this method. The Celera effort was able to proceed at a much more rapid



rate, and about 10% of the cost, of the HGP because it relied upon data made available by the publicly funded project. Venter announced in April 2000 that his group had finished sequencing the human genome during testimony before Congress on the future of the HGP, a full three years before that project had been expected to be complete. Venter's article 'The Sequence of the Human Genome' was published in *Science* ten months later. The publicly funded HGP reported their findings one day earlier in *Nature*, thus preventing Celera from patenting the genetic information. Venter was listed on *Time* magazine's 2007 and 2008 'Time 100' list of the most influential people in the world, and in 2008 he received the National Medal of Science from President Obama. We are not aware of any other copy of this historic article signed by Venter having appeared on the market.

When the HGP was begun in 1990, it was far too expensive to sequence the complete human genome. The National Institutes of Health therefore adopted a 'shortcut', which was to look just at sites on the genome where many people have a variant DNA unit. The genome was broken into smaller pieces, approximately 150,000 base pairs in length. These pieces were then ligated into a type of vector known as 'bacterial artificial chromosomes', which are derived from bacterial chromosomes which have been genetically engineered. The vectors containing the genes can be inserted into bacteria where they are copied by the bacterial DNA replication machinery. Each of these pieces was then sequenced separately as a small 'shotgun' project and then assembled. The larger, 150,000 base pairs go together to create chromosomes. This is known as the 'hierarchical shotgun' approach, because the genome is first broken into relatively large chunks, which are then mapped to chromosomes before being selected for sequencing. Celera used a technique called 'whole genome shotgun sequencing,' employing pairwise end sequencing, which had been used to sequence bacterial genomes of up to six million base pairs in length, but not for anything nearly as large as the three billion base pair human genome.

Celera initially announced that it would seek patent protection on 'only 200–300' genes, but later amended this to seeking 'intellectual property protection' on 'fully-characterized important structures' amounting to 100–300 targets. The firm eventually filed preliminary ('place-holder') patent applications on 6,500 whole or partial genes. Celera also promised to publish their findings in accordance with the terms of the 1996 'Bermuda Statement', by releasing new data annually (the HGP released its new data daily), although, unlike the publicly funded project, they would not permit free redistribution or scientific use of the data. The publicly funded competitors were compelled to release the first draft of the human genome before Celera for this reason.

Special issues of *Nature* (which published the publicly funded project's scientific paper) and *Science* (which published Celera's paper) described the methods used to produce the draft sequence and offered analysis of the sequence. These drafts covered about 83% of the genome (90% of the euchromatic regions with 150,000 gaps and the order and orientation of many segments not yet established). In February 2001, at the time of the joint publications, press releases announced that the project had been completed by both groups. Improved drafts were announced in 2003 and 2005, filling in approximately 92% of the sequence.

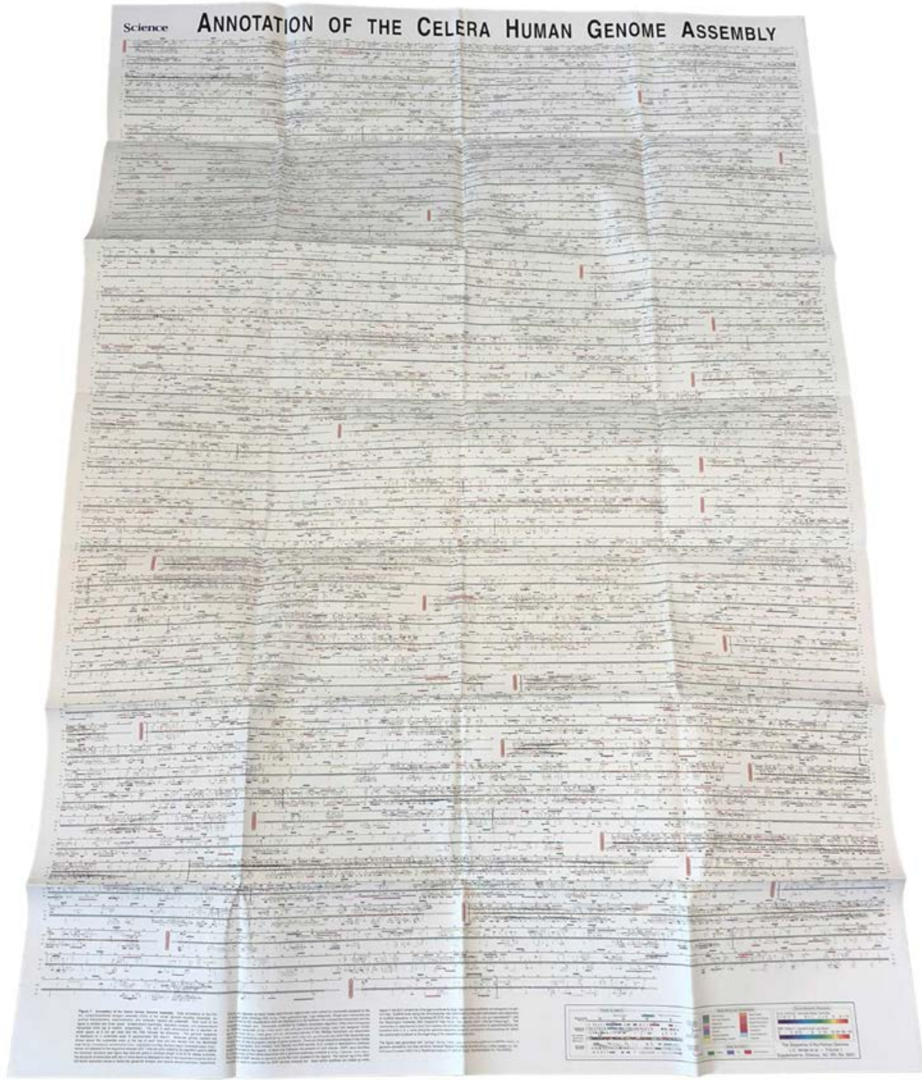
In the publicly funded HGP, researchers collected blood (female) or sperm (male) samples from a large number of donors. Only a few of many collected samples were processed as DNA resources. Thus the donor identities were protected so neither donors nor scientists could know whose DNA was sequenced. In the Celera project, DNA from five different individuals was used for sequencing. Venter later acknowledged (in a public letter to *Science*) that his DNA was one of 21 samples in the pool, five of which were selected for use.

"The work on interpretation and analysis of genome data is still in its initial stages.

It is anticipated that detailed knowledge of the human genome will provide new avenues for advances in medicine and biotechnology. Clear practical results of the project emerged even before the work was finished. For example, a number of companies, such as Myriad Genetics, started offering easy ways to administer genetic tests that can show predisposition to a variety of illnesses, including breast cancer, hemostasis disorders, cystic fibrosis, liver diseases and many others. Also, the etiologies for cancers, Alzheimer's disease and other areas of clinical interest are considered likely to benefit from genome information and possibly may lead in the long term to significant advances in their management.

"There are also many tangible benefits for biologists. For example, a researcher investigating a certain form of cancer may have narrowed down their search to a particular gene. By visiting the human genome database on the World Wide Web, this researcher can examine what other scientists have written about this gene, including (potentially) the three-dimensional structure of its product, its function(s), its evolutionary relationships to other human genes, or to genes in mice or yeast or fruit flies, possible detrimental mutations, interactions with other genes, body tissues in which this gene is activated, and diseases associated with this gene or other data types. Further, deeper understanding of the disease processes at the level of molecular biology may determine new therapeutic procedures. Given the established importance of DNA in molecular biology and its central role in determining the fundamental operation of cellular processes, it is likely that expanded knowledge in this area will facilitate medical advances in numerous areas of clinical interest that may not have been possible without them.

"The analysis of similarities between DNA sequences from different organisms is also opening new avenues in the study of evolution. In many cases, evolutionary questions can now be framed in terms of molecular biology; indeed, many major evolutionary milestones (the emergence of the ribosome and organelles,



the development of embryos with body plans, the vertebrate immune system) can be related to the molecular level. Many questions about the similarities and differences between humans and our closest relatives (the primates, and indeed the other mammals) are expected to be illuminated by the data in this project.

"The project inspired and paved the way for genomic work in other fields, such as agriculture. For example, by studying the genetic composition of *Tritium aestivum*, the world's most commonly used bread wheat, great insight has been gained into the ways that domestication has impacted the evolution of the plant. Which loci are most susceptible to manipulation, and how does this play out in evolutionary terms? Genetic sequencing has allowed these questions to be addressed for the first time, as specific loci can be compared in wild and domesticated strains of the plant. This will allow for advances in genetic modification in the future which could yield healthier, more disease-resistant wheat crops" (Wikipedia, accessed 4 June, 2018).

After high school, John Craig Venter (b. 1946) "joined the U.S. Naval Medical Corps and served in the Vietnam War. On returning to the U.S., he earned a B.A. in biochemistry (1972) and then a doctorate in physiology and pharmacology (1975) at the University of California, San Diego. In 1976 he joined the faculty of the State University of New York at Buffalo, where he was involved in neurochemistry research. In 1984 Venter moved to the National Institutes of Health (NIH), in Bethesda, MD, and began studying genes involved in signal transmission between neurons.

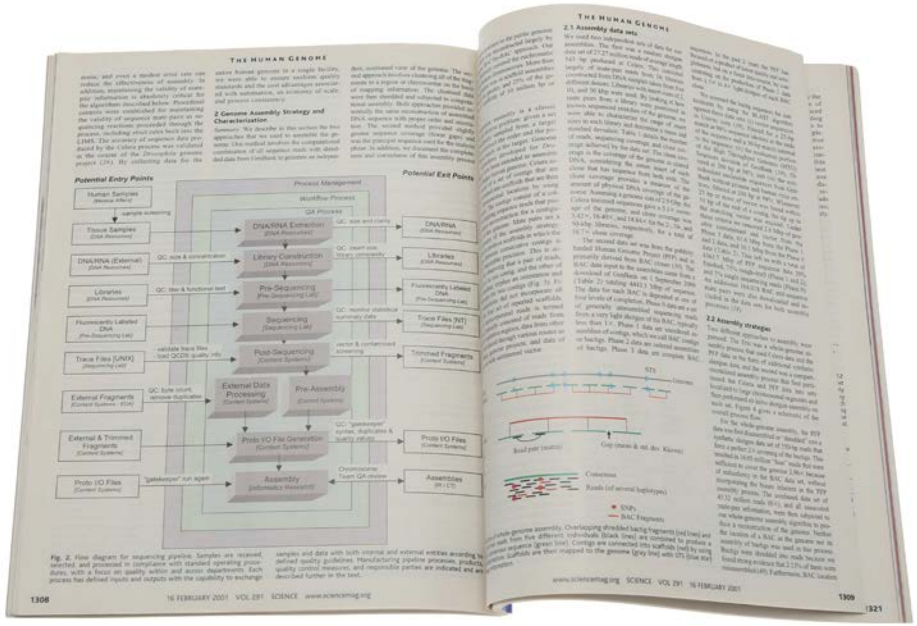
"While at the NIH, Venter became frustrated with traditional methods of gene identification, which were slow and time-consuming. He developed an alternative technique using expressed sequence tags (ESTs), small segments of deoxyribonucleic acid (DNA) found in expressed genes that are used as 'tags' to identify unknown genes in other organisms, cells, or tissues. Venter used ESTs to rapidly identify thousands of human genes. Although first received with scepticism, the approach later gained increased acceptance; in 1993 it was used to identify the gene responsible for a type of colon cancer. Venter's attempts to patent the gene fragments that he identified, however, created a furore among those in the scientific community who believed that such information belonged in the public domain.

"Venter left the NIH in 1992 and, with the backing of the for-profit company Human Genome Sciences, in Gaithersburg, MD, established a research arm, The Institute for Genomic Research (TIGR). At the institute a team headed by American microbiologist Claire Fraser, Venter's first wife, sequenced the genome of the microorganism *Mycoplasma genitalium*.

"In 1995, in collaboration with American molecular geneticist Hamilton Smith of Johns Hopkins University, in Baltimore, MD, Venter determined the genomic sequence of *Haemophilus influenzae*, a bacterium that causes earaches and meningitis in humans. The achievement marked the first time that the complete sequence of a free-living organism had been deciphered, and it was accomplished in less than a year.

"In 1998 Venter founded Celera Genomics and began sequencing the human genome. Celera relied on whole genome 'shotgun' sequencing, a rapid sequencing technique that Venter had developed while at TIGR … Celera began decoding the human genome at a faster rate than the government-run HGP. Venter's work was viewed at first with scepticism by the NIH-funded HGP group, led by geneticist Francis Collins; nevertheless, at a ceremony held in Washington, D.C., in 2000, Venter, Collins, and U.S. President Bill Clinton gathered to announce the completion of a rough draft sequence of the human genome. The announcement emphasized that the sequence had been generated through a concerted effort between Venter's private company and Collins's public research consortium. The HGP was completed in 2003.

"In addition to the human genome, Venter contributed to the sequencing of the genomes of the rat, mouse, and fruit fly. In 2006 he founded the J. Craig Venter Research Institute (JCVI), a not-for-profit genomics research support organization. In 2007, researchers funded in part by the JCVI successfully sequenced the genome of the mosquito *Aedes aegypti*, which transmits the infectious agent of yellow fever to humans.



"JCVI scientists were also fundamental in pioneering the field of synthetic biology. In this effort, Venter was again in collaboration with Smith, who headed the organization's synthetic biology and bioenergy research group. In 2008 Venter, Smith, and their JCVI colleagues created a full-length synthetic genome identical to the naturally occurring genome of the bacterium *Mycoplasma genitalium*. Two years later, Venter and his team created a synthetic copy of the genome of another bacterium, *M. mycoides*, and demonstrated that the synthetic genome was functional by transplanting it into a cell of the species *M. capricolum*. The recipient cell not only survived the transplantation procedure but also assumed the phenotypic characteristics dictated by the *M. mycoides* genome. While the synthetic research conducted by Venter and JCVI scientists was considered scientifically ground-breaking, it also raised significant concerns, particularly about the potential risks associated with the release of synthetic organisms into the environment. Nonetheless, Venter believed that synthetic organisms would ultimately prove beneficial, particularly as sources for alternative energy production" (Britannica).

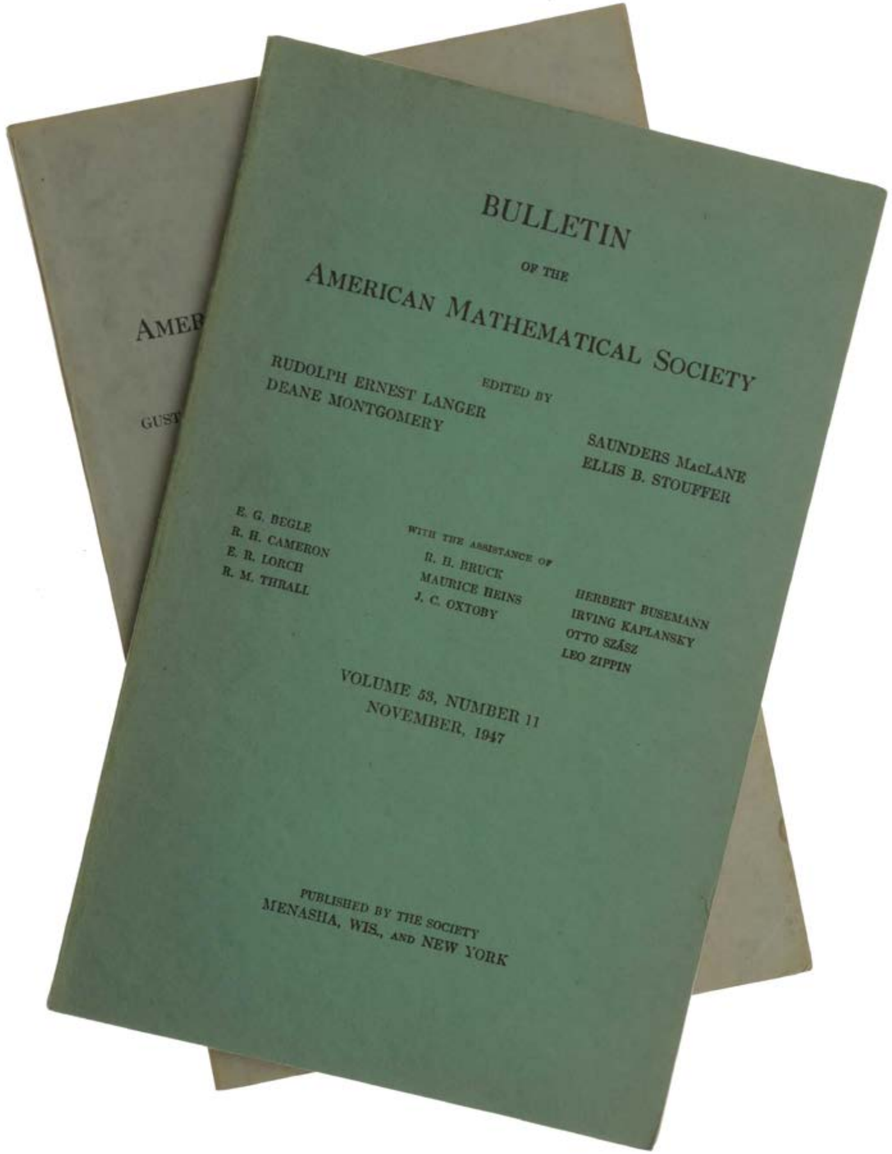
THE BIRTH CERTIFICATE OF NUMERICAL ANALYSIS

VON NEUMANN, John. & GOLDSTINE, Herman H. *'Numerical Inverting of Matrices of High Order,' pp. 1021-1099 in Bulletin of the American Mathematical Society, Vol. 53, No. 11, November, 1947. [Offered with:] 'Numerical Inverting of Matrices of High Order II,' pp. 188-202 in Proceedings of the American Mathematical Society, Vol. 2, No. 2, April, 1951.*

$2,250

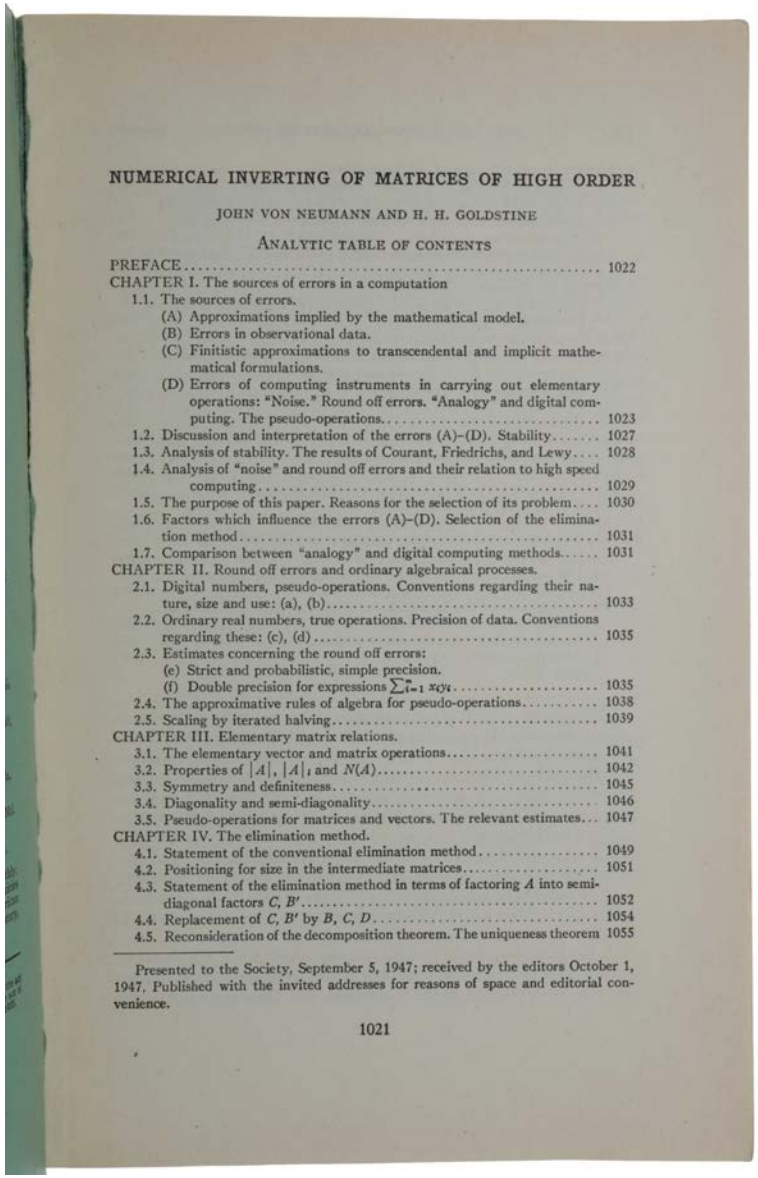
Two vols., 8vo. Part I: pp. 1021-1140 (242 x 152mm); Part II: pp. 175-334 (242 x 150mm). Original printed wrappers. Book label of Erwin Tomash inside front cover of each issue. Fine copies.

First edition, journal issues in the original printed wrappers, of two of von Neumann's major papers. "The 1947 paper by John von Neumann and Herman Goldstine, 'Numerical Inverting of Matrices of High Order' (*Bulletin of the AMS*, Nov. 1947), is considered as the birth certificate of numerical analysis. Since its publication, the evolution of this domain has been enormous" (Bultheel & Cools). "Just when modern computers were being invented (those digital, electronic, and programmable), John von Neumann and Herman Goldstine wrote a paper to illustrate the mathematical analyses that they believed would be needed to use the new machines effectively and to guide the development of still faster computers. Their foresight and the congruence of historical events made their work the first modern paper in numerical analysis. Von Neumann once remarked that to found a mathematical theory one had to prove the first theorem, which he and Goldstine did concerning the accuracy of mechanized Gaussian elimination – but their paper was about more than that. Von Neumann and Goldstine described what they surmised would be the significant questions once computers became



available for computational science, and they suggested enduring ways to answer them" (Grcar, p. 607). "In sum, von Neumann's paper contains much that is unappreciated or at least unattributed to him. The contents are so familiar, it is easy to forget von Neumann is not repeating what everyone knows. He anticipated many of the developments in the field he originated, and his theorems on the accuracy of Gaussian elimination have not been encompassed in half a century. The paper is among von Neumann's many firsts in computer science. It is the first paper in modern numerical analysis, and the most recent by a person of von Neumann's genius" (Vuik). Von Neumann & Goldstine's 1947 paper is here accompanied by its sequel (the 1947 paper comprises Chapters I-VII, the sequel Chapters VIII-IX), in which the authors reassess the error estimates proved in the first part from a probabilistic point of view. The only other copy of either paper listed on ABPC/RBH is the OOC copy of part I (both journal issue and offprint).

"Before computers, numerical analysis consisted of stopgap measures for the physical problems that could not be analytically reduced. The resulting hand computations were increasingly aided by mechanical tools which are comparatively well documented, but little was written about numerical algorithms because computing was not considered an archival contribution. "The state of numerical mathematics stayed pretty much the same as Gauss left it until World War II" [Goldstine, *The Computer from Pascal to Von Neumann* (1972), p. 287]. "Some astronomers and statisticians did computing as part of their research, but few other scientists were numerically oriented. Among mathematicians, numerical analysis had a poor reputation and attracted few specialists" [Aspray, *John von Neumann and the Origins of Modern Computing* (1999), pp. 49–50]. "As a branch of mathematics, it probably ranked the lowest, even below statistics, in terms of what most university mathematicians found interesting" [Hodges, *Alan Turing: the Enigma* (1983), p. 316].



NUMERICAL INVERTING OF MATRICES OF HIGH ORDER

JOHN VON NEUMANN AND H. H. GOLDSTINE

ANALYTIC TABLE OF CONTENTS

PREFACE 1022
CHAPTER I. The sources of errors in a computation
1.1. The sources of errors.
(A) Approximations implied by the mathematical model.
(B) Errors in observational data.
(C) Finitistic approximations to transcendental and implicit mathematical formulations.
(D) Errors of computing instruments in carrying out elementary operations: "Noise." Round off errors. "Analogy" and digital computing. The pseudo-operations 1023
1.2. Discussion and interpretation of the errors (A)–(D). Stability 1027
1.3. Analysis of stability. The results of Courant, Friedrichs, and Lewy 1028
1.4. Analysis of "noise" and round off errors and their relation to high speed computing 1029
1.5. The purpose of this paper. Reasons for the selection of its problem 1030
1.6. Factors which influence the errors (A)–(D). Selection of the elimination method 1031
1.7. Comparison between "analogy" and digital computing methods 1031
CHAPTER II. Round off errors and ordinary algebraical processes.
2.1. Digital numbers, pseudo-operations. Conventions regarding their nature, size and use: (a), (b) 1033
2.2. Ordinary real numbers, true operations. Precision of data. Conventions regarding these: (c), (d) 1035
2.3. Estimates concerning the round off errors:
(e) Strict and probabilistic, simple precision.
(f) Double precision for expressions $\sum_{i=1}^{n} x_i y_i$ 1035
2.4. The approximative rules of algebra for pseudo-operations 1038
2.5. Scaling by iterated halving 1039
CHAPTER III. Elementary matrix relations.
3.1. The elementary vector and matrix operations 1041
3.2. Properties of $|A|$, $|A|_l$ and $N(A)$ 1042
3.3. Symmetry and definiteness 1045
3.4. Diagonality and semi-diagonality 1046
3.5. Pseudo-operations for matrices and vectors. The relevant estimates 1047
CHAPTER IV. The elimination method.
4.1. Statement of the conventional elimination method 1049
4.2. Positioning for size in the intermediate matrices 1051
4.3. Statement of the elimination method in terms of factoring A into semi-diagonal factors C, B' 1052
4.4. Replacement of C, B' by B, C, D 1054
4.5. Reconsideration of the decomposition theorem. The uniqueness theorem 1055

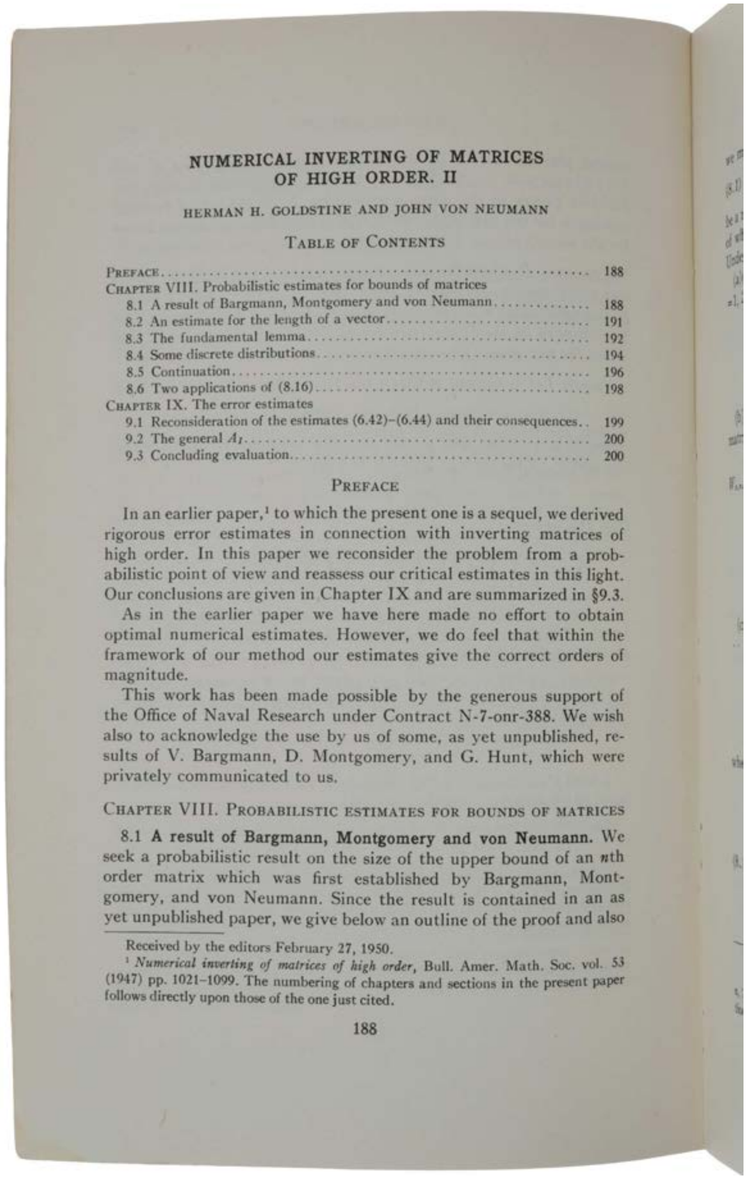
Presented to the Society, September 5, 1947; received by the editors October 1, 1947. Published with the invited addresses for reasons of space and editorial convenience.

1021

"In this environment John von Neumann and Herman Goldstine wrote the first modern paper on numerical analysis, 'Numerical Inverting of Matrices of High Order', and they audaciously published the paper in the journal of record for the American Mathematical Society. The inversion paper was part of von Neumann's efforts to create a mathematical discipline around the new computing machines. Gaussian elimination was chosen to focus the paper, but matrices were not its only subject. The paper was the first to distinguish between the stability of a mathematical problem and of its numerical approximation, to explain the significance in this context of the 'Courant criterium' (later CFL condition), to point out the advantages of computerized mixed precision arithmetic, to use a matrix decomposition to prove the accuracy of a calculation, to describe a 'figure of merit' for calculations that became the matrix condition number, and to explain the concept of inverse, or backward, error. The inversion paper thus marked the first appearance in print of many basic concepts in numerical analysis.

"The inversion paper may not be the source from which most people learn of von Neumann's ideas, because he disseminated his work on computing almost exclusively outside refereed journals. Such communication occurred in meetings with the many researchers who visited him at Princeton and with the staff of the numerous industrial and government laboratories whom he advised, in the extemporaneous lectures that he gave during his almost continual travels around the country, and through his many research reports which were widely circulated, although they remained unpublished. As von Neumann's only archival publication about computers, the inversion paper offers an integrated summary of his ideas about a rapidly developing field at a time when the field had no publication venues of its own.

"The inversion paper was a seminal work whose ideas became so fully accepted



NUMERICAL INVERTING OF MATRICES OF HIGH ORDER. II

HERMAN H. GOLDSTINE AND JOHN VON NEUMANN

TABLE OF CONTENTS

PREFACE 188
CHAPTER VIII. Probabilistic estimates for bounds of matrices
8.1 A result of Bargmann, Montgomery and von Neumann 188
8.2 An estimate for the length of a vector 191
8.3 The fundamental lemma 192
8.4 Some discrete distributions 194
8.5 Continuation 196
8.6 Two applications of (8.16) 198
CHAPTER IX. The error estimates
9.1 Reconsideration of the estimates (6.42)–(6.44) and their consequences 199
9.2 The general A_I 200
9.3 Concluding evaluation 200

PREFACE

In an earlier paper,[1] to which the present one is a sequel, we derived rigorous error estimates in connection with inverting matrices of high order. In this paper we reconsider the problem from a probabilistic point of view and reassess our critical estimates in this light. Our conclusions are given in Chapter IX and are summarized in §9.3.

As in the earlier paper we have here made no effort to obtain optimal numerical estimates. However, we do feel that within the framework of our method our estimates give the correct orders of magnitude.

This work has been made possible by the generous support of the Office of Naval Research under Contract N-7-onr-388. We wish also to acknowledge the use by us of some, as yet unpublished, results of V. Bargmann, D. Montgomery, and G. Hunt, which were privately communicated to us.

CHAPTER VIII. PROBABILISTIC ESTIMATES FOR BOUNDS OF MATRICES

8.1 **A result of Bargmann, Montgomery and von Neumann.** We seek a probabilistic result on the size of the upper bound of an nth order matrix which was first established by Bargmann, Montgomery, and von Neumann. Since the result is contained in an as yet unpublished paper, we give below an outline of the proof and also

Received by the editors February 27, 1950.

[1] *Numerical inverting of matrices of high order*, Bull. Amer. Math. Soc. vol. 53 (1947) pp. 1021–1099. The numbering of chapters and sections in the present paper follows directly upon those of the one just cited.

188

that today they may appear to lack novelty or to have originated with later authors who elaborated on them more fully. It is possible to trace many provenances to the paper by noting the sequence of events, similarities of presentation, and the context of von Neumann's activities" (Grcar, pp. 609-610).

We are fortunate to have an account of the genesis and content of these two important papers in Goldstine's own words. In the years immediately following the end of World War II, Von Neumann, Goldstine and others instituted the 'electronic computer project' at the Institute for Advanced Study at Princeton, NJ. One of the first topics discussed "was the solution of large systems of linear equations, since they arise almost everywhere in numerical work. V. Bargmann and D. Montgomery collaborated with von Neumann on a paper on this subject. Then H. Hotelling, the well-known statistician, wrote an interesting paper in 1943 in which he studied a number of numerical procedures, including the Gaussian method for inverting matrices. He pointed out in a heuristic and as it turned out, inaccurate, way that the Gaussian method for inverting statistical correlation matrices [of order n] would require about $k + 0.6n$ digits during the computation to obtain k-digit accuracy. Thus to invert a matrix of order 100 would in his terms require 70 digits to be used if one wanted 10-digit accuracy.

"Johnny and I never quite believed that Gauss would have used a procedure so lacking in elegance, given his great love for computation. Indeed, his collected works contain a considerable amount of material on both astronomy and geodesy that shows his love for and great skill in computation. As some partial evidence of this, we know he certainly used the so-called Cooley-Tukey method to handle Fourier transforms. Taking his skill as given, we looked closely at the procedure and wrote a paper on the subject that we used as an elaborate introduction to errors in numerical calculation [the first offered paper]. We tried in that paper to alert the practitioners in the field to a phenomenon that had not been particularly relevant in the past but was to be a constant source of anxiety in the future: numerical instability. In the course of the analysis we also brought to the fore the now obvious notion of well- and ill-conditioned matrices …

"In a second paper we raised a question that we thought might become more important than in fact it ever became [second offered paper]. We said, let us not worry so much about what might happen in a very small number of pathological cases; instead let us see what occurs on the average, what we can expect if we need to do this same task many times. To achieve this probabilistic result I had to develop proofs for several theorems in probability theory, which I did with considerable difficulty, only to receive a letter from a statistician named Mulholland after the paper appeared in which he showed me how to do one part with the slightest work: A mere flip of the wrist sufficed to demonstrate some obvious thing. My only consolation was that Johnny had not seen how to do it simply either. In the even, I suppose that our second paper sacred practitioners of the subject away from the field of probabilistic estimates instead of bringing them in" (Goldstine, pp. 10-11).

"Von Neumann and Goldstine's paper [I] has been called the first in this 'modern' numerical analysis because it is the first to study rounding error and because much of the paper is an essay on scientific computing (albeit with an emphasis on numerical linear algebra). The list of error sources in Chapter 1 is clearer and more authoritative than any since. The axiomatic treatment of rounding error in Chapter 2 inspired later analyses. The discussion of linear algebra in Chapters 3 to 5 contains original material, including the invention of triangular factoring … The rounding error analysis in Chapter 6 accounts for just one-quarter of the paper, with the analysis of triangular factoring a fraction of that … The bulk of Chapter 6 then bounds the residual of inverting symmetric positive definite matrices … The concluding Chapter 7 interprets the rounding error analysis … The paper closes by evaluating the residual bound for "random" matrices, and by counting arithmetic operations" (Vuik).

Hook & Norman, *Origins of Cyberspace* 957 (first part only). Bultheel & Cools (eds.), *The Birth of Numerical Analysis*, 2009; Goldstine, 'Remembrance of things past,' in *A History of Scientific Computing*, Stephen G. Nash (ed.), pp. 5-16; *DSB* XIV, 92; Grcar, 'John von Neumann's Analysis of Gaussian Elimination: the Founding of Modern Numerical Analysis,' *SIAM Review* 53 (2011), 607-682; Kees Vuik, *Birthday of Modern Numerical Analysis*, (ta.twi.tudelft.nl/users/vuik/wi211/num_anal.html).

A VERY FINE SET OF THE DNA-PAPERS

WATSON, J. D. & CRICK, F. H. C. *Molecular Structure of Nucleic Acids: A Structure for Deoxyribose Nucleic Acid*; **WILKINS, M. H. F., STOKES, A. R. & WILSON, H. R.** *Molecular Structure of Deoxypentose Nucleic Acids*; **FRANKLIN, R. E. & GOSLING, R. G.** *Molecular Configuration in Sodium Thymonucleate*, pp. 737-41 in Nature, Vol. 171, No. 4356, April 25, 1953. [WITH:] **WATSON, J. D. & CRICK, F. H. C.** *Genetical Implications of the Structure of Deoxyribonucleic Acid*, pp. 964-7 in Nature, Vol. 171, No. 4361, May 30, 1953. [WITH:] **FRANKLIN, R. E. & GOSLING, R. G.** *Evidence for 2-Chain Helix in Crystalline Structure of Sodium Deoxyribonucleate*, pp. 156-7 in Nature, Vol. 172, No. 4369, July 25, 1953. [WITH:] **WILKINS, M. H. F., SEEDS, W. E. STOKES, A. R. & WILSON, H. R.** *Helical Structure of Crystalline Deoxypentose Nucleic Acid*, pp. 759-62 in Nature, Vol. 172, No. 4382, October 24, 1953; [WITH:] **PAULING, L. & COREY, R. B.** *Structure of the Nucleic Acids*, p. 346 in Nature, Vol. 171, No. 4347, February 21, 1953.

$16,500

Five complete journal issues, 4to, 4347: pp. cxiii-cxxii, 317-336, i-xii, 337-358, cxiii-cxxx; 4356: pp. cclxix-cclxxviii, 709-732, i-xii, 733-758, cclxxix-cclxxxvi; 4361: pp. ccclv-ccclxii, 943-964, i-xvi, 965-986, ccclxiii-ccclxx; 4369: pp. li-lx, 131-150, i-xii, 151-172, lxi-lxviii; 4382: pp. ccxciii-ccc, 737-758, i-xvi, 759-780, ccci-cccviii. Original printed wrappers. A virtually mint set. Rare in such fine condition.

First edition of Watson & Crick's paper which "records the discovery of the molecular structure of deoxyribonucleic acid (DNA), the main component of chromosomes and the material that transfers genetic characteristics in all life forms. Publication of this paper initiated the science of molecular biology. Forty

years after Watson and Crick's discovery, so much of the basic understanding of medicine and disease has advanced to the molecular level that their paper may be considered the most significant single contribution to biology and medicine in the twentieth century" (*One Hundred Books Famous in Medicine*, p. 362). Watson & Crick's paper is here accompanied by their paper published one month later "in which they elaborated on their proposed DNA replication mechanism" (*ibid.*), together with the four papers which provided the experimental data on which their proposed structure was based, and further data confirming its correctness. In 1962, Watson, Crick, and Wilkins shared the Nobel Prize in Physiology or Medicine "for their discoveries concerning the molecular structure of nucleic acids and its significance for information transfer in living material."

DNA was first isolated by the Swiss physician Friedrich Miescher in 1869, and over the succeeding years many researchers investigated its structure and function, with some arguing that it may be involved in genetic inheritance. By the early 1950s this had become one of the most important questions in biology. Maurice Wilkins of King's College London and his colleague Rosalind Franklin were both working on DNA, with Franklin producing X-ray diffraction images of its structure. Wilkins also introduced his friend Francis Crick to the subject, and Crick and his partner James Watson began their own investigation at the Cavendish Laboratory in Cambridge, focusing on building molecular models. After one failed attempt in which they postulated a triple-helix structure, they were banned by the Cavendish from spending any additional time on the subject. But a year later, after seeing new X-ray diffraction images taken by Franklin (notably the famous 'Photo 51', which is reproduced in the third offered paper), they resumed their work and soon announced that not only had they discovered the double-helix structure of DNA, but even more importantly, that "the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material."

"When Watson and Crick's paper was submitted for publication in *Nature*, Sir Lawrence Bragg, the director of the Cavendish Laboratory at Cambridge, and Sir John Randall of King's College agreed that the paper should be published simultaneously with those of two other groups of researches who had also prepared important papers on DNA: Maurice Wilkins, A.R. Stokes, and H.R. Wilson, authors of "Molecular Structure of Deoxypentose Nucleic Acids," and Rosalind Franklin and Raymond Gosling, who submitted the paper "Molecular Configuration in Sodium Thymonucleate." The three papers were published in *Nature* under the general title "The Molecular Structure of Nucleic Acids." Shortly afterwards, Watson and Crick published their paper "Genetical Implication of the Structure of Deoxyribonucleic Acid," in which they elaborated on their proposed DNA replication mechanism" (*ibid.*). In this last paper Watson & Crick state their conclusion simply and elegantly: "we feel that our proposed structure for deoxyribonucleic acid may help solve one of the fundamental biological problems--the molecular basis of the template needed for genetic replication." The two papers in Vol. 172 provide confirmation of the double-helix structure based on further X-ray diffraction data.

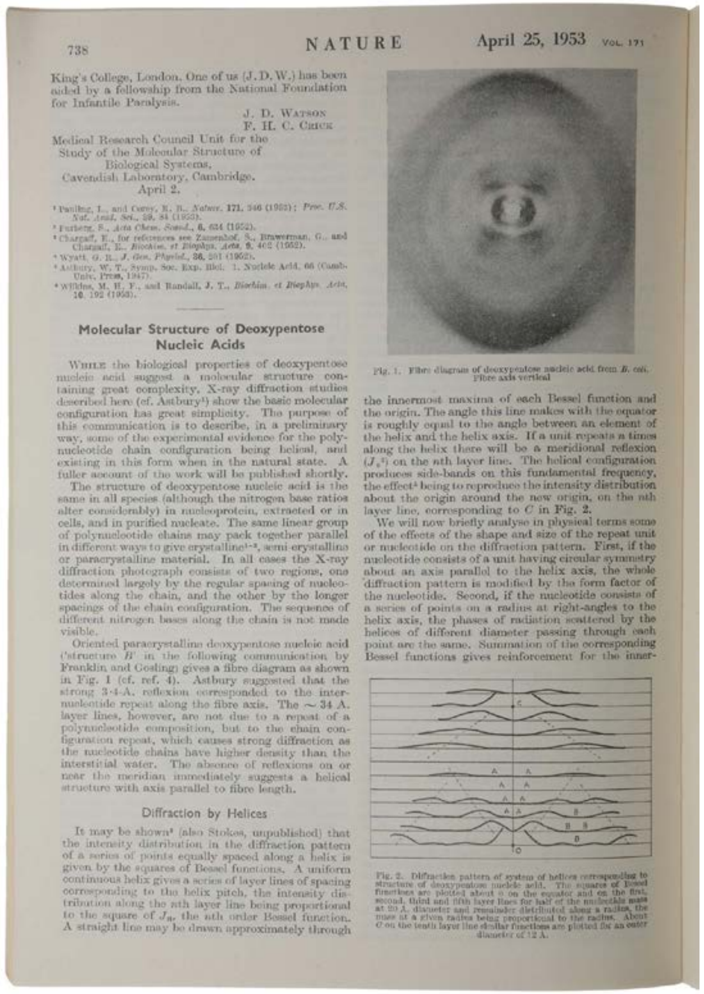
The papers of the Cambridge and King's College, London scientists are here accompanied by an earlier attempt at elucidating the structure of DNA by the great Caltech chemist Linus Pauling, who had already solved the secondary structure of proteins. Pauling's hypothetical DNA structure – a triple helix with the phosphates in the middle and the bases radiating outwards – was similar to one Watson and Crick had first advanced a year earlier and then rejected on both chemical and physical grounds. It failed to accommodate Chargaff's observation that the abundance of A in DNA approximately equals T, and C equals G; it also fails to explain the biology and replication of DNA. Watson described his feelings upon reading the Pauling manuscript in *The Double Helix* (p. 102): "At once I felt something was not right. I could not pinpoint the mistake, however, until I looked at the illustrations for several minutes. Then I realized that the phosphate groups in Linus' model were not ionized, but that each group contained a bound hydrogen atom and so had no net charge. Pauling's nucleic acid in a sense was not an acid at all. Moreover, the uncharged phosphate groups were not incidental features. The hydrogens were part of the hydrogen bonds that held together the three intertwined chains. Without the hydrogen atoms, the chains would immediately fly apart and the structure vanish."

The realization that Pauling was not, as they had feared, on the right track gave Watson and Crick the green light to pursue their own model of DNA. A few days after first seeing their structure, Pauling received an advance copy of the Watson/Crick manuscript. In a letter to Watson and Crick written on March 27, 1953, Pauling noted:

"I think that it is fine that there are now two proposed structures for nucleic acid, and I am looking forward to finding out what the decision will be as to which is incorrect."

However, he had still not seen Rosalind Franklin's data; Watson and Crick had. (Interestingly enough, Robert Corey had traveled to England in 1952 and viewed Franklin's photographs. It is unknown whether or not he purposely failed to provide Pauling with the details of the images.) In April 1953, on his way to a conference in Belgium, Pauling stopped in England to see the Watson and Crick model of DNA as well as Franklin's photographs. After examining both, Pauling was finally convinced that his structure was wrong and that Watson and Crick had found the correct DNA structure. For Pauling, this event was a single failure in a sea of successes. In fact, the very next year, he would win the Nobel Prize in Chemistry – the first of his two Nobel Prizes.

Grolier Club, *One Hundred Books Famous in Medicine*, 99; Dibner, *Heralds of Science*, 200. Garrison-Morton 256.3; Judson, *Eighth Day of Creation*, pp. 145-56.



738 NATURE April 25, 1953 VOL. 171

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J. D. WATSON
F. H. C. CRICK

Medical Research Council Unit for the Study of the Molecular Structure of Biological Systems, Cavendish Laboratory, Cambridge. April 2.

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Molecular Structure of Deoxypentose Nucleic Acids

WHILE the biological properties of deoxypentose nucleic acid suggest a molecular structure containing great complexity, X-ray diffraction studies described here (cf. Astbury[1]) show the basic molecular configuration has great simplicity. The purpose of this communication is to describe, in a preliminary way, some of the experimental evidence for the polynucleotide chain configuration being helical, and existing in this form when in the natural state. A fuller account of the work will be published shortly.

The structure of deoxypentose nucleic acid is the same in all species (although the nitrogen base ratios alter considerably) in nucleoprotein, extracted or in cells, and in purified nucleate. The same linear group of polynucleotide chains may pack together parallel in different ways to give crystalline[1-3], semi-crystalline or paracrystalline material. In all cases the X-ray diffraction photograph consists of two regions, one determined largely by the regular spacing of nucleotides along the chain, and the other by the longer spacings of the chain configuration. The sequence of different nitrogen bases along the chain is not made visible.

Oriented paracrystalline deoxypentose nucleic acid ('structure B' in the following communication by Franklin and Gosling) gives a fibre diagram as shown in Fig. 1 (cf. ref. 4). Astbury suggested that the strong 3·4-A. reflexion corresponded to the internucleotide repeat along the fibre axis. The ~ 34 A. layer lines, however, are not due to a repeat of a polynucleotide composition, but to the chain configuration repeat, which causes strong diffraction as the nucleotide chains have higher density than the interstitial water. The absence of reflexions on or near the meridian immediately suggests a helical structure with axis parallel to fibre length.

Diffraction by Helices

It may be shown[5] (also Stokes, unpublished) that the intensity distribution in the diffraction pattern of a series of points equally spaced along a helix is given by the squares of Bessel functions. A uniform continuous helix gives a series of layer lines of spacing corresponding to the helix pitch, the intensity distribution along the *n*th layer line being proportional to the square of J_n, the *n*th order Bessel function. A straight line may be drawn approximately through

Fig. 1. Fibre diagram of deoxypentose nucleic acid from *B. coli*. Fibre axis vertical

the innermost maxima of each Bessel function and the origin. The angle this line makes with the equator is roughly equal to the angle between an element of the helix and the helix axis. If a unit repeats *n* times along the helix there will be a meridional reflexion (J_0^2) on the *n*th layer line. The helical configuration produces side-bands on this fundamental frequency, the effect[5] being to reproduce the intensity distribution about the origin around the new origin, on the *n*th layer line, corresponding to *C* in Fig. 2.

We will now briefly analyse in physical terms some of the effects of the shape and size of the repeat unit or nucleotide on the diffraction pattern. First, if the nucleotide consists of a unit having circular symmetry about an axis parallel to the helix axis, the whole diffraction pattern is modified by the form factor of the nucleotide. Second, if the nucleotide consists of a series of points on a radius at right-angles to the helix axis, the phases of radiation scattered by the helices of different diameter passing through each point are the same. Summation of the corresponding Bessel functions gives reinforcement for the inner-

Fig. 2. Diffraction pattern of system of helices corresponding to structure of deoxypentose nucleic acid. The squares of Bessel functions are plotted about 0 on the equator and on the first, second, third and fifth layer lines for half of the nucleotide mass at 20 A. diameter and remainder distributed along a radius, the mass at a given radius being proportional to the radius. About *C* on the tenth layer line similar functions are plotted for an outer diameter of 12 A.